# The Rise of ‘New Corruption’: British MPs during the Railway Mania of 1845

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**Abstract**

In the 1840s, speculation in railway shares in the UK prompted the creation of hundreds of new railway companies. Each company needed to petition Parliament for the approval of new railway routes. In this paper, we investigate whether parliamentary regulation of the new railway network was distorted by politicians’ vested interests. Drawing on methods from peer-effects analysis, we identify situations where MPs could have traded votes with specific colleagues in order to get their preferred projects approved (logrolling). We confirm that logrolling was both prevalent and significant. Our estimates suggest that at least a quarter of approved lines received their bills because of logrolling. Companies approved through logrolling also underperformed in the stock market during the railway bubble and after its final crash in 1847.

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# 1 Introduction

Between the end of the Napoleonic wars and the Reform Bill of 1832, the British state underwent a profound transformation. Crown patronage was slashed and the size of government was reduced through the elimination of lucrative sinecures, a process referred to as the 'waning of Old Corruption.'(Harling 1996; Rubinstein 1983).

The reform process appears as a puzzle: how did the élite reform itself? While some opinion attributes the reform process to the political pressures arising from the French Revolution and the spread of a genuinely disinterested culture of public service, one would expect that reform would only be self-enforcing if the élite could be compensated for its lost rents. As Rubinstein asked, what 'took its [Old Corruption's] place as a source of revenues for the aristocracy and its minions?"(Rubinstein 1983: 73).

Measured in sinecures, rent-seeking in Britain definitively waned in this period, but this way of studying the problem focuses only on the efficiency of the government apparatus, while ignoring the policy choices made by the government. We believe an answer to the question of where the British élite found alternative sources of revenue may lie in studying how policy was made at the time. In particular, by elucidating the types of rent-seeking that arose as the British state grappled with regulating the market in the mid-19th century.

In this paper, we focus on one particular case: the process whereby British politicians picked the winners in the market for one of the leading technologies of the steam age – the railway. Moreover, we concentrate on a particular subset of political behaviour: vote-trading amongst British politicians to ensure their preferred projects won Parliamentary approval. This study therefore offers a way into the larger transformations in British political economy that occurred during the first half of the 19th century, and posits as an explanation for the surprising reform of 'Old Corruption' the emergence of a 'New Corruption'.

The context for our study is the wave of railway company promotion that occurred in Britain in the 1840s. Over 1,200 railway projects were registered in 1845 alone and this mania resulted in an unprecedented expansion of the British railway network that formed the basis of the railway system that endured into the 20th century (Dyos and Aldcroft 1969; Campbell and Turner 2015; Casson 2009).

Even though incorporation had been liberalised by the Joint Stock Companies Act of 1844, railway companies still had to petition Parliament for a Private Act that would allow them to begin construction of their intended lines. More than 700 did so at the height of the mania, between 1844 and 1845. Parliament screened the applications and approved fewer than half (60% of the companies applying for an act in 1844, and 45% in

1845).[1]

Why did Parliament pick the railway bills that it did? Given the importance of this selection process to understanding the British railway network, it is surprising that this question has received scant attention. It is particularly puzzling as the modern scholarly consensus tends towards the view that the British railway network involved wasteful duplication and that Parliament approved too many railways resulting in an inefficient network (Campbell and Turner 2015; Casson 2009; Odlyzko 2016). Casson estimates that "equivalent social benefits could have been obtained with only 13,000 miles of track" rather than the approximately 20,000 miles that were built (Casson 2009: 2). The only hypothesis advanced in the contemporary literature is that MPs voted for their vested interests – in particular, their interest in bringing railway projects to their constituents (Casson 2009).

The hypothesis that MPs were influenced by constituency pressure, or some other vested interest, is a particularly intriguing one in this context because Parliament amended its standing orders to ensure that no MP could directly influence the granting of a Private Act to a project in which he held a vested interest. This institutional set-up was devised to guarantee that railway projects were selected on the basis of their relative merits. However, the set-up did not completely exclude the possibilities for strategic voting as MPs could agree to vote for each others' interests. In this paper, we investigate whether logrolling swayed the process of parliamentary approval of railway lines.

The peculiarities of the institutional mechanisms created for the granting of Acts to railway companies enable us to evaluate the extent of legislative logrolling in this period. A common problem in estimating the impact of strategic voting is the fact that opportunities to logroll are not randomly allocated. Dealing with this endogeneity issue is easier in our case as MPs could not have foreseen their ability to trade votes strategically in advance of sinking in their interests in particular railway companies. This quasi-natural experiment setting, combined with direct information on MPs railways interests, offers a unique opportunity to observe which projects MPs were motivated to pass and whether they were able to logroll in order to obtain the approval of their preferred bills.

Our methodological approach for the detection of logrolling rests on social network analysis and the econometrics of peer-effects. MPs were mapped to railway companies through two networks: through their interests and through their role in selecting which lines would be allowed to be built. We model the networks of MP investments in railways, and the network of MP oversight of railway projects, and combine these two networks to

[1] Authors' calculations from data taken from a variety of Parliamentary Papers, namely, *1845 (637) Railway bills. –Railways. Return of all bills for the construction of railways in England and Wales, Scotland and Ireland, which have passed during the present session of Parliament.* (1845). Parliamentary Papers 637. House of Commons; *1847 (708) Railway Acts. Return of the number of railways for which acts were passed in session 1846.* (1847). Parliamentary Papers 708. House of Commons.

identify opportunities to trade. We then infer whether trading occurred by testing for significant endogenous effects between the voting outcomes of individual MPs and the MPs they were connected to in the network of potential trades.

In addition to responding to an unanswered question in economic history, this study contributes to the study of legislative behavior, and in particular the question of what determines a politician's vote on a bill. Formal political theory has long grappled with the question of the degree to which bargaining occurs between political actors, and what the ramifications of that bargaining might be (Buchanan and Tullock 1965; Haefele 1971; Koford 1982; Riker and Brams 1973; Uslaner and Davis 1975).

Empirically, logrolling is difficult to study systematically. This is because any given vote by a politician may well reflect their preferences and not a trade (Clinton and Meirowitz 2004). In consequence, although legislative histories and qualitative studies are rich in examples, it is difficult to measure the prevalence of vote-trading. In addition, bills often embed multiple issues and concerns, making it difficult to disentangle what is being voted for, and legislators often have many political priorities making it difficult to ascertain in what manner they have been politically 'repaid.' Indeed, the tendency for a given piece of legislation to cover a variety of issue areas is itself often taken as an indication of logrolling, as winning voting coalitions are constructed by adding items to legislation in order to win the votes of those with minority interests. This practice is sometimes labeled 'pork-barrel politics' in the US context, and has been the subject of several important studies (Ferejohn 1974; Evans 2004).

Despite the difficulties inherent in measuring logrolling, a small number of studies have nonetheless attempted to articulate a statistical framework within which one might test for evidence of logrolling (Stratmann 1992; Stratmann 1995; Irwin and Kroszner 1996; Kardasheva 2013; Aksoy 2012; Cohen and Malloy 2014). The clearest framework has been laid out by Stratmann, who argues for using the predicted votes of a potential trading interest group to test for the presence of a trade within a linear vote model.[2] The problem with this approach is that it completely depends on the correct specification of the voting model. In particular, it requires a valid method to pre-specify the set of possible vote trades. In contrast, our approach allows us to easily cope with trades across a very large number of bills simultaneously. Moreover, because we focus on a case in which opportunities to trade were not endogenous to MP characteristics, we can cleanly solve the statistical endogeneity problem that plagues attempts to separate preferences from

[2]Specifically, Stratmann argues that for a trade between issue $y$ and $w$, the presence of logrolling can be identified from the linear voting model $y = \alpha\hat{w} + X\beta + \epsilon$, where $X$ is a vector of politician and constituency controls, $y$ and $w$ are binary outcomes equal to one if a given politician voted yes, and $\hat{w}$ is the predicted value of $w$ obtained from an analogous specification $w = \gamma\hat{y} + X\delta + \eta$ (Stratmann 1992: 1164).

strategic voting.

Cohen and Malloy approach the problem in a spirit similar to ours, when studying the networks between politicians in the US senate (Cohen and Malloy 2014). However, their focus is on shared social characteristics facilitating vote-trading, and again the analysis depends on the correct pre-identification of cases in which trading should be likely. In contrast, we use a network approach to define the set of feasible trades, and then test whether the trades did occur. This gives a much cleaner estimate of the degree to which trading occurs.

The paper is organized as follows. In section 2 we introduce the historical context. We focus on the nature of the institutions created by the British Parliament to approve Private Railway Bills, and how that institutional structure enables us to identify logrolling. In section 3, we describe the methods we employ to detect feasible opportunities for logrolling. Section 4 introduces the data, which was compiled from a variety of 19th century sources. Section 5 sets up the estimation and reports our results. In section 6 we report a number of robustness checks, designed to interrogate the validity of our research design. Finally, in section 7 we shed some light on the question of whether logrolling is beneficial – by promoting gains from trade – or detrimental – by providing gains to the few while passing on the costs to the many. We offer a partial answer to this question by evaluating how the companies approved through logrolling fared in the stock-market in relation to their peers. Our results suggest that, at least in this case, logrolling was a private benefit to the politicians (or their constituents) who profited by it, but the companies involved were less valuable on average.

## 2 The Parliamentary Approval of Railway Bills in the mid-19th Century

The years 1844 and 1845 saw the explosion of speculative activity in joint-stock railway companies (; Odlyzko 2010; Evans 1849; Dyos and Aldcroft 1969; Casson 2009; Campbell 2013; Campbell and Turner 2012; Campbell 2014; Campbell and Turner 2015; Campbell and Turner 2010). Encouraged by low interest rates and the new Companies Act of 1844, which simplified the registration and promotion of joint-stock companies, interest in railway equities boomed and the creation of new railway companies rose in tandem.

Registering and promoting railway companies was a relatively simple affair, but building the line was more tightly regulated. Unlike a regular joint-stock company, railway companies required Acts of Parliament in order to begin construction on the line (Williams 1949). Thus every mile of British railway line that was constructed was done under the authorization of an Act of Parliament. The mania for railway speculation was so pronounced that in the year 1845 Parliament was overrun with applications from railway companies. The number of applications in 1844 (decided on in the parliamentary session of 1845) exceeded 200, and the number of applications in 1845 reached 550 individual lines. The significance of the promotional boom of the 1840s for the creation of the British railway network can hardly be overstated. Figure 1 shows the mileage of new railway lines authorized by Parliament annually, and the anomalous impact of 1845 and 1846 is apparent. Moreover, it must be recalled that this is merely the amount of line that was authorized, the amount of line *applied for* was easily double the amount approved in 1844 and 1845, and this should give some indication of the mania for promoting railway companies in this period.

[Figure 1 about here.]

The extent of the promotional activity implied an enormous amount of work for the individual MPs, who were required to hear evidence on each line seeking parliamentary authorization. In order to cope with the 'crush of business', a method of splitting and grouping railway projects into sub-committees was proposed by Gladstone in 1844. This was the system in operation in the parliamentary session of 1845 as Parliament tackled what was – at the time – the unprecedented number of railway companies promoted in 1844. However, as the number of applications trebled in 1845, the business of parliament was nearly overwhelmed by the necessity of hearing evidence in the committees on railway bills.

The number of railways promoted in 1844 and 1845 entailed the creation of very large numbers of subcommittees involving a very significant proportion of the British Parliament. In the parliamentary session of 1845 (which adjudicated the 1844 projects) there were 46 active subcommittees –each with five members– deciding on 210 railway bill applications (there were more applications, some of which dropped out before committee allocation); and in the parliamentary session of 1846 there were 67 subcommittees deciding on 508 railway bill applications. In the first years, 121 companies received an Act, whilst in 1846 Parliament granted 272 Acts.[3]

The institutional process of parliamentary approval was structured around the principles of avoiding conflicts of interest and considering competing schemes together. These were not uncontroversial choices, and in the 1830s many – including Robert Peel – had

[3] These numbers do not exhaust the universe of railway promotions, as many provisionally registered companies ended up not applying to Parliament (Geisler Mesevage 2016). We also had to exclude 42 companies applying to Parliament in 1846 for railway construction in Ireland for lack of information on the boundaries of the Irish constituencies.

argued that local MPs should oversee bills that affected their constituents, both as they had a duty to represent their interests and on the grounds that they possessed useful 'local knowledge' (Williams 1949: 81-84). However, by 1840 feeling had tilted towards the view that the conflicts of interest outweighed the value of a local representative, and committees were supposed to be composed of solely disinterested members. The result of this approach was a strategy of grouping: in each parliamentary session proposed railways would be grouped by the region they served and then a decision as to which of these schemes would be approved was passed to a committee in which in theory each member had no vested interest in the outcome of the decision. The committee would then hear evidence from the promoters in support of their bill, usually testimony from engineers and those who claimed their region would benefit from improved railroad communication. In addition, the committee would hear evidence from the railway's rivals, who would make efforts to disparage the plan and present it as lacking support in the community.

The precise mechanics of the committee system were laid out in 1844 by Gladstone's report on Railway Bills and the Standing Orders.[4]. In order to group the bills into sub-committees, Gladstone proposed a system whereby a *Select Committee on Railway Bills Classification* – composed of five members with three constituting a quorum – would group competing schemes, and schemes that competed with existing railways. Each group would then go to a five member committee who would hear evidence and decide which schemes to approve (Williams 1949: 85-86). Most of the filtering of railway bills occurred at this stage.[5] However, following committee approval the bills went to the House of Lords where they could be again overturned – although a much smaller proportion of bills were rejected at this stage.[6] The concept behind the method was modeled on judicial decision-making. The system was considered a success and by 1847 this method was being recommended for all bills not simply railway ones (Williams 1949: 87).

The timing of this system of railway grouping and committee formation is important. In the first step, typically occurring up to a year before the application to Parliament, railway companies acquired subscribers for their shares and plotted their proposed route. At the end of the year, all railways applied to Parliament by a November 30th deadline in order to have their application considered in the parliamentary session of the following

[4] *1844 (37) Railways. First report from the Select Committee on Railways* (1844). Parliamentary Papers. House of Commons; *1844 (79) Railways. Second report from the Select Committee on Railways* (1844). Parliamentary Papers. House of Commons; *1844 (166) Railways. Third report from the Select Committee on Railways* (1844). Parliamentary Papers. House of Commons.

[5] Formally, the process ended with a vote in the full House of Commons, but practically all recommendations from the committees were rubber-stamped by the House.

[6] For the railway applications we have data for, 85% of the projects approved by a sub-committee went on to receive an Act of Parliament – meaning they passed the House of Lords as well. We do not, however, model here the approval process in the Lords.

year. At the time of their application, railways submitted the plans of their proposed route and a list of the subscribers for their shares. These applications were then sorted by the Select Committee on Railway Bill Classification, which grouped the railways geographically and assigned MPs to the sub-committees. As a consequence of this sequencing, the route the railway would ultimately take and the railway's initial investors were fixed prior to the assignment of railways to committees.

All groups of railways were then evaluated in parallel – as the committees sat during the parliamentary session and heard evidence up until the point at which they had decided on all the railways in their subcommittee. The date at which a decision would be rendered on any given line could not be predicted *a priori* as it depended on the degree of opposition any individual line faced, and the time it took to gather evidence for and against the line. To give an indication, in 1845 the median committee sat for 8 working days, or about 2-weeks given the working hours of MPs, and the average committee sat for about 12.5 days.[7]

The key element of Gladstone's report was a rule to select the MPs who would sit in each sub-committee. MPs were barred from sitting on a sub-committee if that would entail them overseeing a railway in which they held an interest. Moreover, they were barred specifically on the criteria of local constituent interests and personal pecuniary interests, with the parliamentary motion stipulating that "each Member... before he be entitled to attend and vote on such Committee sign a Declaration that his Constituents have no local interest, and that he himself has no personal interests, for or against any Bill or project referred to him."[8]

We can actually verify that Parliament abided by this criterium to exclude conflicts of interest. Since a component of parliamentary oversight entailed the committees evaluating the quality and geographic distribution of the railway company's investors,[9] railway companies tendered to Parliament lists of their subscribers. From these lists we were able to observe the numerous MPs who invested in railway companies, with 42 MP investors in 1845 and 120 in 1846. In no single case was an MP assigned to a sub-committee that regulated a railway he had invested in.

Despite the enormous significance of these years in determining 19th century British

[7]Authors' calculations from *1845 (620) Railways. A return of the railway bills and projects, classified in their groups, which have been considered...* (1845). Parliamentary Papers 620. House of Commons , p. 8.

[8]*Hansard*, 4 March 1845, vol. 78, cc272.

[9]Parliament interrogated the investors, or more accurately subscribers, to a railway company on the basis of two criteria: First, they wanted to be sure that the investors were of sufficient means to pay the calls on the railway shares when they were made, but in addition, and more importantly, they used the geographical dispersion of railway investors to assess the degree to which there was 'local interest' in a railway project. Projects with more local subscribers were favored.

transport infrastructure, what actually accounts for why certain railway projects were chosen by Parliament and others were not has received scant attention. Nevertheless, a general view has emerged, as noted in the introduction, that a far greater number of railway schemes were approved than was strictly economically rational (Campbell and Turner 2015: 1250; Casson 2009: 18). Contemporaries tended to blame this generosity on venal influence – vaguely defined – and lamented how "a rush of sinister interests overwhelmed both the Government and Parliament" (Denison 1849: 618). Peel viewed the results of the system of sub-committees dimly, describing his reflections on it in a letter to then President of the Board of Trade, Lord Dalhousie: "I saw before me the results of active canvass by powerful companies ... members [of parliament], few of whom had read a word of the evidence ... were prepared to vote on other considerations than those of the merits of the questions."(Letter from Peel to Dalhousie, 22 June 1845, cited in Parris 1965: 86). Denison perhaps sums up the contemporary view when he writes that "at this present time everybody is ready to believe any story which imputes corruption to parliament" (Denison 1849: 607).

Given the institutional bulwarks designed specifically to hold back the 'rush of sinister interests', it is small wonder that modern historians have conjectured that logrolling must have played a prominent roll in explaining the suspected interference by vested interests. Mark Casson pins the blame for Parliament's generosity in granting Acts on the unwillingness of MPs to deny any constituency its desired line.[10] And given the restrictions on MPs voting for lines intended for their own constituency, Casson attributes this generosity to 'tacit collusion', and argues that "in a classic 'log-rolling' manoeuvre, they [MPs] collectively protected their local reputations as champions of the local railway schemes in order to safeguard their electoral popularity" (Casson 2009: 18).

In effect, modern historians are fairly unified in the view that too many railway companies were approved by Parliament. If this occurred due to vested interests those interests must have manifested indirectly, such as through logrolling, as Parliamentarians could not approve their own projects directly. It is this setting that forms the basis for our investigation into logrolling in the 19th century British Parliament. In the next section, we discuss methods for the identification of opportunities for parliamentarians to engage in logrolling.

## 3 Logrolling: A Social Network Approach

The historical context we have described creates an interesting setting for the study of logrolling. To see this, it is helpful to start by considering the problem in a more abstract

[10] Casson (2009), but see Odlyzko (2016).

framework.

We begin with a set of objects, here companies, which we denote $C$. We also have a set of politicians, $P$. There are two sorts of relationships that can obtain between any $c$ in $C$ and $p$ in $P$: relationships of *vested interest* and relationships of *oversight*. These two kinds of relationships define two matrices. First, there is a *vested interest* matrix of dimensions $P \times C$, where the $p, c$th entry is equal to 1 if politician $p$ is interested in company $c$ and 0 otherwise. Likewise, we define the *oversight* matrix of dimensions $P \times C$ such that the $p, c$th entry is 1 if $p$ oversees the approval of $c$ and 0 otherwise. We denote the *vested interest* and *oversight* matrices $V$ and $O$ respectively.

Our matrices of relationships can be combined to reveal logrolling opportunities. Following the intuition behind the use of affiliation graphs in social network analysis, we can compute a type of adjacency matrix:

$$M = O \times V^T. \tag{1}$$

This yields a $P \times P$ matrix that has the property that the value of the $i, j$th entry denotes the number of railways that politician $i$ oversees that politician $j$ is interested in. If we represent this network of connections as a directed graph, we can see that an arrow running from a node $i$ to a node $j$ means that $i$ oversees one of $j$'s interests. Thus the $M$ matrix encodes the network of relationships between MPs showing who is overseeing the interests of whom.

Given that we are able to graph who oversees whom, we can now easily identify opportunities for logrolling: a logroll is possible between two politicians wherever those politicians are in a cycle in our directed graph. Thus in the simplest case, an arrow running from $i$ to $j$ and from $j$ to $i$ would depict a cycle of length two, but longer cycles also exist, which would permit the construction of more complicated trades, say from $i$ to $j$ to $k$ and back to $i$. In that scenario, $i$ would oversee $j$'s project favorably, who would in turn go easy on $k$'s project, who would settle the trade by treating $i$'s interests generously.

By starting with simple collections of objects and actors, and defining two types of relationships that pertain between these two collections, we can generate the sets of feasible trades that form on the basis of these relationships. Of course, in practical terms, cycles beyond a certain length would be infeasible as trading strategies, as they would involve the coordination of too many participants. In consequence, we limit our attention to cycles up to length 3.

We might note a concern with this method here. The method is contingent upon the researcher measuring the relationships between companies and politicians. While the capacity of politicians to directly influence a given company's prospects (whether or not they vote on the company) is usually a matter of public record, it is far harder to uncover when a politician might have a vested interest: shares can be bought through proxies, not all interests are always disclosed, etc. Since, in the terms of our approach, interests are the currency with which political advantage is purchased, a failure to observe an interest will lead us to undercount the number of opportunities for logrolling. Practically, a failure to observe a politician's interest in a company will result in us missing a directed link between two politicians in our logrolling graph, and where that occurs we may believe that a politician does not possess an opportunity for logrolling when in fact he did. In general, this should lead us to underestimate any logrolling effect. In section 6.2 we employ a Monte Carlo analysis to show how these 'omitted links' bias our parameter estimates downward. We turn in the next section to a description of the data, and in particular, a description of how we measured and encoded the interests of MPs.

# 4 Data

Any analysis of this sort requires three sets of data. First, we need to define the oversight matrix $O$, which is given by the allocation of MPs to oversight roles in subcommittees. This is simply the MPs reported in *Parliamentary Papers* that sat on a committee, and the list of railways that they oversaw.[11] Second, we need to define and measure MPs vested interests in railway bills in order to define the matrix $V$. Finally, we incorporate data on the characteristics of MPs themselves, using the dataset compiled by William Aydelotte for the 1841-47 House of Commons (Aydelotte 1984). The unit of analysis in our data collection exercise is the individual MP, and the criteria for inclusion in our data set is that an MP was assigned to a sub-committee. Given that the individual railways, and in certain cases the individual MPs, were different in the two parliamentary sessions of 1845 and 1846 we treat each year as a separate cross-section.

## 4.1 Defining MP Interests

We use two sorts of connections between MPs and railways to define an MP as having an interest in a railway. The first source is whether an MP was a subscriber to a railway, which we compile from *Parliamentary Papers*.[12] These sources list all subscribers to railways for

[11] *1845 (620) Railways. A return of the railway bills and projects, classified in their groups, which have been considered...* (1845). Parliamentary Papers 620. House of Commons; *1846 (723-II) Sittings of the House. Divisions of the House. Private Bills and Acts. Private Bills.* (1846). Parliamentary Papers 723-II. House of Commons.

[12] *1845 (317) Railways. An alphabetical list of the names, descriptions and places of abode of all persons...* (1845). Parliamentary Papers 317. House of Commons; *1846 (473) Railways. Return ro an order the Honourable the House of Commons...* (1846). Parliamentary Papers 473. House of Commons.

sums in excess of £2,000. Even though this was a large sum, it required but a subscription payment of £100 to £200 depending on the size of the deposit – thus a reasonably large but not exorbitant investment – and likely not a particularly large sum for a member of Parliament.[13]

The lists of initial subscribers show an MP's initial investment in the company and are no guarantee that he still owned the shares he had originally subscribed. In consequence, the lists are an imperfect measure of MP investment in companies, but should be well-correlated with actual MP share ownership. Moreover, even if an MP had sold his subscription contract, he retained a liability for company debts until the railway was approved, at which point ownership transfers would be officially recorded by the company (Anon. 1847). If the company failed to obtain parliamentary approval, its creditors could sue the MP to recover the company's expenses. In consequence, even if an MP had sold, he might still have an interest in seeking the approval of a company in which he had subscribed.

The lists of initial subscribers are sometimes criticized for containing factually inaccurate information, or information fabricated by railway companies.[14] Such criticism may hold merit in general, but it is unlikely to apply to the investments of MPs in particular. Since the MPs themselves were set to verify the lists of shareholders, companies would be foolish to pretend to possess a connection with an MP as this would be uncovered and disproved. An encouraging sign that the information on MP subscriptions is accurate (that is, that companies were not pretending to have MP investors) is that when we compute our matrix $M$ as in 3 it is always hollow – meaning no MP ever overseas a company in which he is an initial subscriber.

We noted above that measuring interests can be problematic as there can easily be hidden interests. This is *not* the case for the second kind of interest we consider: whether the route of the proposed railway crossed the constituency of the MP in question. Data on the proposed routes of railways was not uniformly or reliably available. To resolve this issue we relied upon *Tuck's Railway Shareholder's Manual* which contained short descriptions of almost all projected railways, including lists of the towns they proposed to pass through (Tuck 1846). We geo-referenced the lists of towns provided with the description of each railway, and then matched the path of the railway back to shapefiles of the electoral districts as they existed in 1845. Thus, if a railway crossed an MP's electoral district we recorded a 1, and if it did not a 0. The shapefiles for England, Scotland and Wales were taken from the Visions of Britain website.[15]

[13]For the issue of partially-paid up railways shares see Campbell (2013).

[14]See the discussion in Campbell and Turner (2012: 9).

[15]Our computations are based on data provided through www.VisionofBritain.org.uk and uses historical material which is copyright of the Great Britain Historical GIS Project and the University of Portsmouth.

For those railways for which we could not find lists of towns they intended to pass through, we used the name of the railway to reconstruct an approximation of their route. This was possible as the convention was for all railway companies – with a handful of exceptions – to be named after their proposed route, such as the "Direct London and Portsmouth." This method is obviously not exact, as the actual routes could have crossed a constituency that would not have been predicted by a simple line between the terminal cities. Nevertheless, these companies account for only 12.8% of the railways in our sample in 1845 and 11.4% in 1846, and the listed towns are rarely separated by more than one constituency, so that even in this subpopulation classification errors are very unlikely.

The way we coded vested interests accords with the contemporary concerns voiced in Parliament. Moreover, they have the advantage that they were fixed prior to the assignment of railways to committees and thus avoid the problem of the endogenous formation of interests with an aim to create opportunities to trade. Naturally, however, they cannot represent the sum total of all interests MPs might have had in particular railways. Within our framework, these omitted interests would manifest as a 1 in the interest matrix $V$ where we currently record a 0. As a result, it may be the case that MPs we think do not have logrolls in fact do, and can act accordingly. However, as we show in section 6.2, these omitted interests are likely in expectation to lead to attenuation bias in our estimates. Consequently, the subsequent estimates should be taken as a lower bound for the true prevalence of logrolling in Parliament.

Finally, we draw on *Parliamentary Papers* to record which companies were approved and which were rejected. On this basis, we can compute the percentage of companies that an MP had an interest in that were approved. This is the primary dependent variable of interest. In certain cases, companies that were approved by a parliamentary sub-committee would end up having their final approval vetoed by the House of Lords. However, this was relatively uncommon, and in any event is not material to an analysis of whether logrolling was occurring in the House of Commons.

On the basis of our encoding of the $V$ and $O$ matrices, we can use definition 3 to compute the matrix $M$ that depicts all feasible logrolls. From this matrix, we need to extract the actual logrolls themselves so that we can create matrices that encode who had an opportunity to trade with whom. We identify the individual logrolling opportunities using a graph search algorithm designed for the identification of subgraph isomorphisms[16]

We were not able to locate historical constituency shapefiles for Ireland and in consequence our analysis is restricted to England, Wales and Scotland. As a result of excluding the Irish railways the sample of railways we consider in 1845 drops from 210 to 185, and in 1846 drops from 508 to 399.

[16] A subgraph isomorphism is a one to one incidence preserving correspondence of some smaller graph $g$ and a subsets of nodes and edges of some target graph $G$. Intuitively, it is the identification of the pattern described in a small graph in a sub-set of a larger graph.

from the *igraph* package in R. We searched for all subgraph isomorphisms that take the form of directed cycles of lengths 2 and 3.

Once we identified these individual opportunities to trade, and the names of the MPs involved, we constructed matrices directly encoding the opportunities to trade. The key to this transformation consists in making the connections within the cycles we identified complete: that is, if we found a cycle on the logrolling graph between MPs $i$, $j$ and $k$, we rewrite the matrix so that $i$, $j$ and $k$ are all connected to each other with multi-directional links, as opposed to a path of the form $i \to j \to k \to i$. This transformation reflects the fact that if a logroll of length greater than two is to occur, it must occur by everyone involved in the trade jointly compacting to coordinate their voting behavior. Thus we transform the matrix $M$ into a new matrix $\Lambda$ subject to

$$\Lambda = \begin{cases} 1, \text{if} M_{i,j} \in L_{i,j} \\ 0, \text{otherwise} \end{cases} \tag{2}$$

where $L$ is the list of connections between all MPs $i$ and $j$ that have a logroll, as determined from our search for cycles using the subgraph-isomorphism finder. For logrolls of lengths 2 and 3 this transformation is equivalent to making the graph undirected.

## 5 Specification and Estimation

We are positing that the logrolling relationship is one in which politicians can coordinate to influence the outcomes of projects in order to further their interests. However, due to the nature of the sub-committee system, not all politicians can coordinate to influence their outcomes, only the specific politicians that are connected to each other via an inter-committee logrolling opportunity. We can fully describe the set of these opportunities with the social network $\Lambda$. The empirical quantity of interest, therefore, is whether the outcome for a given politician is positively associated with the outcomes of the other politicians with which that politician has a logrolling opportunity. Additionally, however, we might imagine that MPs decided on whether or not to approve a project taking into account the characteristics of the other MPs to whom they were connected through the approval procedure by sub-committee. We can model this potentiality using the entire network $M$, which describes which MP oversaw the interests of any other M

If logrolling is occurring then MPs are agreeing to approve each other's projects, and as a result we anticipate that we should observe a correlation in outcomes for MPs who have the possibility of trading with each other. These types of models of strategic interaction are commonly found in the literature on so-called "peer effects," which seeks to model the impact of the outcomes of one's peer group on one's own outcome. Blume et al. (2013) have argued that simple utility functions based on linear gains from cooperation have stable equilibria and yield models that strongly resemble the canonical models used in the peer-effects literature, suggesting that the reduced form specifications are a good approximation to the underlying structural interaction.

Early work on the econometrics of peer-effects focused on the difficulty of achieving identification in these models, what has come to be known as the 'reflection problem' (Manski 1993). However, more recent econometric work has demonstrated that this problem can be easily overcome provided a sufficiently rich network structure underpins the analysis (Bramoullé, Djebbari, and Fortin 2009; Blume et al. 2013; Kelejian and Prucha 1998; Lee 2007). Our model of interest is the same as the model used in Bramoullé, Djebbari, and Fortin (2009), and takes the form

$$y = \alpha\iota + \rho\Lambda y + \beta X + \delta M^T X + \epsilon, \quad E[\epsilon|X] = 0. \tag{3}$$

The dependent variable is the success rate of an individual MP $i$ which we measure as the fraction of projects in which he has an interest and that get approved, or $y_i = approved_i/interests_i$. In this model, the expression $\rho\Lambda y$ represents what Manski termed the *endogenous effect*, with the parameter $\rho$ capturing the propensity of an individual's outcome to vary with the outcomes of their peers. $\Lambda$ is a row-normalized matrix that takes on a non-zero value whenever two-MPs could trade votes, so the term $\Lambda y = \bar{y}_g$, or the mean outcome value amongst the group of MP's who can logroll.

The expression $\delta M^T X$ captures what Manski termed *contextual effects*, which are the effects of the exogenous characteristics of one's peers on one's own outcome.[17] If we think of the vector of covariates $X$ as describing an MP's 'type', than the expression $M^T X$ is computing the average type in the group of MPs to whom MP $i$ is connected through the social graph $M^T$.

Models of the form of (3) emerged out of the literature in spatial econometrics (Ord 1975; LeSage and Pace 2009; Arbia 2014; Bivand, Pebesma, and Gómez-Rubio 2013; Bivand and Piras 2015; Leenders 2002). These models are identical in inspiration to the underlying structural model articulated by Manski, with the exception that group membership is defined by connections on a normalized network graph. The normal practice in the spatial econometrics literature is to represent group membership by an $N \times N$ matrix $W$ that has been row-normalized so each row sums to 1.[18] We follow this practice and row-

[17] The $M$ matrix is transposed so that it captures the characteristics of the MPs who oversee the projects of MP $i$, rather than the characteristics of the MP's whose projects MP $i$ oversees.

[18] Leenders (2002) provides a good discussion of different approaches to encoding the $W$ matrix in social network analysis.

normalize both $\Lambda$ and $M^T$. This model is typically referred to as a SAC model (LeSage and Pace 2009: 32) or, occasionally, as a SARAR model (Bivand and Piras 2015: 7). The close resemblance between Manski models of peer effects and spatial econometric models has been noted by a handful of authors (Lee 2007; Lee, Liu, and Lin 2010; Gibbons and Overman 2012).

A recent literature in econometrics has demonstrated that the parameters of these models can be reliably identified (Bramoullé, Djebbari, and Fortin 2009; Blume et al. 2013) and have provided the tools to do so (Kelejian and Prucha 1998; Kelejian and Prucha 2010; Lee 2003; Lee 2007; Lee, Liu, and Lin 2010). Given that the sociomatrix describing our endogenous effect $\Lambda$ and the sociomatrix describing the contextual effect $M$ are not identical, Blume et al. (2013: 18) show that we should have no difficulty identifying the parameters of our model using the generalized spatial two stage least squares (GS2SLS) estimator adopted in Bramoullé, Djebbari, and Fortin (2009: 49). The estimator is an adaptation of the models developed by (Kelejian and Prucha 1998) and (Lee 2003). We follow Bramoullé et. al's example in simplifying the model by assuming that the error term does not also follow a spatial process, but simply computing heteroskedasticity robust standard errors.

Our estimate of $\rho$ will be endogenous if the types of MPs who obtain logrolls differ from those that do not in a way that causes their outcomes to be correlated. We take up this point at length in the subsequent robustness checks in section 6. If our identification strategy is correct, then an MP's logrolling opportunities should be increasing in the number of interests he had, and the number of projects he oversaw. In consequence, we compute all our estimates controlling for the number of investments an MP has, the number of lines projected for his constituency, and the number of projects he oversaw.

[Table 1 about here.]

Table 1 shows the results of the GS2SLS model with a logrolling matrix covering cycles that include up to three participants for the year 1845. In addition to controlling for the predictors of logroll opportunities, we also control for a variety of MP social, political and economic characteristics.[19] Among the first, we included a categorical variable for MPs who had graduated from university and who shared a membership in the Athenaeum club. Contrary to other clubs, which were divided along party lines, the Athenaeum accepted members from both sides of the aisle (Cowell 1975). Consequently, we introduce it to test whether club membership could have lowered the costs of brokering trades among MPs

[19] For details see the online Data Appendix.

from different political parties.[20] We introduced three proxies for political affiliation: the conventional two-party classification computed by Aydelotte (1984), as well as dummies for "Reform MP" and "Free-trade Club Membership," which capture of political activity at the time not entirely spanned by party membership. In addition, we include dummies for whether the MP had a known connection to business interests, as coded by Aydelotte, and whether the MP had specific connections to canals which were reputed to be hostile to the railways. All of these covariates are also included as spatial lags, allowing us to see whether having one's project regulated by MPs who displayed these characteristics impacted on an MP's success rate. Table 2 reproduces this specification for the year 1846.

[Table 2 about here.]

The coefficient of interest, $\rho$, is significant and large across all specifications – averaging 0.5 in both 1845 and 1846. Given that coefficients in these models should not exceed 1 (econometrically and theoretically), this is a large result. Conceptually, if one's potential trading partners get one extra project approved, then, on average, one gets 0.5 extra projects approved. This could be thought of as indicating that half of all potential trades were acted on and successfully enforced (assuming that there was no reneging on a promised vote).

The number of railways projected to cross an MP's constituency is persistently negatively associated with the proportion of railways that are approved. This is sensible as it signals growth in the denominator of the outcome variable. Moreover, we might imagine that an MP would not be equally attached to all the railway lines projected in his district, thus some of these connections would not constitute significant motivation for logrolling. MPs with known business connections are more likely to get their projects approved in both years, but MP characteristics otherwise tend not to be stable across years. In certain cases this is fully consistent with the secondary literature: for instance, a canal connection strongly reduces the fraction of projects approved in 1845, which would make sense if canal MPs were uniformly hostile to railways including those in their own constituency in 1845. However, by 1846 it was argued that opposition had dimmed as canals sought to link up with railways and float their own routes, which is consistent with the coefficient dropping by an order of magnitude and becoming insignificant.

The differences between the coefficients on some variables in 1845 and 1846 should not be shocking, as the investment context was markedly different between these two years. During 1845 a growing bubble in railway equities lured many investors into the share

[20] We also ran the specifications with the ten other club membership variables in Aydelotte (1984) and the results were unchanged.

market and it is likely that the type of MPs who were involved in railways in the session of 1846 were as a result more heterodox (Campbell and Turner 2010).

It may well be that politicians were not overly sophisticated in their coordination of trades, and thus that a path length of 3 is too long. In consequence, we re-estimated the model using only logrolls of length 2 (Tables 3 and 4). The estimate of $\rho$ with logrolls of length-2 increases for 1845, but remains identical on average for 1846. The associated significance and magnitude of the coefficient estimates remain analogous across the two logroll lengths. This might indicate a higher propensity to logroll in 1845, but it may also reflect sampling variability.

[Table 3 about here.]

[Table 4 about here.]

Observed across specifications, there is very clear evidence of logrolling occurring. Marginal effects for spatial models can be difficult to interpret, and the normal geographic interpretation that focuses on the diffusion of the effect is not appropriate in this context. It is helpful to think about the impact of the logrolls in terms of the passage of an additional company. The dependent variable is the fraction of railways approved, or the number approved over the total number of railways in which an MP has an interest, $approved/interest$. If a MP approved one additional railroad, he would, on average, receive $\rho$ additional railways in return. On that basis, we could loosely estimate the total number of 'logrolled' railroads as $\rho$ times the number of MPs in logrolls. For 1845 this yields 12 additional railroads as a result of logrolling, or 12% of all the railways approved in 1845. For 1846 this yields 44 railroads which constitutes 23% of all railways that were approved. Although these estimates are approximate, they indicate that the size of the effect is indeed significant for understanding how Parliament may have approved too many railway companies.

# 6 Robustness Checks

In this section we consider two threats to identification: the endogenous formation of network ties and measurement error in the observation of network ties.

## 6.1 Endogeneity

Inferring causality from the characteristics of individuals linked via a social network is a fundamentally difficult problem due to what has been dubbed the 'generic confounding' of homophily and contagion (Shalizi and Thomas 2011). Our study, in simplistic terms, is a 'contagion' style model, insofar as we are arguing that the existence of a link on a graph (a logroll) can exert a causal impact on outcomes associated with the two individuals joined via the link – specifically, we suspect them of entering into unobserved compacts to aid each other. The potential confounding arises if the existence of the link itself is due to some characteristic of the MPs. A 'homophily' style of argument would postulate that the probability of being linked is the product of some other common characteristic of the MPs that itself may then account for the association of outcomes experienced by the MPs. In more familiar language, some omitted variable may be generating both linkage and outcomes.

We have been interpreting the results of the spatial models as evidence of the strategic behavior of parliamentarians, because we believe that the design of the committees constituted a sort of natural experiment – assigning logrolls to MPs accidentally, such that there will not be a latent factor driving both acquiring a logroll and experiencing a favorable outcome. In this section, we interrogate that claim by considering ways in which it could be falsified, and then testing to see if it can be sustained. We will consider violations of the research design in three broad categories: can MPs arrange to be appointed to committees so as to trade with their friends, can they maneuver to ensure a logroll in general, and finally is there a filtering process whereby MPs with vested interests ensure they at least get committee assignments (with or without logrolls) thus suggesting that the Select Committee could be pressured. We will take these problems in turn, beginning with the problem that would be most problematic for the research design.

The most obvious way in which the random assignment of logrolls to MPs can be violated is if MPs can maneuver to place sympathetic friends in positions to logroll with them. This might occur if MPs anticipated the benefits that could be obtained from a logroll, could perceive the distribution of their colleagues interests, and could lobby the Select Committee in charge of allocating committee assignments so as to obtain a posting that would grant them a logroll. *Ex ante* this seems unlikely, both because arranging to have the right people placed in the correct committee would be difficult, but also because if an MP was intent on having a colleague vote in their favor there were undoubtedly less complicated ways to achieve this: for instance, direct payments would elicit an effect comparable to a logroll with less hassle. This is one reason why it is likely that a logroll might be used if the opportunity presented itself, but was unlikely to be endogenously created by particular MPs.

Nevertheless, we will evaluate the game-ability of the committee assignment mechanism in two ways: first, we can test whether MPs got logrolls with other MPs that were more likely to be sympathetic to them ('placing friends in the right places'). And second, we can test if MPs arranged logrolls by seeing if the number of logrolls is anomalously high. We take these tests in turn.

If MPs were gaming the committee allocation system so as to obtain logrolls, they would attempt to have their project overseen by a sympathetic colleague. We would then expect that who an MP is linked to in a logroll would not be random, and correlation in their outcomes may be driven by some other type of similarity exhibited by these linked MPs. This can be evaluated using a network balance test: testing for evidence of network autocorrelation in any of the characteristics of an MP. The advantage of a balance test is that it provides some direct evidence for our claim that obtaining a logroll is randomly assigned.

Given some MP characteristic $x_i$, we can test random assignment by seeing whether the coefficient $\beta$ in the regression $x_i = \alpha + \beta\Lambda x_j + \epsilon$ is equal to zero. Since these are simple bivariate regressions we cannot employ the GS2SLS approach used in the main estimation, but must rely on OLS. It is well-known that in this context the OLS estimate of $\beta$ will be biased upwards due to reflection bias (Manski 1993). We use the formula offered in Caeyers and Fafchamps to correct for reflection bias and compute the true $\beta$ as a function of the estimated $\hat{b}$ (Caeyers and Fafchamps 2016: 24).

More problematic is what is known as 'exclusion bias': a mechanical negative correlation between an outcome and the mean of that outcome within a social network that can arise in naïve OLS estimates (Caeyers and Fafchamps 2016). However, correct inference in the presence of exclusion bias can be obtained by randomly permuting the social network in question $N$ times – generating $N$ random counter-factual networks – and computing an estimate $\hat{b}_n$ $N$ times, and then computing an exact p-value as a result of this bootstrapping procedure. Since the coefficients $\hat{b}_n$ are computed for the case in which there genuinely is no effect (since we have randomized peer groups manually), there is no need to apply a reflection-bias correction, as $\hat{b} = \beta$ when $\beta = 0$ as the bias stemming from the reflection problem is multiplicative (Caeyers and Fafchamps 2016).

Table 5 shows the results of estimating bivariate network peer-effects regressions for each covariate in our sample with a bootstrapped mean and standard deviation of the mean, and a two-sided p-value computed using the network randomization inference procedure elucidated in Caeyers and Fafchamps (2016). We compute the network autocorrelation coefficient for both years, using for each year the $\Lambda$ matrix with logrolls up to length 3. For 3 of the variables it is not possible to compute a network autocorrelation coefficient as they are binary measures and no MP with the condition happened to be linked to an MP who also had it. For no single variable for which we can compute a p-value in any year is the network autocorrelation coefficient significant. This suggests that of the things that we can measure about MPs, nothing we can measure about them appears to drive the correlation in outcomes estimated in the main model.

[Table 5 about here.]

Another way to conceptualize the selection problem is to imagine that if the logrolling opportunities are randomly distributed, then given the constraints imposed by the allocation of MPs to projects in such a way as to avoid direct conflicts of interest, and given the observed distribution of interests, there is some probability distribution that describes the number of MPs that will receive a logroll by chance. If MPs are actively maneuvering in some manner to ensure that they will oversee the project of a colleague who oversees their investments, and thus logrolls are not being randomly generated, then this should result in an inflation of the number of logrolls relative to the number that would occur by chance. If in fact MPs are manipulating the system to ensure that they receive a logroll, and if the kinds of MPs who do this are more similar in their characteristics than those that do not, our estimate of the value of the parameter could be biased upward. It is worth noting that this would be an odd form of bias, as it would arise as the result of the politicians we are studying seeking to engage in precisely the behavior whose existence we have set out to uncover. Moreover, the network autocorrelation test should already provide some evidence that this practice is not occurring.

Nevertheless, we can evaluate the proposition that MPs are engineering more logrolls than what would have emerged due to chance provided we can come up with a good characterization of the number of logrolls that should arise given a set constellation of interests. Our hypothesis is that MPs were allocated to committees subject to the sole criteria that they could not have an interest in the railways they were overseeing. We can simulate a process of that nature by generating random allocations of MPs to committees, while maintaining the restriction that nobody can oversee a project they have an interest in. We then compute the number of MPs that receive a logroll in these randomized allocations. By doing that many times, we are able to simulate the distribution of the number of MPs that receive logrolls under the assumption that MPs are allocated to committees randomly subject to the condition that they not have a conflict of interest. We then check whether the observed number of MPs that receive logrolls appears to be drawn from the distribution governing the number that receive logrolls randomly.

We implement this procedure by keeping the interests of MPs fixed, and randomly re-populating the membership of committees. For each committee, we re-populate it by randomly picking MPs from the pool of MPs who sat on committees. We then check whether our allocation of MPs to committees violates the restriction that MPs cannot regulate projects they have an interest in, and if it does we discard the iteration and re-allocate MPs to committees. A valid random committee allocation results in a new oversight matrix $O_{rand}$, which we use to compute a new $M$ matrix,

$$M_{rand} = O_{rand} \times V^T. \quad (4)$$

Once we have computed $M_{rand}$, we ascertain how many MPs on this new network received logrolls. To do so, we exploit a property of adjacency matrices, namely that if you raise the square adjacency matrix $A$ to the $k$th power, the $i, j$th entry of $A^k$ is equal to the number of paths of length-$k$ between $i$ and $j$. Since a cycle on a graph is a path from $i$ to $i$, the diagonal entries of the matrix $A^k$ tell us the number of cycles of length $k$ that each MP sits on (Newman 2010 : 136-37). Thus, we can compute the total number of MPs that receive logrolls up to a cycle length of 3 with the formula:

$$N = \sum_{i=1}^{i=I} 1[\sum_{k=1}^{k=3} diag(M_{rand}^k) > 0], \quad (5)$$

where 1[.] is an indicator function that returns 1 if the expression in square-brackets is true, and $diag()$ is the diagonal of the matrix.[21] The restriction we impose on our simulation is that the sum of the diagonal of $M_{rand}$ when $k = 1$ must equal 0. For each year we generate 500 random committee allocations, and the results of equation 5 for each iteration are plotted in Figure 2.

[Figure 2 about here.]

Figure 2 shows histograms of the number of MPs that received a logrolling opportunity in our simulation exercise. The red line on each graph represents the observed number of MPs that receive logrolling opportunities in our sample. In both simulations, the observed number of logrolls appears consistent with having been generated by the random allocation of MPs to committees subject to the condition that MPs cannot oversee railway projects they might have a vested interest in. The simulation exercise provides no evidence that the number of logrolls we observe is anomalously high, and thus it appears unlikely that MPs were gaming the allocation system in order to ensure that they obtained an opportunity to trade.

Finally, an MP could only receive a logrolling opportunity if they got to sit on a committee. Not all MPs were assigned to committees, and if all the MPs with vested interests in railway companies were assigned to committees that might constitute some evidence that they were pressuring the selection committee with the intention of acquiring

[21] The inner summation is across logrolling cycle lengths. The outer summation is across the number of rows in the square matrix, which we have called $I$, and adds up the number of MPs that received a logroll (have a non-zero entry on the sum of diagonals).

a logrolling opportunity. We can evaluate this by simply looking at some differences in the covariates of MPs that did and did not receive logrolls. We would anticipate there to be some differences, as some members of Parliament were barely active, and would not be likely to be tapped for membership of a committee.

[Table 6 about here.]

The most important covariates to compare between MPs that did and did not receive committee assignments is whether they differ in the number of railways they invested in, or the number of railways that were projected to be built in their constituency. We can see from Table 6 that there is no statistical difference in the value of these covariates for MPs that did and did not receive committee assignments. Aydelotte's measure of whether an MP was active in business is significant, but the mean is higher for MPs who did *not* obtain committee assignments. We suspect this reflects the selection committee's interest in screening out those with potentially conflicting interests. In addition, the MPs selected to sit on committees had slightly higher average education, and the difference is statistically significant. The substantive significance of this is not apparent.

### 6.2 Measurement Error

We evaluate the measurement error problem of unobserved interests by simulating network data for which the parameter of interest $\rho$ is known, and then estimating the value of $\rho$ when interests are randomly deleted from the social network graph.

We begin with a graph of 500 observations and set $\rho$ equal to 0.7. We create a social network graph $\Lambda$ with density .005 and $M$ with density .01. We also generate a matrix of covariates $X$, and draw a vector of errors $\epsilon$ from a standard normal distribution. We generate our outcome variable $y$ with the formula

$$y = (1 - \rho\Lambda)^{-1}[X\beta + MX\delta + \epsilon]. \tag{6}$$

We then randomly delete a certain percentage of connections on the $\Lambda$ matrix and compute $\hat{\rho}_{bs}$, which is an estimate of the value of $\rho$ using a GS2SLS estimator under the condition of missing links. For each percentage of links that we delete, we re-estimate $\hat{\rho}_{bs}$ 500 times. Figure 3 shows the distribution of our bootstrapped estimates of $\rho$ for different degrees of missingness. It is clear that the median of the distributions shift to the left as the number of unobserved links increases. Moreover, for small degrees of missingness the average bias is negligible. The tendency of missing links to push the estimator towards zero provides some evidence to support the hypothesis that the characteristic impact of unobserved interests will be to induce attenuation bias of our coefficient estimates.

[Figure 3 about here.]

Taken together, our interrogation of the assignment of MPs to committees, the similarities of MPs on committees, and the number of logrolling opportunities we observe in our data are all consistent with our identification strategy. There is no evidence of strategic maneuvering to obtain a logroll, and thus no evidence that the logrolling network itself – our sociomatrix $\Lambda$ – is endogenously formed. Thus it would appear that when the opportunity to trade votes presented itself, MPs availed themselves of it, but that they were not able or motivated to manipulate the system in order to acquire logrolling opportunities.

## 7 Assessing the consequences of logrolling

In this section we try to quantify the social consequences of logrolling. Theoretical debate has been split as to whether logrolling should improve welfare (gains through trade) or deplete it by allowing concentrated but minority interests to push negative externalities onto the majority.[22] More specifically, we might recall the vigorous debate in Hansard as to whether local knowledge would improve the committee process, and imagine that logrolling might allow MPs with local interests to insure the best-placed line for their constituency. However, knowing that historians have tended to disparage both the number of bills approved and the resulting network, it seems more probable that logrolling facilitated local, or even personal, interests, while foisting the externality of a poor-quality company and an ill-designed network onto the public.

If the practice of trading votes to ensure the passage of railway bills that were favored by MPs ushered into existence companies of lower quality than might otherwise have been approved, we can evaluate the companies' quality in a partial way by looking at their financial performance. Specifically, we evaluate whether logrolling had socially negative externalities by testing whether the companies that were 'logrolled' compare unfavorably to their peers. This comparison is rendered more complicated by the difficulty of cleanly identifying which companies were approved as the product of logrolling (as mentioned in section 5). To simplify the matter in this section, therefore, we default to a binary measure, sorting the companies that were passed in the parliamentary sessions of 1845 and 1846 into two groups: those that could have been logrolled, due to their connections to MPs, and those that could not.

To evaluate firm performance, we collected share price data from the *Economist* Railway Monitor, and aggregated the data up to weekly observations in order to deal with

[22] The classic citation for the positive case is Buchanan and Tullock (1965), chapter 10 in particular, although the authors hardly offer a ringing endorsement. Riker and Brams (1973) made the classic case for the deleterious effects of logrolling.

the fact that many firms were only occasionally quoted. The price data was pooled across quotations stemming from different exchanges, as certain companies were more frequently quoted in Liverpool or Manchester than London.

We then computed the Tobin's-Q (Tobin 1969) for each firm for each week from the end of the parliamentary session in 1846 until 1848 – thus capturing firm performance in the window following the completion of the parliamentary approval process. In order to compute a correct Tobin's-Q, it was necessary to convert partially paid shares into the equivalent fully paid shares, using the correction derived by Campbell (2013)[23]. Table 7 displays summary statistics for the Tobin's Q data we computed, as well as other company-level covariates.

[Table 7 about here.]

[Figure 4 about here.]

Figure 4 shows how the mean Tobin's-Q varies between the two groups and over time. The evidence is broadly consistent with our intuitions about the relative quality of firms that were and were not logrolled. The mean-difference is robust to the inclusion of a variety of covariates, as can be seen in Table 8. Since logrolls are not time-varying, we estimate fixed-effects models in which treatment is interacted with time, and random effects and pooled models in which it is not. In all specifications being in the group that could have been logrolled is associated with a lower Tobin's-Q, with parameter estimates in the random and pooled models suggesting a mean difference in the range of $-0.09$ to $-0.12$. If we compare the size of these estimates to the summary statistics in Table 7 we can see that companies that may have owed their existence to logrolling suffered a penalty of a little over half a standard deviation of the Tobin's Q. This is a very significant quantity, especially in a period of high volatility in stock prices, as the years between 1846 and 1848.

[Table 8 about here.]

The impact of logrolling on a company does not appear to be time-varying, nor do we have prior reasons to think it would be, and therefore the pooled-OLS or random effects models appear most likely to capture the difference in mean Tobin's-Q that would appear to be the central effect of interest. Coefficients on a dummy for whether or not a firm could have been logrolled suggest that on average logrolled firms had Q values approximately 0.10 lower than their non-logrolled peers. This would be consistent with a pessimistic view

[23] See the online Data Appendix for details.

of logrolling in this period – namely, that politicians' pursuit of local or private interests generated negative externalities for the broader public. The findings in this section are merely exploratory, and designed to gesture towards what we believe to be a fruitful path forward, namely, the exploration of the ramifications of collusive political behavior.

There are a number of ways in which this test of company quality may be understating the true effect. For instance, we did not identify pricing data for all the companies that were approved by parliament, and in consequence our comparison of firms that could have been logrolled to firms that could not already reflects a layer of filtering that may have removed the most problematic firms from our analysis. If the worst logrolled firms were more likely to fail, and thus less likely to generate observable price data, this would result in us underestimating the size of the performance penalty associated with a logrolled firm. Likewise, our measure for whether or not a firm was approved as a consequence of logrolling is measured with error, and a more precise identification of the 'treated' population might yield a concomitantly greater effect.

More broadly, however, this exercise illustrates a predictable and sensible consequence of logrolling, namely that the companies that are approved as a consequence of interested rent-seeking may be of lower quality than others. But the social costs of this behavior will not be fully reflected in the pricing of the companies' equity. The total costs of rent-seeking behavior should have been manifest through the impact these practices had on the resulting network that was built.

Recent work in economic history has emphasized the importance of initial endowments in pushing economies into better and worse equilibria through path-dependence (Bleakley and Lin 2012). Some recent work has highlighted the impact that the articulation of a given transport network structure can have on trade – reassessing Fogel's estimates of the impact of railways on the US economy (Swisher IV 2017). What we would emphasize is that the compounded impact of the chosen railway network over time – and the effects that may flow from the path-dependence induced by initial endowments – is, at least partly, the consequence of political rent-seeking and merits the greatest future exploration.

## 8 Conclusion

The mid-1840s saw the promotion of an enormous part of the British transport network, and the proliferation of numerous railway lines. Parliament's role in the creation of this network has remained puzzling, as MPs appeared inexplicably lenient in their granting of railway acts, despite occasional vocal opposition by landowners and canal companies. The claim that this leniency might have been explained by Parliament succumbing to vested interests was complicated by an institutional design intended to thwart outside pressure, but susceptible to logrolling. The historical context thus offers both an opportunity to better understand strategic voting, and to explain an enduring historical puzzle. Our findings indicate that logrolling was prevalent and significant, that it accounted for a large fraction of the railways approved by Parliament, in the region of 12% to 23% depending on the parliamentary session. Moreover, if anything, these results are a lower bound on the true prevalence of logrolling, as a consequence of unobservable trades.

In methodological terms, this study offers a way for detecting evidence of logrolling. We show that the crucial requisite for the detection of logrolling is that the set of feasible trades is restricted – an MP cannot trade with all other MPs – by an exogenous process. If this holds than methods common in spatial econometrics and social network analysis can be used to test statistically for evidence of logrolling. The method allows for testing the extent of logrolling in a given legislative body and time period across a very large number of bills, and thus allows the evaluation of the prevalence of logrolling more generally, rather than restricting analysis to studying a trade between two specific issue areas as is common in previous methods. Moreover, the articulation of logrolling in social network terms offers a flexible framework for thinking about complex strategic arrangements. We believe the method offers a useful framework for investigating vote-trading and other forms of tacitly collusive behavior.

Our analysis verifies not only that logrolling was prevalent, but that in this instance it had associated costs, decreasing the quality of the resulting companies. Whether it also contributed to degrading the efficiency of the railway network that emerged as a result of Parliament's deliberations is an important question that merits further research.

The broader context within which this study is inscribed is an effort to understand the transformations that occurred in the British state between the end of the 18th century and the middle of the 19th century. The process of governmental reform, which has been variously described as the dismantling of a British *ancien régime* or the reform of the excesses that emerged during the American and Napoleonic wars, presents a puzzle for social scientists seeking to explain how the revenue of the élite was dramatically curtailed by its own doing.[24] We believe that a focus on "New Corruption," by which we mean the opportunities to profit from the rents that emerged at the intersection of politics and the market, offers a fruitful avenue for understanding the reform process. Viewed in this way, the waning of 'Old Corruption' can be depicted as a shift in élite strategy from the direct extraction of rents from the state, to the use of state power to extract rents from the market.

[24]The view that much of 'Old Corruption' was of relatively recent vintage and emerged during the wars was argued by Brewer (1990: 72).

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Table 1: General Spatial 2SLS, 1845, Logroll up to Length 3

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| $\rho$ | $0.55^{*}$ | $0.53^{**}$ | $0.48^{**}$ | $0.49^{*}$ |
| | (0.27) | (0.18) | (0.17) | (0.21) |
| Intercept | $0.13^{*}$ | $0.13^{**}$ | $0.13^{**}$ | $0.18^{***}$ |
| | (0.05) | (0.05) | (0.05) | (0.03) |
| Num RW Projected in Constituency | $-0.00^{*}$ | $-0.00^{**}$ | $-0.00^{**}$ | $-0.00^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Num RW Investments | $-0.03$ | $-0.03$ | $-0.03$ | $-0.00$ |
| | (0.05) | (0.04) | (0.04) | (0.02) |
| Num RW Overseen | 0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.01) |
| Liberal Dummy | 0.06 | 0.07 | 0.07 | |
| | (0.05) | (0.04) | (0.04) | |
| Education Dummy | 0.03 | 0.03 | 0.04 | |
| | (0.04) | (0.05) | (0.05) | |
| Athenaeum Memb. Dummy | $-0.02$ | $-0.02$ | | |
| | (0.04) | (0.05) | | |
| Business MP Dummy | $0.10^{\cdot}$ | $0.13^{*}$ | $0.13^{*}$ | |
| | (0.06) | (0.06) | (0.06) | |
| Reform MP Dummy | $-0.20^{***}$ | $-0.18^{***}$ | $-0.18^{***}$ | |
| | (0.05) | (0.05) | (0.05) | |
| Canal MP Dummy | $-0.20^{***}$ | $-0.20^{***}$ | $-0.20^{***}$ | |
| | (0.04) | (0.04) | (0.03) | |
| Freetrade Club Dummy | $0.19^{***}$ | | | |
| | (0.05) | | | |
| Constituency Pop. | 0.00 | | | |
| | (0.00) | | | |
| Lag RW Proj. in Const. | 0.00 | $0.01^{**}$ | $0.01^{**}$ | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Num RW Invest. | $-0.09^{***}$ | $-0.11^{***}$ | $-0.11^{***}$ | $-0.07^{**}$ |
| | (0.02) | (0.03) | (0.03) | (0.03) |
| Lag Num RW Overseen | 0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Liberal | $0.14^{**}$ | 0.06 | 0.06 | |
| | (0.05) | (0.05) | (0.05) | |
| Lag Education | $-0.13^{**}$ | $-0.04$ | $-0.04$ | |
| | (0.05) | (0.03) | (0.03) | |
| Lag Atheneum Memb. | $-0.04$ | $-0.01$ | | |
| | (0.06) | (0.07) | | |
| Lag Business MP | 0.06 | 0.10 | 0.10 | |
| | (0.09) | (0.09) | (0.08) | |
| Lag Reform MP | $-0.13$ | $-0.10$ | $-0.10$ | |
| | (0.09) | (0.07) | (0.07) | |
| Lag Canal MP | 1.03 | $1.11^{\cdot}$ | $1.11^{\cdot}$ | |
| | (0.63) | (0.62) | (0.62) | |
| Lag Freetrade MP | $-0.10$ | | | |
| | (0.15) | | | |
| Lag Const. Pop. | $0.00^{*}$ | | | |
| | (0.00) | | | |
| Num. obs. | 195.00 | 195.00 | 195.00 | 195.00 |
| $R^2$ | 0.21 | 0.18 | 0.17 | 0.06 |

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table 2: General Spatial 2SLS, 1846, Logroll up to Length 3

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| $\rho$ | $0.50^{**}$ | $0.52^{**}$ | $0.46^{**}$ | $0.41^{*}$ |
| | (0.18) | (0.17) | (0.17) | (0.17) |
| Intercept | $0.09^{**}$ | $0.09^{**}$ | $0.09^{**}$ | $0.12^{***}$ |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| Num RW Projected in Constituency | −0.00 | $-0.00^{**}$ | $-0.00^{*}$ | $-0.00^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Num RW Investments | $0.03^{***}$ | $0.03^{***}$ | $0.03^{***}$ | $0.03^{***}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Num RW Overseen | −0.00 | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Liberal Dummy | 0.02 | 0.01 | 0.02 | |
| | (0.03) | (0.03) | (0.03) | |
| Education Dummy | 0.04 | 0.03 | 0.04 | |
| | (0.03) | (0.02) | (0.02) | |
| Athenaeum Memb. Dummy | 0.01 | 0.02 | | |
| | (0.03) | (0.03) | | |
| Business MP Dummy | $0.09^{*}$ | $0.10^{*}$ | $0.10^{*}$ | |
| | (0.04) | (0.04) | (0.04) | |
| Reform MP Dummy | −0.04 | −0.04 | −0.04 | |
| | (0.04) | (0.04) | (0.04) | |
| Canal MP Dummy | −0.02 | −0.02 | −0.02 | |
| | (0.08) | (0.08) | (0.08) | |
| Freetrade Club Dummy | 0.02 | | | |
| | (0.08) | | | |
| Constituency Pop. | $-0.00^{*}$ | | | |
| | (0.00) | | | |
| Lag RW Proj. in Const. | $-0.00^{\cdot}$ | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Num RW Invest. | 0.01 | 0.00 | 0.00 | $-0.01^{\cdot}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Lag Num RW Overseen | 0.01 | $0.01^{\cdot}$ | $0.01^{\cdot}$ | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Liberal | 0.06 | 0.06 | 0.06 | |
| | (0.10) | (0.09) | (0.09) | |
| Lag Education | −0.07 | −0.06 | −0.06 | |
| | (0.05) | (0.04) | (0.04) | |
| Lag Atheneum Memb. | −0.01 | −0.00 | | |
| | (0.09) | (0.06) | | |
| Lag Business MP | −0.13 | −0.12 | −0.12 | |
| | (0.09) | (0.09) | (0.09) | |
| Lag Reform MP | −0.16 | −0.13 | −0.12 | |
| | (0.13) | (0.13) | (0.11) | |
| Lag Canal MP | −0.31 | −0.24 | −0.24 | |
| | (0.23) | (0.20) | (0.20) | |
| Lag Freetrade MP | 0.27 | | | |
| | (0.27) | | | |
| Lag Const. Pop. | 0.00 | | | |
| | (0.00) | | | |
| Num. obs. | 285.00 | 285.00 | 285.00 | 285.00 |
| $R^2$ | 0.21 | 0.20 | 0.19 | 0.15 |

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table 3: General Spatial 2SLS, 1845, Logroll Length 2

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| $\rho$ | $0.85^{***}$ | $0.67^{***}$ | $0.53^{**}$ | $0.58^{***}$ |
| | (0.17) | (0.17) | (0.18) | (0.13) |
| Intercept | $0.13^{**}$ | $0.13^{**}$ | $0.13^{**}$ | $0.18^{***}$ |
| | (0.05) | (0.05) | (0.05) | (0.03) |
| Num RW Projected in Constituency | $-0.00$ | $-0.00^{*}$ | $-0.00^{*}$ | $-0.00^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Num RW Investments | $-0.02$ | $-0.03$ | $-0.03$ | $-0.00$ |
| | (0.05) | (0.04) | (0.04) | (0.02) |
| Num RW Overseen | 0.01 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.01) |
| Liberal Dummy | 0.06 | 0.06 | 0.06 | |
| | (0.05) | (0.05) | (0.04) | |
| Education Dummy | 0.03 | 0.03 | 0.04 | |
| | (0.04) | (0.05) | (0.05) | |
| Athenaeum Memb. Dummy | $-0.02$ | $-0.03$ | | |
| | (0.04) | (0.05) | | |
| Business MP Dummy | $0.11^{\cdot}$ | $0.14^{*}$ | $0.14^{*}$ | |
| | (0.06) | (0.06) | (0.06) | |
| Reform MP Dummy | $-0.20^{***}$ | $-0.18^{***}$ | $-0.18^{***}$ | |
| | (0.05) | (0.05) | (0.05) | |
| Canal MP Dummy | $-0.21^{***}$ | $-0.21^{***}$ | $-0.20^{***}$ | |
| | (0.04) | (0.03) | (0.03) | |
| Freetrade Club Dummy | $0.17^{***}$ | | | |
| | (0.05) | | | |
| Constituency Pop. | 0.00 | | | |
| | (0.00) | | | |
| Lag RW Proj. in Const. | $0.01^{**}$ | $0.01^{***}$ | $0.01^{**}$ | $0.00^{*}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Num RW Invest. | $-0.11^{***}$ | $-0.13^{***}$ | $-0.12^{***}$ | $-0.08^{***}$ |
| | (0.02) | (0.03) | (0.03) | (0.02) |
| Lag Num RW Overseen | $0.00^{*}$ | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Liberal | 0.08 | $-0.01$ | $-0.00$ | |
| | (0.05) | (0.05) | (0.05) | |
| Lag Education | $-0.12^{***}$ | $-0.01$ | $-0.01$ | |
| | (0.04) | (0.03) | (0.02) | |
| Lag Atheneum Memb. | $-0.04$ | $-0.00$ | | |
| | (0.05) | (0.08) | | |
| Lag Business MP | 0.09 | 0.13 | 0.12 | |
| | (0.10) | (0.09) | (0.08) | |
| Lag Reform MP | $-0.10$ | $-0.08$ | $-0.08$ | |
| | (0.07) | (0.07) | (0.07) | |
| Lag Canal MP | $1.18^{\cdot}$ | $1.26^{*}$ | $1.25^{*}$ | |
| | (0.62) | (0.59) | (0.59) | |
| Lag Freetrade MP | $-0.16$ | | | |
| | (0.12) | | | |
| Lag Const. Pop. | $0.00^{**}$ | | | |
| | (0.00) | | | |
| Num. obs. | 195.00 | 195.00 | 195.00 | 195.00 |
| $R^2$ | 0.21 | 0.17 | 0.16 | 0.05 |

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table 4: General Spatial 2SLS HAC, 1846, Logroll Length 2

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| $\rho$ | $0.49^{*}$ | $0.49^{*}$ | $0.47^{*}$ | $0.43^{\cdot}$ |
| | (0.22) | (0.21) | (0.22) | (0.26) |
| Intercept | $0.08^{**}$ | $0.08^{**}$ | $0.08^{**}$ | $0.13^{***}$ |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| Num RW Projected in Constituency | −0.00 | −0.00 | −0.00 | $-0.00^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Num RW Investments | $0.04^{***}$ | $0.04^{***}$ | $0.04^{***}$ | $0.04^{***}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Num RW Overseen | −0.00 | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Liberal Dummy | 0.03 | 0.02 | 0.02 | |
| | (0.03) | (0.03) | (0.03) | |
| Education Dummy | $0.05^{*}$ | $0.05^{*}$ | $0.05^{*}$ | |
| | (0.02) | (0.02) | (0.02) | |
| Athenaeum Memb. Dummy | 0.00 | 0.01 | | |
| | (0.03) | (0.03) | | |
| Business MP Dummy | $0.09^{*}$ | $0.10^{*}$ | $0.10^{*}$ | |
| | (0.04) | (0.04) | (0.04) | |
| Reform MP Dummy | −0.04 | −0.04 | −0.03 | |
| | (0.04) | (0.04) | (0.04) | |
| Canal MP Dummy | 0.01 | 0.01 | 0.01 | |
| | (0.10) | (0.10) | (0.10) | |
| Freetrade Club Dummy | −0.01 | | | |
| | (0.08) | | | |
| Constituency Pop. | −0.00 | | | |
| | (0.00) | | | |
| Lag RW Proj. in Const. | −0.00 | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Num RW Invest. | −0.00 | −0.01 | −0.01 | $-0.02^{*}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Lag Num RW Overseen | 0.01 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Lag Liberal | 0.09 | 0.08 | 0.08 | |
| | (0.10) | (0.10) | (0.09) | |
| Lag Education | −0.06 | −0.05 | −0.05 | |
| | (0.05) | (0.04) | (0.04) | |
| Lag Atheneum Memb. | 0.02 | 0.02 | | |
| | (0.09) | (0.06) | | |
| Lag Business MP | $-0.18^{*}$ | $-0.16^{\cdot}$ | $-0.16^{\cdot}$ | |
| | (0.09) | (0.08) | (0.08) | |
| Lag Reform MP | −0.11 | −0.08 | −0.07 | |
| | (0.13) | (0.13) | (0.11) | |
| Lag Canal MP | −0.21 | −0.16 | −0.18 | |
| | (0.24) | (0.21) | (0.21) | |
| Lag Freetrade MP | 0.26 | | | |
| | (0.27) | | | |
| Lag Const. Pop. | 0.00 | | | |
| | (0.00) | | | |
| Num. obs. | 285.00 | 285.00 | 285.00 | 285.00 |
| $R^2$ | 0.19 | 0.18 | 0.18 | 0.14 |

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table 5: Tests for non-random logroll assignment: exclusion-bias corrected p-values via network randomization inference

| Covariate | Year | $\beta$ | $\bar{\beta}_{null}$ | $\beta_{null}$ Std. Dev. | P-value | Significance |
|---|---|---|---|---|---|---|
| RW Proj. in Const. | 1845 | 0.079 | -0.536 | 0.746 | 0.676 | |
| RW Invest. | 1845 | -0.028 | -0.134 | 0.194 | 0.824 | |
| RW Overseen | 1845 | 0.070 | -0.042 | 0.170 | 0.656 | |
| Liberal | 1845 | -0.114 | -0.048 | 0.240 | 0.648 | |
| Education | 1845 | 0.013 | -0.017 | 0.140 | 0.934 | |
| Athenaeum Memb. | 1845 | -0.250 | -0.114 | 0.294 | 0.676 | |
| Business MP | 1845 | -0.167 | -0.072 | 0.305 | 0.740 | |
| Reform MP | 1845 | | -0.095 | 0.272 | | No Links |
| Canal MP | 1845 | | -0.022 | 0.008 | | No Links |
| Freetrade MP | 1845 | | -0.058 | 0.127 | | No Links |
| Const. Pop. | 1845 | 0.006 | -0.156 | 0.306 | 0.980 | |
| RW Proj. in Const. | 1846 | 0.079 | -0.089 | 0.346 | 0.774 | |
| RW Invest. | 1846 | 0.061 | -0.056 | 0.237 | 0.810 | |
| RW Overseen | 1846 | -0.017 | -0.005 | 0.055 | 0.754 | |
| Liberal | 1846 | 0.099 | -0.010 | 0.141 | 0.478 | |
| Education | 1846 | -0.001 | -0.010 | 0.084 | 0.994 | |
| Atheneum Memb. | 1846 | -0.032 | -0.046 | 0.219 | 0.874 | |
| Business MP | 1846 | -0.016 | -0.061 | 0.220 | 0.944 | |
| Reform MP | 1846 | 0.083 | -0.047 | 0.220 | 0.670 | |
| Canal MP | 1846 | -0.019 | -0.027 | 0.192 | 0.770 | |
| Freetrade MP | 1846 | -0.041 | -0.056 | 0.128 | 0.796 | |
| Const. Pop. | 1846 | 0.042 | -0.043 | 0.204 | 0.818 | |

Table 6: Means and P-Values for MPs in and out of Committees

| | Mean.No.Comm. | Mean.Comm. | P.Value |
|---|---|---|---|
| Num. Inv. | 0.546 | 0.709 | 0.293 |
| Num. Proj. | 18.487 | 12.748 | 0.203 |
| Liberal Score | 0.445 | 0.431 | 0.733 |
| Educ. Lev. | 0.543 | 0.696 | 0.0001 |
| Athenaeum Memb. | 0.116 | 0.147 | 0.254 |
| Active in Business | 0.221 | 0.137 | 0.006 |
| Reformer | 0.149 | 0.134 | 0.580 |
| Interests in Canals | 0.015 | 0.013 | 0.842 |
| Free Trade Advocate | 0.009 | 0.020 | 0.261 |
| Constituency Population | 76,511.170 | 74,696.100 | 0.882 |

Table 7: Summary Statistics: Tobin's Q and Company Information

| Statistic | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Log Tobin's Q | 1,531 | 0.315 | 0.208 | −0.386 | 0.731 |
| Logroll Dummy | 1,531 | 0.343 | 0.475 | 0 | 1 |
| Liquidity (Log 1 + % quotes) | 1,531 | 0.654 | 0.066 | 0.429 | 0.693 |
| Liquidity (Log 1 + size) | 1,531 | 3.597 | 0.355 | 3.045 | 3.932 |
| Log Total Vol. | 1,531 | 2.698 | 1.196 | 0.236 | 4.698 |
| Log Monthly Vol. | 1,531 | 0.446 | 0.644 | 0.000 | 3.110 |
| Log 1 + MP count | 1,531 | 0.709 | 0.800 | 0.000 | 2.485 |

Table 8: Regression of Tobin's-Q on firm logroll status

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Log Tobin's Q | | | |
| | Within | RE | Pooling | Pooling |
| | (1) | (2) | (3) | (4) |
| LR Dummy | | −0.097** | −0.115*** | −0.085*** |
| | | (0.048) | (0.011) | (0.009) |
| Liquidity (% quotes) | | 0.532*** | 0.560*** | |
| | | (0.202) | (0.040) | |
| Liquidity (size) | | −0.011 | −0.013 | |
| | | (0.050) | (0.013) | |
| Total Vol. | | −0.051*** | −0.048*** | |
| | | (0.015) | (0.003) | |
| Monthly Vol. | −0.008*** | −0.009*** | −0.013** | |
| | (0.003) | (0.003) | (0.006) | |
| LR Dummy * Week | −0.012*** | | | |
| | (0.001) | | | |
| MP count | | −0.041 | −0.054*** | |
| | | (0.026) | (0.005) | |
| Constant | | 0.389* | 0.389*** | 0.515*** |
| | | (0.210) | (0.058) | (0.022) |
| Time FE | Yes | Yes | Yes | Yes |
| Company FE | Yes | No | No | No |
| Company RE | No | Yes | No | No |
| $R^2$ | 0.89 | 0.56 | 0.57 | 0.48 |
| N Companies | 32 | 32 | 32 | 33 |
| Observations | 1,531 | 1,531 | 1,531 | 1,547 |

*Note*: *p< 0.1; **p< 0.05; ***p< 0.01. Heteroskedasticity Robust SEs clustered at the company level. Hausman test of (2) vs (3) suggest RE are inconsistent: p-value < .001. All variables in logs except the LR Dummy.

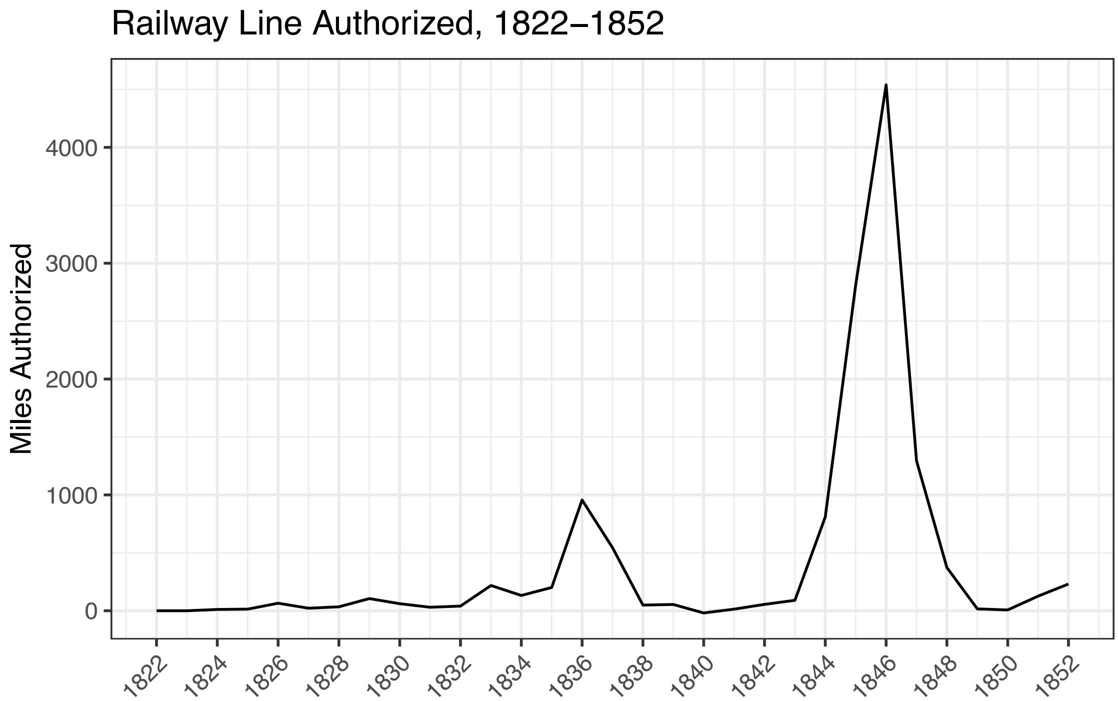


Figure 1: Annual Mileage of New Railway Lines Authorized by Parliament. Data from FRED, series A0284DGBA374NNBR.

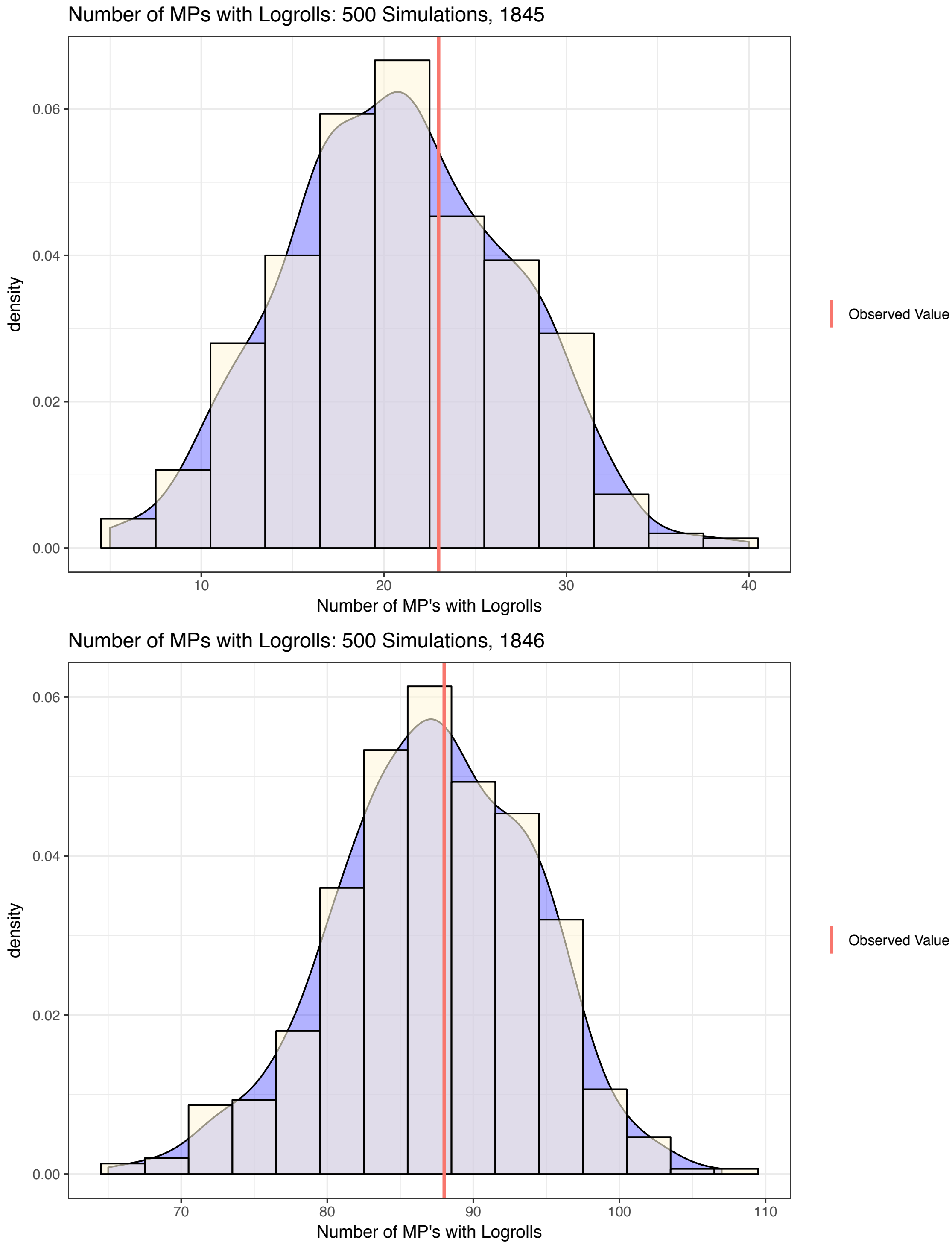


Figure 2: Simulation of Distribution of Number of MPs with Logrolling Opportunities if MPs are Randomly Allocated to Committees

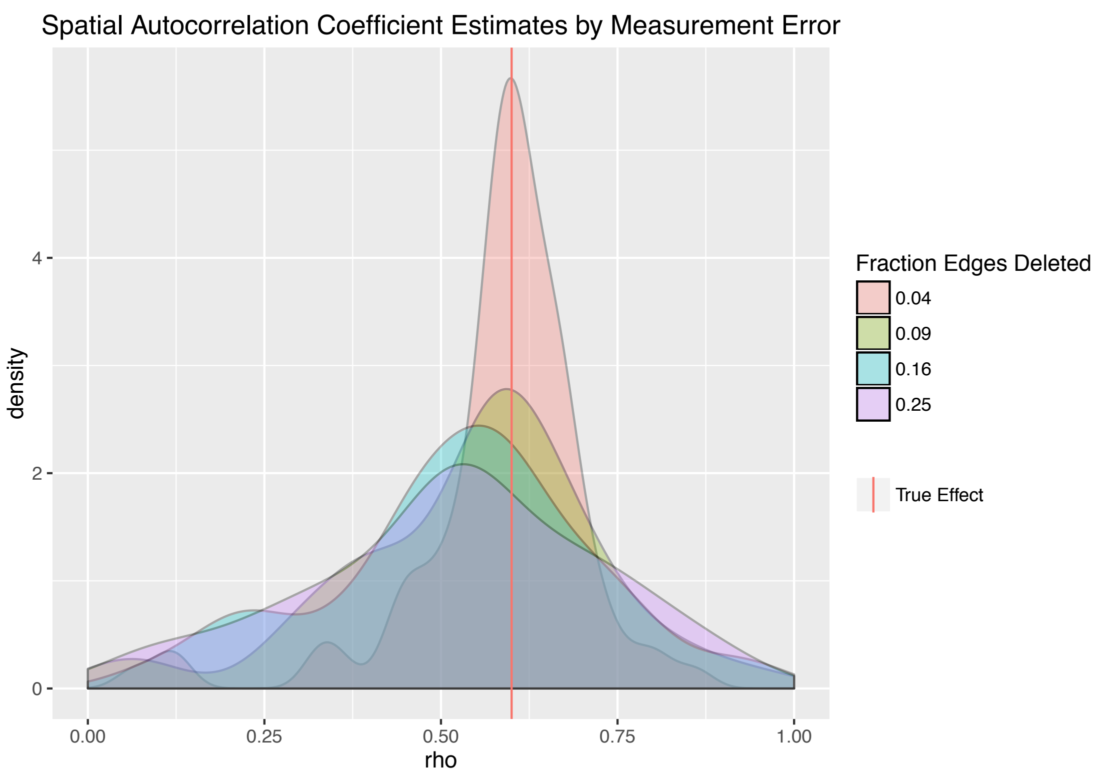


Figure 3: Distribution of $\hat{\rho}_{bs}$ at different rates of unobserved links.

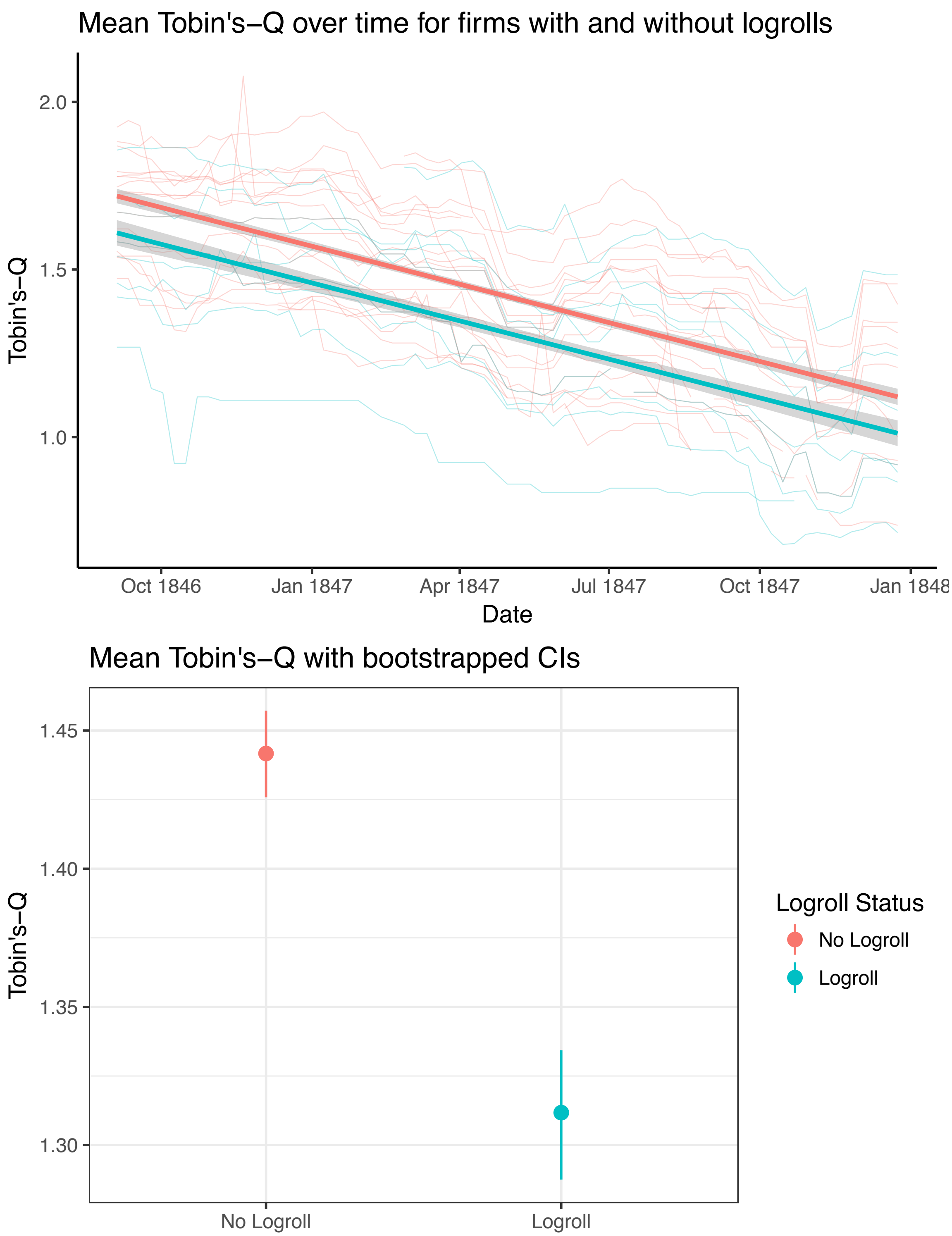


Figure 4: Tobin's-Q by logrolling status: railway companies approved in 1845 and 1846

# Online Data Appendix: Not for Publication

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## 1 MP Investment in Railways

We collected data on how much MPs had invested in railways and which railways they had invested in by drawing on two parliamentary papers: *1845 (317) Railways*, and *1846 (473) Railways*. The papers listed all subscribers to railways for sums upwards of £2,000. Typically, only 5% to 10% of the value of a share had be paid in upfront, with no more than 10% of the value of the share allowed to be collected, thus a £2,000 cutoff implies investments of approximately £100 to £200.

The Parliamentary Papers that reported the investments also listed the profession of the investor, and it is on the basis of MP's identifying themselves as such that we can identify MP investors. All the MPs are then cross-checked with the list of MPs seating in Parliament in 1845 and 1846.

## 2 Data on Railways Crossing MP Constituencies

Our encoding of a 'geographical' interest by MPs in railways required multiple steps: determining the intended routes, geocoding the route information, establishing the boundaries of historical political constituencies, and, finally, checking if a railway crossed an MP constituency.

Since the majority of the railways included in our sample never built any track, ascertaining their proposed route was not trivial. To do so, we drew on *Tuck's Railway Shareholder's Manual*, editions 6 through 8, which covered the years 1845 through 1847. The manual listed towns through which the railway intended to pass in detail. Figure 1 shows an example of the way in which the source described the route of a railway projected to connect two towns in Devonshire.

Figure 1: Excerpt from *Tuck's Railway Shareholder's Manual*, 7th Edition

| | |
|---|---|
| **69. Bideford and Tavistock Railway.—To commence at the port of Bideford, passing through Torrington, Hatherleigh, and Okehampton, terminating in junction with the South Devon Railway at Tavistock. Length, 42 miles Capital ......................................** | **350,000** |
| **14,000 *Shares of* 25*l. each.*** | |

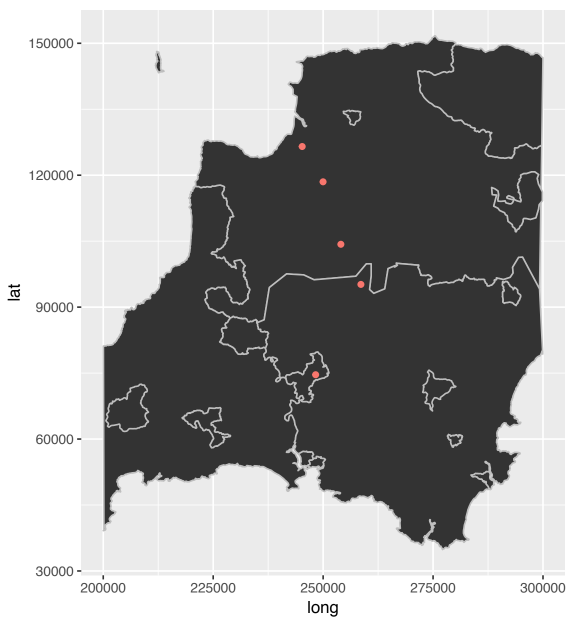
We geocoded the location of all the towns a given railway was intending to pass through using google maps, which yielded a vector of latitude-longitude coordinates associated with each railway line. We then plotted the historic constituency boundaries, using the shapefiles created by the Great Britain Historical GIS Project and the University of Plymouth, and hosted at `www.VisionofBritain.org.uk`. We could then overlay the vector of points that defined a railway's route on the shapefiles of historical constituency boundaries and determine which constituencies were crossed by a railway. Figure 2 shows the map corresponding to the geocoded place names we collected from Figure 1 projected on the historical constituency boundaries.

Since projected railways were typically short, the coding rarely required any judgment calls about which constituencies intersected which railways. Furthermore, we coded which railways crossed which constituencies directly from the stopping points listed in *Tuck's Railway Shareholder's Manual*, rather than by interpolating lines between the points. Our reasoning was that whereas the points were definite, any interpolation could result in the introduction of a link between an MP and a railway when in fact no such link existed. On the basis of our analysis of the behavior of the estimator under measurement error, we are confident that a failure to record links results in attenuation bias, but the direction of bias as a result of recording links when in fact they did not exist is unclear. Consequently, we chose to default to the more conservative encoding that relied on points.

## 3 MP Oversight of Railways

We determined which MPs oversaw which railways by drawing on Parliamentary Papers that published the allocation of MPs to groups and railways to groups. For 1845 this was easily recorded using Parliamentary Paper *1845 (620).* For 1846 this was done by consulting the report on the Sittings of the House for Private bills and Acts, which were published in paper *1846*

Figure 2: Bideford and Tavistock Route Mapped Across Historic Constituency Boundaries



*(723-II)*.

## 4 MP Covariates and Constituency Covariates

*Dependent variable: % Railways Approved*: The primary dependent variable of interest in all spatial specifications is the fraction of railway projects that were approved. The numerator in the fraction is the number of railway companies that an MP had an interest that are approved by parliament. Interests are computed from geographical and investment matrices described in the main text. We determined whether or not a railway was approved by collecting data on committee outcomes published in *Parliamentary Papers 1845 (637)* and *1847 (708)*. The denominator in the fraction is simply the number of railways in which an MP has a vested interest as defined by our interest matrices.

*Number of Railways Projected in Constituency*: This covariate is a count of the number of railways that are projected to cross an MP's constituency. It is computed as the row-sums of the matrix that describes geographical links between MPs and railways.

*Number of Railway Investments*: This covariate is a count of the number of railways an MP has invested in. It is computed as the row-sums of the matrix that defines MP interests in railways stemming from their investments.

*Number of Railways Overseen*: This covariate records a count of the number of railways that each MP oversaw. It is computed as the row-sums of the MP oversight matrix.

*Liberal Dummy*: This covariate is Aydelotte's two-way party breakdown of MPs (variable 173), that classifies them as either conservative or liberal, with the variable taking on a 1 if the MP is classified as liberal.

*Education Dummy*: This is a dummy variable constructed by Aydelotte (variable 17), that takes on a 1 if the MP was university educated.

*Athenaeum Member Dummy*: This is a dummy variable recorded by Aydelotte (variable 4), that takes on a 1 if the MP is a member of the Athenaeum Club. It is included under the hypothesis that clubs could have functioned as locations in which to broker trades. We selected the Athenaeum (as opposed to other clubs) as it ostensibly selected MPs along dimensions other than party affiliation.

*Business MP Dummy*: This is a dummy constructed by Aydelotte (variable 118) and that summarizes the data he collected on MP business interests. The measure discretizes MPs into those who were active in business of any sort and those who were not, with active business MPs coded as a 1.

*Reform MP Dummy*: This is a dummy variable computed from Aydelotte's five-way party breakdown (variable 174). It was included as reforming MPs might have had a different willingness to participate in logrolling. In addition, the reform block constituted a somewhat distinct dimension of British political affiliation that is not well captured by the Liberal Dummy alone.

*Canal MP Dummy*: This is dummy variable number 140 in Aydelotte's dataset, which records whether or not an MP had an active business interest in the canal sector. Canal's were reputed to be opposed to the encroachment of railway companies.

*Freetrade Club Dummy*: This is dummy variable number 9 in Aydelotte's dataset and measures whether or not an MP was a member of the Freetrade Club. We included this to capture a political dimension that was not as well captured by the political controls, and because the free-trade lobby constituted an important interest group in this period.

*Constituency Population*: Total constituency population as measured by Aydelotte's variable number 403.

## 5 Railway Company Share Prices and Covariates

*Tobin's Q*: Tobin's Q was computed using data from the *Railway Monitor*, which was a supplement published by the *Economist* that compiled share prices of railway companies across the London, Liverpool, Manchester and Leeds exchanges. Since the railway mania burst during our estimation period, the vast majority of companies refrained from calling up the remainder of the par value of the shares until after 1847. Leverage was equally small, as companies had trouble selling debt during this period. Consequently, we calculated a simplified version of Tobin's Q (ignoring debt) as the ratio between share prices and their par values. However, as listed shares were only partly paid, they effectively traded as derivative-like assets (Campbell 2013). For this reason, we compute the shadow price of a fully-paid share ($S$) from the observed prices of partly-paid shares ($P$):

$$S = P + Ke^{-r+q}t$$

where $K$ is the amount of future capital calls, $r$ is the risk-free rate and $q$ the dividend rate on the share. Because companies did not publish schedules for calling up their capital, we need to make assumptions about the timing of the calls $K$. Similarly to Campbell (2013), we assume equally distributed calls over a period of 5 years. We used the Bank of England discount rate as a proxy for $r$ and assumed an expected 5% dividend rate. Realised dividends varied a lot across companies, but the overall average dividend/par ratio in railways from 1845 to 1847 was very close to 5% and so we took it as a proxy for the reasonable expectation of railway dividends in the period (**campbellturner2012**).[1]

*Logroll Dummy Variable*: Our primary independent variable of interest was a dummy variable that encoded whether or not a railway could have been the product of logrolling. This was derived by transposing the interest and oversight matrices, and computing a new logrolling matrix defined at the company-by-company level. Let $O$ be the $P \times C$ *oversight* matrix and $V$ the $P \times C$ matrix of *vested interests* defined in the main text. We identified which railways had logrolling opportunities by listing the cycles in the following $C \times C$ matrix:

$$M' = O^T \times V$$

*Liquidity (% quotes)*: The first measure of liquidity we use is the percentage of days on which a given railway share was quoted in the *Railway Monitor* share-price tables in London, Liverpool, Manchester or Leeds.

*Liquidity (size)*: The second measure of liquidity that we use the size of the company, measured by the its total nominal capitalisation (par value $\times$ number of authorised shares). The data was collected from the *Railway Monitor*.

*Total Volatility*: was computed as the variance of each share's prices over the entire sample period.

*Monthly Volatility*: This is a time-varying measure of share-price volatility computed from the price data we collected from the *Railway Monitor*. Monthly volatility is computed by taking the variance of each share's prices measured weekly.

*MP count*: This covariate controls for the level of direct political involvement in a company as measured by the number of MPs who had invested in a given company. The number of MP investors is taken from the *Parliamentary Papers* detailing the size and allocation of MP investments, which we used to calculate the MP investment data (see above).

[1] Experimenting with different dividend rates cannot affect the estimation results since it is equivalent to a level re-scaling of the left-hand side variable. Moreover, OLS estimates in a linear model remain unbiased under general assumptions, even if the left-hand side is measured with error. Efficiency, however, is lower (Hausman 2001).

## References

Campbell, Gareth (2013). "Deriving the railway mania". In: *Financial History Review* 20.1: pp. 1–27.

Hausman, Jerry (2001). "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left". In: *Journal of Economic Perspectives* 15.4: pp. 57–67.