Financial development and patents during the First Industrial Revolution: England and Wales

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Abstract

Using a district-level dataset on patents and banks in England and Wales during the First Industrial Revolution, I show that better access to financial services increased patents of invention between 1750 and 1825. My baseline estimation includes district and year fixed effects. I also construct an instrumental variable based on the locations of historical post towns before country banks appeared. Better banking access increased patents by increasing the supply of short-term credit. The effects are larger for the patents in the manufacturing sector that lacked credit, and in districts where credit supply was insufficient.

Keywords: banking access, patents, post towns

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1 Introduction

Did the development of country banks in England and Wales stimulate patenting between 1750 and 1825? It is not clear whether banks contributed to innovation or not during the First Industrial Revolution. The records of country banks, small private banks with at most six partners outside London, were much lost. Therefore, there exists only some qualitative evidence about the interactions between bankers and inventors (Brunt, 2006; Allen, 2009a). The lack of detailed historical records provides scope for me to use quantitative evidence to improve our understanding of the impacts of country banks on innovation during the British Industrial Revolution. The relationship between finance and innovation is an important strand of the literature discussing the finance-growth nexus. Empirical evidence from the United States shows that increases in banking access lead to increases in innovation (Mao & Wang, 2021) by new innovative private firms (Chava et al., 2013), especially those that rely on external finance (Nanda & Nicholas, 2014; Cornaggia et al., 2015). American banks actively lent to innovative activities as early as the Antebellum period (Mao & Wang, 2021) while English country banks rarely did so during the First Industrial Revolution. In this paper, I seek to understand how country banks that mainly provided short-term credit increased innovation. I also provide external evidence about the relationship between banks and innovation outside the United States.

In this paper, I introduce district-level panel dataset on patents and country banks in England and Wales between 1750 and 1825. I collect the dates of patents and the names, locations, and occupations of patentees from a chronologically arranged index of patents of invention in England (Woodcroft, 1854). I map patents and country banks into 595 distinct registration districts outside London and Middlesex. My baseline regression is a two-way fixed effects model, estimated using ordinary least squares (OLS). I control for district and year fixed effects and explore the relationship between banking access and the number of patents per capita in England and Wales during the First Industrial Revolution. My baseline OLS estimates return an elasticity between the dependent variable, the natural logarithm of one plus the number of patents per capita with respect to the independent variable, the natural logarithm of one plus the number of country banks per capita, that ranges between 0.044 and 0.049. The estimated result corresponds to about 6% of a standard deviation increase in the dependent variable in response to one standard deviation increase in the independent variable. The impacts of banks on patents are positive but smaller than the effects observed in Antebellum America.

As the OLS estimation might be subject to endogeneity concerns due to omitted variables that contributed both to the establishment of country banks and patents, I employ the instrumental variable (IV) strategy. My principal instrument is constructed based on historical post-town status following Heblich and Trew (2019). Country banks tended to operate in post towns for safety and information reasons and demand for financial services from the postal system. I use the fact that the number of country banks per capita grew faster in districts with post towns than in those without post towns and construct the instrument by interacting the dummy of post towns recorded in *Britannia* (Ogilby, 1675) with the linear year variable. The elasticities returned by IV estimation are larger than OLS estimates, ranging from 0.163 to 0.218. The difference in magnitudes might be because of downward bias caused by speculative banks that emerged after the suspension of convertibility in 1797 and measurement errors in banking access and the impacts of banks on patents.

To understand the mechanisms that drive the effects of country banks, I first examine the difference in the effects of banks on patents in different sectors and districts suitable for agriculture and manufacturing. Next, I show in heterogeneity analysis that the effects were larger in districts with higher initial interest rates. The results show that country banks stimulated industrial patents mainly by providing short-term credit to industrial manufacturers that lacked access to credit. I also extract information from inventors' and bankers' biographies to provide qualitative evidence about how banks contributed to invention.

I also show that my results are robust to different specifications and transformations of the dependent variables. The results survive when I restrict the sample to districts with at least one country bank or one patent during the period that I examine. The results are also robust when I count the number of patents in the future 3 years or 10 years instead of 5 years in the baseline.

This paper contributes to the literature on the role of financial development and banks during the Industrial Revolution. Traditional wisdom believes that financial development did not contribute much to the Industrial Revolution. The timing of the Financial Revolution was much earlier than the Industrial Revolution (Neal, 1990) and the gravity centres of finance and the Industrial Revolution were different (Mokyr, 2009). Moreover, the development of public finance might have increased military spendings and crowded out investments in the industrial sector while war expenditure far exceeds industrial investments (Temin & Voth, 2013). As for private finance, private banks in London mainly served clients in London and paid little attention to industrialists outside London (Voth, 2018). Country banks contributed to industries including textiles, iron metallurgy, mining, brewery, and ship-making, mostly by providing short-term loans and overdrafts (Pressnell, 1956). They were unwilling to finance risky long-term investments and invention projects due to their limited sizes and information asymmetry (Michie, 2016) or lending to outsiders (Hudson, 1986).

There is not enough evidence about how country banks affected innovation during this period. Some country banks lent in a similar way to modern venture capital firms and financed the adoption of new technologies (Brunt, 2006). In contrast, Richard Arkwright was refused when he attempted to borrow enough money to build his first water frame model (Allen, 2009a). My study fills in the gap and provides the first quantitative analysis about the impacts of banks on innovation across England during this period. I also complement my quantitative evidence with cases from biographies of inventors and bankers during the Industrial Revolution.

This paper also engages with the literature on the relationship between financial development and innovation. Innovation plays an important role in economic growth (Romer, 1990; Grossman & Helpman, 1991). Following Schumpeter's argument that 'disposable wealth' is necessary for innovation (Schumpeter, 1961), a large literature has documented the importance of financial development in stimulating innovation (King & Levine, 1993; Hall & Lerner, 2010; Hsu et al., 2014). Access to external finance enables firms without enough funds to invest more optimally by alleviating financial constraints (Egger & Keuschnigg, 2015). Small firms benefit most from financial development as better access to finance encourages the entry of new firms, and innovation (Demirguc-Kunt et al., 2007; Brown et al., 2009) by reducing cash flow sensitivity of fixed investment spending (Benfratello et al., 2008).

Given my focus on banks, there is a debate on the impacts of banks on innovation. Some research argues that loans are poor funding sources for innovation (Stiglitz, 1985; Beck & Levine, 2002; Brown et al., 2012) because banks are risk-averse and could claim profits from innovative firms to secure payments instead of allowing firms to continue to invest in innovative projects (Rajan, 1992). The impacts of banks on innovation were different in industries with specific characteristics (Hsu et al., 2014; Cornaggia et al., 2015). Using the number of state-level restrictions on interstate banking in the 1980s as exogenous shocks, scholars have discovered that better access to external finance induced by banking competition spurs innovation, especially in the industries that rely heavily on external finance (Amore et al., 2013; Chava et al., 2013). Evidence from American history shows that access to banks can assert persistent and positive influence on innovation (Nanda & Nicholas, 2014; Mao & Wang, 2021). Since the Antebellum period, banks in the United States provided not only short-term credit but also long-term loans for fixed-capital and innovative activities. Besides equities and long-run loans, short-term credit can also serve the needs of growing firms that do not have good access to capitals or are constrained by credit, supporting small and middle sized firms in goods production and distribution (OECD, 2015). As English country banks rarely lent to fixed-asset investments and inventions, this paper isolates the impacts of short-term credit from those of long-term loans and shows that short-term credit alone could also spur innovation. It also provides external evidence about the relationship between banks and innovation outside the United States.

The remainder of this paper is organized as follows. Section 2 briefly introduces the historical background and the instrumental variable that I employ. Section 3 discusses my empirical strategy and data source. In section 4, I report the baseline results, validity tests of the instrument, and robustness checks. In section 5, I explore the mechanisms and Section 6 concludes the paper.

2. Historical backgrounds

2.1 Patents in England

To measure innovation during the British Industrial Revolution, I rely on patent statistics (Griliches, 1990) when there was no detailed Research and Development expenditures data. Bennet Woodcroft, the first leader of the new Patent Office, tracked all patents of invention during the period between 1617 and 1852 when the old Patent system was in place. He published a chronologically arranged index of patents in England in 1854, a subject-matter index of patents in 1857 and a reference index in 1862. I construct my dataset about patents based on the three indexes edited by Bennet Woodcroft.

I construct my district-level measurement of innovation based on patent counts in the baseline regression, as patent counts are a standard measure of innovation when no systematic data that covers all innovation exists (Moser, 2013). The patent system did not include all the meaningful innovations as some inventors chose to keep secrecy instead of acquiring a patent (Moser, 2005). Some inventors simply gave up patenting due to the high costs of application (MacLeod, 1988). Therefore, I take patent quality into consideration in robustness checks, as citation provides a measurement of patent quality (Moser et al., 2018).

Nuvolari and Tartari (2011) constructed a measurement of the quality of patents during the British Industrial Revolution based on a reference index of patents (Woodcroft, 1862). They argued that their quality indicator, the adjusted *Woodcroft Reference Index* could reflect both the quality and economic values of patents during the Industrial Revolution. Therefore, I complement my patent data with the adjusted *Woodcroft Reference Index* proposed by Nuvolari and Tartari (2011) in robustness checks.

The period that I examine covers 70% of the top 0.5% inventions from 1702 to 1841, as measured by the number of citations (Nuvolari & Tartari, 2011). These prominent patents include macro inventions (Mokyr, 1992), including the Boulton-Watt steam machine, and mark the most important technological breakthroughs during the First Industrial Revolution. Unlike the first half of the 18th century when most of the patentees were from London, half of the patents acquired between 1750 and 1825 were acquired by patentees outside London. The distribution of patents during the period that I examine provides spatial variations across England and Wales to help understand why innovation during the First Industrial Revolution was more prominent in some parts of England.

2.2 Country banks

In the 18th and the first quarter of the 19th century, there were three different kinds of formal financial institutions in England. They were the Bank of England, private banks in London, and country banks. Country banks are small private banks outside London with no more than six partners. Due to the Bubble Act in 1720, country banks could not be formed as joint-stock companies and operated with unlimited liability (Michie, 2016).

Therefore, country banks were much smaller and less influential than large joint-stock banks in modern England as the average capital of country banks was about ￡10,000 by the end of the eighteenth century (Pressnell, 1956). Using GDP per capita as the unit of measurement, ￡10,000 in 1750 is worth about 21 million pounds in 2016 and￡10,000 in 1825 is worth about 9.3 million pounds in 2016 (Beers et al., 2020). These numbers are dwarfed by the assets of modern gigantic banks as the total asset of Lloyds in 2019 amounts to about 834 billion pounds while that of Barclays arrives at 1.14 trillion pounds.

Country banks provided short-term loans, overdrafts, and issued notes for transactions (Pressnell, 1956). When manufacturers needed to pay for raw materials and workers before they collected payments from the merchants, they could issue a bill and promise to repay it in the future. Country banks bought the bills at a discounted price. They could wait, sell the bill to other local banks or send the bill to London bill brokers to make profits (Michie, 2016). By discounting bills, country banks provided manufacturers with short-term credit that usually lasted for 60 to 120 days. Although the returns to industrial investments were higher than the returns to agricultural investments (Ventura & Voth, 2015), the usury law placed a 5% cap on the interest rates that banks could charge (Voth, 2018). Therefore, country banks that were vulnerable to credit shocks could not claim enough returns from long-term loans to cover the risks that they faced. They were generally reluctant to lend extensively, except for clients that they knew well (Hudson, 1986) or industries that they had good knowledge about (Brunt, 2006).

The number of country banks and their branches was only 10 in 1750 and reached 395 in 1795. During the Napoleonic War, people panicked and tended to exchange Bank of England notes for gold. Due to the increasing demand for gold, the British government decided to suspend the requirement of convertibility on the Bank of England. Between 1796 and 1810, the notes issued by the Bank of England more than doubled, and incomes from discounting bills quadrupled (Michie, 2016). The number of banks and their branches rose to its peak of 942 in 1812 and kept relatively stable until 1825. However, these new banks were weaker and more risk-taking than earlier ones and failed faster (Michie, 2016; Heblich & Trew, 2019)

The areas that country banks served were limited (Hudson, 1986). They tended to serve the local communities and familiar industries to alleviate the information asymmetry problem. For example, Bros. Swaine & Company in Halifax was set up by textile entrepreneurs and served borrowers mostly from similar industries near its location (Hudson, 1986). The effects of country banks on industrialization in the 19th

century were restricted to a distance smaller than 10 km (Heblich & Trew, 2019). There are some cases when country banks lent to their relatives several dozen miles away, like in the case of Ed. Byrom, Wm. Allen, Roger Sedgwick & Ed. Place in Manchester. It lent extensively to a Walton-le-Dale firm, Livesey, Hargreaves, and Company, which was connected to William Allen, one of the partners of the bank, by marriage (Riello, 2010).

The legal restriction on country banks lasted until the Country Bankers Act in 1826. Joint-stock banks became legal in areas more than 65 miles away from London and the Bank of England began to set up branches in other cities. Some country banks began to merge into new joint-stock banks.

Evidence about the contribution of country banks to innovation during the Industrial Revolution remains anecdotal. There are examples that country banks contributed to the adoption of the latest technology. Praed & Co. in Truro provided loans to copper mines in Cornwall to adopt Boulton-Watt steam machines and made large profits (Brunt, 2006). However, there are also examples that banks refused to help innovative partners and firms. It was not until the 1790s when the Boulton-Watt partner set up their Soho Foundry and produced their own steam machines (Tann, 2014). Similarly, Richard Arkwright was refused by two Nottingham bankers because they believed that his water frame did not have a large chance to succeed (Allen, 2009a).

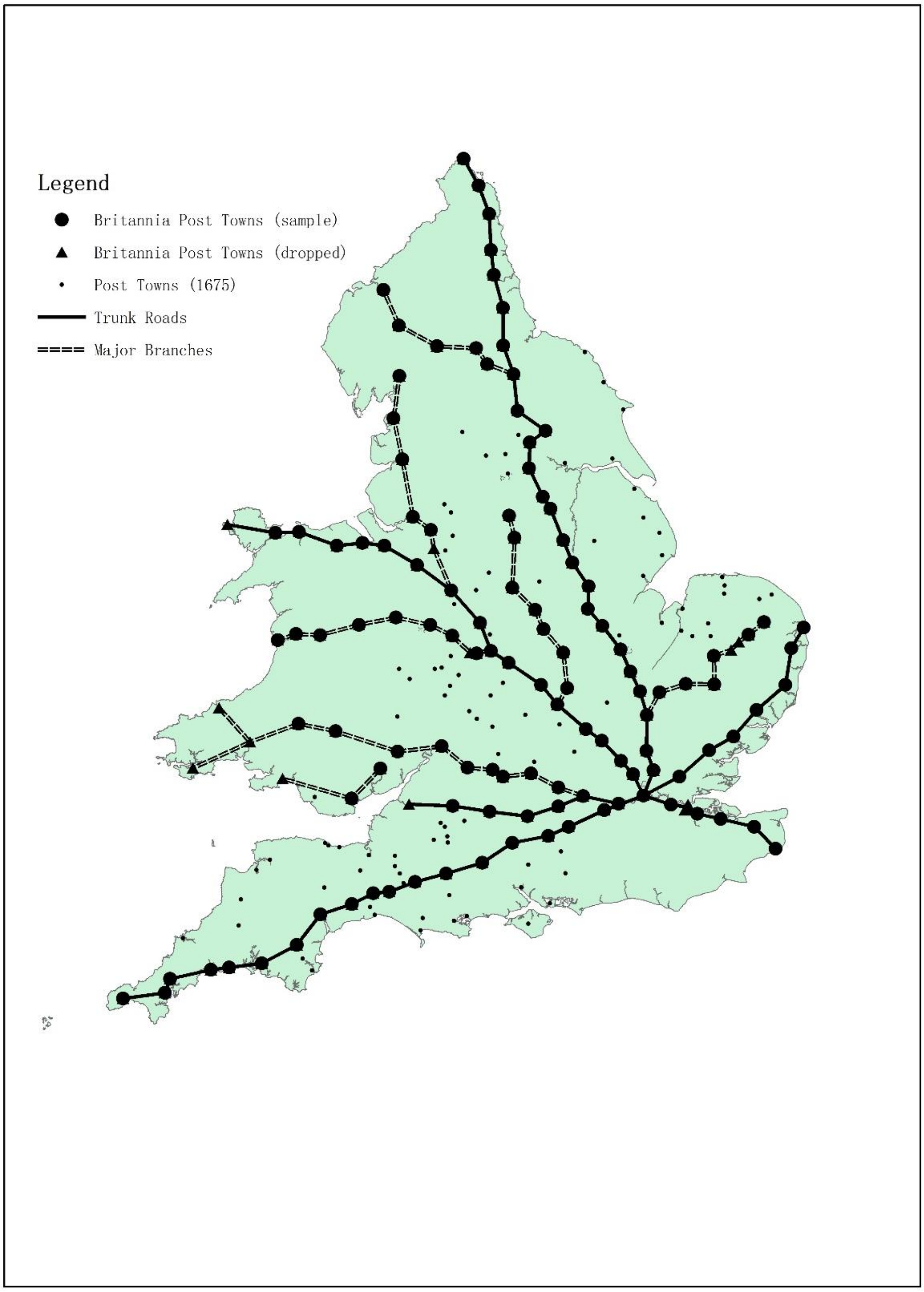

Figure 1 Post roads in 1675. This figure shows the main post roads recorded in Britannia compiled by John Ogilby in 1675. Post towns in the sample are shown in large dots and triangles are some of the post towns dropped due to too large or too small gap distances. The solid lines are trunks while dashed lines are branches with sampled post towns.

2.3 Post Towns status to construct the instrument

Following Heblich and Trew (2019), I use post-town status to construct the instrumental variable. Posts were set up along the post roads that connected to strategic destinations on borders to provide fresh horses for couriers. Towns with post-houses that procured horses became post towns. The history of the postal system in England dates to as early as the reign of Henry VIII. The Master of the Posts was responsible for setting up posts in England to deliver royal letters and important information as fast as possible. Post roads were set up temporarily for wars and abandoned after wars due to high maintenance costs. In 1635, Thomas Withering revived the postal system on the basis of historical routes (Joyce, 1893).

The postal system designed by Thomas Withering adopted most of the historical post routes designed for strategic aims. First, post routes were designed to connect to Scotland, Ireland, and the European continent to keep London updated about other countries. In the 17th century, postmen changed horses every 15 miles on average to travel as fast as possible (Frajola et al., 2005; Heblich & Trew, 2019). Therefore, posts were set up along post roads every 10 to 15 miles according to the physical strength of horses and road conditions in the 17th century. In the second half of the 18th century, roads were paved, and traveling on roads became much faster (Bogart, 2005) but post towns remained.

There are several advantages of setting up a bank in a post town (Dawes & Ward-Perkins, 2000). First, staying close to post roads facilitates learning the latest news and making business decisions. Next, it was safer and faster to travel and transport gold on post roads than following other routes. Moreover, traveling on post roads was still dangerous for postmen. They tended to rely on banks for financial services. This demand stimulated the development of country banks in post towns instead of other locations along the post roads. Among the towns recorded in the *Universal British Directory* published in the 1790s, 130 out of the 150 towns with banks were post towns (Dawes & Ward-Perkins, 2000).

The impacts of post towns on financial development in England

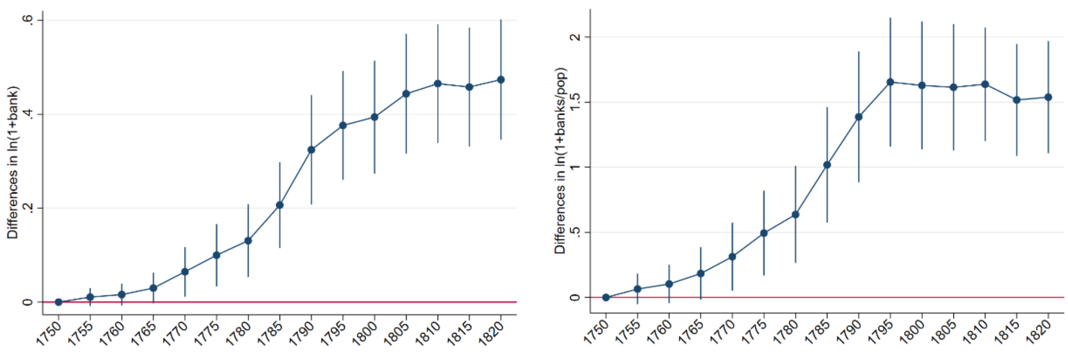

Figure 2 The impacts of post towns on country banks. The left figure shows the differences in ln(1+banks) across districts with and without post towns in different years. The right figure shows the differences in ln(1+banks/population) across districts with and without post towns in different years.

I use the fact that the number of country banks grew faster in districts with post towns in the construction of the instrument. As shown in Figure 2, compared to 1750, the number of country banks and banks per capita grew faster in districts with post towns than in districts without post towns, especially in the second half of the 18th century. The suspension of convertibility spurred the establishment of banks and the difference between districts with and without post towns remained stable between 1800 and 1825.

To construct the instrument, I first restrict the list of posts towns that distances between two post towns were between 16 km and 32 km. I follow Heblich and Trew (2019) in this step as they argued that the average distances between two post towns were about 24 km. Distances smaller than 16 km or larger than 32 km might be due to other unobserved factors that also affect patents. To rule out the effects of destinations of post roads that were likely to be richer and more populous than other towns, I drop the destinations of all post roads[2] from the sample to rule out the selection of destinations according to population. The main idea is that post towns other than destinations became post towns simply because they were on the post roads that were designed to connect other important cities and locations.

I construct an instrumental variable by interacting the dummy of post-town status with the linear variable year. The list of post towns is collected from *Britannia* (Ogilby, 1675).

Identification is based on the exogeneity assumption that post-town status was not selected according to some unobserved pre-existing characteristics that might affect patent growth trends in the future and the exclusion restriction assumption that post towns affected patents only through the channel of banks conditional on taking control variables into account. As shown in Table 2, I test whether pre-existing characteristics were different across districts with and without post towns. The characteristics that I assess include a dummy of access to coal resources[3], a dummy of access to seaports,

[2] Berwick is included in districts with post towns as the destination of the Northern Road is Edinburgh.

[3] The coal data is based on the parish-level data of Heblich and Trew (2019). I aggregate the data to district level.

the natural logarithm of the distance to the nearest seaports, the nearest coast[4], the natural logarithm of the area (in km^2), the average slope[5], and suitability for wheat, rye, barley and oats[6], the four main crops in the crop price records published since 1771. Panel A in Table 2 shows that these pre-existing characteristics are not significantly different across districts with and without post towns. It adds to my confidence that the post towns were not selected based on some unobserved economic factors.

I also control for other channels that post towns might affect patents besides increasing banking access. There could be more people and better access to the transportation network in post towns due to economic opportunities brought about by post roads. I control for population, access to waterways, and traveling time to London. Inland transportation of goods relied heavily on waterways while the transportation of passengers rely more on turnpike roads. The spread of turnpike roads lowered the traveling time to London and facilitated the spread of information. I also control for information access by including the number of local newspapers published within 50 kilometres of the centroid of the district. Newspapers in the second half of the 18th century usually spread within the local county, spanning no more than 100 km (Black, 1991). People outside London were able to search for information about London, where the Patent Office was in, from newspapers (Black, 1991) and this was likely to include information about recent patents.

Panel B of Table 2 shows that access to the transportation network and information was not significantly different across districts with and without post towns. Population growth is slower in districts with post towns and results in OLS regression show that population is positively correlated with patents. The impacts of post towns on patents via other channels, if there were any, might even be negative. In robustness checks, I further restrict the sample to districts on post roads to rule out the effects induced by the access to post roads. The results are similar.

For robustness, I use different sets of post towns to construct alternative instruments. Firstly, I drop the post roads connecting to Kendal and Derby because they are not on the borders. I also drop the post road to Carlisle because this line was

[4] The maritime data about coasts and sea ports is constructed based on Alvarez-Palau and Dunn (2019) and I have excluded the ports on rivers.

[5] The ruggedness data is calculated based on the SRTM data with the resolution of 90m. The unit of slope is percentage rise.

[6] The agricultural suitability is the crop suitability index (value) in the session of agro-ecological suitability and productivity in the Global Agro-ecological Zones (GAEZ) data published by Food and Agriculture Organization of the United Nations (FAO) spanning the period 1961–1990. I assume rain-fed water supply and low input.

redesigned by Thomas Withering and might involve economic concerns. Next, I drop towns that might involve detours due to omitted factors. At last, I drop all the towns with populations larger than 5,000 in 1600, as recorded in Bairoch (1991), from the post town list to rule out towns that were systematically richer since the Medieval Times.

3. Empirical strategy and Data

3.1 Baseline estimation

I test the relationship between banking access and patents using a two-way fixed effect model as in equation (1). The identification variation comes from the change of banks per capita above common trends given by district and year fixed effects.

$$ln[1 + N(Patents/Population)_{i,t+1\ to\ t+5}] = \beta_0 + \beta_1 * ln[1 + N(Banks/Population)_{i,t}] + x'_{i,t}\gamma + \delta_i + \delta_t + \varepsilon_{i,t} \qquad (1)$$

$N(Bank/Population)_{i,t}$ is the number of country banks per million people in district i in year t and $N(Patent/Population)_{i,t+1\ to\ t+5}$ is the number of patents per million people[7] in district i from year t+1 to year t+5, within 5 years after year t. $x'_{i,t}$ includes time-varying variables including travelling time to London via turnpike roads, number of newspapers published within 50 kilometres, population and access to navigable rivers. δ_t is year fixed effects and δ_i is district fixed effects. In baseline regression, I estimate Equation (1) using OLS. In robustness checks, I interact time-invariant variables with year fixed effects to rule out the effects of time-invariant controls[8]. I also include county linear trends in robustness checks as there was a common upward trend in both the number of patents per capita and the number of country banks per capita during the period I examine. The standard errors are clustered on the registration district level. In robustness checks, the standard errors are clustered on the county level. I also use Conley standard errors, setting the cut-off distances to be 50 km, 100 km, 200 km up till 500 km.

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[7] The unit of population is million people as the number of patents per capita and number of banks per capita were so small that the ln(1+x) transformation approximates more to x instead of ln(x). Based on the summary statistics, the mean numbers of patents and banks per capita were about at the order of magnitude of 10^{-5},

[8] Time-invariant controls include latitude, longitude, natural logarithm of the area, natural logarithm of the distance to the nearest sea port, natural logarithm of the distance to the nearest coast, with coal fields, with sea ports, ruggedness and suitability of main crops.

3.2 Data

I combine different data sources to construct the panel data on patents and country banks that spans from 1750 to 1825.

Information about English patents between 1750 and 1825 includes names, occupations, and locations of patentees and the application date and was collected from Woodcroft (1854). As there is more than one patentee for about 10% of the patents, I use patentee counts, counting one for each of the patentees of one single patent, in the baseline regression. In robustness checks, I divide the patent equally among all patentees and count the number of divided patents.

To measure banking access, scholars use different proxies, including the marginal product of capital (Long & Zhang, 2011) and the ratio of banking credit to GDP (Hsu et al., 2014). However, only the archives of a few country banks survived and records about interest rates were limited, only available in some cases (Keller et al., 2021). Therefore, I proxy banking access with the number of country banks per capita. I collect the locations of country banks and the years during which they survived from Dawes and Ward-Perkins (2000). The authors combined historical directories, bankers' records, and news records to construct a list of country banks across England since 1688. I geolocated the patents and banks and mapped them into 624 registration districts. Locating the patents and banks is one contribution of this paper.

To control for other factors that might affect patents, I include the natural logarithm of population, the natural logarithm of one plus number of newspapers within 50 km, access to navigable waterways, and the natural logarithm of hours taken to travel to London in the control variables. District-level population data for 1801, 1811, 1821, and 1831 is collected from census reports provided by the Great Britain Historical GIS Project (Southall, 2007). I use interpolation to fill in the data for other years between 1801 and 1825, assuming that the population grew at a constant rate between each two consequent census years. However, the Census started in Britain in 1801. To calculate district-level population before 1801, I use extrapolation based on population data from the 1801 Census and assume that the population growth rates of different districts in one single county are the same between 1750 and 1800. I calculate county-level population growth rate based on the estimates of the county-level population between 1750 and 1800 by Wrigley (2007).

To control for access to information, I control for the number of newspapers published within 50 km of a district. I collect the locations and surviving periods of newspapers from Richard Heaton's Index to Digitalised British and Irish Newspapers (Heaton, 2015). The newspaper index database includes more than 600 newspapers published outside London. I aggregate newspapers published within 50 km from the centroid of the district, approximately the distance that newspapers could cover and influence in the 18th century (Black, 1991).

I also control for traveling time to London for passengers using turnpike roads. People could collect more information in London and lower transportation time could facilitate information collection. To calculate traveling time to London, I use the turnpike road network by Rosevear et al. (2017). Bogart (2005) calculates the average traveling speed on turnpike roads in the 18th and early 19th century. Canals were more important in the transportation of bulk goods instead of passengers (Bogart et al., 2017). Assuming that passengers travel 2 km per hour from their residence to the nearest turnpike roads, I calculate traveling time to London as $T_{i,t} = \frac{Turnpike\ distance_{i,t}}{Turnpike\ speed_{i,t}} + \frac{distance\ to\ Turnpike\ Road_{i,t}}{normal\ speed_{i,t}}$. Changing the speed of traveling on normal roads, $normal\ speed_{i,t}$, to 3 km per hour does not change the results significantly.

Besides turnpike roads, another important component of the transportation network was navigable rivers that bulk transportation relied on. Based on the historical map of waterways in England and Wales in 1820 (Satchell & Shaw-Taylor, 2018). I retrieve the waterway map with descriptions of navigable waterways from 1750 to 1810 by the London Canal Museum[9].

3.3 Summary statistics

I evaluate how financial development affected innovation for the period between 1750 and 1825 because joint-stock banks became legal after the Country Banker Act in 1826. Joint-stock banks were much larger than country banks as they had more partners and partners had limited liability (Michie, 2016). Therefore, using the number of banks per capita as the proxy for banking access becomes more inaccurate and the impacts of joint-stock banks on patents might be different.

[9] The rough descriptions are found at https://www.canalmuseum.org.uk/history/menu-decades.htm.

Descriptive statistics are shown in Table 1. I choose to conduct my analysis at the registration district level, the smallest unit that I can map most patents and banks into. The district map that I use is based on the historical GIS of parishes in England and Wales (Satchell et al., 2017). The digital map was made for the parishes and places enumerated in the 1851 census for England and Wales. In this project, I stick to the same fixed boundaries across the whole period that I examine. There were 624 registration districts in England and Wales and 595 were outside London and the county of Middlesex.

I exclude the City of London and the county of Middlesex in the sample for the following reasons. First, country banks were located outside London and Middlesex. Both the Bank of England and London private banks served the government, businesses, and aristocrats in London and operated in the international market without paying much attention to British industrialists outside London (Michie, 2016; Voth, 2018). Second, the inter-district connection of people outside London was much weaker than within London due to transportation and information costs. Heblich and Trew (2019) argued that the radius a country bank could cover would be about the size of two parishes, no more than 10 km. Based on the patent records, I found that in Greater London, today's London City, Middlesex, and Surrey, there were some primitive regional specializations across different districts. For example, coach makers clustered around the Long Acre. Private London banks clustered in London City and Westminster. My empirical strategy might not apply to Greater London. Last, some patentees' addresses in Greater London were just where their hotels were. They did not fill in where they were from, but where they stayed during the application period, especially for patentees from colonies. The mistakes in addresses of patentees might contaminate the results.

[Insert table 1]

As I construct the dependent variable using the number of patents within 5 years after year t, the panel data is made up of 595 districts and 15 periods. In Table 1 I select data from the years of 1750, 1780, 1800, and 1820 to show changes in the number of patents, the number of banks, and time-varying controls across the period I examine. While the population grew by about 40% in the second half of the 18^{th} century, the number of patents increased by about 10 times and the number of banks increased by almost 50 times. Meanwhile, the mean travelling time to London witnessed a significant fall from 60 hours in 1750 to 25 in 1780 after the turnpike road mania ended. Traveling time to London continued to decrease slowly after 1780, mostly relying on improvements in horses and coaches.

4. Results

4.1 Baseline results

Table 3 presents my baseline results of how banking access affected innovation in the 595 sampled registration districts outside London and Middlesex. The variable $patents/pop$ refers to the number of patents per million people and $banks/pop$ refers to number of country banks per million people. As the influences of country banks were confined to second-order neighbouring parishes (Heblich & Trew, 2019), I cluster the standard errors on the registration district level in the baseline regression. I cluster the standard errors on the county level and use Conley standard errors in robustness checks. The results do not differ significantly.

[Insert Table 3]

In Table 3, I report the OLS and IV estimation results of equation (1). Column (1) reports OLS estimates with only district and year fixed effects and column (2) includes time-varying control variables. Column (3) and (4) show analogous specifications for my instrumental variable estimates. Column (5) and (6) show corresponding first stage results of the IV estimation in column (3) and (4). Note that there are fewer observations in the IV estimation as I dropped the destinations of post roads in case that the destinations were selected based on some omitted factors that might affect patents. My identification focuses on districts with post towns that were selected based on their locations on the post roads from London to destinations and distances from the previous post town, which were decided by horse strength and road conditions in the 16th century.

My OLS estimates suggest that the elasticities of patents per capita with respect to banks per capita range from 0.044 to 0.049. At the mean value of the independent variable, one standard deviation increase in the independent variable (2.099) increases the dependent variable by 5.97% to 6.70% of a standard deviation. This translates into an increase of 21.9% in the number of patents per capita in the next five years. My instrumental variable estimates are larger. The elasticities implied by IV estimation range from 0.163 to 0.218 and the effects expressed in standard deviation range between 22.2% to 29.7%.

My results can be compared to other estimates of how banks affected patents in the 19th century. Mao and Wang (2021) estimated that the elasticities are about 0.36 at the county level in Antebellum America. They considered the changes in the number of patents and free banks within 3 years of the passage of free banking laws, my estimates using a similar setting are about 0.080 as shown in Table A6.2. The impacts of English country banks on patents were much smaller than their peers in Antebellum America. This might be due to the smaller sizes and more conservative operations of country banks in England comparing to American free banks. The estimated average capital of country banks was about ￡10,000 by the end of the eighteenth century (Pressnell, 1956). While the average free bank assets in Antebellum America were about 500,000 US dollars (Mao & Wang, 2021). Considering that the exchange rate between pounds sterling and US dollars was about 1:5 (Davis & Hughes, 1960) in the 19th century. An average country bank was about one-tenth as large as an average American Free Bank. Due to their small sizes and the 5% interest cap placed by the usury law, country banks were reluctant to lend to risky fixed-asset investments and inventive activities. Their peers in Antebellum America actively sponsored manufacturers and small businesses and were widely involved in innovation and entrepreneurship (Mao & Wang, 2021).

My results could also be compared to results in different periods and settings, but I need to be cautious to interpret the comparisons. Cornaggia et al. (2015) reports that a one standard deviation decrease in interstate branching restrictions would increase the number of patents achieved in the next three years in a state by industries dependent on external finance in the United States by 3%. Meanwhile, Ayyagari et al. (2011) find that, for small and medium-sized firms in developing countries, getting access to bank loans would increase the chance of introducing a new product line by 20% and new technology by 26.1%. The comparison results show that the impacts of banks are larger in developing markets comparing to developed markets. Also, the effects are larger for the adoption of new technologies comparing to the invention of new technologies that is riskier.

4.2 Instrument validity

In Section 2, I have argued that districts with and without post towns were balanced in pre-existing characteristics. It is unlikely that post towns were selected based on some pre-existing characteristics that might affect patents in the period that I examine. There is also no evidence that time-varying variables that measure access to transportation networks and information were different across districts with and without post towns. Population in districts with post towns were lower while the population is positively correlated with patents per capita according to OLS and IV estimates in the baseline regression. Column (4) in Table 3 shows that after controlling for access to transportation networks and population, the impacts of banks on patents are still robust.

Although I have controlled for channels besides banks via which post towns might have affected patents, it is still possible that being on post roads might affect patents. For robustness checks, I do a balance test among districts crossed by post roads. The results are reported in Table A2 and are similar to the balance test of the full sample in Table 2.

Furthermore, being on post roads could directly affect patents. Post towns were on post roads that were safer and had better access to information (Dawes & Ward-Perkins, 2000). To exclude the possible impacts driven by omitted variables that existed on post roads, I do permutation tests. There were 383 districts that were crossed by some post roads and there were post towns in only 112 of them. In the permutation test, I randomly assign post towns to 112 of the 383 districts that were crossed by some post roads. As I cannot exhaust all possible combinations, I do 1,000 randomizations, rerun the IV estimation, and compare the coefficients to the estimates in the baseline regression reported in Table 3. If the impacts were driven by some omitted post road factors, I would expect that the baseline results are not significantly different from the results based on my randomized samples.

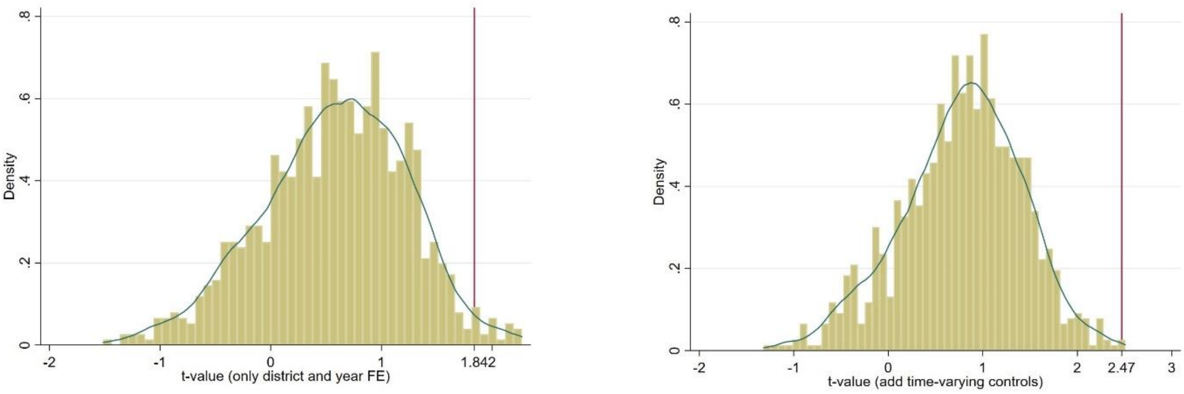

Figure 3 The distribution of t-values estimated in IV regressions based on random samples

In Figure 3, I show the histograms of t-values calculated using random post town samples on post roads. In the left panel, I show the results of the setting with only year and district fixed effects. The t-value in the baseline regression is 1.842 and only fewer than 2% of the t-values calculated based on randomized samples are larger than the baseline t-value. In the right panel, I add time-varying controls into the regression. The t-value in the baseline regression is 2.47 and it is among the top 3 largest t-values calculated from the random samples. The results show that it is unlikely that some omitted factors related to post roads drove the empirical results that I observed.

I also conduct placebo tests to test the validity of my instrumental variable. As post roads were designed to connect London to strategic locations, I draw straight pseudo post roads between London and the destinations of post roads. Then I create placebo post towns that divide pseudo post roads into equal distances that are approximately equal to 24 km, the average distances between real post towns. I use placebo post towns to construct instrumental variables and do IV estimation.

[Insert Table 4]

The results of placebo tests are reported in Table 4. In column (1) and (2), I create placebo post towns on pseudo post roads connecting to all destinations that I included in the baseline regression in Table 3. I control for only district and year fixed effects in column (1) and add time-varying controls in column (2). The IV estimates are negative and insignificant. The first stage coefficient in column (1) is positive and significant, but is only 1/3 the size of the coefficient in the baseline regression, as shown in column (5) of Table 3. The first stage coefficient in column (2) is insignificant. The KP F statistics is only about 4 in column (1) and 2.6 in column (2). In column (3), I include only placebo post towns on the pseudo post roads that connect to destinations near the borders, and I further restrict the placebo post towns to those connecting only to strategic destinations in column (4). My results show that placebo post towns do a poor job in predicting banking access. As terrains might affect the speeds of horses, the distances between real post towns would not strictly be 24 kilometres. It is unlikely that the locations of post towns affected banks and patents.

For robustness, I also use different post town sets to construct the instrument. I show the results of 2SLS regressions using different instruments in Table A8. Column (1) and (2) are the same as column (3) and (4) in Table 3. In column (3), I drop the post towns on the post roads connecting to Derby, Kendal, and Carlisle from the post town set. As Derby and Kendal are not near borders and the post road to Carlisle was redesigned in 1635, connection to these destinations might be subject to economic concerns. Comparing to column (2), the coefficient drops by about 15%. In column (4), I drop detouring points on post roads which might be important cities that were more prosperous. In column (5), I further restrict the range of post towns to those with populations smaller than 5,000 in 1600 (Bairoch, 1991). The results add to my confidence in the validity of my instrument.

According to the results in Table 3, IV estimates are about 5 times as large as OLS estimates. There are several possible reasons for these differences. One potential explanation is downward bias caused by the period after suspension of convertibility in 1797. In response to panics and loss of confidence in the notes issued by the Bank of England, the British government suspended convertibility. The public could no longer exchange Bank of England notes for gold. Going off the gold standard doubled the amount of notes issued by the Bank of England in 14 years (Michie, 2016). Larger amount of notes in circulation contributed to increases in credit supply and the establishment of new banks. New country banks that formed during the boom in the early 19th century were more speculative than earlier banks (Heblich & Trew, 2019). According to Figure 4, districts where the growth speed of banks per capita were higher than the common trend predicted by district and year fixed effects before 1797 witnessed slower growth speed of banks per capita after 1797. Those where banks per capita grew more slowly prior to suspension of convertibility witnessed higher growth speed after 1797. Figure 4 shows the relationship between the residuals of the independent variable after regressing on district and year fixed effects in 1775 and 1820. Almost all districts had the reverse of the growth speed of banks per capita. The relationship is still similar if I use different years from before and after 1797.

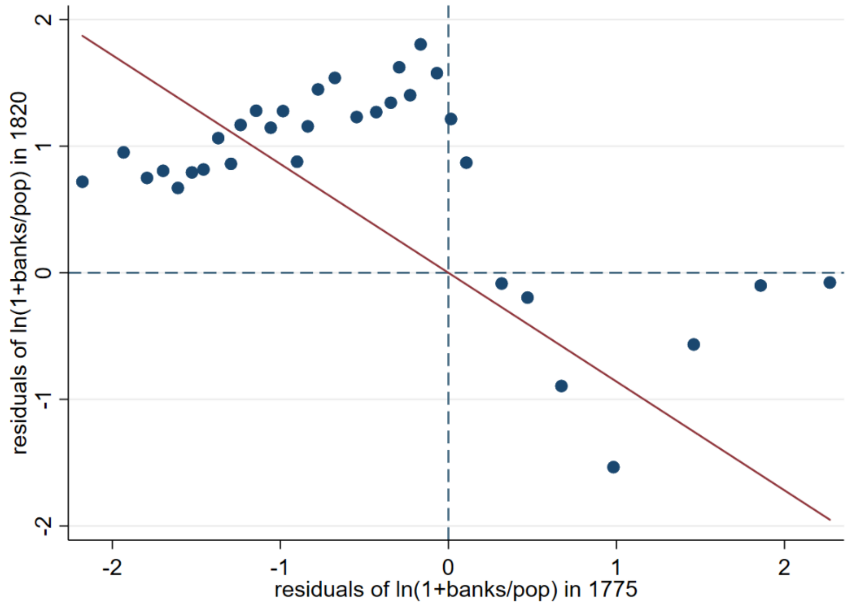

Figure 4 The relationship between residuals of the independent variable before and after 1797

In Table A10, I also do OLS and IV estimation for the period before 1797 and compare the results to the baseline regression. In column (1) and (2) of Table A10, the OLS estimates of the elasticities between the dependent variable and the independent variable before the suspension of convertibility range between 0.080 and 0.081, almost twice as large as the results in the baseline regression. In column (3) and (4), the IV estimates range between 0.198 and 0.236, close to the results in the baseline regression. The results also support that the subsample after the suspension of convertibility might downward bias the OLS estimates in the baseline regression.

One possible explanation would be the differences between local average treatment effects estimated by IV and average treatment effects of the whole population. The instrumental variables approach returns the results among compliers, districts with post towns that had higher growth speed of country banks per capita due to some specific pre-existing characteristic than other districts with post towns. Table A11 shows in Panel A how the effects of banks are different in districts with different pre-existing characteristics and in Panel B how the effects of post towns vary in different districts. In column (1), the impacts of post towns are smaller in districts in northern England, but the impacts of banks are similar in southern and northern districts. In column (3), the impacts of banks on patents are larger in districts with coal mines and the impacts of post towns are smaller in districts with coal mines. In column (8), the impacts of banks on patents are smaller in larger districts and the impacts of post towns are similar in districts with different sizes. In column (9) to (12), the impacts of banks on patents are smaller in districts more suitable for agriculture and the impacts of post towns are larger in districts more suitable for agriculture. Therefore, there is no evidence to support that local average treatment effects is an explanation for the IV estimates being larger than OLS estimates.

Another potential explanation is measurement error in measuring banking access and the impacts of banks on patents. The archives of country banks have vanished as country banks failed and there were no such public records as Bankers Almanac that recorded the amount of assets and loans in the 18^{th} century and early 19^{th} century. The number of banks per capita could not capture the differences in sizes of assets and loans of country banks and lead to measurement errors. Moreover, the backgrounds of country banks varied and their operation strategies varied. Country banks were usually thought of as providing short-term loans instead of lending to risky long-term investments (Pressnell, 1956; Michie, 2016). There were banks that mainly lent to industries that they were familiar with and encouraged adoption of advanced new technologies (Brunt, 2006). The impacts of banks on patents also differed from bank to bank, from district to district and might introduce measurement error. Another possible concern is weak instrument. According to Table 3, the Kleibergen-Papp F statistics range from 47 to 50. I do not believe that weak instrument is a plausible explanation in my setting.

4.3 Robustness checks

First, I control for the interaction of time-invariant variables and year fixed effects and county linear trends to rule out the impacts of pre-existing characteristics and common growth trends in banks and patents during the period that I examine. The results are reported in column (1) and (2) of Table A3.1. In column (3) to (6), I report OLS estimates similar to the setting in baseline regression and cluster the standard errors on the county level. Table A3.2 reports the results of using Conley standard errors. The results do not change significantly.

Next, I deal with concerns about the workhorse transformation of ln(1+x). Although I have used number of patents per million people instead of number of patents per capita when measuring innovations at the registration district level, concerns about using the workhorse ln(1+x) model remain. Therefore, I use inverse hyperbolic sine model instead of ln(1+x) in measuring innovations. The results are reported in Column (1) and (2) of Table A1 and the coefficient is about 20% larger.

Also, I would like to use other methods to measure innovation. I first use a binary model, setting the dummy variable 1(patent>0) to be 1 if any resident of a district achieved a patent in the next five years and 0 if there were no patents. Results are reported in Column (3) and Column (4). Better financial access is not only correlated with larger number of patents per capita, but also with the emergence of a patent. In Column (5) and (6), I measure innovation by number of patents and estimate a count model as there are many 0's, some 1's and 2's and a few larger number in patent counts. Consistent the baseline results, better banking access is correlated with larger patent numbers.

In the baseline regression, my dependent variable was constructed based on patentee counts. In Table A4.2, I also test whether the results are robust when I divide patents among all the patentees that co-authored specific single patent instead of using patentee counts.

If patents and banks can only be accessed by some wealthy people, per capita number of country banks might not be a good measurement of banking. Such is also the case for number of patents per capita. Therefore, I run baseline regression using natural logarithm of one plus number of patents in the next 5 years as the explained variable and natural logarithm of one plus number of banks as the explanatory variable. Results are reported in Table A4.3.

As mentioned in Section 2.1, simple patent counts might not reflect the quality of patents. I weight the patents using the *Woodcroft Reference Index* and the adjusted index proposed by Nuvolari and Tartari (2011) to reflect the economic values and importance of patents during the First Industrial Revolution. The results are reported in Table A5.1 and Table A5.2. Country banks not only led to more patents, but also patents of higher quality.

The window that other scholars (Cornaggia et al., 2015) use for patent counts the 20th century is usually 3 years. Inventors began to use scientific methods in their works (MacLeod, 1988) but standardized methods and procedures did not exist yet by then. There were few professional inventors or research and development professions, therefore, research and development activities in the 18th century might take longer time than today. In baseline regression, I use 5 years as the window and count patents from year t+1 to year t+5 as shown in equation (1). In Table A6.1, I report the results of using the windows of 3 years and 10 years and the results are still robust. In Table A6.2, I change the setting to one similar to the county-level regression in Mao & Wang (2021).

There were few patents in districts without country banks, so including districts without banks would make the effect larger. I run the regression in districts ever with at least one country bank during the period I examine in case that the result is driven by districts without country banks. The results are reported in column (1) to (4) in Table A7. As expected, the coefficients are smaller than the coefficients in the baseline. Similarly, to rule out the effects of districts without patents, I also run the regression in districts ever with at least one patent during the period I examine. The results are reported in column (5) to (8) in Table A7.

5. Mechanism

5.1 Heterogeneous effects on different sectors

The impacts of banks on patents in different sectors might vary as exposure to credit shortage varies. In the 18th and 19th century, the return to investments in agriculture was lower than the return to industrial investments in UK (Allen, 2009b; Ventura & Voth, 2015). The gap between returns to the industrial sector and those to agriculture shows that there was insufficient supply of credit in the industrial sector and sufficient credit supply in the agricultural sector.

As country banks mostly offered short-term credit and faced a 5% interest rate cap placed by the usury law, I expect that the impacts of country banks would be more expressed in the industrial sector and might not affect patents in the agricultural sector. To categorize patents, I use two taxonomy, one based on the occupations of the patentees and the other one based on the subject index of patents[10] (Woodcroft, 1857).

In Table 5, I categorize the occupations of patentees into five groups based on the Primary-Secondary-Tertiary (PST) system (Wrigley, 2010). Five categories are agriculture and mining, industrial manufacturer, traders, other services and professions and other occupations. One example of the patentees that belonged to other occupations is Archibald Cochrane, the 9th Earl of Dundonald. He patented for his new chemical in 1794.

[10] The taxonomy of patents based on the subject index is proposed in Nuvolari and Tartari (2011). I am grateful to Alessandro Nuvolari for the provision of the taxonomy of patents.

I report the effects of banks on patents acquired by people in agricultural sector, industrial sector, trading sector, non-trading service sector and other occupations respectively in column (1) to (5) in Table 5. Column (2) of Table 5 shows that the effects of banks on patents were mainly driven by patents acquired by patentees in the industrial sector. While only 58% of all the patents in my sample were acquired by industrial patentees, the coefficient in column (2) is almost as large as 90% of that in the baseline regression. A one standard deviation increase in the independent variable will lead to a 28.3% increase in the number of patents acquired by patentees in the manufacturing sector per capita. The coefficient in column (2) is statistically different from the coefficients in other columns[11] and the impacts of banks on patents in the manufacturing sector are significantly larger than patents in other sectors. As expected, there is no evidence that banks affect patents acquired by people in agriculture and mining where there was abundant capital and the returns to investments were lower. As for patents acquired by traders, most of them were acquired by merchants, ironmongers and chemists. Looking into the patents acquired by the previous three groups, I found that there are some patents directly related to the Industrial Revolution. There were improvements in textile machines, steam machines, metallurgy and making of chemicals. The insignificant result might be due to the fact that only no more than 20% of the patents were acquired by traders. I will dress the limitation of taxonomy based on jobs of patentees in the following part.

[Insert Table 5 here]

Some patentees failed to report their occupations and some patentees in the trading sector could patent a patent for manufacturing purposes. For example, James Watt did not claim his occupation in the patent record of his famous steam machine. For robustness, I use the taxonomy proposed by Nuvolari and Tartari (2011) and categorize 21 different industries[12] into the primary and secondary sector. Whether Construction, Leather, Military equipment and weapons and Medicines could be classified as

[11] The χ^2 value of testing differences between the coefficients in column (2) and column (1), (3), (4), (5) are 8.27, 8.87, 4.05 and 12.04 respectively. The p-vales are 0.004, 0.003, 0.044 and 0.001 respectively.

[12] They are Carriages, vehicles and railways, Chemical and allied industries, Clothing, Engines (steam engines, water wheels), Furniture, Glass, Hardware (edge tools, locks, grates), Instruments (scientific instruments, watches, measuring devices), Manufacturing machinery (other), Metal manufacturing, Paper, printing and publishing, Pottery, bricks and artificial stone, Shipbuilding, Textiles, Construction, Leather, Military equipment and weapons, and Medicines (drugs, surgical and dental instruments, other medical devices).

manufacturing industries might be subject to doubts. Therefore, I do not include them in the manufacturing sector in column (2) of Table 6 and gradually add them in column (3) to (6). See Table A12 for my classification of the 21 industries. The results are also consistent with my theory that the impacts of banks were mainly on the manufacturing sector without enough credit. There is no evidence that country banks added to patents in the primary sector where returns to investments were lower than returns in the industrial sector.

[Insert Table 6 here]

Table 5 and Table 6 show that the impacts of banks on patents were more expressed in the industrial sector that lacked credit and capital. This is consistent with the theory that banks contributed to innovation by relieving financial constraints and lowering financial costs. Although country banks mostly provided short-term credit to industrial manufacturers instead of directly lending to highly-risky fixed-asset capital investments and invention, they still contributed to the increases in patents. Provision of short-term credit of country banks allowed manufacturers to keep fewer cash reserves and invest more to innovative activities.

I also look into the heterogeneous impacts of banks on districts with higher agricultural suitability and districts with coal mines that might be more suitable for the industrial sector. As there was no occupation census or census of manufactures for England during the period that I examine, I use agricultural suitability as a proxy for the importance of agriculture in local economy and access to coal mines as a proxy for suitability for industries. The results are reported in Table 7. In Table 7, I use suitability for wheat, the most important crop in England in the 18th century, and use suitability for oats, ryes and barleys in robustness checks.

[Insert Table 7]

In column (1) and (2) of Table 7, I look into the heterogeneous impacts of banks on patents in districts with different suitability for wheat. I include only district and year fixed effects in column (1) and add time-varying controls in column (2). The interaction term of banking access and wheat suitability shows that the impacts of banks are smaller in districts more suitable for wheat and the average effects become negative when wheat suitability is around 44 to 48, 80 to 87 percentile among all districts. In column (3) and (4), I look into the different impacts of banks in districts with and without coal mines. The results show that the impacts of banks are larger in districts with access to coal mines. In robustness checks, I show that the results for wheat suitability are robust when I use suitability for oat, barley and rye. The effects were smaller in districts more suitable for agriculture and larger in districts with coal mines that were important for industrialization. The results in Table 7 are also consistent with my theory that country banks spurred patents by relieving credit constraints of manufacturing firms that lacked sufficient credit supply.

5.2 Initial interest rates

Due to financial frictions, the integration of financial markets in England was only limited and interest rates in different districts in England varied. In districts with higher interest rates, short-term credit provided by country banks might have larger impacts on patents. As there were no systematic records of interest rates for Britain during the 18th century (Brunt & Cannon, 2009), I follow Brunt and Cannon (2009) and construct county-level interest rates for England based on the price of wheat. As the recording of crop prices started only in 1771 in England, I calculate the average interest rates based on the price gaps between wheat prices in week 1 and week 29 of a year for the years between 1771 and 1774 and use the average rates as the initial interest rates of districts. I assume that all the districts within the same county face the same interest rate. In Table 8, I test how the impacts of country banks on districts differ in districts with different interest rates.

[Insert Table 8]

In column (1) and (2) of Table 8, I report the OLS estimation results for the full sample with only district and year fixed effects. In column (3) and (4), I add time-varying controls. The impacts of banks on patents are larger in districts with above-median interest rates. As the records of wheat prices started in 1771, the existence of country banks before 1771 might also affect interest rates. I report the results for the subsample after 1775 in column (5) and (6). The results are also consistent with the theory that banks relieved financial constraints faced by manufacturers in districts with lower credit supply and higher interest rates.

In robustness checks, I also test whether the effects are robust when I adopt other calculation of interest rates. Using the filtered crop price changes and crop price changes with adjustment for climates in Keller et al. (2021), I show in Table A9 that the results are similar to the results in Table 8.

5.3 Qualitative Evidence

In this section, I would like to discuss several mechanisms that country banks contributed to patents during the British Industrial Revolution. As the details of many patents and inventions have been lost, I could only rely on qualitative evidence that I extract from the biographies of some famous patents and inventions.

The first mechanism is that banks supported the invention process by providing credit to industrial manufacturers and is similar to what modern day banks do. Country banks sometimes directly supported the invention and patent process, usually when the banker knows the client well. John Kendrew, a Quaker, and Thomas Porthouse from Darlington developed a flax-spinning process in 1787 (Woodcroft, 1854). They were financially supported by James Backhouse, who was also a Quaker and founded a family bank in Darlington in 1774. James Backhouse not only supported them during the process of invention and patenting, but also helped them set up a small factory in the 1780s and 1790s (Cookson, 2003). James Backhouse's family bank was an established one and lasted for more than a century until was incorporated with Barclay & Co. in 1896.

Bankers could also provide credit to firms and enable invention by firm employees. In 1783, Thomas Bell registered a patent for the rotary printing machine that could print several different colours at the same time (Woodcroft, 1854). He came from Scotland and was working at Livesey, Hargreaves Hall and Co. in Preston (Riello, 2010) when he applied for the patent. In 1784, he also registered a patent that enables the user to print in six colours (Donnachie, 2004). The firm's bank was Byrom, Allen, Sedgwick and Place of Manchester, which was founded in 1771 (Smith, 2012). One of the partners, William Allen, made extensive loans to Livesey, Hargreaves and Company through the connection of marriage.

Some bankers directly participated in industrial production and spurred invention and patents. Walter Taylor of Southampton held 4 nautical patents as he was the owner of his family business that produced wooden rigging blocks for the Royal Navy. In the 1780s, he formed a partnership in Southampton with Richard Moody, a banker and brewer. Then he patented an invention related to malting and brewing in 1786 (Nuvolari

& Sumner, 2013). The partnership with a banker and brewer might have contributed to Taylor's patents in brewery.

Another potential mechanism is through infrastructure construction and investments. In the late 18th and early 19th century, local bankers usually involved in the construction of canals (Bogart, 2014). They would be appointed as the treasurer of a canal company's treasurer or provide short-term credit. William Mackworth Praed came from the Praed family who owned the Cornwall bank of Praed & Co that once lent a lot to promote the usage of the Watt-Boulton steam machine (Brunt, 2006). He was a partner of his family bank and the first Chairman of the Grand Junction Canal. In 1802, the Grand Junction Canal employed John Woodhouse and his brother Jonathan as members of a syndicate to complete the Bilsworth Tunnel (Petticrew & Austin, 2012). In 1805, John Woodhouse became the area engineer of the Northern district of the Canal Company and he acquired his patent of boat lifts that was used for canals in one year.

6. Conclusions

In this paper, I use panel data on banks and patents in England to argue that banks contributed to innovation during the First Industrial Revolution by providing short-term credit to manufacturing enterprises. This paper presents new evidence for evaluating the link between banks and innovation when banks rarely provided loans for fixed-asset investments and innovative activities and outside the United States. I find that better banking access led to more innovation, as measured by the number of patents per capita, in England and Wales during the First Industrial Revolution. Registration districts where there were more banks witnessed a faster growth of patents between 1750 and 1825. The estimate results suggest that credit market was beneficial for innovation during the period I examine when access to external finance is limited.

My finding shows that a standard deviation increase in banking access would lead to a 21.9% increase in patents per capita in the following 5 years. The effects are smaller than free banks in Antebellum America and the smaller effects might be due to smaller sizes and more conservative operations of country banks. I further show that the effects of banks are more expressed in the manufacturing sector and in districts that lacked sufficient credit supply. Qualitative evidence extracted from the biographies of inventors and bankers show that there were multiple direct and indirect channels that banks contributed to invention. My finding supports the claim that financial development stimulates innovation and helps explain why some parts of England started off earlier in the First Industrial Revolution from the perspective of finance.

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Table 1 Registration-level descriptive statistics for four selected years, 1750, 1780, 1800 and 1820

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	year	N	mean	sd	min	max
number of patents in the next 5 years	1750	595	0.0370	0.214	0	2
	1780	595	0.195	0.769	0	10
	1800	595	0.420	1.279	0	12
	1820	595	0.822	3.390	0	46
number of country banks	1750	595	0.0168	0.129	0	1
	1780	595	0.166	0.572	0	5
	1800	595	0.840	1.286	0	8
	1820	595	1.506	1.880	0	14
population	1750	595	9,663	5,029	1,086	35,784
	1780	595	11,333	6,173	1,165	49,602
	1800	595	13,474	8,130	1,306	79,115
	1820	595	17,969	12,215	1,778	120,731
hours to London via turnpike roads	1750	595	60.48	37.51	0.453	187.4
	1780	595	25.52	14.96	0.289	84.29
	1800	595	20.63	11.88	0.209	74.35
	1820	595	17.37	9.974	0.197	66.87
number of newspapers within 50 km	1750	595	4.267	15.49	0	67
	1780	595	7.486	25.21	0	109
	1800	595	8.466	28.00	0	121
	1820	595	9.790	29.27	0	128

Notes: This table presents summary statistics of country banks, patents and time-varying control variables. All variables are means across 595 registration districts outside London and Middlesex.

Table 2 Balance Tests of pre-existing characteristics and time-varying controls

		Coefficient	Standard Error
Panel 1: Pre-existing characteristics			
(1)	1 (Coal field in the district)	0.399	(0.535)
(2)	1 (Sea port in the district)	-0.0398	(0.0428)
(3)	Natural logarithm of the distance to the nearest sea port	0.105	(0.112)
(4)	Natural logarithm of the distance to the nearest coast	0.122	(0.143)
(5)	Natural logarithm of the area	-0.100	(0.114)
(6)	Average slope (percentage rise)	-0.644	(0.472)
(7)	Oat suitability	-0.610	(1.957)
(8)	Barley suitability	-0.526	(1.634)
(9)	Rye suitability	-0.411	(1.645)
(10)	Wheat suitability	-0.599	(1.647)
Panel 2: Time-varying characteristics			
(1)	ln (1+num of newspapers within 50 km)	0.00103	(0.000903)
(2)	ln (hours to London via turnpike roads)	0.000163	(0.000207)
(3)	ln(population)	-0.00113***	(0.000371)
(4)	1(waterway access)	-0.000121	(0.000739)

Notes: In Panel A, I report the results of regressing pre-existing time-invariant characteristic on the post town dummy. Panel A shows the differences in pre-existing characteristics across districts with and without post towns. In Panel B, I report the results of regressing time varying controls on the interaction of the post town dummy with linear year variable. Panel B shows the differences in growth rates of time-varying controls across districts with and without post towns. The coefficient column reports the coefficient of the main variable. Standard errors are clustered on the registration district level.

Table 3 Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)				ln(1+banks/pop)	
	OLS		IV		First Stage	
ln(1+banks/pop)	0.0437***	0.0490***	0.163*	0.218**		
	(0.0145)	(0.0141)	(0.0884)	(0.0881)		
1(post town)*year					0.0285***	0.0280***
					(0.00400)	(0.00406)
Observations	8,925	8,925	8,775	8,775	8,775	8,775
Within R2	0.00204	0.0125				
KP F Statistics			50.66	47.55		
Time-Varying Controls	None	Yes	None	Yes	None	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year
LHS SD	1.536	1.536	1.522	1.522		
RHS SD	2.099	2.099	2.077	2.077		
Standardized B	0.0597	0.0670	0.222	0.297		

Notes: Column (1) and (2) report OLS estimates of Eq. (1) and column (3) and (4) report the IV estimates. Column (5) and (6) report the first stage results of IV estimation. Time-varying controls include log population, log (1+newspapers in 50 km), log (traveling time to London) and access to waterways. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 4 Placebo tests

	(1)	(2)	(3)	(4)
	ln(1+patents/pop)			
ln(1+banks/pop)	-0.393	-0.239	-0.598	-1.113
	(0.333)	(0.353)	(1.030)	(3.267)
First Stage				
1(Placebo post town)*year	0.00897**	0.00714	0.00398	0.00210
	(0.00435)	(0.00439)	(0.00552)	(0.00576)
Observations	8,775	8,775	8,775	8,775
Destination sets	Baseline	Baseline	Drop non-border destinations	Strategic destinations
KP F Statistics	4.246	2.641	0.521	0.133
Time-Varying Controls	None	Yes	Yes	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year

Notes: This table reports IV estimation results using instruments constructed based on placebo post towns. Column (1) reports IV estimates of Eq. (1) with only district and year fixed effects and column (2) includes time-varying controls. In column (3), I keep only placebo post towns on post roads connecting to borders when I construct the instrument. In column (4), I further refine the post town sets to post roads connecting to strategic locations on borders. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 5 Heterogeneous effects on different sectors (by patentee's self-claimed occupation)

	ln(1+patents/pop)				
	(1)	(2)	(3)	(4)	(5)
	Agriculture & Mining	Manufacturing	Trading	Non-trading services	Others
ln(1+banks/pop)	0.00515	0.0393***	0.00495	0.0129	-0.00114
	(0.00317)	(0.0115)	(0.00506)	(0.00931)	(0.00100)
Observations	8,925	8,925	8,925	8,925	8,925
Time-varying Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year
Clustering	District	District	District	District	District

Notes: This table reports OLS regression estimates of Eq. (1) while the dependent variable is the natural logarithm of one plus the total number of patents acquired by patentees from different sectors in a district in year t+1 to year t+5 per million people in the district. Column (1) reports the result of patents whose patentees were from agriculture and mining. Column (2) reports the result of patents whose patentees were from the manufacturing sector. Column (3) reports the result of patents acquired by traders, column (4) reports the result of non-trading services and column (5) are other occupations. Standard errors are clustered on the registration district level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 6 Heterogeneous effects on different sectors (based on patent subjects)

	ln(1+patents/pop)					
	(1)	(2)	(3)	(4)	(5)	(6)
ln(1+banks/pop)	0.00856	0.0414***	0.0439***	0.0458***	0.0432***	0.0447***
	(0.00611)	(0.0124)	(0.0129)	(0.0130)	(0.0134)	(0.0135)
Observations	8,925	8,925	8,925	8,925	8,925	8,925
Time-varying Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year
Sector	Primary Sector	Secondary Baseline	(2) + Construction	(3) + Leather	(4) + Military	(5) + Medicine

Notes: This table reports OLS regression estimates of Eq. (1) while the dependent variable is the natural logarithm of one plus the total number of patents in different sectors in a district in year t+1 to year t+5 per million people. The taxonomy of patents are based on Nuvolari and Tartari (2011). Column (1) reports the result of patents related to Agriculture, Food and drink and Mining. Column (2) reports the result of patents in the baseline manufacturing sector. See Table A5 for detailed classification. Column (3) reports the result of secondary sector patents after including Construction and column (4) further includes Leather. Column (5) includes Military equipment and weapons while column (6) includes Medicines. Standard errors are clustered on the registration district level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 7 The impacts of banks in districts with different wheat suitability and access to coal

	(1)	(2)	(3)	(4)
	ln(1+patents/pop)			
ln(1+banks/pop)	0.167***	0.150***	0.0195	0.0346**
	(0.0344)	(0.0326)	(0.0155)	(0.0151)
ln(1+banks/pop)*wheat suitability/10	-0.0374***	-0.0309***		
	(0.00887)	(0.00839)		
ln(1+banks/pop)*1(coal access)			0.0701***	0.0408
			(0.0267)	(0.0267)
Observations	8,925	8,925	8,925	8,925
Within R2	0.00745	0.0160	0.00409	0.0131
Time-Varying Controls	None	Yes	None	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year

Notes: Column (1) and (2) report the different effects of banks in districts with different wheat suitability. Column (3) and (4) report the different effects of banks in districts with different access to coal mines. I include only district and year fixed effects in column (1) and (3) and add time-varying controls in column (2) and (4). Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 8 The impacts of banks in subsamples with different interest rates

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)					
ln(1+banks/pop)	0.0735***	0.0170	0.0732***	0.0329	0.0488**	0.00246
	(0.0195)	(0.0215)	(0.0192)	(0.0204)	(0.0233)	(0.0198)
Observations	4,350	4,575	4,350	4,575	2,900	3,050
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year
Within R2	0.00598	0.000298	0.0139	0.0152	0.00618	0.00978
Period	Full	Full	Full	Full	After 1775	After 1775
Subsample	Higher interest	Lower interest	Higher interest	Lower interest	Higher interest	Lower interest
Time-Varying Controls	No	No	Yes	Yes	Yes	Yes

Notes: This table reports the different effects of banks in districts with different interest rates. Column (1), (2), (3) and (4) report OLS estimates of two subsamples with the full time period and column (5) and (6) report the OLS estimates of the two subsamples after 1775. Districts with interest rates higher than the median interest rate are included in the subsample with high interest rates. Time-varying controls include log population, log (1+newspapers in 50 km), log (traveling time to London) and access to waterways. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Appendix

Table A1 Data sources

Data	Source	Notes
Patents	Woodcroft (1854)	correct errors in texts digitized by Google, geocode locations, and map into registration districts
Country banka	Dawes & Ward-Perkins (2000)	digitize, geocode locations, and map into registration districts
Post towns	Ogilby (1675)	
Population	Great Britain Historical GIS Project & Wrigley (2007)	extrapolation
Newspapers	Richard Heaton's Index to Digitalised British and Irish newspapers (2015)	
Turnpike road network	Rosevear, Satchell, Bogart, Sugden & Shaw Taylor (2017)	
Canals	Satchell & Shaw-Taylor (2018) & London Canal Museum	navigable waterways from 1750 to 1810 retrieved according to the records at https://www.canalmuseum.org.uk/history/menu-decades.htm.
Crop suitability	Global Agro-ecological Zones by FAO	
Slope	SRTM data with resolution of 90 metres	
Sea ports	Alvarez-Palau & Dunn (2019)	
Map of English registration district (and coast)	Satchell, Kitson, Newton, Shaw-Taylor & Wrigley (2018)	merged to one polygon to draw the coastline
Woodcroft Reference Index	Nuvolari & Tartari (2011)	
Taxonomy according to subjects	Nuvolari & Tartari (2011)	
PST system	Wrigley (2010)	
Crop prices	London Gazette	to use the result of Keller, Shiue & Wang (2021) in the future

Table A2 Robustness checks: balance tests on post roads

		Coefficient	Standard Error
Panel A: Pre-existing characteristics			
(1)	1 (Coal field in the district)	0.0488	(0.0545)
(2)	1 (Sea port in the district)	-0.0689	(0.0469)
(3)	Natural logarithm of the distance to the nearest sea port	0.205	(0.126)
(4)	Natural logarithm of the distance to the nearest coast	0.237	(0.155)
(5)	Natural logarithm of the area	-0.0542	(0.134)
(6)	Average slope (percentage rise)	0.155	(0.446)
(7)	Oat suitability	-2.279	(2.122)
(8)	Barley suitability	-1.764	(1.778)
(9)	Rye suitability	-1.638	(1.801)
(10)	Wheat suitability	-1.883	(1.805)
Panel B: Time-varying characteristics			
(1)	ln(1+num of newspapers within 50 km)	0.000843	(0.000992)
(2)	ln(hours to London via turnpike roads)	0.000161	(0.000220)
(3)	ln(population)	-0.000620*	(0.000373)
(4)	1(waterway access)	-0.000283	(0.000810)

Notes: In this table, I do balance tests across districts on post roads. In Panel A, I report the results of regressing pre-existing time-invariant characteristic on the post town dummy. Panel A shows the differences in pre-existing characteristics across districts with and without post towns. In Panel B, I report the results of regressing time varying controls on the interaction of the post town dummy with linear year variable. Panel B shows the differences in growth rates of time-varying controls across districts with and without post towns. The coefficient column reports the coefficient of the main variable. Standard errors are clustered on the registration district level.

Table A3.1 Robustness: additional controls and standard errors clustered on the county level

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)					
ln(1+banks/pop)	0.0451***	0.0439***	0.0437**	0.0490***	0.0451***	0.0439***
	(0.0140)	(0.0139)	(0.0178)	(0.0159)	(0.0145)	(0.0152)
Observations	8,925	8,925	8,925	8,925	8,925	8,925
Within R2	0.0537	0.0663	0.00204	0.0125	0.0537	0.0663
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year
Time-Varying Controls	Yes	Yes	No	Yes	Yes	Yes
Time invariant controls X Year FE	Yes	Yes	No	No	Yes	Yes
County Linear Trends	No	Yes	No	No	No	Yes
Cluster	District	District	County	County	County	County

Notes: In column (1) and (2), standard errors are clustered on the district level. In column (1), I include the interaction of time-invariant controls with year fixed effects. In column (2), I further add country linear trends. In column (3) to (6), the standard errors are clustered on county level. I include only district and year fixed effects in column (3), add time-varying controls in column (4), interaction of time-invariant controls and year fixed effects in column (5) and county linear trends in column (6). ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A3.2 Conley standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)					
Distance cut-off	50km	100km	200km	300km	400km	500km
Panel A: With district and year fixed effects						
ln(1+banks/pop)	0.044***	0.044***	0.044***	0.044***	0.044***	0.044***
	(0.0117)	(0.0125)	(0.0126)	(0.0122)	(0.0121)	(0.0122)
Panel B: With time-varying controls						
ln(1+banks/pop)	0.049***	0.049***	0.049***	0.049***	0.049***	0.049***
	(0.0114)	(0.0121)	(0.0123)	(0.0120)	(0.0120)	(0.0121)
Observations	8,925	8,925	8,925	8,925	8,925	8,925
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year

Notes: This table reports the estimation results when I use Conley standard errors. I use different distance cut-offs of 50 km, 100 km, 200 km, 300 km, 400km, and 500 km in column (1) to (6). The lags are set to 2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A4.1 Robustness checks with different models

	IHS(patents/pop)		1(patent>0)		patent	
	(1)	(2)	(3)	(4)	(5)	(6)
ln(1+banks/pop)	0.0509***	0.0571***	0.0107***	0.0120***	0.0398*	0.0480**
	(0.0166)	(0.0162)	(0.00316)	(0.00308)	(0.0224)	(0.0224)
Observations	8,925	8,925	8,925	8,925	5,325	5,325
Model	Hyperbolic sine	Hyperbolic sine	Binary	Binary	Poisson	Poisson
Time-varying Controls	No	Yes	No	Yes	No	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year

Notes: This table reports robustness checks conducted using different models. In column (1) and (2) the dependent variable is the hyperbolic sine transformation of the total number of patents acquired in a district in year t+1 to year t+5 over the population in the district. In column (3) and (4) the dependent variable is a binary variable. It is 0 if the number of patents acquired in a district in year t+1 to year t+5 is 0 and it is 1 if the number of patents is larger than 0. I use a count model in column (5) and (6). The dependent variable is the total number of patents acquired in a district in year t+1 to year t+5. Standard errors clustered on the registration district level are reported in paratheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A4.2 Robustness checks with different measurements of innovation

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)				ln(1+banks/pop)	
	OLS		IV		First Stage	
ln(1+banks/pop)	0.0427***	0.0480***	0.155*	0.209**		
	(0.0142)	(0.0138)	(0.0864)	(0.0858)		
1(post town)*year					0.0285***	0.0280***
					(0.00400)	(0.00406)
Observations	8,925	8,925	8,775	8,775	8,775	8,775
Within R2	0.00202	0.0125				
KP F statistics			50.66	47.55		
Time-Varying Controls	Yes	Yes	None	Yes	None	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year

Notes: This table reports OLS estimates of Eq. (1) and the dependent variable is the natural logarithm of one plus the total number of patents in a district in year t+1 to year t+5 per million people in the district. In this table, I divide patents among patentees before adding to district patent counts. In column (1) I only control for district and year fixed effects. I add time-varying controls in column (2). Column (3) and (4) show IV estimates and column (5) and (6) report first stage results. Standard errors are clustered at the registration district level. The results do not change significantly when I cluster standard errors at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively

Table A4.3 Robustness checks with different measures of banking access and innovation

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents)				ln(1+banks)	
	OLS		IV		First Stage	
ln(1+banks)	0.393***	0.393***	0.559*	0.723***		
	(0.0695)	(0.0670)	(0.285)	(0.270)		
1(post town)*year					0.00831***	0.00844***
					(0.00111)	(0.00111)
Observations	8,925	8,925	8,775	8,775	8,775	8,775
Within R2	0.00893	0.0188				
KPF			56.43	57.61		
Time-Varying Controls	None	Yes	None	Yes	None	Yes
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year

Notes: This table reports OLS regression estimates of Eq. (1) and the dependent variable is the natural logarithm of one plus the total number of patents in a district in year t+1 to year t+5 in the district. In this table, I divide patents among patentees before adding to district patent counts. In column (1) I only control for district and year fixed effects. I add time-varying controls in column (2). Column (3) and (4) show IV estimates and column (5) and (6) report first stage results. Standard errors are clustered on the registration district level. The results do not change significantly when I cluster standard errors at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A5.1 Robustness: patent counts weighted with WRI

	(1)	(2)	(3)	(4)
	ln(1+weighted patents/pop)			
ln(1+banks/pop)	0.0486***	0.0555***	0.0544***	0.0513***
	(0.0167)	(0.0162)	(0.0170)	(0.0164)
Observations	8,925	8,925	8,925	8,925
Within R2	0.00191	0.0147	0.204	0.221
Fixed Effects	District and Year	District and Year	District and Year	District and Year
Time-Varying Controls	None	Yes	Yes	Yes
Time invariant controls X Year FE	No	No	Yes	Yes
County Linear Trends	No	No	No	Yes

Notes: The dependent variable is constructed based on patent counts weighted with Woodcroft Reference Index proposed by Nuvolari & Tartari (2011). I add only district and year fixed effects in column (1), time-varying controls in column (2), the interaction of time-invariant variables and year fixed effects in column (3) and county linear trends in column (4). Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A5.2 Robustness: patent counts weighted with adjusted WRI

	(1)	(2)	(3)	(4)
	ln(1+weighted patents/pop)			
ln(1+banks/pop)	0.0407***	0.0461***	0.0453***	0.0426***
	(0.0140)	(0.0137)	(0.0144)	(0.0139)
Observations	8,925	8,925	8,925	8,925
Within R2	0.00184	0.0129	0.202	0.219
Fixed Effects	District and Year	District and Year	District and Year	District and Year
Time-Varying Controls	None	Yes	Yes	Yes
Time invariant controls X Year FE	No	No	Yes	Yes
County Linear Trends	No	No	No	Yes

Notes: The dependent variable is constructed based on patent counts weighted with adjusted Woodcroft Reference Index proposed by Nuvolari & Tartari (2011). I add only district and year fixed effects in column (1), time-varying controls in column (2), interaction of time-invariant variables and year fixed effects in column (3) and county linear trends in column (4). Standard errors are clustered on the registration district level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A6.1 Robustness: patent counts within a 3-year or a 10-year window

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(1+patents/pop)							
Window of patent counts	3-year				10-year			
ln(1+banks/pop)	0.0272***	0.0308***	0.0308***	0.0295***	0.0621***	0.0663***	0.0611***	0.0557***
	(0.0102)	(0.00985)	(0.00985)	(0.00969)	(0.0199)	(0.0196)	(0.0194)	(0.0196)
Observations	14,280	14,280	14,280	14,280	4,759	4,759	4,759	4,759
Years of Lag	3	3	3	3	10	10	10	10
Fixed Effects	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year	District and Year
Time-Varying Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Time invariant controls X Year FE	No	No	Yes	Yes	No	No	Yes	Yes
County Linear Trends	No	No	No	Yes	No	No	No	Yes

Notes: Instead of counting patents within 5 years in the baseline regression. I count patents within 3 years after year t in column (1) to (4) and patents within 10 years in column (5) to (8). I add only district and year fixed effects in column (1), time-varying controls in column (2), interaction of time-invariant variables and year fixed effects in column (3) and county linear trends in column (4). The settings in column (5) to (8) are similar to those in column (1) to (4). Standard errors are clustered on the registration district level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A6.2 Comparison of coefficients to Mao & Wang (2021)

	(1)	(2)	(3)	(4)
	ln(1+patents)			
ln(1+banks)	0.0750***	0.0736***	0.0736***	0.0759***
	(0.0171)	(0.0162)	(0.0162)	(0.0153)
Observations	14,874	14,874	14,874	14,874
Within R2	0.00963	0.0268	0.0268	0.0422
Years of Lag	3	3	3	3
Fixed Effects	District and Year	District and Year	District and Year	District and Year
Time-Varying Controls	No	Yes	Yes	Yes
Time invariant controls X Year FE	No	No	Yes	Yes
County Linear Trends	No	No	No	Yes

Notes: I count patents within 3 years after year t in this table. The independent variable is the natural logarithm of one plus the number of banks and the dependent variable is the natural logarithm of one plus the number of patents in district i. This setting is similar to county-level analysis in Table 6 of Mao & Wang (2021). I add only district and year fixed effects in column (1), time-varying controls in column (2), interaction of time-invariant variables and year fixed effects in column (3) and county linear trends in column (4). Standard errors are clustered on the registration district level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A7 Robustness checks: Restricted samples

	ln(1+patents/pop)							
	districts with banks				districts with patents			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(1+banks/pop)	0.0327*	0.0418**	0.0335**	0.0330*	0.0329	0.0405*	0.0458**	0.0445**
	(0.0173)	(0.0174)	(0.0170)	(0.0171)	(0.0216)	(0.0213)	(0.0210)	(0.0209)
Observations	6,000	6,000	6,000	6,000	5,325	5,325	5,325	5,325
Time-varying Controls	None	Yes	Yes	Yes	None	Yes	Yes	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year
Time-invariant controls X Year FE	No	No	Yes	Yes	No	No	Yes	Yes
County Linear Trends	No	No	No	Yes	No	No	No	Yes

Notes: This table reports OLS regression estimates of Eq. (1) with restricted samples. The results in Column (1) to (4) are results from the sample of registration districts that at least one country bank ever established in. The results in Column (5) to (8) are results from the sample of registration districts that at least one patentee was from. The dependent variable is the natural logarithm of one plus the total number of patents acquired in a district in year t+1 to year t+5 over the population in the district. The unit of population is million people. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A8 Two stage least squares regression results

	(1)	(2)	(3)	(4)	(5)
	ln(1+patents/pop)				
ln(1+banks/pop)	0.163*	0.218**	0.183**	0.191**	0.184*
	(0.0884)	(0.0881)	(0.0921)	(0.0971)	(0.106)
First Stage					
1(post town)*year	0.0285***	0.0280***	0.0281***	0.0273***	0.0253***
	(0.00400)	(0.00406)	(0.00425)	(0.00429)	(0.00443)
Observations	8,775	8,775	8,820	8,820	8,820
Time-varying Controls	No	Yes	Yes	Yes	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year
Kleibergen-Paap F statistics	50.66	47.55	43.80	40.49	32.67

Notes: This table reports 2SLS regression estimates of Eq. (1). The dependent variable is the natural logarithm of one plus the total number of patents acquired in a district in year t+1 to year t+5 per million people in the district. The instrument I use is the interaction of the dummy of having a post town in the registration district and linear year variable. In Column (1) and (2), I construct the instrumental variable based on all post towns that satisfy gap distances falling between 16 and 30 km. In column (3), I drop towns on post roads connecting to Derby, Kendal and Carlisle. In column (4), I drop detouring towns when I construct the instrumental variable. In column (5), I drop towns with population larger than 5,000 in 1600 when I construct the instrumental variable. There are 112 post towns in the first two columns, 98 in column (3), 96 in column (4) and 90 in column (5). Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A9 Different effects of banks in districts with different interest rates (Based on (Keller et al., 2021))

	(1)	(2)	(3)	(4)
	ln(1+patents/pop)			
Panel A: Non-adjusted interests				
ln(1+banks/pop)	-0.245**	0.0612	-0.255**	-0.000707
	(0.112)	(0.0799)	(0.128)	(0.0851)
ln(1+banks/pop)*interest	1.649***	-0.244	1.547**	0.0851
	(0.609)	(0.658)	(0.697)	(0.699)
Observations	3,960	4,215	2,640	2,810
Panel B: Interests adjusted for climate				
ln(1+banks/pop)	-0.208**	0.0668	-0.229**	0.0426
	(0.101)	(0.0506)	(0.105)	(0.0549)
ln(1+banks/pop)*interest	1.843***	-0.392	1.809**	-0.461
	(0.705)	(0.562)	(0.740)	(0.604)
Observations	4,020	4,155	2,680	2,770
Period	Full	Full	After 1775	After 1775
Subsample	Higher interest	Lower interest	Higher interest	Lower interest
Adjustment	None	None	None	None
Time-Varying Controls	Yes	Yes	Yes	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year

Notes: This table reports the effects of banks in districts with different interest rates. Column (1) and (2) report OLS estimates of the full time period and column (3) and (4) report the OLS estimates of the subsamples after 1775. I use filtered interest rates in Panel A and filtered interest rates adjusted for climate in Panel B. Time-varying controls include log population, log (1+newspapers in 50 km), log (traveling time to London) and access to waterways. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A10 Before the suspension of convertibility in 1797

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+patents/pop)				ln(1+banks/pop)	
ln(1+banks/pop)	0.0797***	0.0811***	0.198*	0.236**		
	(0.0231)	(0.0227)	(0.104)	(0.102)		
1(post town) * year					0.0367***	0.0362***
					(0.00595)	(0.00593)
Observations	5,950	5,950	5,850	5,850	5,850	5,850
KPF			38.01	37.23		
Model	OLS	OLS	IV	IV	First Stage	First Stage
Time-Varying Controls	None	Yes	None	Yes	None	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year

Notes: This table reports the estimation results when I use the sample before the suspension of convertibility in 1797 and run regression separately in two subsamples. Column (1) and (2) report OLS estimates of Eq. (1) and column (3) and (4) report the IV estimates. Column (5) and (6) report the first stage results of IV estimation. Time-varying controls include log population, log (1+newspapers in 50 km), log (traveling time to London) and access to waterways. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A11 Different impacts of banks on patents and post towns on banks in districts with different pre-existing characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline OLS						
	ln(1+patents/pop)					
ln(1+banks/pop)	0.0339*	0.0595***	0.0187	0.0358**	0.0541***	0.0558***
	(0.0174)	(0.0188)	(0.0156)	(0.0160)	(0.0179)	(0.0193)
ln(1+banks/pop)* 1(variable above median)	0.0197	-0.0372	0.0704***	0.0312	-0.0249	-0.0251
	(0.0245)	(0.0241)	(0.0272)	(0.0280)	(0.0244)	(0.0242)
Panel B: First stage						
	ln(1+banks/pop)					
1(post town)*year	0.0364***	0.0261***	0.0333***	0.0282***	0.0284***	0.0251***
	(0.00514)	(0.00548)	(0.00487)	(0.00426)	(0.00585)	(0.00582)
1(post town)* year * 1(variable above median)	-0.0139**	0.00487	-0.0120*	0.00220	0.000165	0.00604
	(0.00686)	(0.00705)	(0.00713)	(0.00997)	(0.00716)	(0.00716)
Observations	8,775	8,775	8,775	8,775	8,775	8,775
Interaction variable	Latitude	Longitude	1(coal mine)	1(port)	ln(port distance)	ln(coast distance)
Time-Varying Controls	None	None	None	None	None	None
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year

	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Baseline OLS						
	ln(1+patents/pop)					
ln(1+banks/pop)	0.0220	0.0760***	0.0886***	0.0808***	0.0821***	0.0798***
	(0.0176)	(0.0213)	(0.0219)	(0.0222)	(0.0221)	(0.0219)
ln(1+banks/pop)* 1(variable above median)	0.0392	-0.0643***	-0.0777***	-0.0633**	-0.0656***	-0.0626**
	(0.0239)	(0.0246)	(0.0250)	(0.0253)	(0.0253)	(0.0251)
Panel B: First stage						
	ln(1+banks/pop)					
1(post town)*year	0.0254***	0.0259***	0.0226***	0.0230***	0.0225***	0.0230***
	(0.00519)	(0.00618)	(0.00562)	(0.00565)	(0.00572)	(0.00565)
1(post town)* year * 1(variable above median)	0.00713	0.00471	0.0123*	0.0112	0.0120*	0.0112
	(0.00702)	(0.00725)	(0.00692)	(0.00696)	(0.00697)	(0.00696)
Observations	8,775	8,775	8,775	8,775	8,775	8,775
Interaction variable	Slope	ln(area)	Oat suitability	Barley suitability	Rye suitability	Wheat suitability
Time-Varying Controls	None	None	None	None	None	None
Fixed Effects	District, Year	District, Year	District, Year	District, Year	District, Year	District, Year

Notes: This table reports the different impacts on banks on districts with different time-invariant characteristics in Panel A and the different impacts of post towns on banks in Panel B. This table only includes district and year fixed effects. From column (1) to (12), 1(variable above median) would mean in the northern part of England, in the eastern part of England, having access to coal mines, having access to ports, above median distances to ports, above median distances to coasts, above median ruggedness, above median district area, above median suitabilit for oat, barley, rye and wheat. Standard errors clustered on the registration district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table A12 Classification of patents according to Nuvolari and Tartari (2011)

taxonomy of Nuvolari & Tartari (2011)	secondary sector1	secondary sector2	secondary sector3	secondary sector4	secondary sector5
Carriages, vehicles, railways	√	√	√	√	√
Chemical and allied industries	√	√	√	√	√
Clothing	√	√	√	√	√
Engines (steam engines, water wheels)	√	√	√	√	√
Furniture	√	√	√	√	√
Glass	√	√	√	√	√
Hardware (edge tools, locks, grates)	√	√	√	√	√
Instruments (scientific instruments, watches, measuring devices)	√	√	√	√	√
Manufacturing machinery (other)	√	√	√	√	√
Metal manufacturing	√	√	√	√	√
Paper, printing and publishing	√	√	√	√	√
Pottery, bricks, artificial stone	√	√	√	√	√
Shipbuilding	√	√	√	√	√
Textiles	√	√	√	√	√
Construction		√	√	√	√
Leather			√	√	√
Military equipment and weapons				√	√
Medicines (drugs, surgical and dental instruments, other medical devices)					√
Agriculture	primary	primary	primary	primary	primary
Food and drink	primary	primary	primary	primary	primary
Mining	primary	primary	primary	primary	primary

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Table Drop Influential Observations

	(1)	(2)	(3)	(4)
	ln(1+patents/pop)			
ln(1+banks/pop)	0.0422***	0.0471***	0.0411***	0.0355***
	(0.0136)	(0.0133)	(0.0114)	(0.00993)
Observations	8,835	8,835	8,475	8,025
Within R2	0.00195	0.0114	0.00711	0.00612
Fixed Effects	District, Year	District, Year	District, Year	District, Year
Time-Varying Controls	None	Yes	No	Yes

Notes: In column (1) and (2), I drop the 1% observations that are most influential on the regression results. In column (3), I drop the 5% most influential observations on the regression results. In column (4), I drop the 10% most influential observations. Standard errors clustered on the district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

Table Different impact of banks in districts with different population densities

	(1)	(2)	(3)	(4)
	ln(1+patents/pop)			
ln(1+banks/pop)	-0.0167	-0.0109	0.0110	0.0182
	(0.0164)	(0.0165)	(0.0144)	(0.0140)
ln(1+banks/pop) * higher density	0.106***	0.105***	0.219***	0.204***
	(0.0224)	(0.0220)	(0.0440)	(0.0415)
Observations	8,925	8,925	8,925	8,925
Within R2	0.00708	0.0174	0.0128	0.0218
Density threshold	Median 1750	Median 1750	100	100
Time-Varying Controls	None	Yes	None	Yes
Fixed Effects	District, Year	District, Year	District, Year	District, Year

Notes: This table reports how the impacts of banks were different in districts with different population densities. In column (1) and (2), I use the median population density in 1750, 37, as the threshold. In column (3) and (4), I use 100 as the threshold. Standard errors clustered on the district level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.