

# 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 the cotton textile manufacturing regions of Britain induced by the U.S. Civil War. This is a setting characterized by limited social safety nets, and where migration was a key margin of adjustment. This migratory response introduces a number of empirical challenges, which we overcome by developing a new approach to estimation. Results show a detrimental effect on health for both cotton workers and their families, as well as for residents of migrant-receiving districts, who were exposed to congestion externalities.

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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, which was caused by the U.S. Civil War.<sup>1</sup> 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, in the run-up to the war, 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. Faced with the loss of local employment, and against a backdrop of poor public assistance, many displaced workers chose to migrate in search of work elsewhere.<sup>2</sup> Although contemporary reporters would heatedly debate the human costs of the cotton shortage—one of the defining crises of 19th Century Britain—for decades to come, to this day we have little systematic evidence of its consequences for health.

Beyond its intrinsic importance as a major episode in British history, understanding the health impact of this particular downturn is important for two reasons. First, 19th Century Britain was a setting characterized by high baseline mortality rates, a poor infectious disease environment, limited medical care, and weak social safety nets. Thus, understanding the health effects of this recession, alongside the mechanisms underpinning these effects, helps us gain a deeper understanding of how a negative economic shock can affect health in a relatively poor and unhealthy society.<sup>3</sup>

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<sup>1</sup>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.

<sup>2</sup>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.

<sup>3</sup>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 entire age distribution

Second, and perhaps because of the weak social safety nets of the time, this recession induced a large migratory response. Migration of this sort is not only of substantive interest to understanding the health effects of this recession, but it also has the potential to undermine inference in meaningful ways. The methodology we develop overcomes this threat, and is likely to appeal to other researchers.

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.<sup>4</sup> 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.

Our setting offers two critical features that allow us to overcome these issues.

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comes from analysis of developed countries. Existing work on business cycles and health in low-income settings has focused primarily on infant mortality (see, for instance, the seminal work of Miller & Urdinola (2010)). As we discuss below, the mechanisms behind these two strands of literature may not generalize to working-age or elderly adults in low income settings.

<sup>4</sup>The existing literature tends to assume that migration is not a meaningful threat to inference. Lindo, 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—which we elaborate on below—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.

First, the timing and spatial incidence of the shock allow us both to cleanly identify the specific cohorts exposed to the downturn, and to more easily isolate and correct for spatial spillovers due to migration. The temporal component of the 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 which was in turn 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, and so, to generate accurate estimates of the mortality impact of this recession, even in the presence of migration. 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 adopt the following empirical approach. We begin by defining the cohorts at risk of exposure to the downturn. To do this, we take advantage of the fact that the 1861 British Census was taken just before the onset of the U.S. Civil War.<sup>5</sup> We classify those residing in cotton-producing areas at the time of enumeration as the group directly exposed to the recession. We then link individuals to deaths occurring during the cotton shortage (1861-1865).<sup>6</sup> This process produces an individual-level longitudinal dataset that allows us to accurately identify mortality patterns for the group initially resident in cotton locations, relative to residents of

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<sup>5</sup>Historical evidence makes it clear that people in both the U.S. and abroad failed to anticipate the severity of the conflict, and there is little evidence that the British economy was substantially affected until late 1861 or early 1862.

<sup>6</sup>Our approach, which we discuss further in Section 3.5, synthesizes many of the seminal papers in this literature (e.g., Ferrie (1996), Abramitzky *et al.* (2012, 2014), Feigenbaum (2015, 2016), and Bailey *et al.* (2017)).

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 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 50 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. The benefits of this approach are twofold, allowing us both to more cleanly estimate the mortality impact on those directly treated by the recession, and to identify the recession's effects on groups who were indirectly exposed via the in-migration of their cotton neighbors—effects that would otherwise remain obscured.

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. This result stands in contrast with 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. Nevertheless, some mechanisms are more familiar: as in Sullivan & von Wachter (2009), we find that displaced workers were particularly affected by the shock. Given the richness of our longitudinal microdata, which contain detailed information on occupations and family structure, we can also document a meaningful increase in mortality among other members of cotton workers' households. Our direct visibility into the household is novel in this literature. Together, cotton workers and their families account for much of the mortality increase associated with the cotton shortage.

Our second finding relates to spatial spillovers. Our baseline regressions indicate

that migrant-receiving areas also experienced an increase in mortality during the downturn. This finding is suggestive of possible congestion effects, and accordingly, we examine whether labor market competition from arriving cotton textile workers can explain these spatial spillovers. Studying the mortality rates of those most exposed to this competition—i.e., wool textile workers and their families—we find no evidence that labor market competition is behind the increase in nearby-district mortality. This implies that other congestion-related mechanisms, such as housing overcrowding or the spread of infectious disease, were likely at work.

Finally, we document the importance of our empirical approach for overcoming the bias introduced by migration. Our main linked microdata results, wherein deaths are assigned to the place of residence at the onset of the shock, show that the downturn raised the mortality rate among those residing in a cotton textile district at the onset of the cotton shortage. However, when we intentionally ignore the migration we know has taken place—namely, by re-assigning our linked deaths to the location of death, thus bringing it in line with the structure of the sort of aggregate data 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 from the very same data. We perform a similar exercise using actual aggregate data in place of linked data, and this, too, leads us to erroneously conclude that health improved rather than decayed during the recession. Together, these results provide direct evidence that migration can lead us to draw meaningfully inaccurate conclusions about the mortality impact of a downturn. Thus, our findings have implications for other studies looking at the relationship between local economic conditions and health in settings where migratory responses are prevalent. Moreover, the techniques we introduce offer a simple and intuitive solution for researchers faced with similar challenges.

One contribution of this study is to expand our understanding of the relationship between recessions and mortality outside of modern developed countries. Only a

small number of studies (Fishback *et al.*, 2007; Stuckler *et al.*, 2012) examine the impact of recessions on mortality in historical settings such as the one we consider. However, these studies use an analysis approach based on aggregate data, which, as we show, is vulnerable to bias generated by migration. Our approach overcomes this concern in order to offer more reliable estimates of the historical relationship between business cycles and health. One motivation for generating new historical evidence on the recession-mortality relationship is that the mechanisms through which recessions impact mortality are likely to be different in these than in modern settings—for instance, because of the salience of low incomes and infectious disease burdens, and the limited penetration of public health systems. 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). While some of these channels may operate in historical settings, other channels, such as malnutrition, are likely to be more important in the past than they are in the developed world today.

While our results pertain to a specific historical setting, our findings may also have implications for modern developing countries that share some of the characteristics observed in 19th Century Britain, such as poor public assistance systems, limited family savings, and high baseline mortality rates, particularly from contagious disease. A number of studies, such as Miller & Urdinola (2010), have examined the impact of recessions on infant mortality in developing settings, but much less is known about the impact of these events on the broader population there. The focus on infants mainly reflects the challenges that migration poses for the study of older populations. Because our methods allow us to overcome concerns about migration, we are able to offer new evidence on the recession-mortality relationship in a low-income setting for age groups other than infants.

A second contribution of our study is methodological in nature. The empiri-

cal strategy we introduce to deal with migration bias in our setting may also be useful for the modern literature on recessions and mortality. This large literature, which builds on the seminal contribution of Ruhm (2000), typically compares local unemployment rates to contemporaneous aggregate mortality rates within a panel difference-in-difference approach. This approach may be vulnerable to bias if recessions induce a substantial migration response. Our study provides the first direct evidence, albeit in one particular setting, on the extent to which migration can affect estimates of recession-mortality relationship. Moreover, in cases where migration poses a potential concern, the methods we introduce provide a strategy for addressing these issues in order to obtain more reliable results.

Finally, our results improve our understanding of an important historical episode. The impact of the U.S. Civil War in Britain has been the subject of a long line of work in history and economics (Arnold, 1864; Watts, 1866; Ellison, 1886). Recent studies have focused on the role of poor relief during the crisis (Kiesling, 1996; Boyer, 1997), the impact on innovation (Hanlon, 2015), and the effect on long-run city growth Hanlon (2017). Despite this attention, the health effects of this recession remain poorly understood. Many contemporaries expected the deprivation caused by the recession to raise mortality rates, with some observers reporting that “There is a wan and haggard look about the people...”<sup>7</sup> On the other hand, local health official reports showed a “lessened death-rate throughout nearly the whole of the [cotton] districts...” (Arnold, 1864). Our results help clear up this debate, by showing that migration can reconcile the reduction in deaths in the cotton districts with the evident suffering of the out-of-work cotton operatives and their families.

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<sup>7</sup>Dr. Buchanan, *Report on the Sanitary Conditions of the Cotton Towns*, Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 301.

## 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 1861.<sup>8</sup> This concentration, which dates back to at least 1830, is thought to be driven by 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). The right-hand panel of 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.

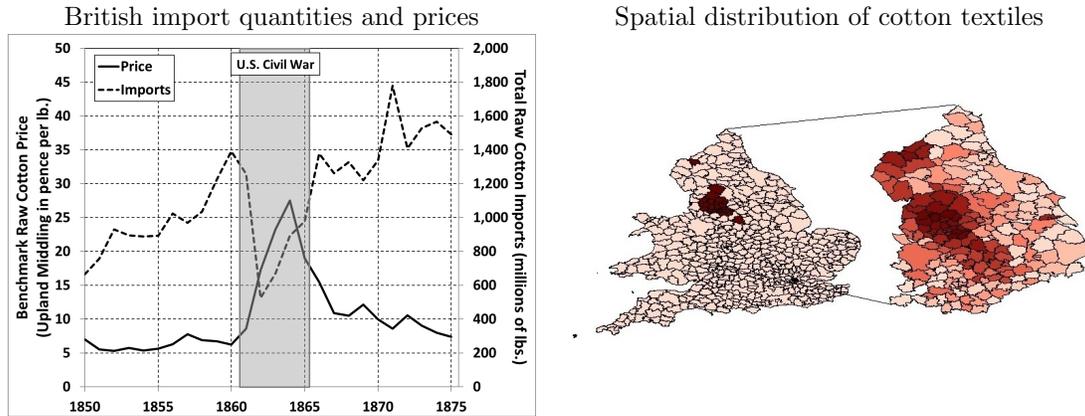
Because Britain did not produce cotton, the success of its cotton textile industry was dependent on reliable access to imported raw cotton—and in the run-up to the U.S. Civil War, 70% of these inputs came from the U.S. South (Mitchell, 1988). The war prompted a sudden and dramatic rise in world cotton prices, sharply reducing British imports of U.S. cotton, and causing a sharp drop in British cotton textile production. These effects are depicted in the left-hand panel of Figure 1. 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.<sup>9</sup>

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<sup>8</sup>Calculation based on data collected by the authors from the 1861 Census of Population reports.

<sup>9</sup>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: Cotton prices, imports, and spatial distribution of cotton textile industry

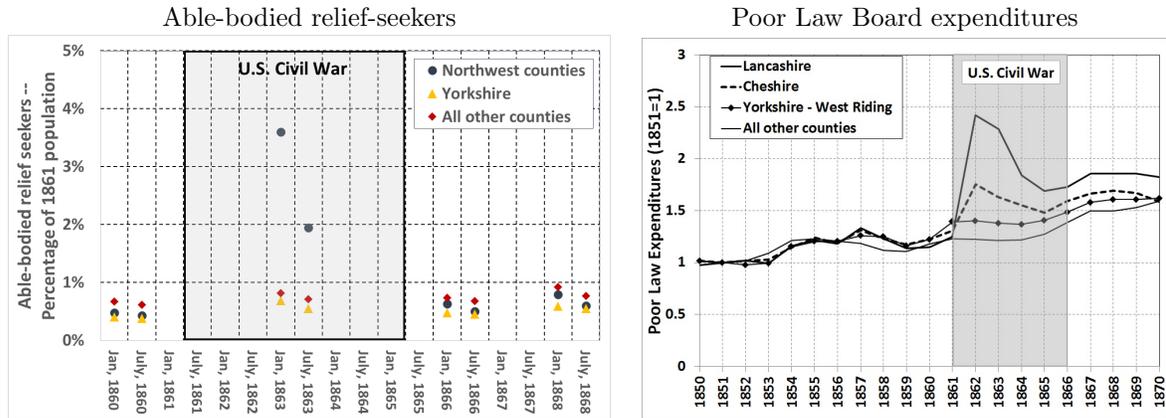


Import data from Mitchell (1988). Price data, from Mitchell & Deane (1962), are for the benchmark Upland Middling variety. Data on the geography of the cotton textile industry are calculated from the 1851 Census of Population. Shaded in the map of England & Wales are districts with over 10% of employment in cotton, while the inset shows the share of employment in cotton in the core cotton region, with darker colors indicating a greater share of employment in cotton.

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.1). 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 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 2 offers additional support for this conclusion. The left-hand panel 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. During the downturn, we see an increase in relief-seekers in the Northwest counties (Lancashire and Cheshire), where cotton textile production was concentrated. Non-

Figure 2: The spatial incidence of the cotton shock



Expenditure data were collected by the authors from the annual reports of the Poor Law Board. Data on relief-seekers come from Southall *et al.* (1998) (left-hand graph reproduced from Hanlon (2017)).

cotton counties, however, were largely unaffected. Similar patterns are documented in the right-hand panel, which examines Poor Law expenditures.

## 2.2 Responses to the cotton shortage

In the face of the cotton shortage, workers in the affected areas employed a variety of coping mechanisms. Reports indicate that at the height of the recession (winter 1862), roughly 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).<sup>10</sup> This relief, however, differs sharply from the social safety nets of today. Poor Law funds were associated with pauperism and only provided funds 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 stig-

<sup>10</sup>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.

matized 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) and Arnold (1864)). Many eventually turned to poor relief, but others migrated in search of work elsewhere.

Figure 3 provides evidence on the geography of these migration patterns. In the left-hand panel of Figure 3, we map implied net migration by district from 1851-1861.<sup>11</sup> The cotton textile districts show a strong pattern of net in-migration (dark colors) in this decade. In the right-hand panel, we plot the change in net migration in 1861-1871 compared to 1851-1861. These maps show that while cotton textile districts had been an important destination for migrants in the 1850s, this pattern reversed during the decade of the U.S. Civil War. In addition, during that decade there is evidence of a substantial increase in migration into districts just outside of the main cotton textile region. Thus, we observe a substantial and short-distance migration response to the cotton shortage.

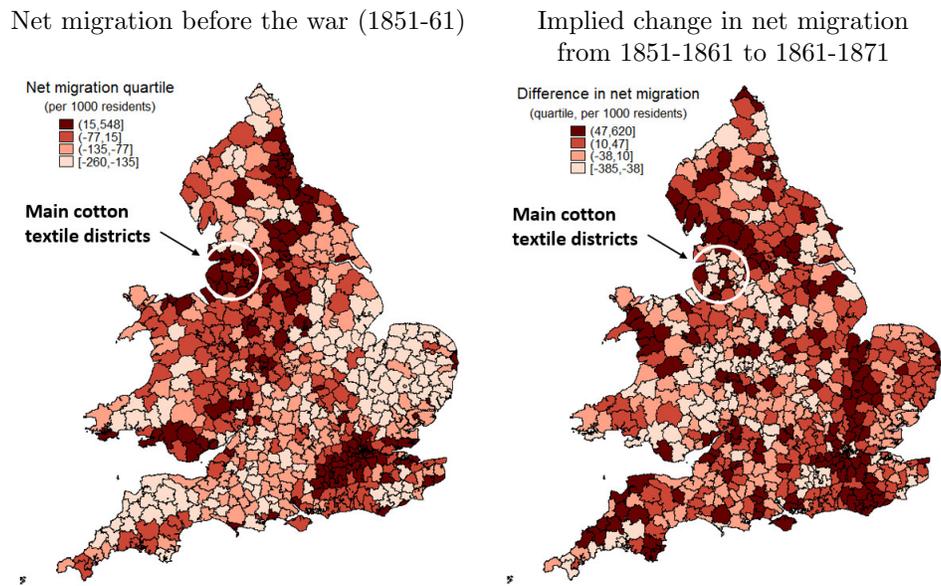
The magnitude of these migratory responses remains debated. To assess this, we look for changes in population growth patterns using data from the 1851-1881 censuses. These data are presented in Figure 4, which describes changes in district population across each decade, normalized by the 1851-1861 change (the decade preceding the downturn).<sup>12</sup> This figure reveals three important patterns. First, it shows a substantial slowdown in population growth in the cotton textile districts in the decade spanning the cotton shortage. This change appears to be driven by both

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<sup>11</sup>These are 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.

<sup>12</sup>The 1861 census was taken in April, the same month that the U.S. Civil War began. Historical evidence makes it clear that people in both the U.S. and abroad failed throughout most of 1861 to anticipate the severity of the conflict that had begun, and there is little evidence that the British economy was substantially affected until late 1861 or early 1862. As a result, this should be thought of as a clean pre-war population observation.

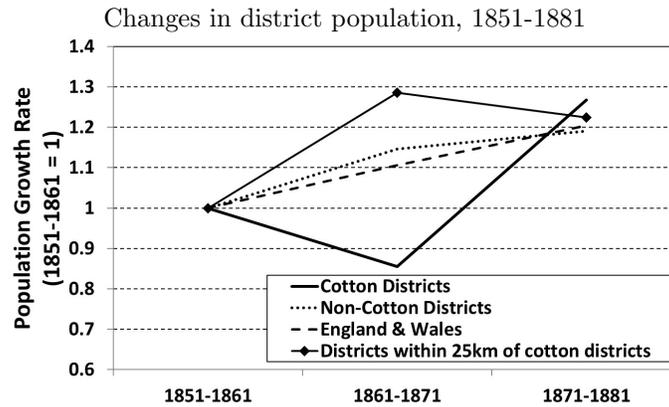
Figure 3: Maps of implied net migration



The left-hand panel maps implied net migration (per 1000 residents) for each district in the decade before the shock, 1851-1861. Darker colors indicate net in-migration. The right-hand panel plots the difference in net migration (per 1000 residents) between the 1851-61 and 1861-71 decade. Lighter colors indicate an increase in net out-migration from a district during the U.S. Civil War decade (1861-71). 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 count plus all births and less all deaths in the intervening years.

increased out-migration and decreased in-migration (a conclusion supported by additional evidence in Appendix A.2). 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.

Figure 4: Migration response to the cotton shortage



This graph describes the change in population for all cotton districts, all non-cotton districts, all districts in England & Wales, and all non-cotton districts within 25km of a cotton district using Census data. 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 are from the Census of Population.

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.<sup>13</sup>

There is also some evidence that migration away from the cotton textile districts during the U.S. Civil War was selective. Appendix A.2 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 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.<sup>14</sup> Thus, those leaving the cotton districts are likely to have 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 was in actuality.<sup>15</sup> Second, migration poses a number of empirical challenges, largely related to the mis-measurement of population size and composition, that necessitate a novel methodological approach. 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.

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<sup>13</sup>These patterns are consistent with the city-level experiences documented in Hanlon (2017).

<sup>14</sup>The crude mortality rate in cotton districts was over 26 deaths per thousand, compared to 25.7 in nearby districts and 23.2 across nationwide, a striking difference since young adults made up a greater share of the population in the cotton textile districts than in the country overall.

<sup>15</sup>This differs from the experience of blacks during the U.S.'s Great Migration, who moved towards, rather than away from, more urban, industrialized, and polluted locations (Black *et al.*, 2015).

### 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.<sup>16</sup> 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.”<sup>17</sup> Notably, these are some of the same channels modern studies cite as an explanation for the pro-cyclical mortality relationship they find.<sup>18</sup> However, 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*. Moreover, they provide a useful framework for thinking about the various mechanisms that may be at play in our setting.

#### 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

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<sup>16</sup>See Appendix A.3 for details.

<sup>17</sup>Quoted from the *Report of the Registrar General*, 1862.

<sup>18</sup>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.

estimating equation:

$$\ln(MR_{dt}) = \beta SHOCK_{dt} + X_{dt}\Gamma + \phi_d + \eta_t + \epsilon_{dt} \quad (1)$$

where  $MR_{dt}$  is the mortality rate in a given location (i.e., district)  $d$ ;  $\eta_t$  and  $\phi_d$  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.<sup>19</sup>

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.<sup>20</sup> Further, even if overall population flows are perfectly

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<sup>19</sup>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 specific group of individuals exposed to the downturn whose effects we wish to estimate.

<sup>20</sup>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 arti-

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 corresponds to the group of people whose deaths appear in the numerator. Accordingly, we modify our specification of interest to,

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

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 1851-55 or 1861-65).

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, 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

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cially high population denominator and fewer observed deaths 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 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.

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.<sup>21</sup> To operationalize this intuition, we modify our specification to separately estimate the impact of the shock on migrant-receiving districts,

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

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 re-

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<sup>21</sup>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.

lies on two main data sources that allow us to recover precisely this information. The first is individually-identified death records for the entire population of England and Wales over the years 1851-55 and 1861-65. The second is the full-count British census for the years 1851 and 1861. The census microdata are from the UK Data Archive and, in addition to preserving the structure of the household, they include individual names, location at the time of enumeration, age, and some additional information.<sup>22</sup> Our death records come from the Registrar General’s master death index, which we obtain from freeBMD.org. In addition to the decedent’s first and last name, these records include information on the district, year, and quarter of death.<sup>23</sup> Because census enumeration took place in April of 1861, just as the U.S. Civil War began, this means that with these two sources in hand, we can identify deaths in the cohort of individuals actually exposed to the cotton shortage.

Using these data, we link deaths from 1851-1855 back to their corresponding 1851 census record, and deaths from 1861-1865 back to their 1861 census entry.<sup>24</sup> Linking deaths from 1861-1865 back to the 1861 census allows us to identify where each individual resided at the onset of the cotton shortage, regardless of where the person ultimately died. Thus, we can compare the mortality rate of individuals initially resident in the cotton districts to the mortality rate of individuals initially resident in other locations. Applying the same linking procedure to data from the 1850s allows us to use a difference-in-difference identification strategy to recover a causal effect of the cotton shortage on mortality.

Our linking process differs in important ways from existing work using linked microdata (e.g., Ferrie (1996), Abramitzky *et al.* (2012), Abramitzky *et al.* (2014)). The most important difference is that our death records do not contain birth year or place of birth, two time-invariant pieces of information that are commonly used to generate

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<sup>22</sup>Individual-level microdata are not available from the next closest censuses, in 1871 or 1841.

<sup>23</sup>Cause of death is not available from the death index.

<sup>24</sup>In order to look at harvesting results, and for some robustness exercises, we also link deaths from 1856-60 back to the 1851 Census and deaths from 1866-70 back to the 1861 Census.

links. Thus, when linking, we rely more heavily on the uniqueness of the individual's first and last name. However, we also have two important advantages over existing work. 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 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.<sup>25</sup> These advantages are important for allowing us to generate a reasonably large linked sample even when relying on a limited set of information for linking.

Our linking methodology begins 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. The more informative match statistic, however, is among death records with unique first and last names: here we are able to link 17% of those death records back to a corresponding census record, a rate comparable to Ferrie (1996).

### 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 increases. This analysis, presented in Appendix B.1.2, shows that deaths are

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<sup>25</sup>Enumerators still visited every household to check and collect the forms and assisted households in the completion of the form when necessary.

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. This pattern, which cannot be a mechanical result of our linking procedure, suggests that our linking approach is performing well.

A second test aimed at assessing whether our linking procedure work as expected is to examine discrepancies in our linked dataset. Here, we examine how often the middle initials in the census and death records agree. Middle initials were not universally reported, and as a result we do not use them for linking. We can, however, use them as a diagnostic tool.<sup>26</sup> Using the sample for which middle names are available, we observe a false positive rate of 28% in the 1850s and 39.8% in the 1860s. This is comparable to the false match rates obtained in other linked papers, including Ferrie (1996) (see Bailey *et al.* (2017)). Note that in our difference-in-difference framework these false positives work against us by pushing our mortality coefficients toward zero. As a robustness check, we present results using sub-samples of our linked data where false positives are less common to further alleviate concerns about false positives driving our results.

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 data covering all deaths in England and Wales—data, taken from the Registrar General’s reports, in which deaths are reported in aggregate form by district of occurrence (henceforth, “aggregate data”). We present these results later, in Table 6. These results show that for those over 14, we are able to recover estimates that are both practically and statistically equivalent to those from

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<sup>26</sup>Note that transcription errors introduce error into this proxy, but those errors should be randomly distributed. Transcription errors may lead us to conclude that two records are not a match (e.g. a cursive “E” may be mistakenly transcribed as “C”). Conversely, transcription errors may lead us to conclude that a match is correct when it is actually false.

aggregate data. Thus, our linked sample is representative for the adult population.

The main dimension on which our linked deaths sample differs from aggregate mortality patterns is in the age distribution. In our linked sample, working-age adults are over-represented, while young children and, to a lesser extent, the elderly, are under-represented. The fact that young children are under-represented 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. The source of the under-representation of the elderly is less clear, but it may be related to an increase in the likelihood of spelling mistakes in the 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 more generally. We find that the change in the number of deaths over time is well reflected in our linked data: the ratio of 1861 to 1851 deaths in the linked data is 1.17, while in the aggregate data it is 1.14. We also analyze representativeness in terms of socioeconomic status using the occupation data in the census. We find that the shares 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 53.7% of deaths, versus 48.9% 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:

$$\Delta \ln \left( \frac{MORT_d}{POP_d} \right) = \alpha + \beta COTTON_d + \sum_{i \in \{25, 50, 75\}} \gamma_i NEAR_d^i + X_{dt} \Gamma + \epsilon_d \quad (4)$$

where  $\Delta$  indicates the change between 1851 and 1861 mortality rates. The variable  $POP_d$  is the population in a district  $d$  at the time of enumeration (i.e., 1851 or 1861) and  $MORT_d$  is the number of deaths among that population during the period of interest (i.e., from 1851-55 or 1861-65).<sup>27</sup> The variable  $COTTON_d$  is an indicator for whether district  $d$  is a cotton district.<sup>28</sup> 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  $\beta$  reflects the impact of the cotton shortage on the mortality rate of the treated population, regardless of where they died. However, one difficulty 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  $\lambda_{dt}$  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  $\widetilde{MORT}_{dt} = MORT_{dt} \lambda_{dt}$ . Substituting out  $MORT_{dt}$  in Eq. 4 and reorganizing, we have,

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<sup>27</sup>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. We outline the reasons to prefer our approach, alongside results from these alternative approaches, in our robustness discussion later in the paper.

<sup>28</sup>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.

$$\Delta \ln \left( \frac{\widetilde{MORT}_d}{POP_d} \right) = \alpha + \beta COTTON_d + \sum_{i \in \{25, 50, 75\}} \gamma_i NEAR_d^i + X_d \Gamma - \ln \left( \frac{\lambda_{d,1861}}{\lambda_{d,1851}} \right) + \epsilon_d. \quad (5)$$

This equation tells us that we will be able to recover  $\beta$  under the assumption that any change in our linking rate,  $\lambda_{d,t}$ , is uncorrelated with the shock. 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 1861-65, say, because they moved abroad. However, if individuals who emigrated are less likely to be linked (e.g., 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. Note that our approach can handle variation in the linking rate across districts that is constant over time, which will be absorbed by the district fixed effects, as well as changes in the linking rate over time that are constant across districts, which will be absorbed by the time effects.

There are a few other points worth mentioning about our empirical specification. First, we standardize our dependent variable. This is necessary because our linked deaths capture only a subset of true deaths. Dividing this subset by the initial census population gives a distribution of linked “mortality rates” with a mean that is far below the true mortality rate and, because of the noise associated with fewer linked deaths, a higher variance relative to the mean. Standardizing the dependent variable to have a common mean (0) and variance (1) allows us to generate estimated effects using linked data that can be directly compared to those from aggregate data.

Second, in the main text we report robust standard errors. Although one may

worry that spatial correlation is a concern, in practice, we find robust standard errors to be the most conservative in our context. Nevertheless, 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.

Third, when looking at all-age mortality results, we control for the share of different age groups in each district. 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.

Fourth, we follow the conventions of existing literature and weight all regressions by population. In practice, we use district population in 1851, 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 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 Main results

Table 1 presents our main findings. The all-age results in Column 1 indicate that total mortality rates increased by about 0.8 s.d. in the cotton districts in the 1861-65 period relative to the 1851-55 period.<sup>29</sup> Columns 2-4 present results for the following age groups: 0-14, 15-54, and 55 and over.<sup>30</sup> We consider these three broad age

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<sup>29</sup>Throughout the linked analysis, when we say that mortality increased in cotton (or nearby) districts, what we technically mean is that mortality increased among the population initially resident in cotton (or nearby) districts.

<sup>30</sup>While 55 is a somewhat young relative to, say, the results of Stevens *et al.* (2015) which show deaths concentrated in the 80s, life expectancy was substantially lower in our study period than in

categories so that each category includes a sufficient number of linked deaths to keep the standard errors from growing too large. These results indicate that the overall effects we observe are being driven primarily by deaths among older adults—individuals who make up the lion’s share of a population’s total mortality risk, and who would have been more vulnerable to deprivation and infectious disease (and also less mobile) than prime working-age adults.

For all age groups, we also observe an increase in mortality rates among those initially resident in nearby districts during the shock period, an effect which disappears outside of about 50 km from cotton districts.<sup>31</sup> There are several factors that may be behind this result. One possible channel is through congestion effects caused by the migration of cotton workers into nearby areas, where they may have competed for jobs, housing, and public services. Similarly, cotton workers, who were coming from some of the least healthy districts in the country, may have also brought infectious disease with them. Alternatively, it may be that the economic effects of the cotton shortage also negatively impacted economic activity in nearby non-cotton areas. Later we will provide evidence on some of these potential channels.<sup>32</sup>

Overall, these results provide evidence that, during the cotton shortage, the mortality rate increased among those initially resident in the cotton textile districts, as well as among those initially resident in nearby non-cotton districts. When looking across all age groups, these results are statistically significant at just outside the 95% confidence level with robust standard errors. When focusing only on the most heavily affected group, older adults, our estimates are statistically significant at the 99% level. These results are somewhat conservative: the permutation test p-values, reported at

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the modern U.S. We also start our working age category at 15 since most people in the 15-20 age group would have been working during this period.

<sup>31</sup>Note that, because deaths are assigned to the district of census enumeration rather than the place of death, these nearby effects cannot reflect the deaths of residents of cotton districts who migrated to the nearby districts and then died.

<sup>32</sup>Note that, because deaths are assigned to the district of census enumeration rather than the place of death, these nearby effects cannot reflect the deaths of residents of cotton districts who migrated to the nearby districts and then died.

Table 1: Baseline effects of the shortage using linked data

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>				
	<b>All ages</b>	<b>Under 15</b>	<b>Age 15-54</b>	<b>Over 54</b>
	(1)	(2)	(3)	(4)
Cotton district indicator	0.839* (0.436)	0.388 (0.365)	0.637 (0.390)	0.981*** (0.367)
Nearby (0-25km)	0.429*** (0.124)	0.259* (0.145)	0.364*** (0.091)	0.456*** (0.128)
Nearby (25-50km)	0.403** (0.163)	0.123 (0.144)	0.330*** (0.124)	0.361*** (0.131)
Nearby (50-75km)	0.017 (0.148)	0.005 (0.156)	0.096 (0.131)	0.016 (0.157)
Log initial pop.	-0.087 (0.089)	-0.100* (0.051)	-0.132*** (0.050)	-0.117** (0.052)
Age share controls	Yes			
Observations	537	534	535	533
R-squared	0.089	0.022	0.055	0.107
Linked deaths	152,787	55,723	69,978	27,086
<b>Permutation test p-values for effect on cotton districts</b>				
p-values	0.0129	0.0797	0.0519	0.0093

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. In the all-age regressions we include age group controls for the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census. Note that the number of observations varies because some small districts with no linked deaths in one period are dropped.

the bottom of the table, suggests that we obtain uniformly stronger results when we account for spatial correlation.<sup>33</sup>

In terms of magnitude, these results suggest that the cotton shortage led to 74,000 additional deaths among residents of cotton districts, or about one-quarter of the deaths in those districts over the shortage period. Among residents of nearby districts within 25 km, our results indicate that the cotton shortage led to around 28,000 additional deaths, or about 14% of the deaths in those districts. These are substantial numbers, but perhaps not unrealistic given that the cotton shock drove half a million people to seek poor relief (however meager), and many others to leave the cotton

<sup>33</sup>See Appendix C.1 for a discussion of the permutation test and our approach to spatial correlation in standard errors.

districts altogether.

Although our linked data do not contain information on cause of death, the results by age are broadly consistent with many of the mechanisms identified by 19th Century public health officials (See Appendix A.3). For instance, that the adverse mortality effects were strongest amongst 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.<sup>34</sup> Further, given the unhealthy environment in the main cotton cities, older adults, who were less likely to out-migrate than their younger counterparts, may have had limited scope to mitigate the adverse effects of lost income. We also see smaller adverse effects for children than for the elderly. One reason for this could be the under-representation of infants—a group with high baseline mortality rates. However, a factor suggested in contemporary reports is 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 substitution effect may have partially offset the adverse effects of material deprivation.

Our linked data can also shed light on migration patterns. Comparing the 1861-65 period to 1851-55, we observe a 4.7 percentage point reduction in the share of individuals enumerated in cotton textile districts that also died in the cotton districts. This indicates that cotton textile residents were more likely to die elsewhere during the cotton shortage. We also see evidence of less migration into cotton areas during the shortage. Of those residing within 50 km of a cotton district in 1851, 9.7% died in a cotton district. However, during the 1861-65 period this figure falls to 8.4%. Overall, the share of linked deaths accounted for by individuals initially living in

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<sup>34</sup>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.

the cotton textile districts rose by about 10% (from 7.08% in the 1851-55 period to 7.75% in the 1861-65 period). The share of linked deaths that actually occurred in the cotton districts also rose, but only by about 0.8% (from 7.77% in the 1851-55 period to 7.78% in the 1861-65 period). Thus, cotton textile residents made up a larger share of deaths in 1861-65 than in 1851-55, while at the same time, the share of these deaths occurring outside of the cotton districts increased.

Appendix D.1 presents results from several robustness exercises. Appendix Table 13, for instance, considers finer age categories. As in our main results, we see positive effects for each age category, but the effects are most pronounced for older adults. For those aged 45-54, 55-64, or 65 and older, we consistently observe a statistically significant 0.6 s.d. increase in the mortality rate. Beyond this, results are also similar regardless of whether or not we weight our regressions by district population, use a continuous treatment variable (district's share of employment in cotton), drop outlier locations such as Manchester, Liverpool, or Leeds, or omit the foreign-born from our linked deaths sample (see Appendix Table 14). We also consider more restrictive linking approaches and samples with fewer false positive links (Appendix Table 15), and find similar results. Finally, in Appendix Table 17 we present results using an entirely different empirical approach. There we run a series of logit regressions at the individual level, and again find effects that are consistent with our main results.<sup>35</sup>

As another check, we generate placebo results where we treat 1856-60 as a placebo shock period and then estimate mortality effects in the cotton districts as compared to the 1851-55 period. These results, reported in Table 2, show no evidence of increased mortality in the cotton districts during the placebo period. Thus, our results do not

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<sup>35</sup>There are three reasons why we prefer to run our analysis at the district rather than the individual level. First, and most importantly, aggregating to the district level makes it easier to compare the results obtained using the linked data to results obtained using the traditional aggregate data common in this literature. Second, while running individual-level regressions offers the advantage of being able to include controls for individual covariates, this methodology is difficult to implement in a dataset as large as ours, which draws on every individual in two full British censuses. Third, it is likely that error terms will be correlated across individuals in the same location. Aggregating the linked microdata eliminates concerns related to this type of correlated error.

appear to be due to differences in underlying mortality trends in the cotton textile districts.<sup>36</sup>

Table 2: Placebo 56-60 against 51-55

<b>DV: Std. change in log mortality rate from 1851-55 to 1856-60</b>				
	<b>All ages</b>	<b>Under 15</b>	<b>Age 15-54</b>	<b>Over 54</b>
	(1)	(2)	(3)	(4)
Cotton district indicator	0.025 (0.191)	-0.184 (0.149)	-0.152 (0.123)	-0.099 (0.137)
Nearby (0-25km)	0.081 (0.123)	0.011 (0.139)	-0.303*** (0.100)	0.015 (0.158)
Nearby (25-50km)	0.066 (0.120)	0.028 (0.122)	0.086 (0.090)	-0.012 (0.102)
Nearby (50-75km)	-0.190 (0.122)	-0.047 (0.170)	0.107 (0.117)	-0.363** (0.170)
Log initial pop.	-0.028 (0.058)	-0.125*** (0.040)	-0.009 (0.034)	-0.044 (0.036)
Age share controls	Yes			
Observations	536	536	535	530
R-squared	0.063	0.031	0.019	0.016

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. In the all-age regressions we include age group controls for the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census. Note that the number of observations varies because some small districts with no linked deaths in one period are dropped.

### 3.6 Mechanisms and additional evidence

Having established that the cotton shortage increased mortality rates both in cotton-district and nearby-district cohorts, we examine the mechanisms underpinning the results presented thus far. We begin by assessing the importance of personal job loss (as potentially distinct from broader recession conditions) in the mortality increase. Specifically, we use occupation information from the census to identify cotton workers

<sup>36</sup>We have also generated results using four five-year periods, 51-55, 56-60, 61-65 and 66-70. The results, in Appendix Table 16, also show elevated mortality in cotton districts in 1861-65. However, we prefer not to use this as our main specification because when using data that links deaths to the preceding census, patterns in the second five-year period after a census are not strictly comparable to those in the first five-year period after the census.

Table 3: Share of deaths accounted for by cotton workers and households

	Cotton workers	Households with at least one cotton worker	Non-cotton workers living in a household with a cotton worker
1851	0.851%	2.449%	1.656%
1861	1.498%	4.123%	2.718%
Percent increase	76.03%	68.35%	64.13%

and those in their household. The share of deaths accounted for by cotton workers or those in their household are described in Table 3.<sup>37</sup> Cotton workers accounted for 0.85% of all deaths in our linked sample in 1851-55, but this increased to 1.5% of deaths in 1861-65. This indicates that the cotton shortage had a substantial direct effect on mortality among the workers most exposed to the shock. The second column shows that deaths among all members of cotton households increased from 2.45% of all deaths to 4.12%, a 68% increase. The last column shows that the share of deaths accounted for by those not listed as cotton workers, but who lived in a household with at least one cotton worker, increased from 1.66% of all deaths to 2.72%. Together, these results suggest that the cotton shortage period was characterized by a substantial increase in the share of deaths accounted for by cotton households. This effect appears both for cotton workers themselves, as well as for other members of cotton households.

To get a sense of how important the cotton-worker and cotton-household deaths were to the overall increase in mortality observed earlier, we run regressions dropping these deaths from our linked deaths sample (see Appendix Table 18). These results show that the coefficient on the estimated effect of the cotton shock drops from 0.839 to 0.746 when cotton workers are dropped from the sample. If we exclude all cotton households the coefficient drops to 0.361 and is no longer statistically significant.

<sup>37</sup>We cannot analyze cotton worker deaths using the difference-in-difference framework from our main analysis since cotton workers were heavily concentrated in the cotton districts.

The direct impact of the shock on mortality through cotton workers and their families appears to be important. These findings support both the notion that personal experiences of job loss are important to health (see, e.g., Sullivan & von Wachter (2009)), as well as the idea that local employment shocks can have a broader social impact on individuals whose own income or labor force status is not personally affected.

We also 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, 1866-70, as compared to those in the 1856-60 period, which is the most comparable available control period. These results, presented in Table 4, indicate that in the 1866-70 period, mortality remained elevated both among the populations initially resident in the cotton textile districts, as well as among those initially resident nearby. 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 panel

Table 4: Results on harvesting vs. scarring

	<b>DV: Std. change in log mortality rate from 1856-60 to 1866-70</b>			
	<b>All ages</b>	<b>Under 15</b>	<b>Age 15-54</b>	<b>Over 54</b>
	(1)	(2)	(3)	(4)
Cotton district indicator	0.689 (0.435)	0.717* (0.416)	0.865** (0.394)	0.843** (0.380)
Nearby (0-25km)	0.316** (0.140)	0.387*** (0.148)	0.606*** (0.136)	0.429*** (0.137)
Nearby (25-50km)	0.146 (0.120)	0.282** (0.115)	0.180* (0.098)	0.048 (0.110)
Nearby (50-75km)	0.262** (0.112)	0.123 (0.202)	-0.122 (0.154)	0.256 (0.158)
Log initial pop.	-0.086 (0.080)	-0.073 (0.055)	-0.054 (0.055)	-0.048 (0.057)
Age share controls	Yes			
Observations	536	534	535	530
R-squared	0.083	0.052	0.095	0.082
Linked deaths	269,863	94,747	127,620	47,496

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. In the all-age regressions we include age group controls for the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census. Note that the number of observations varies because some small districts with no linked deaths in one period are dropped.

data.

Our data also sheds light on the mechanisms underpinning the increase in nearby-district mortality. One possibility is that labor market competition from migrating cotton workers helps explain the rise in mortality among those initially resident in nearby districts. We study this by looking at mortality among wool textile workers, who were those most exposed to competition from unemployed cotton textile workers, and their families. The wool textile industry (including worsted) was located near the cotton textile districts and required very similar skills.<sup>38</sup> Unlike cotton, the wool industry was not negatively impacted by the U.S. Civil War. As a result, contempo-

<sup>38</sup>The fact that wool textile production was concentrated in the West Riding of Yorkshire, close to the cotton districts, means that we cannot separately estimate mortality increases among wool workers based on how close they were in distance to the cotton textile districts. This coincidence of geographic proximity and occupational similarity also makes it impossible to separate these channels driving our migration patterns.

rary reports indicate that many cotton workers migrated to the wool districts seeking employment during the shock (Henderson, 1934, p. 10, 115). In Table 5, we drop wool workers (column 2) and all members of wool households (column 3). Doing so does not meaningfully alter the results, suggesting that direct competition in the labor market is unlikely to explain the elevated mortality of those residing nearby to cotton districts. Instead, migrating cotton workers may have propagated the shock to their new neighbors through other means, for instance, by contributing to residential overcrowding, taxing public infrastructure, or spreading infectious disease. These nearby results suggest an important role for migration as a transmission mechanism for local economic shocks, particularly in settings where migration flows may be large and relatively concentrated.

Finally, in Appendix Table 19, we examine the distributional consequences of the shock on mortality through channels related more to the structure of the broader economy than to spatial patterns of migration: namely, input-output connections. To do this, in addition to dropping cotton's direct competitors in wool, we also eliminate families where a member of the household works in the coal industry (the largest domestic input supplier to cotton textiles), or in the clothing trades (the main buyer of cotton textile products). Dropping these households does not affect our results. Thus, this exercise provides no evidence that the impact in nearby districts operated through input-output connections, nor does it suggest contamination of the broader control group of faraway districts.

Table 5: Estimated impact of wool households on our main results

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>			
	Full linked sample	Dropping wool workers	Dropping all members of wool households
	(1)	(2)	(3)
Cotton district indicator	0.839* (0.436)	0.839* (0.438)	0.872** (0.438)
Nearby (0-25km)	0.429*** (0.124)	0.436*** (0.128)	0.423*** (0.152)
Nearby (25-50km)	0.403** (0.163)	0.397** (0.164)	0.365** (0.152)
Nearby (50-75km)	0.017 (0.148)	0.018 (0.149)	0.017 (0.151)
Log initial pop.	-0.087 (0.089)	-0.087 (0.090)	-0.103 (0.088)
Age share controls	Yes	Yes	Yes
Observations	537	537	537
R-squared	0.089	0.088	0.090

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the initial district of residence (i.e., census enumeration district). Regressions are weighted by district population in 1851. Age group controls include the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census.

## 4 Does migration matter?

Finally, we ask how important it is that we were able to adjust for migration in this setting, given the “no substantial migration” assumption that underlies both Equation 1 and the large existing literature relying on aggregate panel data approaches. That is, we examine whether intentionally failing to account for recession-induced migratory responses would fundamentally alter our conclusions as to the health impact of this historical downturn. In so doing, 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 1861-65 to deaths in 1851-55, but instead uses data taken

directly from aggregate district death counts transcribed from the Registrar General's reports.<sup>39</sup> 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 the results obtained from these aggregate reports to the results obtained from analogous linked data (i.e., 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, by definition, assigned to the location of death, we should expect these results to be similar. Table 6 reports this comparison, and we see that for adults aged 15-54 and those 55 and over, the effects estimated using the place-of-death linked data accord with those obtained from the traditional aggregate data. This tells us that, for these populations, the linked data sample can recover the results obtained from aggregate data, i.e., the linked deaths appear to be representative of aggregate deaths. For the population under age 15, the aggregate and linked data give qualitatively similar results, but the magnitude of the effects is different. This is almost certainly due to the under-representation of infant deaths in the linked sample. Thus, when comparing the linked results to the aggregate results, our main focus should be on the older populations, where our linked sample is representative.

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<sup>39</sup>Further details on the data are available in Appendix B. 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. As in the linked analysis, population data come from the census and are available every ten years starting in 1851.

Table 6: Are linked district-of-death estimates comparable to the estimates obtained from aggregate data?

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>						
	<b>Ages under 15</b>		<b>Ages 15 to 54</b>		<b>Ages 55 and up</b>	
	Agg. data (1)	Linked data (2)	Agg. data (3)	Linked data (4)	Agg. data (5)	Linked data (6)
Cotton dist. ind.	-0.613*** (0.132)	-0.271* (0.154)	-0.226 (0.178)	-0.166 (0.101)	0.505*** (0.148)	0.499*** (0.129)
Nearby dist.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	531	531	534	534	529	529
R-squared	0.089	0.028	0.108	0.018	0.159	0.054
<b>Testing difference between linked and aggregate cotton dist. coef.</b>						
Chi-sq stat.	2.73		0.14		0.00	
P-value	0.099		0.708		0.967	

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Linked results come from data in which deaths are assigned to the district of death. Regressions are weighted by district population in 1851. All regressions include controls for nearby districts 0-25 km, 25-50 km and 50-75 km away from the cotton districts. The sample size changes slightly across specifications if there are small districts where we do not have links when cutting by age group. At the bottom of the table we present tests comparing the cotton district coefficients estimated using linked data assigning deaths to location of death to the coefficients estimated on aggregate data. This test is implemented by running seemingly unrelated regressions with robust standard errors.

Having illustrated the ability of the linked data to mimic aggregate data, we next compare the results obtained from aggregate data to our preferred linked approach, which accounts for migration bias by assigning deaths to each person’s district of residence at the time of enumeration. This comparison provides our most direct test of the ability of results obtained from traditional aggregate data to correctly capture the impact of the cotton shortage on the relevant population at risk of exposure. While we include all three age groups in the table, attention should be focused on the groups over age 15, where we know from Table 6 that our linked sample is representative.

For adults aged 15-54, the evidence in Table 7 leads us to reject the hypothesis that the aggregate data deliver results that are the same as those obtained from our preferred linked approach. That we cannot reject equality when our linked deaths ignore migration (Table 6 columns 3 and 4) but can reject equality when we account

for migration (Table 7 columns 3 and 4) is particularly telling. This means that the difference is due to migration rather than to differences between the linked sample and the aggregate data. For the elderly, the analysis based on aggregate data comes closer to the result obtained with our linked data, and the estimates cannot be statistically distinguished. This indicates that migration bias is less likely to be a problem for older age groups, a pattern that is reasonable given that we would expect the elderly to be less mobile in response to changes in local economic conditions.

Table 7: Do aggregate-data estimates recover the true impact of the recession?

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>						
	<b>Ages under 15</b>		<b>Ages 15 to 54</b>		<b>Ages 55 and up</b>	
	Agg. data	Linked data	Agg. data	Linked data	Agg. data	Linked data
	(1)	(2)	(3)	(4)	(5)	(6)
Cotton district ind.	-0.614*** (0.132)	0.388 (0.365)	-0.228 (0.178)	0.637 (0.390)	0.508*** (0.145)	0.981*** (0.367)
Nearby dist.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	534	534	535	535	533	533
R-squared	0.088	0.022	0.108	0.055	0.163	0.107
<b>Testing difference between linked and aggregate cotton dist. coef.</b>						
Chi-sq stat.	7.32		4.37		1.25	
P-value	0.007		0.037		0.264	

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Linked results come from data in which deaths are assigned to the individual's district at the beginning of the period (i.e., census enumeration district). Regressions are weighted by district population in 1851. All regressions include controls for nearby districts 0-25 km, 25-50 km and 50-75 km away from the cotton districts. The sample size changes slightly across specifications if there are small districts where we do not have links when cutting by age group. At the bottom of the table we present tests comparing the cotton district coefficients estimated in this table to those obtained when deaths are assigned to each person's initial place of residence as in Table 1. This test is implemented by running seemingly unrelated regressions with robust standard errors. Note that each pair of comparisons uses only districts for which death rates from the linked data are available, which causes the aggregate results to differ slightly from those shown in Table 6.

As a final check, we generate results using annual panel data from the Registrar General's health reports. This panel-data approach is closer to that used in most of the literature, though it is less comparable to the analysis we have seen thus far. These results are presented in Appendix D.2. There we consider a range of specifications and tend to find strong evidence that mortality fell in cotton districts during the

downturn. While these pro-cyclical results are consistent with most of the previous studies using aggregate panel data (as well as with the results presented above for aggregate and place-of-death linked data, where we know that migration is the source of the apparent bias), they are contradicted by our preferred results that do account for migration. Thus, it seems unlikely that an analysis based on aggregate panel data, and following the standard approach used in this literature, will correctly and reliably recover the impact of this downturn on mortality.

## 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 unobserved migration.

Specifically, our approach involves tracking individuals over time, so that when people migrate away from areas experiencing severe recessions and then die elsewhere, their deaths can be tied back to their recession experience. To do this, we construct

a large-scale individual-level longitudinal dataset that links the universe of death records for England and Wales back to full-count census microdata. Because the British census of 1861 took place on the very eve of the U.S. Civil War, we are able to accurately identify the cohort of individuals who were resident in cotton textile districts at that time, and thus, exposed to the recession. By linking these individuals to comprehensive and individually-identified death records for the cotton shortage period, we are then able to calculate the mortality rate amongst this exposed group, regardless of whether and where they may have migrated. This is impossible in more aggregated data, and accounts for the ways in which recession-induced migration can change the size and composition of a locality's population. Additionally, our approach allows us to correct for migration spillovers that if left unaddressed would both undermine inference and obscure noteworthy health effects on indirectly treated cohorts.

Our results are twofold. First, we find robust evidence that the cotton shortage increased mortality among those initially resident in cotton districts, particularly the elderly. These adverse effects appear to be driven by cotton workers and their family members, and persisted even after the resumption of normal economic activity, thus providing no evidence of a net harvesting response. We also see an increase in mortality among residents of migrant-receiving regions. This result does not appear to be driven by labor market competition, nor was it transmitted through input-output channels. Instead, likely mechanisms for these spillovers include congestion effects imposed by cotton migrants on their new neighbors, such as competition for housing and the spread of contagious disease.

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 future researchers may wish to seriously consider this possibility when examining settings in which migration may act as a key margin of adjustment.

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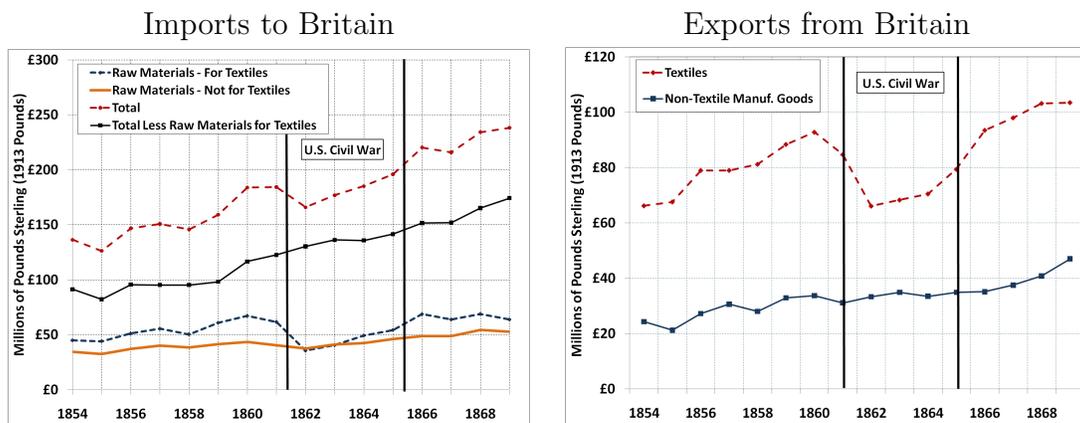
# A Online Appendix: Empirical setting

## A.1 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 5 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 5: British imports and exports, 1854-1869

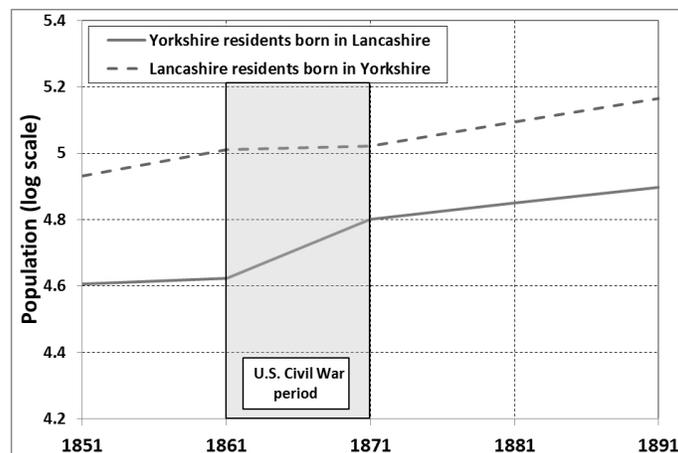


Data from Mitchell (1988).

## A.2 Additional results on migration during the cotton shock

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 only available at the county level, so in Figure 6, which we reproduce from Hanlon (2017), compares 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.

Figure 6: Evidence of migration for Yorkshire and Lancashire from birthplace data

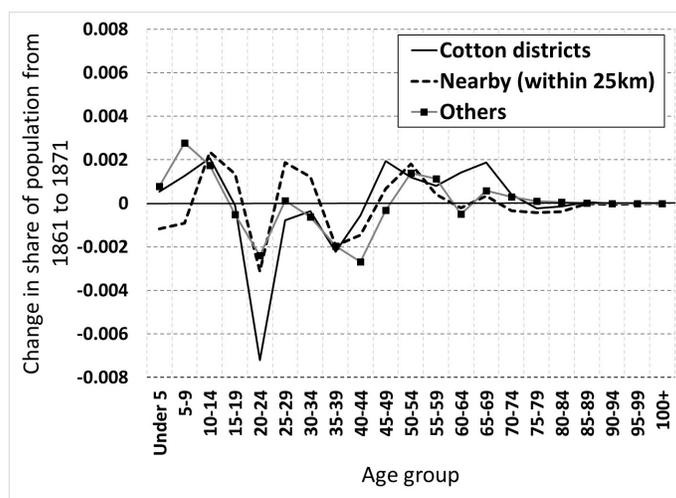


This graph, which is reproduced from Hanlon (2017), presents data on the birthplace of county residents from the Census of Population.

Next, we consider some results that help us think about how migration patterns varied across age groups. Figure 7 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.

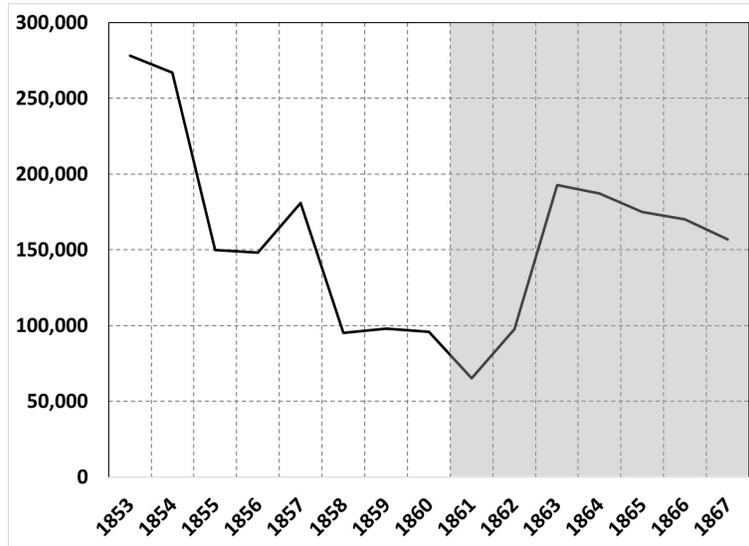
Figure 7: Share of population in each age group in cotton districts



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.

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. Figure 8 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

Figure 8: Emigration from Britain, 1852-1867



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

emigrants from cotton districts need not be English.

### A.3 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.”<sup>40</sup>

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

<sup>40</sup>Quoted from the *Report of the Registrar General*, 1862.

mothers than by the hired nursery keepers.” This channel was likely to be particularly 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, 2013), 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 Linked data

#### B.1.1 Linked data overview

To construct our linked dataset we draw on comprehensive and publicly available death records and census records. Our death records come from The Free Births, Marriages, and Deaths project (FreeBMD). FreeBMD's objective is to transcribe and provide free internet access to the General Register Office's Civilian Registration Index. The General Register Office was set up following the Births and Deaths Registration Act of 1836, which instituted mandatory reporting of births, deaths, and marriages. Of the three indices, the death index is thought to be the most accurate and comprehensive, as registration was required before the body could be legally disposed of Woods (2000).

FreeBMD has currently transcribed the entire death index for the years spanning 1837 to 1978. For our linking exercise, we begin by extracting all death records occurring in the years 1851-55 and 1861-65.<sup>41</sup> During this period, the index reports the following information consistently: first and last name, the registration district where the death occurred, and the year and quarter that the death occurred. As to the accuracy of the transcription, entries in the index were independently transcribed by two or more volunteers. Further, transcribers were encouraged to identify whether an entry was illegible or whether they could not clearly identify the letter (e.g., o vs a). Because names are crucial for our linking, we drop any illegible entries. Roughly 0.5% of the index is dropped because either the first or last name could not be accurately transcribed. After dropping illegible entries, we are left with 1.97 million deaths

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<sup>41</sup>Including the 1851-55 period allows us to analyze the impact of the cotton shock on mortality within a difference-in-differences framework.

during the 1851 to 1855 period and 2.13 million deaths between 1861 and 1865.<sup>42</sup>

We attempt to link each of these deaths to a unique census record. This is possible through the efforts of the Integrated Census Microdata Project (I-CeM), which partnered with findmypast.org to transcribe and standardize the 1851, 1861, 1881, 1891, 1901, and 1911 British censuses. Our application in particular requires access to the corresponding names of each entry in the entire 1851 and 1861 censuses, which we obtain from the UK Data Archive.

Table 8 provides summary statistics for the linked data.

Table 8: Summary statistics for linked data

	Mean	Standard deviation	Min	Max	N
Ln(MR) in 1851	-5.651	0.545	-9.027	-4.656	538
Ln(MR) in 1861	-5.597	0.531	-9.452	-4.734	538
$\Delta$ Ln(MR)	0.049	0.352	-1.97	2.251	537
Cotton dist. ind.	0.045	0.206	0	1	539
Nearby (0-25km) ind.	0.048	0.214	0	1	539
Nearby (25-50km) ind.	0.059	0.237	0	1	539
Nearby (50-75km) ind.	0.067	0.25	0	1	539
Dist. pop. in 1851	33,285	35,048	2,493	284,126	538
Under 5 pop. shr 1851	0.131	0.009	0.098	0.163	538
Age 5-15 pop. shr. 1851	0.326	0.016	0.233	0.373	538
Over 50 pop. shr. 1851	0.154	0.021	0.093	0.213	538
Under 5 pop. shr 1861	0.133	0.01	0.1	0.166	538
Age 5-15 pop. shr. 1861	0.323	0.014	0.249	0.372	538
Over 50 pop. shr. 1861	0.161	0.024	0.098	0.236	538

<sup>42</sup>Because we are looking to link back to the preceding census, we omit any deaths in the first quarter of 1851 and 1861, as those deaths occurred prior to census enumeration.

### **B.1.2 A check: linking against distance**

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.

Figure 9 presents histograms showing the share of linked deaths by distance bins using data from both the 1851-55 and 1861-65 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.<sup>43</sup> Similar patterns are observed when we split the data by period. Overall, these patterns suggest that our linking procedure is producing reasonable matches.

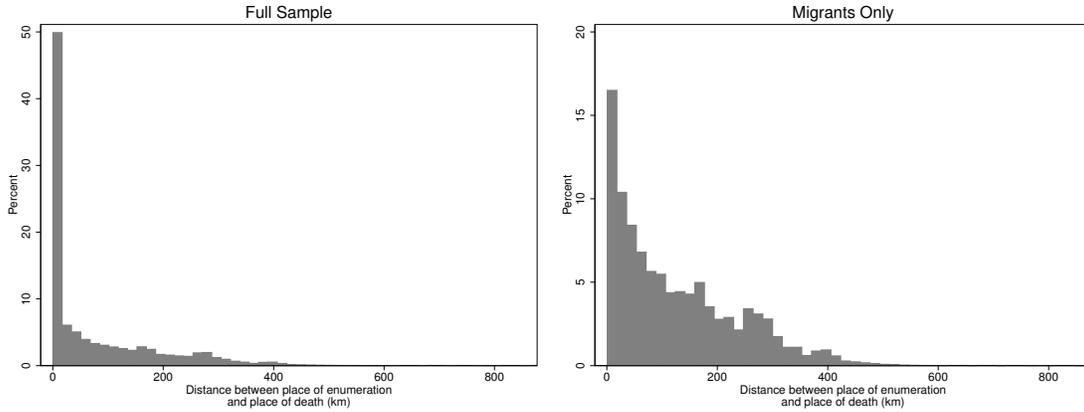
## **B.2 Aggregate data**

To assess the ability of traditional aggregate data to recover accurate estimates of the health consequences of the cotton shortage, we construct a new panel of annual district-level mortality spanning 1851-1871. These data, which we digitized from original reports from the Registrar General's office, cover all of England and Wales at the district level. The registration district-level tabulations are the finest geographic level covering the demography of all of England and Wales annually in this period. Previously available data from the Registrar General's reports, digitized by Woods

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<sup>43</sup>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.

Figure 9: Share of links by distance between census and death districts



(1997), are reported only at the decade level, and so is insufficiently detailed for our analysis. For an in-depth discussion of the Registrar General’s data, see Woods (2000). We also collect information from the Registrar General’s reports on district population from the census years 1851, 1861 and 1871. Summary statistics for the aggregate data appear in Table 9.

Table 9: Summary statistics for aggregate data

<b>Panel A: Full sample of districts</b>				
	(1)	(2)	(3)	(4)
	Mean	Standard deviation	Min	Max
Average annual deaths (1851-1870)	809.23	1108.84	34	12,825
Cotton employment share (1851 census)	0.017	0.07	0	0.51
Population (1851 census)	33,260.87	35,019.68	2493	284,126
<b>Panel B: Cotton districts only</b>				
Average annual deaths prior to shock	1943.89	1546.46	207	7957
Average annual deaths during shock	2133.1	1684.46	199	8900

When using annual data, it is necessary to construct population estimates for non-

census years. This is done using the Das Gupta interpolation method, which is the same method used by the U.S. Census today. Starting with population in a census year, this method adds births and subtracts deaths in each year until the next census year is reached. The difference between this population estimate and the population observed in the current census—called the error of closure—is then allocated proportionally across the intervening years (see *Methodology for the Intercensal Population and Housing Unit Estimates: 2000 to 2010*, U.S. Census Bureau, 2012, as well as the discussion in Arthi *et al.* (2017), for further detail).

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

This subsection analyzes how representative our linked sample of deaths is of aggregate deaths. One dimension along which we can compare the linked sample to aggregate deaths is the in the number of deaths over the two time periods. In the aggregate data, the ratio of deaths in 1861 to deaths in 1851 is 1.14. In the linked sample the ratio is 1.17. Thus, the linked data appears to capture reasonably well the increase in the overall number of deaths across these two periods.

Another 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 10 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. Deaths at age 5-54 tend to be over-represented in the linked sample, while deaths among those over age 65 are also under-represented, though not as badly as for infants. 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

Table 10: Gender shares of linked and aggregate deaths by time period

	Linked deaths		Aggregate deaths	
	1851	1861	1851	1861
Women	0.542	0.536	0.492	0.488
Men	0.458	0.464	0.508	0.512

back to the corresponding 1861 census. The explanation for why deaths in ages 65-85 are somewhat under-represented in the linked sample is not clear. The most likely explanation for this is that there are more spelling mistakes or legibility issues in the names among this age group, which could be a result of old age or the fact that older age groups were more likely to be illiterate. It is worth noting, however, that the linked distributions are nearly identical to each other (see the bottom panel of Figure 10), 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.

Table 10 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 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).

Next we examine the representativeness of our sample by socioeconomic status

Figure 10: Histogram of deaths by age from linked and aggregate data

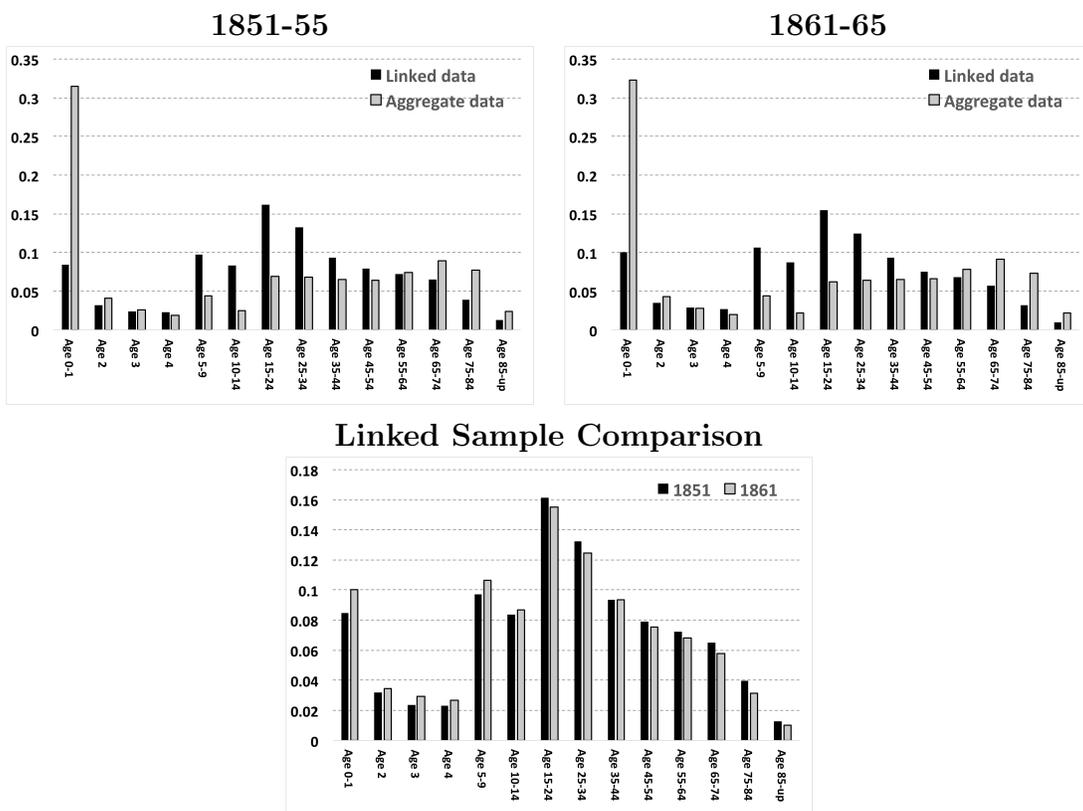


Table 11: Shares of white and blue-collar deaths in linked and aggregate data

	Linked deaths in 1851	Linked deaths in 1861	Aggregate deaths in 1851
White-collar	0.076	0.075	0.076
Blue-collar	0.924	0.925	0.924

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

The results, in Table 11, 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 that respects 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 1861-65 period to the 1851-55 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. However, the permutation p-values reported in the main text suggest that this is not the case. 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, which 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**

#### **D.1.1 Robustness exercises**

Table 12 shows how our main all-age results change as we add control variables. Adding controls for the shares of different age groups in each district has an important effect on our results, while other controls do not play a major role. This makes sense

given that these populations faced very different mortality risks. Note that the results in Column 4 correspond to the specification presented in our main linked results table in the main text.

Table 12: All-age results using linked data and adding controls

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>				
	(1)	(2)	(3)	(4)
Cotton district indicator	0.611 (0.439)	0.659 (0.440)	0.832* (0.441)	0.839* (0.436)
Nearby (0-25km)		0.289*** (0.107)	0.415*** (0.124)	0.429*** (0.124)
Nearby (25-50km)		0.219 (0.154)	0.371** (0.156)	0.403** (0.163)
Nearby (50-75km)		0.029 (0.131)	0.004 (0.138)	0.017 (0.148)
Log initial pop.				-0.087 (0.089)
Age Controls			Yes	Yes
Observations	537	537	537	537
R-squared	0.037	0.047	0.085	0.089

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. Age group controls include the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census.

### Finer age categories

Table 13 looks across more detailed age categories. In these results, we find positive effects across all age groups, but these effects only become large and statistically significant for age groups starting at 45, suggesting that the main effect of the shock on mortality was concentrated among older adults.

Table 13: Linked-data results for additional age categories

Age group:	DV: Std. change in log mortality rate from 1851-5 to 1861-5					
	15-24 (1)	25-34 (2)	35-44 (3)	45-54 (4)	55-64 (5)	65+ (6)
Cotton district ind.	0.414 (0.256)	0.437 (0.290)	0.198 (0.292)	0.663** (0.270)	0.612** (0.249)	0.695*** (0.248)
Nearby (0-25km)	0.264*** (0.084)	0.286** (0.122)	0.232* (0.132)	0.181 (0.117)	0.427*** (0.159)	0.417*** (0.139)
Nearby (25-50km)	0.160 (0.127)	0.166 (0.128)	0.166 (0.117)	0.366*** (0.133)	0.394*** (0.118)	0.219 (0.160)
Nearby (50-75km)	0.015 (0.146)	0.012 (0.136)	0.297* (0.166)	0.278* (0.157)	0.272 (0.186)	-0.161 (0.159)
Log initial pop.	-0.118*** (0.045)	-0.094** (0.046)	-0.061 (0.044)	-0.057 (0.048)	-0.127*** (0.047)	-0.065 (0.054)
Observations	521	521	512	507	500	527
R-squared	0.033	0.028	0.015	0.057	0.065	0.064
Linked deaths	24,272	19,636	14,305	11,765	10,709	16,377

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. The sample size changes across specifications if there are small districts where we do not have links when cutting by age group.

## Various robustness checks

Next, we investigate the robustness of the all-age result in Table 14. In Column 1 we present results where we do not weight the regressions by initial district population. In Column 2 we consider an alternative, continuous, measure of treatment, the share of employment in cotton textiles in each district in 1851. 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 Liverpool, which was not a cotton district but which was the main port serving the industry, as well as 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.

Table 14: Additional linked-data results on all-age mortality rates

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>					
	No weights	Continuous treatment	Without Manchester	Drop Manchester & Leeds	Only native-born
	(1)	(2)	(3)	(4)	(5)
Cotton district ind.	0.882** (0.390)		1.112*** (0.430)	1.090** (0.423)	0.825* (0.426)
Cotton employment share		1.472** (0.609)			
Nearby (0-25km)	0.313** (0.159)	0.273 (0.172)	0.428*** (0.124)	0.414*** (0.124)	0.398*** (0.125)
Nearby (25-50km)	0.441*** (0.145)	0.300 (0.193)	0.369** (0.159)	0.318** (0.126)	0.414** (0.164)
Nearby (50-75km)	0.106 (0.200)	-0.003 (0.147)	0.006 (0.145)	0.019 (0.150)	-0.021 (0.145)
Log initial pop.	-0.152** (0.074)	-0.083 (0.087)	-0.047 (0.079)	-0.087 (0.078)	-0.088 (0.088)
Age share controls	Yes	Yes	Yes	Yes	Yes
Observations	537	537	537	537	537
R-squared	0.056	0.059	0.117	0.119	0.090

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions in Column 2-5 are weighted by district population in 1851. Age group controls include the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census.

### Alternative linking and 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 deaths where there are other potential links that sound similar to the name in question. 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 same after Soundex cleaning. This modification reduces our sample size substantially, to just 56,914 linked deaths, or just over one-third of the size of our preferred sample. In a second, and even more

restrictive, modification, we eliminate any link where, in the census, there is a second record with a similar sounding first and last name. This modification reduces our sample size to 41,552 linked deaths. Results obtained using these two alternative linked samples, and assigning deaths to individuals' initial district of residence (as in Table 1), are presented in Columns 1-2 of Table 15. This table shows that our main results continue to hold, even when using these severely limited samples. Note that the magnitude of the effects appears smaller when using these samples. This may be because we are losing more good links than bad links, which would make type II errors more important, and bias our coefficients towards zero. Alternatively, the difference may be to changes in the set of districts which we can include in the analysis.

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 15 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, as indicated by our comparison of middle initials (using the sample where middle initial is reported for both observations). 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.

It is particularly interesting to consider the results in Column 6, which include only observations where the death district and census district match. In this sample there are no migrants, and yet the results look very different from what we observe when using aggregate data. The explanation for this is that the aggregate data include the deaths of migrants. Thus, comparing the results in Column 6 to results obtained from aggregate data provides another illustration of the impact of migrants on results obtained from aggregate data.

Table 15: Linked-data analysis using alternative linking procedures

	<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>					
	Requiring links to have a unique sounding name		Omitting links where death and enumeration district are far apart			
	Last Name (1)	First and Last (2)	0-200km only (3)	0-100km only (4)	0-50km only (5)	No Migration (6)
Cotton district ind.	0.533* (0.277)	0.436* (0.255)	0.738* (0.406)	0.741* (0.396)	0.715* (0.383)	0.709** (0.291)
Nearby (0-25km)	0.318** (0.149)	0.283* (0.145)	0.360*** (0.118)	0.407*** (0.116)	0.365*** (0.112)	0.332*** (0.104)
Nearby (25-50km)	0.403*** (0.147)	0.384*** (0.139)	0.364** (0.163)	0.330* (0.168)	0.287* (0.151)	0.168 (0.139)
Nearby (50-75km)	-0.066 (0.128)	-0.099 (0.135)	-0.023 (0.139)	-0.060 (0.127)	-0.059 (0.141)	-0.083 (0.189)
Observations	536	533	537	536	535	531
R-squared	0.074	0.064	0.081	0.088	0.084	0.080
False Positive Rate	30.03%	28.70%	30.79%	25.56%	22.45%	15.10%
Linked Deaths	56,623	41,376	129,323	107,270	91,691	61,420

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. The sample size changes across specifications if there are small districts where we do not have links when cutting by age group.

## Results using a four-period panel

One alternative to our preferred regression specification is instead to construct a panel of data with four periods, 1851-55, 1856-60, 1861-65, and 1866-70. Here we have one shock period and one post-shock period. We do not prefer this approach in our

main results because, for example, death rates based on deaths from 1856-60 relative to population in the 1851 census are not strictly comparable to rates using deaths from 1851-55 compared to the 1851 census. One reason behind this is that deaths in 1856-60 correctly linked back to the 1851 census cannot include any deaths at ages under 5, since no one under 5 who died in 1856-60 could have been enumerated in the 1851 census.

Setting aside this concern, we have calculated results using this four-period panel as an additional check on the validity of the patterns obtained from our preferred specification. In Table 16 we present results using all four periods, including district and time fixed effects, and comparing the 1861-65 period to the other three periods. Note that, because we find evidence of scarring effects in the years after 1865, including the 1866-70 period among the controls biases these estimates against our findings. Despite this, we continue to find evidence of an increase in overall mortality in the cotton textile districts in the shock period which is driven primarily by the increase in mortality among older people. We also continue to find evidence of increased mortality in nearby districts within 50 km.

Table 16: Four-period analysis

	<b>DV: Std. log mortality rate in a given period</b>			
	<b>All ages</b>	<b>Under 15</b>	<b>Age 15-54</b>	<b>Over 54</b>
	(1)	(2)	(3)	(4)
Shock X Cotton	0.179* (0.099)	0.095 (0.098)	0.137 (0.099)	0.292** (0.124)
Shock X Nearby (0-25km)	0.056* (0.033)	0.023 (0.045)	0.087*** (0.027)	0.101** (0.049)
Shock X Nearby (25-50km)	0.065* (0.039)	0.010 (0.037)	0.037 (0.033)	0.144*** (0.043)
Shock X Nearby (50-75km)	-0.005 (0.036)	-0.031 (0.053)	0.038 (0.040)	0.063 (0.059)
District FEs	Yes	Yes	Yes	Yes
Death Period FEs	Yes	Yes	Yes	Yes
Observations	2,147	2,142	2,143	2,132
R-squared	0.857	0.842	0.843	0.796

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Regressions are weighted by district population in 1851. Note that the number of observations varies because some small districts with no linked deaths in one period are dropped.

### Logit regressions at the individual level

In our main results we run regressions on mortality rates at the district level. An important advantage of this approach is that it delivers results which can be easily compared to estimates obtained from aggregate data. An alternative is to work with the individual-level data and run logit regressions. The main advantage of the logit approach is that it allows us to directly control for observable individual characteristics, such as age and gender. This appendix explores results obtained from this alternative approach.

In running individual-level regressions we face substantial computational issues. This is because our sample covers the population in two full censuses of England and Wales, or roughly 37 million observations. Running logit regressions with fixed effects on a sample of this size is not trivial. To reduce these computational challenges, instead of working with the full population we select a random ten percent sample of all unlinked census records, together with our full sample of linked census records. We

then run logit regressions where the outcome of interest is whether the enumerated individual appears in the death index in the 5 years following enumeration. That is, if the individual is in the census but does not appear as a linked death in the next five-year period, the outcome variable takes a value of 0, while if they do appear as a linked death, they are assigned a 1.

Our regression includes period fixed effects (i.e., an indicator for whether the individual was enumerated in 1851 or 1861), district of enumeration fixed effects, age fixed effects and sex fixed effects. Our explanatory variables of interest are the interaction between being enumerated in the treatment period (1861) and being enumerated in either a cotton district or a district that falls within 25 km, 50 km, or 75 km of a cotton district.

Results from this regression are presented in Table 17. These results are largely consistent with our main regressions in that we continue to find strong evidence that mortality rose in nearby areas during the shock period. In cotton districts we continue to find positive mortality effects, particularly for those over the age of 54.

Table 17: Estimating the impact of the cotton shortage at the individual level

<b>DV: Appears in the BMD death index</b>				
	<b>All ages</b>	<b>Under 15</b>	<b>Age 15-54</b>	<b>Over 54</b>
Avg. Marginal Effects	(1)	(2)	(3)	(4)
Cotton district X Shock	0.0006 (0.0008)	-0.0019 (0.0014)	0.0001 (0.0009)	0.0127*** (0.0036)
Nearby (0-25km) X Shock	0.0049*** (0.0008)	0.0047*** (0.0013)	0.0033*** (0.0009)	0.0104*** (0.0035)
Nearby (25-50km) X Shock	0.0021*** (0.0007)	0.0001 (0.0012)	0.0021** (0.0009)	0.0066** (0.0029)
Nearby (50-75km) X Shock	0.0000 (0.0010)	-0.0021 (0.0018)	0.0011 (0.0013)	0.0021 (0.0021)
Observations	3,707,596	1,305,981	2,000,984	399,130

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the district of initial residence (i.e., district of census enumeration). Each regression includes age fixed effects, a sex indicator, period fixed effects, and district fixed effects.

### D.1.2 Evidence on mechanisms

Next we consider some results that speak to the mechanisms behind our effect. In Section 3.6 we showed that deaths among cotton textile workers and their family members increased during the cotton shortage period. In Table 18 we assess the extent to which our overall results are driven by cotton textile workers and their families by progressively dropping them from our data and the re-estimating the results. For comparability, Column 1 shows the all-age results from our full linked sample. In Column 2, we generate new results while dropping cotton textile workers from the data. We can see that dropping cotton textile workers only slightly reduces the estimated coefficient on the effect in cotton districts. Next, in Column 3, we drop all cotton textile workers and their family members from the regressions. Now we see a much larger decline in the estimated effect of the recession on mortality. The coefficient on the cotton textile districts is reduced by more than half and becomes statistically insignificant, though it remains positive. Thus, this exercise indicates that deaths among cotton textile workers their families explain most of the estimated impact of the recession on mortality among those initially resident in the cotton textile districts, though the fact that we continue to observe a positive coefficient suggests that other groups were likely to have also experienced an increase in mortality.

Next, we look at the impact of dropping workers in the industries sharing substantial input-output connections with the cotton textile sector. Coal supplied the main domestically-provided input to cotton textile production, while clothing trades, such as shirt and dressmakers, were the main domestic buyers. In Column 2 of Table 19 we drop these workers as well as wool workers from our sample and re-estimate the results (for comparison Column 1 contains our baseline results). In Column 3 we also drop any member of a household that includes one of these workers. We can see that dropping these workers has essentially no impact on our results. This tells us that the mortality effects observed in nearby districts are not operating primarily

Table 18: Estimated impact of cotton households on our main results

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>			
	Full linked sample	Dropping cotton workers	Dropping all members of cotton households
	(1)	(2)	(3)
Cotton district indicator	0.839* (0.436)	0.746* (0.442)	0.361 (0.479)
Nearby (0-25km)	0.429*** (0.124)	0.417*** (0.126)	0.364*** (0.131)
Nearby (25-50km)	0.403** (0.163)	0.395** (0.166)	0.371** (0.170)
Nearby (50-75km)	0.017 (0.148)	0.016 (0.150)	0.004 (0.152)
Log initial pop.	-0.087 (0.089)	-0.092 (0.090)	-0.106 (0.093)
Age share controls	Yes	Yes	Yes
Observations	537	537	537
R-squared	0.089	0.078	0.051

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the initial district of residence (i.e., census enumeration district). Regressions are weighted by district population in 1851. Age group controls include the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census.

through input-output connections.

Table 19: Estimated impact of coal, wool, and other clothing households on our main results

<b>DV: Std. change in log mortality rate from 1851-5 to 1861-5</b>			
	Full linked sample	Dropping coal wool and clothing workers	Dropping all members of households with coal, wool or clothing workers households
	(1)	(2)	(3)
Cotton district indicator	0.839* (0.436)	0.839* (0.436)	0.808* (0.424)
Nearby (0-25km)	0.429*** (0.124)	0.424*** (0.130)	0.442*** (0.158)
Nearby (25-50km)	0.403** (0.163)	0.399** (0.166)	0.333** (0.147)
Nearby (50-75km)	0.017 (0.148)	0.014 (0.147)	-0.019 (0.163)
Log initial pop.	-0.087 (0.089)	-0.084 (0.090)	-0.068 (0.088)
Age share controls	Yes	Yes	Yes
Observations	537	537	537
R-squared	0.089	0.085	0.079

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses. Deaths are assigned to the initial district of residence (i.e., census enumeration district). Regressions are weighted by district population in 1851. Age group controls include the share of district residents under 5, the share 5-15, and the share over 50 for both the 1851 and 1861 census.

## D.2 Results from alternative aggregate-data approaches

In the main text, for purposes of easy comparability, our aggregate-data approach mirrored the first differences approach necessitated by the structure of our linked data. This section presents results applying an estimating approach more similar to that used in Ruhm (2000). In particular, these results are based on Eq. 1, which is as close to the standard specification as we can get in our setting, given that we do not have measures of district-level unemployment. We also check the robustness of our panel-data results by considering alternative time periods and by including

time trends. As is relatively standard in the literature, we weight the regressions by district population. Note that this approach requires the use of annual population estimates. These are generated using the standard Das Gupta approach, as discussed in Section B.2.

Table 20 presents the results. We consider a variety of different specifications that a reasonable empirical economist might consider. In the first four columns we use data from 1851-1870, which cover the period over which the population data are interpolated. This gives us both pre-shock and post-shock observations which we compare to the treatment period, 1861-65. We consider results with and without accounting for spillovers into nearby districts, and with and without time trends. These results consistently indicate that mortality fell as a result of the cotton shortage.

In an attempt to obtain results that come closer to recovering the effects documented in our main analysis, Columns 5-8 compare just the pre-shock and cotton shortage periods, dropping data after 1865. When using this time window and including time trends, we can see that the aggregate approach does indicate that mortality increased during the cotton shortage, and we also begin to see evidence that mortality increased in nearby areas. The time trends clearly have a large effect on the results when there is no post period.

Overall, however, our conclusion is that an analysis based on aggregate data is unlikely to reliably and consistently recover accurate estimates of the impact of recessions on mortality in the presence of unobserved migration. While certain specifications do generate results similar to what we obtain using the linked data, this relies on the researcher choosing just the right analysis period and specification.

Table 20: Results from aggregate panel data regressions

Sample:	Dependent variable is std. log mortality rate 1851-1871				1851-1865			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cotton district ind.	-0.228*** (0.041)	-0.238*** (0.038)	-0.221*** (0.042)	-0.242*** (0.038)	-0.163*** (0.054)	0.265*** (0.058)	-0.138** (0.054)	0.294*** (0.058)
Nearby (0-25km)			0.040 (0.053)	-0.007 (0.041)			0.129 (0.080)	0.285** (0.111)
Nearby (25-50km)			-0.019 (0.071)	-0.077 (0.055)			0.075 (0.107)	0.063 (0.146)
Nearby (50-75km)			0.095** (0.043)	0.076* (0.039)			0.118** (0.056)	-0.016 (0.074)
Time Trends	N	Y	N	Y	N	Y	N	Y
Observations	10,780	10,780	10,780	10,780	8,085	8,085	8,085	8,085
R-squared	0.139	0.253	0.139	0.253	0.168	0.277	0.170	0.279
No. districts	539	539	539	539	539	539	539	539

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses