# Estimating the Recession-Mortality Relationship when Migration Matters*

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**Abstract**

Are recessions good for health? A large literature following Ruhm (2000) addresses this question by applying a fixed-effects approach that implicitly assumes either that recessions do not generate a substantial migratory response, or that such responses are accurately reflected in intercensal population estimates. These assumptions may pose a serious methodological concern in settings, such as developing countries, that are characterized by weak social safety nets, mobile populations, and poor intercensal data. We illustrate this point by drawing on a natural experiment—the recession in Britain's cotton textile-producing regions caused by the U.S. Civil War (1861-1865)—to provide evidence that migration-induced bias can substantially affect, and even reverse, estimates of the recession-mortality relationship. To deal with this bias, we propose a strategy based on accounting for mortality spillovers in migrant-receiving locations. Applying this methodology to our historical setting, we find evidence that, if anything, the recession we study increased mortality. In contrast, we show that existing approaches, which do not account for migration bias, would lead us to exactly the opposite conclusion. After adjusting for migration, we do find evidence that infant mortality fell, but this was offset by large increases in mortality among the elderly.

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# 1 Introduction

Are recessions good or bad for health? The relationship between macroeconomic conditions and public health has become the subject of growing interest among economists following Ruhm (2000), who found evidence that in the U.S., total mortality rates *fall* during recessions. Methodologically, Ruhm compared state-level unemployment rates (or similar indicators of economic conditions) to overall state-level mortality rates. This panel data approach has since been applied to a variety of settings yielding largely consistent results: health appears to improve during recessions.[1] While the literature has largely focused on developed economies, recent work has begun to extend this approach to the study of developing countries and historical settings.[2]

One critical assumption of the standard empirical approach in this literature is that the population denominators used to calculate death rates are accurate. This requires either that there is no large migration response in recession-stricken locations, or that short-run population flows are accurately reflected in intercensal population estimates. If this assumption fails, then in locations more severely affected by an economic downturn, we may observe a spurious fall in the observed death rate driven by unobserved out-migration, a phenomenon we call *migration bias*. Unobserved migration also raises two further concerns. First, standard errors are likely to be correlated across locations. Second, key explanatory variables such as the unemployment rate are likely to be endogenously related to migration. These issues may not be critical in developed countries, such as those studied by Ruhm (2000), where data are of high quality and strong social safety nets may reduce the migratory response to recessions. However, migration bias is likely to become increasingly important as the focus of the literature turns to contexts, such as low-income and historical settings, in which

[1]See Section 2 for a review of this literature.

[2]For example, Gonzalez & Quast (2011) study the relationship between recessions and mortality in Mexico, while Stuckler *et al.* (2012) examined the U.S. during the Great Depression.

intercensal population estimates may be less accurate, and where weaker social safety nets may lead to larger migration responses to crisis.

Accordingly, the primary goals of this study are first, to assess the bias generated by migration, and second, to offer a simple approach to help identify and mitigate this bias. Despite the fact that concerns about migration bias have been raised in the literature, to our knowledge no study has sought to assess the extent of this bias, nor has any study proposed an approach to both identify and deal with this issue.[3] Our results suggest that accounting for migration bias can substantially change, and even reverse, the estimated relationship between recessions and mortality in at least some settings. Our secondary goal, which stems from the specific recession that we study, is to examine the recession-mortality relationship in a historical setting where social safety nets and access to medical care were limited.[4] Thus, in addition to offering methodological improvements to aid identification, our study also contributes to a broader and more nuanced understanding of the public health effects of recessions. In particular, our results show that the impact of recessions on mortality can differ substantially across age groups. In our setting, reductions in infant mortality during the recession are more than offset by increases in deaths among the elderly. One implication of these findings is that focusing on just one of these groups, such as infants, can yield results that differ substantially from the effect observed on aggregate.

In order to measure the impact of migration bias on estimates of health during recessions, we require a setting in which we can observe the local incidence of a re-

[3] The issue is mentioned in Ruhm (2007), Stuckler *et al.* (2012), and Lindo (2015). There are, however, a small number of studies that, by using individual-level panel data, inherently avoid issues of migration bias. These include Ruhm (2003), Dehejia & Lleras-Muney (2004), Gerdtham & Johannesson (2005), and Edwards (2008). Individual-level panel data is a natural way to address migration bias, but such high quality data are rarely available outside of high-income settings; to wit, all of the studies listed above use evidence from either the U.S. or Sweden.

[4] The relatively weak social safety net in 19th century Britain—the setting we study—is likely to be an important contributor to migration. In modern developed countries, work by Autor *et al.* (2013) suggests that the migratory response to local economic shocks is small. For contrast, studies in historical and developing-country settings, such as Hornbeck (2012) and Hornbeck & Naidu (2014), and Jayachandran (2006), respectively, often document substantial population responses to local shocks.

cession without relying on measures, such as the unemployment rate, which drive and are endogenously affected by migration decisions.[5] To find such a setting, we turn to history. Specifically, we draw on a unique natural experiment: the temporary but severe economic downturn in the cotton textile-producing regions of Britain that resulted from the U.S. Civil War (1861-1865). The cotton textile industry was England's largest industrial sector in the second half of the 19th century and, prior to the U.S. Civil War, received the majority of its raw cotton inputs from the U.S. South. The onset of the Civil War sharply reduced these supplies, leading to a severe short-run economic downturn that left several hundred thousand workers unemployed.[6] Because the cotton textile industry was highly geographically concentrated, we are able to clearly identify the spatial distribution of this economic shock using detailed data on district-level occupational structure on the eve of the war.[7]

To examine the impact of the downturn on mortality, we digitize annual mortality data (tabulated by age, gender, and cause of death) for over 600 districts covering all of England and Wales from 1851-1870. We add to this population data digitized from the census years 1851, 1861, and 1871, which allow us to generate estimated population levels and test the extent to which traditional approaches to estimating the recession-mortality relationship may suffer from mis-measurement of the death-rate denominator. Together, these data allow us to exploit spatial and temporal variation in economic conditions to determine the public health impact of a localized recession.

We begin our study by applying the standard panel data approach prevalent in the

[5]If unemployed workers leave an area to obtain employment elsewhere, then this migration will not only respond to but also affect the local unemployment rates in both sending and receiving locations.

[6]Historians often refer to this event as the "Cotton Famine," where the term "famine" is used metaphorically to describe the dearth of cotton inputs. However, since our study pertains to health outcomes, we prefer to use terms such as "cotton shortage" or "downturn" so as to avoid confusion with the more literal definition of famines as episodes of nutritional deprivation resulting in ill health and death.

[7]This approach is somewhat similar to Miller & Urdinola (2010), which looks at how changes in the global coffee price affect mortality (as inferred from cohort size) in Colombia.

literature. Consistent with previous literature on developed economies, these results suggest that during the recession, mortality rates fell in districts that specialized in cotton textile production. However, as mentioned above, these results may be biased if people migrated away from recession-stricken areas during this period. Indeed, we present evidence that the recession brought about substantial out-migration, and that most of this migration took place between geographically proximate districts. Relying on these facts, we examine the potential role of migration in producing these results by testing how mortality changed during the recession period not only in the cotton districts, but also in the nearby districts which were the destination of most migrant flows. To avoid the bias generated by using estimated population denominators, we focus our analysis on changes in the log number of deaths rather than in the mortality rate.

Using this approach, we find that deaths increased in districts near those that were most affected by the recession, consistent with the expected effect of migration from severely affected districts into nearby areas.[8] In the standard approach, these districts—which were also affected by the shock in that they received recession-driven population inflows, and with these, a corresponding increase in raw deaths—are included in the set of comparison districts. As a result, this approach leads to the erroneous impression that the cotton districts had become healthier during the economic downturn. Our approach, however, effectively removes these nearby districts from the control group. Once this is done, we no longer find evidence of an overall mortality reduction in districts directly affected by the cotton shortage. Instead, we find evidence that, if anything, mortality *increased* during the recession in the most severely impacted locations. Put another way, the positive overall health effects of recessions obtained using the standard panel-data approach essentially disappear when

[8]Notably, this result need not stem from selective migration or real changes in health due to recession or migration; rather, it may merely reflect the mechanical relocation of deaths that would normally have occurred in one district to another. We discuss the mechanisms by which migration might affect health and estimates of health in greater depth in Section 3.2.

we account for migration bias. These results highlight the potential of migration bias to undermine inference, and the tendency of traditional methods to underestimate a recession's adverse effects on public health in the presence of migration. Such migration bias is likely to be most problematic in settings where people are more mobile and intercensal population estimates are less accurate.

Beyond biasing the coefficient estimates, the possibility of migration, along with the spatial clustering of industry, also raises concerns about spatial correlation in the standard errors. In our review of the literature on recessions and public health, we have come across only one study, Lindo (2015), that raises the possibility of spatially correlated standard errors.[9] Instead, the vast majority of existing studies tend to either cluster by the geographic unit to address serial correlation, or to use robust standard errors. In our setting, standard errors are roughly twice as large after accounting for spatial correlation, relative to simple robust standard errors. While the large cross-sectional dimension of our data makes spatial correlation a particular concern, we also find that the spatial standard errors are much larger than those obtained when clustering by the 55 counties in the data. In fact, adjusting for spatial correlation alone is enough to eliminate statistical significance in the recession-mortality relationship estimated using the standard approach, even without adjusting for the coefficient bias due to the inclusion in the control group of districts receiving migrant spillovers. Overall, this highlights the fact that in studies with larger cross-sectional dimensions and relatively few years, spatial correlation may be an important concern, and that clustering on aggregated spatial units alone will not fully account for this.[10]

[9]Lindo (2015) posits that errors may be correlated across space, and so employs two-way clustering and clustering on larger geographic units. Each of these approaches partially addresses concerns regarding spatial spillovers. In contrast, we recommend correcting for both serial and spatial correlation.

[10]In the Appendix, we provide a review of 26 of the leading papers which apply panel data methods to study the recession-mortality relationship, starting with Ruhm (2000). In many of these studies, the cross-sectional dimension of the data exceeds the time-series dimension, a setting in which spatial correlation (if it exists) may be an important consideration.

In addition to these methodological contributions, our paper also provides new evidence on the historical relationship between adverse economic shocks and mortality. While we find evidence that overall mortality after accounting for migration bias if anything *increased* as a result of the downturn that we study, we also find substantial differences in the response among different age groups. For one, infant mortality rates appear to have fallen in the cotton districts during the recession. Consistent with modern evidence, contemporary reports attribute these improvements to greater time spent on childcare by mothers, though changing fertility decisions and selective migration by mothers may have also played a role. The recession, however, does not appear to have affected mortality among the working-age population. We find evidence that this was likely due to health improvements stemming from a reduction in industrial accidents, alcohol consumption, and deaths from maternal causes, factors which offset the negative effects of deteriorating living conditions. Finally, we find strong evidence that mortality increased among older adults, who were less mobile and whose mortality patterns appear to be linked to their vulnerability to the deprivation generated by the recession. Overall, the increase in mortality among older adults was large enough to offset the health improvements among infants, such that on net, after accounting for recession-driven migration, our evidence suggests that the recession most likely resulted in an increase in total mortality.

The rest of the paper proceeds as follows. The next section provides a brief overview of the related literature, highlighting both positive and negative mechanisms that affect health during times of economic distress. Section 3 describes the empirical setting we consider. The data are introduced in Section 4, followed by the analysis in Section 5, and concluding remarks in Section 6

## 2 Related literature

In the 1970s and 1980s, influential work by Harvey Brenner highlighted the adverse effects that economic recessions could have on health.[11] These findings, which were based on an analysis of aggregate time-series patterns, fit existing conceptions of recessions as broadly harmful events. However, these conclusions were challenged by Christopher Ruhm (2000) in a seminal paper that applied a fixed-effects methodology to state-level panel data from the U.S. to show that health actually *improved* during recessions in the modern United States. Ruhm's study has now been cited over 1,400 times, and the methodology that it introduced has been applied in a wide variety of contexts, including Germany (Neumayer, 2004), Spain (Tapia Granados, 2005b), Sweden (Svensson, 2007), Canada (Ariizumi & Schirle, 2012), Mexico (Gonzalez & Quast, 2011), and across multiple countries (Gerdtham & Ruhm, 2006; Economou *et al.*, 2008; Lin, 2009).[12] These studies, which focus largely on modern developed or middle-income countries, have consistently found evidence that mortality decreased during recessions.[13]

Nevertheless, the broader literature suggests that adverse economic shocks may have countervailing effects on health, making the net effects of a recession on mortality ambiguous a priori. On the one hand, the associated negative income shock may compromise access to proper nutrition, shelter, and medical care (Griffith *et al.*, 2013; Painter, 2010). Job loss, in particular, may cause psychological stress, raising rates of suicide and other risky behavior (Eliason & Storrie, 2009; Sullivan & von Wachter,

[11] See, e.g., Brenner (1979).

[12] Typically these studies follow Ruhm in applying fixed effects estimation across states or countries with the log mortality rate as the dependent variable. More recent studies generally cluster their standard errors by state or country to address serial correlation concerns, following Bertrand *et al.* (2004). See Table 6 in the appendix for further details.

[13] There are a few exceptions that find mixed evidence or countercyclical mortality, such as Svensson (2007) and Economou *et al.* (2008). In addition to the panel-data studies cited above, a number of other studies apply time-series approaches to examine the recession-mortality relationship. Similar countercyclical patterns have also been documented in historical settings in the U.S. and Britain (Fishback *et al.*, 2007; Stuckler *et al.*, 2012). Other studies look at the historical relationship between recessions and mortality using time-series approaches, including (Tapia Granados, 2005a, 2012).

2009). On the other hand, these events may remove individuals from environmental and work-related hazards such as pollution, traffic accidents, and on-the-job injuries (Muller, 1989; Chay & Greenstone, 2003); free up time for breastfeeding, childcare, exercise, and other salutary activities (Dehejia & Lleras-Muney, 2004; Ruhm, 2000); raise the quality of elder-care (Stevens *et al.*, 2015); and limit the capacity for unhealthy behaviors such as smoking and alcohol use (Ruhm & Black, 2002; Ruhm, 2005).[14]

Recently, a small number of studies have offered an alternative approach to assessing the recession-mortality relationship which relies on individual-level panel data from Sweden (Gerdtham & Johannesson, 2005) and the U.S. (Edwards, 2008). These studies deliver more mixed results than those found using the standard Ruhm approach. Importantly, studies using individual-level data are not subject to migration bias. Accordingly, their mixed findings hint at the possibility that such bias may confound studies using aggregate data. While this offers a promising approach to addressing migration bias, data limitations mean that it cannot be applied in many of the settings economists may be interested in, particularly developing countries. Relative to these studies, we offer a simple approach that can be applied broadly, even in settings where data are limited.

Finally, our study is related to a growing set of studies which examine the relationship between economic conditions and infant health in developing countries.[15] In these studies, one important reason for focusing on infant deaths is that they can be

[14]As we will discuss in the next section, some of these mechanisms, such as changes in alcohol use, pollution exposure, time spent caring for children, and access to adequate food, clothing, and shelter, are likely to be relevant in our setting. Others, such as traffic accidents, smoking, and obesity, were less important in 19th century Britain. There are also some factors, such as on-the-job injuries, which are likely to be much more important in our setting than in modern developed countries.

[15]Recent studies in this literature include Paxson & Schady (2005), Ferreira & Schady (2009), Bhalotra (2010), Baird *et al.* (2011), Cruces *et al.* (2012), Friedman & Schady (2013), and Bozzoli & Quintana-Domeque (2014). The approach of Miller & Urdinola (2010) is perhaps most closely related to our study in that they exploit a trade shock interacted with the initial spatial distribution of industry-specific production for identification. These studies generally find that negative economic shocks increase infant mortality, consistent with the dominance of income effects.

compared to births, which largely eliminates concerns about migration bias. However, our results suggest that to study infants alone is to tell only part of the story: as we show in the analysis that follows, the relationship between economic shocks and mortality for infants may be substantially different than for other age groups, and may even differ in sign from the impact of recessions on overall mortality.

# 3 Empirical setting

## 3.1 The cotton textile industry and the U.S. Civil War

The cotton textile industry was the largest and most important sector of the British economy during the 19th century, employing 2.3% of the total population of England & Wales in 1861. This figure, which constituted 9.5% of all manufacturing employment at the time, still understates cotton's importance to Britain: over four times this number depended on the industry either directly or indirectly for their livelihood.[16]

The industry itself was highly geographically concentrated. For historical reasons dating to the 18th century, British cotton textile production was centered in the Northwest counties of Lancashire and Cheshire, which together held over 80% of the cotton textile workers in England & Wales in 1861.[17] Although a large proportion of Lancashire and Cheshire residents were employed in cotton textiles (13% of their total population in 1861, or 26% of their working population), there was nevertheless considerable variation within these counties in the extent of dependence on cotton textiles: in major cotton towns such as Manchester, Blackburn, Bolton, Preston, and Stockport, cotton textile employment could exceed 50% of the total working

[16]Estimates of indirect cotton employment come from the *Report of the Commissioners of Public Health*, v. 13, 1863, p. 16. All other calculations in this section, unless otherwise specified, are based on data collected by the authors from the 1861 Census of Population reports.

[17]Crafts & Wolf (2014) suggest that the main factor determining the location of the cotton textile industry prior to 1830 was the location of rivers, which were used for power, access to the port of Liverpool, and a history of textile innovation in the 18th century.

population.[18]

Britain's dependence on raw cotton imports made its cotton textile industry especially vulnerable to supply shocks. British cotton textile production was entirely reliant on imported raw cotton supplies, and in the decade before the U.S. Civil War, over 70% of these imports came from the U.S. South (Mitchell, 1988). The war, however, sharply reduced these supplies due to factors including the Confederate government's adoption of an embargo as a means of eliciting European support, the blockade of southern shipping by the Union Navy, and wartime economic disruptions more generally. The consequence was a precipitous drop in British imports of U.S. cotton, and an accompanying rise in cotton prices, both illustrated in the left-hand panel of Figure 1.[19]

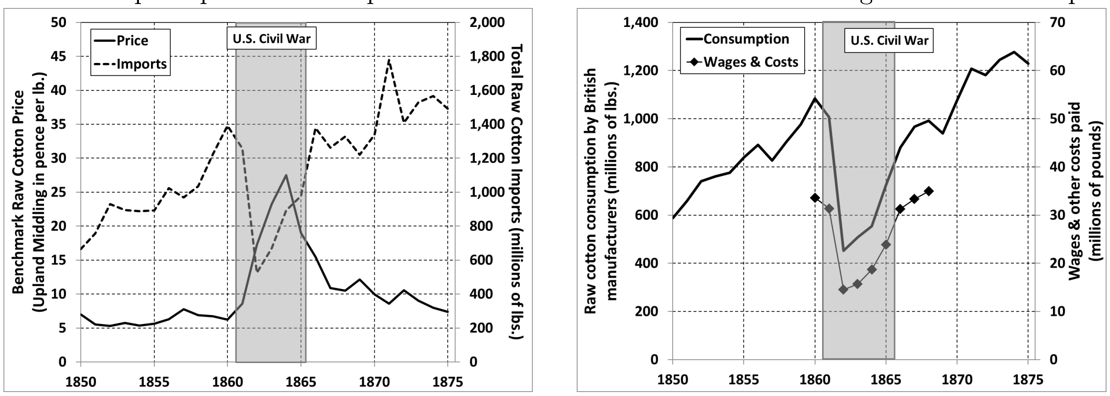
The cotton shortage had severe economic consequences for areas specializing in cotton textile production. The right-hand panel of Figure 1 shows that domestic raw cotton consumption, a good measure of industry output, fell by around half from the peak of 1860 (an exceptionally good year for the industry) to the trough in 1862-1863. However, the impact of the shock was only temporary: by the second half of the 1860s, domestic cotton consumption had rebounded to pre-war levels. A similar pattern is evident in the level of wage payments and other (non-cotton) costs paid by British cotton textile manufacturers from 1860-1868. In turn, cotton workers suffered: Figure 2 shows that both expenditures on Poor Law relief (one of the main sources of government support for destitute workers during this period), in the left panel, and the share of able-bodied workers seeking relief payments under the Poor Law, in the right panel, rose dramatically during the crisis in the cotton-producing counties of

[18] See Figure 7 in the appendix for a map detailing the spatial distribution of the cotton textile industry.

[19] While other cotton-producing countries, such as India, Egypt and Brazil, rapidly increased their output during the Civil War period, these increases were not large enough to offset the lost U.S. supplies, though they did contribute to the rapid rebound in imports after 1865. Producers also faced technological barriers when switching from U.S. cotton to lower quality cotton supplies from India, the second largest supplier. Hanlon (2015) describes the technological improvements that were undertaken as part of the switch to Indian cotton.

Figure 1: British cotton prices, imports, consumption and wage payments 1850-1875

Import quantities and prices — Cotton consumed and wages & other costs paid




Import data from Mitchell (1988). Price data, from Mitchell & Deane (1962), are for the benchmark Upland Middling variety. Domestic raw cotton consumption data are also from Mitchell & Deane (1962). The data on wage and other costs paid by the industry come from Forwood (1870).

Lancashire and Cheshire in Northwest England.[20] The absence of concurrent increases in relief payments and relief-seekers elsewhere in the country serves to underscore the highly localized nature of this adverse shock.

The evidence above shows that the cotton shortage caused by the U.S. Civil War generated a severe local recession for areas specialized in cotton textile production. The length and severity of these events came as a surprise to both workers and manufacturers, who initially expected only a short disruption in trade. The moderate response of the cotton price in 1861, shown in Figure 1, also attests to the failure of the market to anticipate the severity of the U.S. Civil War, or the cotton shortage it would generate.[21] Nevertheless, conditions worsened in 1862 until reaching the

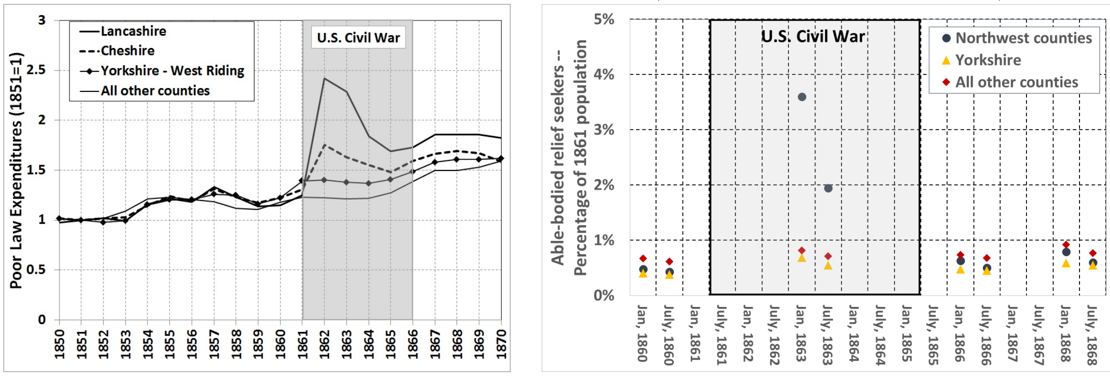
[20]The right-hand panel of Figure 2 describes the share of able-bodied relief seekers before the Civil War (1860), during the crisis (1863), and after the war (1866 and 1868) for a selection of Poor Law Unions using data collected by Southall *et al.* (1998). The Unions are divided into those in the cotton producing counties of Northwest England, the nearby area of Yorkshire, where the economy was specialized in wool textiles, and all other areas of the country for which data are available. Note that counts of relief seekers are available for only a subset of Poor Law Unions, and at only a few points in time.

[21]See, also Watts (1866, p. 112). Even in the U.S., the conflict was expected to be short, as illustrated by the fact that the initial enlistment period for Union soldiers was only 90 days.

Figure 2: Poor law expenditures and relief seekers as a share of 1861 population

Expenditures on Poor Law relief

Able bodied relief-seekers (share of 1861 population)




Expenditure data were collected from the annual reports of the Poor Law Board. Data on relief seekers come from Southall *et al.* (1998) (right-hand graph reproduced from Hanlon (2014)).

nadir of the crisis in the winter of 1862-63, by which time half a million people were receiving government or charitable relief in the cotton districts.[22] The depression continued into early 1863, with the recovery beginning in the middle of 1863 and continuing through 1865.

Contemporary reports offer a mixed view of the impact that these events 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." These gains were attributed primarily to "more freedom to breathe the fresh air, inability to indulge in spirituous liquors, and better nursing of children" in the words of the Registrar of Wigan.[23] The importance of childcare is highlighted in a number of reports.[24] On the other

Similarly consistent with the belief that the U.S. Civil War would be short-lived, in 1861 many mill owners simply employed workers on a short-time basis, which reduced but did not eliminate their income.

[22] See Appendix 7.2.3 for further details.

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

[24] See, for example, Dr Buchanan's 1862 *Report on the Sanitary Conditions of the Cotton Towns* (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 304), which discusses the importance of the "greater care bestowed on infants by their unemployed mothers than by the

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.[25] 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.

Despite the best attempts of institutions and individuals to cope with the crisis—for instance, through short-time work, public relief funds, in-kind transfers, and public works employment, strategies which are discussed in greater depth in Appendix 7.2.3—these efforts were insufficient in the face of such an intense and unexpected shock. Accordingly, migration became a popular means of adjustment as many erstwhile cotton operatives left the cotton districts in search of work in other areas.

## 3.2 Migration during the recession

Given the intensity of the downturn, it is natural to assume that it may have spurred migration. Indeed, contemporary reports document that out-migration took place, but disagreements remain over the magnitude of these flows. Drawing on contemporary sources, Henderson (1969) suggests that 4,000 workers migrated from cotton districts during this period. In contrast, the Factory Inspector Alexander Redgrave stated in October of 1863 that "It appears from the Returns of the Manchester Committee that there are 33,969 fewer persons of the operative class in the cotton districts

hired nursery keepers.".

[25] *Report on the Sanitary Conditions of the Cotton Towns* (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863).

than in 1862..."[26] Similarly, Arnold (1864) reports that "thousands had passed to east and south, and thousands had gone over the sea westward..." Despite these reports, there is nevertheless evidence that high-skilled cotton operatives resisted moving outside of the cotton districts, where their skills would be of little use, and that mill owners and municipal leaders did their best to maintain their skilled labor force in anticipation of a resumption of cotton supplies.[27]

To assess the magnitude of migratory responses to the cotton shortage, we collect census data on district population from 1851-1881, and look for changes in the patterns of district population growth. These data are presented in Figure 3, which describes changes in district population across each decade, normalized by the change in 1851-1861. This figure shows that population growth in the cotton districts fell substantially during the decade spanning the U.S. Civil War, while population growth accelerated in nearby non-cotton districts. 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 50,000 additional residents in 1871, a figure equal to 2.3% 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 64,000 fewer residents, which is equal to 5% of the districts' 1861 population. These figures will understate the migration response if some migrants returned between 1865 and 1871.[28]

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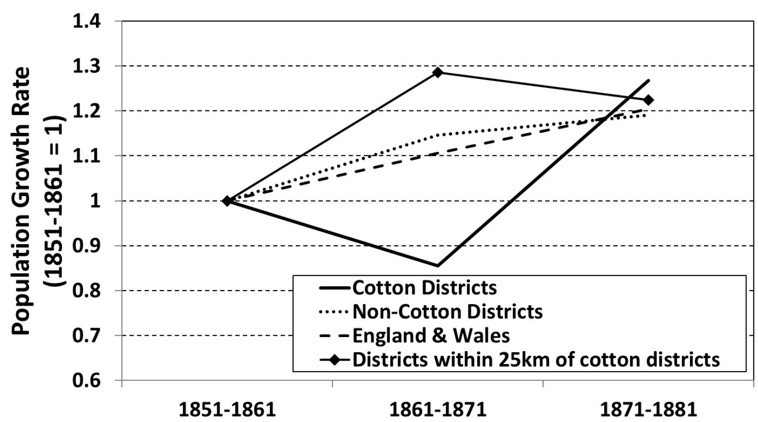
[26]Part of the difference between Henderson's figures and those cited by Redgrave may be due to differences in how they define the cotton districts. It appears that the term "cotton districts" was sometimes used to describe all of the counties of Lancashire and Cheshire, while at other times it may have referred only to the main cotton producing districts within these counties. This would have been an important difference, since much of the migration away from the main cotton textile districts appears to have been to other non-cotton districts within Lancashire and Cheshire. When we use the term "cotton districts" it will always refer to only the cotton textile districts rather than the cotton counties as a whole, where cotton districts are defined as those districts with more than 10% of employment in cotton textile production in 1851.

[27]See Arnold (1864, p. 470) and Watts (1866, p. 213). Strategies to retain the local workforce in slack periods are discussed in the context of other 18th- and 19th-Century British industries in Naidu & Yuchtman (2013).

[28]The figure also illustrates that these growth-rate changes were temporary and largely disappeared by the 1871-1881 (i.e. post-cotton shortage) decade. These patterns are consistent with the

Figure 3: Changes in district population, 1851-1881




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. 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.

A second piece of evidence on migration patterns can be found by examining data on the birthplace of county residents from the Census of Population. Appendix 7.2.4 presents data showing that the number of residents of neighboring Yorkshire county who were born in Lancashire (the main cotton textile county) increased substantially from 1861-1871, consistent with a story of out-migration during the U.S. Civil War. At the same time, the number of Lancashire residents born in Yorkshire stagnated, reflecting reduced in-migration to the cotton textile areas during the downturn.

There is also evidence that this migration was concentrated among young workers. Evidence presented in Appendix 7.2.4 shows that in 1861 the cotton districts had a substantially larger share of young workers in their populations than other districts in England & Wales. This difference had largely disappeared by the 1871 census, which suggests that young workers disproportionately migrated out of the cotton textile districts during the 1861-1871 decade.

city-level experiences documented in Hanlon (2014).

Migration could have affected observed mortality patterns through several channels. First, even if migration has no direct impact on health, and does not occur selectively, it can affect observed mortality patterns purely mechanically, through the *relocation of deaths*. That is, by relocating population across districts, migration serves to relocate deaths that normally would have occurred in one location to another.[29] In this scenario, migration will reduce mortality in cotton (migrant-sending) districts and increase mortality in migrant-receiving districts, making it appear as though the cotton shortage had beneficial effects on cotton-district mortality. Moreover, if death rates are calculated using district population at the beginning of the period (or using values interpolated intercensally), then migration will mechanically affect the *observed* death rate even if the real death rate had remained unchanged.

Second, migration can affect observed mortality patterns through *migrant selection*. Even if net migration between two locations was zero, if those migrating out of a district are less healthy relative to those coming in, then we should see a fall in both deaths and the death rate in the location receiving the healthier migrants, even if the act of migration itself conferred no real health benefit.[30] This biasing effect may be even more pronounced where there is selective net out-migration from recession-stricken locations. Importantly, when migration affects observed mortality rates through either a *relocation of deaths* or *migrant selection*, some districts will gain while others will lose. As a result, we should be able to observe changes in mortality in both migrant-sending and migrant-receiving districts.

Third, migration can have a direct effect on health if people move from less healthy

[29]Such reductions in the population of cotton districts could be achieved through a reduction in in-flows to these districts, an increase in out-flows from them—or both, as suggested by the evidence provided above.

[30]More likely in our case, given the evidence presented above, was that there was out-migration from cotton districts with little countervailing in-migration from non-cotton districts. Here, if out-migrants were negatively selected (say, because poorer and more desperate cotton operatives were likelier to move), the beneficial effects of the downturn on cotton-district mortality would be overstated. For contrast, if out-migrants were positively selected (say, because wealthier and more able-bodied workers were likelier to move), then the effects would be understated.

to more healthy districts, or vice versa, a phenomenon which we call the *protective effects of migration.* In our setting, because the cotton manufacturing towns were relatively unhealthy places to live, migration was likely to have had a beneficial impact on the individual migrant's survival chances, and thus, on aggregate-level mortality.[31] In this scenario, migration-driven reductions in mortality rates reflect true improvements in health, although these "averted deaths" would accrue to non-cotton districts. In this case, we would expect the aggregate number of deaths to fall during a recession.[32]

# 4 Data

To assess the extent to which the recession generated by the cotton shortage affected health, we construct a new panel of annual district-level mortality data drawn from the reports of the Registrar General's office. These detailed data, which we digitize from hundreds of pages of source documents, include information on both the age and cause of death, and cover over 600 registration districts spanning all of England and Wales for each year from 1851-1870.[33] We also collect information from the Registrar General's reports on district population and births, which allows us to

[31]Given the limited and often incorrect medical knowledge available during this period, relocation was perhaps the most important way that people protected their health. There is evidence from as early as the 1840s that people were making their location decisions in part based on health concerns. For example, Engels (1845) mentions that wealthier residents avoided pollution exposure by avoiding living in neighborhoods that were downwind from major industrial areas. In a lecture in Manchester in the 1880s, Robert Holland described how, "[t]he rich can leave the sordid city and make their homes in the beautiful country far away from their business; the poor cannot do so. They must breathe the stifling smoky atmosphere from one year's end to another" (quoted from Thorsheim (2006, p. 44.).

[32]We would expect deaths to decrease in the districts that migrants left, while the impact in the receiving district is ambiguous.

[33]Although the data were published at several levels of geographic aggregation, we focus on the registration district-level tabulations, 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 report, digitized by Woods (1997), is reported only at the decade level, and so is insufficiently detailed for our analysis. However, Woods's data do provide a breakdown of deaths simultaneously by cause *and* age group, which we draw on for comparison when analyzing our results by age group. For an in-depth discussion of the Registrar General's data, see Woods (2000).

calculate mortality rates. The population data are based on information from the census years 1851, 1861 and 1871, while the births data were collected annually. When calculating mortality rates, we use population estimates based on linear interpolation between the census years, except for in infant and maternal mortality rates, where we use observed births as the denominator. Studying the births data also allows us to analyze how fertility responded to the recession.

We treat the period 1861-1865 as the cotton shock period in our analysis, and we focus on comparing patterns before and during the shock.[34] We do not include the post-shock period in the analysis because health in the cotton districts after 1865 was likely to have been affected by the experience of the recession.[35]

In order to establish the spatial distribution of the shock, we measure the importance of the cotton textile industry in each registration district prior to the U.S. Civil War. This is done using data from the full-count 1851 Census of Population, which includes information on occupation, by district, for every person in England and Wales. We use data from 1851 rather than from 1861 so as to avoid the possibility that our measure may be influenced by events occurring in the cotton districts in 1861. Nevertheless, we provide robustness results using occupation data from the 1861 Census.[36] Using these occupation data, we calculate the number of cotton textile workers as a share of the total working population for each district, which provides us with a cross-sectional measure of the importance of the cotton textile industry in

[34]It may be surprising that mortality effects could appear in 1861, but contemporary reports indicate effects occurring in the winter of 1861-1862. For example, the Registrar General's Report for 1861 indicates that there may have been an increase in mortality in the cotton textile districts concentrated in the fourth quarter of that year. In the summary of the fourth quarter, the Report states (p. xxxi), "The deaths in Lancashire were about 2000 more than in either of the two previous December quarters. In Manchester they were in the three corresponding periods 1743 [in 1859], 1682 [in 1860], and 2123 [in 1861]. Amongst other places in the same county that discover an increase may be mentioned Liverpool, West Derby, Wigan, Leigh, Bolton, Chorlton, Salford, Blackburn, and Preston." Almost all of the districts on that list were major cotton-producing areas.

[35]In particular, a number of public works projects focused on sanitary improvements were undertaken during the recession in the cotton districts. In addition, migration occurring during the shock period was also likely to have affected mortality patterns in the post-shock period.

[36]The location of industries is highly persistent over time, so data from 1851 provides a good indicator of local cotton textile employment throughout the study period.

each district on the eve of the shortage.

One factor complicating the use of these data is the change in district boundaries over time. To deal with this issue, we manually review the boundary changes for every district over our study period and combine any pair of districts experiencing a boundary change that resulted in the movement of over 100 people from one to the other. This leaves us with 453 consistent districts in the main analysis.[37] Summary statistics for these 453 districts appear in Appendix Table 8.

# 5 Analysis

## 5.1 Results using the standard approach

We begin our analysis by examining the mortality-recession relationship within the standard Ruhm framework, though in place of unemployment rates we use district cotton textile employment shares to infer the spatial distribution of the economic shock caused by the cotton shortage.[38] Specifically, we estimate:

$$\ln(MR_{jt}) = \beta_0 + \beta_1(COT_j * SHOCK_t) + \chi_j + \theta_t + \epsilon_{jt} \tag{1}$$

where $MR_{jt}$ is the log mortality *rate* (measured per the standard definition as deaths per 100,000 persons) in district $j$ in year $t$, $COT_j$ is a measure of the importance of the cotton textile industry in the economy of district $j$, and $SHOCK_t$ is an indicator for the shock period (1861-1865).

[37] One area where boundary changes create major issues is in a set of districts around Leeds. Ultimately, to obtain a consistent series we combine several neighboring districts into a single "Greater Leeds" district. In the Appendix we assess the robustness of our main results to excluding this district.

[38] Without accurate population denominators, unemployment rates will be biased by the presence of migration. Unfortunately, we cannot examine the extent of this bias in our setting because district-level unemployment data are not available on a consistent basis.

Our measure of cotton importance will be either the district's cotton employment share or an indicator that equals one if the district's cotton employment share is greater than 10%.[39] These interactions provide a plausibly exogenous approximation of the local economic shock caused by the cotton shortage.[40] Estimates corresponding to this specification appear in Panel A of Table 1.

Column 1 in Panel A provides results of the specification closest in form to those used in the existing literature. Here, the regressions are weighted by district population (measured in 1851), and standard errors are clustered by county. In row 1, the explanatory variable is the interaction of the cotton employment share and the shock. In row 2 we present results from a different set of regressions, in which the key explanatory variable is built using an indicator for districts where the cotton employment share was greater than 10% in 1851, rather than each district's cotton textile employment share. Under these specifications, and consistent with most existing studies of total mortality, we find a large and significant reduction in the mortality rate of cotton districts during the downturn. Column 2 provides the results of the same regression without population weighting.[41] Here, the coefficient is slightly

[39]We consider the second measure, based on a strict cutoff of ten percent, to address the possibility that there may be measurement error in the extent to which the local cotton textile employment share captures the economic effects of the cotton shock. Thus, our aim is to identify regions for which cotton production was unambiguously important to the local economy.

[40]Previous work by Hanlon (2014) shows that the interaction of a shock-period indicator and each district's 1851 cotton textile employment share is a good predictor of local economic distress, as indicated by the increase in able-bodied workers seeking Poor Law relief.

[41]Previous panel data studies typically weight their regressions using district population. When using the death rate as the dependent variable, this accounts for the increased precision with which this value is measured when it represents more underlying observations. In this study we typically do not weight our regressions. One reason for this is that we are using deaths rather then the death rate as the dependent variable. Also, because we have many districts, and these districts are relatively more similar to each other than, say, U.S. states, weighting is less valuable. We also avoid weighting because it can introduce two issues into the regressions. First, it puts considerable weight on large outliers, where effects may be different than those experienced in the average location. For example, national or local government centers may experience recessions differently than the typical town, either because of the stability of government employment or because they are more likely to receive relief. Table 10 in the Appendix shows how weighting increases the sensitivity of our coefficient estimates (but not our main findings) to the exclusion of large cities like London, Manchester, Liverpool and Leeds. A second reason to avoid weighting is that the weights themselves are estimates of the unobserved underlying values. For example, weighting regressions by initial population to

smaller in magnitude when using cotton employment share and slightly larger when using the cotton employment cutoff; nevertheless, and overall, weighting has little impact on these regressions.

In Columns 3-5 we provide results using alternative approaches to standard errors: standard errors adjusting for spatial correlation within a 25 km radius (Column 3), and standard errors adjusted for both serial and spatial correlation (Columns 4 and 5). These estimates highlight our first important finding: accounting for spatial correlation can be crucial.[42]

account for variation in the precision of estimated values ignores the fact that population changes over time, which in turn affects the precision of the observed value, but which is not reflected in the weights. If there is substantial migration, then this will increase the error with which the weights are estimated, giving too much weight to observations from districts that lost population and too little weight to districts that gained.

[42]An alternative approach to allowing spatial and serial correlation is to implement two-way clustering by a larger geographic unit. Lindo (2015) shows that this can also lead to substantially larger standard errors than those obtained by allowing for serial correlation alone. Our preferred approach is to use spatially correlated standard errors following Conley (1999) rather than two-way clustering because clustering by a larger spatial unit, such as a county, implies that two neighboring districts which fall into different counties should have uncorrelated errors. This assumption is likely unrealistic when migration occurs. The same issue is not present when using spatially correlated standard errors based on bilateral distance cutoffs.

Table 1: Estimated mortality effects of the shock using the standard approach

| | (1) Weighted by 1851 pop; SE clustered by county | (2) SE clustered by county | (3) Conley SE (25 km) | (4) Serial & spatial correction (w/ decay) | (5) Serial & spatial correction (w/o decay) |
|---|---|---|---|---|---|
| **Panel A: Dependent variable is ln(death rate)** | | | | | |
| Cotton emp. share × Cotton shortage | -0.112*** | -0.094*** | -0.094 | -0.094** | -0.094 |
| | (0.025) | (0.030) | (0.065) | (0.047) | (0.064) |
| **1**[Cotton district] × Cotton shortage | -0.031*** | -0.037*** | -0.037** | -0.037*** | -0.037** |
| | (0.008) | (0.010) | (0.019) | (0.014) | (0.019) |
| **Panel B: Dependent variable is ln(deaths)** | | | | | |
| Cotton employment share × Cotton shortage | -0.036 | 0.037 | 0.037 | 0.037 | 0.037 |
| | (0.046) | (0.061) | (0.066) | (0.043) | (0.060) |
| **1**[Cotton district] × Cotton shortage | -0.002 | 0.006 | 0.006 | 0.006 | 0.006 |
| | (0.014) | (0.030) | (0.019) | (0.011) | (0.017) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors reported in parentheses and described in column headers. N = 6795 district-year observations covering 1851-1865. Each regression includes district fixed effects and year fixed effects. The results in Column 1 are weighted by each district's 1851 population, while the results in all other columns are unweighted. Cotton districts are defined as those with a cotton employment share greater than 10%. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Death rates are measured as deaths per 100,000 persons. Population estimates from intercensal years are obtained via linear interpolation. Mortality data are from annual reports of the Registrar General. "SE clustered by county" indicates standard errors clustered by county for 55 counties. "Conley SE" allows spatial correlation for districts within 25 km of each other using a uniform kernel. "Serial and spatial correction (with decay)" allows spatial correlation across 25 km bands with a linearly decaying Bartlett kernel and serial correlation over the entire sample following Newey-West. "Serial and spatial correction (without decay)" allows serial correlation over the entire sample and spatial correlation across 25km using a uniform kernel.

Relative to standard errors clustered by the 55 counties in our data, correcting for spatial correlation nearly doubles the size of the standard errors. The comparison between the results allowing spatial correlation (Column 3) and those obtained while clustering by county (Column 2) are particularly salient given that the number of counties in our data is similar to the number of geographic units used in many existing studies, though of course our counties differ in important ways from, say, U.S. states. In addition, we can see that serial correlation appears to be much less important than spatial correlation; allowing for both serial and spatial correlation has effectively no impact on the size of the standard errors relative to allowing for spatial correlation alone. While these results are in part a function of the structure of our data, which has a broad cross section relative to its time-series dimension, a number of other studies in this literature also use data with a similar structure.[43]

Since interpolated intercensal population estimates may be suspect if migration—particularly short-term migration—took place in response to the crisis, we estimate versions of the specifications above wherein the outcome of interest is the log of the total number of deaths rather than the death rate. In these results, shown in Panel B of Table 1, the estimated impact of the shock on mortality is closer to zero and generally statistically insignificant at standard confidence levels.

The contrast between the results in Panel A and Panel B of Table 1 highlights an important issue in the analysis. Because our best intercensal estimates are simple linear interpolations between 1861 and 1871, any population changes will be averaged across each decade. If the cotton shock led to outward migration from 1861-1865, but this trend partially reversed after 1865, then interpolated population denominators will overstate the population during the 1861-1865 period, leading to understated death rates during the shock. Thus, even if the number of deaths were flat during this period, it would appear as though death rates had fallen. Correspondingly, the population estimates for the non-cotton, migrant-receiving districts would be unrealistically small, leading the mortality-rate gap to widen in both directions, in violation of the traditional assumptions necessary for causal inference. Since death rates incorporate population data that may be meaningfully inaccurate in the presence of migration, our preferred outcome variable in the analysis that follows will be the log of deaths.

## 5.2 Results accounting for migration

Figure 4 can help us better understand the source of the results shown in Table 1. In the left-hand panel of this figure, we group districts into three categories: cotton

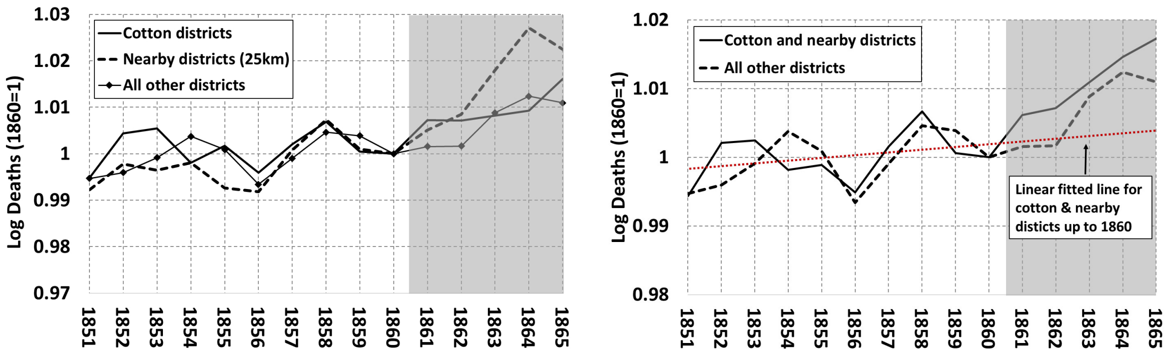
[43] Of the major studies included in our literature review in Appendix 7.1, the majority of those using the standard panel data approach are working with data that has a cross-sectional dimension that is larger than the time-series dimension.

textile districts, nearby districts within 25 km of the cotton districts, and all other districts. We have then summed the deaths in each group, taken logs, and normalized these values (1860=1). Several important patterns appear in this figure. First, prior to 1861, mortality in the cotton textile districts looks reasonably similar to patterns in the other two groups. In 1861 and 1862, we see evidence of an increase in the number of deaths in the cotton districts as well as in the nearby districts, relative to all other districts. Starting in 1863, we see evidence of large increases in deaths in the nearby districts, while deaths in the cotton districts fall back towards the level observed in the non-cotton areas. This pattern is consistent with an increase in mortality in the cotton districts in 1861-1862, as well as with migration from the cotton districts into other nearby areas.

In the right-hand panel of Figure 4, we consider what happens if we combine deaths in cotton districts with deaths in nearby districts. If it is migration from the cotton districts into nearby areas that leads to the appearance that cotton districts became healthier during the Civil War period, then aggregating the cotton and nearby areas can help us get a sense of the net effect of the Civil War on mortality. What we see in the right-hand panel of Figure 4 is that once the cotton and nearby districts are combined, it is clear that, starting in 1861, mortality in these areas was higher than what we would expect given the patterns observed in all other districts. A similar result holds if we focus just on the time series of deaths within the cotton-and-nearby-districts group. The graph includes a fitted line generated using data from the cotton-and-nearby-districts group up to 1860, and then projected forward to 1865. Starting in 1861, we can see that deaths in this group of districts lie substantially above the fitted line.

The patterns illustrated in Figure 4 suggest that migration to nearby districts may have played an important role in generating the apparent mortality improvements observed in Table 1. To account for this potential issue, we modify the standard Ruhm framework to allow for the possibility that unemployed cotton textile workers

Figure 4: Comparison of deaths in different groups of districts




These figures show the log of total deaths in each group of districts. The fitted line in the right-hand panel is based on observations in the cotton & nearby group up to 1860, but projected forward to 1865.

migrated into other nearby districts. Specifically, we estimate the following equation:

$$\ln(MORT_{jt}) = \beta_0 + \beta_1(COT_j * SHOCK_t) + \beta_2(NearCOT_j^{0-25} * SHOCK_t) + \chi_j + \theta_t + \epsilon_{jt} \quad (2)$$

where $MORT_{jt}$ is the number of deaths in district $j$ in year $t$ and $NearCOT_j^{0-25}$ is a measure of log cotton employment in districts within 25km of district $j$. The goal of including the $NearCOT_j^{0-25}$ variable in this regression is to provide a measure of the intensity of the recession caused by the cotton shortage on employment in nearby districts. Since evidence suggests that migration decayed rapidly with distance, this term will help capture the extent to which each non-cotton district was likely to have experienced in-migration as a result of unemployment in other nearby districts directly affected by the cotton shortage. The $NearCOT_j^{0-25}$ variable is calculated using,

$$NEARcot_j^{0-25} = \ln\left(\sum_{i \neq j} 1[d(i,j) < 25km] * (COT_i + 1) * 1[COTDIST = 0]\right)$$

where $1[d(i,j) < 25km]$ is an indicator variable that takes the value of one when the distance between districts $i$ and $j$ is less than 25 km and $COT_i$ is cotton textile employment in district $i$.[44] The $1[COTDIST = 0]$ component of this equation is an indicator variable that takes a value of zero for cotton textile producing districts, defined as those with more than 10% of employment in cotton textile production. We include this because we expect that the impact of the recession in nearby districts will influence migration into non-cotton districts, but that this is unlikely to influence net migration into other cotton districts.[45] Finally, because nearby cotton textile employment is zero for districts which were far from the cotton textile producing areas, we add one before taking logs. Thus, for non-cotton district $j$, the $NEARcot_j^{0-25}$ variable captures the log of total cotton textile employment in other districts near to district $j$. We also explore results with additional values for distance bands of 25-50 km and 50-75 km, as well as alternative approaches to capturing the impact of the recession in nearby areas.

The evidence presented in Section 3.2 shows that the destinations of the migration flows generated by the cotton shortage were strongly related to geographic proximity. Given this, we should expect a rise in mortality for non-cotton districts that are closer to cotton districts, an effect which should dissipate with distance. Accordingly, if cotton districts experience out-migration during the shock, then failing to include these distance terms (as was done in Table 1) will produce attenuated results. Indeed, Column 1 in Table 2 matches these predictions: $\beta_1$ becomes more pronounced, and the non-cotton districts within 25 km of a cotton district see a statistically significant rise in mortality consistent with the relocation of deaths. Moreover, there is no significant effect on mortality at further distances: in Columns 2 and 3 we add in additional distance bands, which underscore that the impact of the recession on mortality in non-

[44]To calculate the distance between any pair of districts, we collect the latitude and longitude of the main town or district seat for each district, which we call the district center. For a small number of very rural districts, we use the geographic center of the district.

[45]Contemporary evidence consistently shows that those leaving the cotton districts were not migrating to other major cotton-producing areas. See, e.g., Arnold (1864).

cotton areas is concentrated within 25 km of the cotton textile districts. The results in Columns 4-6 apply a similar approach, using a 10% cutoff for cotton employment share in place of a continuous measure, and show similar patterns.

The results presented in Table 2 reflect the key methodological contributions of this paper. First, these results show how studying mortality patterns in districts that were likely to receive any migrant outflows from the recession-stricken districts can be used to identify cases in which migration bias is present. In our setting these districts were geographically close to the affected districts.[46] Second, these results show that accounting for the impact of migration on mortality in migrant-receiving areas can substantially affect estimates of the impact of the recession on mortality more generally.[47] On this point, it is clear that a substantial and statistically significant increase in mortality in migrant-receiving districts may account for the apparent reduction in cotton-district mortality seen in Table 1. Since the results in Table 2 suggest that the migration effects are concentrated amongst districts within 25 km of cotton districts, our preferred specification going forward (unless otherwise stated) uses the log of deaths as the dependent variable, adjusts simultaneously for spatial and serial correlation, and accounts for migration within 25 km. Finally, in terms of magnitude, our preferred results, from Columns 1 and 4 of Table 2, suggest that mortality in the cotton textile districts increased by 3.1-4.0 percent, which implies

[46] In other settings factors such as transportation networks or previous migration patterns may play a larger role in determining migrant destinations.

[47] As might be expected if migration flows not only varied with distance but also affected the population denominators underlying death rates, this methodological modification does not fully eliminate the cotton-district results when the outcome variable is ln(death rates). In Table 11 in the appendix, we replicate Table 2 taking ln(death rates) as our outcome variable. While the inclusion of nearby cotton employment (interacted with the cotton shock) attenuates results, it does not fully remove statistical significance and the point estimates remain negative. The persistence of the results when using ln(death rates), even after adjusting for migration, accords with our earlier results, wherein specifications using ln(death rates) consistently find effects where specifications using ln(deaths) do not. This is likely due to the incorporation of (erroneous) population information into the calculation of death rates. That is, if migration causes intercensal population estimates to be inaccurate, then the resulting death rates are spuriously low for cotton districts during the recession, and so the *appearance* of recession-driven mortality improvements in cotton districts may survive even after removing nearby districts from the control group.

between 1,607 and 2,060 additional deaths per year.

Table 2: Results accounting for migration to nearby districts

| | Dependent variable: ln(deaths) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cotton employment share × | 0.097 | 0.102 | 0.109* | | | |
| Cotton shortage | (0.060) | (0.063) | (0.065) | | | |
| **1**[Cotton district] × | | | | 0.040** | 0.044** | 0.047** |
| Cotton shortage | | | | (0.017) | (0.018) | (0.019) |
| 0-25 km exposure × | 0.011*** | 0.010*** | 0.010*** | 0.011*** | 0.011*** | 0.010*** |
| Cotton shortage | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| 25-50 km exposure × | | 0.001 | -0.000 | | 0.001 | -0.000 |
| Cotton shortage | | (0.002) | (0.002) | | (0.002) | (0.002) |
| 50-75 km exposure × | | | 0.001 | | | 0.002 |
| Cotton shortage | | | (0.002) | | | (0.002) |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors, in parentheses, allow for spatial correlation across districts within 25 km as well as serial correlation across the entire sample. N = 6795 district-year observations occurring between 1851 and 1865. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of total cotton employment in other districts falling within each distance bin. This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

In Table 3 we explore the overall robustness of our preferred specification. Panel A presents results using a continuous measure of cotton exposure (cotton employment share) while Panel B presents results classifying cotton districts as those with a cotton employment share greater than 10 percent. The first two columns restrict the sample such that the control group of districts becomes more similar to cotton districts. Specifically, in Column 1 we drop control districts with a population density less than 0.089, the minimum density among cotton districts, so as to exclude highly rural districts; and in column 2 we drop London, Manchester, Liverpool, and Leeds, each of which was an outlier in some respect.[48] In Column 3 we balance the pre-

[48] London was by far the largest city in our data, as well as the center of government. Manchester was the main trade center for the cotton textile producers during this period, in addition to being an import manufacturing center, while Liverpool was the main cotton textile port. We explore the exclusion of Leeds because of the issues faced in generating a consistent district for this city (see discussion in Section 4). In Table 9 in the appendix, we present results dropping each of these districts separately, and find nearly identical results.

and during-shock periods of our analysis by dropping observations occurring between 1851 and 1855. Column 4 presents results where we define the shock period as 1862-65 (instead of 1861-65), since we might be concerned that the effects of the cotton shortage might not be felt immediately. In Column 5, we present results where cotton employment shares and exposure to cotton employment are based off of the 1861 census enumeration instead of the 1851 census enumeration. Finally, in Column 6, we show results using a less stringent (5%) employment share cutoff for cotton district membership. Results are generally consistent with Table 2: mortality increased in both cotton districts and nearby districts during the cotton shortage. These results largely contradict the negative and statistically significant results obtained from the standard approach (presented in Table 1), and further highlighting the importance of accounting for migration.

We have also assessed the robustness of our results to alternative approaches to measuring nearby districts. These results, presented in Table 4, are similar to those obtained using our preferred measure. In Columns 1-3, we present results in which cotton districts are those with over 10% of employment in cotton textiles in 1851. Nearby districts are then identified as those within bands of 0-25 km, 25-50 km or 50-75 km of these cotton districts; in contrast to our preferred measures, these are not weighted by cotton employment As before, we find that mortality increased in the nearby districts during the Civil War period and that this effect diminished for districts further from the cotton textile districts. One difference between the results in Columns 1-3 of Table 4 and those shown in Table 2 is that we find evidence that mortality increased in districts more than 25 km from the cotton textile districts. The explanation for this difference is that the measure used in Table 2 captures nearby cotton textile employment regardless of whether it occurs in districts with more than 10% of employment in cotton textiles. In contrast, in Columns 1-3 of Table 4, nearby districts are identified based only on their proximity to districts with more than 10% of employment in cotton textile production. Thus, districts that are within 50 or 75 km from the main cotton districts may be even closer to districts where there is some cotton textile production, but it is less than 10% of total employment.

Table 3: Assessing the robustness of the migration specification

| Dependent variable is ln(deaths) | | | | | | |
|---|---|---|---|---|---|---|
| **Panel A: Continuous cotton measure** | | | | | | |
| | (1) Population density >0.089 | (2) No London, Liverpool, Leeds, or Manchester | (3) Data from 1856-65 only | (4) Shock is 1862-65 | (5) Cotton emp. based on 1861 Census | (6) Cotton dist. defined as emp. share > 0.05 |
| Cotton emp. share × Cotton shortage | 0.102* | 0.098* | 0.072 | 0.037 | 0.020 | 0.118* |
| | (0.061) | (0.059) | (0.064) | (0.068) | (0.101) | (0.061) |
| 0-25 km exposure × Cotton shortage | 0.011*** | 0.009*** | 0.009*** | 0.011*** | 0.010*** | 0.010*** |
| | (0.002) | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) |
| **Panel B: Discrete cotton indicator** | | | | | | |
| **1**[Cotton district] × Cotton shortage | 0.042** | 0.038** | 0.032* | 0.026 | 0.010 | 0.057*** |
| | (0.017) | (0.017) | (0.018) | (0.020) | (0.018) | (0.013) |
| 0-25 km exposure × Cotton shortage | 0.012*** | 0.009*** | 0.009*** | 0.011*** | 0.011*** | 0.011*** |
| | (0.002) | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 6630 | 5405 | 4530 | 6795 | 6795 | 6795 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for spatial correlation across districts within 25 km as well as serial correlation across the entire sample. District-year observations span 1851 to 1865. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1 + total cotton employment in other districts that lie within 25km). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

In Columns 4 and 5 of Table 4 we consider two continuous measures of proximity to cotton textile employment. The first discounts more distant employment by $exp(-distance/1000)$, while the second discounts linearly. Both of these deliver qualitatively similar results to our preferred specification. Overall, the results in Table 4 suggest that our main results are not particularly sensitive to reasonable alternative measures of proximity to cotton textile employment.

Next, in Figure 5, we apply an event study approach that allows us to look at these patterns on an annual level, and also to check for evidence of pre-trends that may invalidate the analysis. These results are generated from a specification based on Eq. 2 which allows for the impact of both cotton employment within a district and nearby cotton employment to vary by year, both during the U.S. Civil War period and for several years before the war.

Table 4: Robustness to alternative measures of nearby districts

| Dependent variable is ln(deaths) | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **1**[Cotton district] × Cotton shortage | 0.010 | 0.011 | 0.013 | 0.243*** | 0.042** |
| | (0.017) | (0.017) | (0.017) | (0.052) | (0.018) |
| Cotton shortage × districts within 0-25km | 0.072*** | 0.075*** | 0.077*** | | |
| | (0.012) | (0.013) | (0.013) | | |
| Cotton shortage × districts within 25-50km | | 0.054*** | 0.057*** | | |
| | | (0.012) | (0.012) | | |
| Cotton shortage × districts within 50-75km | | | 0.034*** | | |
| | | | (0.009) | | |
| Cotton shortage × nearby cotton discounted by $exp(-distance/1000)$ | | | | 1.283*** | |
| | | | | (0.253) | |
| Cotton shortage × nearby cotton discounted by 1/distance (×10) | | | | | 2.472*** |
| | | | | | (0.304) |
| Observations | 6795 | 6795 | 6795 | 6795 | 6795 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for spatial correlation across districts within 25 km as well as serial correlation across the entire sample. District-year observations span 1851 to 1865. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865.

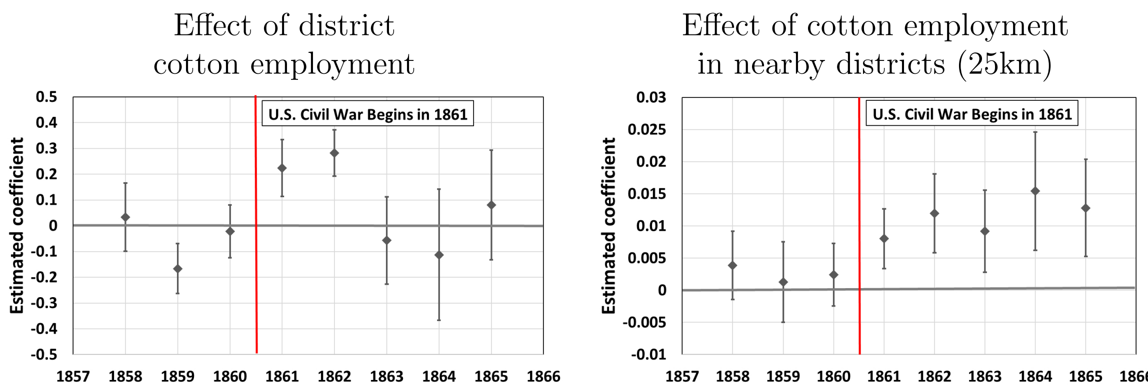
The left-hand panel shows the relationship between each district's cotton employment share and mortality in each year. Here we see evidence that the cotton shortage led to an increase in mortality in 1861 and particularly in 1862. After 1862, we see no evidence of higher mortality rates in districts that were more reliant on cotton textile production. Importantly, we observe no evidence of a clear trend in mortality in the cotton districts in the years before 1861, though the estimates are somewhat noisy. This suggests, as did Figure 4, that the parallel trends assumption is likely to be reasonable in this setting. The right-hand panel of Figure 5 plots the estimated coefficients for mortality in non-cotton districts near the cotton areas. These estimates show a clear increase in mortality in nearby districts starting in 1861 and continuing throughout the Civil War period, while there is no evidence of a pre-trend.

The patterns described in Figure 5 suggest that the onset of the recession in the cotton textile districts had the effect both of increasing mortality in 1861-62 as well as of pushing migrants into other nearby districts soon thereafter. The fall in mortality in the cotton districts after 1862 is likely due to four factors. The first is culling; if the most vulnerable individuals died in 1861-62, then those who survived into 1863 were more likely to survive thereafter, since they were drawn from the higher end of the health distribution. Second, it is possible that out-migration from cotton districts was not immediate, and that it took some time for cotton operatives to grasp the full scale of the shock and to move accordingly. Third, migration away from the cotton districts in 1861-62 meant that on a mechanical level, there were fewer people in those areas, and so, fewer people remaining to die. Fourth, starting in 1863, conditions in the cotton textile districts began improving substantially, meaning that health may have rebounded accordingly.[49] The sustained high level of mortality observed in the nearby districts is also consistent with a migration story, and particularly with the mechanical relocation of deaths. With populations growing as a result of the cotton-district out-migration that took place beginning in 1861-1862, we would expect migrant-receiving districts to have a sustained increase in mortality, as some of these additional residents died each year.

Relating the patterns shown in the left-hand panel of Figure 5 back to the estimates in Table 2, it appears that the modest and often statistically insignificant mortality increase in the cotton districts found in Table 2 is due to the combination of two key effects: first, a substantial increase in mortality in cotton districts over 1861-1862, and second, the disappearance of these effects after 1862, particularly as migrants flowed into nearby regions.

[49]For example, the Report of the Registrar General for 1863 (p. xxvii) states that, "The improvement in the cotton districts, by which forty-seven local [relief] committees have been enabled to suspend operations, has arisen from various causes: the emigration or removal of operatives, the increase of out-door work, the partial revival of industry."

Figure 5: Event study regression results




The left-hand panel shows the coefficient estimates from a term that interacts each district's 1851 cotton textile employment share with year dummies for 1858-1865. The right-hand panel shows the coefficient estimates from terms reflecting cotton textile employment within 25 km, i.e., $NearCOT_j^{0-25} * YEAR_t$ where $YEAR_t$ is a year indicator variable.

As a final check on our results, we can look at how the recession-mortality relationship varies as we move to a higher level of spatial aggregation. Given the magnitude of the economic shock generated by the cotton shortage, if this event led to a reduction in mortality in the cotton districts, as suggested by the results obtained using the standard Ruhm approach, then we should expect to see some improvement in deaths at higher levels of aggregation.[50] In contrast, if the mortality reductions found in Panel A of Table 1 were due primarily to the migration of people out of the cotton districts and into other nearby areas, then when we aggregate migrant-sending and migrant-receiving districts such that spatial spillovers are captured within the same observation unit we should observe no evidence that mortality decreased.

In Appendix 7.4 we explore these patterns at two higher levels of aggregation, looking first at counties and then at the nation as a whole. Results obtained across counties show a positive but statistically significant effect on mortality rates in cotton areas during the cotton shock. Thus, these results are consistent with those obtained

[50]The cotton districts accounted for over 10% of national deaths during this period.

at the district level while accounting for migration. When we aggregate to the national level, we also fail to find any evidence of mortality improvements during the U.S. Civil War period.[51]

### 5.3 Channels in the recession-mortality relationship

What, if any, mortality effects did the cotton shortage have, after accounting for migration bias? With our methodological adjustments in place, we now turn to the channels by which recessions may have impacted mortality in our setting. We begin by examining how mortality effects differed across the age distribution. These results are presented in Table 5. Column 1 presents the results for the log of total deaths (as before). In Column 2, we present the results for infant mortality rates. Here, unlike in the other results, we are able to examine rates—but rates which, crucially, are unaffected by migration bias or fertility change,[52] since the denominator is live births, which are directly observed annually. In the remaining columns we test how the cotton shortage affected deaths in different age bins: under 1, 1 to 9, 10 to 34, 35 to 54, and 55 and over.

There are three distinct patterns in Table 5. First, we observe evidence that there were fewer deaths among the young in the cotton districts during the recession. This is particularly true when we look at infants, for whom the confounding impact of migration bias should be minimal. Second, we see mixed results among the working-age population during the downturn. Third, there is evidence that health deteriorated among older adults. Overall, this suggests that the positive relationship between the recession and cotton-district mortality documented in Table 2 and in Figure 5 was likely driven by an increase in deaths among the older cotton-district

[51] Lindo (2015) found that results obtained using the standard approach also vary in the U.S. at different levels of aggregation. One likely explanation for this pattern is that migration bias may be influencing results, even in U.S. data, particularly when using observations that are finer than the state level.

[52] This is apart from, of course, the possibility of changes driven by selection into motherhood.

residents, particularly those over 55, which offset mortality reductions among the young. This result—that mortality increased among the oldest cohorts during the recession—accords with the findings of two modern studies on Mexico (Cutler *et al.*, 2002; Gonzalez & Quast, 2011), a setting which, among those analyzed in existing papers, is probably the most similar to the one that we consider.

It is worth contrasting the results presented in Table 5 to the results obtained when we do not account for migration. In Appendix Table 12 we present additional results looking at mortality by age category, but without accounting for migration into nearby districts. In these results, we continue to find that mortality for those over the age 55 increased during the cotton shortage period; that is, accounting for migration appears to have a relatively modest effect on the estimates for the oldest age group, perhaps because they were relatively less mobile. In contrast, when we study the 10-34 age group, we find that the results are sensitive to whether or not we control for migration, suggesting that migration may be an important factor in generating these results. Indeed, these findings make sense if we expect that many of the migrants were in the prime working-age group, while older adults were less likely to migrate in response to the recession, as is suggested by the changes in population shares shown in Appendix 7.2.4.

What channels may have contributed to these effects? Because we see different patterns for infants, the working age, and the elderly, we discuss each of these three groups in turn.

Table 5: Age-specific mortality during the recession

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | ln(Total deaths) | Infant deaths per 1000 births | ln(Deaths under 1) | ln(1 to 9 deaths) | ln(10 to 34 deaths) | ln(35 to 54 deaths) | ln(55 and up deaths) |
| **Panel A: Continuous cotton measure** | | | | | | | |
| Cotton emp. shr. × | 0.097 | -15.775* | -0.050 | 0.054 | -0.003 | 0.161*** | 0.323*** |
| Cotton shortage | (0.060) | (8.484) | (0.076) | (0.147) | (0.063) | (0.049) | (0.039) |
| 0-25 km exposure × | 0.011*** | 0.493** | 0.012*** | 0.009** | 0.010*** | 0.011*** | 0.011*** |
| Cotton shortage | (0.002) | (0.205) | (0.002) | (0.003) | (0.002) | (0.002) | (0.001) |
| **Panel B: Discrete cotton indicator** | | | | | | | |
| Cotton district × | 0.040** | -5.354** | -0.007 | 0.015 | 0.009 | 0.059*** | 0.116*** |
| Cotton shortage | (0.017) | (2.685) | (0.023) | (0.045) | (0.018) | (0.015) | (0.012) |
| 0-25 km exposure × | 0.011*** | 0.448** | 0.012*** | 0.009** | 0.010*** | 0.011*** | 0.012*** |
| Cotton shortage | (0.002) | (0.212) | (0.002) | (0.003) | (0.002) | (0.002) | (0.001) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for serial correlation across all years as well as spatial correlation between districts within 25 km. N = 6795 district-year observations occurring between 1851 and 1865. Each regression includes district fixed effects and year fixed effects. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1 plus total cotton employment within 25km of the district). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

#### 5.3.1 Infants

The decline in the infant mortality rate shown in Table 5 may result from changing fertility decisions, selective migration of mothers before birth, or direct health improvements in infants. Ultimately, the available data will not allow us to clearly differentiate between these channels. Nevertheless, it can offer some suggestive evidence on these factors.

As a starting point, we study how fertility responded during the crisis. Using regressions based on Eq. 2, but now with log births as the outcome variable, we find a positive but statistically insignificant effect on births in cotton districts during the cotton shortage (see Appendix Table 13). In nearby districts, however, we see a small but statistically significant increase in births. If the migration of mothers was selective, then it could be the case that the observed decline in the infant mortality rate (Column 2 of Table 5) simply reflects a change in the set of conditions that the average child was born into. Similarly, if there was differential selection into birth by maternal characteristics, these mortality changes may reflect a change in the composition of mothers, and so too in the downturn birth cohort's average "maternal quality" or household circumstances. Indeed, Dehejia & Lleras-Muney (2004) provide a useful discussion of how the selection of different types of women into motherhood during a recession can play an important role in determining how both fertility and the infant mortality rate responds to such an event.

It is of course also possible that the reduction in infant mortality in the cotton textile districts may have been due to real improvements in health for the children resident in those areas.[53] This channel finds some support in contemporary sources, which suggest that the substitution effects of maternal time outweighed the potential

[53]It is difficult to examine these channels in greater detail in our data. What we can do is too look at changes in mortality in causes of death that particularly affected young children, such as infectious diseases. These results, presented in Table 13 in the appendix, show no clear evidence of reductions in deaths due to either waterborne infectious diseases or infectious diseases due to other causes, both categories where deaths were concentrated among the very young.

income effects associated with job loss: mothers who were out of work breastfed for longer and were able to devote more attention to childcare. For example, in 1862 the Registrar of Wigan highlighted health improvement due to “better nursing of children” while the Registrar of Little Bolton “holds that the decrease of deaths is mainly due to a greater amount of domestic superintendence.”[54] Dr Buchanan, in his 1862 report, states that “Medical men and registrars agreed that, apart from special epidemics, the ordinary maladies of childhood have been very lightly felt up to the present time. This fact was imputed with almost equal unanimity to the greater care bestowed on infants by their unemployed mothers than by hired nursery keepers. Though the mothers, from poverty or ignorance, still feed their children very injuriously, at least the little ones are safer against death by neglect or opium.” This channel is consistent with results from Miller & Urdinola (2010), who find that in modern-day Colombia, falling international coffee prices resulted in a reduction in maternal work hours and a corresponding decrease in child mortality.

### 5.3.2 Working-age adults

The results in Table 5 suggest that deaths among the working-age population did not decrease in the cotton textile districts during the shock period. One explanation for this pattern suggested by contemporary sources is that the adverse effects of the recession on health through deteriorating nutrition and poorer living conditions may have been offset by other beneficial factors, such as reduced alcohol consumption and fewer on-the-job accidents. On the first of these effects, Dr Buchanan reports that “the parents have lost their health much more generally than the children, and particularly that the mothers, who most of all starve themselves, have got pale and emaciated” (Dec. 1862, p. 301).[55] At the same time, he reports that “Drunkenness, with the diseases and accidents produced by it, is unequivocally less in the mass of

[54] Quoted from the Report of the Registrar General for 1862, (p. xxxiii).
[55] Reports from Commissioners, British Parliamentary Papers, Feb-July 1863.

the cotton towns" (p. 304).

Consistent with this latter observation, Figure 6 provides two pieces of evidence relevant for understanding the impact of the recession among the adult working population. In the left-hand panel, we plot the number of injuries sustained in industrial accidents, which we collected from the Factory Inspector's Reports. These figures suggest that there was an overall decrease in industrial injuries during the 1861-1865 period. This reflects a direct channel through which unemployment should positively affect worker health.[56] In the right-hand panel, we provide evidence on trends in beer and liquor consumption using data from the Inland Revenue Service.[57] Consistent with contemporary reports, these indicate that beer and liquor consumption was somewhat reduced during the 1861-1865 period. These results echo modern-day studies which suggest that unemployment improves health by reducing alcohol consumption (Ruhm & Black, 2002). Also, consistent with both of these channels, we find evidence that there was an imprecisely estimated reduction in deaths due to accidents and violence in the cotton textile districts during the 1861-1865 period.[58]

Reductions in maternal mortality rates—which encompass deaths due to childbirth, metria, and puerperal fever—was yet another channel through which the recession appears to have reduced mortality among the prime-age adult population. Importantly, here, as in our analysis of the infant mortality rate, we can conduct our analysis in rates without being subject to migration bias due to the mechanical relocation of births or deaths. This is because here the appropriate denominator is annually-observed live births. In Table 13 in the appendix, we provide evidence that

[56] Existing studies, such as Gerdtham & Ruhm (2006), suggest that unemployment reduces deaths due to accidents, though much of this is due to motor vehicle accidents, which would not have mattered in our setting.

[57] The Inland Revenue Service kept careful statistics on liquor and beer production because these products were taxed during this period. It is important to take the liquor consumption figure with a grain of salt because liquor taxes were increased in 1861, which leads to the fall in that year. No similar increase occurred on taxes related to beer (which was through taxes on malt and hops). In fact, the duty on hops was reduced in 1860 and repealed in 1863.

[58] See Appendix Table 13, Column 4.

there was a large and statistically significant drop in the maternal mortality rate during the cotton shortage in the cotton textile districts as well as in other nearby districts. One likely explanation for the reduction in maternal mortality was that women were working late into their pregnancies when unemployment in the cotton textile districts was low.[59]

These results suggest that there were a number of active channels through which the recession had beneficial effects on adult health in the cotton textile districts, and that for some segments of the population, these largely dominated any adverse health effects of reduced income, poorer nutrition, and declining living standards. Some channels, such as alcohol consumption and maternal investments in infants, have been highlighted in the modern literature, while others, such as industrial accidents and maternal mortality, are more novel.

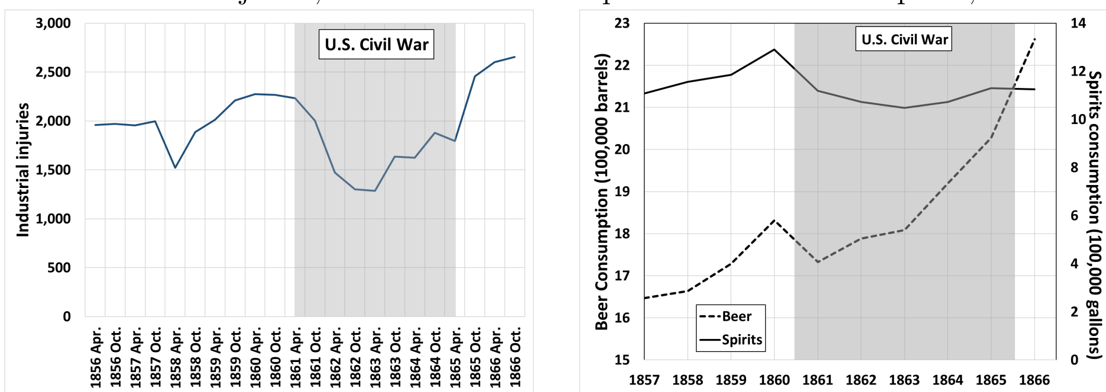
#### 5.3.3 Older adults

While younger people may have been net beneficiaries of the recession, for other segments of the population, the income effects of the downturn clearly dominated. To wit, the results in Table 5 indicate that there was a substantial increase in mortality among adults 35 and over, and particularly those 55 and over. Some contemporary reports link these deaths to the impact of the recession. For example, Dr Buchanan writes that "...while actual death from starvation has been the rarest occurrence, there is a peculiarly low state of health among the unemployed operatives of the cotton towns, showing itself particularly in the elder people, and predisposing to various

[59] Female labor force participation in these areas was high during this period. One somewhat surprising feature of these results is that infant mortality appears to have increased in districts near to the cotton districts while maternal mortality rates (that is, maternal deaths relative to births) decreased. One potential explanation for this difference is that poorer women (such as those working in the cotton factories) may have been more likely to migrate. Infant mortality was higher among the poorer classes of people during this period, so this selection could explain the rise in infant mortality in nearby districts. However, maternal mortality was *lower* among poorer women during this period (Loudon, 1986b,a), so this could also explain the reduction in maternal mortality in nearby districts.

Figure 6: Industrial injuries and alcohol consumption

Industrial injuries, 1856-1866 — Liquor and beer consumption, 1857-1866




The data in the left panel describe industrial injuries reported by the Factor Inspectors for all of England, Wales and Scotland. The data were collected from the Factory Inspector reports in the British Parliamentary Papers. The data in the right panel were collected from the *Report of the Commissioners of Her Majesty's Inland Revenue* for 1866. These are national figures.

diseases" (Dec. 1862, p. 304).[60] Among the diseases most likely to affect the elderly were those of the respiratory system, such as influenza and pneumonia. Of these, Dr Buchanan remarks that, "lung diseases of a sort to be induced and aggravated by exposure have been rife..." Consistent with these statements, in Table 13 in the appendix, we estimate that there was an increase in deaths due to respiratory causes in the cotton districts during the shock period. This respiratory category, which includes pneumonia, influenza, bronchitis and asthma, was a major killer of older adults during this period.[61]

Thus, the evidence indicates that the increase in mortality among older adults was most likely due to diminished living standards. Relative to working-age adults, we expect that the elderly were especially vulnerable to adverse income shocks; at the same time, older adults were also less likely to benefit from other changes, such as reductions in on-the-job accidents or maternal mortality, that acted to offset some of

[60]Reports from Commissioners, British Parliamentary Papers, Feb-July 1863.

[61]While the annual mortality data do not allow us to look at deaths by age and cause-of-death simultaneously, in the decadal mortality data provided by Woods (1997), we can see that from 1851-1860 roughly one third of deaths due to respiratory diseases were among those aged 55 or older, and that among this group, respiratory deaths accounted for over 17% of all mortality.

these effects among their younger counterparts. Some existing studies, such as Cutler *et al.* (2002), have also found similar evidence that health among older populations deteriorates during recessions.

## 6 Conclusion

This paper provides both a methodological and a factual contribution to our understanding of the relationship between recessions and health. Our methodological contribution consists of showing that migration undertaken in response to a recession has the potential to introduce substantial bias into estimates of the recession-mortality relationship using the standard approach—particularly if these population flows are not well measured. This bias is likely to be greater in settings, such as developing countries, where weak social safety nets induce migration in response to recessions, and where the intercensal population data used to track these movements are poor. Studies applying the standard approach in these settings are likely to generate misleading results, which may lead to poorly targeted public health responses.

We provide a simple approach that can be used to identify and deal with the influence of migration bias. Specifically, we recommend (1) focusing on deaths, rather than death rates, as the key dependent variable, since the latter measures may suffer from systematic migration-induced mis-measurement, (2) studying mortality patterns in those locations that are the most likely to receive migrants, and (3) adjusting standard errors to account for spatial correlation. Our results suggest that taking these simple steps can improve the accuracy of studies looking at the relationship between economic conditions and health, particularly in developing countries or historical settings; these improvements in accuracy can in turn can improve policy responses to these events. Furthermore, and especially when underlying populations are not well measured, these suggestions may be relevant for a broader set of literature applying panel-data methods across regions in order to analyze events that may also generate migration responses.

This study also contributes new evidence on the relationship between recessions and mortality in a historical setting. Although results using the traditional approach suggest that the cotton shortage had beneficial effects both on total mortality and on mortality across a wide range of age and cause categories, these reductions in mortality appear to be largely spurious, an artifact of migratory responses to the recession that relocated deaths mechanically. After accounting for the bias generated by the substantial migration that took place in response to the recession that we study, our results suggest that mortality actually increased. However, this overall effect masks substantial underlying heterogeneity. In particular, we find that mortality increased substantially among older adults, remained steady among working-age adults, and decreased among infants. One implication of these findings is that studies focused on just one of these groups, such as infants, may generate results that are not representative of other segments of the population, or indeed of the overall relationship between recessions and mortality.

Taken as a whole, our results contrast with the reduction in mortality during recessions documented in most of the literature following Ruhm (2000). This suggests that the recession-mortality relationship is likely to change over time, a pattern also found in recent work by Ruhm (2015). Despite these differences, there are a number of parallels between our findings and the current literature. These similarities suggest that the factors determining the recession-mortality relationship in our setting may be closer those operating in modern developing or middle-income countries than to the factors at work in the developed nations studied in most of the existing literature. Importantly, in ours as is likely in these emerging settings, migration appears to have been a crucial margin of adjustment to local economic distress.

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# 7 Appendix

## 7.1 Related literature appendix

Below, in Tables 6 and 7, we provide a review of select leading studies on the relationship between recessions and public health. In particular, we highlight the methodological approaches used, the main findings, and the setting in which these results are found.

Table 6: Review of selected related literature

| | Study | Data | Dependent variable | Specification | Standard errors | Result |
|---|---|---|---|---|---|---|
| **1** | Ruhm (2000) QJE | 50 states, 20 years | Ln(mortality rate), Ln(mortality) | Fixed effects | Robust, weighted by population | Procyclical mortality |
| **2** | Ruhm & Black (2002) J. Health Ec. | 13 years, 15-45 states (repeated cross-sections of individual-level data) | Alcohol use | Linear probability mode with state fixed effects and time trends | Clustered by state-month | Procyclical alcohol use |
| **3** | Ruhm (2003) J. Health Ec. | 20 states (31 MSAs), 10 years (individual-level data) | Various health indicators | Linear probability model with state FEs | Clustered by state | Countercyclical health |
| **4** | Chay & Greenstone (2003) QJE | 3 years, 1200 counties | Infant mortality rate | Fixed effects at the county level with state time trends | Robust, weighted by births | Recessions reduce mortality |
| **5** | Dehijia & Lleras-Muney (2004) QJE | Individual data, state level explanatory variables, 50 states, 25 years | Mothers characteristics, infant health indicators, prenatal care | Fixed effects at state level, with some state time trends | Clustered at state level, weighted and unweighted | Improved infant health during recessions |
| **6** | Neumayer (2004) Soc. Sci. & Medicine | 20 years, 11-16 German states | Ln(mortality rate), mortality by cause | Fixed effects at state level with lagged dependent variable (Arellano-Bond) | Robust, weighted by state population | Procyclical mortality |
| **7** | Ruhm (2005) J. Health Ec. | 34-45 states, 14 years (repeated cross-sections of individual-level data) | Smoking, overweight | Probit regressions | Robust, with correlation within state-month or by state | Smoking and obesity are procyclical |
| **8** | Gerdtham & Johannesson (2005) Soc. Sci. Med. | Individual-level panel data, 10-16 years | Prob. of death | Probit model, individual level with time-series explanatory variable | Robust, clustered by individual | Mortality risk countercyclical for men, unclear for women |
| **9** | Tapia Granados (2005) European J. of Pop. | 18 years, 50 provinces (Spain) | Ln(mortality rate) | Fixed effects with some province time trends | Weighted by population | Procyclical mortality |
| **10** | Gerdtham & Ruhm (2006) Ec. and Human Bio. | 23 OECD countries, 37 years | Ln(mortality rate) | Fixed effects at country level | Robust and AR1, weighted by country pop. | Procyclical mortality |
| **11** | Svensson (2007) Soc. Sci. & Medicine | 21 Swedish regions, 17 years | Heart disease | Fixed effects | Robust | Mixed results |
| **12** | Ruhm (2007) Demography | 50 states +DC, 20 years | Coronary heart death rates, all heart-related death rates | Fixed effects | Robust, AR1, weighted by population | Recessions decrease coronary mortality |
| **13** | Fishback et al. (2007) Review of Ec. and Stat. | 114 U.S. cities, 1929-1940 | Infant mortality rate, overall death rate | Fixed effects | Robust | Procyclical mortality |

Table 7: Review of selected related literature (continued)

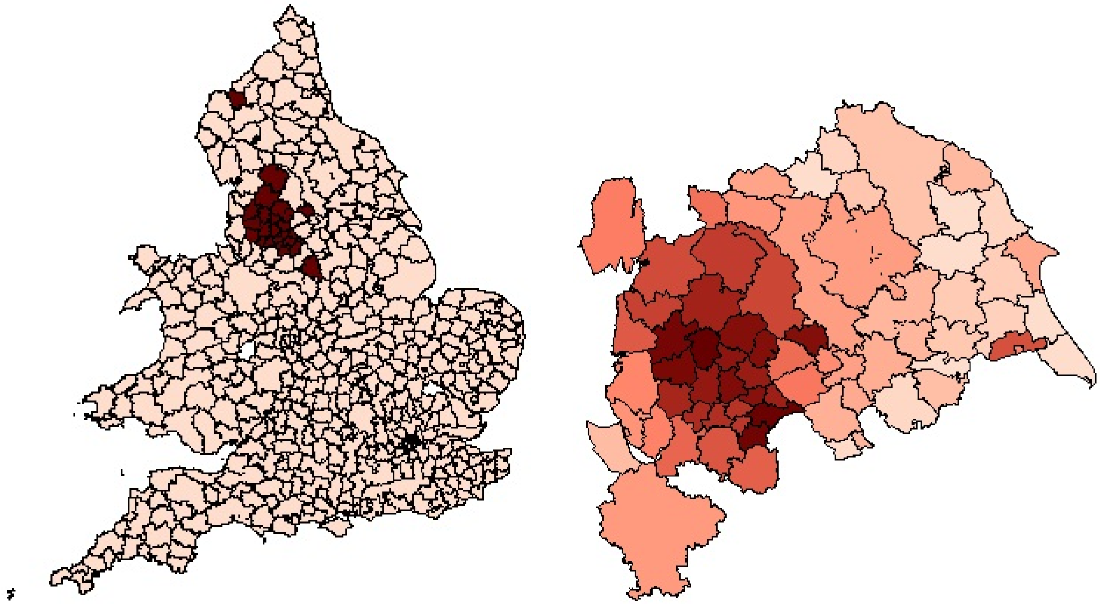
| | | | | | | |
|---|---|---|---|---|---|---|
| **14** | Edwards (2008) Soc. Sci. & Medicine | Individual level data panel data by state & year | Mortality rate | Logit regressions | | Procyclical mortality |
| **15** | Economou et al. (2008) J. of Economic Studies | 13 EU countries, 20 years 1977-1996 | Mortality rate | Fixed effects | Robust | Countercyclical mortality |
| **16** | Miller et al (2009) AER P&P | 50 states + DC, 1978-2004 | Ln(mortality rate) by group | Fixed effects Poisson at state level, some time trends | Clustered by state, weighted by state population | Procyclical mortality |
| **17** | Lin (2009) Applied Econ. | 8 Asia-Pacific countries, 1976-2003 | Ln(mortality rate) | Fixed effects, with some country time trends | Robust, weighted by population | Procyclical mortality |
| **18** | Stuckler, et al. (2009) Lancet | 26 EU countries, 1970-2007 | Mortality rate by cause | Fixed effects in differences | Clustered by country | Mixed |
| **19** | Gonzalez & Quast (2011) Empir. Econ. | 32 Mexican states, 1993-2004 | Ln(mortality rate) | Fixed effects | Clustered by state | Procyclical mortality |
| **20** | Stuckler et al (2012) J. of Epid. & Community Health | 114 cities in 36 states, 9 years | Ln(mortality rate) | Fixed effect and distributed lag | Clustered by state | Procyclical mortality |
| **21** | Ariizumi & Schirle (2012) | 10 Canadian provinces, 33 years 1977-2009 | Ln(mortality rate) | Fixed effects, with provincen time trends | Clustered by province or bootstrapped | Procyclical mortality |
| **22** | McInerney & Mellor (2012), J. Health Econ. | 50 US states from 1976-2008, Individual-level data repeated cross-sections from 1994-2008 | Ln(mortality rate) and other senior health indicators | Fixed effects with location time trends | Unclear | Countercyclical health among seniors in recent decades |
| **23** | Tekin et al. (2013) NBER Working Paper No. 19234 | Repeated cross-sections of individual-level data, 2005-2005-2011 | Variety of health indicators (reported health, smoking, etc.) | Fixed effects at the state level with some state time trends | Clustered by state-month | Zero recession-mortality relationship |
| **24** | Ruhm (2015) J. Health Ec. | 50 states, 35 years 1976-2010 (using different time windows) | Ln(mortality rate) | Fixed effects, with some state time trends | Clustered by state | Mortality becoming less procyclical recently |
| **25** | Stevens et al (2015) AEJ: Policy | 50 US states, 1978-2006 | Ln(mortality rate) by group | Fixed effects with location time trends | Clustered by state | Procyclical mortality |
| **26** | Ruhm (2015) NBER Working Paper No. 21604 | 50 US states or 3,142 US counties, 1976-2013 | Ln(mortality rate) | Fixed effects with location time trends | Clustered by state | Procyclical mortality |

## 7.2 Empirical setting appendix

### 7.2.1 Spatial distribution of the cotton textile industry

Figure 7: Geographic concentration of the cotton industry

Districts with over 10% cotton employment — Cotton employment share in core cotton region



Data for these figures are taken from the 1851 census. In the left-hand map of England and Wales, the districts which are shaded are those in which over 10% of employment is in cotton textiles. The right-hand-side map zooms in on the cotton regions of northwestern England, with districts shaded darker based on the percentage of cotton employment.

### 7.2.2 Mortality patterns in 19th Century Britain

Between 1851-1875, the relevant period for this study, the mortality rate in Britain averaged over 20 deaths per thousand residents per year, more than twice as high as modern levels.[62] The mortality rate was relatively stable during most of this period, but began dropping rapidly in the 1870s and 1880s, decades that experienced

[62]The historical mortality figures discussed in this section were constructed by the authors from the reports of the Registrar General's office. See also Woods (2000). The raw death rate in Britain in 2016 was 9.93 deaths per thousand according to the *Provisional Analysis of Death Registrations* published by the Office of National Statistics (April 7, 2016); notably, this figure pertains to a population that is much older on average than the one we study.

some of the most rapid reductions in mortality rates ever observed in Britain, due in large part to improvements in sanitation and the urban water supply (see, e.g., Chapman (2016)). The age distribution of this mortality was heavily skewed, with very high rates of infant mortality (roughly 180 per thousand in 1861). The spatial distribution of mortality was also highly unequal at this time: mortality was much higher in urban areas, with major cities experiencing mortality rates over 30 deaths per thousand, twice as high as in some rural areas.

Among the various causes of death, infectious diseases were the most important category and played a particularly large role in driving mortality among the very young. In 1861, for example, the eight major infectious disease categories accounted for 30% of all deaths.[63] These diseases were associated with poor nutrition and life in dense urban environments where diseases spread easily and water and food sources were often contaminated.[64] Most of these diseases were concentrated among the young. For example, more than two thirds of deaths from waterborne diseases (cholera, diarrhea, and dysentery) were among those under ten years old, while more than 90% of combined deaths from measles, scarlet fever, smallpox, and whooping cough were among children.[65] However, tuberculosis (TB), the largest single killer, was more likely to affect the working-age population, with over 80% of TB deaths occurring between the ages of 10 and 55.

Outside of the major infectious diseases, the next largest cause of mortality was respiratory diseases, a category composed primarily of bronchitis, asthma, and pneumonia. This category, which generated about 15% of deaths in 1861 and was rising in importance across the study period, was concentrated among the elderly and young

[63] These categories are tuberculosis, cholera, diarrhea & dysentery, typhus, scarlet fever, whooping cough, measles, smallpox, and diphtheria.

[64] Studies highlighting the importance of clean water in the 19th and early 20th century include Cutler & Miller (2005), Ferrie & Troesken (2008) and Alsan & Goldin (2014). Fogel (2004) and Fogel & Costa (1997) emphasize the role of nutrition in health as well as the interaction between poor nutrition and infectious diseases.

[65] These figures are based on data for 1851-1860 from the Registrar General's reports which were digitized by Woods (1997).

children. Another cause of death that is important in the context of our study was violence and accidents (including on-the-job accidents), which accounted for 3.4% of total mortality in 1861, a rate that was fairly stable throughout the study period. This cause of death was particularly important for the working-age population, where it accounted for over 5% of all deaths.

### 7.2.3 Responses to the cotton shortage

The response of both individuals and institutions to the recession caused by the cotton shortage played an important role in influencing health outcomes during this period.[66] Workers who found themselves unemployed responded, first, by reducing costs and dipping into any available savings, and later, by pawning or selling items of value, including furniture, household goods, clothing and bedding (Watts (1866, p. 214), and Arnold (1864)). Evidence suggests that many workers exhausted these private resources before turning to public relief—indeed, some previously proud workers were even found begging or busking on the streets (Henderson, 1969, p. 98-99). Even those who remained employed generally suffered substantial reductions in income, due to working short-time or to the substitution of Indian for U.S. cotton, a practice which slowed down production and reduced pay, which was largely based on piece rates. Finally, as we discussed in Section 3.2, many left the cotton districts in search of work in other areas.

The recession also generated an unprecedented institutional response aimed at relieving the suffering in the cotton districts. Contemporary reports largely credit public relief efforts for the fact that no widespread famine occurred during the recession.[67] Relief funds came in two main forms. First, funds were provided at the

[66] For further details on mortality patterns in Britain during this period, see Appendix 7.2.2.

[67] For example, the Registrar General's report of 1864 states that (p. xv), "that famine did not bear the fruit which in the history of nations it has too often borne, the spectacle of thousands struck by fever and death,—is mainly due to that legal provision for the poor which Christian civilization has established, and to the spontaneous munificence of a people amongst whom the seeds of charity have been liberally scattered."

local level through the Poor Law Boards, the primary system for poor relief in Britain during this period.[68] However, because Poor Law funds were associated with pauperism, provided funds for only the barest level of subsistence, and required "labour tests" such as rock-breaking, which cotton workers found demeaning, there is evidence that workers tried to avoid drawing on this stigmatized source of support (Kiesling, 1996; Boyer, 1997). The second source of funds was a large number of charitable contributions. These funds could take the form of cash, vouchers, and in-kind assistance, and came from voluntary subscriptions from across the country and even as far away as Australia (Watts, 1866). Direct relief was not the only institutional response. Additional relief programs included schools for children and adults, such as girl's sewing schools, as well as public works employment for unemployed cotton workers, though most public works employment began in 1863, after the worst of the crisis had passed.[69]

At the height of the recession in the winter of 1862, reports indicate that roughly 500,000 persons 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 the voluntary relief funds (Arnold, 1864, p. 296). The number of persons supported by public sources would fall to 264,014 by mid-summer 1863, and by 1865, the number of persons on relief fell back to where it had been at the beginning of the crisis (Arnold, 1864; Ellison, 1886).

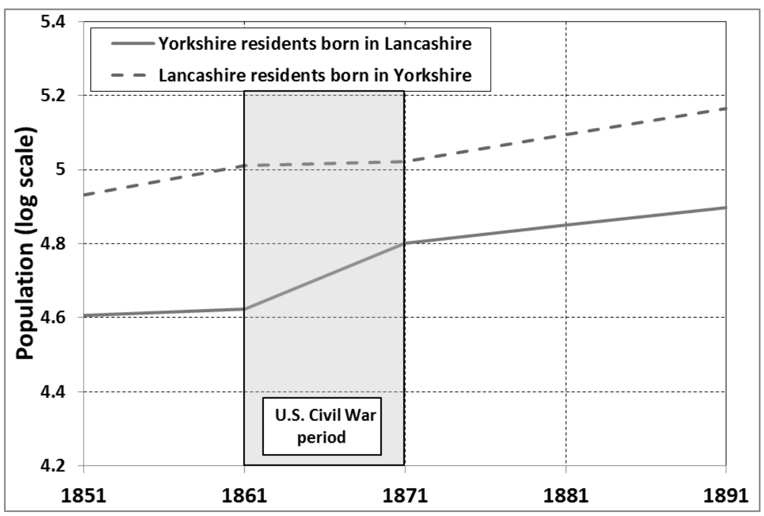
[68]These funds were provided by taxes levied on local property owners. See Watts (1866) for a description of the workings of the Poor Law Boards during the Cotton Famine.

[69]See Arnold (1864) for a discussion of public works. The availability of public works expanded substantially starting in the summer of 1863, when Parliament passed the Public Works (Manufacturing Districts) Act. This Act used the central government's borrowing authority to provide long-term low interest rate loans to municipal governments so that they could undertake needed public works projects using unemployed cotton operatives. Most of these projects were aimed at improvements to roads and water or sewer systems.

### 7.2.4 Additional evidence on migration during the shock

Figure 8, which is reproduced from Hanlon (2014), presents data from the largest cotton textile county, Lancashire, and 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 the cotton county.

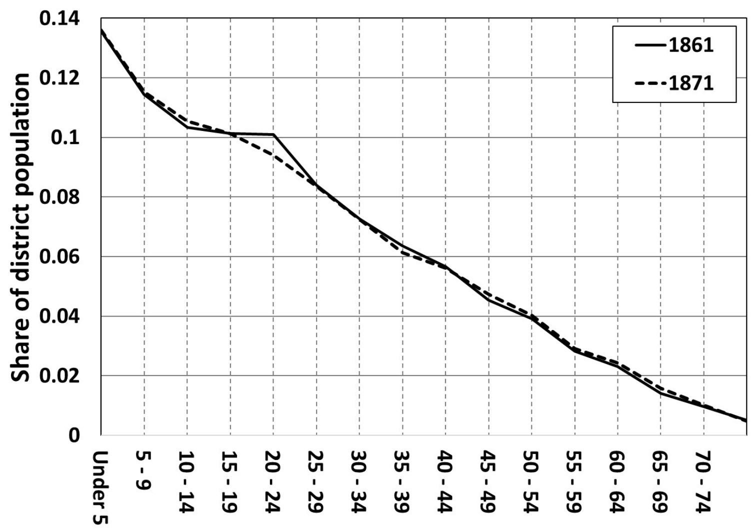
Figure 8: Evidence of migration for Yorkshire and Lancashire from birthplace data




This graph, which is reproduced from Hanlon (2014), presents data on the birthplace of county residents from the Census of Population.

Next, we consider some results that can help us think about how migration patterns varied across age groups. Figure 9 describes the share of the population in each age category up to 79 in the cotton textile districts. The most prominent feature in this graph is that there was a substantial excess of young workers in the 20-24 age group in the cotton textile districts in 1861, which had largely disappeared by 1871.

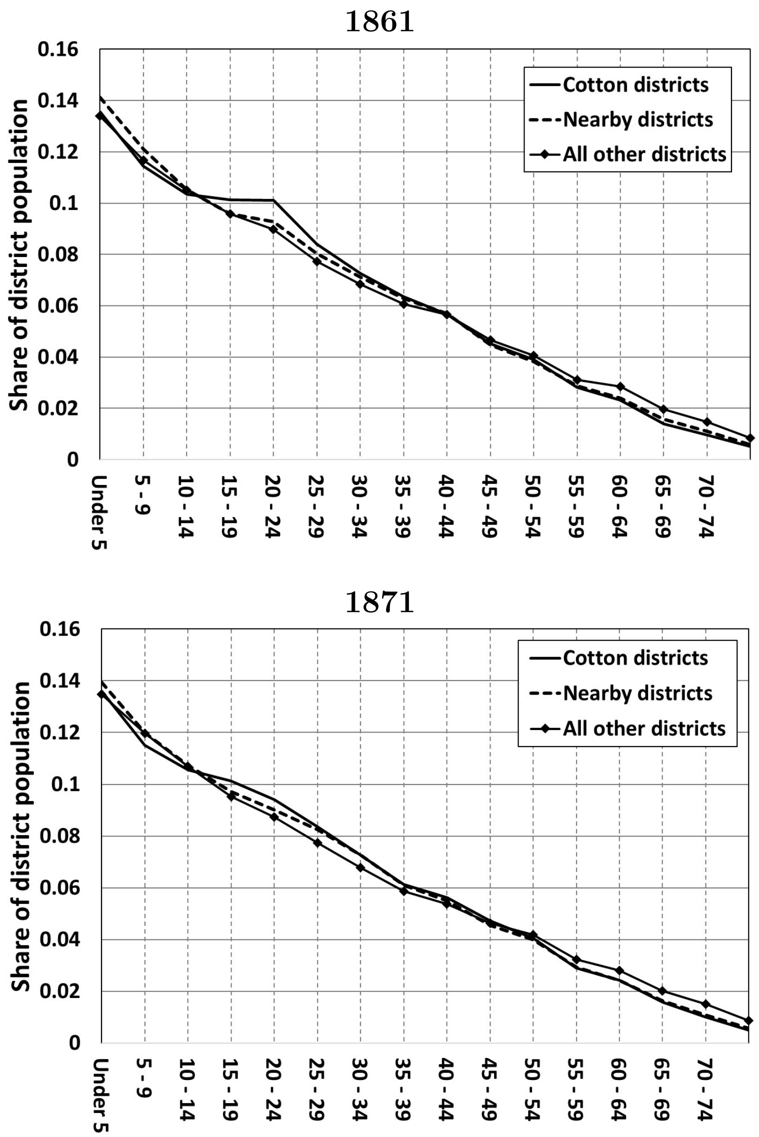
Figure 9: Share of population in each age group in the cotton districts




Population data are from the Census of Population for 1861 and 1871. Cotton districts are identified as those with over 10 percent of workers employed in cotton textile production in the 1851 Census, as in the main analysis.

An alternative view of the same pattern is provided in Figure 10, which compares the share of population by age group in the cotton districts, nearby districts, and all other districts. This is done for 1861 in the left-hand panel and for 1871 in the right-hand panel. In 1861, we can see that the cotton textile districts had a much larger share of young workers, particularly in the 15-19 and 20-24 age groups, than the other districts. By 1871, most of that difference had disappeared. It is also worth noting that in 1871, the nearby districts had substantially more population in the 25-29 and 30-34 age groups than the "all-other" districts. This pattern is consistent with the migration of workers who were in the 15-19 and 20-24 age group and living in the cotton textile districts in 1861, into nearby districts where, by 1871, they appear in the 25-29 and 30-34 age groups.

Figure 10: Share of population in each age group by type of district




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

## 7.3 Analysis appendix

### 7.3.1 Summary statistics for main analysis

Table 8 reports summary statistics for the main variables used in our analysis. These data are described in greater detail in Section 4.

Table 8: Summary Statistics

| **Panel A: Full sample of districts** | | | | |
|---|---|---|---|---|
| | (1) Mean | (2) Standard deviation | (3) Min | (4) Max |
| Average annual deaths (full sample) | 926.86 | 1279.99 | 34 | 12,209 |
| Cotton employment share (1851 census) | 0.018 | 0.07 | 0 | 0.51 |
| Nearby cotton employment (1851 census) | 4,682.10 | 20,152.38 | 0 | 143,723 |
| Population (1851 census) | 39,575.30 | 43,908.37 | 2,493 | 315,956 |
| **Panel B: Cotton districts only** | | | | |
| Average annual deaths prior to shock | 2,341.56 | 2,264.63 | 207 | 10,775 |
| Average annual deaths during shock | 2,570 | 2502.02 | 199 | 12,209 |

Full sample includes 6795 district-year observations spanning 1851-1865 for 453 unique districts. For the statistics that only draw on district-level data, there are District-level annual death data transcribed from annual reports of the Registrar General. Cotton employment share is simply the share of the total workforce (in 1851) that was employed in the cotton indsutry. Nearby cotton employment refers to the total number of workers in the 0-25 km radius of each district that were employed in the cotton industry in 1851. Nearby cotton employment is set to 0 for cotton districts (those with an 1851 cotton employment share greater than 10 percent). Pre-shock period is 1851-1860 while the shock period is 1861-1865.

### 7.3.2 Assessing the validity of our preferred specification

In Table 3 we showed that excluding London, Manchester, Liverpool, and Leeds from our sample did not affect our results. The rationale for excluding these districts in the robustness check is as follows. First, we drop London because of its size and the fact that it was the capital, which means that workers in London may have been particularly insulated from the shock, for instance, because of disproportionate employment in government. We drop Manchester because this was the largest of the cotton textile towns, and also because, unlike most of the other cotton textile manufacturing centers, it acted as a major trading center and had a more diversified

Table 9: Robustness results dropping outlier districts

| Dependent variable is ln(deaths) | | | | |
|---|---|---|---|---|
| **Panel A: Continuous cotton measure** | | | | |
| | (1) No London | (2) No Leeds | (3) No Liverpool | (4) No Manchester |
| Cotton employment share × Cotton shortage | 0.099* | 0.098 | 0.097 | 0.094 |
| | (0.059) | (0.060) | (0.060) | (0.061) |
| 0-25 km exposure × Cotton shortage | 0.009*** | 0.011*** | 0.011*** | 0.011*** |
| | (0.001) | (0.002) | (0.002) | (0.002) |
| **Panel B: Discrete cotton indicator** | | | | |
| **1**[Cotton district] × Cotton shortage | 0.038** | 0.041** | 0.040** | 0.040** |
| | (0.017) | (0.017) | (0.017) | (0.017) |
| 0-25 km exposure × Cotton shortage | 0.009*** | 0.011*** | 0.011*** | 0.011*** |
| | (0.001) | (0.002) | (0.002) | (0.002) |
| Observations | 6450 | 6780 | 6780 | 6780 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for spatial correlation across districts within 25 km as well as serial correlation across all sample years. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1+total cotton employment in other districts falling within the 0-25 km distance bin). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

economy. We drop Liverpool because the experience of that city during the U.S. Civil War was somewhat unique. As the main cotton port, Liverpool was affected by the cotton shortage, but it was also impacted by changes in the shipping industry generated by the U.S. Civil War. Finally, we explore dropping Leeds because of the data issues faced in constructing a consistent series for that district. The results below show that results are qualitatively identical when we drop these districts one at a time as opposed to when we drop them all at once.

Relatedly, in the main text we argue that one reason to avoid weighting our regressions is that this can make results sensitive to large outliers. To illustrate this point, Table 10 presents weighted regression results dropping London, Manchester, Liverpool, and Leeds.[70] While these results are all qualitatively similar to those shown in Table 9, we can see that the point estimates are sensitive to the inclusion of these

[70] Note that these results use robust standard errors, which are easier to implement when running weighted regressions. Weighting is implemented using the standard aweight option in Stata.

Table 10: Sensitivity of weighted regressions to the inclusion of outliers

| | | | | | |
|---|---|---|---|---|---|
| **Dependent variable is ln(deaths)** | | | | | |
| **Panel A: Continuous cotton measure** | | | | | |
| | (1) No London | (2) No Leeds | (3) No Liverpool | (4) No Manchester | (5) Excluding all |
| Cotton employment share × Cotton shortage | 0.052 | 0.066 | 0.050 | 0.046 | 0.063 |
| | (0.055) | (0.055) | (0.058) | (0.058) | (0.053) |
| 0-25 km exposure × Cotton shortage | 0.008*** | 0.010*** | 0.009*** | 0.009*** | 0.009*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| **Panel B: Discrete cotton indicator** | | | | | |
| **1**[Cotton district] × Cotton shortage | 0.036* | 0.043** | 0.037* | 0.038* | 0.044* |
| | (0.020) | (0.019) | (0.021) | (0.023) | (0.022) |
| 0-25 km exposure × Cotton shortage | 0.009** | 0.011*** | 0.010*** | 0.010*** | 0.010*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 6450 | 6780 | 6780 | 6780 | 6405 |

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the county-level in parentheses. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1+total cotton employment in other districts falling within the 0-25 km distance bin). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

large districts.

Table 2 highlights the importance of accounting for migration when examining the health consequences of recessions. We argue that it is important to use log(deaths) as the dependent variable because log(death rates) relies on population denominators that are incorrect in the presence of migration. In Table 11 we control for migration but take as our outcome variable the logged death rate. While controlling for migration attenuates the recession-mortality relationship, the relationship remains negative (though not statistically significant). This highlights the importance of both corrections.

### 7.3.3 Additional results by age group

Table 12 presents age-specific results using several alternatives to our preferred approach. We focus here on results for ages 10 and over because in Panels B and C we focus on mortality rates, and we do not have appropriately age-disaggregated population denominators for children under 10. Panel A presents results with log deaths as

Table 11: Mortality rates accounting for migration

| | ln(death rates) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cotton employment share × Cotton shortage | -0.081 | -0.072 | -0.067 | | | |
| | (0.064) | (0.066) | (0.067) | | | |
| **1**[Cotton district] × Cotton shortage | | | | -0.031 | -0.028 | -0.026 |
| | | | | (0.019) | (0.020) | (0.020) |
| 0-25 km exposure × Cotton shortage | 0.002* | 0.002 | 0.002 | 0.002 | 0.001 | 0.001 |
| | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) |
| 25-50 km exposure × Cotton shortage | | 0.001 | 0.001 | | 0.001 | 0.000 |
| | | (0.002) | (0.002) | | (0.002) | (0.002) |
| 50-75 km exposure × Cotton shortage | | | 0.001 | | | 0.001 |
| | | | (0.002) | | | (0.002) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for spatial correlation across districts within 25 km as well as serial correlation across the entire sample. N = 6795 district-year observations occurring between 1851 and 1865. Each regression includes district fixed effects and year fixed effects. Cotton districts are defined as those with a cotton employment share greater than 10 percent. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1+total cotton employment in other districts falling within each distance bin). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

the dependent variable, but ignores migration into nearby districts. Panel B presents results with the log death rate as the key dependent variable. Panel C presents results with log deaths as the dependent variable, while also accounting for migration into nearby districts.

### 7.3.4 Additional results exploring the channels

Table 13 provides some additional results related to our discussion of the channels through which economic conditions affected mortality in our study. In the first column, we investigate how the number of births responded to the recession. Here we find no statistically significant evidence that births decreased in the cotton districts, and evidence of a small increase in births in nearby areas. In Columns 2-3, we investigate two groups of diseases that were particularly important for children. The first, waterborne illnesses, includes diarrhea, dysentery, and cholera. The second, infectious diseases, includes smallpox, measles, scarlet fever, and whooping cough. The infectious disease category was particularly concentrated among children; in the decadal mortality data from Woods (1997), 92.5% of all deaths from the infectious

Table 12: Age-specific mortality during the recession under alternative specifications

| **Panel A: ln(deaths) patterns ignoring migration** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Total | 10 to 34 | 35 to 54 | 55 and up |
| Cotton employment share × Cotton shortage | 0.037 | -0.057 | 0.103** | 0.260*** |
| | (0.060) | (0.063) | (0.049) | (0.038) |
| **Panel B: ln(death rates) patterns ignoring migration** | | | | |
| Cotton employment share × Cotton shortage | -0.094 | -0.182*** | -0.067 | 0.097*** |
| | (0.064) | (0.067) | (0.048) | (0.036) |
| **Panel C: ln(death rates) patterns accounting for migration** | | | | |
| Cotton employment share × Cotton shortage | -0.081 | -0.173** | -0.053 | 0.123*** |
| | (0.064) | (0.067) | (0.049) | (0.036) |
| 0-25 km exposure × Cotton shortage | 0.002* | 0.002 | 0.003 | 0.005*** |
| | (0.001) | (0.002) | (0.002) | (0.001) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for serial correlation across all years as well as spatial correlation between districts within 25 km. N = 6795 district-year observations occurring between 1851 and 1865. Each regression includes district fixed effects and year fixed effects. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1+total cotton employment in other districts falling within 25km). This variable is set to zero for the cotton districts. Mortality data are from annual reports of the Registrar General.

diseases included in this category occurred among children under 10. Neither of these categories show clear and statistically significant results in the cotton districts.

In Columns 4-6, we consider cause-of-death categories that were more important for working-age adults. In Column 4, we can see that deaths due to accidents and violence, which includes industrial accidents, fell in cotton districts during the shock period, though this reduction is not statistically significant. Columns 5-6 provide evidence that maternal mortality also fell.

Finally, Column 7 describes death due to respiratory causes, a category that includes bronchitis, pneumonia, influenza, asthma, and other related diseases. These diseases were a particularly important killer among the elderly population. Here we find evidence that deaths due to these diseases increased in the cotton districts during the cotton shortage period.

Table 13: Additional results describing channels of the effects

| | Ln(births) | Ln(deaths) due to waterborne diseases | Ln(deaths) due to infectious diseases | Ln(deaths) due to violence & accidents | Ln(deaths) due to childbirth | Maternal deaths per 100,000 births | Ln(deaths) due to respiratory diseases |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Cotton employment share × Cotton shortage | 0.041 | 0.157 | 0.155 | -0.133 | -0.148 | -188.903** | 0.345*** |
| | (0.036) | (0.163) | (0.311) | (0.099) | (0.188) | (83.939) | (0.092) |
| 0-25 km exposure × Cotton shortage | 0.006*** | 0.012** | 0.008 | 0.010*** | 0.002 | -7.096** | 0.009*** |
| | (0.001) | (0.006) | (0.009) | (0.003) | (0.005) | (2.797) | (0.003) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, in parentheses, allow for serial correlation across all years as well as spatial correlation within 25 km. N = 4530 district-year observations occurring between 1856 and 1865. Each regression includes district fixed effects and year fixed effects. Cotton famine is an indicator equal to 1 for the years 1861 through 1865. Nearby cotton exposure is calculated as the log of (1+total cotton employment in other districts that fall within 25km). Mortality data are from annual reports of the Registrar General.

#### 7.3.5 Synthetic control results

Table 2 in the main text establishes the distance bands for which spatial spillovers exist (namely, the 0-25 km band), and thereafter excludes the districts falling within these bands from the set of control districts. In Table 3 we show that this specification is robust to a variety of sample restrictions aimed at obtaining a more comparable control group.

In this section we take a more agnostic approach to the choice of comparators, by adopting a modified synthetic control strategy.[71] Specifically, we estimate for each district, after excluding from the synthetic control donor pool any cotton district or non-cotton district within 25 km of a cotton district, the difference between the effect of the cotton shortage on the log of total deaths for that district and the effect of the shock on the district's synthetic control counterpart. The inputs to the analysis are annual births, annual deaths by age and gender, annual deaths by granular cause, population in 1851 and 1861, population density in 1851, and the following occupational structure measures in 1851: the employment-to-population ratio, the share of employment in cotton, the share of employment in other textiles, and the share of employment in agriculture.

This approach appears to confirm our original choice of control and excluded districts: in nearly every case, the distribution of treatment effects of the cotton shortage for cotton districts falls further left of the distributions for placebo districts (i.e. districts within 25 km, and all other districts), and the treatment effects on non-cotton districts within 25 km of cotton districts are markedly positive. Figure 11 shows the distributions of treatment effects on the log of total deaths by district type for 1861, 1862, 1863, 1864, and 1865; on the left-hand side, the values are normalized

[71] We also consider conceptually similar matching strategies. Although we are unable to pursue propensity score matching approaches due to the data's violation of the overlap assumption, nearest neighbor approaches (adopting a specification similar to that used in the synthetic control analysis, and averaging district characteristics over the pre-treatment period) indicate a negative but insignificant effect of the cotton shortage on the log of total deaths in cotton districts in each of the shortage years, consistent with our main findings (not reported).

by the value in the last pre-treatment year, 1860,[72] while on the right-hand side, we present the raw values. Figure 12 plots the mean of yearly treatment effects for each group of districts, with normalized values on the left and raw values on the right.

## 7.4 Results at higher levels of aggregation

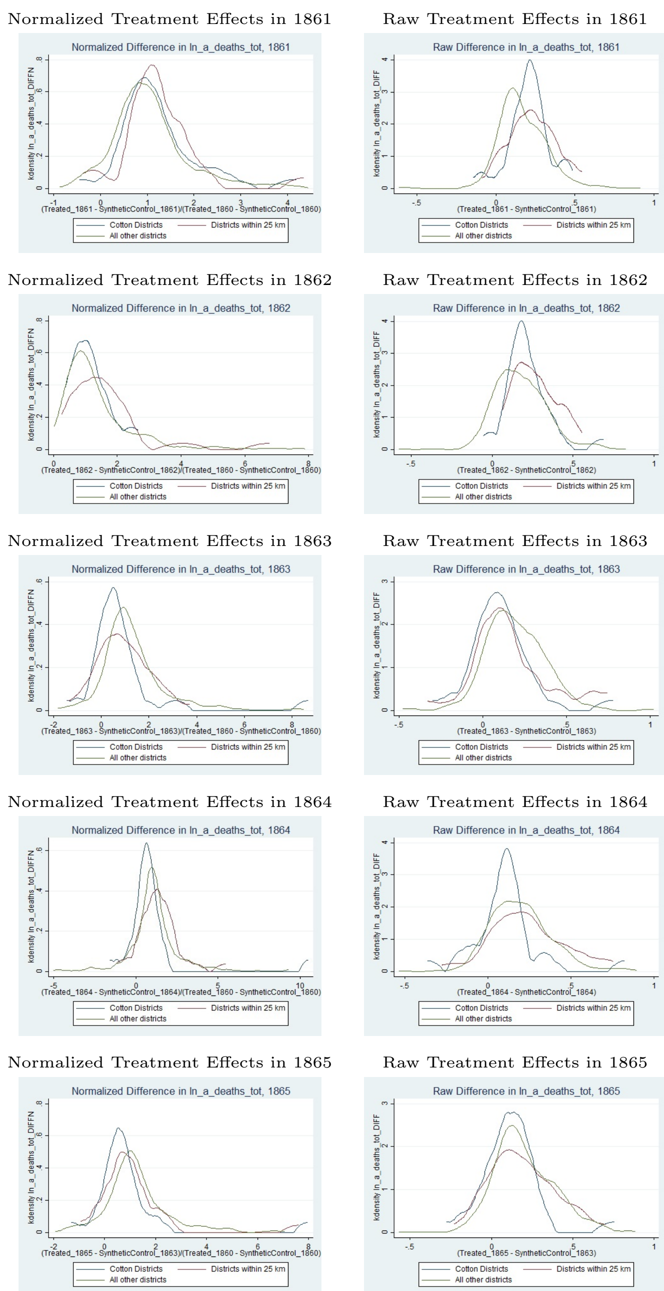
As a second check on our results, in this appendix we look at whether our findings continue to hold when we look at higher levels of aggregation. As we move to higher levels of aggregation, such as the county or national level, both migrant-sending and migrant-receiving locations will generally fall within the same unit of observation, thus capturing short-distance spatial spillovers. As a result, this offers an alternative approach to dealing with the impact of migration bias.

We begin, in Table 14, by looking at the results at the county level. In this table, we replicate the results presented in Table 1, but using observations aggregated to the 55 counties available in the data. This aggregation allows for much of the migrant out-flows and in-flows to net out, since recession-induced migration appears to have taken place largely over very short distances. The results in Table 14, particularly those in Columns 3-4 where we use the log of deaths as the outcome, show that the recession was associated with an *increase* in mortality in the cotton textile districts. Thus, these findings are consistent with the results that we obtain at the district level when accounting for migration flows.

Next, in Figure 13 we aggregate mortality to the national level. Because the cotton textile districts accounted for more than 10% of total mortality in England & Wales during this period, it is reasonable to expect that any substantial improvements in health in these districts, of the like suggested by the standard Ruhm-style results presented in Panel A of Table 1, could show up in national statistics. However, the

[72] We normalize these values to abstract from consistent level differences resulting from the somewhat poor quality of the synthetic matches, which generally follow the trends present in the treated districts but exist at different levels.

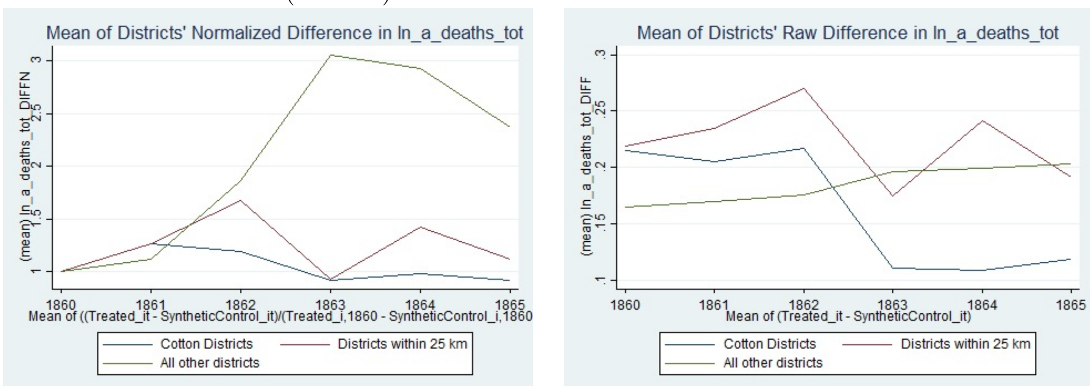
Figure 11: Distribution of Treatment Effects by District Type




The figure presents the distribution of treatment effects on the log of total deaths by district and treated year as estimated by synthetic control methods. The values in the panels on the left-hand side have been normalized by the value of the treated district-synthetic control district difference in 1860, while those on the right-hand side present the raw differences.

Figure 12: Trends in Treatment Effects by District Type

Normalized (1860=1) | Raw



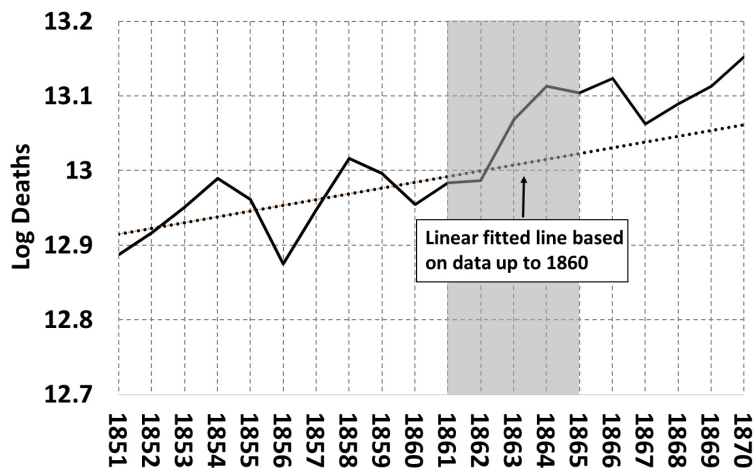

The figure presents, by district type and over the course of the cotton shortage period, the average of district-level treatment effects on the log of total deaths as estimated by synthetic control methods. In the left-hand side panel, the values have been normalized by the value in 1860, while those on the right-hand side present the raw differences.

Table 14: Estimated mortality effects of the shock at the county level

| | **ln(death rate)** | | **ln(deaths)** | |
|---|---|---|---|---|
| | (1) Weighted by 1851 pop; SE clustered by county | (2) SE clustered by county | (3) Weighted by 1851 pop; SE clustered by county | (4) SE clustered by county |
| Cotton employment share × Cotton shortage | 0.054 | 0.055 | 0.259*** | 0.330*** |
| | (0.044) | (0.098) | (0.080) | (0.110) |
| **1**[Cotton county] × Cotton shortage | 0.008 | 0.007 | 0.048*** | 0.060*** |
| | (0.007) | (0.009) | (0.014) | (0.011) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are described in the column headers and are reported in parentheses. N = 825 county-year observations covering 1851 and 1865. Each regression includes county fixed effects and year fixed effects. The results in Column 1 and 3 are weighted by 1851 population. Cotton counties are Cheshire and Lancashire. Cotton shortage is an indicator equal to 1 for the years 1861 through 1865. Death rates are measured as deaths per 100,000 persons. Population estimates from intercensal years obtained via linear interpolation. Mortality data from annual reports of the Registrar General. Clustered SE indicates standard errors clustered by county for 55 counties.

Figure 13: Trend in total deaths for all England & Wales




This figure shows the log sum of deaths in all districts in England & Wales. The fitted line is based on observations up to 1860, but projected forward to 1865.

patterns shown in Figure 13 provide no evidence that mortality at the national level improved during the years of the U.S Civil War.