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The investment banks represented on a railroad's board gained access to the firm's private information.<sup>8</sup> Thus enabled to act as monitors, these banker-directors often led the underwriting syndicates for the firm's debt issues. However, the presence bankers on railroad boards was nearly ubiquitous, and in some cases board seats were not accompanied by an underwriting relationship. These latter banker-directors may have been sought as financial or industry experts.<sup>9</sup>

In the first decade of the twentieth century, the role of financiers in the economy became politically controversial, particularly following the Panic of 1907.<sup>10</sup> In 1912, the US House of Representatives authorized an extensive investigation of the "money trust" by a committee headed by Representative Arsène P. Pujo. The Pujo Committee concluded that a small and collusive group of elite New York securities underwriters held total control over access to finance, at least among major corporations. The committee's report highlighted the conflicts of interest posed

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<sup>7</sup>The transcript of this 1913 conversation appears in T.W. Lamont Papers, Folder 84-16, Baker Library, Harvard University.

<sup>8</sup>Access was not only facilitated through board meetings but it was sometimes contractually mandated. For example, a 1913 agreement between J.P. Morgan & Company, Deutsche Bank and the Northern Pacific required the railroad to give the bankers "full information regarding the accounts, operations and conditions of the company and its property" (Pujo Committee, 1913a: 1342).

<sup>9</sup>In Appendix Section 3.3, we show that railroads that would benefit more from informed bankers were more likely to have top investment bankers on their boards, even when these banks did not provide the firm with underwriting services.

<sup>10</sup>On the role of financial intermediaries during this panic, see Frydman, Hilt and Zhou (2015).



by the extensive network of directorships held by elite financiers, and argued that their power and influence was dangerous for society. As if to demonstrate the committee's point, during the hearings the partners of J.P. Morgan quietly attempted to influence public opinion by planting articles in prominent newspapers.<sup>11</sup>

The committee's report argued that the vast influence of financiers was "destructive of competition," and resulted in the "exaction of excessive profits," but it presented little evidence of resulting economic harm, or even specific conjectures regarding the form such harm could take (Pujo Committee, 1913b: 161, 133). The potential consequences of the conflicts of interest identified by the Pujo Committee were described by Brandeis in an influential series of essays published as the book *Other People's Money and How the Bankers Use it* (1914). Brandeis argued that banker-directors had a malign influence on railroads' financial performance, "both through issuing of securities which ought not to be issued, and from selling them at a price less favorable to the company than should have been obtained." Bankers profited from enabling railroads to make unproductive acquisitions, forming "unwise combinations" motivated by "personal ambition and greed," by issuing costly securities (Brandeis, 1914: 183, 197). Drawing particularly on the experience of the New Haven, a long-successful railroad that suffered significant losses after a series of unprofitable acquisitions undertaken while J.P. Morgan himself sat on its board, Brandeis argued that inefficient empire building, and poor returns, were the "natural result" of banker management (Brandeis, 1914: 196).

There is no doubt that in 1913 financiers held an extraordinary degree of influence over American railroads, and that their positions as banker-directors created significant conflicts of interest. Yet their presence on railroads' boards would also have enabled them to gain access to information and monitor management, potentially helping to resolve financial frictions and enabling their affiliated railroads to undertake worthwhile investments. Whether or not the costs outweighed the benefits—and whether or not the experience of the New Haven was typical, or an aberration—is an empirical question, requiring careful analysis of all railroads' outcomes, and a credible source of variation in railroads' relationships with financiers. The restrictions on underwriting relationships imposed by the Clayton Act provide a unique opportunity to examine this question.

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<sup>11</sup>The firm sent an employee to Washington "to arrange for propaganda to issue from that point," provided articles to syndicated columnists, and initiated negotiations to acquire newspapers, including the *Washington Post*. "Memorandum for H.P.D[avison], Publicity Matters," T.W. Lamont Papers, Folder 125-10, Baker Library, Harvard University.

## 2.2 The Clayton Act of 1914

Both the Pujo Committee's report and Brandeis called for Congress to enact far-reaching regulations on the role of financiers in the economy. Brandeis in particular called for a prohibition of contracts between railroads and firms such as investment banks or equipment suppliers represented on their boards, arguing that this would eliminate conflicts of interest. In an effort to forestall such regulation, the partners of J.P. Morgan & Company announced they would resign from 30 directorships at the beginning of January 1914.<sup>12</sup> Those resignations, however, did not deter efforts to impose new regulations on bankers. In January 1914 President Woodrow Wilson gave a special address to Congress calling for new antitrust legislation to address "the great harm and injustice" done to railroad systems, which had had their "interests subordinated to the interests of the men who financed them" (Wilson 1914, vol 29, p. 155).

In October 1914, Congress passed the Clayton Antitrust Act. Besides its many clauses intended to clarify and strengthen the Sherman Antitrust Act of 1890, Section 10 explicitly prohibited transactions between railroads and firms with which they had a director or executive in common, and specifically forbade railroads from having "dealings in securities" with any financial institution that had a partner or director on the railroads' board.<sup>13</sup> The law did not actually prohibit financiers from sitting on railroad boards, but instead forbade banks that held board seats from underwriting securities for those railroads. Bankers who sat on the boards of railroads could choose to remain on those boards and cease to act as their underwriters, or they could continue to underwrite securities for the railroads, and resign from their boards. We therefore design our empirical strategy so as not to confound the estimates of the effects of the regulatory change on firm outcomes with the endogenous choice of underwriters of whether to step down from corporate boards. Consistent with Brandeis' proposal, the Act outlawed self-dealing by directors representing firms other than investment banks as well; we discuss the effects of these other prohibitions in detail in Section 6.3.

The implementation of Section 10 of the Clayton Act did not occur immediately in 1914. A two-year delay in its enactment was written into the statute, so that the Interstate Commerce

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<sup>12</sup>See "Morgan Firm Out From Thirty Boards," and "May Modify Legislation: Concessions by Financiers Likely to Stop Radical Action by Congress," *New York Times*, 3 January 1914. In the Appendix, we discuss the selected nature of these resignations, and argue that this episode does not offer an opportunity to determine the value of bank-firm relationships.

<sup>13</sup>Oct. 15, 1914, ch. 323, 38 Stat. 730. After lobbying by the railroads, this term was finally repealed in 1988.

Commission (ICC) could develop the capacity to enforce it (see House Judiciary Committee, 1917). Further delays were then enacted in 1916, and in later years—see Appendix Section 3.1 for a detailed history of these decisions.<sup>14</sup> Although Congress passed an additional one-year delay in December 18, 1920, President Wilson vetoed it on December 30, and Section 10 finally went into effect on January 1, 1921. Thus, the timing of the implementation of the reform was generally unexpected, and exogenous to firm outcomes. The late implementation of Section 10 also helps ensure that our empirical findings are not confounded by the other antitrust provisions of the Clayton Act, which were enacted in 1914.

Throughout this process, the railroad industry—the very firms that the legislation sought to protect—argued strenuously against Section 10, stating that they “ought not to be required to elect whether or not they will cut themselves off from sources of money supply or will leave off of their boards some of their strongest directors” (New York Central Railroad, 1921: 16). Brandeis himself confided that railroad executives often supported the notion that the directors of supply companies ought not to serve on their boards, but that they “balked when it came to the banking directors.”<sup>15</sup> When Section 10 finally was implemented, it was “deplored by railway officials.”<sup>16</sup>

### 2.3 Effects of Section 10 on Bank-Firm Relationships

We begin our analysis of the effects of Section 10 by documenting the impact of the regulation on railroads’ relationships with their underwriters. The extent to which Section 10 was actually obeyed is explored in Figure 1, which presents a three-year moving average of the percentage of the value of corporate bonds underwritten by investment banks represented on the boards of NYSE-listed railroads, and industrial firms and utilities, from 1907-1929.<sup>17</sup> The figure indicates that prior to 1913, around 60 percent of railroad debt, and around 45 percent of the debt of industrials

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<sup>14</sup>The effects of World War I severely disrupted the operations of American railroads, and strengthened their arguments for these delays. In 1918, the federal government assumed control over the industry, leasing the railroads’ assets in exchange for a guaranteed rate of return based on historical averages. Federal control, which was welcomed by the industry, suspended many railroad regulations and coordinated the operations of individual firms to serve the needs of the war effort. Control was restored to the railroads themselves in March 1, 1920. In Appendix Section 4.1.2 we show that government control over railroad operations cannot explain our main findings.

<sup>15</sup>Brandeis made this statement to Thomas Lamont, also mentioning that the President of the Atchison, Topeka and Santa Fe railroad objected to prohibitions on underwriter directorships on the grounds of the “very large sums of money that the Atchison Road has saved in its financing” through the work of a J.P. Morgan partner on its board (T.W. Lamont Papers, Folder 84-16, Baker Library, Harvard University).

<sup>16</sup>“Many Changes Soon in Railroad Boards,” *New York Times*, 23 January 1921.

<sup>17</sup>Our findings are robust to restricting the sample of industrials and utilities to those with above-median leverage, a set of firms that are arguably more comparable in terms of financing needs to the railroads in our sample.

and utilities was underwritten by bankers with board representation. Following 1914, when the Clayton Act was passed, the ratio declined for railroads, reaching a level of 35 percent by 1920, while the decline for industrials and utilities was somewhat smaller. Importantly, following the implementation of Section 10 in 1921 underwriting by bankers on railroad boards fell to almost zero (the use of a moving average obscures the sharp fall to zero in 1921 and near-zero in the following years). In contrast, there was no equivalent shift in the underwriting done by bankers represented on the boards of industrials and utilities, which were not subject to Section 10, indicating that the change among railroads was in fact due to that statute.

Section 10 did not mandate that underwriters step down from boards; banker-directors could comply by ceasing to provide underwriting services. However, many railroads and underwriters apparently concluded that the optimal response was to sever board interlocks. The resignations of prominent bankers from major railroads' boards in the months following the implementation of the regulation attracted considerable attention in the press.<sup>18</sup> The overall effect of the imposition of Section 10 on the composition of boards is presented in Figure 2, which plots the number of major investment banks represented on the boards of all sample railroads, and industrials and utilities, at two- to five-year intervals from 1905 to 1925.<sup>19</sup> In 1913, NYSE-listed railroads had an average of 2.2 such institutions represented on their boards, and industrial firms and utilities had about one. Following the Morgan resignations and the passage of the Clayton Act in 1914, the number for railroads fell modestly to an average of about two in 1920. But the implementation of Section 10 in 1921 caused a substantial, discreet change in underwriter representation on railroad boards: it fell by about 50 percent.<sup>20</sup> In contrast, industrial firms and utilities saw very little change throughout the post-1914 period.

Finally 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 1 presents the evolution of a Herfindahl-Hirschman index of concentration of underwriting services by lead

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<sup>18</sup>“Many Changes Soon in Railroad Boards,” *New York Times*, 23 January 1921. On the resignation of important bankers, see also “Schiff and Kahn Quit Union Pacific,” “Mellon Leaves the P. R. R.,” “Two New Erie Directors,” *New York Times*, 2 December, 13 January, and 24 July 1921, respectively.

<sup>19</sup>Major investment banks are defined as the top 25 in underwriting volume. Those 25 institutions, listed in Appendix Table A1, accounted for 87 percent of all bond offerings (by volume) up to 1929.

<sup>20</sup>In the Appendix we show that those railroads with a stronger relationship with their underwriters experienced a larger number of resignations.

underwriters.<sup>21</sup> 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 1 shows that underwriting services were more concentrated (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 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 turned to a broader range of investment banks for underwriting services once those intermediaries could no longer have a presence on their clients’ boards.

### 3 Theoretical Predictions: Effects of Section 10

Both economic theory (Sharpe, 1990; Rajan, 1992; Diamond, 1984; Holmstrom and Tirole, 1997) and history suggest that close relationships between financial intermediaries and firms can have costs as well as benefits. This implies that the effects of restricting the relationships between firms and securities underwriters are potentially ambiguous. In this section, we develop predictions for several firm outcomes which would result from the imposition of Section 10 if banker-directors monitored management and improved access to external finance. We then discuss predictions for those same outcomes which would result if the primary role of banker-directors was instead consistent with the descriptions presented by the Pujo Committee and Brandeis.

#### 3.1 Banker-Directors as Corporate Monitors

In the Appendix we present a simple model of the role of underwriter-monitors in alleviating firms’ financial constraints. The key friction in the model arises from an information asymmetry: a firm’s insiders observe the cash flows it generates, but its providers of external financing, the bondholders,

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<sup>21</sup>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  $\sum_{b=1}^B (\frac{n_b}{N})^2$ .

do not. As in Diamond (1984), this creates a moral hazard problem—the insiders will be tempted to underreport cash flows to reduce the payouts to the bondholders. The creditors use the threat of inefficient liquidation to induce truthful revelation of the value of the cash flows, but this raises the cost of debt and reduces the range of investments that can get financed. However, the investment bank that underwrites a firm’s debt can alleviate these problems by monitoring the firm through board membership. If underwriters with board seats can learn the firms’ private information, and if they can commit to reporting this information to the bondholders, the asymmetric information problem is resolved. On the other hand, monitoring is costly and the underwriter will charge a fee for this service, which may be too expensive relative to its benefits for some firms.

In this setting, firms with more valuable investment opportunities are more likely to choose to enter into a relationship with an underwriter on their board.<sup>22</sup> This feature is consistent with a fact we document below, that close affiliations with underwriters were most frequently established among the largest and best-known railroads, which suggests that adverse selection motives—such as screening or certification—were less likely to have played an important role in establishing these relationships at that time.<sup>23</sup>

When banker-directors perform such a monitoring role, implementing the rules of Section 10 would unambiguously harm their affiliated firms. Railroads that would have chosen to enter into a relationship with their underwriters in the absence of the rule would instead suffer from asymmetric information problems, which would increase their cost of debt and limit their ability to finance valuable investment opportunities. The loss of valuable projects would cause firms’ values to fall. The imposition of Section 10 would therefore lead to a reduction in investment, leverage and market valuations, and to an increase in the cost of debt financing for the affected railroads.

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<sup>22</sup>Holmstrom and Tirole (1997) present a related model of monitoring by financial intermediaries where firms vary in their initial capital. In this case, well-capitalized firms do not select into relationships and instead raise financing directly from uninformed investors. Firms with moderate levels of capital resort to an informed intermediary, while those with very weak balance sheets cannot finance their projects. Although the sorting into relationships in this setup is different than in ours, the restrictions imposed by Section 10 would harm firms that selected into a relationship with a monitor in similar ways than those predicted by our model.

<sup>23</sup>On the certification role of underwriters more generally, see Booth and Smith (1986) and Chemmanur and Fulghieri (1994).

### 3.2 Banker-Directors as Rent Seekers

An alternative view focuses on the possibility that access to external finance could allow managers to engage in empire building or to divert cash flows from the shareholders. In this case, they may be willing to pay substantial fees and high interest rates to finance projects that destroy value for the firm overall, if the private benefits they receive are large enough. This alternative description befits the role of banker-directors envisaged by the Pujo Committee and Brandeis.

Brandeis argued that the conflicts of interest inherent in the bankers' position would lead them to aggravate the agency problems between insiders and other shareholders. To maximize the banks' revenues from underwriting fees, banker-directors would encourage the issuance of excessive levels of debt to finance inefficient investments, such as "recklessly extravagant acquisitions" in which management would pay more for acquiring enterprises than they were actually worth (Brandeis 1914: 192). Railroads with close affiliations with their underwriters would therefore be more likely to make investments that would destroy value for the shareholders. According to this view, Section 10 would benefit railroads by eliminating the influence of securities underwriters on their management.

In some respects, the effects of Section 10 on firm outcomes predicted by these two views actually coincide, although for very different reasons. If banker-directors used their influence to earn high fees by inducing railroads to issue too much debt, then a rule restricting relationships with underwriters would also reduce the affected railroads' leverage and investment.

But the additional investments made by railroads affiliated with underwriters would have been inefficient. If this were the case, then the effect of Section 10 would be to raise, rather than lower, firm values. Brandeis, and, to a lesser extent, the Pujo Committee's report, also argued that close affiliations gave underwriters on a railroad's board bargaining power to charge higher fees for their services. Moreover, the undertaking of reckless investments would have likely made the high underwriting firms riskier, thereby increasing their costs of credit financing. The Pujo-Brandeis view therefore implies that the imposition of Section 10 would lower railroads' borrowing costs and increase firm values. These two predictions contrast sharply with those following from a view in which underwriter-monitors relax firms' financial constraints.

In what follows, we analyze the effects of Section 10 on railroad outcomes, and discuss which of

these two views better describes the role of banker-directors in early twentieth century America.

## 4 Data

### 4.1 Data on Railroads and their Ties to Underwriters

The majority of the data utilized in the analysis were hand-collected for this paper. Here we briefly describe the sources and methods used in the creation of the dataset; we provide more complete details in the Appendix.

We construct a panel dataset of accounting information for 1905-29 for all railroads with NYSE-listed common or preferred stock collected from *Moody's Manuals*, which provide data obtained from annual reports. Most of the analysis focuses on the 71 railroads that were listed in 1913, the year prior to the passage of the Clayton Act. We also collect similar information for the 79 industrial firms and utilities that were listed on the NYSE in 1913, and that issued debt during our sample period. We impose these restrictions to be able to calculate our treatment variables, and to ensure reasonable comparability with the railroads in our sample.<sup>24</sup> In order to reduce the influence of outliers, the accounting variables are trimmed at the top and bottom 1 percent.

These accounting data are supplemented with information on board composition, collected at two- to five-year intervals from *Moody's Manuals*, stock price data obtained from *The New York Times* and *Global Financial Data*, and other railroad characteristics collected from the annual reports of the ICC. Bond underwriting data for issues up to 1929 was collected from various editions of the *Fitch Bond Book* and from *Moody's*, and includes 638 bonds issued by the sample railroads and 293 from the industrials and utilities. We identify the names of directors or partners of the 245 different institutions that underwrote at least one of those debt issues from various bank directories. To determine board interlocks between these financial intermediaries and nonfinancial firms, we cross-reference the names of underwriters with those of directors on the boards of our sample firms. We discuss our methods and the accuracy of our name matching procedure in the Appendix.

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<sup>24</sup>There were a total of 167 industrials and utilities listed on the NYSE in 1913, but 88 of them had not issued debt prior to 1914.



## 4.2 Summary Statistics, Railroads

Table 2 presents summary statistics of railroad characteristics for 1913, the year our treatment variable is defined; similar data for industrial firms and utilities are presented in the Appendix. In Panel A, we investigate railroads' connections with underwriters. Column (1) presents data for railroads that were most reliant on the investment banks represented on their boards for their bond underwriting, defined as those among the top 25% in this statistic in 1913. This "high board underwriting" group was treated most strongly by Section 10 of the Clayton Act, which mandated that such underwriting be reduced to zero. The banks on the boards of the high board underwriting group underwrote on average 99.4 percent of their firms' debt issues, compared to only 18 percent for the others. The high board underwriting railroads had about one more major underwriter on their boards than the other railroads, and relied on relatively fewer (as measured by the HHI index) and more highly ranked investment banks as lead underwriters.

As Panel B indicates, NYSE-listed firms were quite interconnected in the early twentieth century. Both groups of railroads had at least one director in common with more than six NYSE-listed industrial firms on average. The average railroad in the high underwriting group had board interlocks with nearly 15 other railroads; the other group had interlocks with nearly 11 other railroads. To the extent that interconnected directors facilitated anti-competitive arrangements, this statistic provides some support for the Pujo Committee's view on the role of banker-directors as a mechanism for collusion. Ownership was relatively concentrated, with nearly 70 percent of both groups of railroads having an owner that held at least 10 percent of its shares outstanding.<sup>25</sup> Most of these characteristics, as well as board size, firm age, and location (as measured by the numeric ICC region), did not differ substantially across the high and low board underwriting groups.

Additional insights into the differences in financial outcomes and policies between the two groups of firms can be found in Table 3. Comparing columns (1) and (2) reveals that the railroads in the high board underwriting group were substantially larger in 1913, as measured by total mileage or log assets. This difference suggests that firms with the strongest reliance on underwriters represented on their boards prior to the passage of the Clayton Act were positively selected on firm size. In

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<sup>25</sup>These owners tended to be other railroads. In 1909, the only year in which we observe ownership, we find that the median stake of the underwriters of railroads was 0, and the mean stake was 1.8%. Thus, bank control rights flowing from equity ownership do not appear to be a plausible alternative explanation for our findings.

addition, they were slightly more levered and enjoyed a small advantage in the average interest rate paid on their debt, measured as total interest expense divided by total debt. They did not differ in their accounting rate of return or market valuations, payout policies, or investment in physical assets. Column (4) presents the coefficients of separate regressions for each outcome variable estimating differential trends across the two groups for the years prior to 1913. None of the differential trends are large in magnitude or statistically significant, suggesting that treated and control firms were evolving along parallel trends prior to the passage of the Clayton Act.

## 5 Impact of Section 10 of the Clayton Act on Firm Outcomes

### 5.1 Stock Market Reaction to Wilson's Veto

Before proceeding with the analysis of the accounting data, we study the effects of the Presidential veto of the postponement of Section 10 on stock returns. This provides a market-based indication of the expected impact of the regulation. The Appendix provides a more detailed description of the events surrounding the President's decision.

President Wilson vetoed the postponement on December 30, 1920. We assess the market's response by relating the change in market values to the strength of railroads' relationship with the underwriters on their boards in 1920, measured as the fraction of the total value of a railroad's bond offerings up to 1920 that was underwritten by bankers represented on its board in that year.<sup>26</sup> Table 4 presents the cross-sectional differences in returns for the 47 railroads whose shares traded on the NYSE in the days surrounding the veto. Column (1) shows that the market perceived the regulation to have detrimental effects on the most affected railroads; a one standard deviation increase in the percent of past underwriting done by bankers on the board reduced returns by 186 ( $= -0.0466 \times 0.3991 \times 100$ ) basis points on December 31, the date when the market responded to news of the veto. To take into account the possibility that the President's decision became known before the markets closed on December 30, in column (2) we cumulate returns over the one-

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<sup>26</sup>In the analysis of accounting data, we focus mostly on the underwriters' boards in 1913 to avoid confounding the difference-in-difference estimates with endogenous decisions that may have occurred between 1914 and 1920. For this event study, we utilize the strength of relationships in 1920 because rational investors should have altered their trading decisions only for those railroads actually affected by the enforcement of Section 10 on the day of the veto. The mean of the percent underwriting done by bankers on the firms' boards in 1920 with traded shares was 35.4%, and the standard deviation was 39.9%.

day window surrounding the Presidential veto. The results are similar but no longer statistically significant.<sup>27</sup> To assess the persistence of the effect, we next cumulate returns forward in time to January 5, 1921, the day a proposal was introduced in the Senate to amend the law in ways that would have exempted “dealings in securities” from its purview (House Committee on Interstate and Foreign Commerce, 1921). The results, reported in column (3), show an even larger effect. The negative market response is consistent with accounts in the press that followed the veto, which reported that resignations resulting from the implementation of Section 10 would “work to the great disadvantage” of the affected railroads.<sup>28</sup>

Was the President’s decision anticipated by the market? Wilson’s weak health kept his agenda to a minimum in the last 17 months of his Presidency, and we are not aware of any public pronouncements regarding his views on the postponement legislation. However, he did refer the one-year postponement bill passed by Congress on December 18 for consideration to the ICC, the executive agency charged with enforcing Section 10. On December 28, the majority of the commissioners, who had been appointed by Wilson, advised him to sign the postponement legislation. The ICC’s recommendation provided a strong indication of how the Administration would act. Column (4) of Table 4 presents estimates of the cross-sectional variation in returns on December 29, when the markets could likely first react to the ICC’s decision. Railroads with close ties to underwriters on their boards experienced an *increase* in their valuations relative to other firms, and this effect was roughly of the same magnitude as the decline in response to the veto. This finding suggests that the veto was indeed at least partly a surprise.

A potential concern with our analysis is that the estimated effect may have been caused by some other event that impacted all firms with close affiliations with investment banks, rather than just railroads. In column (5) we present estimates of the variation in the cross-section of returns on the sample industrials and utilities that traded on the days around the veto. Reassuringly, we find no differences between the firms that relied heavily on the banks on their boards for underwriting, and those that did not. In the Appendix we also show that our results are unlikely to be driven by systematic differences in the returns of the most affected railroads, and are robust to alternative definitions of the control group.

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<sup>27</sup>The number of observations declines between columns (1) and (2) because the lack of transactions precludes us from cumulating returns for three railroads. See Appendix for details.

<sup>28</sup>“Many Changes Soon in Railroad Boards,” *New York Times*, 23 January 1921.

It is important to note that other factors may have muted the expected impact of the veto. For example, the President’s decision coincided with efforts in Congress to alter Section 10 with new legislation that would have repealed its provisions regarding banker-directors. These efforts began in early December 1920 and gained momentum in January 1921 after the regulation went into effect, although they were ultimately unsuccessful. We are therefore cautious in the interpretation of these results, which we see mostly as an indication that market participants expected Section 10 to have a negative impact on the affected railroads.

## 5.2 Panel Data on Firm Outcomes: Empirical Strategy

We now turn to an analysis of the impact of the Section 10 on railroads’ values, interest rates, leverage, and investment rates, using our panel of NYSE-listed railroads from 1905-29. In order to test the theoretical predictions discussed in Section 3, we analyze firm outcomes before and after the regulation went into effect in 1921. Although the timing of the implementation of the law was largely exogenous due to the unexpected Presidential veto, Figures 1 and 2 suggest that underwriting relationships began to change from 1914 to 1920, possibly in anticipation of its eventual sanction. To prevent these endogenous responses from influencing the assignment of the treatment to firms, we instead use variation in the degree of underwriting done by bankers on boards in 1913, before the Clayton Act was considered.

Since our methodology consists of a difference-in-differences analysis, a natural concern is that any effect in the post-reform period could be driven by preexisting trends. Figure 3 presents a simple graphical analysis of the difference between the high board underwriting railroads and the others for the four variables of interest, as estimated from regressions that include firm and year fixed effects and control for the log of lagged assets.<sup>29</sup>

The lines in the figure show considerable short-term volatility, perhaps due to the relatively small number of available firms (about 62 firms on average in a given year). However, several significant patterns can be discerned. First, in the years up to the passage of the Clayton Act in 1914, the differences are relatively flat (for the interest rate and investment) or have the opposite

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<sup>29</sup>Figure 1 in the Appendix shows the evolution of the four variables for the treated and control firms separately. The evidence supports the view that Section 10 primarily affected the treated railroads—the figure shows no drastic improvement in the outcomes of control firms following the regulation; instead, the behavior of railroads with stronger relationships with underwriters became more similar to those of other railroads after 1921.

slope of what our model would predict for the post-1920 period (for Tobin’s Q and leverage). The differences change substantially following the implementation of Section 10 in 1921; there is a negative slope for Tobin’s Q and leverage, it is relatively flat but at a lower level for investment, and there is a positive slope for the interest rate. For some outcomes, however, the figures suggest that the change may have begun somewhat earlier, between 1914 and 1920. This pattern would be consistent with the endogenous changes in bank-firm relationships made prior to 1921 having effects on firm outcomes before Section 10 of the Clayton Act was actually implemented.

Figure 3 suggests that the post-1920 changes are unlikely to be purely the result of preexisting differential trends. Nevertheless, to be conservative our main specification consists of a deviation-from-trend model that explicitly controls for differential trends in our treatment variable. Our basic estimating equation is:

$$y_{it} = \alpha_i + \gamma_t + \theta_1 \text{percent underwriting by banks on board 1913}_i \times \text{post1920}_t + \theta_2 \text{percent underwriting by banks on board 1913}_i \times \text{trend}_t + \beta X_{it} + \epsilon_{it}, \quad (1)$$

where  $y_{it}$  is one of the four firm outcomes of interest for railroad  $i$  in year  $t$ ;  $\alpha_i$  and  $\gamma_t$  are firm and year fixed effects to control for time-invariant unobserved firm characteristics and for any macroeconomic or industry-wide effects over time; the ‘percent underwriting by banks on board 1913’ is the percent (by value) of each railroad’s debt underwritten in the years up to 1913 by the banks represented on the firm’s board in 1913; ‘trend’ is a linear time trend; ‘post 1920’ is an indicator for the years 1921-1929, when Section 10 was in force; and  $X_{it}$  includes time-varying controls. In this framework,  $\theta_2$  estimates any trends in the differences in outcomes for railroads of varying degrees of reliance on banks represented on their boards for underwriting. The main coefficient of interest is  $\theta_1$ ; it indicates the differential shift after the implementation of Section 10 of the Clayton Act on railroads’ outcomes by the level of their underwriting done by bankers represented on their boards. The predictions of the monitoring view presented above are that  $\theta_1$  should be positive for the interest rate, and negative for all other outcomes; instead, the rent-seeking view predicts a negative coefficient for the interest rate, and a positive one for Tobin’s Q. To account for possible serial correlation over time within firms, all standard errors are clustered at the firm level.

### 5.3 Main Results

Table 5 presents results for the reduced-form specification (1) for the variables of interest: Tobin’s Q (our measure of firm value), investment rates, average interest rates, and leverage. For all outcomes, the estimated coefficient  $\theta_1$  shows a negative impact that is increasing in the railroads’ reliance on underwriters represented on their boards in 1913. The estimates in column (1) imply that for a railroad with the mean value of the percentage underwriting done by bankers on their boards prior to 1913 (40.9%), Tobin’s Q fell by 1.8% after 1920, equivalent to a 2% decline relative to its mean level in the sample of 0.783 in that year. Similarly, the results in columns (3) and (4) indicate that the interest rate and leverage ratio for a railroad with the average ‘percent board underwriting’ in 1913 would have risen by about 0.2 percentage points and fallen by 1.8 percentage points, respectively.<sup>30</sup> These estimates are equivalent to a 4.1% increase in interest rates and a 5.2% decline in leverage in the post-1920 period, relative to the mean 1920 levels. The effect on investment was larger: the investment rate contracted by 0.9 percentage points, about 28.1 percent of the 1920 mean. This suggests that railroads’ investment decisions were quite sensitive to the availability and costs of external financing.

These results imply that strong relationships with underwriters benefited railroads overall by allowing them to finance larger investments at lower costs, which raised their valuations. Our estimates are consistent with such relationships enabling the bankers to act as corporate monitors and relax the firms’ financial constraints. The authors of the Clayton Act considered banker-directors to be rent seekers, and some of the effects we observe, namely the fall in investment and leverage, could be consistent with such views. However, the fall in railroads’ valuations and the increase in their interest rates contradict their predictions. If underwriter representation on railroads’ boards led to the adoption of inefficient investment projects and to excessive interest costs, then Section 10 should have raised valuations and lowered interest rates. To the extent that bankers actually utilized their positions to extract rents from railroads, our results indicate that such costs were far outweighed by the benefits from monitoring and improved access to outside finance.

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<sup>30</sup>Our data only contain information on the total stock of debt and the interest expense on this debt. We therefore estimate average (rather than marginal) effects on leverage and interest rates, and we expect these variables to adjust slowly over time. A limitation of these data is that they would not reflect changes in yields for bonds issued prior to 1921 in response to changes in their perceived credit risk.

A potential concern with our strategy, however, is that our difference-in-difference estimates may instead reflect other forces that could have affected the role of financiers in the American economy in the post-1920 period, such as changes in securities markets that may have diminished the influence of underwriters as monitors. To address this possibility we perform falsification tests by replicating the empirical analysis on the sample industrials and utilities in columns (5) through (8). Consistent with the results above, which showed no change in these firms' relationships with their underwriters, and no change in their share prices when the postponement of Section 10 was vetoed, these regressions show no substantial differences in the years following 1920 among industrials and utilities with different degrees of reliance on the bankers represented on their boards, across all our outcomes.

#### 5.4 Alternative Specifications

The summary statistics presented in Table 3 suggest that the railroads more reliant on the bankers on their boards for underwriting were somewhat different from the other railroads across a range of characteristics at the time of the passage of the Clayton Act. The 'high board underwriting' railroads had slightly higher leverage and paid lower average interest rates, for example. But more importantly, they were considerably larger than the other railroads. This raises the concern that our estimated effects could be the result of changes in the performance of large firms relative to small firms in the 1920s, and raises questions about the external validity of our results for other contexts. Table 6 presents alternative specifications designed to explore these issues.

Panel A shows that our results are robust to using a binary indicator for the high board underwriting railroads, rather than our continuous measure of the percent of underwriting done by bankers on the board in 1913. These estimates will serve as a basis of comparison for the alternative specifications, which also identify the treated railroads with the binary indicator.

In Panel B, we address the possibility that differences in observable traits between the treated and control railroads may drive our results, using inverse propensity score weighting. In particular, we estimate probit regressions of an indicator for membership in the 'high board underwriting' group on 1913 characteristics. We use the resulting estimates to restrict the sample to the common support in the propensity to be in the 'high board underwriting' group, and to weight the railroads

by their inverse propensity scores.<sup>31</sup> This procedure creates a control group that is quite similar to the treatment group: the smallest railroads from the control group are eliminated (as their propensity score is outside the common support), and a greater weight is applied to the largest railroads in the control group. Despite these changes, the estimated effects are similar to the baseline estimates of Panel A, if a bit stronger. This suggests that differences in observable characteristics are unlikely to be responsible for our results.

The propensity score strategy provides further support for the internal validity of our estimates. As in any quasi-experiment, however, generalizing the results to other contexts is challenging. To take a step in this direction, we investigate whether our results are merely the product of focusing on railroads that selected into underwriting relationships. In Panel C of Table 6 we compare the outcomes of the 20 high board underwriting railroads (the strongly treated firms) with only the 20 largest industrials and utilities in the sample, whose average size is close to that of the treated railroads.<sup>32</sup> Reassuringly, we find that the treated railroads performed far worse than the control group of large industrials and utilities, a set of firms not bound by Section 10. The magnitude of the coefficients are in some cases considerably larger than the baseline estimates in Panel A, suggesting that differences in the performance of railroads relative to other industries may magnify the estimated effects of the Act. But these findings contradict the notion that our main results are simply driven by railroad characteristics that are correlated with their selection into relationships with underwriters, such as firm size.

Our baseline estimates in Panel A actually include some partially treated firms in the control group, which includes 20 railroads that had medium or low levels of underwriting done by bankers on their boards in 1913. The presence of these partially treated firms in the control group may introduce bias into our estimates, making it more difficult to detect an effect of the treatment. In addition, partially treated firms may have experienced an increase in the availability of external funds following the implementation of Section 10, if banks that could no longer monitor the high board underwriting firms instead shifted to the railroads with which they had preexisting weak

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<sup>31</sup>A detailed discussion of our methods, and robustness to alternative specifications, is presented in the Appendix.

<sup>32</sup>The mean value of log assets for 1913 for the high board underwriting railroads is 19.47, and the mean value for the large industrials and utilities is 19.0. The mean value for the control railroads included in Panel A is 18.47. Unfortunately, we cannot restrict the control group further to match the treated railroads in both size and leverage since even the largest industrials and utilities had much lower borrowing ratios than the railroads in the high underwriting group.



relationships. If this were the case, our estimates would be biased upward in absolute magnitude.<sup>33</sup> To address these concerns, in Panel D we restrict the control group to only those 31 railroads that had never had any underwriting done by bankers on their boards in 1913.<sup>34</sup> These railroads should have been unaffected by the regulation, and therefore constitute a more ‘pure’ control group with which to test the theoretical predictions. The results are, if anything, somewhat stronger than those in Panel A, providing further validation for the monitoring view of banker-directors.

## 5.5 Selection Effects in Bank-Firm Relationships

In the empirical analysis presented thus far, we assigned the treatment variables in 1913, the year before the Clayton Act was passed, and eight years before Section 10 was implemented in 1921. We adopted this deliberately conservative approach to ensure that any endogenous responses (arising for example from banker resignations from selected boards from 1914 to 1920 in anticipation of the regulation) did not determine railroads’ assignment to the control or treated groups, and introduce selection bias into our estimates. Indeed, railroads’ reliance on the bankers on their boards for underwriting services decreased substantially between 1913 and 1920.<sup>35</sup> This strategy leads us to understate the effects of underwriting relationships since some of the railroads in the treated group, as defined in 1913, were not actually treated when Section 10 was implemented. But understanding the nature of these endogenous choices can help us determine the extent of this bias. The resignations and shifts to new underwriters prior to the imposition of Section 10 may also offer valuable insights into the selection mechanisms by which relationships between underwriters and railroads were created or ended.

We use an instrumental variables framework to assess the impact of these endogenous changes in bank-firm relationships. In particular, we compare the OLS and IV estimates of the following

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<sup>33</sup>The estimated effects presented in Panel C of Table 6, which show that our results are robust to comparing the most affected railroads solely to large industrials and utilities, provide some evidence against this concern. It is less likely that the regulatory change altered the prices or quantity of credit available for non-railroad firms. Indeed, Figures 1 and 2 suggest that there were no changes in underwriting relationships for industrials and utilities in the post-1920 period.

<sup>34</sup>The majority of these firms, 65 percent, actually had top underwriting firms represented on their boards.

<sup>35</sup>The percent underwriting done by underwriters on boards in 1913 and in 1920 changed on average by -8.6 percentage points, a decline of more than 20 percent; for six railroads that had relied on underwriters on their boards in 1913, the fraction of their underwriting done by the bankers on their boards in 1920 was 0.

regression:

$$y_{it} = \alpha_i + \gamma_t + \delta_1 \text{percent underwriting by banks on board } 1920_i \times \text{post}1920_t + \delta_2 \text{percent underwriting by banks on board } 1920_i \times \text{trend}_t + \beta X_{it} + \epsilon_{it}, \quad (2)$$

where the ‘percent underwriting by banks on board 1920’ is measured as the fraction of each railroad’s debt underwritten up to 1920 by banks represented on the firm’s board in that year, rather than 1913 as in equation (1). The endogenous changes in bank-firm relationships between 1914 and 1920 will bias the OLS estimates of (2). We can get a sense of the direction of this bias by comparing the OLS estimates to the IV estimates obtained from instrumenting for the intensity of bank-firm relationships in 1920 with the intensity of bank-firm relationships in 1913, which were determined prior to the passage of the Clayton Act.<sup>36</sup>

Table 7 presents the results. For each outcome, the first column displays the estimates from the OLS regressions, and the second column presents the results from the 2SLS specification. Both sets of estimates are statistically significant and validate our earlier findings: the interest rate increased, and all other outcomes declined for the treated railroads after 1920. Moreover, the Kleibergen-Paap F statistics vastly exceed the Stock and Yogo critical values, minimizing concerns of weak instruments in the first-stage regressions, which are presented in the Appendix. The exclusion restriction also is likely to be satisfied in our context, since the strength of underwriting relationships in 1913 arguably only affected firm outcomes post-1920 through the intensity of these relationships in 1920.

For each outcome variable, the 2SLS estimates are larger in absolute magnitude than the OLS estimates, revealing that the bias resulting from endogenous resignations in anticipation of the regulation lead us to underestimate the effect of relationships with underwriters when using OLS. The difference between the OLS and 2SLS estimates presented in Table 7 suggests that the railroads that maintained strong ties with the bankers on their boards between 1914 and 1920 were positively selected, in the sense that they did not suffer much from financial constraints, and underwriting

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<sup>36</sup>This strategy is similar to the ones used by Stevenson (2010) and Ahern and Dittmar (2012). Since underwriting intensity appears twice in (2) due to its interactions with the time trend and the trend shift, our specification contains two endogenous regressors. We therefore use two instruments: the fraction of underwriting done by bankers on boards in 1913 interacted with the time trend, and the same fraction interacted with the indicator for the post-1920 period.

relationships had less of an impact on their outcomes.<sup>37</sup> To the extent that unwinding board positions is costly, one interpretation of this response is that bankers may have voluntarily resigned from the boards of more constrained railroads if these firms were more likely to need new financing. The selection in anticipation of the regulation could also be consistent with the underwriters wishing to remain on the boards of their stronger clients, perhaps out of reputational concerns.

These results also imply that our main empirical strategy based on 1913 bank-firm relationships understates the economic impact of Section 10. Our main specifications using the 1913 bank-firm affiliations defined by equation (1) and presented in Table 5 have the interpretation of the reduced-form relationships between our instruments and our outcome variables in equation (2). Since our econometric model is exactly identified, in Appendix Section 3.8.2 we show that it is possible to use the relationship between the structural equation (2) and the first-stage parameters to quantify the magnitude of the bias of the estimates of the reduced-form equation, (1). Depending on the outcome variable, the reduced form estimates range in magnitude from about 70% to 78% of the structural values. Thus, our conservative estimation strategy understates the true effects by about 28 to 43 percent. Since the reduced-form estimates are generally modest in magnitude, the estimated effects remain plausible when we take into account the bias from endogenous responses in underwriting relationships from 1914 to 1920.

## 6 Mechanisms Behind the Results

Our estimates of the impact of Section 10 are consistent with the implications of underwriters on railroads' boards acting as monitors and improving railroads' access to outside finance. However, no direct evidence can be obtained to measure the underwriters' ability to monitor or to ameliorate agency problems between managers and outside investors. To further validate this mechanism, we present additional evidence that is consistent with a monitoring role for banker-directors. We also assess the possibility that other forces that affected the railroad industry in the post-1920 period, such as shifts in demand, could instead be responsible for our results.

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<sup>37</sup>In the Appendix, we show that a similar type of selection guided J.P. Morgan's decision to resign from the boards of railroads in an attempt to forestall the restrictions to board interlocks introduced by the Clayton Act in 1914.

## 6.1 Underwriting Relationships and Access to External Finance

The monitoring view of the role of banker-directors implies that underwriting relationships may benefit railroads by facilitating their access to credit to fund valuable investment opportunities. If this were the case, we would expect firms with higher financing needs to suffer the most once their relationships were restricted by the imposition of Section 10. In particular, the treated railroads that were more likely to need to issue debt (for example, because a substantial fraction of their outstanding debt would mature) in the years 1921-23 would have been more affected following the regulatory change than other firms with similarly strong underwriting relationships. Importantly, railroad bonds typically had very long maturities.<sup>38</sup> Thus, the fraction of a railroad's debt maturing in 1921-23 is likely unrelated to any changes in railroad demand or in the firm's investment opportunities.

We test this hypothesis by estimating a modified version of equation (1) where we include separate interactions of the main variable of interest (*pct. underwriting by banks on board 1913<sub>i</sub> × post 1920<sub>t</sub>*) with indicators for railroads that we expect to be differentially affected by Section 10—those in the top decile of debt maturing in the years 1921-23—and for the other railroads.<sup>39</sup> Panel A of Table 8 presents the results. The estimated negative effects of underwriting relationships are indeed larger for railroads with the most debt maturing in 1921-23. These findings confirm that our main results are likely driven by the supply of credit being curtailed as a result of the regulatory change.

To the extent that bankers on boards acted as monitors, our main measure of relationships, which is based on the amount of underwriting services provided by those banks, should be correlated with their incentives to monitor their clients. Yet underwriters may have also varied in their monitoring abilities. Railroads that relied on the most highly ranked investment banks, which were also likely to be the more skilled monitors, since they had such extensive connections and experience with railroad management, should have been most affected by the implementation of Section 10. To test this hypothesis, we interact our main treatment variable with indicators for the railroads whose lead underwriters were above-median in rank, and for those whose underwriters were below-

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<sup>38</sup>The value-weighted average of the time to maturity for all the railroad debt issues we observe in our sample is 37.49 years.

<sup>39</sup>Most of the sample railroads had very little debt maturing over this short period; the median value is 1%. But the median value for those railroads in the top decile is much higher, 17%.

median in rank. Panel B of Table 8 presents the results. The estimated effects are again larger for the differentially affected railroads. In particular, those railroads with strong relationships with the highest ranked underwriters experienced marked declines in their valuations relative to others, providing further evidence against the rent-seeking hypothesis. Brandeis and the Pujo Committee argued that the strongest and best-known banks were better able (and more prone) to use their positions of power to extract rents from their clients, and would therefore have expected to find the largest *increases* in valuations for the railroads connected to highly ranked banks.

In sum, the railroads likely to be differentially affected by Section 10 as a result of disruptions to the monitoring role of underwriters appear to have suffered to a greater extent in the years following 1920. In many cases the differences are not actually statistically significant, which may be due in part to the small sample size. But we interpret these findings as suggestive evidence that underwriters acted as corporate monitors, and that disruptions to this role created by Section 10 of the Clayton Act is the main mechanism behind our results.

## 6.2 Demand Changes and the Railroad Industry

We next 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 4 shows that freight traffic, which accounted for about 70 percent of the industry's revenues, continued to grow throughout this period.<sup>40</sup> 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% of their revenues

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<sup>40</sup>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.

from passenger traffic in 1920, compared to 21.4% 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 network was much better developed and automobiles accounted for a greater share of passenger transportation.

To test whether our 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.<sup>41</sup> 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.

Table 9 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.

### 6.3 Other Prohibitions of Section 10

Section 10 of the Clayton Act specifically forbade “dealings in securities” between railroads and firms represented on railroads’ boards, and much of the history of the Act suggests that bankers were its primary target. But Section 10 also prohibited other forms of self-dealing by railroad directors, such as purchasing supplies or capital equipment from affiliated firms. The Act was also intended to limit the ability of banker-directors to facilitate collusion by holding board seats on

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<sup>41</sup>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. For sources and methods used in computing these values, see the Appendix.

multiple railroads.

Section 10 explicitly prohibited “contracts for construction or maintenance” or “supplies” by firms in which the director of a railroad was a partner or director. To address the possibility that these forms of self-dealing may have been curtailed by Section 10, we add a control for the number of interlocks with NYSE-listed industrial firms that were likely suppliers of capital equipment to railroads in 1913, defined as the manufacturers of steel (used in construction of rails and bridges) and of locomotives, railroad cars, and parts of railroad cars. These connections were rather common: the average railroad in our sample was interlocked with about 1.8 equipment suppliers in 1913.

Panel A of Table 10 presents the results. The interlocks with equipment suppliers do seem to affect valuations and investments in the post-1920 period. The estimated effect on Tobin’s Q is positive and statistically significant, implying that railroads that were highly interlocked with capital equipment suppliers had depressed valuations prior to the enforcement of the rule, relative to other firms. This effect is consistent with the concern expressed by the Pujo Committee that self-dealing in equipment contracts harmed the railroads. However, these affiliations had a positive effect on investment after 1920, which is inconsistent with the notion that the railroads were induced to purchase excessive levels of equipment from affiliated suppliers, and suggests that perhaps firms may have invested in fewer projects in response to a higher cost of new equipment. Importantly, the estimated effects of the underwriting variables (shown in the first row of the table) are mostly unaffected by the inclusion of these controls. Thus, our main results are unlikely to be driven by disruptions of these alternative self-dealing practices.

Another possible effect of Section 10 might have been to reduce collusion among railroads. To the extent that having a presence on competing railroads’ boards helped financiers facilitate anti-competitive practices, the resignations induced by the implementation of the regulation may have reduced collusion within the industry. This is a particular source of concern for our empirical strategy, because some of the implications of an increase in competition could in fact be observationally equivalent to those of severing underwriting relationships. Increased competition may have led to lower profits and therefore lower valuations. Although less straightforward, one could imagine that a decline in profitability may have raised the firms’ cost of borrowing, thereby reducing borrowing levels. Finally, existing theoretical models offer contradictory predictions for the effects of increased competition on firm investment (see Levenstein and Suslow, 2006, for a literature review).

In the Appendix, we show that our results are robust to controlling for the number of interlocks with other railroads. Yet this may be a weak test of the effects of Section 10 on collusion, since only those connections created through underwriter-directors were at risk of being severed by Section 10. Thus, we use a measure of board interlocks created by railroads' underwriters with their competitors. Specifically, we measure the number of direct competitors, defined as railroads located within the same region as designated by the ICC, with which each railroad had a director interlock created by a bank that had underwritten for that railroad. The average railroad in our sample was interlocked with 2.0 competitors through their underwriters. Although we cannot measure collusion or price-setting directly, these tests can at least provide suggestive evidence of the consequences of any changes in the degree of collusion following the implementation of Section 10.

In Panel B of Table 10, we estimate our baseline specifications with the addition of controls for these interlocks with competitors created by banker-directors, interacted with trends. The signs of the estimated effects of our measure of underwriter-led collusion are consistent with railroads being harmed by losing interlocks with competitors after 1921, but the magnitude of the effects are quite small, and none of them is statistically significant. More importantly, our main estimates are generally unaffected by the inclusion of the collusion variables—the magnitudes of the estimated effects are only slightly diminished, and significance is only lost for the interest rate.

## 7 Discussion and Conclusion

Close affiliations between banks and firms are quite common in many countries around the world today, and they were once prevalent in the United States as well. These arrangements have the potential to relax financial constraints, but they also enable bankers to engage in self-dealing. In the early-twentieth century, major American corporations established close relationships with their securities underwriters, but these associations became politically controversial. In this paper, we use Section 10 of the Clayton Antitrust Act, which forbade railroads' securities underwriters from holding seats on their boards, to estimate the effect of those relationships. Consistent with banker-directors acting as delegated monitors, we find that following the imposition of Section 10, railroads that had previously maintained strong relationships with investment banks saw their valuations,



leverage and investment rates fall, and their borrowing costs rise. Our empirical design can not rule out the possibility that the influence of bankers was detrimental for some specific railroads, as was claimed by proponents of the Clayton Act. But our findings strongly suggest that the regulation reduced the ability of securities underwriters to monitor railroad management, and harmed the interests of the industry overall.

The changes induced by Section 10 enable us to identify the value of relationship underwriting only for railroads, but these financial arrangements may have been even more valuable for other types of nonfinancial corporations in the early twentieth century. Investment banks also had a strong presence on the boards of industrial firms, and were often instrumental in organizing the mergers that gave rise to large industrial trusts during this period. These firms had less established reputations, less collateral, and worse disclosure of financial information than railroads, suggesting that the value of a monitor with the ability to constrain managerial malfeasance may have been even higher. Whether investment banks played a similar role in the U.S. that universal banks may have had on Germany's industrial development is an interesting open question.<sup>42</sup>

The implications of our findings for modern firms, however, may depend on the context. Although it is far less common for major American corporations to have bankers on their boards today, banker-directors appear to exacerbate, rather than to ameliorate, agency problems (Güner, Malmendier and Tate, 2008). Thus, relationships with intermediaries apparently do not serve the same role today as they did in the early twentieth century. The dramatic evolution of American financial markets and regulatory frameworks since the 1920s may explain this difference. Despite the recent outcries over the governance of large corporations, investors have stronger legal protections and much greater access to information than they had prior to the advent of the Securities and Exchange Commission and federal securities laws in the 1930s. In the early twentieth century, investors trusted reputable investment banks to defend their interests to a much greater extent than the management of nonfinancial firms. This suggests that our results may be most relevant to countries around the world with more limited protections of investors or strong asymmetries of information between insiders and outsiders. In such environments, affiliations with financial

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<sup>42</sup>Guinnane's (2002) discussion of the historical development of Germany's banking system highlights important similarities with our presentation of the American case, including the value of relationships with intermediaries that have advantages in monitoring and information processing, and the potential costs from their opportunistic behavior or their ability to restrict competition.

intermediaries that can effectively act as monitors may benefit firms, and contribute to economic growth more generally.

The results of this paper provide new insights into the effects of rules intended to address conflicts of interest. Railroads were made better off by the prohibitions against self-dealing in equipment supply contracts, suggesting that the benefits from relationships with suppliers (such as a timely delivery of high quality inputs) were not large. But Section 10 of the Clayton Act was motivated by a theory that bankers used their influence over railroads' access to finance to enrich themselves at the expense of other investors, and was designed to prevent financiers from engaging in self-dealing. In its attempt to address the conflicts of interest inherent in the bankers' positions as corporate directors, Section 10 disrupted relationships between underwriters and railroads that had developed in order to facilitate valuable flows of information, and to constrain powerful insiders. Restricting these relationships ultimately harmed most of the firms and investors that the regulation was intended to protect. Our empirical analysis does not measure the potential effects of this regulation on other economic agents, and we therefore cannot estimate the welfare effects created by Section 10. However, our findings do suggest that legal prohibitions or rules aimed at safeguarding against conflicts of interest that arise in the economy more generally can have substantial economic costs, particularly if they disrupt the flow of information often inherent in these transactions.

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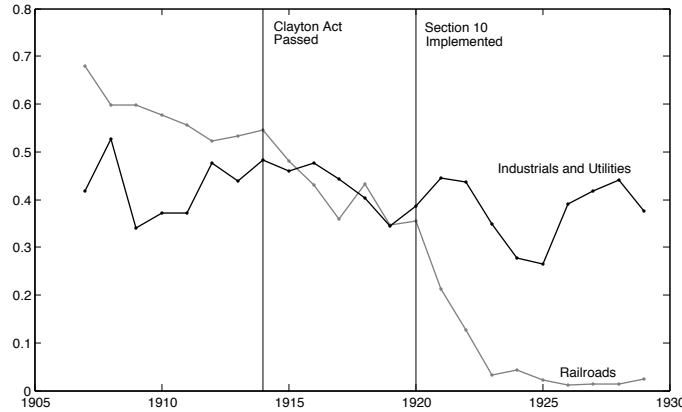
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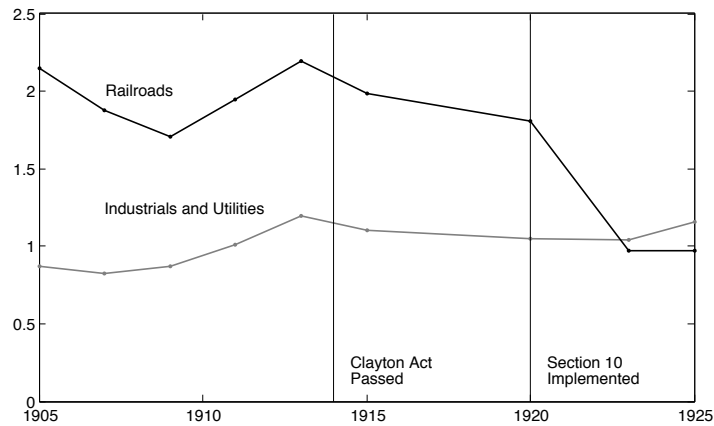
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**Figure 1:**

**Proportion of bond underwriting by banks represented on issuer boards, 1907-29**

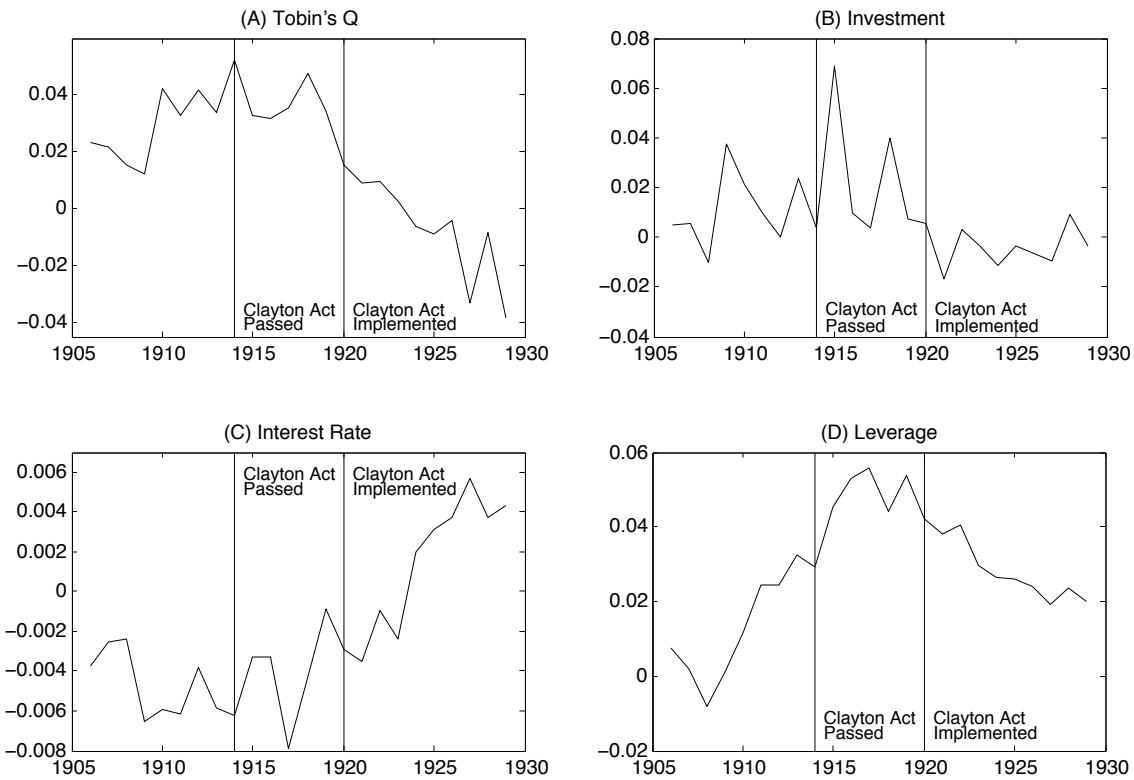
*Note:* The figure presents a three-year moving average of the proportion of debt that was underwritten by investment banks represented on the boards of NYSE-listed railroads, and industrial companies and utilities listed on the NYSE in 1913. The figure is calculated from all sample railroads', industrials' and utilities' debt issues, which totalled 638 different bond offerings for the railroads, and 293 for the industrials and 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. 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. For additional information, see the Appendix.



**Figure 2:**

**Representation of major investment banks on the boards of NYSE-listed companies**

*Note:* The figure presents the number of major investment banks, defined as those among the top 25 in debt underwriting for the sample railroads, industrials and utilities up to 1929, on the boards of all NYSE-listed railroads, and on the boards of industrial corporations and utilities, whose common or preferred stock was listed on the NYSE in 1913. The points in time at which the presence of underwriters are measured are indicated as the circled points along the lines in the figure. At the time, the major investment banks included several firms organized as trust companies or commercial banks; the list of the top underwriting firms is presented in the Appendix.

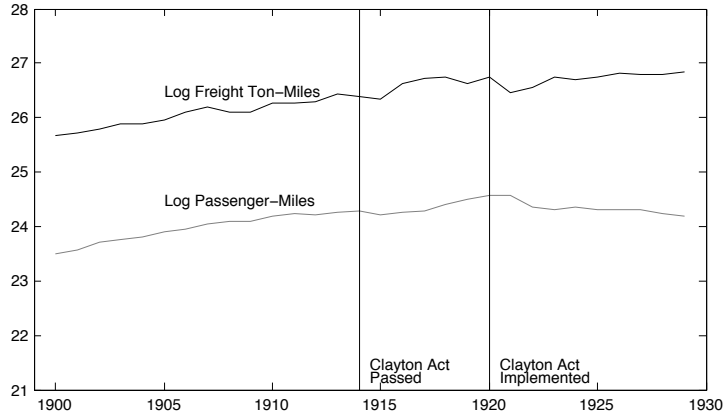


**Figure 3:**

**Annual differences: High board underwriting vs. other railroads**

*Note:* The figure presents annual differences between railroads in the high board underwriting group in 1913 and the others. The differences are estimated from regressions that include firm and year fixed effects and control for the log of lagged assets. Specifically, the figure presents estimates of the  $\phi_t$  parameters obtained from the regression  $y_{it} = \alpha_i + \gamma_t + \delta \log(\text{assets}_{t-1}) + \sum \phi_t \text{HighBoardUnderwriting}_i \times \text{Year}_t + \epsilon_{it}$ , where  $y_{it}$  is the outcome of interest,  $\alpha_i$  and  $\gamma_t$  are firm and year fixed effects, and the  $\phi_t$  reflect annual differences relative to 1905.





**Figure 4:**  
**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.

**Table 1:**  
**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 (0.075)	0.491 (0.049)	0.321** (0.089)
1921-1929	0.625 (0.075)	0.638 (0.046)	-0.013 (0.086)
Change: 1921-1929 vs. 1900-1920	-0.187+ (0.106)	0.147* (0.066)	-0.334** (0.114)

*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. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 2:**  
**Summary Statistics: Railroads, 1913**

	High Board Underwriting Railroads (1)	All Other Railroads (2)	Difference, (1) vs. (2) (3)
<i>A. Relationships with underwriters</i>			
Number of major underwriters on board	2.900 [1.373]	1.922 [1.647]	0.978* (0.382)
Percent of debt underwritten by banks on board in 1913	0.994 [0.023]	0.180 [0.280]	0.814** (0.040)
HHI index, lead underwriters	0.854 [0.245]	0.592 [0.285]	0.263** (0.072)
Average rank, underwriters (top = 1, bottom = 98)	5.918 [7.249]	14.490 [15.917]	-8.572** (3.081)
<i>B. Other firm characteristics</i>			
Board Size	12.800 [2.858]	12.275 [3.914]	0.525 (0.838)
Number of industrials interlocked with firm	6.600 [4.070]	6.118 [5.082]	0.482 (1.149)
Number railroads interlocked with firm	14.800 [9.384]	10.941 [7.232]	3.859+ (2.311)
Indicator: firm has a 10% owner	0.647 [0.493]	0.690 [0.471]	-0.043 (0.148)
Firm age (years)	33.100 [22.007]	30.196 [22.763]	2.904 (5.824)
Firm location: ICC region (1-8)	5.105 [2.492]	4.540 [2.261]	0.565 (0.650)

*Note:* Column (1) reports means and standard deviations in brackets for firms in the top quartile of the distribution of the percent of underwriting done by bankers that were represented on their boards in 1913. Column (2) reports means and standard deviations for all other sample railroads. Column (3) presents the differences between the values in columns (1) and (2), along with the standard errors of those differences, which are presented in parentheses. The differences are estimated from a regression with robust standard errors. Definitions of all variables, and the methods and sources used to create them, are presented in the Appendix. \*\*, \* and + denote significance at 1%, 5% and 10%, respectively.

**Table 3:**  
**Summary Statistics, Railroad Financials**

	High Board Underwriting Railroads	All Other Railroads	Difference, (1) vs. (2)	Differential Trend: High Board Underw. vs. Others, 1905-1912
	(1)	(2)	(3)	(4)
Mileage, 000s	3.888 [2.868]	2.368 [2.855]	1.520* (0.751)	0.041 (0.029)
Log(assets)	19.479 [0.943]	18.466 [1.199]	1.013** (0.268)	0.006 (0.007)
Book leverage	0.506 [0.090]	0.445 [0.178]	0.061+ (0.032)	0.004 (0.004)
Average interest rate	0.037 [0.007]	0.045 [0.019]	-0.008** (0.003)	-0.0004 (0.0004)
Return on Equity (ROE)	0.153 [0.088]	0.137 [0.055]	0.016 (0.022)	0.001 (0.024)
Return on Assets (ROA)	0.042 [0.014]	0.047 [0.022]	-0.004 (0.004)	-0.0006 (0.0006)
Tobin's Q	0.903 [0.217]	0.888 [0.192]	0.015 (0.069)	-0.0001 (0.005)
Dividend rate	0.196 [0.223]	0.195 [0.193]	0.001 (0.058)	0.005 (0.008)
Investment	0.043 [0.077]	0.021 [0.036]	0.021 (0.018)	0.002 (0.002)

*Note:* Column (1) presents 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) presents means and standard deviations for all other railroads. Column (3) presents the differences between columns (1) and (2). These differences are estimated from a regression with robust standard errors, which are presented in parentheses. Column (4) presents the differential trends for firms with the highest degree of underwriting done by bankers on their boards in 1913, relative to the others, estimated from regressions with year fixed effects and firm fixed effects over the period 1905-1912, and presents standard errors adjusted for clustering by firm. Definitions of all variables, and the methods and sources used to create them, are presented in the Appendix. \*\*, \* and + denote significance at 1%, 5% and 10%, respectively.

**Table 4:**  
**Stock Returns: Wilson's Veto of Measure to Postpone Section 10**

	Railroads				Falsification Test: Industrials and Utilities
	Daily Returns, December 31, 1920 (1)	Cumulative Returns, One-day Window (2)	Cumulative Returns, December 31 to Jan 5, 1921 (3)	Daily Returns, December 29, 1920 (4)	Daily Returns, December 31, 1920 (5)
Percent board underwriting, 1920	-0.047* (0.021)	-0.044 (0.029)	-0.074* (0.034)	0.0437+ (0.024)	-0.007 (0.015)
Constant	0.055** (0.015)	0.062** (0.021)	0.056** (0.027)	0.033+ (0.017)	0.051** (0.007)
Dependent variable stats:					
Mean	0.038	0.046	0.027	0.051	0.048
SD	0.068	0.081	0.095	0.061	0.050
Observations	47	44	42	40	63
R-squared	0.075	0.047	0.101	0.086	0.004

*Note:* Column (1) analyzes the variation in the cross-section of returns on the day the stock market became informed of President Wilson's veto of the postponement of Section 10, 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 cross section of 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. Column (4) analyzes the cross-sectional variation in daily returns on December 29, 1920, the date when the ICC recommended that President Wilson sign the postponement into law. Column (5) reports the results of a falsification test on the daily returns for industrials and utilities, which were not subject to Section 10, on December 31. Robust standard errors. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 5:**  
**Regressions: Firm Outcomes**

	Railroads				Falsification Test: Industrials and Utilities			
	Tobin's Q (1)	Investment Rate (2)	Interest Rate (3)	Leverage (4)	Tobin's Q (5)	Investment Rate (6)	Interest Rate (7)	Leverage (8)
Percent board underwriting in 1913 $\times$ post-1920	-0.044* (0.021)	-0.022** (0.008)	0.005+ (0.002)	-0.045** (0.016)	0.066 (0.079)	0.007 (0.030)	-0.011 (0.007)	0.008 (0.024)
Percent board underwriting in 1913 $\times$ time trend	0.003 (0.003)	0.001 (0.001)	0.0002 (0.0002)	0.001 (0.002)	0.005 (0.008)	-0.003 (0.002)	0.0002 (0.0005)	-0.004+ (0.002)
Log(lag assets)	-0.118* (0.053)	-0.015 (0.011)	-0.004 (0.005)	0.003 (0.029)	0.072 (0.099)	-0.010 (0.015)	-0.004 (0.004)	-0.009 (0.025)
Constant	3.208** (1.002)	0.299 (0.208)	0.119 (0.084)	0.404 (0.526)	-0.048 (1.753)	0.259 (0.270)	0.123+ (0.076)	0.320 (0.372)
Dependent variable stats, 1920:								
Mean	0.783	0.032	0.048	0.350	0.713	0.111	0.062	0.210
SD	0.095	0.029	0.013	0.142	0.163	0.164	0.023	0.160
Observations	1,084	1,286	1,475	1,552	970	1,416	1,021	1,535
R-squared	0.776	0.192	0.465	0.856	0.571	0.227	0.489	0.795
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

*Note:* This table reports equation (1), estimated using firm-level panel data from 1905-29. The first row corresponds to  $\theta_1$ , the main parameter of interest. This captures the effect of our measure of the strength of each railroad's relationship with the bankers on its board, measured as the percent of total underwriting up to 1913 done by the bankers on its board in that year, interacted with a dummy for the time period during which Section 10 of the Clayton Act was in force (the years 1921-29). The second row shows  $\theta_2$ , the estimated effect of that same bank relationship variable interacted with a time trend, to capture any differential trends related to the relationship variable. Columns (1)-(4) present our main empirical tests on our sample of NYSE-listed railroads; columns (5)-(8) present the results of falsification tests in which the same regressions are estimated for NYSE-listed industrial firms and utilities, which were not subject to Section 10. Sources and definitions for all variables are presented in the Appendix. Standard errors clustered by firm. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 6:**  
**Alternative Specifications: Validity of Comparisons Between**  
**Treated and Control Firms**

	Tobin's Q (1)	Investment rate (2)	Interest rate (3)	Leverage (4)
<b>A: Baseline: Binary Treatment</b>				
High underwriting by bankers on board 1913 $\times$ post-1920	-0.044+ (0.023)	-0.024** (0.007)	0.004+ (0.002)	-0.039** (0.014)
High underwriting by bankers on board 1913 $\times$ trend	0.001 (0.003)	0.001 (0.001)	0.0002 (0.0002)	0.003 (0.002)
Observations	1,084	1,286	1,475	1,552
R-squared	0.777	0.194	0.463	0.856
<b>B: Restricted to Common Support; Propensity Score Weighted</b>				
High underwriting by bankers on board 1913 $\times$ post-1920	-0.060* (0.024)	-0.028** (0.006)	0.003 (0.002)	-0.048** (0.014)
High underwriting by bankers on board 1913 $\times$ trend	0.003 (0.003)	0.001 (0.001)	0.0001 (0.0002)	0.004+ (0.002)
Observations	1,057	1,183	1,382	1,429
R-squared	0.784	0.191	0.480	0.810
<b>C: Control Group: Largest Industrials and Utilities</b>				
High underwriting by bankers on board 1913 $\times$ post-1920	-0.157* (0.064)	-0.039+ (0.023)	0.019* (0.009)	-0.071** (0.026)
High underwriting by bankers on board 1913 $\times$ trend	0.0006 (0.008)	0.0003 (0.002)	-0.00003 (0.0004)	-0.002 (0.003)
Observations	568	686	757	860
R-squared	0.631	0.265	0.573	0.840
<b>D: Control Group: Railroads with No Bankers on Board, or Bankers that did not Underwrite</b>				
High underwriting by bankers on board 1913 $\times$ post-1920	-0.047+ (0.025)	-0.020* (0.008)	0.005+ (0.003)	-0.053** (0.016)
High underwriting by bankers on board 1913 $\times$ trend	0.003 (0.003)	0.0003 (0.0007)	0.0001 (0.0002)	0.003 (0.002)
Observations	702	904	1,020	1,097
R-squared	0.738	0.203	0.468	0.891

*Note:* This table presents alternative specifications of equation (1). In Panel A the continuous measure of the strength of railroads' relationships with the bankers on their boards in 1913 is replaced with an indicator variable for the railroads in the top quartile of the distribution of the share of underwriting done by bankers on railroad boards (the "high board underwriting group"). Panel B restricts the observations to those within the common support for the propensity to be in the high board underwriting group, and weights each remaining observations by the inverse of the propensity score. For details, see Appendix Section 3.7. In Panel C the control group consists of the 20 largest industrials and utilities in the sample. In Panel D the control group is restricted to only those railroads that were totally unaffected by Section 10: those that had major investment banks on their board that did not underwrite for them, and those that had no underwriters on their boards. All specifications include year and firm fixed effects, and lagged log assets and a constant term. Standard errors clustered by firm. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 7:**  
**IV Regressions, Railroads**

	Tobin's Q		Investment Rate		Interest Rate		Leverage	
	OLS (1)	IV-2SLS (2)	OLS (3)	IV-2SLS (4)	OLS (5)	IV-2SLS (6)	OLS (7)	IV-2SLS (8)
Pct underwriting, bankers on board 1920 $\times$ post-1920	-0.056* (0.025)	-0.066* (0.032)	-0.026** (0.008)	-0.031** (0.010)	0.005* (0.003)	0.006 (0.003)	-0.034+ (0.017)	-0.054* (0.022)
Pct underwriting, bankers on board 1920 $\times$ trend	0.001 (0.004)	0.005 (0.005)	0.001 (0.001)	0.001 (0.001)	0.0001 (0.0002)	0.0003 (0.0003)	0.002 (0.003)	0.001 (0.003)
Kleinbergen-Paap F-statistic	–	21.57	–	41.87	–	32.50	–	37.10
Observations	1,025	1,025	1,224	1,224	1,401	1,401	1,470	1,470
R-squared	0.773	0.411	0.197	0.084	0.461	0.198	0.851	0.309
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

*Note:* This table presents OLS and IV estimates of equation (2), which analyzes the relationship between the percentage of a railroad's underwriting done by bankers on its board in 1920 and differences in firm outcomes during the period in which Section 10 of the Clayton Act was in effect. The parameters are estimated using firm-level panel data from 1905-29. The first row reports the estimated effect of the new 1920 treatment variable interacted with the post-1920 period. The IV estimates use the percentage underwriting done by the bankers on the board in 1913 to instrument for the 1920 value. The first-stage regressions are reported in the Appendix. The differences between the OLS and IV estimates indicate the direction and magnitude of the bias created by endogenous resignations of bankers from railroad boards in anticipation of the eventual enforcement of Section 10. All specifications include the log of lagged assets and a constant term, which are not reported. Standard errors clustered by firm. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 8:**  
**Further Evidence Consistent with Monitoring**

	Tobin's Q (1)	Investment rate (2)	Interest rate (3)	Leverage (4)
<b>A: Railroads With and Without Debt Maturing in 1921-23</b>				
Pct undw 1913 × high debt maturing 1921-23 × post-1920	-0.102 (0.069)	-0.027** (0.010)	0.011* (0.005)	-0.054+ (0.029)
Pct undw 1913 × low or 0 debt maturing 1921-23 × post-1920	-0.003 (0.030)	-0.020* (0.008)	0.003 (0.003)	-0.041* (0.017)
Pct board undw 1913 × time trend	0.002 (0.003)	0.001 (0.001)	0.000 (0.000)	0.001 (0.002)
Observations	1,034	1,232	1,414	1,458
R-squared	0.784	0.200	0.464	0.823
<b>B: Relationships with High vs. Low-Ranked Underwriters</b>				
Pct undw 1913 × above-median undw rank × post-1920	-0.068** (0.025)	-0.025** (0.008)	0.006* (0.003)	-0.029 (0.020)
Pct undw 1913 × below-median undw rank × post-1920	0.007 (0.035)	-0.014 (0.009)	0.002 (0.003)	-0.080** (0.029)
Pct board undw 1913 × time trend	0.003 (0.003)	0.001 (0.001)	0.000 (0.000)	0.001 (0.002)
Observations	1,084	1,286	1,475	1,552
R-squared	0.780	0.193	0.467	0.858

*Note:* This table presents estimates of modified versions of equation (1), in which the term corresponding to  $\theta_1$  is estimated separately for firms with varying levels of some other characteristic related to bank-firm relationships. That is, instead of a single treatment interacted with a post-1920 variable, in this table we split the treatment into two groups: one for railroads with high levels of some other variable, and another for the other railroads. In Panel A, the railroads are divided between those in the top 10 percent in the distribution of the share of railroad debt maturing in 1921-23, and those in the bottom 90 percent. In Panel B, the railroads are divided between those with underwriters whose rank is above the median, and those with underwriters of below-median rank. All specifications include year and firm fixed effects, as well as the log of lagged assets and a constant term. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.



**Table 9:**  
**Effects of Demand Changes Faced By Railroad Industry**

	Tobin's Q (1)	Investment rate (2)	Interest rate (3)	Leverage (4)
<b>A: Proliferation of Automobiles in Region</b>				
Pct underwriting by bankers on board 1913 $\times$ post-1920	-0.047* (0.023)	-0.024** (0.008)	0.005+ (0.003)	-0.040** (0.015)
Pct underwriting by bankers on board 1913 $\times$ trend	0.003 (0.003)	0.001 (0.001)	0.000 (0.000)	0.001 (0.002)
Automobiles per person 1920 $\times$ post-1920	-0.256 (0.277)	-0.246* (0.120)	0.005 (0.034)	0.011 (0.265)
Automobiles per person 1920 $\times$ trend	-0.029 (0.044)	0.004 (0.009)	-0.004 (0.004)	0.065+ (0.038)
Observations	1,059	1,249	1,432	1,509
R-squared	0.779	0.195	0.473	0.866
<b>B: Development of State Highway Network in Region</b>				
Pct underwriting by bankers on board 1913 $\times$ post-1920	-0.054* (0.023)	-0.029** (0.009)	0.005+ (0.003)	-0.047** (0.016)
Pct underwriting by bankers on board 1913 $\times$ trend	0.005 (0.003)	0.001 (0.001)	0.0001 (0.0002)	0.002 (0.002)
(Miles of road in region / miles of track) $\times$ post-1920	-0.001* (0.001)	-0.0001+ (0.00007)	-0.000006 (0.00002)	-0.0002+ (0.0001)
(Miles of road in region / miles of track) $\times$ trend	0.0002* (0.00008)	0.000003 (0.000005)	-0.000002 (0.000002)	0.00003* (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). 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. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.

**Table 10:**  
**Alternative Effects of Section 10 on Railroads**

	Tobin's Q (1)	Investment rate (2)	Interest rate (3)	Leverage (4)
<b>A: Interlocks With Capital Equipment Suppliers</b>				
Pct underwriting by bankers on board 1913 $\times$ post-1920	-0.044* (0.021)	-0.024** (0.007)	0.004 (0.003)	-0.038** (0.014)
Pct underwriting by bankers on board 1913 $\times$ trend	0.0006 (0.003)	0.0006 (0.0006)	0.0002 (0.0002)	0.003 (0.002)
Interlocks with equipment suppliers 1913 $\times$ post-1920	0.012* (0.006)	0.004+ (0.002)	0.0001 (0.001)	-0.0007 (0.004)
Interlocks with equipment suppliers 1913 $\times$ trend	-0.001 (0.0008)	-0.0001 (0.0002)	0.0003 (0.0007)	-0.0004 (0.0006)
Observations	1,084	1,286	1,475	1,552
R-squared	0.780	0.196	0.465	0.857
<b>B: Interlocks With Competitors Through Firm's Underwriters</b>				
Pct underwriting by bankers on board 1913 $\times$ post-1920	-0.044+ (0.024)	-0.024** (0.007)	0.005+ (0.003)	-0.036* (0.014)
Pct underwriting by bankers on board 1913 $\times$ trend	0.0002 (0.003)	0.0007 (0.0006)	0.0002 (0.0002)	0.003 (0.002)
Competitor interlocks via bankers on board 1913 $\times$ post-1920	-0.002 (0.004)	-0.002 (0.001)	0.0002 (0.001)	-0.003 (0.002)
Competitor interlocks via bankers on board 1913 $\times$ trend	0.0006 (0.0004)	0.0001 (0.0001)	-0.00004 (0.00004)	-0.0003 (0.0003)
Observations	1,034	1,227	1,407	1,488
R-squared	0.785	0.198	0.473	0.863

*Note:* This table presents estimates of modified versions of equation (1). Panel A includes controls for the number of interlocks with capital equipment suppliers for railroads, interacted with a post-1920 indicator and with a time trend. Panel B includes controls for the number of interlocks with competing railroads (defined as those within the same ICC region) created by banks that had actually underwritten for the railroad, 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. \*\*, \*, and + denote significance at 1%, 5%, and 10%, respectively.