# Estimating the Recession-Mortality Relationship when Migration Matters*

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**Abstract**

A large literature following Ruhm (2000) suggests that mortality falls during recessions and rises during booms. This relationship, however, tends to be analyzed within a panel-data framework that implicitly assumes either that local economic shocks do not induce migration, or that insofar as they do, these movements are accurately reflected in intercensal population estimates. In this paper, we argue that unobserved migratory responses have the potential to bias results. To study the extent of this bias, we draw on two natural experiments: the recession in cotton textile-producing regions of Britain during the U.S. Civil War and the Appalachian coal boom that followed the OPEC oil embargo in the 1970s. In both settings, we find evidence of a substantial migration response. Furthermore, we show that estimates of the business cycle-mortality relationship obtained using the standard approach are highly sensitive to assumptions about both the accuracy of interpolated population values and the short-run relationship between population and mortality. We also show that control regions may be indirectly affected by migration into or away from treatment regions, leading to unobserved treatment spillovers. Together, our findings suggest that, when left unaddressed, large migratory responses can meaningfully undermine inference. Once we adjust for migration, we find no evidence that the coal boom substantially affected mortality, and we find that mortality *increased* during the cotton recession.

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# 1 Introduction

How do business cycles affect mortality? This question has attracted substantial attention following an influential study by Ruhm (2000), which found that mortality rates in the U.S. fall during recessions and rise during booms. Methodologically, Ruhm compared state-level unemployment rates to state-level total mortality rates. This panel-data approach has now been applied to a wide variety of settings in developed and developing countries, both modern and historical. The majority of studies in this literature yield consistent results: mortality appears to be pro-cyclical.[1]

One critical assumption embedded in this empirical approach is that either there is no large migration response to business-cycle fluctuations, or that short-run population flows are accurately captured by intercensal population estimates.[2] If this assumption fails, say, because unobserved migration changes either the size or the composition of an affected location's at-risk population, then we may observe a spurious change in the observed mortality rate which we will misinterpret as reflecting the true impact of business cycles on health. We call this phenomenon *migration bias.* Migration also raises two further concerns. First, migration may cause unemployment rates, the key explanatory variable used in most of the studies following Ruhm (2000), to become endogenous.[3] Second, since business cycle-induced migration is likelier to occur between treatment and control locations, rather than within them, treatment spillovers across migrant-sending and migrant-receiving regions have the potential to

[1]We review this literature in Appendix A.1. Notable exceptions which find either mixed health effects or evidence of counter-cyclical mortality include Svensson (2007), Economou *et al.* (2008), and Miller & Urdinola (2010).

[2]This potential issue is mentioned in Ruhm (2007), Stuckler *et al.* (2012) and Lindo (2015), but we are not aware of a study that assess the extent to which migration may affect the results in this literature.

[3]This is because unemployment rates both affect and are affected by migration. That is, a change in the unemployment rate may induce migration, and this migration will in turn affect both the numerator and denominator used to calculate unemployment rates in the next period. Similarly, unemployment rates may affect and be affected by patterns in mortality and fertility, patterns which may also be influenced by migration. The endogeneity of unemployment rates has been raised as a concern in Miller & Urdinola (2010).

bias coefficient estimates obtained in panel-data regressions.

In light of these issues, the aims of this study are twofold: first, to assess the migration response to short-run changes in economic conditions; and second, to examine the impact of such migration on estimates of the relationship between business cycles and mortality.

To do this, we draw on two empirical settings in which we can observe the local incidence of an economic shock without relying on measures, such as the unemployment rate, which drive and are endogenously affected by migration. The first setting is the large recession in the cotton-textile producing regions of Britain caused by the U.S. Civil War (1861-1865), an event which sharply reduced the supply of raw cotton.[4] The second setting is the boom in the coal-producing counties of the Appalachian U.S. over the period 1970-1977, an event precipitated by the OPEC oil embargo and national regulatory changes.[5] Both settings were chosen because they offer plausibly exogenous variation in the timing and spatial distribution of short-term economic shocks, allowing us to more cleanly identify changes in local economic conditions.[6] In both cases, the temporal component of the economic shock was short, sharp, and generated by outside forces that were largely unexpected. Meanwhile, the spatial incidence of each shock was determined by the pre-existing distribution of economic activity, which was in turn due to underlying natural endowments.[7] Importantly, migration was not a consideration in the choice of these settings.

We make three primary contributions in this study. First, we show that local eco-

[4]This setting has previously been studied by Hanlon (2015) and Hanlon (Forthcoming), which examined outcomes related to technological change and urban growth, respectively.

[5]This setting has previously been studied by Black *et al.* (2002), Black *et al.* (2005), and Black *et al.* (2013), which investigated outcomes related to social welfare provision, local economic performance, and fertility, respectively.

[6]This approach follows work by Miller & Urdinola (2010).

[7]In the case of the coal boom, the key natural endowment was coal reserves. In the case of the cotton shortage, work by Crafts & Wolf (2014) shows that the spatial distribution of the cotton textile industry in the 1830s was driven by factors including the availability of water power sources, rugged terrain, and access to a natural port. There was strong persistence in industry location, so that the locations where the industry was concentrated in the 1830s remained the main centers of the industry in the 1860s.

nomic shocks can induce substantial and systematic migration responses. In the case of the cotton shortage, we observe evidence that workers left cotton textile-producing areas during the recession, settling temporarily in nearby non-cotton locations. In the case of the coal boom, we observe that the pattern of out-migration from Appalachia in the decades prior to the 1970s saw a short-lived reversal during the boom decade of 1970-1980. In this setting in particular, migration appears to have been highly selective, with boom-time population changes driven by the return of healthy working-age adults.

Second, we provide evidence that the estimated relationship between business cycles and mortality is highly sensitive to modeling assumptions related to migration. In particular, we show that the standard approach can produce results that are unstable—and even misleading—in the presence of migration. In our cotton setting, for instance, the standard approach produces estimates that are opposite in sign to those obtained under specifications that more fully account for migration bias. This is because the standard estimating equation introduced by Ruhm (2000) embeds important assumptions about the accuracy of intercensal population estimates and the relationship between population and mortality in the short run. To be more specific, by using the mortality rate as the dependent variable, the Ruhm approach implicitly assumes a one-to-one relationship between changes in population and those in mortality. This assumption is likely to be violated when migration has taken place—for instance, if migration is selective, if it causes intercensal population to be poorly measured, or if it induces congestion effects.

Accordingly, we use three strategies to diagnose and account for the impact of migration on estimates of the relationship between business cycles and mortality. First, we propose a set of alternative estimation strategies, each embedding different assumptions about the relationship between mortality and short-run changes in population. Comparing the results obtained from these alternative approaches shows that estimates of the business cycle-mortality relationship depend heavily on the choices made to model migration. Second, we use the available evidence on the migration response in each setting in order to choose between these alternative approaches and the underlying assumptions they imply. Third, we validate our choice of modeling assumptions by generating additional results using windows around census years, when population estimates are more accurate and less subject to migration bias.

Our final contribution is to provide substantive evidence on the relationship between business cycles and mortality in two very different environments. In the case of the cotton shortage, our results after adjusting for migration bias suggest that mortality *increased* during this recession. In the case of the Appalachian coal boom, there is evidence that population sorting through migration, with young, healthy workers migrating temporarily to coal areas during the boom, had a substantial impact on the *observed* mortality rate. We find little evidence, however, that the Appalachian coal boom had any real effect on underlying population mortality. Strikingly, neither our cotton nor our coal findings are consistent with the pro-cyclical mortality results obtained in most of the existing literature.

Together, our results suggest that migration bias is an important concern, and that it may cause the standard panel-data approach used in the literature to generate meaningfully inaccurate estimates of the relationship between business cycles and mortality. One approach to dealing with these effects is to focus on specific shocks, assess the magnitude and direction of migration bias, and to use additional information on those migratory responses to make empirical choices that will mitigate this bias. An alternative approach to dealing with migration bias—but one that is only possible in settings with unusually rich data—is to use individual-level panel data. This approach has been applied in Sweden by Gerdtham & Johannesson (2005) and in the U.S. by Edwards (2008). Notably, these papers find mixed or counter-cyclical effects. Together with our results, the findings in these studies suggest that accounting for migration bias may substantially change our understanding of the relationship between business cycles and mortality. Although studies using individual-level panel data may be less vulnerable to migration issues, current data constraints mean that this approach cannot be widely applied—for instance, in developing-country settings or over longer periods of time, where we may nevertheless wish to study the relationship between business cycles and mortality.

In addition to the large literature following Ruhm (2000), which we describe in more detail in the next two sections, our results are also related to recent work by Cutler *et al.* (2016), which uses national cohort data to study the impact of recessions on mortality on a cross-country basis. Because migration is more difficult across national borders, this approach, like the individual panel-data ones above, is less likely than other studies in this literature to suffer from migration bias. Indeed, Cutler *et al.* (2016) find mixed evidence of pro-cyclicality as well: large recessions are bad for health, while small ones may improve health. Given that the cotton shortage we study was a very large shock, our results upon adjusting for migration appear to be consistent with their findings. Finally, our emphasis on the statistical impact of migration on estimates of health is related to work emphasizing the impact that migration can generate when panel-data approaches are applied to answer other questions. For example, Borjas (2003) and Borjas (2014) provide evidence that migration responses can be an important source of bias in estimates of the impact of foreign migrants on local labor markets.

In the next section, we briefly describe the mechanisms through which local economic shocks may affect health. Section 3 describes the impact that migration can have on observed mortality rates, while Section 4 describes the empirical approach that we will use to investigate these issues. We illustrate the impact of migration bias empirically in our cotton shortage and coal boom examples, which are presented in Sections 5 and 6, respectively. Section 7 concludes.

## 2 Business cycles and health

The existing literature highlights a number of channels through which booms or recessions can affect mortality rates. For example, recessions may improve health by removing individuals from environmental and work-related hazards such as pollution, traffic accidents, and on-the-job injuries (Muller, 1989; Chay & Greenstone, 2003); by freeing up time for breastfeeding, childcare, exercise, and other salutary activities (Dehejia & Lleras-Muney, 2004; Ruhm, 2000); by raising the quality of elder-care (Stevens *et al.*, 2015); and by limiting the capacity for unhealthy behaviors such as smoking and alcohol use (Ruhm & Black, 2002; Ruhm, 2005). On the other hand, the negative income shocks associated with recessions may compromise access to proper nutrition, shelter, and medical care (Griffith *et al.*, 2013; Painter, 2010).[8] Job loss, in particular, may cause psychological stress, raising rates of suicide and risky behavior (Eliason & Storrie, 2009; Sullivan & von Wachter, 2009). Accordingly, the net effects of business cycles on mortality are ambiguous ex ante.

## 3 Migration and the standard approach

Despite the variety of channels through which business-cycle fluctuations might impact mortality, studies applying the standard panel-data approach introduced by Ruhm (2000) have consistently found evidence that, on net, health improves during recessions, although there is some evidence that this relationship may have weakened in recent years (Ruhm, 2015).[9] A review of leading papers in this literature is provided in Appendix A.1.

Typically, studies in this literature apply an estimating equation such as,

[8]In the case of many of these mechanisms, we would expect the opposite effect during boom times, such that health worsens.

[9]Although we confine our discussion here to studies of total mortality, we acknowledge that a number of developing-country studies find evidence that recessions increase infant mortality.

$$\ln(MR_{it}) = \beta E_{it} + X_{it}\gamma + \phi_i + \eta_t + \epsilon_{it} \quad (1)$$

where $MR_{it}$ is the mortality rate in a given location (e.g., state) $i$; $\eta_t$ and $\phi_i$ are a full set of time-period and location fixed effects; $E_{it}$ is the location's unemployment rate (or another similar variable representing local economic conditions); and $X_{it}$ is a set of controls.

There are three channels through which migration can affect estimates generated using Eq. 1. First, migration may affect the key explanatory variable, $E_{it}$. Second, migration can affect the dependent variable $MR_{it}$, which includes population in the denominator. Third, migration spillovers across locations may affect results through the comparison, implicit in Eq. 1, between treated and control locations. Below we discuss each of these potential channels in more detail. Before doing so, it is worth noting that some of these channels reflect a true effect of migration on mortality rates, while in other cases, migration affects estimates of the mortality rate without having any effect on the true mortality rate. Below we will differentiate between channels that represent real versus purely statistical effects.

One channel where migration can affect the results of Eq. 1 is through the $E_{it}$ term. As pointed out in previous work (Miller & Urdinola, 2010), the measures of $E_{it}$ typically used in the literature, such as the unemployment rate, may be endogenous. This is particularly true if migration is an important factor: for instance, if unemployed workers leave an area to obtain employment elsewhere, then this migration will not only respond to the local unemployment rate, but it will also directly affect the local unemployment rate.[10] One solution to this issue, which we adopt, is to study contexts in which we can observe plausibly exogenous variation in the timing and spatial distribution of local economic shocks.

[10] Furthermore, it will affect the unemployment rate in both migrant-sending and migrant-receiving locations, which will compound the migration bias through spillover effects, which we will discuss later.

Migration can also affect estimates obtained from Eq. 1 through the interpolated annual population variable, which appears directly in the denominator of the mortality rate term, and which can also implicitly bias the numerator. In particular, using the mortality rate as the dependent variable in Eq. 1 involves an implicit assumption that mortality scales one-for-one with population.[11] Migration can cause this assumption to fail in several ways.

First, if short-run population changes are not accurately captured by intercensal population estimates, we should not expect a one-for-one mortality response to changes in (estimated) annual population. This is a purely mechanical phenomenon that arises from the fact that while the mortality-rate numerator (deaths) is observed annually, direct measures of the denominator (population) are not available outside of census years. Indeed, in nine out of every ten population observations, these denominator values must be constructed, introducing room for error. Typically, population in a non-census year is interpolated using observed population counts from two censuses, as well as data on births and deaths occurring in the interim. In data from modern developed countries, additional sources are used to help capture that portion of migration which can be observed.[12] However, as we discuss in Appendix A.2.2, even in modern developed countries there remains some unobserved migration that must be allocated across intercensal years. This means that while deaths in the (annually-observed) numerator may increase or decrease as a function of the true at-risk population, the denominator may not move in step. If we expect that people migrate from places with worse economic conditions to those with better ones (as we find in both of our empirical examples), and if migration is not fully captured by intercensal population estimates, then this will bias results towards finding pro-cyclical

[11] One way to see this point is to separate the log mortality rate into log mortality and log population. Moving log population to the right-hand side of the equation shows that the specification in Eq. 1 is assuming that population is reflected in mortality with a coefficient of one.

[12] For example, in the U.S., the Census uses tax information from the IRS as well as Medicare data to track migration among working-age and older adults, respectively.

mortality.[13]

A second channel through which migration can cause the assumption that mortality moves one-for-one with estimated population to fail is through migrant selection. In particular, if those who migrate in response to business-cycle fluctuations are not representative of the population as a whole, then we should not expect mortality to scale one-for-one with estimated population, even if estimated population is perfectly observed. For example, if healthy young workers are more likely to leave locations with worse economic conditions than, say, retirees (as is the case in the two settings that we study), then we should expect the mortality rate in the location experiencing worse economic conditions to rise even if the change in economic conditions itself has no causal impact on mortality. Focusing on age-standardized mortality can partially deal with the selective sorting of population, but it cannot account for selection of migrants within age groups.

While the first two channels illustrate ways in which migration can affect the *observed* mortality rate without affecting true underlying population health, it is also possible for migration to influence the *true* mortality rate. One such channel is through congestion costs (e.g., by straining fixed local resources), which will increase mortality in migrant-receiving areas while reducing mortality in migrant-sending areas. Another channel by which the act of migration itself can change underlying health is by relocating people across locations with different intrinsic conditions. If, for example, people move from less healthy to healthier locations, then migration will have a direct and beneficial impact on health.[14] As with the first two channels, the congestion effects and protective effects of migration will undermine the one-for-one relationship between mortality and population.

To help address the potential issues generated by assuming a one-to-one relationship between population and mortality, we consider alternative specifications that do

[13] For further discussion of this issue, see Appendix A.2.

[14] This would also be an example of an indirect but true effect of business cycles on mortality—it is indirect in that it occurs as a result of the migration that business cycles induce.

not impose this strict relationship. In particular, we consider a range of specifications embodying assumptions that vary from a strictly proportional relationship between population and mortality to no short-run relationship. Estimating results using this range of alternative specifications will help us assess the sensitivity of estimates to assumptions about the relationship between interpolated population and mortality. We also offer a second strategy to understanding the impact of migration bias that relies on the fact that intercensal population estimates are more accurate, and less vulnerable to migration-related error, closer to census years. Thus, by looking at treatment windows close to census years and expanding these windows iteratively, we can assess the stability of the results obtained from our range of specifications as the wedge between the true unobserved population and our best interpolated estimates of it becomes larger. Combining these strategies with additional information about the migration response in the setting under study allows us to not only to diagnose migration bias, but also to mitigate it so that we can arrive at a more accurate understanding of the relationship between business cycles and health.

It should be noted that a key feature of several of the sources of bias listed above—the endogeneity of unemployment rates, the mechanical relocation of population, migrant selectivity, and congestion effects—is that the impacts of migration in migrant-sending locations will be matched by opposite responses in migrant-receiving locations. Such spillovers violate the assumptions behind the panel-data approach in Eq. 1. Accordingly, treatment spillovers reflect yet another channel through which migration may bias results generated using the standard approach. Indeed, it is likelier to exacerbate than to attenuate the migration bias arising from other sources. However, the issues caused by spatial spillovers can be addressed if migrant-sending and migrant-receiving locations can be identified and compared to a third set of locations that were not contaminated by substantial spillovers. This intuition provides the foundation for our empirical approach to addressing spillover concerns.

## 4 Empirical approach

Our starting point is the standard estimating equation introduced by Ruhm (2000), shown in Eq. 1 above. To deal with the possibility that measures of local economic conditions might be endogenous, we modify Eq. 1 by replacing the unemployment rate $E_{it}$ with a plausibly exogenous measure of the incidence of local economic shocks, $SHOCK_{it}$. In our examples, this variable is generated by interacting time-varying industry-specific global price shocks with measures of the initial importance of the affected industry in each location. Our modified specification is

$$\ln(MR_{it}) = \beta_1 SHOCK_{it} + \phi_i + \eta_t + \epsilon_{it}\,. \tag{2}$$

Conditional on $SHOCK_{it}$ being exogenously determined, Eq. 2 removes concerns about the measure of local economic shock being endogenous in the presence of migration. Still implicit in Eq. 2, however, is the assumption that that mortality and population move one-for-one. As discussed in the previous section, migration is likely to cause this assumption to fail, either because the true at-risk population is not well measured by intercensal population estimates, because migration is selective, or because of congestion effects. Thus, the specification in Eq. 2 is only likely to be valid under ideal circumstances, i.e., when intercensal population estimates are accurate and neither migrant selection nor congestion effects are important.

Motivated by this concern, we consider two alternative regression specifications. The first is,

$$\ln(MORT_{it}) = \tilde{\beta}_1 SHOCK_{it} + \tilde{\beta}_2 \ln(P\tilde{O}P_{it}) + \phi_i + \eta_t + \epsilon_{it} \tag{3}$$

where $MORT_{it}$ is the number of deaths in location $i$ and period $t$ and $P\tilde{O}P_{it}$ is interpolated population in period $t$. The key difference in Eq. 3, relative to Eq. 2, is that the relationship between population and mortality is no longer constrained to be one-to-one. Rather, it is estimated in the data.

The estimated values of $\tilde{\beta}_2$ obtained from Eq. 3 can be a useful tool for diagnosing estimation issues created by migration. In particular, the approach shown in Eq. 2 assumes that $\beta_2 = 1$. If instead we estimate a $\tilde{\beta}_2$ significantly different from one, then this signals that migration is likely to be affecting results obtained using Eq. 2. One reason that we might find $\tilde{\beta}_2 < 1$ is that intercensal population estimates are inaccurate reflections of the true population at that moment in time. In that case, and if migrants move from places with worse economic conditions to places with better ones, then $\tilde{\beta}_2$ will be systematically biased downwards. Attenuation bias due to random mis-measurement of population will also push $\tilde{\beta}_2$ towards zero. We should also expect to see $\tilde{\beta}_2 < 1$ if migrants tend to be healthier than the general population. This is because such population movements will cause less-than-proportional changes in receiving-location mortality.[15]

While we view Eq. 3 as an improvement on Eq. 2 because it does not require the perhaps unrealistic assumption that mortality moves one-for-one with population, it still suffers from two potential problems. First, we may be concerned that the $\ln(POP_{it})$ term is not only systematically mis-measured, but is also endogenously affected by both mortality and migration. This is because since best-available interpolation methods use the observed population in the following census as an explicit input to intercensal interpolations, and distribute unobserved net migration occurring in other intercensal years across all intercensal years. In this way, all intercensal values are interdependent and may be tainted by migration-related mis-measurement and mortality effects in other years.

[15]On the other hand, if migrants are negatively selected on health, or if congestion forces are strong, then we may observe $\tilde{\beta}_2 > 1$. For example, if congestion is important, when population moves away from a location we should expect a more-than-proportional fall in deaths because the fall in deaths generated by a reduction in congestion is added to the reduction in deaths due to the mechanical removal of population. We do not emphasize this possibility here because it does not appear to be the dominant force in our empirical examples.

A second, and more important issue for our purposes, is due to the fact that the estimated relationship between population and mortality reflects a combination of the long-run relationship and the short-run relationship between these variables. Over the long-run, we expect that population and number of deaths in a location will move together; as population iincreases, there are more people at risk of mortality, and it is reasonable to expect the number of deaths to also rise. However, in the short-run, the relationship between population and mortality may be very different from the long-run relationship. For example, if short-run population changes are driven by young workers who have very low mortality risk, then there may be little or no short-run relationship between population and mortality.[16]

One way to partially address issues raised by the difference between the short-run and long-run relationship between mortality and population in Eq. 3 is to include location-specific time-trends. Because population growth rates tend to be fairly persistent over the time periods that we study, the inclusion of time trends in Eq. 3 will serve to absorb differences in long-run population growth, allowing $\tilde{\beta}_2$ to pick up the short-run relationship between population and mortality. To allow this, we will incorporate linear location-specific time trends in some regression specifications.

An alternative approach to dealing with the concerns we have raised about Eqs. 2-3 is to consider a third specification:

$$\ln(MORT_{it}) = \bar{\beta}_1 SHOCK_{it} + \phi_i + \eta_t + \epsilon_{it} \,. \tag{4}$$

Note that Eq. 4 differs from Eq. 3 only in that we omit the log population variable. Implicitly, this specification assumes that in the short run, there is no relationship between population and mortality, reflecting the opposite extreme to the strictly proportional relationship assumed in Eq. 2.

[16]This issue will be exacerbated by the use of interpolated population values in non-census years. In Appendix A.2 we provide a short example illustrating this concern.

To summarize, Eqs. 2, 3, and 4 reflect a range of assumptions about the short-run relationship between population and mortality, and therefore generate a range of estimates of the relationship between business cycles and mortality. When these estimates are close to agreement, we can be reasonably confident in the results, even in the presence of migration. When estimates diverge substantially, this signals that assumptions about how to model underlying population and its impact on mortality play an important role in determining the results. In those cases, carefully studying the available evidence on migration patterns in the setting under study can help us choose between alternative specifications.

The last issue we need to address before moving to our empirical results is the impact of spillovers across locations. The specifications in Eqs. 2-4 rely on a comparison between a set of treated locations and a set of control locations. However, if migration in response to local economic shocks in the treated locations affects mortality in the control locations, then the control locations cannot provide a valid counterfactual. We implement a relatively simple approach for dealing with this issue which involves identifying those locations that are likely to send or receive migrants to or from the treated locations. Since migrant flows are often related to distance, we refer to these as "nearby" locations.[17] Once we have identified nearby locations that are likely to share migrant flows with the treated locations, we can estimate the impact of the shock on both the treated and nearby locations by comparing these to a third set of clean control locations. This not only allows us for a cleaner view of the true mortality treatment effect of a change in local economic conditions, but it also reveals the indirect effects that the standard empirical approach obscures.

[17] Of course, in many, and particularly, in modern settings, geographic distance may not be the dimension over which migration takes place. For instance, migrant-sending regions may be identified on the basis of occupational similarity, social or transportation networks, etc.

# 5 The Lancashire cotton shortage

Having laid out our basic empirical approach, we now turn to the first of our two empirical examples: a sharp, severe, and short-lived downturn in the cotton textile-producing region of Britain in the 19th Century.[18] This historical example is useful because it allows us to cleanly identify the spatial and temporal incidence of an adverse shock without having to rely on potentially endogenous measures of local economic conditions, such as unemployment. In addition, high migration costs in this setting make it easier for us to track and demonstrate the impact of migrant spillovers, which occur mainly between geographically proximate locations. Finally, this historical setting is somewhat similar to a modern low- or middle-income country, such as those in Gonzalez & Quast (2011) or Cutler *et al.* (2002), than it is to the developed-country settings typically studied in this literature (or, indeed, in our second empirical example). Accordingly, evidence from this empirical setting contributes to the generalizability of our findings. Below we provide a brief description of this setting and data we use. Further details are available in Appendix B.

## 5.1 Background and Data

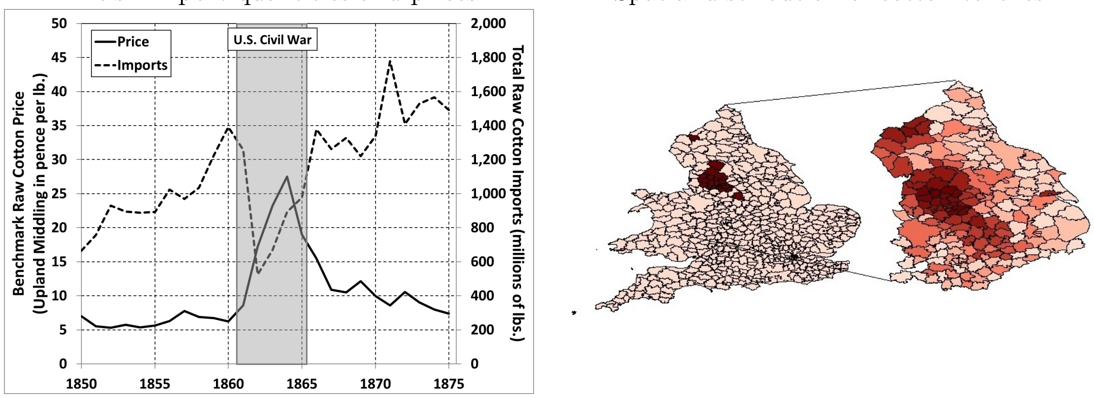
The cotton textile industry was the largest and most important industrial sector of the British economy during the 19th century. For historical reasons dating to the 1700s, British cotton textile production was geographically concentrated in the Northwest counties of Lancashire and Cheshire, which held over 80% of the cotton textile workers in England & Wales in 1861.[19] The industry was entirely reliant on

[18]This event has previously been studied by Hanlon (Forthcoming) and Hanlon (2015). Historians often refer to this event as the "Cotton Famine," where the term "famine" is used metaphorically to describe the dearth of cotton inputs. We avoid using this term because it can be misleading in a study focused on health.

[19]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. Calculation based on data

Figure 1: Cotton prices, imports, and spatial distribution of cotton textile industry

British import quantities and prices | Spatial distribution of cotton textiles




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

imported raw cotton and, in the run-up to U.S. Civil War, 70% of these imports came from the U.S. South (Mitchell, 1988). The war sharply reduced British imports of U.S. cotton, prompting a sudden and dramatic rise in cotton prices and a sharp drop production. These effects are depicted in the left-hand panel of Figure 1.[20] The right-hand panel shows the spatial distribution of the British cotton textile industry on the eve of the U.S. Civil War. Additional information, in Appendix B.1, shows that the direct impact of the U.S. Civil War on the British economy was largely confined to the cotton textile sector.

Both contemporary reports on public assistance and data on relief-seekers suggest

collected by the authors from the 1861 Census of Population reports.

[20]Appendix Figure B.1 shows British firms' raw cotton consumption and variable operating costs (excluding cotton), good proxies for industry output. These show a sharp drop in production and factor payments during the 1861-1865 period equal to roughly a 50% reduction compared to pre-war output levels. As described in Hanlon (2015), other cotton-producing countries such as India, Egypt and Brazil rapidly increased their output during the U.S. Civil War period. However, 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.

that the shock had a substantial and geographically-concentrated impact on workers. One reflection of these effects is presented in Figure 2. The left-hand panel of this figure describes the number of able-bodied relief-seekers who obtained aid from local Poor Law Boards, the main source of government support for the destitute in our setting. During the U.S. Civil War, we see an increase in relief seekers in the Northwest counties, where cotton textile production was concentrated. Non-cotton counties, however, were largely unaffected. Contemporary reports suggest that at the nadir of the recession in 1862 and early 1863, roughly half a million people in the cotton-producing districts relied on relief from government sources or private charities. The right-hand panel of Figure 2 describes expenditures by local Poor Law Boards. These spiked in Lancashire and Cheshire, the two main cotton textile counties, during the U.S. Civil War. In the face of the cotton shortage, workers in the affected areas employed a variety of coping mechanisms, including running down savings, pawning valuables and furniture, seeking government and charitable support, and migrating. In Appendix B.2, we review contemporary evidence on all these private and institutional responses to the cotton shortage, with the exception of migration, which we discuss in Section 5.2.

To analyze the impact of the U.S. Civil War on migration and mortality in the cotton-textile regions of Britain, we draw on population data from the British Census for every decade from 1851-1881, and mortality data over the period 1851-1865, the latter taken from reports produced annually by the Registrar General's office.[21] Our analysis is conducted at the district level using 539 geographically-consistent districts covering all of England & Wales over this period.[22]

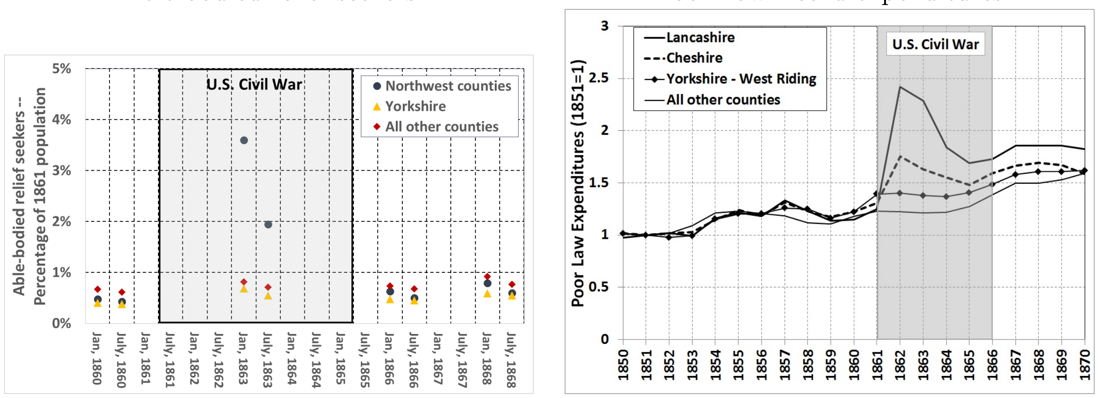
To identify treated districts, we use information on the industrial composition of

[21]Further details on the data are available in Appendix B.3.

[22]The available data cover around 630 districts in each year, but some districts experienced boundary changes over time. To obtain geographically consistent districts, 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 539 consistent districts in the main analysis.

Figure 2: The spatial incidence of the cotton shock

Able-bodied relief-seekers　　　　Poor Law Board expenditures




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

employment in each district on the eve of the U.S. Civil War, based on occupation data from the 1851 Census. In the main analysis, we define as cotton (i.e., "treated") districts those districts with over 10% of employment in cotton textile production.[23] Our study covers the years 1851-1865, with the U.S. Civil War years (1861-1865) defining the cotton shortage period. We focus only on the periods before and during the shortage because of concerns that the post-shortage years may have been influenced by persistent effects of the shortage.[24] Wherever intercensal population estimates are used, these are generated using Das Gupta interpolation, which accounts for annual changes in population due to observed births and deaths within each district, and

[23]In robustness checks we also generate results using a continuous measure of local cotton textile employment shares. We focus our main results on a discrete cutoff both in order to make interpretation easier, and in order to reduce the impact of measurement error. In particular, within the set of major cotton textile districts, variation in cotton textile employment share does not necessarily correspond to variation in shock intensity because the impact of the cotton shortage depended in part on the type of cotton textile products produced in each district. For example, districts that produced finer fabrics, where raw cotton was a smaller portion of total cost, were less affected by the cotton shortage.

[24]For example, we know that one form of relief during the cotton shortage was public works employment, which was mainly focused on projects improving health or transportation infrastructure. These projects were undertaken relatively late in the study period, so they are unlikely to have substantially affected mortality during the Civil War.

distributes any residual population change at the decade level (the error of closure, a measure of implied net migration) smoothly across intercensal years.[25]

## 5.2 Migration

Contemporary accounts indicate that out-migration occurred in response to the cotton shortage, but the extent of these flows remains debated. Thus, to assess the magnitude of the migratory response, we look for changes in population growth patterns using data from the 1851-1881 censuses. These data are presented in Figure 3, which describes changes in district population across each decade, normalized by the change in 1851-1861 (the decade preceding the downturn).[26] This figure reveals three important patterns. First, it presents evidence of a substantial slowdown in population growth in the cotton textile districts in the decade spanning the cotton shortage. This suggests either that the downturn coincided with a rise in out-migration from those districts, or a reduction in in-migration. Indeed, additional evidence on the birthplaces of Northwest county residents, which we present in Appendix B.4, suggests that both an increase in out-migration and a reduction in in-migration occurred during this period. Second, we observe an acceleration in population growth in nearby districts, which we define as non-cotton districts that are within 25 km of a cotton district. Meanwhile, there is little change in the population growth trend in districts beyond 25 km. These patterns are consistent with short-distance migration from cotton textile districts during the downturn. Third, we see that these changes essentially disappear after 1871, a fact which highlights the temporary nature of the shock.

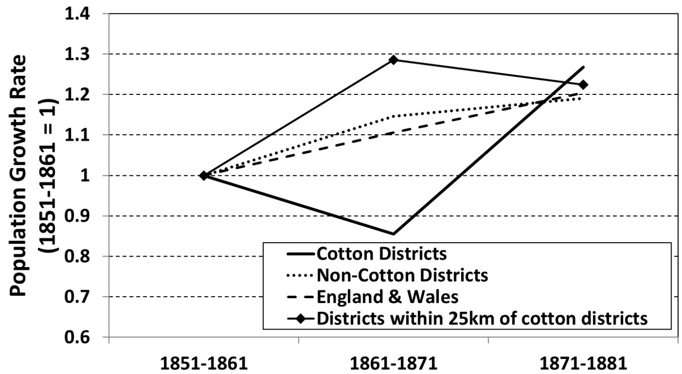
These implied migration flows were meaningfully large. In terms of magnitude,

[25]The Das Gupta method is the same approach used by the U.S. Census Bureau. The main difference between our approach and modern census population interpolations is that modern estimates draw on additional information (for instance in the U.S., information such as IRS tax data and Medicare records) that partially capture migration taking place between censuses. This type of additional data are not available in the period we study. For more on best practices in interpolation, see Appendix A.2.2.

[26]The 1861 Census was collected in April of that year, before the U.S. Civil War had any substantial impact. As a result, this should be thought of as a clean pre-war population observation.

Figure 3: Migration response to the cotton shortage

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 using Census data. Cotton districts are defined as those districts with more than 10% of employment in cotton textile production in 1851. The population growth rate for each group of districts is normalized to one in 1851-1861. Data are from the Census of Population.

had the population of the cotton districts grown from 1861-1871 at the same rate that it grew in 1851-1861, these districts would have had 54,000 additional residents in 1871, a figure equal to 2.2% of the districts' 1861 population.[27] Similarly, if nearby districts had grown in 1861-1871 at the rate they grew during 1851-1861, they would have had 61,000 fewer residents, which is equal to 4% of the districts' 1861 population. Note that these figures will understate the migration response if some migrants returned between 1865 and 1871.[28]

An alternative view of migration is provided by calculating implied net migration rates at the district level. These are calculated as the difference between the observed population count in a district in a given census year and the population that we

[27]As a point of comparison, during the Great Depression in the U.S., Fishback *et al.* (2006) report that from 1935-1940, 11% of the U.S. population moved, with 60% of the moves occurring within state. This suggests that the migration response observed in our setting may not be unusually large compared to other historical recessions.

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

would have expected in that district-year given the population in the previous census plus all births and less all deaths in the intervening years.[29] In the left-hand panel of Figure 4, we map implied net migration by district from 1851-1861. The cotton textile districts show a strong pattern of net in-migration (dark colors) in this decade. In the right-hand panel we plot the change in net migration in 1861-1871 compared to 1851-1861. This figure provides evidence of a large reduction in net in-migration in most of the cotton textile districts during the Civil War decade. We also see evidence of an increase in migration into districts surrounding the cotton areas, consistent with migration from the cotton districts into nearby locations.[30] Overall, the results in Figures 3-4 show a substantial and short-distance migration response to the cotton shortage.

Although selectivity will become much more important in our second empirical example, there is also some evidence of selective migration away from the cotton textile districts during the U.S. Civil War period. In particular, in Appendix B.4 we provide evidence showing that young adults were somewhat more likely to migrate in response to the cotton shock than the elderly. However, the change in population in the 20-39 age group accounts for only about three-fifths of the overall change in population of the cotton districts between 1861 and 1871. Thus, even though this group was important, a substantial amount of migration likely occurred among other segments of the populations as well.

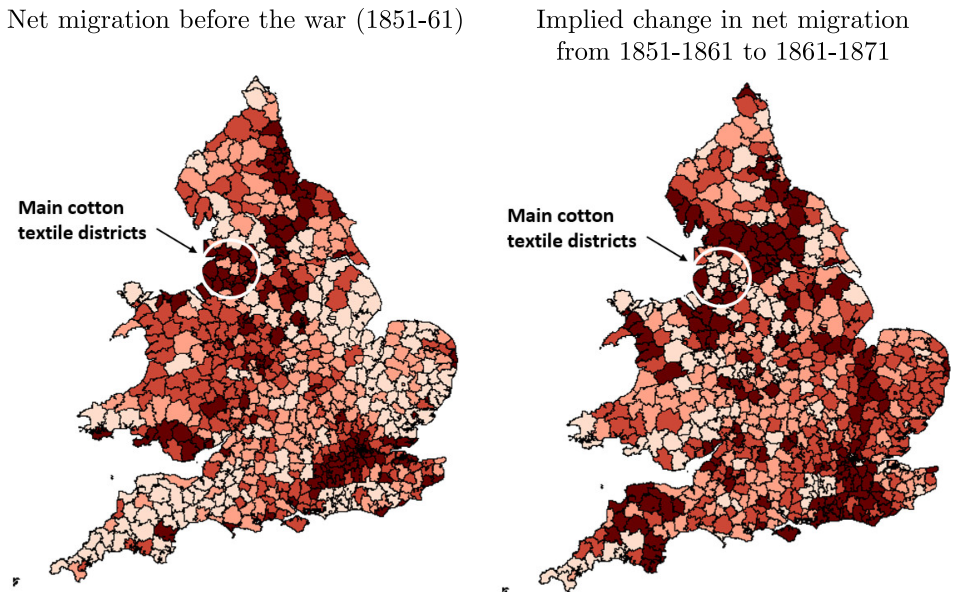
## 5.3 Mortality

Having established that the cotton shortage induced a substantial and spatially concentrated migratory response, we next analyze the impact of this event on mortality patterns. As a first step, Figure 5 describes the evolution of log mortality across the

[29]Put another way, implied net migration is the difference between the census count and the postcensal estimate obtained via the components of change method. This conceptual approach has been used in studies of migration such as Fishback *et al.* (2006).

[30]Additional graphs are available in Appendix B.4.

Figure 4: Maps of implied net migration




The left-hand panel maps implied net migration for each district in the decade before the shock, 1851-1861. Darker colors indicate net in-migration. The right-hand panel plots the difference in net migration between the 1851-61 and 1861-71 decade. Lighter colors indicate an increase in net out-migration from a district during the Civil War decade (1861-71). Implied net migration is calculated as the difference between the observed population count in a district in a given census year and the population that we would have expected in that district-year given the population in the previous census count plus all births and less all deaths in the intervening years.

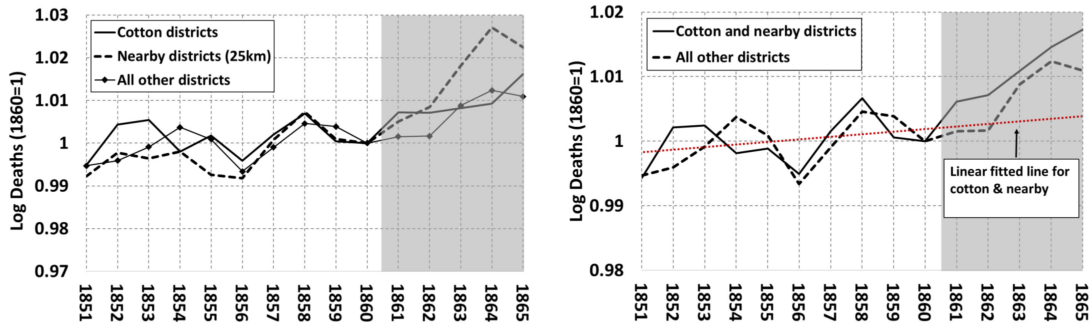
study period (normalized such that 1860=1).[31] In the left-hand panel we separate cotton districts, nearby districts (those within 25 km), and all other districts. There are three notable features here. First, in the pre-shortage period, mortality in the three groups track each other well. Second, in the first two years of the U.S. Civil War, there is evidence of elevated mortality in the cotton districts, though this increase disappears after 1862. Third, in the nearby districts we observe a substantial increase in mortality, particularly after 1862. One potential cause of this increase in mortality in nearby districts is migration from cotton to nearby districts.

Since our evidence suggests that most migration out of cotton districts went to nearby areas, combining the migrant-sending and migrant-receiving districts can help us get a sense of the aggregate mortality impact of the downturn. Recall that most of the channels through which migration generates bias, discussed in Section 3, suggest that we should observe offsetting effects in migrant-receiving and migrant-sending districts. Given this, aggregating migrant-sending and migrant-receiving regions will allow us to assess the net impact of the recession on mortality. In the right-hand graph we combine the cotton and nearby districts. These results provide evidence that overall mortality in the cotton-and-nearby-districts category increased during the U.S. Civil War period.

[31] Graphs of the mortality rate are available in Appendix B.5.

Figure 5: Mortality effects of the cotton shortage

Cotton, nearby and control districts — Combining cotton and nearby districts




Mortality data from the reports of the Registrar General. Data cover all of England & Wales. Cotton districts are those with more than 10% of employment in cotton textile production in 1851. Nearby districts are non-cotton districts within 25km of the cotton districts. The linear trend line in the right-hand panel is fitted to data from the cotton and nearby districts from 1851-1860.

Next, we analyze these patterns econometrically. Table 1 present our results, with results in Columns 1-3 corresponding to the specifications in Eqs. 2-4, respectively. In these regressions standard errors are clustered by district to adjust for serial correlation.[32] In Column 1, we present results using the log mortality rate as the dependent variable, as in the standard approach. Here we find evidence that the mortality rate fell during the recession, a finding that is consistent with the pro-cyclical mortality results obtained in most of the studies following Ruhm (2000). In Column 2, we move the population denominator the the right-hand side of the equation. This weakens the relationship between the cotton shock and mortality. Moreover, we can see that the estimated relationship between population and mortality is well below (and statistically different from) one, suggesting that the one-to-one relationship between population and mortality embedded in the specification in Column 1 may be inaccurate. Column 3 presents results based on the specification in Eq. 4, which

[32]Clustering standard errors by district is similar to the approach used in most studies in this literature. In Appendix B.5, we present additional results including those clustered by county (a larger geographic unit), and those adjusting simultaneously for serial and spatial correlation across locations. Results are qualitatively unaffected as all three approaches produce similar standard errors.

assumes no short-run relationship between population and mortality. Here the sign of the relationship between the cotton shock and mortality becomes positive. This difference makes it clear that assumptions about the relationship between population and mortality have a meaningful impact on the results.

Table 1: Preliminary estimates of the mortality effect of the cotton shortage

| Dependent variable: | Ln(MORT. RATE) | Ln(MORTALITY) | Ln(MORTALITY) |
|---|---|---|---|
| | (1) | (2) | (3) |
| Cotton district × shortage | -0.0276** | -0.0221* | 0.0155 |
| | (0.0108) | (0.0114) | (0.0189) |
| Ln(Pop) | | 0.872*** | |
| | | (0.0283) | |
| Observations | 8,085 | 8,085 | 8,085 |
| R-squared | 0.137 | 0.334 | 0.195 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. Data cover 539 districts from 1851-1865. All regressions include district fixed effects and year effects. Shock period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%.

The results in Table 1 show that the estimated relationship between population and mortality is sensitive to assumptions about the relationship between short-run population changes and mortality. To provide substantive evidence on the true effect of the cotton shortage on mortality, we need a way to choose between these alternative sets of assumptions. However, before we tackle that issue, it is useful to deal with a second concern that is not addressed in Table 1: the impact of spillovers across treated and putative control districts due to migration.

In order to deal with the issue of migrant spillovers, we include a term to capture the impact of migration from cotton textile districts to other nearby areas. Our primary approach to dealing with the impact of migration on nearby districts is to construct a variable, $NEARcot_j^{0-25}$, which reflects, for each non-cotton district, the amount of cotton textile employment in other districts within 25 km (or alternative distance windows). Specifically, we calculate,

$$NEARcot_j^{0-25} = \ln\left(\sum_{i \neq j} 1[d(i,j) < 25km] * (COT_i + 1) * 1[COTDIST_j = 0]\right) \tag{5}$$

where $1[d(i,j) < 25km]$ is an indicator variable that equals one when the distance between districts $i$ and $j$ is less than 25 km and $COT_i$ is cotton textile employment in district $i$.[33] The $1[COTDIST_j = 0]$ component of this equation ensures that we only cacluate nearby cotton exposure for non-cotton districts, defined as those with less than 10% of employment in cotton textile production.[34] Finally, because nearby cotton textile employment is zero for districts that were far from cotton textile producing areas, we add one before taking logs. We then include this employment-weighted distance term, interacted with an indicator for the shock period, in our regressions. Including this variable effectively removes indirectly-treated districts from the control group, allowing us to separately evaluate the direct and indirect mortality effects against a clean counterfactual. In robustness exercises in Appendix Table 11, we iteratively add increasingly distant employment-weighted distance bands to show that the majority of systematic spillovers are indeed captured within 25 km. We also consider an alternative approach to measuring "nearby" districts that uses unweighted (i.e., indicator) variables for districts within particular distance bands of the major cotton textile districts. These alternatives deliver similar results.

Table 2 presents results that account for the impact of cotton textile employment in nearby districts. Again, we display results that correspond to each of our three estimating equations in Columns 1-3. The results in Table 2 provide evidence that mortality increased in nearby districts during the cotton shortage, consistent with

[33] 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.

[34] We include the $1[COTDIST_j = 0]$ term 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. Contemporary evidence consistently shows that those leaving cotton districts were not migrating to other major cotton-producing areas. See, e.g., Arnold (1864).

the relocation of migrants from the cotton to nearby districts. Once we account for migration spillovers, the results in Column 3 suggest that the recession led to a statistically significant increase in mortality, while the results in Column 1 continue to suggest that the recession reduced mortality. Again, it is clear that the results are highly sensitive to assumptions about the relationship between population and mortality.

Table 2: Accounting for spillovers to nearby districts

| Dependent variable: | Ln(MORT. RATE) | Ln(MORTALITY) | Ln(MORTALITY) |
|---|---|---|---|
| | (1) | (2) | (3) |
| Cotton district × shortage | -0.0229** | -0.0125 | 0.0494** |
| | (0.0114) | (0.0123) | (0.0195) |
| Nearby cotton emp. × shortage | 0.00154 | 0.00287** | 0.0109*** |
| | (0.00117) | (0.00122) | (0.00187) |
| Ln(Pop) | | 0.857*** | |
| | | (0.0293) | |
| Observations | 8,085 | 8,085 | 8,085 |
| R-squared | 0.137 | 0.335 | 0.207 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by district. Data cover 539 districts from 1851-1865. All regressions include district fixed effects and year effects. Shock period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%.

Given the sensitivity of these results, we consider two approaches to help choose among the alternative assumptions. First, we review the evidence on migration during the cotton shortage to assess the validity of the assumptions underlying each of these specifications. Second, we show how our results change when we look at narrower shock windows that are closer to the census year and so less vulnerable to population mis-measurement due to migration. In this case, the census year (1861) falls conveniently at the beginning of the cotton shortage period.

The evidence in Section 5.2 shows that, relative the the previous decade, cotton textile districts experienced a substantial reduction in population growth during the

the downturn. Despite this reduction, however, population in most cotton districts increased over the 1861-1871 decade. This fact is important because when we construct our intercensal population estimates, they will imply positive growth across all years, which is at odds with the available evidence on the migration response. The available evidence suggests that people left the cotton districts during the cotton shortage, but that population in these areas rebounded from 1866-1871 as the cotton industry recovered.[35] When this pattern is combined with the one-to-one relationship between population and mortality embedded in the specification in Column 1 of Table 2, the result will be to bias results towards finding that the mortality rate decreased during the shortage, simply because the interpolated population denominator overstates the true population.[36] Under these population growth conditions (a point that we discuss further in Appendix A.2), the specification in Column 3 of Table 2 clearly offers a more suitable approach to modeling mortality. Since the available evidence on migration during this event implies that the assumptions underlying the results in Column 3 of Table 2 are most realistic, these are accordingly our preferred results.

Another way to differentiate between the alternative results in Table 2 is to study their robustness. In particular, in Appendix 9 we consider how these specifications react to the inclusion of district time-trends, which are included in some specifications in the Ruhm literature. The results in Appendix 9 show that once district time trends are included, all three alternative specifications deliver results that are very similar to those produced without trends in Column 3 of Table 2.[37] This reinforces our choice of Eq. 4 as our preferred specification. These results also suggest that the cotton shortage statistically significantly *increased* mortality. The robustness of this result is assessed in Appendix 10.

[35]For example in the 1871 Census the Registrar for Salford, near Manchester, an important cotton textile area, wrote that the population of the district experienced an "Increase due to the revival of trade since the conclusion of the American war..."

[36]Of course, even the numerator may be spuriously low if the exodus of cotton-district residents led cotton districts to have an at-risk population that was, in actuality, unobservedly smaller.

[37]That all three specifications deliver very similar results once time trends are included suggests that most of the differences across specifications are due to long-run population trends.

As a final way to validate our results, we exploit the fact that population estimates should be more accurate, and less subject to migration-related mis-measurement, closer to census years. Table 3 presents additional results showing how the estimates obtained from each specification change as we iteratively expand the shock period window to include years further from the census year 1861. The top panel presents results using the log mortality rate as the dependent variable, as in Column 1 of Table 2. The middle panel corresponds to Column 2 of Table 2, where the outcome variable is log mortality and log population is on the right-hand side. The bottom panel presents results with log mortality, as in Column 3 of Table 2.

There are two important features to note in Table 3. First, near the census year, all three specifications deliver very similar results: the downturn results in higher mortality. Second, as we consider larger windows which include years further from the census year, the results in Panel C consistently show that the cotton shortage increased cotton-district mortality. In contrast, the results in Panel A switch from positive and statistically significant to negative and statistically significant. In Panel B the results switch from positive and stastistically significant to negative and insignificant. Both of these patterns provide support for our preferred specification, which consistently suggests that mortality increased during the cotton shortage.

Table 3: Results for various windows starting in 1861

| Shock years: | 1861<br>(1) | 1861-62<br>(2) | 1861-63<br>(3) | 1861-64<br>(4) | 1861-65<br>(5) |
|---|---|---|---|---|---|
| | | **Panel A – DV: Log Mortality Rate** | | | |
| Cotton × shortage | 0.0246* | 0.0334*** | 0.000559 | -0.0212* | -0.0229** |
| | (0.0146) | (0.0116) | (0.00980) | (0.0115) | (0.0114) |
| Nearby × shortage | -2.77e-05 | 0.00152 | 0.00134 | 0.00205* | 0.00154 |
| | (0.00155) | (0.00139) | (0.00133) | (0.00123) | (0.00117) |
| | | **Panel B – DV: Log Mortality** | | | |
| Cotton × shortage | 0.0405** | 0.0482*** | 0.0154 | -0.00887 | -0.0125 |
| | (0.0158) | (0.0122) | (0.0100) | (0.0120) | (0.0123) |
| Nearby × shortage | 0.00188 | 0.00333** | 0.00319** | 0.00361*** | 0.00287** |
| | (0.00156) | (0.00140) | (0.00135) | (0.00128) | (0.00122) |
| Ln(Pop) | 0.727*** | 0.761*** | 0.774*** | 0.822*** | 0.857*** |
| | (0.0398) | (0.0359) | (0.0334) | (0.0308) | (0.0293) |
| | | **Panel C – DV: Log Mortality** | | | |
| Cotton × shortage | 0.0829*** | 0.0953*** | 0.0660*** | 0.0478*** | 0.0494** |
| | (0.0203) | (0.0168) | (0.0152) | (0.0178) | (0.0195) |
| Nearby × shortage | 0.00695*** | 0.00909*** | 0.00949*** | 0.0108*** | 0.0109*** |
| | (0.00176) | (0.00168) | (0.00173) | (0.00182) | (0.00187) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. All regressions include district fixed effects and year effects. N=5,929 in Column 1; 6,468 in Column 2; 7,007 in Column 3; 7,546 in Column 4; 8,085 in Column 5.

Overall, the results in Tables 1-3 show how sensitive estimates are to assumptions about the relationship between population and mortality in the short run. However, by using additional information on migration patterns, and by looking closer to census years where population is less vulnerable to mis-measurement, we can select a specification, Eq. 4, that delivers consistent results that more accurately reflect the business cycle-mortality relationship. Under this specification, results indicate that mortality in the cotton districts appears to have increased during the downturn. Importantly, this finding is corroborated by the patterns shown in Figure 5. In addition to the downturn's direct effects, we find evidence that mortality increased in districts proximate to cotton textile areas, consistent with the impact of the relocation of population to these areas.[38] Thus, we conclude that after adjusting for the statistical appearance of health change due to migration bias, the overall effect of the cotton downturn was to increase mortality.

# 6 The Appalachian coal boom

In the second of our two empirical examples, we consider a more recent setting in which we can identify a spatially concentrated shock to local economic conditions. In particular, following Black *et al.* (2005) (hereafter BMS), we study the impact of the commodity price boom that affected the Appalachian coal-mining region of the U.S. between 1970 and 1977.[39] This example allows us to consider the impact of migration bias on the relationship between temporary local economic shocks and mortality in a setting that is more similar to most of the previous literature.[40] As before, we describe the empirical setting briefly. Further details on this setting can be found in Black *et al.* (2005).

## 6.1 Background and Data

The Appalachian coal boom of the 1970s was generated by regulatory changes that took place in 1969, together with a rise in oil prices due to the OPEC oil embargo in 1973-74.[41] The left-hand panel of Figure 6 presents data from BMS showing the

[38] It is important to recognize that the increase in mortality in nearby districts does not necessarily imply that true mortality rates rose in those areas; it may simply be due to an increase in the at-risk population. However, mortality in nearby areas may have increased through channels such as selective migration or the congestion effects generated by new arrivals.

[39] In a related paper, Black *et al.* (2002) study the impact of the coal boom and bust on participation in Disability Insurance. They find that disability program participation fell during the boom and then increased during the bust.

[40] In fact, the time period covered by this example partially overlaps with the period covered in the original Ruhm (2000) paper.

[41] See Black *et al.* (2005). The Appalachian region also benefited from new roads during this period as a result of the Appalachian Development Highway Program (Jaworkski & Kitchens, 2016), but this affected the region as a whole rather than just those areas with coal resources.

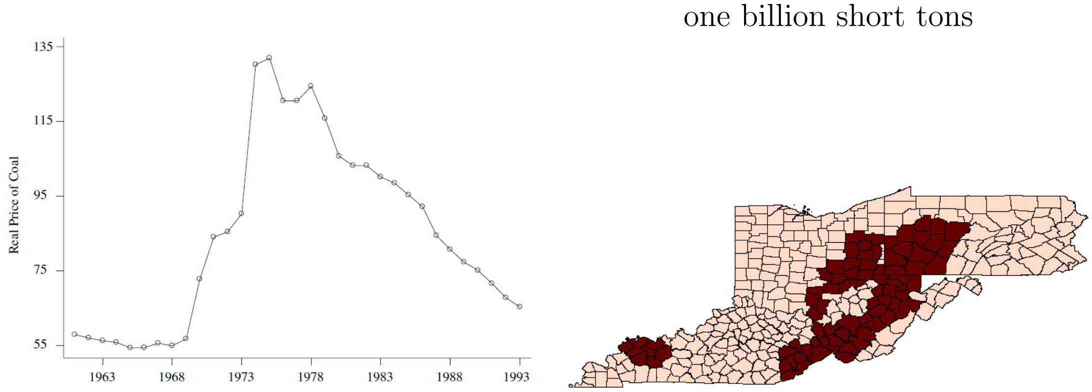
evolution of coal prices across this period. Coal prices rose sharply starting in 1970 and then fell after 1982, as competition from western coal mines increased. Additional data provided by BMS show that these price increases were matched by a similar increase in coal industry earnings. BMS divide their study into three periods—the boom (1970-1977), a stable peak period (1978-1982), and the bust (1983-1989)—a convention which, for consistency, we will also adopt.

The spatial distribution of the economic impact of these price increases was dictated by the naturally-occurring coal reserves available in each county. The right-hand panel of Figure 6 provides a map showing the distribution of coal reserves across the four states studied by BMS: Kentucky, Ohio, Pennsylvania, and West Virginia. This figure shows that the main coal reserves were located in a band of counties stretching from southern Pennsylvania, across West Virginia and Ohio, and into Kentucky.

Figure 6: Coal prices and the spatial distribution of coal reserves

Coal prices

Counties with coal reserves over one billion short tons




Left-hand panel is from Black *et al.* (2005). The right-hand panel is produced using the USCOAL database provided by the U.S. Geological Survey using data from surveys conducted mainly in the 1960s.

The main data used in our analysis come from the the decennial Census of Population and from the annual records of deaths and births given by Bailey *et al.* (2016) and the Centers for Disease Control. In the main analysis, we focus on the four states studied by BMS, though we add additional neighboring states in some robustness checks. To identify coal counties, we use data from the U.S. Geological Survey's US-COAL database, which provides estimates of coal reserves by county from surveys taken mainly in the 1960s. In our main analysis, we identify coal counties as those with over one billion short tons of reserves, though we consider alternative cutoffs in robustness checks. There are 72 counties that satisfy this criterion in the four analysis states.[42] Our control counties are those with coal reserves below the cutoff, and following BMS, we limit the set of control counties to those counties in the four analysis states with populations ranging from 8,000 to 225,000. This population criterion is applied to control counties in order to obtain a set that is similar to the fairly rural coal counties, and results in our dropping the main urban counties in these states from the control group. This yields 202 non-coal control counties. In robustness exercises, we consider a larger set of control counties including all non-coal counties in the four states studied by BMS as well as additional counties in a set of four further neighboring states.

We define the coal boom as spanning the period 1970-1977, and we limit our analysis to the pre-boom (1950-1969) and boom (1970-1977) periods. This parallels the approach used in our first example, and also avoids questions about whether the period after 1977 should be treated as a downturn or merely as the end of a boom. As in the cotton shortage example, intercensal population estimates are generated using the Das Gupta interpolation method. These account for births and deaths in each county-year, and distribute decade-wide implied net migration across intercensal years. It is worth noting that for reasons of data availability, these interpolations use slightly less information than estimates produced by the census today, the latter of which take advantage of additional information on migration from IRS and Medicare data. However, in Appendix C.1 we provide evidence that in the 1970s, when we

[42]This differs slightly from the approach used by BMS, which identifies coal counties as those where more than 10% of earnings came from the coal industry in 1969, a criterion that yields 32 coal counties. We prefer to use coal reserves to identify the treated counties rather than earnings because reserves are less likely to reflect other factors affecting the counties in the period just before the coal boom. However, in the end, both approaches yield fairly similar results.

have both our intercensal estimates and the census interpolations (which are partially adjusted for migration), these two series are quite similar.[43]

## 6.2 Migration

Figure 7 describes the migration response generated by the coal boom and bust in the coal and control counties. Population in the coal counties was shrinking in the decades before the coal boom, but increased substantially from 1970-1980 before resuming their decline. These patterns suggest that how we deal with differential initial population growth trends and longer-run migration flows will play an important role in our results.[44]

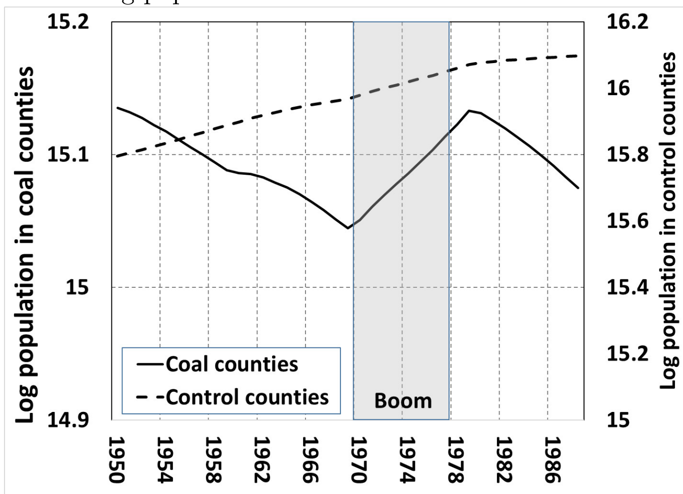
In control counties, we see no evidence of a change in population growth in response to the coal boom. This suggests that either the control counties do not share migration flows with the coal counties, or that these flows are so small relative to the size of the migrant-sending regions that they have little impact in these locations. There are several likely explanations for this pattern. First, our control counties do not include major urban areas, which may have been the preferred destination of Appalachian migrants before the boom, and a source of return migrants during the coal boom. Second, coal counties were relatively small in terms of population, so

[43] Annual county-level interpolated population data are not provided by the Census for years prior to 1970.

[44] These migration patterns are consistent with the results of BMS, who also find evidence of an increase in employment and population growth in the coal counties from 1970-1980, followed by a reduction from 1980-1990. Using data from the Bureau of Economic Analysis, BMS find that employment, overall earnings, and earnings per worker in the mining sector increased during the 1970-1977 boom, were stable from 1978-1982, and then decreased starting in 1983. They also find the same pattern for workers outside of the mining sector. These effects were concentrated in construction and retail, which primarily provide non-traded goods and services. Using Census data, they provide evidence that between 1970 and 1980, a period which includes mainly boom years, there was a statistically significant increase in population in the 20-29 age group. There were decreases in population across a broader set of working-age groups from 1980-1990, a period which includes mainly bust years. Finally, they estimate that the number of families living in poverty in the coal counties decreased from 1970-1980, and then increased from 1980-1990. It is notable that very similar results were obtained by Carrington (1996) in a study of the impact of the temporary boom in the Alaskan economy that occurred during the construction of the Trans-Alaska Pipeline.

Figure 7: Migration response to the coal boom

Log population in coal and control counties




This figure describes the change in population for all coal counties and the control counties in the four states in the original BMS study. Coal counties are defined as those in which more than 10% of income came from coal in 1969. Intercensal population estimates are constructed using the Das Gupta interpolation method.

even if the migration inflows had a large effect in those counties, they may not have had any meaningful impact elsewhere, particularly if the set of locations was more diffuse than in our cotton example. Third, the low costs of migration in this setting may have caused many of those returning to the coal counties to have originated outside of the four states covered in our study. Regardless of the cause, the fact that we find no large population response in the control counties has important implications for our analysis, since it implies that spillovers between migrant-sending and migrant-receiving counties are not likely to be an important source of bias in this setting.

Figure 8 presents maps of implied net migration by county in the decade before the coal boom, and the difference in implied net migration during the coal boom decade relative to the preceding decade.[45] In the left-hand panel we see that coal

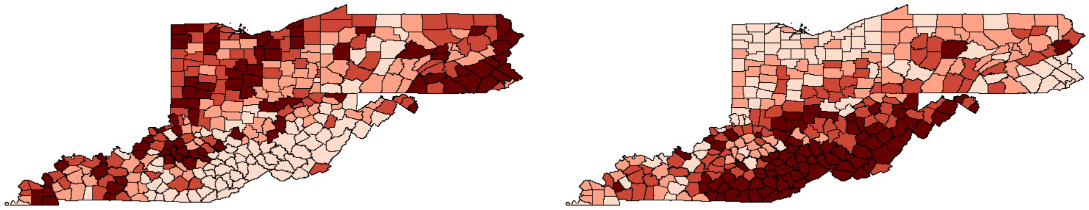
[45]As before, we focus only on the four states included in the BMS analysis; maps including four

districts were experiencing strong net out-migration in the decade before the coal boom. Similar patterns are also observed in 1950-60, as well as in the decade following the coal boom (see Appendix C.2). However, the right-hand panel reveals a striking reversal in this trend during the coal boom decade. Furthermore, in Figure 8 we see no evidence that counties proximate to coal counties experienced a stark reversal in migration patterns during the boom period, as we had observed for nearby counties in the cotton shortage example. This suggests that spillovers between coal and nearby counties are unlikely to be an important concern in this particular setting, consistent with the results in Figure 7.

Figure 8: Maps of migration before and during the coal boom

Implied net migration prior to the boom (1960-70)

Implied change in net migration from 1960-70 to 1970-80



The left-hand panel describes implied net migration over the decade from 1960-70. Lighter colors indicate counties experiencing net out-migration. The right-hand panel describes the change in net migration from the 1960-70 decade to the 1970-80 decade. Darker colors indicate districts that experienced an increase in net in-migration in the 1970s relative to the previous decade. Implied net migration is calculated as the difference between the observed population count in a district in a given census year and the population that we would have expected in that district-year given the population in the previous census count plus all births and less all deaths in the intervening years.

The evidence presented in Appendix C.2 suggests not only that coal counties saw massive in-migration flow during the boom, but also that this migration was selective. The population increase in the coal counties during the decade spanning the boom, 1970-1980, was 342,000. Of this, over 210,000 of the increase was in the 25-34 age group, while another 101,000 were in the 15-24 age group. Thus, the vast majority of

additional states adjacent to these four core states are provided in Appendix C.2.

new residents of the coal counties during the boom were young adults, the healthiest segment of the population. Moreover, these increases, especially the increase among 25-34 year-olds, were much larger in percentage terms than the change in population in the control counties.[46] Thus, selective migration in response to the coal boom was stronger than the selectivity of migration in response to the cotton shortage. The age distribution of the implied migration flows is important for our study because young adults have low mortality rates relative to those in other age groups, meaning that even large in-flows of this group may contribute little to total mortality.[47]

## 6.3 Mortality

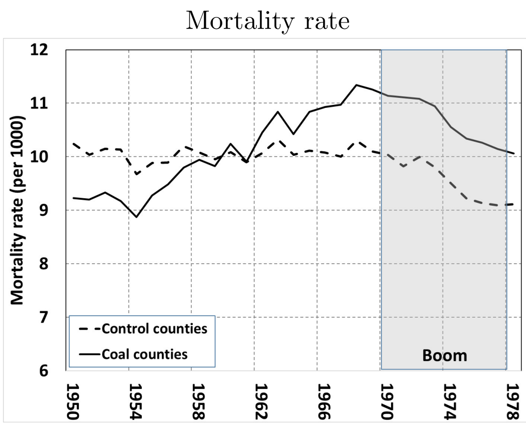
As in our cotton example, we begin in Figure 9 by providing graphical evidence on the impact of the boom—and the migration it induced—on mortality. The left-hand panel shows that mortality rates in the coal counties were rising prior to the boom and fell rapidly during the coal boom period. While the mortality rate was higher in the coal counties during the coal boom period, this figure does not give the impression that health became worse in the coal counties during the boom. Rather, the mortality rate in the coal counties appears to evolve roughly in parallel to the mortality rate in control counties during the boom.

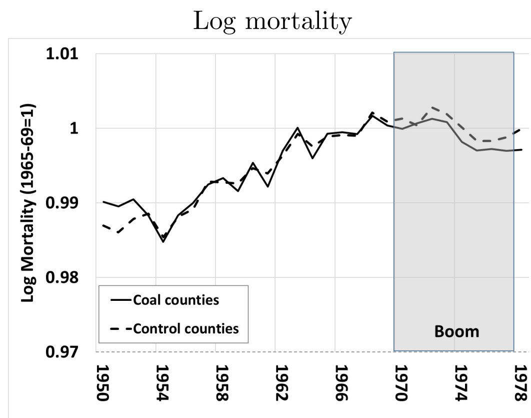
The right-hand panel of Figure 9 plots log mortality in the coal and control counties, normalized for comparability. Here we see that log mortality in both sets of counties track each other very closely from about 1955 through the main coal boom period. If anything, it appears that mortality in the coal counties may have dropped relative to the control counties during the boom. This is somewhat surprising given evidence that population was flowing into the coal counties during the boom.

[46] Similar results are also reported by BMS, who find that most of the increase in population in the coal counties from 1970-1980 was concentrated among those aged 20-29, particularly men.

[47] In particular, in the four states used in our main analysis the mortality rate for those aged 20-24 was 1.40 deaths per thousand in 1970 and for people aged 25-34 it was 1.44 per thousand. In contrast, the average death rate across the entire population in that year was 10.24 deaths per thousand, with the vast majority of deaths concentrated among those over age 55.

Figure 9: Mortality effects of the coal boom and bust




Mortality data from Bailey *et al.* (2016). Population data are from the Census and annualized using Das Gupta interpolation.

The striking difference between the left and right-hand panels of Figure 9 tells us that *essentially all* of the mortality-rate differences observed in these two groups of counties from the mid-1950s through 1977 are due to population movements. Comparing the mortality-rate movements illustrated in Figure 9 to the population movements in Figure 7 reinforces this result: as population in the coal counties falls in the 1950s and 1960s, the mortality rate increases, and as population rebounds in the 1970s, the mortality rate falls. Despite all this, deaths in the coal and control counties trend together throughout. In Appendix Figure 22 we present additional evidence comparing the evolution of mortality within different age groups in coal and control counties. These results are similar to the all-age results shown in Figure 9—that is, the evolution of mortality looks similar in the treatment and control counties.

The fact that the increase in population in the coal counties during the boom was not matched by an increase in deaths suggests that the migrants entering these counties during the boom were less likely to die than the average resident of these counties on the eve of the boom. This pattern is consistent with evidence suggesting that most migrants were young adults.[48] Because these migrants had very low mortality risk, they had little effect on the number of deaths, but their presence in the population did make it *appear* that the overall mortality *rate* had declined. Overall, the patterns in Figure 9 suggest that the economic shock had little impact on total mortality, and that changes in the the mortality rate primarily reflect changes in the size and composition of the population.

Next, we examine the results obtained when applying the three alternative econometric approaches discussed in Section 4. To illustrate the problems caused by the migration-driven violation of the parallel trends assumption when using the log mortality rate as the dependent variable (as evidenced in the left panel of Figure 9), we show results both without and with county time trends. Our results are in Table 4. The top panel presents estimates our three alternative specifications without county time trends, while the bottom panel presents estimates with time trends. Standard errors are clustered by county.[49]

As in the cotton example, the results in Table 4 are highly sensitive to assumptions made about how to model the relationship between population and mortality in the short run. Results change substantially both as we move from Columns 1-3, and and as we include or exclude county time trends. The results obtained using the mortality rate as the dependent variable, in Columns 1a and 1b, are particularly sensitive to the inclusion of time trends. In contrast, results in Columns 3a and 3b are less sensitive to the inclusion of time trends. It is also worth noting that in the results in Columns 2a and 2b, the coefficient on the population term is well below one, suggesting that the one-to-one relationship between population and mortality embedded in the specification used in Columns 1a and 1b is likely to be violated. Indeed, this coefficient is consistent with the boom-time in-flows of healthy workers

[48]We provide further evidence of the boom-induced change in the age distribution of the coal-county population in Appendix C.2.

[49]Results using spatial standard errors are available in Appendix Table 15. These deliver similar results.

who contributed less-than-proportionally to coal-county deaths.

Given the patterns described in Figure 9 and the evidence on migration, our assessment is that the results in Columns 1a and 1b of Table 4 are likely to provide misleading results that are driven primarily by changes in the population denominator. The results in Columns 2 and 3 are likely to be more reliable. These estimates suggest that the coal boom had little effect on total mortality.

Table 4: Estimated mortality effects of the coal boom

| Dependent variable: | Ln(MR) | Ln(MORT) | Ln(MORT) |
|---|---|---|---|
| | **Panel A: Without county time trends** | | |
| | (1a) | (2a) | (3a) |
| Coal county × boom | 0.0958*** | 0.0104 | -0.0225* |
| | (0.0172) | (0.0113) | (0.0117) |
| Ln(Pop) | | 0.278*** | |
| | | (0.0298) | |
| | **Panel B: With county time trends** | | |
| | (1b) | (2b) | (3b) |
| Coal county × boom | -0.0375*** | -0.00306 | 0.00947 |
| | (0.00958) | (0.00906) | (0.0104) |
| Ln(Pop) | | 0.267*** | |
| | | (0.0427) | |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. All regressions include county fixed effects and year effects. N=7,672.

As in our cotton example, our shock begins in a census year, which provides a convenient way to examine the stability of the results as population measures become more vulnerable to migration-related error. Specifically, by looking at how results change as we expand the shock window away from the census year, provides additional evidence on how to amongst the specifications in Table 4. This is done in Table 5 for various windows starting in 1970. Panel A presents results using the log mortality rate as the dependent variable as per the standard approach, Panel B presents results using log mortality as the dependent variable while still controlling

Table 5: Results for various windows starting in 1970

| Shock years: | 1970-1971 (1) | 1970-73 (2) | 1970-75 (3) | 1970-77 (4) |
|---|---|---|---|---|
| | **Panel A – DV: Log Mortality Rate** | | | |
| Coal county × boom | -0.00876 | -0.0205* | -0.0302*** | -0.0347*** |
| | (0.0102) | (0.0105) | (0.0102) | (0.0106) |
| | **Panel B – DV: Log Mortality** | | | |
| Coal county × boom | 0.00409 | -0.00161 | -0.00717 | -0.00739 |
| | (0.0103) | (0.0106) | (0.00989) | (0.00988) |
| Ln(Pop) | 0.155** | 0.224*** | 0.280*** | 0.267*** |
| | (0.0771) | (0.0622) | (0.0523) | (0.0437) |
| | **Panel C – DV: Log Mortality** | | | |
| Coal county × boom | 0.00644 | 0.00387 | 0.00183 | 0.00258 |
| | (0.0104) | (0.0110) | (0.0106) | (0.0108) |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. All regressions include county fixed effects, year effects, and linear county time trends. N=6,028 in Column 1; 6,576 in Column 2; 7,124 in Column 3; 7,672 in Column 4..

for log population, and Panel C presents results with log mortality as the dependent variable, without including log population. There are two important patterns to note in Table 5. First, for windows closer to the census year, all three approaches yield similar results: the coal boom has very little impact on mortality. Second, as the size of the window increases, the results in Panel B continue to show evidence that the boom had little impact on mortality, while the results in Panel A change substantially, becoming statistically significant. Both of these patterns justify our using the third, log mortality, specification as our preferred approach. Moreover, the fact that the results in Panel A become negative and statistically significant as we move away from the census year is consistent with the expected effect of migration in this setting, as the inflow of young healthy workers pushes the population denominator up without much affecting the number of deaths.

The overriding message from Figure 9 and Tables 4-5 is that the boom had little impact on mortality, but a substantial impact on observed mortality rates. Our interpretation of these results is that the coal boom induced a large migration response, but one that was concentrated among young workers who carried very little mortality risk. For those who were at a higher risk of mortality (i.e., older residents), the changing economic conditions seem to have had little effect, such that we see no evidence of a change in their patterns of mortality. This is most likely because older residents and those with increased mortality risk were insured against economic shocks by retirements savings and programs such as Social Security, Medicare and Medicaid, and because they were unlikely to have been the recipients of the rise in mining incomes.

In summary, we draw two main conclusions from the experience of the Appalachian coal boom. First, as in the cotton downturn, we find evidence that the coal boom caused a substantial change in migration patterns. Second, we observe changes in the observed mortality rate across locations, but little effect on the number of deaths. Thus, it appears that changes in mortality rates were largely statistical, and were driven by changes in the population denominator. Consistent with this pattern, we also find evidence that much of the migration that took place was selective and concentrated among young workers, a population with very little mortality risk. Overall, we conclude that the coal boom had little effect on mortality, but that an analysis focused on the overall mortality rates may find large and spurious results driven by the unobserved short-term migration of young, healthy workers. Put more simply, as in the cotton downturn, migration has the capacity to meaningfully undermine inferences about the causal impact of business cycles on mortality—at times even flipping the *sign* on the estimated relationship.

# 7 Conclusion

Drawing on two natural experiments in very different settings, this paper assesses the sensitivity of estimates of the relationship between business cycles and mortality to unobserved migration. In both settings, we find that changes in local economic conditions generated a substantial migration response, and that this migration has the potential to systematically bias estimates of the impact of business cycles on mortality. In particular, we show that when short-run population flows are not well measured, estimates are highly sensitive to the choice of assumptions about the relationship between these changes in population and those in mortality. The standard approach used in the literature following Ruhm (2000), which takes the mortality rate as the dependent variable, appears to be especially vulnerable to bias generated by migration because it imposes strong assumptions on the relationship between population and mortality in the short run. These assumptions are likely to be violated in the presence of migration, resulting in misleading estimates. Importantly, migration need not be selective, nor does it need to confer a true health benefit or detriment, for it to generate the false impression of a change in health where there has been none. Taken as a whole, our results raise questions about the validity of the widely-used panel-data approach to estimating the relationship between business cycles and health.

If migration bias may pose serious problems in estimating the business cycle-mortality relationship, how can we correct for it? An ideal solution to migration bias concerns is to use individual-level panel data, as has been done by Gerdtham & Johannesson (2005) and Edwards (2008).[50] Unfortunately, this type of data is only available in a very small number of modern developed countries. In settings where such detailed data are not available, however, one alternative is to focus only on infant mortality, where migration is less of a concern.[51] However, the impact of

[50]Dehejia & Lleras-Muney (2004) also uses individual-level panel data in a study focused on infant health.

[51]Recent studies in this literature include Paxson & Schady (2005), Ferreira & Schady (2009),

business-cycle fluctuations on infant mortality may differ from that on other segments of the population, meaning that to focus on this outcome for the purposes of clean identification may be to miss a large part of the story. A second alternative is to focus on patterns at the national level, as is done by Cutler *et al.* (2016), whose cross-country approach abstracts from difficult-to-track internal migration flows. Finally, a third approach, proposed in this paper, is to carefully study individual events that provide plausibly exogenous variation in the incidence of temporary economic shocks, and to use the available evidence in the empirical context to diagnose and mitigate migration bias.

Bhalotra (2010), Miller & Urdinola (2010), Baird *et al.* (2011), Cruces *et al.* (2012), Friedman & Schady (2013), and Bozzoli & Quintana-Domeque (2014). Many, though not all, of these studies find that negative economic shocks increase infant mortality.

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# A Appendix: Literature, concepts and methods

## A.1 Review of selected related literature

Below, 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.

| | 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 |

| | | | | | | |
|---|---|---|---|---|---|---|
| 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 |

## A.2 Intercensal population estimation

### A.2.1 Assumptions about population growth in the presence of migration

A key issue in this literature has to do with the fact that in most intercensal years (indeed, in 9 out of every 10), the available population measure is an interpolated variable that does not perfectly reflect the true underlying population. This can seriously bias estimates of the true relationship between business cycles and health, because in the presence of short-term migration flows, this interpolation error is systematic rather than random.

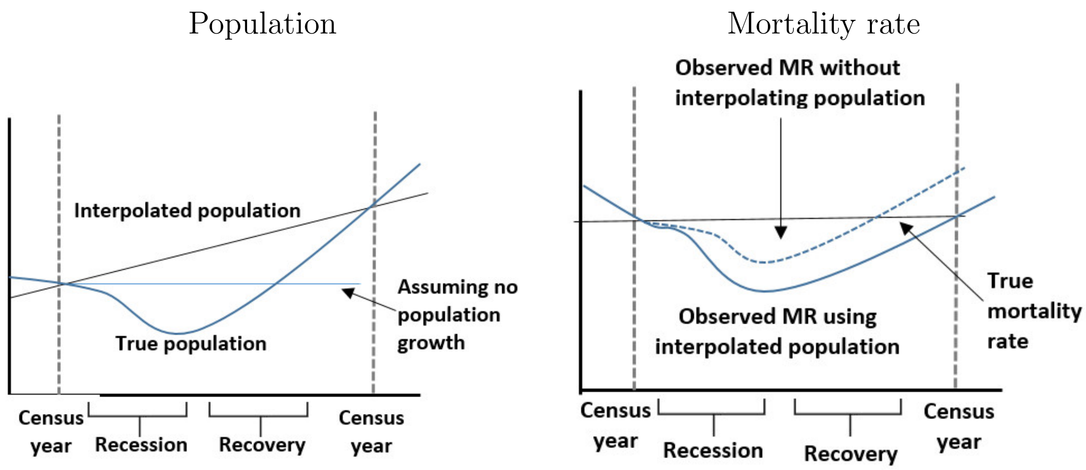
As an example of the potential issue generated by interpolation, consider a hypothetical situation in which a district experiencing long-run population growth is hit by a short-run recession, followed by a recovery. Further, assume that the recession causes out-migration which is reversed during the recovery period. These are essentially the conditions observed in the case of the cotton shortage. Finally, to keep things simple, assume that the recession has no impact on the mortality rate.

The left-hand panel of Figure 10 describes the evolution of population in this hypothetical setting. In this example, the interpolated population builds in population growth during the recession, even though population was falling during that period. If that interpolated population is used in the denominator when calculating the mortality rate, as shown in the right-hand panel, then it will make it appear that the mortality rate fell during the recession even when it did not.[52] If instead we do not use the interpolated population information (as in Eq. 4), then in this case, we will obtain a mortality rate estimate that is closer to the true value. Thus, including the interpolated population value in the regression does not necessarily lead to a more accurate counterfactual.

The main conclusion to draw from this discussion is that when there is a migration response to economic shocks, assumptions made about the evolution of population and the relationship between population and mortality in the short versus long run are important. Moreover, in the absence of accurate annual population estimates, it is not necessarily true that using interpolated values will lead to more accurate results. Indeed, erroneous assumptions about population change may *introduce* new sources of bias into our calculations.

[52]Note also that this interpolation does not merely result in population mis-measurement in the recession period. Rather, by forcing part of the growth that occurred at one point in the intercensal period into another, this approach also leads population in the recovery sub-period to be underestimated, which will further contribute to the spurious impression that the recession was good for health.

Figure 10: Example




### A.2.2 Errors of closure and Das Gupta interpolation

The results presented in this study suggest that migration bias has an important impact on the estimated business cycle-mortality relationship in both of our empirical settings.[53] Should we be concerned that migration may cause similar issues in other contexts? The answer to this question depends in large part on the accuracy of intercensal population estimates.

Except in the rare few studies that make use of individual-level panel data (e.g. Ruhm (2003), Dehejia & Lleras-Muney (2004), Gerdtham & Johannesson (2005), and Edwards (2008)), and which thus abstract from denominator issues altogether, even modern developed-country studies rely on intercensal figures obtained via interpolation from decennial censuses of population.

For instance, the standard approach used by the U.S. Census Bureau, termed the Das Gupta method, calculates annual estimates of population using a base and

[53]Indeed, despite the fact that we might expect this bias to be most problematic in settings—such as historical and developing-country ones—with mobile labor forces and a poor capacity to track such movements intercensally, our coal boom analysis nevertheless indicates that migration bias can be an issue even in a modern rich-country setting.

terminal year census count combined with annual information on births, deaths, and net internal and international migration.[54] Specifically, for each intercensal year, they calculate a cumulative population value that adds to observed population in a base census year all the net change in population (up to that year) due to natural increase (i.e. births minus deaths) and migration (i.e. internal and international migrant inflows minus internal and international migrant outflows). Working in this manner until they reach the next census year, they obtain what is called a "postcensal estimate" for the entire 10-year period. This represents the population we would expect to find in the terminal census, based on the observed initial census count and all known entries and exits from the population. The difference between this postcensal estimate and the terminal year's observed census count is termed the *error of closure*, and is equivalent to -1 times the residual implied net migration that has gone unobserved in the intercensal period. We will return to this concept shortly. Finally, to create the intercensal estimates of population, this error of closure is then distributed across intercensal years geometrically, thus "topping up" the year-specific value obtained on the way to the postcensal estimate (see U.S. Census Bureau (2012a) for further details of the procedure).[55]

Importantly, in addition to introducing systematic measurement error by distributing implied residual migration agnostically across intercensal years (in the manner described by the stylized example in Section A.2.1), Das Gupta interpolations will also be endogenous, since intercensal values will always depend on the terminal census count, which is itself a function of any shock-induced changes in population during the intercensal period (whether through shock-induced migration or shock-induced changes in fertility and mortality).

As mentioned above, on the way to calculating Das Gupta estimates, we also

[54] To compute the latter two measures, census officials rely on a wealth of information, such as Social Security and Medicare records and records of troops stationed abroad.

[55] It is this Das Gupta method that we use in all the intercensal population estimates presented in this paper (albeit, without adjustments for net migration, since in both samples we lack the data to do so).

calculate a useful measure which can serve as a diagnostic of data quality, particularly as it pertains to unobserved migration: the error of closure. The error of closure can be interpreted as the (negative of)[56] implied net migration that has gone unobserved: the higher the magnitude of the error of closure, the less well observed is migration. Furthermore, the larger the error of closure, the more important are assumptions about how to distribute growth across intercensal years.[57] [58]

To get a sense of the quality of best-available intercensal population estimates and the likelihood of bias related to unobserved migration in studies using these data, we now compare the errors in the intercensal population estimates available in our settings to the errors found in several modern developed and developing countries. In particular, we calculate the error of closure at several levels of geographic aggregation for three wealthy countries (England and Wales, the United States, and Canada), and for one developing country (India). We compare these to similar figures obtained from our historical data for England and Wales. These errors of closure, reported in absolute value as a percentage of final-year population, are presented in Table 6.

Here, we find evidence that migration is imperfectly observed—and so, may be imperfectly reflected in intercensal population—even in modern rich-country settings that we might expect to be least prone to such error. Panel A shows that migration-adjusted errors of closure at the national level are non-negligible, ranging from 0.10% to 2.42% in magnitude. Here, our historical estimates for England & Wales, which, crucially, cannot adjust for migration, compare favorably. For contrast, modern errors

[56]Indeed, the error of closure is conceptually identical to the net migration rates used in papers like Fishback *et al.* (2006), which study migration.

[57]It is worth noting that the error of closure is not a perfect measure of the potential for migration bias because it only reflects unobserved net migration. This will miss, for example, migration away from areas experiencing a recession which is reversed before the next census is taken.

[58]Thus, errors of closure resulting from best-available data can be thought of as the residual implied net migration above and beyond that which was observable, and which was therefore accounted for in the postcensal estimate. In settings, such as ours, where postcensal estimates do not adjust for migration (since migration in these settings cannot be directly observed), the error of closure represents the *total* implied net migration. Accordingly, we would naturally expect our errors of closure to be larger than those calculated using best-available methods and modern data.

of closure from India are several times higher, ranging from 4.61-9.94%.

Naturally, errors of closure are larger at lower levels of spatial aggregation, where postcensal statistics may fail to fully capture internal migration. In Column 5 of Panel B, we present estimates for the comparator that, given the country and the size of the spatial unit we consider in our main analysis,[59] is perhaps most relevant to our empirical setting: modern England and Wales at the Local Authority District (LAD) level. Using best-available methods, LAD-level errors of closure are on average roughly one quarter the size of those we obtain from historical England and Wales at the Registration District (RD) level.[60] Comparing measures on a more consistent basis (that is, where neither error of closure adjusts for migration) reduces this accuracy gap substantially: average historical errors of closure at the RD level are only half that for modern LADs. Indeed, comparing migration-adjusted to non-migration-adjusted errors of closure for those modern developed-country units for which it is available, it is clear that even while partially adjusting for migration, best-available modern errors of closure still leave a substantial portion of migration unaccounted for.[61]

[59]Disaggregating our historical errors of closure by district type, and considering raw (rather than absolute) errors of closure so that we can account for the direction of implied net migration flows, we find that at the district level, our average error of closure is 6.2% over 1851-1861 and 5.6% over 1861-1871. For cotton districts, this average is -0.54% and 1.3%, respectively; for nearby (within 25 km) districts, 2.6% and 0.9%; and for all other districts, 6.7% and 6.0%. These figures outline broad patterns in migration during the downturn. Specifically, they suggest that total net implied migration was no different before the downturn in "all other" districts than it was during it, but that cotton districts that had been net gainers of migrants in the decade preceding the downturn became net donors during it. Similarly, nearby districts that had been net donors of migrants prior to the downturn (for instance, fueling the industrial expansion of neighboring cotton districts), saw a large drop in their net "export" of population during the 1860s. Taken together, these findings are consistent with the broader results in our study, and particularly with the migration patterns implied by Figure 3.

[60]Errors of closure look similar for other small, sub-provincial units in modern rich countries. See, e.g., errors of closure reported for census metropolitan areas and economic regions of Canada, which in 2011 were as high in magnitude as 3.0% and 4.4%, respectively (Statistics Canada, Demography Division, 2016).

[61]Note that while non-zero errors of closure may represent a source of bias due to cumulative unobserved migration over the entire decade, Das Gupta and similar annual interpolated population estimates may exacerbate this bias by introducing further error into the data and estimation process. Namely, such estimated denominators make implicit assumptions about the distribution of the decade-wide residual net migration implied by the error of closure, and thus about the timing

Furthermore, there is considerable cross-unit heterogeneity in the error of closure: even adjusting for migration, 65% of modern English and Welsh LADs have errors of closure above 1.00%, with the City of London boasting a whopping 52.71% rate of error. More importantly from an estimation standpoint, it should be noted that although this table presents the absolute value of the error of closure, inaccuracy is liable to be compounded where errors of closure move in opposite directions in recession-stricken and migrant-receiving spatial units—say, because of unobserved migration from one unit to the other. Indeed, distributions of the errors of closure for modern sub-national units tend to be roughly centered on zero, whereas the historical distributions lie much farther to the right (not reported). This in turn means that intercensal denominator gaps in a differences-in-differences framework may be more highly exaggerated in these modern settings, biasing the shock coefficient downward.

What influences the size of the error of closure? Stratifying further, and using the U.S. over 1990-2000 as an example, we find that at its smallest, the error of closure is 0.37%, among the relatively immobile elderly (age 85+). Meanwhile, the modern U.S. error of closure is understandably largest in the lower end of the prime working-age population (25-34 years old), where it is -5.92% (U.S. Census Bureau, 2010). This pattern is consistent with labor mobility and other age-related factors that may make this age group particularly hard to track as they move, given the sort of tax and Medicare data the U.S. Census Bureau uses to make these migration adjustments. Indeed, the 1990-2000 census period in the U.S. has been widely acknowledged as having an unusually large error of closure, and this large error in turn has been attributed to precisely those issues we highlight as concerns in this analysis: the mis-measurement of highly mobile populations—in the case of the U.S. in the 1990s, the growing Hispanic population (Robinson & West, 2005).[62] Recent studies

of migration flows.

[62]Further undermining the accuracy of such analyses is the fact that those populations that may be especially vulnerable to mis-measurement (for reasons including but not limited to their higher geographic mobility) may also face different mortality risk profiles and access to medical and social welfare services.

in the medical and biostatistics literatures have also raised the issue of denominator measurement error, highlighting in particular the issue of measurement error in small demographic strata, such as those by age and race (see Phipps *et al.* (2005) for a discussion of intercensal interpolation-driven bias in estimates of breast cancer incidence rates, and Hund (2012, Ch. 4-5) for a broader methodological discussion of related denominator issues).[63]

[63]For instance, Phipps *et al.* (2005) suggest that "areas with a high growth rate, a large population of retirees, or a large population of foreign-born individuals are likely to be underestimated, while areas with high poverty, and areas with a negative growth rate are likely to be overestimated." Using alternative denominators, they find "the DOF-based [breast cancer incidence] rates for Marin County were approximately 22% higher than census-based rates based on the same numerators." It should be noted that this 22% bias does not include the additional bias that could occur in the presence of spillovers, which in a differences-in-differences framework could compound denominator error if undercounts in one area lead to overcounts in the region used as a comparator.

Table 6: Absolute error of closure (%)

**Panel A: National**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Country | E&W | E&W | E&W | E&W | US | US | Canada | Canada | Canada | India | India |
| Period | 1851-1861 | 1861-1871 | 1991-2001 | 2001-2011 | 1990-2000 | 2000-2010 | 1996-2001 | 2001-2006 | 2006-2011 | 1991-2001 | 2001-2011 |
| EOC (Natural increase only) | 0.61 | 0.25 | 2.21 | 4.41 | | 3.29 | | | | 9.94 | 4.61 |
| EOC (Migration-adjusted) | | | 0.92 | 0.40 | 2.42 | 0.16 | 0.20 | 0.10 | 0.50 | | |

**Panel B: Sub-national**

| Country | E&W | E&W | E&W | E&W | E&W | E&W | US | Canada | Canada | Canada | India |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Geo Unit | RD | County | RD | County | LAD | County | State + DC | Province | Province | Province | State |
| Period | 1851-1861 | 1851-1861 | 1861-1871 | 1861-1871 | 2001-2011 | 2001-2011 | 2000-2010 | 1996-2001 | 2001-2006 | 2006-2011 | 2001-2011 |
| Mean EOC (Natural increase only) | 10.68 | 7.23 | 10.44 | 6.56 | 4.97 | 4.93 | 4.26 | | | | 6.31 |
| Min | 0.02 | 0.32 | 0.01 | 0.20 | 0.01 | 0.82 | 0.16 | | | | 0.59 |
| Max | 31.91 | 18.68 | 68.44 | 19.55 | 16.02 | 8.85 | 19.28 | | | | 21.73 |
| Share < 1.00% | 0.03 | 0.13 | 0.05 | 0.13 | 0.10 | 0.02 | 0.20 | | | | 0.11 |
| Share > 5.00% | 0.78 | 0.69 | 0.76 | 0.60 | 0.44 | 0.49 | 0.33 | | | | 0.46 |
| | | | | | | | | | | | |
| Mean EOC (Migration-adjusted) | | | | | 2.26 | 1.10 | 0.96 | 0.73 | 0.95 | 0.97 | |
| Min | | | | | 0.01 | 0.02 | 0.00 | 0.00 | 0.00 | 0.00 | |
| Max | | | | | 52.71 | 2.69 | 4.80 | 2.20 | 3.20 | 2.10 | |
| Share < 1.00% | | | | | 0.35 | 0.53 | 0.73 | 0.54 | 0.62 | 0.54 | |
| Share > 5.00% | | | | | 0.10 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | |
| | | | | | | | | | | | |
| Observations | 539 | 55 | 539 | 55 | 347 | 55 | 51 | 13 | 13 | 13 | 35 |

We report the absolute value (magnitude) of the error of closure in the total population, calculated as a percentage of the final-year census count of population. Figures are given for England & Wales, the United States, Canada, and India, as well as for smaller geographic units within these countries (i.e. Registration Districts, Local Authority Districts, counties, and states/provinces). "EOC (natural increase only)" refers to the error of closure based on accounting for births and deaths only. "EOC (migration-adjusted)" refers to the error of closure based on accounting for births, deaths, internal and international migration, and other similar adjustments. With the exception of the figures for all of Canada and for the US in 1990-2000, which are taken directly from Statistics Canada, Demography Division (2016) and U.S. Census Bureau (2010), respectively, the table reports the authors' calculations based on data from United Kingdom Office for National Statistics (2002, 2003, 2011, 2012, 2014), U.S. Census Bureau (2011a,b, 2012b, 2016), and Office of the Registrar General & Census Commissioner, India (2013, 2016).

Together, these findings suggest that the bias resulting from poor intercensal population denominators is likely to be a significant factor in a variety of settings, even beyond the obvious historical and developing-country ones. From a practical perspective, as both our coal boom analysis and these error-of-closure comparisons imply, even modern rich-country studies should be concerned about denominator-interpolation problems that could arise when studying granular, sparsely populated,[64] or rapidly growing geographic regions; small strata (for instance, by age, gender, race, and the interactions of these, especially where the sub-population in question may also experience mortality risk that is different from that of the general population); and populations that are vulnerable, mobile, and/or prone to under-registration (e.g. in the modern U.S., Hispanics and immigrant populations and young working-age people more generally). What's more, in the presence of migration spillovers, this bias will be further magnified when using standard panel data approaches to estimation.

For simpler comparisons, in Table 7 we also report the error of closure in our two empirical settings (which should be interpreted as total implied net migration) alongside just the best-available ones from other settings (which should be interpreted as residual implied net migration).[65] Columns 1-4 compares the error of closure in England & Wales during the historical period that we study to those in the most recent census. We can see that the modern census is doing much better than the census in our setting at both the district and county level. However, we can also see that unobserved net migration in the modern census is still equal to over 1% of population at the county level and over 2% at the district level. Similarly, for U.S. states or Canadian provinces, we observe errors of around 1% of the population. This

[64]In sparsely populated or difficult-to-monitor regions, errors can be even higher: for instance, average absolute errors of closure for sub-provincial units are highest in rural provinces/territories, such as the Northwest Territories, whose census divisions had an average absolute error of closure of 3.5% in 2011 (Statistics Canada, Demography Division, 2016).

[65]When analyzing this table it is useful to remember that errors of closure will generally be smaller at higher levels of aggregation. For example, unobserved net migration between two districts within the same county will influence the error of closure at the district level but will not increase the error of closure at the county level.

Table 7: Absolute error of closure (%), best available technique

| Country | England & Wales | England & Wales | England & Wales | England & Wales | US | Canada | India |
|---|---|---|---|---|---|---|---|
| Geo. Unit: | District | District | County | County | State | Province | State |
| Period | 1861-71 | 2001-11 | 1861-71 | 2001-11 | 2000-10 | 2006-11 | 2001-11 |
| Mean EOC | 10.44 | 2.26 | 6.56 | 1.10 | 0.96 | 0.97 | 6.31 |
| Min | 0.01 | 0.01 | 0.20 | 0.02 | 0.00 | 0.00 | 0.59 |
| Max | 68.44 | 52.71 | 19.55 | 2.69 | 4.80 | 2.10 | 21.73 |
| Share $< 1\%$ | 0.05 | 0.35 | 0.13 | 0.53 | 0.73 | 0.54 | 0.11 |
| Share $> 5\%$ | 0.76 | 0.10 | 0.60 | 0.00 | 0.00 | 0.00 | 0.46 |
| Observations | 539 | 347 | 55 | 55 | 51 | 13 | 35 |

This table reports the absolute value (magnitude) of the error of closure in the total population, calculated as a percentage of the final-year census count of population. For England & Wales, the values reported for 1861-1871 are from the consistent registration districts used in this study while the values reported for 2001-2011 are for local area districts. For the U.S. we report values for the 50 states plus the District of Columbia. In each case, the best-available technique is used to calculate the EOC; for instance, the figures from historical England & Wales and modern India adjust for natural increase only, while those for the modern England & Wales, the U.S., and Canada adjust for natural increase as well as observed internal and international migration. With the exception of the figures for all of Canada which are taken directly from Statistics Canada, Demography Division (2016) the table reports the authors' calculations based on data from United Kingdom Office for National Statistics (2002, 2003, 2011, 2012, 2014), U.S. Census Bureau (2011a,b, 2012b, 2016), and Office of the Registrar General & Census Commissioner, India (2013, 2016).

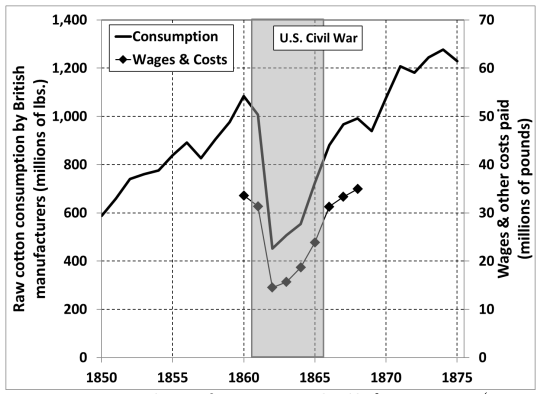
suggests that even with the additional data used to track migration in the modern census, migration bias may still be a concern, though any bias is likely to be less severe than in the settings that we study. In the last Column of Table 7, we look at errors of closure at the state level in India. Here we observe that the average error of closure is over 6% of the state population and the largest error was over 20% of the state population. This suggests that in India, and likely in other developing countries, migration bias may be a serious concern.

# B Appendix: The Lancashire cotton shortage

## B.1 Additional evidence on the effects of the cotton shock

Figure 11 describes domestic raw cotton consumption in Britain from 1850-1875. This is the best available measure of the change in production in the industry across this period. The graph also describes the evolution of payments for wages and other variable costs, other than cotton, in the industry, from 1860-1868. Both of these statistics suggest that the shock period was characterized by a large reduction, equal to roughly half of pre-war production, in both industry production and wage payments.

Figure 11: British domestic cotton consumption and input payments, 1850-1875



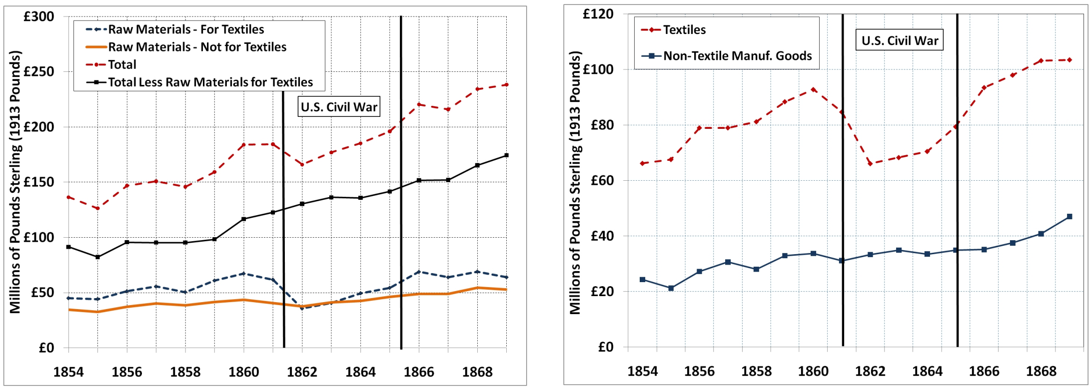

Domestic raw cotton consumption data from Mitchell & Deane (1962). Wage and cost data from Forwood (1870).

To look for other effects of the U.S. Civil War on the British economy, a natural starting point is to look at imports and exports. The left-hand panel of Figure 12 focuses on imports. This figure shows that, once imports of raw cotton are excluded, there do not appear to be any substantial changes in either total imports or raw material imports to Britain. This makes sense given that raw cotton made up 67% of total British imports from the U.S. in 1860. Of the other major U.S. exports to Britain, only tobacco was heavily sourced from the South, and that made up only 2.6% of imports from the U.S.

The right-hand panel of Figure 12 shows the behavior of exports from Britain over the study period. There was a substantial drop in exports of textiles during the U.S. Civil War period, which was due entirely to exports of cotton (these were relatively good years for other textile industries such as wool and linen). However once textile exports are removed there is no evidence of a substantial change in British exports

Figure 12: British imports and exports, 1854-1869

Imports to Britain　　Exports from Britain




Data from Mitchell (1988).

during the Civil War period.

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

One sector of the British economy that was substantially affected was the shipping industry. To avoid the risk of capture by Southern privateers, many U.S. merchant ships, which were primarily owned by Northern shipping interests, were transferred to British ownership during the U.S. Civil War. This resulted in a substantial expansion of the British merchant fleet, which had impacts in major shipping centers particularly Liverpool. To account for the potential effect of these changes, in robustness exercises we explore the impact of dropping Liverpool and London, the two most important British ports, from our data.

## B.2 Responses to the cotton shortage

Contemporary reports offer a mixed view of the impact that the cotton shortage had on health. Some 19th century observers, such as Arnold (1864), report that there was a "lessened death-rate throughout nearly the whole of the [cotton] district, and, generally speaking, the improved health of the people." In the words of the Registrar of Wigan, these gains were attributed primarily to "more freedom to breathe the fresh air, inability to indulge in spirituous liquors, and better nursing of children."[66] On the other hand, there were also reports of negative health effects due to poor nutrition and crowded living conditions.[67] 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.

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.[68] Workers who found themselves unemployed responded, first, by reducing costs and dipping into any available savings, and later, by pawning or selling items of

[66]Quoted from the *Report of the Registrar General*, 1862. The importance of childcare is highlighted in a number of reports, such as Dr Buchanan's 1862 *Report on the Sanitary Conditions of the Cotton Towns* (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 304), which discusses the importance of the "greater care bestowed on infants by their unemployed mothers than by the hired nursery keepers.". This channel was likely to be particularly important in the setting we study because female labor force participation rates were high, even among mothers. Using 1861 Census occupation data, we calculate that nationally, 41% of women over 20 were working and they made up 31% of the labor force. This rate was much higher in major cotton textile areas. In districts with over 10% of employment in cotton textiles in 1861, the average female labor force participation rate for women over 20 was 55% and women made up 38% of the labor force. For comparison, these are similar to the levels achieved in the U.S. in the 1970s and 1980s (Olivetti, 2013), though of course the nature of the work done by women was quite different.

[67]Dr Buchanan, in his *Report on the Sanitary Conditions of the Cotton Towns*, states that "There is a wan and haggard look about the people..." (Reports from Commissioners, British Parliamentary Papers, Feb-July 1863, p. 301). Typhus and scurvy, diseases strongly associated with deprivation, made an appearance in Manchester and Preston in 1862 after being absent for many years, while the prevalence of measles, whooping cough, and scarlet fever may have also increased (*Report on the Sanitary Conditions of the Cotton Towns*, Reports from Commissioners, British Parliamentary Papers, Feb-July 1863).

[68]For further details on mortality patterns in Britain during this period, see Appendix **??**.

value, including furniture, household goods, clothing and bedding (see 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 discussed briefly in Section 5.2, many left cotton districts in search of work in other areas.

The recession also generated an unprecedented institutional response aimed at relieving the suffering in cotton districts. Contemporary reports largely credit public relief efforts for the fact that no widespread famine occurred during the recession.[69] Relief funds came in two main forms. First, funds were provided at the local level through the Poor Law Boards, the primary system for poor relief in Britain during this period.[70] 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

[69]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."

[70]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.

works employment began in 1863, after the worst of the crisis had passed.[71]

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).

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—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 cotton districts in search of work in other areas.

## B.3 Data used in the cotton shock analysis

To assess the health consequences of the cotton shortage, we construct a new panel of annual district-level mortality spanning 1851-1865. These detailed data, which we digitized from original reports of the Registrar General, include information on both the age and cause of death for over 600 registration districts covering all of England and Wales.The registration district-level tabulations are the finest geographic level covering the demography of all of England and Wales annually in this period. Previously available data from the Registrar General's reports, digitized by Woods (1997), is reported only at the decade level, and so is insufficiently detailed for our

[71]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.

analysis. For an in-depth discussion of the Registrar General's data, see Woods (2000).

We also collect information from the Registrar General's reports on district population and births, which allows us to calculate mortality rates and to assess fertility responses to the crisis. 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 interpolate intercensal population using the Das Gupta method, per U.S. Census Bureau best practices (U.S. Census Bureau, 2012a).[72]

In our main analysis, we take the entire U.S. Civil War, 1861-1865, as shock period. As noted above, contemporary reports suggest that most of the adverse impacts of the U.S. Civil War were concentrated in the first three years of the event, but we focus primarily on the entire Civil War period so as to avoid concerns that our main results may be dependent on the choice of shock years that we consider. In particular, as part of the government response to the cotton shortage, a number of public works projects focused on sanitary improvements were undertaken during the recession. Most of these did not come into operation until late in the 1861-1865 period. In addition, migration occurring during the shock period was also likely to have affected mortality patterns in the post-shock period. Focusing on the pre-shock and shock periods avoids concerns about how to treat these factors.

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. Since the location of industries is highly persistent over time, we use

[72]The Das Gupta method accounts for the number of births and deaths in a district in each year, and distributes decennial residual population (the "error of closure," or the difference between enumerated population in the terminal year and population estimates at that date based on natural increase over the preceding decade) across the intercensal years. This method is used by the U.S. Census, though the Census Bureau also uses additional information from tax returns and Medicare claims to provide a partial adjustment for migration. For further discussion see Section A.2.2.

use data from 1851 rather than from 1861 to avoid the possibility that our measure may be influenced by events occurring in cotton districts at the time of enumeration. Nevertheless, we provide robustness results using occupation data from the 1861 Census. 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 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 539 consistent districts in the main analysis.[73] Summary statistics for these 539 districts appear in Table 8.

Table 8: Summary Statistics

| Panel A: Full sample of districts | | | | |
|---|---|---|---|---|
| | (1) Mean | (2) Standard deviation | (3) Min | (4) Max |
| Average annual deaths (full sample) | 809.23 | 1050.19 | 34 | 11,256 |
| Cotton employment share (1851 census) | 0.017 | 0.07 | 0 | 0.51 |
| Nearby cotton employment (1851 census) | 4569.01 | 20,175.54 | 0 | 158,490 |
| Population (1851 census) | 33,260.87 | 35,019.68 | 2493 | 284,126 |
| **Panel B: Cotton districts only** | | | | |
| Average annual deaths prior to shock | 1943.89 | 1546.46 | 207 | 7957 |
| Average annual deaths during shock | 2133.1 | 1684.46 | 199 | 8900 |

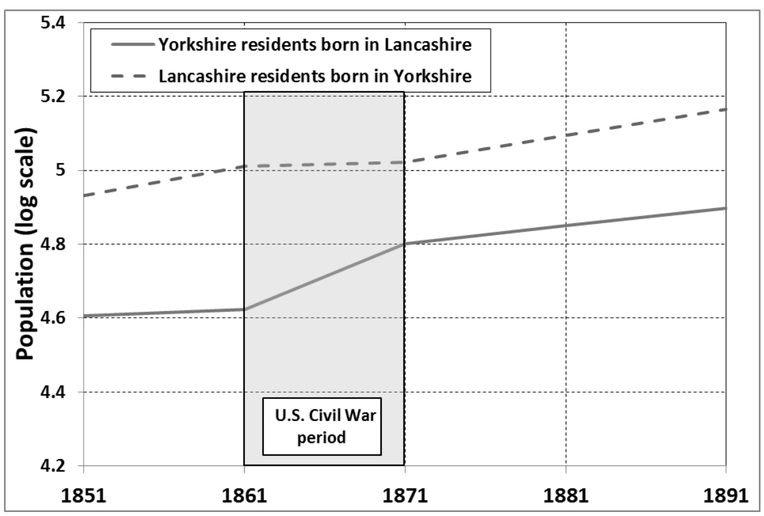
Full sample includes 8085 district-year observations spanning 1851-1865 for 539 unique districts. For the statistics that only draw on district-level data, there are District-level annual death data transcribed from annual reports of the Registrar General. Cotton employment share is simply the share of the total workforce (in 1851) that was employed in the cotton industry. 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.

[73] 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.

## B.4 Additional results on migration during the cotton shock

**B.4.0.1 Internal migration** Additional evidence on migration during the cotton shock can be gleaned from the location-of-birth information provided in the census. Specifically, changes in people in the population of people born in one location who are resident in another can be used to provide evidence on net migration between locations. The location-of-birth data are only available at the county, level, so in Figure 13, which is reproduced from Hanlon (Forthcoming), we compare the largest cotton textile county, Lancashire, with the neighboring wool textile county of Yorkshire. The figure indicates that the number of Yorkshire residents who were born in Lancashire increased substantially from 1861-1871, while the number of Lancashire residents born in Yorkshire stagnated. This suggests an out-migration of Lancashire residents during the U.S. Civil War, as well as reduced in-migration to Lancashire.

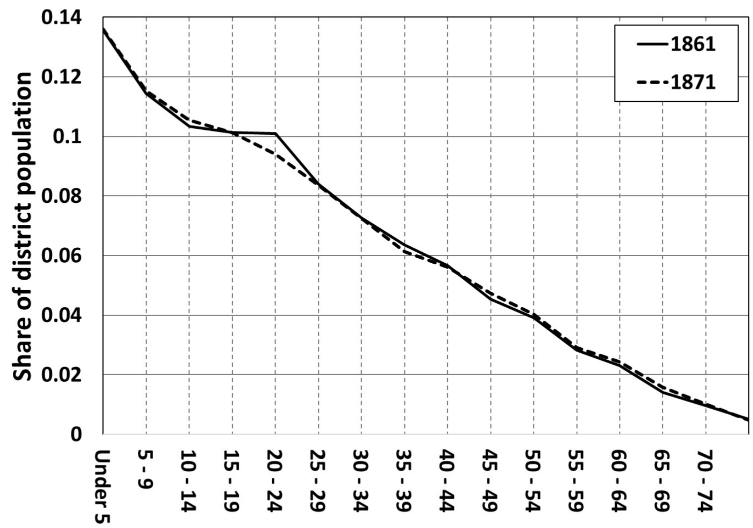
Figure 13: Evidence of migration for Yorkshire and Lancashire from birthplace data




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

Next, we consider some results that help us think about how migration patterns varied across age groups. Figure 14 describes the share of the population in each age category up to 79 in the cotton 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 cotton districts in 1861, which had largely disappeared by 1871. This suggests that the migration response to the shock was strongest among young adults.

Figure 14: Share of population in each age group in cotton districts

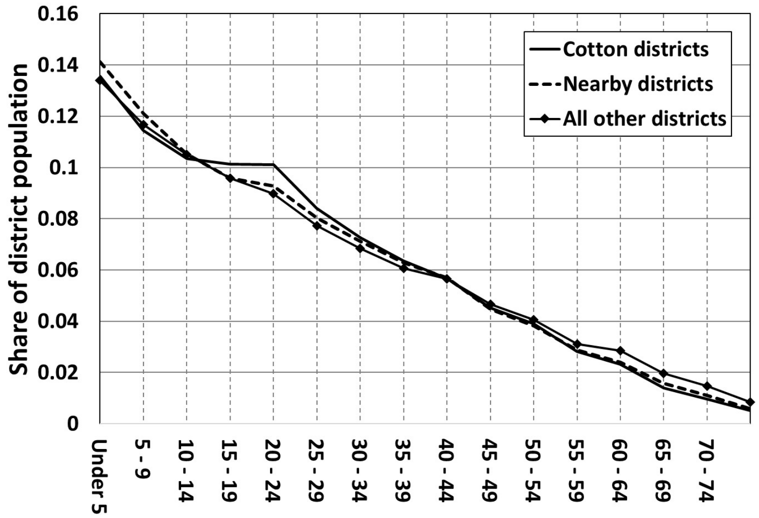



Population data are from the Census of Population for 1861 and 1871. Cotton districts are identified as those with over 10 percent of workers employed in cotton textile production in the 1851 Census, as in the main analysis.

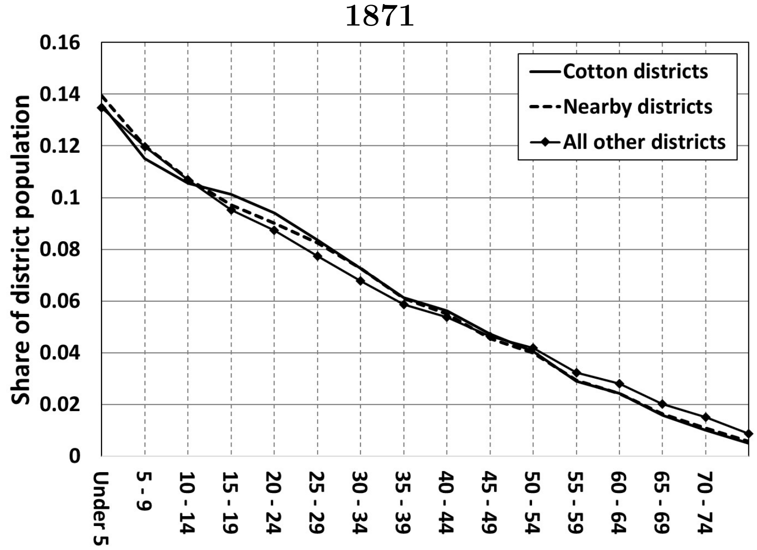
An alternative view of the same pattern is provided in Figure 15, which compares the share of population by age group in cotton districts, nearby districts, and all other districts. This is done for 1861 in the top panel and for 1871 in the bottom panel. In 1861, cotton districts had a much larger share 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 cotton districts in 1861, into nearby districts where, by 1871, they appear in the 25-29 and 30-34 age groups.

Figure 15: Share of population in each age group by type of district

**1861**



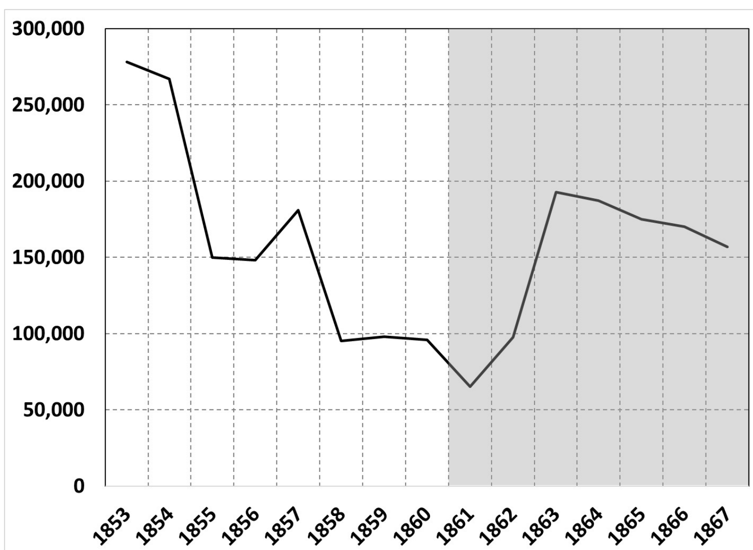

**1871**




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 cotton districts.

**B.4.0.2 Emigration from the UK** Tracking emigration from the U.K. in response to the cotton shock is more difficult that tracking internal migration. What information is available was collected at the ports of embarkation and reported in the British Parliamentary Papers. Figure 16 uses data from the 1868 report to the House of Commons, which provides total emigration numbers for 1853-1867. This graph shows that the total number of emigrants leaving the U.K. fell almost continuously from 1851-1861 and then increased substantially from 1861-1863. Unfortunately we don't know what areas these emigrants were coming from, though we do know that most emigrants were Irish by birth. The English made up roughly one-third of emigrants across this period. However, by 1860 there were many Irish and Scottish living in cotton districts, so international emigrants from cotton districts need not be English.

Figure 16: Emigration from the U.K., 1852-1867

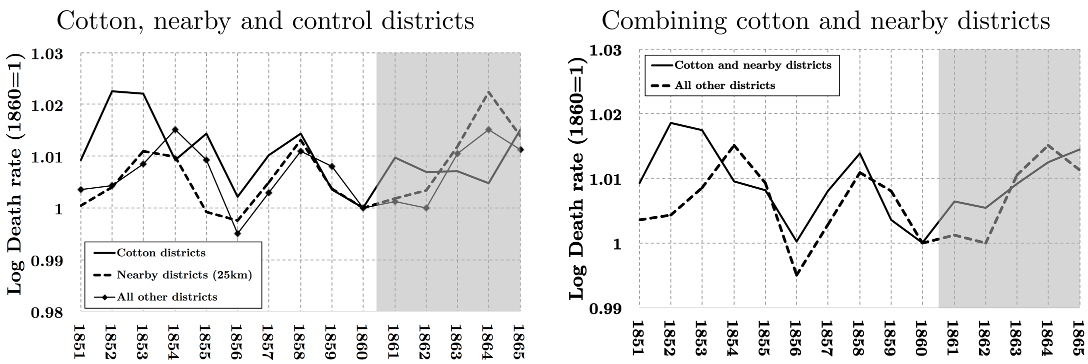



Data from the British Parliamentary Papers (1868, no. 045515).

## B.5 Additional results on mortality during the cotton shock

Figure 17 presents results similar to those shown in Figure 5 of the main text, but using mortality rates rather than mortality.

Figure 17: Mortality rates during the cotton shortage




Mortality data from the reports of the Registrar General. Data cover all of England & Wales. Population denominators are based on Census data and iterpolated using information on births and deaths from the Registrar General's reports using the Das Gupta method. Cotton districts are those with more than 10% of employment in cotton textile production in 1851. Nearby districts are non-cotton districts within 25 km of the cotton districts.

Table 9: Accounting for spillovers to nearby districts with time trends

| Dependent variable: | Ln(MORT. RATE) | Ln(MORTALITY) | Ln(MORTALITY) |
|---|---|---|---|
| | (1) | (2) | (3) |
| Cotton district × shortage | 0.079*** | 0.069*** | 0.072*** |
| | (0.017) | (0.016) | (0.016) |
| Nearby cotton emp. × shortage | 0.007*** | 0.007*** | 0.007*** |
| | (0.002) | (0.002) | (0.002) |
| Ln(Pop) | | -0.385** | |
| | | (0.170) | |
| Observations | 8,085 | 8,085 | 8,085 |
| R-squared | 0.239 | 0.421 | 0.420 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. Data cover 539 districts from 1851-1865. All regressions include district fixed effects, year fixed effects, and district-specific time trends. Shock period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%.

Table 10 presents some additional robustness results for the cotton shock example. We focus on results that include variables capturing the impact of the cotton shock in nearby districts, which is our preferred specification.

In the first column we impose a population density cutoff which limits the set of comparison districts to be more similar to the cotton districts, which tended to be relatively dense urban areas. Specifically, we use a population density cutoff of 0.89, which is the minimum density among cotton districts. In the second column we drop London, Liverpool, Leeds and Manchester from the data. London is a clear outlier because of its size and status as the capital. Liverpool is dropped because as the main port for cotton shipments it may have also been affected by the shock. Leeds is dropped because it experienced several border changes during the 1861-1871 decade. Manchester is dropped because it was an outlier relative to other cotton districts; it was the largest cotton town and the main market for the industry. In Column 3, we use a shorter pre-shock period. In Column 4 we define cotton districts as those with an employment share greater than 5% and in Column 5 we define cotton districts based on 1861 Census data. Both columns show an increase in mortality for both cotton and nearby districts, however, the increase for cotton districts is not statistically significant when we define cotton districts as those with greater than 5% employment share.

Table 10: Assessing the robustness of the cotton shock results

| | Dependent variable is ln(deaths) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Population density >0.089 | No London, Liverpool, Leeds, or Manchester | Data from 1856-65 only | Cotton dist defined as emp. share>5% | Cotton emp. based on 1861 census |
| **1**[Cotton district] × Cotton shortage | 0.048** | 0.039** | 0.067*** | 0.020 | 0.051*** |
| | (0.020) | (0.018) | (0.017) | (0.018) | (0.019) |
| 0-25 km exposure × Cotton shortage | 0.009*** | 0.009*** | 0.011*** | 0.010*** | 0.011*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| District effects | Yes | Yes | Yes | Yes | Yes |
| Year effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 7905 | 7680 | 5390 | 8085 | 8085 |
| R-squared | 0.212 | 0.197 | 0.272 | 0.207 | 0.205 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the district-level in parentheses. Data cover 539 districts from 1851-1865. Downturn period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%. 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 cotton districts. Mortality data are from annual reports of the Registrar General.

Table 11 considers several alternatives to identifying nearby cotton districts. In Column 1 we add to our baseline specification the effect of cotton employment in the 25-50 km, 50-75km, and 75-100 km bins. There we see that the effects are concentrated in the 0-25 km bin. In Column 2 we replicate this specification; however, instead of using log(nearby cotton employment) we use a series of indicators for whether the district was within 0-25 km, 25-50 km, 50-75 km, or 75-100 km of a cotton district. Again, we see that the nearby effects are concentrated in the 0-25 km bin. In both specifications the main effect for cotton districts is unaffected. In Columns 3 and 4 we consider continuous measures of cotton exposure. Specifically, for each non-cotton district, we calculate the distance to all other districts and then discount cotton employment for further away districts. In Column 3 we discount by exp(-distance/1000) and in column 4 we discount linearly. While discounting linearly produces qualitatively similar results, the effect of nearby cotton employment (discounted exponentially) is positive but insignificant.

Table 11: Alternative measures of nearby cotton exposure

| Dependent variable is ln(deaths) | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **1**[Cotton district] × Cotton shortage | 0.059*** | 0.031 | 0.241*** | 0.050** |
| | (0.022) | (0.019) | (0.056) | (0.020) |
| 0-25 km exposure × Cotton shortage | 0.012*** | | | |
| | (0.002) | | | |
| 25-50 km exposure × Cotton shortage | -0.004 | | | |
| | (0.003) | | | |
| 50-75 km exposure × Cotton shortage | -0.001 | | | |
| | (0.003) | | | |
| 75-100 km exposure × Cotton shortage | 0.004 | | | |
| | (0.003) | | | |
| **1**[0-25 km of cot. dist.] × Cotton shortage | | 0.077*** | | |
| | | (0.024) | | |
| **1**[25-50 km of cot. dist.] × Cotton shortage | | 0.054*** | | |
| | | (0.018) | | |
| **1**[50-75 km of cot. dist.] × Cotton shortage | | 0.039** | | |
| | | (0.018) | | |
| **1**[75-100 km of cot. dist.] × Cotton shortage | | 0.051*** | | |
| | | (0.015) | | |
| Nearby cot. emp. discounted as exp(-distance/10000) × Cotton shortage | | | 1.221*** | |
| | | | (0.287) | |
| Nearby cot. emp. discounted as 1/(distance×10) × Cotton shortage | | | | 2.378*** |
| | | | | (0.461) |
| District effects | Yes | Yes | Yes | Yes |
| Year effects | Yes | Yes | Yes | Yes |
| Observations | 8085 | 8085 | 8085 | 8085 |
| R-squared | 0.209 | 0.206 | 0.201 | 0.205 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the district-level in parentheses. Data cover 539 districts from 1851-1865. Downturn period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%. 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 cotton districts. Mortality data are from annual reports of the Registrar General.

Table 12 presents estimated results corresponding to Table 1 in the main text, but using two additional approaches to dealing with standard errors. In this table, the parenthesis contain standard errors clustered by district, which is the approach used in the main text. This approach is the most similar to the one taken in most existing studies in this literature. In square brackets, we report standard errors clustered by county, which is the next largest geographic unit. There are 42 counties in our data. In the curly brackets, we present standard errors that allow for serial correlation using the approach from Conley (1999), up to a cutoff distance of 25km. All three approaches yield fairly similar results, though allowing spatial correlation does increase the confidence intervals somewhat.

Table 12: Assessing the importance of standard error adjustments

| **Dependent variable:** | **Ln(MR)** | **Ln(MORT)** | **Ln(MORT)** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Cotton district × shortage | -0.023 | -0.013 | 0.049 |
| | (0.011) | (0.012) | (0.019) |
| | [0.010] | [0.012] | [0.026] |
| | {0.020} | {0.020} | {0.020} |
| Cotton emp. within 0-25km × shortage | 0.002 | 0.003 | 0.011 |
| | (0.001) | (0.001) | (0.002) |
| | [0.002] | [0.002] | [0.003] |
| | {0.001} | {0.001} | {0.002} |
| Ln(Pop) | | -0.857** | |
| | | (0.029) | |
| | | [0.028] | |
| | | {0.032} | |
| District effects | Yes | Yes | Yes |
| Year effects | Yes | Yes | Yes |

Standard errors clustered by district in parentheses. Standard errors clustered by county in brackets. 25 km spatially corrected standard errors in curly brackets. Data cover 539 districts from 1851-1865. Downturn period is 1861-1865. Cotton districts are defined as those with a cotton employment share greater than 10%.

# C Appendix: The Appalachian coal boom

This section contains additional background information and results related to the coal boom example. Many additional details can be found in the following previously published papers looking at the coal boom: Black *et al.* (2002) and Black *et al.* (2005).

## C.1 Data used in the coal boom analysis

The mortality data used in our analysis of the coal boom come from Bailey *et al.* (2016) files deposited at IPUMS. These files contain data on mortality by both location at the county level.[74] In addition to deaths, the Bailey *et al.* (2016) data also provide the number of births, which we use when constructing intercensal population estimates.

The second type of data used in our study is a set of population data from the Census. These data are available at the county level for every decade. For intercensal years, we generate estimated population values using the Das Gupta method, which uses the previous and next census population values for each county as well as birth and death information.

In the main analysis we use data starting covering 1950-1977. Summary statistics for the main variables used in the analysis across these years are presented in Table 13.

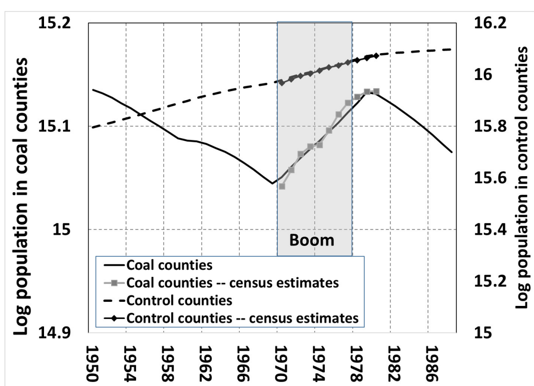
[74]Our analysis uses the location of residence mortality data.

Table 13: Summary statistics for variables used in the main coal boom analysis

| Variable | Mean | Std. Dev. | Min. | Max. |
|---|---|---|---|---|
| Mortality rate (per 1000) | 10.239 | 1.93 | 3.568 | 19.568 |
| Log mortality rate | 2.307 | 0.201 | 1.272 | 2.974 |
| Mortality | 432.16 | 411.531 | 43 | 2681 |
| Log mortality | 5.734 | 0.793 | 3.761 | 7.894 |
| Population | 43,189 | 40,276 | 8,008 | 231,122 |
| Log population | 10.335 | 0.803 | 8.988 | 12.351 |
| N | | 7672 | | |

The interpolated population values used in the main analysis were generated by the authors using census population data together with births and deaths. In the 1970s we also have intercensal population estimates from the census, though these are not available at the count level in earlier periods. In Figure 18 we compare our intercensal population estimates for the coal and control counties to those produced by the census. This figure shows that our estimates are quite similar to those produced by the census.

Figure 18: Comparing our population estimates to those produced by the census




In addition to the data used in the main analysis, we have also collected mortality data by age group. Unfortunately, however, we have only found these data at the county level starting in 1968, so we do not have a long enough pre-shock period to replicate the analysis that we applied to the total mortality data. Instead, in Appendix C.3 we present graphs describing the evolution of mortality within age categories from 1968 through the coal boom period.

Table 14 describes mortality rates by age group. We focus on mortality rates in 1970 in order to take advantage of the census population figures in that year and we use only data from the counties used in the main analysis to construct these figures. The main take-away from Table 14 is that mortality rates among those in their 20s and 30s were much lower than for older age groups. This is an important fact because, as discussed in the next appendix section, this group was the most likely to migrate in response to the coal boom.

Table 14: Mortality rate by age group in 1970 (per thousand)

| 0-4 | 5-9 | 10-14 | 15-19 | 20-24 | 25-34 | 35-44 | 45-54 | 55-64 | 65-74 | Over 75 |
|---|---|---|---|---|---|---|---|---|---|---|
| 4.821 | 0.43 | 0.387 | 0.994 | 1.405 | 1.475 | 3.054 | 7.385 | 17.454 | 38.554 | 102.823 |

## C.2 Additional results on migration during the coal boom

Figure 19 presents a map showing the difference in net migration by county in the 1970-80 decade compared to the 1960-70 decade including more surrounding states than the map in Figure 7. The additional states we include here are Maryland, North Carolina, Tennessee, and Virginia. Dark colors indicate counties that experienced an increase in in-migration in the coal boom decade, 1970-1980, relative to the decade before.

Figure 19: Difference in net migration with additional states

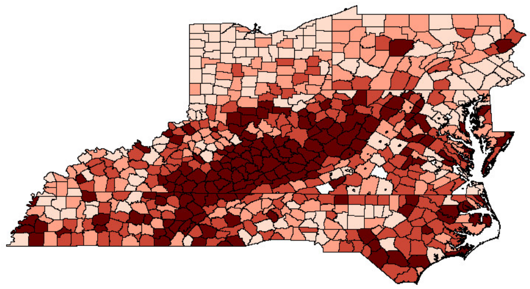
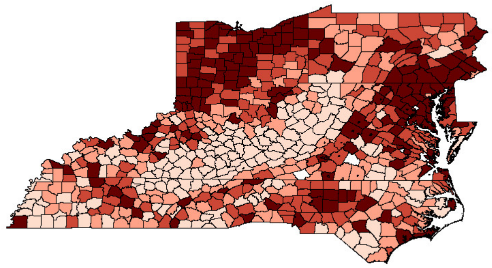


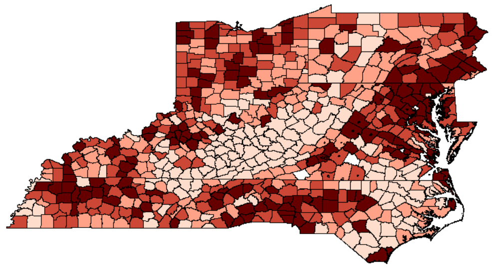
Figure 20 presents maps showing net migration based on the error of closure in the decade before the coal boom, the decade covering most of the boom, and the decade after the boom. Light colors indicate counties experiencing net out-migration. We can see that the coal counties experienced a clear pattern of out-migration during the decades before and after the boom, but that this pattern of out-migration completely disappeared during the 1970-1980 period.

Figure 20: Estimated net migration before, during and after the coal boom

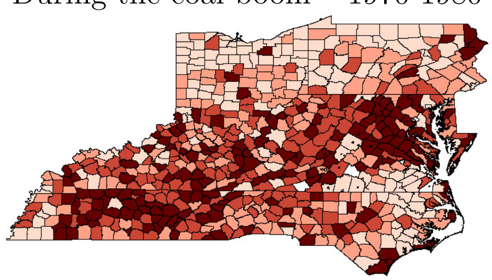
Two decades before the coal boom – 1950-1960



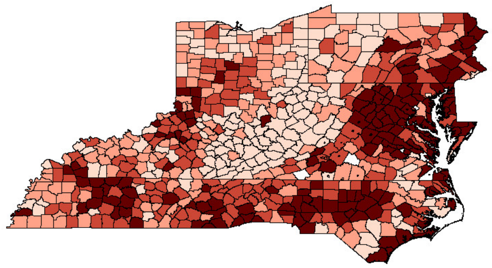
Decade before the coal boom – 1960-1970



During the coal boom – 1970-1980

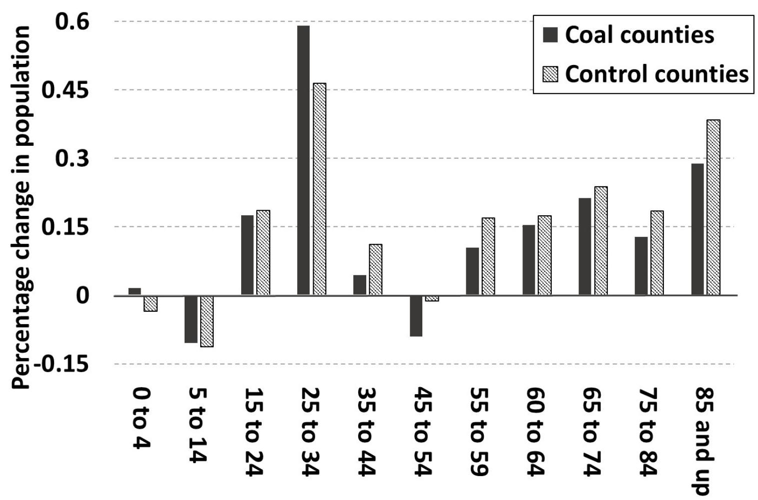


After the coal boom – 1980-1990



Next, we look at the distribution of migrants into the coal counties by age groups. Figure 21 plots the percentage change in population in the coal and control counties between 1970 and 1980. This shows that population growth in the coal counties during this period was concentrated in the 25-34 age group. While this age group also experienced growth in the control counties, this was less rapid than in the coal counties. Moreover, in terms of numbers, the change in population aged 25-34 in the coal counties accounts for 100,259 people out of a total population increase of 180,903 during the 1970-1980 decade. The 15-24 year old age group accounted for another 43,870 of this increase. Thus, a substantial majority of the population increase in the coal counties from 1970-1980 was concentrated among young adults.

Figure 21: Percentage change in population by age group




## C.3 Additional results on mortality during the coal boom

**C.3.0.1 Results using spatial standard errors** In the main text we present results with standard errors that are clustered by county to allow for serial correlation. In Table 15 we present alternative standard errors that allow for spatial correlation between counties within 100km of each other, following Conley (1999), and serial correlation for up to two years on each side of the observation year, following Newey & West (1987). We focus only on results with county time-trends because of evidence that the parallel trends assumption is violated when time-trends are not included. These alternative standard errors are presented in square brackets. In general these

Table 15: Estimated effects of the coal boom with alternative standard errors

| Dependent variable: | Ln(MR) (1) | Ln(MORT) (2) | Ln(MORT) (3) |
|---|---|---|---|
| Coal district × boom | -0.0375 | -0.00306 | 0.00947 |
| | (0.00958) | (0.00906) | (0.0104) |
| | [0.00858] | [0.00712] | [0.00780] |
| Ln(Pop) | | 0.267 | |
| | | (0.0427) | |
| | | [0.0332] | |

Standard errors clustered by district in parenthesis. Spatial standard errors allowing correlation for districts within 100km and serial correlation up to two lags in square brackets. All regressions include county fixed effects, year effects and county time trends. N=7,672.

Table 16: Weighted coal boom regression results

| Dependent variable: | Ln(MR) (1) | Ln(MORT) (2) | Ln(MORT) (3) |
|---|---|---|---|
| Coal district × boom | -0.0412*** | -0.00191 | 0.0126 |
| | (0.0105) | (0.00871) | (0.0106) |
| Ln(Pop) | | 0.270*** | |
| | | (0.0336) | |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. All regressions include county fixed effects, year effects and county time trends. N=7,672.

standard errors are smaller than those obtained when clustering by county, so we present the more conservative clustered standard errors in the main text.

**C.3.0.2 Weighted results** Next, we present results where each observation is weighted by county population in 1960. We focus only on results with county time-trends because of evidence that the parallel trends assumption is violated when time-trends are not included. These weighted results, presented in Table 16, are qualitatively similar to those shown in the main text.

Table 17: Coal boom regression results with additional control counties

| Dependent variable: | Ln(MR) (1) | Ln(MORT) (2) | Ln(MORT) (3) |
|---|---|---|---|
| Coal district × boom | -0.0288*** | 0.00253 | 0.00947 |
| | (0.00958) | (0.0102) | (0.0103) |
| Ln(Pop) | | 0.181 | |
| | | (0.164) | |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. All regressions include county fixed effects, year effects and county time trends. N=12,330.

**C.3.0.3 Coal boom results with additional control counties** In our main analysis we use only the treatment and control counties considered by BMS. Here we expand the set of control counties to include all counties in the four states that they study as well as counties in three neighboring states, Maryland, Tennessee, and Virginia. Results obtained with these additional control counties are presented in Table 17. We focus only on results with county time-trends because of evidence that the parallel trends assumption is violated when time-trends are not included.

**C.3.0.4 Coal boom results with alternative coal counties** In our main analysis we identify coal counties as those with over one billion tons of coal reserves. Below we present results using two alternative definitions. In Columns 1-3, coal counties are identified as those with over 2.5 billion tons of reserves prior to the boom. We can see that using this alternative cutoff has relatively little impact on the results. In Columns 4-6 we identify coal counties as those with over 10% of earnings coming from coal in 1969, based on the list provided by BMS. Here the main change is in results obtained using log mortality as the outcome without including log population as a control, in Column 6, where we see evidence that mortality may have increased during the recession. However, it is worth noting that this increase is sensitive to the inclusion of time-trends; without time trends the coefficient obtained from the

Table 18: Coal boom regression results with alternative coal county definitions

| | Coal counties are those with more than 2.5 billion tons of reserves (42 coal counties) | | | Coal counties are those with 10% of earnings from coal in 1969 (32 coal counties) | | |
|---|---|---|---|---|---|---|
| Dependent variable: | Ln(MR) (1) | Ln(MORT) (2) | Ln(MORT) (3) | Ln(MR) (4) | Ln(MORT) (5) | Ln(MORT) (6) |
| Coal district × boom | -0.0347*** | -0.00739 | 0.00258 | -0.0478*** | 0.0234 | 0.0469*** |
| | (0.0106) | (0.00988) | (0.0108) | (0.0124) | (0.0151) | (0.0156) |
| Ln(Pop) | | 0.267*** | | | 0.248*** | |
| | | (0.0437) | | | (0.0468) | |

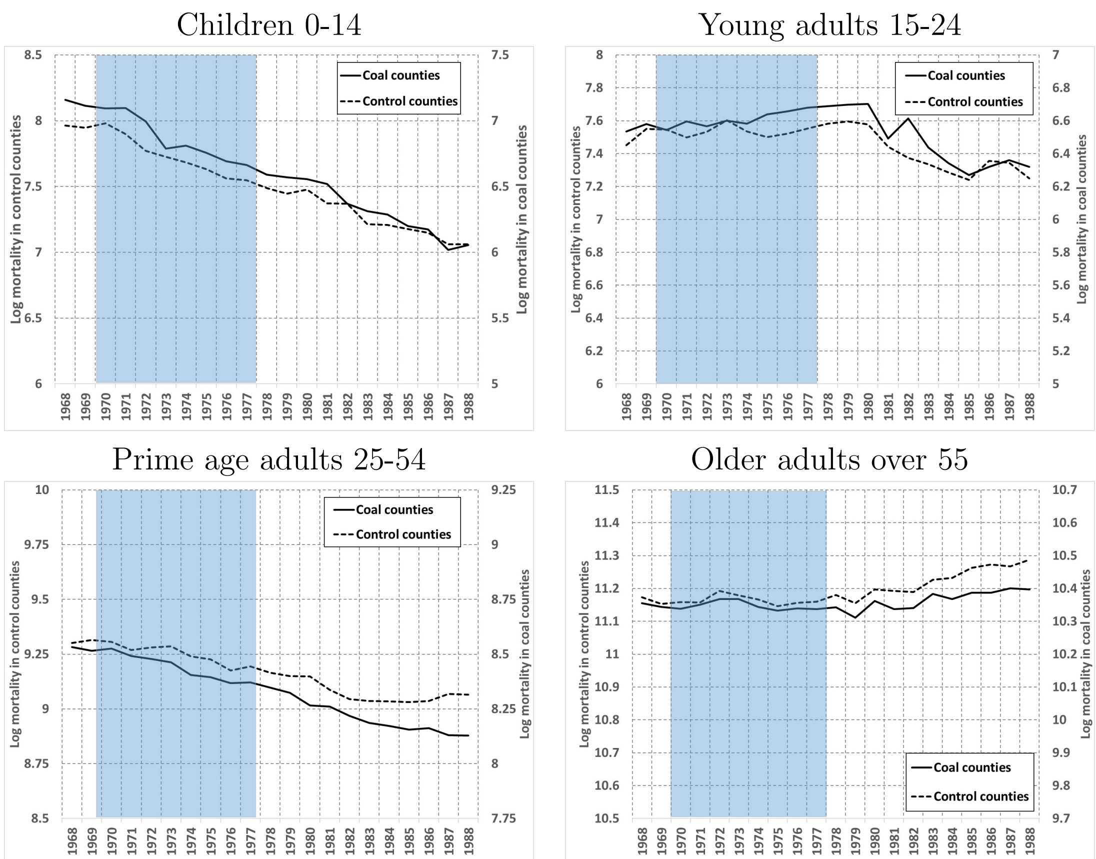
*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by district. All regressions include county fixed effects, year effects and county time trends. N=7,672.

specification in Column 6 is much smaller and not statistically significant.

**C.3.0.5 Results by age group** In Figure 22, we present graphs showing the evolution of log mortality for different age groups in the coal counties and control counties. In each graph we plot log mortality for the two series on separate axis, since there are more control counties with a larger overall population, but we use scales covering the same range for comparability.

For children, we can see that the number of deaths are falling in both locations although there is an increase in deaths around 1971 that is more pronounced in the coal counties. One likely explanation for this is that births in the coal counties increased in the early 1970s relative to the nearby counties. For young adults mortality is quite flat and similar across the two groups of counties, though there is some evidence of higher mortality in the coal counties towards the end of the boom and in the following years. Among working age adults over 24, the number of deaths was smoothly decreasing in both sets of counties across the study period, though this decline was somewhat more rapid in the coal counties. Among older adults, both locations experienced a brief increase in mortality from 1972-1974. This timing corresponds to the recession of 1973-75 caused by the oil embargo and provides further suggestive evidence that mortality may increase, rather than decrease, during recessions, particularly for the elderly. The number of deaths also increased in both locations starting in the 1980s. Overall, there does not appear to be any clear changes in relative mortality between the treatment and control counties during the coal boom period.

Figure 22: Mortality by age categories in the coal and control counties




**C.3.0.6 Evidence on births** The next graph plots the number of births, in logs, in the coal counties compared to the control counties. This figure show that there was a substantial increase in births in the first two years of the coal boom which was somewhat larger in coal counties than in the control locations. The relative increase in births in coal counties during the coal boom may reflect changing fertility choices, or this may simply reflect the relative increase in adults in their 20s and 30s in the coal counties during the boom.

Figure 23: Births in the coal and control counties