Recessions, Mortality, and Migration Bias: Evidence from the Lancashire Cotton Famine*

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Abstract

We examine the health effects of the Lancashire Cotton Famine, a sharp downturn in Britain's cotton textile manufacturing regions that was induced by the U.S. Civil War. Migration was an important response to this downturn, but as we document, migration also introduces a number of empirical challenges, which we overcome by introducing a new methodological approach. Our results indicate that the recession increased mortality among households employed in the cotton textile industry. We also document localized spillover effects on households providing non-tradeable services in the areas affected by the recession.

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1 Introduction

We examine the health consequences of the Lancashire "Cotton Famine," a large, temporary, and negative economic shock to the cotton textile manufacturing regions of England and Wales caused by the U.S. Civil War.[1] On the eve of the war, cotton textile production was Britain's most important industrial sector, employing 2.3% of the total population and accounting for 9.5% of the manufacturing workforce. This sector, however, was entirely reliant on raw cotton imports, and 70% of those imports came from the U.S. South. The Civil War disrupted this flow of cotton, generating a sharp and geographically-concentrated economic contraction that displaced hundreds of thousands of mill workers.

The magnitude of this economic shock, and its importance in British history, has attracted attention from researchers for many years (Arnold, 1864; Watts, 1866; Ellison, 1886; Henderson, 1934; Farnie, 1979). Recent studies have examined the impact on marriage rates (Southall & Gilbert, 1996), poor relief (Boyer, 1997; Kiesling, 1996), innovation (Hanlon, 2015), and long-run city growth (Hanlon, 2017). Despite this attention, the human costs of the cotton shock remain debated. Even contemporary reports are contradictory: some observers remarked on the "wan and haggard look about the people," while at the same time local health officers reported a "lessened death-rate throughout nearly the whole of the [cotton] districts."[2]

Qualitative accounts from the time suggest that many displaced workers chose to migrate in search of work elsewhere—and as we show, accounting for this migration is a key challenge to estimating the health effects of this downturn. In the first part of our paper, we corroborate these contemporary reports by providing evidence of

[1] Historians often refer to this event as the "Cotton Famine," where the term "famine" is used metaphorically to describe the dearth of cotton inputs. In this paper we largely avoid this term since it can be misleading in a study focused on health.

[2] The first quote comes from Dr. Buchanan, *Report on the Sanitary Conditions of the Cotton Towns*, Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 301. The second quote is from Arnold (1864).

substantial and systematic out-migration from the cotton textile districts in response to the cotton shortage.[3]

While migration is a natural response to changes in local economic conditions, the existing literature on recessions and health offers little guidance for how to overcome the empirical challenges introduced by migration.[4] The fundamental issue is as follows. A typical mortality-rate calculation normalizes death counts by the area's underlying population. Population counts, however, are generally only well-measured in census years (i.e., decennially), whereas death counts are reported more frequently (e.g., annually). Thus, if recessions induce migration, and if these movements are not perfectly captured in intercensal population estimates, unobserved migration can change the size and composition of a location's true at-risk population relative to what is observed, generating a spurious change in mortality rates that we will misinterpret as reflecting the true impact of local shocks on health. A second issue introduced by migration is spillovers: to the extent that individuals migrate towards areas offering better economic opportunities, we are likely to observe migration between treatment and putative control locations, which has the potential to bias coefficient estimates obtained in panel-data regressions.

We adopt an empirical strategy that leverages two features of this setting in order to overcome the identification challenges introduced by migration. The first feature is the plausibly exogenous timing and spatial incidence of the shock, which allows us both to cleanly identify the cohorts exposed to the downturn, and to better isolate and correct for spatial spillovers due to migration. The temporal component of the

[3]Our most conservative estimates suggest the population of cotton-textile producing regions fell by 2.2% during the downturn. As a point of comparison, Fishback *et al.* (2006) report that 11% of the U.S. population moved during the Great Depression, with 60% of moves occurring within state.

[4]The existing literature tends to assume that migration is not a meaningful threat to inference. Lindo (2015), however, shows that estimates of the recession-mortality relationship differ depending on the level of aggregation in the analysis (e.g., whether we examine county vs. state-level data). Lindo posits that this may be due to migration, but he is not able to rule out other possibilities. The features of our setting allow us to construct estimates of the recession-mortality relationship that differ only based on whether they account for migration. Thus, we are able to explicitly test the extent to which migration can undermine inference.

economic shock was short, sharp, and generated by outside forces that were largely unexpected. Meanwhile, because the shock was transmitted through the cotton textile industry, its direct effects were concentrated in locations where the firms in that industry clustered, a spatial pattern due to underlying natural endowments. A second and equally important feature of our setting is that it allows us to draw on comprehensive, individually-identified, and publicly available census and death records for all of England and Wales. We link these sources to construct a large sample of longitudinal microdata that allows us to follow individuals across time and space. We leverage these features to answer two main questions. First, what impact did this recession have on health, and through what channels? Second, would our estimates of these effects fundamentally differ if we were unable to overcome the bias introduced by migration?

To answer these questions, we begin by defining the cohorts directly at risk of exposure to the downturn: those residing in major cotton textile-producing areas of Britain on the eve of the U.S. Civil War, as enumerated in the 1861 British census.[5] We then link those individuals to deaths occurring during the downturn (1861-1865) regardless of where those deaths occurred.[6] This process produces an individual-level longitudinal dataset that allows us to hold the size and composition of cohorts fixed, and thus to accurately identify mortality patterns for the group initially resident in cotton locations, relative to residents of other locations, irrespective of where they may have subsequently migrated and died. Conducting a similar linking exercise for the 1851 census (linked to 1851-1855 deaths) allows us to adopt a difference-in-differences framework to recover a causal effect. Next, we deal with the potential

[5] The 1861 British census was taken just before the onset of the U.S. Civil. Historical evidence makes it clear that people in both the U.S. and abroad failed to anticipate the severity of the conflict (one contemporary observer, Arnold (1864), wrote that (p. 40) the bombardment of Fort Sumter "took the world by surprise") and there is little evidence that the British economy was substantially affected until late 1861 or early 1862.

[6] Our linking approach, which we discuss further in Section 3.6, follows seminal papers in this literature (e.g., Ferrie (1996), Abramitzky *et al.* (2012, 2014), Feigenbaum (2015, 2016), and Bailey *et al.* (2020)).

spillovers between migrant-sending and migrant-receiving areas in the following way. First, we provide evidence that during the downturn, large numbers migrated out of the cotton districts and into nearby non-cotton districts, mostly within a 25 km radius. Given this spatial concentration, we then separately estimate the mortality effects of the cotton shortage on each of these sets of districts, relative to a third set of more distant control districts which offer a cleaner counterfactual.

Our analysis generates three main sets of findings. First, we show that the cotton shortage had an adverse impact on mortality for the population initially residing in cotton districts at the time of the shock, especially for the elderly. We estimate that the shock generated around 24,000 excess deaths within the cotton textile districts, equal to 9.5% of total deaths. Around 10,000 of these occurred among those aged 55 or over, an increase in deaths of 18.8% for that age group. This substantial increase in mortality stands in contrast to existing research on modern developed economies. That literature, which we discuss below, consistently finds that health improves during recessions. Our findings indicate that this relationship may be very different in settings with weaker social safety nets and higher baseline mortality.

Second, we provide new evidence examining the impact of the cotton shortage on those households reliant on the industry for employment and those households that did not work in the cotton textile sector but resided in locations where it was the main employer. This is possible given the richness of our longitudinal microdata, which contain detailed information on occupations and family structure. Our direct visibility into the household is novel in this literature, and our results show both that cotton workers, and the family members of cotton workers, experienced substantial mortality increases as a result of the shock. However, we also show substantial effects among non-cotton households residing in the cotton textile areas. Thus, in addition to treatment through employment, we observe substantial treatment through location. Digging deeper, we find evidence that the effect of the shock on non-cotton households in cotton regions was particularly severe for those providing non-traded local services, as well as those working in sectors sharing input-output linkages to the cotton textile sector. This evidence provides a richer view of how a shock to one important industry can ripple through a local economy.

Finally, we document the importance of our empirical approach for overcoming the bias introduced by migration. Our methods allow us to isolate the impact of migration from other factors, enabling us to provide the first direct evidence of the impact of unobserved migration on estimates of the recession-mortality relationship. We find that this impact is substantial in our setting: while our main linked microdata results show that the downturn raised mortality rates, when we intentionally ignore migration, by inferring treatment status based on the location of death, and thus adopting a data structure similar to what is commonly used in the literature—we fail to recover this effect. Indeed, in some cases, we find the opposite result. Thus, addressing migration bias substantially alters the conclusions that we draw, as the recession would have looked much healthier had we not adequately dealt with these issues.[7]

The methodological approach that we apply to deal with the impact of migration has the potential to be useful for studying the relationship between recessions and mortality in other settings where migratory responses are prevalent. Work on modern developed countries suggests that recessions improve health through channels such as increasing exercise, reducing smoking and alcohol use (Ruhm, 2000; Ruhm & Black, 2002; Ruhm, 2005), and freeing up time to care for children and the elderly (Dehejia & Lleras-Muney, 2004; Ruhm, 2000; Aguiar *et al.*, 2013; Stevens *et al.*, 2015). A number of these studies use aggregate-data methods following Ruhm (2000). Our results suggest that, in cases where migration is a meaningful margin of adjustment, it is important to deal with this source of bias in order to accurately measure the recession-mortality relationship. To that end, the techniques we introduce offer a

[7]This offers an explanation for the disparate assessments of local versus national contemporaries, the former of whom described a reduction in deaths in the cotton districts, and the latter of whom attested to considerable suffering among out-of-work cotton operatives and their families.

simple and intuitive solution for researchers faced with similar challenges.

Our results also extend our understanding of the relationship between recessions and mortality into a historical setting characterized by high baseline mortality rates, a poor infectious disease environment, limited medical care, and weak social safety nets. While there is a large literature on the relationship between business cycles and health, most of the evidence on how temporary income fluctuations affect health across the age distribution comes from analysis of developed countries. Much less evidence is available from low-income settings (Miller & Urdinola (2010) being a notable example), and only a few studies (Fishback *et al.*, 2007; Stuckler *et al.*, 2012) examine the impact of recessions on mortality in historical contexts such as the one we consider.[8] Augmenting this existing evidence is useful because it can help us begin to map out how and why the recession-mortality relationship varies within and across settings. In addition, our ability to harness extremely rich data, and to deal with potential migration bias concerns, enables us to push our results beyond what has been possible in these previous studies—by, for example, separating out "occupation" and "location" effects.

2 Empirical setting

2.1 The timing and incidence of the cotton shortage

The cotton textile industry was the largest and most important industrial sector of the British economy during the 19th century. For historical reasons, British cotton textile production was geographically concentrated in the Northwest counties of Lancashire and Cheshire, which held over 80% of the cotton textile workers in England & Wales in

[8]There is, of course, a related historical literature on longer-term income fluctuations and mortality (i.e., the Malthusian Trap). Our paper differs substantially from this literature in that we are focused on economic fluctuations occuring over short time-scales, while that literature focuses on changes over long periods.

1861.[9] This concentration, which dates back to at least 1830, is thought to be driven by the location of rivers, which were used for power; access to the port of Liverpool; and a history of textile innovation in the 18th century (Crafts & Wolf, 2014). Figure 1 depicts this spatial distribution by plotting the share of employment accounted for by the cotton textile industry in each district using data from the Census of Population of 1851.

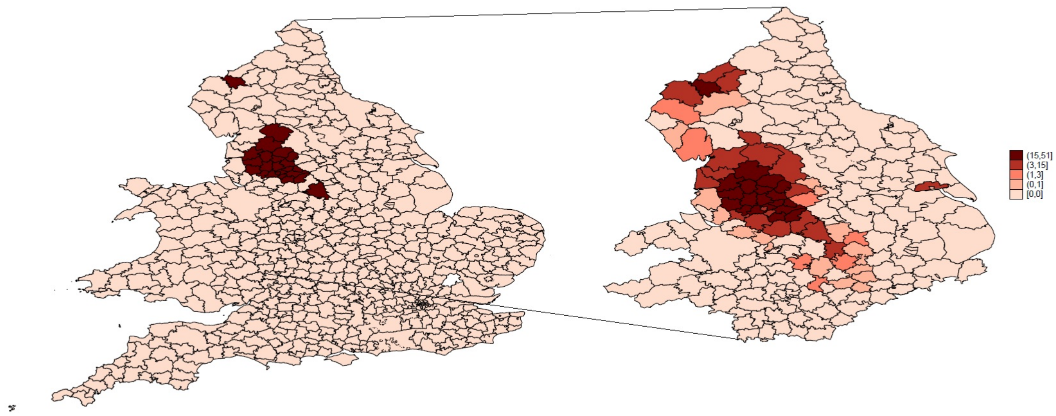
Because Britain did not produce cotton, the success of its cotton textile industry was dependent on reliable access to imported raw cotton—and in the run-up to the U.S. Civil War, 70% of these inputs came from the U.S. South (Mitchell, 1988). The war prompted a sudden and dramatic rise in world cotton prices, sharply reducing British imports of U.S. cotton, and causing a sharp drop in British cotton textile production. These effects are depicted in Figure 2. During the U.S. Civil War period, other cotton-producing countries such as India, Egypt, and Brazil rapidly increased their output, and British inventors produced new technologies to make use of these new sources of supply (Hanlon, 2015). Nevertheless, these increases were not large enough to offset the lost U.S. supplies, although they did contribute to the rapid rebound in imports after 1865.[10]

The direct effects of the U.S. Civil War were largely confined to the cotton textile sector and the districts where it was located, and there is little evidence of a broader reallocation of economic activity. One indicator of this is that there was little effect on imports or exports other than those associated with textiles (see Appendix A.2). Another factor was that the cotton textile industry had very weak input-output connections (Thomas, 1987; Horrell *et al.*, 1994). Almost all inputs were imported, with the exception of machinery (which was produced in the cotton textile districts) and coal. Downstream, some output was sold to clothing producing firms, though much

[9]Calculation based on data collected by the authors from the 1861 Census of Population reports.

[10]Consistent with this, alternative proxies for industry output (firms' raw cotton consumption and variable operating costs (excluding cotton)) exhibit a similar pattern. See, Hanlon (2015) and Mitchell & Deane (1962) on cotton consumption and Forwood (1870) for wage and cost data.

Figure 1: Spatial distribution of cotton textile industry



Data on the geography of the cotton textile industry are calculated from the 1851 Census of Population. Shaded in the map of England & Wales are districts with over 10% of employment in cotton, while the inset shows the percent of employment in cotton textiles in the core cotton region, with darker colors indicating a greater share of employment in cotton.

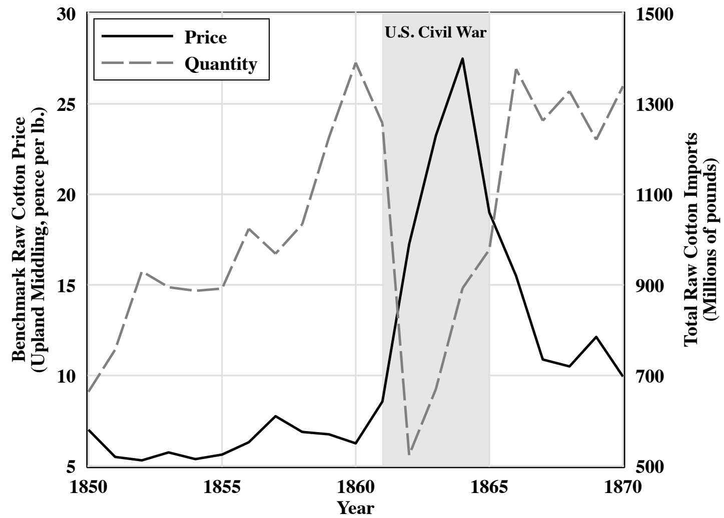
was exported or sold directly to households. As a consequence, the cotton shortage did not lead to a larger nationwide recession (Henderson, 1934, p. 20).

Figure 3 offers additional support for this conclusion. This graph describes the expenditures by local Poor Law boards in the main cotton textile counties (Lancashire and Cheshire) across the study period. For comparison, we also present data for nearby Yorkshire County, which was not heavily dependent on cotton textile production, as well as for the remainder of the country. During the downturn, we see an increase in Poor Law expenditures in the cotton textile areas, while the remainder of the country was largely unaffected. Appendix A.1 shows that similar patterns are observed if we focus on the number of able-bodied relief seekers, rather than Poor Law expenditures.

2.2 Responses to the cotton shortage

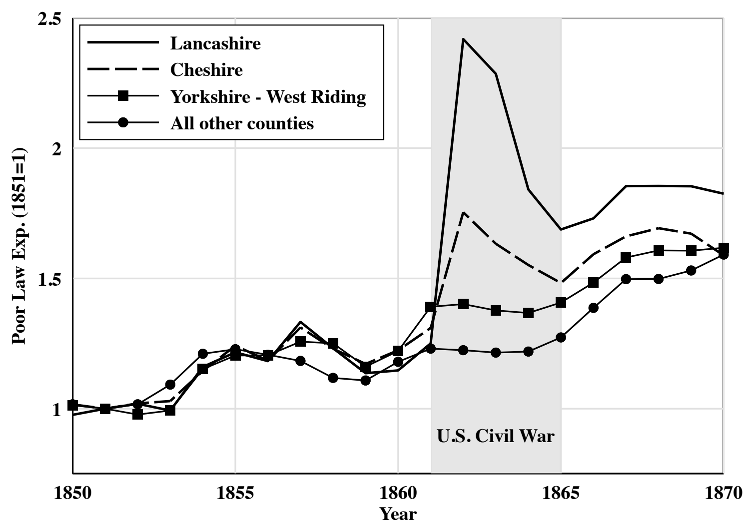
During the downturn, workers in the affected areas adopted a variety of coping mechanisms. Reports indicate that at the height of the recession (winter 1862), roughly

Figure 2: Cotton prices and imports



Import data from Mitchell (1988). Price data, from Mitchell & Deane (1962), are for the benchmark Upland Middling variety.

Figure 3: The spatial incidence of the cotton shock



Data collected by the authors from the annual reports of the Poor Law Board.

500,000 persons in cotton-producing regions depended on public relief funds, with over 270,000 of these supported by the local Poor Law boards, and an additional 230,000 reliant on private charities (Arnold, 1864, p. 296).[11] This relief, however, differs sharply from the social safety nets of today. Poor Law funds were associated with pauperism and only provided for the barest level of subsistence. They also required "labour tests" such as rock-breaking, which workers found demeaning. Indeed, there is evidence that workers tried to avoid drawing on this stigmatized source of support (Kiesling, 1996; Boyer, 1997). Instead, displaced workers tended to respond by reducing consumption and dipping into any available savings. Once their savings were depleted, workers pawned or sold items of value, including furniture, household goods, clothing, and bedding (Watts 1866, p. 214; Arnold 1864). Many eventually turned to poor relief, but others migrated in search of work elsewhere.[12]

One way to examine migration patterns is to study the evolution of population, population growth rates, and net migration rates across decades using census data. These patterns are given in Figure 4. The top-left panel describes the evolution of log population in cotton districts, nearby districts, and all other districts from 1851-1881. The top-right panel describes the growth in district population across each decade, normalized by the 1851-1861 change (the decade preceding the downturn).[13] The bottom panel describes implied net in-migration rates over the same period.[14] This figure reveals three important patterns. First, it shows a substantial slowdown in

[11] Additional relief programs included public works employment for unemployed cotton workers, though most public works employment began in 1863, after the worst of the crisis had passed. See Arnold (1864) for a discussion of public works.

[12] Watts (1866), for example, describes how (p. 226-7), "The trade of Yorkshire has received such an impetus during the famine...many thousands of operatives have only crossed Blackstone Edge [which divides Yorkshire from Lancashire]." Arnold (1864) described how "thousands had passed to east and south".

[13] As noted in Footnote 5 the 1861 census was taken in April, the same month that the U.S. Civil War began, and so it should be thought of as a clean pre-war population observation.

[14] Implied net migration is calculated as the difference between the observed population count in a district in a given census year and the population that we would have expected in that district-year given the population in the previous census plus all births and less all deaths in the intervening years. We then divide by initial population to create rates. This conceptual approach has been used in studies of migration such as Fishback *et al.* (2006) and Bandiera *et al.* (2013).

population growth in the cotton textile districts in the decade spanning the cotton shortage.[15] This change appears to be driven by both increased out-migration and decreased in-migration (a conclusion supported by the bottom panel, as well as additional evidence in Appendix A.3). Second, we observe an acceleration in population growth in nearby districts, which we define here as non-cotton districts within 25 km of a cotton district. Meanwhile, there is little change in the population growth trend in districts beyond 25 km. These patterns are consistent with short-distance migration from cotton textile districts during the downturn. Third, these changes essentially disappear after 1871, highlighting the temporary nature of the shock.

These implied migration flows were meaningfully large. In terms of magnitude, had the population of the cotton districts grown from 1861-1871 at the same rate that it grew in 1851-1861, these districts would have had 54,000 additional residents in 1871, a figure equal to 2.2% of the districts' 1861 population. Similarly, if nearby districts had grown in 1861-1871 at the rate they grew during 1851-1861, they would have had 61,000 fewer residents, which is equal to 4% of the districts' 1861 population. Note that these figures will understate the migration response if some migrants returned between 1865 and 1871.[16]

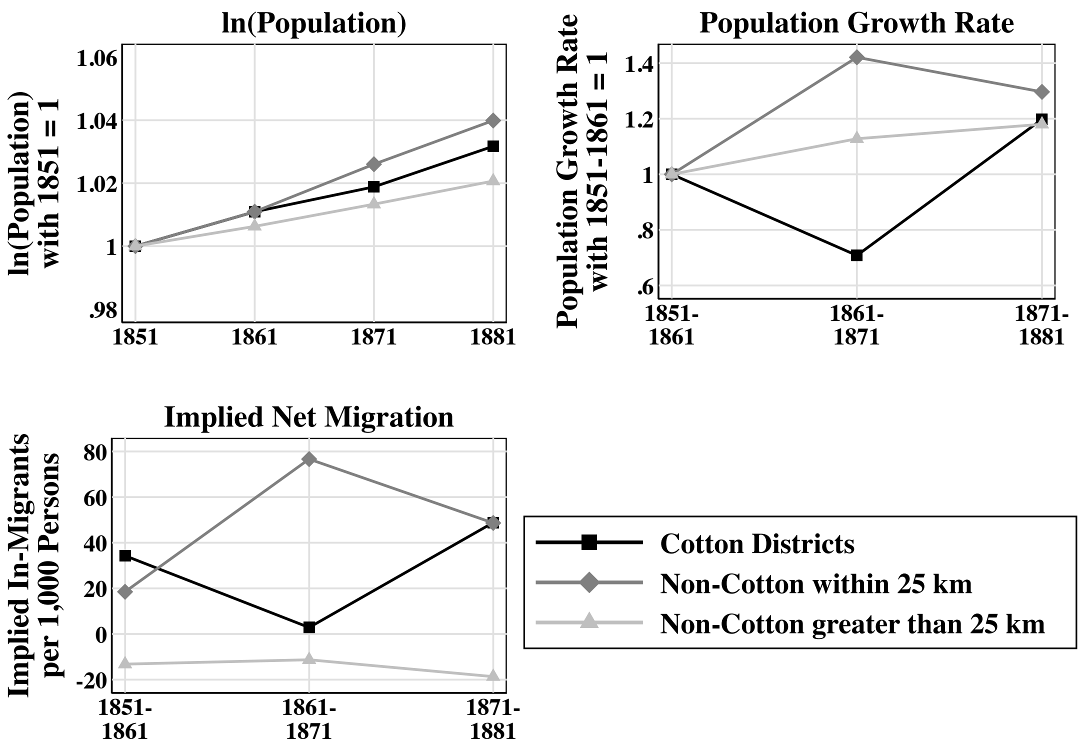
There is also some evidence that migration away from the cotton textile districts during the U.S. Civil War was selective. Appendix Figure 8 shows that young adults were somewhat more likely to migrate. However, the change in population in the 20-39 age group accounts for only about three-fifths of the overall change in population of the cotton districts between 1861 and 1871. Thus, a substantial amount of migration likely occurred among other segments of the population as well.

Migratory responses of the sort documented here have two important implications

[15] Note that overall population growth in the cotton areas remains positive from 1861-1871, as it does in all other locations. This growth reflects the very high rate of fertility in all locations, which continued until the fertility transition that began at the end of the 1870s (Beach & Hanlon, 2020). Given this strong underlying forcing factor, the impact of migration is visible mainly in the changes in the growth rates shown in the middle panel.

[16] These patterns are consistent with the city-level experiences documented in Hanlon (2017).

Figure 4: Migration response to the cotton shortage



This graph describes population dynamics for all cotton districts, all non-cotton districts within 25 km of a cotton district, and all remaining non-cotton districts. Cotton districts are defined as those districts with more than 10% of employment in cotton textile production in 1851. The population growth rate for each group of districts is normalized to one in 1851-1861. Data for the top two panels are from the Census of Population. Implied net migration is given as the difference between the terminal census estimate of population and the postcensal (i.e., initial census population less intervening deaths, plus intervening births) estimate of population, all divided by initial population (such that positive values represent in-migrants). Data for the bottom panel are from the Census of Population and the Registrar General's reports of annual vital statistics.

for our analysis. First, there are good reasons to expect that this migration impacted health in very real ways. For instance, the cotton textile districts were the least intrinsically healthy locations in Britain at this time, because they were highly industrialized, densely populated, and heavily polluted.[17] Thus, those leaving the cotton districts likely enjoyed some protective effects of migration that will work against the results that we find here, causing the recession we study to appear healthier in our results than it actually was.[18] Second, migration poses a number of empirical challenges, largely related to the mis-measurement of population size and composition. In the mortality analysis that follows, we discuss both these substantive and methodological concerns related to migration, and develop an approach to estimation wherein the spurious health effects related to unobserved migration can be stripped from the real health effects of the downturn.

3 Mortality analysis

How did health respond to this temporary local shock? Contemporary reports suggest a number of channels through which the cotton shortage affected health.[19] Some local Registrars—the officials responsible for compiling death records—described a reduction in deaths in the cotton districts. One such official attributed this to "more freedom to breathe the fresh air, inability to indulge in spirituous liquors, and better nursing of children."[20] Notably, these are some of the same channels modern studies cite as an explanation for the pro-cyclical mortality relationship they find.[21] How-

[17]The crude mortality rate in cotton districts was over 26 deaths per thousand, compared to 25.7 in nearby districts and 23.2 nationwide.

[18]This differs from the experience of blacks during the U.S.'s Great Migration, who moved toward, rather than away from, more urban, industrialized, and polluted locations (Black *et al.*, 2015).

[19]See Appendix A.4 for details.

[20]Quoted from the *Report of the Registrar General*, 1862.

[21]See Dehejia & Lleras-Muney (2004) and Ruhm (2000); Aguiar *et al.* (2013) on freeing up time for breastfeeding, childcare, exercise, and other salutary activities; see Stevens *et al.* (2015) on raising the quality of elder-care; and see Ruhm & Black (2002) and Ruhm (2005) on limiting the capacity for unhealthy behaviors such as smoking and alcohol use.

ever, other reports indicate that the inability to afford food, clothing, and shelter negatively affected health, particularly for the elderly. The effect of reduced income is illustrated by the reappearance of typhus—a disease spread by lice and strongly associated with poverty—in Manchester in 1862, after many years of absence. These conflicting reports highlight the fact that the net effect of the cotton shortage on mortality is ambiguous ex ante.

3.1 Methodological issues introduced by migration

One thing contemporary reporters cannot tell us, however, is whether mortality among those initially resident in cotton districts increased during the U.S. Civil War. This is because local registrars had visibility only into the health of individuals currently living (and dying) in their district. Given the substantial migration response we have documented, the fact that these officials were unable to track individuals over time and space poses a problem to us as well. To see why, consider the following estimating equation:

$$MR_{dt} = \beta\, SHOCK_{dt} + X_{dt}\Gamma + \phi_d + \eta_t + \epsilon_{dt} \tag{1}$$

where MR_{dt} is the mortality rate in a given location (i.e., district) d; η_t and ϕ_d are a full set of time-period and location fixed effects; $SHOCK_{dt}$ is an indicator equal to one if district d is a cotton district and time t is the shock period (1861-1865); and X_{dt} is a set of district-level controls. This equation closely follows the existing literature examining the impact of business cycles on health within a panel framework.[22]

[22]Within that literature, this estimating equation is most similar to Miller & Urdinola (2010), who use coffee price shocks and spatial variation in coffee cultivation as an exogenous shock to local economic conditions in Colombia. As in that paper, we do not use $SHOCK_{dt}$ as an instrument for unemployment because suitable unemployment data do not exist. In our setting, the best proxy available to us is the number of Poor Law relief-seekers, but it is not consistently available for the entire study period. Another reason we prefer this explanatory variable to annual unemployment-rate fluctuations is that it presents a more plausibly exogenous shock to local economic conditions (particularly in the presence of migration), one that enables us to cleanly identify and track the

While this equation is a natural starting point, migration may affect estimates obtained from Equation 1 in two key ways. First, migration may cause the dependent variable, MR_{dt}, to be systematically mis-measured. Second, migration-induced spillovers may affect results through the comparison, implicit in Eq. 1, between treated and control locations. Below we discuss each of these potential channels for bias, and how they are addressed in our analysis.

On the first point, migration may affect estimates obtained from Equation 1 through mis-measurement of the true at-risk population. Migration changes both the size of the population, which appears in the denominator used to calculate the mortality rate, as well as the composition of the population, which determines the population's average mortality risk, in ways that are unobservable to the researcher. If some migration is unobserved, the population denominator used to calculate the mortality rate will be incorrect.[23] Further, even if overall population flows are perfectly observed, migration may still be selective, which will cause the underlying mortality risk faced by the population in a given location to be different from what is observed.

Linked individual-level longitudinal data offers a solution to these issues. By fixing individuals to their location at the onset of the shock, their deaths can be correctly attributed to their experience of the shock whose effects we are trying to estimate, irrespective of where these deaths ultimately occur. Thus, this approach ensures that the population represented in the denominator of the mortality rate (i.e., the population at risk) corresponds to the group of people whose deaths appear in the numerator.[24] Accordingly, we modify our specification of interest to,

specific group of individuals exposed to the downturn whose effects we wish to estimate.

[23]If people migrate to locations offering better economic conditions, and if migration is not fully captured by intercensal population estimates, then unobserved out-migration will lead to an artificially high population denominator and fewer observed deaths in the numerator because of a smaller at-risk population. Conversely, unobserved in-migration will lead to an artificially low population denominator but more observed deaths because the true at-risk population has increased. Thus, the unobserved relocation of individuals from one region to another can mechanically generate the false appearance of health change where there has been none.

[24]In other words, this approach holds fixed the size and composition of the population at risk.

$$\left(\frac{MORT_{dt}}{POP_{dt}}\right) = \beta\, SHOCK_{dt} + X_{dt}\Gamma + \phi_d + \eta_t + \epsilon_{dt} \tag{2}$$

where POP_{dt} is the population in a district d at the beginning of period t (in our empirical setting, at the 1851 or 1861 census) and $MORT_{dt}$ is the number of deaths among that population during the period (i.e., from 1851-1855 or 1861-1865).

It is worth noting that migration may have very real effects on mortality. For example, migration may affect mortality in both migrant-sending and migrant-receiving areas through congestion effects (e.g., disease contagion, strain on fixed local resources, or labor market competition). Alternatively, the act of migration can change underlying population health by, say, depleting the migrant's health stocks, or by relocating people across locations with different intrinsic conditions. If, for example, people move to healthier locations, then migration will have a real and beneficial impact on health. While estimates obtained from a linked-data approach will purge the spurious impact of migration on observed mortality patterns, they will capture—alongside the direct effects of the recession on mortality—any *real* effects of recession-induced migration on mortality.

On the second point, migration can affect results obtained from Equations 1, and 2 by generating spillovers from treated to control locations, thus violating the assumptions necessary for causal inference in a difference-in-difference approach. This issue can be addressed if migrant-sending and migrant-receiving locations can be identified and compared to a third set of locations that were not contaminated by spillovers.[25] To operationalize this intuition, we modify our specification to separately estimate the impact of the shock on migrant-receiving districts,

[25] An alternative approach is to aggregate to higher geographic levels. For instance, one could combine migrant-sending and migrant-receiving areas and, in essence, treat the two areas as a single unit. This type of aggregation ignores the fact that the various local labor markets within the aggregated study area are likely experiencing dramatically different economic conditions, which may undermine the researcher's ability to recover the causal effect of economic conditions on mortality.

$$\left(\frac{MORT_{dt}}{POP_{dt}}\right) = \beta\, SHOCK_{dt} + \gamma RECEIVING_{dt} + X_{dt}\Gamma + \phi_d + \eta_t + \epsilon_{dt} \qquad (3)$$

where $RECEIVING_{dt}$ is an indicator equal to one for districts receiving migrants from the treated districts during the treatment period. In the case of the cotton shortage, most migration occurred to nearby locations. Thus in our setting, $RECEIVING_{dt}$ will simply be an indicator variable (or variables) identifying districts within a specified radius of cotton districts. With these modifications in hand, we now turn our attention to constructing our linked dataset.

3.2 Constructing our linked sample

To estimate the relationship between recessions and mortality in the presence of unobserved migration, we require individual-level longitudinal data that identifies both an individual's place of residence at the beginning of the recession and whether that individual died within the specified recession period thereafter. Our linked sample relies on two main data sources that allow us to recover this information. The first is individually-identified death records for the entire population of England and Wales over the years 1851-1855 (our control period) and 1861-1865 (the recession period). The second is the full-count British census for the years 1851 and 1861. Because census enumeration took place in April of 1861, just as the U.S. Civil War began and before it had any meaningful effect on the British economy, this means that we can identify deaths in the cohort of individuals actually exposed to the cotton shortage.

We obtain census microdata from the UK Data Archive.[26] In addition to preserving the structure of the household, these data include individual names, location at the time of enumeration, age, and some additional information.[27] Our deaths data

[26]Schurer & Higgs (2020a,b).

[27]Individual-level microdata are not available from the next closest censuses, in 1871 or 1841.

come from the records of the General Registrar's Office (GRO),[28] which we have collected for the years 1851-1855 and 1861-1865. These data include information on the decedent's first and last name, age, and location of death. Further details on the deaths data and how they were obtained can be found in Appendix B.1.

We construct our longitudinal dataset by linking the census and death records. A valid link is defined as one where: the first name and last name are an exact match between the GRO data and the census, and where the inferred birth year is no more than 5 years apart.[29] We allow for a 5-year threshold, which is standard in the linking literature (see, e.g., Abramitzky *et al.* (Forthcoming)) because neither data source explicitly asks about birth year. The census asks for the individual's age at the time of enumeration, while the death index reports age at time of death. Because the assigned birth year depends on when these events occur relative to the individual's birth month, it is natural to expect some disagreement. We allow the threshold to span five years to account for any other misreporting of age (e.g., age heaping). This strategy yields a final sample of 150,792 deaths (or about 7.1% of all deaths) for the 1851-1855 period and 126,509 deaths (or 5.8%) for the 1861-1865 period.[30]

While our linking strategy attempts to follow the best practices in the literature (e.g., Ferrie (1996), Abramitzky *et al.* (2012), Abramitzky *et al.* (2014)), there are some important differences with respect to linking in our setting. First, we are linking people over relatively short periods of time, never more than five years. This means that name changes, such as those due to marriage, are less common. As a result, women are well-represented in our linked sample. A second advantage is that the name information provided in the British census is likely more accurate than contemporaneous U.S. Census records. One reason for this is that there were few recent

[28] General Registrar's Office (2019)

[29] A consequence of this 5 year threshold is that the first name, last name, and inferred birth year must be unique within a five year window.

[30] There are a number of potential reasons why our linking rate differs across these two periods, including differences in the care that went into collecting the census, differences in the way the data were transcribed, changes in name uniqueness over time, emigration, etc.

foreign migrants in Britain, who may have changed their names as they assimilated. A second reason is that the British procedure for collecting the census differed in that households filled out their own census forms, rather than verbally providing their information to an enumerator.[31] Because of this, we refrain from using name cleaning algorithms like Soundex. The main disadvantage relative to the existing literature is that we are not able to leverage birthplace information, as that is not reported in the death index. For this reason, the next section summarizes results from a battery of empirical tests to illustrate the reliability of our linked sample.

As a check on the impact of the linking procedure on our results, in Appendix D.3 we present a second set of findings. These results are obtained from a different set of underlying death records, linked to census data using a different procedure. The results obtained from this alternative sample are similar to those obtained from our preferred data, which we view as strong evidence that the specific nature of our linking procedure is not driving our results.

3.3 Assessing our linked sample

One way to check whether our linked sample is reasonable is to see how the probability of finding a link declines as the distance between the death location and enumeration location rises. This analysis, presented in Appendix B.2.1, shows that deaths are much more likely to be matched to individuals previously enumerated in the same district, and that the chance of observing a link falls off rapidly and fairly smoothly as the distance between the death district and the census enumeration district increases. These results are consistent with the intuition that migration tends to decline with distance, and suggest that our linking procedure is performing well. As a point of comparison, we can also link between the full 1851 and 1861 censuses. The distribution of distances between district of enumeration in 1851 and that in 1861 looks

[31]Enumerators still visited every household to check and collect the forms and assisted households in the completion of the form when necessary.

nearly identical to what we see when in our sample of linked deaths. Finally, we can plot the distribution of distances when we randomly link census records within the 1851 census. There we see a very different "hump-shaped" pattern, suggesting that the previous results are not simply a mechanical artifact of the linking procedure.

A second way to assess the quality of our links is to run a falsification test. We classify every individual in our linked dataset as having died in the five years following enumeration. If we are correct, then if we were to look for these individuals in the subsequent census (i.e., 5-10 years after we say they died), we should not find any of them. Unfortunately, the 1871 microdata are not yet digitized, so we can only run this test for those that we classify as having died between 1851 and 1855. Of the 150,792 individuals that we classify as dead, 17.27% (or 26,041) link to a record in 1861 (same first name, same last name, and age within a 5-year threshold). However, the advantage of this exercise is that we can also leverage birthplace information, and it turns out that if we require the birthplace to also match, then only 8.67% of our "dead" sample can be linked to the 1861 census. These will be upper-bound estimates if families recycled names following the deaths of relatives within a five year period. This suggests that an upper bound on our false positive rate is between 8.67 and 17.27%.[32] Overall, our false positive rate is on the lower end of what is obtained in other linked papers (see Bailey *et al.* (2020) and Abramitzky *et al.* (Forthcoming)). Note that in our difference-in-difference framework, these false positives likely work against us by pushing our mortality coefficients toward zero.

[32]Imposing various other criteria provides us with information on how to lower our false positive rate. The first new criterion that we impose is that a "linkable" record should have a unique first name, a unique-*sounding* last name (as determined by NYSIIS codes), and a unique age (within 5 years). This criterion lowers our false positive rate range to between 7.72 and 13.16% (where the lower bound estimate requires that the potential links have the same birthplace). If we further require that a "linkable" record be one with a unique *sounding* first and last name, the range falls to 7.50 to 12.38%. As a second set of criteria, we could require that the distance between place of enumeration in 1851 and place of death be within a certain threshold. If we set that threshold to 200 km, then our false positive rate range is between 8.19 and 16.35%. When the threshold is 100 km, the range becomes 7.57 to 15.23%. With a 50 km threshold, the false positive rate is between 6.88 and 14.15%. Finally, if we impose that there be no migration, the false positive rate is between 5.31 to 11.80%. These criteria will form the basis of various robustness checks.

In addition to linking accuracy, it is also important to know whether the mortality patterns in our linked sample are representative. One way to test this is to generate results assigning our linked deaths to the district in which they occur, and then to compare these to results obtained from comprehensive data on all deaths in England and Wales—data, taken from the Registrar General's reports,[33] in which deaths are reported in aggregate form by district of occurrence (henceforth, "aggregate data"). We present these results later, in Table 5. These results show that we are able to recover estimates that are both practically and statistically equivalent to those from aggregate data. The fact that we can reproduce the results obtained with comprehensive aggregate data when we structure our linked sample to mimic the structure of these aggregate data suggests that our linked sample is reasonably representative overall.

The main dimension on which our linked sample of deaths differs from aggregate mortality is in the age distribution. In our linked sample, young children, and particularly infants, are under-represented. This is a mechanical consequence of our procedure, since an infant death in, say, 1865, can never be linked to someone alive in the 1861 census. We take two approaches to dealing with this issue. One approach is simply to analyze different age groups separately. Alternatively, when estimating effects across all age groups, we re-weight our linked sample so that its age distribution is representative of that in the corresponding aggregate deaths data.

In Appendix B, we check our linked sample against aggregate deaths data on the dimensions of socioeconomic status and sex. Drawing on the occupation data listed in the census, we see the share of deaths among white- vs. blue-collar workers in the linked sample are very similar to those generated from aggregate mortality data. Thus, for the working population, our linked sample appears to be quite representative in terms of socioeconomic status. In terms of gender, women are slightly over-represented in our linked sample, where they account for 51.4% of deaths in the

[33] Registrar General of England and Wales (1851-1871)

1851 period, versus 49.2% in the aggregate data, and 50.5% of deaths in the 1861 period, versus 48.8% in the aggregate data. This is most likely due to women's names being more unique than those of men (Rossi, 1965).

3.4 Estimation strategy

Building upon the empirical framework introduced in Section 3.1, our estimating equation of interest is the following differences-in-differences specification:

$$\left(\frac{MORT_{dt}}{POP_{dt}}\right) = \beta\, COTDIST_d * POST_t + \sum_{i\in\{25,50,75\}} \gamma_i NEAR_d^i * POST_t + X_{dt}\Gamma + \phi_d + \eta_t + \epsilon_{dt} \quad (4)$$

The variable POP_{dt} is the population in a district d at the time of enumeration (i.e., 1851 or 1861) and $MORT_{dt}$ is the number of deaths among that group of people during the period of interest (i.e., from 1851-1855 or 1861-1865).[34] The variable $COTDIST_d$ is an indicator for whether district d is a cotton district, and $POST_t$ is an indicator for the 1861-1865 period.[35] The variables $NEAR_d^{25}$, $NEAR_d^{50}$, and $NEAR_d^{75}$ are indicator variables equal to 1 if district d is within 0-25 km, 25-50 km, or 50-75 km from a cotton district. The inclusion of these variables is informed by the spatial concentration in migration that we documented in Section 2.2. The vector X_{dt} is a set of additional district-level controls.

This equation deals with both migration-induced mis-measurement of mortality rates and spillovers between migrant-sending and -receiving areas, such that β reflects the impact of the cotton shortage on the mortality rate of the treated population,

[34] While our approach collapses microdata to the district-of-origin level, thus creating district-of-origin cohort mortality rates, an alternative approach is to run logit or probit regressions at the individual level.

[35] Cotton textile districts are defined as those with greater than 10% of employment in cotton textiles in 1851, a decade before the U.S. Civil War, although in robustness exercises we also consider continuous measures of cotton employment. The location of industry was relatively persistent, and so results are similar when using the spatial distribution of industry in 1861.

regardless of where they died. However, one challenge with estimating Eq. 4 is that because our data do not include unique individual identifiers (e.g., a social security number), we are not able to link every death back to a census record. To see how this affects our analysis, let $MORT_{dt}$ be the number of deaths of individuals initially resident in district d and let λ be the share of these deaths that we are able to match back to census records. What we can observe in our linked data is $\widetilde{MORT}_{dt} = MORT_{dt}\,\lambda$. Substituting out $MORT_{dt}$ in Eq. 4 and reorganizing, we have,

$$\left(\frac{\widetilde{MORT}_{dt}}{POP_{dt}}\right) = \widetilde{\beta}\,COTDIST_d * POST_t + \sum_{i \in \{25,50,75\}} \widetilde{\gamma}_i NEAR_d^i * POST_t + X_{dt}\widetilde{\Gamma} + \phi_d + \eta_t + \epsilon_{dt} \quad (5)$$

where $\widetilde{\beta} = \beta\lambda$ and $\widetilde{\gamma}_i = \gamma_i\lambda$. This shows that we can obtain β estimates by multiplying the $\widetilde{\beta}$ coefficients (and standard errors) obtained from our linked data by the linking rate λ. To ease the interpretation of our results, we make this adjustment in all of our main analysis tables.

One identification assumption in our analysis is that the probability of linking should not be correlated with the treatment. Our analysis approach will not be biased due to variation in linking rates across locations that are fixed over time, nor by changes in linking rates over time that are common across locations. However, we may worry that there were time-varying changes in the probability of linking. The most plausible violation of this assumption is that migration generated by the shock may have made it more difficult to link cotton-district residents observed in the 1861 census to deaths over the period 1861-1865, say, because they moved abroad. However, if individuals who emigrated are less likely to be linked (i.e., because they left Britain altogether), and emigration increased from the cotton districts during the shock, then this will bias the estimated effect of the shock downwards, since it will cause the number of linked deaths among those initially resident in the cotton districts to *understate* the true number of deaths. Thus, if anything, this form of bias will work against the counter-cyclical results that we find.

There are a few other points worth mentioning about our empirical specification. First, in the main text we report standard errors clustered by district. To address the possibility of spatial correlation, our main results also report p-values from a permutation test that provides an alternative assessment of statistical significance, while respecting the spatial structure of our data. For a detailed description of our permutation test, as well as a discussion of why we prefer this to alternative methods such as clustering or spatial standard errors, see Appendix C.1. Second, when looking at all-age mortality results, we control for the share of different age groups in each district, which naturally influence total mortality. We also include initial district population as a control because the period we study saw substantial improvements in sanitary technology which were most important in larger cities with high population density. In addition, we control for what we call the "linkability rate," which is given by the number of individuals enumerated in the census in a district that can be uniquely identified by their first name, last name, and age (within a 5-year window), divided by the total number of individuals enumerated in the census in that district. We calculate this rate for each district-by-period cell. Third, we follow the conventions of existing literature and weight all regressions by population, although as we show in our robustness checks, weighting does not affect the results.

As a final point, it is worth noting that, while our linked sample allows us to identify deaths among both migrants and stayers, it is not possible to separately assess the mortality rates for these two groups, and so, to comment on the causal impact of migration on health. This is because we are not able to observe the population of migrants; we only observe migrants in the linked sample conditional on their death.

3.5 Summary statistics

Table 1 presents summary statistics for key analysis variables. The first two rows highlight Britain's high mortality during this period. As a point of comparison, the population-weighted mean death rate across the two periods, as as calculated from the aggregate Registrar data, is 23.2 deaths per 1000 persons, and even in the aggregate data we see a few districts with mortality rates of 40 deaths per 1000 persons or higher. Relative to the rates observed in aggregate data, our data exhibit more variation across districts due to the fact that they are based on a (linked) sample of deaths rather than all deaths. In addition, Table 1 shows that cotton districts make up 4.5% of the sample while an additional 4.8% of the sample are nearby districts within 25km of the cotton districts. In terms of age, the population was fairly young, with 36% under age 15 compared to 12% over 54.

Table 1: Summary Statistics: Linked Data Assigned to District of Enumeration

	Mean	Standard Deviation	Min	Max	N
MR in 1851	27.801	8.513	0.569	46.517	538
MR in 1861	21.38	6.394	0.15	42.62	538
Cotton dist. ind.	0.045	0.207	0	1	538
Nearby (0-25km) ind.	0.048	0.215	0	1	538
Cotton Employment share	0.017	0.071	0	0.513	538
Under 15 pop. shr 1861	0.36	0.021	0.201	0.416	538
Age 15-54 pop. shr. 1861	0.52	0.028	0.468	0.705	538
Over 54 pop. shr. 1861	0.12	0.021	0.069	0.183	538

3.6 Main results

Before turning to our main regression results, we examine patterns in the raw data to help fix ideas about the results that follow. First, we see evidence that residents of cotton textile districts faced a greater mortality risk during the downturn. Over the 1851 to 1855 period, 6.2% of our linked deaths originated from cotton districts. During the 1861-1865 period, however, 7% of our linked sample originated from cotton districts. Second, we see evidence consistent with an increase in migration during the downturn. Among the linked deaths from the 1851-1855 period, 73.2% of individuals that were enumerated in a cotton district also died in a cotton district. In the 1861-1865 period, however, this figure falls to 67.4%.

Next we examine these patterns within our formal regression framework. Table 2 presents our main findings. Note that these coefficients have been adjusted for the linking rate, such that they can be interpreted as the change in the mortality rate in the population per 1,000 persons per year.[36]

Column 1 presents our simplest specification, while the results in Column 2 include controls for district population density, the population shares of individuals in different age groups, and a control for whether the district had more people with "linkable" (more unique) names. While these controls do predict the change in mortality rates, particularly the age controls, they do not have a substantial impact on the cotton district coefficient. In Column 3, we address the possibility that the effects of the shock may have spilled over into nearby districts by separately estimating the impact on non-cotton districts in various distance bands around the cotton districts. These show some marginally statistically significant evidence of adverse spillover effects in the population initially residing in the nearest set of districts. Consistent with this, including these controls results in an increase in the cotton district coefficient. Finally, Column 4 replaces the cotton district indicator variable with a continuous measure of treatment: the share of district employment in cotton textiles. Here we observe similar results, which are stronger in terms of statistical significance.[37] Note that, in this specification, the spillover effect in nearby districts appears weaker, a

[36] When adjusting by the linking rate we use the average linking rate across the full data sample.

[37] In terms of magnitude, a one s.d. increase in the explanatory variable in Column 3 leads to an increase in deaths of 0.525 per thousand, while a one s.d. increase in the explanatory variable in Column 4 leads to an increase in deaths of 0.477 per thousand.

Table 2: Baseline effects of the shortage using linked data

	(1)	(2)	(3)	(4)
DV: Deaths per 1,000 Individuals (per year)				
Cotton District × Cotton Shortage	2.194***	2.024***	2.534***	
	(0.463)	(0.519)	(0.605)	
Cotton Emp. Share × Cotton Shortage				6.766***
				(1.654)
Nearby (0-25 km) × Cotton Shortage			1.054*	0.785
			(0.597)	(0.576)
Nearby (25-50 km) × Cotton Shortage			0.191	0.081
			(0.623)	(0.628)
Nearby (50-75 km) × Cotton Shortage			0.586	0.509
			(0.656)	(0.645)
District Controls		Yes	Yes	Yes
Observations	1,076	1,076	1,076	1,076
R-squared	0.022	0.395	0.398	0.397
Permutation test p-values for effect on cotton districts				
p-values	0.119	0.089	0.050	0.002

*** p<0.01, ** p<0.05, * p<0.1. Underlying sample includes 277,057 linked deaths. Standard errors in parentheses are clustered at the district level. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population. All regressions include period fixed effects and district fixed effects. District controls include: ln(population density), share of individuals enumerated in the census with a "linkable" name, the share of the population in each of the following age categories (under 15, 15-54, and over 54, with 15-54 as the omitted category) and region-by-period fixed effects.

result that may reflect the presence of a few cotton textile workers in those areas.

At the bottom of the table, we report p-values from a permutation test of the key explanatory variable. Our permutation test, described in detail in Appendix C.1, provides an alternative approach to constructing confidence intervals that involves iterating across a large set of potential placebo cotton districts, estimating results, and then comparing estimates based on the true cotton districts to the distribution of placebo coefficients. Because our placebo sets of cotton districts incorporate the bunched spatial pattern observed in the true cotton districts, these placebo results can help address potential concerns about spatial correlation.

Table 3 breaks these results down by age group. The clearest pattern here is the mortality increase experienced by older residents of the cotton districts. We observe substantial effects for adults over 25, which become statistically significant starting at age 45. In contrast, for the two younger groups, the estimated effect is very close to zero. Recall that we should be cautious in interpreting the coefficient for the under-15 age group, since our linking procedure will mechanically miss many deaths among infants and young children, a group that contributed a large share of the deaths in this category.

The by-age pattern of effects is consistent with contemporary reports describing the health effects of the shock (see Appendix A.4). For example, assessments from local health officials at the time suggest that the health of young children during this period improved when working mothers in cotton textiles—a heavily female industry—lost their jobs and were able to spend more time on breastfeeding, household hygiene, and childcare. For young children, this likely offset the adverse effects of material deprivation, an explanation in line with similar recent findings in modern Colombia (Miller & Urdinola, 2010). For further discussion of infant health, see Appendix Table 14, where we examine results on births and infant mortality using aggregate data.

To put these magnitudes in context, our preferred all-age results in Column 3 of Table 2 imply that the cotton shock generated 24,418 excess deaths in the cotton textile districts from 1861-1865, equal to 9.5% of total deaths in the cotton districts over this period.[38] Using the age-group regressions we estimate roughly 10,191 deaths among those aged 55 and over (an 18.8% increase in deaths in that age group), 7,402 among adults aged 15-54 (a 11.2% increase) and 2,595 among those under 15 (1.9% increase). Thus, the shock appears to have substantially elevated mortality over the

[38]To provide an alternative benchmark, we can think about the excess deaths over the period 1861-1865 among the cohort initially residing in cotton textile districts as being equivalent to roughly twice the number of deaths from diarrhea in these districts over the preceding 5-year period—or, to compare to some of the other leading causes of disease of the time, 86% of the deaths from tuberculosis, 66% of the deaths from other respiratory causes, or 209% of the deaths from scarlet fever in these districts over 1856-1860.

Table 3: Decomposing the change in mortality by age

	DV: Deaths per 1,000 Individuals (per year)						
Age group:	Under 15	15-24	25-34	35-44	45-54	55-64	Over 64
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cotton District × Shortage	0.224	0.171	0.894	1.512	3.066***	6.740***	13.477***
	(1.078)	(0.551)	(0.678)	(0.939)	(1.086)	(1.861)	(3.899)
Nearby (0-25 km) × Shortage	0.777	0.355	0.897	0.988	1.231	4.028*	4.418
	(1.149)	(0.471)	(0.693)	(1.077)	(1.129)	(2.109)	(3.333)
Nearby (25-50 km) × Shortage	-0.811	0.387	1.005	0.649	0.377	2.045	2.805
	(1.342)	(0.443)	(0.655)	(0.910)	(1.038)	(1.619)	(2.813)
Nearby (50-75 km) × Shortage	0.241	0.317	1.214*	1.848*	1.481	2.199	-0.371
	(1.211)	(0.561)	(0.678)	(1.069)	(1.207)	(1.616)	(3.259)
Observations	1,076	1,076	1,076	1,076	1,076	1,076	1,076
R-squared	0.130	0.084	0.087	0.089	0.122	0.180	0.317
Linked deaths	75,795	19,390	20,784	22,180	25,095	32,489	80,545
	Permutation test p-values for effect on cotton districts						
p-values	0.476	0.422	0.206	0.227	0.089	0.003	0.028

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered at the district level, in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population. All regressions include district fixed effects, period fixed effects, and controls for ln(population density), the share of individuals (within each age category × district of enumeration × census cell) that have a "linkable" name, and region-by-period fixed effects.

period 1861-1865 among the population initially residing in cotton districts. That the adverse mortality effects were strongest among older adults is consistent both with contemporary reports of a rise in respiratory ailments, to which the elderly are especially vulnerable, and with the temporary accentuation of seasonal patterns in mortality found during the 1861-1865 period.[39]

Appendix D.1 presents results from several robustness exercises. These show that results are similar regardless of whether we weight our regressions by district population; drop outlier locations such as Manchester, Liverpool, or Leeds; or omit the foreign-born from our linked deaths sample (see Appendix Table 8). We also consider more restrictive linking approaches and samples with fewer false positive links (Appendix Table 9). That analysis, too, produces similar results.

While the evidence laid out in Section 3.3 establishes that our linked sample

[39]These results also fit with some existing studies, such as Stevens *et al.* (2015), that show that recession-induced changes in the mortality risk of older adults are responsible for much of the effect of business cycles on total mortality.

is largely representative of the universe of deaths over the period in question, we conduct an additional validation exercise to show that our results are not influenced by features of our linked data. Specifically, in Appendix D.3, we present results using an entirely different linked database based on both alternative death data (taken from the freeBMD website)[40] and an alternative linking method (where links are based entirely on unique first and last name combinations). Results obtained using this linked dataset show patterns similar to those documented in our main results. We view the fact that this alternative linked sample yields similar results as strong evidence that our linking procedure is not driving the results that we document.

Because this alternative linked dataset spans additional years not available to us via the GRO, it also allows us to generate a number of additional results—among them, results on the phenomenon of harvesting, which speaks to the overall mortality costs of the recession (see Appendix D.3). Here, however, we focus on one particular advantage of this alternative linked dataset: namely, the fact that it also covers the years from 1855-1860.[41] This allows us to generate placebo results wherein we treat 1856-1860 as a placebo shock period, and then estimate mortality effects in the cotton districts over that period as compared to the 1851-1855 period. These findings, reported in Appendix Table 12, show no evidence of increased mortality in the cotton districts during the placebo period. This indicates that our results are not due to differences in underlying mortality trends in the cotton textile districts prior to the U.S. Civil War.

3.7 Results by sector of employment

Next, we examine the extent to which our results are driven by the experience of those directly reliant on income from the cotton textile industry (effects through

[40]FreeBMD (2018)

[41]We were unable to collect data on additional years via the GRO due to restrictions on the use of their site. See Appendix B.1 for further details.

"employment"), versus the extent to which they are driven by broader local distress, which might have affected families who resided in cotton textile districts, even though they themselves were dependent on other sectors (effects through "location"). To examine these issues, we take advantage of the fact that our census data include rich occupation information, and we classify families based on the sector that the household head worked in. These occupations, which are closer to industry identifiers than what we would call occupations today, allow us to study how mortality varied by sector of employment.

As a starting point, consider the share of total deaths accounted for by cotton textile workers and their households in our linked data. Deaths of cotton textile workers (i.e., those reporting cotton textile work as their occupation), accounted for 1.20% of all linked deaths among employed workers in our sample from 1851-1855, but this rose to 1.58% in 1861-1865, a 32% increase. Similarly, if we focus on entire households, members of cotton households accounted for 1.14% of deaths in 1851-1855 but 1.42% from 1861-1865, a 25% increase. These figures provide preliminary evidence that mortality among cotton workers may have been increasing during the cotton shock, though we acknowledge that in these raw data the increase could be due in part to an increase in the share of cotton workers in the population.

To provide more rigorous evidence on the incidence of the shock across sectors and locations, we organize our data into occupation-by-district bins and run regressions looking at how the shock affected mortality for households with heads linked to specific sectors (e.g., cotton textiles) as well as those located in the cotton districts, while including a full set of occupation and district fixed effects. While we focus on the household head occupation in these results, in Appendix D.2, we present additional evidence looking only at the employed population, and using a worker's own occupation.

The results are presented in Table 4. We begin, in Column 1, with a specification that is comparable to the results in Table 2, except that our data are now organized into occupation-by-location bins and we now control for occupation fixed effects. The coefficient estimate shows that we still find evidence of elevated mortality in the cotton districts when using this specification, though the results are somewhat attenuated. In Column 2, we instead look at how mortality changed among households with a head employed in the cotton textile sector (regardless of location) during the shock period. Here we see strong evidence that mortality increased among cotton households. In Column 3, we study both the impact of being in a cotton textile district and the impact of being a cotton textile household. These estimates suggest that most of the increase in mortality is explained by being in a location experiencing the shock, though there is some (not statistically significant) evidence that even in those locations, cotton textile households were worse off than others.

In the last column, we then consider effects accross all types of households in the cotton textile districts, separated into the type of occupation held by the household head. Since these occupation groups span all of the households in the cotton textile districts with observed occupations, we do not include a separate cotton district effect in the regression. The results show that within the cotton textile districts we see strong evidence of an increase in mortality among cotton textile households as well as equally-strong effects among households that produced non-traded services. There is also somewhat weaker evidence of increased mortality among households in industries linked to cotton textile production through input-output connections (e.g. textile finishing and clothing). We do not see statistically significant effects among households working in industries that mainly produced other traded goods, transportation, or those households with a head outside of the labor force.

We break these results down further in Appendix D.2, where we present two additional sets of results. First, we show that the mortality increase in households dependent on the cotton textile sector during the shock was larger than the change observed among almost every other occupation group. Second, we break estimates of effects within the cotton textile district down into more disaggregated occupation groups.

These results shed new light on how a shock to one important local industry can ripple across other sectors of a local economy. To our knowledge this is the first study within this literature that is able to separate these "employment" and "location" effects.[42]

Table 4: Decomposing effects by sector of employment

DV: Deaths per 1,000 Individuals (per year)	(1)	(2)	(3)	(4)
Cotton District × Cotton Shortage	1.443*		1.292*	
	(0.714)		(0.732)	
Head Employed in Cotton × Cotton Shortage		1.416***	0.860	
		(0.510)	(0.542)	
Head Employed in Cotton × Cotton Dist. × Shortage				2.476***
				(0.704)
Head Employed in Non-Tradeables × Cotton Dist. × Shortage				3.032***
				(0.866)
Head Employed in Linked IO × Cotton Dist. × Shortage				1.713*
				(0.981)
Head Employed in Other × Cotton Dist. × Shortage				0.595
				(0.733)
Head Employed in Tradeable Manuf. × Cotton Dist. × Shortage				0.764
				(0.945)
Head Employed in Transport × Cotton Dist. × Shortage				0.320
				(0.671)
Head Outside Labor Force × Cotton Dist. × Shortage				-0.936
				(1.444)
Observations	32,677	32,677	32,677	32,677
R-squared	0.023	0.023	0.023	0.024

*** p<0.01, ** p<0.05, * p<0.1. Two-way clustered standard errors (by district and occupation), are reported in parentheses. All regressions include period and district fixed effects, a series of indicators for whether the district is 0-25, 25-50, or 50-75 km from a cotton textile district in the post period, region-by-period fixed effects, ln(population density), the share of the population that is under 15, and the share of the population that is over 54. Deaths are assigned to the initial district of residence (i.e., census enumeration district). Regressions are weighted by district X industry population.

[42] Note, however, that some papers in this literature do touch on similar themes. For instance, Sullivan & von Wachter (2009) focuses on an individual's experience of job loss (a pure "employment" channel), while the impact on traffic fatalities found in Ruhm (2000) imply more general "location" effects. Though perhaps not perfectly analogous, Stevens *et al.* (2015), too, identifies spillovers through the labor market—a "location"-channel result—showing that because recessions raise the quality of elder care staff, the health of elderly people rises.

4 Does migration matter?

Finally, we ask: how important is it that we were able to adjust for migration in this setting? That is, we examine whether intentionally failing to account for recession-induced migratory responses fundamentally alters our conclusions as to the health impact of this historical downturn. By doing so, we provide the first direct evidence of the impact of unobserved migration on estimates of the recession-mortality relationship. Our main approach follows the methodology applied thus far to the linked microdata, comparing total deaths in 1861-1865 to deaths in 1851-1855, but instead uses data taken directly from aggregate district death counts transcribed from the Registrar General's reports.[43] This allows an apples-to-apples comparison between results obtained using the linked data and those generated from the more commonly available aggregate data.

As a first step, we compare results obtained from these aggregated reports to the results obtained from analogously organized linked data—that is, linked data wherein deaths are assigned to the location of death rather than to the residence at the time of enumeration. Because the aggregate data are assigned to the location of death, we should expect these results to be similar. Panel A of Table 5 reports results obtained using aggregate data. Panel B reports results using "aggregate-like" data, i.e., our linked data in which deaths are assigned based on the location of death. As in our main results, to allow comparability, the linked coefficient estimates have been inflated by the linking rate, so that these estimates reflect β rather than $\widetilde{\beta}$. The similarity between the estimates in Panel A and Panel B is striking. Moreover, these estimates are statistically indistinguishable, as given by the p-values at the bottom of Panel B. This tells us that our linked sample can recover the results obtained from aggregate

[43]These data cover the same districts used in the linked data analysis, and in fact are the same data used to test the representativeness of the linked microdata (for more on these data, see Section 3.3, as well as Appendix B.3). As in the linked analysis, population data come from the census and are available every ten years starting in 1851.

data, i.e., the linked deaths appear to be representative of aggregate deaths. This provides a powerful check confirming the quality of our linked data.

Next, consider the results obtained using our preferred linked data, where deaths are assigned to each person's district of residence at the time of enumeration rather than their district of death. These estimates are in Panel C. Note that the only difference between the results in Panel B and those in Panel C is whether deaths are assigned to location of death or initial location of residence. Thus, a comparison between the results in Panels B and C provides a direct test of the impact of migration on our estimates. Clearly migration has a substantial impact; the results that account for migration, in Panel C, indicate a much more severe mortality effect than the results suggested by Panel B.

Finally, having illustrated the ability of the linked data to mimic aggregate data, we compare the results obtained from aggregate data (Panel A) to our preferred linked approach (Panel C), which accounts for migration bias by assigning deaths to each person's district of residence at the onset of the shock. This comparison provides a direct test of the extent to which an analysis based on aggregate data can recover the true impact of the cotton shortage on the relevant population at risk of exposure. Our results show that the equality of these coefficient estimates can be strongly rejected, suggesting that, in the presence of a substantial migration response, an analysis based on aggregate data would fail to accurately recover the impact of the cotton shock on mortality—and in fact, would lead to meaningfully inaccurate results. That we cannot reject equality when our linked deaths intentionally ignore migration, but can reject equality when we do account for migration, is particularly telling. This means that the difference is due to migration rather than to differences between the linked sample and the aggregate data. The direction of this bias is also noteworthy: the analysis based on aggregate data makes the cotton shock appear much healthier than we know it to be from our migration-corrected linked data—in many cases, even flipping the sign of the estimated relationship.

Table 5: Aggregate-to-linked comparison: Does migration have a meaningful impact on results?

	DV: Annual Deaths per 1,000 Individuals (per year)						
	Under 15	**Age 15-24**	**Age 25-34**	**Age 35-44**	**Age 45-54**	**Age 55-64**	**Over 64**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Actual Aggregate Data (Drawn from Registrar's Reports)							
Cotton District × Cotton Shortage	-4.660***	-1.325***	-1.511***	-0.499	-0.624	0.267	2.094
	(1.019)	(0.284)	(0.346)	(0.496)	(0.632)	(0.793)	(1.807)
Panel B: Aggregate-Like Linked Data (Links Assigned to District of Death)							
Cotton District × Cotton Shortage	-4.248	-1.361**	-0.952	0.052	-2.035	2.104	4.989
	(3.035)	(0.654)	(0.975)	(1.399)	(1.918)	(3.285)	(6.991)
Different from Panel A? (P-value)	0.889	0.961	0.596	0.702	0.485	0.587	0.674
Panel C: Preferred, Migration-Corrected Linked Data (Links Assigned to District of Enumeration)							
Cotton District × Cotton Shortage	0.224	0.171	0.894	1.512	3.066***	6.740***	13.477***
	(1.078)	(0.551)	(0.678)	(0.939)	(1.086)	(1.861)	(3.899)
Different from Panel A? (P-value)	0.000	0.018	0.002	0.049	0.003	0.002	0.009

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered at the district level, in parentheses. Regressions are weighted by district population. All regressions include controls for ln(population density), proximity to cotton (0-25 km, 25-50 km and 50-75 km), and the share of the population in each period-by-age-by-place of enumeration cell with a linkable name. Regressions also include district fixed effects, period fixed effects, and region-by-period fixed effects.

5 Conclusion

We examine the mortality consequences of the Lancashire Cotton Famine, a recession in Britain's cotton textile producing regions that was precipitated by the U.S. Civil War. In addition to its intrinsic historical interest as one of the defining crises of industrializing Britain, two features of this setting are of particular significance to the study of the recession-mortality relationship. First, ours is a setting with limited social safety nets and high baseline mortality. Accordingly, evidence on the mortality impact of this recession helps deepen our understanding of the interplay between economic conditions and health in low-income settings, particularly across the age distribution. Second, and perhaps related to the limited safety nets of the time, the cotton shortage was a recession that generated a systematic migratory response. While migration is a natural means of coping with an income shock, it also poses threats to inference which have largely been ignored by the existing literature on recessions and health. In this paper, we offer an empirical strategy that overcomes these issues, and allows us to recover clean causal estimates of the mortality impact of the cotton downturn even in the presence of migration.

Our results are twofold. First, we find robust evidence that the cotton shortage increased mortality among those initially resident in cotton districts, both across the age distribution, and particularly for the elderly. Thus, one conclusion to draw from our results is that recessions can cause a substantial increase in mortality in a poor setting. In addition, we show that economic shocks hitting one industry can have substantial local impacts beyond just the families reliant on that industry for employment. In the setting we consider, these local spillover effects appear to be concentrated among providers of non-traded local services.

Second, we show that an analysis that does not explicitly deal with migration would have led us to conclude that this recession improved health, when in reality, health deteriorated in response to the downturn. This is true not only when we analyze aggregate data using the empirical methodology standard in this literature, but it is also true when we reorganize our linked microdata to intentionally ignore migration by defining exposure based on the district of death. These results illustrate that large migratory responses can pose a meaningful threat to inference, and suggest that it is important to account for such responses in order to accurately recover the impact of a localized shock on mortality. We present an approach that can help deal with the bias arising from a recession-induced migratory response. Given that migration remains a key margin of adjustment to local shocks in many settings, these methods may be useful for the broader literature studying the relationship between recessions and mortality.

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Online Appendix

Recessions, Mortality, and Migration Bias: Evidence from the Lancashire Cotton Famine

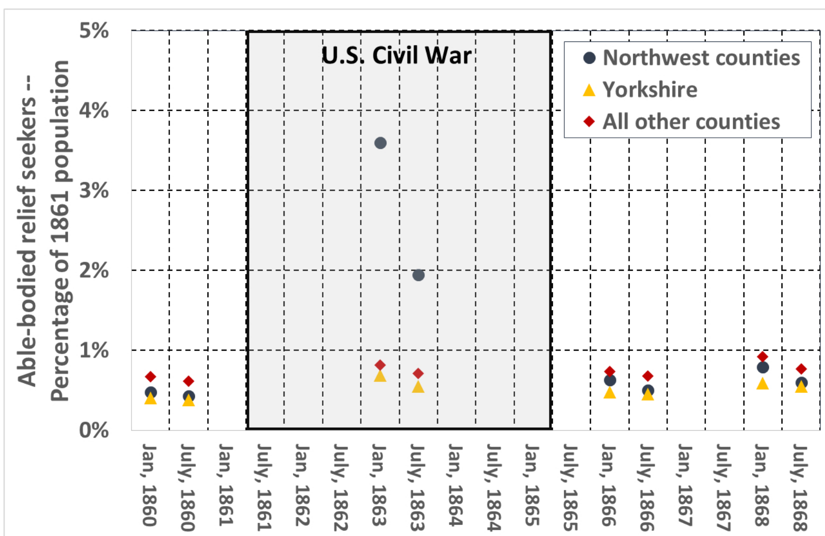
Vellore Arthi, Brian Beach, and W. Walker Hanlon

A Online Appendix: Empirical setting

A.1 Additional evidence of distress in Lancashire

Figure 5 describes the number of able-bodied relief-seekers who obtained aid from local Poor Law Boards, the main source of government support for the destitute in our setting. Consistent with the graph of Poor Law expenditures shown in the main text, during the downturn we see an increase in relief-seekers in the Northwest counties (Lancashire and Cheshire), where cotton textile production was concentrated. Non-cotton counties, however, were largely unaffected.

Figure 5: Evidence from able-bodied relief seekers



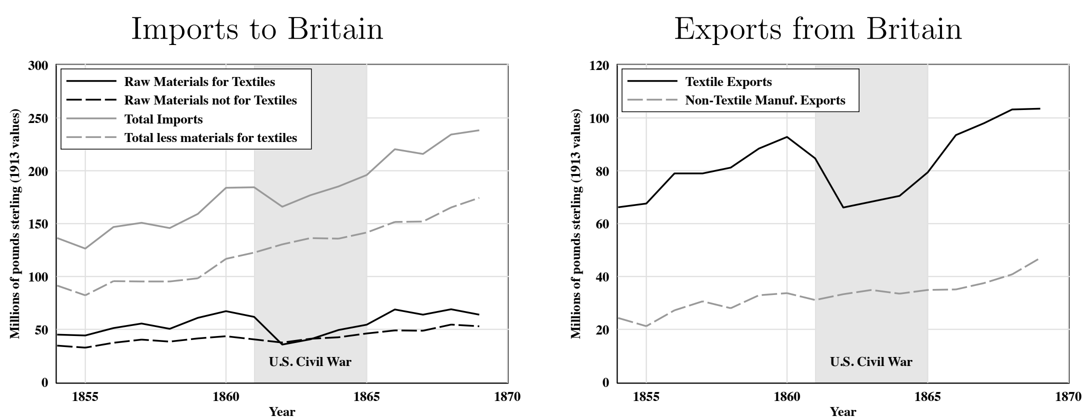
Data from Southall *et al.* (1998), Graph reproduced from Hanlon (2017).

A.2 Was the broader British economy affected?

To look for other effects of the U.S. Civil War on the British economy, a natural starting point is to examine imports and exports. The left-hand panel of Figure 6 shows that, aside from raw cotton, there does not appear to be a substantial change in total imports or raw material imports. This makes sense given that raw cotton made up 67% of total British imports from the U.S. in 1860. The right-hand panel examines exports. Aside from textiles, there is no evidence of a substantial change in British exports during the Civil War period.

One may expect that the U.S. Civil War would have had an impact on particular sectors of the British economy, such as arms or warship production. However, British producers were prohibited from selling arms to either side during the U.S. Civil War. While some producers were able to circumvent these restrictions, in general, these restrictions limited the impact that the conflict had on these industries.

Figure 6: British imports and exports, 1854-1869



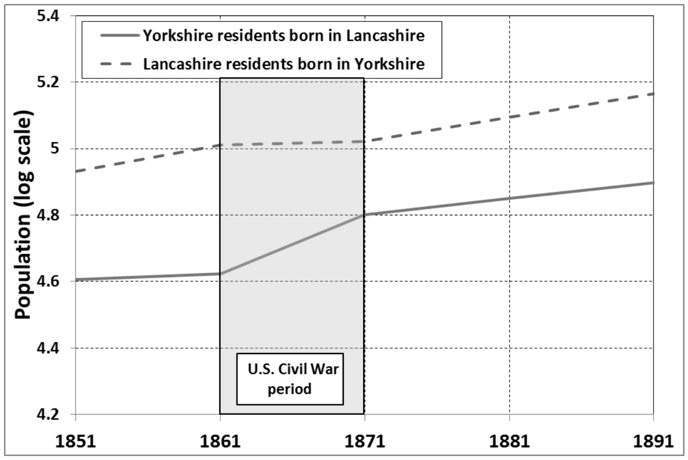
Data from Mitchell (1988).

A.3 Additional results on migration during the cotton shock

Evidence from birthplaces

Additional evidence on migration during the cotton shock can be gleaned from the location-of-birth information provided in the census. Specifically, changes in the share of the population born in one location who are resident in another can be used to provide evidence on net migration between locations. The location-of-birth data are

Figure 7: Evidence of migration for Yorkshire and Lancashire from birthplace data



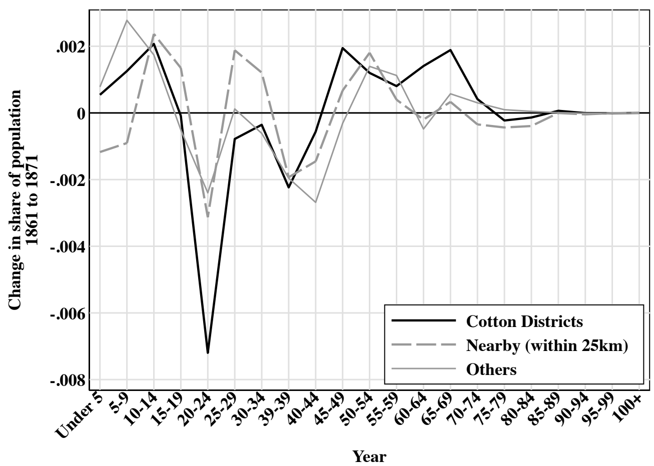
This graph, which is reproduced from Hanlon (2017), presents data on the birthplace of county residents from the Census of Population.

only available at the county level, so in Figure 7, which we reproduce from Hanlon (2017), we compare the largest cotton textile county, Lancashire, with the neighboring wool textile county of Yorkshire. The figure indicates that the number of Yorkshire residents who were born in Lancashire increased substantially from 1861-1871, while the number of Lancashire residents born in Yorkshire stagnated. This suggests an out-migration of Lancashire residents during the U.S. Civil War, as well as reduced in-migration to Lancashire.

Evidence on the age distribution of migrants

Next, we consider some results that help us think about how migration patterns varied across age groups. Figure 8 plots the change in population shares for several age categories between 1861 and 1871. We plot these changes separately for cotton districts, districts that were proximate to cotton districts, and all other districts. The most prominent feature in this graph is that there was a substantial decline in the share of 20-24 year-olds between 1861 and 1871. This suggests that the migration response to the shock was strongest among young adults.

Figure 8: Share of population in each age group in cotton districts



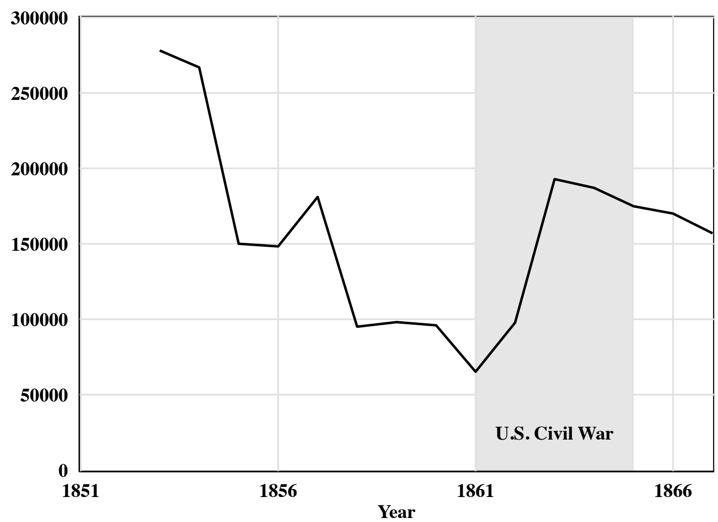
Population data are from the Census of Population for 1861 and 1871. Cotton districts are identified as those with over 10% of workers employed in cotton textile production in the 1851 Census. Nearby districts are those within 25 km of cotton districts.

Evidence on emigration

Tracking emigration from Britain in response to the cotton shock is more difficult than tracking internal migration. What information is available was collected at the ports of embarkation and reported in the British Parliamentary Papers.[44]. Figure 9 uses data from the 1868 report to the House of Commons, which provides total emigration numbers for 1853-1867. This graph shows that the total number of emigrants leaving Great Britain fell almost continuously from 1851-1861 and then increased substantially from 1861-1863. Unfortunately, we do not know what areas these emigrants were coming from, though we do know that most emigrants were Irish by birth. The English made up roughly one-third of emigrants across this period. However, by 1860 there were many Irish and Scottish living in cotton districts, so international emigrants from cotton districts need not be English.

[44]UK Parliamentary Archives (1868)

Figure 9: Emigration from Britain, 1852-1867



Data from the British Parliamentary Papers (1868, no. 045515).

A.4 Contemporary reports on health effects

Contemporary reports offer a mixed view of the impact that the cotton shortage had on health. Some 19th century observers, such as Arnold (1864), report that there was a "lessened death-rate throughout nearly the whole of the [cotton] district, and, generally speaking, the improved health of the people." In the words of the Registrar of Wigan, these gains were attributed primarily to "more freedom to breathe the fresh air, inability to indulge in spirituous liquors, and better nursing of children."[45]

The importance of childcare is highlighted in a number of reports, such as Dr. Buchanan's 1862 *Report on the Sanitary Conditions of the Cotton Towns* (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 304), which discusses the importance of the "greater care bestowed on infants by their unemployed mothers than by the hired nursery keepers." This channel was likely to be particularly

[45]Quoted from the *Report of the Registrar General*, 1862.

important in the setting we study because female labor force participation rates were high, even among mothers. Using 1861 Census occupation data, we calculate that nationally, 41% of women over 20 were working, and they made up 31% of the labor force. This rate was much higher in major cotton textile areas. In districts with over 10% of employment in cotton textiles in 1861, the average female labor force participation rate for women over 20 was 55%, and women made up 38% of the labor force. For comparison, these are similar to the levels achieved in the U.S. in the 1970s and 1980s (Olivetti, 2014), though of course the nature of the work done by women was quite different.

On the other hand, there were also reports of negative health effects due to poor nutrition and crowded living conditions. Dr Buchanan, in his *Report on the Sanitary Conditions of the Cotton Towns*, states that "There is a wan and haggard look about the people..." (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 301). Typhus and scurvy, diseases strongly associated with deprivation, made an appearance in Manchester and Preston in 1862 after being absent for many years, while the prevalence of measles, whooping cough, and scarlet fever may have also increased (*Report on the Sanitary Conditions of the Cotton Towns*, Reports from Commissioners, British Parliamentary Papers, Feb-July 1863). Seasonality features prominently in these reports, with conditions worsening during the winters, when the shortage of clothing, bedding, and coal for heating increased individuals' vulnerability to winter diseases such as influenza.

B Online Appendix: Data

B.1 Obtaining linkable death records

The death records used in our main analysis were obtained from the General Registrar's Office website for the years 1851-1855 and 1861-1865. This process involved several steps. First, we identify "linkable" records in the 1851 and 1861 censuses, where a linkable record is defined as one where the name is unique within a 5 year age band. This means that no two records in the same census can have the same first name, same last name, and a birth year within 5 years of each other. This yields 4,291,185 "linkable" records in 1851, and 5,228,528 "linkable" records in 1861. Of course, not all of the people underlying these linkable records will die in the subsequent five years. Thus, to help guide our search of the GRO index, we take these "linkable" records and identify whether there is at least one person in the Registrar's master death index with the same first name and same last name. Again, the master death index does not include age, so this process simply tells us who among the "linkable" names might have died in the next five years. This allows us to generate a list of surnames for each five year period (1851-1855 or 1861-1855), which we then fed into the GRO search query to obtain a new death index with age at time of death.

Note that we could have obtained all names, rather than only those likely to be linkable. However, the nature of the website meant that there are difficulties involved when searching for very common names, and moreover, this commonality means that such names are very unlikely to ultimately end up in any linked dataset. Thus, we decided to restrict our attention to only the set of death records associated with names that were likely to ultimately be linkable.

It is worth pointing out that in addition to having to provide a surname, sex, and year of death, the GRO website only displays up to 50 records per page, with a maximum of display of 250 records. We built an automated algorithm to feed in queries of surnames and death years, prioritizing surnames with fewer than 50 unique deaths by year, as identified by the master death index. Unfortunately, shortly after extracting the surnames with less than 40 unique queries, the GRO changed their terms of conditions, banning automated search algorithms. Nevertheless, we were able to extract 964,567 complete records for the 1851-1855 period and 790,623 complete records for the 1861-1865 period. To put this in perspective, the death master index lists 2,122,875 deaths over the 1851-1855 period and 2,196,602 deaths over the 1861-1865 period. Thus, we were able to extract complete information for roughly 45% of all deaths occurring between 1851-1855 and 36% of all deaths occurring between 1861-1865. Moreover, because our surnames were drawn from "linkable" census records, these percentages correspond to the records where we have the greatest change of establishing a valid link.

B.2 Linked data

B.2.1 Assessing the quality of our linked dataset

A natural check on the accuracy of our linking procedure is to compare the distance between census district and death district in the linked sample. We would expect the share of matches to diminish rapidly with the distance between the census and death district, since migration should be less common between more distant locations. This provides an opportunity to test the reasonableness of our results.

Panel A of Figure 10 presents histograms showing the share of linked deaths by distance bins using data from both the 1851-1855 and 1861-1865 periods. Distance is calculated using latitude and longitude coordinates for the main town or administrative center for each district (or the geographic center for a few very rural districts). The left panel includes links within the same district, while these are dropped in the right panel in order to make it easier to view the pattern for links across districts. In the left graph, we can see that just under half of all links occurred within a district. In the right graph, we see that the share of links across districts declines rapidly with distance.[46]

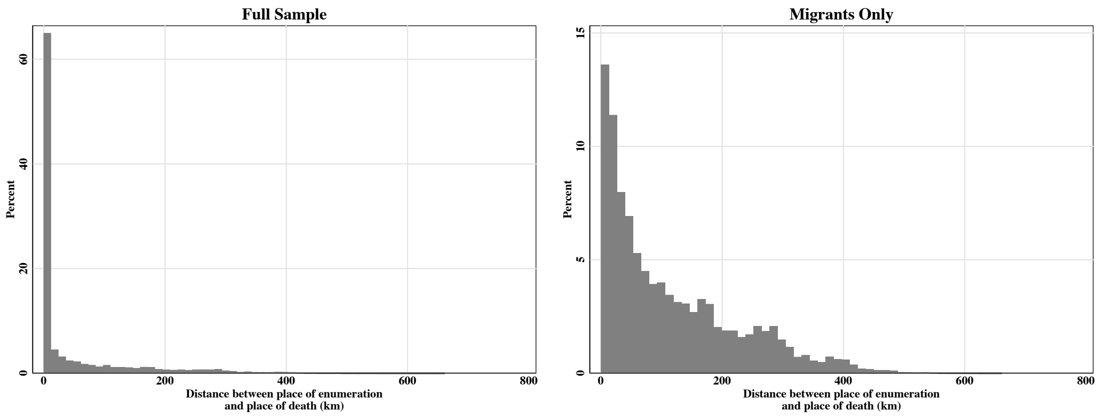
As a point of comparison, we can apply our linking algorithm to link the universe of living individuals between the 1851 census and the 1861 census. Note here that we

[46]The bump at about 250 km corresponds to the distance between the two major population centers in the country, London in the Southeast, and Manchester and Liverpool in the Northwest.

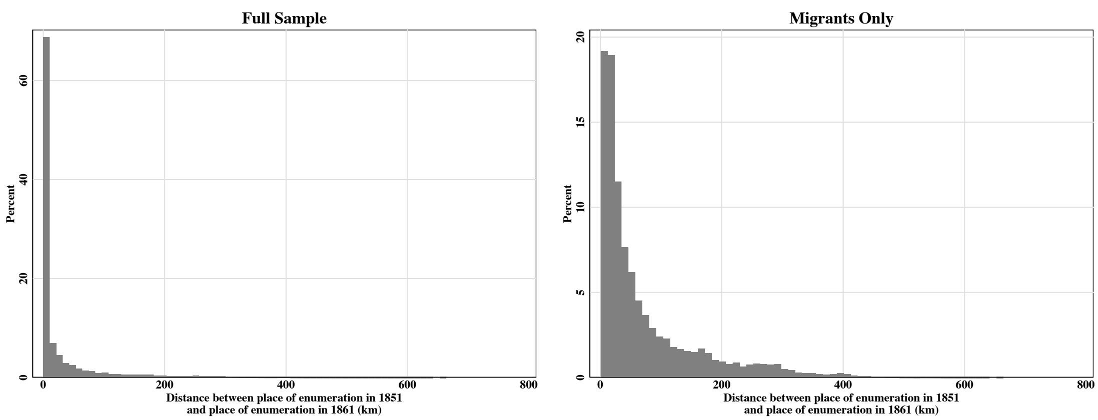
are linking individuals that we think survived to be enumerated in the 1861 census. After generating those links, we can then compute the distance between the district of enumeration in 1851 and that in 1861. The histogram of these distances is provided in Panel B. Here we see a pattern that is nearly identical to Panel A. One may worry that this is a statistical artifact. To address this, in Panel C we randomly link individuals from the 1851 census to other records in the 1851 census. In this sample, essentially all of the records are false positives. We then calculate the distance between the two districts of enumeration. Here, we see a distribution that has more of a hump shape, and is clearly different from both Panel A and Panel B. Thus, to summarize, the results of this exercise suggest that the patterns of migration in our linked dataset mimic the patterns of migration that we should expect to see during this time period, and do not appear to be an artifact of the linking procedure.

Figure 10: Share of links by distance between census and death districts

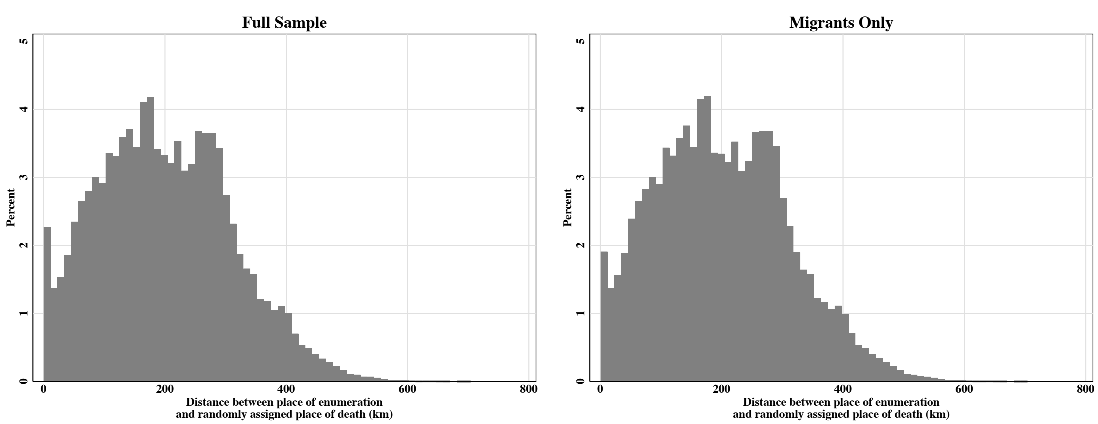
Panel A: Distance between district of death and district of enumeration



Panel B: Distribution of distances after applying our linking algorithm to link individuals between the 1851 and 1861 censuses



Panel C: Distribution of distances when randomly linking individuals from the 1851 census to other records in 1851



B.3 Representativeness: Comparing the linked sample to aggregate data

Representativeness by age

This subsection analyzes how representative our linked sample of deaths is of aggregate deaths, i.e., the comprehensive mortality aggregates published by the Registrar General. One dimension that we can compare is the age distribution in the two datasets. This is perhaps the most important dimension to consider, given the strong relationship between age and mortality risk. The top panels of Figure 11 compare the share of linked deaths in each age group and the share of aggregate deaths in each age group for the 1851 and 1861 periods. These graphs show that infant and young child deaths are substantially under-represented in the linked sample. The fact that our linked sample struggles to reflect deaths among young children is a mechanical result of our approach, since deaths among infants born after enumeration cannot be linked back to the corresponding census. It is worth noting that the linked distributions are nearly identical to each other (see the bottom panel of Figure 11), and so this potential bias is unlikely to be related to treatment. As discussed in the main text, we consider two approaches for dealing with differences in the age distribution of deaths in the linked sample relative to true distribution. In one approach we re-weight each linked death such that our linked sample mirrors the aggregate age distribution before collapsing to the district level. Alternatively, we analyze different age groups separately.

Representativeness by gender

Table 6 breaks down gender shares of deaths in the linked and aggregate data by time period. We can see that women are over-represented in the sample of linked deaths relative to their share of aggregate deaths. It is worth noting that this feature appears in both the 1851 and 1861 data. Also, both the linked and aggregate data

Figure 11: Histogram of deaths by age from linked and aggregate data

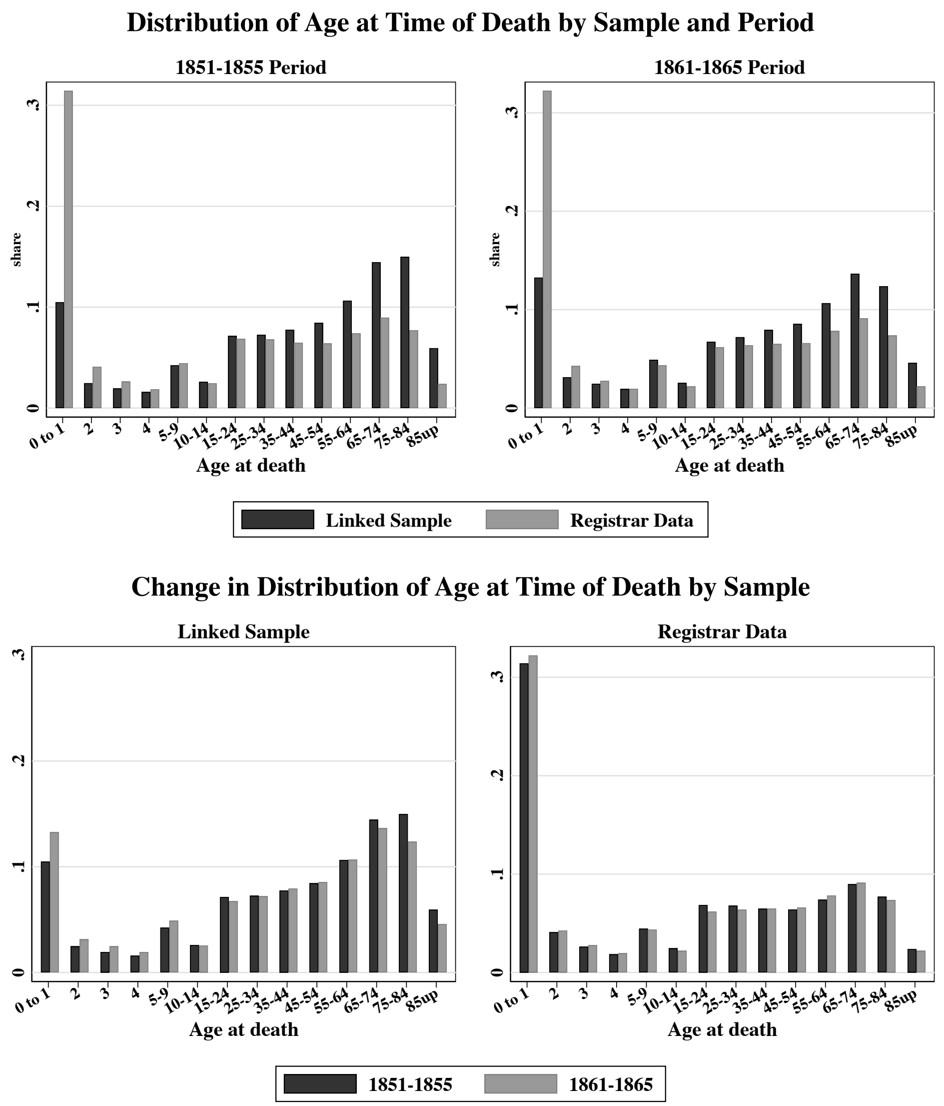


Table 6: Female shares of linked and aggregate deaths by time period

	Linked deaths		Aggregate deaths	
	1851	1861	1851	1861
Women	51.4%	50.5%	49.2%	48.8%

show very similar declines in the female share of deaths between the two periods. The most likely explanation for the higher share of female deaths in the linked sample is that women had more unique names, allowing us to generate more unique matches. This is consistent with evidence from the sociology literature suggesting that parents are more likely to give male children traditional names (Rossi, 1965).

Representativeness by SES

Next we examine the representativeness of our sample by socioeconomic status (SES). To do so, we take advantage of the occupation data available in the linked census data, which has been classified by HISCO score. We can compare this to aggregate data that we have gathered from the Registrar General's report for 1851, which lists the number of deaths among people in each occupation in that year. In the analysis below, we focus on comparing shares of white-collar and blue-collar workers among the linked sample for which occupations are given in the census. We define white-collar workers as HISCO groups 1-3, which includes professional and technical workers, administrative and managerial workers, and clerical workers. We focus on the white-collar vs. blue-collar comparisons because an analysis at more detailed occupation levels is made difficult by the fact that the HISCO classes used in the individual-level census data are not a great fit for the groupings used in the aggregate data, so it is difficult to do this comparison at a detailed occupation level. For example, those working in sales are classed with their industry in the aggregate data (e.g., "Other workers, dealers in flax and cotton") while in the HISCO classifications, a cotton dealer would be categorized differently from a cotton worker. As similar issue exists

Table 7: White-collar share of deaths in linked and aggregate data

	Linked deaths in 1851	Linked deaths in 1861	Aggregate deaths in 1851
White-collar share	6.0%	6.4%	7.6%

for foremen and managers, who are classed with their industry in the aggregate data but not in the HISCO classifications.

The results, in Table 7, show that the shares of deaths among white-collar vs. blue-collar workers in the linked sample are very similar to the shares observed in the aggregate data for 1851. Thus, our linked sample appears to be fairly representative of the aggregate data in terms of SES.

C Online Appendix: Methods

C.1 Description of the permutation exercise

One potential worry in our analysis is that the spatially concentrated nature of our set of treated districts may be influencing the statistical significance of our results. This section describes a permutation test designed to provide an alternative approach to evaluating the statistical significance of our results, while still respecting the spatial structure of our data. The basic approach here is to construct alternative sets of spatially concentrated placebo treatment districts, surrounded by rings of placebo "nearby" districts, apply our standard estimating procedure to each of these sets of placebo treated and nearby districts, and then compare the distribution of estimated results to the coefficients obtained from our true treated and nearby districts.

To implement this approach, we start each permutation with a different "anchor" district. Since there are 538 districts in our main linked data analysis, we run 538 different permutations using each district as an anchor district. For each anchor district, we identify the 23 nearest districts and call them our placebo treated districts. This gives us 24 treated districts (23 plus the anchor district), matching the number of cotton districts that were actually treated. We then identify the next 26 nearest districts and call them the first set of nearby districts, matching the 26 districts within 25 km of the cotton districts in our main analysis. The next 32 districts are called the second set of nearby districts (matching the 32 districts 25-50 km from the cotton districts), while the next 36 nearest districts are the third set of nearby districts (matching the 36 districts 50-75 km from the cotton districts). Thus, we end up with a set of placebo treated and placebo nearby districts which are both spatially concentrated and the same, in terms of number, as the true treated and nearby districts used in the main analysis. Note that, as is standard in permutation exercises, we apply this approach to all districts in the data, including the districts that were actually treated (the cotton districts).

Given each set of placebo treated and nearby districts, we then apply our standard estimation procedure and recover estimated coefficients for the change in mortality in each group of placebo treated districts comparing the 1861-1865 period to the 1851-1855 period. This provides a distribution of coefficients which can then be compared to the coefficients obtained when running the analysis on the actual cotton districts. The permutation test p-values reported in the main text reflect the share of coefficients for the placebo treated districts which exceed the coefficient obtained from the actual cotton districts.

Intuitively, the idea behind this exercise is that if having spatially clustered treated districts leads to understated standard errors in our main analysis, then applying our analysis to spatially clustered sets of placebo districts should generate a more spread-out distribution of placebo coefficients than we would expect given our standard errors, and as a result, the p-values from the permutation exercise should be larger than the standard p-values obtained from our main regressions. Of course, there are a number of reasonable alternative ways to implement a permutation exercise. For example, we could have used districts within 25 km of the placebo treated districts as our first set of nearby districts, rather than the 26 nearest districts. Ultimately, we think it is unlikely that variations like this will make any substantial difference in the results.

There are two natural alternative approaches to dealing with spatial correlation in our data. One approach is to cluster standard errors at some higher geographic level, such as the county. However, we think this approach is undesirable for two reasons. First, many of the cotton districts are in Lancashire, which is a large and diverse county with different areas that have starkly different economic structures. For example, Barrow-in-Furness was a major steel and shipbuilding center with an economy that was starkly different than that of the cotton textile districts, while Liverpool was a major trading center. As such, we do not think it is reasonable to include these together. Second, if we cluster by county, the cotton districts fall into only two counties. Clustering data in this way is likely to cause statistical issues (Donald & Lang, 2007). Despite these concerns, we have generated results clustering standard errors by county. We find that these tend to deliver smaller standard errors (more statistically significant results) than those reported in the main text, suggesting that there may be negative spatial correlation across districts within the same county. This is not surprising given that other studies looking across British districts during this time period have found evidence of negative spatial correlation (Hanlon, 2017). See that paper for a discussion of why negative spatial correlation is not surprising in this context.

A more promising alternative to clustering is to implement spatial standard errors following Conley (1999). However, we are also hesitant to take this approach because the statistical properties when the treated districts are spatially concentrated are not well-studied. Despite these concerns, we have also implemented this approach on our main linked data sample, and we find that it delivers smaller standard errors (more statistically significant results) than those reported in the main text. Again, this finding is consistent with negative spatial correlation across districts.

D Online Appendix: Analysis

D.1 Additional analysis of the linked data

Various robustness checks

Here, we investigate the robustness of the all-age result in Table 8. For comparison purposes, in Column 1 we reproduce our preferred specification. In Column 2, we present results where we do not weight the regressions by initial district population. In Column 3 we drop Manchester, which is an outlier among the cotton districts in terms of city size and because it was the commercial center of the industry. In Column 4 we also drop both Liverpool, which was not a cotton district, but which was the main port serving the industry; and Leeds, which was an important nearby wool-producing center. In Column 5, we use the birthplace information in our linked census data to confine our linked sample to only those workers born in England or Wales ("native-born") in order to assess whether deaths among immigrant workers are driving our results.

Additional linking criteria/handling of false positives

Next, we consider how our main results are affected when we modify our underlying linking procedure. In particular, we consider two modifications that eliminate from our linked data any deaths where there are other potential links that sound similar to the name in question when spoken. In the first modification, we eliminate a linked individual if, for that individual, the first and last names are unique in the death records, but in the census record, there is another record where the first name matches exactly and there is a similar-sounding last name, i.e., the last name is the

Table 8: Robustness of linked-data results on all-age mortality rates

DV: Death per 1,000 Individuals (per year)	Baseline	No weights	Without Manchester	Drop Manchester Liverpool & Leeds	Only native-born
	(1)	(2)	(3)	(4)	(5)
Cotton District × Shortage	2.534***	2.715***	2.609***	2.424***	2.291***
	(0.605)	(0.769)	(0.657)	(0.661)	(0.564)
Nearby (0-25 km) × Shortage	1.054*	1.447	1.058*	0.853	0.609
	(0.597)	(0.925)	(0.596)	(0.602)	(0.591)
Nearby (25-50 km) × Shortage	0.191	0.735	0.194	0.729	0.233
	(0.623)	(0.686)	(0.624)	(0.559)	(0.507)
Nearby (50-75 km) × Shortage	0.586	0.143	0.583	0.482	0.245
	(0.656)	(0.646)	(0.656)	(0.645)	(0.596)
Observations	1,076	1,076	1,074	1,070	1,076
R-squared	0.398	0.333	0.396	0.401	0.404

same after Soundex cleaning. Results obtained using these two alternative linked samples, and assigning deaths to individuals' initial district of residence (as in Table 3), are presented in Columns 1-2 of Table 9. This table shows that our main results continue to hold, even when using these severely limited samples.

As a second way to assess the impact of false positives on our results, we limit our analysis to samples where false positives are less common. In particular, we take advantage of the fact that, mechanically, false positives are more likely for links where the death district is further from the census district. This is because people are less likely to migrate over longer distances, but false matches are just as likely to occur between distant districts as they are between nearby districts. As a result, the share of false matches to true matches will rise as the distance between the census district and the death district increases.

Columns 3-6 of Table 9 present results obtained when limiting our linked sample to those where the census district and death district are proximate to one another.

In Column 3 we consider only links under 200km and we further reduce this distance in Columns 4 and 5. In Column 6 we consider only results where the census district and the death district match. At the bottom of the table we can see that applying these restrictions progressively reduces the rate of false positives in our sample (with these rates coming from the test described in in section 3.3). If bias generated by false positives was affecting our results, then we should expect our estimates to fall as we limit the sample to observations with fewer false positive links. Instead, the results remain very stable across Columns 3-6.

Table 9: Linked-data analysis using alternative linking restrictions

	DV: Deaths per 1,000 Individuals (per year)					
	Requiring links to have a unique-sounding name		Omitting links where death and enumeration district are far apart			
	Last Name	First and Last	0-200 km only	0-100 km only	0-50 km only	No Migration
	(1)	(2)	(3)	(4)	(5)	(6)
Cotton District × Shortage	2.592***	2.375**	2.826***	2.533***	2.352***	5.545***
	(0.900)	(1.027)	(0.607)	(0.685)	(0.735)	(1.316)
Nearby (0-25 km) × Shortage	0.342	0.605	1.064*	0.307	0.077	1.039
	(0.835)	(0.926)	(0.612)	(0.728)	(0.798)	(1.013)
Nearby (25-50 km) × Shortage	0.193	0.731	0.137	-0.741	-0.943	-1.741**
	(0.887)	(1.002)	(0.605)	(0.641)	(0.676)	(0.853)
Nearby (50-75 km) × Shortage	0.435	0.642	0.484	-0.056	0.101	0.572
	(0.950)	(0.993)	(0.644)	(0.709)	(0.819)	(0.937)
Observations	1,074	1,074	1,076	1,076	1,074	1,066
R-squared	0.315	0.281	0.406	0.413	0.402	0.405
Lower Bound False Positive Rate	7.72%	7.50%	8.19%	7.57%	6.88%	5.31%
Upper Bound False Positive Rate	13.16%	12.38%	16.35%	15.23%	14.15%	11.80%
Linked Deaths	127,676	112,297	252,667	227,660	208,109	166,406

D.2 Additional results by employment and location

In the results breaking effects down both by location and sector of employment in main text Table 4, we classify individuals based on the occupation of the household head. That is a useful approach because of the important role of household head earnings, as well as the fact that it allows us to study the mortality effect on nonworkers, such as children. However, an alternative approach that can be used for workers is to classify them by their own occupation, rather than the household head's occupation. Results obtained using this alternative approach, for those over age 15 with an occupation provided in the census, are presented in Table 10. These results show that being located in a cotton textile district had a strong influence on workers, while we see weaker effects if the workers were themselves employed in the cotton textile industry. Note that the magnitude of the results are larger than those shown in Table 4 (except the results for older adults) because these data do not include those under 15, where we saw no increase in mortality associated with the shock.

Table 10: Effects by employment sector and location using own occupation

	DV: Deaths per 1,000 persons (per year)		
	(1)	(2)	(3)
Cotton District × Cotton Shortage	2.306***		2.254***
	(0.699)		(0.643)
Employed in Cotton × Cotton Shortage		1.090	0.204
		(0.886)	(0.813)
Observations	33,445	33,445	33,445
R-squared	0.015	0.015	0.015

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered at the district level, in parentheses. All regressions include period and district fixed effects, a series of indicators for whether the district is 0-25, 25-50, or 50-75 km from a cotton textile district in the shortage period, region-by-period fixed effects, ln(population density), the share of the population that is under 15, and the share of the population that is over 54. Deaths are assigned to the initial district of residence (i.e., census enumeration district). Regressions are weighted by district population.

Next, we exploit the occupation-by-location data to study changes in mortality patterns across all occupation groups. As in the main analysis, we begin by classifying each individual in the data based on the occupation of their household head. We then run regressions using the following specification:

$$\left(\frac{\widetilde{MORT}_{dst}}{POP_{dst}}\right) = \sum_{s \neq COT} b_{1s} OCC_s * POST_t$$

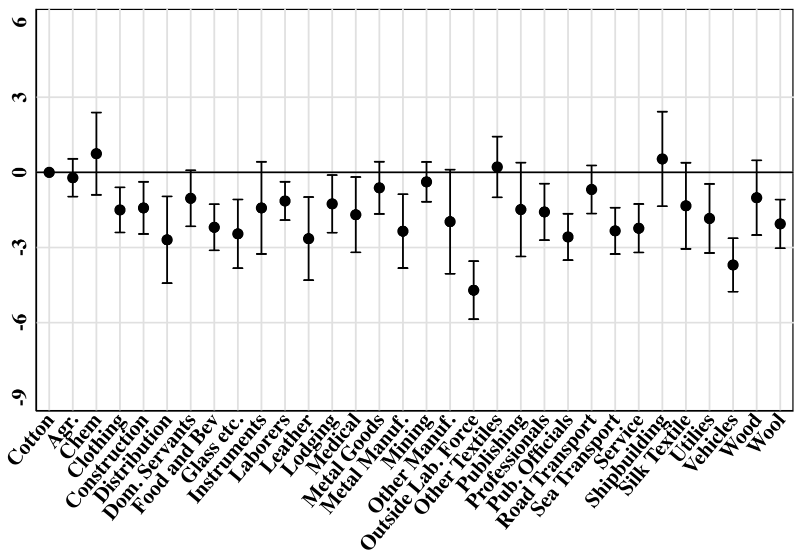
$$+ \sum_{s \neq COT} b_{2s} OCC_s * COTDIST_d * POST_t + X_{dt}\Gamma + \phi_d + \eta_t + \theta_s + \epsilon_{dot} \, ,$$

where OCC_s is an indicator for households classified in occupation s. In this specification, the b_{1s} coefficients allow us to study how mortality changed in each occupation in the 1861-1865 period compared to 1851-1855. The omitted industry here is cotton textiles, so that the b_{1s} coefficients reflect how mortality for households associated with each non-cotton industry changed compared to the mortality change observed in the cotton textile industry. The b_{2s} coefficients allow us to study how mortality for households associated with each non-cotton industry changed in the cotton textile districts. These coefficients allow us to learn about how mortality changed for households not directly reliant on cotton textile employment, but living in the areas affected by cotton shock.

Figure 12 presents the estimated b_{1s} coefficients and confidence intervals. The main pattern to note here is that the vast majority of coefficient estimates are negative, and many are also statistically significant. Since these estimates reflect mortality rates relative to cotton textile households, this is telling us that cotton textile households experienced a greater increase in mortality during the shock period than households reliant on almost any other occupation.

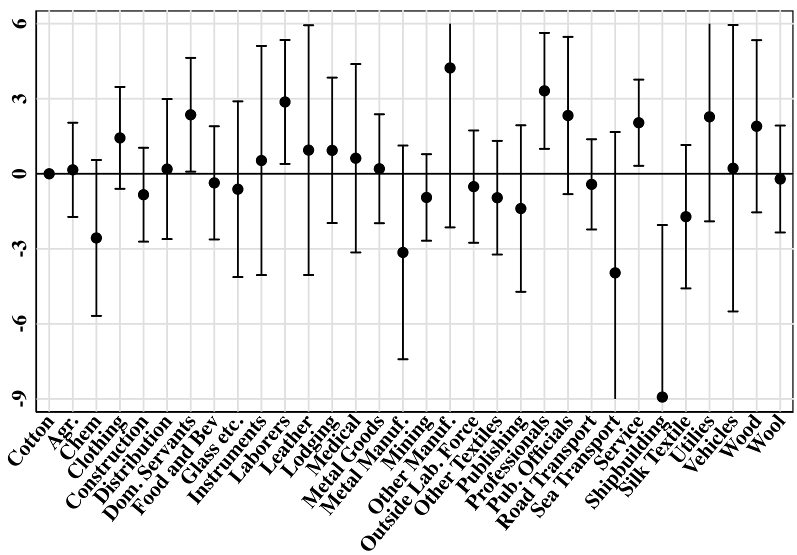
Figure 13 presents the estimated $b_2 s$ coefficients and confidence intervals. These reflect how mortality among households reliant on non-cotton industries, but resident in the cotton textile districts, fared during the shock, relative to mortality in those industries in other locations. The households that experienced particularly large increases in mortality in the cotton districts during the shock are those with household heads classified as domestic servants, laborers, service workers, and professional services. There is a common thread here; these are all providers of non-traded local services. In addition, all except professionals are low-wage occupations where households would likely have already been near poverty. It is worth contrasting this

Figure 12: Estimated change in mortality in 1861-1865 compared by 1851-1855 by occupation



list with the set of households that experienced relatively smaller mortality effects (negative coefficients). In order, these are: shipbuilding, sea transport, metal manufacturing, chemicals, silk textiles, and publishing. Other than sea transport, these are all producers of tradable goods. While we should be careful not to draw overly-strong conclusions from these results, the patterns certainly suggest that being in an area negatively impacted by the cotton shock hurt those employed in providing local services more than those reliant on manufacturing traded goods.

Figure 13: Estimated change in mortality in 1861-1865 compared by 1851-1865 by occupation in the cotton textile districts



D.3 Results using an alternative linked dataset

This subsection presents results from a different linked dataset which uses "BMD" deaths data. This alternative set of death records comes from freeBMD.org, which has separately digitized the underlying death records from the General Registrar's Office. These records include the decedent's first and last name, as well as district, year, and quarter of death. These data are available for the full 1851-1870 period.

Since the BMD data come from a different source and are linked using a different procedure, they can provide a useful robustness check on our main results using the GRO deaths data. The BMD data are inferior to the GRO data in that they do not include information on age at death, which is why the GRO data are preferred for our main analysis. However, the BMD data do have one advantage, which is that they are available for 1856-1860 and 1866-70, which allows us to undertake some additional placebo exercises as well as to look at effects post-shock.

The linking procedure applied to this dataset involves matching unique first and last name combinations between the census and death records. We begin by restricting our set of potential links to the set of deaths and census records with unique first and last names. We then link those that match perfectly as written. This procedure yields a matched sample of 71,566 individuals who died between 1851 and 1855, and 81,221 individuals for the 1861-1865 period, representing 3.6% and 3.8% of all deaths over the respective periods. Note that, without the age information used in our main GRO death data, we obtain lower matching rates and a substantially smaller matched dataset. We also link deaths from 1856-1860 back to the 1851 census and deaths from 1866-70 back to the 1861 census.

Below, we begin by examining the representativeness of the BMD data and comparing it to the GRO data. We then replicate our main results using the BMD data. That is followed by a placebo exercise and then an examination of longer-run outcomes after 1865.

Representativeness

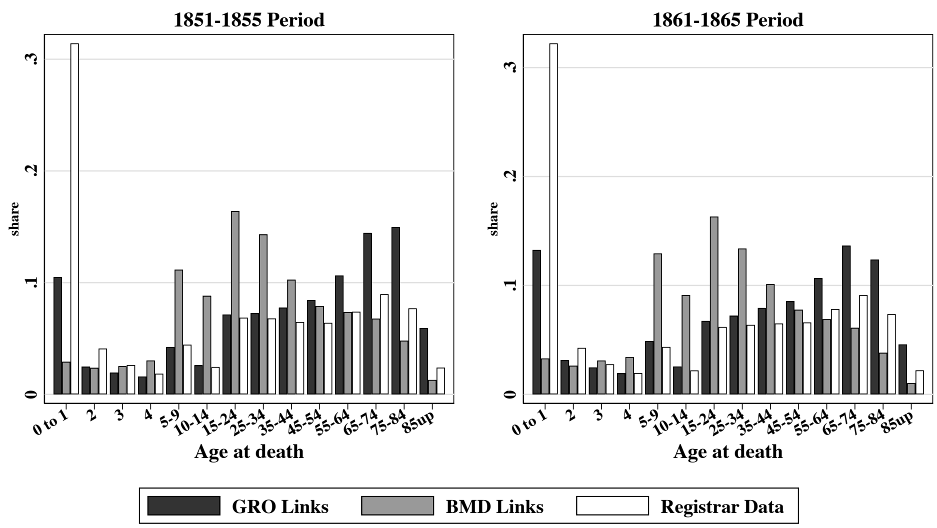
As with the GRO data, the representativeness of the linked BMD data can be assessed along several dimensions. One of these is the breakdown by gender. In the BMD data, the female share of deaths in 1851-1855 is 54.3 percent and in the 1861-1865 period it is 53.4 percent. Thus, females are overrepresented relative to the universe of deaths, reported in the aggregate Registrar General's reports, where the female share is 49.2 in 1851-55 and 48.8 in 1861-65. The BMD data is also less representative than the GRO data in this dimension; the GRO shares, 51.4 in 1851-55 and 50.5 in 1861-65 are substantially closer to the shares reported in the aggregate statistics. As discussed in the main text, females are most likely overrepresented because female names are more likely to be unique than male names. This has a bigger effect in the BMD data, which rely only on unique names to make links, than in the GRO data, where name uniqueness is less important because we are also able to use age information in linking. This is a potentially important issue since females may have been affected by the shock in a way that differed from males.

Another dimension along which we can assess representativeness is age. Figure 14 provides histograms comparing the share of deaths in the aggregate Registrar's data to both the GRO and BMD data. As discussed in the main text, our data will not be representative of deaths at young ages for mechanical reasons. However, as Figure 14 shows, this issue is more severe in the BMD data than in the GRO data. In addition young adults are substantially overrepresented in the BMD data while older adults are underrepresented.

Replicating our main results

In this section we replicate our main analysis results using the alternative BMD data set. We follow a procedure that is essentially identical to that used in our main analysis of the GRO data. Specifically, we link deaths from 1851-1855 back to their corresponding 1851 census record, and deaths from 1861-1865 back to their 1861 census entry and then conduct a difference-in-difference analysis.

Figure 14: Histogram of deaths by age at time of death from linked and aggregate data



Results obtained using the BMD data are presented in Table 11. Column 1 presents results calculated across all age groups while Columns 2-4 break results down by age group. Note that, because the linked BMD data set is substantially smaller than the GRO data set, it is necessary to use more aggregated age groups in the results in Columns 2-4 than those used in the main text.

As in our main analysis, the BMD results show evidence of a substantial increase in mortality among older adults, but no measurable effect among the young or prime-aged adults. The magnitude of the effect among the elderly is similar to what we obtained for older ages in our main GRO analysis. The estimated all-age effect presented in Column 1 is smaller than that observed in the GRO data. This is clearly driven by negative estimated effect among younger populations. It is notable that these are populations where the BMD data set is less representative than the GRO data. In general, the broad patterns observed in the BMD data are generally in-line with those presented in our main analysis of the GRO data, despite the fact that substantial differences exist between these two alternative linked datasets.

Table 11: Baseline regressions from BMD to Census links

DV: Deaths per 1,000 Individuals (per year)				
	All Ages	**Under 15**	**Age 15-54**	**Over 54**
	(1)	(2)	(3)	(4)
Cotton District × Cotton Shortage	0.869	-1.026	-0.244	11.255***
	(0.835)	(1.408)	(0.469)	(3.497)
Nearby (0-25 km) × Cotton Shortage	1.766	4.250**	0.488	8.124**
	(1.188)	(1.848)	(0.442)	(3.434)
Nearby (25-50 km) × Cotton Shortage	0.819	0.937	0.343	3.872
	(1.164)	(1.690)	(0.483)	(3.294)
Nearby (50-75 km) × Cotton Shortage	-1.229	-1.507	-0.048	0.224
	(0.932)	(1.327)	(0.467)	(3.269)
Observations	1,074	1,074	1,074	1,074
Linked deaths	147,434	53,927	67,284	26,223

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered at the district level, in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by population. All regressions include district fixed effects, period fixed effects, region-by-period fixed effects, and controls for ln(population density), and the share of the population (in each district-by-period cell) that has a linkable name. The all-age regression in Column 1 also includes controls for the share of the population in each of the following age categories: under 15, 15-54, and over 54 (with 15-54 as the omitted category).

Placebo exercise

One advantages of the BMD database is that we also have linked deaths for a longer period than in our main analysis. This allows us to undertake other tests to validate and extend our main findings. For one, the BMD data contain linked deaths for 1856-1860. In Table 12 we use these links to conduct a placebo exercise in which we treat the 1856-1860 period as a placebo "treated" period and look at whether we observe any differential mortality patterns in the cotton textile districts in that period relative to the 1851-1855. These results show no evidence of differential mortality patterns in the cotton textile districts in the 1856-1860 period. This tells us that there do not appear to be differential pre-trends in mortality that might be behind our main results.

Table 12: Placebo test: 1856-1860 (placebo) against 1851-1855 (control)

DV: Deaths per 1,000 Individuals (per year)	**Under 15** (1)	**Age 15-54** (2)	**Over 54** (3)
Cotton District × Placebo Period	-0.804	-0.736	-3.087
	(1.403)	(0.450)	(3.206)
Nearby (0-25 km) × Placebo Period	1.703	-1.175**	1.957
	(1.566)	(0.498)	(3.138)
Nearby (25-50 km) × Placebo Period	-0.212	0.002	-1.412
	(1.462)	(0.337)	(2.466)
Nearby (50-75 km) × Placebo Period	0.313	0.053	-6.507*
	(1.592)	(0.427)	(3.363)
Observations	1,074	1,074	1,074
R-squared	0.067	0.032	0.040
Linked deaths	40,376	57,809	22,424

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors, clustered at the district level, in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by population. All regressions include district fixed effects, period fixed effects, region-by-period fixed effects, and controls for ln(population density, and the share of the population (in each district-by-period cell) that has a linkable name.

Examining harvesting versus scarring effects

For another, the linked BMD dataset also includes data from 1866-1870, which can be used to assess whether the cotton shock had persistent impacts on mortality—that is, we investigate whether our results reflect "harvesting," (i.e., the possibility that the cotton shortage merely hastened the deaths of those who would have anyway died within a short period thereafter), a phenomenon which has important implications for the overall mortality cost of the downturn. This is a particular concern given that our strongest adverse effects appear among older adults. To evaluate harvesting, we look at mortality rates among the treated populations in the period after the shock, 1866-1870, as compared to those in the 1856-1860 period, which is the most comparable available control period.

These results, presented in Table 13, indicate that mortality levels remained elevated among the adults exposed to the cotton shock (i.e., those resident in the cotton districts in 1861) in the years after 1865, especially older adults. This tells us that either no substantial harvesting occurred, or that any harvesting effect was dominated by the persistent effect of the recession on health. This sustained effect could be due to a number of factors including "scarring" i.e., a reduction in health capital during the shock that increased mortality risk in the next period; and persistent economic effects that continued even after cotton supplies resumed, for instance, through ongoing congestion. Regardless of channel, the fact that we do not find strong evidence of harvesting indicates that the pattern observed during the cotton shock was not merely confined to populations that would have died imminently in the absence of the downturn. These results represent an important contribution to the literature on business cycles and mortality, which has engaged relatively little with the mortality dynamics of local economic shocks. Here, scarring and harvesting are not only of substantive interest as phenomena affecting health, but they may also confound inference in traditional approaches using annual aggregate panel data.

Table 13: Harvesting test: 1866-70 (post-shortage) against 1856-60 (control)

DV: Deaths per 1,000 Individuals (per year)			
	Under 15	**Age 15-54**	**Over 54**
	(1)	(2)	(3)
Cotton District × Post-Shortage Period	-0.726	0.851*	10.026**
	(1.356)	(0.488)	(4.328)
Nearby (0-25 km) × Post-Shortage Period	3.099	1.761***	5.466
	(1.989)	(0.609)	(4.182)
Nearby (25-50 km) × Post-Shortage Period	2.051*	0.208	-4.083
	(1.223)	(0.399)	(3.409)
Nearby (50-75 km) × Post-Shortage Period	0.366	-0.636	2.088
	(1.883)	(0.444)	(3.702)
Observations	1,076	1,076	1,076
R-squared	0.064	0.115	0.058
Linked deaths	46,580	66,235	23,003

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors, clustered at the district level, in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by population. All regressions include district fixed effects, period fixed effects, region-by-period fixed effects, and controls for ln(population density, and the share of the population (in each district-by-period cell) that has a linkable name.

D.4 Additional results using aggregate data

In Table 14, we examine the results on infants in more detail.[47] We find evidence that the infant mortality rate fell in cotton districts during the crisis, as did the birth rate. The former results are consistent with the possibility that infant health improved as a function of maternal time reallocation (a story consistent with contemporary reports discussed in Section 3.6, as well as with the results in Miller & Urdinola (2010), which suggest that in poor settings, child health may be more intensive in maternal time than in income), the possibility of negatively-selected out-migration (thus raising the health "quality" of remaining mothers), or the possibility of positive selection into childbearing due to the downturn (consistent with work like Dehejia & Lleras-Muney (2004), which finds low- but not high-SES mothers deferring childbearing in a modern recession). Given the concurrent reduction in birth rates, the last explanation of the infant health results appears most plausible, though it is difficult to draw strong conclusions: because birth rates are taken from aggregate data, these rates may have fallen in cotton districts during the shortage at least in part as a mechanical result of out-migration.[48]

[47] When considering these results, there is one important caveat: namely, because this analysis uses aggregate data on births and infant deaths rather than linked data, these results may still be subject to migration bias, and so cannot be directly compared to those presented elsewhere in our age-specific analysis. Specifically, because these outcomes are reported by place of occurrence, we will only capture the births and infant deaths to accruing to those individuals actually living in a given district in the year in question. For instance, the births and infant deaths we observe in the cotton district will be just those coming from a dynamic subset of stayers. Practically speaking, this simply means that because of unobserved/imperfectly observed migration, the number of births may be affected by the size of the remaining population, and the infant mortality rate, which takes annually-reported births as its denominator, may be affected by the composition of the remaining population.

[48] Similarly, if out-migration was highly selective, leaving behind women with preferences for relatively low fertility, this too could contribute to a reduction in the birth rate.

Table 14: Examining Fertility and Infant Health (Aggregate Data)

	Births per 1,000 persons (1)	**Infant Deaths per 1,000 births** (2)
Cotton District × Shortage	-5.019***	-13.748***
	(0.648)	(4.369)
Nearby (0-25 km) × Shortage	-2.316**	-1.207
	(0.936)	(4.315)
Nearby (25-50 km) × Shortage	-0.311	-1.270
	(0.502)	(4.107)
Nearby (50-75 km) × Shortage	0.700	1.950
	(0.474)	(3.698)
Observations	1,076	1,076
R-squared	0.926	0.940

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered at the district level, in parentheses. Regressions are weighted by population. All regressions include district fixed effects, period fixed effects, region-by-period fixed effects, and controls for ln(population density, the share of the population (in each district-by-period cell) that has a linkable name, the under 15 share, and the over 54 population share.