The Bankruptcy Express
Market Integration and Labor Reallocation in Industrializing Britain *

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Abstract

We investigate two determinants of firm exits: technological change and market integration. Contrary to previous studies, we argue that these two factors should not be considered separately: their interaction spurs firms' exit even more. To test this hypothesis, we introduce a new dataset on individual bankruptcies at the location-sector-year level in late 19^{th} century Britain, which we combine with rich micro-level census data. In this period, we investigate the effect of the British railway expansion on firm exits and employment changes. We find that the manufacturing sector – the one with most heterogeneous firms – experienced an increase in job creation and in firms' exits following the arrival of the rail. Accordingly, technological change and market integration work together to explain firms' failure and within-sector reallocation.

Keywords: Bankruptcies, Economic Growth, Structural transformation
JEL Codes:

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1 Introduction

With their 2030 Agenda for Sustainable Development, UN member states pledged that “no one will be left behind".[1] As policy-makers want to find the path to inclusive growth, we require a better understanding of the factors creating losers in the process of economic growth. Recently, the literature has proposed two main mechanisms explaining why economic growth may generate losers: (i) a reduction in trade costs (Autor et al., 2016; Melitz, 2003), and (ii) new technologies (Acemoglu and Restrepo, 2022; Juhász et al., 2020). In this paper, we ask whether these two factors can interact with each other. Hypothetically, if there are barriers to trade, differences in technology adoption should not endanger non-adopters. Similarly, if trade is open but firms are homogeneous, no intra-sector reallocation occurs.

To study the interplay between these two factors, we introduce a new measure of financial distress at the individual-level in England and Wales from 1788 until today: the universe of private bankruptcies. We leverage differences in the occupational exposure to technologies and difference in spatial exposure to the railway expansion during the second half of the 19th century to empirically estimate the interaction of trade and technologies as factors of individual distress. This way we go beyond the aggregate distributional effects of the railways shown in previous studies (Gregory and Henneberg, 2010; Donaldson, 2018; Bogart et al., 2022). Melitz (2003) notably emphasizes how trade fosters intra-industry reallocation. This model is consistent with aggregate welfare gains at the industry level despite individual losses for some firms. If firms have different levels of productivity and entering the export market is costly, the less productive firms exit the market whereas the more productive firms gain from trade. This argument has been tested empirically in the trade literature but has not yet been considered in the context of the industrial revolution and the market integration it generated (Autor et al., 2016). Yet, Melitz (2003) shows that intra-industry reallocation follows from costly transitions, should it be due to new technologies, trade or labor upskilling.

This mechanism resonates with the literature stating that industrialization potentially reduces the demand for some skills and hence occupations (Goldin and Katz, 1998). Such a destruction of occupations has also been captured indirectly by looking at the violent reactions of labor following the arrival of labor-saving technologies (Caprettini and Voth, 2020). Yet, the mechanisms driving this destruction remain to be understood. In this paper, we assess how the development of the railway impacted market structures by looking both at the number of workers in each occupational class and also the number of bankruptcies per occupational class. The development of the rail fostered industrialization and urbanization (Bogart et al., 2022). We argue that under such circumstances, the development of the

[1] See for more information https://sdgs.un.org/2030agenda, last visited Nov. 6th, 2023.

Table 1: Within versus Between sector reallocation

	Increasing Jobs	Decreasing Jobs
Increasing Bankruptcies	Within-Sector	Between Sectors
Decreasing Bankruptcies	Between Sectors	Within-Sector

industrial sector might have led to the personal bankruptcies of the less adaptable to foster aggregate growth.

To test this hypothesis, we analyze the factors explaining 150,000 bankruptcy cases focusing on the period of the British railway expansion between 1851 and 1890. We have information on the dates the bankruptcy cases were announced, the profession of the bankrupt, and their geographic location, aggregated to 708 hexagonal grid cells spanning England and Wales. In addition, we leverage data from the full British microcensuses in 1851, 1861, and 1881 to construct grid-cell specific employment data at the occupational class level (Schurer and Higgs, 2023). Our estimation strategy relies on the differences in the i) geographical exposure to trade from the railway expansion, and ii) sectoral exposure to technological change. Conceptually, we compare the effect of the rail expansion across two variables: the number of bankruptcies, and the number of workers. Using the logic depicted in Table 1, we are able to determine whether structural change shaped a specific sector mostly through within-sector reallocation or through between-sector reallocation. In this framework, bankruptcies result either from the loss of activity in a sector (captured by sector-level employment loss) or from changes in the sector market structure as the least productive firms/individuals are pushed out of the market. In periods of structural change, bankruptcies may result from either one of these channels. We leverage within-sector variation in exposure to the expansion of the railway combined with within-geographic unit sectoral differences in the effect of this exposure to disentangle the two channels. Intuitively, if the number of bankruptcies increases and employment does not decrease, then these bankruptcies result from intra-sector reallocation (upper-left cell in Table 1). In such a case, bankruptcies exhibit a pattern that does not match with estimators of between sector reallocation, or structural change (captured by changes in sectoral employment shares). This paper empirically investigates the different types of employment reallocation during the British railway expansion. We build on the work by Bogart et al. (2022) to use the expansion of the railway network as a proxy for structural transformation, and estimate its effect on sectoral employment and bankruptcies. This way, we disentangle the effect of the railway on market structure from its effect on the dynamism of the market.

Our results imply that the technologies interaction with market integration explains an increase in the number of bankruptcies. We find this effect only for the two sectors that transformed the most during the industrial revolution: trade and industry. As for the trade sector, the industrial sector was experiencing massive change and heterogeneous technological capabilities during our study period. Theses sector-specific changes generated heterogeneity between firms in those sectors. This heterogeneity is at the core of Melitz (2003) for the trading sector. We moreover show that the extension of the rail had no effect on the number of workers in those two sectors but negatively impacted the agricultural sector – in line with the effect of the rail on structural transformation. Both our baseline estimation strategy and an instrumental variable strategy show that connection to the railway network increased bankruptcies by 6 to 13 percent in the industrial sector while it had no effect on its employment level. The industrial sector and the trade sector are the only two sectors following this pattern.

Our research mainly contributes to two strands of literature. First, it offers a reinterpretation of Melitz (2003)'s theory emphasizing how economic changes may trigger intra-sector reallocation. In this paper, the trade shock comes from the railway connections. We moreover are able to distinguish the effect of this shock on between sector reallocation and on within sector reallocation by using bankruptcies as a new proxy for this within-sector reallocation. In the words of Melitz (2003), bankruptcies capture "least productive firms exiting". For that aspect, this paper's first contribution relies on the development of this new measure of reallocation. Second, this paper enters in the debate on the sectoral dynamics during the Industrial Revolution (Temin, 1997). Juhász et al. (2020) present evidence of intra-sector reallocation in the case of cotton-spinning in France. In their case, productivity was highly dispersed among firms and the low productive firms exited as mechanized cotton spinning developed. As Juhász et al. (2020) define intra-sectoral dynamics, our study adds a geographic dimension to such reallocation. Market integration increased intra-sectoral reallocation in a fastly-evolving sector. This intra-sectoral reallocation also has changed the nature of the firm and increased the number of clerical and administrative workers. Third, our paper offers a new perspective on the geographic impact of railways: increasing productivity (Donaldson, 2018), increasing the diffusion of ideas (Tsiachtsiras, 2022) and spurring economic growth (Donaldson and Hornbeck, 2016). The approach of this paper is similar to Bogart et al. (2022) and complements their estimate of the effect of the railways in 19th century England and Wales on urbanization and structural change. Our paper characterizes the nature of this structural change: it is biased against individuals in the sectors most impacted by market integration.

2 Historical background

2.1 Bankruptcy procedures – 19th century England and Wales

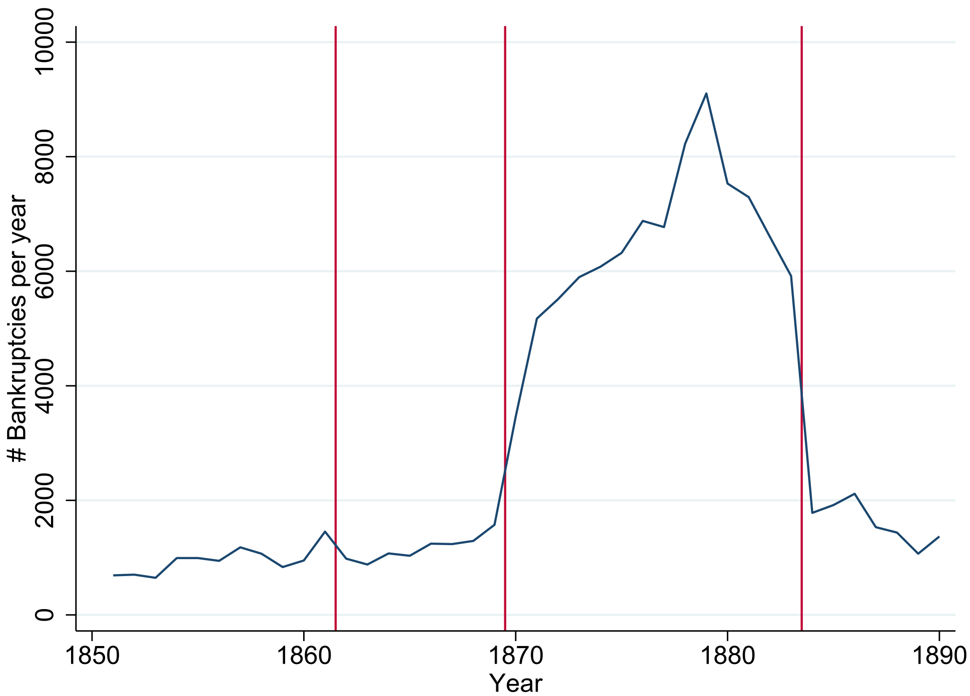
Bankruptcy procedures were at the forefront of political conversations throughout 19th century England (Lester, 1991). Debtors' prison illustrates well both the consequences of bankruptcy, how complex the system was and the importance of bankruptcy in the collective image of 19th century England.[2] Debtors that could not repay their debts were sent to prison until their labour could repay their debt. Throughout the century several reforms modernized both the procedure and the role of debtor's prison. From 1831, the procedure implied that officials would be appointed to collect and distribute the assets of bankrupts. Bankruptcy could then be initiated by both debtors and creditors. This doctrine of bankruptcy law called "officialism" was deemed inefficient by entrepreneurs and the business elites. The system of officialism was costly and its ability to recover unpaid debt limited. The 1869 Bankruptcy Act and Debtor Act massively changed this institution. After this series of reforms, debtors' prison was limited to debtors that were believed to have the financial means to repay their debt, but did not do so. Moreover the doctrine of officialism was repealed and a new system of bankruptcy management put in place. In this case, if a majority of creditors agreed, they could proceed to the management of the bankruptcy themselves.

This new management of bankruptcies advantaged creditors. Recovery rates were higher as creditors had direct incentives to recover as much of their debt as possible and they could avoid recovering small debts whose costs to recover were greater than the debt itself. Our dataset illustrates those changes. Figure 1 presents the evolution of the number of bankruptcies per year in the time-frame of our study.

Three reforms occurred during the period of our study. The 1861 reform broadened the scope of the bankruptcy procedures to apply it to all citizens and not only those having a trading activity. The 1869 reform repealed officialism whereas the 1883 reform reintroduced it. Figure 1 evidences the importance of the bankruptcy regime to determine the number of bankruptcies. In section 6, we leverage upon these differences in regimes to inform on the mechanisms potentially explaining more bankruptcies. Figure 1 shows the massive increase in the number of bankruptcies following the repeal of officialism. This shift shows how much creditors' incentives determine whether or not a bankruptcy takes place (through the official channel).

[2]Debtors are, for example, a common figure of Charles Dickens' work reflecting the author's father own experience as an inmate in a debtor's prison.

Figure 1: The Evolution of Bankruptcies



Notes: This figure presents the total numbers of bankruptcies by year based on our newly collected dataset. Red vertical lines indicate significant reforms to the bankruptcy laws in 1861 (bankruptcies extended to all occupations), 1869 (privatization of bankruptcy management), and 1883 (return to "officialism").

2.2 The expansion of the railways

Between 1851 and 1881, the railway network in England and Wales nearly doubled (Bogart et al., 2022). In 1851, the network covered mostly the central region of England whereas it had expanded to Wales and the South-Western part of England by 1881. By the end of the 19th century, the rail became the main mode of transportation for passengers and materials (Bogart et al., 2022).

The impact of the rail on the British has been at the center of academic debates for decades. Early scholars argued that the effect of the railways expansion on the economy was not clear and immediate in Britain, as opposed to other areas such as the U.S. (Mitchell, 1964). New Economic Geography models on the contrary emphasized the changes brought about by the rail (Lafourcade and Thisse, 2011). With decreasing transportation costs, the rail encouraged urbanization and structural change (Bogart et al., 2022). Similarly railways fostered growth in Germany (Hornung, 2015) and in the US (Donaldson and Hornbeck, 2016). It also increased firms' productivity (Hornbeck and Rotemberg, 2019) and increased the diffusion of innovative ideas (Tsiachtsiras, 2022). Beyond these rather positive effects, a few studies show that the transformations generated by the rail also generated sometimes negative externalities and partially reduced life quality for some citizens (Waugh, 1956).

3 Empirical strategy

3.1 Data

Bankruptcy Data. We collect information on individual bankruptcy cases from publications in the London Gazette. Already early in the 18th century, British bankruptcy law required making insolvencies public such that potential creditors had the chance to make their claims official and be considered in the debt clearing process. For this purpose, the London Gazette contained a separate section that announced new bankruptcy adjudications and informed debtors on ongoing cases. The London Gazette started out as the main public mouthpiece of the British government in 1665, was delivered on average two to three times per week, and is still being published today. The first bankruptcy notice was published in the issue of June 5th 1712, however still in an unstructured manner. We accessed all digitized London Gazette issues from June 1778 until today via the official London Gazette homepage.[3] From 1778 until 1986, the publications of bankruptcy announcements followed a relatively fixed structure, which allows us to easily collect and encode individual cases.

[3] For more information and to access the London Gazette issues, see https://www.thegazette.co.uk/.

Figure 2: Examples of Bankruptcy Announcements

WHereas a Commiſſion of Bankrupt is awarded and iſſued forth againſt Joſeph Fernandes, late of Chelſea in the County of Middleſex, Wine-merchant, Dealer and Chapman, but now a Priſoner in the Fleet-Priſon, and he being declared a Bankrupt is hereby required to ſurrender himſelf to the Commiſſioners in the ſaid Commiſſion named, or the major Part of them, on the 18th Day of Auguſt next, at Twelve o'Clock at Noon, on the 19th Day of the ſame Month, and on the 9th Day of September following, at Eleven o'Clock in the Forenoon, at Guildhall, London, and make a full Diſcovery and Diſcloſure of his Eſtate and Effects; when and where the Creditors are to come prepared to prove their Debts, and at the Second Sitting to chuſe Aſſignees, and at the laſt Sitting the ſaid Bankrupt is required to finiſh his Examination, and the Creditors are to aſſent to or diſſent from the Allowance of his Certificate. All Perſons indebted to the ſaid Bankrupt, or that have any of his Effects, are not to pay or deliver the ſame but to whom the Commiſſioners ſhall appoint, but give Notice to Mr. Moſely, Shoe-lane, London.

(a) Bankruptcy Announcement 1788

FIRST MEETINGS AND PUBLIC EXAMINATIONS.

ROSENBERG, Lewis (formerly trading as the VICTORIA TIMBER COMPANY), of and carried on business at 43A, Durant-street, Hackney-road, London. TIMBER MERCHANT.
Court—HIGH COURT OF JUSTICE.
No. of Matter—437 of 1929.
Date of First Meeting—June 26, 1929. 11 a.m.
Place—Bankruptcy Buildings, Carey-street, London, W.C. 2.
Date of Public Examination—July 16, 1929. 11 a.m.
Place—Bankruptcy Buildings, Carey-street, London, W.C. 2.

(b) Bankruptcy Announcement 1929

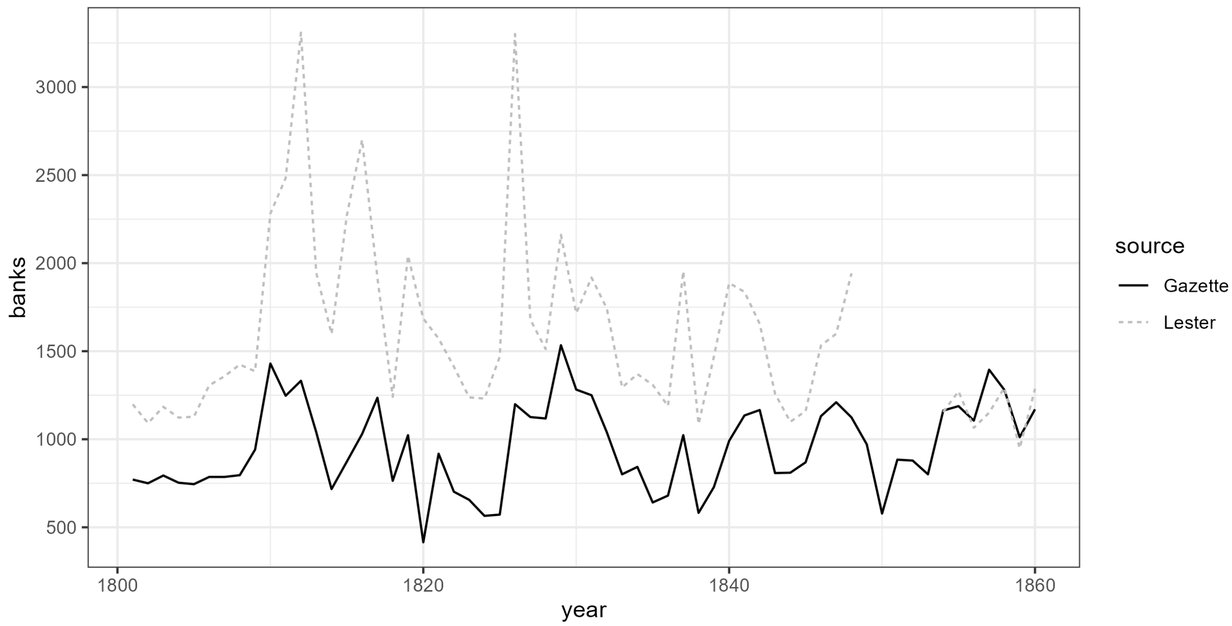
Within our sample period, bankruptcies were announced in the London Gazette in a rather standardized way as Figure 2 shows. All announcements within a certain time frame start in a similar way. To gather the individual bankruptcy announcements, we web-scraped scans of the 42,771 London Gazette issues published from 1788–1986 from the London Gazette homepage. These 41,771 issues include several supplemental publications that contain special information, but no bankruptcy announcements. We found 21,292 regular issues to include at least one bankruptcy statement each. Next, we used Optical Character Recognition (OCR) software to convert the scans into a machine-readable text format and started the computational processing.[4]

Figure 3 compares the yearly number of bankruptcies in our dataset to officially published statistics at the national level as collected by Lester (1991). Indeed, even though during the early 19th century our dataset contains significantly less observations that official statistics suggest due to unrecognized and uncoded cases due to bad scan qualities, our coding follows the general trend very closely. In addition, around the start of the sample period for our study when official publications become scars, our numbers are very close to the national aggregates. This makes us confident that sampling bias is unlikely to affect our estimations other than increasing standard errors due to random measurement error.

Our analysis would suffer from sampling bias if the coded information for (a) geographic coordinates or (b) occupational codes was missing for reasons related to our explanatory variables. Even though it is unfortunate that we are not able to provide full information of all bankruptcy cases, we are confident that this random sample assumption holds. The

[4]This process is detailed in Appendix B.

Figure 3: Comparison to National Statistics



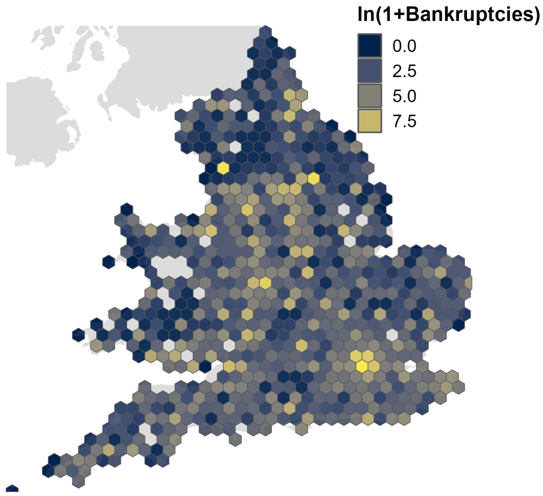
Notes: This figure compares the yearly number of bankruptcy cases in our dataset to the number in official national statistics collected by Lester (1991).

main reason why bankruptcy cases were incompletely coded are errors in the scan-to-text conversion via OCR. We think it is unlikely that the scan quality of certain gazette pages or bankruptcy announcements is non-random.

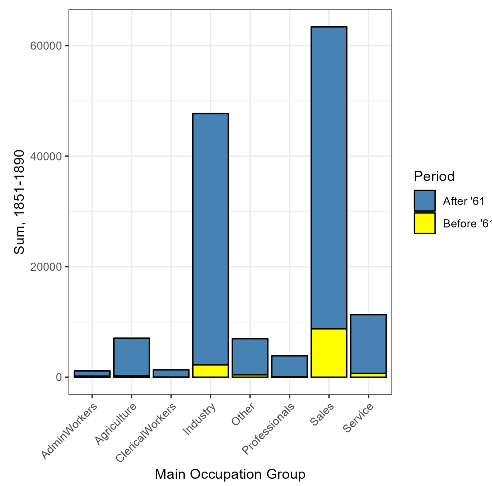
Our bankruptcy data show significant variation over space and occupations. We illustrate the spatial variation of bankruptcy cases in Figure 4 (a). We created a hexagonal spatial grid dataset with an area of 220km per grid cell (ca. 9km edge length), and aggregated the geocoded bankruptcy cases at the grid cell level. Clearly, the metropolitan areas around London, Liverpool and Manchester stick out. However, we also observe a significant amount of bankruptcy cases outside these metropolitan areas, e.g. in the South-East or in Cornwall. We see similar variation among the broadest, one-digit occupation categorization in Panel (b). By far, most bankruptcies occurred among trade workers. This includes all people that described themselves as "merchants" or "chapmen", by far the most often occupational title in the dataset. While the extra in bankruptcies in the sales sector accrues in part due to the fact that until the reform in 1861, bankruptcies were restricted to merchants, it also remains the predominant sector for bankruptcies after the reform, closely followed by the industrial sector.

British Microcensus. To observe sectoral employment together with a number of additional covariates, we make use of British microcensus data that were made available as part

Figure 4: Variation in Bankruptcies across Space and Occupation Groups



(a) Bankruptcies by Grid Cell, 1851–1890



(b) Bankruptcies by Occupation, 1851–1890

Notes: Figures display the variation in our bankruptcy data across space and across occupation groups. Panel (a) displays the natural logarithm of the bankruptcies that occurred across our sample period from 1851–1890 per grid cell. Panel (b) displays the number of bankruptcies during the same time frame split by the main occupation groups on which we base our dataset. Colors distinguish bankruptcies that occurred before and after the 1861 reform which extended the bankruptcy law to all occupations than only merchants.

of the Integrated Census Microdata (I-CeM) dataset (Schurer and Higgs, 2023). The I-CeM project digitized full, individual-level census data for England and Wales in 1851, 1861, 1881, 1891, 1901, and 1911. Importantly for us, all census entries contain information on people's occupation, for which the I-CeM project already coded the associated HISCO codes. Other control variables we add via the microcensus data are, among others, local population, age structures, gender ratios, and internal migration stocks. We assigned coordinates to all census observations based on the sub-district people answered in the survey, and intersected the subdistrict-coordinates with our grid cell. While the dataset also contains spatial information at the higher-resolution parish level, we restricted ourselves to the sub-district level because many historical parishes do not exist anymore today and names were not unique, such that an accurate geocoding of parishes across census waves was impossible.[5]

Additional Data. We complement our dataset with additional data sources that vary at

[5]The spatial representativeness of subdistricts varies with population densities. Bigger cities like London or Liverpool consist of tens of subdistricts. Yet, in very rural regions, especially in Wales and Cornwall, the subdistrict-density is rather low. As can be seen in Figure 5 below, we end up with some grid cells in these rural regions that do not contain any subdistrict coordinates. We therefore drop these grid cells from our sample.

the grid-cell level, over time, or both. First, we use data on the locations of railway stations in England and Wales in 1851, 1861, and 1881 from Martí-Henneberg et al. (2017a,b) and Martí-Henneberg et al. (2017c). We spatially intersect the railway shapefile with our grid cell dataset, and assign to each grid cell the sum of stations in a given year. We further leverage data from Fernihough and O'Rourke (2014), which locate the British towns with access to coal. We calculate the distance of each grid cell's centroid to the closest town with coal access as a proxy for coal availability in a location. As final geographic covariates, we calculate the distance to London, the coast, and UK ports from every grid cell's centroid.

Our final dataset follows the structure of the British census waves at the grid cell-sector-decade level. This is, we observe each occupation sector in each grid cell for every decade from 1851 to 1881, with the exception of 1871 when no British census data are available. For each census period, we add the sector-level annualized number of bankruptcies as our main dependent variable. For this, we aggregate all bankruptcy cases in a sector and grid cell over between two census periods, divide it by the number of years between the two census periods to control for the longer time span between 1861 and 1881, and assign this number to sector-grid cell observations in the year that begins the respective decade. We aggregate occupation sectors to the highest, 1-digit occupation category which divides occupation into seven main occupation groups plus the group we term "Other", which contains pensioners, rentiers, or unemployed individuals. Our main estimations will focus on the census years 1851, 1861, and 1881 for which we have separate data on the spatial distribution of railway stations at this point in time.

3.2 Econometrics

The main econometric specification leverages upon the three dimensions of information we have on bankruptcy. To estimate the specific effect of a railway connection on a sector, we use the variation within areas becoming connected to the railway network. The first set of estimations is based on the Difference-in-Differences estimator in which the treatment is the intensity of the railway connection – and the treatment varies across sectors. To do so we estimate the following equation:

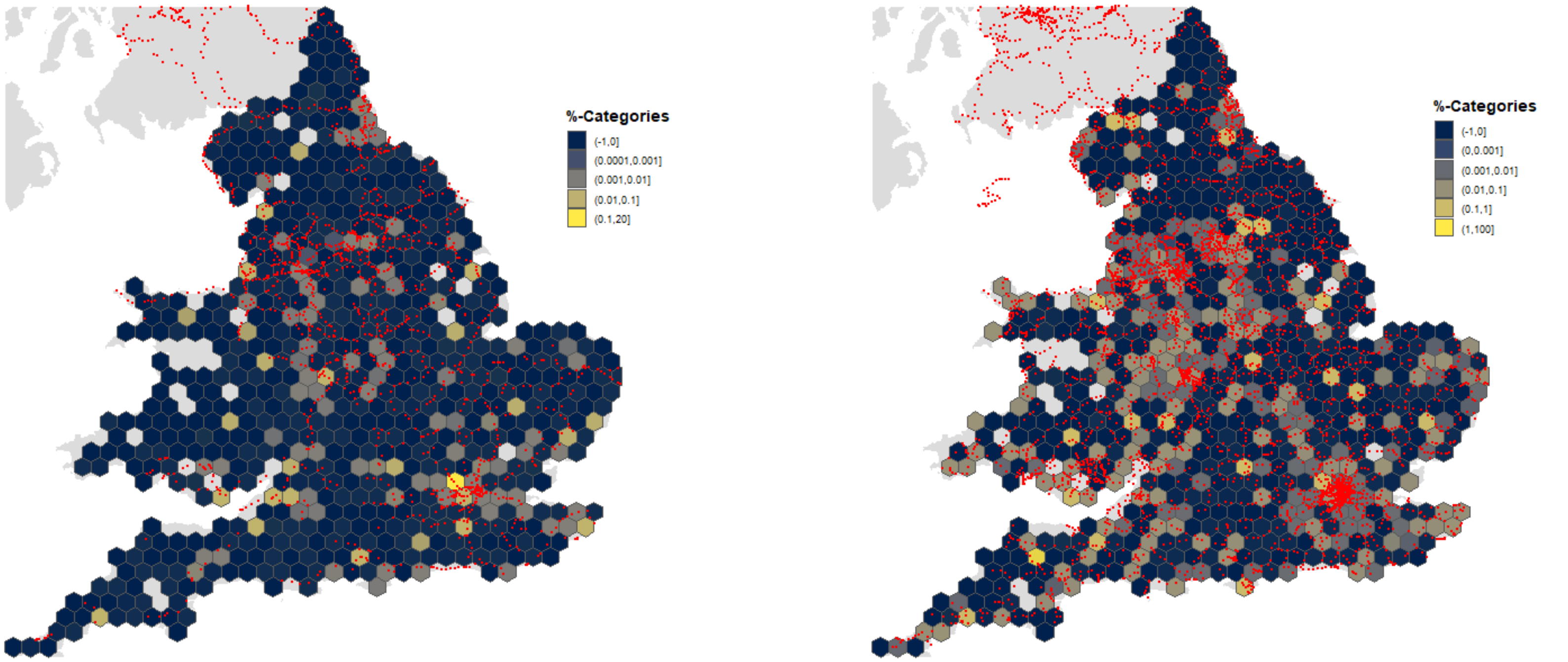
$$Bankruptcies_{i,s,t} = \beta_s Rail_{i,t} \times Sector_s + \Gamma X_{i,s,t} + \nu_{s,t} + \eta_{i,t} + \epsilon_{i,s,t}. \quad (1)$$

Our dependent variable, denoted by $Bankruptcies_{i,s,t}$, is the natural logarithm of the number of bankruptcies in some grid cell i, sector s, and year t. $Rail_{i,t}$ is the natural logarithm of the number of railway stations in a grid cell, our preferred measure of a location's exposure to the rail network.[6] With $\nu_{s,t}$ and $\eta_{i,t}$, we include sector-year and location-year fixed effects, respectively. This reduces the identifying variation to the location-sector(-year) level, i.e., possible confounding factors must be shocks that are specific to local occupation groups but can be temporally constant or time-variant. To minimize the remaining bias in our OLS estimations, the matrix $X_{i,s,t}$ adds a number of relevant control variables. Most importantly, we control for the distance to coal, the distance to London, and the distance to the nearest port, each interacted with time- and sector fixed effects. Distance to coal is an important control variable as it is one important proxy for a location's propensity to industrialize (Fernihough and O'Rourke, 2020). Holding the distance to London constant is necessary to account for differences in the availability of investment capital, as London was the main financial center of the time. Finally, by controlling for the distance to the closest port, we account for locational differences in the exposure to international trade and migration. By interacting each of these three variables with sector- and time fixed effects, we allow these confounders to have different impacts on bankruptcies or employment across sectors and across time. The remaining unexplained variation in the number of bankruptcies or employment is denoted by the error term $\epsilon_{i,s,t}$.

Our coefficient of interest is β_s, which captures the sector-specific reaction of the number of bankruptcies or employment to additional railway stations in a location. Due to the high-dimensional fixed effects, our estimations can be interpreted similarly to Difference-in-Differences estimations. Hence, β_s gives an unbiased estimate of the causal effect of railway stations on $Bankruptcies_{i,s,t}$ if, conditional on our control variables and for each sector, the potential outcomes follow parallel trends across time irrespective of a location's (increased) access to the railway network. Below, we test this assumption for plausibility and provide 2SLS regressions as an alternative identification approach.

[6] Our results are robust to using other measures

Figure 5: Bankruptcy Rates and Railway Expansion



a) Bankruptcies 1851-1860, Rail Stations 1851. b) Bankruptcies 1881-1890, Rail Stations 1881.

Notes: The figures show the share of bankruptcies in total employment by location. Brighter colors mean higher bankruptcy categories. The red points indicate railway stations that were established until the final year of the respective data sample. Light-gray cells are low-populated places and were omitted from the dataset because they do not contain a census subdistrict, returning no census information.

Note that our specifications derive β_s from the interaction of two baseline variables that are both collinear to the fixed effects. This is, a location's changed access to the railway over time is controlled for by the location-time fixed effects $\eta_{i,t}$, while sector-specific shocks in each census period are controlled for by the sector-year fixed effects $\nu_{s,t}$. Hence, one must interpret β_s as the *differential effect* the railway expansion has on a specific sector. In our main estimations below, we will set the service sector, which especially in the 19th century was mostly non-tradable and therefore likely unaffected by railway access, as the reference category. Hence, each β_s will resemble the differential effect of railways on sectoral bankruptcies or employment relative to the service sector.

As emphasized by Juhász et al. (2020), some sectors reorganize following the arrival of new technologies. In this study, we investigate how the arrival of the rail had accelerated the re-organization of the two sectors most likely affected by increased access to trade: industrial employment and trade (Melitz, 2003). Therefore, in a number of specifications, we will focus specifically on the industrial and trading sectors and compare them to the average of all other sectors.

4 Results

This section first presents the effect of rail connections on bankruptcies and employment across different sectors. Then it asserts the causal interpretation of our results using an instrumental variable approach based on the least cost path between important cities to build an "exogenous" railway network. The third part presents robustness checks of our results.

4.1 Illustration

Our empirical strategy relates the extension of the British railway network to the occurrence of individual bankruptcies. We expect the increased market integration from the railway connection to increase the number of bankruptcies in a location, as bigger competitors gain the main profits of the market integration and in turn displace smaller firms. Figure 5 illustrates our empirical analysis. The two maps show England and Wales covered by hexagonal grid cells, our unit of observation. Colors indicate the share of bankruptcies with respect to the location's total employment, where we assign the shares to categories for ease of display. The red dots indicate the locations of railway stations, which constitute our main independent variable.

Figure 5 a) to the left plots the extent of the railway by 1851 together with the aggregate number of bankruptcies from 1851-1860 relative to 1851 employment.[7] Figure 5 b) to the right again illustrates the geographical correlation between the railway expansion and the occurrence of bankruptcies, but for bankruptcies in the 1881-1890 period and the railway network in 1881.

Both maps illustrate the intuition behind our analysis very well. In 1851, the British railway system was still in its infancy. Many locations were not yet connected to the railway network, and the overall density of railway stations was low. Similarly, we only observe a small number of bankruptcies over the following ten years, with many grid cells not even experiencing one. Remarkably however, the gridcells experiencing bankruptcies do almost all already contain at least one railway station. In 1881, the railway network is much more advanced. Now, most grid cells contain at least one train station, and the overall density of stations increased significantly. And indeed, we see observe the same spatial expansion of bankruptcy cases. Not only does the overall number of bankruptcy cases increase; we also see many grid cells lighting up now that did not exhibit any bankruptcy cases in the period before. And yet still, the bankruptcy cases closely trace the spatial extent of the railway network.

4.2 Baseline results

Table 2 presents our main results. Column 2.1 presents the coefficient from regressing the log number of bankruptcies on the number of railway stations in a grid cell over time. We do not yet include any controls for fixed effects, and let the railway affect bankruptcies across all sectors to the same extent. Our results demonstrate that on average, an increase in the number of train stations in a gridcell is associated with a higher number of bankruptcies. According to this estimate, doubling the number of stations in a gridcell increases the number of bankruptcies in that gridcell by around 17 percent. In Column 2.2, we add an indicator variable for the industrialized manufacturing sector together with an interaction term of both explanatory variables to differentiate the effect of the rail on the industrial sector from the effect it had on other sectors. The interaction term suggests that the relationship between the railway expansion and the number of bankruptcies is most predominant in the industrial sector. The coefficient implies that doubling the number of stations in a cell would increase bankruptcies in the industrial sector by around 36 percent while other sectors would

[7] Note that the map contains a number of gray cells, especially in the rural regions of Wales or Cornwall. These are grid cells that we dropped from the dataset because our coding of census sub-districts did not yield any matches for these grid cells in these very rural parts of Great Britain.

experience an increase of only 13 percent. Across the following columns, we progressively add fixed effects for the sector-year and location-year level, and control for both the level of employment in each gridcell, sector, and time as well as for the triple interaction of other factors that determine economic activity such as distance to coal, distance to London, and distance to ports with an industry dummy and time fixed effects. These estimates are very conservative as they control for all time-varying covariates at the gridcell level. Note that, importantly, the fixed effects control for grid cells receiving access to the railway network. Hence, the coefficients our interaction term picks up is the differential impact of the railway on bankrutpcies occuring in the industrial sector. All of our estimates imply that doubling the number of stations in a gridcell would lead to an increase of bankruptcies in the industrial sector between 14 and 22 percent compared to the average of other sectors. This result is robust to clustering standard errors at the Sector × time level (Appendix A.4) and to using alternative measures of bankruptcies and of railway development (Appendix A.1, A.2).

The industrial sector hence seems to stand out and to experience more intra-sector market exits via bankruptcy following the expansion of the railway system. Table 3 shows our results in more detail by providing the coefficient estimates for each sector, leaving the service sector as the reference category. Columns 3.1 to 3.6 display coefficients from estimations that include Geo × Year and Sector × Year fixed effects. As services are the omitted sector, all coefficients have to be interpreted as the relative effect of the rail on bankruptcies in the specific sectors in comparison to its effect on people working in the service sector. Two sectors stand out: Industry and Trade. These are the only two sectors for which the rail variable systematically bears a positive coefficient. According to these coefficients, the rail generated 17 percent more bankruptcies in the trade sector than in services. It also generated 13 percent more bankruptcies in the industrial sector than in the service sector.

The other sectors, experienced less bankruptcies from the railway. The effect varies between 3 to 9 percent less bankruptcies compared to the service sector. Column 3.7 presents an estimation with no Geo × Time fixed effects to keep the variation generated by the rail and to interpret the coefficients attached to sectors as the level effect of the rail. We do observe that the coefficients are of similar magnitude. Doubling the number of train stations increases the number of bankruptcies in the industrial sector by 13 percent, in trade by 17 percent. These specifications control for the level of employment and for distance to coal, distance to London and distance to the nearest port interacted with sector and time fixed effects. Consequently, these effects cannot be explained by between-sector reallocation, a size effect, or the railway expansion targeting areas with specific industry structures due to the various control variables we consider.

Table 2: Bankruptcies in England – The conditional effect of market integration

	(2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)	(2.8)	(2.9)	(2.10)
Dep Variable					Log(Bankruptcies$_{i,s,t}$)					
Log(Rail$_{i,t}$)	0.166***	0.134***	0.139***							
	(0.0179)	(0.0155)	(0.0175)							
Industry$_s$		-0.0480***		-0.0482***						
		(0.0138)		(0.0139)						
Industry$_s$ × Log(Rail$_{i,t}$)		0.224***	0.216***	0.224***	0.216***	0.212***	0.201***	0.199***	0.212***	0.141***
		(0.0177)	(0.0197)	(0.0177)	(0.0198)	(0.0196)	(0.0213)	(0.0177)	(0.0195)	(0.0155)
Log(Employment$_{i,s,t}$)						0.0271***	0.0242***	0.0317***	0.0285***	0.0278***
						(0.00701)	(0.00659)	(0.00675)	(0.00708)	(0.00657)
Controls										
Coal	NO	NO	NO	NO	NO	NO	YES	NO	NO	YES
London	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Port	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Observations	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293
Adjusted R-squared	0.103	0.153	0.252	0.514	0.618	0.619	0.621	0.621	0.619	0.629
Sector × Year FE	NO	NO	YES	NO	YES	YES	YES	YES	YES	YES
Geo × Year	NO	NO	NO	YES	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the gridcell level. Sector × Year fixed effects are included in specification 2.3, 2.5, 2.6, 2.7, 2.8, 2.9, 2.10. Geo × Year fixed effects (Gridcells × Year fixed effects) are included in specification 2.4, 2.5, 2.6, 2.7, 2.8, 2.9, 2.10. Controls – Coal: Log(Distance to Coal$_i$ interacted with year fixed effects and an industrial dummy variable. London: Log(Distance to London$_i$ interacted with year fixed effects and an industrial dummy variable. Port: Log(Distance to Port$_i$, the (log-transformed) distance to the nearest port interacted with year fixed effects and an industrial dummy variable.

As we have seen that both trade and industry experienced more bankruptcies following the arrival of the railway, Table A.3 in the Appendix clarifies whether these patterns occur because between sector reallocation made some sectors less attractive – in that case bankruptcies would occur more in sectors losing employment – or if between sector reallocation generated bankruptcies because some firms in the booming sector could not benefit from it. We summarize the effects for a better overview in Figure 6. The estimates have the same structure as Table 3, and are consistent with the findings in Bogart et al. (2022). For the agricultural sector, we find a negative effect of the railway on employment, which implies that doubling the number of stations in a gridcell would reduce the number of workers in agriculture by 33 percent. Our estimates also suggest that the rail would decrease employment in the service sector and of professionals by around 5 percent. Similarly, the coefficients imply that the railway increased the number of trade, industry, administration, and clerical workers by around 5 to 10 percent.

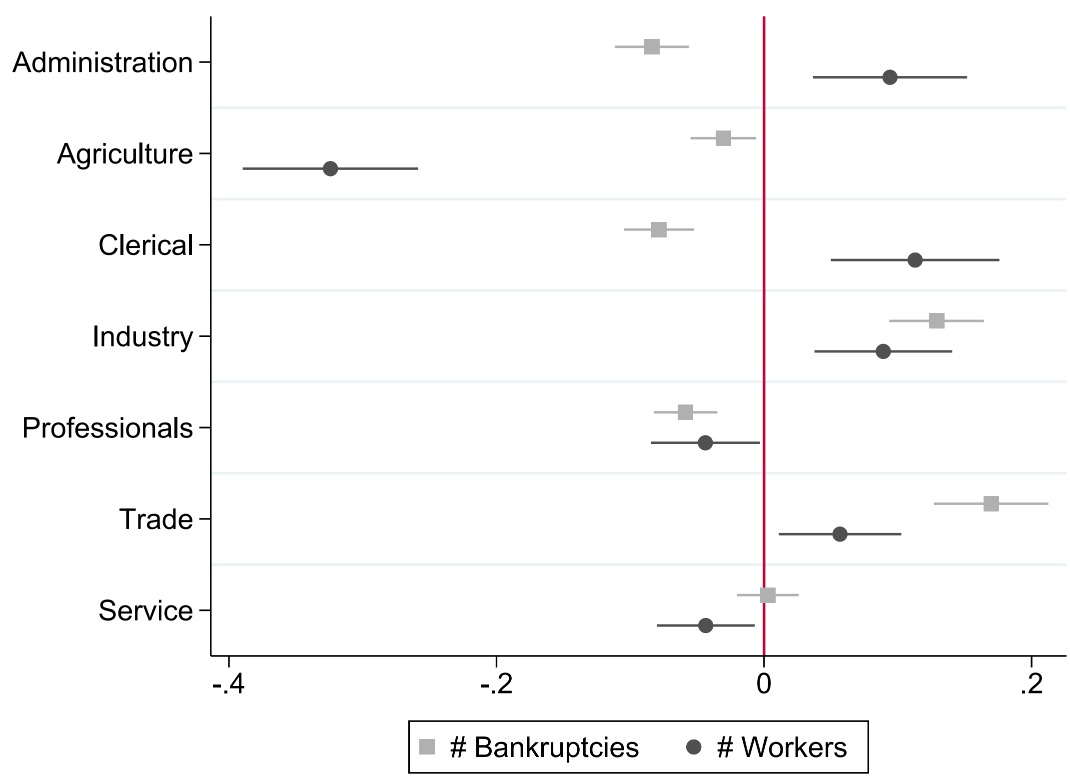
Table 3: Railway and Bankruptcies – Effects by sectors

	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)	(3.6)	(3.7)
Dep Variable				Log(Bankruptcies$_{i,s,t}$)			
Administrative$_s$ × Log(Rail$_{i,t}$)	-0.133***	-0.138***	-0.123***	-0.137***	-0.139***	-0.0874***	-0.0860***
	(0.0160)	(0.0163)	(0.0173)	(0.0158)	(0.0163)	(0.0134)	(0.0142)
Agriculture$_s$ × Log(Rail$_{i,t}$)	-0.0747***	-0.0640***	-0.0526***	-0.0607***	-0.0653***	-0.0320***	-0.0305**
	(0.0123)	(0.0119)	(0.0129)	(0.0104)	(0.0118)	(0.00839)	(0.0125)
Clerical$_s$ × Log(Rail$_{i,t}$)	-0.124***	-0.132***	-0.117***	-0.130***	-0.132***	-0.0819***	-0.0792***
	(0.0146)	(0.0152)	(0.0157)	(0.0149)	(0.0152)	(0.0124)	(0.0133)
Industry$_s$ × Log(Rail$_{i,t}$)	0.187***	0.182***	0.177***	0.168***	0.183***	0.126***	0.133***
	(0.0154)	(0.0153)	(0.0164)	(0.0138)	(0.0152)	(0.0129)	(0.0179)
Professionals$_s$ × Log(Rail$_{i,t}$)	-0.101***	-0.103***	-0.0895***	-0.101***	-0.103***	-0.0616***	-0.0596***
	(0.0124)	(0.0125)	(0.0133)	(0.0120)	(0.0124)	(0.00988)	(0.0121)
Trade$_s$ × Log(Rail$_{i,t}$)	0.258***	0.254***	0.238***	0.240***	0.255***	0.166***	0.170***
	(0.0214)	(0.0211)	(0.0222)	(0.0202)	(0.0210)	(0.0180)	(0.0217)
Service$_s$ × Log(Rail$_{i,t}$)							0.00268
							(0.0117)
h Log(Employment$_{i,s,t}$)		0.0244***	0.0209***	0.0307***	0.0228***	0.0183***	0.0138**
		(0.00731)	(0.00688)	(0.00702)	(0.00747)	(0.00703)	(0.00558)
Control							
Coal	NO	NO	YES	NO	NO	YES	YES
London	NO	NO	NO	YES	NO	YES	YES
Port	NO	NO	NO	NO	YES	YES	YES
Observations	14,293	14,293	14,293	14,293	14,293	14,293	14,293
Adjusted R-squared	0.676	0.677	0.679	0.682	0.680	0.704	0.633
Sector × Year FE	YES	YES	YES	YES	YES	YES	NO
Geo × Year FE	YES	YES	YES	YES	YES	YES	NO
Sector FE	NO	NO	NO	NO	NO	NO	YES
Time FE	NO	NO	NO	NO	NO	NO	YES
Geo FE	NO	NO	NO	NO	NO	NO	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo	Geo

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

The railway increased the number of bankruptcies for only two sectors: Trade and Industry. These two sectors are among those two experiencing growth in employment. In the meantime, administrative and clerical staff also increased in numbers but did not suffer from more bankruptcies (if anything the opposite). We interpret this as evidence that as some sectors grow, the increase in demand for labor in those sectors decreases bankruptcies. In other sectors, the growth is accompanied by changes in the market structure – less productive firms/workers being laid off for example. In this case, the effect of a changing market structure trumps the increasing demand for workers in those sectors and more workers then experience financial difficulties. Note further that the increase in administrative and clerical workers suggests that the railway did speed up a reorientation in the production process. To engage in large-scale production organized in factories, firm owners must employ lower-tier managerial stuff, e.g. to oversee workers, handle the finances and paperwork, or to assist in making strategical decisions. These are all workers that according to the HISCO coding would show up in either the clerical or administrative sector. Finally, three sectors experienced a decrease in employment and also a decrease in bankruptcies. These cases suggest that this decrease has been a voluntary change from one sector to the other by the workers. The ensuing decreased competition in turn reduced the bankruptcy rate in these declining sectors.

Figure 6: Coefficient plot – Railways' effect on bankruptcies and sector size



Notes: The figure reports the coefficients for the railway expansion by sector. Occupations in the category "Other", which include pensioners, rentiers, and unemployed, are the reference groups. All regressions control for the control variables and fixed effects as outlined in equation 1. Confidence intervals depict the 95% confidence level. Standard errors are clustered at the grid cell level.

These results are robust to using an approach without fixed effects but controlling for socio-demographic correlates at the gridcell level (Appendix A.5) and to deleting the least and the most populated gridcells (Appendix A.6). As described in Bogart et al. (2022), the railway indeed triggered a structural change. This structural change created pressure for the least productive firms – which then exited the market as emphasized in Melitz (2003).

5 Identification and Robustness

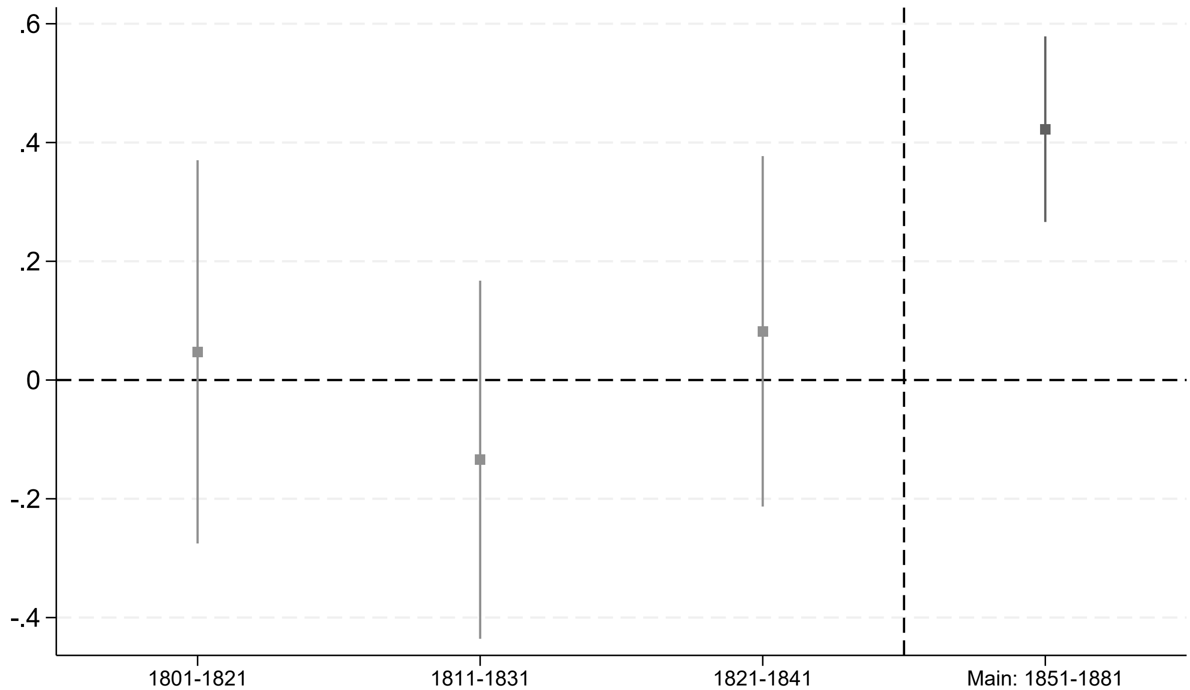
We provided evidence above that the extension of the British rail network led to an increase in industrial and trade employment in better connected locations, which went hand-in-hand with an increase in bankruptcies in these, but not in other sectors. This section investigates the causality of these findings. We first discuss the plausibility of the parallel trends assumption that underlies the panel regression estimates we presented before. Afterwards, we test our panel regressions for robustness to alternative variable and sample definitions, before we provide 2SLS estimates from a Least Cost Path (LCP) instrument as an alternative identification strategy.

5.1 Time Dimension – Pre-"treatment" placebos

Our panel estimations follow the logic of triple Difference-in-Differences estimations, as we exploit variation across place, time, and sector.We are interested in the coefficients of the interaction between railway expansion and an indicator variable for either the industrial or trade sectors. Our identifying assumption is hence that the potential outcomes of employment and bankruptcies in the industry and trade sectors in better connected locations followed parallel trends as those in less well connected locations, conditional on our control variables.

We can test the plausibility of parallel trends assumption for bankruptcies as we collected all bankruptcy statements since 1788. To test for pre-trends, we estimate cross section estimates for different synthetic census years following the set-up of our main dataset. This is, we select the years 1791, 1801, 1811, 1821, 1831, and 1841 as hypothetical census years and, as above, calculate the average yearly bankruptcy rate between each hypothetical census year and the next. We then run simple cross-section OLS regressions, regressing the annualized bankruptcies on the 1851 railway stations as well as the railway's interaction with the sector indicator. Absent sectoral pre-trends, we should not observe significant effects of 1851 railway stations on sectoral bankruptcies in pre-railway decades.

Figure 7: Testing Parallel Pre-Trends – Coefficients Rail$_i$ × $Industry_s$ on pre-sample



Notes: The Figure shows the estimators of placebo tests estimating the placebo effect of the 1851-1881 railway network before it was constructed. We have created a fake "Rail" equal to the values of our rail dummy to 3 decades before our estimators (and the development of the rail). These three decades are 1801-1811-1821, 1811-1821-1831, 1821-1831-1841.

Figure 7 presents the estimates by year in a coefficient plot. The results encourage the parallel trends assumption for the industrial sector (Panel a)). We find consistently insignificant results for the periods before 1851, i.e. before the railway expansion was actually realized. This suggests that bankruptcies of people working in the industrial sector did not follow different trends in locations that would later be connected to the early railway network and those who would not. The results are different for the trading sector, however. Here, we find consistently positive and significant results or the 1851 railway network on bankruptcies in the trading sector. Note however that this test presents a very conservative test for parallel trends; our actual estimations employ high-dimensional fixed effects that make use of the railway expansion *over time*, i.e. between 1851 and 1881. Hence, we cannot reject that parallel trends hold for the trading sector in our main estimations when we look at the railway expansion over time. Yet, while we are confident that the parallel trends assumption is fulfilled for bankruptcies of people employed in the industrial sector, we must interpret the effects on the trading sector with a grain of salt.

5.2 Space dimension – IV estimates

In addition to our panel fixed effects regressions, we estimate reduced form regressions with an instrument for the potentially endogenous railway expansion. Even though the location-year fixed effects in our main specifications directly control for locations' different exposure to the railway construction over time, we follow Bogart et al. (2022) to use the Least Cost Path (LCP) between early railway nodes to leverage the quasi-random variation in the expansion of the railway.

Following Bogart et al. (2022), we select the 100 biggest towns in 1850 as natural railway nodes, i.e. as towns that with almost certainty would have been among the first towns to receive a railway station. We then construct a LCPs between each of these nodes. These LCPs measure the easiest way to build railway lines between to locations, taking into account the bilateral distance together with a variation in building costs due to elevation (which requires building tunnels) and rivers (which require building railway bridges). We code use these LCPs to code grid cells' propensity to being connected to the railway. The main assumption here is that if a location lies along the LCP between two nodes, the railway lines must go through this location, which automatically increases the location's likelihood to receive a railway station.

Table 4: Instrumental variable – Least Cost Path

	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)	(4.6)	(4.7)	(4.8)
Dep variable	Log(Bankruptcies$_{i,s,t}$)			Log(Employment$_{i,s,t}$)	Log(Bankruptcies$_{i,s,t}$)			Log(Employment$_{i,s,t}$)
Industry$_s$ × LCP$_{i,t}$	0.0153***	0.0117***	0.0144***	0.00288				
	(0.00265)	(0.00163)	(0.00266)	(0.00348)				
Industry$_s$ × AccessIV$_{i,t}$					0.0856***	0.0331**	0.0601**	0.0569*
					(0.0176)	(0.00995)	(0.0214)	(0.0248)
Observations	16,340	16,340	16,340	16,992	16,340	16,340	16,340	16,992
Adjusted R-squared	0.540	0.551	0.612	0.800	0.524	0.544	0.600	0.800
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Geo FE	YES	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES	YES
Cluster	Geo,Sector	Geo,Sector	Geo,Sector	Geo,Sector	Geo,Sector	Geo,Sector	Geo,Sector	Geo,Sector

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

We construct two types of instruments. First, a simple LCP instrument which indicates with 0 or 1 whether an LCP crosses the grid cell, and second and Access IV which uses the total length of the LCP crossing through a grid cell. Since our LCPs measure the situation before the initial railway expansion in 1851, it only resembles a cross-sectional variable. We therefore interact it with the yearly number of newly built railway stations to add temporal variation. This makes the interpretation of our instruments similar to Shift-Share designs. In Table 4, we provide the results from reduced form regressions, interacting each of the two instruments with the indicator variable for the industrial sector. We overall find similar but smaller effects for the number of bankruptcies, but insignificant effects for employment growth. Hence, at least the effect of bankruptcies is robust to the IV strategy. The lower significance for both outcome variables probably accrues due to the measurement error in the IV compared to the actually measured railway expansion.

6 Structural change and bankruptcies – Creditors' demand for capital or debtors' insolvency?

The pressure put by the rail on the least productive firms can be of two natures: it could come from their lack of competitive edge in a market in which it is costly to adapt or they can come from creditors pushing for the bankruptcy of their debtors in order to reinvest some of the proceedings of the bankruptcies. Disentangling the two mechanisms is nearly impossible. These sections however documents these two dynamics by using discontinuities in the procedures leading to bankruptcies in our period of study.

In 1869, England repealed the "officialism" doctrine for bankruptcy Lester (1991). Before this reform, bankruptcies were managed by local officials, often took a long time to be resolved and their outcome was uncertain. We hypothesize that during this period the "reinvestment" motive of creditors to file bankruptcy was then limited. After the 1869 reform, bankruptcies were managed by creditors if a majority of them agreed. This procedure advantaged creditors and increased their incentives to file for bankruptcies for quick reinvestment. In 1883, England went back to the "officialism" doctrine.

Figure 8 illustrates the first fact about the repeal of officialism. It presents the annual number of bankruptcies around the reform. Creditors indeed appreciated the reform as we see two discontinuities at the time of the two reforms repealing and re-introducing officialism. The financial constraints of debtors could not have changed so dramatically overnight. Hence these reforms created variation in creditors incentives to file bankruptcies. Bankruptcies before the

1869 reform and after the 1883 reform can be thought as an imprint of the financial situation of debtors. In between the two reforms, bankruptcies capture both the financial situation of debtors and creditors' interest. After the 1883 reform, the number of bankruptcies returns to the pre-1869 reform level lending more credence to our interpretation that the surge in bankruptcies in the 1869-1883 period was mainly due to the repeal of officialism and variation in creditors' incentives to file for bankruptcies.

Figure 8: Number of bankruptcies around the 1869 and 1883 reforms

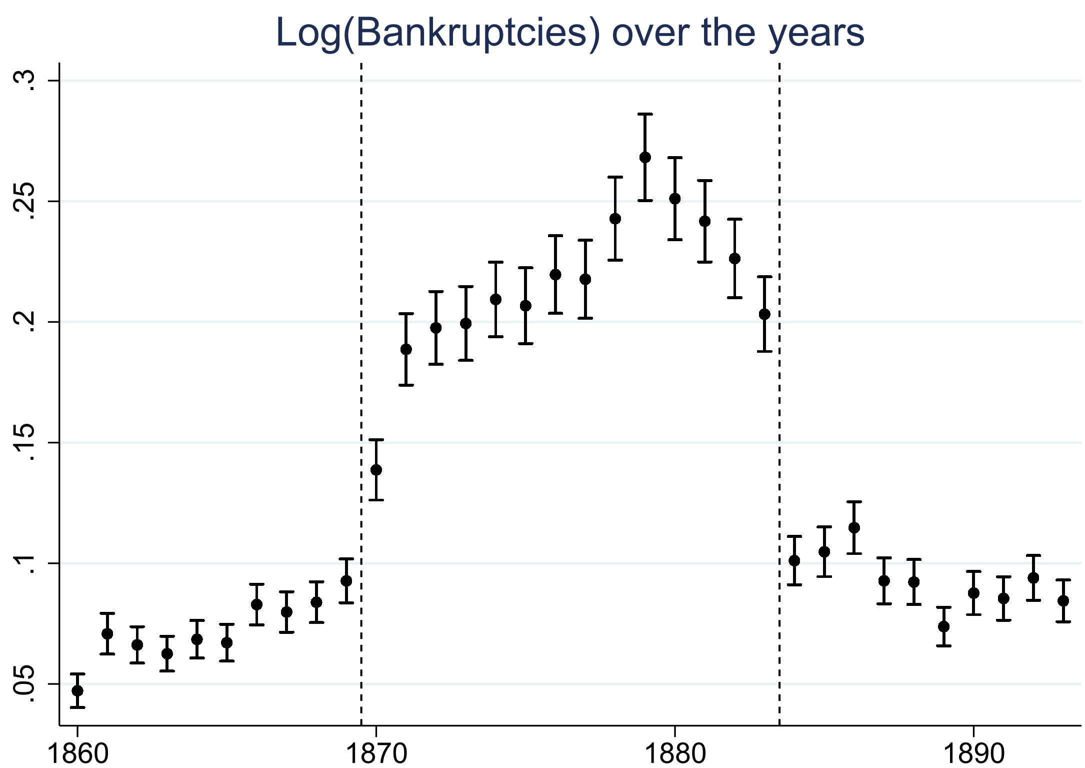


Table 5 presents the results a test comparing the effect of the rail before and after the reform. It uses the same type of specification than previous tables on a database recording bankruptcies at the annual frequency. It computes the coefficients attached to the interaction between railways and the industry dummy variable and also its triple interaction adding a reform dummy variable equal to one between 1869 and 1883. The intuition of this test is simple. Should the specific effect of the railway arise only because of market structure, then the triple interaction should not be significant. Similarly should the interaction be significant but not the simple interaction between the industry dummy and the railway variable, then it would imply that market structure plays a minor role to explain our effect and that the effect of the rail appears *only* when bankruptcies reflect creditors' incentives. To have a more

Table 5: Railway, Creditors and Debtors – The 1869 and 1883 reforms

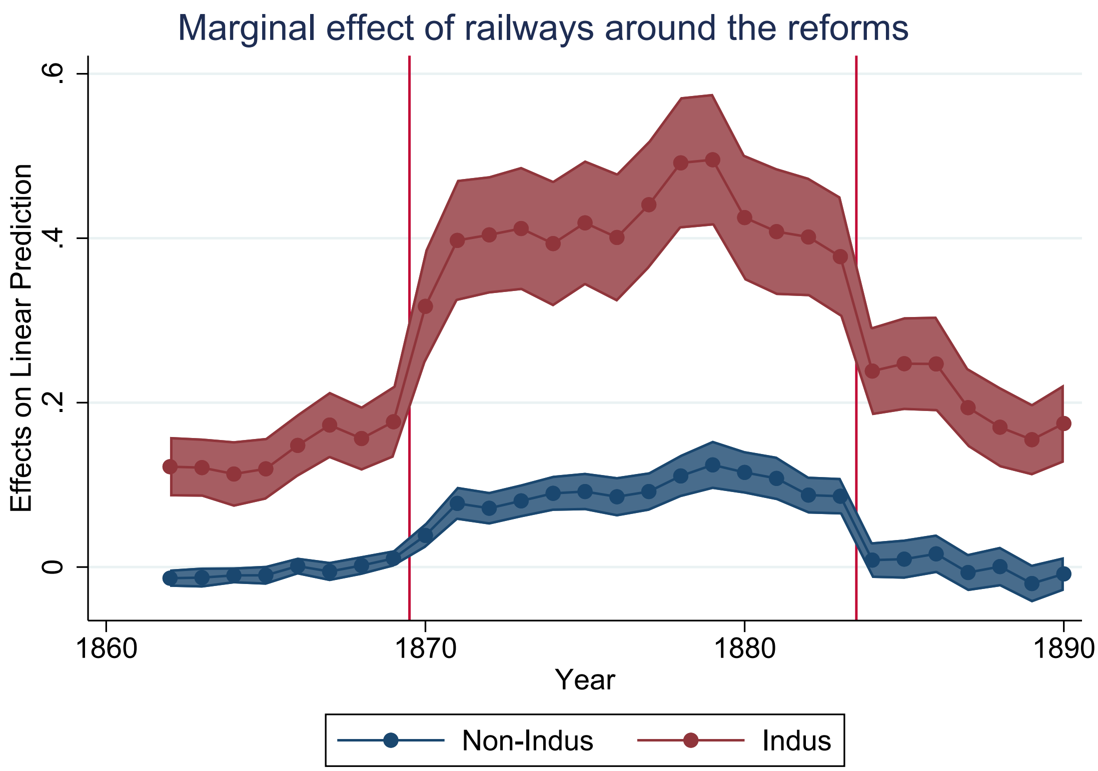
	(5.1)	(5.2)	(5.3)	(5.4)	(5.5)	(5.6)
Dep variable			Log(Bankruptcies$_{i,s,t}$)			
Indus$_s$ × Log(Rail$_{1861,i}$)	0.132***	0.0853***	0.179***	0.116***	0.214***	0.127***
	(0.0162)	(0.0117)	(0.0168)	(0.0143)	(0.0222)	(0.0192)
Reform$_t$ × Indus$_s$ × Log(Rail$_{1861,i}$)	0.191***	0.134***	0.144***	0.104***	0.104***	0.0854***
	(0.0162)	(0.0179)	(0.0157)	(0.0167)	(0.0173)	(0.0200)
Observations	226,560	226,560	373,824	373,824	56,640	56,640
Adjusted R-squared	0.523	0.530	0.504	0.506	0.547	0.549
Sample	1851-1890	1851-1890	1865-1874	1865-1874	1878-1888	1878-1888
Controls						
Coal	NO	YES	NO	YES	NO	YES
London	NO	YES	NO	YES	NO	YES
Port	NO	YES	NO	YES	NO	YES
Sector × Year FE	YES	YES	YES	YES	YES	YES
Geo × Year	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo

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

causal assessment of these effect we estimate the effect both on the full 1851-1890 dataset (Columns 5.1 and 5.2) and around the two discontinuities created by the reforms (Columns 5.3 to 5.6). In all these estimates, both the simple interaction and the triple interactions are positive and significant. The magnitude of the coefficients attached to the triple interaction is equal to 60 to 140 % the one of the simple interaction. Accordingly, creditors' incentives to file for bankruptcies is a driver of our effect. Given that the coefficient attached to the simple interaction between the industry dummy and the railway variable is also significant and in most specifications greater than its triple interaction with the reform dummy variable however suggest that market structure is of most importance to explain our effect. The industrial sector in places connected to the train experienced more bankruptcies because its workers experienced financial hardship. Even before the repeal of officialism, when bankruptcies could be instigated by the debtor and was dealt with by officials, the industrial sector experienced more bankruptcies.

Figure 9 presents estimators of the effect of railways on the industrial sector and on other sectors over time controlling solely time fixed effects, geographic fixed effects and sector fixed effects. The graphic impression is similar to the estimates in Table 5. Before the reform, a difference in this correlation exists: the expansion of railways increase the number of bankruptcies in the industrial sector whereas it add almost no effect on other sectors. After the reform, the correlation increases for both the industrial sector and other sectors, this indeed suggests that when the bankruptcy system puts the incentives of creditors at the center of the system, then bankruptcies increased in places connected to the railway.

Figure 9: Coefficients for rail in the industrial sector versus and in other sectors over time (1851-1890)



The variation created by the reforms reveal that creditors' incentives were an important factor explaining how a connection to the rail increased bankruptcies in the industrial sector more than in any other sector. However a difference between the industrial sector and other sectors already existed under the doctrine of "officialism" during which bankruptcies were much less the expression of creditors' interests. This suggest that both creditors incentives to reinvest the deeds of bankruptcy and also the pressure from structural change explains the pattern uncovered in this paper.

7 Conclusion

This paper replaces the geographic dimension to explain who loses the from structural change. Previous studies suggest that the least productive firms suffer from disruptive technologies (Juhász et al., 2020) and from market integration (Melitz, 2003). This article combines the two approaches to show that both factors interact with each-other. The intuition is simple: the competitive disadvantage of technology non-adopters is not relevant if markets are not integrated. Similarly, market integration does not trigger firms' exit if there is no heterogeneous costs associated to this integration (Melitz, 2003). In other words, our results directly reconcile the economic geography literature on intra-market reallocation and the literature on the effect of technology adoption without a geographic dimension.

The extension of the railway in England and Wales during the 19^{th} provides us with a perfect setting to test the complementarities between these theories. Some sectors experience dramatic technological changes whereas others did not. We hypothesize that these difference in the technology available for each sector can theoretically be associated to the heterogeneous costs faced by firms once trade cost decrease. In the meantime, the expansion of the railway provides us with variation in trade costs over space and over time. Our estimates assess how many individuals in each occupational class exited due to the shock generated by the railway expansion.

The railway expansion generated more bankruptcies both in the trade sector and the industrial sector. This pattern is not explained by between sector reallocation, since they both experienced an increase in the number of workers. Interestingly, we also observe that the arrival of the rail also increased the number of clerical workers and the number of administrative workers. This suggest that the rail indeed transform the nature of production and consequently pushed the lest adaptable workers out of the market. Geographic concentration and intra-sectoral reallocation then goes hand in hand. They fuel each-other.

These results clarify some of the dynamics driving the evolution of market structure, trade and inequality during the industrial revolution and its immediate aftermath (Nye, 1987; O'rourke and Williamson, 2005; Desmet and Parente, 2012; Desmet et al., 2020; Juhász et al., 2020). They also shed a new light on the factors potentially explaining how spatial and sectoral inequality may interact today (Autor et al., 2020). This research emphasizes that despite positive agregate effects technology and trade generate losers. This redistribution has important (political) consequences (Frey et al., 2018; Caprettini and Voth, 2020; Autor et al., 2020). Future research could build on these new results to better understand how to mitigate the redistributional consequences of growth.

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A Appendix A – Additional Tables

Table A.1: Railway and Bankruptcies – Losers of growth? (Robustness with Access to rail as independent variable)

	(A.1.1)	(A.1.2)	(A.1.3)	(A.1.4)	(A.1.5)	(A.1.6)	(A.1.7)	(A.1.8)	(A.1.9)	(A.1.10)
Dep Variable	$\text{Log}(\text{Bankruptcies}_{i,s,t})$									
$\mathbb{1}(\text{Rail}_{i,t}>0)$	0.166***	0.132***	0.112***							
	(0.0146)	(0.0123)	(0.0117)							
$\text{Industry}_s \times \mathbb{1}(\text{Rail}_{i,t}>0)$		0.236***	0.181***	0.236***	0.180***	0.176***	0.155***	0.145***	0.178***	0.0744***
		(0.0171)	(0.0162)	(0.0171)	(0.0162)	(0.0160)	(0.0161)	(0.0163)	(0.0161)	(0.0163)
$\text{Log}(\text{Employment}_{i,s,t})$						0.0376***	0.0268***	0.0432***	0.0390***	0.0307***
						(0.00781)	(0.00682)	(0.00769)	(0.00786)	(0.00674)
Controls										
Coal	NO	NO	NO	NO	NO	NO	YES	NO	NO	YES
London	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Port	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Observations	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293
Adjusted R-squared	0.027	0.060	0.164	0.495	0.601	0.603	0.608	0.607	0.603	0.623
Sector × Year FE	NO	NO	YES	NO	YES	YES	YES	YES	YES	YES
Geo × Year	NO	NO	NO	YES	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo

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

Table A.2: Railway and Bankruptcies – Losers of growth? (Robustness with Dummy variable for Bankruptcies)

	(A.2.1)	(A.2.2)	(A.2.3)	(A.2.4)	(A.2.5)	(A.2.6)	(A.2.7)	(A.2.8)	(A.2.9)	(A.2.10)
Dep Variable					$\mathbb{1}(\text{Bankruptcies}_{i,s,t}>0)$					
$\text{Log}(\text{Rail}_{i,t})$	0.162***	0.154***	0.146***							
	(0.00774)	(0.00791)	(0.00884)							
$\text{Industry}_s \times \text{Log}(\text{Rail}_{i,t})$		0.0563***	0.0408***	0.0558***	0.0401***	0.0375***	0.0441***	0.0298***	0.0374***	0.0294**
		(0.00922)	(0.0102)	(0.00921)	(0.0102)	(0.0103)	(0.0115)	(0.0105)	(0.0103)	(0.0123)
$\text{Log}(\text{Employment}_{i,s,t}$						0.0173***	0.0191***	0.0200***	0.0171***	0.0198***
						(0.00636)	(0.00650)	(0.00637)	(0.00640)	(0.00650)
Constant	0.149***	0.123***	0.160***	0.298***	0.327***	0.223***	0.206***	0.239***	0.223***	0.242***
	(0.0100)	(0.00944)	(0.0116)	(0.00123)	(0.00166)	(0.0379)	(0.0398)	(0.0384)	(0.0379)	(0.0426)
Controls										
Coal	NO	NO	NO	NO	NO	NO	YES	NO	NO	YES
London	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Port	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Observations	16,340	16,340	16,340	16,340	16,340	16,340	16,340	16,340	16,340	16,340
Adjusted R-squared	0.088	0.122	0.330	0.370	0.536	0.537	0.537	0.537	0.537	0.538
Sector × Year FE	NO	NO	YES	NO	YES	YES	YES	YES	YES	YES
Geo × Year	NO	NO	NO	YES	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo	Geo

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A.3: Railway and Employment: Labor Reallocation?

	(A.3.1)	(A.3.2)	(A.3.3)	(A.3.4)	(A.3.5)	(A.3.6)	(A.3.7)
Dep Variable	Log($\text{Employment}_{i,s,t}$)						
$\text{Administrative}_s \times \text{Log}(\text{Rail}_{i,t})$	0.152***	0.152***	0.117***	0.180***	0.156***	0.138***	0.0933***
	(0.0250)	(0.0250)	(0.0274)	(0.0246)	(0.0245)	(0.0298)	(0.0293)
$\text{Agriculture}_s \times \text{Log}(\text{Rail}_{i,t})$	-0.411***	-0.411***	-0.351***	-0.421***	-0.416***	-0.281***	-0.325***
	(0.0290)	(0.0290)	(0.0335)	(0.0284)	(0.0285)	(0.0288)	(0.0333)
$\text{Clerical}_s \times \text{Log}(\text{Rail}_{i,t})$	0.261***	0.261***	0.226***	0.249***	0.262***	0.156***	0.112***
	(0.0265)	(0.0265)	(0.0294)	(0.0264)	(0.0264)	(0.0313)	(0.0321)
$\text{Industry}_s \times \text{Log}(\text{Rail}_{i,t})$	0.217***	0.217***	0.110***	0.244***	0.214***	0.133***	0.0911***
	(0.0233)	(0.0233)	(0.0192)	(0.0225)	(0.0222)	(0.0213)	(0.0261)
$\text{Professionals}_s \times \text{Log}(\text{Rail}_{i,t})$	0.0185	0.0185	0.0138	0.0126	0.0181	-0.000290	-0.0437**
	(0.0154)	(0.0154)	(0.0169)	(0.0151)	(0.0154)	(0.0188)	(0.0207)
$\text{Trade}_s \times \text{Log}(\text{Rail}_{i,t})$	0.148***	0.148***	0.134***	0.149***	0.151***	0.100***	0.0568**
	(0.0187)	(0.0187)	(0.0199)	(0.0188)	(0.0186)	(0.0209)	(0.0233)
$\text{Service}_s \times \text{Log}(\text{Rail}_{i,t})$							-0.0429**
							(0.0186)
$\text{Log}(\text{Population}_{i,t})$							0.630***
							(0.00713)
Control							
Coal	NO	NO	YES	NO	NO	YES	YES
London	NO	NO	NO	YES	NO	YES	YES
Port	NO	NO	NO	NO	YES	YES	YES
Observations	14,868	14,868	14,868	14,868	14,868	14,868	14,868
Adjusted R-squared	0.946	0.950	0.952	0.951	0.951	0.954	0.947
Sector × Year FE	YES	YES	YES	YES	YES	YES	NO
Geo × Year FE	YES	YES	YES	YES	YES	YES	NO
Sector FE	NO	NO	NO	NO	NO	NO	YES
Time FE	NO	NO	NO	NO	NO	NO	YES
Geo FE	NO	NO	NO	NO	NO	NO	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo	Geo

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

Table A.4: Railway and Bankruptcies – Losers of growth?

	(A.4.1)	(A.4.2)	(A.4.3)	(A.4.4)	(A.4.5)	(A.4.6)	(A.4.7)	(A.4.8)	
Dep Variable					Log(Bankruptcies$_{i,s,t}$)				
Log(Rail$_{i,t}$)	0.166***	0.134***	0.139***						
	(0.0375)	(0.0356)	(0.0355)						
Industry$_s$ × Log(Rail$_{i,t}$)		0.224***	0.216**	0.224***	0.216***	0.212***	0.201***	0.199***	
		(0.0657)	(0.0949)	(0.0500)	(0.0646)	(0.0670)	(0.0595)	(0.0656)	
Log(Employment$_{i,s,t}$)						0.0271	0.0242	0.0317	
						(0.0369)	(0.0384)	(0.0368)	
Constant	-0.0346*	-0.0277	-0.0382	0.125***	0.119***	-0.0424	-0.0147	-0.0174	
	(0.0180)	(0.0175)	(0.0375)	(0.0368)	(0.0105)	(0.216)	(0.231)	(0.216)	
Controls									
Coal	NO	NO	NO	NO	NO	NO	YES	NO	
London	NO	NO	NO	NO	NO	NO	NO	YES	
Port	NO	NO	NO	NO	NO	NO	NO	NO	
Observations	14,293	14,293	14,293	14,293	14,293	14,293	14,293	14,293	
Adjusted R-squared	0.103	0.153	0.252	0.514	0.618	0.619	0.621	0.621	
Sector × Year FE	NO	NO	YES	NO	YES	YES	YES	YES	
Geo × Year	NO	NO	NO	YES	YES	YES	YES	YES	
Cluster	Sector × Year	Sector × Year	Sector × Year	Sector × Year	Sector × Year	Sector × Year	Sector × Year	Sector × Year	Se

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

A.1 Additional Tables and Graphs about Sectors

Table A.5: Robustness Checks – Socio-economic controls

	(A.5.1)	(A.5.2)	(A.5.3)	(A.5.4)	(A.5.5)	(A.5.6)
Dep variable	Log($\text{Bankruptcies}_{i,s,t}$)			Log($\text{Employment}_{i,s,t}$)		
$\text{Industry}_s \times \text{Log}(\text{Rail}_{i,t})$	0.146***	0.145***	0.145***	0.0786***	0.0784***	0.0782***
	(0.0157)	(0.0157)	(0.0157)	(0.0180)	(0.0179)	(0.0179)
$\%\text{Unemployed}_{i,t}$	-0.0631		-0.0697	0.125		0.113
	(0.0567)		(0.0567)	(0.0819)		(0.0828)
$\%\text{Migrant}_{i,t}$		0.224***	0.229***		0.622**	0.617**
		(0.0435)	(0.0450)		(0.253)	(0.259)
Log($\text{Employment}_{i,s,t}$)	0.0170***	0.0187***	0.0186***			
	(0.00531)	(0.00538)	(0.00537)			
Log($\text{Population}_{i,t}$)				0.983***	1.002***	1.003***
				(0.0254)	(0.0136)	(0.0138)
Constant	0.224***	0.193***	0.216***	-5.727***	-5.920***	-5.964***
	(0.0410)	(0.0385)	(0.0411)	(0.318)	(0.165)	(0.171)
Control						
Coal	YES	YES	YES	YES	YES	YES
London	YES	YES	YES	YES	YES	YES
Port	YES	YES	YES	YES	YES	YES
Observations	14,293	14,293	14,293	14,329	14,329	14,329
Adjusted R-squared	0.564	0.564	0.564	0.940	0.940	0.940
Year FE	YES	YES	YES	YES	YES	YES
Geo FE	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A.6: Robustness Checks – Deleting high population density and low population density areas

	(A.6.1)	(A.6.2)	(A.6.3)	(A.6.4)	(A.6.5)	(A.6.6)
Dep variable		Log(Bankruptcies$_{i,s,t}$)			Log(Employment$_{i,s,t}$)	
Industry$_s$ × Log(Rail$_{i,t}$)	0.105***	0.141***	0.105***	0.0962***	0.0776***	0.0593***
	(0.0130)	(0.0156)	(0.0130)	(0.0254)	(0.0185)	(0.0187)
Constant	0.164***	0.142***	0.165***	5.636***	5.951***	5.847***
	(0.0415)	(0.0439)	(0.0416)	(0.0372)	(0.0270)	(0.0276)
Controls						
Coal	YES	YES	YES	YES	YES	YES
London	YES	YES	YES	YES	YES	YES
Port	YES	YES	YES	YES	YES	YES
Dropping	Top 5%	Bottom 5%	Top 5% Bottom 5%	Top 5%	Bottom 5%	Top 5% Bottom 5%
Observations	13,831	14,265	13,803	14,406	14,287	13,825
Adjusted R-squared	0.593	0.629	0.593	0.941	0.939	0.940
Year FE	YES	YES	YES	YES	YES	YES
Geo FE	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES
Cluster	Geo	Geo	Geo	Geo	Geo	Geo

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

	loc	sector	number_firms	big_firms	share_big_firms	sd	ratio_90_10
1	Global	Other	166	23	0.14	98.56	31.00
2	Global	ClericalWorkers	204	14	0.07	73.03	14.70
3	Global	Professionals	404	35	0.09	85.24	16.00
4	Global	Sales	11863	496	0.04	31.49	10.00
5	Global	Service	1469	124	0.08	134.13	15.00
6	Global	AdminWorkers	6199	835	0.13	55.05	24.00
7	Global	Agriculture	117986	5407	0.05	25.84	12.00
8	Global	Industry	56167	3561	0.06	90.22	12.00

B Appendix B – Detecting Bankruptcies

For example, the earliest issues starting in 1788 and going until 1861 listed bankruptcy announcements towards the end of an issue. Each announcement received its own paragraph, starting with the introduction "Whereas a Commission of Bankrupt(cy) was awarded (and issued forth) against." Starting in 1861, the sections of bankruptcy announcements received their own headlines and internal structure. Since then, announcements became separated into first meetings, i.e. the assessment of bankruptcy and collection of claims, later meetings to distribute funds, and final meetings to resolve open cases. For example, first meetings would be introduced under the headline "The Bankruptcy Act, 1861. Notice of Adjudications and First Meeting of Creditors." The London Gazette maintained this structure for most of the time. One exception is a short intermezzo in 1919, when they published notices of first meetings, intermediate meetings, and final meetings in separate tables at the end of an issue. Finally, from 1920 until 1986 the London Gazette went back to the structured text format illustrated in Figure 2 (b). Only after 1986, lawyers and solicitors took over the management of bankruptcy cases and published announcements in their own, individual way. We therefore focus our systematic data collection on the 1788–1986 period when bankruptcy announcements followed systematic and easy-to-code patterns.

To extract individual announcements from an issue, we wrote various algorithms that, depending on the announcement pattern of a given time period, identified the start of a new announcement. For example, in the early issues from 1788–1861, the algorithm looked for different variations of the text pattern "Whereas a Commission of Bankrupt is awarded and issued forth against" to determine the start of a bankruptcy announcement.[8] From 1861 onwards, we searched the issues for the headlines introducing the "First Meetings" of bankruptcies to focus our algorithm on the text between this headline and the following one, and then collecting the individual announcements with the procedure explained above.[9]

Our algorithm detected a total of 422,769 bankruptcy cases, i.e. on average 19.9 bankruptcy announcements per issue, with a median of 14 announcements per issue. For each bankruptcy case we detected, we extracted the first 300 letters after the start of a bankruptcy paragraph for further processing. Within each text sample, we let our algorithm find the information on a) the name of the person, b) the person's current address, and c) the person's current occupation. To identify this information, we used detected commas in the text to separate the information. Usually, the information would be presented in the format *name, address, occupation*, such that detecting commas as break points helped structure the text. Using these comma-break points as general hints for where to look for certain information, we ran the specific text-subsets against lists of city-, county-, borough-, and parish-names as well as a list of (historical) census occupations respectively to detect matches.

Due to the occasionally bad quality of the scans, this required a lot of pre-processing.

[8] The actual pattern switch occurred with the new bankruptcy act in the issue 22,564 from November 12th, 1861. While the overall pattern remained stable across announcements, the individual solicitors who published the announcements would vary the text pattern somewhat, e.g. using past tense ("was awarded" or "has been issued forth") or dropping the "awarded" or "issued forth" part of the introduction. We went through several issues manually to include as good as all variations in our algorithm, and went back to issues with an unusually low number of detected announcements to look for pattern variations that we might have overlooked before.

[9] For the short period when the announcements were published in a table format, we accessed the Google Vision API to detect the table structure accurately and directly transfer the relevant information into a digital table format.

Among other things, we corrected common typos that the OCR introduced by mis-reading certain letters and used fuzzy text matching procedures where direct pattern matching did not yield a result. Finally, we used the information on locations and occupations to encode it in a useable format. We geocoded the place information as accurately as possible. For many locations, we were able to link them to the coordinates for a specific parish or city, some we could only geocode at the county level. To make use of the occupation titles, we assigned them to 6-digit historical international classification of occupations (HISCO) codes as defined by the international institute of social history Amsterdam.[10] Despite the pre- and post-processing steps, we were not able to acquire full information for all bankruptcy cases that our algorithm collected. Overall, we were able to geocode 373,555 bankruptcy cases (of this, 343,091 cases to the city- or parish level) and assign HISCO codes to 373,010 cases.

[10]See their homepage https://iisg.amsterdam/en/data/data-websites/history-of-work for further information