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The spatial impacts of a massive rail disinvestment program: The Beeching Axe[☆]

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ABSTRACT

This paper investigates the reversibility of the effects of transport infrastructure investments, based on a programme that removed much of the rail network in Britain during the mid-20th century. We find that a 10% loss in rail access between 1950 and 1980 caused a persistent 3% decline in local population relative to unaffected areas, implying that the 1 in 5 places most exposed to the cuts saw 24 percentage points less population growth than the 1 in 5 places that were least exposed. The cuts reduced local jobs and shares of skilled workers and young people.

1. Introduction

Transport infrastructure is durable, but its costs and benefits can shift over time. Consequently, decision-makers may need to remove or repurpose historical infrastructure investments. For example, the second half of the 20th century saw widespread reductions in railway infrastructure, while more recently, cities are reallocating road space to walking or cycling or removing highways to create public spaces.[1] Interestingly, we know relatively little about the economic impacts of dismantling a transport network, aside from the fact that it is not guaranteed to simply reverse the effects of its construction. This follows from existing research on the construction of transport infrastructure, which generally concludes that building such infrastructure induces path dependencies through its positive effects on agglomeration economies, durable housing and local infrastructure (see Redding and Turner (2015) for a review and Jedwab and Moradi (2016) or Brooks and Lutz (2019) for specific examples).

This paper investigates the economic impacts of large-scale infrastructure removal by examining a policy of rail decommissioning in Britain during the third quarter of the 20th century. This policy became known as the 'Beeching Axe', after a 1963 report, *The Reshaping of British Railways* authored by a Dr Beeching. This report led to the elimination of over two-fifths of all railway lines and nearly three-fifths of all stations. Despite major rail cuts being commonplace in other countries in the period we study, to our knowledge, this is the first paper to characterize their long-term impacts. Our analysis shows that the cuts caused a spatial redistribution of population that persists

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[1] Between 1916 and 1987, around 35 percent of the US rail network was abandoned (Frye, 2018). Other countries that implemented major rail cut policies include France (1930s), Ireland (1958–66), Argentina (from the 1960s), Spain (1980s), South Africa (1980s), and Canada (1980s/1990s). Regarding roads, many European cities have recently designated car free areas (Nieuwenhuijsen and Khreis, 2016). Los Angeles, San Fransisco, and New York are currently experimenting with similar policies, while these and other cities are also debating the removal of specific urban highways, see e.g. https://www.nytimes.com/interactive/2021/05/27/climate/us-cities-highway-removal.html.

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through to 2001, as well as localized losses of productive individuals and jobs. These results suggests that the effects of rail infrastructure construction are at least partially reversible, which stands in contrast to the evidence on natural advantages, even when those advantages have dissipated (Davis and Weinstein, 2002; Bleakley and Lin, 2012). Crucially however, we also go on to show that the effects of the rail cuts were offset by new freeways created by the contemporaneous construction of the UK motorway network. A central message of our work is therefore that continued access to viable transportation systems may be necessary to sustain previous concentrations of economic activity in cases where natural advantages do not dominate.

To conduct our analysis, we collect small scale aggregated historical decennial census data from 1901 through to 2001 and then link this data to a historical GIS of Britain's railway network that contains details of the rail lines and stations that were opened and closed in each decade.[2] During the 1950s–1970s, around 13,000 km of rail lines (42%) and 3700 stations (58%) were closed. To estimate how changes in accessibility due to these cuts drive changes in population and other demographic and socioeconomic area characteristics, we have to face the following fundamental challenges.

Firstly, we need a measure of the local intensity of rail network changes. As traditional metrics like the proximity of local lines or stations miss network-wide diffusion of local changes in a network, recent papers have adopted more sophisticated measures of accessibility, centrality or market access that consider the entire network structure (e.g. (e.g. Ahlfeldt and Feddersen, 2017; Donaldson and Hornbeck, 2016) for rail, or Gibbons et al. (2019) and Herzog (2021) for roads). We follow this approach by using a network centrality/market access metric that is based on travel time changes between all origin–destination pairs of stations in the network to capture rail accessibility changes.[3]

Secondly, we must address the natural concern that the rail cuts may have targeted places that were already declining, or that were expected to decline to in the future. The rail cuts were driven by the need to reduce severe operating losses on the rail network. The reasons for these losses were complex but include the shift to buses and roads, failed investment, poor management, and oversupply from 19th century private development. British Railways, the state-owned company formed when railways were nationalized in 1948, explicitly targeted cuts to currently unprofitable lines and stations. This raises identification concerns as current profitability is likely to be strongly driven by local population trends or other factors that determine future outcomes. Critically however, unprofitable lines and stations were identified by a series of statistical studies which were hastily constructed and incomplete. Memos between the architects of the cuts and reports by contemporary and subsequent commentators strongly suggest that closure decisions at the margins were often based as much on guesswork and arbitrary factors as on hard evidence. Bearing this context in mind, our identification strategy compares future outcomes between matched locations with different rail accessibility changes but effectively identical pre-existing population trends. This strategy reflects that controlling for these trends will account for the bulk of the variation in current profitability, and that conditional on these trends decisions about whether to retain or close local stations and lines were arbitrary and exogenous.

A third and remaining concern is that the cuts, and the changes in accessibility they generated, might be correlated with other shocks which had a direct influence on local populations and changed the spatial structure of the economy in ways that favored central and urban areas. Specific concerns in our context are centers of planned population growth that were developed over the middle of the 20th century (so-called New Towns) and the contemporaneous construction of a national motorway network.[4] To mitigate potential biases, we introduce three novel instruments in our regression analysis. The first two use features of British railway history and network geometry respectively to predict local accessibility changes that arise due to rail cuts. The third uses Britain's north–south orientation to predict accessibility changes based on east–west line lengths in parishes. As an alternative strategy, we directly control for contemporaneous shocks from changes in centrality, the development of the motorway network, and New Towns. Estimations using these instruments or controls produce similar results to our baseline estimates, suggesting changes in accessibility from the rail cuts were effectively random, conditional on pre-trends in population.[5]

Our findings suggest that areas with significant reductions in rail centrality experienced relative declines in population. The elasticity of population with respect to network centrality we estimate is around 0.3.[6] These population results indicate that, as theory would suggest, transport has a major role to play in changing patterns of land use, and that removal reverses some of the effects of construction. As the national population grew during the period of rail cuts, it was redistributed towards areas with preserved rail access and new motorway access. However, this reversal was not complete. There was considerable persistence in the spatial population distribution, as expected in the presence of agglomeration economies and durable housing and other infrastructure. Population loss was linked to a decline in high skilled and working age individuals, an increase in retirees, and fewer local jobs per resident. At the same time, commuting patterns remained unchanged, suggesting that population movements were driven by job-relocations to better-connected areas.[7]

This paper contributes to our understanding of the role of infrastructure in two important ways. Firstly, it complements the literature

[2] The spatial units in our analysis are either *Civil Parishes* or *Local Government Districts*, which are historical administrative units. Parishes represent the smallest geographical units available for the period of our analysis and are used in the majority of our estimations.

[3] We calculate a network centrality index that closely resembles standard measures of market access. Therefore, we use the terms 'market access' and 'network centrality' interchangeably.

[4] Concerns that some local political leaders were successful at lobbying against closures and determining the future economic development would also be covered by this strategy. However, the historical record suggests that there was little political influence behind Beeching's proposals. The lack of political considerations is consistent with Quiroz Flores and Whiteley (2018) who show that the location of the 2000 stations listed for closure in the 1963 Reshaping report were not related to political marginality in the 1959 election. We will specifically look at the performance of regions with proposed but not closed stations in the robustness tests section.

[5] As noted on pages 55–57 of the 1963 report, the authors of the cuts explicitly chose not to take account of anticipated future population movements when determining the cuts. For example, the section entitled Long-Term Trends in the Location of Industry and Population begins. "No novel assumptions have been made about the future distribution of population and industry in the country as a whole. Implicitly, it has been assumed that the pattern will continue to be basically similar to that which exists at present and that, while there may be a continuation or a reversal of existing trends, there is not likely to be any change so radical as to affect the desirability of [the proposals]". The similarity of the IV and OLS estimates provides reassurance that this was indeed the case. Nevertheless, we control for past population trends throughout as these are strongly correlated with the local changes in accessibility.

[6] A more easily interpretable ancillary regression we use indicates that in that places in the top quintile of accessibility cuts saw 24 percentage points less growth in population than the places that were in the least exposed quintile.

[7] Note that we are investigating the impact of infrastructure changes on population distribution across space. Studies focused on aggregate productivity or employment face the challenge to distinguish between casual effects on growth from reorganization. Our study explicitly examines displacement and sorting. We ask whether transport cuts in one location relative to another lead to population shifts, thus identifying relative effects of infrastructure losses. We do not claim effects on national aggregate population, age, or skills, but discuss potential changes to agglomeration economies in the conclusion.

on the economic effects of transport infrastructure investments by explicitly considering the effects of infrastructure removal. Secondly, it contributes to the literature on the geography of path dependence and assesses the relative importance of agglomeration economies and durable locational investments versus market access. To further illustrate the asymmetry between transport infrastructure construction and deconstruction, we can compare our estimates to findings from the construction of the railway system. Our back-of-the-envelope calculations suggest that the population loss in parishes where the nearest station closed between 1951 and 1981 was less than half of the population gain upon the station's opening (see Appendix Table A5).

Specific examples of work on the impacts of rail infrastructure construction include Ahlfeldt and Feddersen (2017), who examine the long-term effects of railway access on urban growth in Germany; Baum-Snow et al. (2017), who analyze the impact of railroads on city populations in China; Bogart et al. (2018b), who study the economic effects of early railways in England; Donaldson (2018), who investigates the impact of railways on market integration and welfare in colonial India; Donaldson and Hornbeck (2016), who explore the broader economic impacts of railroads in the United States; Heblich et al. (2020), who assess the role of historical railroads in shaping modern economic geography in Germany; Garcia-López et al. (2016), who look at the effects of rail infrastructure on urban form in Spain; Gonzalez-Navarro and Turner (2018), who analyze how rail transit investments influence city development in the US; Hornung (2015), who examines the impact of Prussian railroads on economic development; and Qin (2016), who studies the effects of high-speed rail in China on local economies. Of particular relevance is (Bogart et al., 2022), who investigate the effects on local growth from of the construction of the rail network in Britain, the dismantling of which is the topic of our research. While all the papers above focus on the effects of large-scale railway infrastructure investments, we are looking at an equally large disinvestment. This is a worthwhile exercise because large scale railway cuts were common throughout the last 100 years, and because decision-makers are currently considering restoring previous services in several settings.

More generally, studying large scale infrastructure reductions allows us to address questions of path dependence. Important contributions to the path-dependence literature include Davis and Weinstein (2002), who investigate the impact of World War II bombings on the economic recovery and growth of Japanese cities; Bleakley and Lin (2012), who analyze the long-term effects of historical portage sites on modern economic activity in the United States; Michaels and Rauch (2018), who study the persistent impacts of historical events on current economic outcomes using the example of the US slave trade; and Allen and Donaldson (2023), who examine how historical trade routes have influenced present-day economic geography and development patterns. Closest to our topic is a small set of papers that study persistent effects of rail infrastructure investments. Jedwab and Moradi (2016) and Jedwab et al. (2017) study colonial railroads in Africa and find evidence for path dependence of cities along the railway transport corridors. Brooks and Lutz (2019) zoom into one city, L.A., and find evidence of agglomeration in neighborhoods along streetcar lines, leading to subsequent 1922 zoning designations which, in turn, become a force of persistence once the streetcar lines were removed. These papers look at relatively simple rail networks and do not explicitly measure changes in accessibility caused by their removal or disuse. In contrast, we examine major cuts to a large and complex national rail network and show that these cuts generated changes in rail accessibility across the country. These changes in accessibility shifted populations towards more accessible places, a corollary of which was a move towards large urban centers.

We organize our analysis around three core themes: how does rail infrastructure removal affect local populations, jobs, and skills?; to what degree are community-wide impacts driven or mitigated by local circumstances such as car ownership, proximity to new highways, or the retention of a local station?; and what are the implications for long-run economic geography, agglomeration and aggregate productivity? In the next section we provide more details on the political background of the Beeching cuts. After that, we outline our methods, present our key results, assess their robustness and finally conclude.

2. Historical background

This section provides a brief history of the British railway network and introduces the political context of the cuts before, during and after the 'Beeching' era (a more detailed discussion is available in Appendix D). In contrast to other countries, the development of the railways in Britain prior to 1900 was led by private enterprise with an emphasis on market forces. As is illustrated in Fig. 1, this led to 'Railway Mania' periods in the 1840s, 1850s and 1860s (see Bogart et al., 2018a). After that, further development came at a much slower pace. Around 1914, the network consisted of some 20,000 route miles of track that included a dense network of rural branch lines, suburban lines with irregular demand, and duplicated lines between many locations.

During the First World War the railways came under national control. A 1921 Act reorganized the then 120 railway companies into four large regional groups and subjected them to wage and price controls. The associated consolidation led to the closure of around 1200 miles of lines (Loft, 2006). In 1948 the railways were nationalized. The newly formed British Railways (later British Rail) launched a modernization program and authorized the closure of 3000 miles of highly unprofitable lines between 1950–1962 (See Gourvish (1986), p. 119 and Waller (2013)). Despite these cuts, continuing deficits led the government to create the *British Railway Board* (BRB) in 1962. Dr Richard Beeching was appointed as the BRB's first Chairman and tasked with restructuring the railways.

Under Beeching, the BRB proposed to withdraw a third of the tracks and 2363 stations (British Railways Board, 1963, 1965).[8] The proposals were based, in part, on information gathered in a series of statistical studies which collected data on rail usage, including passenger density and receipts. However, the proposed closures did not rely on any single source of evidence but rather on a combination of factors, suggesting the decision rules were fuzzy and subjective. The rules on which the decisions were based were not set out precisely in any public information, and the information on historical ticket sales and service frequencies or freight traffic is now unavailable. Close inspection of the historical records reveals considerable uncertainties, inconsistencies, and faults in the data that were available to the BRB.[9] The pressure to make closures quickly to reduce deficits means that the report was based on hastily constructed figures obtained using guesswork rather than actual data.[10] The subjective nature of these judgments adds a quasi-random element to the procedures used to determine the closure proposals. The

[8] The reports contain explicit proposals for line and station closures but make no mention of what should but done with the associated land and infrastructure thereafter. Many former stations and lines have subsequently been redeveloped as housing or for other private purposes, which suggests the general policy of British Rail was to dispose of these assets where possible.

[9] For example, Munby (1963, p.162) writes "One hoped that the Beeching Report would provide a more solid foundation of facts and argument to substantiate this policy [of closures] than was available before. Unfortunately, it must be stated that the Plan is disappointing, not so much in what it recommends, as in the inadequacy of the facts, the thinness of the arguments in several places, and in the extent to which it accepts official railway viewpoints without critical scrutiny".

[10] A Confidential Memorandum of Meeting held at BTS Headquarters Friday 21st July 1961 kept in the National Archives reports "The General Managers had given an undertaking that the Traffic Studies could be completed within the suggested timetable. It was essential that as many hard facts as possible should be incorporated: to the extent to which this was not possible there must be reliable managerial assessments and estimates... . As to accuracy of traffic flows, it was thought better to estimate a full year's figures by a combination of

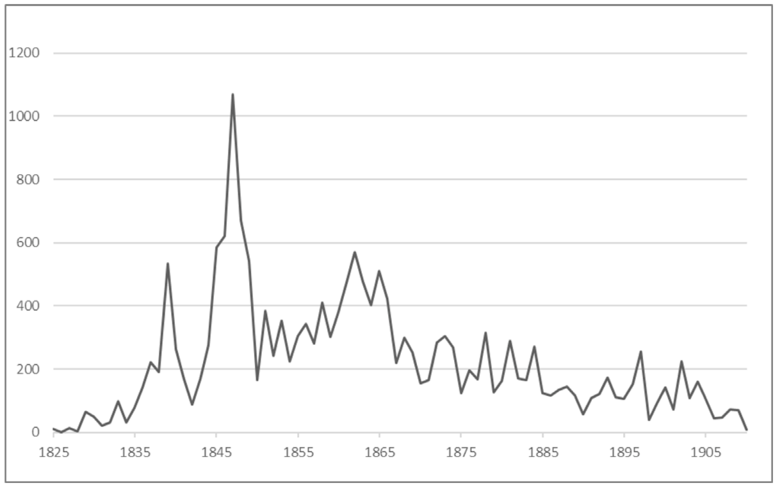


Fig. 1. Annual change in railway mileage in Britain, 1825–1911. Notes: The figure is taken from Bogart et al. (2018a).

implication of these historical factors is that there were a lot of spurious cuts and retentions, meaning it is potentially possible to identify the causal impacts of the cuts.

The Beeching-era cuts were part of an ongoing and wider program of network rationalization and cost savings, so the closures taking place in the period we study between 1950 and 1980 do not perfectly coincide with those proposed in the 1963 report: some closures occurred before the publication of the report, some closures proposed in 1963 were reprieved, and other subsequent closures were never listed in the Beeching reports. A sketch map of the proposed closures is shown in panel (a) of the Appendix Figure A1. Fig. 2 shows the railway line network as it was in 1950 (Panel a) and as it became by 1980 (Panel b). Evidently, the cuts to lines were severe. As the lower two panels of Fig. 2 illustrate, the changes in the distribution of stations were even more dramatic, so looking at the effects of cuts based on line length alone – as is common in many studies of rail and road infrastructure – is inadequate. Many areas retained lines but lost all their stations. Instead, our analysis will make full use of both the cuts in lines and the cuts in stations, through the network centrality index defined in following section.

3. Methods and data

3.1. Outline empirical strategy and identification concerns

Our goal is to estimate how changes in accessibility across the rail network in Britain, caused by cuts to rail lines and stations, affected population and employment outcomes in small areal units in Britain. We estimate these effects using regressions of changes in population (or other outcomes) on changes in accessibility over the period between 1951 and 1981. Our index of rail accessibility is an index of network closeness centrality, based on imputed travel times between stations on the network and between these stations and residential locations. We define this index in more detail in Section 3.3. There are two fundamental challenges to estimation. First, places affected by cuts might have already been on varying population trends, not due to deliberate targeting of declining areas, but because cuts targeted unprofitable lines with low traffic. Second, contemporaneous shocks might have influenced both the likelihood of experiencing cuts and future prosperity. Notable changes in our context include the development of a national motorway network and the strategic planning of centers for population growth.

To address the concern about differential pre-trends, we carefully control for (or match on) pre-existing population trends in our regressions. Specifically, we either: (i) include lags of historical census population variables back to 1901; (ii) control directly for population pre-trends using dummies for quantiles of the distribution of these trends; or (iii) use pairwise differences in a semi-parametric estimator to difference out population pre-trends.

More formally, we estimate flexible time-differences specifications for geographical units i, with the following form:

$$\ln y_{i81} = \beta(\ln cent_{i81} - \ln cent_{i51}) + \gamma \ln cent_{i51} + \delta \ln y_{i51} + \mathbf{x}_i'\lambda + \epsilon_i \quad (1)$$

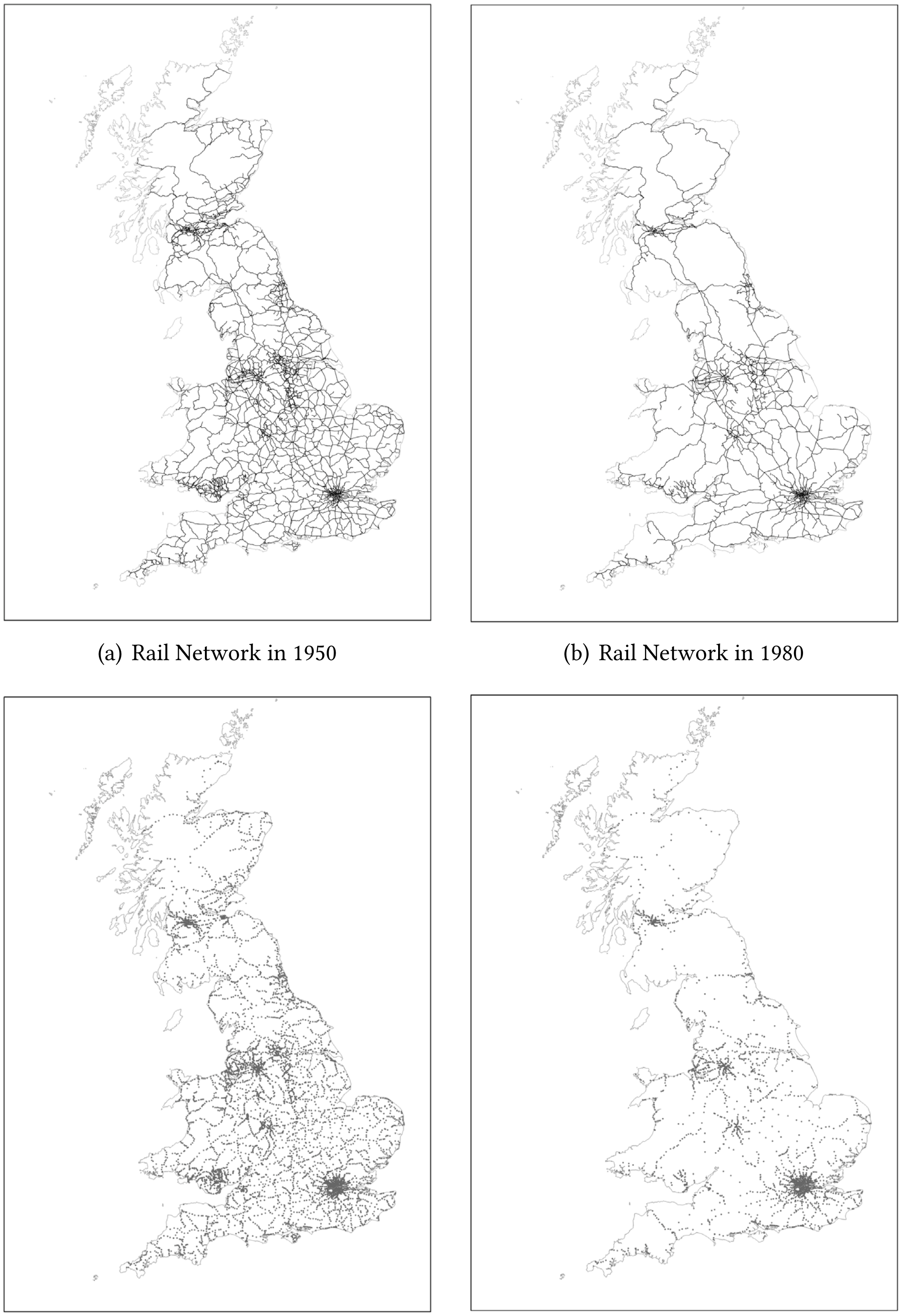
The dependent variable, sourced from the Census, represents growth or changes in population composition. The variable *cent* is the centrality/accessibility of place i via the rail network in a given year, as described in Section 3.3. It is important to note that the estimate of β in Eq. (1) matches the result from regressing the 1951–1981 change in $\log y$ on the change in log centrality during the same period, conditional on log rail centrality and $\log y$ in 1951. The vector of control variables x_i includes: (i) log population in 1921, 1931, 1911 and 1901 and squares of these log populations; or (ii) sets of dummies for 5 percentile intervals in the distribution of the pre-1951 population trends, for 1901–51, 1911–51, 1921–51 and 1931–51. The geographical units i are parishes for our main population analysis, or Local Government Districts for other socioeconomic variables. These units are described in Section 3.4.

To construct the pairwise-difference estimator, we rank observations by an index of the population pre-trends and then transform Eq. (1) into differences between adjacent ranked observations. This ensures that we are comparing places which are on nearly identical pre-trends. The index used for this ranking is either: (i) the 1901–1951, 1911–1951, 1921–51 or 1931–51 log population change; or (ii) the linear prediction of a regression of the 1951 to 1981 parish rail centrality changes on a flexible polynomial in the population growth in preceding decades:

$$(\ln cent_{i81} - \ln cent_{i51}) = \pi_1 \ln pop_{i01} + \pi_2 (\ln pop_{i01})^2 + \sum_{t=11}^{51} \left(\sigma_{1t} \Delta \ln pop_{it} + \sigma_{2t} (\Delta \ln pop_{it})^2\right) + v_i \quad (2)$$

In Eq. (2), Δ represents the difference between census period t and the preceding census year. The advantage of pairwise differencing is that

facts and managerial assessment than simply to multiply, with all the attendant risks, one week's figures to produce yearly totals... . In regard to passenger traffics, the Costings Divisions passenger traffic analyses might be used, test weeks in March and October probably giving the most balanced results. Some Regions had conducted tests in recent months: others had not had one for many years".



(a) Rail Network in 1950 (b) Rail Network in 1980

(c) Rail Stations in 1950 (d) Rail Stations in 1980

Fig. 2. Rail network and stations in Britain in 1950 and 1980.

it controls flexibly for non-linearities in the relationship between the outcome variables and the pre-trends. This approach has been proposed for partially linear models (Yatchew, 1997; Honore and Powell, 2005; Aradillas-Lopez et al., 2007). However, our focus is not on estimating the non-linear part, but merely controlling for it.

To mitigate the remaining concern that policy shocks correlated with the cuts may determine future local economic development and therefore bias our estimates, we employ several strategies. First, we use instrumental variables derived from historical network developments and geometry to predict cuts that are independent of contemporary local shocks. Second, we control directly for contemporaneous shocks from changes in centrality, the development of the motorway network, and New Towns. Third, we add geographical grid-square unit fixed effects to Eq. (1) to partial out simultaneous shocks at the local geographical level.

In additional analysis, we conduct a 'placebo' test with stations proposed for closure but not closed, to ensure that our results are not influenced by targeted cuts to places where adverse economic shocks were expected. We describe the instrumental variables strategy next and other strategies along with the results in Section 4.

3.2. Instrumental variables

Our first two instruments build on insights from the history of the railways that is presented in Section 2. The key insight is that laissez-faire policies led to an oversupply of lines due to private railway companies' wasteful competition for local monopolies. After the trunk network's completion, speculation often resulted in line duplication and investments in less optimal routes that were cutting though difficult terrain. Over time, as national rail demand declined, these sub-optimal lines became unprofitable and more likely to be closed.

To exploit this insight, our first instrument calculates the distance to redundant lines predicted in 1950. To implement this approach, we first use the 1950 network to predict trunk routes between major cities with populations over 80,000 and between the same major cities and London. These trunk routes are calculated using least cost 'traveling salesman' paths. Appendix Figure A2, Panel (a) shows the predicted trunk lines. Next, we remove these trunk routes from the network, and re-predict connections between the same major cities using the remaining routes.[11] These second-best trunk routes shown in Appendix Figure A2, Panel (b) are our predictions of potentially redundant and unprofitable routes which were more likely to be closed.

Our second instrument follows a similar logic and splits the distribution of opening years into quintiles. Lines in the third to the fifth quintile were built after 1860. Appendix Figure A2, Panel (c) shows these lines. At this time, the trunk network was established and they are more likely wasteful line additions. To address concerns about recent line additions being endogenous to local policy shocks, we can control for these more recent openings, and exclude only the 3rd and 4th quintiles (the years between 1860–84) in our second-stage regression. The identifying assumption is that, conditional on pre-trends and 1951 network centrality, a parish's future development is unaffected by past competition for local monopolies, except through the higher likelihood of line cuts post 1950.

Our third instrument exploits a by-product of the decision rules outlined above. Travelers in Britain today will recognize that cross country journeys are challenging without going through London as many central and northern lines were cut between 1950 and 1980, a pattern confirmed by Fig. 3. This east–west pattern is a by-product of cutting less profitable cross-country lines, rather than the more profitable lines on the north–south axis. Based on this empirical observation, we devise another instrument which predicts rail centrality loss based on the length of local lines running in an east–west orientation, as shown in Appendix Figure A2, Panel (d).[12] Specifically, we aggregate the length of line segments with less than 10 km difference between their south and north endpoints across each parish and use this east–west parish line length, conditional on total line length, as our instrument. The identifying assumption is that, conditional on total line length, 1951 population, rail centrality, and pre-1951 population trends, future population growth in a parish is unaffected by it having east–west running train lines in 1951, except through the fact that these lines were likely to be cut after 1950.

[11] Instead of fully removing the predicted trunk routes, which would render the network incomplete, we make them virtually inaccessible by setting their cost at ten times their length.

[12] Michaels (2008) uses a similar instrument that exploits the orientation of US highways.

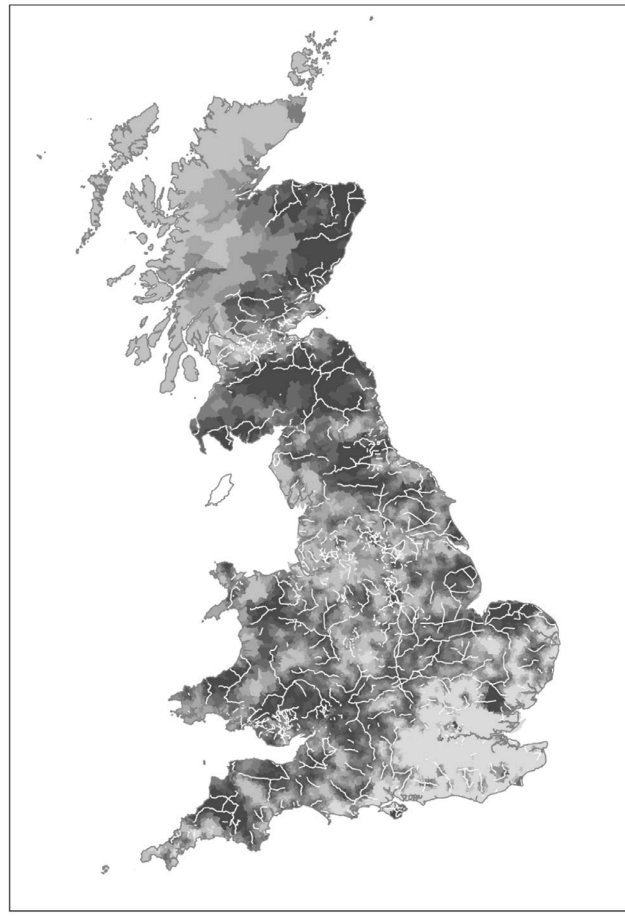


Fig. 3. Rail lines cut 1950 to 1980 and changes in centrality/accessibility at parish level. Notes: The figure shows sections of rail line in white that were removed between 1950 and 1980. Shading illustrates changes in Parish rail centrality, with darker shading indicating bigger reductions.

3.3. Measuring centrality and market access

This section describes the construction of the network centrality indices. The main index we use is an unweighted network closeness centrality index. We also show results using a node population weighted centrality index, which is also known as a population accessibility index in the transport literature, or more recently as market access in the trade and spatial economics literature. Centrality indices of this type have long been used in the transport literature to measure accessibility, and recent applications in analyzing the impact of transport on the economy include Gibbons et al. (2019), Donaldson and Hornbeck (2016), Gibbons and Wu (2020) and Baum-Snow et al. (2020). In this application, we first construct these indices at the rail station level, before we aggregate them to the geographical units of analysis (parishes or Local Government Districts (LGDs)) using inverse distance weighting. Formally, the indices have the structure:

$$cent_{it} = \sum_{j \in J_{it}} \left(\sum_{k \in K_t} m_k \times railtime_{jk}^{-0.5} \right)_j \times roadtime_{ij}^{-0.5} \tag{3}$$

In this expression, i represents a geographical unit, j represents an origin station amongst a set J of stations local to place i, k represents other stations on the network amongst the set K of stations currently open on the network. For our main estimates, we set $J = 3$ so that parish centrality is a weighted average of the rail network centrality of the three nearest stations.

The cost variable $railtime_{jk}$ is an imputed shortest path rail time between station j and station k, derived by network analysis of a historical GIS of the rail network.[13] The cost variable $roadtime_{ij}$ is an imputed shortest path road journey time between a point chosen at random within zone i, and the local station j. Road times are based on 'Manhattan' distances, i.e. 1.4 times the straight-line distance between zone i and station j. To estimate a zone's distance to station j, we average distances from a random set of points within the zone to the station. Weights m_k are station node weights. They are set to 1 for our preferred unweighted centrality indices, or to the 1951 parish population for a market or population access index. Distance decay exponents are set to −0.5, making the index a standard centrality/market access index if only one station is nearby, with travel cost between two parishes as the geometric mean of rail and road journey stages. A doubling of road and rail speeds implies a doubling of accessibility.[14] We are not able to construct full multimodal measures of accessibility incorporating road and rail, because neither the road network nor maps of sufficient quality to construct it are available. However, in Section 4.3 we investigate the interaction between rail accessibility changes and road accessibility changes from the growth of the motorway network, which was the only significant driver of any change in road accessibility over our study period.

The index in Eq. (3) conceptualizes a parish's rail network centrality as a weighted average of the rail network centrality of the stations near to that parish. While other methods exist, like assigning parishes to the nearest station, our less restrictive structure avoids singular assignments in cases of multiple equidistant stations, assigning higher weights to closer ones. Another advantage of this index structure is that it can be easily decomposed into (i) components due to changes in the network (the set of stations K and associated rail links), holding the set of local stations constant, and (ii) changes in the set of local stations J, holding the global set K constant. This decomposition allows us to estimate impacts on local economies from local station removals or network-wide changes.

Fig. 3 shows the lines that were cut over the 1950 to 1980 period, and the resulting changes in rail centrality, computed as described in Eq. (3) without population destination node weights. The picture with parish population weights in the numerator is broadly similar, and as the descriptive statistics in Appendix A Table A1 show, the standard deviation in the two variant indices is similar, although their means differ. The correlation between the changes in the 'market access' indicator using population weights and a pure, unweighted closeness centrality index (with numerator weights of one) is 0.99, so the results we present later are nearly identical whichever index we use. In the following section, we will focus on the unweighted centrality index. As expected, there is a strong link between the locations of cut lines and the magnitude of the cut in centrality. Most, but not all places experiencing the least decline in centrality (the darkest areas) are central and urban. However, some places, such as the north of Scotland, experienced little decline in centrality because they were already poorly connected and peripheral.[15]

3.4. Data sources and construction

The outcome variables we use in our analysis are taken from historical population censuses that were collected every 10 years between 1901 and 1931 and again between 1951 and 2001; there was no census in 1941 for obvious reasons. At the parish level, we observe population for the whole of Great Britain; at the coarser Local Government District (LGD) level, we observe additional variables that capture changes in the composition of the population but only for England and Wales.[16] We focus on variables that we observe consistently over time: population; the number of 'qualified' workers, which means educated to at least age 20 in earlier censuses, or educated to degree or higher in later years; social class groups; broad age categories; total employment of residents; and the number of out-commuters. Our final estimation datasets, see Gibbons et al. (2024), contain around 1470 LGDs in England and Wales, and 13,250 parishes in Britain.

Fig. 4 shows the general patterns in parish population over the 20th century, split by quintiles for the strength of the rail cuts that occurred over the 1950–1980 period. The darkest lines indicate the deepest cut areas; the light dotted line represents least affected areas. Populations are in natural logs normalized to zero in 1951. This figure illustrates the fundamental empirical challenge we are facing: the 20% of parishes facing the least cuts (the dotted line) were already on stronger population growth trends than the remainder, because they were predominantly core city areas. The pre-1950s population trends in the remaining 80% of parishes vary less with cut severity, and our empirical strategy aims to distinguish rail cut impacts from correlated effects of the pre-trends.

Our rail network was digitized from a historical atlas of British railways (Cobb, 2003). The data comprises lists of stations and lines closed by decade from 1900 to 2000.[17] We then calculate minimum distances for all station origin–destination pairs. Since the network does not distinguish between goods and passenger lines, and both types of services typically ran on the same lines, we cannot distinguish the effects of rail cuts on passenger travel from freight transport. To simulate station access, we calculate straight-line distances from random points within parishes or LGDs to stations, with the number of points proportional to parish land area and a minimum of six points.[18]

Converting rail network and parish-station distances into travel times comes with some assumptions. In the absence of data on service frequencies or complete timetables, we omit these features in our journey time estimations. We assume that people would have timed their journeys in accordance with timetables to minimize delays. This implies that our empirical results should be interpreted as 'intent to treat' estimates relating to the provision of rail infrastructure. Since it is hard to find data on road and rail journey times for Britain in the 1950s, we infer appropriate speeds from samples of historical rail and bus timetables. Our baseline assumptions for rail speeds are 65 km per hour for journeys above 75 km and 40 km per hour for journeys below 75 km, plus 6 min on all journeys for transfers and waits. Road travel speeds – which are for our purpose short journeys to local stations – are set to 20 km per hour, plus 12 min for transfers and waits on all

[13] We do not adjust road or rail times for topography. This is unlikely to be a major source of error, given the relatively flat topography of most of populated Britain.

[14] We also tried exponents of −1. The results we report later are not highly sensitive to this parameter, within the range typically found in the literature.

[15] We retain the outlying islands of Scotland in our main estimation samples, but the following results are robust to dropping them and also to dropping Scotland as a whole.

[16] Data prior to 1971 have been digitized from paper records by the Vision of Britain project (http://www.visionofbritain.org.uk/) and we are limited to the records that have been published. At the present time, it is not possible to recover any more comprehensive data from the historical census micro data during the 20th century because these are subject to 100-year confidentiality rules. From 1971 onward, more detailed small-area census data is readily available in electronic form, though for different geographical units so we re-weight this data to parish and LGD units as defined for 1951 using land area.

[17] We made a few corrections, added in the London underground network and cleaned the data to make it usable for a GIS Network Analysis.

[18] Setting the number of points in proportion to land area reduces noise in the distance estimates. Using parish populations instead of area would make little sense given that some large parishes may have small populations and vice-versa. The use of six points as an arbitrary minimum is to prevent there being parishes with zero points.

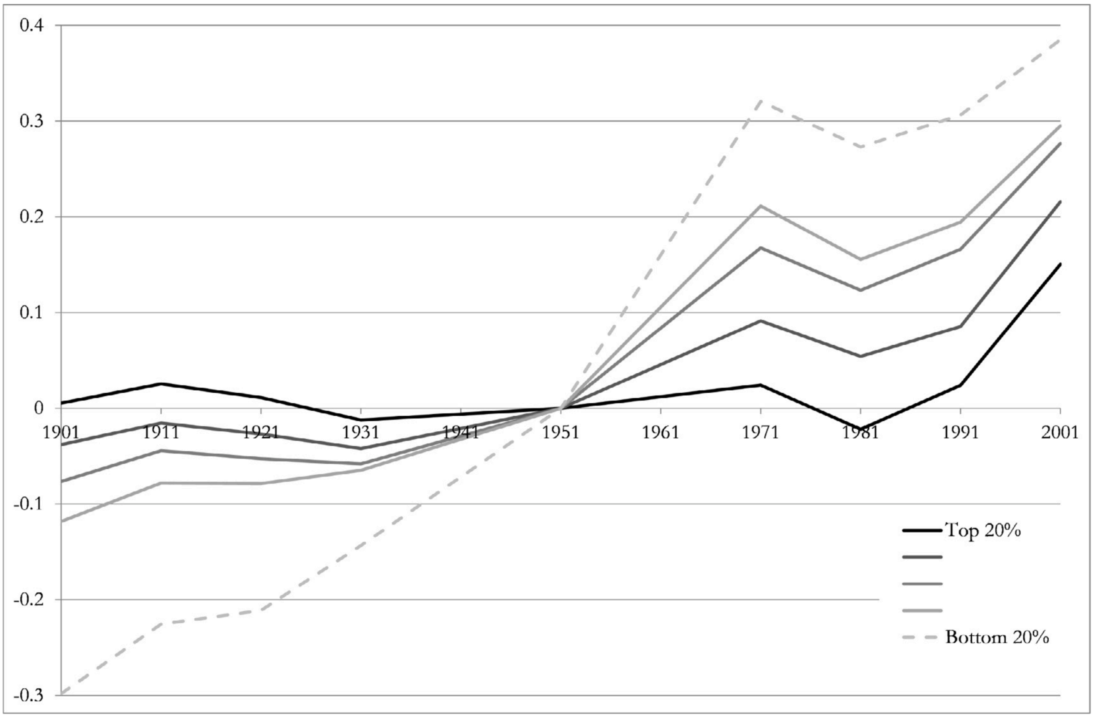


Fig. 4. Trends in log population, by depth of rail cuts 1950–1980. Notes: Figure reports mean log populations for Parishes, in groups corresponding to quintiles of the reductions in rail centrality between 1950 and 1980.

journeys.[19] Since private car use was relatively rare in Britain in the 1950s, this is a good approximation. We use these station-to-station rail travel times and parish-to-station road travel times to compute station-level centrality indices as described in Section 3.3. Although speeds will have changed over the decades of the analysis, we fix them at the 1950s speeds to ensure that all changes in centrality reflect network structure alterations and not arbitrary changes in the assumed travel speeds. That said, the estimates are not particularly sensitive to these assumptions because they are primarily driven by changes in physical network structure. For part of the analysis, we also use a centrality index based on the motorway network which is described alongside corresponding results. Descriptive statistics of all variables are presented in Appendix Table A1.

4. Results

4.1. Baseline regression results for 1981 populations, controlling for population pre-trends

Table 1 shows results from our base specifications for residential populations in parishes in Britain in 1981. The table shows regression coefficients and robust standard errors, corresponding to Eq. (1), estimated as discussed in Section 3.1 We experimented with clustering standard errors at higher levels of geographical aggregation (LGDs) but the results are broadly similar. We look at alternative clustering schemes based on grid squares when we discuss estimates with controls for geographical grid-square trends in Section 4.4.

Column 1 is a simple regression of the change in log population on the change in centrality with no control variables other than initial log centrality in 1951 and parish land area. Column 2 adds controls for log population and log population squared in 1901, 1911, 1921 and 1931. Column 3 controls instead for dummies for 5 percentile bins in the distribution of the changes in log populations in previous decades and columns 4–8 implement the pairwise difference approach to eliminating these pre-trends. In the latter, standard errors are robust to the autocorrelation induced by the pairwise differencing, using a Bartlett kernel with lag length 2. All coefficients show the effect of an implied increase in centrality: a positive sign indicates that the rail cuts reduced the outcome variable under investigation.

The most striking feature of Table 1, is that – although controlling for population pre-trends makes some difference, between column 1 and 2 – the exact method of control hardly changes the estimated coefficients. The elasticity of population with respect to centrality remains around 0.3 in all cases, suggesting that a 10 percent decrease in a parish's centrality is associated with a 3 percent decrease in population relative to a parish where centrality is unchanged. It is worth emphasizing at the outset that the centrality measure is simply an index of transport accessibility. Therefore, the scale of this elasticity – though not its qualitative implications – varies based on assumptions about its structure. We revisit this when we discuss alternative measures of exposure to rail cuts in Section 4.4.

To assess the effectiveness of our pairwise difference strategy in Table 1, columns 4–8, we estimated placebo pre-trend regressions of the specification in column 8 where we replace the dependent variable in Eq. (1) with either (i) 1951 population, controlling for 1921 population; or (ii) 1931 population, controlling for 1901 population; or simply (iii) the 1951 population. As expected, the coefficient on the 1981–1951 centrality change variable is zero in all cases. Evidently, matching on pre-trends does reliably eliminate the differences in trends exhibited in Fig. 4. This result is a somewhat mechanical outcome of the estimation method, but demonstrates its effectiveness. We would ideally have other pre-1951 variables on which to base this test, but unfortunately none are available from the census records.

4.2. Instrumental variable estimates

We now turn to the results of our IV estimates where we use the three instruments discussed in Section 3.2. Conditional on the extensive

[19] One additional data point other than bus timetables – though from an earlier period – is the figure of 12 miles per hour (19.2 km per hour) reported for average off-peak speeds of a 'motor driven cab' in 1904 (Hicks and Allen, 1999).

Table 1
Changes in rail network centrality and 1981 populations in parishes, Great Britain.

	(1) OLS no pre-trend	(2) OLS lagged pop	(3) OLS pre-trend all	(4) Pairwise diff 1901
Δ log centrality 51–81	0.367***	0.324***	0.270***	0.296***
	(0.023)	(0.022)	(0.021)	(0.021)
Observations	13,253	13,254	13,254	13,253
R-squared	0.032	0.899	0.901	0.853
	(5) Pairwise diff 1911	(6) Pairwise diff 1921	(7) Pairwise diff 1931	(8) Pairwise diff matched
Δ log centrality 51–81	0.302***	0.313***	0.297***	0.318***
	(0.021)	(0.021)	(0.020)	(0.021)
Observations	13,253	13,253	13,253	13,253
R-squared	0.854	0.863	0.871	0.691

Notes: Robust standard errors in parentheses, *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Dependent variable: parish population in 1981 (based on 1951 parish geographical definitions). All regressions include log centrality in 1951, mean distance to stations 1951, and parish land area. All regressions except column 1 include log population in 1951. Column 1 has no other controls; column 2 includes log population in 1931, 1921, 1911, 1901. Column 3 includes dummies for 5 percentile bins in the distribution of changes in log population between 1901–1951, 1911–1951, 1911–1951, 1931–1951. Columns 4–7 estimated on pairwise differences between observations ranked on changes in log population between the given year and 1951. Column 9 estimated on pairwise differences between matched observations ranked on linear predictions from regression of 1951–1981 change in centrality on quadratic in log population in 1901 and quadratics in change in log population 1901–1951, 1911–1951, 1911–1951, 1931–1951. Sample size in columns 4–7 depends on having population variables in both 1951 and the respective base year (1901, 1911, 1921, 1931). Sample size in column 8 drops one observation relative to 1–3 due to differencing in the ranked sample.

controls for pre-trends, this will help us understand whether there are any other unobserved local shocks that affected both the probability to experience line cuts and prospects for future development. The results are presented in Table 2. Column 1 uses distance to the predicted redundant trunk network in 1950 as an instrument for changes in the network centrality index between 1951 and 1981. Column 2 reports results for the construction-year instrument where the excluded instruments are the 3rd to 5th quintiles of the line opening years (which are tabulated in Appendix Table A2). Column 3 presents results for the east–west instrument and column 4 uses all three instruments jointly. All estimations are conditional on controls for pre-policy centrality, population and population trends (we use pairwise differences between matched observations ranked on linear predictions from Eq. (2)). In column 1, we additionally condition the instrument on straight line distance (and its square) from each parish to the network in general and in column 3, we condition the instrument on the overall length of lines passing through the parish.

The first stages of all three IV regressions have strong F-statistics, implying the instruments work well in predicting which places lost rail connections. In column 1, the IV coefficient of 0.34 is around 10% larger than our preferred estimate from Table 1, column 8. In column 2, the line opening dates instrument gives a somewhat larger coefficient of 0.47.[20] In column 3, the east–west lines instrument gives an even higher coefficient of 0.73, though in both cases the standard errors are large. When we use all three instruments jointly, we find an elasticity of 0.35 which is very similar to the coefficient in column 1 and to Table 1, column 8. None of the IV estimates is statistically significantly different from our preferred estimate from Table 1, column 8. At the same time, the Sargan test suggests that we cannot reject the null that the overidentifying restrictions are valid (p-value 0.35). As apparent from the specifications in columns 1–3, this indicates that our IV parameter estimates are statistically insensitive to the instrument set used, and treatment heterogeneity is not a first order concern (Windmeijer, 2019).

A possible explanation for the higher IV coefficients is that we are smoothing out a lot of the idiosyncratic variation in rail centrality change, and population responds more to these broader spatial patterns of accessibility than to the localized patterns. In other words, our raw centrality index change is a noisy measure of the underlying changes in accessibility which affect population patterns, suggesting our main estimates are downward biased. Given this interpretation and the fact that the OLS and IV estimates on the pairwise-differenced regressions are not significantly different and in a similar range, we conclude that additional biases from shocks after controlling for pre-trends are of second order importance. In the following sections, we will therefore focus on the pairwise-differenced OLS specification of Table 1, column 8.

4.3. Interaction of cuts with existing transport

In the next part of the analysis, we examine interactions with existing transport, first rail and then roads, to assess how prior transport access mitigated the effects of the rail cuts.[21] The results are presented in Table 3. Column 1 extends the base specification by including a dummy for above/below median rail centrality in 1951 and its interaction with the 1951–1981 change in rail centrality. Evidently, initial rail centrality matters (3rd row column 1), with parishes above median rail centrality experiencing 22 percentage points higher population growth than those below the median. However, the coefficient on the interaction between initial centrality and the 1951–1981 changes in centrality is small and insignificant and the coefficient on the change in centrality is unchanged at 0.3. Column 2 takes this further by controlling for an indicator that a parish does not have station access within 10 km in both the 1951 and 1981 periods, and its interaction with the rail centrality change index. The idea is to distinguish the effects of the cuts in peripheral parishes which were not targeted by the cuts – they were remote from rail both before and after the cuts – but nevertheless experienced centrality changes. Panel (b) of the figure in Appendix Figure A1 illustrates the geographical distribution of these areas: they are rural and peripheral. The results in column 2 show that the effects of the cuts were quite general within both remote-from-rail and less remote areas.

Next, we turn to roads. The specification in column 3 includes the distance between the parish and the nearest A or B road, the major roads before the construction of motorways from the 1960s onward. Population growth falls with distance to the nearest main road, but the coefficient on the interaction between main roads and the rail cuts is insignificant and the main effect of the rail cuts is similar to that in our baseline specifications. In other words, there is no evidence that our main estimates are biased by pre-existing trends related to road infrastructure. In column 4 we consider an indicator

[20] If we use only quintiles 3 and 4 as excluded instruments and include quintile 5 in the second stage to control for potential direct effects from newer openings on subsequent population changes, the results are largely unchanged with a coefficient of 0.499, and standard error of 0.270.

[21] Remember that the main specifications already controlled linearly for rail centrality in 1951.

Table 2
IV estimates based on network geometry and history.

	(1)	(2)	(3)	(4)
Δ log centrality 51–81	0.341*	0.469+	0.730**	0.348**
	(0.139)	(0.269)	(0.272)	(0.118)
Opening period 1st quintile	–	0.031	–	−0.005
		(0.023)		(0.016)
Distance to predicted trunk lines 1950 (10 km)	−0.056***	–	–	−0.053***
	(0.005)			(0.005)
Distance to predicted trunk lines 1950 squared (10 km)	0.005***	–	–	0.004***
	(0.000)			(0.000)
Length lines passing through Parish 1950 (km)	–	–	0.002***	0.001***
			(0.000)	(0.000)
First stage				
Excluded instruments	Distance to pred. redundant lines	3rd and 4th quintile opening date	E–W running line length	All
Opening period 1st quintile	–	0.035***	–	0.025***
		(0.008)		(0.008)
Opening period 3rd quintile	–	−0.031***	–	−0.024***
		(0.008)		(0.008)
Opening period 4th quintile	–	−0.066***	–	−0.063***
		(0.008)		(0.008)
Opening period 5th quintile	–	−0.036***	–	−0.037***
		(0.008)		(0.008)
Distance to predicted redundant lines (10 km)	0.038***	–	–	0.036***
	(0.003)			(0.003)
Length E–W lines passing through Parish (km)	–	–	−1.900***	−1.567***
			(0.240)	(0.236)
First stage F-Stat.	234.78	31.19	62.52	81.75
p-value	0.000	0.000	0.000	0.000
Sargan chi-squared	–	–	–	3.296
Overidentification p-value				0.348
Observations	13,160	13,160	13,160	13,160

Notes: Estimated on pairwise differences between matched observations ranked on linear predictions from regression of 1951–1981 change in centrality on quadratic in log population in 1901 and quadratics in change in log population 1901–1951, 1911–1951, 1911–1951; 1931–1951. HAC Robust standard errors in parentheses *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, +0.10. Instrument in columns 1 is distance to predicted redundant trunk network based on least cost 'traveling salesman' routes between LGDs of populations $> 80k$ and least cost routes between LGDs of population $> 80k$ and London, after eliminating routes that are predicted trunk routes on the full network. See text for details. Instruments in column 2 are dummies for quintiles of line opening dates, as set out in the table. Instrument in column 3 is length of lines passing through Parish in an E–W direction (based on less than 10k north to south between line end points). Regressions include controls for log population in 1901–1951, log centrality in 1951, parish land area and distance to 1950 rail network. Sample smaller than main estimates due to missing opening date instrument data.

Table 3
Effects on 1981 population of changes in rail centrality; interactions with other transport.

	(1) Above median 1951 rail centrality	(2) Parish >10 km from stations, 1981 and 1951	(3) Dist. pre-motorway main roads
Δ log rail centrality 51–81	0.293***	0.329***	0.323***
	(0.026)	(0.022)	(0.029)
×Column heading variable	0.072	0.130	−0.004
	(0.041)	(0.069)	(0.017)
Column heading variable	0.201***	−0.063	−0.048*
	(0.053)	(0.084)	(0.021)
Observations	13,253	13,253	13,253
R-squared	0.693	0.695	0.693
	(4) Above median cars 1951	(5) Δ 1951–1981 motorway cent. (pop weighted)	(6) Δ 1951–1981 motorway cent. (pop weighted)
Δ log rail centrality 51–81	0.415***	0.326***	0.454***
	(0.031)	(0.021)	(0.134)
×Column heading variable	−0.147***		−0.187
	(0.041)		(0.194)
Column heading variable	−0.148**	0.304***	0.074
	(0.051)	(0.063)	(0.246)
Observations	13,229	13,245	13,245
R-squared	0.692	0.691	0.691

Notes: HAC robust standard errors in parentheses *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. parish level regressions based on matched pairwise differences, as in Table 1, column 8. First row shows baseline effect of change in centrality in parishes in low access group. Second row shows interaction with high access indicator. High/Low access defined in column headings. Distance to pre-motorway main roads is distance to A or B classified roads Change in road centrality 1951–81 is the change in a population weighed centrality index due to construction of the motorways and general road speed increases, 1951–1981. Sample smaller in columns 4–6 due to missing motorway centrality values for islands and missing car data.

for car ownership in 1951 instead.[22] The results are interesting: car ownership itself is associated with lower population growth, but the interaction implies that the cuts had less impact in places with high car ownership. Nevertheless, the implied effect of the railway cuts in high-car-ownership parishes is still large, with an elasticity of 0.268.

The final part of this analysis looks at the interaction of the changes in the rail network with changes in the road network, primarily the growth of the motorway network. In the UK, motorways are dual carriageway highways, typically with 3 lanes per carriageway and a 70mph speed limit. Notably, there were no motorways in 1950 although the basic road network was already highly developed. As a result, the construction of motorways over the 1960s, 70s and beyond mainly increased speeds while distance reductions were rare. We construct the index of motorway closeness centrality/market access using a standard inverse travel time-weighted population centrality index. Unfortunately, we do not have a road network for 1950 or for 1980. Instead, we construct the road network for 1980 by deleting motorways constructed after 1980.[23] We assume vehicle speeds of 60 miles per hour on motorways (97 km per hour), 30 miles per hour (48 km per hour) on A-roads (the highest category road in Britain at the time), and 18.6 miles per hour (30 km per hour) on the imputed links between parishes and their nearest A-road network connections in 1980. These figures are approximations based on the Department for Transport's current average speed data, which have been stable over the past decades. For 1950, we use the same road network but limited travel times to 18.6 miles per hour (30 km per hour). This approach may overestimate journey time reductions and road-based centrality, but our aim is to give roads the best shot at explaining changes in population. The map in panel (c) of Appendix Figure A1 shows these imputed market access/centrality changes in relation to the motorway network at the end of the 1970s.

Columns 5 and 6 show results where we consider the impact of motorways and their interaction with rail. In column 5 we include a measure of the change in accessibility induced by the construction of motorways over the 1950–1980 period. Introducing this control for the improvement in road transport and the growth of the motorway network makes no difference to our estimate of the effects of the change in the rail network. The reason for this is that, conditional on 1951 rail centrality, 1951 population, and the population pre-trends, there is almost no correlation between the motorway and rail-based centrality changes. To illustrate this, we regress our log motorway centrality change variable on the log rail centrality change variable and find a coefficient of only 0.01 (although statistically significant, not tabulated). Interestingly, the coefficient on the change in motorway centrality in the population regression in column 5 is itself very similar to that on rail. Given that there is no reason to expect the impacts of access by one mode to be markedly different according to mode of travel, this result provides some degree of confidence that our results have an economically meaningful interpretation.

The final specification in column 6 adds in an interaction between the changes in road centrality and changes in rail centrality. This accounts more flexible way for road and rail accessibility than a single multi-modal index because it permits distinct and complementary effects on population change, rather than assuming perfect substitutability for given travel times.[24] The main effect of rail centrality in row 1, corresponding to parishes that experienced little growth in motorway centrality, is twice as large as in previous columns. The coefficient on the interaction term (row 2) is also large albeit imprecise. This suggests that the rail cuts had a much bigger impact on population decline in places which did not benefit from the growth of the motorway network and improvements in road speed. Put differently, the effects of the rail cuts were mitigated by motorway centrality. Conversely, motorways had a much more limited effect on population change in areas that were unaffected by the rail cuts (row 3), but their effects were enhanced by the decline of railways.

4.4. Robustness checks

This section summarizes a battery of checks that assess the robustness of our main estimates, described in more detail in Appendix C. We first consider potential biases that may arise from general changes in the spatial structure and the contemporaneous, policy-led development of 'New Towns' throughout our study period. Results, reported in Appendix Table A3, indicate little support for the idea that our results are biased by spatial centrality, rurality, or the New Town policy. Furthermore, we find the impact of the cuts is not strongly heterogeneous in more or less spatially peripheral places, or in distance from large cities, although the results do highlight that New Towns that were affected by rail cuts experienced much less population growth than they would have done otherwise.

As an alternative way to control for possible local confounders, in a second set of robustness checks reported in Appendix Table A4, we allow for arbitrary spatial trends by introducing fixed effects for grid squares of different magnitudes (20 km, 50 km, 100 km). To assess possible concerns about inference, we cluster standard errors at the level of the grid squares. It is reassuring that parameter estimates are almost identical to the baseline estimates reported in Table 1 and while standard errors are larger, they still imply t-statistics of at least 9, so Type I errors seem unlikely.

In a third exercise reported in Appendix Table A5, we experiment with different definitions of rail access. We obtain results that are qualitatively similar to our baseline estimates if we use simpler measures of rail accessibility such as the change in the distance to the nearest station, or an indicator for the closure of the nearest station. Next, we revert to using the accessibility index from our base specification (Eq. (3)) but modify the assumptions. First, applying destination node parish population weights yields parameter estimates that are highly similar to the unweighted index of column 8, Table 1. Clearly, changes in network structure and station loss are more pivotal than specific weights on destinations in the centrality index. Second, we show that computing accessibility over larger and smaller sets of nearest stations rescales the accessibility index and hence coefficients but this does not affect interpretation, as is readily apparent by the similar patterns we obtain for quintiles of centrality index changes across variants.

Lastly, to further support our claim of not capturing effects from targeted cuts, Appendix Table A6 presents results using 474 stations proposed for closure but never actually closed. However, we note that these proposed closures are not an ideal 'placebo' test, as they often remained open for specific reasons, like social necessity in remote areas with limited roads. As a result, parishes near proposed closures are not necessarily comparable to those where closures went ahead. With this caveat in mind, we find that there are population changes associated with the proposed but unenacted closures, but they are 50%–75% smaller than those linked to actual closures.

5. Mechanisms and longer-run effects

5.1. Global versus local centrality effects

As outlined in Section 3.3, we can decompose an area's overall rail centrality index change into two separate centrality indices, a local centrality index measuring the effect of local station removals, holding

[22] We only observe this measure at the Licensing Authority, a higher geographical level than parishes.

[23] This method overlooks improvements on A-roads and new non-motorway links. However, it is important to note that (i) non-motorway road accessibility improvements mostly stemmed from speed increases, not distance reductions; and (ii) compared to motorway construction, these minor physical network changes are of second-order importance for our estimates.

[24] Additionally, constructing multi-modal network is infeasible due to data limitations.

Table 4
Local versus global centrality changes.

	(1)
Δ log centrality 51–81 due to removal of local stations	0.423***
	(0.029)
Δ log centrality 51–81 due to global network changes	0.144***
	(0.041)
Observations	13,253
R-squared	0.691

Notes: HAC Standard errors in parentheses *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Parish level regressions based on matched pairwise differences, as in Table 1, column 8.

the network constant, and a global centrality index assessing the effect of network changes, holding the set of local stations constant (see Gibbons and Wu, 2020) Doing so helps us understand whether it is the additional road travel time to the nearest local station that matters, or changes in the preserved stations' global network centrality, e.g. because some links to cities nearby have been removed. The results are shown in Table 4. The average reduction in the global centrality index is much greater (−63%) than the reduction in the local centrality index (−21%). This reflects the massive changes to the national railway network (see Appendix Table A1). However, there is more variance across parishes in the local centrality index than the global centrality index (standard deviations of 23% and 15% respectively). The specifications in Table 4 are otherwise as for Table 1, column 8. The estimates show that local and global centrality matter, although the elasticity on local changes is much larger than that on global changes. The larger variance and effect size of local centrality changes suggests that costs from changes in the distance to a local station are key in shaping the spatial population distribution, more so than overall accessibility losses. This explains why Beeching era cuts are seen as primarily affecting rural communities with station closures, rather than areas retaining their stations, but still impacted by broader rail network removals.

5.2. Longer run and broader geographical population impacts

So far, we have looked at 1981 outcomes, and localized population redistribution at parish level. One might wonder whether these effects were only temporary. Perhaps the subsequent growth of car transportation meant that people gradually moved back to these areas that were disconnected from the rail network. A second question is how the population redistribution at parish level affected the more general pattern of populations across cities and regions: were the movements highly localized or are there implications for broader geographical patterns of population change?

Table 5 explores the first question by repeating the specification of Table 1, column 8 but with parish populations from the 1991 and 2001 censuses. Columns 1 and 3 clearly show that the effects were not temporary. The elasticity of 1991 and 2001 populations with respect to changes in centrality is much the same as for 1981 populations, implying that the effects of the rail cuts were permanent. In columns 2 and 4, we look at the effects conditional on previous census years. Controlling for 1981 populations in the 1991 population regression wipes out the effects of centrality: evidently the 1950–1980 rail cuts affected 1981 populations but had no additional impacts after that. The story for 2001 is slightly different. Now, conditional on 1981 and 1991 populations, we find that the 1950–1980 rail cuts had an additional impact on population growth up to 2001. The coefficient implies that a 10% cut in rail access in the 1950s, 60s or 70s led to further declines in population of around 0.5% after 1991. We have no data that can shed light on the reasons for this additional impact post-1991, but potential explanations are increased congestion on roads, or the shift from manufacturing to services in the UK economy, both of which may have favored places that remained better connected by rail in recent years. An alternative possibility is that the gradual depreciation

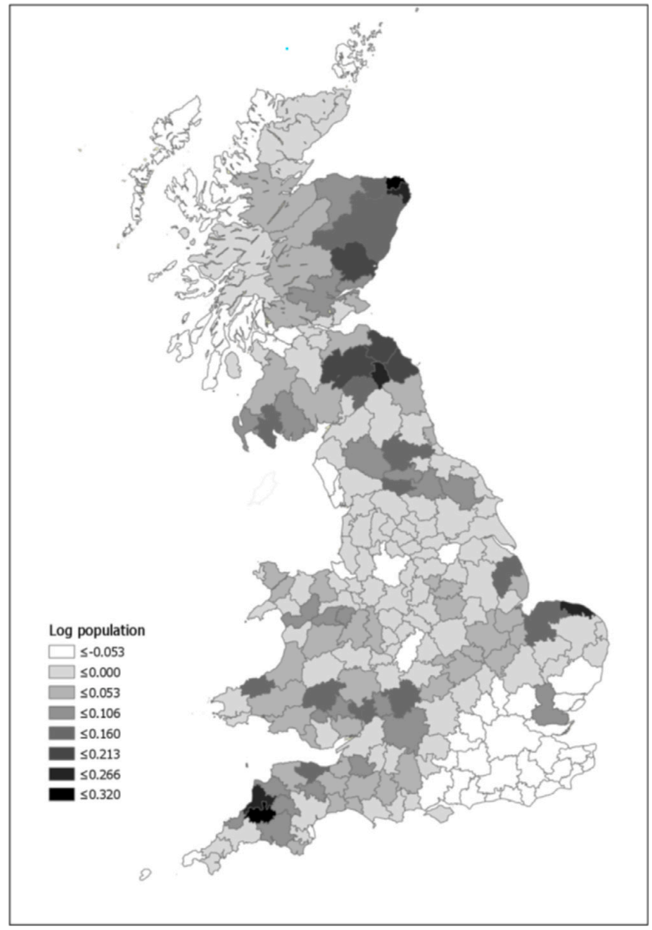


Fig. 5. Predicted counterfactual log population changes, without rail cuts, at TTWA, 2001. Notes: Figure shows Travel to Work Areas and shading indicates predicted change in 2001 log population under the counterfactual scenario in which the rail network in 1950 is preserved.

of houses and other durable capital built before the cuts means the benefits of these places naturally erode over longer horizons.

We looked deeper into the way these long run population changes related to use of housing, given its durability (Glaeser and Gyourko, 2005). Did the fall in local demand in places affected by the rail cuts just mean fewer houses were built, or did houses fall vacant in the long run, or did the use of housing change towards other uses like second homes or holiday-let accommodation? Our analysis using data from 1981, 1991 and 2001 on parish housing occupancy suggests that the number of houses occupied as a main residence grew in line with the population changes, but there was an increase in housing not used as a main residence in places most affected by the cuts (with no change in vacant housing). The results are reported in Appendix Table A7. Although the exact mechanisms at work here cannot be uncovered by these data, an obvious explanation is that reduction in demand for housing in places affected by the cuts led to re-use of housing in these areas as second homes. This is consistent with the observed rise in ownership of rural and village second homes by urban residents in the UK during the second half of the 20th century.

By construction, the patterns of population redistribution at the parish level mechanically follow the patterns of change in centrality shown in Fig. 3. To investigate what these patterns imply for the city-size distribution, we first predicted the counterfactual 2001 population distribution across parishes, by subtracting the component attributable to the centrality cuts based on our estimates in Table 5. We then aggregated the actual and predicted parish populations to 2001 Travel

Table 5
Long run effects on parish populations in 1991 and 2001.

	(1) 1991	(2) 1991, conditional on 1981	(3) 2001	(4) 2001, conditional on 1991 & 1981
Δ log centrality 51–81	0.296***	−0.003	0.299***	0.047***
	(0.024)	(0.014)	(0.021)	(0.013)
Observations	13,249	13,249	13,253	13,249
R-squared	0.635	0.872	0.643	0.871

Notes: HAC Standard errors in parentheses *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Parish level regressions based on matched pairwise differences, as in Table 1, column 8. Sample smaller in columns 1, 2 and 4 due to missing 1991 and 1981 population data.

to Work Area (TTWA) level (commuting areas). The results are mapped in Fig. 5, which shows the difference in logs between the counterfactual and actual 2001 TTWA population distributions. The figures are adjusted such that the total population is the same under the actual and counterfactual scenarios and a negative number implies the TTWA population would have been lower without the rail cuts. The most obvious feature is that populations throughout London and the South East of Britain would have been at least 5% lower. The population of London itself comes out as 8.9% lower. Other major cities – Birmingham, Manchester, Glasgow – also show up as having lower counterfactual populations in the absence of the rail cuts.[25] Overall, without the cuts, population would have been more evenly distributed across TTWAs. This is a result of shrinking the larger TTWAs: the standard deviation of populations in the actual distribution is 580,000 compared to 550,000 in the counterfactual. London's population shrinks from 8.2 million to 7.8 million in the absence of the rail cuts. In the Conclusion, we provide some remarks about what this might mean for productivity, given the well-established links between city size and productivity in the urban literature (Combes and Gobillon, 2015).

5.3. Age, education, occupational structure and jobs

In a last exercise, we use census data for a range of socioeconomic outcomes at the Local Government District Level (LGD) covering England and Wales. Looking at changes in the composition of the local population will give us a better understanding of the longer-run effects. Table 6 presents results from regressions with a specification similar to Table 1, column 8, but with different dependent variables relating to 1981 male education (educated to age 20+), occupation-based social class (class 1 is professional, class 2 is intermediate, class 3 is skilled, class 4 is partly skilled, class 5 is unskilled), population age structure, jobs per resident, or commuting patterns in 1971.

In column 1 we see that reductions in centrality reduced the proportions of high-qualified (defined as education to age 20+) in the district. Similarly, in columns 2 and 3, we observe relative reductions in professional and managerial male workers, offset by a relative increase in workers in lower skill occupations in columns 4–6. Note these regressions are conditional on the log total numbers in all social class groups, so should be interpreted as changes in the share of one group holding the total constant. Looking at the age structure in columns 7–9, there is clear evidence of a negative association between centrality and the share of workers over 65 (i.e. a decline in centrality implies an older population) and a positive association with working age populations. These LGD results suggest that changes in rail centrality had non-negligible impacts on local population composition. For example, places that were one standard deviation above the mean in the distribution of cuts (the standard deviation is around 0.27 in the LGD data) would have seen 4% less growth in the number of educated males in the population relative to the mean, and 3% more growth in the number of males over retirement age (holding total population constant). Column 10 further shows that this reduction in working age population went in hand with a loss of local jobs per person, where we calculate employment in LGDs in 1951 using the Census Report on Usual Residence and Workplace, and in 1981 by reweighting the ward level Census flows available from the UK Data Service. Using the same data sources, columns 11 and 12 show equal sized effects on the log number of residents working inside and outside the local area, suggesting no change in net commuting.[26] We interpret this as evidence that it was a loss of local jobs in response to the rail cuts that led to population movements, rather than a loss of commuting opportunities.

6. Conclusions

We examined the impact of a controversial rail disinvestment program that occurred in Britain in the mid-20th century. Unlike other work focusing on the spatial economic impacts of transport network expansions, ours is the first to investigate large-scale transport infrastructure removal. Our results offer general lessons for the role of transport infrastructure in shaping the spatial economy, and they address the longstanding debate over the 'Beeching Axe' in Britain: did the cuts cause relative decline in affected areas, or were these places already on a downward trajectory?

The broad finding is that the cuts in rail infrastructure caused falls in population in affected areas relative to less affected areas, loss of educated and skilled workers, a loss of jobs, and an aging population. Housing in affected areas saw an increase in use for second homes. A 10% reduction in rail from 1950 to 1980 was associated with a 3% fall in population by 1981, relative to unaffected areas. Put another way, the 1 in 5 places in Britain that were most exposed to the rail network cuts saw 24 percentage points less growth in population than the 1 in 5 places that were least exposed. Populations did not recover in subsequent decades. A key lesson is that rail infrastructure affects the spatial distribution of population—a relevant finding for those interested in the role of transport in land use and the spatial structure of the economy. A second key lesson is that some of the effects of rail infrastructure development on the population are impermanent, as population readjusts once the infrastructure is removed. To compare the population effects of station closures with construction, we turn to Bogart et al. (2018b) who studied population changes during the evolution of the railways in 19th century Britain. Their results suggest that parishes receiving a new station between 1831 and 1841 experienced on average a 30 percentage point increase in population compared to parishes with a station opening in the 30 years from 1831 to 1861.

Our roughly-corresponding estimates (presented in Appendix Table A5) suggest that the population loss in parishes where the nearest station closed between 1951 and 1981 was less than half what was gained upon opening. This translates into a 13 percentage point fall

[25] There are also remote low population areas in Scotland that show up as having relative population losses in the absence of the cuts: as noted earlier, this is because their rail centrality change was small given that they were already poorly connected.

[26] Note that we report regressions for 1971 here because the local areas in 1971 are comparable to 1951. The same cannot be said for 1981 because major administrative reorganizations in 1974 reduced the number of districts by a factor of five.

Table 6
Changes in rail network centrality and 1971/81 outcomes in Local Government Districts in England and Wales.

	(1) Education	(2) Soc 1	(3) Soc 2	(4) Soc 3	(5) Soc 4	(6) Soc 5
Δ log cent. 1951–81	0.132*** (0.037)	0.175** (0.059)	0.102*** (0.029)	−0.037* (0.018)	−0.107*** (0.031)	−0.015 (0.085)
Observations	1465	1465	1465	1465	1465	1465
R-squared	0.782	0.659	0.849	0.920	0.795	0.401
	(7) Age 0–15	(8) Age 15–64	(9) Age 65+	(10) Jobs p.c.	(11) Work Out LA	(12) Work in LA
Δ log cent. 1951–81	−0.008 (0.012)	0.021*** (0.005)	−0.102*** (0.018)	0.128** (0.046)	0.170** (0.057)	0.151*** (0.043)
Observations	1465	1465	1465	1465	1465	1465
R-squared	0.958	0.992	0.880	0.376	0.605	0.618

Notes: Robust standard errors in parentheses *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Dependent variables are: (1) 1981 log higher educated males; (2)–(6) 1981 log males in social class 1–5, (7)–(9) 1981 population in age groups, (10) 1981 jobs per resident, (11) 1971 residents working outside the Local Authority area, (12) 1971 residents working inside the Local Authority area. Class 1 is professional, class 2 is intermediate, class 3 is skilled, class 4 is partly skilled, class 5 is unskilled. All regressions include log centrality in 1951, log population in 1951, log denominator for dependent variable in 1981 and 1951, log dependent variable in 1951 Estimated on pairwise differences between matched observations ranked on linear predictions from regression of 1951–1981 change in LGD centrality on quadratic in LGD log population in 1901 and quadratics in change in log population 1901–1951, 1911–1951, 1911–1951; 1931–1951 (as Table 1, column 8).

compared to other parishes.[27] Although we have no data that would allow us to empirically analyses the precise reasons for this asymmetry, standard theories of the persistence of the effects of infrastructure are likely to apply. Transport connections trigger the development of other forms of infrastructure, growth in population and firms. The consequent local agglomeration economies encourage population to stay long after the transport infrastructure is removed or relocated.

An important additional finding, though not our main focus, is that growth in road network accessibility due to the construction of motorways also affected the distribution of population, interacting with changes in rail centrality. Places that experienced improvements in accessibility through the motorway network were less affected by the rail cuts. In general, though, the places losing rail access were not those targeted by improvements in road access – the changes in rail and road centrality are uncorrelated – so the motorway network in Britain did little to compensate the places worst affected by loss of rail.

All these estimates relate to population movements and sorting. Unfortunately, we do not have the data to directly answer the question of whether there were aggregate, national gains and losses in terms of productivity, employment and welfare. However, by extrapolating from previous estimates of the relationship between access to economic mass and firm productivity or wages – Combes and Gobillon (2015) suggest ‘agglomeration elasticities’ around 0.05 at most – we can cautiously conclude that the effects from cutting the railways were probably not that large. There are two channels through which aggregate productivity changes might emerge. Firstly, by cutting connectivity between places, the rail cuts had a direct effect on the centrality and access to economic mass. This is part of the so called ‘wider benefits’ of transport accessibility. The mean reduction in centrality in Britain was around 40%, implying a direct reduction in productivity of around 2%. Secondly, cities in the South East, and especially London, might have gained population from areas with major cuts, potentially enhancing productivity through enhanced city size. We assess the scale of these effects by looking at what our estimates imply about the counterfactual distribution of population across cities, had the rail cuts not occurred. Our back-of-the envelope calculations suggest that the aggregate gains from population redistribution across cities were very small, at around 0.2%, leaving a net productivity loss from the rail cuts of around 1.8%.[28,29]

CRediT authorship contribution statement

Stephen Gibbons: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Stephan Heblich:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Edward W. Pinchbeck:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jue.2024.103691.

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[27] Unfortunately, their approach is not the same as ours. The nearest comparable figures are for 10-year population changes related to new stations in a parish, and appear in their Appendix Table A1. From these estimates, it looks like the population increase in a parish getting a new station between 1831 and 1841 was about 14 percentage points over a 10-year period. Their main results suggest growth of 15 percentage points over the subsequent 20-year period (their Figure 8) for parishes within 2 km of a station in 1841, relative to those more than 70 km away. Their published version of the paper Bogart et al. (2022) does not feature easily comparable estimates.

[28] From these actual and predicted city size distributions described in Section 5.2, we can estimate the additional productivity attributable to agglomeration that was caused by the rail cuts. We follow previous literature in assuming a benchmark city-size productivity elasticity of 0.05 such that aggregate city productivity is proportional to population raised to the power of 1.05.

[29] This has echoes of Robert Fogel’s (1964) claim that the social savings from the railroad system in the US were less than 3% of GNP, although the social savings methodology is based on the value of time, rather than any productivity impacts.

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