Online Appendix

“Investment Banks as Corporate Monitors in the Early 20th Century”

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This appendix presents theoretical model discussed in the paper. It also includes a discussion of the definitions, sources, and methodologies used in constructing the variables of the paper; additional detail regarding the analysis presented, along with some supplemental results; and information about the historical context.

Contents

1 Theoretical Model of Banker-Directors as Corporate Monitors

Section 3 of the paper discusses the implications of restricting relationships with underwriters for firm outcomes when banker-directors act as corporate monitors. Here, we present the theoretical model and its predictions formally. Underwriters with board seats act as delegated monitors, in the spirit of Diamond (1984). We take as given the need to issue bonds to finance new investments, and analyze the consequences of imposing Section 10 for railroads.

1.1 Setup

The model has two periods. There is a continuum of firms f, which differ at time 0 in the probability $\lambda_f \sim F(\lambda)$ of having access to an investment opportunity at time 1. The probability λ_f can be thought as the firm's growth opportunities or its size.[1]

An investment opportunity requires an outlay of 1 unit of capital in period 1, and yields a stochastic payoff in period 2. Projects vary ex-ante in their quality p: with probability p the project is successful and its cash flows are worth V_H, with probability $1-p$ the project is only worth $V_L < V_H$. For simplicity, we assume that p is observable and verifiable by all parties, and that p is distributed $U[0,1]$ and independent across firms. The realization of V_H or V_L is observed only by the firm's insiders, who will therefore be tempted to report V_L to outside investors regardless of the true realization, and keep any additional cash flows for themselves.

If a project arrives in period 1, we assume that it must be financed by the issuance of debt, and the firm needs a banker to underwrite these securities. The underwriter sells the debt to risk-neutral investors, who have a required expected rate of return equal to $R \in (V_L, V_H)$.

At time 0, prior to the arrival of an investment opportunity, firms decide the type of underwriter that will market its securities. Specifically, they choose whether or not to offer a board seat to an underwriter. If an underwriter has a seat on the firm's board, they can monitor the firm and verify the true value of the project's payoff, V_L or V_H. If there is no banker on the firm's board, then the firm must use an arms-length underwriter who cannot monitor the firm. For simplicity, we assume that the underwriters receive no fee for marketing securities, but that they are compensated for providing monitoring services in the form of a fee of M.[2]

We denote F the face value of the debt issued by the firm, which is sold for a price of 1. At time 2, bondholders can choose to liquidate the project, in which case they only recover an amount $L < V_L$, and the insiders receive a payoff of 0.

With an arm's-length underwriter, outside investors anticipate that insiders have an incentive to lie about the payoff of the project. To guarantee truth-telling by the insiders, investors will always

[1] Although for simplicity our framework is static, in a dynamic setup firms with higher λ_f would acquire more projects over time, and therefore be larger in equilibrium.

[2] Effectively, we assume a zero marginal cost of exerting monitoring effort if there is a banker on board, and infinite otherwise. Our model can be extended to allow for the monitoring fee to depend on the arrival of an investment opportunity. As long as there is a fixed component to this fee—that is, the monitoring fee is not purely proportional to λ_f—larger firms will choose to have a monitor. Note that if the fee was exactly proportional to the probability of receiving a project, there would be no selection of firms into monitoring relationships based on their ex-ante growth opportunities: either all firms would choose to have bankers on boards, or no one would. Our assumption delivers a selection pattern of firms into underwriting relationships that is consistent with what we observe in the data.

liquidate the firm if the insiders report that the payoff is V_L.[3] Since investors will therefore receive L in the low state and require an expected rate of return R to invest in the firm, the payment in the good state (and therefore the maximum amount that the firm can borrow) is given by:

$$F_N(p) = \frac{R - (1-p)\,L}{p} \tag{1}$$

Since the firm's insiders will only take on an investment opportunity with positive expected payoffs, investment will take place as long as $p\,(V_H - F_N) \geq 0$, or

$$p \geq p^* \equiv \frac{R - L}{V_H - L} \tag{2}$$

If instead the firm uses an investment bank with a board seat to underwrite their debt, the bank will learn the true value of the project's payoff and report it to the bondholders, who no longer have incentives to liquidate the firm when V_L is reported. As a result, the amount that the firm must promise to repay outside investors in the good state becomes:

$$F_M(p) = \frac{R - (1-p)\,V_L}{p} \tag{3}$$

Thus, investment will occur for a project p as long as

$$p \geq \underline{p} \equiv \frac{R - V_L}{V_H - V_L} \tag{4}$$

A comparison of (2) with (4) reveals that $p^* > \underline{p}$. Thus, the lack of monitoring leads to under-investment: positive NPV projects $p \in [\underline{p}, p^*)$ cannot be financed in the case of arm's-length underwriting. This distortion occurs because the costs of raising external funds under non-monitoring are higher, as investors need to be promised a higher amount relative to the efficient case $(F_N(p) > F_M(p))$.

Having a banker on the board who can potentially act as a monitor is costly, and results in a loss of value to the firm's insiders equal to M. By incurring this cost at $t = 0$, the firm has the option of using this banker to underwrite its securities if it needs external funds. Even though we refer to M as monitoring costs, these costs could include not only the direct fees paid to the underwriter-monitor but also other indirect costs associated with having bankers on boards, likely including rents that bankers may extract from the firm, as the Progressives argued during the Pujo Committee hearings.

Firms will choose at $t = 0$ to have a banker on the board if the potential benefits from doing so outweigh its costs. The total value of the equity when there is no underwriter-monitor on the board equals the expected net present value of new investments that are implemented, which is given by:

$$S_N \equiv \lambda_f \int_{p^*}^{1} p\,(V_H - F_N)\;dp = \frac{1}{2}\,\lambda_f\,\frac{V_H - V_L}{c_0} \tag{5}$$

[3] We assume that investors can commit to this liquidation strategy ex-ante.

where $c_0 = (V_H - V_L)\,(V_H - L)/(V_H - R)^2 \geq 0$.

If the firm chooses instead to have a potential underwriter on the board of directors, it can invest more efficiently. In this case, the net present value of the firm's new investments, ignoring monitoring costs, at $t = 0$ is equal to:

$$S_M \equiv \lambda_f \int_{\underline{p}}^{1} p\,(V_H - F_M)\;dp = \frac{1}{2}\,\lambda_f\,\frac{V_H - L}{c_0} \tag{6}$$

The firm will choose to have a banker on the board if the gains from doing so are greater than the costs, $S_M - S_N \geq M$, or:

$$\lambda_f \geq \lambda^* \equiv \frac{2\,M\,c_0}{V_L - L} \tag{7}$$

Examining equation (7), we see that the benefits from monitoring are increasing in the firm's availability of investment opportunities λ_f. Firms that are more likely to invest (and that therefore will need to raise external funds), are also more likely to establish a relationship with an underwriter because avoiding inefficient liquidation is more valuable to them. Using our loose interpretation of λ_f as firm size, we see that larger firms (i.e., firms with $\lambda_f \geq \lambda^*$) will choose to have bankers on their boards to underwrite their securities.

1.2 Predicted Consequences of the Clayton Act

We use this simple framework to obtain predictions of the effect of Section 10 of the Clayton Act for railroads. The imposition of the rule should have affected firms that selected into monitoring relationships with an underwriter—that is, firms with $\lambda_f > \lambda^*$—and should have had no effect on firms that optimally chose arm's-length underwriting. We compare the market values, investment levels, debt outstanding, and borrowing costs for a firm that was forced by Section 10 to switch to arm's-length underwriting, at time 0.

1. *Investment and New Borrowing.* Following the imposition of Section 10, firms that would have selected into a relationship with an underwriter on their board will, on average, have lower levels of borrowing and investments.

$$\Delta I = \lambda_f \left(\int_{p^*}^{1} 1\;dp - \int_{\underline{p}}^{1} 1\;dp \right) = -\lambda_f\;\frac{1}{c_0}\,\frac{V_L - L}{V_H - R} < 0, \tag{8}$$

   Projects with $p \in (\underline{p}, p^*)$ will not be implemented when monitoring is not allowed. Since all new investment is financed externally, (8) also describes the decline in borrowing experienced by affected firms.

2. *Market values.* Firms that would have selected into a relationship with an underwriter on their board will experience a decrease in market valuations:

$$\Delta S = S_N - (S_M - M) = -M \left(\frac{\lambda_f}{\lambda^*} - 1 \right) < 0. \tag{9}$$

   The fall in value follows immediately from the fact that firms with $\lambda_f \geq \lambda^*$ will be forced to invest less efficiently than they would choose to in the absence the regulation.

3. *Borrowing costs.* Firms that would have selected into a relationship with an underwriter-monitor will experience a higher marginal cost of borrowing, or[4]

$$\Delta\hat{R} = \frac{\int_{p^*}^{1}(F_N(p)-1)dp}{\int_{p^*}^{1} 1\ dp} - \frac{\int_{\underline{p}}^{1}(F_M(p)-1)dp}{\int_{\underline{p}}^{1} 1\ dp} \approx \frac{1}{2}\,\frac{V_L - L}{c_0} > 0, \tag{10}$$

where the total amount of interest paid $\hat{R} = F - 1$ is the difference between the the face value of the debt issued by the firm and the amount it borrowed.[5]

Note that in our model, the book value of assets at $t = 0$ is independent of the type of underwriter. Thus, these predicted changes in the level of investment, new borrowing, and market values are also applicable to investment rates, leverage ratios, and Tobin's Q. This simple framework therefore predicts that the railroads with a stronger preexisting association with their underwriters through their boards would experience a decline in market values, investment rates and leverage, and an increase in their borrowing costs as a consequence of the implementation of Section 10.

1.3 Discussion of the Model's Assumptions and Implications

The friction at the heart of the model, that only insiders can observe the true state of the firm's cash flows, seems consistent with the history of the railroad industry, which is replete with examples of the condition of firms being "sedulously hidden" from investors as insiders engaged in "plundering" (Campbell, 1938: 92). That bankers with board seats could get access to this hidden information, and that they often restricted management from engaging in value-destroying behavior, is also well supported by the history.[6] Consistent with the view of underwriters as corporate monitors, anecdotal evidence suggests that bankers required the access to private information as a condition for establishing some relationships. For example, J.P. Morgan & Company, Deutsche Bank and the Northern Pacific signed an agreement on January 7, 1913 for the reorganization of the railroad that specified that "the new company will give to the bankers, upon request of either of them, full information regarding the accounts, operations and conditions of the company and its property" (Pujo, 1913a: 1342).

In our simple framework, we have assumed that underwriters with board seats can commit ex-ante to monitor and reveal the true state of firms to investors. We make this assumption for simplicity; in its absence, there will be no underwriter-monitors in equilibrium in our static model.[7]

[4]The approximation below uses uses the relation $\log(1+x) \approx x - \frac{1}{2}x^2$.

[5]The marginal interest rate paid by firms when the Clayton Act restriction is enforced is affected by two forces. First, for a given project of quality p, firms will pay a higher interest rate when the underwriter is not allowed to monitor. This direct effect will increase interest expenses for the affected firms. However, there is also an indirect effect due to project selection: under the Act's restriction, firms will pass up on marginal investment opportunities (that is, projects with $p \in (\underline{p}, p^*)$). Since these investments are ex-ante riskier, this second force will tend to reduce the interest expense paid by firms. In our setup, the first effect dominates.

[6]For example, when J.P. Morgan took a seat on the board of the Northern Pacific Railroad in 1883, he found that the expenses for construction and equipment had vastly exceeded the estimated costs, and that Villard, the President of the company, had liberally spent funds on various other projects. The railroad quickly fell into financial trouble, and Morgan restored the credit and earnings of the firm by reorganizing its financing, appointing a strong committee to oversee the activities of the firm, and encouraging Villard to resign (Strouse, 1999). Other such examples are presented in Campbell (1938).

[7]If monitoring entails some costs to banks, they will never choose to monitor ex-post in the absence of commitment. Investors will therefore anticipate the lack of monitoring, and would price the firm's debt equally regardless of the type of underwriter. Firms would therefore choose not to pay the cost M required to hire a monitor.

(In multi-period settings, incentives to monitor can be sustained by the fear of loss of reputation; see Boot (2000) for a survey.) An alternative interpretation of our framework that does not require assuming truthful revelation of information by bankers is that informed underwriters can smooth the bankruptcy process and avoid costly liquidation. Indeed, the theoretical formulation of the case in which underwriters on the board can help creditors recover the full value of the assets V_L in the event of default would be isomorphic to the monitoring framework we present above. While we pose this as a possibility, it is difficult to ascertain whether this interpretation is entirely consistent with the historical record. On one hand, management initiated receiverships became more common in the late nineteenth century, which reduced the rights of creditors during the legal process (Dewing, 1918; Berk, 1994). Perhaps in response to this change, representatives of the creditors–primarily bankers–became more likely to be appointed as receivers (Cleveland and Powell, 1920: 237-38). Anecdotal evidence also suggests that influential bankers were sometimes able to design reorganization plans that satisfied the conflicting interests of the very diverse pool of railroads' stakeholders, thereby achieving a more prompt restructuring of the firms' liabilities.[8] But railroad receiverships were certainly complex, in part due to the intricate capital structures of the large railroads systems. It is possible that bankers took advantage of their privileged positions and exploited the frictions that arose during the process of reorganization for their own benefit.[9]

2 Data: Sources, Definitions, Methods, and Additional Details

2.1 Bond Underwriting Data

Data on bond underwriting was collected from various editions of the *Fitch Bond Book*, beginning with the first volume in 1913, and from editions of the *Moody's Manuals*, which began reporting underwriting information in the 1920s. All outstanding issues of bonds of the sample companies were recorded. That is, we use the outstanding stock of bonds, along with information about the dates of their issuance, to reconstruct the annual flows. This procedure was then repeated using subsequent editions of the data sources at five- to eight-year intervals.

For each bond, the names of the original underwriters and the total amounts underwritten were recorded. Railroads issued more bonds, and their securities had a more complex structure than firms in other industries. Many railroads issued multiple offerings of the same bond over time; in this case, each offering was recorded as a separate bond issuance. Only bonds issued in the name of the sample firm, rather than in the name of a different firm that was later acquired by the sample firm, were included. Using this approach, the sample railroads issued 638 bond offerings totaling approximately \$7.2 billion, the industrial firms issued 141 bond offerings for a total value of \$1.5 billion, and the utilities issued 152 bond offerings totalling \$2.15 billion up to 1929.

Fitch lists the institutions that led the selling syndicates for bond offerings in order of their importance. On average, the number underwriters named for each bond offering was two, but as few as one and as many as eleven were listed for different offerings. These underwriting institutions

[8] In the 1895 reorganization of the Philadelphia and Reading, for example, the intense conflicts of the interested parties led to the failure of the first two plans proposed by the reorganization committee. It was not until J.P. Morgan & Co. was put in charge of this process that an agreeable plan came to fruition. Cleveland and Powell (1920: 253) attribute the advantage of banking houses in leading railroad reorganizations to their ability to learn the "actual state of affairs" of the firms, suggesting that the importance of bankers in the reorganization process may also partly stem from their ability to monitor firms.

[9] For example, critics accused Morgan of forcing railroads to issue too much equity during railroad reorganizations (Ripley, 1915: 407).

included private investment bank partnerships, commercial banks, trust companies, and the securities affiliates of commercial banks and trust companies. (For simplicity, we refer to all of them as "investment banks," broadly defined.) In cases where multiple branches of the same partnership firm appeared in the underwriting data, they were all treated as the same firm. For example, the underwriting of Philadelphia's Drexel & Co., which was a branch of New York-based J.P. Morgan & Co., was ascribed to J.P. Morgan & Co. in the data. Similarly, the commercial banks and trust companies and their securities affiliates were treated as the same firm; underwriting by National City Bank and National City Company was treated as if it was all done by the same firm, as was the underwriting done by Guaranty Trust Company and Guaranty Company. Using this classification system a total of 245 different institutions appeared at least once as an underwriter.

Since the underwriting information is based on the stock of bonds outstanding in a given year, collected at discrete intervals of time, a potential concern is that the our data could miss short-term securities that were issued and then matured between dates when the stock of outstanding debt was recorded. This could potentially bias our data towards underwriters of securities with long maturities. However, this bias does not appear to be severe because our sample firms primarily issued bonds of very long maturities. For example, the average maturity of the 638 railroad bond offerings, weighted by maturity, was 37.49 years. Moreover, about 92 percent of railroad bonds (and 70 percent of industrial bonds) issued in 1911 and 1912, collected immediately thereafter in the 1913 volume, had a maturity of at least 10 years. Similarly 94 percent of the railroad bonds and 74 percent of the industrial bonds issued in 1928 and 1929, collected from the 1930 *Moody's Manual*, had a maturity of at least 10 years.

The data sources do not provide information on the specific amounts subscribed by each underwriter for a given offering. When n underwriters are listed for a given bond, we ascribe $\frac{1}{n}$ of the total value of the offering to each bank. This assumption will likely lead us to underestimate the concentration of underwriting services since lead underwriters often have a higher participation than other members of the syndicate. Despite this assumption, we find that the underwriting of securities was extremely concentrated during our sample period. Among the 245 different institutions that were present in the underwriting data, the top 25 accounted for 87.1 percent of total underwriting, by value. Appendix Table A1 presents a list of the top 25 underwriting institutions over the entire sample period by their total underwriting volumes.

For the analysis of firms' relationships with their underwriters, and of the frequency with which they used particular underwriters, we sometimes focus on the lead underwriter for each offering. The lead underwriter is defined as the institution listed first by Fitch or Moody's. The construction of the data on underwriting by financial institutions represented on the boards of railroads, industrial and public utilities, and the construction of variables measuring the extent of such underwriting, are discussed below.

Table A1:
The Top 25 Underwriters

Institution	Underwriting volume (Millions of Dollars)
Kuhn, Loeb & Co	2,880
J P Morgan & Co	1,800
National City Bank	991
First National Bank	709
Guaranty Trust Co	494
Speyer & Co	475
Lee, Higginson & Co	306
Harris, Forbes & Co	284
Bankers Trust Co	237
Kidder, Peabody & Co	219
Dillon, Read & Co	154
J & W Seligman & Co	136
Blair & Co	124
Kissel, Kinnicutt & Co	122
Halsey, Stuart & Co	105
White, Weld & Co	103
Hallgarten & Co	100
Wm A Read & Co	91
Brown Brothers & Co	89
Ladenburg, Thalmann & Co	89
Union Trust Co of Pittsburgh	85
Chase Securities Co	79
Goldman, Sachs & Co	64
Hayden, Stone & Co	64
Harris Trust & Savings	57

Note: The figures in the table present the total volume of bond underwriting done for sample railroads, industrials and utilities up to 1929. Debt issues ascribed to JP Morgan & Co include those underwritten by Drexel & Co; those of National City Bank include those of National City Co; First National Bank's include First National Co's; and Guaranty Trust's include those of Guaranty Co. For sources and methods, see Appendix text.

2.2 Accounting Data

All accounting data were collected from various editions of the *Moody's Manuals of Railroads and Corporation Securities*, the *Moody's Manuals of Investments of Industrial Securities*, and the *Moody's Manuals of Investments of Public Utility Securities.* The financial statements from which the data were recorded are quite varied in their content and level of detail. It should be kept in mind that although the NYSE did require listed firms to report annual financial statements, the content of the statements they produced was not regulated or supervised in the way that those of modern firms are. Many NYSE-listed industrials, for example, chose not to disclose their gross revenues and/or costs of sales, perhaps to avoid revealing strategically valuable information to competitors. In other cases, firms refused to report other items such as interest expenses for particular years. In sum, our newly constructed dataset is based on the very best available accounting data for the period, and we have made every effort to calculate accounting ratios in a consistent way across all sample firms. But our data are not as complete or as accurate as it would be with modern public companies.

The definitions of the variables utilized in the empirical analysis, along with some discussion of how the underlying data was coded, are presented below:

Book Leverage: Long-term debt/total assets. Long-term debt consisted principally of bonds, but also included other long-term interest-bearing obligations such as mortgages. For most railroads long-term debt also included ‘equipment trusts,’ which were bonds collateralized by railroads’ rolling stock.

Average Interest Rate: Total interest expense/interest-bearing liabilities. Interest-bearing liabilities include bonds, equipment trusts, mortgages and any other long-term interest-bearing obligations, as well as notes payable. For a handful of sample years, railroads consistently reported the total amount of ‘fixed charges,’ rather than interest expense on their income statements, which may have included items such as lease payments as well as interest. Some industrials and utilities also reported interest expenses in this way in some years. Interest expense is available for fewer firm-years for the industrials than for the railroads, because a substantial number of industrials had no long-term debt for many years of the sample. In addition, this variable was sometimes not disclosed by industrials that had outstanding debt, or if it was disclosed, it was sometimes lumped together with other expense items. The interest costs disclosed were usually labeled as “interest, discount, etc.” on railroads’ financial statements. As the “discount” is the difference between the price that the company received for its debt and its face value, the interest rate measure should include the underwriters’ spread.[10]

Return on Equity (ROE): Earnings before interest and taxes/common shareholders’ equity. Earnings before interest and taxes is defined as operating revenues minus operating expenses. Common shareholders’ equity includes the book value of the common shares as well as the firm’s surplus (retained earnings).

Return on Assets (ROA): Earnings before interest and taxes/total assets. Earnings before interest and taxes is defined as operating revenues minus operating expenses.

Investment rate: Change in the property and equipment account/previous year’s value of the property and equipment account. Accounting statements from the sample period generally did not disclose capital expenditures. We infer investment as the change in the property account for railroads. Railroads treated improvements in their track and equipment as cash expenses and did not account for depreciation. We therefore do not account for depreciation in our calculation of the change in their property account. In contrast, many industrial firms and utilities depreciated their property and equipment; when information on depreciation is available, we add it to the change in property account to estimate capital expenditures. The investment variable is not utilized for years when individual firms radically changed their accounting statements, making the reported values of their property accounts inconsistent over time. As an example, in 1907, AT&T reported to their shareholders that they had changed their accounting procedures significantly, “thus rendering it impossible to give a comparative general balance sheet for a series of years.” For similar reasons, the investment variable is not utilized for years when firms consolidated their subsidiaries within their own accounting statements, underwent a major restructuring, or when a significant merger occurred.

Tobin’s Q: (Book value of total assets + market value of equity - book value of equity)/book value of total assets. The market value of equity is calculated as the number of common shares outstanding multiplied by the price as of the last day of the year, as reported in the *New York Times* for railroads, and in the *Global Financial Data,* for industrials and utilities. The book

[10] There is scant data on the fees and spreads earned by underwriters during our period. Sparse anecdotal evidence suggests that these rates varied between 1.5 percent and 4 percent for railroad bonds.

value of equity is calculated as the book value of common and any preferred shares, plus the firms' surplus (retained earnings). As the stock market was considerably less liquid in the early 20th century than it is today, this variable is frequently unavailable due to the lack of a market price for firms' equities.

As the discussion above makes clear, there was occasionally some variation in the way the sample firms reported the accounting information that was used to construct these variables. As we entered and coded these data, we carefully noted problems and inconsistencies in each firms' financial statements, and we used this information to construct indicator variables that we set equal to one when a variable is mismeasured. In our panel regressions using the accounting data, we include these indicators as controls. These do not substantially affect the results, but they do improve the precision of the estimates. The indicators that are included in the regressions are as follows: in the regressions for Tobin's Q, we include an indicator that is equal to one when a component of Q, the company's 'surplus' (retained earnings), is mismeasured; in the regressions for the interest rate, we include an indicator for cases where interest is disclosed together with other expenses, usually as 'fixed charges'; and in the regressions for leverage, we include an indicator for cases where a firm reports debts of an ambiguous character.

2.3 Other Railroad Characteristics

For the sample railroads, a number of other firm characteristics were obtained from a variety of sources, as follows:

Board Size: The total number of directors, as reported in *Moody's*.

Firm Has 10 Percent Owner: The presence of an owner (generally another railroad) holding more than ten percent of the firm's outstanding shares. The variable reflects data collected by the Interstate Commerce Commission (ICC), and reported in the *New York Times* on 16 January, 1909.

Firm Age: The current year minus the year of the oldest date of incorporation reported in *Moody's*.

Firm Location: Region to which the ICC assigned the railroad. We use the more detailed regional classification adopted by the ICC in the 1920s, as reported in *Annual Report on the Statistics of Railways in the United States for the Year Ended December 31, 1929.* These regions were: New England, Great Lakes, Central Eastern, Pocahontas, Southern, Northwestern, Central Western, and Southwestern. We assign numbers to each region, with 1 for New England and 9 for Southwestern.

Fraction Total Revenues From Freight: Freight revenue/total revenue, as reported in *Moody's*.

Mileage: Total miles operated, as reported in *Moody's*.

2.4 Board Data, Director Names & Matching Procedure

We obtain the names of officers and directors of sample railroads, industrial firms and public utilities from *Moody's*, at two- to five-year intervals from 1905 to 1925. In order to determine whether a director or partner of those investment banks held a board seat with a sample railroad, the names of all partners and directors of the financial institutions engaged in underwriting were collected. For the commercial banks and trust companies, the director names were obtained from the *Rand McNally Bankers' Directory.* Most investment banking partnerships were members of the NYSE;

we obtain the names of their partners from the *NYSE Directory.* For institutions that were not members of the NYSE and not commercial banks or trust companies, a number of different sources were consulted to obtain the names of the directors or partners, such as the directory *Securities Dealers of North America*; the *Directory of Directors* of the city in which the firm was based; the *Baltimore Blue Book*; newspapers such as the *New York Times* and the *Commercial and Financial Chronicle*; and other publications such as *U.S. Investor* and *Moody's*.

We match the names of individuals across these samples to identify board interlocks between railroads and bond underwriters, between industrials and bond underwriters, between public utilities and bond underwriters, between industrials and railroads, and between different railroads. We follow a thorough procedure to clean the collected names and ensure the accuracy of the matches. First, we verify that matching is not hampered by transcription errors or inconsistencies in the source material. Since the management of both financial and nonfinancial firms was relatively stable over time, data obtained for the same firm for different years was compared. For the names of individuals that do not match perfectly across years, we use an algorithm to find approximate matches in names in surrounding years. This procedure identifies cases in which only one letter of the entire name differs across years. In this manner, we are able to identify transcription errors and inconsistencies in the source materials.

Most inconsistencies in the source materials result from alternative spellings of names that would hinder our ability to correctly identify the same individual across firms or years. Many of these inconsistencies are resolved by a set of rules that we developed to standardize names. We use these rules only to address issues of capitalization, spacing, hyphens, and apostrophes. As an example, "DuPont" was chosen to represent the following variations, all of which appeared in source materials: "du Pont," "Dupont," "duPont," and "Du Pont." For the remaining inconsistencies, alternative sources such as biographies, newspapers, and various historical books are used to determine whether two names represent the same person. When we find that two names refer to the same individual, we resolve the inconsistency by ascribing the version used more frequently to the individual.

For the purpose of determining interlocks between different groups of firms (railroads, underwriters, industrials, and public utilities), we would ideally use each director's full name. However, *Moody's* often reports only initials for first and middle names. Thus, we are constrained to match on names using only first initial, middle initial, last name, and suffix. This data restriction could lead to overestimating interlocks across boards whenever two individuals who share a last name have different first and middle names with the same initials. To address this potential source of overmatching, we use data on names across firms, institutions, and years to identify cases where two or more individuals share the same first initial, middle initial, last name, and suffix, but where there is variation in their full names for at least one of all their observations. We then use information from alternative sources to provide first and middle names for these cases. Using this information we develop a new full name variable, fullname_d, which separately identifies individuals that would otherwise collapse to one person if we were to use the uncorrected first and middle initials. For example, Walter H Taylor from Norfolk & Western, William H Taylor from Bowling Green Trust Company and W H Taylor from American Writing Paper would all be incorrectly identified as the same person using only first and middle name initials. The use of additional sources allows determining whether W H Taylor was Walter, William, or a third person altogether (he was William). As we discuss below, this corrected name variable consistently identifies individuals across samples and years. Interlocks between nonfinancial firms and securities underwriters, as well as across firms within each of these samples, are found by identifying exact matches in the variable fullname_d, which consists of first initial, middle initial, surname, and suffix, but includes additional information for individuals that share first and middle initials, as described above.

Even after carefully cleaning the data, it is possible that our sample could suffer from some degree of overmatching. Of course, there is no reason to think that the degree of overmatching would change over time, so this problem is unlikely to be responsible for the patterns described in the paper. Nonetheless, two or more individuals may have shared the same full name, in which case we would falsely identify a board interlock across the firms where those different individuals held board seats. In order to test the reliability of our matching procedure and assess the extent that it leads to overmatching, we use an external source to provide a validity check.

As part of their investigation of the "money trust," the Pujo Committee constructed a detailed description of board interlocks between the major financial institutions (regardless of whether they underwrote securities or not) and nonfinancial firms in 1913. The report contains the information on a total of 179 bankers who held a total of 155 seats on railroad boards. We independently use our data and matching procedure to identify the interlocks between these same financial institutions and railroads. Our own data produces nearly identical patterns of board interlocks. Only four railroad board seats held by bankers identified by the Pujo report were not identified in our data; in each case this was because these individuals were not listed as directors in the *Moody's Manual* of 1913. That is, it was not due to a fault in the matching procedure, but rather to an inconsistency in our underlying source. Moreover, only one banker-director identified in our sample was not listed in the Pujo report. We conclude from this that our matching procedure is highly accurate, and it is unlikely to generate significant biases in the patterns present in our data.

2.5 Interlock and Underwriting Figures and Data

Figure 1 in the paper presents a three-year moving average of the percentage of total underwriting conducted by investment banks (broadly defined) currently represented on the boards of railroads, industrial and public utilities. For each issue, the amount underwritten by bankers on the firms' boards is calculated as the proportion of the underwriters that were represented on the firm's board multiplied by the value of the issue. That is, if the issue had 3 underwriters with 2 represented on the board, the amount underwritten by bankers on the board would be $\frac{2}{3}$ times the value of the issue. For each year, the overall proportion of debt underwritten by bankers on boards is calculated as the sum of those amounts divided by the total amount of debt issues for the sample firms in that year. The figure plots a three year moving average of those annual proportions. Interlocks between nonfinancial firms and underwriters were determined using the matching procedure described above. As the data in the figure present a moving average, the line for railroads does not fall immediately to zero in the figure in 1921. However, the value in that year was in fact zero, and in subsequent years it was close to zero.

Figure 2 in the paper presents the number of major underwriters—that is, those among the top 25 according to total value underwritten up to 1929, listed in Appendix Table A1—that were represented on the boards of railroads, industrials and public utilities whose common or preferred stock was listed on the NYSE in 1913. These data were generated by matching the names of the directors of the nonfinancial firms, in each year indicated on the figure, to the names of the partners and directors of the 25 top underwriters, using the procedure described in the previous section. A total of 138 industrials, 29 utilities, and 71 railroads, were listed on the NYSE in 1913 and are therefore included in the figure. The figure simply plots the average number of those underwriting firms represented on the nonfinancial firms' boards during the indicated years.

The underwriting data used in the tables and regressions are calculated slightly differently. These variables, along with the board interlock variables, are defined as follows:

Percent board underwriting in 1913 (abbreviated "Pct board undw 1913" in the tables) is our main treatment variable. It is calculated as the fraction of the total value of a firm's bond offerings up to 1913 underwritten by bankers represented on its board *in 1913*. That is, unlike the data in Figure 1, which shows the extent of underwriting done by bankers currently on the firms' boards, this variable captures the fraction of underwriting prior to 1913 done only by the bankers represented on the firms' boards at the end of that period. It therefore presents a measure of the strength of the relationship between the underwriters currently represented on the firms' boards (if any), and the firm, in 1913, the year before the Clayton Act was debated and implemented. For each issue, the amount underwritten by bankers that were on the firms' boards in 1913 is calculated as the proportion of the underwriters that were represented on the firm's board in 1913, multiplied by the value of the issue. That is, if the issue had 2 underwriters with 1 represented on the board in 1913, the amount would be $\frac{1}{2}$ times the value of the issue. These amounts are then summed, and divided by the total amount of underwriting by the firm up to 1913.

Percent board underwriting in 1920 is used in Tables 3 and 6. It is calculated in the same way as the percent board underwriting in 1913, except it focuses on the bankers on the board in 1920, and includes all debt issues up through 1920.

High board underwriting is an indicator variable used in Tables 1, 2, and 5, and in Figure 3. It is equal to one for those firms in the top quartile of the "percent board underwriting in 1913" variable. Given the distribution of the percent board underwriting in 1913 variable in our sample, this measure is equivalent to an indicator variable for those railroads with above-median levels of underwriting by banks represented on their boards, conditional on having some nonzero amount of underwriting done by bankers on the railroads' boards.

Herfindahl-Hirschman Concentration Index (HHI) is used in Tables 1 and A3. It is calculated from the shares of a railroad's bond offerings in which each bank was the lead underwriter. That is, if bank b was the lead underwriter for n_b of a railroad's N bond offerings, and there were B different banks that acted as lead underwriters, the HHI index for the railroad would be $\sum_{b=1}^{B}(\frac{n_b}{N})^2$. In Table A3 the index is calculated for all bonds issued prior to 1921, and then again for all bonds issued from 1921-29. In Table 1 the data presented are for all bonds issued prior to 1913.

Interlocks with equipment suppliers is used in Table 8, and is calculated for railroad i in 1913 as the number of NYSE-listed industrial firms that were likely suppliers of capital equipment to railroads that shared at least one director in common with railroad i in that year. We define firms that manufactured steel, locomotives, railroads cars, and parts of railroad cars as likely capital equipment suppliers. The interlocks were identified using our matching procedure defined above.

Competitor interlocks via underwriters on board is used in Table 8. It is calculated for railroad i in 1913 as the number of direct competitors—defined as the railroads that operated in the same region, as defined by the ICC—that shared at least one director in common with railroad i, where that director is also a partner or director of a bank that has underwritten debt for railroad i up to 1913. The interlocks were identified using our matching procedure defined above.

Number of major underwriters on board is the number of the top 25 underwriting firms represented on a firm's board. The top 25 underwriters are determined by the total volume of debt underwriting for the sample industrials and railroads, and are listed in Table A1 above. The presence of these top underwriters on the sample firms' boards is identified using our matching procedure defined above.

Average rank of lead underwriters is used in Tables 1 and 7, and it is defined as the average rank of the lead underwriters for the firm, up to 1913. Lead underwriters are the institutions listed first for each debt offering. A total of 69 institutions were listed as lead underwriters for railroads. The rank of each underwriter is defined based on the total value underwritten up through 1929, ranging from 1 for the highest-ranked underwriter to 69 for the lowest-ranked underwriter. For each firm, the average rank is calculated as follows. If bank b of overall rank r_b was the lead underwriter for bond offerings summing to v_b in value for a railroad, and if the total value of the railroad's debt offerings were V, then the average rank of the railroad's lead underwriters would be $\sum_b (\frac{v_b}{V}) r_b$.

Board interlocks with NYSE-listed industrials is used in Table 1, and it is defined as the number of industrial firms in the sample with which the railroad has at least one director in common. The interlocks were identified using our matching procedure defined above.

Board interlocks with NYSE-listed railroads is used in Tables 1 and A8, and it is defined as the number of railroads in the sample with which the railroad has at least one director in common. The interlocks were identified using our matching procedure defined above.

2.6 Summary Statistics, Industrials and Utilities

Summary statistics for the railroads are presented in the main text. Here we present and discuss similar data for the industrials and utilities.

To ensure comparability with the sample railroads, we restrict the accounting data to the 64 industrials and 15 utilities that were listed in the NYSE in 1913, and that had issued debt during our sample period. Our focus on industrials and utilities listed on the NYSE in 1913 produces a sample that is more comparable to the railroads. Many small, technology-oriented firms went public in the 1920s, and they had little in common with railroads (White, 1990; Nicholas, 2008). Industrials and utilities were also much less likely to issue corporate bonds than railroads, perhaps because they had less salable collateral. Out of 167 industrial firms and utilities listed on the NYSE in 1913, 88 had not issued debt prior to 1914. Restricting the sample to those that issued debt at some point improves the comparability of the leverage ratios across industry groups.

Table A2:
Summary Statistics: Industrials and Utilities, 1913

	High Board Underwriting Firms	All Other Firms	Difference, (1) vs. (2)
	(1)	(2)	(3)
Firm age: years	14.444	14.211	0.233
	[11.646]	[12.801]	(2.852)
Log(Assets)	18.050	17.740	0.310
	[1.053]	[0.823]	(0.232)
Book leverage	0.225	0.190	0.035
	[0.173]	[0.161]	(0.041)
Average interest rate	0.058	0.054	0.004
	[0.023]	[0.016]	(0.006)
Return on equity (ROE)	0.179	0.135	0.044
	[0.122]	[0.083]	(0.028)
Return on assets (ROA)	0.104	0.077	0.028
	[0.097]	[0.059]	(0.021)
Tobin's Q	0.923	0.723	0.201
	[0.548]	[0.233]	(0.076)
Investment	0.022	0.063	-0.041
	[0.129]	[0.109]	(0.031)

Note: Based on the 79 industrial firms and public utilities that were listed in the NYSE in 1913, and that had issued debt during our sample period. Column (1) reports means for industrials and utilities in the high board underwriting group, defined as those in the top quartile of the distribution of the percentage of debt underwriting done by bankers on their boards, with standard deviations in brackets. Column (2) reports means and standard deviations for the other sample industrials and utilities. Column (3) presents the differences between the values in columns (1) and (2), with standard errors in parentheses estimated from a regression with robust standard errors. All variables are defined in detail in Section 2 of the Appendix.
Source: Authors' calculations.

Means and standard deviations for the 79 industrials and utilities are presented in Table A2 for the year 1913. Column (1) presents data for the firms that were most reliant on the underwriters on their boards for their bond underwriting, defined as those among the top 25 percent. of this statistic in 1913. Column (2) presents the means for all other industrials and utilities. These data illustrate several differences between railroads and firms in other industries. Relative to railroads, industrials and utilities in 1913 were smaller and younger. The mean value of log assets for railroads in the high board underwriting group was 19.5, compared to 18 for industrials and utilities with similar strong relationships with their underwriters. The mean age for railroads in the high board underwriting group was 33 years, compared to 14 years for the comparable industrials and utilities. The industrials and utilities were less levered: the mean book leverage ratio in column (1) is 0.23, compared to 0.51 for the high board underwriting railroads. The industrials and utilities were slightly more profitable on average according to accounting measures—the ROE among the high board underwriting firms was 0.18, compared to the railroads' 0.15–but had lower market valuations–the average Tobin's Q in column (1) is 0.72, compared to 0.92 for the high underwriting railroads. These differences across industries are also similar when comparing those firms that did not have strong relationships with the underwriters represented on their boards.

Column (3) of Table A2 presents tests of differences in means between industrials and utilities with and without a high reliance on banks represented on their boards in 1913 for their bond underwriting up to 1913. Among industrials and utilities, selection into close relationships with underwriters appears to have been quite similar on observable characteristics than among railroads: the sign of the differences in size, leverage, profitability, and valuation are the same than those found for railroads in Table 2 of the paper. However, none of these differences are statistically significant. Moreover, unlike the railroads, the high board underwriting group of industrials and utilities invested at lower rates and had higher average interest rates. The exceedingly high level of investment among the industrials and utilities that were not in the high board underwriting group in 1913 is due in part to the very rapid growth of two young industrial firms in that year; eliminating those two firms from the calculation reduces the average investment rates to 4.4 percent, which is also very close to the rate for those firms as a whole in 1912 and 1914.

3 Analysis: Additional Details

3.1 A Brief History of Section 10 of the Clayton Act

President Wilson's address to Congress on January 1914 called for new antitrust legislation and emphasized the importance of addressing conflicts of interest by preventing interlocking directorates that made "those who borrow and those who lend practically one and the same, those who sell and those who buy but the same persons trading with one another." But he also gave clear directives that "sufficient time should be allowed [...] to effect these changes of organization without inconvenience or confusion" (Wilson 1914, vol 29, p. 155). To carry out these recommendations, Section 10 of the Clayton Act, which restricted common carriers from having dealings in securities and supplies, or to make contracts for construction and maintenance with any firm with which they had a director in common, specifically stated that the law would only apply after two years from the approval of the Clayton Act. After this period, such transactions could take place through a competitive bidding process that would be regulated by the ICC.

At the urging of the President, the Act also stipulated that officers and directors who violated Section 10 would be personally liable—specifically, they would be charged with a misdemeanor, and be fined for a maximum amount of $5,000, or sent to prison for at most a year, or both.[11] Railroads violating this section would also be fined by an amount not to exceed $25,000.

But the outbreak of World War I in Europe put great strain on American railroads, shifting the attention of the government to issues that guaranteed an adequate functioning of the railroad system, such as those related to rate regulation and labor policies (Kerr, 1968). Overburdened with other concerns, the ICC did little to develop the rules for the implementation of the bidding system required by Section 10. On July 19, 1916, less than three months before Section 10 was to go into effect, railroad officers met with President Wilson to ask for a two-year postponement to allow for an investigation that would determine the potential harmful effects of the Act, as well as

[11] As a point of reference, note that a household with a pre-tax annual income level higher than $5,864 belonged to the top 0.5 percent of the income distribution in 1914 (Piketty and Saez, 2003; Table A4, 2014 update).

how to implement a system of bids to allocate contracts.[12] On August 15, 1916 Congress passed Public Resolution No. 33 to delay the effective date of Section 10 from October 15, 1916 to April 15, 1917, and President Wilson approved the resolution on August 31, 1916. At a time of extreme turmoil, this postponement was largely unnoticed by the public.[13]

As the war effort increased, the efficient functioning of the railroad system became critical. To minimize any disruptions, Section 10 was further postponed on March 4, 1917 to become applicable on January 8, 1918 (Public Resolution No. 55, 64^{th} Congress), and then again on January 12, 1918 until January 1, 1919 (Public Resolution No. 20, 65^{th} Congress). When this last postponement expired, the railroads were still under government operation and little was done to enforce the Act. The ICC established a procedure for the implementation of Section 10 on October 6, 1919 (ICC Ex Parte Order No. 54, amended on October 4, 1920), but it specified that the regulations would only become effective on January 1, 1920. Before these were ever implemented, however, Section 501 of the Transportation Act of 1920, which passed on February 28, 1920, further deferred the enactment of Section 10 until January 1, 1921.

Shortly after the Transportation Act was passed, a bill was introduced in the Senate to amend Section 10; it was sent for counsel to the ICC, where it was misplaced. When Congress resumed functions on December 6, 1920, it had no counsel from the ICC and little time remaining to determine whether and how to amend the Act before it went into effect. On December 18, Congress approved a further postponement of Section 10 until January 1, 1922. In arguing for this bill, Representative Esch emphasized the conflicts between some of the provisions of the Transportation Act and those of the Clayton Act, particularly regarding dealings in securities.[14] This further postponement was "indorsed [sic] by the railroad security holders, the railway executives and the great financial interests which control the carriers."[15]

President Wilson referred the bill to the ICC for consideration, which recommended the President to sign the proposed amendment to the Transportation Act on December 28.[16] At that time, Wilson's decision was likely uncertain. On the one hand, he had been a forceful proponent of the restrictions against board interlocks when the Clayton Act was first passed in 1914. On the other hand, he had nominated the commissioners to the ICC who recommended to grant a new extension by majority vote–reports suggested that 6 out of 10 members had voted in favor. Moreover, Wilson had signed all earlier postponements enacted by Congress during the war period. But more importantly, access to the President and knowledge of his views on specific policies was limited. Having suffered from hypertension and cerebral vascular disease for over two decades, President Wilson had a devastating stroke on October 2, 1919 that left him paralyzed (Berg, 2013: 569, 639-641). The seriousness of his condition was largely hidden from the public, cabinet members and, at least for a few days, even the Vice-President. Edith Wilson, the First Lady, effectively became his steward and decided which issues were important enough to deserve his consideration

[12] An important criticism of Section 10 was that it limited transactions between controlling and subsidiary companies; railroad men argued that a strict interpretation of the Act could break up large transportation systems. Critics also argued that the criteria to be used for determining the most favorable bids was unclear, and that the regulation could lead to additional expenses and delays in procuring inputs, which would in turn increase the railroads' operating costs. See "Want Clayton Act Suspended 2 Years," *New York Times*, 23 July 1916.

[13] "Laws Enacted by 64^{th} Congress," *Federal Trade Reporter*, 1 October 1916.

[14] "President Wilson Vetoes Bill Amending Transportation Act Suspending Provision of Antitrust Act," *Commercial and Financial Chronicle*, 1 January 1921.

[15] "Appointment of John J. Esch as Member of Interstate Commerce Commission Confirmed by Senate," *Commercial and Financial Chronicle*, 30 April 1921.

[16] "Advises President to Sign," *New York Times* 29 December 1920.

(Berg, 2013: 642-643, 661). Although the President recovered somewhat over time, his agenda was kept to a relative minimum until he left office (Berg, 2013: 678). We are aware of no public or private pronunciations on his views regarding the regulation, and his address to Congress (read by a clerk) on December 7 barely mentioned the railroads. On December 30, 1920, President Wilson's veto of the bill to further defer Section 10 likely came as a surprise.

Railroad advocates quickly sprang into action. On January 5, 1921, the Senate began holding hearings on the Ferlinghuysen bill to modify Section 10.[17] Although various proposals were made over time to amend the regulation, the restrictions to limit the presence of underwriters on railroads' boards finally became effective and enforced after January 1, 1921.

3.2 Changes in the Strength of Underwriting Relationships

Table A3:
Underwriting Relationships

	HHI Concentration index Underwriting among bankers		
	High board undw (1)	Other RRs (2)	Difference: High-Other (3)
Time Period:			
1900-1920	0.812	0.534	0.278
	(0.062)	(0.048)	(0.082)
1921-1929	0.614	0.651	-0.037
	(0.071)	(0.047)	(0.086)
Change: 1921-1929 vs. 1900-1920	-0.198	0.117	-0.349
	(0.094)	(0.067)	(0.169)

Note: Column (1) reports means for firms with the highest degree of underwriting done by bankers on their boards in 1913, defined as those in the top quartile of the percent of underwriting done by bankers that were represented on their boards in 1913; column (2) reports means for all other firms. Column (3) presents the difference in means between columns (1) and (2). The data in the table are restricted to the 51 railroads for which we observe underwriting in both periods. Standard errors in parentheses.
Source: Authors' calculations.

We analyze the impact of Section 10 on the strength of affiliations between railroads and banks, as measured by the degree of reliance on particular underwriters. Table A3 presents the evolution of a Herfindahl-Hirschman index of concentration of underwriting services by lead underwriters.[18] We split the sample between the "high board underwriting" railroads, defined as those in the top quartile of the amount of underwriting done up to 1913 by the bankers on their boards in that year, and the other railroads. The high board underwriting group were the railroads most strongly affected by Section 10. The first row of Table A3 shows that underwriting services were more concentrated (average HHI above 0.8) for the railroads that relied most heavily on the bankers on their boards prior to 1920. The HHI number for this group declined sharply to 0.625 after Section 10 was implemented, and the differences across the two groups became statistically indistinguishable (see column 3). The difference across groups over time is -0.33, equivalent to 56 percent of

[17] "Commission Says Changes in Act Needed," *Wall Street Journal* 6 January 1921. See also "Amending section 10 of Clayton Act : hearings before a subcommittee of the Committee on Interstate Commerce," S. 4576, 66^{th} Congress.

[18] The index is calculated from the shares of a railroad's value of its bond offerings in which each bank was the lead underwriter. That is if bank b was the lead underwriter for n_b of a railroad's N bond offerings, and there were B different banks that acted as lead underwriters, the HHI index for the railroad would be $\sum_{b=1}^{B}(\frac{n_b}{N})^2$.

the 1920 overall mean, which was 0.622. This suggests that the regulation significantly altered underwriting relationships: railroads that had previously maintained very close affiliations with particular underwriters utilized a broader range of investment banks for underwriting services once those intermediaries could no longer have a presence on their clients' boards.

3.3 Banker Resignations, 1920-23

The enforcement of Section 10 had a significant effect on the boards of directors of American railroads. As shown in Figure 2 of the paper, the implementation of Section 10 of the Clayton Act on January 1, 1921 resulted in a large decline in the representation of major investment banks on the boards of railroads. Between 1920 and 1923, the median railroad had one elite financier—defined as a partner or director from one of the top 25 underwriting firms—step down from its board. We identify as 'resignations' those cases in which an individual appears on the railroad's board in 1920, but does not in 1923—we can not, however, determine whether the banker actually resigned or was asked to step down by the railroad. In this section we relate the variation across railroads in the number of banker resignations to the railroads' needs for external finance.

The rules of Section 10 allowed underwriters to remain on a railroad's board as long as they did not underwrite any new issues. Only those banker-directors hoping to perform underwriting services for a railroad in the near future would have reason to resign following the implementation of Act. Bankers who had stronger preexisting relationships with a railroad probably expected to participate in any future underwriting business, and may therefore have chosen to step down from the railroad's board. Banker-directors who had previously underwritten debt for a railroad would have had an even stronger incentive to resign from its board if they anticipated that the railroad was likely to issue new securities soon after the enactment of Section 10.

Table A4 provides evidence supporting our intuition. Panel A shows the mean number of resignations of directors from the top 25 investment banks for railroads in the high board underwriting group, and for the other railroads. On average, railroads in the high board underwriting group had more than 2 elite investment bankers resign from their boards, compared to only one for the other railroads, and the difference is statistically significant. This finding suggests that these resignations likely undid the more advantageous connections that the high board underwriting railroads had with major underwriters prior to any regulatory action against board interlocks with financiers. Indeed, in Table 1 we show that the high underwriting railroads had one additional major underwriting firm represented on their boards in 1913, relative to all other railroads.

The determinants of banker-director resignations are explored in more detail in cross-sectional regressions in Panel B, which control for board size (as railroads with larger boards may have been more likely to see greater numbers of resignations). In column (1), the effect of the strength of the railroads' underwriting relationships with the bankers on their boards in 1920 is shown to have a large, and statistically significant effect on the number of resignations. An increase in one standard deviation in the fraction of underwriting done by banks represented on the boards in 1920 is associated with 0.6 more resignations from 1920 to 1923, about half of the number of resignations for the average railroad in the sample over this period. In column (2), we also control for whether or not a railroad had a substantial amount of debt maturing in the years 1921-23, which would have been a strong indication that the railroad would need to issue new corporate bonds to refinance the maturing liabilities. Specifically, we include an indicator for railroads in the top decile of the fraction of their debt maturing in those years.[19] While the estimated coefficient is positive, it is not statistically significant. Finally, in column (3), we interact the indicator for maturing debt with the percent board underwriting variable. The estimated coefficient on the interaction term is large and statistically significant. Among those railroads with a large fraction of their debt maturing, the strength of their relationships with the investment banks on their boards was a powerful predictor of the number of resignations that would occur.

Table A4:
Determinants of Banker Resignations, 1920-23

A: Resignations	High Board undw. Railroads	Other Railroads	Difference
Mean number of resignations	2.176	1.060	1.116
	[1.845]	[1.202]	(0.390)

B: Regressions: Resignations	(1)	(2)	(3)
Percent board underwriting, 1920	1.511	1.410	0.996
	(0.482)	(0.440)	(0.440)
High debt maturing 1921-23		0.768	-0.715
		(0.684)	(0.435)
Pct board undw × high debt maturing 1921-23			3.017
			(1.271)
Board size, 1920	0.088	0.091	0.092
	(0.052)	(0.051)	(0.048)
Constant	-0.305	-0.392	-0.275
	(0.729)	(0.734)	(0.701)
Observations	67	67	67
R-squared	0.222	0.247	0.326

Note: Panel A of this table reports means and standard deviations (in brackets) of the number of resignations of directors who were also partners or directors of at least one of the top 25 securities underwriting firms. Panel B of the table reports regressions with the count of these resignations as the dependent variable. Percent board underwriting 1920 measures the fraction of underwriting done up to 1920 by the banks represented on the railroad's board in that year. The high debt maturing variable is defined as an indicator for the firms in the top decile in the share of their total debt maturing in the years 1921-23. Robust standard errors are reported in parentheses.
Source: Authors' calculations.

Archival evidence indicates that elite bankers did indeed consider railroads' maturing debt in deciding the boards they would resign from, or potentially join. For example, in correspondence with the president of the Northern Pacific Railroad, from which he had just resigned, J.P. Morgan partner Thomas W. Lamont asked, "Perhaps some time when you are talking with Hale Holden [President of the Chicago, Burlington and Quincy RR], of whom I am very fond, you can, in your own way, find out whether he would ever want me to go on his Board. Of course, I should not wish to in case we were likely to have bond dealings with them, but it seems to me that their

[19] For all railroads in the sample, the average fraction of debt maturing in 1920-23 was around 4 percent, a very small amount, and the median was only 1 percent. The railroads in the top decile, however, had a minimum of 13 percent of their debt maturing in those years, and an average of 24 percent.

financing is over for a long time to come and there is not much chance of that."[20] This anecdotal evidence and our quantitative analysis suggests that the railroads' needs for external funds, and the continuation of the provision of underwriting services, were important considerations in the resignations of bankers from boards following the implementation of Section 10.

3.4 Railroad Characteristics and Selection into Bank-Firm Relationships

The paper presents comparisons between the characteristics of railroads in the high board underwriting group and the other railroads in Tables 1 and 2. Here, we present comparisons based on more fine-grained distinctions in the strength of railroads' relationships with their underwriters. These comparisons are more revealing in regards to the determinants of railroads' relationships with underwriters, and the selection process behind the formation of those relationships. To avoid any confounding effects of regulatory changes on this selection process, we analyze the characteristics of railroads in 1913, before the Clayton Act was passed. Due to the low number of observations, we base our comparisons simply on differences in means, and we urge caution in interpreting this evidence.

In Table A5, we divide the sample railroads into four groups: (1) the high board underwriting group, defined as before as those in the top quartile in the fraction of underwriting performed up to 1913 by bankers on their boards in 1913; (2) a "medium or low board underwriting group," defined as railroads that had at least some underwriting done up to 1913 by the bankers on their boards in 1913, but that did not belong to the high board underwriting group; (3) railroads with no underwriting done by bankers on their boards up to 1913, who nonetheless had top investment banks represented on their boards in 1913; and (4) railroads that did not have any top underwriters represented on their boards in 1913. The table presents means and standard deviations by group for a variety of characteristics, as well as p-values for tests of differences in means between each group and group (1), the high board underwriting railroads. (In Panel D of Table 5 of the paper, we present regressions in which the railroads in group (2), those partially treated, are eliminated from the sample, and the treated group (1) is compared to the control group of (3) and (4) only.)

Panel A of the table shows how the groups are defined, and characterizes their relationships with underwriters. The average percent of the railroads' debt underwritten by bankers on their boards ranges from 99 for group 1 to 46 for group 2, and is by construction zero for groups 3 and 4. The number of top underwriters represented on the railroads' boards is very similar for groups 1 and 2, considerably lower for 3, and zero (by construction) for 4. The average ranking of the investment banks that performed underwriting services for the railroads declines linearly across the groups: group 1 used the highest-ranking underwriters, group 2's underwriters were about five ranks lower on average, group 3's were about five ranks lower than 2's, and group 4's were about nine ranks below group 3's.

[20] Letter to Howard Elliot, 10 April 1922, T.W. Lamont Papers, Folder 226-7, Baker Library, Harvard University.

Table A5:
Summary Statistics: Railroads by Underwriter Relationships, 1913

	High Board Underwriting Railroads ($N = 20$)	Medium or Low Board Underwriting Railroads ($N = 20$)	Railroads With Top Bankers on Boards that Did Not Underwrite ($N = 20$)	Railroads Without Top Bankers On Their Boards ($N = 11$)
	(1)	(2)	(3)	(4)
A: Relationships with Underwriters				
Share debt undw. by banks on board in 1913	0.994	0.458	0.000	0.000
	[0.023]	[0.267]	[0]	[0]
p-value, difference from column (1)		0.000	0.000	0.000
Major underwriters on board	3.250	3.750	2.050	0.000
	[1.482]	[1.832]	[0.999]	[0]
p-value, difference from column (1)		0.349	0.005	0.000
Avg. rank, underwriters	5.918	11.025	16.412	25.567
	[7.249]	[9.595]	[19.247]	[26.750]
p-value, difference from column (1)		0.065	0.067	0.122
B: Characteristics and Outcomes				
Log(assets)	19.479	19.192	18.170	17.685
	[0.943]	[0.828]	[0.983]	[1.466]
p-value, difference from column (1)		0.313	0.000	0.001
Book leverage	0.506	0.514	0.394	0.416
	[0.090]	[0.144]	[0.188]	[0.188]
p-value, difference from column (1)		0.826	0.022	0.144
Investment	0.043	0.024	0.025	0.009
	[0.077]	[0.034]	[0.036]	[0.034]
p-value, difference from column (1)		0.343	0.405	0.265
Avg. interest rate	0.037	0.046	0.044	0.044
	[0.007]	[0.026]	[0.010]	[0.016]
p-value, difference from column (1)		0.133	0.025	0.177
Tobin's Q	0.903	0.849	0.901	1.015
	[0.217]	[0.221]	[0.154]	[0.151]
p-value, difference from column (1)		0.523	0.982	0.265

Note: Column (1) reports means and standard deviations for firms in the top quartile of the percent of underwriting done by bankers that were represented on their boards in 1913. Column (2) reports means and standard deviations firms that had some underwriting done by bankers on their boards, but not enough to be in the top quartile. Column (3) presents means and standard deviations for firms that had 0 underwriting done by bankers on their boards, and that had major underwriters on their boards. Column (4) presents data for firms that had no major underwriters on their boards in 1913. The rows headed by the statement "p-value, difference from column (1)" report the p-value of a t-test obtained from a regression with robust standard errors. All variables are defined in detail in Section 2 of the Appendix.
Source: Authors' calculations.

Panel B of Table A5 presents differences in characteristics across the groups, and provides some insight into the factors that led the railroads to select into having or not having a close relationship with an underwriter. The first row shows that firm size, as measured by log assets, declines linearly across the groups. The second row shows that leverage also declines, although not linearly: groups (1) and (2) have substantially higher leverage than groups (3) and (4). The third row shows that investment also declines across groups: group (1) invested at higher rates than groups (2) and (3), which in turn invested at higher rates than group (4). Taken together, the larger size, higher leverage, and higher investment rates of the railroads in group (1) indicates that these railroads had much greater needs for external finance than those in group (2). The same factors suggest that the railroads in group (2), in turn, had higher needs for external finance than those in (3) and (4). Thus, the need for external finance appears to have a strong association with the strength of railroads' relationships with underwriters.

Were the railroads in groups (2)-(4) negatively selected? They all paid slightly higher interest rates on their debt, which could indicate that that they were regarded in the bond market as slightly riskier issuers. Yet their valuations are not systematically different from those of group (1). (The average value of Tobin's Q for the railroads in group (4) was actually higher than those of group (1), although the difference is not statistically significant.) One reasonable conclusion that can be drawn from these comparisons is that railroads with different relationships with their underwriters were indeed different from one another, although not in ways that lead to a clear hierarchy or quality ranking.

These simple differences in means can also be informative on the role of bankers. For example, a comparison of groups (3) and (4) may shed light on the role played by bankers on boards that did not underwrite. The greatest differences across these two groups were in their size—those without bankers were a lot smaller—and in their investment rates—those without bankers invested at far lower rates. This suggests a role for the non-underwriting banker-directors as financial experts or perhaps industry experts, as their advice could be potentially more valuable for these firms.

3.5 Event Study Analysis

3.5.1 Data and Methodology

No readily available data on daily stock prices exists for our period of interest. To calculate stock returns around the Presidential veto, we collect stock price and dividend data for all sample railroads, industrials and utilities on the days around December 31, 1920. We also collect similar information to perform a placebo analysis at the beginning of the month, on December 1, 1920. For each day, we obtain the closing price of common shares from the Stock Quote tables published in *The New York Times.*

To calculate returns, we adjust stock prices by the dividend payout when shares go ex-dividend. We determine the ex-dividend date if the traded share was listed as such in the Stock Quote table, and we obtain information on dividend payouts from the *Moody's Manual* for the year 1921. Because the dividend payouts were mostly quoted as a percentage of the value of shares, we use the par value of common shares, obtained from the same manual, to determine the dividend dollar amount when necessary. However, these adjustments make a negligible difference; the correlation between adjusted and unadjusted returns is above 0.98 for both dates in our analysis. There are also no cases of stock splits within the days of our analysis.

On day t, we calculate firm's i return as:

$$R_{i,t} = \frac{(P_{i,t} + d_{i,t}) - P_{i,t-1}}{P_{i,t-1}} \tag{11}$$

Cumulative returns for a window of days [-k,k] centered on December 31 or December 1, are calculated as:

$$Cumulative\ R_i = \sum_{t=-k}^{t=k} R_{i,t} \tag{12}$$

Our strategy diverges from the standard event study methodology because we cannot calculate abnormal returns as the difference between the actual return and the return predicted from a market model. Estimating such a model would require collecting price and dividend data from primary sources for about a year prior to the veto, an exceedingly large data collection effort. Our strategy is therefore close to the market-adjusted-return model that assumes $\alpha = 0$ and $\beta = 1$ for every share.[21] The lack of information on stock returns prior to the veto also limits our ability to adjust the standard errors using the pre-period variance in returns. Instead, we present placebo tests based on an earlier date, and on the returns of industrial firms and utilities.

Another limitation of historical stock market data is that securities markets were fairly illiquid. To be able to cumulate returns when some returns are missing within the event window, we fill in a missing price in a given day with the last observed transacted price. This methodology creates a large number of zero returns for shares that do not trade frequently. Thus, we restrict the observations to those firms for which the number of non-missing returns observed during the event window $[-k, k]$ was above the 25^{th} percentile of non-missing returns in the sample for that window. This restriction causes the number of observations to vary across event windows. For example, the constraint requires observing at least two non-missing returns in the three-day window around the veto for the sample railroads. The constraint is not satisfied for three railroads for which we observe returns on December 31, and accounts for the decline in the number of observations from column (1) to column (2) of Table 3.

3.5.2 Additional Results

Section 5.1 of the paper presents an event study analysis of the stock market reaction to Wilson's veto to the postponement of Section 10 of the Clayton Act on December 30, 1920. Here, we extend the analysis to address concerns of persistent differences in returns across treated and control railroads, and replicate our main results for an alternative definition of treated and control groups.

Our main empirical strategy in Table 3 of the paper simply compares the cross-sectional differences in returns at the time of the President's veto. One concern is that these results may not be specific to the veto, since railroads with strong relationships with underwriters may have lower returns on average. We construct a placebo test of cross-sectional differences in returns around December 1, 1920, an arbitrarily chosen date prior to the resumption of activities in Congress to further postpone Section 10 that we discussed in detail in Appendix Section 3.1. Table A6 shows that the estimated effects are essentially zero on this date–the magnitude of the estimated coefficient in column (1) is about one-fifth of the estimated effect on December 31, and it is not

[21] Because the event date is the same for all firms in the sample, subtracting the overall market return from the daily individual stock return for all firms would only affect the constant term of the regression.

statistically different from zero. Column (2) replicates this finding for the cumulated returns on a one-day window around the placebo date. Columns (3) and (4) report similar, essentially zero effects on the placebo date for all traded industrials and utilities. The similarity of the results across sectors on this arbitrarily chosen date stands in sharp contrast with our findings on the veto, when the market values of railroads with strong relationships with their underwriters declined, but those of industrials and utilities with similarly strong ties did not. This finding further suggests that the estimated effects of the passing of the veto do not reflect differential underlying trends for the treated railroads.

Table A6:
Stock Returns: Placebo Analysis

	Railroads		Industrials and Utilities	
	Daily Returns, December 1, 1920	Cum. Returns, One-day Window	Daily Returns, December 1, 1920	Cum. Returns, One-day Window
	(1)	(2)	(3)	(4)
Pct board underwriting, 1920	-0.001	0.006	0.003	-0.003
	(0.010)	(0.017)	(0.006)	(0.014)
Observations	40	37	53	48
R-squared	0.000	0.004	0.004	0.001

Note: Column (1) analyzes the variation in the cross-section of returns for the sample railroads on December 1, 1920, an arbitrary day prior to any activities in Congress to introduce a new postponement of Section 10, and well in advance of President Wilson's veto on December 31, 1920. Daily returns are regressed on the percentage of underwriting up to 1920 done by bankers represented on the railroads' boards in 1920. Column (2) presents an analysis of the returns cumulated over a one-day window around December 1. Column (3) analyzes the cross-section of returns for the sample industrials and utilities on December 1 . Column (4) reports the cumulated returns over a one-day window around December 1 for the sample industrials and utilities. All regressions include a constant term. Robust standard errors.
Source: Authors' calculations.

Our main analysis on Table 3 exploits the intensity of treatment for all sample railroads. An alternative strategy is to compare railroads that had the strongest relationships with their underwriters only with firms in which the perceived conflicts of interest that Section 10 intended to address were negligible because board representation was unrelated to the provision of underwriting services. Table A7 contrasts the stock market reaction to the veto for the most treated railroads in our sample, defined as those in the top quartile of the percent underwriting done by bankers on railroad's boards in 1920, to the reaction for a control group which includes only railroads where the top underwriters represented on their boards in 1920 did not underwrite—that is, group (3) railroads—and those that did not have any top underwriters represented on their boards in 1920—group (4) railroads. The railroads in group (2)—a total of 14 railroads with low but positive levels of underwriting done by banks on their boards in 1920—are excluded from the analysis. This strategy is similar to the alternative control group specification utilized in Panel D of Table 5 for our main variables of interest.

The results are entirely consistent with our baseline estimates presented in columns (1) to (3) of Table 3. In column (1) of Table A7, we find that the returns of the most treated railroads were 480 basis points lower on December 31 than those of railroads that were unaffected by Section 10. As in our main analysis, the effects are negative but imprecisely estimated when we cumulate over a one-day window around the veto in column (2). Finally, column (3) indicates that the returns of the treated railroads remained depressed relative to those of the non-affected firms until January 5, 1921. This finding suggests that our main effects are not merely the result of a selected group of firms that chose to utilize bankers on their boards for underwriting services.

Table A7:
Stock Returns: Alternative Control Group
Railroads with No Bankers on Board, or Bankers that did not Underwrite

	Daily Returns, December 31, 1920 (1)	Cum. Returns, One-day Window (2)	Cum. Returns, December 31 to Jan 5, 1921 (3)
High underwriting by bankers on board 1920	-0.048	-0.042	-0.076
	(0.021)	(0.030)	(0.037)
Observations	33	30	28
R-squared	0.109	0.049	0.120

Note: The high underwriting indicator takes a value of 1 for those firms in the top quartile of the percent underwriting done up to 1920 by banks represented on the railroad's board in 1920–a total of 13 railroads in which the bankers on their board had underwritten between 64 and 100 percent of all their bond financing. The control group includes only firms that were unaffected by Section 10: 13 railroads that had major investment banks on their board that did not underwrite for them, and 9 railroads that had no underwriters on their boards in 1920. Column (1) analyzes the variation in the difference in returns for treated and control railroads on December 31, 1920. Column (2) presents an analysis of the returns cumulated over a one-day window around December 31. Column (3) analyzes the cumulative returns from December 31 through January 5, the date when a proposal was announced in Congress to override the veto. All regressions include a constant term. Robust standard errors.
Source: Authors' calculations.

3.6 Outcomes: Treated vs. Control Railroads

Figure 3 in the main text presents the annual differences between the high board underwriting railroads, and all other railroads in the sample, for our four main outcomes of interest. In this section, we further describe the variation in those differences over time by presenting a figure that illustrates the treated and control firms' outcomes separately.

To construct the figure, we estimate regressions of the form: $y_{it} = \alpha_i + \delta \log(\text{assets}_{t-1}) + \sum \phi_{1t} \text{HighBoardUnderwriting}_i \times \text{Year}_t + \sum \phi_{2t} \text{OtherRailroad}_i \times \text{Year}_t + \epsilon_{it}$, where y_{it} is the outcome of interest, and α_i is a firm fixed effect. The ϕ_{1t} and ϕ_{2t} parameters describe the performance of the high board underwriting railroads and the other railroads separately, relative to the excluded year 1905. It is important to note that in this framework, it is not possible to include year fixed effects, as we do in all of our regression specifications. (The average values of ϕ_{1t} and ϕ_{2t} fully capture the annual variation in the outcome variables, net of the firm fixed effects.) The large degree of macroeconomic volatility during our sample period will therefore be reflected in the figure, potentially obscuring the differences among the two groups of railroads, which are better displayed in Figure 3.

Figure 1 presents the estimated ϕ_{1t} and ϕ_{2t} coefficients, showing the annual differences relative to 1905 for both groups of railroads. In panel (A), which shows the values for Tobin's Q, several noteworthy patterns are evident. Most importantly, throughout the years up to 1920, the high board underwriting railroads enjoy higher valuations. There is a significant decrease in both groups' valuations during the Panic of 1907, but they recover relatively quickly. During the ensuing decade, both groups' valuations fall considerably, and then they rise again during the 1920s. But high board underwriting railroads' valuations increase by less than that of the other railroads during this period, and by the end of our sample period their average value of Tobin's Q was actually lower than that of the other railroads.

Panel (B) depicts investment rates. Again, in nearly every year up to 1920, the high board underwriting railroads invested at higher rates. Their investment appears to spike upward during some years prior to 1920, which sometimes correspond to periods of strong economic growth, whereas the investment rates of other railroads are smoother. One possible interpretation for this pattern is that strong relationships with underwriters gave railroads access to the necessary funds to take on very large projects, when those arose. Then following the enforcement of Section 10, investment by the high board underwriting group becomes lower, and less volatile. In fact for most of the 1920s, these railroads invest at lower rates than other railroads, and the differences between these two groups are much smaller than in earlier years.

Panel (C) presents the evolution of average interest rates over time. Before we proceed, however, it is important to note that the measure of interest costs that can be observed in the data—the average interest expense on the stock of outstanding debt—adjusts slowly to changes in marginal interest costs. We would therefore expect any changes in marginal interest rates due to the enforcement of Section 10 to be reflected in the differences across railroads with a substantial lag. The Figure shows a substantial and persistent advantage among the high board underwriting group, which had lower average interest rates until the early 1920s. There is an increase in both groups of railroads' rates in 1918, which reflects somewhat higher borrowing costs in that year, but also, much more importantly, a change in the reporting of interest expenses by *Moody's* that artificially inflated interest costs in that year. (In the volume we used to record data for 1918, Moody's reported "fixed charges" rather than "interest costs" for railroads; fixed charges may include lease payments and other payments unrelated to borrowing.) But in the 1920s the differences across the two groups of railroads begin to narrow and, by the second half of the decade, the average interest costs of the high underwriting railroads reach levels well above those of the other railroads. Thus, the advantage enjoyed by those railroads in the earlier years of the sample is completely reversed by the end of the sample period.

Finally, Panel (D) depicts leverage. Here, an advantage by the high board underwriting railroads first develops around 1909, and grows over the following decade. Leverage for all railroads falls between 1916 and 1920, and then recovers somewhat up through 1925. During the second half of the 1920s, the leverage of the high board underwriting group begins to fall more quickly than the leverage of the other railroads, and the difference between them narrows considerably. As with interest rates, the leverage ratio should also reflect any constraints in the railroads' ability to obtain new debt financing after Section 10 went into effect with a substantial lag.

The patterns that emerge from these four graphs are overall consistent with the negative effects of Section 10 on the high underwriting railroads. Until 1920, these railroads had an advantage relative to others, but this advantage diminished or, for some outcomes, it was reversed in the 1920s. In many ways, the high underwriting railroads became more similar to others after the regulatory change—their higher valuations and their ability to implement larger investments disappeared. This is precisely what we would expect if any advantages in their access to external funds that were obtained by selecting into relationships with underwriters were disrupted by the Act.

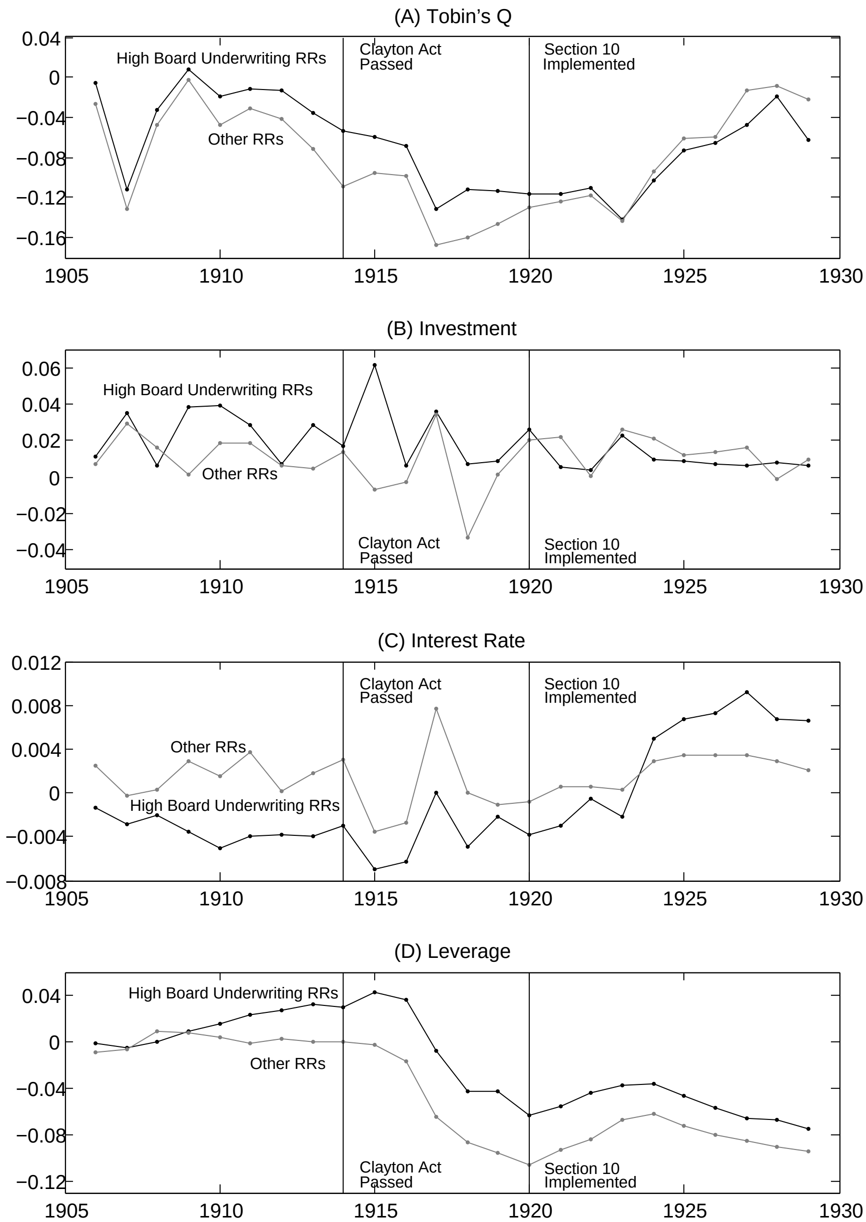

Figure 1:
Annual changes: High board underwriting and other railroads

Note: The figure presents annual changes among railroads in the high board underwriting group and among the others, relative to 1905. The changes are estimated from regressions that include firm fixed effects and control for the log of lagged assets. Specifically, the figure presents estimates of the ϕ_{1t} and ϕ_{2t} parameters obtained from the regression $y_{it} = \alpha_i + \delta \log(\text{assets}_{t-1}) + \sum \phi_{1t} \text{HighBoardUnderwriting}_i \times \text{Year}_t + \sum \phi_{2t} \text{OtherRailroad}_i \times \text{Year}_t + \epsilon_{it}$, where y_{it} is the outcome of interest, and α_i is a firm fixed effect.

Two other characteristics of the figure are worth noting. First, consistent with Figure 3 in the paper, the patterns in Figure A1 make it quite clear that the treated and control railroads were evolving along similar trends prior to the imposition of Section 10; the differences that emerge following 1920 are not simply an artefact of preexisting differential trends. Secondly, the patterns observed following 1920 help address any concerns regarding effects of Section 10 on the control railroads. If the enactment of Section 10 had somehow benefitted the control railroads, perhaps through increases in bank-firm relationships following the underwriters' resignations from the boards of the treated firms, then any resulting improvement in the control railroads' outcomes could be responsible for our results. Yet the figure generally shows a deterioration in outcomes for the treated railroads following 1920, rather than an improvement in outcomes for the control railroads.

3.7 Alternative Specifications of Main Regressions

One source of concern regarding the main results presented in Table 4 is that the most affected railroads may differ in important ways from the least affected ones. In the paper, we use a variety of approaches to address this concern, including propensity score weighting, and the construction of an alternative control group of industrials and utilities (Table 5). Here, we analyze the robustness of our main estimates to including detailed controls for observable differences in firm characteristics.

Tables 1 and 2 in the paper show that the railroads with close relationships with underwriters on their boards differ from other railroads in important ways: they are somewhat smaller, less levered, and have fewer board interlocks with other railroads. In addition, they have fewer top financiers on their boards. Thus, one concern about our results could be that they are driven by differences in the post-1920 performance of railroads with these characteristics, rather than by Section 10 of the Clayton Act. In order to address this concern, we re-estimate our main equation with the 1913 values of every characteristic that was significantly different between the treated and control firms, interacted with a time trend as well as a break-in-trend for the post-1920 period.

Table A8:
Regressions With Additional Firm Characteristics as Controls

	Tobin's Q	Investment Rate	Interest Rate	Leverage
	(1)	(2)	(3)	(4)
High board undw 1913 × post-1920	-0.072	-0.031	0.004	-0.038
	(0.029)	(0.008)	(0.003)	(0.015)
High board undw 1913 × time trend	0.004	0.001	0.0001	0.004
	(0.003)	(0.001)	(0.0002)	(0.002)
Log(assets)1913 × time trend	-0.009	-0.001	0.0004	-0.001
	(0.003)	(0.001)	(0.0001)	(0.002)
Log(assets)1913 × post-1920	0.068	0.013	-0.002	0.009
	(0.031)	(0.006)	(0.002)	(0.010)
Leverage 1913 × time trend	0.039	-0.0001	0.001	
	(0.013)	(0.003)	(0.001)	
Leverage 1913 × post-1920	-0.220	-0.006	0.009	
	(0.109)	(0.035)	(0.008)	
Mileage 1913 × time trend	0.002	0.0002	-0.0002	-0.0001
	(0.001)	(0.0002)	(0.00004)	(0.001)
Mileage 1913 × post-1920	-0.018	-0.002	0.001	0.002
	(0.007)	(0.002)	(0.000)	(0.004)
Interlocks w/RRs 1913 × time trend	0.0001	-0.00002	-0.00006	0.00002
	(0.0002)	(0.00004)	(0.00002)	(0.0001)
Interlocks w/RRs 1913 × post-1920	-0.001	0.0001	0.00002	-0.002
	(0.002)	(0.0004)	(0.00002)	(0.001)
Underwriters on board 1913 × time trend	-0.001	0.0004	0.0001	0.0004
	(0.001)	(0.0002)	(0.0001)	(0.001)
Underwriters on board 1913 × post-1920	0.003	-0.004	0.0001	-0.003
	(0.006)	(0.003)	(0.001)	(0.006)
Log(lag assets)	-0.119	-0.015	-0.006	0.017
	(0.059)	(0.013)	(0.004)	(0.029)
Constant	3.364	0.320	0.138	0.153
	(1.101)	(0.234)	(0.071)	(0.532)
Observations	1,059	1,264	1,450	1,552
R-squared	0.809	0.201	0.519	0.858
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table reports regressions of the same specification as those reported in Table 4, with the addition of the levels of the 1913 characteristics along which the treated and control railroads differed, interacted with both a post-1920 indicator and with a time trend. In column (4), we exclude 1913 leverage from the regression with leverage as the outcome. All variables are defined in detail in Section 2 of the Appendix. Standard errors are clustered by firm.
Source: Authors' calculations.

Table A8 presents the results. Some of the included characteristics do seem to exert significant effects on the outcome variables, either through a time trend or through a post-1920 level change. However, our main parameters of interest, those associated with the percent board underwriting variable interacted with the post-1920 indicator, displayed in the first row of the table, are essentially unchanged, if not slightly stronger in some cases. These results provide further validating evidence that our main findings are not driven by differences in characteristics that are correlated with the strength of underwriting relationships.

3.8 Inverse Propensity Score Weighted Regressions

In Panel B of Table 5 of the paper we report estimates obtained from inverse propensity score weighted regressions to address concerns that our main estimated effects may reflect differences in observable characteristics between the treated and control railroads. In this section, we provide the details of this estimation, and explore the robustness of the results to alternative specifications.

We use what is commonly termed "inverse probability of treatment weighting" to create a sample in which the distribution of baseline characteristics is independent of the treatment. In the standard notation for the literature, define Z to be an indicator variable denoting membership in the treated group. The propensity score is the probability of being in the treated group conditional on baseline covariates: $e_i = Pr(Z_i = 1|X_i)$. For railroads with the same propensity score, the distribution of baseline characteristics will be the same.

The procedure we use is as follows. First, we estimate the propensity score itself. This is obtained from a probit regression of the treatment (defined as being in the high board underwriting group) on 1913 firm characteristics—specifically, firm size (measured by both assets and mileage) and leverage. The results of the probit regression for the specification used in Panel B of Table 5 are reported in column (1) of Table A9:

Table A9:
Probit Regressions: Propensity Score

	(1)	(2)
Log assets	0.764	0.578
	(0.279)	(0.327)
Mileage	-0.102	-0.048
	(0.091)	(0.104)
Leverage	1.652	0.443
	(1.357)	(1.927)
Board interlocks with industrials		-0.034
		(0.057)
Number of top underwriters on board		0.142
		(0.180)
Interest rate		-49.368
		(29.317)
Observations	70	66
Pseudo R-squared	0.175	0.219
Firm FE	YES	YES
Year FE	YES	YES

Note: This table reports estimates of a probit regression of an indicator variable for inclusion in the high board underwriting group on 1913 railroad characteristics. Standard errors are clustered by firm.
Source: Authors' calculations.

In the next step, we restrict the analysis to the observations in the region of common support in the propensity score distribution.[22] That is, we eliminate railroads in the treated group with propensity score values outside the range of those of the control group, and eliminate those in the control group with scores outside the range of the treated group. This results in the loss of five railroads from the sample, all from the control group—these railroads had propensity scores below the minimum value from the treated group.

Finally, we use the estimated propensity scores to construct weights for each railroad in our main regression. These weights are applied to the treated and control railroads so that both groups resemble the population. The weights are defined as follows: $w_i = \frac{Z_i}{e_i} + \frac{1-Z_i}{1-e_i}$. That is, the treated railroads receive a weight of $w_i = \frac{1}{e_i}$ and the control railroads receive a weight of $w_i = \frac{1}{1-e_i}$. With

[22] The importance of the common support condition is discussed in Heckman, Ichimura and Todd (1997; 1998).

these weights, a least-squares regression will produce consistent estimates of the Average Treatment Effect (see the discussion in Imbens, 2004).

Alternatively, one can apply a different set of weights to obtain the Average Treatment Effect on the Treated (ATT). These weights are defined as $w_i = Z_i + \frac{(1-Z_i)e_i}{1-e_i}$. Rather than weighting both groups to resemble the population as above, this scheme weights the control group to resemble the treatment group. Results obtained with these weights, reported in Table A10, are generally slightly larger than those reported in Table 5, which show the Average Treatment Effect (ATE).

Table A10:
Regressions: Average Treatment Effect on the Treated (ATT)

	Tobin's Q (1)	Investmt rate (2)	Interest rate (3)	Leverage (4)
High underwriting by bankers on board 1913 × post-1920	-0.068	-0.036	0.005	-0.042
	(0.033)	(0.008)	(0.002)	(0.016)
High underwriting by bankers on board 1913 × trend	0.003	0.001	-0.0001	0.004
	(0.004)	(0.001)	(0.0002)	(0.002)
Observations	1,057	1,183	1,382	1,429
R-squared	0.777	0.186	0.449	0.774

Note: This table reports estimates from regressions in which the observations are restricted to the common support in the propensity to be in the treated group, and weighted using inverse propensity scores to produce the Average Treatment Effect on the Treated. Standard errors are clustered by firm.
Source: Authors' calculations.

Finally, we investigate whether our results are robust to alternative specifications of the propensity score. Our propensity score regression utilized a parsimonious specification, with a limited set of only the most important railroad characteristics used to predict membership in the high board underwriting group. In column (2) in Table A9 above, we re-estimate this regression, using a much larger set of firm characteristics—nearly all of the characteristics that differed between the treated and control railroads, including average interest rates. (There is a loss of four observations due to the lack of interest rate data for four railroads for 1913.) With this new specification, the estimated propensity scores are slightly different, and in particular, the region of common support between the treated and control railroads changes somewhat. Restricting the analysis to the region of common support results in the elimination of 8 railroads, all from the control group (all with propensity scores below the minimum value in the treated group).

When we re-estimate our regressions using the weights for the Average Treatment Effects, we obtain the results reported in Table A11. In general, the results are quite similar to those reported in Table 5 in the paper. We conclude that our estimates are robust to alternative specifications of the propensity scores.

Table A11:
Regressions: Average Treatment Effect, Alternative Propensity Scores

	Tobin's Q (1)	Investmt rate (2)	Interest rate (3)	Leverage (4)
High underwriting by bankers on board 1913 × post-1920	-0.056	-0.025	0.002	-0.048
	(0.029)	(0.006)	(0.002)	(0.013)
High underwriting by bankers on board 1913 × trend	0.004	0.001	0.0001	0.004
	(0.003)	(0.001)	(0.0002)	(0.002)
Observations	998	1,103	1,313	1,316
R-squared	0.799	0.212	0.477	0.772

Note: This table reports estimates from regressions in which the the propensity scores are estimated as reported in column (2) of Table A9, the observations are restricted to the common support in the propensity to be in the treated group, and weighted using inverse propensity scores to produce the Average Treatment Effect. Standard errors are clustered by firm.
Source: Authors' calculations.

3.9 Instrumental Variables Approach

3.9.1 First Stage Estimates

Table 6 in the paper reports the results of the second-stage of IV-2SLS regressions. The first-stage regressions corresponding to those results are reported in Tables A12 and A13.

Table A12:
First-Stage Regressions, Railroads: Q, Investment Rates

	Tobin's Q		Investment Rate	
	Pct undw, bankers on board in 1920 × post-1920 (1)	Pct undw, bankers on board in 1920 × trend (2)	Pct undw, bankers on board in 1920 × post-1920 (3)	Pct undw, bankers on board in 1920 × trend (4)
Pct underwriting, bankers on board 1913 × post-1920	0.731	0.541	0.763	0.243
	(0.102)	(0.469)	(0.081)	(0.210)
Pct underwriting, bankers on board 1913 × trend	-0.0004	0.681	-0.001	0.737
	(0.002)	(0.111)	(0.001)	(0.084)
Observations	1,025	1,025	1,224	1,224
R-squared	0.767	0.758	0.809	0.811
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: Standard errors clustered by firm. All specifications include the same controls as those of Table 4.
Source: Authors' calculations.

Table A13:
First-Stage Regressions, Railroads: Interest Rates, Leverage

	Interest Rate		Leverage	
	Pct undw, bankers on board in 1920 × post-1920	Pct undw, bankers on board in 1920 × trend	Pct undw, bankers on board in 1920 × post-1920	Pct undw, bankers on board in 1920 × trend
	(1)	(2)	(3)	(4)
Pct underwriting, bankers on board 1913 × post-1920	0.762	0.586	0.761	0.445
	(0.081)	(0.378)	(0.081)	(0.370)
Pct underwriting, bankers on board 1913 × trend	-0.001	0.701	-0.0002	0.722
	(0.001)	(0.093)	(0.001)	(0.086)
Observations	1,401	1,401	1,470	1,470
R-squared	0.807	0.797	0.809	0.803
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: Standard errors clustered by firm. All specifications include the same controls as those of Table 4.
Source: Authors' calculations.

3.9.2 Magnitude of Bias of Main Results

Section 5.5 of the paper utilizes the intuition from indirect least squares to assess the magnitude of the bias of our main results resulting from endogenous responses between the passing of the Clayton Act in 1914 and the actual implementation of Section 10 in January 1, 1921. In this section, we provide the details for this calculation.

In principle, the instrumental variable estimates of δ_1 in equation (2), presented in Table 6 of the paper, should provide a measure of the effects of the strength of underwriting relationships on railroads' boards in 1920 that is not biased by selective resignations in anticipation of the implementation of Section 10. In contrast, the OLS estimates of θ_1 in equation (1), presented in Table 4 of the paper, provide a biased measure of the true effects of underwriting relationships. Since our econometric model is exactly identified—our problem contains two endogenous variables and two instruments—it is possible use the relationship between these two equations to quantify the magnitude of this bias.

We start by deriving an expression for the relationship between our main estimates, and the true effects, using an argument equivalent to indirect least squares. Note that our main specification in the paper:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \theta_1 &\text{percent underwriting by banks on board } \mathbf{1913}_i \times \text{post1920}_t + \\ &\theta_2 \text{percent underwriting by banks on board } \mathbf{1913}_i \times \text{trend}_t + \nu_{it}, \end{aligned} \tag{1}$$

is the reduced-form of the 2SLS estimates of:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \delta_1 &\text{percent underwriting by banks on board } \mathbf{1920}_i \times \text{post1920}_t + \\ &\delta_2 \text{percent underwriting by banks on board } \mathbf{1920}_i \times \text{trend}_t + \epsilon_{it}, \end{aligned} \tag{2}$$

because we instrument for the endogenous 'percent underwriting by banks on board in 1920' in equation (2) above with the percent underwriting done by the bankers on the railroads' boards in 1913, which determines the two main variables in equation (1) above.

For the purpose of clarity, we will simply the expressions for the remainder of the analysis. Let's express the structural relationship that we estimate using IV as follows:

$$y_i = \alpha + \delta_1 x_1 + \delta_2 x_2 + \nu_i, \tag{3}$$

where x_1 and x_2 represent the two endogenous variables based on the percent underwriting done by bankers on the railroad boards in 1920, and the fixed effects are ignored. In Table 6, we estimate this equation using instruments z_1 and z_2 for x_1 and x_2.

We can then express the first-stage equations of our IV estimation as:

$$x_1 = \pi_{10} + \pi_{11} z_1 + \pi_{12} z_2 + \eta_1 \tag{4}$$

$$x_2 = \pi_{20} + \pi_{21} z_1 + \pi_{22} z_2 + \eta_2 \tag{5}$$

where the instruments z_1 and z_2 are the 1913 board underwriting variable interacted with post-20 and a trend.

We also simplify the reduced-form equation–which, again, is our main specification—as follows:

$$y_i = \theta_0 + \theta_1 z_1 + \theta_1 z_2 + \epsilon_i. \tag{6}$$

Substituting the first-stage equations (4) and (5) into the structural equation (3), we obtain:

$$y_i = (\alpha + \delta_1 \pi_{10} + \delta_2 \pi_{20}) + (\delta_1 \pi_{11} + \delta_2 \pi_{21}) z_1 + (\delta_1 \pi_{12} + \delta_2 \pi_{22}) z_2 + (\delta_1 \eta_1 + \delta_2 \eta_2 + \nu_i). \tag{7}$$

A comparison of equation (7) with equation (6) implies the following relationships between the reduced-form parameters and the structural parameters:

$$\theta_1 = \delta_1 \pi_{11} + \delta_2 \pi_{21} \tag{8}$$

and

$$\theta_2 = \delta_1 \pi_{12} + \delta_2 \pi_{22}. \tag{9}$$

We are primarily interested in equation (8), which relates the reduced-form OLS estimates for the post-1920 effect of the percent underwriting done by bankers on boards in 1913 to the estimated parameters from the first-stage regressions (π_{11} and π_{21}) and our IV-2SLS estimates of the structural parameters δ_1 and δ_2. In the case of only one endogenous variable and one instrument, this equation simplifies to:

$$\theta_1 = \delta_1 \pi_{11}, \tag{10}$$

which implies that the bias between the reduced-form estimates and the structural parameters will simply be given by the first-stage coefficient:

$$\theta_1 / \delta_1 = \pi_{11}. \tag{11}$$

In the case of two endogenous variables and two instruments, as in our setup, the expression for the bias of the reduced-form estimate relative to the structural parameter is a bit more cumbersome:

$$\theta_1/\delta_1 = \pi_{11} + (\delta_2\pi_{21}/\delta_1). \tag{12}$$

In our setup, however, the second term of the right-hand side of equation (12) is empirically very small (primarily because the linear trends have a minor impact on our outcome variables). Table A14 presents estimates of the bias based on both calculations for each of the main outcome variables.

Table A14:
Estimated bias of reduced-form parameters

Outcome (y)	$\widehat{\pi}_{11}$ (1)	$\widehat{\pi}_{21}$ (2)	$\widehat{\delta}_1$ (3)	$\widehat{\delta}_2$ (4)	$\widehat{\pi}_{11} + ((\widehat{\delta}_2 \times \widehat{\pi}_{21})/\widehat{\delta}_1)$ (5)
Tobin's Q	0.7308	0.5412	-0.066	0.0045	0.6939
Investment	0.7630	0.2432	-0.031	0.0011	0.7544
Interest rate	0.7623	0.5863	0.006	0.0003	0.7887
Leverage	0.7614	0.4450	-0.054	0.0011	0.7523

Note: Columns (1) and (2) present the estimates from the first-stage regressions, as shown in the first row of Tables A12 and A13. Column (3) shows the estimates for the 'percent underwriting by bankers on board in 1920 x post-1920' from the IV-2SLS regressions, as reported in the first row of Table 6. Column (4) presents the estimates for the 'percent underwriting by bankers on board in 1920 x trend' from the IV-2SLS regressions, as shown in the second row of Table 6. Column (5) combines the coefficients from the first four columns to estimate θ_1/δ_1 as defined in equation (12) above.

Column (1) in Table A14 shows that our main specifications will produce estimates equivalent to between 73 and 76 percent of the structural values if we use the approximation in equation (11).[23] In column (5) we use instead the full expression for the bias from equation (12), and obtain estimates that range instead from 70 to 79 percent of the structural values. Thus, our conservative estimation strategy understates the true magnitudes by about 27 to 43 percent.

The magnitudes of our estimated effects therefore remain plausible when we take into account the bias from endogenous responses in underwriting relationships in anticipation of the imposition of Section 10. For example, our main (reduced form) estimates indicate that leverage declined by 1.8 percentage points for a railroad of average underwriting strength as a consequence of the Act. If we assume that this represents only 75 percent of the true effect, then the leverage ratio would have declined by 2.4 percentage points. This correction suggests that leverage declined by almost 7 percent relative to mean 1920 levels—this is clearly higher than the 5.2 percent decline relative to the mean that we obtain from the reduced-form regressions, but certainly not an implausible magnitude.

Finally, we note that the estimated coefficients from the first-stage regressions summarized in column (1) of Table A14 differ somewhat from the ratio of the reduced-form coefficients θ_1 presented in Table 4 to the IV-2SLS estimates of δ_1 reported in Table 6. (This ratio varies between 67 and 83 percent, depending on the outcome variable.) The reason for this discrepancy is that the sample in Table 4 differs somewhat from the sample used in Table 6. Specifically, the reduced-form

[23]Note that our first-stage regressions produce different estimated coefficients for the same instruments solely because the sample changes somewhat across outcome variables.

estimates in Table 4 do not limit the sample to those railroads for which we observe the percent of underwriting done by bankers on their board in 1920. When we estimate both specifications utilizing a consistent sample, the ratios between the reduced-form estimates and the IV estimates match perfectly those displayed in column (1) of Table A14 (results not shown).

4 Additional Topics

4.1 Demand Changes and the Railroad Industry

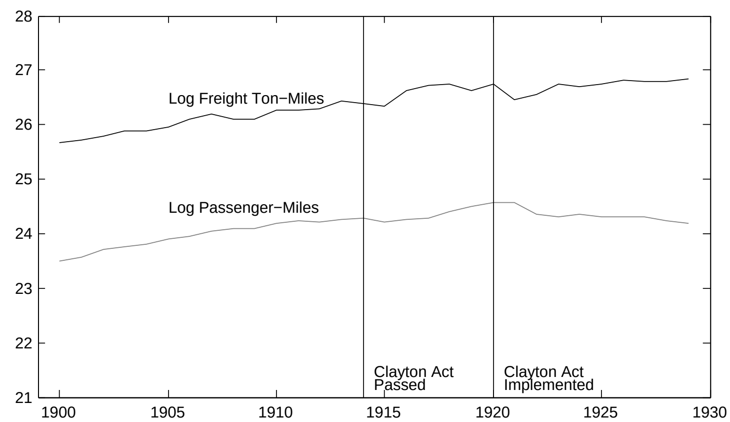

Figure 2:
Evolution of demand: freight and passengers

Note: The figure presents total freight traffic for all railroads in the United States, measured as log ton-miles, and total passenger traffic, measured as log passenger-miles. Data collected from the annual reports of the ICC.

We study whether a differential contraction in demand faced by our treatment group of railroads could be responsible for the effects we ascribe to Section 10 of the Clayton Act. Indeed, the proliferation of motor vehicles in the 1920s, and the expansion of roads and highways, could have reduced the demand for railroad services.

We begin by noting that the demand for most railroad services was quite robust overall in the 1920s. Figure 2 shows that freight traffic, which accounted for about 70 percent of the industry's revenues, continued to grow throughout this period.[24] However, passenger traffic on railroads peaked around 1920, and it declined slightly in the remainder of the decade. If the treated railroads had been differentially exposed to the decline in the demand for passenger transportation, this could explain our results.

At the time of the imposition of Section 10, however, the high board underwriting railroads were not disproportionately dependent on passenger revenues: they received 20.9 percent of their revenues from passenger traffic in 1920, compared to 21.4 percent for other railroads, and the difference is not statistically significant ($p = 0.872$). But the decline in passenger traffic may not have been uniform across all regions of the United States; in some parts of the country the road

[24]Even by the end of the 1920s, the volume of freight transported on trucks was negligible relative to the railroads' volumes, perhaps 5 percent. No official data exists for truck freight volumes for this period, but estimates are presented in Moulton (1933: 18). The 1920s were also a period of high profits for the railroad industry. For example, the average return on assets in our sample rose from 3.9 percent in 1922 to 5.4 percent in 1928 and 1929.

network was much better developed and automobiles accounted for a greater share of passenger transportation.

Table A15:
Effects of Demand Changes Faced By Railroad Industry

	Tobin's Q (1)	Investment rate (2)	Interest rate (3)	Leverage (4)
A: Proliferation of Automobiles in Region				
Pct underwriting by bankers on board 1913 × post-1920	-0.047	-0.024	0.005	-0.040
	(0.023)	(0.008)	(0.003)	(0.015)
Pct underwriting by bankers on board 1913 × trend	0.003	0.001	0.000	0.001
	(0.003)	(0.001)	(0.000)	(0.002)
Automobiles per person 1920 × post-1920	-0.256	-0.246	0.005	0.011
	(0.277)	(0.120)	(0.034)	(0.265)
Automobiles per person 1920 × trend	-0.029	0.004	-0.004	0.065
	(0.044)	(0.009)	(0.004)	(0.038)
Observations	1,059	1,249	1,432	1,509
R-squared	0.779	0.195	0.473	0.866
B: Development of State Highway Network in Region				
Pct underwriting by bankers on board 1913 × post-1920	-0.054	-0.029	0.005	-0.047
	(0.023)	(0.009)	(0.003)	(0.016)
Pct underwriting by bankers on board 1913 × trend	0.005	0.001	0.0001	0.002
	(0.003)	(0.001)	(0.0002)	(0.002)
(Miles of road in region / miles of track) × post-1920	-0.001	-0.0001	-0.000006	-0.0002
	(0.001)	(0.00007)	(0.00002)	(0.0001)
(Miles of road in region / miles of track) × trend	0.0002	0.000003	-0.000002	0.00003*=
	(0.00008)	(0.000005)	(0.000002)	(0.00001)
Observations	1,051	1,249	1,432	1,492
R-squared	0.775	0.194	0.472	0.845

Note: This table presents estimates of modified versions of equation (1) in the paper. Panel A includes controls for the average number of automobiles per person in the states within a railroad's ICC region, interacted with a post-1920 indicator and with a time trend. Panel B includes controls for a measure of total state highway mileage within a railroad's ICC region, divided by the total mileage operated by the railroad itself, interacted with a post-1920 indicator and with a time trend. All specifications include year and firm fixed effects, as well as the log of lagged assets and a constant term. Standard errors clustered by firm. *Source:* Authors' calculations.

To test whether the treated railroads faced differential declines in passenger revenues due to greater levels of motor vehicle usage, we construct two measures related to the penetration of automobiles in a railroad's market, defined as its ICC region. First, we use the number of automobile registrations per person in 1920 to capture the variation in the adoption of cars at the time Section 10 was enforced, as well as the expected growth in automobile utilization over the following decade.[25] The second measure is the total mileage of state highways in a railroad's region, scaled by the mileage of the railroad, in 1923 (the earliest available year). The size of the automobile transportation network relative to the railroad's own network provides a measure for the substitutability in the transportation of individuals between these two competing technologies.

These measures were both obtained from the volume *Highway Statistics: Summary to 1955* published by the Bureau of Public Roads of the U.S. Department of Commerce (Washington: US Government Printing Office, 1957). The data on automobiles is automobile registrations in 1920, presented in Table MV-201 (page 18). State highway miles for 1923 (the earliest available year) were obtained from Table SM-200 (page 107). Both are reported at the state level. We aggregated data for individual states up to the entire ICC region using the map presented in the ICC's *Annual Report on the Statistics of Railways in the United States for the Year Ended December 31, 1929*, pg. xxiv. For states whose boundaries encompassed two ICC regions, we allocated half of their automobiles or roads to each of the two. To scale automobile registrations by population, we used the 1920 state populations as reported in the federal census, and allocated them to the ICC regions in the same way.

Table A15 presents the estimates of a modified version of equation (1) that includes these proxies for demand changes, interacted with a post-1920 indicator and with a trend. Panel A includes the automobile variable, and Panel B utilizes the roads variable instead. Both measures do indeed seem to be correlated with worse railroad outcomes, particularly a fall in Tobin's Q and in investment in the post-1920 period. Yet the estimated parameters associated with our underwriting variable remain essentially unchanged when these variables are included. The effects on railroads with close relationships with underwriters we ascribe to Section 10 are unlikely to be the product of changes in demand.

4.2 World War I and Federal Operations of Railroads

In response to unprecedented problems encountered by railroads following the onset of World War I, the federal government assumed control over the operations of the industry. This section discusses the implications of that episode for the analysis of the paper.

4.2.1 Wartime Problems and Federal Response

World War I placed the railway system of the United States under extreme pressure. Freight volumes surged, interchanges between different lines became congested, rail cars sat idle in some cities while there were shortages in others, and several eastern seaports became paralyzed with rail cars backed up for miles (Martin, 1971). This congestion posed acute problems for the war effort, as high priority shipments were held up behind an enormous backlog of often less important shipments. Relieving this congestion required a coordinated response among all railroads. In 1917 the railroads attempted their own voluntary effort at such a response, but they could not effectively resolve the problem of how to compensate a railroad for traffic held back or diverted to other lines, in part due to the restrictions of antitrust laws. In addition, the railroads' own officials sometimes refused to take actions that hampered their divisions' self-interest (Kerr, 1968:57). In response, the railroads worked closely with the government to design and implement a system of federal operations.

[25] The number of cars per person in 1920 ranged from around 0.05 in the Northeast to 0.11 in the West. We use the 1920 value in part because automobile use may have responded endogenously to changes in railroad service over the following decade.

In March 1918, Congress passed the Federal Control Act, which gave the federal government control over railway operations, including the power to set rates and to allocate traffic. As this was understood to involve diverting traffic away from some railroads, the Act included a provision guaranteeing each railroad compensation equal to its "average railway operating income for the three years ended June 30, 1917" (Section 1). The federal government did not take ownership of the railroads; they remained in private hands. Effectively, the government leased the railroads from their owners. The membership of the U.S. Railroad Administration was dominated by interests friendly to the railroads themselves, and the period of federal operation was generally quite favorable to the railroads, relative to shippers, who had previously enjoyed much greater influence over rate setting and traffic patterns (Kerr, 1968).

Control over operations was returned to the individual railroads on March 1, 1920 in the Transportation Act of 1920. In order to ease the transition, the Act included a provision (Section 209) that offered continued guarantees of the level of operating income of the railroads equal to half the amount they were given during the War, for the initial six month period after federal control was ended. The federal government also offered loans to railroads during the two-year period following federal control (Section 210).

4.2.2 Implications for the Analysis of the Role of Bankers

Government control of railroads could pose a threat to our identification strategy if it led those railroads with stronger connections to their underwriters to perform better during the war period, relative to others. If this were the case, the lower relative performance of the railroads with stronger ties to their bankers in the post-1920 period could be due to the return to normalcy once railroads were returned to private hands, instead of representing the negative effects from the restrictions of Section 10 of the Clayton Act. In this section, we utilize newly collected data on the period of federal operations to rule out this potential concern.

The main financial effects of federal control arose from the guaranteed level of income, and the financial support offered in 1920-21 to ease the transition back to private control. The amount of this guarantee and financial support was based on the income generated by each railroad over the three years up to 1917. If railroads with close ties to bankers had performed better than other railroads over that period, then the level of guaranteed income they received during the War years would have been higher than that of other railroads, when perhaps in the absence of the guarantee and of federal control, their income during the War might have been different. It is also possible that the political influence of financiers may have enabled them to obtain more favorable treatment for their client railroads under federal control, for example in the form of higher guaranteed income. If either was true, then our estimates of the post-1920 difference in performance between railroads with and without close ties to bankers would be biased upwards, relative to the pre-1920 years.

Fortunately, the U.S. Railroad Administration and the Interstate Commerce Commission kept many detailed, firm-level records of the various subsidies and guarantees provided to railroads. We use these data to test directly for any differential affects among railroads with close ties to bankers.

Panel A of Table A16 investigates whether railroads with close ties to financiers received more favorable treatment during the period of federal operation. The first row presents the level of income guaranteed to the railroads pursuant to Section 1 of the Federal Control Act, as a fraction of 1918 total assets. The data indicate that in fact there were no differences in the level of income guaranteed to railroads with the highest reliance on bankers on their boards for debt underwriting, relative to other railroads, and the correlation between the guaranteed income and the fraction of underwriting done by bankers on the railroads' boards was essentially zero.

One might also imagine that bankers with close ties to railroads could have been able to manipulate the computation of the amount of guaranteed income their clients would receive. In 1921, the ICC carefully audited and corrected the historical income levels originally certified to the U.S. Railroad Administration. If the initial guarantees were manipulated by bankers, then the corrected figures should have been revised downward to a greater degree, but the second row of Panel A of Table A16 shows that this was not the case. There was no difference in the amount by which the guarantee, again expressed as a fraction of 1918 assets, was revised for railroads in the high board underwriting group relative to the others, and the corrections are uncorrelated with the percent of underwriting done by bankers on firms' boards.

Table A16:
Period of Federal Operations

	Mean, High Board Underwriting Railroads (1)	Mean, Other Railroads (2)	Difference, High Board Underwriting vs. Other (3)	Correlation, Pct. Board Underwriting, 1913 (4)
A. Federal Operations (1918-20)				
Guaranteed income, fraction of 1918 assets	0.036	0.036	0.0001	-0.029
	(0.002)	(0.002)	(0.003)	
Revision to income guarantee, fraction of 1918 assets	-0.0001	-0.0002	0.00002	0.009
	(0.0002)	(0.0002)	(0.0003)	
B. Transition Period (1920-21)				
Income support, 1920, fraction of 1920 assets	0.016	0.017	-0.0008	-0.120
	(0.002)	(0.001)	(0.003)	
Borrowed from federal government (binary), 1920	0.438	0.410	0.027	0.055
	(0.128)	(0.080)	(0.149)	
Amount borrowed from gov't, fraction of 1920 assets	0.017	0.028	-0.011	-0.096
	(0.007)	(0.017)	(0.027)	

Note: Columns (1) and (2) report means for railroads in the high board underwriting group, and those not in that group, respectively. Column (3) reports differences in means for the two groups. Column (4) reports the correlation coefficient between the row variable and the main treatment variable in the empirical analysis of the paper, the percent underwriting by the bankers on firms' boards up to 1913.
Source: Authors' calculations.

The comparison of means may not fully reflect the potential channels through which railroads with strong ties to influential bankers may have benefited under federal control and the subsequent transition period. In order to address this possibility more thoroughly, we explicitly test for differences in the differences in our outcome variables between railroads with and without close ties to bankers during this period.

Table A17 presents results of regressions of the same specification as those presented in Table 4, but with the addition of an interaction term between the percent board underwriting and an indicator for the period of federal control and transition (1918-21). If railroads with close ties to bankers benefited differentially from federal control, then this interaction should be positive. Yet for every outcome the estimate is very small and not statistically significant. More importantly, the inclusion of the interaction does not affect the magnitude of the main effects.[26] We conclude that differential effects of the period of federal control are not responsible for our main results.

It is also possible that influential bankers could have lobbied for higher levels of the support provided by the 1920 Transportation Act for the period of transition from federal control. Yet Panel B of Table A16 shows that railroads in the high board underwriting group received no more income support in 1920, were no more likely to obtain a loan from the federal government, and actually borrowed less from the federal government.

Table A17:
Regressions: Period of Federal Operations

	Railroads			
	Tobin's Q	Invstmt Rate	Interest Rate	Leverage
	(1)	(2)	(3)	(4)
Pct board undw 1913 × post-1920	-0.044	-0.021	0.004	-0.047
	(0.023)	(0.008)	(0.003)	(0.018)
Pct board undw 1913 × fed control and transition (1918-21)	0.0003	0.0003	-0.0008	-0.004
	(0.024)	(0.006)	(0.003)	(0.014)
Pct board undw 1913 × time trend	0.003	0.001	0.0002	0.001
	(0.003)	(0.001)	(0.0002)	(0.002)
Log(lag assets)	-0.118	-0.015	-0.004	0.002
	(0.053)	(0.011)	(0.005)	(0.029)
Constant	3.207	0.299	0.121	0.409
	(1.002)	(0.209)	(0.084)	(0.532)
Observations	1,084	1,286	1,475	1,552
R-squared	0.776	0.192	0.465	0.856
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table reports estimates of regressions similar to those in Table 4, but with the addition of an interaction with the period of federal operations. Standard errors clustered by firm.
Source: Authors' calculations.

Finally, the Transportation Act of 1920 also revised some important railroad regulations (see Cunningham, 1922, for a summary). In particular, it included provisions more favorable toward consolidations among railroads, signalling a change in antitrust policy. It also included new provisions relative to the regulation of rate setting which held that the determination of permissible rates would take into account the returns earned by railroad. More specifically, the new regulations held that rates should be set so that railroads earned a fair return on their investment, which for the first two years of the Act was set at 5.5 percent. It also held that one half of any income earned by railroads in excess of this amount would be 'recaptured' by the federal government, for the purpose of establishing a 'contingent fund' for railroads. Partly inspired by the lessons learned under federal control, these provisions intended to turn railroading into even more of a regulated

[26] The main estimate on the interest rate barely misses significance at the 10 percent level. All other estimated effect remain statistically significant.

industry, permitting further cartelization for reasons of efficiency and also so that weaker roads could absorbed by the stronger ones, while at the same time ensuring that the resulting levels of profitability did not become excessive.

It is possible that these provisions could have differentially affected railroads with strong connections to their underwriters, relative to others. However, in spite of the significance that is often attached to the Transportation Act of 1920 as a change in regulatory doctrine, the provisions regarding consolidations and rate recaptures had little effect. As a practical matter, regulations under the Transportation Act were little changed from the pre-War regime. For a number of reasons, including the fact that the ICC maintained a fairly strict interpretation of antitrust doctrine, the wave of consolidations anticipated by the framers of the Transportation Act did not occur. More importantly, although the ICC calculated the "recapture liability" owed by the most profitable railroads throughout the 1920s, disputes over the accounting values used to calculate returns, and legal challenges by the railroads, successfully blocked the collection of the excess income. In fact, through the end of 1931, only 2.7 percent of the railroad industry's total recapture liability had actually been collected (Moulton, 1933: 384). Given the very minor nature of the regulatory changes effected by the Transportation Act, we conclude that they are unlikely to be responsible for our results.

4.3 Alternative Sources of Finance

In the paper, we focus exclusively on bond finance, which were indisputably the main source of external finance for American railroads in the early twentieth century. In this section, we briefly discuss the relevance of other sources of funds for these firms.

Other sources of debt finance, such as bank loans, had a very minor impact on the railroads' capital structure during our period. Prohibitions against branching constrained the size of commercial banks, which could not satisfy on their own the very large capital needs of railroads. Moreover, the market for syndicated loans had not yet developed. To gauge the importance of these other sources of credit, we obtain information on notes, bills and loans payable from the firms' balance sheets.[27] In 1913, these additional sources of debt financing accounted for only 0.59 percent of total assets for those railroads in the high underwriting group, and for 1.02 percent of total assets for all other railroads.

The main alternative to corporate bonds to raise additional external finance for new investments was the issuance of equity. During the years 1905-29, 16 of the 71 sample companies issued shares of common stock to the public, and 3 sample companies issued shares of preferred stock to the public. The total value of the issuance of common stock was $571.5 million, and the total value of the issuance of preferred stock was $74.73 million.[28] In contrast, the total issuance of bonds among sample companies was much larger, $7.2 billion.

[27] A limitation of these data is that these accounting items are often reported together, and sometimes they also include accounts payable. Moreover, only the total stock at a point in time is observed, and there is no available information on the identity of the providers of these sources of capital.

[28] These totals exclude shares issued in exchange for other outstanding securities. Such exchanges included the conversion of convertible debt into stock, and the conversion of preferred stock into common stock. The totals also exclude shares granted to the stockholders in the form of stock dividends. Finally, they also exclude new shares issued as part of restructurings out of bankruptcy, which were typically exchanged for existing debt and/or equity shares. There are nine such restructurings of railroads within our sample.

In addition, consistent with the preemptive rights granted to shareholders in many states' corporation laws, new share issuances were offered to the existing shareholders. Unlike most debt issuance, in which an underwriter distributed the shares to investors, with the issuance of equity, the shareholders were typically granted the right to subscribe directly from the company. The terms of one railroad's offering in 1910, as reported in the *Wall Street Journal,* illustrate the typical terms under which subscriptions were offered: "Stockholders of the Lehigh Valley Railroad...will have the privilege of subscribing for new stock to the extent of 50 percent of their holdings...payment is to be made in four equal installments or in full. The first installment of 25 percent, or $12.50 per share, is payable at the time of making the subscription" ("Lehigh Valley Capital Stock is increased to $80,000,000," 23 June 1910). The use of rights offerings implies that equity finance did not present the same opportunity for a monitoring relationship with an underwriter.

The restrictions of Section 10 did not exclusively apply to corporate bonds, and it is possible that the regulation may have disrupted any relationships that railroads could have established with the providers of loans, or the underwriters of equity issuances. The limited use of these sources of external funds during our period suggest that any such effects on firm outcomes may have been relatively minor.

4.4 The Resignations of J.P. Morgan & Co. Partners of January 1914

As mentioned in the paper, in January 1914, J.P. Morgan & Company announced that its partners were voluntarily resigning from 30 directorships, in an effort to forestall new regulations on the role of financiers on corporate boards. Some of these resignations were the focus of Cantillo Simon (1998), who compared the cumulative abnormal returns of those companies from which boards the Morgan partners stepped down relative to those of firms in which they remained. In this section, we investigate the determinants of the Morgan resignations.

A substantial fraction of the companies from which the Morgan partners resigned were not publicly traded and disclosed scant financial information. Of the 30 board seats, seven were with commercial banks or trust companies, and 10 were with minor subsidiary companies of railroads, such as the "New England Steamship Company" which was controlled by the New Haven. Effectively, the Morgan partners resigned from eight major railroads, and remained on the boards of nine major railroads, all of which are included in our sample. (They also resigned from three industrials and two public utilities, but given the small numbers involved, we focus only on the railroads). Keeping in mind the limitations from the small sample size, next we study Morgan's decision of whether to resign or remain on the boards of these 17 railroads.

Cantillo Simon's (1989) analysis used the stock market's response to the resignations to make inferences about the value of ties to J.P. Morgan. Yet any estimates of the value of firm-bank relationships obtained from endogenous resignations may confound the pure effect of these relationships with selection bias. In what follows, we investigate whether the Morgan resignations were quasi-random, or whether the firm behaved strategically and resigned from a selected group of railroads.

The first characteristic we examine is the strength of the railroads' relationship with the firm, as measured by the share of the railroads' underwriting that had been performed by J.P. Morgan up to 1913. As shown in Panel A of Table A18, J.P. Morgan & Co. had conducted more than 80 percent of the underwriting of the railroads where they remained, compared to just over 30 percent for the railroads where they resigned, and the large difference of fifty percentage points is statistically significant. Thus, the Morgan partners resigned from the boards of railroads with whom they did not have a strong underwriting relationship.

Table A18:
Selection: Morgan Resignations From Railroad Boards, 1914

	Morgan Partners Resigned	Morgan Partners Did Not Resign	Difference
	(1)	(2)	(3)
A: Strength of Underwriting Relationship With J.P. Morgan & Co.			
Morgan underwriting share up to 1913	0.322	0.821	-0.499
	[0.421]	[0.341]	(0.184)
B: Firm Characteristics and Outcomes, 1913			
Log assets	18.832	19.462	-0.899
	[1.100]	[0.727]	(0.417)
Tobin's Q	0.788	0.971	-0.113
	[0.163]	[0.278]	(0.133)
Investment	0.042	0.037	0.011
	[0.110]	[0.067]	(0.040)
Interest rate	0.040	0.040	0.004
	[0.005]	[0.008]	(0.004)
Leverage	0.470	0.474	-0.041
	[0.082]	[0.111]	(0.050)

Note: This table reports comparisons between firms from which Morgan parters resigned in early 1914 ($n = 8$) and firms where Morgan partners were represented on the board in 1914 and did not resign ($n = 9$). In one case, the Reading, one Morgan partner resigned while another remained on the board, and this railroad is included among those where the Morgan partners did not resign. Columns (1) and (2) present means and standard deviations in brackets for the two groups. Column (3) presents the difference between the group means, with standard error estimated from a regression with robust standard errors. Finally column (4) presents the coefficient of an indicator for whether the Morgan partners resigned × a linear trend variable estimated in a regression with year and firm fixed effects from 1905 to 1913, and reports standard errors clustered by firm.
Source: Authors' calculations.

Panel B of the table presents comparisons of other railroad characteristics and outcomes prior to the resignations. The two groups had similar leverage levels and average interest rates. However, the railroads from which the Morgan partners resigned were smaller, and had lower valuations.

This analysis indicates that the Morgan resignations may not present a clean opportunity to make causal inferences on the effects of severing relationships between banks and firms. The resignations were chosen strategically, and the factors influencing the decision to resign would confound any comparisons of firms' outcomes following the resignations. Specifically, such study may understate the value of firm-bank relationships since the Morgan partners resigned from the railroads with which they had a weak relationship, but it may also lead to overestimating the true effects—especially for Tobin's Q—because these firms were already experiencing a stronger decline in their market performance prior to the resignations. Without a valid instrument for the endogenous decisions made by the bank, it would be difficult to know which of these two effects dominates.

In Section 5.5 of the paper we study the endogenous resignations of underwriters from the boards of railroads from 1914 to 1920, in anticipation of the enforcement of Section 10 of the Clayton Act. Our instrumental variables approach allows us to sign the direction of the selection bias. We find that bankers chose to remain on the boards of stronger firms–that is, not taking selection into account would have led us to underestimate the value of relationships with underwriters.

These two instances of endogenous severance of bank-firm relationships suggest that, when given a choice, banks may wish to remain on the boards of their strongest or most highly valued issuers, perhaps out of concern for their reputation. Prior to 1914, however, the Morgan partners also tended to remain on the boards of the railroads with which they had particularly close relationships, and step down from the others. Resigning from the boards of railroads for which J.P. Morgan had not done much underwriting would likely create less of a disruption. This suggests that these strategic resignations may have also been influenced by a desire to minimize their impact on railroads, while still drawing enough public attention to their cause.

These two exercises also highlight the challenges in establishing general conclusions on the sources of selection. The Morgan resignations were primarily intended to stop the rule, not to respond to it. In contrast, the breakup of relationships that occurred from 1914 to 1920 were likely in anticipation of the eventual imposition of Section 10. Thus, we would expect the sources of selection to differ between these two events.

5 Appendix References

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