The London School of Economics and Political Science

Essays on the Economy of 19th Century England

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 29,970 words.

Statement of co-authored work

I confirm that Chapter 2 was jointly co-authored with Jane Olmstead-Rumsey.

Statement of inclusion of previous work

I can confirm that Chapter 1 is a significantly revised version of a paper I submitted at the conclusion of my MRes at the London School of Economics in 2021.

Abstract

The thesis consists of three chapters that examine the economy of England in the 19th century. Chapter 1 studies the impact of the construction of the railroad network on local patenting. Using novel data, I find that, by increasing market access, the railroad caused an increase in local patenting. Chapter 2 studies how the Panic of 1825 affected the local economy. The financial system was subject to strict rules requiring small, local banks to have a working relationship with a London agent bank, and we exploit the sudden financial collapse of these London agent banks to establish how the collapse of a local bank connected to a failed London agent caused an increase in local non-financial firm bankruptcies. Chapter 3 studies how the railroad affected economic growth. I find little evidence that the construction of the network increased local land values or had any impact on local bankruptcies. I provide evidence that this could be due to a creative destruction effect in which the destruction element was particularly strong.

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Contents

Chapter 1

The Effect of Transportation Infrastructure on Innovation: The Role of Market Access in the English Railway Boom

1.1 Introduction

I study how the construction of the British railway network increased innovation during the 19th century. The railway was one of the most transformative projects in history, revolutionizing domestic trade; its construction caused the average freight cost between any two districts in England to decline by a factor of 2.5 within two decades.[1] The impact of this transportation revolution is illustrated by the experience of James Nasmyth, an inventor and entrepreneur who is listed as an applicant for at least 16 patents related to his machine tools manufacturing firm in Salford, England. In his autobiography, Nasmyth cites the railroad's impact on his firm:

> *[T]he railway alongside enabled a communication to be kept up by rail with every part of the country ... After the machines or engines had been finished, it was the business of the same workmen to remove them from the workshops to the railway-siding alongside the foundry... (Nasmyth 1897)*

The railroad created new markets for Nasmyth; the great reduction in freight costs increased demand for his firm's goods. The effect of market demand on innovation is at the heart of a major strand of the economic growth literature. Innovation has long been believed to be demand-driven (Schmookler 1966) and from a firm's perspective, an increase in demand allows fixed cost R&D investments to be spread over a larger customer base (Sokoloff 1988). While prior work has established a positive relationship between market size and innovation (Acemoglu and Linn 2004; Aghion et al. 2024), the role of transportation networks in driving this dynamic remains unexplored, despite a nascent literature on how these networks affect innovation through other channels (Agrawal et al. 2017). This paper addresses that gap by examining the relationship between railway expansion and innovation in 19th-century England from a static trade perspective.

I present two main findings in this paper. First, I show that the establishment of a rail station causes a significant increase in local patenting activity. Second, I identify market access as a key mechanism to explain these returns to patenting.

Districts in England that receive a rail station experience a 77% increase in patent applications per capita between 1823-1861. By comparison, the effect on population is a 3% increase. This effect is persistent across several decades and

[1] I calculate this number in Section 1.5.

robust to a wide range of sample restrictions and estimation procedures. Further, there is no differential impact by patent quality; eliminating patents in the bottom 75% by quality between 1823-1850 using the Nuvolari et al. (2021) Bibliographic Composite Index, which assesses the historical impact of patents, does not substantially change the results.

I follow the transportation network literature to deal with selection bias with respect to the rail treatment (Redding and Turner 2015). To focus on plausibly exogenous variation, I restrict the data to "accidentally connected" locations - districts between the major economic centers whose connection was the primary focus of the transportation projects. The "accidentally connected" locations are plausibly exogenously treated in that they are connected to the rail network because they happen to lie on a convenient path between cities. To further ensure exogeneity, I create a lowest cost path algorithm between districts and match this to the actual transportation network. If an "accidentally connected" town lies on the lowest cost path between cities it is reasonable to assume that the town was only connected by chance because of its location between places that were deemed economically important to the rail firms.[2] For the majority of the analyses in this paper, I restrict the treatment group to the districts which are both "accidentally connected" and which lie on the least cost path network, though I explore more alternate treatment definitions as robustness tests.

A rail network increases the size of the market to which firms can sell their goods by significantly lowering freight costs between districts. This expansion of market access boosts demand and enables firms to undertake costly R&D by enlarging their potential customer base. Notably, a third of the rail companies' revenue in this period came from freight, underscoring the role of railroads in changing domestic trade (Railway Commission 1848). I provide empirical evidence that this increase in market access is a key mechanism through which rail stations stimulate local innovation.

I estimate the effect that changes in market demand, driven by the railway network, had on local innovation. To do so, I create a transportation map of 19th century England using railroads, inland navigable waterways, and overland routes to measure each district's annual change in market access, where a district's market access is the sum across all other districts of the product of local GDP and the transportation cost between district pairs (following Donaldson and Hornbeck

[2]See Chandra and Thompson (2000), Faber (2014), Hornung (2015), Banerjee et al. (2020), Bogart et al. (2022), and Andersson et al. (2023) for examples which incorporate some or all elements of this methodology.

2016). Annual changes in market access are determined by any railway construction which decreases the travel cost between any two districts. This means that rail construction does not necessarily need to be local to a district to influence its market access and changes in market access from "distant" rail construction is plausibly exogenous. Indeed, controlling for local rail construction does not greatly affect the market access coefficient estimate indicating that distant rail construction causes much of the increase in local patenting. The setting provides a relatively clean measure of changes to market access due to rail as there was no concurrent major expansion of infrastructure since the majority of the turnpike and canal networks were built in the previous century.

I find that a doubling of market access - approximately the median increase between 1823-1861 - causes an increase in local patenting per capita of 51%. Including the local rail station indicator in the regression and observing the change in magnitude of the coefficient implies that over 40% of the local station effect on patenting is explained by the increase in market access that a station brings. I also find that the local rail effect is driven by districts which are in the top quintile by the change in market access between 1823-1861. These districts have low market access in 1823, indicating that there are greater gains in connecting places with poor transportation infrastructure prior to the rail network. A back of the envelope calculation finds that the change in market access caused a 49% increase in aggregate patenting and a 5% increase in national TFP.

I exploit sector heterogeneity to empirically test the theory that greater market access causes an increase in innovation because firms which operate in small markets are unable to spread high fixed cost R&D expenditures to a large enough customer base and that a larger market allows for precisely this type of R&D spending (Sokoloff 1988). I examine sectors which I identify as having high fixed, but low variable costs or sectors which produce niche products. The latter gain from the rail network because they can ship their goods further and customers typically have to travel a greater distance to purchase a niche product compared with non-niche products (Brynjolfsson et al. 2003). In both cases I find that regressing patents on the interaction between market access and these sectors' labor shares produces large and statistically significant estimates, implying that these sectors are driving the returns to patenting. Additionally, market access increases concurrently with districts' labor share in these sectors, suggesting that firms in these sectors are benefiting from increased demand and increasing expenditures. I then use an input-output table for 1841 England created by Horrell et al. (1994) and disaggregate market access to the district-sector-year level and find that within sector patenting increases by 40%. Using an input-output table means that I am limited to the sectors used in that table and identifying high fixed cost or niche product sectors is not viable as the sectors are too broadly defined. I instead adopt the manufacturing definition used by Horrell et al. (1994) and find that market access increases patenting activity most in manufacturing sectors and in districts with a high share of manufacturing labor, in line with expectations that firms which produce goods benefit more from increased trade through the rail network than firms which produce services. Additionally, the returns to within sector patenting from an increase in market access are greatest for district-sectors with a high number of workers operating in that sector, implying that a district which does not produce (for example) textile goods does not gain textile patents because of an increase in market access.

It is possible that the market access effect I find is actually due to an increase in knowledge flows; previous papers have shown that transportation networks induce innovation by fostering knowledge flows between inventors by facilitating communication (Agrawal et al. 2017; Pauly and Stipanicic 2024). I adapt my market access variable to create a "knowledge flows" variable which measures a district's exposure to other districts' patents by substituting the stock of patents in a sector in place of local GDP.[3] The knowledge flows variable shows whether easier transportation between a district and a location with many inventors in the same field (rather than a large market) causes innovation in that district. By using within sector knowledge flows I mimic patent citations which, following Jaffe et al. 1993 are widely used in the literature to measure knowledge spillovers and which were not recorded during the period I study.

A doubling of this knowledge flows measure is associated with a 43% increase in within sector patenting per capita in a district. Large towns are disproportionately affected. Intuitively, a large enough existing knowledge base is a prerequisite for a district to benefit from incoming knowledge flows as there is a higher chance that a resident possesses the relevant knowledge to benefit from the incoming flows. Interacting the knowledge flows variable with the top quintile of workers in a district and sector yields a positive coefficient, indicating that most of the returns are in districts and sectors with a high number of workers with relevant expertise.

The market access and knowledge flows channels are distinct. Different districts receive the largest returns to patenting from the two measures: knowledge

[3]This measure is similar to what Pauly and Stipanicic (2024) use in measuring knowledge flows during the American Jet Age.

flows have a disproportionate effect on large towns and market access on districts with a high share of manufacturing labor. The reverse is not true, indicating that different districts are driving the effects. Though the variables exhibit high serial correlation that makes direct comparisons difficult, I find no correlation between the variables when comparing their change from the beginning of the period to the end. Additionally, the interaction between knowledge flows and starting point market access is insignificant (as is the reverse case), further pointing to a lack of relationship between the channels.

I do not find empirical evidence that the increase in patenting is due to a displacement of innovation caused by the railroads. The patenting rate of "not yet treated" and "never treated" districts does not significantly decrease after a district within 50KM receives a station, indicating that there is no outflow of patenting from the not treated into the treated districts. Similarly, the mean number of patents in the districts neighboring a treated district does not decrease after that district's treatment (and before the neighbors receive treatment). Using the 1851 Census, I proxy inventors (who are not identified as such in the Census) with highly skilled individuals, a group that includes entrepreneurs and engineers. Conditional on moving from their district at birth, these individuals are no more likely to move to districts with a rail station than are others. I also construct a structural model in the style of Kline and Moretti (2014) to explain changes to aggregate productivity following a large scale infrastructure investment and the results indicate that the productivity returns from railways do not cancel out at the aggregate level.

This paper builds on multiple strands of economic literature. First, it contributes to the literature on the impact of market size on innovation (Acemoglu and Linn 2004). Exporter firms have been shown to be more productive (Melitz and Redding 2014), with several papers finding evidence that access to new export markets causes firms to increase their productivity either through direct innovation or through investing in better technology.[4] In a similar vein, Aghion et al. (2023) and Aghion et al. (2024) find that French firms invest in innovation after a positive shock to market size. Related to this literature is research focusing on the impact of an influx of imports on local firm productivity and innovation (Bombardini et al. 2017; Bloom et al. 2016; Autor et al. 2020). My research is closely related to both the export and import strands of the literature but operates through a domestic trade channel rather than through an international one.

[4]For example: Lileeva and Trefler (2010), Damijan et al. (2010), Aw et al. (2011), and Bustos (2011).

This distinction has implications for the findings. Transportation networks facilitate trade because of a new technology or investment in infrastructure rather than tariff reduction, offering policymakers a distinct avenue for increasing productivity. Little empirical work has been done on how new infrastructure, and specifically, transportation networks, can cause such a demand-driven increase in innovation (Juhász and Steinwender 2023). Perlman (2017) studies this question for 19th Century United States, but the results suggest an increase in innovation is limited to low quality patents. I use the definition of "market access" developed by Redding and Venables (2004) and Donaldson and Hornbeck (2016) to measure the rail network's returns to innovation that manifest through a trade channel and find strong, robust effects. This is the first paper to create an annual map of market access in England during this time period, which could be useful for future research on the rail network linking trade with fast changing dynamics, such as firm productivity.[5]

Second, this paper contributes to the literature on knowledge flows and on how increased communication between individuals leads to higher levels of innovation (Duranton and Puga 2004; Glaeser and Gottlieb 2009; Hanlon and Miscio 2017; Davis and Dingel 2019; Buera and Oberfield 2020). Specifically, this paper finds evidence that a decrease in communication costs fosters an increase in knowledge flows which causes an increase in within-sector patenting. In doing so, it builds on a nascent literature which spans a variety of settings: Agrawal et al. (2017) use the construction of highways in the 1980s in United States; Catalini et al. (2020) and Pauly and Stipanicic (2024) use changes in the American airline industry; and Hanlon et al. (2022) study the impact of decreased mail costs in 19th century England. This paper is the first to find direct evidence that knowledge flows fostered by a rail network increase innovation (Juhász and Steinwender 2023), though there is a growing literature on the effect of the railroad to diffuse knowledge.[6]

Third, this paper contributes to the literature regarding transportation infrastructure's general economic effects. Several papers have studied the impact of transportation networks on productivity (Donaldson and Hornbeck 2016; Donaldson 2018; Faber 2014; Baum-Snow et al. 2020). This paper is distinct in that it focuses more specifically on innovation. Transportation infrastructure has been found to increase local population (Gregory and Marti 2010; Bogart et al. 2022;

[5]While You et al. (2021) build a comprehensive, detailed map of market access in England, they only observe the years 1830 and 1911.

[6]See, for example, Andersson et al. (2023), Yamasaki 2017, and Américo 2022, which study the Swedish, Japanese, and Brazilian railroad networks respectively.

Büchel and Kyburz 2020) and urbanization (Atack et al. 2010; Hornung 2015).[7] The literature also finds firm level effects: Atack and Margo (2011) and Hornung (2015) find evidence that transportation networks are associated with increased firm size, possibly due to their role in increasing competition through market expansion, while other papers find evidence of higher firm productivity (Ghani et al. 2016; Gibbons et al. 2019; Holl 2016; Tang 2014). Michaels (2008) finds that highway connections increased demand for high skilled labor. In finding that the railway system provided large returns to patenting, this paper contributes to the literature which finds positive returns to transportation infrastructure. The trade channel I find could also be useful to more comprehensively measure the benefits of such transportation systems through a trade framework. Jaworski et al. (2023) estimate that removing the entire US highway system would decrease US GDP between 2.6%-3.6%, with a quarter of this figure due to higher trade costs; pinning down the returns to patenting could increase precision in these types of estimates.

The structure of the paper is as follows: Section 1.2 discusses the data used in this paper; Section 1.3 describes the identification strategy; Section 1.4 presents the main results; Section 1.5 discusses the potential mechanisms with a particular focus on the market access channel; Section 1.6 describes various robustness tests on the main results; Section 1.7 concludes. The Appendix includes additional tables, maps, and graphs as well as discussion of a model on aggregate productivity effects that follows Kline and Moretti (2014).

1.2 Data

1.2.1 Patents

I use patent applications as a proxy for innovation. I digitize patent data from Woodcroft (1854), which details all patent applications from 1823-1852. Each application includes the application date and a description of the patent, as well as the names, occupations, and addresses of the inventors. I exclude all patents which are filed from outside of England or which are filed by patent agents as the stated address corresponds with the agent or the Patent Office, rather than with the inventor.[8] Rail patents are also excluded from most analyses; they are identified by key words in the patent description (e.g. "rail" or "locomotive"). This

[7] Transportation networks have also been found to increase suburbanization (Baum-Snow 2007; Garcia-López et al. 2015).

[8] These are identified in the patent descriptions as "communications".

is perhaps overly restrictive as it removes patents that are only broadly related to rail, such as engines that can be used for stationary or locomotive power, but it ensures that rail patents cannot enter into the analysis. I include these patents as a robustness check in Section 1.6 and find that they do not meaningfully alter the results.

I use the addresses from the patent data to match individuals to a geographic dataset (Satchell et al. 2023) which includes a shapefile denoting the boundaries of places at various geographic levels in England in 1851.[9] I aggregate to the district level (the second largest below county) as many of the addresses outside of London include only broad geographic areas.[10] There are 575 districts in England. Matching at the district level also allows me to use population data from the decadal Censuses prior to 1851, as the locations in these Censuses were at a district or union level. The mean area of a district is 86.97 KM^2.[11]

I supplement the patent applications with data collected and shared by Coluccia and Dossi (2023) which extends the time field through to 1891. I use this to extend the main analysis to 1861 and to show the persistence of the rail effect until 1891. Due to limits in the rail dataset, the main results are limited to the 1823-1861 time period.

The number of patent applications in a given district and year is low, with a mean of 1.13 patents. The data are even more sparse in the 1823-1852 period; there are 6,875 total patents across 575 districts and 30 years (for a mean of .40 patents per district and year). The number of patents is disproportionately high in the cities. The 33 districts which comprise London and Birmingham account for 47% of these 6,875 patents. The Patent Law Amendment Act of 1852 greatly reduced the cost of filing a patent, and there was a large increase in applications following its implementation. I control for this by always including a full set of time dummies and further robustness tests present no evidence that the Patent Law affects my results. Most analyses in this paper utilize a Poisson regression model to deal with the sparsity and the accompanying non-normality of residuals in an OLS model.

I categorize each patent into one of 55 sectors.[12] For the 1823-1852 data, I use the patent description field to do so. The post 1852 data provided by Coluccia and Dossi (2023) contains a CPC industry field for the patents and I match these

[9]The same patent can be matched to multiple locations if the applications listed multiple inventors.

[10]I am able to match 87.3% of the 1823-1852 patents in England in this way.

[11]By comparison, modern day Greater London is 1,572 KM^2

[12]The full list of sectors can be found in the Appendix.

to the 55 sectors to have a uniform categorization across all years.

1.2.2 Rail

The rail data are obtained from multiple sources. First, I use the shapefiles provided by Henneberg et al. (2018a), Henneberg et al. (2018b), Henneberg et al. (2018c), and Satchell et al. (2018), to obtain precise rail line and station locations. These datasets are a snapshot of the stations and lines that existed in 1851 and 1861. In order to obtain precise opening years, I match the stations and lines in the shapefiles with Cobb (2015), which contains detailed maps of England with information on rail lines and stations' opening dates. I then match the rail lines to the districts contained in the geographic shapefile provided by (Satchell et al. 2023). The result is a shapefile that shows the exact year in which each district first received a rail line or station from 1825-1861.

I define a district as having a rail station if there is a station within the district's geographic bounds. Additionally, districts with a total area under 28.27KM^2 (equivalent to a radius of 3KM for a perfect circle) are considered to have access if there is a station within 3KM of the district's geographic centroid. This is to ensure that districts that are small in area are not excluded when the inhabitants of the district could reasonably walk to a nearby station.

Though the first rail station opened in 1825, the opening of the Liverpool and Manchester Railway in 1830 is considered the birth of the Rail Era in England as it was the first inter-city railway and the first to rely exclusively on steam power. Rapid expansion occurred in the 1840s in a period known as Rail Mania, during which there was high speculative interest in rail.

1.2.3 Other Data

I use the Natural England (2023) dataset, which is a shapefile that divides England into 250m x 250m squares with a land description of each square, to create simulated rail lines that act as a low cost path network between locations to address endogeneity concerns regarding placement of the actual rail lines. I explain the methodology in Section 1.3.

For years prior to 1851, I use decadal district level population data from the Census of Great Britain (1852). As the districts belonging to Gloucestershire and Staffordshire counties are missing from the available scans of some years, I remove all districts from these counties from the analysis, decreasing the total count of districts from 575 to 542. The first "modern" census was taken in 1851, and from

1851-1891 I use census microdata from (Schurer and Higgs 2023) to obtain more accurate decadal population estimates, occupations, and birth districts.[13]

Lastly, I use the return of gross annual value of property and profits measured through tax schedules for every parish in England in 1815, 1843, and 1860.[14] Scanned copies of the returns are held by the British Parliamentary Papers. Unlike the Poor Law land value measurements, the returns were collected by qualified officials sent from the House of Commons, making them less likely to be subject to the distorted values recorded in the Poor Law data.[15] The values are at the parish or union level and are aggregated or approximated to the district level using the Satchell et al. 2023 geographic dataset.[16]

1.3 Identification

A key aim of this paper is to identify the causal impact of a local rail station on innovation. The main econometric challenge is that rail stations are unlikely to be randomly assigned. This issue is endemic to the transportation network literature and I employ a common strategy referred to as the "inconsequential place approach" to deal with it (Redding and Turner 2015). The core concept is that rail lines are built to connect two places of interest. Districts which lay in between them can be considered "inconsequential" in that they are only connected to the network because the rail line has to run through them in order to connect the important districts. I present here the main empirical Poisson model used in the paper to describe the strategy in greater detail:

$$E(Patents_{i,t}) = exp(\alpha_i + \lambda_t + \beta D_{i,t}) \tag{1.1}$$

$D_{i,t}$ is an indicator variable which equals one for any year t in which a district i has a local rail station and zero otherwise and $Patents_{i,t}$ is the number of local patents per capita. α_i and λ_t are respectively district and time fixed effects.

The first step is to identify districts which rail firms likely sought to connect. A firm is likely to build a greater proportion of its stations in cities to maximize its

[13]The 1871 Census is mostly destroyed, leaving only data from the 1851, 1861, 1881, and 1891 Censuses.

[14]British Parliamentary Papers (1854a), British Parliamentary Papers (1854b), and British Parliamentary Papers (1860)

[15]See Purdy (1860) for a comparison of the two.

[16]Parish names do not always perfectly match with the parish names in Schurer and Higgs (2023). When using this dataset, I drop all districts for which the parishes that constitute them do not contain any matched data.

potential customer base; population is therefore likely a key, observable determinant in station placement and I control for the imputed population by measuring innovation as patents per capita. I also remove all districts that are included as part of London and Birmingham, the two largest economic centers at this time which rail firms clearly sought to connect.[17] Additionally, the inclusion of year and district fixed effects in equation (1.1) allows me to control for any unobservable characteristics that rail firms might have used to make placement decisions.

Even with the inclusion of fixed effects and controlling for population, there is still the potential that growth trends of unobservable variables might differ. Districts which were expected to experience faster economic growth, which may be correlated with faster patent growth, would have been more profitable to the rail firms. If rail firms could correctly identify which districts were expected to grow faster, the treatment would not be randomly assigned, even with the inclusion of the fixed effects controls. To that end, I remove from the analysis all districts which were plausibly chosen by rail firms as important to connect and only retain districts which appear to have received rail connections "accidentally." The districts which are retained are those that lie between the "important" districts and are therefore only connected to the rail network because they happen to lie along the path that connects the "important" districts.

I designate a district as "important" - and therefore remove it from the analysis - if a rail firm clearly sought to connect it to the network. I establish this intent using rail line placement. Any district which contains a rail line that is not connected on both ends is a clear start or end point of a rail line and therefore a district that a rail firm determined important enough to connect to the network. Given the evolving nature of the rail network, it is possible that these points served as temporary rail termini while the rest of the line was still under construction. To address this, only points which see no construction for the five years following their opening date are considered end points. Figure 1.1 shows a segment of the rail network in 1850. A line running from Bedford to Banbury was opened between 1846 and 1850. As the Bedford and Banbury districts house the ends of the rail line, they are removed; Brackley, Buckingham[18], Winslow, Newport Pagnell, Woburn, and Ampthill lie between these end points and have rail stations and are therefore considered treated districts.

It is possible that firms constructed lines to certain locations but then continued the line onwards. In such a case, there is no terminus and the district would be

[17]Indeed, one of the first prominent rail firms was the London and Birmingham Railway.
[18]The station in Buckingham is not visible.

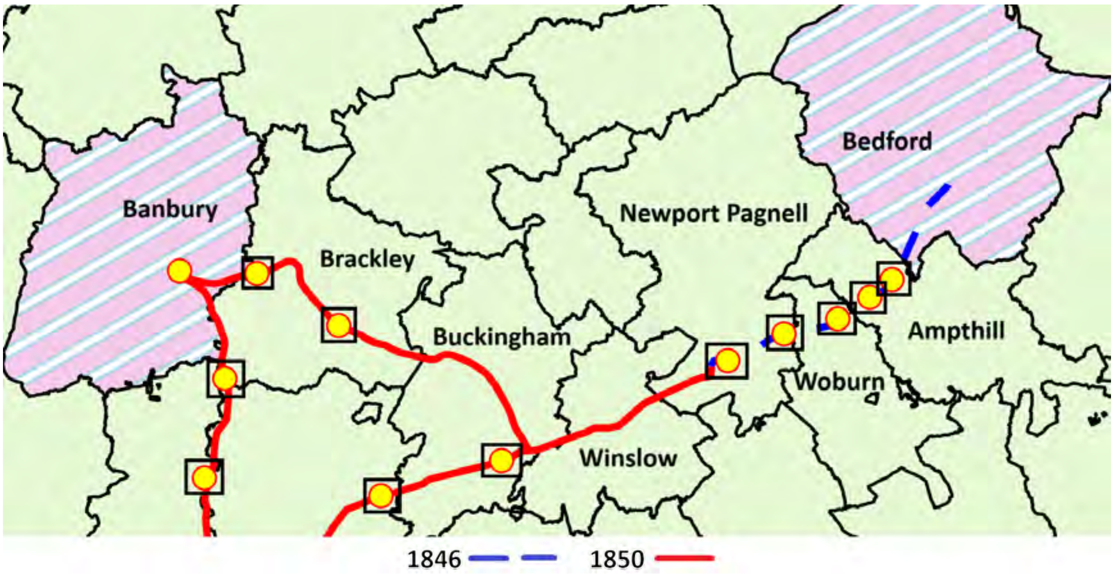


Figure 1.1: Example of a rail line that was opened in 1846 and 1850 to demonstrate how the start and end points of rail lines are identified. The opening years of the rail line segments are denoted at the bottom. Banbury and Bedford house line end points and so are excluded from the main treatment definition. Brackley, Buckingham, Winslow, Newport Pagnell, Woburn, and Ampthill all contain stations (circles marked by squares) and lie between the Bedford and Banbury end points and are therefore retained and considered treated.

retained. To remedy this, I also remove all districts which are in the top quintile of rail kilometers per square kilometer of the district's total area in 1851. Districts with high rail density likely had multiple lines passing through it. I view this as an indication of intention for a particular destination by the rail firms because they are either competing to serve a district or using a location as a rail hub to connect otherwise separate lines.

While this methodology should in theory remove districts which were potentially identified by rail firms as exhibiting high expected economic growth, the possibility remains that the remaining treated districts were chosen in a non-random manner. These remaining districts may not have been chosen simply because they happened to lie between two of the "important" districts that were removed in the first step, but because the rail companies actively sought to connect them to the network as they were themselves "important." For example, a rail firm might have sought to connect London and Birmingham, but deviated from the straight line path between them in order to connect Oxford.

To address this, I follow the recent literature (Banerjee et al. 2020; Faber 2014; Atack et al. 2010) and I create a network of simulated rail lines which connect every station in England. The algorithm connects points with a straight line that moves along a grid of 250 x 250 meter squares of land identified and described by the Natural England (2023) dataset. The constructed line can only deviate from the straight line path to avoid a square that is defined as "unimproved land." This simulates the notion that rail firms chose to connect the "important" districts in the cheapest manner possible (by connecting them with straight lines and avoiding areas that required additional construction costs). Each simulated line segment connects only two points at a time. To preserve the time element of the dataset, two points can only be paired if they are in the same year. For example, in Figure 1.1 the 1846 line segment between Bedford and Newport Pagnell and the 1850 segment between Newport Pagnell and Banbury are simulated separately.

To avoid choosing the exact points on the actual line, I simulate each line five times, using five random points near the real start and end point of each line segment. For example, to simulate the Bedford-Newport Pagnell line segment, I choose a random point within a 1.1KM radius of the line end point in Bedford and a random point within a 1.1KM radius of the end point in Newport Pagnell. I repeat this four more times, and then choose the shortest simulated line to add an additional "cost saving" element to the process. Figure 1.2 shows an actual

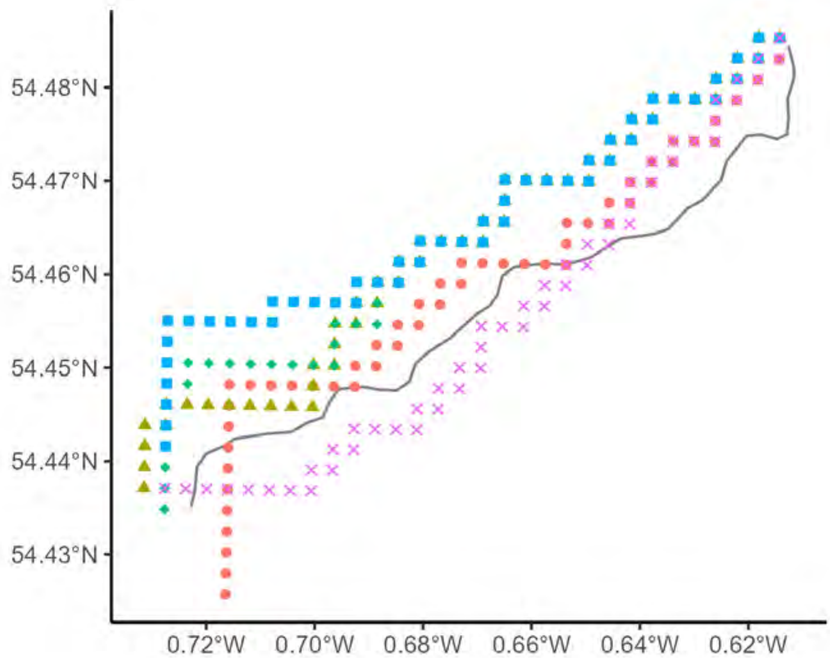


Figure 1.2: An example of the simulated lines. Shapes represent the paths of the simulated lines. Each shape/color represents one of the five simulated rail lines. The black line is the actual line they are simulating. The line composed of blue squares is the shortest line and is the one that is chosen to compare against the actual line.

rail line in black and its five associated simulated lines. The line composed of blue squares is the shortest and therefore the line against which the black line is compared.

Once the shortest simulated line is chosen, I match the simulated line network with the real rail line network. Only districts which are treated in the same year in both the real and simulated networks are retained for the analysis. This is to ensure that the districts that remain in the analysis are only districts whose rail lines fall along the geographic path of least resistance in terms of rail construction. The group of remaining districts comprise the "main" treatment definition I use for most analyses in this paper.

Table 1.1 presents summary statistics for the three treatment definitions discussed above. These definitions are: "All Districts," which includes all treated districts; "Ends and Hubs Removed," which removes treated districts which house rail line end points and rail hubs as well as all districts which comprise London and Birmingham; and the main definition which excludes all treated districts that the previous definitions exclude in addition to districts whose simulated line network year of treatment differs from the actual treatment year. Each of the treatment definitions shows a large increase in the pre-treatment to the post-treatment mean number of patents. The relative difference in values between the "Never Treated" and the "Pre Treatment" columns decreases from the "All Districts" definition, but is still relatively sizeable and potentially problematic. This is due to a few districts within London which are near a station but never receive their own station inflating the "Never Treated" mean in Panel A before they are removed in Panel B (because they are part of London).

One concern with this methodology is that by removing districts from the analysis the results are not representative of the nation. This is potentially a valid point; urban centers are "important" for rail firms and so most are removed. The decrease in the mean population across treatment definitions is evident in Table 1.1. Nevertheless, I find that the composition of the data which comprises the main treatment definition is fairly similar to that which comprises all districts (Figure 1.3). The total number of districts treated in each year follows a similar trend across treatment definitions as does the number of log patents. This suggests that the main treatment definition is not altering the time component of the panel in a meaningful way. The main definition includes a higher proportion of agricultural patents, as one might expect given that many of the urban centers are removed. The composition of the other 55 sectors however is quite similar. A table comparing the patent sectoral composition across definitions is in the Appendix.

Table 1.1: Summary Statistics Across Treatment Definitions

A. All Districts	Treated	Pre Treatment	Post Treatment	Never Treated
Mean Population	28,386.12	24,593.03	31,602.12	23,912.81
Mean Population Density	3,200.73	2,723.52	3,535.86	23,607.29
Mean Patents per District per Year	1.13	0.23	2.34	2.54
Total Districts	409	409	409	70

B. Ends and Hubs Removed	Treated	Pre Treatment	Post Treatment	Never Treated
Mean Population	18,884.56	17,927.07	18,884.56	14,897.61
Mean Population Density	220.72	209.14	220.72	971.98
Mean Patents per District per Year	0.26	0.08	0.26	0.13
Total Districts	176	176	176	42

C. Main Treatment Definition	Treated	Pre Treatment	Post Treatment	Never Treated
Mean Population	19,066.35	18,092.05	20,415.37	14,897.61
Mean Population Density	224.38	212.15	243.49	971.98
Mean Patents per District per Year	0.28	0.09	0.60	0.13
Total Districts	134	134	134	42

Notes: Summary statistics for the three treatment definitions discussed in Section 1.3. Panel A includes all districts. Panel B excludes treated districts which contain rail line end points and rail hubs as well as all districts which comprise London and Birmingham. Panel C uses the main treatment definition, which further restricts the previous definition to districts which are treated in the same year both in reality and according to the simulated lines network. The number of "Never Treated" districts decreases from the top panel due to 12 districts which house the end of a rail line but never receive a station and districts which comprise London or Birmingham. Districts which have zero patents between 1823-1861 are excluded as they are excluded from most regressions in this paper due to the use of district and year fixed effects.

It is possible that firms' rail placement decisions had a political dimension that is not covered by this methodology. Parliamentary approval was required for each line and politicians with an interest in the rail lines were able to steer projects (Esteves and Geisler Mesevage 2021). To ensure that I am not removing lines which deviate from the least cost path due to political interference I use project approval from an apolitical team of engineers as a robustness check. This is discussed in greater detail in Section 1.6.

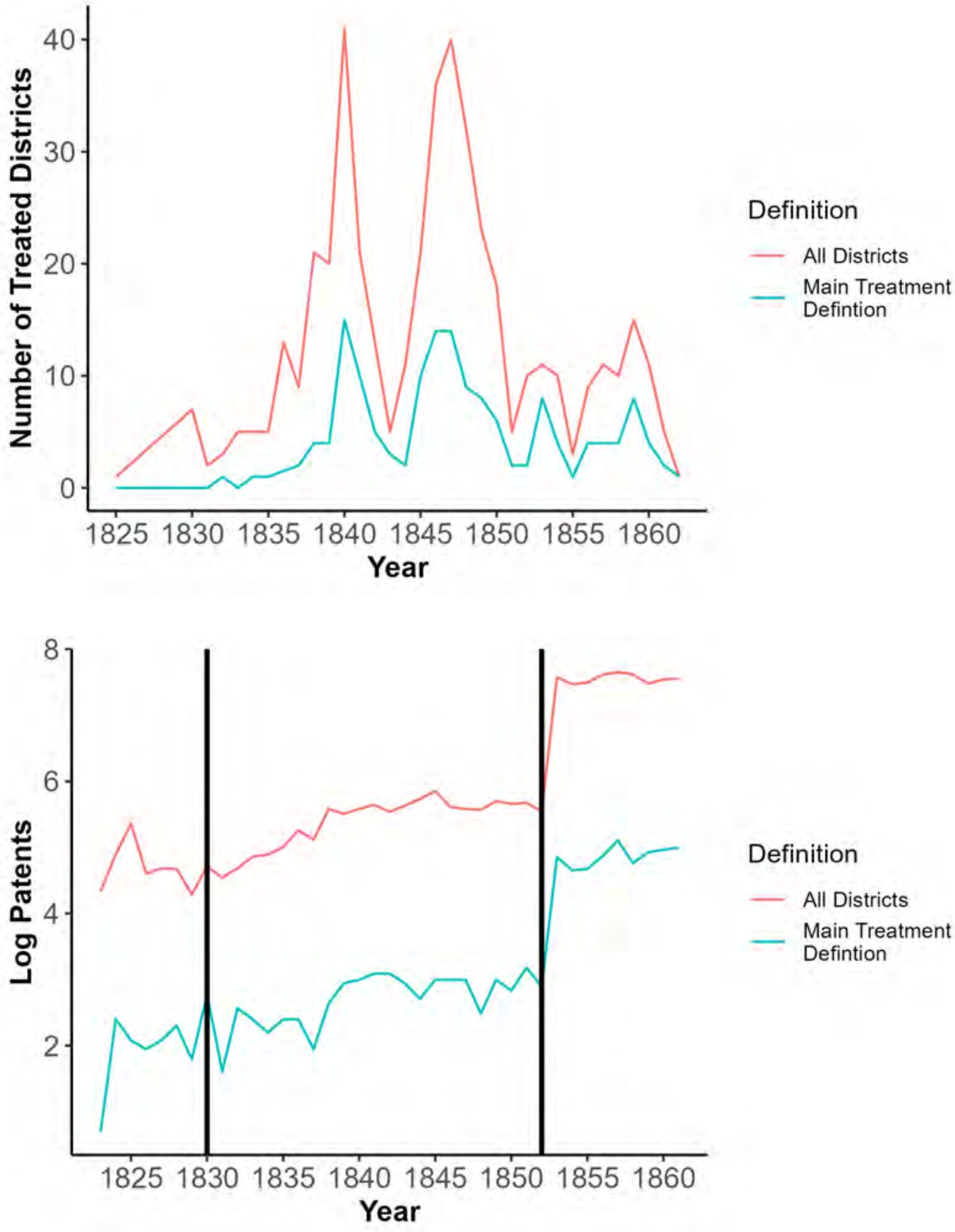


Figure 1.3: The top graph shows the number of districts first receiving a rail station in each year by treatment definition. The red line includes all districts and the blue line includes only districts that are either never treated or those which constitute the main treatment definition as defined in Section 1.3. The first station opened in 1825 and the first inter-city line opened in 1830. The bottom graph shows the log of patents applications by inventors by sample. Vertical lines at 1830 and 1852 represent the beginning of the Rail Age and the 1852 Patent Law Amendment Act respectively.

1.4 Main Empirical Results

1.4.1 Causal Effect of Rail

As a starting point to the analysis, I plot an event study graph using the difference in difference estimation strategy proposed by Callaway and Sant'Anna (2021) where districts which are not treated by 1861, the last year for which I have annual rail data, are considered "never treated." I estimate the effect of a rail station on a district's patents per capita during the period 1823-1891. The event study corresponds to Table 1.2, Panel A, column (5).

The event study plot (Figure 1.4) shows a clear and durable increase in patenting activity following a district's connection to the rail network. The pre-treatment period is flat with no period statistically different from zero, indicating a lack of evidence that the pre-trends of the treated districts behave differently compared with the control. A similar graph with population as the dependent variable likewise shows no pre-trends. The post-treatment period shows an immediate jump

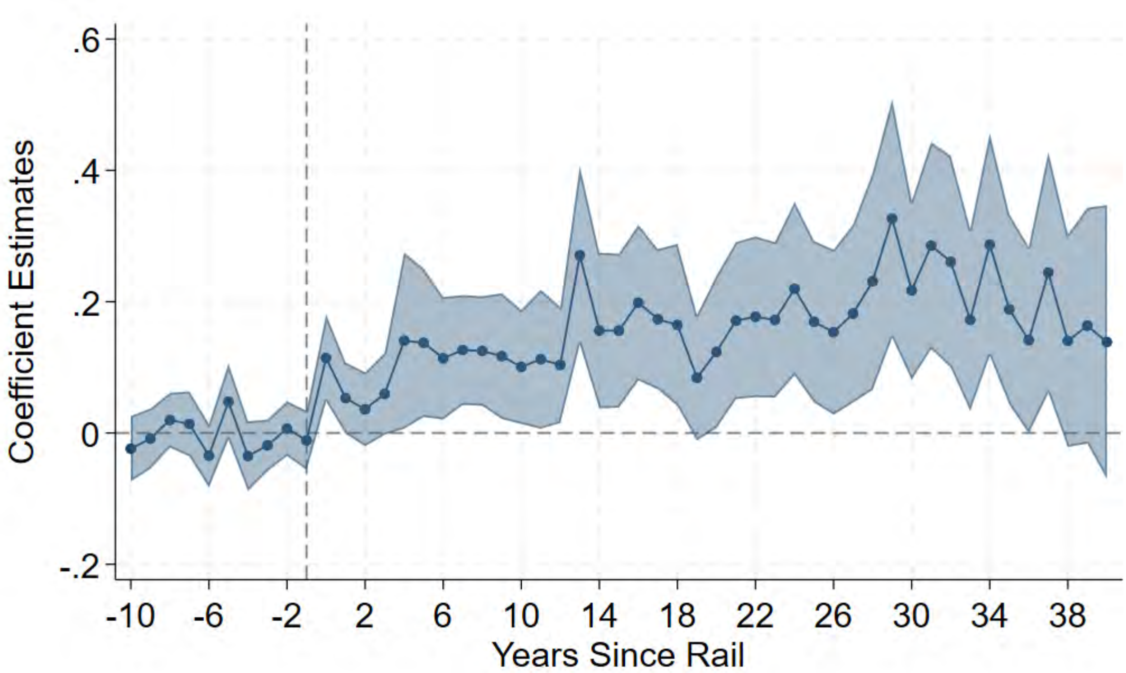


Figure 1.4: Event study plot with patents per capita as the dependent variable and 1823-1891 as the time period. Districts are treated in the year in which they receive a rail station. Because of limitations to the rail data, districts which are not yet treated by 1861 are considered as "never treated" districts. I use the difference-in-difference estimator proposed by Callaway and Sant'Anna (2021) to correctly estimate the "never treated" control group.

at time zero - the year in which a district receives a rail station. It is plausible that patent applications were affected quickly for two reasons. First, the complexity of inventions was lower in the 19th century, requiring less time for an idea to come to fruition. Second, patent applications did not require significant details to be included in the initial application. Per the National Archive, "specifications were required and patentees had several months to submit these after the patent was sealed"(The National Archives 2023).

A concern with using patents as the dependent variable is that inventors could rely on secrecy and a small market to shield their innovation and then patent with the arrival of a rail station (to protect against rival entrepreneurs in the larger market). The long-running positive returns to patenting in Figure 1.4 make this implausible as the patenting effect would die shortly after the initial period post rail.

I estimate equation (1.1) using a Poisson model across 1823-1861 and present the results in Table 1.2. Errors are clustered by district to deal with potential serial correlation of the residuals over time. I present results where the dependent variable is the number of patent applications per capita (using an exposure variable for the imputed population) and when it is the number of patent applications. A positive coefficient for the former indicates that patent applications are growing faster than the population. I interpret the coefficients as the average effect of the treatment on the treated in the post-treatment period (ATT). In all specifications, districts are only retained in the estimation results if there is at least one positive value in the dependent variable because of the inclusion of district and time fixed effects.

The baseline result in column (1) indicates that having a rail station increases local patenting per capita by 77%.[19] The results are quite similar when excluding the population exposure variable in Panel B. This is possibly because the estimated increase of population caused by a local rail station is only 2.9%, an almost negligible figure when compared with the increase in patenting.

In column (2) I estimate the model using the broader "Accidental" treatment definition which includes districts otherwise excluded due to the simulated line network. The estimated coefficient of .479 is smaller than the baseline coefficient of .571 in column (1), though reasonably similar. This suggests that the simulated rail line network I create is not significantly changing the results, even as it removes 42 treated districts.

In column (3) I use the patent quality index created by Nuvolari et al. (2021).

[19] I find the implied elasticity by using the transformation of (exp(coefficient)-1)*100.

Table 1.2: Effect of Rail Network Access on Innovation

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Patents per Capita						
Rail	0.571***	0.479***	0.807*	0.394**	0.173***	1.033***
	(0.139)	(0.130)	(0.487)	(0.164)	(0.039)	(0.275)
Analysis	Main	No End Pts.	Quality	Long Run	C & S	SDID
Observations	6864	8502	1300	12144	12144	2464
Districts	176	218	50	176	176	176
Years	1823-1861	1823-1861	1823-1850	1823-1891	1823-1891	1823-1861
Mean of Dep. Var.	0.247	0.237	0.075	0.759	.255	1.007
Panel B: Patents						
Rail	0.576***	0.480***	0.859*	0.343*	0.625**	0.338***
	(0.147)	(0.136)	(0.485)	(0.180)	(0.254)	(0.095)
Analysis	Main	No End Pts.	Quality	Long Run	C & S	SDID
Observations	6864	8502	1300	12144	12144	2464
Districts	176	218	50	176	176	176
Years	1823-1861	1823-1861	1823-1850	1823-1891	1823-1891	1823-1861
Mean of Dep. Var.	0.247	0.237	0.075	0.759	0.759	.229

Notes: Poisson estimation of equation (1.1) where the dependent variable is the number of patent applications per capita in Panel A and patent applications in Panel B. The per capita rate is calculated using an exposure variable for population. The main independent variable is an indicator equal to one if a district has a rail station in year t. District and time fixed effects are included in all columns. The main results are displayed in column (1). Column (2) uses the "Ends and Hubs Removed" treatment definition which excludes rail line end points and hubs, but does not use the simulated line network to further restrict districts. Column (3) only includes patents in the top quartile by patent quality as the dependent variable. The patent quality dataset only runs to 1850. Column (4) extends the time period to 1891. Districts treated after 1861 are considered untreated for the entire period due to constraints with the rail data. Column (5) reports the estimates of a Callaway and Sant'Anna (2021) style model only using "Never Treated" districts as the control. Column (6) reports the estimates of the SDID proposed by Arkhangelsky et al. (2021). Due to the low patent count, in column (6) periods are aggregated to three years to ensure a more reasonable pre-trend comparison between the treated and control groups and patent counts and population are averaged across the three years. Districts with zero patents in every year between 1823-1861 are removed from the analysis in all columns due to the inclusion of district and time fixed effects. Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The authors' index is designed to evaluate the relative importance of patents between 1700-1850, assigning a value to each patent based on inventors' biographies and the history of technology. I limit the dependent variable to patents in the top 25% of the Nuvolari et al. (2021) index within the 1823-1850 period. This removes most of the patents that had little historical impact. I find that the increase in patenting activity was not limited to low quality patents; a connection to the rail network caused local patenting to increase by 124%. The error term however is higher than in the baseline result in column (1). This is likely due to a large decrease in observations. The dataset provided by Nuvolari et al. (2021) ends in 1850, which significantly curtails the number of observations. Patenting was more expensive prior to the 1852 Patent Law Amendment Act and there are fewer patents in this period. Nevertheless, the large coefficient provides assurance that the effect on top quality patents is likely positive.

For the baseline result, I restrict the data to 1823-1861, the last year in the rail dataset. I extend the time frame to include an additional thirty years in column (4) by considering all districts not yet treated by 1861 as untreated until 1891. The coefficient of .391 is somewhat lower than in the shorter time frame, though it still demonstrates a sizeable, long lasting effect from rail treatment. A likely reason for the decrease in the coefficient is that additional districts were treated during this time period, increasing patenting activity in the control group. Indeed, by 1881 (the only additional year of rail data) the vast majority of districts are treated. Ignoring new rail construction beyond 1861 is an even larger issue when exploring the market access mechanism as market access depends on construction of the entire network. Restricting the data to 1861, as I do for most estimates, avoids this issue.

The staggered expansion of the rail network presents a potential issue in that the empirical specification dictates that treated districts are compared against all districts which are not treated in year t. This includes both "Never Treated" districts and "Not Yet Treated" districts. It is possible that a rail station might have heterogenous effects across time. This would entail a larger effect for districts that were treated in some years than in other years. To remedy this, I estimate the model using the estimation strategy proposed by Callaway and Sant'Anna (2021), which only compares treated districts with never treated districts for the extended 1823-1891 period. The results are in column (5) of Table 1.2. Unlike the Poisson estimates in columns (1)-(4), these estimates are of a linear model. To compare, I divide the coefficient by the mean, which produces an implied semi-elasticity of 68%, somewhat similar to the implied elasticity in column (4) of 48%.

I utilize the "Synthetic Difference in Difference" (SDID) estimation strategy proposed by Arkhangelsky et al. (2021) to verify that the difference-in-difference parallel trend assumption is not violated. The SDID estimator optimally weights the data to ensure that the parallel trends assumption holds. Due to the low count patent data, I aggregate each time period to three years and take the mean patent and population values. A district is shocked in period t if it receives a station in any of the three years that comprise that period. This aggregation allows for a more meaningful comparison of the pre-trends between the control and treated groups. Like the Callaway and Sant'Anna (2021) estimator, the SDID estimator is linear; the implied elasticity of 105% is somewhat larger than the baseline estimate in column (1) but still reasonably close, indicating that the weighting does not meaningfully affect the results, even as it produces a control group with pre-trend lines that appear close to parallel with the treated group's pre-trends.[20]

1.4.2 Comparison of Results with the Literature

A few recent papers also find that gaining railroads produces large, positive effects on innovation. Andersson et al. (2023) find that Swedish municipalities were 15-30 percentage points more likely to patent relative to their baseline measure, similar to the 35% elasticity I find when estimating the extensive margin effect, while Perlman (2017) finds that over the two decades following a rail connection, patenting per capita in an American counties doubles.

While my results are in line with other papers' findings on the effect of railroad networks, these findings are much larger than what researchers have found in other settings. Agrawal et al. (2017) and Bottasso et al. (2022) find that that a 10% increase in the amount of highways in a region causes a 1.7% - 4% increase in regional patenting over a five year period. Those papers both use a single five year change as the dependent variable and measure the treatment as an intensive margin effect rather than an extensive effect, complicating comparisons. Nevertheless, interpreting the results as an average treatment of the treated implies that the impact to innovation is much larger in the setting I study than in either of those papers. The upper bound of their estimates is still half the size of the baseline estimate in Table 1.2. Pauly and Stipanicic (2024) study the airline industry and find that an increase in knowledge access caused patenting to increase by 3.5% annually.

One plausible reason for the difference in magnitude between the railroad lit-

[20]Parallel trend graphs utilizing SDID weighting can be found in the Appendix.

erature and the non-railroad literature is the setting. National railroad networks were built a century before the airline and highway networks studied by other researchers. A railroad made overland travel dramatically cheaper for both goods and people; highways provided more marginal gains as they complemented the existing railway networks. Another possibility is that railroads increase innovation through multiple channels: by increasing market access and by increasing knowledge flows as I discuss below. The literature on the returns to innovation from non-railroad transportation networks has not focused on market access as a source of the increase; rather these papers all focus on the knowledge channel. Because of this, it is difficult to say whether a market access driven increase in innovation is ignored by these papers or unique to railroads.[21]

1.5 Mechanism

I now present evidence that much of the returns to patenting can be explained through a demand channel: an increase in market access. A rail network lowers freight costs and therefore increases trade and a firm's potential market size. An increase in a firm's market size causes an increase in the firm's expected profits which induces greater R&D investment by the firm because it can spread the fixed costs of that investment across larger returns (Sokoloff 1988; Melitz and Redding 2023). I explore whether this setting provides evidence that an increase in market access increases local innovation.

For this mechanism to be plausible inventors must act as profit maximizing agents, rather than "hobbyist inventors," and they must be able to take advantage of the decrease in freight costs from the rail network. Using full names and patent years, I am able to match 51 patentees to the Oxford Dictionary of National Biography (2024). Of these, 35 (76%) are unambiguously patenting in this period as firm owners or managers within the sector of their firms. These inventors would not gain from a decrease in freight costs if the rail lines were used only to carry goods between the cities, ignoring the freight needs of the small towns in between them. The first intercity line, the Liverpool and Manchester Railway, was built primarily to carry freight between Manchester and the port of Liverpool as the potential of rail with respect to moving people had not yet been fully understood (Wolmar 2008). The 31 mile line included 7 stations between these end points, suggesting that firms built rail lines to facilitate freight movement between the end

[21] Pauly and Stipanicic (2024) include a control for market access as a robustness check, but the authors do not report the coefficient.

points as well as the major destinations they connected. Additionally, the 1845-1846 Railway Returns shows that one third of firms' revenue came from freight, a significant amount suggesting that rail provided a sizeable boost to domestic trade more generally (Railway Commission 1848).

1.5.1 Market Access

Defining Market Access

In order to measure the market size available to firms within a district, I adopt the definition of market access developed by Donaldson and Hornbeck (2016). Market access for a district i is the summation of the product of local GDP j multiplied by a transportation cost function, where j is any district in England not equal to i. As the transportation cost between districts decreases, market access increases.

$$\begin{aligned} MA_{i,t} &= \sum_{j=1}^{J} GDP_j * {c_{ij,t}}^{-\theta} \\ c_{ij,t} &= distance_{ij} * cost_{ij,t} \end{aligned} \tag{1.2}$$

The cost function $c_{ij,t}$ represents the cost of transportation between districts i and j and depends on the distance and transportation options between those districts in each year. I calculate the optimal route between the centroids of each district pair using Dijkstra's algorithm where a journey can occur on roads, railroads, and navigable waterways. The district centroids, railway stations, and points along the navigable waterways[22] are the nodes between which the optimal journey progresses; the waterways, railways, and overland straight line routes are the edges which connect these nodes. The algorithm uses any of these three transportation technologies depending on what is most efficient for each stage of the journey. Because of this, $c_{ij,t}$, and by extension $MA_{i,t}$, can change even if there is no new construction of local rail infrastructure; if the districts between i and j are conveniently connected by rail in year t, the cost of transportation between i and j will decrease accordingly as freight can now move along rail for that part of the journey.

I use the 1851 navigable waterway network data from Satchell and Shaw-Taylor (2018) to map canals. Using static data for the entire time period could cause

[22] Lacking data on canal docks along the navigable waterways, I assign the nearest point on a waterway to the centroids of all districts through which a waterway passes as a waterway node.

market access to be incorrectly measured. However, much of the canal network was constructed in the 18th century. Construction slowed even further in the 19th century as the rail network was developed. Between 1800-1829, there was an annual average of 5.0 canal projects approved by Parliament. In the subsequent 30 year period, there was an average of 2.7 approved canal projects and 57.8 railway projects (Casson 2009).

I assign a value of .36 shillings per ton per mile to move freight along railroads and .54 shillings per ton per mile to move along canals. These values are derived from the freight cost estimates of transportation between Liverpool and Manchester in 1832 (UK Parliament 2024). Movement along roads is assigned a value of 4.99 shillings per mile per ton, a value which I derive from the average 1820-1827 carriage freight rates reported by Bogart (2005).[23] Lastly, I assign a cost of 10.78 shillings to load and unload freight.[24]

I assign the trade elasticity parameter θ a value of 8.22 following Donaldson

[23]The costs that Bogart (2005) reports are incurred by carriages along the turnpike network. I use this cost to approximate the cost along the straight line path between districts, canals, and rail stations. Given the ubiquity of the turnpike network by 1830 (Rosevear et al. 2017), this is unlikely to represent a large deviation from the actual overland freight costs.

[24]I derive this value from the proportional cost between the overland freight costs and loading and unloading costs in Donaldson and Hornbeck (2016), which in turn derives these costs from Fogel (1964) which estimates freight costs for the US economy in the 19th century.

Figure 1.5: Mean change in $c_{ij,t}$ across time where $c_{ij,t}$ is the average cost of moving a ton of goods along the most efficient path between any district pair i and j.

Table 1.3: Summary Statistics for the Market Access Variable

Definition	Median MA 1823	MA Ch. (%) 1823-1861	MA Ch. (%) Rail Treatment
All	2.79e-08	73.42	11.25
—–Top Population	2.98e-07	24.60	0.23
—–Top 1823 MA	6.57e-05	0.23	0.03
—–Top MA Growth	3.22e-09	1,598.66	171.23
Main	1.95e-08	95.40	34.87
—–Top Population	1.82e-08	217.77	42.28
—–Top 1823 MA	1.74e-07	25.13	10.58
—–Top MA Growth	7.45e-09	1,316.52	270.28

Notes: Summary statistics for market access using various subsets of the data. The top half of the table uses data from all districts and the bottom half from districts which are included in the main treatment definition. I restrict these districts to the top quintile by 1831 population, 1823 market access, and 1823-1861 market access growth. Column 1 reports the median market access in 1823; column 2 reports the median market access change between 1823-1861; column 3 reports the median market access change in the year in which a district receives a local rail station. Only districts treated between 1823-1861 are included in column 3.

and Hornbeck (2016). I approximate local GDP using the 1815 district land value (British Parliamentary Papers 1854a).[25] Utilizing values from a year that predates the rail network eliminates endogeneity concerns arising from the rail network inducing local changes in income levels and land values.

Summary Statistics

Rail decreased freight costs dramatically in this period. $c_{ij,t}$ is the freight cost between district pairs across time. Figure 1.5 shows the mean of $c_{ij,t}$ across time. As only the rail data is time varying, the change in freight cost is only due to new rail construction. The average cost of shipping a ton between any district pair in England decreased by 60% from 1840 - the start of the first major rail construction wave - to 1861.

For most districts, market access is a very small number; the median value of market access in 1823 is 2.79e-08. For all regressions I take the log of market access and therefore measure the change in market access. Table 1.3 reports summary statistics: the median market access in 1823 (column 1), the median market access percent change between 1823-1861 (column 2), and the median change in the year in which treated districts receive a local rail station (column 3). The difference in

[25]As a robustness test, I also use the 1821 district level population as a proxy for GDP.

the values of columns 2 and 3 between all districts and districts which are included in the main treatment definition is mainly due to the population composition of the treatment definitions. The districts excluded by the main definition skew more urban. Districts with larger populations will receive a smaller increase in market access than the districts around them because they do not receive the benefit of connecting with their own large market.

For districts included in the main treatment definition, market access doubles between 1823-1861. Market access increases by 35% in the year of treatment compared with the previous year. Districts with the highest starting level market access see disproportionately smaller increases. This largely holds across the entire distribution of starting level market access. There is also a large gap between districts in the top quintile by market access change between 1823-1861 and those in the bottom 80%. The gap suggests that the broader increases in market access are driven by this top quintile. I find evidence that the districts in the top quintile by market access growth are driving the effect on local patenting below.

Figure 1.6 shows that there is a constant increase in market access following a district's connection to the rail network. As the network grows, travel between districts becomes cheaper which increases market access. The period prior to

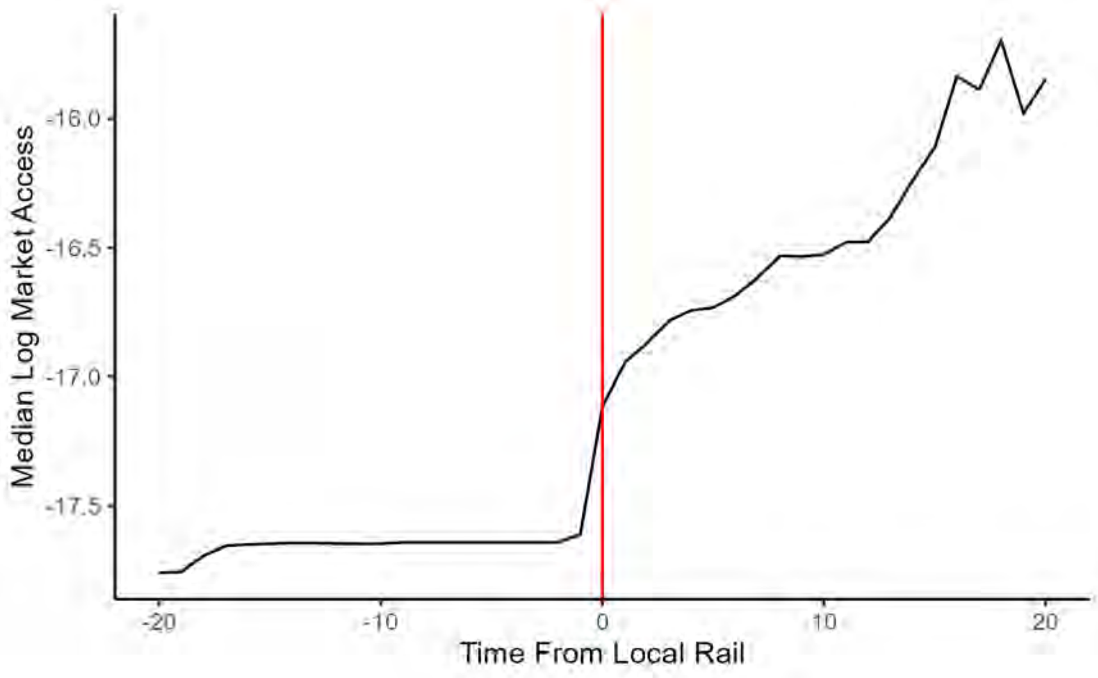


Figure 1.6: Log market access averaged across time from rail station construction in a district. Time zero is the year in which a district received a rail station. Only districts which constitute the main treatment definition are included.

the district's connection year is marked by little to no change in market access. This is likely because overland freight costs were 13 times higher on roads than on railroads - travel to a nearby district's rail station may well have been prohibitively expensive and therefore unlikely to shift market access.

Effect of Market Access on Patenting

To estimate the effect of market access on patenting I modify equation (1.1) by replacing the difference in difference estimator $D_{i,t}$ with $log(MA_{i,t})$:

$$E(Patents_{i,t}) = exp(\alpha_i + \lambda_t + \beta * log(MA_{i,t})) \tag{1.3}$$

The estimation results are presented in Table 1.4 and the baseline result in column (1) indicates a patenting elasticity of .51 with respect to market access. Including the local rail station term $D_{i,t}$ as an independent variable in column (2) only somewhat reduces the baseline estimate to an elasticity of .42. This result demonstrates that the increase in market access is not driven solely by local rail construction. Further, the coefficient for $D_{i,t}$ decreases to .326 from .571 in column (1) of Table 1.2. This implies that 43% of the local rail station effect is explained by the change in local market access, indicating that market access is a major channel.

Local rail stations produced much larger returns to patenting in districts which experienced large market access increases. Dividing districts by their overall 1823-1861 market access growth, I estimate equation (1.1) where all districts in the bottom 80% by market access growth are considered as treated districts in all years following the construction of a local rail station and those in the top 20% are considered as control districts (as are never treated districts). The results are statistically insignificant and with a relatively small coefficient. The reverse of this exercise - considering the top 20% as the treated and the bottom 80% as the control - produces a statistically significant coefficient that is twice as large as the baseline case in Table 1.2. This exercise acts as a heterogeneity test of the market access results more broadly in that districts with a higher level of market access growth experience greater patenting growth. Additionally, these results indicate that rail treatment only increases local patenting if there is a concurrent large increase in market access; a local rail station must serve an important function for it to cause an increase in patenting.

Table 1.4: Effect of Market Access on Innovation

	(1)	(2)	(3)	(4)	(5)
Log Market Access	0.511***	0.427***			0.145**
	(0.130)	(0.141)			(0.066)
Rail		0.326**	0.120	1.120***	
		(0.164)	(0.190)	(0.311)	
Observations	6864	6864	6864	6864	18642
Districts	176	176	176	176	478
Years	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861
Definition	Main	Main	Main	Main	All

Notes: Columns (1), (2), and (5) report the results of a Poisson estimation of equation (1.3) where the dependent variable is the log of patents per capita in district i and year t and the main independent variable is log market access ($MA_{i,t}$). Population is measured as an exposure variable. Column (2) adds the rail indicator $D_{i,t}$ which is equal to one for a district i for all years in which i is connected to the rail network. Column (5) includes all districts - all other columns use the main treatment definition. Columns (3) and (4) estimate equation (1.1) where the main independent variable is the rail indicator $D_{i,t}$. In column (3), all districts which are treated but which receive an increase in market access between 1823-1861 that is in the top 20% of all districts are assigned as control districts for all years and the bottom 80% are considered treated. In column (4) districts in the bottom 80% are assigned as the control. In both columns, the treated districts receive treatment in the year in which they receive a rail station. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Endogeneity

A feature of the market access definition is that it does not rely only on local rail construction for variation. Variation arises from changes to the entire transportation network linking districts i and j. Decisions regarding rail construction in district j or any district between i and j will increase market access in district i and are plausibly orthogonal to changes in district i.

Column (2) of Table 1.4 reports the coefficient estimate of log market access when controlling for changes in the local rail status of district i. The control only slightly lowers the coefficient on log market access, indicating that it is indeed the rail network beyond district i rather than the construction of a local station that causes much of the increase in local patenting.

If changes in districts' market access are orthogonal to changes in those districts' railroad infrastructure, it may no longer be necessary to restrict locations to those which comprise the main treatment definition. Including all districts in

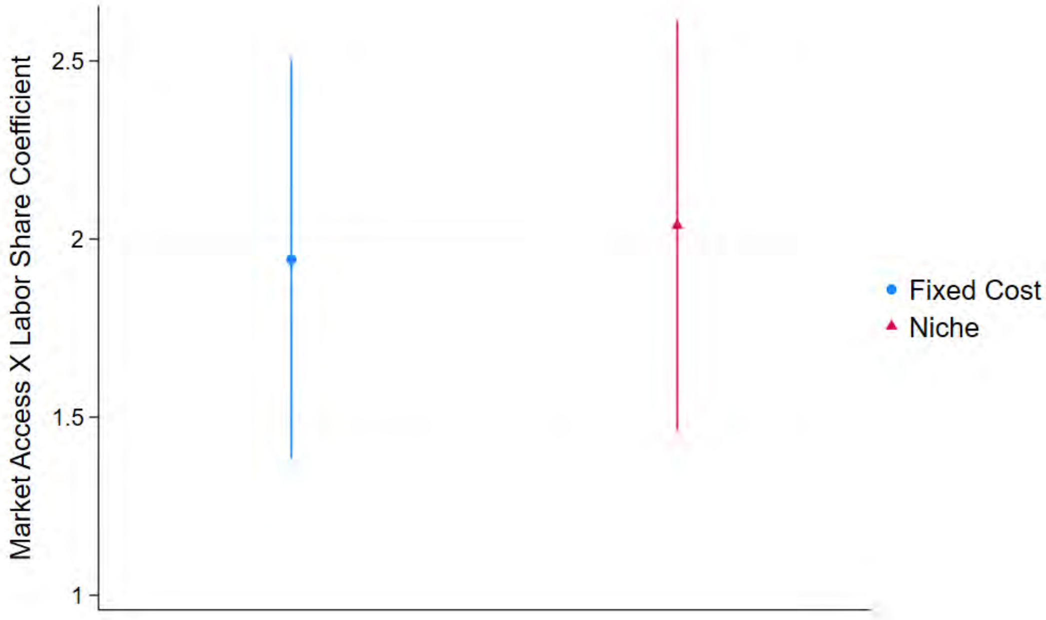


Figure 1.7: Coefficient estimates of equation (1.3) where the dependent variable is the number of patents and the main independent variable is log $MA_{i,t}$ and the interaction between log $MA_{i,t}$ and the 1851 share of labor in high fixed, low variable cost sectors (left) or niche sectors (right). Only the interaction coefficient estimates are included in the graph. Only districts which are included in the main treatment definition are included. Errors are clustered by district.

England produces a smaller coefficient of .15 (column 5). This result is less than a third of the size of the baseline coefficient. Much of this discrepancy can be attributed to the fact that the main treatment definition includes few cities and a smaller proportion of districts close to canals. Removing districts in the top decile by total 1815 income level (i.e., large local markets) or with immediate access to the main inland navigable waterway[26] increases the coefficient to .33.[27] It is plausible that places that are isolated from large markets and with poor transportation options can engage in high "catch-up growth" in patenting due to the big jump in market access.

Sector Heterogeneity

The theory underlying these empirical results is that a larger market spurs innovation because it spreads R&D costs over a larger customer base (Sokoloff 1988). I empirically test this theory by examining sector heterogeneity. Figure 1.7

[26]Access is defined as the waterway passing within the geographic boundary of a district.

[27]Excluding all districts with any waterway access, rather than just access to the main waterway, produces an almost identical result, but the regression includes 47 fewer districts.

shows the coefficient estimates of regressing patents on the interaction between market access and the 1851[28] labor share of two sector groups: high fixed, low variable cost sectors and niche product sectors.

Districts that are more exposed to high fixed cost, low variable cost sectors experience a larger increase in patenting than other districts as they gain more from the increase in R&D cost spreading and lowered transportation costs.[29] The coefficient estimate is large and statistically significant, implying that the high fixed, low variable costs sectors are driving the returns to patenting.

A niche product is more likely to require shoppers to travel greater distances than a general good because it is sold at fewer locations (Brynjolfsson et al. 2003). An increase in market access is analogous to a decrease in this higher transportation cost for consumers. I define niche sectors using the 1851 national labor shares. Any sector for which only five districts make up at least 25% of the sector's national labor force are considered niche. As in the case with high fixed, low variable cost sectors, the coefficient estimate of the interaction between the 1851 niche sector labor share and market access is statistically significant.

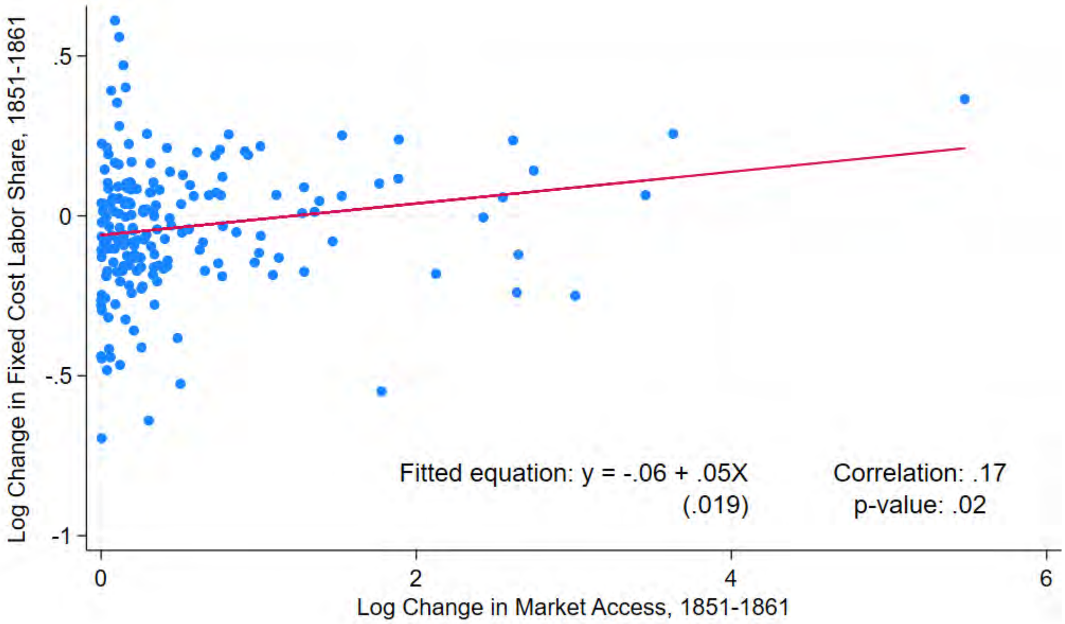
These sectors also exhibited a small increase in labor, suggesting that there was an increase in demand for their products and is consistent with the the firms increasing expenditures. Figure 1.8 shows a scatter plot of districts' log change in market access between 1851-1861 and the concurrent log change in the high fixed cost or niche labor share. The relationship is generally positive though with a small coefficient.

To further examine sector heterogeneity in the market access term, I construct a sector level definition of market access using an input-output table for 1841 England compiled by Horrell et al. (1994). The drawback is that I am forced to use the 13 sectors defined by Horrell et al. (1994), rather than the 55 sectors I defined to precisely allocate patents. There is no clear designation of high fixed cost or niche sectors among the Horrell et al. (1994) definitions. Instead, I turn to the authors' designation of manufacturing sectors as sectors which are plausibly more affected by an increase in market access.[30] Manufacturing firms can reasonably be expected to gain disproportionately from the rail network as these firms can more easily ship their goods via rail than firms in other sectors, such as "gas and

[28]This is the earliest year of labor data in the English Census.

[29]I designate the following sectors as high fixed cost, low variable cost: metallurgy, energy and gas manufacture, distilling, infrastructure, photography, chemistry, and all textile sub-sectors.

[30]These sectors are: construction, food, drink, and tobacco, gas and water, metal goods, metal manufacture, mining and quarrying, other manufacturing, soap, candles, and dyes, and textiles, clothing, and leather goods.



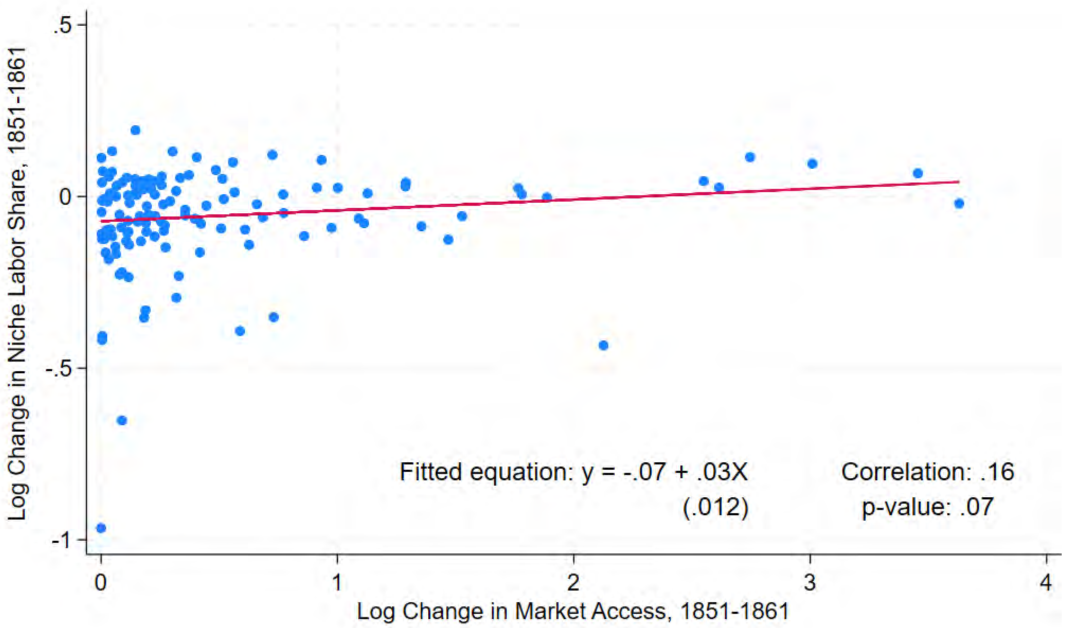


Figure 1.8: Scatter plot of the relationship between districts' log change in market access between 1851-1861 and the 1851-1861 log change of a district's high fixed, low variable cost labor share in the top graph and the log change in the niche labor share in the bottom graph. Only districts which are included in the main treatment definition and which have at least 100 workers in the given sector groups in both 1851 and 1861 are included. The pairwise correlation coefficients and its p-values are included in the bottom right hand corner of each graph and the fitted line equations are included at the bottom.

water."

I denote the sectors from the input-output table as $\overline{s}$. Sectors are mapped from the 55 sectors I assigned to each patent and occupation in the census micro-data to the 13 sectors defined by Horrell et al. (1994). Each district-sector receives input values from all other district-sectors which are then assigned as weights to local GDP, which is proxied by the 1851 local labor force:

$$MA_{i,\overline{s},t} = \sum_{j=1}^{J} \sum_{\overline{k}=1}^{\overline{K}} Labor_{j\overline{k}1851} * (\frac{input_{\overline{k}\overline{s}}}{\sum_{\overline{k}=1}^{\overline{K}} input_{\overline{k}\overline{s}}}) * c_{ij,t}^{-\theta} \tag{1.4}$$

Market access for district i, sector $\overline{s}$, and year t is defined as the sum of sector $\overline{k}$ labor within each district j not equal to i multiplied by the share of total inputs into sector $\overline{s}$ that $\overline{k}$ constitutes and by the cost function $c_{ij,t}^{-\theta}$, which is unchanged from equation (1.2).

I use labor data from the 1851 Census to obtain local labor in district j. Since this is the first census to include occupational responses, it is the earliest year in which I can obtain these values. I restrict the regressions to 1851-1861 in order to minimize endogeneity concerns. Column (1) of Table 1.5 reports the coefficient estimate obtained when restricting the baseline regression of $MA_{i,t}$ on patenting to these years in order to provide a comparison when using $MA_{i,\overline{s},t}$.

I modify equation (1.3) by incorporating sectors to both the left and right hand sides of the equation such that the log of $MA_{i,\overline{s},t}$ is regressed on the log of patents in district i and sector $\overline{s}$ to obtain the effect of $MA_{i,\overline{s},t}$ on within sector patenting.

$$E(Patents_{i,\overline{s},t}) = exp(\alpha_i + \lambda_t + \beta * log(MA_{i,\overline{s},t})) \tag{1.5}$$

The estimation results in column (2) of Table 1.5 indicate that the within sector elasticity of patenting per capita with respect to market access is .40. This is marginally larger than the sector-less baseline estimate of .35 in column (1). The larger coefficient provides confirmation that the incorporation of the input-output table yields additional explanatory variation as the inputs act as weights on local GDP, though the small magnitude of the increase is likely because the 19th century English economy was not particularly inter-connected. Indeed, the input-output table is quite sparse.

Column (3) reports the interaction between log market access and an indicator equal to one if the 1851 sector $\overline{s}$ share of labor in district i is in the top quintile by all districts' sector $\overline{s}$ share of labor and zero otherwise. The positive coefficient indicates that patents were disproportionately affected by the increase in market

Table 1.5: Heterogenous Effect of Market Access on Patenting Across Sectors

	(1)	(2)	(3)	(4)	(5)
Log Market Access	0.349**	0.401***	0.281***	0.286***	0.164***
	(0.161)	(0.144)	(0.080)	(0.086)	(0.049)
Log Market Access X 1851 Lab. Sh.			0.291***		
			(0.095)		
Log Market Access X Manuf. Sector				0.219*	
				(0.119)	
Log Market Access X Manuf. 1851 Lab. Sh.					0.672*
					(0.345)
Observations	1896	20856	20856	20856	20856
Districts	158	158	158	158	158
Years	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861

Notes: Column (1) estimates equation (1.3) where the time period is restricted to 1851-1861 where patents are not at the sector level and the main independent variable is $MA_{i,t}$. All other columns estimate equation (1.5) which uses log $MA_{i,\overline{s},t}$ as the main independent variable and log patenting per capita in district i, sector $\overline{s}$, and time t as the dependent variable. Columns (3)-(5) include an interaction term between log $MA_{i,\overline{s},t}$ and an indicator; the indicator in column (3) is equal to one if the 1851 sector $\overline{s}$ share of labor in district i is in the top quintile by all districts' share of sector $\overline{s}$ labor and zero otherwise; the indicator in column (4) is equal to one if $\overline{s}$ is a manufacturing sector; the indicator in column (5) is equal to one if district i is in the top quintile by districts' 1851 manufacturing share of labor. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

access in sectors for which there were enough workers to ramp up production. Seen another way, if London produces no output of textiles, an increase in market access does not produce an increase in textile patents.

In column (4) I estimate the interaction between log $MA_{i,\overline{s},t}$ and an indicator equal to one if sector $\overline{s}$ is a manufacturing sector to estimate the differential effect on patenting of manufacturing against all other sectors and find that manufacturing sectors experience larger returns to patenting. Repeating the exercise with districts in the top quintile by their 1851 manufacturing labor share shows (in column 5) that these "manufacturing towns" drive the increase in patenting.

Core Robustness Exercises

The results in this section are robust to a variety of tests, which are presented in the Appendix. Adjusting the parameter θ to either end of the range proposed by Donaldson and Hornbeck (2016) only slightly lowers the coefficient estimates for the main results in Table 1.4, and the results remain statistically significant. Using the 1821 population of district j as a proxy for GDP, rather than the 1815 land values, when defining market access similarly does not meaningfully change the results. Using stacked decadal differences instead of a difference-in-difference estimation framework produces a larger coefficient, but the results are still roughly similar. Systemically removing each of the ten census divisions (each of which represents a geographic area of England) does not greatly alter the estimation results, indicating that no single area is driving the results.

Aggregate Effects

A simple back of the envelope calculation shows that the change in market access due to rail caused a 48.55% increase in aggregate patenting. I calculate this by taking the median change in market access between 1823-1861 (95%) and multiplying by the baseline coefficient of .511 in column (1) of Table 1.4. Using figures from all districts, rather than those in the main treatment definition implies a 10.65% increase in aggregate patenting (from a 73% increase in market access and a .145 estimation coefficient). Using data from OECD countries between 1973-1996, Furman et al. (2002) find that the elasticity of TFP with respect to patents is .11. Multiplying 48.55% and 10.65% by 11% respectively produces 5.34% and 1.17% increases in aggregate TFP. These are naive estimates designed to produce an idea of aggregate, static results and make no attempt to estimate general equilibrium effects. Additionally, the elasticity of TFP could well be different in 19th Century England than in the latter half of the 20th century.

1.5.2 Other Mechanisms

This paper focuses on the market access channel to explain the increase in patenting from a local rail station. However, other channels may also explain some of the local station effect. I present evidence here that an increase in knowledge flows arising from easier communication due to the railways is a plausible mechanism and that it is likely distinct from the market access channel. I also show that I find no evidence that the increase in local patenting is due to inventors moving to districts with rail stations.

Knowledge Flows

Previous research on the effect of railroads on innovation has focused on the diffusion of technology, rather than an increase in innovation (Juhász and Steinwender 2023). However, research on other types of transportation networks has found knowledge flows to be a mechanism driving local increases in innovation (Agrawal et al. 2017; Bottasso et al. 2022; Pauly and Stipanicic 2024). I present evidence for the existence of this channel in a railroad setting to establish that this is a plausible mechanism for the results described in Section 1.4.

A transportation network can increase knowledge flows because the network, in lowering travel costs, decreases the cost of face to face communication. This then induces innovation (Storper and Venables 2004). Rail construction had an immediate impact on individuals' movement. The first intercity rail line between

Table 1.6: Effect of Local Rail on Wealthy Districts and Districts with No Prior Patenting

	(1)	(2)	(3)	(4)
Rail	0.588***	0.652***	0.571***	0.342**
	(0.152)	(0.160)	(0.139)	(0.174)
Rail X Top Income		-0.606**		
		(0.289)		
Rail X No Patents				0.852***
				(0.261)
Observations	5304	5304	6864	6864
Districts	136	136	176	176
Years	1823-1861	1823-1861	1823-1861	1823-1861
Definition	Main	Main	Main	Main

Notes: Poisson regression estimation of equation (1.1) where the dependent variable is patents per capita in year t and district i and the main independent variable is an indicator equal to one if district i has a rail in year t. In column (2) the rail indicator $D_{i,t}$ is interacted with an indicator equal to one if district i is in the top quintile by 1843 income per capita. In column (4) the rail indicator is interacted with an indicator equal to one if district i had no patents in any year between 1823-1839. Columns (1) and (2) exclude districts treated before 1843 to ensure that the income variable does not reflect changes due to rail. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Manchester and Liverpool opened in 1830 and was almost 40% faster than the average speed on turnpike roads. Within three months of the line's opening, over half of the stagecoach companies along this route ceased running, even as the rail line carried over 50% more daily passengers than the stagecoach companies had combined (Wolmar 2008). Following the 1844 Railway Regulation Act, affordable tickets were mandated by law on every route (Shaw-Taylor and You 2018).

Before establishing a formal definition of knowledge flows to measure the railroad's impact, I first highlight three pieces of evidence that support the notion that the railroad decreased communication costs. First, unlike poorer districts, districts that were in the top quintile by income per capita saw no benefit from a local rail station (Table 1.6). This finding suggests that wealthy individuals could engage in distant communication even when it was costlier in the years before rail. Second, districts that were isolated from innovation - those with no patents between 1823-1839 - saw the biggest increase in patenting due to a local rail station. Individuals in these districts would have had to incur additional costs to travel outside of their towns to find inventive minds with whom they could discuss new ideas. Third, districts that were served by lines that carried more individuals saw

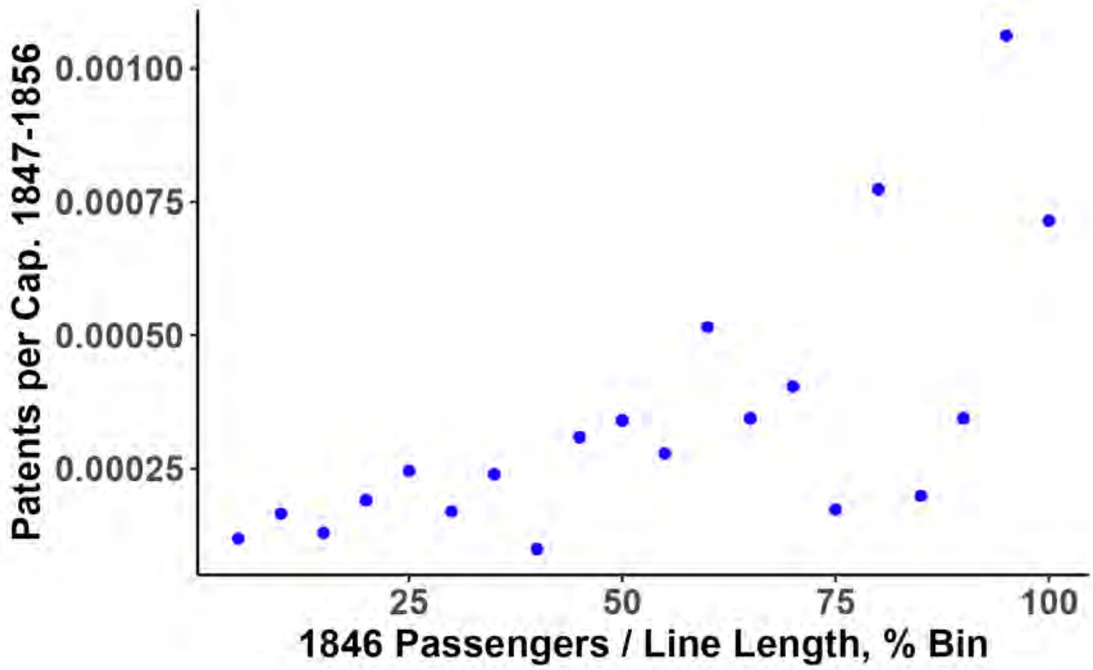


Figure 1.9: The x-axis shows the number of passengers in the 12 months period ending in June 1846 in a district divided by the total length of the rail line in each district. Districts are aggregated into five percentage point bins. The y-axis shows the mean number of patents per capita between 1847-1856 for each bin. The data exclude districts treated after 1846.

a higher rate of future patenting (Figure 1.9). If ideas are produced by communication between individuals, it is reasonable that an increase in the total amount of potential communication between unique individuals leads to a higher rate of inventions.

Many papers follow Jaffe et al. (1993) and use patent citations to measure knowledge flows (for example, Agrawal et al. 2017; Atkin et al. 2022; Pauly and Stipanicic 2024). Patent citations were not recorded in the time period I study so I adapt my definition of market access to construct a variable to measure knowledge flows:

$$\begin{gathered} KF_{i,s,t} = \sum_{j=1}^{J} Patents_{j,s} * {c_{ij,t}}^{-\theta} \\ c_{ij,t} = distance_{ij} * cost_{ij,t} \end{gathered} \tag{1.6}$$

The key difference between this definition and the market access definition described by equation (1.2) is that rather than including the sum of local GDP (as proxied by local land values) in other districts j, I include here the number of sector s patent applications in those districts. This proxies their inventive capacity so that cheaper travel between i and j produces an increase in knowledge flows into sector s only if district j has inventors with the relevant knowledge. The sector level brings this definition of knowledge flows closer to patent citations and provides greater precision to the estimation. A distiller who becomes more easily connected to a district filled with textile inventors is unlikely to receive any knowledge flows that will improve the distilling process. However, connecting the distiller with other inventive distillers may foster relevant knowledge flows. $Patents_{j,s}$ is the cumulative total sector patents in a district between 1823-1838. Using a 16 year time frame allows for enough variation across districts despite the sparsity of the patent data.[31] I use 1839 as the cutoff as this is the year in which the first major rail expansion begins. This cutoff limits districts' exposure to the treatment in the calculation of $KF_{i,t}$ as the rail network was still in its infancy. Regressions which include $KF_{i,t}$ use 1839 as the starting point to limit this potential endogeneity. I estimate the following equation and report the results in Table 1.8:

$$E(Patents_{i,s,t}) = exp(\alpha_i + \lambda_t + \beta * log(KF_{i,s,t})) \tag{1.7}$$

[31] 277 of the 541 districts in England have at least one patent during this period.

Table 1.7: Knowledge Flows Summary Statistics

Definition	**Median KF 1839**	**KF Ch. (%) 1839-1861**	**KF Ch. (%) Rail Treatment**
All	5.91e-16	$2,915.23$	338.94
All - Top Population	1.35e-14	531.49	28.17
All - Top 1839 KF	1.1e-12	11.36	1.55
All - Top KF Growth	3.65e-19	$1,085,759.00$	$1,411.42$
Main	2.81e-16	$4,235.80$	437.07
Main - Top Population	1.77e-16	$9,013.53$	476.92
Main - Top 1839 KF	7.06e-14	104.89	14.33
Main - Top KF Growth	4.15e-19	$721,561.10$	$1,180.49$

Notes: Summary statistics for the knowledge flows variable using various subsets of the data. The top half of the table uses data from all districts and the bottom half from districts which are included in the main treatment definition. I restrict these districts to the top quintile by 1831 population, 1839 knowledge flows, and 1839-1861 knowledge flows growth. Column 1 reports the median knowledge flows in 1839; column 2 reports the median knowledge flows change between 1839-1861; column 3 reports the median knowledge flows change in the year in which a district receives a local rail station. Only districts treated between 1839-1861 are included in column 3.

Table 1.8 column (1) shows that the within sector patenting elasticity with respect to knowledge flows is .43. Aggregating the data to the sector $\bar{s}$ level (which reduces the number of sectors from 55 to 13) in column (2) produces a coefficient nearly twice as large. While this could be due to the decrease in sparsity of patents for many sectors, there is also a clear loss of precision with regards to the knowledge flows due to the more aggregated level.

Including sector fixed effects decreases the coefficient from .427 to .185 in column (3). The sparsity of the data could be driving the decrease in magnitude; many sectors have few patents for the entirety of the period throughout all of England. The more aggregated sector $\bar{s}$ level produces a smaller difference in coefficients of .828 (column 2) and .576 (column 4).[32]

Including all districts of England in column (5) reduces the effect of $KF_{i,s,t}$ on within sector patenting to .093. This is unsurprising as knowledge appears to flow from cities and towards large towns and the districts which comprise the main treatment definition are less urban.

Controlling for district i's local rail status in column (7) only marginally re-

[32] Additionally, restricting the sector s data to only sectors in the top half by count of patents between 1823-1838 eliminates most of this difference, producing coefficients of .336 without sector fixed effects and .240 with fixed effects.

Table 1.8: Effect of Knowledge Flows on Patenting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Knowledge Flows	0.427***	0.820***	0.185**	0.576***	0.077***		0.179**
	(0.062)	(0.097)	(0.075)	(0.220)	(0.019)		(0.075)
Rail						0.466***	0.299*
						(0.148)	(0.169)
Observations	203573	49933	188209	49933	566812	188209	188209
Districts	167	167	167	167	465	167	167
Years	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	Main	Main	All	Main	Main
Sector FE			Y	Y	Y	Y	Y

Notes: Results of the Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Columns (3) - (7) include sector fixed effects. Columns (2) and (4) are estimated at the more aggregated sector $\overline{s}$ level. Columns (5) includes all districts while all other columns include only districts which comprise the main treatment definition. Column (6) includes the rail treatment variable $D_{i,t}$ as the main independent variable and column (7) includes both $KF_{i,s,t}$ and $D_{i,t}$. As $KF_{i,s,t}$ is calculated using patents from 1823-1838, I restrict the data to 1839-1861 in all columns. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

duces the coefficient. The coefficient on the local rail station term decreases from .466 in column (6) to .299 in column (7) when including log knowledge flows, indicating that 35.8% of the rail station effect on within sector patenting can be explained by knowledge flows.

Knowledge flows produce disproportionately higher returns to patenting in districts which occupy the top quintile by 1831 population. I find this result by interacting log $KF_{i,s,t}$ with an indicator for districts in the top quintile (Table 1.9). Given that cities are mostly excluded from the main treatment definition, these districts can be considered to be "large towns." It is unsurprising that districts with the highest population receive the largest gains. Having a larger number of individuals increases the chances that someone in the district is a potential inventor who can benefit from the increased knowledge flows. I find further evidence of this when interacting log $KF_{i,s,t}$ with the top quintile by 1851 sector s labor. The positive result shows that it is necessary to have a large number of people with expertise in the given field to produce returns to innovation. In other words, knowledge flows require a large knowledge base in the relevant sector to foster innovation. Districts in the top quintile by population are likelier to have a large

Table 1.9: Effect of Knowledge Flows on Patenting - Population and Labor Interactions

	(1)	(2)	(3)
Log Knowledge Flows	0.185**	0.108**	0.086*
	(0.075)	(0.050)	(0.052)
Log Knowledge Flows X Top Pop.		0.190**	
		(0.090)	
Log Knowledge Flows X Top Lab.			0.139***
			(0.053)
Observations	188209	188209	188209
Districts	167	167	167
Years	1839-1861	1839-1861	1839-1861
Definition	Main	Main	Main
Sector FE	Y	Y	Y

Notes: Results of the Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Column (2) includes the interaction between log $KF_{i,s,t}$ and an indicator variable equal to one if a district is in the top quintile by 1831 population. Column (3) includes the interaction between log $KF_{i,s,t}$ and an indicator variable equal to one if a district's 1851 labor in sector s is in the top quintile (for that sector). Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

enough knowledge base as they simply have more people. Additionally, the labor composition of these districts is over-indexed in the sectors in which they produce a patent, further pointing towards the benefits of a large, relevant knowledge base.

It is plausible that sectors with relatively new technology are disproportionately affected by an increase in face-to-face communication as the knowledge in these sectors has not yet fully disseminated. For example, someone seeking to work in AI is likely to learn the most from people who are working at the cutting edge of the field rather than from a textbook that might be out of date within five years of publishing. The same is unlikely to be true for an older field such as plumbing. I find that sectors which grew the most by stock of patenting between 1823-1841 and 1842-1861 experienced a disproportionately large effect from knowledge flows. The effect is present in sectors above the median by patent growth and more pronounced in sectors in the top quintile.

I also find that sectors which have a culture of sharing ideas are disproportion-

Table 1.10: Effect of Knowledge Flows on Fast Growing and Knowledge Sharing Sectors

	(1)	(2)	(3)	(4)	(5)	(6)
Log Knowledge Flows	0.370***	0.394***	0.418***	0.130**	0.130**	0.177**
	(0.059)	(0.054)	(0.064)	(0.062)	(0.059)	(0.075)
Log Knowledge Flows X Growth (Median)	0.078*			0.081*		
	(0.043)			(0.043)		
Log Knowledge Flows X Growth (Quintile)		0.109***			0.183***	
		(0.040)			(0.051)	
Log Knowledge Flows X Knowledge Sharing			0.176*			0.172*
			(0.100)			(0.095)
Observations	203573	203573	203573	188209	188209	188209
Districts	167	167	167	167	167	167
Years	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	Main	Main	Main	Main
Sector FE				Y	Y	Y

Notes: Results of Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Columns (1) and (4) include the interaction of $KF_{i,s,t}$ with sectors above the median by patent growth and columns (2) and (5) the sectors in the top quintile by patent growth. Columns (3) and (6) include the interaction between $KF_{i,s,t}$ and the metallurgy and dyes sectors. Columns (4)-(6) include sector fixed effects. As $KF_{i,s,t}$ is calculated using patents from 1823-1839, I restrict the data to 1840-1861 in all columns. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ately affected. The iron industry in England relied on "collective invention" during this period - the practice of competitors openly sharing secrets with each other (Allen 1983). The dyes industry relied on secrecy to prevent knowledge extraction rather than patenting (Moser 2013). In both cases, an increase in face-to-face communication allows outsiders to travel to a sector hub, learn trade secrets, and return home with insider knowledge. The interaction between log $KF_{i,s,t}$ and an indicator equal to one if sector s is either metallurgy or dyes produces a positive coefficient indicating that these sectors are indeed more likely to see a higher rate of patenting than other sectors due to an increase in knowledge flows.

As a robustness check (results are in the Appendix), I recreate the main results in Table 1.8 using a five year rolling aggregate of patents in the definition of $KF_{i,s,t}$, rather than the static stock of patents between 1823-1839, such that $KF_{i,s,1845}$ uses all sector s patents between 1840-1844. While this ignores potential endogeneity concerns in that the rolling stock of patents which comprises $KF_{i,s,t}$ is produced in districts which are increasingly treated by rail stations, it is a more realistic view of knowledge flows. An inventor in 1860 is more likely to improve upon an invention from the previous few years than upon an invention from 1830. The results are similar to the main results.

As in the case with market access, systematically removing every district in one of the ten Census Divisions does not meaningfully change the results, indicating a lack of evidence that knowledge flows have a geographic component.

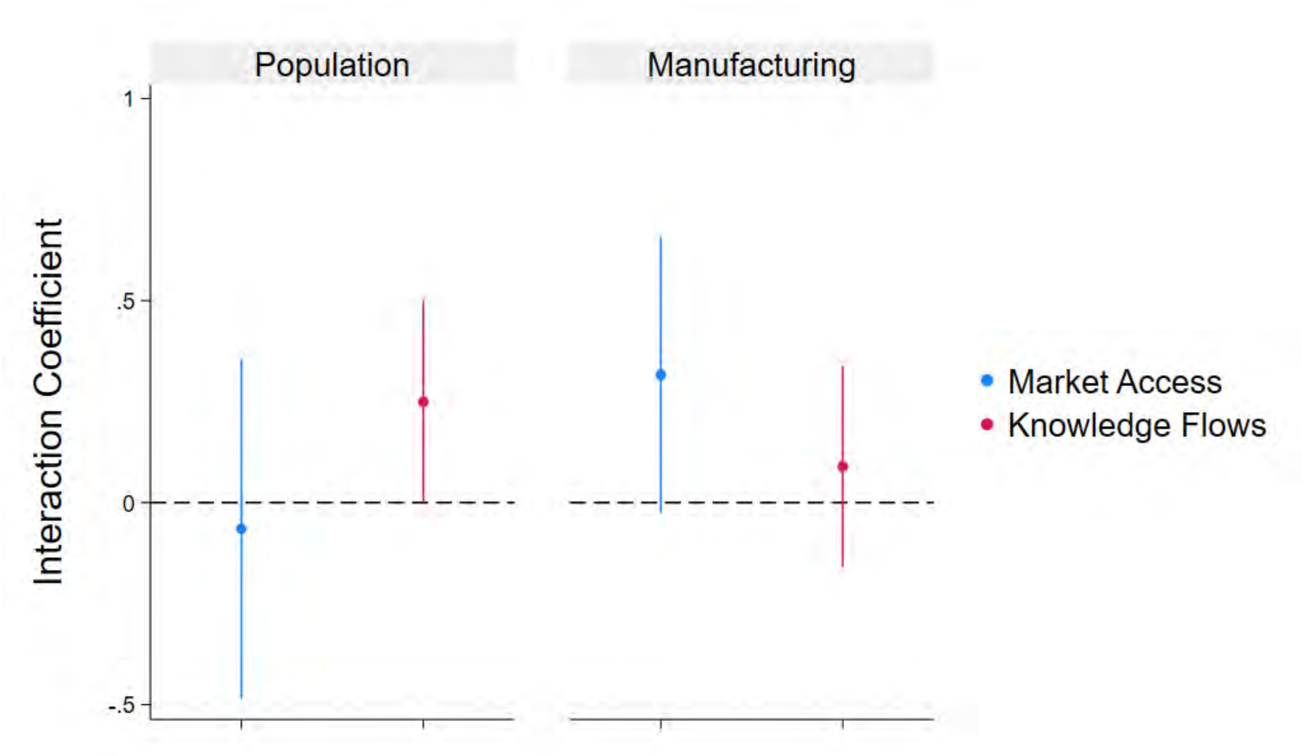


Figure 1.10: The left graph shows the coefficient of the interaction between an indicator equal to one for districts in the top half by 1831 population and either $MA_{i,t}$ (in blue) or $KF_{i,s,t}$ (in red). The right graph shows the same interaction using the top half by the 1851 manufacturing share of labor in a district. Halves are calculating using districts which comprise the main treatment definition.

Disentangling of Market Access and Knowledge Flows

The market access and knowledge flows channels appear distinct. However, serial correlation is very high due to variation across time being driven entirely by changes to the rail network which creates difficulties in disentangling the variables. I present three pieces of evidence that these mechanisms measure distinct effects.

First, the market access and knowledge flows channels' largest effects go to different districts. In Sections 1.5.15 and 1.5.2 I showed that an increase in market access disproportionately affects patenting in districts with high manufacturing employment shares and knowledge flows disproportionately affects large towns. The reverse does not hold; the interaction term between $MA_{i,t}$ and a dummy variable equal to one for districts in the top half by 1831 population is essentially zero (Figure 1.10). The same is true for the interaction between $KF_{i,s,t}$ and a dummy for districts in the top half by the share of labor employed in manufacturing. This finding implies both that the channels affect different types of districts and that the channels' largest effects do not overlap.

Second, the correlation between the 1839-1861 log change in KF_i and log change in MA_i is negative and statistically insignificant. A lack of a relationship

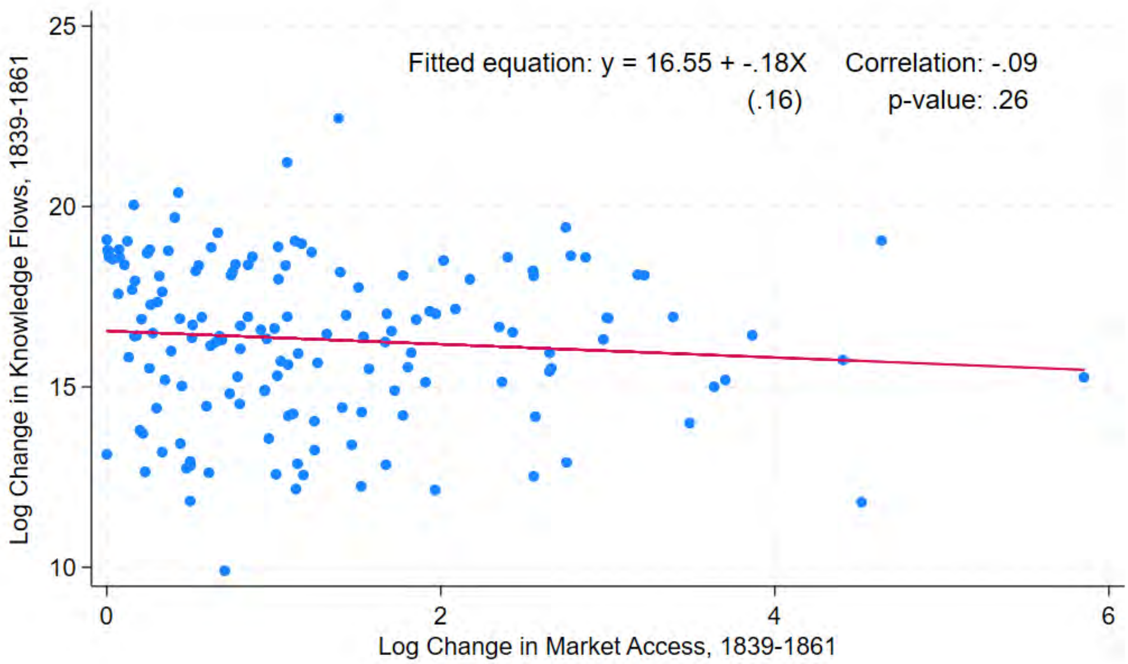


Figure 1.11: Districts' 1839-1861 log change in MA_i plotted against the 1839-1861 log change in KF_i. Only districts treated by 1861 and which are included in the main treatment definition are included.

between these variables (when removing the serial time component) suggests that they are measuring different effects. The difference between the variables stems from the different weights that they place on the cost to travel between districts: other districts' GDP for market access and patents for knowledge flows. Population and patents are only somewhat correlated in this time period as half of the districts have zero patents between 1823-1838, though they clearly have a positive population. The lack of correlation between KF_i and MA_i could therefore perhaps be mechanical; given a positive number of patents across all districts the correlation might be positive. However, it is equally possible that in the first half of the 19th century, many towns had zero inventors (potential or otherwise) and connecting with that town would not produce any positive knowledge flows.

Third, access to a large knowledge base is not necessary for a change in market access to increase patenting as I show in Table 1.11. Similarly, knowledge flows do not require a large market. Interacting $MA_{i,\bar{s},t}$ with districts in the top quintile by the 1851 starting point knowledge flows does not yield a statistically significant coefficient. The reverse is also true. This again points to a lack of a relationship between the two channels. A plausible explanation for this finding is that the districts in which market access causes an increase in innovation already have all the knowledge they require to innovate, but are missing the market size to engage in high fixed cost R&D spending. This explanation fits with the result that high fixed cost, low variable cost sectors benefit most from an increase in market access (Figure 1.7) and with the finding that districts with a high sector $\bar{s}$ labor share produce greater returns to patenting from an increase in market access (Table 1.5 column 3). Another plausible explanation is that by 1851 many places with isolated markets or no stock of patents had been connected to the rail network and had already reaped the benefits and engaged in catch-up growth, flattening the difference in market access and causing all districts to have "large enough" markets to support the commercialization of incoming knowledge flows. However, $KF_{i,\bar{s},t}$ can reasonably be measured as far back as 1839, prior to the first wave of large scale rail construction. Using the 1839 value to calculate the top quintile does not change this result, suggesting that these findings are not due to catch-up growth.

Market access and knowledge flows are strongly correlated within the panel as variation across time for both variables is due solely to changes in the rail network. Including both $MA_{i,\bar{s},t}$ and $KF_{i,\bar{s},t}$ in the same regression produces statistically significant effects, but the high serial correlation means that the estimates are unlikely to be robust and should be taken with caution. Beyond the correlation

Table 1.11: Effect of Market Access and Knowledge Flows on within Sector Patenting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Market Access	0.401***	0.340***	0.300***			0.271***	0.160***
	(0.144)	(0.074)	(0.066)			(0.077)	(0.028)
Log Market Access X Top KF Quintile		0.127	0.140				
		(0.254)	(0.216)				
Log Knowledge Flows				0.883***	0.872***	0.826***	0.887***
				(0.114)	(0.069)	(0.091)	(0.025)
Log Knowledge Flows X Top MA Quintile					0.013		
					(0.152)		
Observations	20856	20856	20856	20856	20856	20856	59268
Districts	158	158	158	158	158	158	449
Years	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861
Definition	Main	Main	Main	Main	Main	Main	All
Pseudo R2	0.238	0.263	0.272	0.317	0.319	0.326	0.643

Notes: Poisson estimation where the dependent variable is the log of sector $\overline{s}$ patents per capita and the main independent variables are log $MA_{i,\overline{s},t}$ and log $KF_{i,\overline{s},t}$. Columns (2) and (3) include the interaction between log $MA_{i,\overline{s},t}$ and districts in the top quintile by 1851 and 1840 $KF_{i,\overline{s},t}$ respectively. Column (5) includes the interaction between log $KF_{i,\overline{s},t}$ districts in the top quintile by 1851 $MA_{i,\overline{s},t}$. Column (6) includes both log $MA_{i,\overline{s},t}$ and log $KF_{i,\overline{s},t}$ as regressors. Columns (7) includes all districts and all other columns include only districts which comprise the main treatment definition. The time period is restricted to 1851-1861 due to the use of labor data from the 1851 Census in the definition of $MA_{i,\overline{s},t}$. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

issues, there are also data constraints; the coefficient estimates when regressing knowledge flows on patenting are much larger when using $KF_{i,\overline{s},t}$ instead of $KF_{i,s,t}$, yet, as market access cannot be measured at the sector s level (because it requires the input-output table which is at the sector $\overline{s}$ level), a comparison between how the two knowledge flows definitions compare with market access cannot be made.

Movement of Innovation

It is possible that railroads do not increase aggregate innovation, but rather move innovation from one location to another by displacing inventors.[33] I do not find evidence that these negative spillovers are driving the innovation effects described in Section 1.4.

I modify equation (1.1) such that $D_{i,t}$ is equal to one if at least one district within 50KM of district i has a station in year t and remove observations for years in which district i has a rail station. In essence, this regression shows whether there is an outflow of patenting from district i when its residents can relocate to a nearby district with a station before their district is treated. The estimated coefficient in column (1) of Table 1.12 is statistically indistinct from zero, indicating a lack of evidence for such spillovers.

In column (2) I estimate the direct effect of a district's station on its neighbors. I again estimate a modified form of equation (1.1) where the dependent variable is now the mean number of patents per capita of a district's neighbors.[34]

$$E(\overline{Patents_{j,t}}) = exp(\alpha_i + \lambda_t + \beta D_{i,t}) \tag{1.8}$$

The difference-in-difference estimator $D_{i,t}$ is equal to one for all years in which district i has a local rail station. Neighboring districts j are only included as part of the mean patents calculation until they are treated. Neighbors which are treated prior to district i are always excluded. The coefficient is close to zero and statistically insignificant. I interpret this to mean that there is essentially no effect on the patenting activity of the neighboring districts.

In column (3) I examine the movements of the inventors themselves. I remove all patents by inventors who move between districts. Movers are identified as inventors whose full name is linked to multiple districts.[35] The limitation of this methodology is that location data only exists when an individual patents. Movement prior to patenting and after the individual's final patent application is therefore not seen. There is also the potential that individuals with the same name are treated as movers because their names are linked to multiple districts. It is unlikely that there is a bias in removing these individuals as they should be

[33] Pogonyi et al. (2021), Faber (2014), and Baum-Snow et al. (2017) find displacement effects in different settings when looking at economic or population growth following transportation improvements.

[34] This is calculated as the mean patents in the neighboring districts with the inclusion of the neighbors' mean population as an exposure variable.

[35] I only have access to inventors' names for the 1823-1852 portion of the dataset, which restricts this analysis to these years.

Table 1.12: Reallocation of Patenting Activity

	(1) Patents	(2) Patents	(3) Patents	(4) Moved to Rail
Nearby Rail	-0.122			
	(0.258)			
Rail		-0.079	0.375	
		(0.129)	(0.276)	
High Skilled				0.025
				(0.032)
Observations	3814	4901	3060	145703
Districts	127	137	102	402
Years	1823-1861	1823-1861	1823-1852	1851
Sample	Not Treated	Neighbors	Non Movers	Movers (ICEM)

Notes: Column (1) reports the Poisson regression estimates of a modified version of equation (1.1) where the dependent variable is patents per capita and the independent variable is an indicator equal to one if there is a district within 50KM of district i that has a local rail station in year t. A district is included up until it receives a local station. Column (2) reports the Poisson regression estimates of equation (1.8) where the dependent variable is the mean number of patents in the neighbors of district i in year t and the main independent variable is an indicator equal to one for years in which district i has a rail station. Neighboring districts are only included as part of the dependent variable until they receive a rail station; neighboring districts treated prior to district i are excluded. I control for the neighbors' mean population as an exposure variable. Column (3) reports the coefficient estimate of equation (1.1) using the subset of the patent data which includes names (1823-1852). Movers are identified by full name and inventors in the patent data who are matched to multiple Census districts are removed. Column (4) reports the logit model estimates of equation (1.9) where the dependent variable is an indicator equal to 1 if the individual has moved to a district which has a rail station. The main independent variable is an indicator equal to 1 if the individual is classed as a high skilled worker. I restrict the data to individuals from the 1851 Census whose birth districts are identified and are different from the districts in which they were born. Only individuals above 20 years of age are included. Additionally, only individuals who in 1851 reside in a district which is included in the main treatment definition are included. I control for individuals' age and include birth district fixed effects. Errors are clustered by district in columns (1)-(3) and by birth district in column (4). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

randomly distributed across England. After removing all patent applications by "movers" I estimate equation (1.1). The results are still positive, though no longer statistically significant. This is potentially due to a large decrease in the number of observations; a third of the patent applications are removed from the dataset as they pertain to "movers" and the time frame is limited to the lower patenting period prior to the 1852 reform. The coefficient implies an elasticity of 46%, lower than the baseline elasticity of 64% using the 1823-1852 sample, but still roughly similar.

I find no evidence that highly skilled workers are more likely to move to rail districts, indicating that inventors are no more likely than the rest of the population to move to rail districts. Individuals cannot reasonably be matched across censuses as they can only be linked by name. Additionally, individuals did not identify themselves as "inventors" in the 1851 Census. However, Schurer and Higgs (2023) provides the birth district of individuals in the census as well as the HISCO occupational category for employed individuals. The latter can be matched to a skill class using HISCLASS (Leeuwen and Maas 2011). Using only the universe of employed "movers" - individuals who moved from their district of birth - and who are above age 21 in 1851, I estimate a logit model wherein I regress an indicator equal to one if the individual is living in a district with a rail station in 1851 on an indicator for a high skilled occupation:

$$Station_{i,p,1851} = \alpha_j + \beta_1 Skilled_{p,1851} + \beta_2 Age_{p,1851} + \epsilon_{i,p,1851} \tag{1.9}$$

Station is equal to one if the district i that individual p is registered as living in for the 1851 Census has a station and zero otherwise, *Skilled* is equal to one if individual p is considered highly skilled in 1851 and zero otherwise, *Age* is a control variable for the individual's age as the decision to move to certain places is likely influenced by age, and α_j is a fixed effect term for the individual's birth district. Leeuwen and Maas (2011) divide individuals across eleven categories of skilled labor. I categorize individuals in top two categories as highly skilled. These categories include entrepreneurs and managers, as well as engineers, occupations which 19th century inventors are likely to have been categorized as. The coefficient estimate in column (4) is essentially zero, indicating that the highly skilled were no more drawn to rail districts than lower skilled workers.

1.6 Additional Robustness Tests

The results presented presented above are robust to a variety of robustness checks. Outputs for these robustness exercises can be found in the Appendix.

The main results use an exposure variable to control for population changes. However, Poisson estimation cannot control for population in an exogenous manner by including it as part of the dependent variable as Poisson regression can only estimate a dependent variable that is a count. I replicate the main results in Table 1.2 with an OLS model estimating patents per 100,000 people. The implied elasticity of 83% is quite similar to the 77% implied by the main Poisson estimation, indicating that the Poisson estimation is unlikely to be misspecified by excluding population from the dependent variable. Given the sparsity of the data, I also apply an Inverse Hyperbolic Sine (IHS) transformation to the dependent variable. The results are positive and statistically significant, but the elasticity of 20% is much smaller than in the case without the transformation. Recent critiques of the IHS transformation (Mullahy and Norton 2022 and Chen and Roth 2023) argue that it should not be applied when there is a mass of zeros, as is the case in this data sample. In particular, one of the conclusions of Chen and Roth (2023) is to avoid interpreting the coefficient as a percent change, which likely explains the large difference between the coefficients and implied elasticities of the IHS model and the OLS and Poisson models.

As an additional test on the patent data sparsity, I estimate a Poisson model where the dependent variable is an indicator equal to one if the district has at least one patent in year t. The coefficient of .306 is somewhat lower than the baseline coefficient estimate of .571.[36] More than half of the difference can be attributed to the top four districts by maximum number of patents in a year. Given that the median number of patents in a district and year, conditional on patenting, is one, it can be expected that the extensive and intensive margin are similar.

I estimate the model using 2SLS with the treatment definition in which only the end point and rail hub districts are removed. The simulated lines are used to instrument the real lines. Treatment is assigned to districts through the 2SLS process, and the year of treatment is dictated by the opening of the district's first rail station. The implied elasticity of 93% is quite similar to the elasticity implied by the OLS model using the main treatment definition. The high Kleibergen-Paap Wald F statistic shows that the instrument is not weak.

One concern with regards to the empirical framework is serial correlation in the error term. To deal with this, the regressions thus far have all clustered errors by district, the unit of analysis. It is possible that there is serial correlation across a wider geographic area. Clustering at the spatial unit above district, the county level, however, yields a similar error term, indicating that this is not a concern.

[36] The coefficients are much more similar when restricting the data to 1823-1851, the period in which patenting was more expensive and in which there was significantly less patenting activity and therefore fewer districts with multiple patents in a single year.

The results are robust to a variety of definition and sample restrictions. Restricting the time component of the sample to 1823-1851 removes the years after the passage of the Patent Law Amendment Act when patenting became significantly cheaper. This period exhibits a much lower patenting activity, making it prone to larger error terms. The coefficient is slightly smaller than the baseline coefficient but broadly similar indicating that the new law did not have an impact on the local rail station effect.[37]

As a district can occupy a large area, there is potential that assigning treatment according to whether a station is in a district is vulnerable to concerns over the precision of that definition. For example, a station can be near a district's border and serve multiple other districts, or even primarily serve a neighboring district depending on the population dispersion in the districts. I adopt an alternative treatment definition based on the 3KM radius circle of land around each station.[38] A district is considered treated if it has the largest percentage of land within the circle or if the district's land occupies a majority of the area of the circle. In this definition, a station can treat multiple districts when the former is not equal to the latter.[39] The results are very similar to the baseline results, indicating that the treatment definition is not sensitive to geographic concerns.

Rail construction decisions were likely to be more similar in a twenty-year window than across a forty or fifty year window. Removing districts treated after 1861 (the "Never Treated" districts) from the control group does little to change the results. The resulting control group is smaller, and so prone to larger errors, but is also a more similar comparison to the treated group as the vast majority of treated districts were treated between 1839-1861. This control group, comprised only of "Not Yet Treated" districts, and the treated group are therefore close to each other across time of treatment.

Parliamentary approval was required for most rail construction, leading to the possibility of political interference in the planning and construction process. Esteves and Geisler Mesevage (2021) find that Members of Parliament with connections to a rail project managed to get their projects approved more often than rail projects which did not have politically influential backers. The authors use the 1845 Board of Trade recommendations as a placebo test for their findings.

[37] Using the full 1823-1861 sample with an interaction between an indicator equal to one during the post Patent Law Amendment Act years and the local rail station variable yields a statistically insignificant coefficient, offering additional evidence that the effect of a rail station is not dependent on the change in patenting cost.

[38] Each circle encompasses a 28.27KM area.

[39] This can arise when a smaller district is fully within the circle, yet a larger district makes up most of the surrounding land.

The recommendations were issued only in the 1845 session by the Board's team of engineers who evaluated projects on their overall efficiency and feasibility. The Board of Trade engineers' recommendations were viewed as so impartial that rail firms used their political power to prevent the Board of Trade from issuing recommendations ever again (Casson 2009). Of the 68 rail projects that were evaluated by the board and built within the next fifteen years, 51 were approved by the board. A majority of the rejected rail lines were built in districts that were already treated and therefore have no effect on the difference in difference term in the model (which only considers first treatment).[40] The fact that so many of these projects were rejected offers some confirmation on the decision making process by the Board of Trade's engineers - building many lines in the same place is not necessarily efficient. I estimate equation (1.1) and include an interaction term between the rail treatment variable D and an indicator equal to one if the district was treated by a line approved by the Board of Trade. The coefficient of the interaction term is small and negative and with a large error term. This indicates that the districts treated by the Board of Trade approved rail lines did not exhibit different patenting effects. In the broader context, this implies that districts that were treated by rail lines that were plausibly free of political considerations had a similar treatment effect as districts treated by rail lines that were more exposed to potential political interference.

Rail construction in the 1840s saw two large spikes in 1840 and 1847, a period coinciding with the height of speculative interest in rail ("Rail Mania"). It is plausible that districts treated in these years are more likely to be exogenous - speculative booms are characterized by a decrease in rational behavior and functionality of markets. I estimate equation (1.1) and include an interaction term between the rail treatment variable D and an indicator equal to one if the district was treated in 1839, 1840, 1846, or 1847. The coefficient is positive, but statistically insignificant, indicating no meaningful difference between the districts treated during the peak Rail Mania years and districts treated in any other year.

I create a balanced panel dataset of individual inventors from 1823-1851 (the years in which I can match inventors by full name). The dependent variable is an indicator equal to one if inventor p applies for a patent in year t. Fixed effects for year and individual are included in the estimation equation as is a vector of population controls for district i. Treatment D is equal to one if the district in which the inventor lives in year t has a rail station.

[40] Only three districts are treated by the rejected lines.

$$Patents_{i,t} = \lambda_t + \gamma_p + \beta_1 * D_{i,t} + \beta_2 * Pop_{i,t} + \epsilon_{i,t} \tag{1.10}$$

Inventors are matched across time by full name. Addresses are only known for years in which an inventor patents. In all other years, inventors are assigned districts by the known district nearest in time. For example, if an inventor patents in 1830 in Bradford and 1840 in Clun, the inventor's district i is Bradford during the period 1823-1835 and Clun during 1836-1851. The model is estimated using an OLS estimation which clusters errors by inventor. The results imply a slightly lower patenting elasticity of 43% than the elasticity of 58% implied by the district level model for the same time period.

Rail innovation is potentially local to rail construction, and I therefore exclude all rail related patents from my main data sample to avoid this potential correlation. Adding back in the rail patents that were removed from the data only marginally changes the coefficient. It is also possible that industries which are adjacent to rail also exhibit spillover effects from rail construction. For example, steam engines were used to produce power for both trains and factories and it is possible that the same invention could be used in both sectors. While I remove steam engine related patents which directly mention rail technology, I generally retain other engine patents which do not. As an additional robustness check, I remove all patents which are classified as part of the engine and carriages sectors. The results are almost identical to the baseline estimates. Removing agricultural patents to account for compositional differences between the "Main" treatment definition and the full set of districts somewhat increases the coefficient estimates, but the results are broadly similar.

To assess whether the results are specific to patenting, and therefore potentially prone to some error in the patenting data, I assess the effect of a rail station on local population. I modify equation (1.1) by changing the dependent variable to local population:

$$log(Pop_{i,t}) = \alpha_i + \lambda_t + \beta D_{i,t} + \epsilon_{i,t} \tag{1.11}$$

I find that a local rail station increases local population by 2.9%, indicating that the rail station shifts other economic variables, rather than just having an effect on patenting.[41]

[41] I compare this result with the results found by Bogart et al. (2022) by dividing 2.9% by the mean number of treatment years (13.5) and I find a mean annual growth rate of .22%. While this number is smaller than the .87% rate that Bogart et al. (2022) find from 1851-1891, the authors use a different estimation technique, time period, and geographic treatment area. Given

Though patents are widely used to measure innovation, they may be an imperfect proxy. For example, individuals may have patented low quality or useless inventions in the hope of reaping money through the legal system. To a large extent, this issue is accounted for by measuring the effect on high quality patents as I do in Table 1.2. Using data from the 1851 World's Fair (Moser 2012), I use exhibitions as an alternative proxy for innovation. As the data are only for the 1851 Fair, potential analysis is limited. However, I find a positive correlation between districts that have a rail station in either 1850 or 1851 and the number of exhibitions per capita that the district had at the Fair.

1.6.1 Postage Act

The introduction of the rail network coincided with the Postage Act of 1839 which made the cost of sending mail uniform, replacing the previous distance-based system. Hanlon et al. (2022) posit that the decreased cost of communication caused by the Postage Act led to an increase in innovation independent of a rail effect. Given the shared timing of these events, it is possible that my findings are misidentified and that the knowledge flows caused by the lower cost of mail are driving the results.

I parse the effects by exploiting the fact that a single patent office in London served all of England. Individuals who were not in London could mail their applications to the patent office. If the increase in patenting which occurs following a rail connection is caused by the decreased cost of posting the patent application to London, one would expect the mean distance between an inventor and London to increase as inventors further afield see a decrease in their patenting costs. However, regressing patents per capita against the triple interaction of a local rail station, the distance between an inventor and London, and an indicator equal to one for every year following the Postage Act yields a negative, weakly significant coefficient. This suggests that the elimination of distance-based mail fees is not a confounding factor as distance to London is not positively correlated with patenting activity.[42]

Following the 1852 patent reform, which decreased the overall cost of patenting, the number of patent applications greatly increased. This is in line with an inverse relationship between the cost of patenting and patenting. However, Figure A.9 shows that there is no change in the mean distance between coauthors following

these differences, the difference in magnitude is not surprising.

[42] The coefficient from regressing patenting against the interaction between having a local rail station and the distance to London is effectively zero.

the 1839 Postage Act until 1852, whereas an increase in distance is expected if the cost of communication across larger distances decreases and is relevant to patenting. This implies that the lowered cost of written communication from the Postage Act is not having a conflating effect.

The discrepancy between these results and the results that Hanlon et al. (2022) find could perhaps be due to the authors' focus on basic research rather than final inventions. Another possibility is that in-person communication - the type of knowledge flows that might be expected from a decrease in the cost of rail tickets - interacts with patenting activity in a distinct manner than mail correspondence.

1.7 Conclusion

I find that the English railroad network had a transformative impact on innovation. Exploiting the manner in which the railroad was built to study plausibly exogenously treated districts, I find large returns to local patenting: having a rail station causes a 77% increase in a district's patents per capita. By comparison, the railroad increases local population by less than 3%. The returns to patenting do not appear to be driven by sorting among inventors. Rather, using annual transportation network maps that I created to measure the cost of moving between districts along roads, canals, and railroads, I find that the construction of the rail network causes an increase in local market access, which in turn causes an increase in local patenting. 43% of the local rail station effect is explained by the increase in market access. I also find evidence that the railway causes an increase in patenting due to fostering knowledge flows and that this mechanism is distinct from the market access channel. The returns from knowledge flows disproportionately increase innovation in large towns which house "large enough" relevant knowledge bases and the returns from trade disproportionately increase innovation in locations with more manufacturing jobs. These findings fill a gap in the literature - previous research on transportation networks find a knowledge flows channel, but no papers explore the market access mechanism. My results indicate that these papers are potentially missing a large channel through which transportation networks affect innovation. This has important implications both for improving economists' understanding of innovation and for improving measurements of the gains from static trade.

Future work includes: obtaining data on local trade flows to more precisely measure the imports and exports of a district and how this changes with rail, disentangling the market access mechanism from a potential financial constraint channel, matching the data to 21st century data on local employment and patenting to see if an earlier rail connection produced effects that were so durable as to have permanently shifted the sectoral composition of a town, and obtaining data on Royal Society of Arts awards as a robustness measure in defining innovation.

Chapter 2

Country Banks and the Panic of 1825

2.1 Introduction

Understanding the transmission of financial shocks to the real economy is important for designing policies to stabilize output and the business cycle. However, financial crises occur infrequently, leaving researchers with limited observations of crises to draw from. Each episode of crisis provides a chance to shed light on the real costs of financial shocks and on propagation mechanisms. Episodes where the initial shock is plausibly exogenous to local economic conditions are especially useful for understanding the causal effects of financial crises on real activity.

The British Panic of 1825 is exactly such a crisis. It involved the first cases of sovereign default in international capital markets and has been called the first emerging-market induced financial crisis (Bordo, 1998; Dawson, 1990; Morgan and Narron, 2015). It provides a natural setting to study the effect of a banking panic on economic activity, since the source of the initial shock is external. The crisis was significant from a macroeconomic perspective: among Britain's banking crises over the past 200 years, Turner (2014) puts only the Panic of 1825 on par with the Great Recession of 2007-8 in terms of financial distress and output costs. The Panic exhibited some classical features of modern financial crises, but has not been previously studied because of a lack of microdata. This paper fills that gap by collecting detailed new data on the entire banking network of 1820s Britain as well as new data on local economic activity from bankruptcy records.

We show that the Panic of 1825 triggered bank failures beyond the London banks directly exposed to the bad debt and that this in turn had a negative effect on the real economy, which we proxy with non-financial firm bankruptcies. Using novel data on the 1820s banking network and firm failures, we exploit exogenous exposure to the sovereign debt crisis through correspondent banking ties to identify a causal effect: districts with affected country banks experienced a sharp increase in bankruptcies.

Like many modern financial crises, including the global financial crisis of 2008, the Panic of 1825 occurred after a large number of new securities appeared in financial markets. Surprising news about the low quality of these assets caused runs on the unregulated financial institutions that were exposed to them. Through the correspondent banking network of relationships between London banks and small, so-called "country" banks in English towns outside of London, the crisis exerted significant stress on the country banks. Credit and financial intermediation both within and outside of the financial center (London) contracted.

More than 10% of England's country banks went bankrupt during the Panic and real activity declined dramatically. Construction activity, measured by brick production, fell by 30% (Shannon 1934). The value of exported cotton manufactures fell 20% and the quantity of raw wool imports fell by more than 50% (Gayer et al. 1975). Bankruptcies more than doubled. We provide the first causal evidence that bank failures during the crisis contributed to the decline in real activity, measured by non-financial firm failures. We present historical evidence for two mechanisms: an aggregate demand channel and a credit supply shock channel. In doing so, we also shed light on the role England's banking system played during the Industrial Revolution.

Our data links each bank outside of London ("country banks") to a particular "correspondent" London bank ("agent"). Country banks were compelled to maintain an account with a large deposit with a London agent. Failure of a London agent therefore had a significant impact on the operations of country banks' with accounts with that agent. The instrumental variable approach compares the number of non-financial firm failures in districts by local bank failure status, instrumenting for bank failures using district-level exposure to the sovereign debt crisis through the correspondent banking network. The first stage shows that transmission of financial stress occurred through the banking network. The second stage demonstrates the local bank failures led to the failure of non-financial firms in a wide range of industries from manufacturing to non-tradable services.

Districts with country banks that failed due to the crisis experienced a higher number of non-financial firm bankruptcies than districts that were not exposed. The estimation results imply a 237% increase in bankruptcies. We parse the effect of the initial financial shock with a subsequent contagion effect. This result is robust to a range of variations on our main specification. Bankless districts were largely insulated from the crisis, providing further proof that the crisis spread through the financial network. We also find that a district's trading partners bank failures and non-financial firm failures did not affect the district's economy more so during the crisis than at other periods. We discuss two possible ways financial stress on the country banks was transmitted to local economic conditions: (i) a drop in aggregate demand due to lost household wealth; (ii) a negative credit supply shock, particularly to working capital lending.

Related Literature This paper contributes to two strands of literature. First, it adds to the literature measuring the cost of financial crises for non-financial firms. Chodorow-Reich (2014) and Fernando et al. (2012) use a similar identification strategy to study the effects of particular institutions' collapse on the availability of credit during the Great Recession. Amiti and Weinstein (2018) and Mian and Sufi (2008) use matched firm-lender data in Japan and Pakistan, respectively, to identify firms' credit supply shocks. Peek and Rosengren (2000) study the effects of the 1990s Japanese banking crisis on construction activity in the U.S. using local variation in Japanese bank penetration. A concern in many of these studies is the extent to which firms can switch banks to avoid loan supply shocks. The highly localized nature of English lending and rigid bank-firm relationships during this period provide an ideal setting to isolate the effects of financial stress when switching is not possible.

The focus of much of this literature has been on identifying the effects of a credit supply shock. Yet financial crises that disrupt the payment system or otherwise negatively affect household wealth may also feature reductions in aggregate demand. One study that focuses on the local demand effects of banking crises is Huber (2018), though the demand effects he documents for Germany are essentially second-round effects of the credit supply shock due to employment losses. We instead highlight the role of country banks in the payment system and study the first-order effects of payment disruptions for local aggregate demand. This channel bears a resemblance to India's 2016 demonetization, where Chodorow-Reich et al. (2019) show that districts experiencing more severe cash shortages suffered greater reductions in economic activity in the short run, and is also consistent with the Friedman and Schwartz (1963) hypothesis that the money supply has first-order effects on output.

The source of exogenous variation in financial stress in this setting merits special attention. Several papers have documented the effects of foreign financial crises on the credit supply decisions of domestic lenders (Bottero et al., 2017; Huber, 2018; Ongena et al., 2015). To the best of the authors' knowledge, only in one other study (Acharya et al. 2018) is the case where *foreign* sovereign debt devaluation was the driving force behind deterioration of the domestic banking system's balance sheet considered, focusing on the European debt crisis. The authors show that banks in EU countries contracted lending due to the balance sheet effects of sovereign debt devaluation (see Bocola (2016) for a model of this channel) but the effects were concentrated in banks from the five countries (Greece, Ireland, Italy, Portugal, Spain) most affected by the sovereign debt crisis. The present analysis provides further evidence of this particular cost of sovereign default: contraction of financial intermediation in the investing country, even by domestic banks. The existence of this channel in early 19th century capital markets is a novel finding.

Second, the role of finance, and of country banks in particular, during England's first industrial revolution is not well understood. Contemporaries pointed to the high failure rate of country banks as evidence that these banks harmed the towns they served and excessive note issuance by country banks was blamed for credit boom and bust cycles in the early 19th century. Some have argued that restrictions on the maximum number of partners and the maximum interest rate banks could charge caused the industrial revolution to be "financed out of the pockets of tinkerers and manufacturers, not through bank lending" (Calomiris and Haber 2014). Others argue that public finance of the Napoleonic wars largely crowded out private finance (Murphy 2014; Temin and Voth (2013)).

Those arguing for the importance of private finance include Crouzet (1972), Mathias (1973), and Pollard (1964), who point out that banks' provision of short term credit freed up internal profits for reinvestment in longer term capital. Indeed, these so-called plough-backs were the primary source of fixed capital formation during this period, and would not have been possible had firms been obliged to meet their short-term needs with profits.[1] Heblich and Trew (2019) find that employment in the financial sector in 1817 was associated with structural transformation and industrialization by 1881. This paper complements those findings by focusing on the short term consequences of losing financial services.

2.2 Historical Context

2.2.1 Country Banks in England

The banking system in England during this period was a three-tiered structure comprised of the Bank of England, London banks, and country banks. Country banks began to appear in England in the mid-18th century, around the time the first industrial revolution began. By 1815 it was estimated that there was a country bank within 15 miles of anywhere in England, according to Pressnell (1956).

Each country bank maintained an account with a bank in London, referred to as its London agent. The London agent performed important functions for country banks, transferring excess capital from one part of the country to another, particularly from agricultural areas to industrial areas (Pressnell 1956). London agents also settled transactions among different country banks. For these services, country banks compensated their agents by promising to leave a large permanent

[1] Brunt (2006) suggests country banks also played a role in industrialization by funding fixed capital investment but evidence on this point is sparse. Policy changes after the Panic (described in detail at the end of section 2.2.2) make it difficult to study the longer-term causal effects of bank failures on town-level outcomes.

deposit at their London bank (Pressnell 1956). This scheme meant that if a country bank's London agent failed, as many did during the Panic of 1825, the country bank could face the substantial capital loss of their London balance. In line with recent findings for the U.S.'s correspondent banking network of the 1920s and 1930s (see Calomiris et al. (2022)) and with the broader literature on the relationship between interbank linkages and financial contagion (Anderson et al. 2019 and Acemoglu et al. (2015)), we show that this network was an important source of propagation of financial shocks.

Legal restrictions capped the number of partners in a bank at six, so country banks were small and served a limited geographic area. Country banks took deposits, but these tended to only be from wealthy individuals and large firms. The main assets, other than cash reserves, were local loans to firms and purchases of "London assets": British government securities and interest-bearing balances with London agents. Country bankers tended to match their assets to their liabilities. Private country bank note issues were backed by cash reserves and government securities in London; deposits were used to discount local or London bills of exchange, and longer term loans came from bankers' capital (Pressnell 1956).

2.2.2 Exogeneity of the Panic of 1825 to Local Conditions

The key to our identification strategy will be that the Panic of 1825 crisis originated outside the small town economies we study. In this section we first discuss the causes of the crisis, then provide a timeline of events during the crisis, and finally discuss the conduct of the Bank of England and other policy responses.

The Panic of 1825-26 has been called the first Latin American debt crisis (Dawson 1990). The success of Barings' French bond offerings in 1820, combined with the prospect to invest in metal-rich newly independent Latin American governments, and the low return on British government consols, created a huge demand for Latin American securities in the early 1820s (Neal 1998). As John Horsley Palmer, the Governor of the Bank of England in 1832, put it: "the excitement of that period was further promoted by the acknowledgement of the South American republics by this country, and the inducements held out for engaging in mining operations, and loans to those governments" (House of Commons Repts. and papers, 722 1832). These Latin American issuances constituted the "largest single category of new investment" in the lead up to the crisis (Gayer et al. 1975). Kenny et al. (2021) classify eight financial crises in England from 1750-1938 as endogenous (caused by economic depression or government policy) or exogenous (fraud

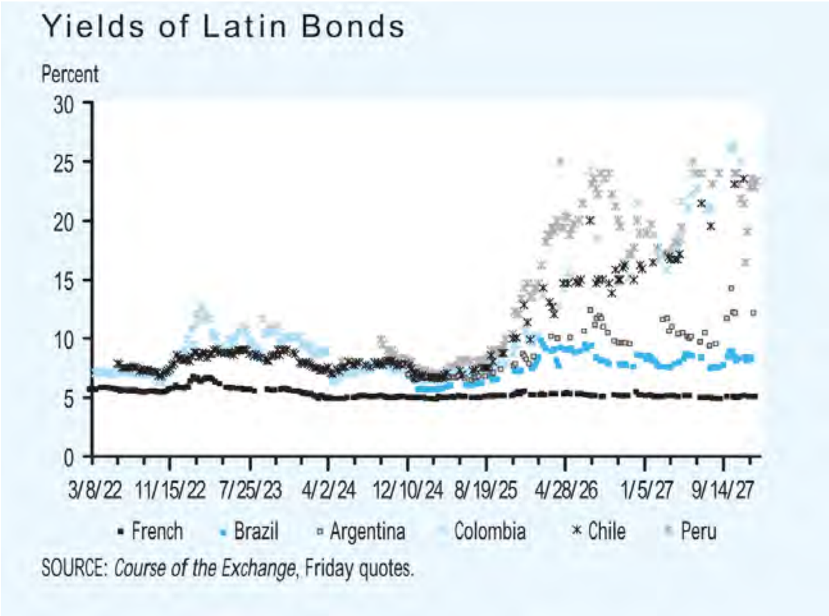


Figure 2.1: Bond yields in London, 1822-1827. Source: Neal (1998).

or risk management related) and find that the Panic of 1825 was exogenous.

Information on the quality of Latin American bonds emerged in London slowly over the years following the first issuances in 1822. Because little was known about each individual government, all Latin American bonds were priced at a heavy discount, and prices for all countries tended to move together before 1825 (see Figure 2.1). So little information was available about Latin America at the time that Scottish explorer Gregor McGregor was able to issue bonds for the fictional Central American government of Poyais on similar terms as bonds issued for Chile (Morgan and Narron 2015). Latin yields surged during the fall of 1825 as the Poyais and other schemes were revealed. A November 20, 1825 article in The Examiner (1825b) questioned Buenos Aires' ability to meet its next coupon payment. A December 8 report (The Morning Chronicle 1825) noted that "every description of Foreign Security continues under a cloud–but more especially South America." By January 1826 it was clear that most, if not all, Latin American borrowers were insolvent (Dawson 1990), though it was not until April 1826 that the first country, Peru, formally defaulted.

London banks assumed two roles in sovereign debt issuances. First, banks could underwrite foreign government debt, assuming liability in case of default. Second, banks could assume a "payee" role, responsible for collecting the payments of loan subscribers and then acting as a "window" on behalf of the issuing country and paying out coupon payments.[2] Dawson (1990) describes a case in 1823 when the London bank Everett & Co., the payee for Peru, temporarily froze payments it had collected from loan subscribers to Peru because of uncertainty about regime stability in Peru. The Peruvian government in turn needed these payments to make coupon payments back to bondholders as it had no other gold or Bank of England notes on hand, and ended up suing Everett & Co. to release the money. Incidents like these undermined confidence in both Peru's and Everett's ability to meet their obligations and suggest that payee banks were sometimes expected to supply the coupon payments when the sovereign could not come up with the money.

Rumors of impending defaults put immense pressure on the London banks involved in the debt issuances of these countries. The failures of sovereign borrowers associated with a bank could have devastating reputational consequences for the bank, according to Flandreau and Flores (2009) and Indarte (2024). Often the only information published about sovereign debt issuances in the 1820s were the amount of debt, the interest rate, the underwriter, and which bank would make the coupon payments.

Three of the six largest London agents (by number of country bank correspondents) that failed during the crisis had been the party responsible for paying interest on Latin American debt issuances, (Dawson 1990). Perring & Co. paid interest on Poyais, Everett & Co. on Peru, and Fry and Chapman on Mexico and on a portion of the Peruvian debt. These three London agents accounted for nearly half of the country banks exposed to the failure of their London agent during the crisis.

Other London banks that had no country bank correspondents but were also involved in underwriting and issuing South American securities also failed during the panic: Goldschmidt failed in February 1826 and Barclay, Herring, Richardson & Co. in April 1826. Flandreau and Flores (2009) cite evidence that underwriting banks, particularly the market leaders Barings and Rothchilds, would intervene to support securities' prices by buying them up during selloff periods. Holding these assets on one's balance sheet would be extremely costly: Peruvian bonds earned a negative return of 15 percent during this period, for example (Flandreau and Flores 2009).

Other causes of the Panic of 1825 have been suggested. Neal (1998) identifies

[2] Flandreau and Flores (2009) make the distinction between underwriting and being the window/payee on sovereign debt in the 1820s. They argue that "the risks and revenues of the last two operations were much smaller than those from the first, but leads and lags could cause trouble."

624 companies floated in England from 1824 to 1825, only 127 of which survived to 1827. Many of these were international companies, particularly mining companies in South America. The Bank of England had also created highly accommodative monetary conditions since 1819 by lowering its discount rate from 5 percent to 3 percent during this period. Pressnell (1956) argues that a fall in the rate of interest on their London balances encouraged country banks to search for higher yield by increasing their own note issues from December 1823 to December 1825, but also explains that these increases were matched by increases in the demand for credit due to good harvests and increased foreign trade. All three of these causes, the sovereign debt crisis, the decline in foreign private stock prices, and accommodative monetary policy, were more or less external to real economic activity in the interior of England.

A concrete timeline of the crisis helps to clarify our identification strategy. Against the backdrop of increasing tightness in the London money market caused by the sovereign debt crisis, the panic began on December 12, 1825 when the London bank Pole, Thornton & Co. stopped payment.[3] News of the crisis spread rapidly and runs began the next day on country banks known to be Pole's correspondents. Runs also occurred on other London banks suspected to be in trouble. By the end of that week four major London banks with a total of 65 country correspondents had failed. By the end of December, 30 country banks had been declared bankrupt and 41 more would follow suit from January to May of 1826 (see Figure 2.2).

How did the government respond to the panic in the money market? James (2012) called the Bank of England's response to panics from 1790 until 1825 "limited, episodic and inconsistent", but noted the more active role the Bank played in addressing the 1825 crisis through liberal discounting. However, the Bank also increased the discount rate from 4% to 5% on December 14, 1825. The Bank also began to issue one pound notes during the crisis as an emergency measure (all small notes had been removed from circulation in 1821). Parliament set up special boards to make advances to economically distressed country towns.

The Panic of 1825 eventually caused significant reforms to the English banking system. In 1826 Parliament lifted the prohibition on joint-stock banking outside London that had been in place since 1708 (Black 1995). These larger banks slowly

[3] On the causes of Pole, Thorton & Co.'s failure, The Examiner (1825a) writes "The decline of this house is generally attributed to the anxiety felt by the partners at the time when the rate of interest was low, to make a profitable use of their capital, and hence they were led to employ it on securities capable of being realized only at a distant period, or of an inferior degree of credit."

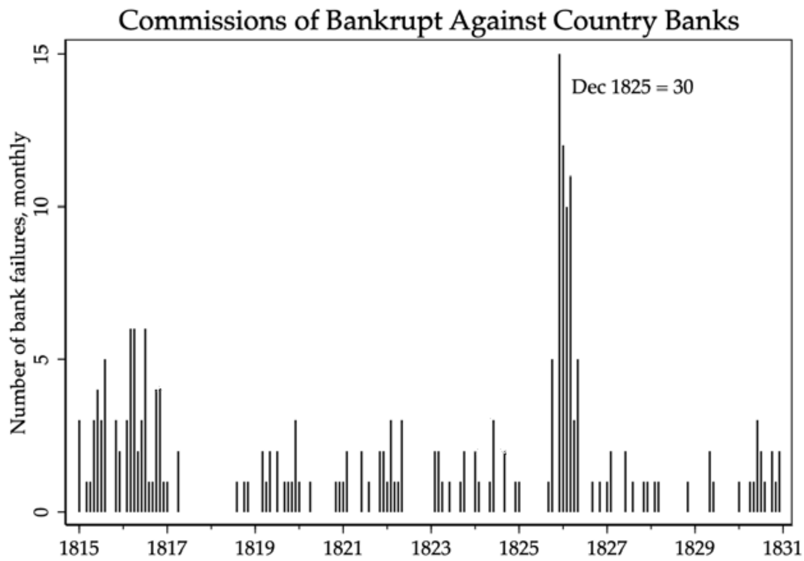


Figure 2.2: Source: Report from the Committee of Secrecy on the Bank of England Charter, 1832, Appendix No. 101.

absorbed or out-competed the country banks. At the same time, the Bank of England sought to expand its sphere beyond London by opening branches in seven major English cities. Because small note issues were blamed for the crisis, in March 1826 Parliament declared that all private notes below five pounds had to be withdrawn by 1829 (Black 1995). These additional changes to the banking system after 1826 make it difficult to study the longer term effects of the crisis, so we focus our analysis on quite a narrow period, as described in further detail in the next section.

2.3 Data

To undertake this study we created two novel datasets. One database records the universe of country banks in the United Kingdom, matching each country bank to a London agent. Using this data, we assign each district in England a bank status (at least one failed bank vs all active). We then assign each district a failed agent status (at least one bank with a failed agent vs no banks with a relationship with a failed agent). We use these values to create the independent and instrumental variables in our main estimating equation described in Section 2.5. The other database includes all bankruptcy records in England for the period January 1, 1820 to January 1, 1831, which we use to create the dependent variable in our main specification.

Table 2.1: Post-Office London Directories, English Banks Only

	1820	1823	1825	1827	1830
Number of districts with a country bank	262	279	281	251	236
Number of country banks	408	425	425	370	328
Number of country bank branches	492	528	538	447	429
Number of London agents	57	55	54	48	43
Average bank (branch) failures per year	16	30	58	49	

Source: Post-Office London Directories, 1820, 1823, 1825, 1827, 1830. Average bank (branch) failures denotes the average number of failures per year in the years between the given year and the next year in which data is available.

We aggregate both datasets to the district level using the geographic boundaries provided by Schurer and Higgs (2023). As we do not have population data for the counties of Gloucestershire and Staffordshire, we remove districts in those counties from all analyses. We also exclude districts in London as firms in London did not have to rely on country banks. Additional details on the datasets are provided in Appendix B.1.

2.3.1 Banking Network

We construct the banking network characterizing the English financial system from 1820-1830 using five years of data from the Post-Office London Directory, a business directory published by the Postmaster General: 1820, 1823, 1825, 1827[4], and 1830. We identify country bank branch failures on the basis of their disappearance from the 1827 London Directory relative to 1825. Table 2.1 describes some of the features of the data. Consistent with narrative evidence about the banking system at the time, the number of country bank branches in England peaked in 1825 and declined afterwards. The greatest number of failures per year came between 1825 and 1827. However, a large number of disappearances occurred between 1827 and 1830, likely driven by other changes like the prohibition of small note issues, the introduction of joint stock banking, and competition from the branches of the Bank of England that opened during this period.

[4]We thank a researcher at Reed College for providing access to the 1827 volume.

2.3.2 Firm Bankruptcies

The second set of data we collect is individual bankruptcy statistics from the Edinburgh Gazette.[5] The records include the date of the announcement of the bankruptcy proceedings to the general public, the bankrupt individual's name, location of residence, and occupation. To our knowledge, this is the first time these records have been collected at the town level rather than at the national level.

Bankruptcy commissions could seize an individual's assets, determine which creditors would be paid, and how much each creditor would receive. To be eligible for bankruptcy, an individual's total debt had to exceed 100 pounds, equivalent to around 8000 pounds today, and the individual had be classified as a trader rather than a professional (Duffy 1973). These criteria remained fixed over the study period. This means that our data omits gentlemen and professionals like attorneys and doctors, and merchants owing amounts under one hundred pounds. Private businesses were not entitled to limited liability during this period because of the Bubble Act of 1720, so we treat individual and firm bankruptcies as equivalent and refer to bankruptcies as firm failures throughout the paper.

The sample we collect covers January 1, 1820 to January 1, 1831 and includes 6,979 bankruptcies in 312 districts, 283 of which had a country bank branch in 1823. The bankruptcy data is quite sparse, with a mean number of monthly bankruptcies at 0.181 and a comparatively large standard deviation of 0.620. Figure 2.3 shows the total number of bankruptcies across time, marking our preferred start and end dates for the Panic - December 1825 and February 1827. This timeline covers two spikes in bankruptcies that are well above any other period in the decade.[6] We categorize each bankruptcy using the occupation of the first listed partner as "Tradable", "Non-Tradable", "Construction", and "Other".[7] Bankruptcies increased substantially in 1826 across all occupation classes (see Table B.1 in the Appendix).

While the bankruptcy data does report banker bankruptcies (which we exclude from the analysis on non-financial firm bankruptcies), they are understated compared with the Post-Office directory data. One reason for this is that each

[5] Bankruptcy notices for all of Britain had to be printed in the London, Dublin, and Edinburgh editions of the Gazette. For English bankruptcies the Edinburgh Gazette prints the information most readably. However, using Edinburgh Gazette entries means we don't reliably capture bankruptcies in Scotland and Ireland, so we exclude these countries. See Appendix B.1.2 for further discussion.

[6] In a robustness check, we use only the first spike to mark the end of the Panic.

[7] We discuss our classification methodology in Appendix B.1.2.

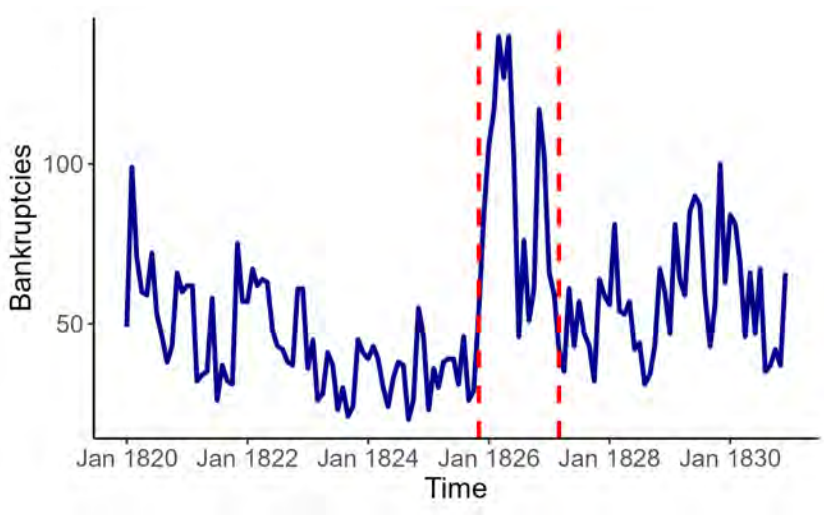


Figure 2.3: Number of non-financial firm bankruptcies across time. The vertical dotted lines mark our preferred beginning and end months of the Panic - December 1825 and February 1827.

bankruptcy notice lists a single location, where the individual actually lived. In many cases multi-branch banks failed corresponding to just one individual named in bankruptcy proceedings. For this reason, we generally use the bank failure data from the Post-Office directories, though using only bank failures in December 1825 provides a similar estimate of our results in Section 2.5.

A final concern with the bankruptcy statistics is that many troubled debtors may not appear in the statistics at all due to the inefficacy of bankruptcy commissions during this period.[8] Hearings on bankruptcy laws conducted in 1818 suggested that in cases of debts less than £1000 the costs of bankruptcy commissions usually exceeded the amount recovered from bankrupts' estates (Duffy 1973) and were rarely initiated as a result, so we are likely measuring truly large firms. Because of concerns like these, Silberling (1919) and others have used bankruptcies as a barometer of economic activity rather than a measure of activity in itself. Gayer et al. (1975) show that bankruptcies strongly comove with many other cyclical indicators like trade volumes, indices of goods production, inflation, and the money supply at the national level.

[8] Duffy (1973) argues that "faulty laws and administration encouraged dishonesty and prevented speedy collection of estates" and that bankruptcy laws were unpopular as a result (p. 153).

2.4 Identification Strategy

To estimate the effect of local bank failures on non-financial firm bankruptcies, we begin with the following difference-in-differences specification:

$$f_{it} = \gamma_i + \lambda_t + \beta\,(BankFailure_i \times Post_t) + \epsilon_{it} \tag{2.1}$$

where $f_{i,t}$ denotes the number of non-financial firm bankruptcies per 10,000 people in district i and month t. The difference in difference product $BankFailure_i \times Post_t$ equals one if district i had at least one bank failure between 1825 and 1827, and zero otherwise. The coefficients γ_i and λ_t are district and month fixed effects, respectively, which account for time-invariant district characteristics. Errors are clustered by district to account for potential serial correlation of the residuals across time. We exogenously control for changes in local population (which may drive bankruptcies) by measuring bankruptcies per 10,000 people as the dependent variable, rather than including population as an independent variable.

2.4.1 Endogeneity Concerns

Although the difference-in-differences specification in equation (2.1) includes district and time fixed effects, several sources of endogeneity remain that may bias the OLS estimates of β. First, there is a risk of omitted variable bias. Local economic shocks may simultaneously increase the likelihood of bank failure and lead to firm bankruptcies. Time varying shocks are not captured by the fixed effects terms, and the coefficient on $BankFailure_i \times Post_t$ will conflate the effect of bank failure with that of broader unobserved local distress. Second, there might be a simultaneity issue in measuring the relationship between bank failures and firm bankruptcies. The estimation of equation (2.1) requires that bank failures affect firms, but not vice versa. However, firms defaulting on their obligations could precipitate the failure of their local bank. If it is indeed the case that firm distress causes bank failure, the OLS estimate would be invalid.

2.4.2 Instrumental Variable Strategy

Our main strategy to overcome these issues is to use an instrumental variable. The instrument leverages the structure of the English correspondent banking network. Each country bank maintained an account with a London bank, its agent, which settled transactions and held a portion of the country bank's reserves. When a London agent failed during the Panic, its country bank correspondents suffered sudden losses to liquidity and capital that could cause the country bank itself to fail. We respectively name the difference in difference variable and the instrument as:

$$BankExposure_{it} = BankFailure_i \times Post_t \tag{2.2}$$

$$AgentExposure_{it} = AgentFailure_i \times Post_t \tag{2.3}$$

where $AgentFailure_i$ equals one if a district had at least one country bank linked to a London agent that failed during the crisis (1825-1827). We then estimate the first stage of a Two Stage Least Squares (2SLS) model by regressing $BankExposure_{i,t}$ on $AgentExposure_{i,t}$ and using the estimated $\widehat{BankExposure}_{i,t}$ as the main independent variable of interest in the second stage:

$$BankExposure_{i,t} = \gamma_i + \lambda_t + \pi AgentExposure_{i,t} + \nu_{i,t} \tag{2.4}$$

$$f_{i,t} = \gamma_i + \lambda_t + \beta \widehat{BankExposure}_{i,t} + \varepsilon_{i,t}. \tag{2.5}$$

This instrument shifts only the likelihood of a bank failure through exposure to a failed agent, plausibly independent of underlying local economic fundamentals as agents were operated exclusively in London (which we otherwise exclude from our dataset) and were subjected to a shock to their holdings of foreign debt.

Agent Failures Affected Country Banks

The first condition for instrumental variable validity is relevance: that the failure of a country bank's London agent had a material effect on the chance that the country bank itself would fail. Evidence from the banking network data shows that bank-agent relationships in the English banking system during the 1820s were sticky: even 10 years later, 76% of country banks that survived until 1830 had the same London agent in 1830 as they did in 1820. This suggests that switching London agents likely involved some cost that country banks were unwilling to pay, and that problems at the London bank would therefore be transmitted to the country bank.

Even if such relationships were sticky, it is still not clear a priori that an agent failure would put financial stress on their country bank clients; it could be that London balances and transactions were an unimportant part of a country bank's balance sheet, in which case the instrument would be weak. Pressnell (1956) uses surviving bank balance sheet data to argue that the London account was the most

Table 2.2: Probit Models for Bank Failure

	(1)	(2)	(3)	(4)	(5)
Bank Failure					
Agent bankruptcy	0.652***	0.627***	0.634***	0.684***	0.933***
	(0.137)	(0.141)	(0.144)	(0.197)	(0.205)
Number of other branches		0.034	0.025	0.043	0.045
		(0.032)	(0.033)	(0.035)	(0.043)
Founded 1821-1825		0.159	0.207	0.223	0.312**
		(0.136)	(0.141)	(0.142)	(0.158)
Other Bank failures in same district			1.309***	1.336***	1.267***
			(0.198)	(0.200)	(0.234)
Agent's number of clients				-0.006	0.000
				(0.007)	(0.008)
Bank failures of same agent				-0.009	-0.035
				(0.023)	(0.024)
County FE					Yes
Observations	538	538	538	538	488

Note: Country bank branch level probit model where the dependent variable is equal to 1 if country bank branch i fails during the Panic. For both the country bank and the agent variables, failure is determined through specific dates listed in the *Edinburgh Gazette*. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

important part of a typical country bank's balance sheet and the first resource in times of liquidity crisis; across nine country banks, the mean share of total assets held by the London agent was 22%.

To demonstrate that agent failures during the Panic of 1825 had material effects on their country bank clients, Table 2.2 shows bank branch-level probit models for bank failures for 538 English bank branches.[9][10] Across all specifications, agent bankruptcy has the expected positive association with bank bankruptcy. Banks with multiple branches were no less likely to fail than unit banks, despite insuring themselves across space. Banks that were founded more recently were perhaps more likely to fail, though the significance is weak and depends on other controls. The number of banks that failed and that had the same London agent as bank i is not predictive, likely because it is strongly correlated with agent bankruptcy. Column (3) provides evidence that the financial crisis was propagated not just

[9]We treat the 13 bank branches with two London agents as separate branches in the regressions.

[10]Linear probability models that avoid the incidental parameter problem for the regressions including county fixed effects show qualitatively similar results and are available upon request.

through agent failures, but also through a contagion effect. Banks in districts where other banks failed were more likely to fail themselves. This is true even when including county fixed effects.[11]

The magnitude of the effect of an agent failure is large. Using the model in column (3) of Table 2.2 at the mean of the other covariates, branches whose London agent did not fail had an 18% chance of failing during the crisis while banks whose agent failed had a 38% chance. Given this, plus the finding that there was a within-district contagion effect, the first stage regression of district level bank failures on district exposure to London agent failures, reported in Section 2.5.1, is not expected to be a weak instrument.

No Selection on London Agents

The second assumption necessary for instrumental variable validity is that banks with London agents that failed were not systematically different from other banks. Irresponsible, insolvent country bankers who were more likely to fail ex ante may have chosen irresponsible London agents who were also more likely to fail, creating an upward endogeneity bias in the previous results. Not much is known about how agents were chosen. Pressnell argues that the choice of a particular London banker was affected largely by the nature of the business of the country banker and of his clients, but family ties also played a role. The fact that relationships were so sticky, as already demonstrated, makes it unlikely that more savvy banks were able to foresee and avert risks related to which London agent they used.[12]

Maps in Appendix B.1.6 show that districts with failed agents did not generally clump together. This is also true at the county level where the number of failed agent with a banking relationship in a county is closely related to the total number of unique agents with a banking relationship in the county. The number of agent failures per bank also does not appear to exhibit geographic clustering. These findings suggest that bad agents did not exhibit a geographic pattern in terms of their relationships with country banks and it is unlikely that there was geographic selection whereby bad agents entered markets that had relatively poor economic outlooks or worse banks.

Table 2.3 compares banks with failed London agents to those whose agent sur-

[11] Certain counties experienced no bank failures, so including county fixed effects omits banks in those counties, thus decreasing the number of observations.

[12] We also show in a placebo test in Section 2.6 that having a London agent that failed in 1825 did not predict bank failure in 1823-1825.

Table 2.3: Comparison of Exposed vs. Not Exposed Banks, 1825

	Exposed	Not Exposed	Difference	
	Mean	Mean	Diff.	t-stat
Number of bank branches	2.56	1.73	0.83***	2.95
Has more than one agent	0.02	0.03	-0.01	-0.68
Founded 1821-1825	0.27	0.26	0.00	0.08
Number of banks in district	2.12	2.31	-0.19	-1.47
District population, 1821, in thousands	21.52	26.13	-4.61**	-2.39
Firm bankruptcies in district, pre-period	1.98	2.39	-0.40	-0.63
Observations	120	418	538	

Source: Post-Office London Directories, 1820-1830; Edinburgh Gazette; Mitchell and Jones (1971). Pre-period for firm bankruptcies is January 1, 1825 - November 30, 1825. Firm bankruptcies exclude banks. Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

vived and shows few differences. Banks in these two groups were equally likely to have more than one London agent, be founded in the last five years, had roughly the same number of competitors in their districts, and their districts had roughly the same number of firm bankruptcies in the pre-crisis period. The only statistically significant differences between the two groups are in the number of bank branches, with exposed banks having more bank branches on average, and exposed banks tending to be in towns with lower populations. In the regression analysis we use per capita firm bankruptcies as our dependent variable to to account for this latter point.

Historical evidence also supports the fact that the exposed banks were no more risky than other banks ex ante. Many country bank failures during the 1825 Panic were caused by illiquidity rather than insolvency. By 1828 23 out of 63 banks that declared bankruptcy during the crisis had resumed payment, and records from the same year show that an additional 31 of these 63 were still attempting to resume operation (Pressnell 1956 p. 491).[13] Still, payment stoppages that were resolved years later could have large consequences in the short run (Section 2.5.3).

Exclusion Restriction

The final requirement for the instrument to be valid is the exclusion restriction: that failures of the London agents serving a town's country banks did not

[13]Surviving bankruptcy records from three of these banks show that two were solvent and the third was short only £6,000 on a debt of £71,000 (Pressnell 1956).

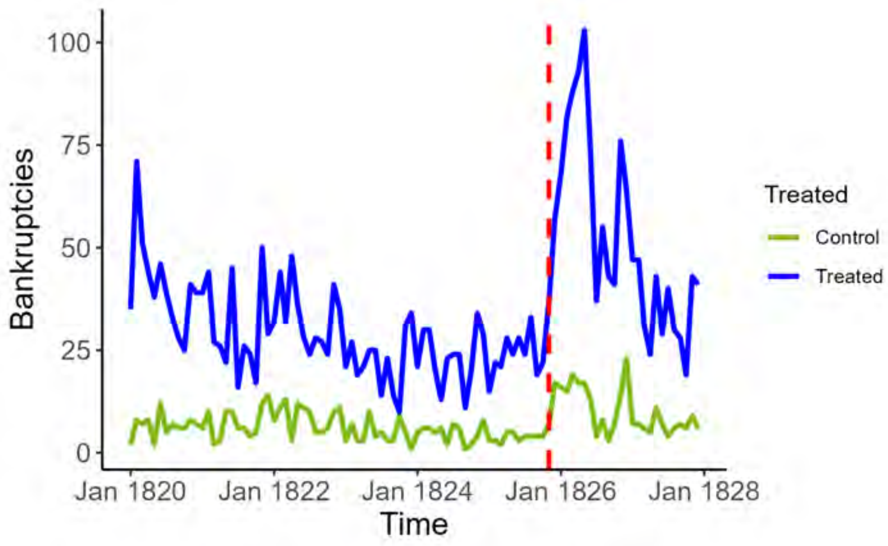


Figure 2.4: Number of non-financial firm bankruptcies across time where "Treated" bankruptcies occur in districts with a bank failure between 1825-1827 and "Control" bankruptcies occur in districts with a bank in 1823 but without a bank failure. The red dotted line marks November 1825, the month before the start of the Panic.

affect local economic conditions, especially firm bankruptcies, in any way other than through financial stress on the town's country banks. London agents only occasionally engaged with firms outside London. It is possible that they may have been more likely to lend to firms in towns where they had a country bank client. Few balance sheets have survived to shed light on this concern. Duffy (1973) collects the claims of major claimants against Brickwood & Co., a London agent that failed in an earlier banking crisis in 1810. For this particular bank, with liabilities of £621,117, only 6% of those were owed to traders outside of London, and just three individuals made up these claims (p. 381). The country bank with the largest balance at Brickwood, Bowles Bank, accounted for about 20% of all outstanding claims.

2.5 Results

2.5.1 Baseline Results

Figure 2.4 compares districts with at least one bank failure to districts with banks but no bank failures and shows that these two groups had similar patterns of firm bankruptcies until the start of the Panic. Bankruptcy rates rose in both groups of districts, though they rose more in districts with a bank failure and start

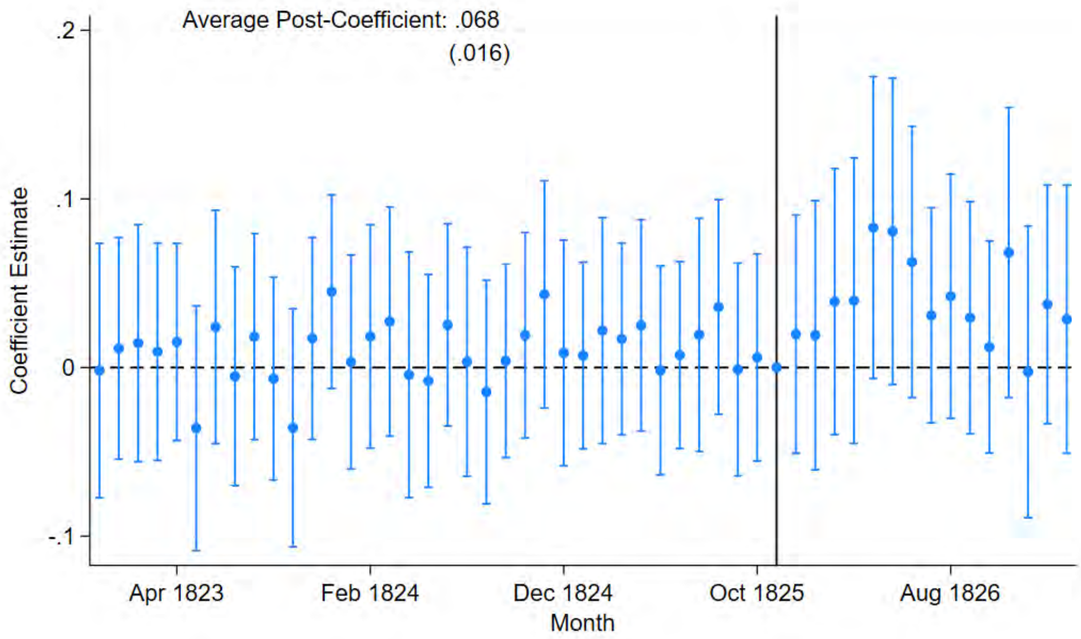


Figure 2.5: Event study plot based on estimation of (2.1) where the main independent variable is the interaction between month and an indicator equal to one if a district has at least one country bank that fails between 1825-1827 and the dependent variable is the number of firm bankruptcies. November 1825 is the comparison month. The average post-period coefficient and error term are in the top left.

from a higher base level.

Figure 2.5 shows an event study plot based on equation (2.1). The period preceding the Panic does not show any apparent trends in firm bankruptcies following Panic-induced bank failures. The comparison is again between districts which experience a bank failure and districts which do not, and as the pre-Panic coefficients are all essentially zero, this indicates that the parallel trends assumption likely holds, in line with the visual comparison of bankruptcy trends in Figure 2.4. Though no single month in the post-period is statistically distinct from zero, the aggregate post-period coefficient is, potentially because the data are noisy due to sparsity. Aggregating at the quarterly level confirms this; the estimate for the second quarter of 1826 - the height of the crisis - is greater than zero and statistically significant.

We estimate the OLS and 2SLS models described by equations (2.1) and (2.5) respectively and present the results in Table 2.4; the results of the first stage can be found in Table B.2 (in the Appendix). We restrict our sample to districts with at least one bank in 1823 so that we are only comparing districts with access to

Table 2.4: Main Results - Effect of Bank Failure on Firm Bankruptcies

	(1)	(2)	(3)	(4)	(5)
$BankExposure$	0.028***	0.138**	0.235**		
	(0.010)	(0.061)	(0.103)		
$AgentExposure$				0.031**	0.031*
				(0.012)	(0.016)
Obs.	18500	18500	16500	18500	18500
Districts	250	250	250	250	255
End Month	Feb 1827	Feb 1827	June 1826	Feb 1827	Feb 1827
Mean	0.058	0.058	0.056	0.058	.059
Analysis	OLS	SLS	SLS	Reduced Form	SDID
Kleibergen-Paap		14.969	14.969		

Notes: Column (1) reports the coefficient estimate of the OLS model described by equation (2.1). Columns (2) and (3) report coefficient estimates of the 2SLS model described by equation (2.5) which includes district and monthly fixed effects. The dependent variable is the number of non-financial firm bankruptcies in district i and month t. Column (3) uses an earlier, alternate date for the end of the Panic. Column (4) estimates the reduced form model where the main independent variable is an indicator equal to 1 if a district contains a bank whose agent failed between 1825-1827. Column (5) reports the estimate of the reduced form SDID model. Errors are clustered by district in columns (1)-(4) and bootstrapped in column (5). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

the financial markets.[14] Additionally, we restrict our analysis to districts with at least one bankruptcy in the time period of study: January 1821[15] - February 1827.

The 2SLS coefficient estimate of .138 (column 2) is statistically significant and implies a large effect on bankruptcies; districts with country banks failures due to exposure to a failed London agent experience an average increase of .138 firm bankruptcies per 10,000 people per month during the Panic, relative to districts without agent exposure. Seen another way, firm bankruptcies in districts with bank failures increase by 237% compared with districts that do not experience agent-driven bank failures.[16] The total number of firm failures caused by each bank

[14] We use 1823 instead of 1825 as the year with which we establish a district's access to the financial network in case banks had already failed during the Panic when they were recorded in the 1825 Post-Office Directory. A robustness test which uses 1825 instead of 1823 indicates that the choice between the two is largely irrelevant.

[15] We begin the analysis in 1821 rather than 1820 due to missing data on districts' population levels prior to 1821.

[16] We approximate the increase by dividing the coefficient of .138 by the mean of the dependent variable of .058.

failure might appear modest, but, as discussed in Section 2.3, we view bankruptcies as a barometer of overall local economic activity. The Kleibergen-Paap F-statistic of 14.97 indicates that the instrument is unlikely to be weak.

The coefficient estimate is almost five times larger than the OLS estimate of .028 in column (1). The 2SLS estimate is even larger when using an alternate, earlier cutoff for the end of the Panic (column 3). A likely reason for the difference in magnitude with OLS is because the local average treatment effect (LATE) is much larger than the average treatment effect (ATE). The reduced form IV estimate is almost identical to the OLS estimate, suggesting that this is indeed the case, rather than other possible explanations such as the instrument causing a sizable reduction in measurement error or in omitted variable bias. One explanation for the difference between LATE and ATE is that the districts which experienced an agent failure experienced a larger effect from a bank failure. Figure 2.6 shows the event study plot including only districts which had a bank failure in the post-Panic period (between 1825-1827) and where districts which experienced an agent failure are considered treated (and those without an agent failure are the control). Though there is no difference in the pre-period, the first few months after the start of the Panic sees an increase in the coefficient indicating that there is indeed a larger effect in agent-failure districts. This also points to a possible contagion effect (which we discuss below) having a weaker effect on firm bankruptcies than the agent-driven failures. The OLS estimate includes both the initial shock and the contagion effect, but as the shock is driven by agent failures, the 2SLS effect will skew heavily towards the initial shock. An immediate shock would give firms no time to adjust and prepare for the downturn, causing a larger effect on firm bankruptcies. This is reflected by the difference in columns (2) and (3); since column (3) uses an earlier cutoff date it includes less of the contagion phase of the crisis and the coefficient is much larger than in column (2).

Figure 2.5 show that there is no pattern in the pre-trends in the OLS model, indicating that the parallel trends assumption likely holds. To further establish parallel trends, we estimate the reduced form model using the Synthetic Difference-in-Difference (SDID) estimation strategy proposed by Arkhangelsky et al. (2021). The results are almost identical to the reduced form estimates in column (4). The event study plot produced by SDID (in the Appendix) is noisy, likely due to the sparsity of the dependent variable. A version in which we aggregate the time element to the quarterly level produces smoother pre-trends which appear parallel.

To understand the aggregate implications of bank failures during the crisis,

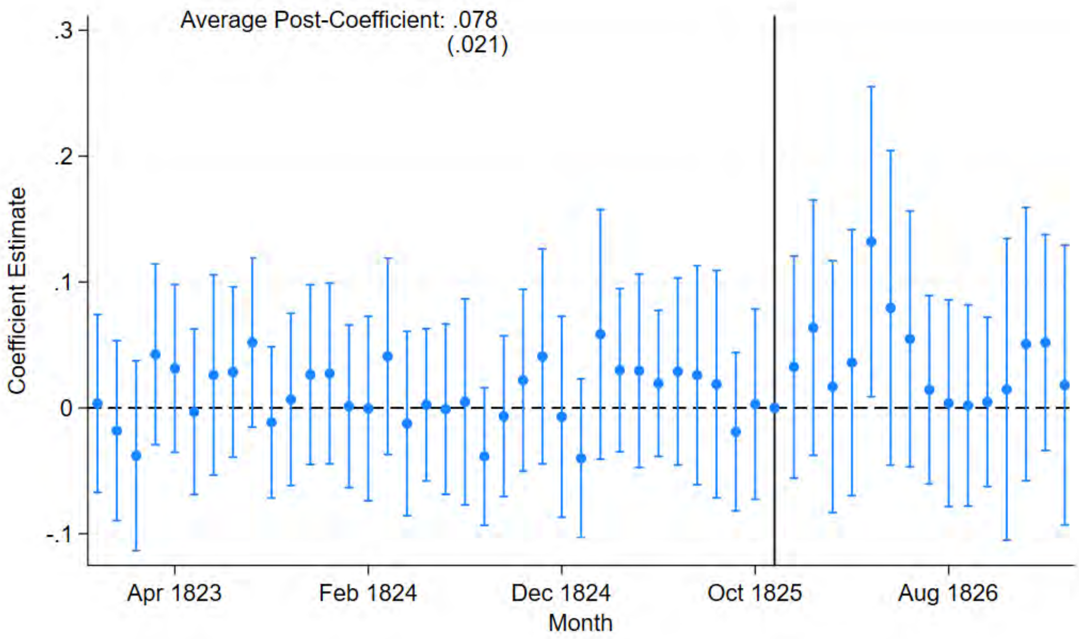


Figure 2.6: Event study plot based on estimation of (2.1) where the main independent variable is the interaction between month and an indicator equal to one if a district has at least one country bank with a relationship with an agent that fails between 1825-1827 and the dependent variable is the number of firm bankruptcies. Only districts with at least one bank failure are included. November 1825 is the comparison month. The average post-period coefficient and error term are in the top left.

we perform a simple back of the envelope calculation. Multiplying the number of districts with a bank failure by the implied elasticity of 2.47 (derived from column 2 of Table 2.4) and by the number of bankruptcies per 10,000 people in 1826 in districts without a bank failure, we find that there were 27.3 additional bankruptcies per 10,000 people than in districts without a bank failure (with a standard deviation of 6.6).

2.5.2 Contagion

We have shown causal evidence for the relationship between firm bankruptcies and bank failures. The sudden collapse of several London agents served as an immediate shock to those agents' country bank partners. We now study the effects of the financial contagion, the effects which were not driven by agent failures.

Second Wave of the Crisis

Figure 2.6 shows the event study plot comparing districts with agent-driven bank failures and non-agent driven bank failures. The measurement is noisy, but 7 months after the start of the Panic, the coefficient returns to zero, indicating that the agent failure channel only caused a spike in bankruptcies beyond what other districts with bank failures experienced until June 1826. This suggests that the second spike in bankruptcies - in the latter half of 1826 - is not driven by agent failures, but perhaps by contagion.

There are 22 districts that had their greatest number of bankruptcies over a 6 month period during the second half of 1826. These districts are driving the second spike in bankruptcies during 1826 seen in Figure 2.3 and are likely to be the districts most affected by contagion following the shock caused by the agent failures. Compared with the 45 districts that experienced the greatest number of bankruptcies in the first half of 1826, these "Second Wave" districts have a roughly similar share of bank failures (77.3% in the Second Wave districts vs 82.2% in the initial spike districts) but a lower level of agent failures (40.9% vs 51.1%), perhaps reflecting that agent failure is less predictive of economic downturns in these districts.

As our focus is now on what occurred after the initial agent failure period, our instrument is no longer valid and we present correlational evidence through OLS estimation. Table 2.5 provides evidence that the Second Wave districts experience the greatest increase in bankruptcies in the second half of 1826. Column (1) provides the baseline estimate of the OLS model and column (2) includes the interaction between *BankExposure* and a indicator equal to one if a district is a Second Wave district. The relatively large, positive coefficient on the interaction term indicates that the Second Wave districts did indeed experience a larger increase in bankruptcies following a bank failure compared against all other districts. In columns (3) and (4) we modify *BankExposure* to equal one for bank failure districts from the starting point of the second wave (June 1826), rather than from the start of the Panic. The overall magnitude of the coefficient is smaller and and with a larger relative error, as expected as we do not consider the initial phase of the crisis as part of the pre-period in this specification. However, the interaction term is much larger in column (4) than in column (2), again indicating that the Second Wave districts see a larger increase in firm bankruptcies during the latter half of 1826 than during the initial phase. Our 2SLS estimate of *BankExposure* is no longer statistically significant, further reinforcing the fact that only the initial

Table 2.5: Effect of Bank Failures on Bankruptcies During Second Wave of Crisis

	(1)	(2)	(3)	(4)	(5)
BankExposure	0.028***	0.021**	0.019*	0.005	0.045
	(0.010)	(0.010)	(0.010)	(0.009)	(0.051)
BankExposure X 2nd Wave		0.063***		0.150***	
		(0.020)		(0.026)	
Model	OLS	OLS	OLS	OLS	2SLS
Post Start	Dec 1825	Dec 1825	June 1826	June 1826	June 1826
Obs.	18500	18500	18500	18500	18500
Districts	250	250	250	250	250
Mean	0.058	0.058	0.058	0.058	0.058
Kleibergen-Paap					14.969

Notes: OLS estimation of equation (2.1) which includes district and monthly fixed effects. The dependent variable is the number of non-financial firm bankruptcies in district i and month t. The main independent variable is defined by equation (2.2) for columns (1) and (2) and modified for columns (3)-(5) such that $BankExposure$ is equal to 1 for all months from June 1826 until the end of the period (February 1827) for districts with a bank failure between 1825-1827. "2nd Wave" is an indicator equal to 1 if district i had its greatest 6 month period of bankruptcies between July 1826 and December 1826 and zero otherwise. Column (5) estimates the 2SLS model using agent failures as the instrument. Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

shock is driven by London agent failures.

Bankless Districts

In Table 2.6, we present evidence that bankless districts were largely insulated from the effects of the financial crisis. We estimate the 2SLS model in column (1), but unlike in our main specification, we include districts with no banks so that the comparison is between a treated group of districts with an agent-driven bank failure and all other districts. The coefficient estimate is smaller than in our main specification and closer in magnitude to the OLS estimate presented in column (2) though still positive and strongly significant. The OLS estimate is unchanged from our main specification. Taken together, these results suggest that bankless districts behaved similarly to districts which had a bank, but which did not fail during the crisis (the control group in our main specification).

In columns (3) and (4) we estimate the OLS model excluding districts with banks that did not fail during the crisis. The results are quite similar to the

Table 2.6: Estimation Results by District Bank Status

	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	0.065***	0.028***	0.030***	0.021**	0.004	0.004
	(0.021)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Model	2SLS	OLS	OLS	OLS	OLS	OLS
Treated	Bank Fail	Bank Fail	Bank Fail	Bank Fail	No Fail	No Fail
Control	No Fail + Bankless	No Fail + Bankless	Bankless	Bankless	Bankless	Bankless
Post Start	Dec 1825	Dec 1825	Dec 1825	June 1826	Dec 1825	June 1826
Obs.	31672	31672	25160	25160	19092	19092
Districts	428	428	340	340	258	258
Mean	0.047	0.047	0.049	0.049	0.035	0.035

Notes: Results of 2SLS estimation of equation (2.5) in column (1) and OLS estimation of equation (2.1) in columns (2)-(6). *BankExposure* is defined in equation (2.2) and equals one if district i has a bank failure from the Post Start date onwards and zero otherwise in columns (1)-(4) and it equals 1 if a district has a bank but no bank failures in columns (5) and (6) and zero otherwise. Columns (1) and (2) include all districts; columns (3) and (4) exclude districts with at least one bank and zero bank failures between 1825-1827; columns (5) and (6) exclude districts with a bank failure between 1825-1827. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

baseline OLS estimate in column (2). Using December 1825 or June 1826 to define the post period of our *BankExposure* variable does not appear to matter, suggesting that the bankless districts were insulated from both phases of the crisis.

In columns (5) and (6) we consider districts which contained at least one bank and which did not experience a bank failure as the treated group and the bankless as the control group. The coefficient estimate is essentially zero and insignificant, further indicating that the bankless districts behaved like districts with banks which did not fail during the crisis.

Trading Partners

We next explore how a district's trading partners contribute to the contagion effect. To identify a district's top trading partners, we use district level market access data from Ravalli (2025), where district i market access is calculated as the sum across all districts j of the product of district j's local GDP in 1815 and the cost of commuting between districts i and j:

$$MA_i = \sum_{j=1}^{J} GDP_{j,1815} * {c_{ij}}^{-\theta}$$
$$c_{ij} = distance_{ij} * cost_{ij} \tag{2.6}$$

The cost c_{ij} remains fixed across time and so district j's contribution to the market access of district i is only a function of distance, market size, and proximity to the canal network. The value for the trade elasticity θ is taken from Donaldson and Hornbeck (2016) and is set at 8.22. To determine the importance of a trading partner, we rank each district j by its contribution to district i's total market access. We define a top trading partner as a district in the top 3 by rank.

Using several interaction terms involving districts' top trading partners, we estimate our baseline OLS model and report the results in Table 2.7. We find that a district's top trading partners exerted little influence during the second phase of the crisis. Columns (1) and (2) report the coefficients of $PostXPartnerFail$ where $Post$ is equal to one for all months after June 1826 and zero otherwise and Partner Fail is equal to one if any of a district's top trading partners had a bank failure (column 1) or agent-driven bank failure (column 2). Columns (3) and (4) include an additional interaction with the variable $BankFail$ which is equal to one if district i suffered a bank failure and zero otherwise. Only the columns with the agent-driven bank failure term are distinct from zero, though not statistically significant for a p-value less than .05. That the agent driven failures in trading partners appear to have a positive effect on local bankruptcies while other bank failures do not further points to the larger impact that the London agent failures had during this period; districts with agent-driven failures had a greater increase in bankruptcies in the initial phase of the crisis (as seen in Figure 2.6 and discussed above) and, unlike the non-agent driven bank failures, this increase may have been large enough to trigger firm bankruptcies in trading partners.

We also find (in columns 5 and 6) that, while the lagged firm bankruptcies in district i's trading partners' is positively correlated with district i firm bankruptcies (indicating that these economies are likely linked), there is no additional effect during the crisis.[17] This result holds even when conditioning on district i bank failure status.

[17]Due to the sparsity in the firm bankruptcy data, we aggregate this specification to the quarterly level.

Table 2.7: Contagion Through Trading Partners

	(1)	(2)	(3)	(4)	(5)	(6)
Post X Partner Fail	-0.006	0.019*				
	(0.012)	(0.011)				
Post X Bank Fail X Partner Fail			-0.004	0.025		
			(0.017)	(0.015)		
Partner Bankruptcies					0.119**	0.098**
					(0.051)	(0.048)
Post X Partner Bankruptcies					-0.058	
					(0.071)	
Post X Bank Fail X Partner Bankruptcies						0.074
						(0.085)
Partner Fail Type	Bank	Agent	Bank	Agent	Firm	Firm
Post Start	June 1826	June 1826	June 1826	June 1826	June 1826	June 1826
Obs.	18500	18500	18500	18500	5750	5750
Districts	250	250	250	250	250	250
Mean	0.058	0.058	0.058	0.058	0.173	0.173
Period	Monthly	Monthly	Monthly	Monthly	Quarterly	Quarterly

Notes: The dependent variable is the number of firm bankruptcies per 10,000 people in district i period t. Each column includes district and period fixed effects. "Post" is equal to one for for all periods starting in June 1826 and zero otherwise. "Partner Fail" is equal to one if any of district i's top trading partners suffered a bank or agent driven bank failure and zero otherwise. "Bank Fail" is equal to one if district i suffered a bank failure. "Partner Bankruptcies" is the mean lagged quarterly bankruptcies per capita averaged across the trading partners. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2.5.3 Discussion

The above results show a causal relationship between bank failures and firm failures in the short run, suggesting a role for banks in promoting economic activity in the towns they served. In this section we describe two possible ways for bank failures to cause firm failures: an aggregate demand mechanism by which the failure of a bank caused a general economic downturn that reduced consumption spending and a credit supply shock mechanism by which banks lent less money to firms.

Aggregate Demand

Towns outside London did not usually use Bank of England notes until after 1826, and instead relied on country bank notes.[18] Note issues were 34% of all liabilities for 12 banks for which we have data between 1790-1826 (Pressnell 1956). Day laborers' wages were usually paid with country bank notes and these notes subsequently circulated through purchases of local goods and services. Failures of the banks backing these notes could wipe out the household wealth of noteholders as well as wealthier depositors. For example, the failure of the bank Turner, Turner, & Morris, was said to have caused "much alarm and difficulty among the middling and lower orders, as the circulation of their notes was very great" (The Examiner 1825a).

Whether the value of bank notes issued by bankrupt bankers was wiped out entirely or whether they continued to circulate at a fraction of their face value is unclear and seems to have varied. There were some cases where a particular local merchant would accept bank notes of a defunct bank in exchange for goods at a fraction of their face value, hoping to recover some of the value in bankruptcy proceedings according to parliamentary testimony (House of Commons Reports and Papers 1848).[19] At other times, small noteholders themselves were forced to participate in bankruptcy proceedings to recover the value. Banks that failed in 1825 eventually paid an average of 85% on their obligations (a figure of 17 shillings on the pound, worth 20 shillings, was cited by House of Commons Reports and Papers (1848).) However, this dividend on the bankrupt's estate was paid out several years after payment was stopped (Duffy 1973). Whether the noteholder received a large fraction of the value in some years' time, a smaller fraction immediately, or nothing at all, all three constitute a drop in household money balances in the short run.

[18]Testimony given to Parliament by the Governor of the Bank of England in 1832 suggested that the public had preferred private notes issued by a banker they knew and trusted to notes issued by the Bank of England that were subject to forgery to a greater degree than country notes (House of Commons Repts. and papers, 722 1832).

[19]A notice posted in the Northampton Mercury (1826) asserted that anyone who purchased or accepted bank notes of already bankrupt banks had no legal claim to recover the value in bankruptcy proceedings, though the legality of this behavior one way or the other is unclear.

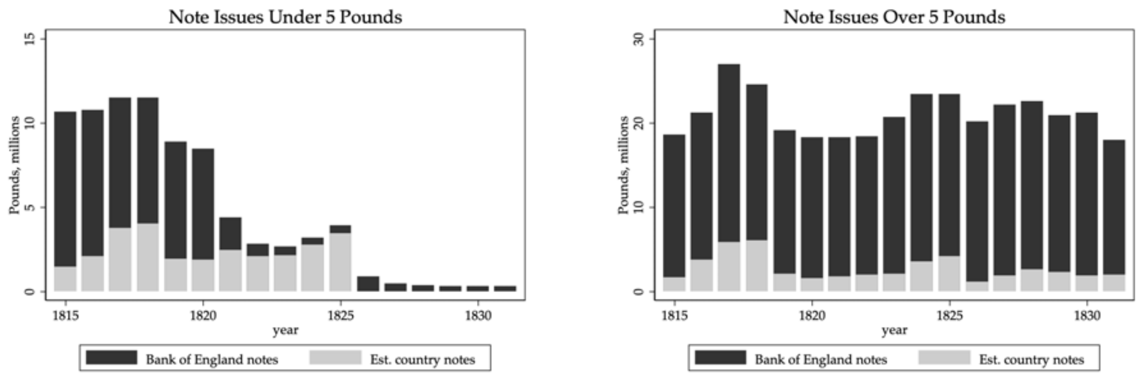


Figure 2.7: Source: Report from the Committee of Secrecy on the Bank of England Charter, Appendix No. 99 (stamp duties) and Appendix No. 82 (Bank of England notes). We estimate the volume of country bank notes using tax rates and the amount of stamp duties collected.

Credit Supply Shock

The devaluation of existing bank notes held by households was part of a larger drop in the availability of country bank notes that were the primary means of payment in British towns. Banks supplied their bank notes to firms as a form of working capital in exchange for longer term promissory notes. Pressnell writes that "the bankrupted banks represented a reduction of the means of payment and an immobilization of much capital. The survivors contracted their lending. . . contraction enforced by caution was reinforced by reduced confidence in the ordinary banks" (p. 491). Thus, much like the 2008 financial crisis, the Panic of 1825 was characterized by a contraction in bank lending to firms. James et al. (2013), studying similar disruptions to the payment system in correspondent banking networks in the U.S., argue that these stoppages act as severe adverse supply shocks, mainly by preventing firms from being able to make payroll. They find that payment stoppages by New York banks (analogous to London banks) from 1866-1914 were associated with declines in real activity of 10-20%.

Figure 2.7 approximates the contraction in short term lending by showing the volume of *new* bank notes issued, differentiating small and large denominated bills. Small denominated bills were commonly used to pay workers' wages.[20] The Bank of England responded to the credit crunch by issuing its own notes[21], but

[20]The cash in advance model of Sargent and Velde (2002) provides an example of how the composition of the money supply can play a role in determining output. Smaller denominations provide greater liquidity services in the model.

[21]The Examiner (1825a) quotes a local Birmingham paper: "the failure of the house of Smith and Gibbins created a good deal of local inconvenience from the quantity of their paper which was in circulation...It appears that the issue of £1 Bank of England notes in Birmingham, has

Table 2.8: Estimation Results of Tradable and Nontradable Firm Bankruptcies

	(1) Tradables	(2) Tradables	(3) Nontradables	(4) Nontradables
BankExposure	0.013*	0.096**	0.003	0.012
	(0.007)	(0.042)	(0.004)	(0.021)
Obs.	18500	18500	18500	18500
Districts	250	250	250	250
Mean	0.026	0.026	0.013	0.013
Model	OLS	2SLS	OLS	2SLS
Kleibergen-Paap		14.969		14.969

Notes: OLS and 2SLS estimation of equations (2.1) and (2.5). The dependent variable is the number of bankruptcies per 10,000 people of firms in sectors that sell tradable goods in columns (1) and (2) and nontradable goods in column (3) and (4). Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

not enough to completely offset the contraction in private notes. The fact that small private bank note issues never recovered was due to the 1826 banking reform requiring that notes under £5 had to be withdrawn by 1829. Unfortunately, no data on note issues at the local level is available so it's not possible to identify the causal effect of credit contractions on firm failures during this period.

The above discussion provides support for the arguments of Crouzet (1972), Pollard (1964), and Mathias (1973) that country banks did indeed contribute, at least modestly, to industrialization, but largely not through long-term lending for fixed capital formation, as Brunt (2006) argued more recently. The above reexamination of the historical evidence suggests that country banks provided liquidity to provincial economies and greased the wheels of nascent factory systems by supplying a means of payment through working capital loans.[22] This is why we can detect effects of country bank failures in the short run period during and after the crisis. If long term lending were the main driver, so many failures of large firms would likely not have occurred so rapidly.

We also find evidence of a credit supply shock by looking at exporter firms. Several recent papers have documented the differential effect of financial crises on trade; exporter firms are more dependent on credit and so are more affected by a disruption to their banking partnership, Xu (2021), Manova (2012), Iacovone et al. (2019). We find evidence that firms which produced tradable goods are driving

been very considerable, and by no means unwelcome."

[22] According to Crouzet (1972) "short-term credit to finance increases in inventories was quantitatively by far the largest need of industry" during the industrial revolution.

the effect on bankruptcies.

We estimate equations (2.1) and (2.5) using only bankruptcies of firms producing tradable or nontradable goods as part of the dependent variable.[23] We report the results in Table 2.8. Our results match with the results in the literature; the estimates using tradable bankruptcies are positive and statistically significant, and the results using nontradable goods are insignificant and close to zero in magnitude.

2.6 Robustness

This section explores various robustness checks for the results already presented. We test various specifications that differ from our main model. We also use a placebo test to show that the instrument only predicts country bank failures during the crisis period.

Changing the criteria that a district must have a bank in 1823 (our baseline rule) to a bank in 1825 does not change the results. Removing districts with multiple banks also does not substantially change the coefficient estimate though the instrument is now much weaker. This is likely because most of the observations are dropped - though the first stage is statistically significant, the error term is much larger. Estimating the model using a continuous treatment variable, such that $BankExposure$ measures the total number of bank failures in district i, rather than an indicator for the extensive margin, produces a statistically insignificant coefficient with roughly half the magnitude of the baseline estimation. While this could reflect that districts with multiple banks receive lower proportional weight from a single failed bank than in the discrete setting, recent criticisms of difference-in-difference estimation with a continuous treatment variable suggest that these results may not be credible Callaway et al. (2024).

All regressions based on the estimation of equation (2.1) or (2.5) cluster errors by district. Clustering errors by the larger geographic unit of county only marginally increases the error term, indicating that any serial correlation of the residuals is mostly at the district level. Similarly, using quarterly fixed effects instead of monthly does not affect the results. Removing outliers, defined as districts in the top 5% by bankruptcies per capita between January 1821 - February 1827, slightly reduces the coefficient but does not materially change the results.

To determine a bank failure, we observe if a bank has dropped out of the

[23] Estimating the share of tradable (or nontradable) firm bankruptcies is not feasible due to the sparsity of the bankruptcy data.

database between issues. This introduces a degree of measurement error as the window of time between issues during our period of interest is 1825-1827. We leverage our bankruptcy dataset to observe bank failures in December 1825 and January 1826 - the start of the Panic. Though this helps us measure bank failures more precisely across time, the bankruptcy dataset is not comprehensive, as discussed in Section 2.3. Estimating our main 2SLS model where we include only districts for which we observe a bank failure in these two months against a control group of districts which did not see a bank failure in 1825 or 1826 yields a positive coefficient for our estimated $BankExposure$ variable though the F statistics is low and the estimate is insignificant. Given that we remove more than half of all observations (districts which we observe as having a bank failure between 1825-1827, but not in December 1825 or January 1826), this is unsurprising. Nonetheless, the positive coefficient is in line with our main results.

Aggregating the data to the quarterly level does not materially change the results. The coefficient of the baseline 2SLS result in column (1) of Table 2.4 increases to .331, reasonably close to an expected three-fold increase. The reduced form and SDID estimates display similar results. As expected, the SDID event study plot is less noisy.

To further check the validity of the instrument and ensure that there were no systematic differences between London agents that did and did not survive the 1825 Panic, we use a placebo test. The probit models reported in Table B.6 show that agent failure during the 1825 crisis did not predict earlier country bank failures between 1823 and 1825, after controlling for agent characteristics like the total number of clients the agent had and the agent's total number of correspondent failures over the same period. Interestingly, in normal times (1823-1825), the failure of one of the bank's local competitors reduced the probability it would fail, presumably because this expanded its business prospects. However, during the crisis the within-town contagion effect dominated and reversed the sign on "Bank failures in same district" (as seen in Table 2.2). This is a novel finding relative to the analysis of Calomiris et al. (2022) who find a consistently negative effect of the failure of other banks in town during the Great Depression in the U.S., perhaps because their analysis covers a protracted period with elevated bank run risk, whereas our sample covers both the boom and bust parts of the credit cycle.

2.7 Conclusion

This paper contributes to two strands of literature. The first is on the output costs of banking crises. Recent studies have generally focused on firms directly connected with financially stressed banks and have not attempted to identify spillovers to local demand. We instead focus on the role of banks in the payment system and show that payment suspensions and bank failures affected local aggregate demand directly during the Panic of 1825.

In general, the effects we find demonstrate that the first modern financial crisis in Britain looked somewhat like financial crises in the twenty-first century, but with important institutional caveats. In particular we argue that the destruction of household wealth when private bank notes lost value was an important channel of transmission from financial shocks to the real economy, but classical features like a contraction in bank lending and a loss of market liquidity for previously safe assets may have also been important.

These findings update arguments in the second strand of literature on the importance of finance in the industrial revolution in England. The rapid spread of bank failures to bankruptcies of non-financial firms suggests that banking services, particularly the means of payment they provided to households and firms, were important for the normal functioning of local economies at a short-term frequency. This point may be useful for understanding the potential consequences of rapid demonetizations like India's in 2016 as well as the potential costs of disruptions to cryptocurrency payment systems as these currencies become more widely used.

Finally, we have found that integration in the form of the correspondent banking network played a critical role in transmitting financial stress induced by the Latin American debt crisis in 1825 to provincial economies. We find that bankless districts seem to have been largely immune to the crisis, and that contagion appears to have spread through the banking network; bank failures in districts' trading partners is correlated with an increase in local non-financial firm bankruptcies. These findings are consistent with models of failures in networks of interdependent financial organizations (see Elliott et al. (2014), for example). These models predict non-monotonic effects of financial integration: integration initially allows contagion to travel farther, but eventually reduces individual organizations' exposure to their own idiosyncratic shocks. For example Scotland, with its more mature and well-integrated banking system, experienced much milder effects of the Panic of 1825 compared to England.[24] Geographic integration via correspon-

[24] Calomiris and Haber (2014) provide a comparison of the two countries' banking systems

dent banking may well have had positive effects in normal times. Policy reforms in England in response to the Panic allowed banks to grow larger and expanded branch banking significantly, eventually enabling banks to more effectively smooth idiosyncratic local shocks.

during this period. Scotland allowed joint-stock banking and Scottish banks were much larger than English country banks.

Chapter 3

The Effect of the 19th Century Railroad on the Local English Economy

3.1 Introduction

In this paper, I investigate how the construction of the 19th century railroad affected the local English economy. I use novel datasets on local land values and bankruptcies to assess the economic effect of a rail station at the parish and district levels. At least since Fogel (1964), economists have assessed the value of railroad networks in different settings and with different criteria. Much of the existing research on how railroads affect economic activity limits analysis to specific sectors of the economy (predominantly agriculture), uses population as a measure of economic activity, or focuses on the inducement or diffusion of technology. In using local land values (which are not restricted to agricultural land) and bankruptcies, I estimate the effect of the railroad using two novel proxies for economic activity. I then explore a creative destruction mechanism with which to interpret these results in conjunction with Ravalli (2025), in which I find that railroads in this setting caused an increase in local patenting through an increase in market access.

I find no evidence that the railroad contributed to a change in local land values, nor to a change in firm bankruptcies. Rather, I find evidence that local rail stations were disruptive to the local economy. In Ravalli (2025) I showed that a local rail station caused an increase in patenting. Here, I present evidence that, by increasing local innovation, the rail station may have contributed to a rise in bankruptcies within the same sector as the newly filed patents. On average, districts with a rail station experienced a same-sector bankruptcy 2 years after a local patent; an increase in sector level market access also increases bankruptcies within the same sector, an effect that is magnified in districts that patent within that sector. Additionally, while an increase in sector level market access increases that sector's local share of labor, overall labor decreases in places that have a rail station. These twin findings are consistent with the theory in Ravalli (2025) that an increase in market access allows firms to expand and increase R&D expenditures while also pointing towards disruptions to the local economy that might hurt economic growth; an increase in unemployment may offset increases in productivity from patenting firms. I find that railroad stations also had a disruptive effect to neighboring districts' labor markets, though this effect is somewhat muted and can only be seen at the sector level.

This paper builds on this research first by estimating the impact of railroads and the changes in market access they cause on general land value and second by introducing a novel setting: 19th century England. Previous research on the question of how railroads affect the value of local economies has focused mainly on agriculture. In the United States during the 20th century, the railroad has been shown to have had a large impact on farmland. Donaldson and Hornbeck (2016) find that a 1% increase in market access, derived from construction of the rail network, causes a .51% increase in the value of agricultural land and Hornbeck and Rotemberg (2024), adopting a definition of market access that incorporates input distortions, find that a 1% increase in market access generates a .29% increase in agricultural land values; Atack and Margo (2011) find that much of the increase in cultivatable land and farm values in the Midwest during this period is due to the railroad. Donaldson (2018) studies India between 1870–1930 and finds that the arrival of the railroad increased district level agricultural income by 16%. My proxy of local land values per capita is perhaps most similar to the methodology used by Banerjee et al. (2020); the authors utilize local GDP per capita in China between 1986 and 2006 and do not restrict analysis to the agricultural sector; the authors find that proximity to transportation networks has no effect on GDP growth, matching my own results.

This paper also contributes to the understanding of the local economy in 19th century England during a time which encompassed several crises - the Panics of 1839 and 1847 and a period of low crop yields in the mid 1840s. I build on work by Olmstead-Rumsey and Ravalli (2025) in greatly extending the time frame of a novel dataset on monthly firm bankruptcies. Additionally, I create a novel dataset of local, parish level land values for two years, 1843 and 1860. These data provide a window into the geography of wealth and how it changed across the 17 years during which much of the railroad network was built.

In applying the bankruptcy dataset to study the railroad, I build upon previous research which studies how railroad construction affects firms. The current literature generally finds that rail increases firm size. Atack and Margo (2011) find that railroads increased establishment size in the United States though Hornbeck and Rotemberg (2024) finds an increase in the number of establishments but not in the number of workers or revenue per establishment. In Prussia, Hornung (2015) finds that firm size was larger in cities connected to the rail network, and that these cities did not see an increase in the number of firms. Tang (2014) finds that firms connected to the rail network had a higher average capitalization in Japan, likely indicating a larger firm size. In finding evidence that the increase in patents caused by the rail led to a simultaneous increase in same-sector bankruptcies and labor share, I contribute to the larger body of evidence that railroads increased average firm size.

This paper builds on Ravalli (2025) in examining both the broader returns to the construction of the railroad in 19th century England and in exploring the market access mechanism proposed in that paper in greater detail. In examining how the railroad changed the local labor market, this paper contributes to a rich literature. Railroads have been found to have increased the local population in studies across a wide variety of settings (for example, in Switzerland (Büchel and Kyburz 2020), England (Bogart et al. 2022), Ghana (Jedwab and Moradi 2016), the United States (Atack et al. 2010), and Prussia (Hornung 2015). I focus more on changes in local labor markets to understand changes in the size of industrial sectors and therefore build more closely on Heblich et al. (2020), which studies the spatial reorganization of labor activity in London, Hornbeck and Rotemberg (2024) which finds that the American railroads increased economic activity without changing the sectoral composition of the economy, Bogart et al. (2022) which finds that English railroads decreased the agricultural share of labor and increased that of secondary sectors, and Berger (2019), which finds that railroads in Sweden caused an increase in manufacturing labor and that this increase was unlikely due to a reallocation of pre-existing local industrial labor.

The rest of the paper is organized as follows: Section 3.2 provides a detailed overview of the novel datasets and a brief overview of the identification strategy adopted from Ravalli (2025); Section 3.3 describes the main results; Section 3.4 explores a mechanism that ties the rail-driven increase in patenting with the main results; Section 3.5 provides robustness exercises; Section 3.6 concludes.

3.2 Data

To find the effect of rail stations on the local economy, I created two novel datasets. First, I collected parish level land value data for 1843 and 1860. Second, I collected historical bankruptcy data from the Edinburgh Gazette for the January-July period from 1837-1854. I matched both datasets to the annual level rail and market access datasets which I describe in greater detail in Ravalli (2025).

3.2.1 Land Values

I transcribe and match tax schedule data detailing the total gross value of property in 1843 and 1860 for every parish in England (British Parliamentary Papers 1854b and British Parliamentary Papers 1860). These values were collected by qualified officials appointed by the House of Commons (Purdy 1860). The values are at the parish level and I aggregate them to the parish level designation created by Schurer and Higgs (2023) to allow parish comparisons across time to account for shifting geographic boundaries[1] and the district level using the geographic dataset provided by Satchell et al. 2023.[2]

Panels A and B of Table 3.1 display summary statistics (gathered from Ravalli 2025 and from the Census micro-data collected by Schurer and Higgs 2023) across parishes in the top, middle (45th-55th percentile), and bottom deciles by land value. Panel A uses land value data from 1843 and the 1851 Census and Panel B uses 1860 land value data and data from the 1861 Census. Across both the 1843 and 1860 land value surveys, the median population in the wealthiest parishes was roughly 9-10 times larger than in the middle decile. Between the 1851 and 1861 Censuses, the population only grew in the wealthiest decile. The median male labor shares are similar across land value deciles and years.[3] However, the female labor share is slightly larger in the wealthiest parishes in 1843 and maintains a constant gap in the 1861 Census, even as the female labor share grows across all land value deciles between censuses.[4] The wealthiest parishes also included much smaller shares of agricultural laborers and larger shares of textile workers. Individuals living in wealthy parishes were also twice as close to rail stations across both years compared with those living in parishes in the middle and bottom land value deciles. Given that the wealthy parishes were also the most populous, this is not surprising as rail firms wanted to connect large population centers.

Panel C provides an overview of the land value data. Across all of England, the median parish land value per capita grows by 7.2% across 17 years.[5] The standard deviations are quite large, indicating that there is a wide range in values. Indeed, the median 1843 land value per capita is 21.78 for parishes in the top decile and 2.94 for the bottom decile. Within each decile, the standard deviation across parishes is 84.66 in the top decile and .74 in the bottom decile, indicating that much of the variation comes from wealthier parishes.

Across the 826 parishes that were within 3KM of a rail station in 1843, land value per capita is notably lower and the relative gap to the median parish only

[1] This unit is called "ConParID"; in this paper, I refer to the "ConParID" level as the "parish" level to avoid confusion.

[2] When using this dataset, I retain only ConParID level parishes or districts for which there is matched data for the subcomponent parishes for both 1843 and 1860.

[3] Individuals with an unknown occupation status are treated as unemployed.

[4] Only formal female employment is included in these shares.

[5] As I only have parish level population data beginning in 1851 - the first modern Census - I divide the 1843 parish land values by the 1851 parish populations to create this per capita measure.

Panel A: 1843 Land Values

Decile by Land Value	Population	Male Worker Sh.	Female Worker Sh.	Agric. Worker Sh.	Textile Worker Sh.	KM to Rail, 1843
Top 10%	4,036	0.359	0.173	0.093	0.010	8.621
Middle 10%	460	0.374	0.149	0.251	0.002	15.870
Bottom 10%	102	0.375	0.140	0.288	0.002	16.470

Panel B: 1860 Land Values

Decile by Land Value	Population	Male Worker Sh.	Female Worker Sh.	Agric. Worker Sh.	Textile Worker Sh.	KM to Rail, 1860
Top 10%	4,412	0.378	0.190	0.078	0.010	2.311
Middle 10%	453	0.393	0.167	0.234	0.003	4.802
Bottom 10%	100	0.389	0.154	0.269	0.001	5.574

Panel C: Land Value Data Overview

Statistic	Land Value per Capita, 1843	Land Value per Capita, 1860	Change in Land Value	— Number of Parishes
Median (All Parishes)	6.903	7.419	0.428	9,339
St. Dev. (All Parishes)	28.829	30.713	19.198	
Median (Rail, 1843)	6.063	6.474	0.405	826
St. Dev. (Rail, 1843)	35.857	60.991	36.684	
Median (Rail, 1860)	6.543	7.004	0.347	2,871
St. Dev. (Rail, 1860)	29.457	39.121	25.641	

Table 3.1: Panels A and B display parish characteristics by land value percentile groups. Characteristics are classified by the median value within the top, middle, and bottom deciles (where middle means 45th to 55th percentile). The mean textile worker share is reported rather than the median as most median values are zero. Parish characteristics are taken from the 1851 (Panel A) and 1861 (Panel B) Census data provided by Schurer and Higgs (2023). Panel C contains an overview of the land value data for all parishes in England for which I have data in both 1843 and 1860, parishes with a rail station in 1843 and parishes with rail stations in 1860. A parish is considered to have a rail station if there is a rail station located within 3KM of the parish geographic centroid.

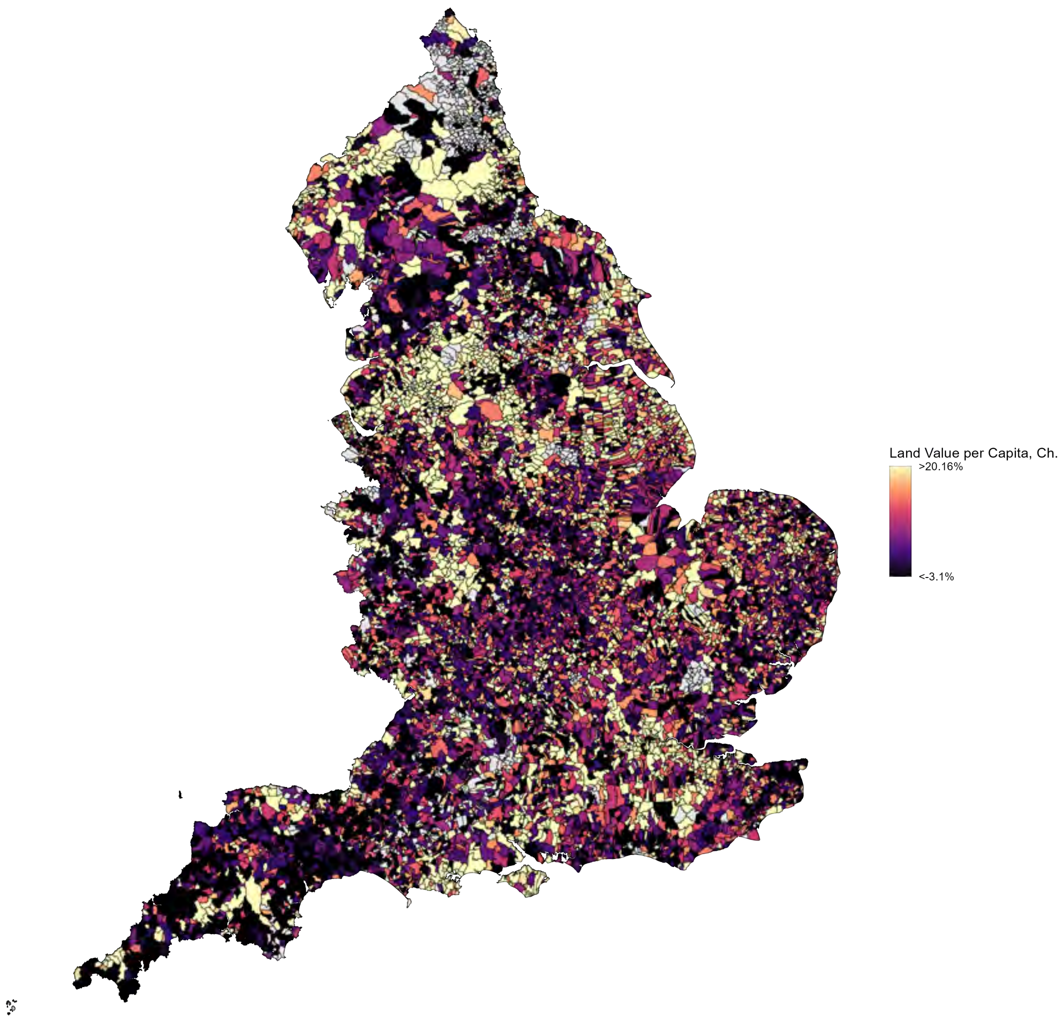


Figure 3.1: Percent change in parish level land values per capita between 1843 and 1860. Lower values have darker colors. Parishes without matched data in either 1843 or 1860 are gray. Colors are capped at the 80th (20.16%) and 20th (-3.1%) percentile cutoffs.

marginally decreases by 1860. The median value for parishes near a rail station by 1860 is also lower than the national median for both years. While Panels A and B indicate that wealthier districts are closer to stations, they are far enough away, at least in 1843, that few of these wealthy parishes are likely to be considered "station parishes" in Panel C (as the cutoff is 3KM).

Figure 3.1 shows the percent change in parish level land values per capita between 1843 and 1860. While there were not great changes in the overall levels of local wealth, there was geographic heterogeneity in economic growth rates. As might be expected given the growth in textiles, the largest concentration of high growth parishes are those around Liverpool and Manchester, while much of the southwest contracted. Both of these areas are among the poorest regions in 1843, indicating that the poorest areas stayed poor in Devonshire and Cornwall, but saw significant economic growth in Lancashire. Other areas of high growth are around cities, likely due to urbanization.

A potential concern with using historical land values as a proxy for local GDP is that they may not have been collected in a cohesive manner and that there is a degree of randomness that makes it impossible to trust the analysis. The data, however, appears in line with expectations. Figures C.5 and C.6 (in the Appendix) show parish level land values per capita. Across 17 years, there were not wild swings in land values; regions that were relatively poor in 1843 generally remained so. As noted, parishes in Lancashire are among the fastest growing, as expected given the boom in textile manufacturers centered in the region. Additionally, interpreting the 1843 land value per capita data as an analogue to income per capita, the 90-10 ratio of 7.41 is comparable (and on the lower end of the spectrum) to data from the urban populations of various countries in Latin America and the Caribbean between 2000-2014 (ranging from 6.3-14.5) World Bank Data (2016).

3.2.2 Bankruptcies

The bankruptcy dataset is a continuation of the work in Olmstead-Rumsey and Ravalli (2025) wherein we gathered data on local bankruptcies in England between 1820-1830. This newly collected additional data, covering the period between 1837-1854, allows me to observe local economic conditions through changes in local bankruptcies. Bankruptcies have been used as a barometer of economic conditions (Silberling 1919) and they have been shown to comove with other economic indicators such as trade and goods production (Gayer et al. 1975). Importantly, the new data covers the period during which much of England's rail construction took place.

Bankruptcies above 100 pounds were recorded in the Edinburgh and London Gazettes, which is where I gather this data. Due to the 100 pound limit, the dataset excludes bankruptcies of small firms. The bankruptcy announcements included the names of the individuals declaring bankruptcy, their occupations, and their towns. From this data, I collect the coordinates and sectors of the bankruptcies. I match the coordinates to Satchell et al. 2023 to obtain annual[6] totals at the district level. Occupations are mapped to sectors using the methodology of

[6] Currently restricted to January-July for each year.

Figure 3.2: The annual number of bankruptcies reported in the Edinburgh Gazette between 1837-1854 during the January-July period.

Ravalli (2025) so that analysis with the Ravalli (2025) patent data in Section 3.4 is undertaken with uniform occupation-sector mapping.

The bankruptcy data are at the district level for three main reasons. First, addresses outside of London were generally documented at the town level and were not given in a consistent manner. For example, bankruptcies in Everton were labelled as occurring in "Everton, Liverpool", "Everton, near Liverpool" or "Everton". Given this variety of inputs, it is possible, if not probable, that many individuals from smaller towns listed their addresses as the nearest large town. Aggregating to the district level removes much of this potential measurement error as a district contains several parishes, some more populous than others. Second, population is likely endogenous to bankruptcies. I control for this with decadal district level population data between 1831-1861; at the parish level, population data only begins in 1851.[7] Third, the mean number of bankruptcies per year in a given district is .411 with a standard deviation of .885.[8] Aggregating to the district level reduces some of this sparsity as compared with a parish level dataset.

[7]While town level data earlier than 1851 does exist, there are measurement issues, such as matching changing names and boundaries across time and inconsistent recording of town populations for smaller towns.

[8]These figures are calculated for the districts in the Main data sample. Across all districts in England the mean and standard deviation are .786 and 2.83 respectively.

A major concern with using local bankruptcies to measure a district's economic health over such a long period of time is that there may be several major macroeconomic events or changes to bankruptcy laws during the period of study. Figure 3.2, which plots the total number of bankruptcies across England between 1837-1854, does indeed display several spikes, though within the broader time period these seem to be typical downturns during business cycles. A change in bankruptcy laws in 1844 seems to have had little immediate impact. The estimation procedures in this paper which use the bankruptcy dataset utilize year fixed effects which account for annual swings in local bankruptcies.

Figure 3.2 also provides reassurance that bankruptcies are a good barometer of economic depressions. Spikes in 1840, 1846, and 1848 correspond (within a two year time frame) to a slump in trade in 1839, the potato blight (and Irish Famine) in 1846, and the Panic of 1847 (which lasted through 1848). Maps showing mean bankruptcies per capita during the two year window of each of these events can be found in the Appendix (Figure C.7). Bankruptcies were concentrated mainly in the north for the Panic of 1839, in the west for the Hungry Forties, and spread throughout the country for the Panic of 1847 (which Calomiris and Gorton 1991 describe as one of the four most famous bank panics of the century). These can be contrasted with the maps in Figure C.8 which show bankruptcies per capita during two year windows throughout the rest of the time period of study. These maps show that there were not areas of England which consistently had higher bankruptcies (though many districts with fewer people and businesses also tended to have zero bankruptcies in any given year; districts with zero bankruptcies in all years are excluded from the bankruptcy analyses).

3.2.3 Rail, Identification, and Market Access

I use rail data collected and described in Ravalli (2025). I provide here a brief description and overview of the rail data, the market access definition, and the identification strategy - greater detail can be found in Ravalli (2025).

I match the shapefiles provided by Henneberg et al. (2018a), Henneberg et al. (2018b), Henneberg et al. (2018c), and Satchell et al. (2018), to obtain precise rail line and station locations. These datasets are a snapshot of the stations and lines that existed in 1851 and 1861. I then match these snapshots with Cobb (2015), which contains detailed rail maps of England with information on rail lines' and stations' opening dates. The result is a shapefile that shows the exact year in which each district in England first received a rail line or station from the

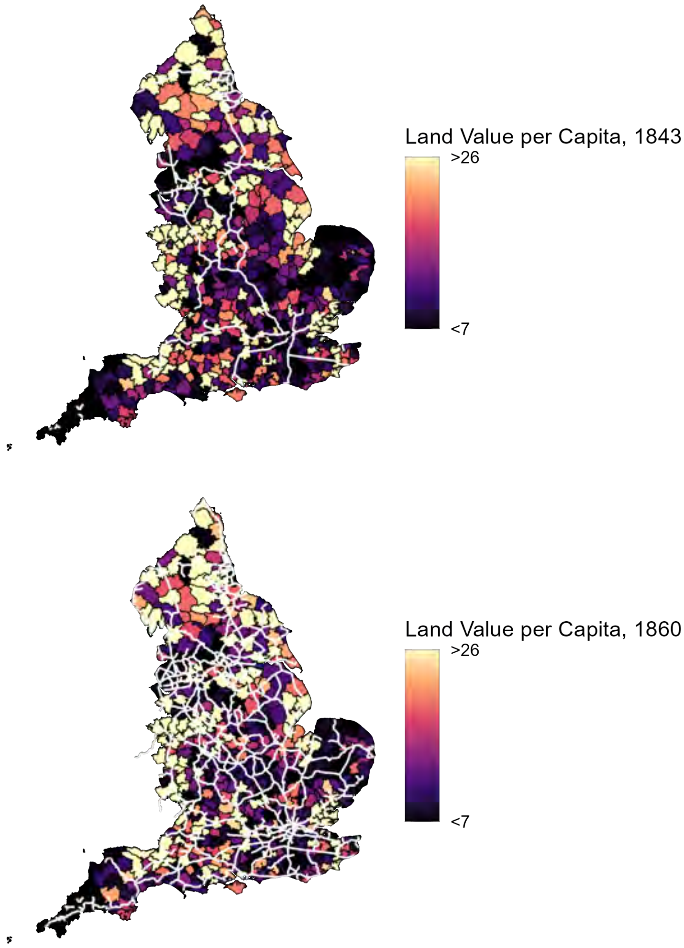


Figure 3.3: District level land values per capita with rail lines (in white) overlaid on top for 1843 (top figure) and 1860 (bottom figure). Lighter colors represent higher values.

first station in 1825 until 1861. Figure 3.3 shows snapshots of the rail system in England in 1843 and 1860 overlaid on districts' shaded by land value per capita.

To properly identify the rail effect, I employ the "inconsequential places" approach pioneered by Chandra and Thompson (2000) and used more recently by Banerjee et al. (2020), Faber (2014), and even in this setting by Bogart et al. (2022). This approach entails finding important nodes to a transportation network, removing them from the analysis, and creating a low cost path network that connects all of the network nodes to instrument the actual network. I use geographic variation to simulate the network between stations and retain only the locations that are treated in the same year by both an actual rail line and a simulated rail line.[9] I refer to the resulting set of districts as the "Main" data sample.

[9]This is essentially using the reduced form of the instrument; in Ravalli (2025) the results

While this methodology does exclude most urban areas, I reproduce some of the main results using all locations for which I have data and I find little difference, indicating that cities are unlikely to be differentially affected by railroads.

I define market access following Donaldson and Hornbeck (2016) and Redding and Venables (2004). Market access (MA) for a district i in year t is the sum of the product of local GDP in another district j and the cost of transporting a ton of goods between districts i and j in year t.

$$\begin{gathered} MA_{i,t} = \sum_{j=1}^{J} GDP_j * {c_{ij,t}}^{-\theta} \\ c_{ij,t} = distance_{ij} * cost_{ij,t} \end{gathered} \tag{3.1}$$

The cost is subject to a trade elasticity parameter θ for which I use the value of 8.1, following Donaldson and Hornbeck (2016). The transportation cost $c_{ij,t}$ depends on the distance between i and j and on the technology available for the journey between i and j: roads, railroads, and canals. The cost changes over time as the railroad is built across the country; the cost of travel on railroads is roughly equivalent to the cost of travel on canals and 14 times cheaper than on roads. I proxy district level GDP using 1815 land values (British Parliamentary Papers 1854a), so market access is only ever calculated at the district level.

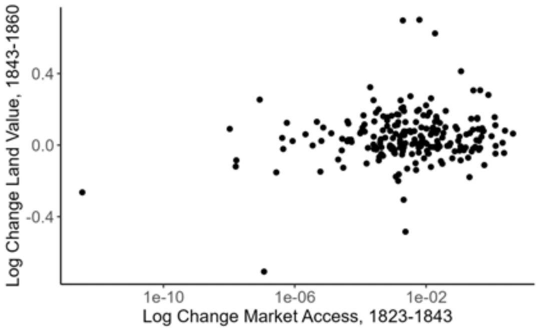
I also create a sector level definition of market access using an input-output table for the 1841 economy of England developed by Horrell et al. (1994). Each district-sector receives input values from all other district-sectors which are then assigned as weights to local GDP, which is proxied by the 1851 local labor force:

$$MA_{i,s,t} = \sum_{j=1}^{J} \sum_{\overline{k}=1}^{\overline{K}} Labor_{j\overline{k}1851} * \left(\frac{input_{\overline{k}s}}{\sum_{\overline{k}=1}^{\overline{K}} input_{\overline{k}s}}\right) * c_{ij,t}^{-\theta} \tag{3.2}$$

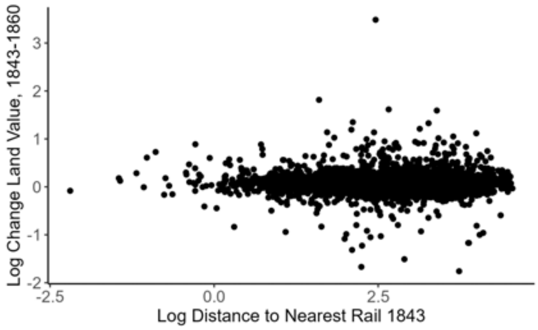
Market access for district i, sector s, and year t is defined as the sum of sector $\overline{k}$ labor within each district j not equal to i multiplied by the share of total inputs into sector s that $\overline{k}$ constitutes and by the cost function $c_{ij,t}^{-\theta}$, which is unchanged from equation (3.1). I use labor data from the 1851 Census to obtain local labor in district j. Since this is the first census to include occupational responses, it is the earliest year in which I can obtain these values.[10]

are similar when using either this reduced form method or a 2 Stage Least Squares estimation.

[10]Potential endogeneity over this late timing is a concern; however, I do not estimate any sector level equations for the main results.



(a)



(b)

Figure 3.4: Subfigure (a) displays a scatterplot of the relationship between the log change in market access between 1823-1843 and the log change in land value per capita between 1843 and 1860 at the district level. Subfigure (b) shows a scatterplot with the log distance between a parish centroid to the nearest rail station in 1843 and the log change in land value per capita between 1843 and 1860. For both graphs, only locations that comprise the Main data sample are included.

3.3 Main Results

3.3.1 Land Values

Subfigure (a) of Figure 3.4 shows that there is no clear correlation between the change in market access between 1823-1843 and the change in land values per capita between 1843 and 1860. This holds both for the subset of districts constituting the "Main" sample of plausibly exogenously treated (and never treated) districts and all districts in England (see Figure C.1 in the Appendix), suggesting that even plausibly endogenously treated cities do not see growth in land values.[11]

There is also no apparent relationship between the distance from a parish centroid to its closest rail station in 1843 and the log change in land values from 1843-1860 (subfigure b of Figure 3.4), further indicating that rail stations do not bring faster economic growth. This is true both at the parish level and district level (Figure C.3, in the Appendix).

Table 3.2 presents regression estimates to further study the relationship between the railroad and changes in local land value. In all columns, the dependent variable is the log change in land value per capita between 1843 and 1860 and the independent variable is some measure of rail treatment:

$$\Delta ln(Value_{i,t}) = \alpha + \beta Rail_{i,t-1} + \Delta\epsilon_{i,t} \tag{3.3}$$

[11]In Section 3.3.3 I discuss London, which did exhibit larger growth rates.

Table 3.2: Relationship between Rail and Land Values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has Rail, 1843	0.016	0.007	-0.006	-0.005				
	(0.019)	(0.019)	(0.018)	(0.019)				
Log Ch. MA					0.005			
					(0.010)			
Gains Station						-0.022*		
						(0.012)		
Log Dist. to Rail, 1843							0.017***	
							(0.004)	
Log Change Station Dist.								-0.011**
								(0.004)
Observations	212	212	4169	4169	212	4169	4169	4169
R-Squared	0.003	0.048	0.000	0.001	0.000	0.001	0.003	0.002
F-Stat.	0.702	3.141	0.091	0.727	0.226	3.577	15.248	6.488
Years	1843-1860	1843-1860	1843-1860	1843-1860	1843-1860	1843-1860	1843-1860	1843-1860
Unit	District	District	Parish	Parish	District	Parish	Parish	Parish

Notes: OLS estimation based on equation (3.3) where the dependent variable is the log change in land value per capita between 1843 and 1860 and the main independent variable is a measurement of a location's rail access. In columns (1)-(4), the independent variable is an indicator equal to one if a location has a rail station in 1843. Columns (1) and (2) are at the district level and columns (3) and (4) are at the parish level. Column (2) includes controls for the 1815 log land value per capita and the log change in land value per capita between 1815-1843. Column (4) includes controls for the 1851-1861 change in the parish labor share and the 1851 share of labor working in agriculture. In column (5) the independent variable is the 1823-1843 log change in market access as defined by equation (3.1). In column (6) the independent variable is an indicator equal to one if a parish which did not have a station in 1843 gains at least one station between 1844-1860 and zero otherwise. In column (7) the independent variable is the log distance to the nearest rail station in 1843. In column (8) the independent variable is the 1843-1860 log change in the distance from the parish center to the nearest station. Robust errors are reported in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The independent variable in columns (1)-(4) is an indicator equal to one if the location has a rail station in 1843, and zero otherwise. In essence, this regression answers the question, does having a local rail station predict future changes in a parish's land value relative to other districts? At the district level (columns 1 and 2), a district has a rail station if it is physically located within its geographic boundaries. At the parish level (columns 3 and 4), a parish has a rail station if there is a station within 3KM of the parish centroid.[12]

One issue with this regression strategy is that I do not control for pre-trends from the era prior to the railroad (or, indeed, prior to 1843). In column (2) I add controls for the log land value per capita in 1815 and the log change in land value per capita between 1815-1843.[13] The coefficient drops close to zero, indicating that failing to control for land value pre-trends is not depressing the value of the coefficient in column (1). In column (4) I add controls for the 1851 agricultural share of labor and the 1851-1861 change in the employment share of population. In all four columns, the coefficient is statistically insignificant, even switching from positive at the district level to negative at the parish level.

Column (5) is analogous to Figure 3.4; the independent variable is the log change in market access between 1823-1843. As expected given the scatterplot, the estimated coefficient is statistically insignificant and close to zero.

The independent variable in column (6) is an indicator equal to one if a a parish which did not have a rail station receives one between 1843 and 1860, zero otherwise. The coefficient is negative and weakly significant, perhaps indicating some small advantage to parishes in the early treatment group compared with parishes treated after 1843.

In columns (7) and (8) I measure the log distance between a parish center and a rail station in 1843 and the log change in this distance between 1843 and 1860, essentially replicating columns (3) and (6) using distance as a continuous variable. As these columns are measuring distance, a positive value indicates that parishes *further* from a rail station see a greater increase in land values per capita. This is the case in column (7); the statistically significant coefficient indicates that being 10% further from a rail station is correlated with a .17% increase in land value. Conversely, in column (8) a 10% reduction in distance is correlated with a .11% decrease in land values. While these results might imply that there is a "sweet spot" far enough from a rail station to reap the benefits and avoid the costs associated with a local station that might have a negative impact on land values

[12] I impose a distance cutoff, rather than checking whether a station falls within parish boundaries as parishes are small, with a median area of $7.59KM^2$ (equivalent to a radius of 1.56KM for a circular parish), and walking to a nearby parish's station seems reasonable; Bogart et al. (2022) find that locations within 3KM of a rail station in 1851 saw higher population growth than locations further from stations.

[13] As far as I am aware, the 1815 land values only exist at the district level and so I cannot replicate this specification at the parish level.

Table 3.3: Relationship between Rail and Land Values - Top Quartile for Variables of Interest

	(1)	(2)	(3)	(4)
Panel A: Top Quartile Only				
Station, 1843	0.037	-0.020	0.010	0.054
	(0.052)	(0.026)	(0.035)	(0.041)
Observations	1042	1042	53	53
R-Squared	0.001	0.000	0.002	0.034
F-Stat.	0.509	0.610	0.088	1.725
Years	1843-1860	1843-1860	1843-1860	1843-1860
Unit	Parish	Parish	District	District
Variable of Interest	Agriculture	Manufacturing	Density	Market Access
Panel B: Interaction				
Station, 1843	-0.015	0.001	0.033	0.013
	(0.019)	(0.025)	(0.021)	(0.020)
Station X Interaction	0.052	-0.021	-0.023	0.041
	(0.056)	(0.035)	(0.041)	(0.045)
Observations	4169	4169	212	212
R-Squared	0.000	0.000	0.039	0.065
F-Stat.	0.415	0.225	3.505	3.925
Years	1843-1860	1843-1860	1843-1860	1843-1860
Unit	Parish	Parish	District	District
Interaction	Agriculture	Manufacturing	Density	Market Access

Notes: OLS estimation based on equation (3.3) where the dependent variable is the log change in land value per capita between 1843 and 1860 and the main independent variable is an indicator equal to one if a location has a rail station in 1843. Panel A restricts the data to only parishes or districts in the top quartile by the agricultural share of labor in 1851 (column 1), the manufacturing share of labor in 1851 (column 2), 1821 population density (column 3), and 1823 market access (column 4). Panel B includes data from all locations in the Main data sample and includes the interaction between the rail station variable and an indicator equal to one if a parish or district is in the top quartile of these variables, zero otherwise. Due to data restrictions, columns (1) and (2) are at the parish level and columns (3) and (4) are at the district level. Robust errors are reported in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(for example, dirtier air), in Section 3.4 I explore the effect of a rail station on neighboring districts' land values and find no effect.

While there appears to be no general effect on land values, it is possible that

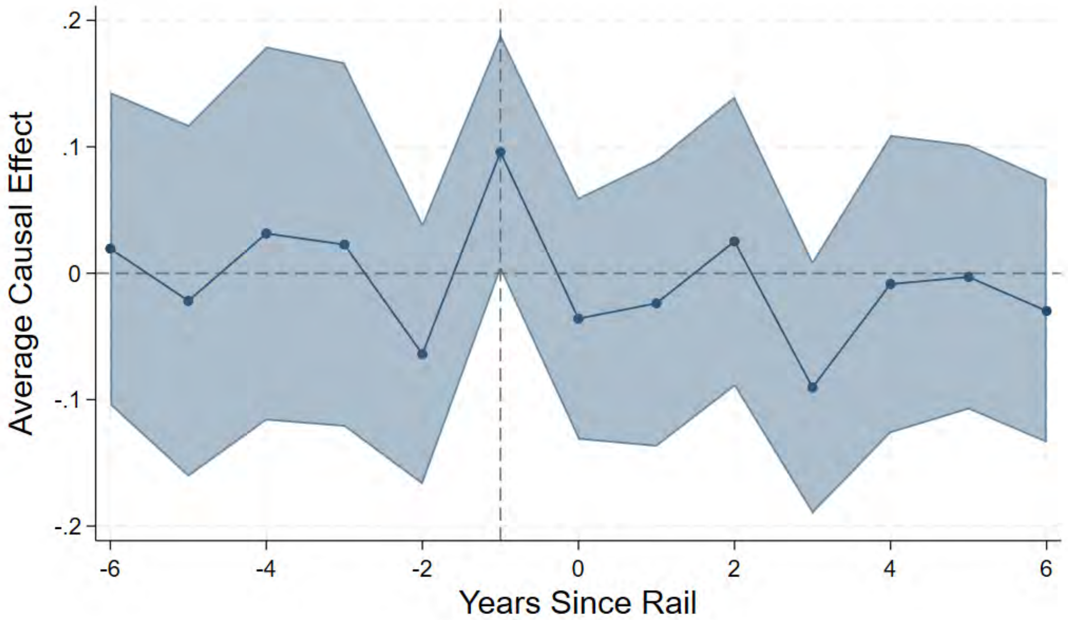


Figure 3.5: Event study plot with bankruptcies per 10,000 people as the dependent variable. Districts are treated in the year in which they receive a rail station. Districts which are not yet treated by 1855 are considered as "never treated" districts. I use the difference-in-difference estimator proposed by Callaway and Sant'Anna (2021) to estimate the "never treated" control group.

certain locations saw disproportionate gains from rail. In Panel A of Table 3.3 I first restrict the data to locations in the top quartile across four variables: 1851 agricultural share of labor, 1851 manufacturing share of labor, 1821 population density, and 1823 market access. In Panel B, rather than restricting the data, I include an interaction term that is equal to one if a district is in the top quartile by one of these metrics. Across both panels, there is no coefficient estimate that is statistically significant, further reinforcing the finding that railroads had little impact on local economic growth.

3.3.2 Bankruptcies

I now use the data on local bankruptcies to see if a station either caused an increase in local firm bankruptcies or shielded them from adverse economic conditions (in other words, if railroads had a positive or negative effect on bankruptcies).

Figure 3.5 shows an event study plot using the difference-in-difference estimation procedure for staggered treatment timing proposed by Callaway and Sant'Anna (2021). The procedure only uses "Never Treated" districts as the control. These are districts which do not have a rail station before 1855 (since the bankruptcy data ends in 1854). In the event study plot, the effect on the never treated districts is stored in time period -1. The relative spike at that time period in Figure 3.5 indicates that the treated districts do not see a change in bankruptcies following treatment relative to the never treated districts. The relatively flat pre-period estimates indicate that there are no pre-treatment trends.

To directly estimate the overall effect of a local rail station on bankruptcies, I estimate the following model:

$$Bankruptcies_{i,t} = \alpha_i + \lambda_t + Rail_{i,t} + \epsilon_{i,t} \tag{3.4}$$

α_i and λ_t are district and year fixed effects respectively. $Bankruptcies_{i,t}$ measures the total number of firm bankruptcies in district i and year t per 10,000 people and $Rail_{i,t}$ is an indicator equal to one if district i contains a rail station in year t and zero otherwise.

The results of this estimation, reported in Table 3.4, indicate that there is no effect on bankruptcies following the opening of a local rail station. This is the case when estimating both the OLS (column 1) and Poisson (column 2) Two Way Fixed Effects (TWFE) models. The (statistically insignificant) coefficients imply similar increases in bankruptcies - the derived Poisson elasticity is .178 and the ratio of the OLS coefficient to the mean of the dependent variable is .173. Since the Poisson model is more suited to count data, the similarity of these estimates suggests that the estimates are unlikely to suffer from issues related to data sparsity. The advantage of the OLS model is that it allows for easier comparisons with other models presented in Table 3.4 and that it exogenously controls for population on the left hand side of the equation (rather than using an exposure variable on the right hand side in the Poisson model).

In column (3) I report the result of the difference-in-difference estimation proposed by Callaway and Sant'Anna (2021)[14] In addition to a high error term, the magnitude of the estimate is essentially zero. In column (4) I report the result of the synthetic difference-in-difference (SDID) estimation proposed by Arkhangelsky et al. (2021) which provides optimal weights in the pre-period to create parallel trends.[15] The estimate is similar to that of the baseline OLS TWFE model, albeit with a higher error term.

In Ravalli (2025), I find that districts with the large increases in market access experience disproportionate increases in patenting. I explore this finding in this

[14] This result is analogous to Figure 3.5.

[15] 5 districts which are treated prior to 1837 are dropped from the model in this specification as SDID does not allow for "already treated" units.

Table 3.4: Effect of Rail on Firm Bankruptcies

	(1)	(2)	(3)	(4)
Panel A: Bankruptcies per 10,000				
Rail	0.036	0.164	-0.005	0.040
	(0.027)	(0.120)	(0.043)	(0.041)
Analysis	TWFE	Poisson	C & S	SDID
Observations	3366	3366	3276	3276
Districts	187	187	187	182
Years	1837-1854	1837-1854	1837-1854	1837-1854
Mean of Dep. Var.	0.208	0.411	.208	.208
Panel B: High MA Increase				
Rail	0.055	0.152	0.027	0.065*
	(0.041)	(0.218)	(0.056)	(0.036)
Analysis	TWFE	Poisson	C & S	SDID
Observations	3366	3366	3330	3276
Districts	187	187	187	182
Years	1837-1854	1837-1854	1837-1854	1837-1854
Mean of Dep. Var.	0.208	0.411	.208	.208

Notes: Estimation results based on equation (3.4). The dependent variable is the number of firm bankruptcies per 10,000 people in district i and year t. Columns (1) and (2) respectively report the OLS and Poisson Two Way Fixed Effects estimation results. Columns (3) and (4) respectively report the results using the difference-in-difference estimation strategies proposed by Callaway and Sant'Anna (2021) and Arkhangelsky et al. (2021). Column (4) includes fewer observations as districts which are already treated in the first time period cannot be included in SDID estimation. Errors are clustered by district in columns (1) and (2) and bootstrapped in columns (3) and (4). In Panel A, the main independent variable *Rail* equals 1 for any year in which district i has a local rail station and zero otherwise. In Panel B, *Rail* equals 1 after district i receives a rail station but only if i is in the top quintile of districts by change in market access between 1823-1861. * p < 0.1, ** p < 0.05, *** p < 0.01

new context in Panel B of Table 3.4. I look at the effect of a local rail station on districts which received the largest increases in market access between 1823-1861. Unlike in Panel A, districts are only considered treated after they receive a rail station if they are in this group of districts in the top quintile by market access change. In other words, if district i receives a rail station in 1850 and is in the group of districts below the 80th percentile by market access change, $Rail_{i,t}$ has a value of zero for all years. Despite the fact that the regression is now skewed to only consider the highest impact rail stations, the results are little changed from Panel A. Only the coefficient estimate from the SDID specification is statistically significant (and only weakly so).

Overall, the results from both panels in Table 3.4 indicate that there is no effect on bankruptcies. In almost every specification the sign is positive, however, signaling the possibility that the introduction of a local rail station increases local firm turnover. It is possible that the effect can only be seen at the sector level or that bankruptcy variable is too imperfect a measurement of local economic activity. I explore the former in Section 3.4 and the latter in Section 3.5.

3.3.3 Comparison with the Literature

As noted above, much of the research on this subject uses agricultural land values or population as proxies for local GDP. Banerjee et al. (2020) is a rare exception which uses county level GDP in 20th Century China. My results are closest to that paper; the authors find that railroads did not lead to an increase in local GDP growth. This is unsurprising given that the land value per capita proxy I use is more similar to GDP per capita than farm values or population levels. Heblich et al. (2020) also uses land values to assess changes in Greater London between 1801-1921 and the authors' counterfactual exercise concludes that removing railroads reduces land values by 53%, a stark contrast with the results I present. A likely explanation for the differences is that London is exceptional; in 1851, London comprised over 10% of the population of England. The railroad may have had a stronger effect there, a possibility supported by the data. The median change in log land values per capita between 1843 and 1860 in parishes outside of London with railroad access in 1843 is 0.067, almost exactly the same as parishes without rail access (0.069). In London, this value is 0.266 for parishes with railroad access and 0.166 for parishes without. Not only did London grow at a much faster rate than the rest of the country, but it seemed to benefit more from the railroad (though I note that I consider London as endogenously treated in this paper).[16]

[16]The difference across medians is less pronounced in other urban areas; of the top 100 parishes in England by 1851 population, parishes with a rail station had growth of 0.209 versus .0.183 for parishes without a rail station. This further suggests that London is exceptional.

3.4 Mechanism

I present here evidence that the railroad caused a "creative destruction" effect which can explain the lack of economic growth attributable to the railroad seen in the previous section. The proposed mechanism in Ravalli (2025) is that rail causes an increase in patenting because the increase in market access gives firms a larger customer base over which to spread R&D costs. This in turn causes the patenting firm to increase its productivity, but perhaps driving rivals out of business and causing turbulence in the local economy in the short to medium term that outweighed the increases in productivity.

3.4.1 Sector Level Bankruptcies and Patents

I find a positive relationship between sector level bankruptcies and within sector patents. Table 3.5 presents the results of logit and OLS models with year and district fixed effects where the dependent variable is sector s bankruptcy in district i and year t (a binary in the logit model and the number of bankruptcies per 10,000 people in the OLS model) and the main independent variable is an indicator equal to one if district i has a sector s patent within the four year window ending in the bankruptcy year and zero otherwise:

$$Bankruptcies_{i,s,t} = \alpha_i + \lambda_t + Patent_{i,s,t} + \epsilon_{i,s,t} \tag{3.5}$$

I use a four year window for $Patent_{i,s,t}$ as I expect a lag between a patent and a bankruptcy as the innovative firm increases its productivity and its rivals lose sales over time. I assign each bankruptcy a sector using the first listed bankrupt partner's occupation. Sector categories are determined by the labor-sector matching process used in Ravalli (2025) and aggregated to the 12 sectors derived from Horrell et al. (1994). I restrict the data to 1839-1854 as I also use this sector level data to analyze changes in sector level market access, which is only computed for 1839-1861.

The estimated coefficients are strongly significant for both the extensive logit model and intensive OLS model. This indicates that there is indeed a positive relationship between patenting and within sector bankruptcies. Notably, when the regression is estimated at the district-year level, rather than the district-sector-year level, as is the case in columns (3) and (4) of Table 3.5, there is no effect. In other words, a patent does not generally precede a bankruptcy if it is not specific to the sector; however, a patent *does* seem to hold predictive power over

Table 3.5: Relationship between Patents and within-sector Bankruptcies

	(1) Has Bankruptcy	(2) Bankruptcies per 10,000	(3) Has Bankruptcy	(4) Bankruptcies per 10,000
Within Sector Patent	0.676***	0.015***		
	(0.136)	(0.005)		
Any Sector Patent			-0.025	0.000
			(0.139)	(0.000)
Observations	33600	33600	2784	2800
Districts	175	175	174	175
Years	1839-1854	1839-1854	1839-1854	1839-1854
Mean of Dep. Var.	0.027	0.015	0.245	0.000
Model	Logit	OLS	Logit	OLS

Notes: Regression estimates based on equation (3.5). The dependent variable in column (1) is a dummy equal to one when a district i has at least one sector s patent in year t and zero otherwise; in column (2) it is the number of sector s bankruptcies per 10,000 people in year t. The independent variable is the an indicator equal to one if there is at least one sector s patent in district i in the year four year leading up to (and including) a sector s bankruptcy. In columns (3) and (4) the regressions are measured at the district-year level. Column (3) is missing the district of Bath as there is at least one bankruptcy in every year and so there is no variation in "Has Bankruptcy." * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

bankruptcies within the same sector. This finding provides evidence that patents in this setting had a disruptive effect on their sectors within the local economy.

Figure 3.6 provides a clearer picture of the timing window between patents and bankruptcies. It shows the median number of years between a patent and bankruptcy in the same sector. While districts which do not yet have a rail station at the time of the bankruptcy have tall error bars that extend beyond zero (indicating that a patent might have even come after a bankruptcy), this is not the case for districts with a rail station. In rail districts, the median gap between a patent and a bankruptcy is 2 years, i.e. on average, after a patent there is a bankruptcy in the same sector two years later. The error bars do not extend to zero (or even to one).

The discrepancy between the rail and no rail groups raises the possibility that rail stations caused an increase in bankruptcies unseen in the main results presented in Section 3.3. I now estimate the results using sector level market access and present the results in Table 3.6. I modify equation (3.4) and use local sector s market access in district i and year t (defined in equation (3.2)) in place of $Rail_{i,t}$

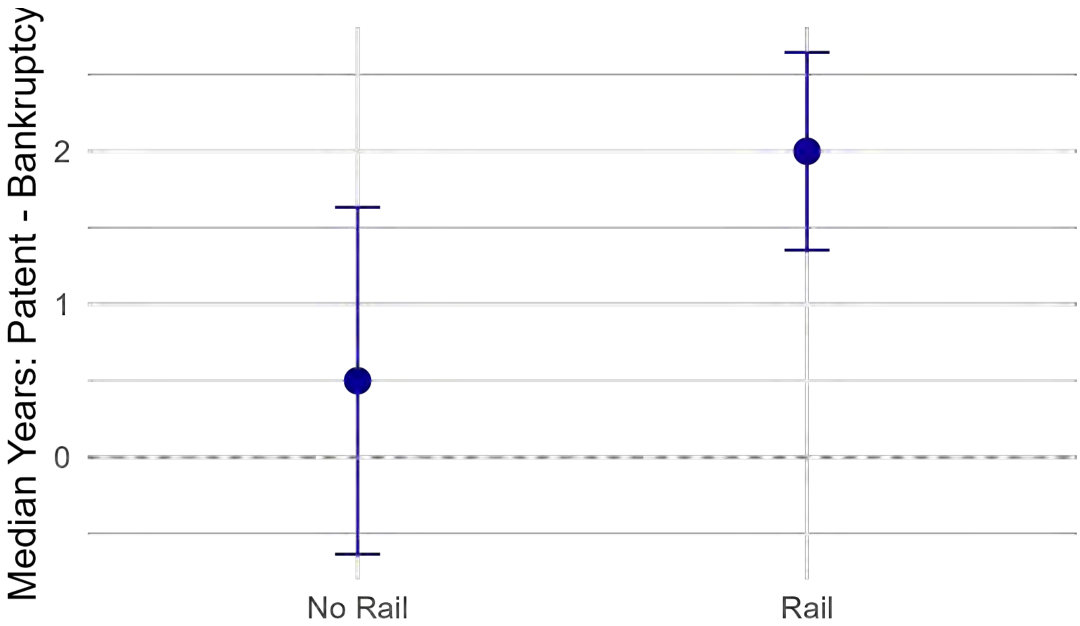


Figure 3.6: Median time in years between a bankruptcy and a patent application in the same sector in a district. "No Rail" includes the districts which are never treated or not yet treated at the time of the bankruptcy announcement.

and measure sector s bankruptcies as the dependent variable:

$$Bankruptcies_{i,s,t} = \alpha_i + \lambda_t + ln(MA_{i,s,t}) + \epsilon_{i,t} \tag{3.6}$$

The coefficient on $ln(MA_{i,s,t})$ (column 1) is strongly significant indicating that there is a positive effect on bankruptcies when the changes to market access account for sectoral differences in how the rail system is connecting local economies. The effect is still present when controlling for local within sector patenting (column 2). The interaction between the within sector patent dummy variable and $ln(MA_{i,s,t})$ in column (3) is also positive and the coefficient is of the same magnitude as that of $ln(MA_{i,s,t})$, indicating that districts see greater levels of bankruptcies when there is both an increase in $MA_{i,s,t}$ and sector s patenting.

These findings further point to a mechanism by which rail fuels a creative destruction effect in the local economy by increasing both patents and bankruptcies in sector s. It is true that, as the bankruptcies and patents are not matched at the firm level, it is possible that this is evidence of an alternate effect wherein the innovative firm "wastes" resources on R&D expenditures inefficiently and then goes bankrupt. However, in Ravalli (2025) I present evidence that the rail effect is not limited to low quality patents; an increase in high quality patents is incongruous

Table 3.6: Effect of Sector Level Market Access on Bankruptcies

	(1)	(2)	(3)
Log Market Access	0.004***	0.004***	0.004***
	(0.000)	(0.000)	(0.000)
Within Sector Patents		0.013***	0.097**
		(0.004)	(0.039)
Log Market Access X Patent			0.004**
			(0.002)
Observations	33600	33600	33600
Districts	175	175	175
Years	1839-1854	1839-1854	1839-1854
Mean of Dep. Var.	0.015	0.015	0.015

Notes: Results of the OLS estimation of equation (3.6). The dependent variable is the number of sector s bankruptcies per 10,000 people in year t and district i. The main independent variable is the log of $MA_{i,s,t}$, defined in (3.2). "Within Sector Patents" is an indicator equal to one if there is at least one sector s patent in district i in the four years leading up to (and including) a sector s bankruptcy. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

with an increase in bankruptcies of the patenting firms.

3.4.2 The Local Labor Market

I next assess changes in the local economy by examining the labor market. I measure the log change in the employment-population ratio between 1851-1861. I regress the change in the labor ratio against the location's rail status in 1843 and 1851:

$$\Delta ln(Labor_{i,t}) = \alpha + Rail_{t-1} + \Delta\epsilon_{i,t} \tag{3.7}$$

I define labor as any employed male above age 20 at the time of the census. Individuals with no known occupation are considered unemployed. I use 1843 as this is the first year in land value data and 1851 as it is the first year with occupational data in the census.

The results are in columns (1)-(4) of Table 3.7. The rail status coefficient is negative and statistically significant in all four instances when measuring employment changes against 1843 and 1851 rail status. At the parish level (columns 1

Table 3.7: Relationship between Rail and the Labor Market

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Has Rail, 1843	-0.034***		-0.065**				
	(0.013)		(0.029)				
Has Rail, 1851		-0.027***		-0.047**			
		(0.008)		(0.023)			
Log Ch. MA, 1851-1861					0.020**		0.020**
					(0.010)		(0.010)
Bankruptcies						-0.072***	-0.072***
						(0.010)	(0.010)
Observations	4169	4169	236	236	2487	2487	2487
R-Squared	0.002	0.003	0.020	0.017	0.002	0.003	0.005
F-Stat.	7.237	11.511	4.827	3.969	4.228	49.341	27.227
Mean of Dep. Var.	0.116	0.116	2.100	2.100	0.105	0.105	0.105
Years	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861
Unit	Parish	Parish	District	District	District	District	District

Notes: Regression coefficient estimates of equation (3.7) where the dependent variable is the log change in the share of employed workers between 1851-1861 in a location in columns (1)-(4). In columns (5)-(7) the dependent variable is the 1851-1861 log change in the share of sector s employment and the independent variables are the log change in $MA_{i,s}$ and the change in the number of sector s bankruptcies between 1851-1854. Robust standard errors are reported. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and 2), the coefficients imply that parishes with a rail station in 1843 and 1851 respectively experience a 3.4% and 2.7% decline in the labor share. At the district level, the equivalent numbers are a 6.5% and 4.7% decline in the labor share. The fact that the coefficients for “Has Rail, 1851” are both smaller suggests that the much of the labor disruption occurred in locations that were treated earlier, perhaps indicating a lag on changes to the local labor markets, though without occupational changes prior to 1851 this is impossible to say.

In columns (5) - (7), I bring equation (3.7) to the sector level. I regress the share of sector s labor on changes to $MA_{i,s}$ between 1851-1861. Given that I cannot control for pre-trends in the labor market, this is essentially just measuring same-period correlation between market access and labor. Nevertheless, the results are instructive.

Changes in $MA_{i,s}$ are positively correlated with changes to the sector s labor share (column 5). Bankruptcies have the expected negative correlation with the within sector labor share (column 6). Including both $MA_{i,s}$ and bankruptcies in the same regression (column 7) does not affect either result, suggesting that neither effect is dominating the other. The bankruptcy coefficient yields an implied elasticity with respect to the labor share of 68.5%, larger than the 20% elasticity given by the coefficient on $MA_{i,s}$. This is in line with the negative effect to the overall labor market seen in columns (1)-(4). However, the theory underpinning Ravalli (2025), which dates back to Sokoloff (1988), is that a firm expands following an increase in market access, so the timing of the events is unlikely to be concurrent (as the bankruptcies should also follow the expansion in market access) and these correlational results should be taken with a grain of salt.

The discrepancy between the overall market and the sector level results points to the possibility that districts experiencing an increase in sector s market access are motivating an influx of migration of sector s workers (and potentially pushing out other workers). Individuals in the census data are not linked across time which makes it impossible to identify movers between censuses. However, Schurer and Higgs (2023) have identified individuals' counties of birth which allows for identification for movers across counties between birth and census. I use this data to assess the relationship between sector level market access changes and moving for the 1851 census. Sectors are matched to the individual's sector of employment:

$$\Pr\left(\text{moved}_{i,s,1851} = 1\right) = \text{logit}^{-1}\left(\alpha + \beta \cdot \Delta \ln(MA_{i,s,1839-1851})\right) \tag{3.8}$$

Equation (3.8) tells us whether an individual working in sector s and district i is more likely to have moved if local sector s market access experienced a bigger increase in the years leading up to the census. I estimate the logistic model and report the results in Table 3.8.

Estimating equation (3.8) without any controls (column 1) or controlling only for age (column 2) produces a negative, statistically significant coefficient. However, the magnitude is small and the pseudo R-Squared is zero to three decimal places, indicating a poor fit to the model. Introducing district fixed effects in column (3) substantially improves the fit, and the sign on the coefficient flips to positive. Including sector fixed effects decreases the coefficient and only marginally increases the pseudo R-Squared value, suggesting that the district fixed effects are the most important control. The coefficients in the columns which include the district fixed effects imply an odds ratio of 14% - 28% for a one unit increase in $\Delta \ln(MA_{i,s,t})$, which is equivalent to a 170% increase in $MA_{i,s,t}$. Due to the data limitations discussed above, it is unknown if these are sector s workers who are

Table 3.8: Relationship between Moving and Sector Market Access Changes

	(1)	(2)	(3)	(4)	(5)
$\Delta ln(MA\_i, s, 1839-1851)$	-0.012***	-0.011***	0.252***	0.135***	0.135**
	(0.004)	(0.004)	(0.012)	(0.014)	(0.054)
Observations	713019	713019	713019	713019	713019
Years	1851-1861	1851-1861	1851-1861	1851-1861	1851-1861
Pseudo R2	0.000	0.000	0.103	0.105	0.105
Errors	Robust	Robust	Robust	Robust	District
Age		Y	Y	Y	Y
District FE			Y	Y	Y
Sector FE				Y	Y

Notes: Logistic regression coefficient estimates of equation (3.8) where the dependent variable is a dummy variable equal to 1 if individual p living in district i and working in sector s in 1851 has moved from his county of birth and zero otherwise. The main independent variable is the log change in $MA_{i,s,1839-1851}$ (the market access changes to the individual's district and sector). Column (2) introduces a control for individual p's age, column (3) introduces district fixed effects, and column (4) introduces sector fixed effects. Robust errors are reported in columns (1)-(4) and errors clustered by district are reported in column (5). * p < 0.1, ** p < 0.05, *** p < 0.01

moving to district i because the increase in market access has caused a sector s boom in employment or if these are individuals who moved to district i and switched jobs into sector s upon arrival. However, these results at least support the possibility that an increase in sector s market access drew sector s workers from outside the district.

3.4.3 Neighboring Districts

I next examine the effect of a rail station on the areas which neighbor a treated district. Changes in a treated district could be either magnified or cancel out when looking at a broader geography. I modify the dependent variable of equation (3.3), such that the various dependent variables are the mean values of district i's neighboring districts. I split the results into two panels in Table 3.9: Panel A only includes neighbors without stations in 1843 and Panel B only includes neighbors with stations.

Column (1) shows the effect of a station in 1843 in district i on the log change in the mean of the neighboring districts' land values per capita between 1843 and 1860. The results are statistically insignificant with large error terms and small magnitudes, indicating that a rail station is not correlated with changes in

Table 3.9: Relationship of Rail and Market Access Changes on Neighboring Districts' Land Values and Labor Markets

	(1)	(2)	(3)	(4)
Panel A: Neighbors without Stations				
Station, 1843	-0.020	0.012		
	(0.029)	(0.022)		
Log Ch. MA, 1851-1861			0.016**	0.017**
			(0.007)	(0.007)
Total Bankruptcies				-0.025***
				(0.004)
Observations	208	208	2495	2495
Years	1851-1861	1851-1861	1851-1861	1851-1861
Mean of Dep. Var.	2.044	1.945	0.090	0.090
R-Squared	0.002	0.001	0.002	0.011
Dep. Var.	Land Value	Employment	Sector Labor Sh.	Sector Labor Sh.
Panel B: Neighbors with Stations				
Station, 1843	0.037	-0.019		
	(0.041)	(0.035)		
Log Ch. MA, 1851-1861			0.012	0.010
			(0.009)	(0.009)
Total Bankruptcies				-0.025***
				(0.005)
Observations	120	120	1438	1438
Years	1851-1861	1851-1861	1851-1861	1851-1861
Mean of Dep. Var.	1.990	1.866	0.090	0.090
R-Squared	0.007	0.002	0.001	0.012
Dep. Var.	Land Value	Employment	Sector Labor Sh.	Sector Labor Sh.

Notes: The dependent variable is the mean log change in: land value (column 1), employment share (column 2), and sector s labor share (columns 3 and 4) in district i's neighbors which do not have a station in 1843 in Panel A and the neighbors which do in Panel B. "Total Bankruptcies" is the number of sector s bankruptcies in district i between 1851-1854. Robust standard errors are reported. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

neighboring land values. Similarly, the results in column (2), which estimates the effect of a station on changes in neighboring districts' labor shares, are statistically insignificant.

Columns (3) and (4) are at the sector level and regress the mean change in the sector s labor share of district i's neighbors on the log change in $MA_{i,s}$ between

1851-1861. The results show that there is a positive correlation between the two variables when looking at the impact on neighbors without stations in Panel A; the value in Panel B is also positive and of a similar magnitude, but statistically insignificant. This suggests that when $MA_{i,s}$ increases, the labor market is affected beyond the boundaries of district i (though I do not claim to show a causal relationship). Column (4) includes total sector s bankruptcies between 1851-1854 in district i as an independent variable. As expected, the individual terms go in opposite directions in both Panels; while an increase in $MA_{i,s}$ is correlated with an increase in sector s labor, sector s bankruptcies in district i are negatively correlated with that labor.

3.5 Robustness Exercises

I present here some additional robustness exercises for the main results. Tables and graphs for this section can be found in the appendix.

3.5.1 Land Values

By 1843 local rail access may have already been effectively "priced in" to land values everywhere. Figure C.4 (in the Appendix) shows that in 1829, a year before the start of the Rail Age, the median distance between the nearest local rail station and a district's geographic center is approximately 300KM. The median falls dramatically a year later, but it is still 32KM in 1839. By 1843, the median is only 15.2KM. While this value does continue to decrease to 3.4KM in 1860, proximity to a rail station may be low enough in enough places by 1843 that the changes in land value over the next 17 years due to rail proximity are marginal. One issue with this theory is that most of the decrease in freight costs comes after 1843. In Ravalli (2025), I show that the majority of the decrease in the cost of moving a ton of goods between district pairs occurs after 1843 (though a substantial amount also occurs prior to 1843). This goes against the theory that access to a nearby rail station would already have been priced in to local land values by 1843.

3.5.2 Bankruptcies

For the OLS and Poisson TWFE columns in Table 3.4, I cluster errors by district, the unit of analysis. Clustering the error term by county, which is a larger geographic unit than district, does not change the results. Additionally, interacting the rail station variable with an indicator equal to one for all years after the passage of a new bankruptcy law in 1844 reveals that the new law had no impact on how rail stations affected bankruptcies.

It is possible that bankruptcies are only a good barometer of economic activity during downturns. During boom periods, the number of bankruptcies may be unlikely to be informative of local conditions. The process by which railroads could cause fewer bankruptcies to occur during booms is unclear; the construction of a local rail station may not decrease bankruptcies at all simply because there are already few bankruptcies. The interaction between the rail station variable and an indicator equal to one for the recessionary years (1839-1840, 1845-1846, 1847-1848) is negative and weakly significant, perhaps suggesting that places which had a rail station in those years were better protected from negative macroeconomic forces. However, there is not enough power to estimate the regression using only the recessionary years.

3.6 Conclusion

The evidence presented in this paper shows that railroads in 19th Century England had a mixed effect on local economies. Generally, there was no effect on land values or bankruptcies, and a negative effect on the employment share of population. At the sector level, bankruptcies appear to be linked to local patents, which are in turn affected by increases in sector level market access. Linking the patent dataset to the bankruptcy dataset would offer a more detailed view of this mechanism and is an avenue for future research.

These results help shed light on the effect of transportation infrastructure at the local level. While the railroad had unquestionable benefits in connecting local economies and boosting domestic trade, the evidence suggests that in the short and medium term this transportation revolution was disruptive enough to offset local gains.

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Appendices

A.1 Chapter 1 Appendix

A.1.1 Additional Figures

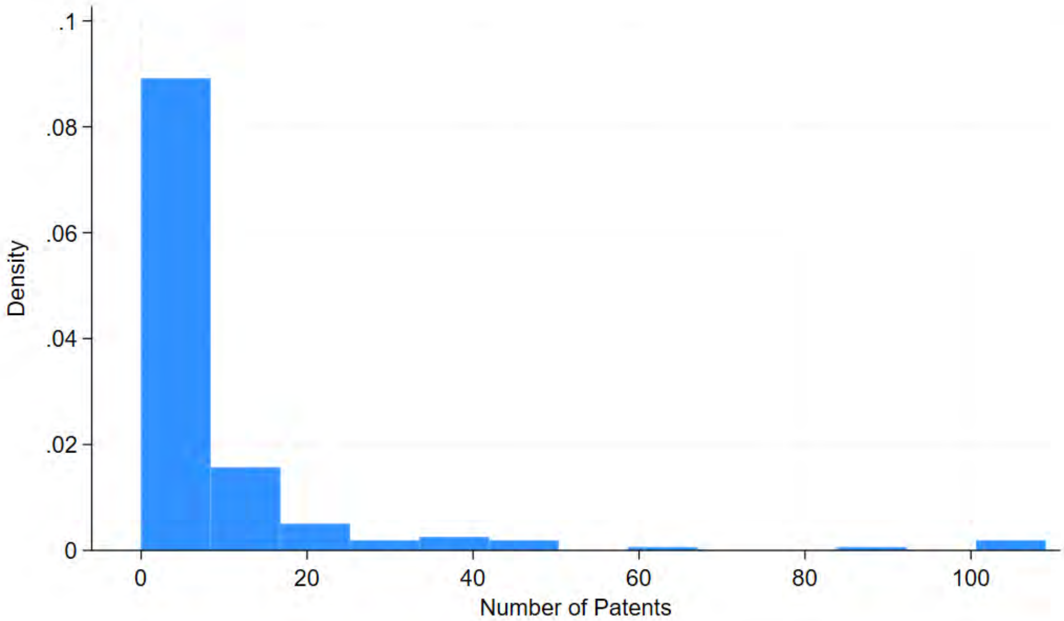


Figure A.1: Histogram of patents produced in a district aggregated across 1823-1861. Only districts in the main treatment definition are included.

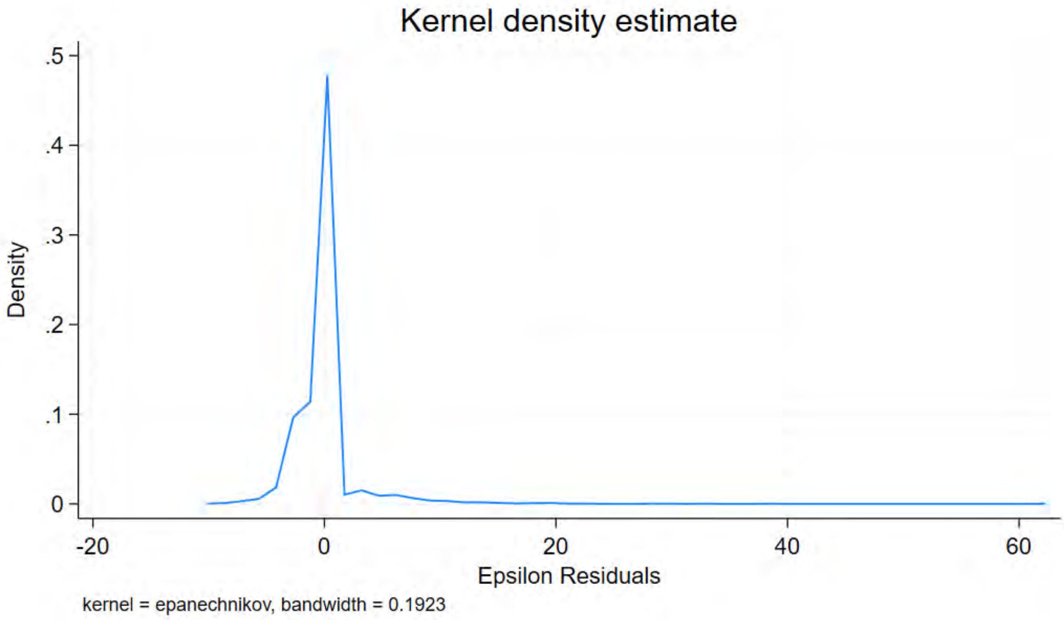


Figure A.2: Kernel density graph of epsilon residuals from the OLS estimation of equation (1.1) where the dependent variable is patents per capita. Only districts in the main treatment definition are included.

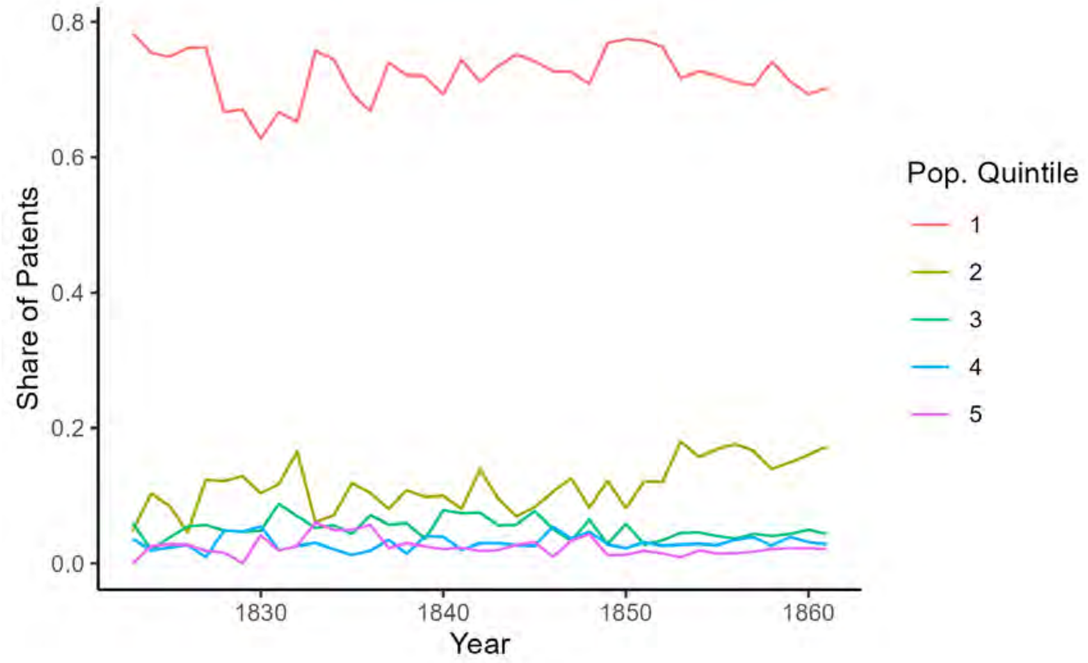


Figure A.3: Share of patenting across time by the 1821 population deciles. The most populous quintile is “1” and the least populous is “5”. Includes all districts.

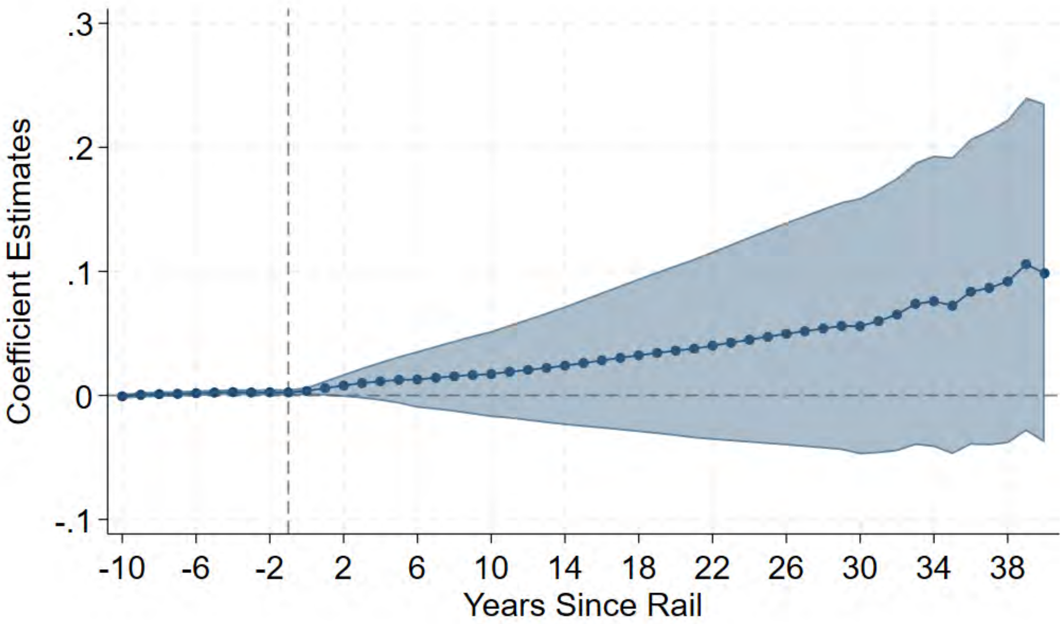


Figure A.4: Event study plot with imputed population as the dependent variable and 1823-1891 as the time period. Districts are treated in the year in which they receive a rail station. Because of limitations to the rail data, districts which are not yet treated by 1861 are considered as "never treated" districts. I use the difference-in-difference estimator proposed by Callaway and Sant'Anna (2021) to correctly estimate the "never treated" control group.

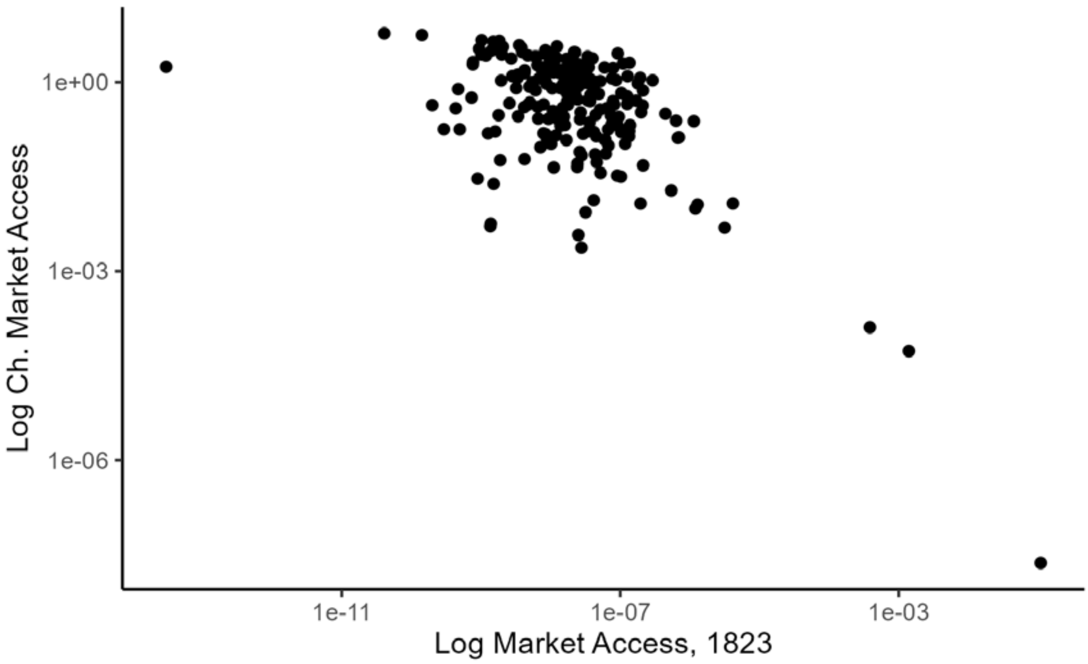


Figure A.5: Scatter plot of the log increase in districts' market access between 1823-1861 by districts' log market access in 1823. Only districts in the main treatment definition are included.

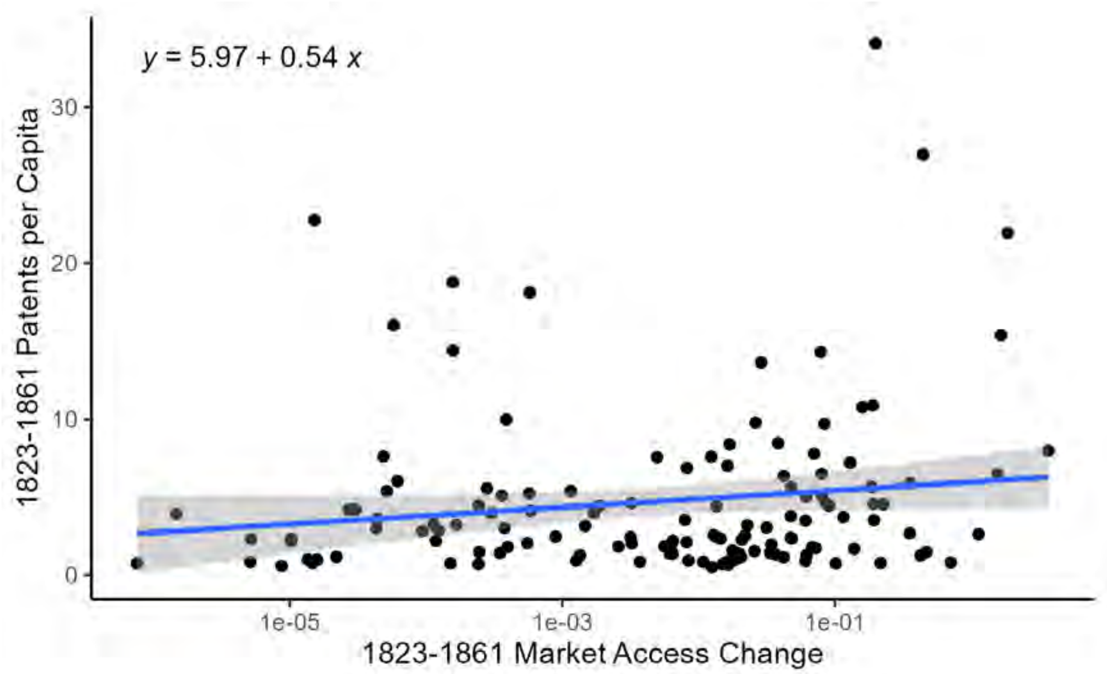


Figure A.6: Scatter plot of the log increase in districts' market access between 1823-1861 by districts' log change in patents between 1823-1861. The stock of patents between 1824-1828 and 1857-1861 is used to measure patents at the beginning and end of the period. I use an IHS transformation to get the log of patents. Only districts in the main treatment definition are included. A fitted polynomial is drawn through the points dictated by the equation in the top left corner.

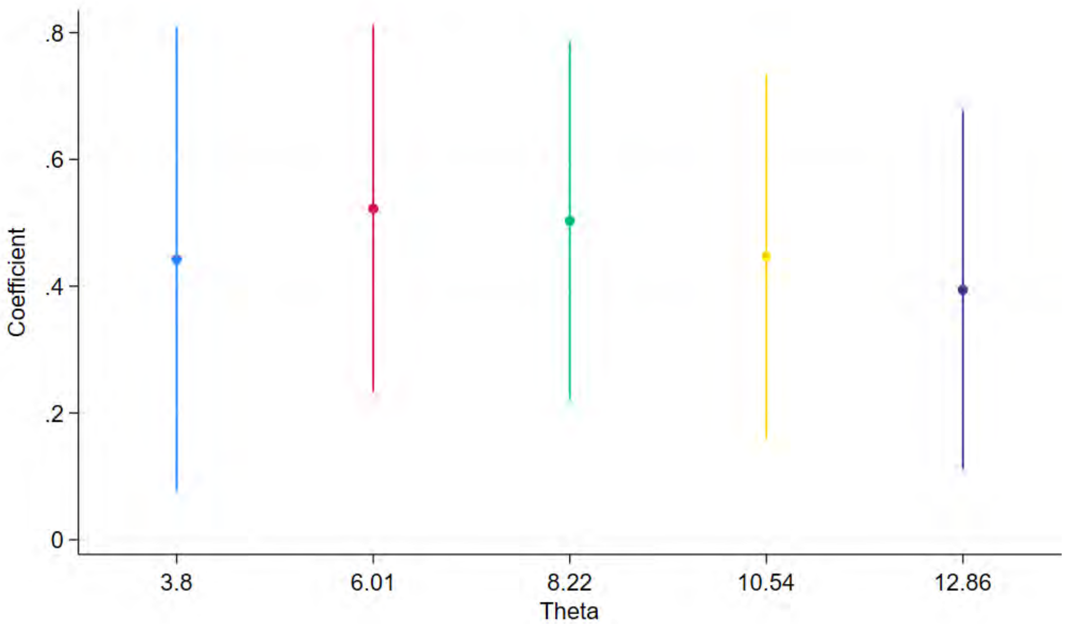


Figure A.7: Estimation of equation (1.3) using different values of theta. Errors are clustered by district. 3.8 and 12.86 are the upper and lower bounds suggested by Donaldson and Hornbeck (2016) and 8.22 is the main value the authors use following a structural estimation of the parameter. 6.01 and 10.54 are chosen as midpoints between the boundary points and 8.22.

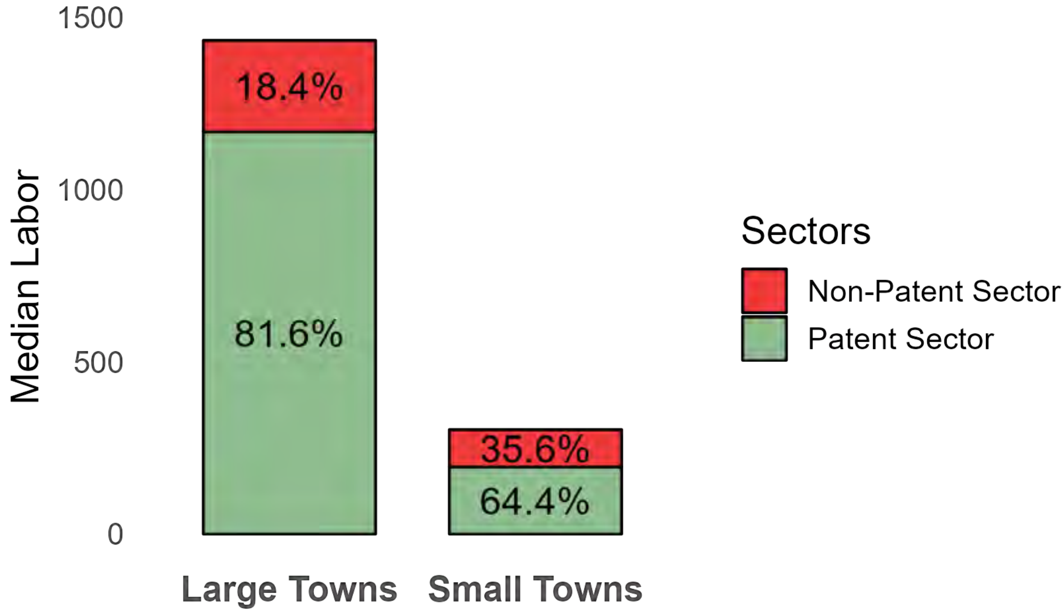


Figure A.8: Median labor across districts and sectors divided by population size. The left bar included districts in the top quintile by 1831 population and and the right bar includes all other districts. The green bars include only labor within sectors that the district produces at least one patent between 1840-1861 and the red bars include the labor in all other sectors. Only districts in the main treatment sample are included and only sectors in the top half by number of total number of national patents across 1823-1861 are included.

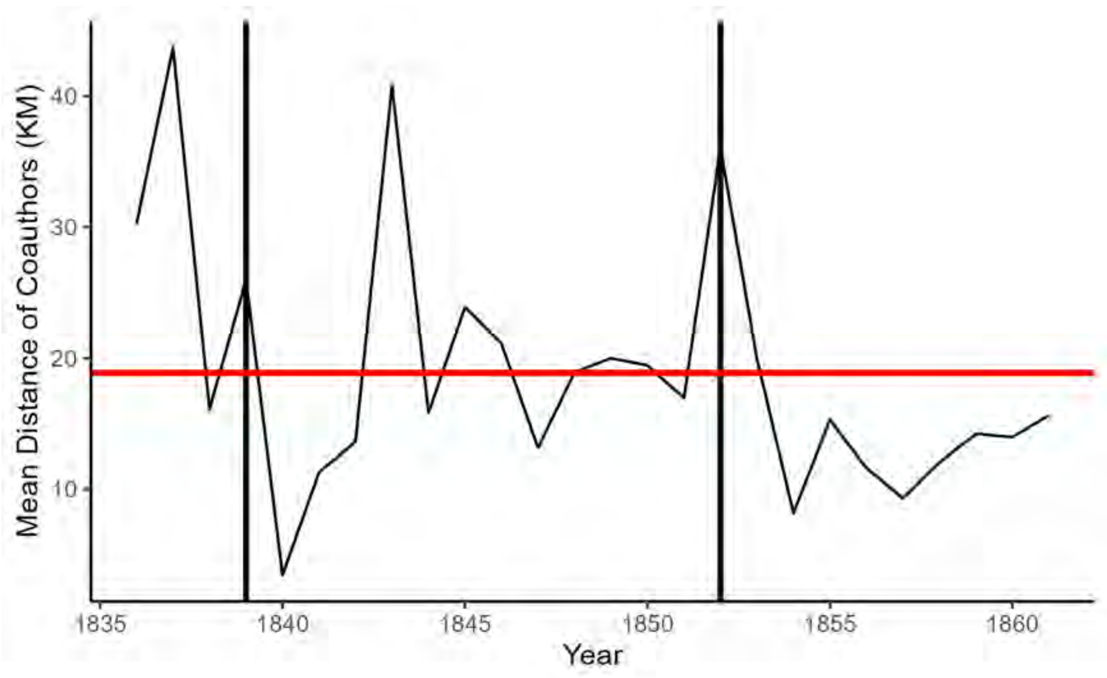


Figure A.9: Mean distance between patent coauthors in years with at least 20 patents in which multiple authors are listed as applicants. The vertical line in 1838 represents the Postage Act of 1838 which decreased mail based communication costs and the vertical line in 1852 represents the Patent Law Amendment Act of 1852 which decreased the cost of patenting. The red horizontal line is the mean distance between coauthors across all years included in the graph.

A.1.2 Additional Tables

Table A.1: Patent Applicants by Sector – Part 1

Sector	All Patents	Main Patents	Sh. of Nat. All	Sh. of Nat. Main	Sh. of Nat. All w/o Ag.	Sh. of Nat. Main w/o Ag.
barrels	217	12	0.9	0.72	0.95	0.84
bottle manufacture	18	1	0.07	0.06	0.08	0.07
carding	92	5	0.38	0.3	0.4	0.35
carpets	24	6	0.1	0.36	0.11	0.42
carriages	334	43	1.39	2.57	1.46	3.02
ceramic glass	409	31	1.7	1.85	1.79	2.17
chemistry	743	28	3.09	1.67	3.25	1.96
clocks	137	9	0.57	0.54	0.6	0.63
clothing	1958	111	8.15	6.63	8.58	7.78
construction	558	36	2.32	2.15	2.44	2.52
distilling	170	16	0.71	0.96	0.74	1.12
dyes	177	2	0.74	0.12	0.78	0.14
energy	653	35	2.72	2.09	2.86	2.45
engines	2275	153	9.47	9.14	9.97	10.73
fertilizer	88	7	0.37	0.42	0.39	0.49
fire safety	354	18	1.47	1.08	1.55	1.26
firearms	657	45	2.74	2.69	2.88	3.16
fluids water	131	14	0.55	0.84	0.57	0.98
food	391	41	1.63	2.45	1.71	2.88
furniture	452	29	1.88	1.73	1.98	2.03
gas liquid_meter	85	0	0.35	0	0.37	0
gas manufacture	18	0	0.07	0	0.08	0
implements goods_other	791	45	3.29	2.69	3.47	3.16
india rubber	88	9	0.37	0.54	0.39	0.63
infrastructure	137	12	0.57	0.72	0.6	0.84
light	352	11	1.47	0.66	1.54	0.77
locks	199	5	0.83	0.3	0.87	0.35

Note: Summary of patent applicants by their patents' sectors. Columns 1, 3, and 5 include patents from all districts. Columns 2, 4, and 6 only include patents in the districts included in the main treatment definition (and the never treated districts). Columns 1 and 2 report the total number of patents between 1823-1861 in each sector. Columns 3 and 4 report the sector's share of national patents. Columns 5 and 6 report the sector's share of national patents excluding agriculture from the calculation. Includes only sectors A-L.

Table A.2: Patent Applicants by Sector – Part 2

Sector	All Patents	Main Patents	Sh. of Nat. All	Sh. of Nat. Main	Sh. of Nat. All w/o Ag.	Sh. of Nat. Main w/o Ag.
machinery	916	54	3.81	3.23	4.01	3.79
measuring navigation	310	15	1.29	0.9	1.36	1.05
medical	208	12	0.87	0.72	0.91	0.84
metallurgy	1627	91	6.78	5.44	7.13	6.38
mining	107	4	0.45	0.24	0.47	0.28
music	225	13	0.94	0.78	0.99	0.91
oils	301	10	1.25	0.6	1.32	0.7
paper printing	880	72	3.66	4.3	3.86	5.05
photography	60	6	0.25	0.36	0.26	0.42
plumbing pumps	466	19	1.94	1.14	2.04	1.33
preserving	51	4	0.21	0.24	0.22	0.28
rail	1130	76	4.71	4.54	4.95	5.33
refrigerators	36	0	0.15	0	0.16	0
rope	45	1	0.19	0.06	0.2	0.07
sea	21	1	0.09	0.06	0.09	0.07
shipbuilding	915	52	3.81	3.11	4.01	3.65
shoes	182	9	0.76	0.54	0.8	0.63
spinning	936	54	3.9	3.23	4.1	3.79
sugar	67	5	0.28	0.3	0.29	0.35
tanning leather	72	3	0.3	0.18	0.32	0.21
telegraphs	266	13	1.11	0.78	1.17	0.91
textiles	1052	58	4.38	3.46	4.61	4.07
timber	154	8	0.64	0.48	0.67	0.56
weaving	1148	115	4.78	6.87	5.03	8.06
weighing	20	5	0.08	0.3	0.09	0.35
wire	11	0	0.05	0	0.05	0
writing implements	113	2	0.47	0.12	0.5	0.14
agriculture	1185	248	4.94	14.81	NA	NA
Median	221	12.5	.92	.75	.95	.84

Note: Summary of patent applicants by their patents' sectors. Columns 1, 3, and 5 include patents from all districts. Columns 2, 4, and 6 only include patents in the districts included in the main treatment definition (and the never treated districts). Columns 1 and 2 report the total number of patents between 1823-1861 in each sector. Columns 3 and 4 report the sector's share of national patents. Columns 5 and 6 report the sector's share of national patents excluding agriculture from the calculation. Includes only sectors M-Z. The median calculation is for the full table.

Tables for Robustness Exercises for Main Results

Table A.3: Econometric Robustness Tests

	(1)	(2)	(3)	(4)	(5)
Rail Access	0.902***	0.202***	0.306**	0.814***	0.571***
	(0.253)	(0.053)	(0.137)	(0.200)	(0.124)
Observations	6864	6864	6864	10374	6864
Districts	176	176	176	266	169
Years	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861
Mean of Dep. Var.	1.085	0.336	0.128	0.874	0.247
Test	OLS	IHS	Indicator	SLS	County Cluster
Kleibergen-Paap Wald rk F				4802.059	

Notes: Robustness exercises for the results presented in Table 1.2. The main independent variable in all columns is an indicator equal to one if district i has a rail station in year t. Column (1) reports the results of an OLS estimation in which the dependent variable is patents per 100,000 people. Column (2) uses the IHS transformation of patents per capita as the dependent variable. Column (3) estimates a Poisson model and uses an indicator equal to one if there is at least one patent in district i in year t as the dependent variable. Column (4) reports the results of a SLS regression using the "Accidentally Connected" treatment definition which excludes rail end points and hubs. The simulated lines are used as the instrument to estimate the treatment status of each district. The treatment year is the actual year of treatment. Column (5) estimates a Poisson regression where the errors are clustered by county, rather than by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Definition and Sample Robustness Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rail	0.571***	0.459**	0.491**	0.496***	0.611***	0.494***	0.019*
	(0.139)	(0.208)	(0.215)	(0.139)	(0.154)	(0.150)	(0.011)
Rail X Board of Trade					-0.133		
					(0.346)		
Rail X Mania						0.420	
						(0.350)	
Observations	6864	3451	6864	5226	6864	6864	7553
Clusters	176	119	176	134	176	176	286
Years	1823-1861	1823-1851	1823-1861	1823-1861	1823-1861	1823-1861	1823-1851
Mean of Dep. Var.	0.247	0.123	0.247	0.284	0.247	0.247	0.044
Test	Main	Pre 1852	Alt. Geo.	Alt. Never	BoT	Mania	Indiv.
Year FE	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	
Individual FE							Y

Notes: This table shows estimation results with varying samples and definitions. In columns (1)-(5) equation (1.1) is estimated with a Poisson model where the dependent variable is patents per capita and the main independent variable is an indicator equal to one if a district has a station in year t. Column (1) reports the baseline Poisson results reported in Table 1.2. Column (2) uses a sample that spans 1823-1851, excluding years after the Patent Law Amendment Act. Column (3) utilizes an alternate geographic boundary to define treatment of a district. A 3KM radius circle is drawn around each station and treatment is assigned to districts that constitute the majority of the circle or districts with the highest percentage of their land within the circle. Column (4) excludes districts that are treated after 1861 from the control group. Column (5) includes an interaction term between the rail indicator variable and an indicator equal to one if a district is treated by a rail line approved by the 1845 Board of Trade. Column (6) includes an interaction term between the rail indicator variable and an indicator equal to one if the district was treated during 1839-1840 or 1846-1847, the years at the height of Rail Mania. Columns (7) reports OLS estimates of equation (1.10) which uses a panel of individual inventors, rather than districts. The dependent variable is an indicator equal to one if the inventor issues a patent application in year t, zero otherwise. Individual and year fixed effects are included as independent variables in both models as is a vector of population controls. Errors are clustered by individual in column (7) and clustered by district in all other columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Sector Robustness Tests

	(1)	(2)	(3)	(4)
Rail Access	0.571***	0.574***	0.687***	0.567***
	(0.139)	(0.152)	(0.152)	(0.137)
Observations	6864	6630	6045	6864
Districts	176	170	155	176
Years	1823-1861	1823-1861	1823-1861	1823-1861
Sector	All	No Rail Adjacent	No Agriculture	Rail Included

Notes: Poisson estimation results of equation (1.1) where the dependent variable is patents per capita and the main independent variable is an indicator equal to one if a district has a station in year t. Column (1) reports the baseline results reported in Table 1.2. Column (2) removes patents in sectors that are adjacent to rail (engines and carriages) and column (3) removes agricultural patents. Column (4) retains patents from all sectors including rail, which are otherwise excluded from all other analyses. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Effect of Rail Network Access on Population Growth

	(1) Population	(2) Population	(3) Log Population	(4) Log Population
Rail Access	609.051*	609.051*	0.029**	0.029**
	(362.542)	(336.691)	(0.013)	(0.014)
Observations	6864	6864	6864	6864
Districts	176	176	176	176
Years	1823-1861	1823-1861	1823-1861	1823-1861
Mean of Dep. Var.	18071.541	18071.541	9.699	9.699
Cluster	District	County	District	County

Notes: OLS estimation based on equation (1.1) where the dependent variable is population in columns (1) and (2) and log population in columns (3) and (4). The main independent variable is an indicator equal to one if district i has a rail station in year t. Errors are clustered by district in columns (1) and (3) and by county in columns (2) and (4). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Relationship between Railroad Stations and World's Fair Exhibitions

	(1)	(2)	(3)	(4)
Rail Station	0.738*	0.781*	0.680**	0.735**
	(0.401)	(0.401)	(0.302)	(0.302)
Observations	214	214	214	214
Mean of Dep. Var.	1.350	1.350	0.318	0.318
Pseudo R-Squared	0.033	0.037	0.019	0.023
Station Year	1851	1850	1851	1850
Model	Poisson	Poisson	Logit	Logit

Notes: Poisson and logit model estimates where the dependent variable is the number of exhibitions per capita for a district in the 1851 World's Fair in columns (1) and (2) and a dummy variable equal to one if a district has at least one award in column (3) and (4). In columns (1) and (3) a district is considered to have a rail station if it has one in 1851 and in columns (2) and (4) if it has one in 1850. One observation is equal to one district; the number of observations differs from other analyses as I include districts from the Main sample that had zero patents in all years. Robust errors are reported. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Effect of Postage Cost Reduction

	(1)	(2)	(3)	(4)
Rail	0.490***	0.344	-0.558	-5.248*
	(0.123)	(0.689)	(0.878)	(2.910)
Rail X Distance to London		0.031		0.889
		(0.147)		(0.551)
Rail X Post 1839			1.059	5.884**
			(0.892)	(2.881)
Rail X Distance to London X Post 1839				-0.918*
				(0.548)
Observations	8658	8658	8658	8658
Districts	222	222	222	222
Years	1823-1861	1823-1861	1823-1861	1823-1861

Notes: Poisson estimation of equation (1.1) where the dependent variable is the number of patents per capita where population is controlled for as an exposure variable. The main independent variables are "Rail," which is an indicator equal to one for years in which district i has a rail station, "Distance to London," which is the distance between district i and London, and "Post 1839," which is an indicator equal to one if the year is after 1839. The treatment definition used in all columns is the "No Ends or Rail Hubs" definition due to lack of variance in the main definition to handle the interaction effects. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Tables for Robustness Exercises for Market Access

Table A.9: Effect of Market Access on Innovation - Navigable Waterways and Market Size

	(1)	(2)	(3)	(4)	(5)
Log Market Access	0.145**	0.181**	0.251***	0.330***	0.511***
	(0.066)	(0.074)	(0.073)	(0.081)	(0.130)
Observations	18642	16419	8892	8190	6864
Districts	478	421	228	210	176
Years	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861
Definition	All	Top Income	Waterways	Both	Main

Notes: Poisson estimation of (1.3) where the dependent variable is log patents per capita and the main independent variable is log market access. Column (1) includes all districts; column (2) excludes districts in the top decile by 1815 income level; column (3) excludes districts with immediate access to the main navigable waterway (defined as the waterway passing within a district's geographic boundaries); column (4) excludes all districts excluded in columns (2) and (3); column (5) uses the main treatment definition. Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Effect of Market Access on Innovation - Various Robustness Exercises

	(1)	(2)	(3)	(4)	(5)
Log Market Access	0.503***	0.443**	0.394***	0.466***	0.815***
	(0.144)	(0.187)	(0.145)	(0.128)	(0.256)
Observations	6864	6864	6864	6864	528
Districts	176	176	176	176	176
Years	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861
GDP def.	Land Value	Land Value	Land Value	Pop.	Land Value
Theta	8.22	3.8	12.86	8.22	8.22

Notes: Various robustness exercises on the market access mechanism. Columns (1) - (3) report Poisson estimates of equation (1.3) where the dependent variable is log patents per capita and the main independent variable is log market access. Column (1) reports the baseline estimate with a value of θ equal to 8.22 and columns (2) and (3) use values of θ on either end of the scale proposed by Donaldson and Hornbeck (2016). Column (4) uses districts' 1821 population as the proxy for GDP in the definition for market access. Column (5) estimates a stacked OLS regression model where each period is ten years. The dependent variable is the change in patents per capita between t and t+1 and the main independent variable is the log change in market access between t-1 and t. Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Effect of Market Access on Innovation - Geographic Variation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log Market Access	0.600***	0.441***	0.521***	0.459***	0.501***	0.618***	0.544***	0.480***	0.524***	0.419***
	(0.149)	(0.118)	(0.136)	(0.128)	(0.131)	(0.151)	(0.137)	(0.140)	(0.140)	(0.129)
Observations	5814	5928	6396	6786	6786	5694	5967	5460	6279	6513
Districts	153	152	164	174	174	146	153	140	161	167
Years	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861	1823-1861

Notes: Poisson estimation results based on equation (1.3) where the dependent variable is the log of patents in district i and year t and the main independent variables is log market access. Each column removes all districts contained in one of the ten Census Divisions. Results can be compared with the baseline result in Table 1.4, which is a coefficient of .511. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Tables for Robustness Exercises for Knowledge Flows

Table A.12: Effect of Knowledge Flows on Patenting - High Patent Count Sectors

	(1)	(2)	(3)	(4)
Log Knowledge Flows	0.336***	0.240***	0.146***	0.333***
	(0.073)	(0.090)	(0.031)	(0.074)
Rail				0.181
				(0.193)
Observations	96278	96278	272090	96278
Districts	161	161	455	161
Years	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	All	Main
Sector FE		Y		

Notes: This table replicates Table 1.8, but restricts sectors to those in the top half by patent count in the 1823-1839 period. Each column shows the results of the Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Column (2) includes sector fixed effects. Columns (3) includes all districts while all other columns include only districts which comprise the main treatment definition. Column (4) includes the rail treatment variable $D_{i,t}$ as a control. As $KF_{i,s,t}$ is calculated using patents from 1823-1839, I restrict the data to 1840-1861. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Effect of Knowledge Flows on Patenting - Aggregated Sectors

	(1)	(2)	(3)	(4)
Log Knowledge Flows	0.820***	0.576***	0.904***	0.821***
	(0.097)	(0.220)	(0.028)	(0.098)
Rail				-0.020
				(0.227)
Observations	49933	49933	139035	49933
Districts	167	167	465	167
Years	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	All	Main
Sector FE		Y		

Notes: Results of Poisson estimation of equation (1.7) where the dependent variable is the log of sector $\overline{s}$ patents in district i and year t and the main independent variable is log $KF_{i,\overline{s},t}$. Column (2) includes sector fixed effects, column (3) includes districts from all of England, and column (4) includes the rail treatment variable $D_{i,t}$ as a control. As $KF_{i,\overline{s},t}$ is calculated using patents from 1823-1839, I restrict the data to 1840-1861 in all columns. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Effect of Knowledge Flows on Patenting - Geographic Variation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log Knowledge Flows	0.200***	0.213***	0.226***	0.192***	0.169***	0.200***	0.199***	0.200***	0.206***	0.191***
	(0.030)	(0.037)	(0.037)	(0.031)	(0.032)	(0.031)	(0.031)	(0.030)	(0.032)	(0.030)
Observations	493218	514206	454740	500214	504878	513040	465234	487388	469898	502546
Districts	423	441	390	429	433	440	399	418	403	431
Years	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861

Notes: Results of Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Each column removes all districts in one of the ten Census Divisions of England. As $KF_{i,s,t}$ is calculated using patents from 1823-1839, I restrict the data to 1840-1861. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Effect of Knowledge Flows on Patenting - Updating Years

	(1)	(2)	(3)	(4)
Log Knowledge Flows	0.534***	0.237***	0.265***	0.535***
	(0.067)	(0.069)	(0.039)	(0.067)
Rail				-0.082
				(0.236)
Observations	191565	175890	537532	191565
Districts	165	165	463	165
Years	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	All	Main
Sector FE		Y		

Notes: Results of the Poisson estimation of equation (1.7) where the dependent variable is the log of sector s patents in district i and year t and the main independent variable is log $KF_{i,s,t}$. Columns (2) includes sector fixed effects. Columns (3) includes all districts while all other columns include only districts which comprise the main treatment definition. Column (4) includes the rail treatment variable $D_{i,t}$ as a control. Unlike the main definition of $KF_{i,s,t}$, $KF_{i,s,t}$ is calculated here using a rolling stock of sector s patents from the five years leading up to year t. I restrict the data to 1840-1861 to allow comparisons with the main results in Table 1.8. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Comparison of Market Access and Knowledge Flows

Table A.16: Effect of Market Access and Knowledge Flows on Patenting

	(1)	(2)	(3)	(4)	(5)	(6)
Log Market Access	0.481***		0.885***	0.482***		-0.164
	(0.119)		(0.177)	(0.118)		(0.162)
Log Knowledge Flows		0.124	-0.274***		0.427***	0.433***
		(0.082)	(0.090)		(0.062)	(0.064)
Observations	3841	3841	3841	203573	203573	203573
Districts	167	167	167	167	167	167
Years	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861	1839-1861
Definition	Main	Main	Main	Main	Main	Main
Pseudo R2	0.457	0.454	0.459	0.195	0.251	0.252

Notes: Poisson coefficient estimates based on equation (1.3) where the dependent variable is the log of patents per capita in columns (1) - (3) and column (7) and the log of sector s patents per capita in columns (4) - (6). The main independent variables are log $MA_{i,t}$ and log $KF_{i,t}$ in columns (1) - (3) and log $MA_{i,t}$ and log $KF_{i,s,t}$ in columns (4) - (6). Errors are clustered by district. * p < 0.1, ** p < 0.05, *** p < 0.01

A.1.3 Maps

Patents by District, 1823-1835

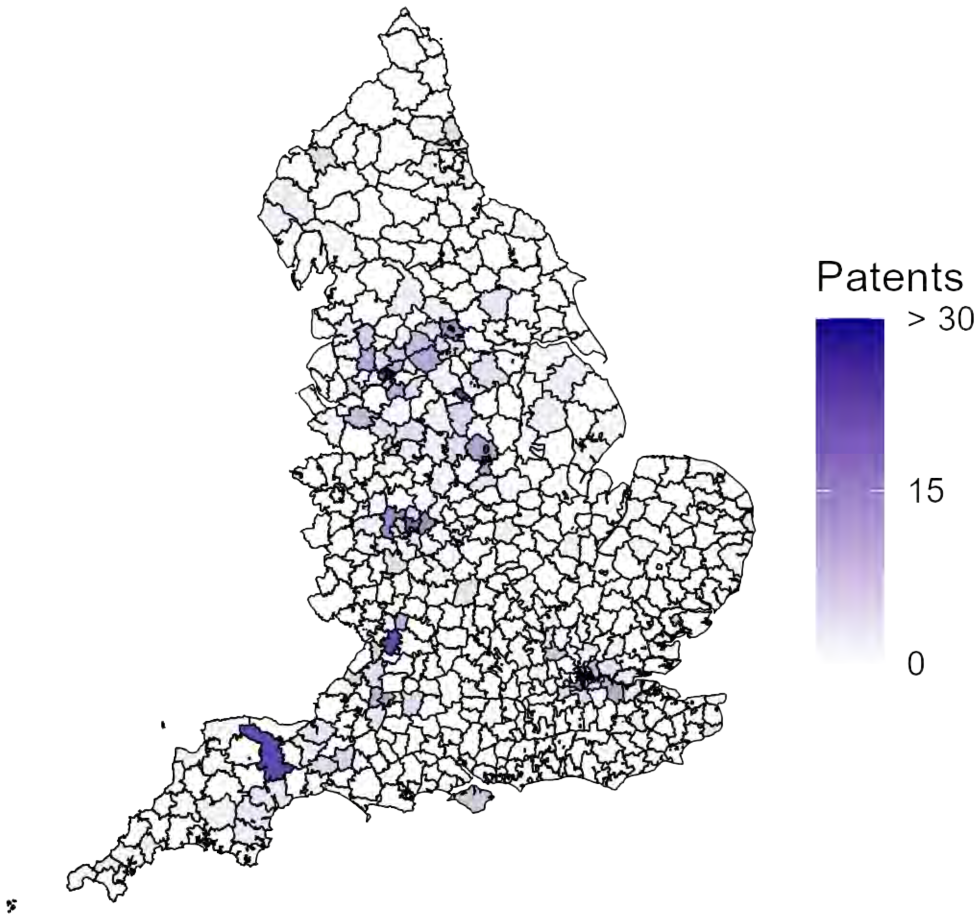


Figure A.10: Heat map of all patents between 1823-1835 by district.

Patents by District, 1836-1848

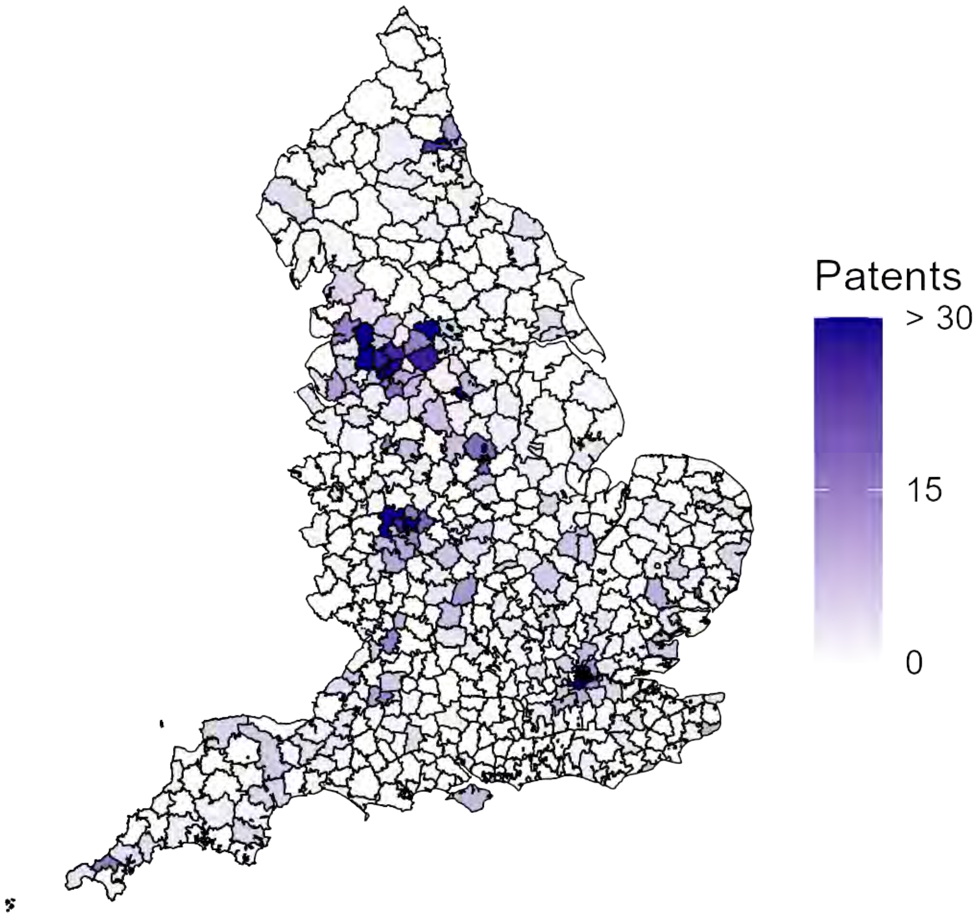


Figure A.11: Heat map of all patents between 1836-1848 by district.

Patents by District, 1849-1861

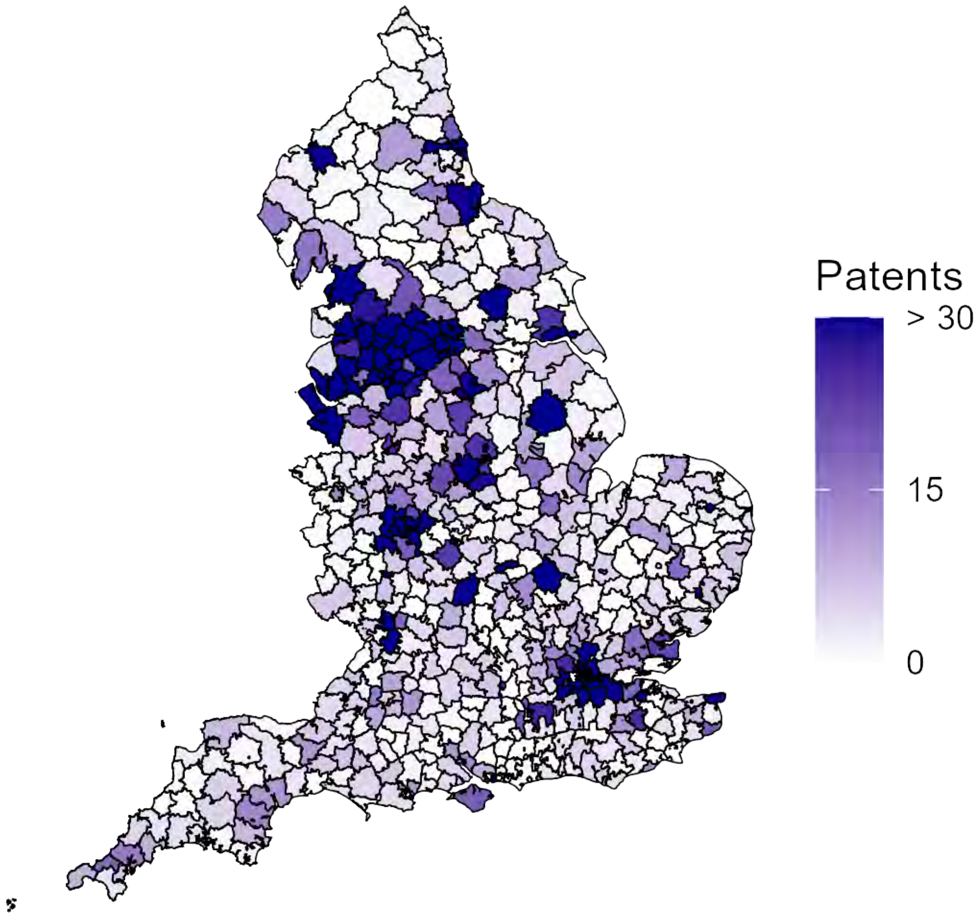


Figure A.12: Heat map of all patents between 1849-1861 by district.

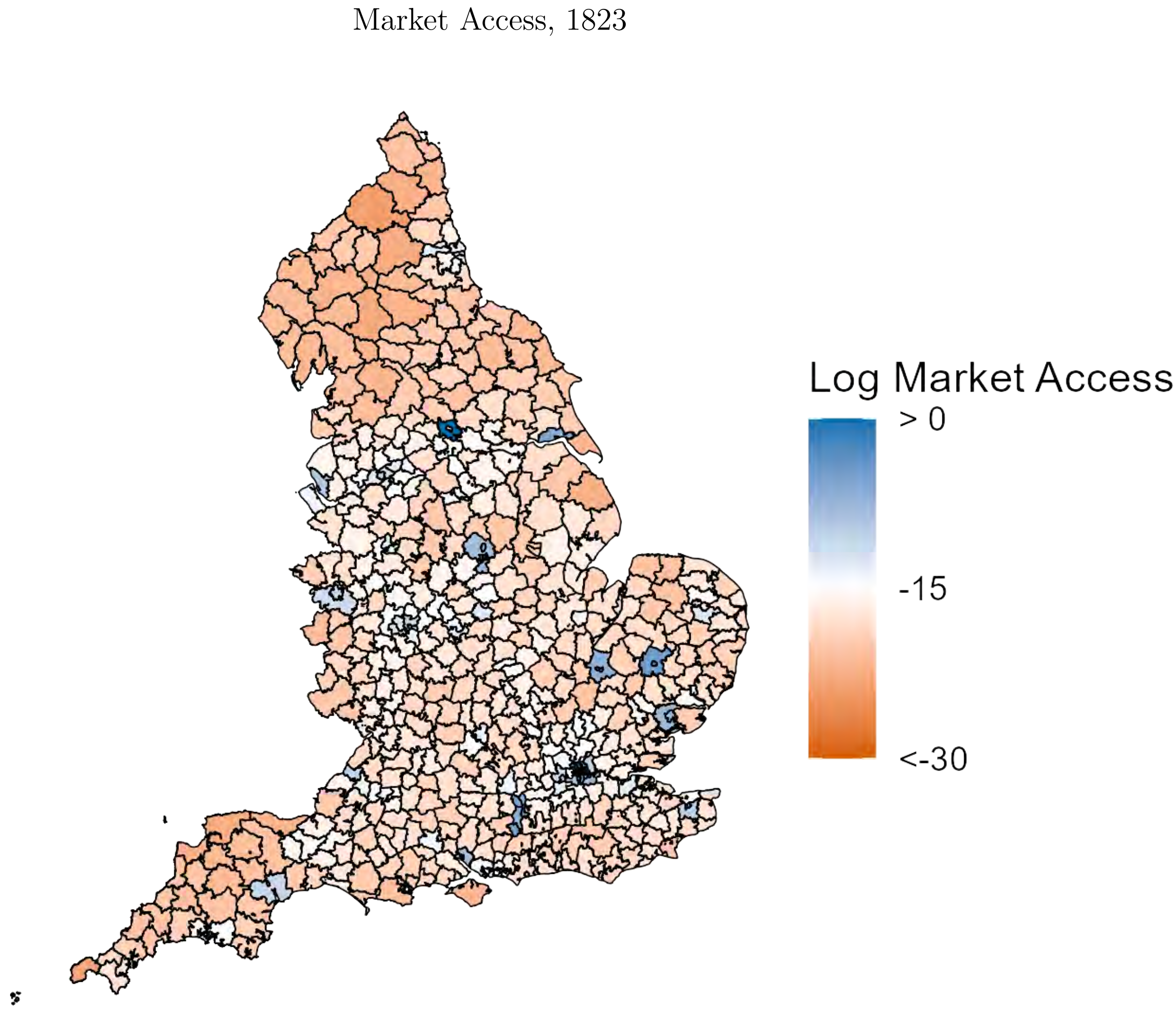


Figure A.13: Log market access value for every district in England in 1823 by district.

Market Access, 1841

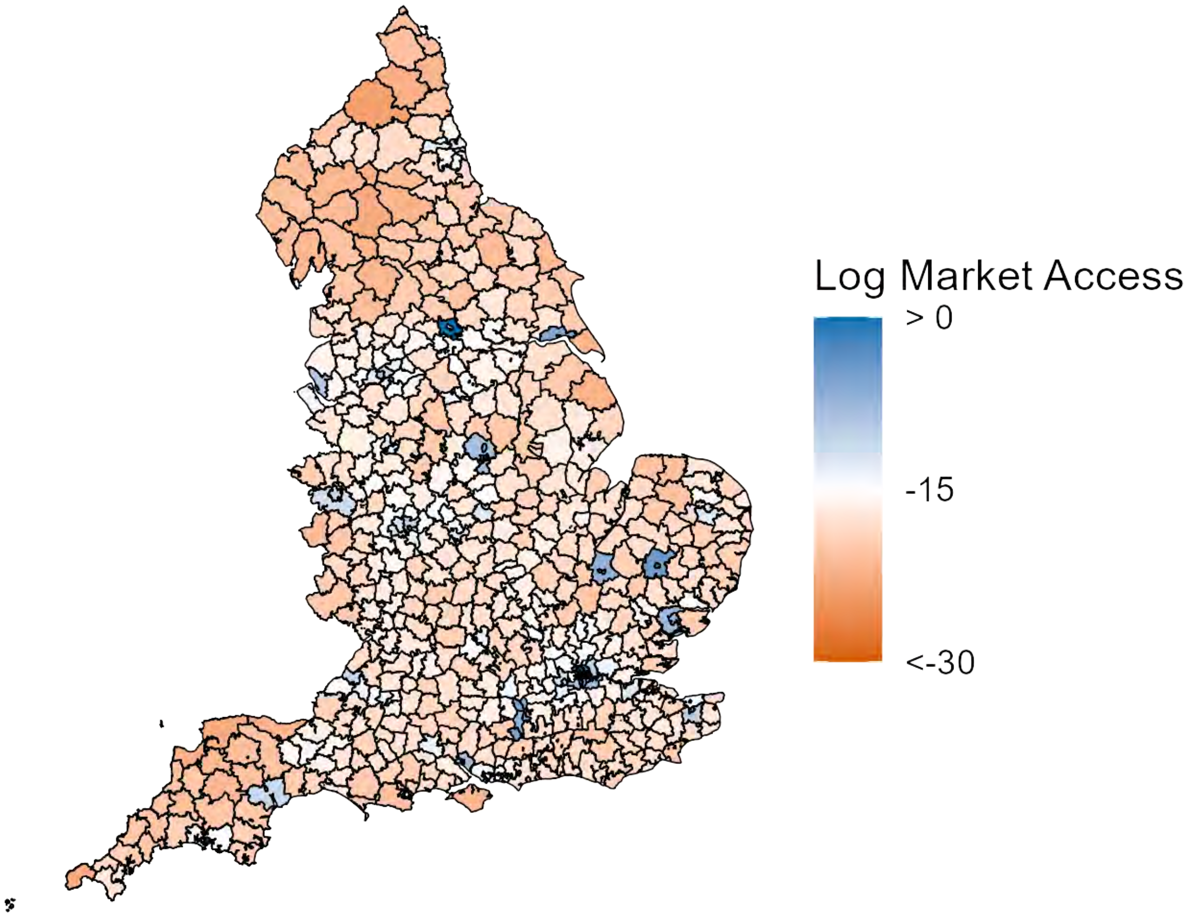


Figure A.14: Log market access value for every district in England in 1841 by district.

Market Access, 1861

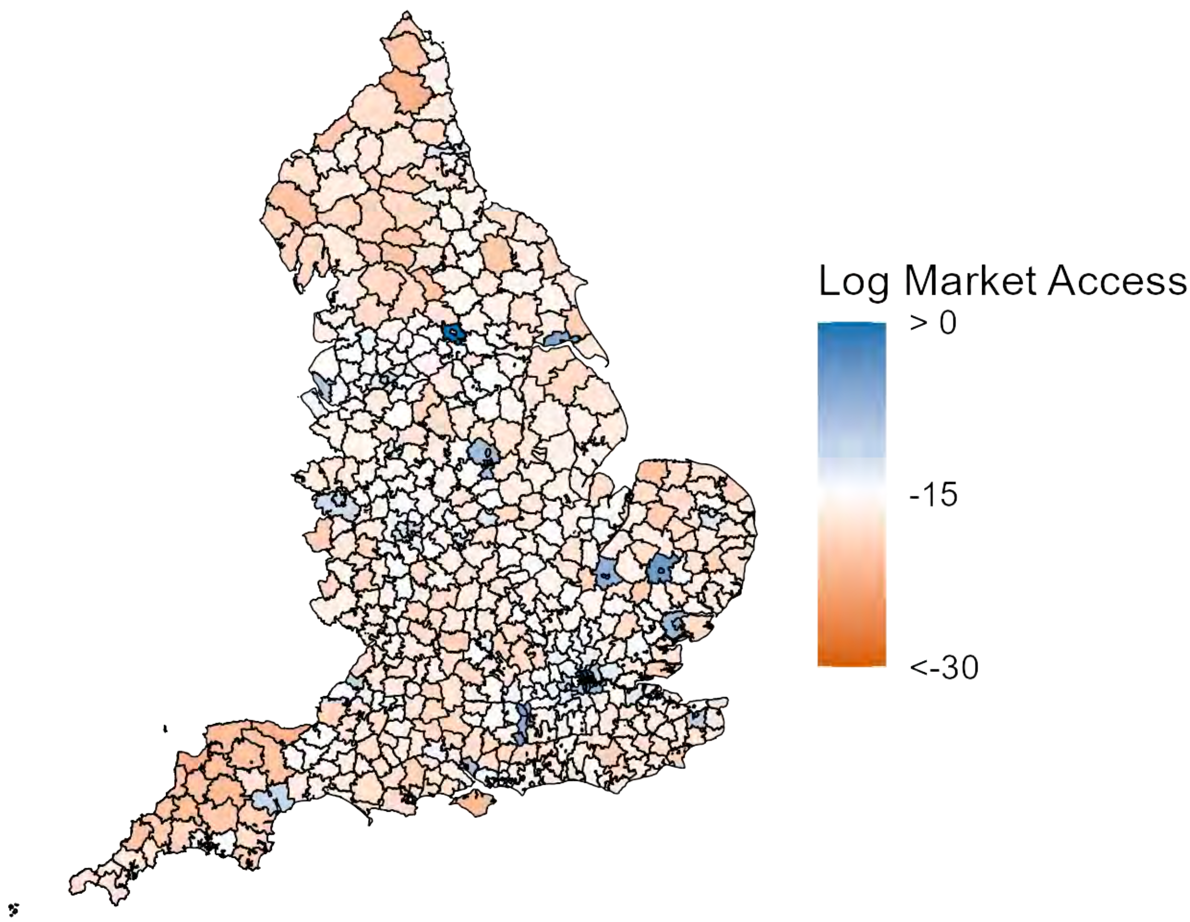


Figure A.15: Log market access value for every district in England in 1861 by district.

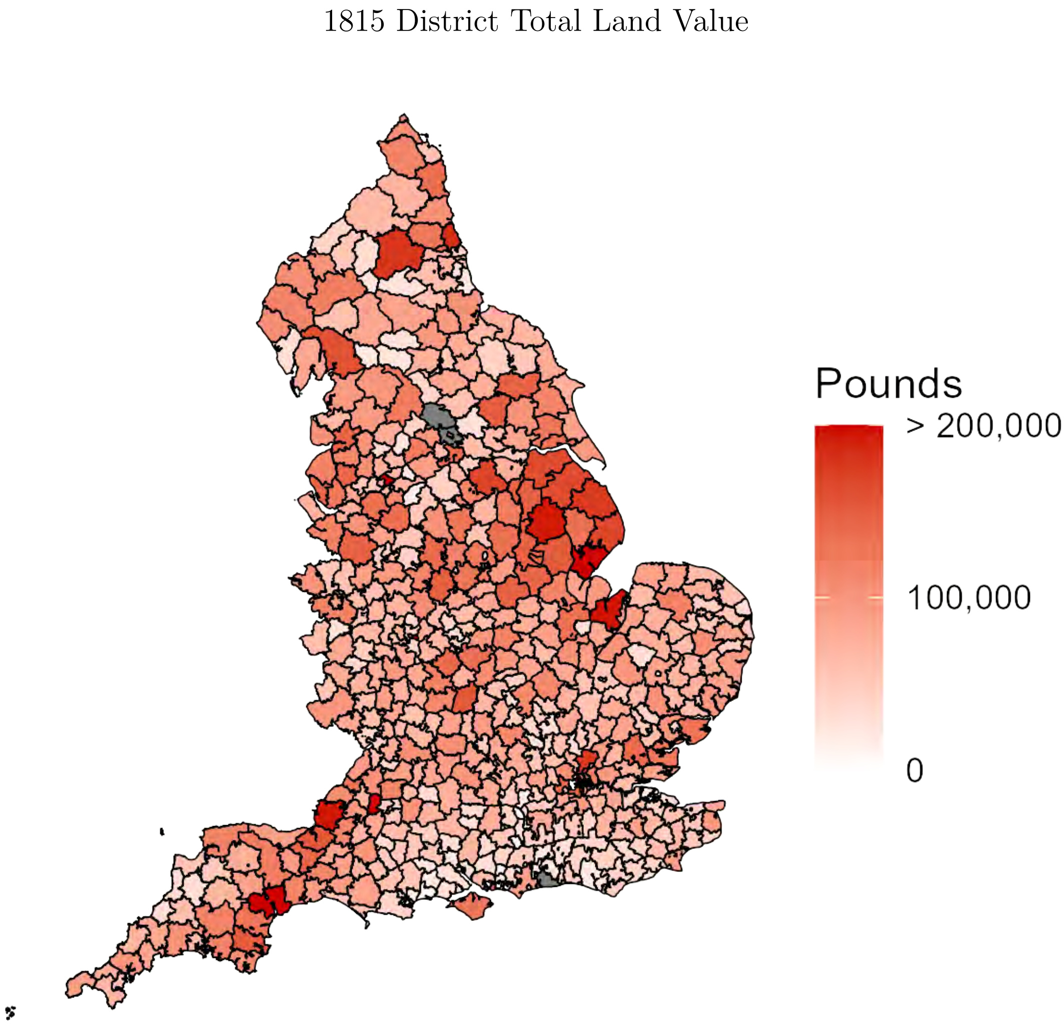


Figure A.16: Districts by 1815 total land value. These values are used in the main definition of market access (equation (1.2)) as a proxy for local GDP. Districts in gray (Hunslet, Kensington Town, Otley, Westminster, and Worthing) have no data. 16 districts have values above 200,000 pounds and three have values above 500,000 pounds.

Patenting Activity Prior to 1840

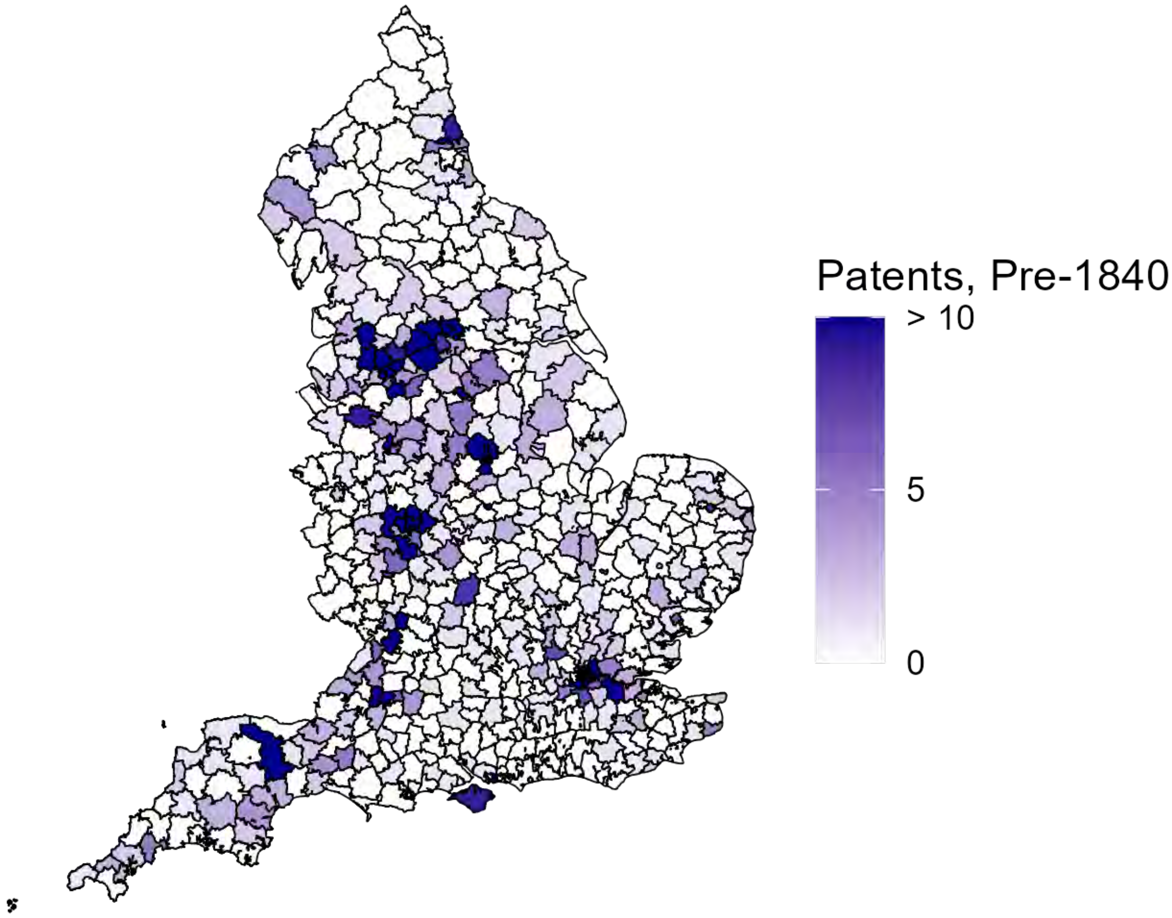


Figure A.17: Maps of England with districts shaded by number of cumulative patents between 1823-1839.

A.1.4 Synthetic Difference in Difference Graphs

I provide here graphs in the three year periods for which I run the Synthetic Difference in Difference (SDID) estimation. The vertical line shows the treatment date. The dotted line is the control and the solid line is the treated. SDID estimation weights the control group to force parallel trends in the pre-treatment period. These graphs shows that this process is largely successful using the three year periods discussed in Section 1.4.

Figure A.18: SDID estimation trend graph where treatment year is between 1832-1834.

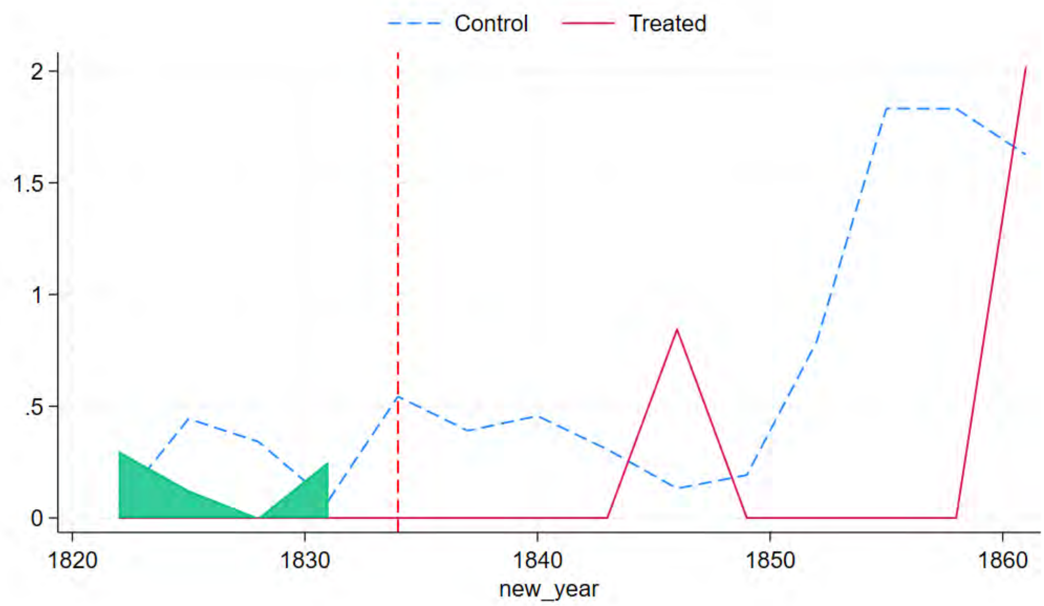


Figure A.19: SDID estimation trend graph where treatment year is between 1835-1837.

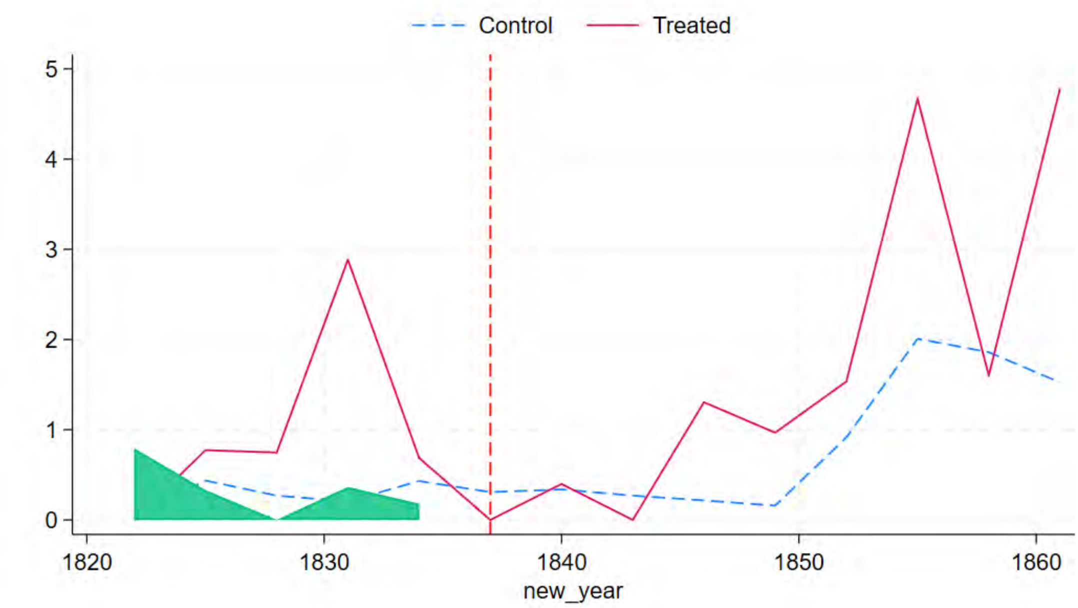


Figure A.20: SDID estimation trend graph where treatment year is between 1838-1840.

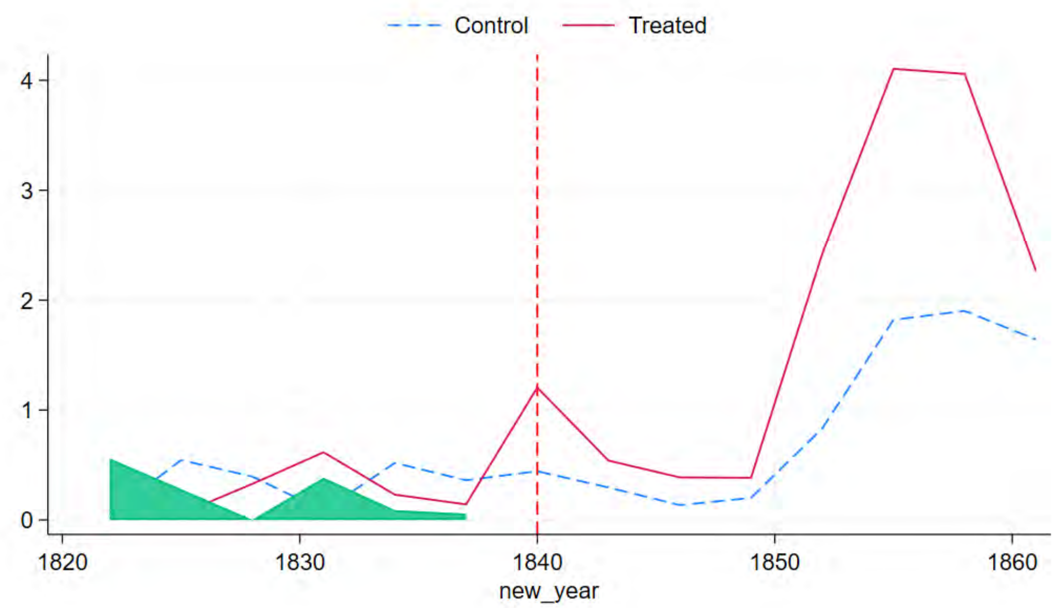


Figure A.21: SDID estimation trend graph where treatment year is between 1841-1843.

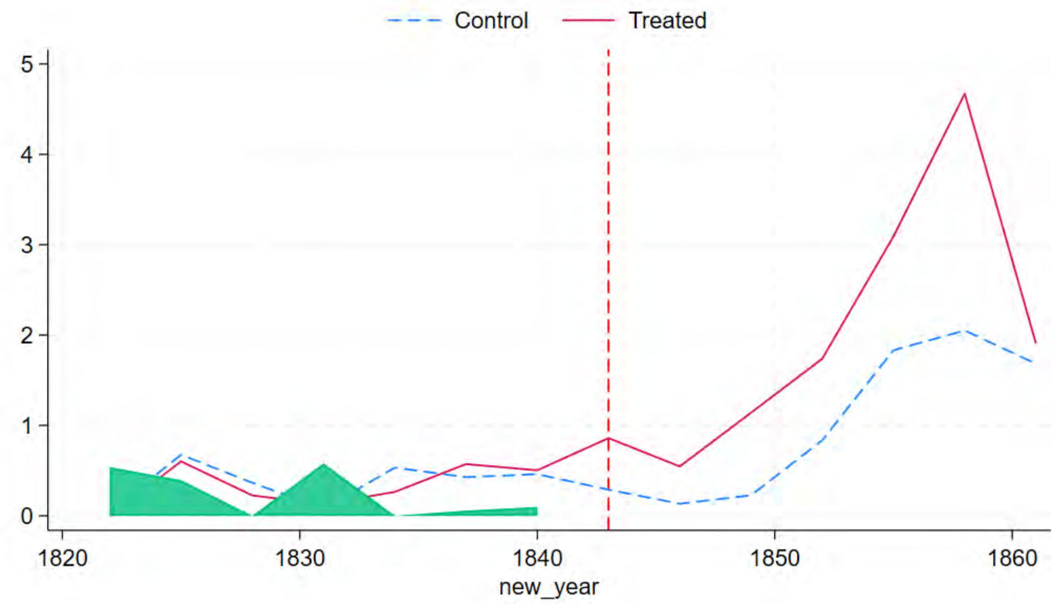


Figure A.22: SDID estimation trend graph where treatment year is between 1844-1846.

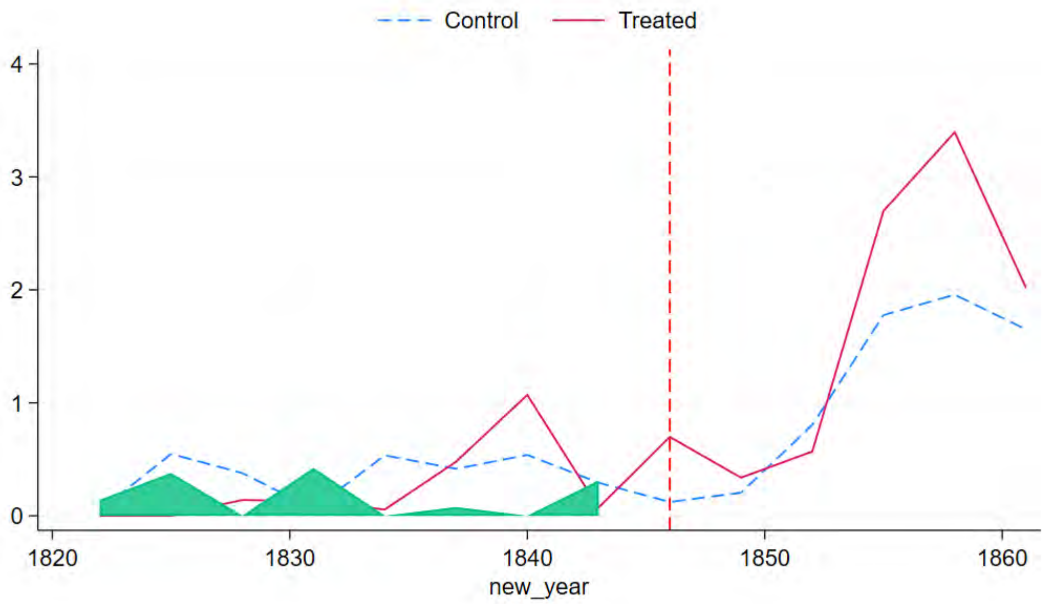


Figure A.23: SDID estimation trend graph where treatment year is between 1847-1849.

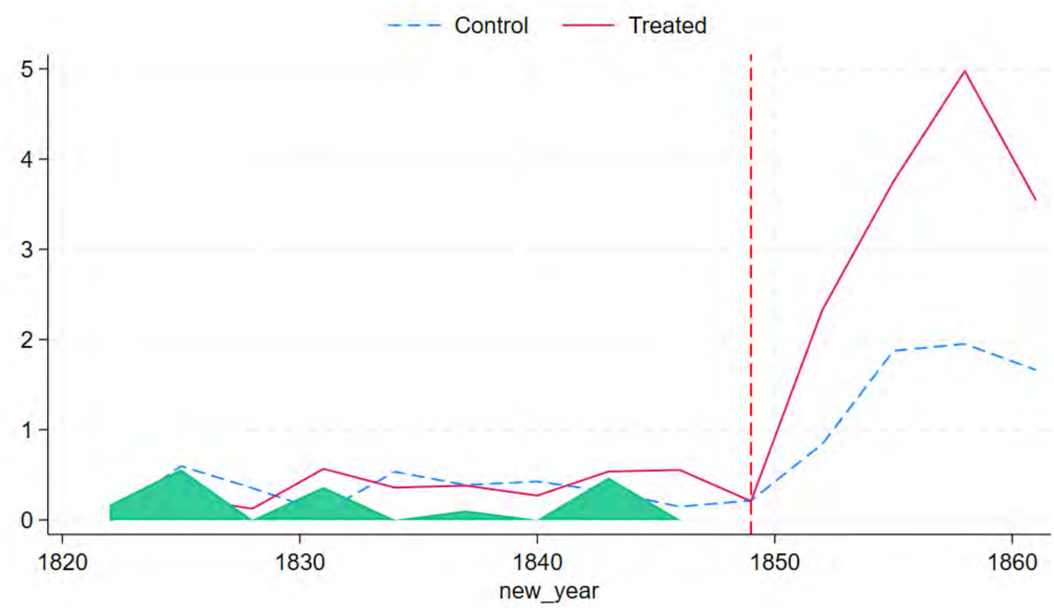


Figure A.24: SDID estimation trend graph where treatment year is between 1850-1852.

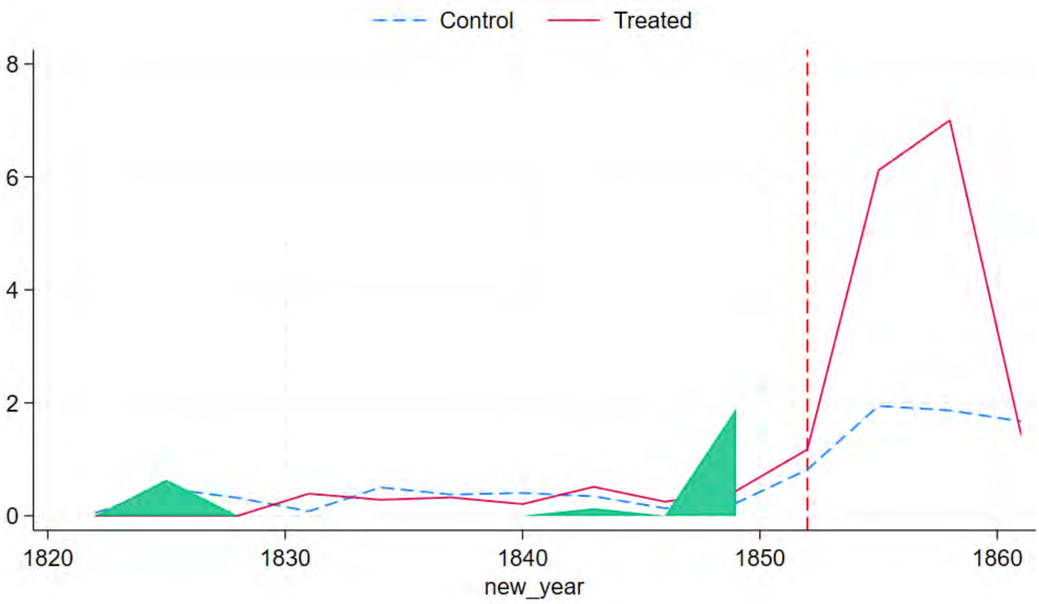


Figure A.25: SDID estimation trend graph where treatment year is between 1853-1855.

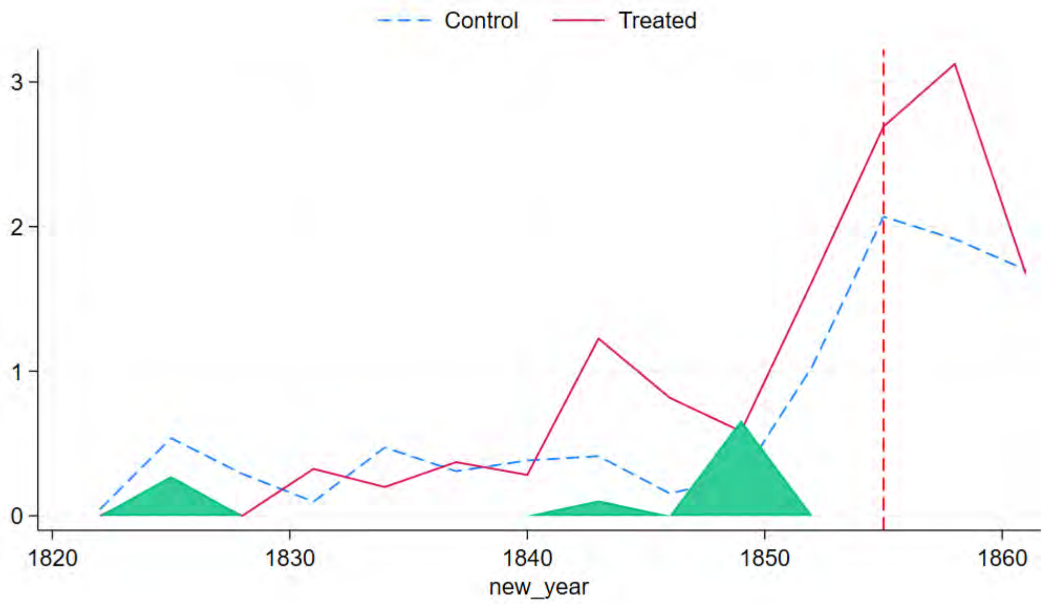


Figure A.26: SDID estimation trend graph where treatment year is between 1855-1858.

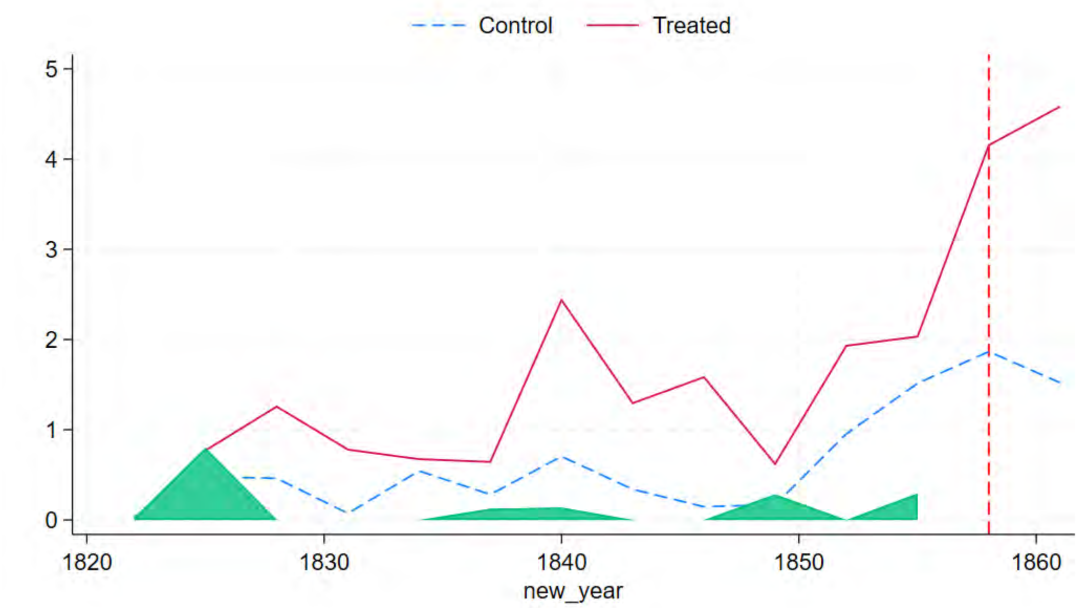
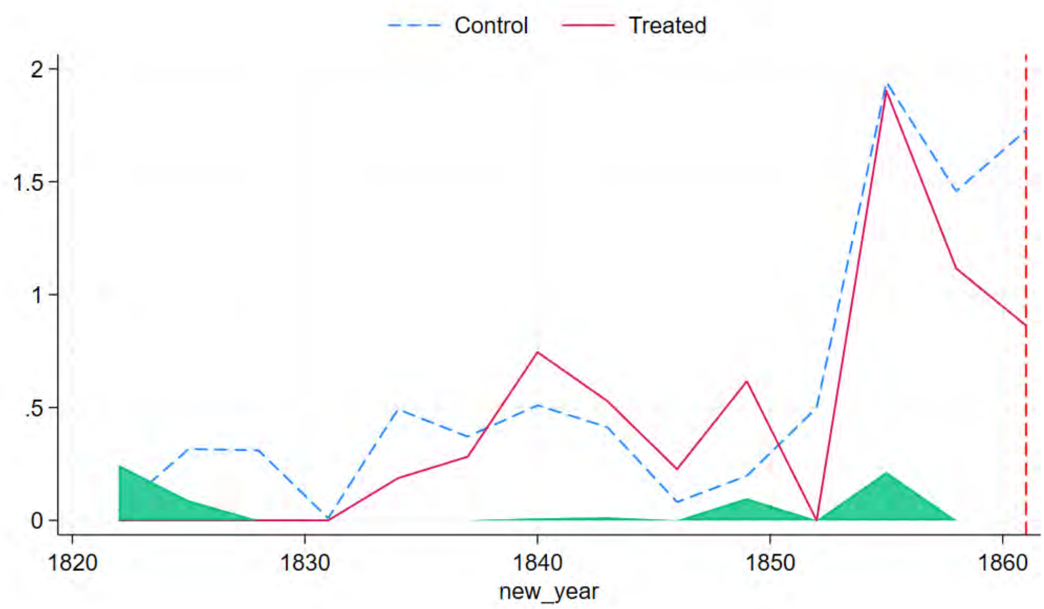


Figure A.27: SDID estimation trend graph where treatment year is between 1858-1861.



A.1.5 Model

The nature of railroads makes it difficult to evaluate national effects on output or productivity. The empirical results provide evidence of a positive effect in districts that received a rail station. However, this could come at the expense of other districts. If higher productivity workers in a district that did not receive a rail station move to a district that did, the positive productivity effects would cancel out at the national level. Section 1.5.2 provides evidence that this is not the case in this setting. I follow Kline and Moretti (2014) and Agrawal et al. (2017) in creating a model that provides additional evidence that disentangles a population agglomeration effect and a patenting effect on labor productivity and I show evidence that the agglomeration channel does not appear to cancel out at the national level.

I model districts as small, open economies. Firms take the prices of capital K, labor L, and output Y as given. Worker utility is modeled as a function of wages w and amenities M:

$$ln(u_{it}) = ln(w_{it}) + ln(M_{it}) \tag{A.1}$$

Output Y is produced according to a Cobb-Douglas production function with a fixed factor F and productivity A:

$$Y_{it} = A_{it} K_{it}^{\alpha} F_t^{\beta} L_{it}^{1-\alpha-\beta} \tag{A.2}$$

I normalize the price of Y to 1 and denote w as the wage and r as the cost of capital to obtain an inverse labor demand curve:

$$\begin{aligned} \ln(w_{it}) = \ln(1-\alpha-\beta) + \frac{\alpha}{1-\alpha}\ln(\alpha) - \frac{\beta}{1-\alpha}\ln(L_{it}) \\ + \frac{\alpha}{1-\alpha}\ln(F_i) - \frac{\alpha}{1-\alpha}\ln(r_t) + \frac{1}{1-\alpha}\ln(A_{it}) \end{aligned} \tag{A.3}$$

I model productivity A as a function of labor L and patents P:

$$ln(A_{it}) = \sigma ln(L_{it}) + \xi ln(P_{it}) \tag{A.4}$$

σ and ξ can respectively be interpreted as the strength of agglomeration forces and patenting on productivity. I find evidence in Section 1.6 that growth in both L and P is dependent on having a rail station and I use a reduced form specification to model this relationship:

$$L = aR^{\mu} \tag{A.5}$$

$$P = bR^{\theta} \tag{A.6}$$

Combining equations (A.3) - (A.6) and taking the derivative with respect to a rail station R yields the following effect of rail on local wages:

$$\frac{dln(w)}{dln(R)} = -\frac{\beta\mu}{1-\alpha} + \frac{\xi\theta}{1-\alpha} + \frac{\mu\sigma}{1-\alpha} \tag{A.7}$$

Thus far, this model replicates Agrawal et al. (2017) and I follow the authors' interpretation and calibration. The negative term is the competitive effect that a rail station has on wages; rail attracts labor and a larger labor force lowers wages. The positive term is the productivity effect through patenting and agglomeration forces; higher labor productivity increases wages. I set $\xi = .11$, $\beta = .47$ and $\alpha = .68$. The regression estimates in Section 1.4 and Section 1.6 yield patenting and labor elasticities and I set $\theta = .77$ and $\mu = .03$. Substituting these values into equation (A.7) yields:

$$\frac{dln(w)}{dln(R)} = -.04 + .27 + .09\sigma \tag{A.8}$$

Given the large discrepancies in the labor and patenting effects I find compared with Agrawal et al. (2017), the predicted effect is expected to be quite different. Indeed, with a value of $\sigma = 0$, Agrawal et al. (2017) find an elasticity of wages of -.14. However, the patenting effect in this setting is so great that even without agglomeration forces the effect on labor productivity would be positive and equal to .23. Setting $\sigma = .72$, as Agrawal et al. (2017) do, yields a total effect of .30, larger than the authors' findings of .20. This is in line with the larger patenting effect I find in this setting.

The model thus far emulates the Kline and Moretti (2014) style model that Agrawal et al. (2017) use in order to highlight the differences between Agrawal et al. (2017) and this paper. I now continue to follow Kline and Moretti (2014) to model national level effects of the rail network.

I now model A as a function of population density and keep the rail station variable invariant across time.

$$ln(A_{it}) = \sigma_i ln(V_{it-1}) + \delta_t D_i + \eta_i + \epsilon_{it} \tag{A.9}$$

V_{it-1} is the population density of district i. It includes a time lag of one period because, as noted by Kline and Moretti (2014) and Adsera and Ray (1998), this ensures that the model yields deterministic predictions which exclude implausible scenarios. D_i is a dummy for whether a district has a rail station and δ_t is the direct effect of that access on local productivity in year t. I include district fixed effects η_i to capture time invariant effects in each district. The error term ν_{it} represents the idiosyncratic component of district productivity.

Rewriting equation (A.3) in terms of labor and taking first differences yields:

$$\begin{aligned}\ln(L_{it}) - \ln(L_{it-1}) = &-\frac{1-\alpha}{\beta}\left(\ln(w_{it}) - \ln(w_{it-1})\right) + \frac{\delta_t - \delta_{t-1}}{\beta} R_i \\ &+ \frac{\sigma_i}{\beta}\left(\ln(V_{it-1}) - \ln(V_{it-2})\right) + \frac{1}{\beta}\nu_{it}\end{aligned} \tag{A.10}$$

The coefficient $\frac{\delta_t - \delta_{t-1}}{\beta}$ gives the change in the effects of having a rail station and the coefficient $\frac{\sigma_i}{\beta}$ gives the indirect effects of rail through a labor agglomeration channel.

One issue in estimating equation (A.10) is isolating the agglomeration effect from the local rail station effect. As productivity increases from receiving a station, labor demand will increase causing individuals to move to the effected district and increasing population density. An agglomeration effect would then cause another increase in productivity. This dual effect necessitates a method to isolate the agglomeration effect. I follow Kline and Moretti (2014) by introducing further lags in the population density variable P_{it}. Given a long enough time frame, the productivity shocks ν_{it} should be independent. I define a time period as five years and use additional lags which measure the change in population density between fifteen and twenty years before 1861 as the instrument:

$$Z_{it} = ln(V_{it-3}) - ln(V_{it-4}) \tag{A.11}$$

For the coefficient to contain an agglomeration effect, the district must already be treated at the time of the lag so that a rail station has an effect on the lagged population density. I therefore restrict the data to districts that were either treated prior by the end of 1841 or never treated as of 1861. I use the 1851 and 1861 population data provided by Schurer and Higgs (2023) to construct annual population data between those years and the original census data from 1841 to 1851 to construct the 1841-1851 annual population data. Given that the data are imputed, sequential years from the same decade would exhibit serial correlation.

Table A.17: Model Structural Estimates

	(1) OLS	(2) 2SLS
Bottom Half	0.916***	0.906***
	(0.257)	(0.259)
Top Half	1.007***	1.126***
	(0.132)	(0.136)
Rail Access	0.006	0.004
	(0.008)	(0.008)
Year Effects	Yes	Yes
Observations	93	93
F Statistic	39.472	43.406
Kleibergen-Paap F		14.039
Equal Slopes	0.751	0.436
All Equal 0	0.000	0.000

Notes: Results of regressions based on estimation of equation (A.10). Column 1 shows the OLS estimates and column 2 shows the 2SLS estimates using the lagged population density variable as the instrument. P-values for the null hypotheses of equal slopes of the bottom and top half density splines and that both spline coefficients are equal to zero are reported at the bottom of the table. Only districts that were either treated prior by the end of 1841 or never treated as of 1861 are included. Regressions are weighted by 1821 population. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

I limit the regression to a single time period (where t = 0 is 1861) to avoid this issue at the cost of additional observations.

Wage data are constructed from the 1843 and 1860 district level land value data and annualized. Following Kline and Moretti (2014), the coefficient on the wage change variable, $\frac{1-\alpha}{\beta}$, is assigned a value of 1.5.

Kline and Moretti (2014) create splines of the instrument to assess whether the agglomeration coefficient is constant across densities. The authors shows that with constant agglomeration elasticities, national level agglomeration effects cancel out. I similarly spline the instrument in estimating equation (A.10). The results in Table A.17 indicate that the coefficient for the instrument representing the top half of districts by 1841 district level population density is 10%-20% higher than the coefficient for the bottom half. While this seems to indicate that distribution of population density elasticities is not constant, a null hypothesis of equal slopes cannot be rejected.

The rail station coefficient is essentially zero. This is not surprising as equation

Table A.18: Population Growth by Population Density

	(1) Bottom Half	(2) Bottom Half	(3) Top Half	(4) Top Half
Rail Access	0.037***		0.044***	
	(0.008)		(0.011)	
Pre-Treatment		0.028***		0.021***
		(0.007)		(0.008)
Observations	3915	3391	3915	3251
Districts	135	135	135	135
R-Squared	0.685	0.666	0.697	0.708

Notes: OLS estimation of equation (1.11) where the dependent variable is the log of the imputed population. I use the same Analytic Sample used in calculating the results in Table 1.1, but divide the districts into halves based on 1831 population density. Columns (1) and (2) include the districts in the bottom half of the data sample and columns (3) and (4) include districts in the top half. In columns (1) and (3) the main independent variable is an indicator equal to one in all periods where the district has a rail station. In columns (2) and (4) it is an indicator equal to one starting exactly ten years prior to having a rail station until 1851, so that it includes pre-treatment population growth. Comparing columns (2) and (4) indicates that pre-treatment growth was higher in the less dense districts and comparing across (1) and (3) indicates that post-treatment growth was higher in the denser districts. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.10 is a first difference effect and only "always treated" and "never treated" districts are included. The coefficient is therefore estimating whether receiving a rail station at least fifteen years ago had a direct effect on labor demand between two consecutive periods.

Taking the results as evidence that lower density locations had lower agglomeration elasticities, I now examine the potential ramifications at the national level. Per Kline and Moretti (2014), endogenous reallocation of workers from a lower elasticity district to a higher elasticity district raises aggregate output. I return to the Analytic Sample used in calculating the results in Table 1.2 to maximize the observation count, divide the districts into halves based on 1831 population density and estimate equation (1.1), where the dependent variable is the imputed population. Districts in the top half by population density grow faster after treatment by approximately .7 percentage points than districts in the bottom half, indicating that individuals from low density districts are relocating to high density districts after the treatment. Crucially, this does not appear to be the case prior to treatment - the low density districts are actually growing faster in the years leading up to the local rail station event, indicating causality of the treatment. Taken together with the results presented in Table A.17, this supports the hypothesis that agglomeration forces from railroad access have a positive effect on national GDP because railroads moved individuals from low density locations (which have lower agglomeration elasticities) to high density districts (which have higher elasticities). Though the results of the model indicate that agglomeration forces are small in this setting, this is still a notable result as it provides evidence that transportation infrastructure can cause a national increase in productivity solely through an agglomeration effect.

B.1 Chapter 2 Appendix

B.1.1 Banking Network

As described in Section 2.3, the banking network data comes from the Post Office London Directories for five years: 1820, 1823, 1825, 1827, 1830. Historical records suggest these directories were published annually, but we were only able to locate and access these five years. The directories were published under the patronage of the postmaster general, and were meant to provide the names and addresses of government officials, merchants, and public companies. The relevant section in each year is called "A List of Country Bankers", which contains the name (almost always the partner names), town, and London agent of all country bank branches that drew on a London bank.

We transcribed these records by hand since the number of entries was manageable, around 600 country bank branches per year. After transcribing each year separately we matched bank branches over time using their name and location. Country banks were limited to six partners, and partnerships changed somewhat frequently over time according to Pressnell (1956), so we chose to consider two branch-year pairs a match as long as the location and at least one partner name was the same. Within each year we also tried to match country banks branches across different locations to identify branches of the same banks. Here we used partner names and London agent, assuming that different branches would have the same London agent. Around 2% of country bank branches had two London agents rather than one and we recorded each of these connections separately.

Identifying Agent and Bank Failures

As discussed in Section 2.3, our main identification of branch failures is based on branches that disappeared from the Post-Office Directories between 1825 and 1827 rather than on direct evidence. London agent failures identified using the same method match narrative evidence from James (2012) and Dawson (1990) closely (both sources list examples of London agents that failed during the crisis). We further use Price (1890)'s Handbook of London Bankers, which attempts to provide a comprehensive guide to all London banks in the 19th century, to verify when agents failed. In any case, agent failures are less likely to be mismeasured by this strategy since, unlike country bank branches which only appear once per year, each agent shows up in the Directories as many times each year as they have clients, which is nine on average.

B.1.2 Firm Bankruptcy Data

The bankruptcy data comes mainly from the Edinburgh Gazette. There are gaps in the Edinburgh Gazette publications available online, and we use the London Gazette for those cases. We collect data from January 1, 1820 - December 31, 1830. The start period for most analyses is January 1, 1821 to ensure maximum coverage with population data (as we are missing data on many districts' reported populations in the 1801 and 1811 Censuses; we also remove districts within Gloucestershire and Staffordshire due to missing population data in 1821 and 1831). The main crisis period is December 1, 1825 - February 28, 1827 and so we cut off most analyses on February 28, 1827. Excluding bankruptcies in London, we are left with 3,797 individual bankruptcies over the study period.

When an individual declared bankruptcy or was sued for bankruptcy by a creditor, a notice was required to be posted in the London, Edinburgh, and Dublin Gazettes so other creditors were aware of the proceedings. Marriner (1980), an authority on bankruptcy statistics from this period, argues that Gazette records provide an accurate picture of bankruptcy statistics because the government-appointed Commission of Bankrupt had to certify that creditors had a legitimate claim before notices were posted in the Gazettes. Duffy (1973) analyzed 50 bankruptcies from 1810-1811 and found that for 41 out of 50 cases of payment stoppage, bankruptcy proceedings began within one month, so the dating is fairly accurate.

By searching all banker names in the Post-Office Directories that we identify in those databases as bankrupt, we find that some bankers' occupations are not listed as banker in their bankruptcy notices in the Gazette. This misclassification could introduce bias by understating banker failures and counting them as non-financial firms, so we drop bankruptcies of individuals with the same name as the bankers who failed during the Panic, but we may miss some since not all partner names are listed in the London Directories. For banks with two or more partners listed, however, we actually find few cases where more than one partner went bankrupt.

Occupation Classifications

We classify the occupations of bankrupt partners into tradables, nontradables, construction and other. We generally only use the first listed partner's occupation for the classification. General dealers, brokers, and merchants are classified as tradables. Nontradables tend to be occupations dealing in finished food products, such as bakers, and services occupations. Occupation strings with only one observation are dropped.

Focus on England

As discussed in Section 2.3, we focus exclusively on English bankruptcies rather than Irish or Scottish bankruptcies. This is acceptable because both countries differed from England in important ways. In Scotland, for example, joint-stock banks were not prohibited, banks had limited liability, and most banks had many branches,[17] more closely resembling modern day financial systems and allowing Scottish banks to better weather crises like the 1825 panic.[18] Both Scotland and Ireland had their own (proto-)central banks, adding an additional layer to the institutional differences between these national contexts. It is not clear how and where Welsh bankruptcies were reported, but we find very few Welsh bankruptcies reported in the Edinburgh Gazette despite Wales having a large number of country banks, so we also exclude Wales.

Geographic Data

We use the OpenCageGeocoder API to find the coordinates for town names listed in both the banking network and the bankruptcy datasets. We then manually verified each of these using Google Maps. The coordinates were then matched with a shapefile of English district boundaries provided by Satchell et al. (2023) to pinpoint the towns' district. We use district level data as the population across time is much more accurate than at the town level.

[17] Black (1995) provides a useful description of how the Scottish system differed from the English system.

[18] The *Edinburgh Courant*, quoted in the December 25th, 1825 edition of The Examiner (1825a), wrote "The consideration of these circumstances forces upon our notice the superior security which our Scottish banking establishments afford...the alarm of the money-market in London has scarcely been at all felt."

B.1.3 Additional Tables

Table B.1: Bankruptcies in England from Edinburgh Gazette by Occupation Category

	1825	1826	1827
Tradables	186	654	180
Non Tradables	109	268	98
Construction	35	95	36
Other	60	105	64

Source: Post-Office London Directories, 1820-1830.

Table B.2: First Stage Results for Table 2.4

	(1)
AgentExposure	0.225***
	(0.058)
Obs.	18500
Districts	250
Mean	0.131
Adjusted R-Squared	0.688
F-Stat	14.968
District FE	Y
Month-Year FE	Y

Notes: First stage results for the main 2SLS specification in column (1) of Table 2.4 where the independent variable *AgentExposure* is an indicator equal to 1 for all months after November 1825 for districts which have at least one bank with an agent which failed between 1825-1827. The dependent variable is *BankExposure*, an indicator equal to 1 for all months after November 1825 for districts which have at least one bank which failed between 1825-1827. District and monthly fixed effects are included. Errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.1.4 Tables for Robustness Exercises

Table B.3: Robustness Exercises - Banks

	(1)	(2)	(3)	(4)	(5)
BankExposure	0.138**	0.134**	0.153*	0.044	0.016
	(0.061)	(0.058)	(0.092)	(0.030)	(0.013)
BankExposure X Old Banks					0.014
					(0.015)
Obs.	18500	18574	7992	18500	18500
Districts	250	251	108	250	250
Mean	0.058	0.057	0.037	0.058	0.058
Analysis	Baseline	Has Bank, 1825	Only One Bank	Cont.	OLS
Kleibergen-Paap F	14.969	16.214	4.197	8.501	

Notes: 2SLS estimation of equation (2.5) which includes district and monthly fixed effects. The dependent variable is the number of non-financial firm bankruptcies in district i and month t. Column (1) reports the baseline results from Table 2.4. Column (2) excludes districts without at least one bank in 1825 (rather than 1823 as in the baseline). Column (3) only includes districts with a single bank. Column (4) changes the definition of $BankExposure_{i,t}$ such that it is calculated using the total number of banks (and agents for the instrument) in district i, rather than measuring the extensive margin. Column (5) estimates the OLS reduced form model and includes an interaction between $BankExposure_{i,t}$ and an indicator equal to one if a district has at least one bank that operated between 1820-1825 and zero otherwise. Errors are clustered by district in all columns. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Robustness Exercises - Econometric

	(1)	(2)	(3)	(4)	(5)
BankExposure	0.138**	0.138**	0.101*	0.124**	0.161
	(0.061)	(0.062)	(0.054)	(0.048)	(0.124)
Obs.	18500	18500	17612	18500	8806
Clusters	250	38	238	250	119
Mean	0.058	0.058	0.050	0.058	0.053
Analysis	Baseline	Cluster County	Outliers	Quarterly FE	Precise Fail
Kleibergen-Paap F	14.969	10.692	13.230	23.855	3.034
Time FE	Month	Month	Month	Quarter	Month

Notes: 2SLS estimation of equation (2.5) which includes district and monthly fixed effects. The dependent variable is the number of non-financial firm bankruptcies in district i and month t. Column (1) reports the baseline results from Table 2.4. Column (2) clusters errors at the county level rather than district as in all other columns. Column (3) excludes districts in the top 5% by the total (across Jan 1821 - Feb 1827) bankruptcies per capita. Columns (4) includes quarterly fixed effects instead of monthly. Column (5) includes only districts which did not suffer a bank failure between 1825-1827 and districts for which we observe a bank failure in the bankruptcy data for the months December 1825 and January 1826. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Robustness Exercises - Quarterly

	(1)	(2)	(3)	(4)
BankExposure	0.331**	0.512**		
	(0.152)	(0.239)		
AgentExposure			0.075**	0.061*
			(0.031)	(0.033)
Obs.	6250	5500	6250	6250
Districts	250	250	250	255
Mean	0.173	0.167	0.173	.059
Analysis	SLS	SLS	Reduced Form	SDID
Last Quarter	1827 Q1	1826 Q2	1827 Q1	1827 Q1
Kleibergen-Paap	14.971	14.971		

Notes: This table replicates the results from Table 2.4 but the data are aggregated at the quarterly level instead of the monthly level. Quarterly fixed effects are included in every column and errors are clustered by district. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Placebo Probit Models for Bank Failure, 1823-1825

	(1)	(2)	(3)	(4)	(5)
Bankruptcy, 1823-1825					
Agent bankruptcy, 1825	0.130	0.136	0.140	0.153	0.073
	(0.174)	(0.179)	(0.179)	(0.183)	(0.200)
Number of other branches		0.001	-0.001	0.017	0.018
		(0.040)	(0.040)	(0.038)	(0.050)
Founded 1821-1823		0.597***	0.597***	0.633***	0.708***
		(0.168)	(0.168)	(0.171)	(0.187)
CB bankruptcies in same city, 1823-1825			-0.119	-0.124	-0.516***
			(0.192)	(0.198)	(0.194)
Agent's number of clients, 1823				-0.002	0.003
				(0.007)	(0.008)
CB bankruptcies of same agent, 1823-1825				-0.046	-0.063
				(0.053)	(0.058)
County FE					Yes
Observations	569	569	569	569	396

Notes: Data from the Post-Office London Directories, 1820–1830. Country bank branch level probit model where the dependent variable is equal to 1 if country bank branch i fails between 1823–1825. The number of observations differs from Table 2.2 due to differences in bank numbers across time. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

B.1.5 Additional Figures

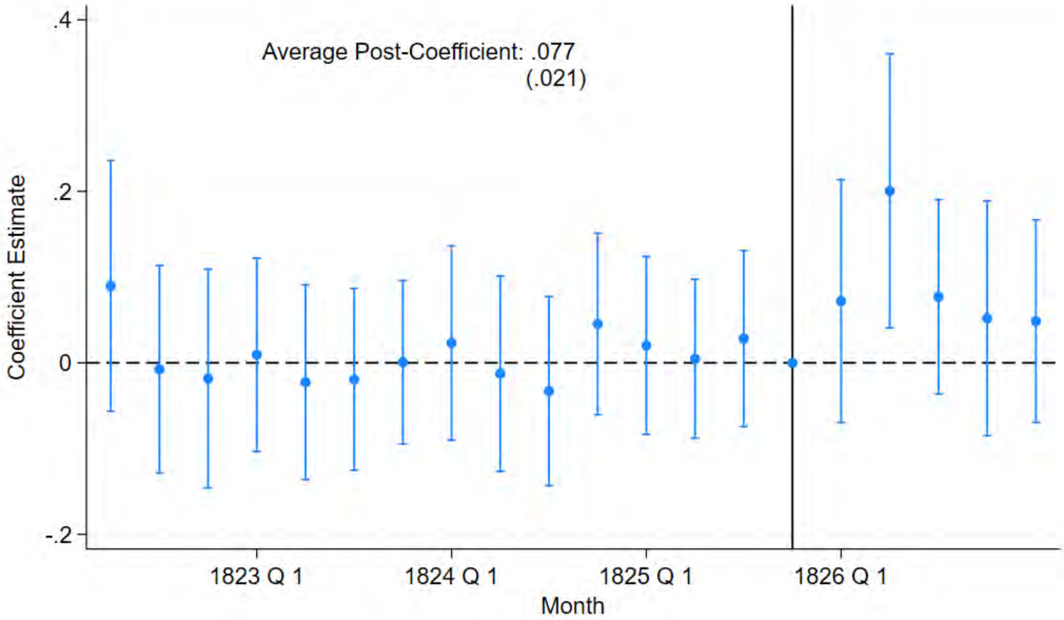


Figure B.1: Quarterly version of Figure 2.5.

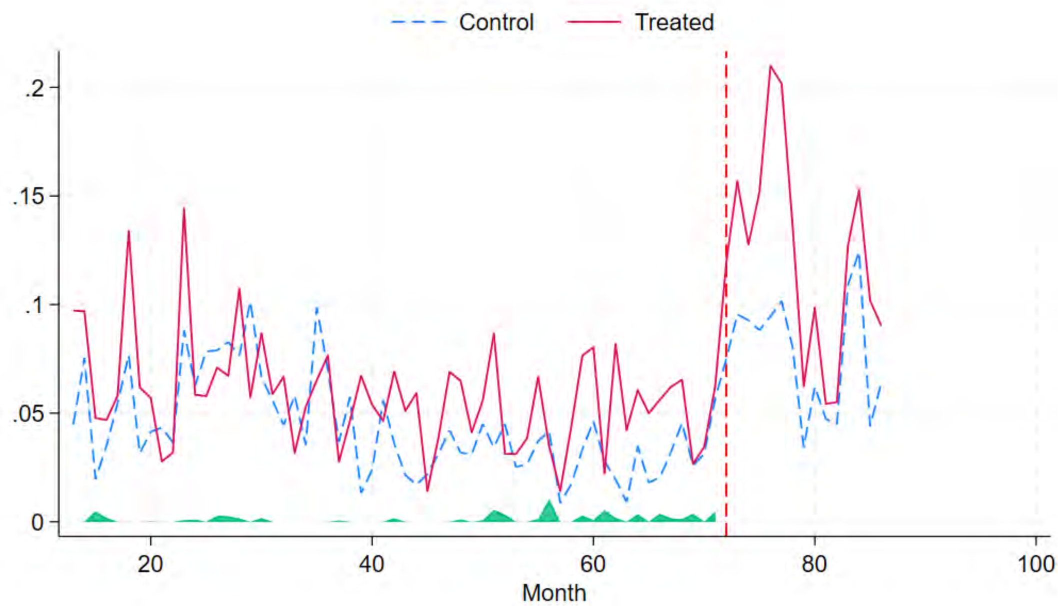


Figure B.2: Synthetic difference-in-difference event study plot at the monthly level of the reduced form estimation of equation 2.5.

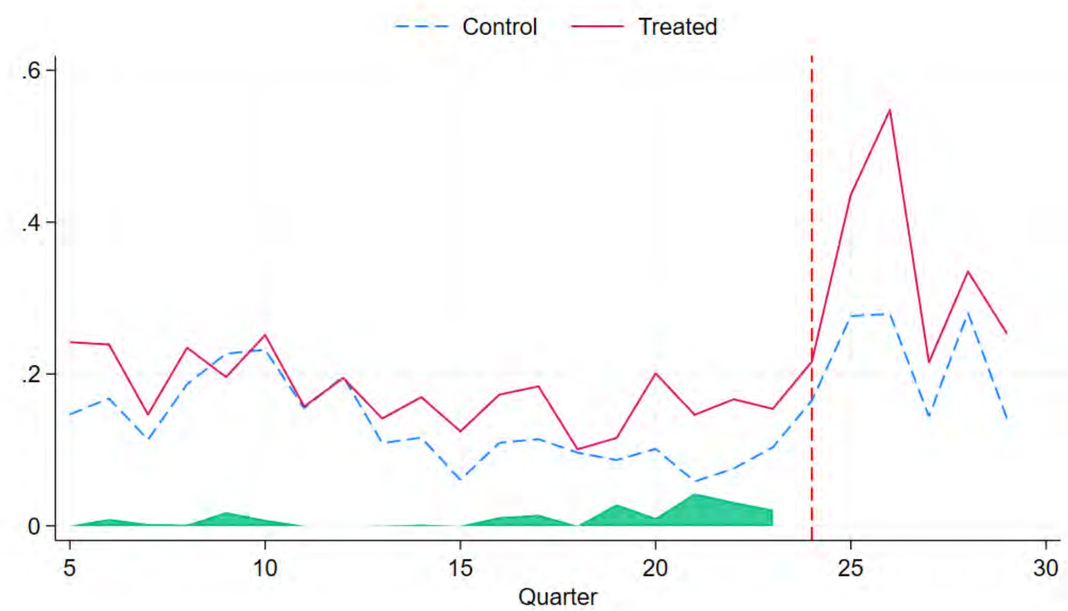


Figure B.3: Quarterly level version of Figure B.2.

B.1.6 Maps

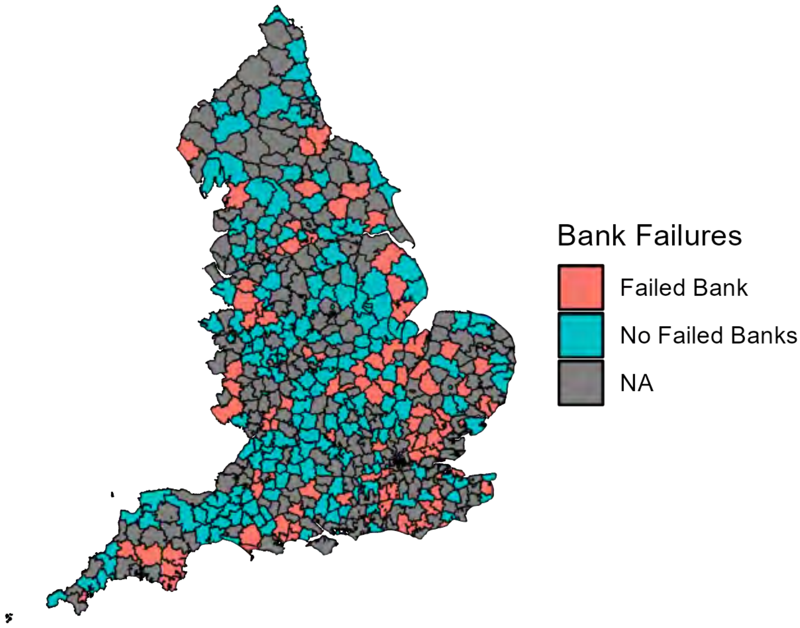


Figure B.4: District level bank failure status for failures that occurred between 1825-1827. NA indicates that there were no failed banks in the district.

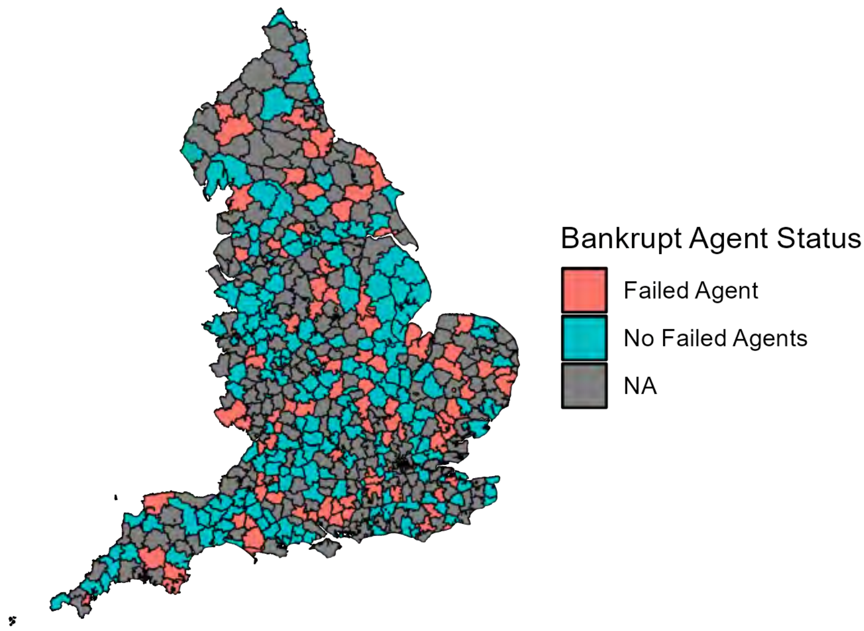


Figure B.5: District level agent-driven bank failure status for failures that occurred between 1825-1827. NA indicates that there were no agent driven bank failure in the district.

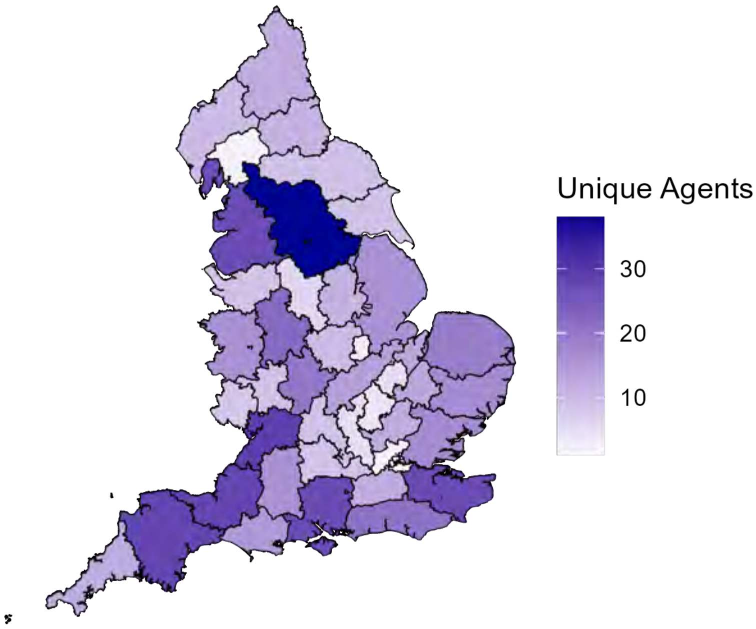


Figure B.6: The number of unique agents with a local banking relationship in a county.

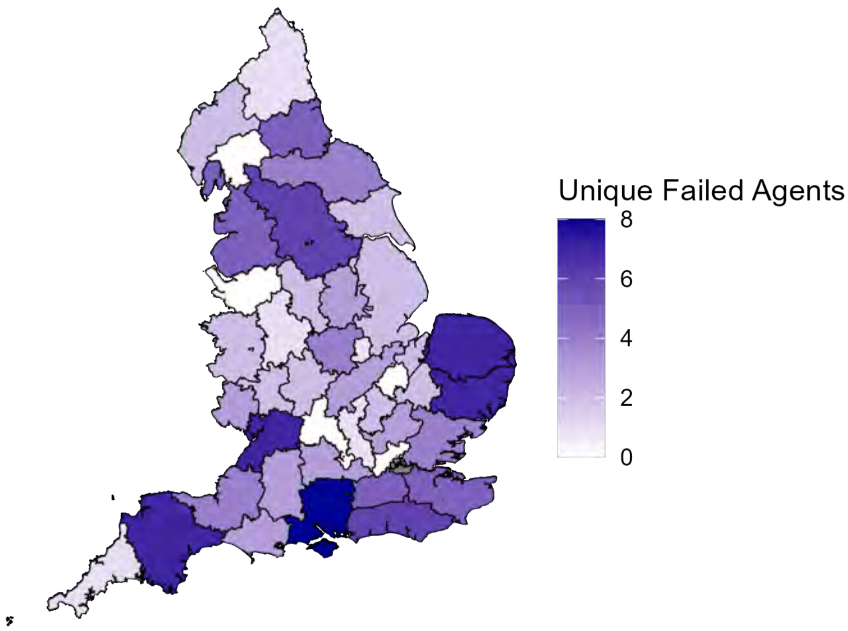


Figure B.7: The number of unique agents that failed during the Panic with a local banking relationship by county.

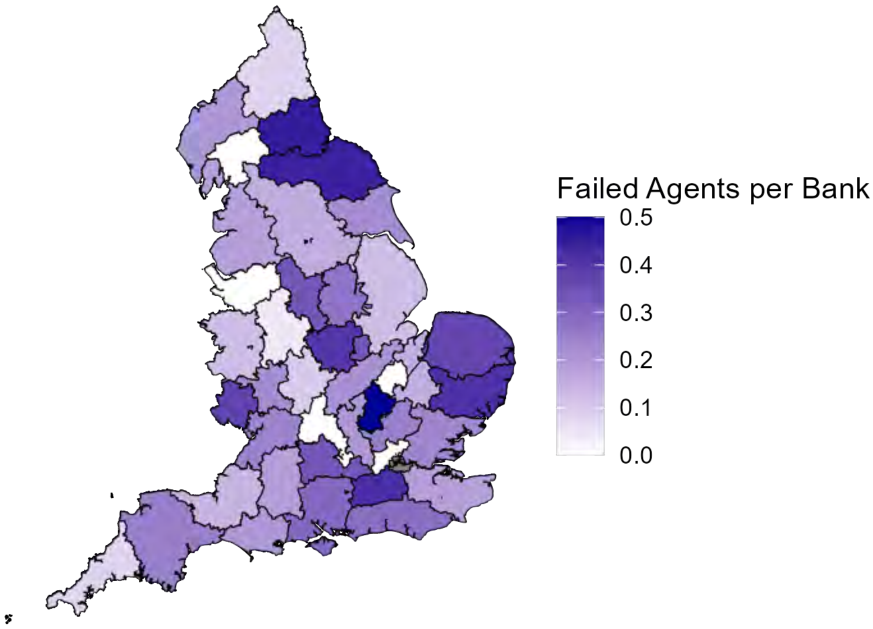


Figure B.8: The number of unique failed agents per country bank in each county.

C.1 Chapter 3 Appendix

C.1.1 Additional Tables and Figures

Table C.1: Various Robustness Exercises on Main Bankruptcy Results

	(1)	(2)	(3)	(4)
Rail	0.036	0.034	0.054*	
	(0.028)	(0.038)	(0.029)	
Rail X 1844 Law		0.003		
		(0.040)		
Rail X Recessions			-0.055*	
			(0.031)	
Rail, 1846				0.119
				(0.470)
Analysis	County Cluster	1844 Law	Recessions	Aggregate
Observations	3366	3366	3366	187
Districts	35	187	187	187
Years	1837-1854	1837-1854	1837-1854	1837-1854
Mean of Dep. Var.	0.208	0.208	0.208	0.273

Notes: TWFE OLS estimation results based on equation (3.4). The dependent variable is the number of firm bankruptcies per 10,000 people in district i and year t. The main independent variable is a dummy equal to one if a district has a rail station in year t and zero otherwise. Column (1) clusters errors by county; all other columns cluster by district. Column (2) includes the interaction between Rail and a dummy equal to one for all years after a new 1844 bankruptcy law. Column (3) includes the interaction between Rail and a dummy equal to one for all years identified as recessionary (1839-1840, 1845-1846, 1847-1848). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

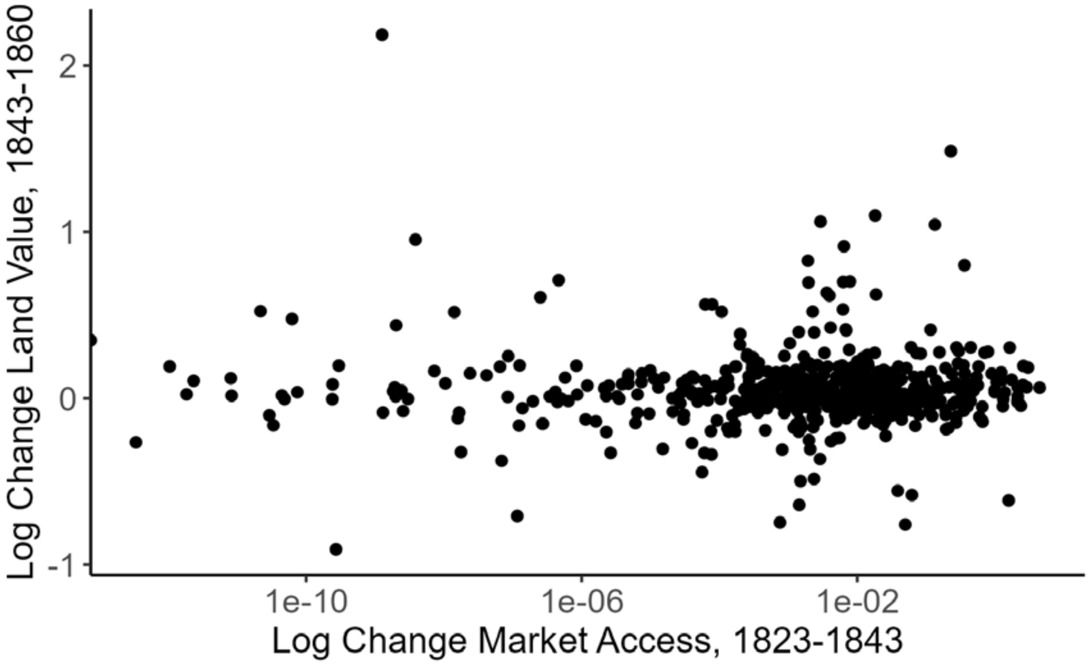


Figure C.1: Scatterplot showing the relationship between the log change in market access between 1823-1843 and the log change in land value per capita between 1843 and 1860 at the district level. All districts in England are included.

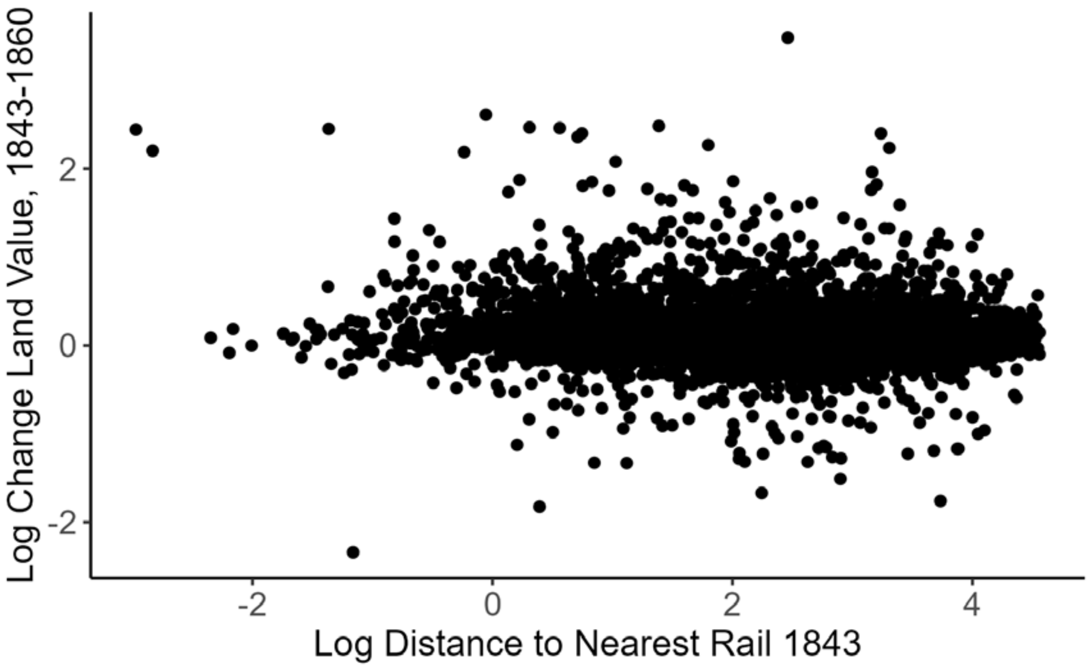


Figure C.2: Scatterplot with the log distance between a parish centroid to the nearest rail station in 1843 and the log change in land value between 1843 and 1860. Includes all parishes in England for which I have matched land value data.

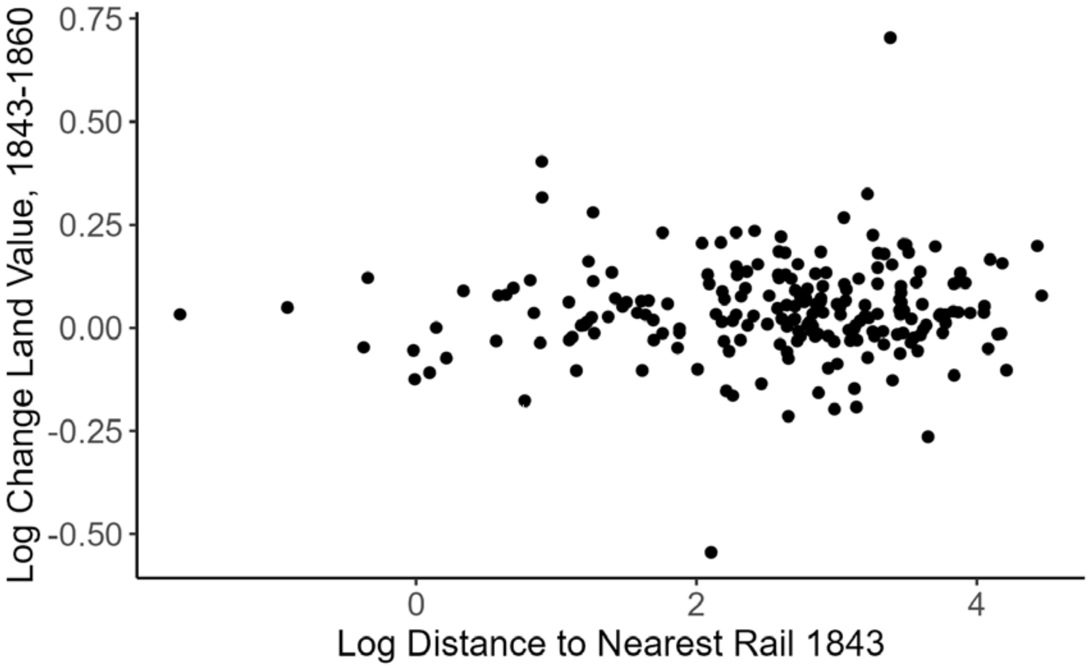


Figure C.3: Scatterplot with the log distance between a district centroid to the nearest rail station in 1843 and the log change in land value between 1843 and 1860. District land values are aggregated from the parish level. Districts are only included if they are part of the Main data sample.

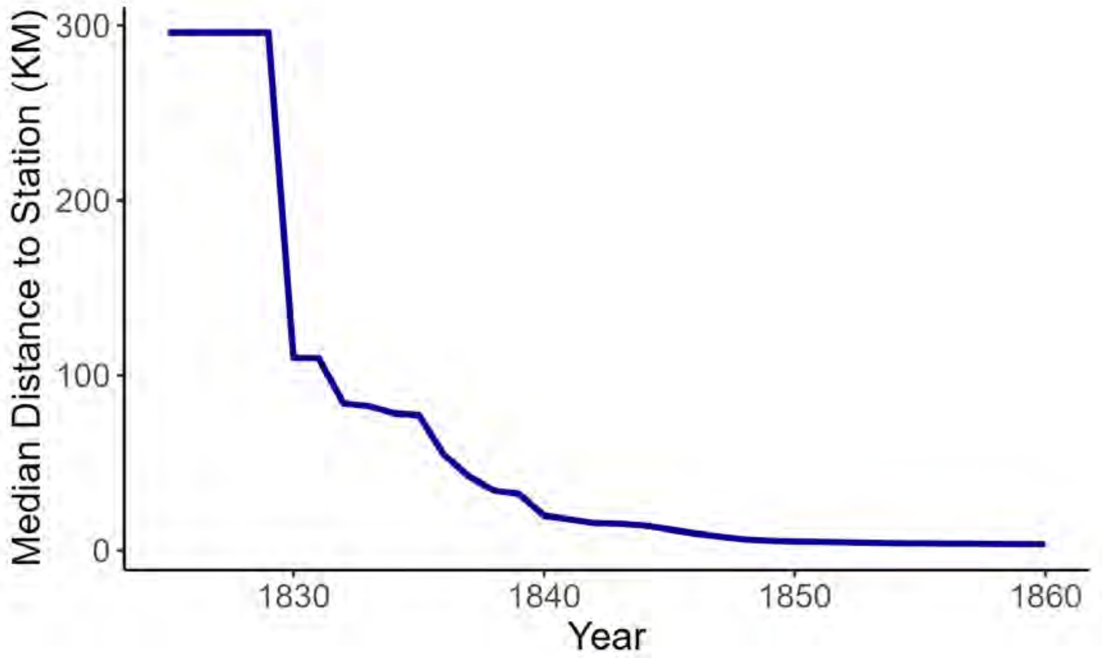
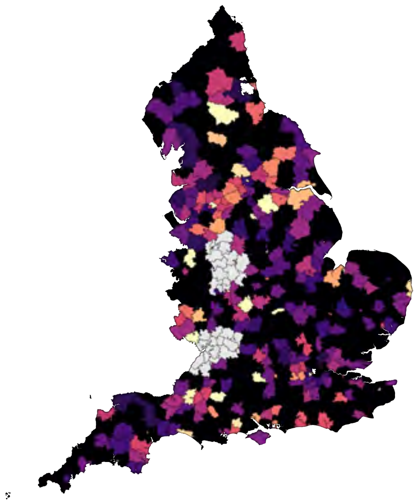


Figure C.4: Median distance in kilometers from a district's geographic centroid to the district's nearest rail station across time.

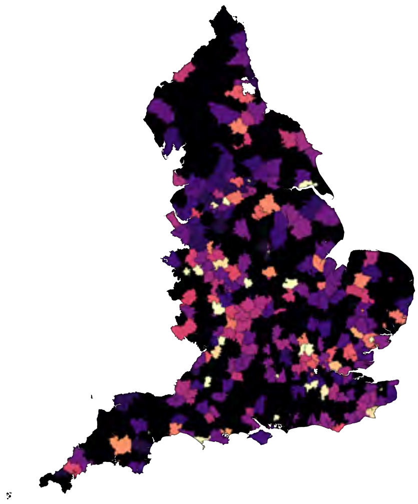
C.1.2 Additional Maps

Figure C.5: Parish level land values per capita for 1843. Lower values have darker colors. Parishes without matched data in either 1843 or 1860 are gray. Colors are capped at the 80th and 20th percentiles.

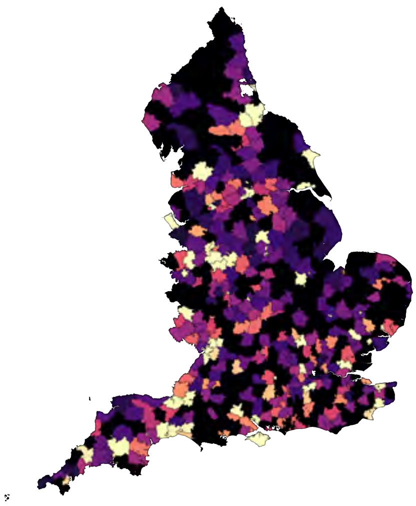
Figure C.6: Parish level land values per capita for 1860. Lower values have darker colors. Parishes without matched data in either 1843 or 1860 are gray. Colors are capped at the 80th and 20th percentiles.



(a) Panic of 1839

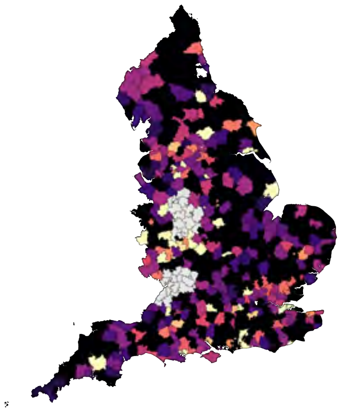


(b) Hungry Forties

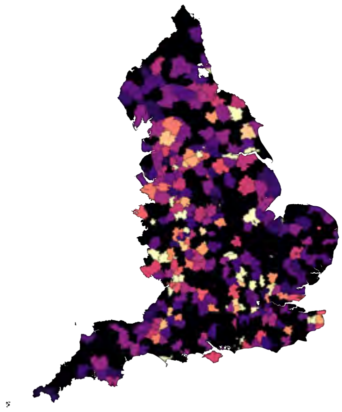


(c) Panic of 1847

Figure C.7: Bankruptcy rates during economic downturns. Subfigure a shows district level mean bankruptcies per capita averaged across 1839-1840, subfigure b for 1845-1846, and subfigure c for 1847-1848. Darker values represent lower numbers. As I do not have access to district level population data for Staffordshire and Gloucestershire for years prior to 1841, districts within these counties are grayed out in subfigure a.

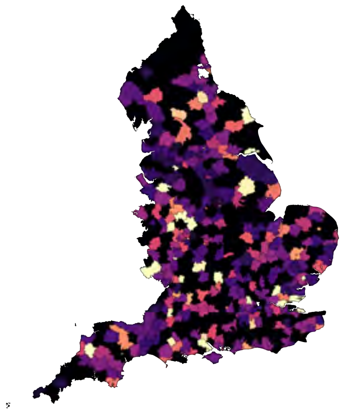


(a) 1837-1838



(b) 1841-1842

(c) 1843-1844



(d) 1849-1850

(e) 1851-1852

(f) 1853-1854

Figure C.8: Mean bankruptcy per capita by district during economic boom periods. Darker values represent lower numbers. As I do not have access to district level population data for Staffordshire and Gloucestershire for years prior to 1841, districts within these counties are grayed out in subfigure a.