Railroad Bailouts in the Great Depression*

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Abstract

The Reconstruction Finance Corporation and Public Works Administration loaned 46 railroads over $802 million between 1932 and 1939. The government's goal was to decrease the likelihood of bond defaults and increase employment. Bailed-out railroads did not increase profitability or employment. Instead, they reduced leverage. Bailing out a railroad had little effect on its stock price, but it resulted in an increase in its bond prices and reduced the likelihood of a ratings downgrade. However, bailouts did not help railroads avoid defaulting on their debt. We find some evidence that manufacturing firms located close to railroads benefited from the bailouts.

KEYWORDS: New Deal, Reconstruction Finance Corporation, Public Works Administration, Railroads
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Introduction

The Reconstruction Finance Corporation (RFC) was created by President Hoover in early 1932 during the depths of the Great Depression. The objective of the RFC was to "make temporary advances upon proper securities to established industries, railways and financial institutions which cannot otherwise secure credit, and where such advances will protect the credit structure and stimulate employment."[1] The Corporation approved $3.9 billion in loans from 1932 until 1939. We call these loans 'bailouts' because they were provided at below-market interest rates and were intended for companies that could not access credit from commercial sources.[2] Most of the RFC's loans went to the financial sector (see e.g., Mason (2001), Calomiris, Mason, Weidenmier, and Bobroff (2013), and Butkiewicz (1995, 1997)), but 8.6% ($802 million, which included rolled-over loans) was approved for the nation's railroads during the eight-year period. We explore whether the RFC's support, along with more limited assistance from the Public Works Administration (PWA), resulted in the achievement of the RFC's twin objectives for the railroad sector. First, did bailouts protect the "credit structure" of the railways, meaning did a government loan help a railroad to avoid default? And second, did bailouts help railroads to increase employment?

We find no evidence that bailouts were successful in reducing railroad defaults, although they reduced the likelihood that the railroads' bonds would receive a ratings downgrade, and they permitted railroads to reduce their leverage. We also find no evidence that government loans were successful in improving bailed-out railroads' employment. However, when a newspaper reported that a railroad applied for a government loan, its bond prices jumped by 0.9% that day. And in the nine days surrounding the application announcement, railroads' bond prices experienced an abnormal return of 4.2%. Similarly, news of a loan approval coincided with a 0.3% increase in bond prices on the day of the approval and a 1.6% abnormal return in the

[1] RFC Final Report (1959), page 1.

[2] In 1932 and 1933, RFC loans were extended at the same interest rate as Federal Reserve loans to member banks. See https://www.federalreservehistory.org/essays/reconstruction-finance-corporation (accessed April 26, 2022).

nine days surrounding the announcement. Government loan applications and approvals are not robustly associated with abnormal returns for the railroads' equity.

Although New Deal railroad assistance was not explicitly aimed at the railroads' customers, we find evidence that firms located in the same city or town as a bailed-out railroad benefited from news of a forthcoming railroad loan. Manufacturing firms that had significant operations overlap with the assisted railroad experienced a 0.9% abnormal return upon announcement of the bailout, compared to a 0.4% abnormal return for manufacturers with low levels of overlap with that railroad.

An advantage in studying railroads during the Great Depression is that most railroads—unlike banks—had publicly traded debt and equity. We can, therefore, study the immediate impact of government assistance on security prices. In addition, there is extremely granular data on railroads, which allows us to know where the railroads operated, the products they transported, and their employment levels. We also have annual financial statements and monthly revenue reports on the railroad firms that received government loans, which are some of the largest firms in existence. The Baltimore and Ohio and the New York Central railroads, for example, had balance sheets in excess of one billion dollars and operated more than 5,000 miles of track. Furthermore, details of government railroad loans were quickly made public by the railroad regulator, the Interstate Commerce Commission (ICC), and reported in the media. It was, by contrast, impossible to observe the immediate impact of government loans in most sectors during the Great Depression. Loans to banks, farms, and industrial firms were largely kept secret, and financial claims on these firms were not usually traded in liquid financial markets.

Railroad bailouts were not intended for railroads in the most precarious financial positions since the RFC was obliged to ensure that loans were "adequately secured." Only nine applications were rejected. Railroads that were successful in obtaining a government loan likely differ from railroads that did not receive government aid. Although we condition our results on the publicly available characteristics of railroads, it is likely that railroads also differed along unobservable dimensions. To address this issue, we take advantage of the political process that was inherent in RFC decision-making. RFC directors were appointed by the President and confirmed by the U.S. Senate. Political considerations appear to have been

important in the decision-making process, as bailouts were more likely to be granted to railroads that operated in the home states of RFC directors. When we use the composition of the RFC board as an instrument for RFC loans, we still find no beneficial effect of loans on railroad employment, profitability, or debt repayment.

Policymakers are often willing to provide aid to the banking system during a financial crisis (see e.g., Bordo and Schwartz (2000), Grossman and Woll (2014), and Lucas (2019)). The objective of such aid is to prevent a reduction in bank loans to the real economy and a resultant recession. Former U.S. Treasury Secretary Hank Paulson looked back in 2018 on the Global Financial Crisis and said, "I would look into the abyss and just see food lines, see a second Great Depression, wondering if one more institution went down how would we put it all back together again."[3] Kelly, Lustig, and Van Nieuwerburgh (2016) show that options markets (correctly) anticipated government assistance to the financial sector, but not other sectors, during the Global Financial Crisis. Direct government aid to the real economy has been rarely attempted during a crisis, although direct aid was a big part of many governments' COVID-19 responses (see e.g., Cirera et al. (2021), Elenev, Landvoigt, and Van Nieuwerburgh (2021), and Granja, Makridis, Yannelis, and Zwick (2020)). A crisis in the financial sector, however, can easily spill over into the real sector, as Benmelech, Meisenzahl, and Ramcharan (2017) demonstrate in the market for automobiles and automobile loans during the 2007-2008 crisis.

The effectiveness of financial system bailouts has been studied extensively, both theoretically and empirically (see e.g., Acharya, Drechsler, and Schnabl (2014), Aghion, Bolton, and Fries (1999), Berger and Roman (2015), Berger, Bouwman, Kick, and Schaeck (2016), Diamond and Rajan (2004), Diamond and Dybvig (1983), Duchin and Sosyura (2012), Ennis and Keister (2009), and Gorton and Huang (2004)). Problems in the financial system during the Great Depression have received much attention (see, among others, Friedman and Schwartz (1963), Bernanke (1983), Calomiris and Mason (1997, 2003), and Benmelech, Frydman, and

[3] https://www.cnbc.com/2018/09/12/bernanke-paulson-and-geithner-say-they-bailed-out-wall-street-to-help-main-street.html

Papanikolaou (2019)). The consensus is that conditions in the financial and banking sector worsened the real effects of the Depression.

Government aid to non-financial firms during a crisis has received little attention in the academic literature. Faccio, Masulis, and McConnell (2006) find that politically connected firms were more likely to be bailed out around the time of the Asian financial crisis, especially if the national government had received an IMF or World Bank aid package. The authors conclude that bailed-out firms that were politically connected continued to underperform non-bailed-out firms in the same industry following the bailout, as measured by the return on assets (ROA). However, the ROA for non-connected firms improved relative to same-industry peers after a bailout. The study does not, therefore, fully determine whether real-sector bailouts are good public policy in a crisis. Bertrand, Kramarz, Schoar, and Thesmar (2018) show that politically connected French firms do not obtain higher subsidies or lower taxes via their connections. In addition, when a company hires a connected CEO, return on assets and profits tend to decline. They conclude that political connections bring few benefits, and notable costs such as an obligation to keep employment high around local elections, to a connected firm.

Goolsbee and Kruger (2015) argue that the bailouts of General Motors and Chrysler in 2008 helped to reduce the economic downturn in the U.S. They conclude, "The rescue has been more successful than almost anyone predicted at the time." Their study is necessarily restricted to two firms since the remaining Troubled Asset Relief Program (TARP) funds went to the financial sector. Berger and Roman (2017) investigate economic spillovers following TARP bailouts of U.S. banks. They find that in the counties in which banks received more TARP funds, there was better net job creation—perhaps because TARP recipients passed on more generous loan terms to their customers (see Berger, Tanakorn, and Roman (2019)). Assistance first went to Wall Street before going to Main Street. While Berger and Roman study indirect assistance to the real sector we examine direct loans (at preferential interest rates) from the government to industry. In contrast, Faccio, Masulis, and McConnell (2006) study direct bailouts from the government to firms.

It is important to study assistance to non-financial firms in a crisis since there are important differences between financial firms and non-financial firms. Financial firms, for example, can

experience 'runs' on the demand deposits that support a bank's assets. In addition, a financial firm can dramatically change its business operations by reducing loans (to preserve cash reserves) or taking on increasingly risky loans to 'gamble on resurrection' (see e.g., Hellmann, Murdock, and Stiglitz (2000) and Dewatripont and Tirole (2012)). In contrast, non-financial firms face few of these issues. U.S. railroads, for instance, often issued 50-year bonds to finance their operations, so there could be no 'run' on the railroad's debt unless their bonds were close to maturity (see Benmelech, Frydman, and Papanikolaou (2019)). Furthermore, taking on increased risk in a crisis is difficult for railroads (or non-financial firms in general) since tracks, and other real assets, are fixed and costly to divert in the search for new customers.

We discuss the economic environment that led to the creation of the RFC and the Corporation's structure in section 2. We describe our data and sources in section 3. We present our main results in section 4 with robustness checks in section 5. We conclude in section 6.

1. The Great Depression and the Reconstruction Finance Corporation

The Great Depression was an unprecedented period of economic and financial collapse worldwide. It struck the U.S. particularly severely, with peak to trough industrial output falling 40% by late 1931 and GDP still 25% below trend six years after the recovery began (see Cole and Ohanian (2004) and Ohanian (2009)). There were several waves of banking crises in the early 1930s (see Bernanke (1983) and Friedman and Schwarz (1963)). In response to the weak economy and runs on troubled banks, President Hoover reluctantly created the Reconstruction Finance Corporation in January 1932, which was a component of what came to be known as the 'New Deal.' The RFC was initially permitted to loan to financial firms and railroads; loans were later permitted to farms, state and local government, infrastructure projects, and industrial loans. The RFC's initial capital stock came from a $500 million appropriation from the Treasury. While it obtained the bulk of its additional funding by issuing notes to the Secretary of the Treasury, a very small part of its operations was provided for by direct borrowing from the public.

The *New York Times* reported on December 19, 1931, that Hoover believed that "the plight of the American railroads is only temporary and that they will be able to work themselves out of the depression."[4] The United States had experienced severe railroad defaults during crises in 1873, 1884, and 1893, in which multiple large lines defaulted, resulting in significant drops in railroad employment (see Schiffman (2003), Giesecke, Longstaff, Schaefer, and Strebulaev (2011), and Cotter (2021)).

Part of the rationale for providing aid to railroads was that many railroads were not capable of repaying their maturing bonds, and it would be exorbitantly expensive for them to obtain alternative funding from the banking sector. In late 1931, Daniel Willard testified in the Senate that railroads "cannot borrow money from banks at less than 8 or 9 per cent interest" when most maturing bonds had coupon rates of around 4 percent.[5] Figure 1 shows the number of new railroad bonds issued by year. Treasury Secretary Andrew Mellon saw the role of the RFC as to provide "a stimulating influence on the resumption of the normal flow of credit into channels of trade and commerce."[6] The Reconstruction Finance Corporation Act became law on January 22, 1932. The initial board of directors was appointed on February 2, 1932, and the first applications were received on February 5, 1932.

RFC loans to railroads were limited to three years duration, had to be 'adequately secured' by collateral, and were restricted to railroads that could not obtain funds on reasonable terms—although no definition of "reasonable terms" was provided. Many railroad loans were made for a three-year duration, and 83.5% of loans in our sample were rolled over. Railroads that were reorganizing under bankruptcy protection were also eligible for RFC loans. Since railroads' normal operations were regulated by the ICC, both agencies had to approve government loans to railroads.

Over the entire period of the RFC's existence (1932-1957), the agency recovered 97.99% of the nominal value of the loans (see RFC Final Report, p. 163). We halt our examination of

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[4] *New York Times*, December 19, 1931, page 4.
[5] *New York Times*, December 23, 1931, page 16.
[6] *New York Times,* December 24, 1931, page 6.

RFC loans in 1939, since the Great Depression is usually considered to have been over by the end of the 1930s.

Although disclosure of RFC loans to banks was sporadic, the ICC had a policy of full and timely disclosure of railroad loans. All railroad loans were publicly disclosed at or near the time of loan application and approval. Loan information was sometimes, however, delayed slightly. The Baltimore and Ohio Railroad's loan application, for example, was kept secret for 10 days in August 1932. In addition, railroads appear to have been occasionally permitted to quietly drop a loan application without being formally rejected. We show the distribution of RFC railroad loans over time in Figure 2. We depict the geographical distribution of loans by state in Figure 3.

The composition of the RFC board was determined by the President and confirmed by the Senate. The initial board's ex-officio members were the Secretary of the Treasury, the Chairman of the Federal Reserve Board of Governors, and the Farm Loan Commissioner. Directorships were balanced by party affiliation, and care was taken for the directors to come from different regions of the U.S. We read press reports and online biographies of the RFC directors to assign, where possible, the directors' 'home' states. For example, the *New York Times* reported that two members of the initial RFC board would be "two Democrats from the Southwest, Harvey C. Couch of Arkansas and Jesse H. Jones of Texas."[7] The RFC final report also describes the home state of most of its directors. We find that the home states reported in the newspaper align with the RFC's designations. We document the composition of the RFC board in Table 1, panel A. Most of the appointed RFC directors were businessmen, and four were former U.S. senators.

New Deal funding decisions are generally considered to have been at least partly politically motivated (see e.g., Wright (1974), Wallis (1987), and Fishback, Kantor, and Wallis (2003)). The RFC's decisions were similarly criticized. In April 1932, Representative La Guardia claimed, "Everybody in the country knows a private wire from J. P. Morgan to the

[7] *New York Times*, January 26, 1932, page 1.

headquarters in Washington dictates the [RFC's] policy."[8] The RFC's initial president, former Vice President Charles G. Dawes, was heavily criticized by Senator Brookhart of Iowa for having loaned over $80 million to Dawes' own Chicago bank.[9] In our analysis, we demonstrate that RFC railroad loans were also partly determined by the geographical origins of the RFC board. We use the composition of the RFC board at the time loans were made as an instrument for loans.

The Public Works Administration also made government loans to railroads from late 1933 until early 1936. PWA loans tended to be smaller than the RFC's disbursements, and they were often used for capital expenditure rather than to service the railroad's debt. Since money is fungible, however, we consider both RFC and PWA loans in our analysis. PWA loans only comprise around 10% of our sample by value, and 15% of our sample by number.

2. Data

3.1 Data sources

We study the Class I railroads of the continental United States. Class I railroads owned over 90% of the nation's tracks by length, they employed roughly 98% of railroad employees (representing 3.4% of the United States' labor force), and they carried over 99% of the revenue-ton-miles of all U.S. railroads in 1929. We collect balance sheet, profit and loss, track network, and employment data for these railroads from the annual reports of the Interstate Commerce Commission, *Statistics of Railways in the United States*.

We compile annual statistics for each railroad's freight revenue sources (i.e., agricultural, animal, mining, forestry, merchandise, or manufacturing items) and monthly revenue from *Moody's Manual of Investments – Railroad Securities*. Data on freight revenue is important because some railroads concentrated on transporting a narrow range of products. For example, the Monongahela Railway Company, the Montour Railroad Company, the Pittsburgh and

[8] *New York Times,* April 7, 1932, page 2.
[9] See *New York Times,* September 16, 1932, page 2.

Shawmut Railroad Company, and the Bessemer and Lake Erie Railroad Company obtained over 90% of their freight revenue from mineral products. As a result of the railroads' varied exposure to product markets, the Great Depression affected railroads unevenly. We use the ICC's classification of railroads' geographical region (i.e., New England, Great Lakes, Central-Eastern, Pocahontas, Southern, Southwestern, Central Western, and Northwestern).

If a railroad had publicly traded equity, we gather stock prices from the Center for Research in Security Prices (CRSP). Many railroads did not have publicly traded equity, usually because they were fully owned by another railway or a related industrial firm. We compile price data on the two most liquid bonds per railroad.[10] We obtain bond prices from the section, 'Bond Sales on the New York Stock Exchange,' in the *New York Times*. We classify a railroad as being in default if it failed to meet a coupon or principal repayment, or if it in any way changed the terms of the issue.[11] Data on bond ratings, coupons, amounts outstanding, and maturity comes from *Moody's Manual of Investments – Railroad Securities*. We use Moody's index of daily railroad bond prices, reported in the *Commercial and Financial Chronicle*, as a proxy for the market return on railroad bonds.

We link the track network of each railroad with two city-level data sources. First, we hand collect data on factories operated by NYSE-listed manufacturing firms from *Moody's Manual of Investments – Industrial Securities*. In total, there are 471 manufacturing firms that have data on factories reported in *Moody's*. Second, we obtain city-level building permits data from Cortes and Weidenmeir (2021). The value of these permits is based on the costs of new commercial and residential buildings for 215 cities across the U.S., taken from *Dun & Bradstreet's Review*.

[10] There are 46 railroads with listed equity and 72 railroads with bonds.
[11] For example, extending the maturity of the bond, reducing the coupon rate, or exchanging the initial bond for another security.

3.2 Bailouts

To identify railroad bailouts, we search the *New York Times* for the phrases "Reconstruction Finance Corporation" or "Public Works Administration" from January 1932 until December 1939. We collect the date of loan applications, approvals, and rejections, as well as the name of the railroad, and the size of the loan. We define an "approval" as the date on which it became clear that the RFC would approve a loan. Informal approval could come before an application. For example, the head of the RFC would occasionally publicly state that the Corporation would be willing to grant a loan to a certain railroad if it were to apply. On February 16, 1939, the *New York* Times quoted RFC chairman Jesse H. Jones: "The RFC was willing to lend $5,000,000 to the Minneapolis and St. Louis Railroad if its reorganization plan is approved by the courts and the Interstate Commerce Commission." Not all approvals, then, were preceded by an application. Similarly, most, but not all, applications are followed by a newspaper report of an approval or a refusal. The approvals/rejections of small applications may not have been newsworthy enough to be reported, and a single approval was occasionally announced for a railroad that had submitted multiple applications in prior months. There were also several occasions when railroads would revise the size of their loan request while the application was under consideration. Therefore, our designations of "applications" and "approvals" occasionally combine multiple information events. Loan decisions were made quickly, usually taking a couple of weeks to a month or two.

We collect information on the composition of the Reconstruction Finance Corporation board from the *Final Report on the Reconstruction Finance Corporation* (1959). We obtain information on the home state of the board members from reports in the *New York Times* and the RFC final report. Bank capital in default comes from the *Annual Report of the Comptroller of the Currency*. Bank capital per state comes from Flood (1998).

4 Results

4.1 RFC Board composition

In Table 1, panel B, we investigate the composition of the RFC board. The composition was supposed to be balanced by party affiliation and geographically diverse. However, it is possible that the appointment of RFC directors was partly determined by economic conditions in the home state of the director or even by financial conditions in the railway sector in their home states. Larger states were more likely to have RFC directors, and we find evidence that directors were less likely to be appointed if there was already a director from the same state. Our results show that the appointment of directors is not robustly associated with economic conditions in the directors' home states. Therefore, concerns are alleviated that causality runs from the poor economic conditions of railroads to the appointment of RFC directors and thence to more railroad bailouts.

4.2 Summary statistics

In Table 2, we present our summary statistics on railroads. We divide railroads into those that were "bailed-out"—which we define as having received at least one loan from the RFC or the PWA between 1932 and 1939—and those that were not bailed-out. In Panel A, we show that there are large differences between the bail-out recipients and others. Bailed-out railroads had less cash to total assets (a mean of 1.5% of assets vs. 2.4% for non-bailed-out railroads), were slightly less levered (mean long-term debt to total assets of 41.6% vs. 43.9%), were less profitable (a mean net income to total assets of 0.9% of total assets vs. 1.6%), and had less volatile operations (monthly volatility of 14.1% vs. 21.4%). Bailed-out railroads were much larger (mean total assets of $260.7 million vs. $22.7 million), employed more people (a median of 12,750 vs. 1,100), had a higher wages component of costs (63.6% of operating expenses vs. 61.6%), and had more (same-state) connections to the RFC board. On average, bailed-out railroads operated in 1.481 states with an RFC director vs. 0.745 for non-bailed-out railroads.

Since the statistics in Panel A combine observations before and after the loans, we examine *ex-ante* differences between loan recipients in Panel B. We find that the differences between

bailed-out railroads and non-bailed-out railroads in 1929 mirror those in the full sample. Loan recipients had less cash, higher employment and assets, and were less volatile than railroads that did not receive a bailout.

In Panel C, we split the bailed-out railroads into two groups: those that received a single loan from the RFC or PWA and those that received multiple bailouts between 1932 and 1939. The railroads that received multiple bailouts tended to have less cash (mean 1.4% of total assets vs. 1.8%) and much lower profitability (0.8% of total assets vs. 1.6%). The companies that obtained multiple bailouts tended to be larger (mean size of $289.6 million vs. $126.3), employ more people (a mean of 13,413 people vs. 4,196 people), focus more on passengers (11.5% vs. 8.5% of total revenue at the mean) and have more (same-state) connections to the RFC board (on average 1.662 vs 0.736).

4.3 Bailout recipients

We use a two-step model to investigate which railroads received government bailouts. In the first stage, we run a probit model of a railroad's application (*Application* equals one) on time-varying firm characteristics. We find that railroads with less cash and those that were younger, larger, and less reliant on passenger revenue carriers were more likely to apply for a bailout. A one percent increase in the railroad's age decreases the probability of applying for a bailout by 5.63%.

In the second stage, the dependent variable is an indicator that equals one if a railroad received a bailout (*Approval*) at least once in a certain year. This two-step regression allows us to address potential selection biases that may arise from the endogenous approval of railroad bailouts. Among railroads that apply for a loan, the number of political connections was a critical factor in determining who was approved. An additional RFC *Connection* increased the probability of receiving a bailout by 21.1% (column 2). This result is robust to using an OLS (column 3) or Heckman (column 4) specification. The political process behind RFC/PWA loan approvals was vital for a railroad to receive funding, in contrast to economic factors such as leverage, profitability, or bonds that were close to maturity.

4.4 Market reactions to bailouts

We examine the reactions of a railroad's stock and bond prices to news of bailout applications and approvals. In Table 4, we compute abnormal returns (AR) and cumulative abnormal returns (CAR) on railroad debt and equity. We choose two benchmark bonds for each railroad, or one if there was only a single bond issued. We select the most liquid bonds traded on the New York Stock Exchange. We compute abnormal returns as the return on the stock or the bond less the CRSP market return or Moody's railroad bond index return, respectively.[12] Mason and Schiffman (2003) calculate that in 1931, 31% of railways' debt was held by insurance firms, 17% by banks, 4% by foundations and educational institutions, and 7% by other railroads, with the remainder held by "other" investors.

We find statistically significant abnormal returns of 0.9% for bonds on the day a loan application was announced and an abnormal return of 0.3% on the day an approval was announced (see Panel A). There is no statistically significant AR on refusal announcement dates, although there is a -5.9% AR the next day. In the window around the news release (t-4 to t+4), we find CARs of 4.2% (applications), 1.6% (approvals), and -2.5% (refusals), although the refusal return is statistically insignificant.

Since many railroads applied for (and were granted) multiple loans, we investigate the differences between the initial loan and subsequent loans. Substantially more private information is likely to have been conveyed to the market by a firm's initial revelation that it desired federal government financial assistance. An application announcement for the first bailout is associated with a 9.8% bond CAR from t-4 to t+4, although the 2.6% AR on day zero is insignificant (see Panel B). An approval announcement for the first bailout has a 0.9% bond AR on day zero (with a 4.0% CAR from t-4 to t+4), all statistically significant. Subsequent bailouts are reflected in more subdued bond responses. A second or subsequent application has a 0.4% bond AR on announcement day (2.4% from t-4 to t+4), both statistically

[12] We only hand collect bond prices in a narrow window around RFC announcements. Therefore, we are unable to estimate a market model for railroad bonds. To maintain consistency in our measurement of abnormal returns between bonds and stocks, we compute abnormal returns for railroad stocks in the same manner. Effectively, we assume that alpha equals zero and beta equals one in the market model.

significant. A second or subsequent approval has a 0.2% AR (insignificant) on day zero and a statistically significant 1.1% CAR from t-4 to t+4.

In contrast to the response of bond prices, there is little statistically significant movement of stock prices in response to bailout news events (see Panels A and B). Most estimates are statistically insignificant, including the extremely large abnormal return of -23.9% on the day of the initial loan approval. News of subsequent RFC approvals resulted in a 1.5% AR on the announcement date and a 2.3% CAR (both statistically significant) from t-4 to t+4.

4.5 Determinants of announcement returns

We examine the association between a railroad's characteristics at the time of the bailout and its announcement returns. In Table 5, we regress the CAR of the railroads' bonds and equity (from t-4 to t+4) on firm and bailout variables. We find few railroad characteristics that are robustly associated with security returns. Most characteristics are insignificant and change signs depending on whether we examine applications versus approvals or bonds versus shares. Railroads with more leverage experience substantially worse returns on their equity upon announcements of loan applications, perhaps because a loan application indicated the railroad would struggle to service its debt, and therefore that equity was next to worthless. A one standard deviation increase in leverage corresponds to a 9.60% larger CAR. Railroads with more employees tended to have lower announcement returns, perhaps because market participants expected government pressure on the railroad to maintain employment.[13] A one standard deviation increase in employment is associated with a 2.03% smaller CAR. In Panel B, we distinguish between initial and subsequent bailouts. Again, we fail to find robust relations between characteristics and abnormal returns, although higher leverage was generally associated with worse returns for debt and equity.

[13] An alternative explanation is that investors may have perceived that railroads that received bailouts would be more generous with their employees' compensation.

5 Effectiveness of government bailouts

5.1 Bond defaults

We now turn to the central question: did the RFC achieve its stated objectives? We start by examining if it achieved its first objective, protecting the "credit structure" of the financial system. All else equal, an RFC loan should have made a railroad less likely to default on its debt. Jones (1951) claims that RFC funding reduced railroad defaults by half, whereas Schiffman (2003) and Mason and Schiffman (2004) claim that bailouts at least delayed defaults. However, defaulting on debt is partly a choice, and Mason and Schiffman argue that bankruptcy "brought relief from high fixed charges that were often a principal cause of financial distress" (p. 61). In Figure 4, we plot Kaplan-Meier (1958) graphs with the cumulative probability of failure for bailed-out vs. non-bailed-out railroads. We observe that railroads that received a bailout are associated with a higher hazard rate of bond defaults and that this difference increases over time. In Panel B, we show that the higher default rate for bailed-out railroads survives the inclusion of control variables. The granting of a below-market-rate loan, *ceteris paribus*, is a good event. Therefore, higher default rates for a bailed-out railroad suggest that unobservable factors are likely influencing a railroad's performance and the government's proclivity to grant bailouts.

We assess the effects of bailouts on bond defaults in Table 6. In column 1 we run a probit model of defaults in which the dependent variable equals one if a railroad defaulted on its bonds in that year. We examine if a government loan *Approval* in the previous year is associated with the railroad defaulting upon its debt. We attempt to capture railroad unobservables by including bond rating fixed effects from Moody's. In this era, Moody's only released ratings once per year in its annual investors' compendium. Most railroads had multiple bonds and bond ratings so we use the rating of the bond closest to maturity. Overall, we show that lower net income, lower cash to assets, more bonds maturing in the depths of the Depression (1930 to 1934), and youth are associated with a higher likelihood of default. Government loan *Approval* does not have a statistically significant relation with defaults.

In column 4 we run a related probit model of defaults, but the dependent variable now equals one if a railroad defaulted on its bond in that year or the following two years. This offers a longer-run investigation of how a government loan *Approval* is associated with a railroad's likelihood of default. We find that *Approval* has a significantly positive correlation with the probability of railroad default (at the 5% confidence level). Indeed, getting an RFC or PWA bailout is associated with a default rate of 6.39%, all other characteristics at sample means, relative to an unbailed out railroad's default rate of 1.51%. This finding is in line with the Kaplan and Meier graphs in terms of magnitude.[14] However, railroad bailouts are unlikely to be awarded at random, and a selection effect is likely to be present. Hence, we turn to an instrumental variable approach to determine if bailouts have a causal effect on railroad defaults.

5.2 Instrumenting for bailouts

Our major concern in determining if bailouts aided railroads is that there are likely to be omitted variables in our econometric specification that partly affect a railroad's financial performance. Railroad management and policymakers on the RFC board were likely to have had access to information that we do not. For example, a railroad that had tried but failed to obtain bank or Wall Street assistance to raise additional funds would be more likely to default than its observable characteristics would otherwise suggest. Railroad management may well have been able to convey that information to the RFC board in order to increase the likelihood that an RFC loan would be granted. In that situation, the error from the regression of bond defaults on bailouts is likely to be correlated with the independent variable *Approval*. Therefore, the coefficient estimates on *Approval*, which measure the effectiveness of government aid, will be biased.

We would like to use an instrumental variable that is correlated with a railroad receiving a government bailout but only affects a railroad's financial performance via the granting of RFC loans. We take advantage of the prior literature (see e.g., Wright (1974), Wallis (1987), and

[14] This finding is robust to changes in the regression specification, such as OLS fixed-effects models.

Fishback, Kantor, and Wallis (2003)) that claims New Deal grants were influenced by politics. Fishback (2017), for example, concludes: "Nearly every study finds that political considerations were important to the Roosevelt administration." There are, however, a few investigations of New Deal funding--such as Mason (2003)--that find little political influence on the process. We use the composition of the RFC board as our instrumental variable. Specifically, we use the number of states a railroad passed through that were the home states of RFC directors in that particular year. We call this the number of a railroad's *Connections* to the RFC. For example, on February 5, 1932, the Chicago and Eastern Illinois railroad applied for an RFC loan for $3.629 million. This railroad passed through Illinois, Indiana, and Missouri. H. Paul Bestor (Missouri) and Charles G. Dawes (Illinois) sat on the board of the RFC at the time of the application. Therefore, our instrument takes a value of two.

In our first stage regression (Table 6, columns 2 and 5) we regress *Approval* on a railroad's lagged characteristics and our instrument, *Connections*. We see that *Approval* is positively and statistically significantly related to a railroad's *Connections*, even with region and year fixed effects. The F-statistic in the first stage regression is 75.791, which indicates that we have a strong instrument.

To have a valid instrument, we also require that the exclusion restriction is satisfied. The exclusion restriction requires that *Connections* are uncorrelated with the error term, the unobservable part of a railroad's financial position that partly determines default behavior. There was, however, no realistic possibility that a railroad that was doing poorly based on unobservable factors would increase its number of *Connections* by altering its operations. Total track mileage in the U.S. declined from 1930 onwards, and it would be expensive and take years of construction for an existing railroad to begin operations in the home state of an RFC director.[15]

It also would invalidate our instrument if railroads that were in worse financial shape than their observable characteristics would suggest were able to influence the president to alter the

[15] See ICC *53rd Annual Report on the Statistics of Railways in the United States* (1939), Table 1-A.

RFC board's composition, such that a new director was appointed from a state in which the railroad operated. RFC directors were responsible for approving all loans that the Corporation made, railroad and non-railroad alike. Total railroad loans comprised less than 10% by value of the RFC's disbursements, and loans to an individual railroad were a tiny fraction of total RFC expenditure. There were only five to seven directors at any one time, and the composition was balanced by political affiliation and by the need to have directors come from different parts of the country. Given these constraints on the composition of the RFC board, we believe it is extremely unlikely that certain railroads could have increased their *Connections* by lobbying. Therefore, we use *Connections* as our instrument for RFC bailouts.

In column 3 of Table 6, we replace *Approval* with the predicted level of *Approval* from our first-stage regression. We see that bailouts increased, rather than decreased, the likelihood that a railroad would default on its bond. This coefficient is statistically insignificant. We also observe that railroads that were more profitable and had more cash were less likely to default, as expected. More bonds falling due from 1930 to 1934 is positively associated with more defaults. Once we focus on the longer-run impact (column 6), the estimated coefficient on government loan *Approval* is still positive but statistically insignificant. Other railroad characteristics are little changed.

Overall, it is difficult to believe that receiving a government loan, all else equal, increased the likelihood of a bond default, but we interpret our findings as a lack of evidence that RFC loans helped railroads to avoid defaulting on their debt.

5.3 Bond ratings

Bailed-out railroads may have been viewed as "too big to fail" or perhaps investors anticipated that a bailout indicated that the government would share the financial losses with bondholders. In this case, the bond market may have perceived the railroad's debt as being safer, despite our evidence in Table 6 showing that bailouts did not help a railroad to avoid default. To investigate perceptions of a railroad's default, we turn to an examination of bond ratings from Moody's.

In table 7 we examine the likelihood that, conditional on receiving a government bailout (*Approval*) last year and last year's observable characteristics, if a railroad would receive a downgrade in the current year. A railroad bond has a 12.65% lower probability of being downgraded in the current year if the railroad was bailed out in the previous year (column 1). Bonds have a 6.52% lower probability of receiving multiple downgrades, in year *t*, *t+1*, or *t+2*, if the firm was bailed out in year *t-1* (column 4). Railroads with more cash and lower employment are less likely to be downgraded.

Since concerns about selection effects remain, we again run instrumental variable probit regressions. The first stage regressions appear in columns 2 and 5, and the second stage results are in columns 3 and 6. The IV results confirm the probit results. Bailouts in the previous year are associated with a decrease in the likelihood of getting one downgrade of 47.46% (column 3) or a decrease in the likelihood of getting multiple downgrades of 87.13% in the subsequent three years (column 6). Railroads that increased their employment or decreased their cash-to-assets ratio were more likely to be downgraded. This result suggests that Moody's perceived increased railroad employment during the Great Depression was incompatible with protecting bondholders' interests. Overall, our results show that government bailouts did not protect railroads against default, although they did alleviate bond ratings downgrades.[16]

5.4 Operating performance, difference-in-differences

We now investigate if the RFC succeeded in their second objective, which was to "stimulate employment." We determine the ability of government bailout recipients to improve their economic performance, including their employment numbers. We first conduct a difference-in-differences approach on RFC loan recipients' profitability, leverage, employment, and

[16] In Table 6, we show that Moody's ratings add information to understand bond default dynamics, even after controlling for firm characteristics. However, Moody's ability to discern between good and bad corporate bonds comes mostly from non-investment grade bonds. Moody's ratings add very little information for investors that is not already conveyed by firm characteristics for investment grade bonds. The only bond rating fixed effects which are significantly different to zero are the lowest rated C and Ca. Most government bailouts went to railroads with investment-grade bonds. Government bailouts helped such bonds to preserve their (high) credit rating (Table 7) and since investment-grade bonds are very unlikely to default, a bailout did not greatly change their default risk (Table 6).

wage bill as a fraction of total expenses. In Table 8, we use the technique of Callaway and Sant'Anna (2020) to deal with staggered bailouts. For each bailed-out railroad's initial government loan, we choose a matched railroad. The matched railroad must operate in the same ICC region and have total assets that are most similar to those of the bailed-out carrier. In addition, the matched railroad must not have received a bailout during the period between two calendar years before the bailout and two calendar years after the bailout. If a railroad received multiple bailouts, we also include those observations—as long as the subsequent bailouts were at least three years after the prior bailout.

In Panel A, we present average treatment effects (ATT).[17] We find negative but statistically insignificant effects of a bailout on railroad profits (columns 1-3). The estimated treatment effect for leverage is a reduction of 3.3 percentage points for the bailed-out railroad (column 4). The starting leverage for the average railroad was a little over 40 percent of total assets (see Table 1). We find positive but economically small and statistically insignificant effects of a bailout on employment (columns 7-9). In contrast, we note statistically significant increases in the fraction of total expenses going to the wage bill of around 4.5%. Our results of weak employment and generous wages align with the findings of Ohanian (2009) and Cole and Ohanian (2004) that New Deal policies deepened the Great Depression.[18] Railroads appear to have used some of the government funds to inflate their wages bill.

In Panel B, we present estimates of the treatment effect by year. There are no statistically significant impacts on profitability or employment after the treatment. In contrast, we find that leverage decreases in years t+1 through t+4, but it is barely affected in the year of the bailout. Wages increase in the year of the bailout and continue to increase for the following three years.

In Panel C, we run a placebo test in which we counterfactually assume that all RFC bailouts took place in 1929, while still focusing on the "actual" bailed-out vs. non-bailed-out railroads (as in Berger and Roman (2017)). For the placebo test, we use the years 1927 to 1928 as the "pre-RFC period," and the period between 1930 and 1932 as the "post-RFC period." We apply

[17] Using a chi-squared test, we highlight that the parallel trends assumption is never violated in Table 8.
[18] Schiffman (2003) finds that railroads that defaulted increased their employment following the default.

the doubly-robust difference-in-differences approach of Sant'Anna and Zhao (2020). We fail to find significant results for profitability, leverage, employment, or the wage bill with our placebo.

5.5 Operating performance, instrumental variables

Although the difference-in-differences approach should give us a good idea of the impact of a railroad bailout, there remain concerns that the comparison group of non-bailed-out railroads does not provide an accurate counterfactual for the bailed-out carriers. In Table 9, we again make use of the board composition of the RFC and our measure, *Connections*, as an instrument for railroad bailouts. In the second stage, we regress railroad profitability, leverage, log employment, and the wage bill fraction on the fitted level of bailouts after conditioning on railroad characteristics.

We find no statistically significant impact of railroad bailouts on profitability (column 2), employment (column 4), or the wage bill fraction (column 5). We do find that a bailout causes an 8.8 percentage point decrease in leverage (column 3). Therefore, we conclude that the RFC failed in their second objective, which was to promote railroad employment via their loan program. All regressions use firm, year, and railroad region fixed effects and condition on lagged characteristics, including the railroad's freight composition. Our results are in line with those of Granja, Makridis, Yannelis, and Zwick (2020) who find that small business support payments during the coronavirus pandemic were often used to make non-payroll payments and to build up savings buffers. Railroads appear to have used bailouts to reduce their leverage with no beneficial impact on employment.

5.6 Economic spillovers

Bailouts do not seem to have provided any direct benefits for the recipient carriers—save a jump in the value of their debt. They may, however, have provided spillover benefits for the regions in which they operated. For example, railroads may have been able to keep operating routes that would otherwise have been closed, or they may have conducted a more frequent

schedule that permitted local businesses to operate more smoothly than if government support had not been made available.

We examine if there were positive economic spillovers that flowed from the bailouts of railways that passed through a city. We create an explanatory variable, *City RFC Approvals*, which equals the fraction of all railroads that operate in a city that received an RFC or PWA loan in the previous year. We again use our instrumental variable, *Connections*, which is measured at the state level, as an instrument. We regress the natural logarithm of city building approvals per capita in a year on fitted *City RFC Approvals*.[19] In Table 10, we see that RFC board connections are very strong instruments for city-level loan approvals. We find a negative relationship between railroad city-level loan approvals and new building approvals (columns 2 and 4). Once we add both year and city fixed effects, however, the estimated impact of *City RFC Approvals* on building approvals becomes statistically insignificant and close to zero in magnitude (column 6).

In Table 11, we examine if news of a railroad's bailout affected other railroads and manufacturing firms listed on the NYSE.[20] We calculate the abnormal returns of other firms on the day of the railroad's approval announcement and the cumulative abnormal return from the day before to the day after the approval for the other railroads and manufacturing firms. The NYSE hosted three main types of firms: railroads, manufacturing firms, and utilities. Therefore, the abnormal returns essentially measure the extent to which the railroad sector outperformed manufacturing and utilities on the day of a railroad bailout (when we calculate the abnormal returns for non-bailed-out railroads), and the extent to which the manufacturing sector outperformed railroads and utilities (when we calculate the abnormal return for manufacturing firms). The more interesting evidence looks at the cross-sectional impact of

[19] Our thanks to Gustavo S. Cortes for sharing his data on building approvals.

[20] We exclude all stocks that have a zero return on all days of the event study.

bailouts: which manufacturing firms and which railroads benefited most from news of one railroad's bailout?

We cross-sectionally split firms on two dimensions. First, did the other railroad overlap at all with the bailed-out railroad, meaning did both railroads service at least one common city (*Yes*) or not (*No*)? Second, was the level of overlap (the fraction of the bailed-out railroad's cities also serviced by the other railroad) above the sample mean (*High*) or was the overlap positive but below the sample mean (*Low*). We construct similar measures of overlap for the manufacturing firms, but we consider joint presences of manufacturing establishments (as reported in *Moody's Manual of Investments – Industrial Securities*) and railroad tracks.

In Panel A, we see that the mean CAR for all types of railroads was large and positive at the time of an application, but there was no statistical difference between railroads that overlapped with the bailed-out railroad and non-overlapping railroads. In Panel B, we observe slightly larger, 0.5% (*No* less *Yes*) to 0.6% (*Low* less *High*), and statistically significant differences in CARs for other railroads. We interpret this result to mean that competing railroads (i.e., those with some overlap with the bailout recipient) suffered from a bailout relative to railroads that had little or no overlap. As this is a cross-sectional test, we are conditioning on any economy-wide railroad shocks such as changes in government railroad policy, input costs, or demand changes.

Manufacturing firms experienced positive abnormal returns at the time of a railroad bailout (relative to utilities and railroads themselves). Again, however, our interest lies in the differences between manufacturing firms that were co-located in the same city as the bailed-out railroad (an overlap of *Yes* or *High*) and manufacturing firms that were not located in cities through which the bailed-out railroad ran (*No* or *Low*). We see that a railroad bailout benefited co-located manufacturing firms relative to manufacturers that were not located near the bailout recipient's tracks. Co-located manufacturing firms outperformed others by 0.1% (*Yes* vs. *No*) at the time of the application and 0.2% at the time of the approval. If we compare *High* vs. *Low* manufacturing firms, we see that high- overlap manufacturers outperformed by 0.6% at the time of application (Panel A) and 0.3% at the time of approval (Panel B). Taken together,

we feel there is some evidence that there were positive economic spillovers to the real economy from railroad bailouts, even if the railroads themselves showed little benefit.

5 Conclusion

The RFC distributed much of the U.S. government's New Deal assistance to the economy as it struggled with the Great Depression. Around 10% of the RFC's loans were given to private firms in the railroad sector. In our study, we ask if RFC assistance aided railroads in avoiding debt defaults and maintaining employment. We find no evidence that government assistance was beneficial for the recipient railroad and some evidence that government assistance caused harm – point estimates are that government bailouts increased the likelihood of a debt default. A bailed-out railroad may have felt pressure to maintain employment at higher than desired levels and/or to keep wages above market levels as the Depression deepened. Our results extend the work of Bertrand, Kramarz, Schoar, and Thesmar (2018). We find that political connections were useful for railroads seeking government assistance, in contrast to their study where connections did not result in additional subsidies or tax relief. However, our conclusion is similar to theirs in that RFC bailouts did not appear to greatly benefit the recipient firms.

Non-bailed-out railroads that competed with the bailout recipient seem to have suffered some harm from the bailout, presumably because one of their competitors was supported financially. We find some evidence that government railroad support was beneficial for manufacturing firms that were co-located near the railroad's tracks. Therefore, although RFC and PWA assistance proved of little benefit to the railroad itself, there were positive economic spillovers from this New Deal program.

Figure 1: New railroad bond issues

Number of new bond issues by all class I railroads between 1927 and 1939

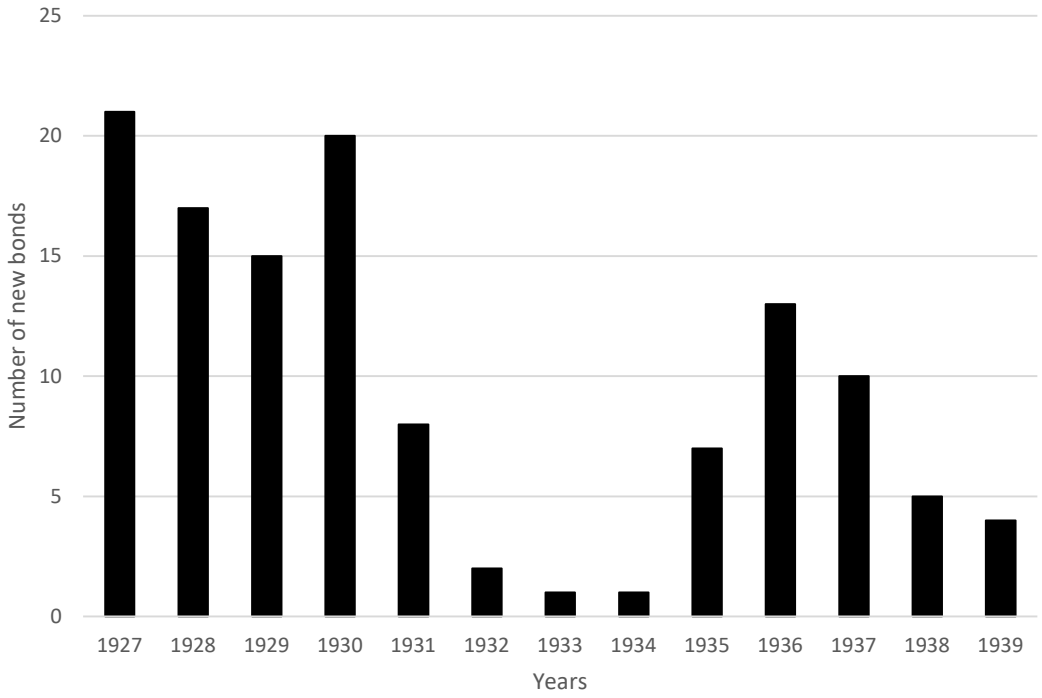


Figure 2: RFC railroad loans ($ million, including roll-overs)

Value of approved bailouts for all class I railroads between 1932 and 1939 on a quarterly basis

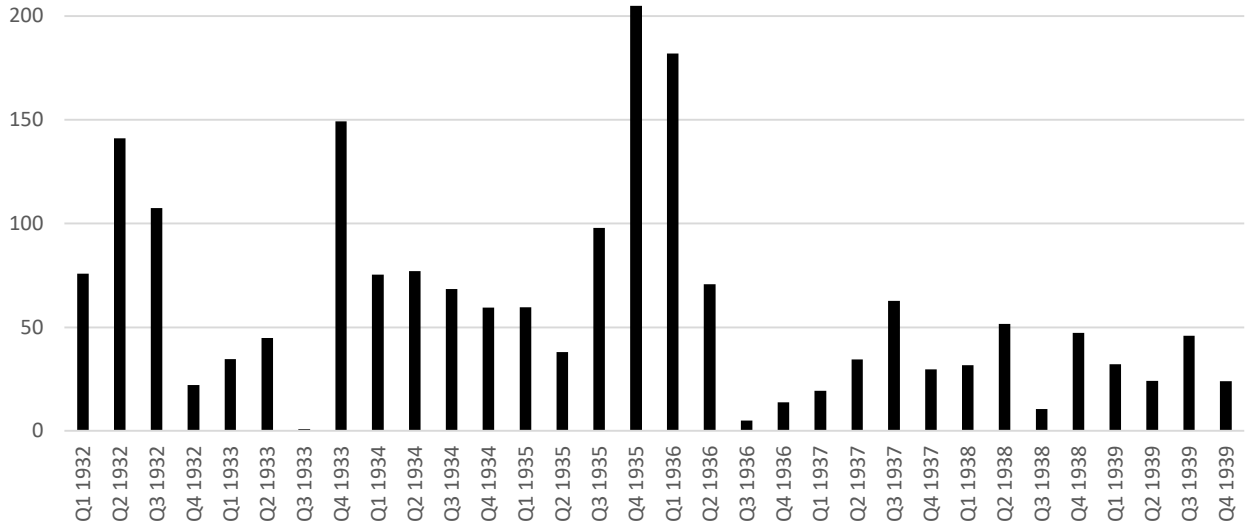


Figure 3: Number of loan approvals

The number of RFC or PWA loans to railroads that operated in each state. For railroads that operated in more than one state, we count each state in which that railroad operated as having received a loan.

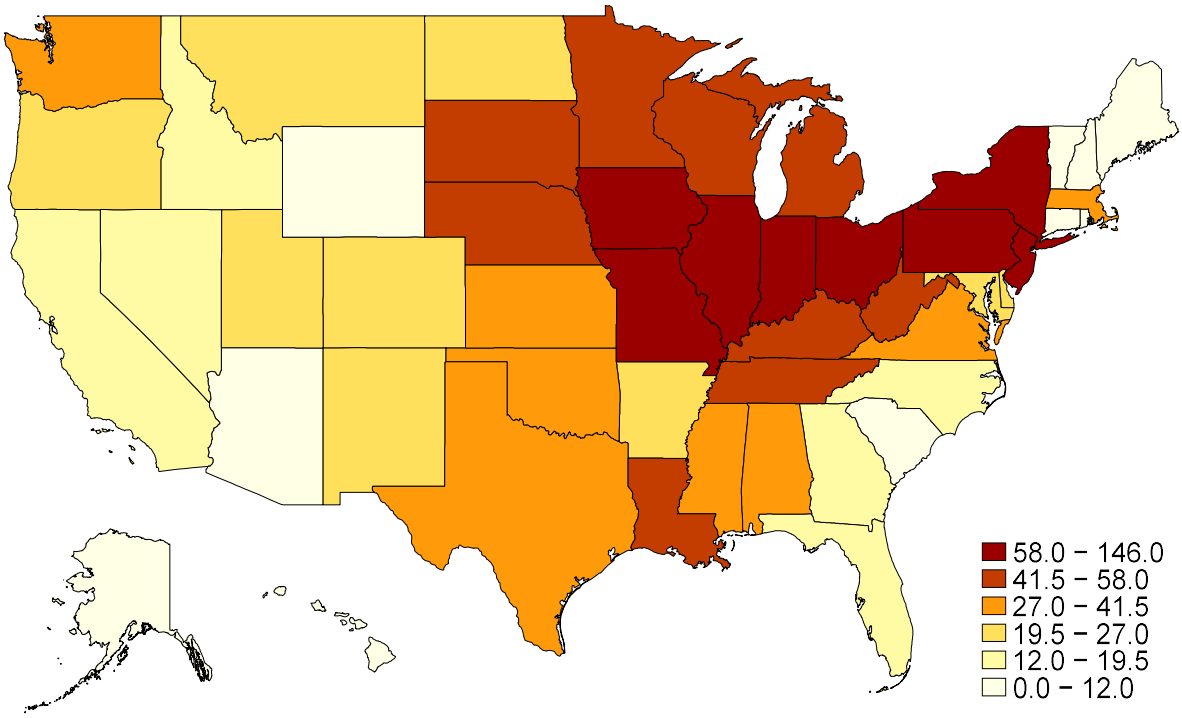
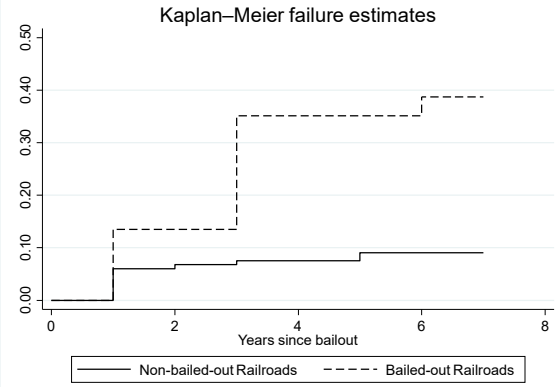


Figure 4: Kaplan-Meier failure graphs

We show the hazard rates of bond defaults in the years after a bailout. Panel A shows the hazard rates for bailed-out vs. non-bailed-out railroads. Panel B shows the hazard rates after controlling for lagged log total assets, net income to total assets, cash to total assets, leverage, log employment, log firm age, volatility, and the freight composition.

Panel A. Benchmark model



Panel B. Controlling for covariates

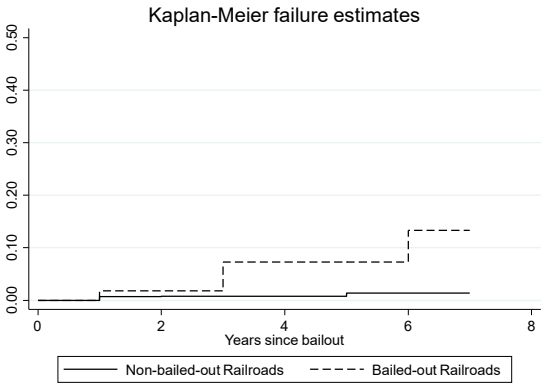


Table 1: RFC board composition

Panel A reports the RFC board composition between 1932 and 1939. *Party* refers to the respective RFC member's political party, where *Dem*. refers to Democrat and *Rep*. refers to Republican. *State* refers to the respective member's home state (abbreviated). *Start* and *End* refers to the member's start and end time on the board, respectively. *Comments* gives insights into their background. In Panel B, the dependent variable equals one if an RFC member from state *y* was appointed to the board in year *t*, zero otherwise. Column 1 includes all RFC members; column 2 excludes the first RFC board; column 3 examines RFC members with previous political experience; column 4 examines RFC members from the private sector. *Bank Capital in Default* is the ratio of national bank capital in default to total national bank capital in state *y*. Log(*Size per Railroad*) is the logarithm of the total assets of railroads active in state *y* divided by the number of railroads active in state *y*. *Number of Railroads* is the number of railroads active in state *y*. *Railroad Bailout Weight* is the ratio of the total assets of bailed-out railroads in year *t* active in state *y* to the total assets of railroads active in state *y*. Log(*Building permits per capita*) is the log of building permits divided by the state population in state *y*. *Banks per capita* is the number of national banks in state *y* divided by state population. *Number of RFC members* is the number of existing RFC members from state *y*. All regressions use year and state fixed effects. We cluster standard errors at the state level. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Name	Party	State	Start	End	Comments
H. Paul Bestor	Rep.	MO	Feb-32	Jul-32	ex officio as Farm Loan Commissioner
Eugene Meyer	Rep.	NY	Feb-32	Jul-32	ex officio as governor of Fed. Reserve
Andrew W. Mellon	Rep.	PA	Feb-32	Feb-32	ex officio as Treasury Secretary
Ogden L. Mills	Rep.	NY	Feb-32	Mar-33	ex officio as Treasury (Under) Secretary
William Woodin	Dem.	NY	Mar-33	Dec-33	ex officio as Treasury Secretary
Arthur H. Ballantine	Rep.	-	Feb-32	May-33	ex officio as Treasury Under Secretary
Dean H. Acheson	Dem.	-	May-33	Nov-33	ex officio as Treasury Under Secretary
Henry Morgenthau (Jr.)	Dem.	NY	Nov-33	Feb-38	ex officio as Treasury (Under) Secretary
Thomas J. Coolidge (III)	Dem.	MA	May-34	Feb-36	ex officio as Treasury (Under) Secretary
Roswell Maginn	Dem.	IL	Jan-37	Feb-38	ex officio as Treasury (Under) Secretary
Harvey C. Couch	Dem.	AR	Feb-32	Aug-34	Arkansas businessman (electricity, railways)

Charles G. Dawes	Rep.	IL	Feb-32	Jun-32	Former Vice President and Chicago banker
Jesse H. Jones	Dem.	TX	Feb-32	Jul-39	Texas businessman (lumber, real estate, banking)
Wilson McCarthy	Dem.	UT	Feb-32	Sep-33	Utah state senator and district attorney
Gardner Cowles (Sen.)	Rep.	IA	Jul-32	Apr-33	Des Moines newspaper proprietor
Charles A. Miller	Rep.	NY	Aug-32	Mar-33	Utica banker
Atlee Pomerene	Dem.	OH	Aug-32	Mar-33	Ohio lawyer and former U.S. senator
Carroll B. Merriam	Rep.	KS	Jun-33	Dec-41	Topeka finance industry
John J. Blaine	Rep.	WI	Jun-33	Apr-34	Lawyer and businessman, former U.S. senator
Frederic H. Taber	Rep.	MA	Jun-33	Jan-38	New Bedford lawyer
Charles B. Henderson	Dem.	NV	Feb-34	Jul-47	Former U.S. senator and lawyer
Hubert T. Stephens	Dem.	MS	Mar-35	Feb-36	Former U.S. senator and lawyer
Charles T. Fisher (Jr.)	Rep.	MI	Mar-35	Dec-36	Detroit banker
Emil Schram	Dem.	IN	Jun-36	Jul-41	Indiana farmer and irrigator
Howard J. Klossner	Rep.	MN	Apr-37	Jul-45	Minnesota banker
Sam H. Husbands	Dem.	SC	Aug-39	Jan-46	South Carolina banker

Panel B – Determinants of RFC Board composition				
	RFC Board	RFC excl. First	Political background	Business background
	(1)	(2)	(3)	(4)
Bank Capital in Default	0.078	-0.037	-0.069	0.032
	(0.530)	(0.690)	(0.410)	(0.326)
Banks per capita	2.577	1.515	-0.659	2.174*
	(0.577)	(0.495)	(0.606)	(0.096)
Log(Building permits per capita)	-0.019	-0.028	-0.003	-0.025
	(0.682)	(0.500)	(0.885)	(0.475)
Log(State Population)	0.716	0.879	0.901*	-0.021
	(0.418)	(0.141)	(0.061)	(0.928)
Log(Size per Railroad)	0.773	-0.452	-0.465	0.013
	(0.428)	(0.310)	(0.231)	(0.943)
Log(Number of Railroads)	0.108	-0.014	-0.024*	0.011
	(0.185)	(0.536)	(0.090)	(0.507)
Railroad Bailout Weight	0.102	0.142	0,985	0.047
	(0.263)	(0.122)	(0.240)	(0.207)
Number of RFC members	-0.159**	-0.008	-0.113*	0.105
	(0.013)	(0.947)	(0.053)	(0.147)
R^2	0.179	0.058	0.084	0.094
Observations	344	344	344	344
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table 2: Summary statistics

The sample consists of 1,928 annual observations for 183 railroads from 1927 to 1939. A bailout is defined as any loan from the Reconstruction Finance Corporation or Public Works Administration from 1932 to 1939. *Connections* is the number of states the railroad operated in that were homes to RFC directors in that year. *Leverage* is the ratio of long-term debt to total assets. *Bonds Due / T.A.* is the value of all bonds due between 1930 and 1934 to total assets in 1929. *Passenger / Total Revenue* equals passenger revenue divided by total revenue. *Volatility* is the standard deviation of the previous 12 month's earnings (if earnings was missing, the 12-month standard deviation of stock returns). *Wage Bill* is the compensation for employees divided by total operating expenses. We report tests of differences in means (t-test) and medians (Wilcoxon) between the groups. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Panel A: Full sample					
	Bailed-out Railroads (N = 585 Railroad-Years)		Non-Bailed-Out Railroads (N = 1,343 Railroad-Years)		Difference Mean	Median
	Mean	Median	Mean	Median	T-test	Wilcoxon
Log (Age, years)	3.533	3.638	3.499	3.611	0.033	0.027*
Cash / T.A.	0.015	0.012	0.024	0.014	-0.009***	-0.002**
Connections	1.481	1.000	0.745	1.000	0.735***	0.000
Log (Employment)	9.185	9.454	7.126	7.012	2.059***	2.442***
Leverage	0.416	0.412	0.439	0.406	-0.023*	0.006
Net income / T.A.	0.009	0.012	0.016	0.010	-0.006**	0.002
Passenger / Total Revenue	0.110	0.959	0.099	0.059	0.011*	0.041***
Log (Total Assets)	19.300	19.379	17.116	16.938	2.189***	2.441***
Volatility	0.141	0.091	0.214	0.131	-0.074***	-0.040***
Wage Bill	0.711	0.636	0.626	0.616	0.086*	0.020***

Panel B: 1929 railroad characteristics

	Bailed-out Railroads (N = 46 Railroads)		Non-Bailed-Out Railroads (N = 118 Railroads)		Difference Mean	Difference Median
	Mean	Median	Mean	Median	T-test	Wilcoxon
Log (Age, years)	3.364	3.526	3.347	3.481	0.017	0.045
Cash / T.A.	0.018	0.014	0.028	0.017	-0.009*	-0.003
Bonds $Due_{1930\text{-}1934}$ / T.A.	0.041	0.011	0.022	0.000	0.019	0.011***
$Connections_{1932}$	1.087	1.000	0.496	0.000	0.591***	1.000****
Log (Employment)	9.491	9.762	7.430	7.372	2.061***	2.390***
Leverage	0.418	0.402	0.418	0.389	0.000	0.013
Net income / T.A.	0.027	0.026	0.030	0.027	-0.004	-0.001
Passenger / Total Revenue	0.096	0.079	0.075	0.039	0.021	0.040***
Log (Total Assets)	19.297	19.431	17.174	16.972	2.122***	2.479***
Volatility	0.077	0.063	0.119	0.084	-0.043***	-0.021***
Wage Bill	0.643	0.644	0.716	0.623	-0.074	0.021

Panel C: Full sample

	Multiple Bailouts (N = 485 Railroad-Years)		One Bailout (N = 100 Railroad-Years)		Difference Mean	Difference Median
	Mean	Median	Mean	Median	T-test	Wilcoxon
Log (Age, years)	3.467	3.611	3.880	4.060	-0.413***	-0.449***
Cash / T.A.	0.014	0.012	0.018	0.013	-0.004***	-0.001
Connections	1.662	1.000	0.736	1.000	0.926***	0.000
Log (Employment)	9.306	9.504	8.533	8.342	0.772***	1.162***
Leverage	0.418	0.419	0.403	0.389	0.015	0.030***
Net income / T.A.	0.008	0.010	0.016	0.019	-0.007**	-0.009***
Passenger / Total Revenue	0.115	0.099	0.085	0.081	0.030***	0.018***
Log (Total Assets)	19.428	19.484	18.609	18.654	0.819***	0.830***
Volatility	0.138	0.090	0.157	0.095	-0.019	-0.005
Wage Bill	0.726	0.638	0.069	0.641	0.633	-0.003

Table 3: Determinants of bailout

We regress bailouts on railroad characteristics. Column 1 presents the first-stage probit coefficients. The dependent variable, *Application*, equals one if the railroad applied for at least one loan in that year, and zero otherwise. Columns 2 to 4 present the second-stage regressions where the dependent variable, *Approval*, equals one if the railroad got at least one application approved, and zero otherwise. We use a probit (column 2), OLS (column 3), and the Heckman Selection Model (column 4) to calculate the second-stage regression coefficients. *Approval (in last 3 Years)* is a dummy variable that equals one if the railroad had an RFC or PWA loan approved in the last three years, and zero otherwise. *Cum. Loan Size / Total Assets* equals the cumulative bailout loan amount a railroad has received since 1932 divided by its total assets. Other variables are as defined in Table 2. All regressions include year and region fixed effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	First Stage	Second Stage		
	(Application)	(Approval)		
	(1)	(2)	(3)	(4)
Connections	0.100	0.975***	1.114***	1.013***
	(0.148)	(0.000)	(0.000)	(0.000)
Log (Total Assets)	0.358**	0.012	0.013	0.012
	(0.017)	(0.220)	(0.204)	(0.234)
Net income / T.A.	-0.965	0.099	0.156	0.121
	(0.763)	(0.315)	(0.202)	(0.473)
Leverage	0.771	-0.003	0.000	0.001
	(0.106)	(0.870)	(0.987)	(0.967)
Cash / T. A.	-15.964**	0.199	0.308*	0.376
	(0.038)	(0.118)	(0.087)	(0.229)
Log (Age, years)	-0.231**	-0.004	-0.007	-0.006
	(0.029)	(0.675)	(0.471)	(0.510)
Volatility	0.159	-0.005	-0.006	-0.005
	(0.500)	(0.875)	(0.692)	(0.842)
Log (Employment)	0.165	-0.004	-0.003	-0.004
	(0.284)	(0.679)	(0.809)	(0.728)
Passenger / Total Revenue	-2.404*	0.033	0.045	0.043
	(0.062)	(0.295)	(0.202)	(0.374)
Bonds $Due_{1930\text{-}1934}$ / T.A.	-0.798	0.059	0.047	0.054
	(0.661)	(0.563)	(0.677)	(0.626)
Approval (in Last 3 Years)		0.159	-0.389*	-0.065
		(0.328)	(0.078)	(0.232)
Cum. Loan size / Total Assets		0.778	2.706	0.0626
		(0.625)	(0.203)	(0.177)
Pseudo R^2 / R^2	0.417	0.345	0.214	0.458
Freight Composition	Yes	Yes	Yes	Yes
Specification	Probit	Probit	OLS	Heckman
Observations	1,554	1,548	1,548	1,548

Table 4: Announcement effects

We calculate the abnormal returns (AR) and cumulative abnormal returns (CAR) of a security from four days before to four days after the announcement of an application, approval, or refusal. We measure abnormal returns as the security's returns less the Moody's bond index / CRSP market index on the same day. We average the AR (CAR) across securities. Standard errors are clustered by railroads. *p*-values appear in parentheses. Panel A presents the results for all bailouts. Panel B presents the results for the initial and subsequent bailouts. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: All bailouts						
	Applications		*Approvals*		*Refusals*	
	Bonds	Equity	Bonds	Equity	Bonds	Equity
Day -4	-0.002	-0.007	0.001	0.005	-0.004	0.048
	(0.275)	(0.237)	(0.384)	(0.289)	(0.526)	(0.320)
Day -3	0.007	0.009	0.002	-0.003	0.005	0.002
	(0.414)	(0.181)	(0.249)	(0.548)	(0.757)	(0.916)
Day -2	0.000	0.002	-0.000	0.005	-0.005	-0.032
	(0.993)	(0.681)	(0.892)	(0.397)	(0.657)	(0.174)
Day -1	0.006	-0.003	-0.002	0.006	0.000	-0.079*
	(0.296)	(0.969)	(0.529)	(0.419)	(0.994)	(0.092)
Day 0	0.009**	0.004	0.003*	-0.025	0.016	0.044
	(0.049)	(0.550)	(0.092)	(0.516)	(0.646)	(0.443)
Day +1	0.003*	0.006	0.004**	-0.008	-0.059**	-0.061*
	(0.097)	(0.302)	(0.019)	(0.161)	(0.047)	(0.100)
Day +2	0.015***	-0.001	0.002	0.002	-0.013	0.027
	(0.008)	(0.922)	(0.188)	(0.698)	(0.548)	(0.121)
Day +3	0.003	0.003	0.005**	-0.009	0.009	-0.029
	(0.146)	(0.646)	(0.023)	(0.175)	(0.351)	(0.337)
Day +4	0.001	-0.005	0.000	0.007	0.019*	0.005
	(0.615)	(0.263)	(0.830)	(0.215)	(0.100)	(0.749)
CAR	0.042***	0.012	0.016***	-0.019	-0.025	-0.075
(t-4, t+4)	(0.001)	(0.281)	(0.009)	(0.618)	(0.971)	(0.247)
Observations	344	134	492	190	19	9

Panel B: First vs. subsequent bailouts

	First bailout				*Subsequent bailouts*			
	Applications		*Approval*		*Applications*		*Approval*	
	Bonds	Equity	Bonds	Equity	Bonds	Equity	Bonds	Equity
Day -4	-0.002	0.004	-0.007*	0.001	-0.002	-0.010	0.003*	0.006
	(0.577)	(0.731)	(0.054)	(0.942)	(0.348)	(0.159)	(0.079)	(0.248)
Day -3	0.020	0.029**	-0.001	0.013	-0.001	0.004	0.002	-0.007
	(0.191)	(0.250)	(0.818)	(0.537)	(0.793)	(0.631)	(0.209)	(0.221)
Day -2	0.001	-	0.004	0.003	-0.000	0.013*	-0.001	0.006
	(0.946)	0.033***	(0.643)	(0.862)	(0.900)	(0.069)	(0.558)	(0.394)
		(0.001)						
Day -1	0.008	0.004	-0.001	-0.012	0.016	-0.001	-0.002	0.009
	(0.424)	(0.785)	(0.864)	(0.575)	(0.304)	(0.860)	(0.545)	(0.248)
Day 0	0.026	0.001	0.009**	-0.229	0.004**	0.005	0.002	0.015*
	(0.171)	(0.931)	(0.043)	(0.317)	(0.023)	(0.505)	(0.351)	(0.089)
Day +1	0.004	-0.007	0.005	0.001	0.003	0.011	0.004**	-0.010*
	(0.372)	(0.591)	(0.206)	(0.974)	(0.163)	(0.125)	(0.042)	(0.084)
Day +2	0.035*	0.029***	0.004	0.011	0.008**	-0.009	0.002	0.003
	(0.074)	(0.008)	(0.259)	(0.461)	(0.029)	(0.211)	(0.319)	(0.995)
Day +3	0.000	-0.009	0.021**	-0.037*	0.004	0.007	0.002	-0.003
	(0.974)	(0.559)	(0.040)	(0.093)	(0.042)	(0.307)	(0.291)	(0.618)
Day +4	0.005	-0.001	0.007**	0.012	-0.000	-0.006	-0.001	0.006
	(0.466)	(0.861)	(0.027)	(0.472)	(0.962)	(0.302)	(0.578)	(0.309)
CAR (t-4, t+4)	0.098**	0.017	0.040**	-0.239	0.024***	0.013	0.011*	0.023*
	(0.042)	(0.563)	(0.015)	(0.302)	(0.008)	(0.362)	(0.092)	(0.081)
Observations	85	32	81	32	259	103	441	159

Table 5: Determinants of announcement CARs

We regress a railroad's cumulative abnormal bond/equity return (CAR) from four days before to four days after an application/approval of an RFC or PWA loan. Variables are as defined in Table 2. *Close to default* equals 1 if the railroad's bond price is below 50 and zero otherwise. We add firm and region fixed effects and cluster standard errors at the railroad level. In Panel A, we focus on all loan approvals and applications. In Panel B, we focus on all first and subsequent loan approvals and applications. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: All observations				
	Application		*Approval*	
Security	Bond	Equity	Bond	Equity
	(1)	(2)	(3)	(4)
Connections	-0.017	0.005	0.009	0.064
	(0.219)	(0.725)	(0.268)	(0.304)
Log (Total Assets)	-0.023	2.133	-0.009	2.755
	(0.717)	(0.161)	(0.267)	(0.586)
Net income / T.A.	-0.321	1.884	-0.420	-7.550
	(0.746)	(0.176)	(0.485)	(0.326)
Leverage	0.052	-2.164***	0.381	8.369
	(0.790)	(0.003)	(0.289)	(0.229)
Cash / T.A.	-0.659	0.638	-3.622	6.077
	(0.801)	(0.877)	(0.251)	(0.735)
Log (Age, years)	0.002***	0.006	0.092	-1.285
	(0.008)	(0.717)	(0.737)	(0.570)
Volatility	-0.029	0.042	-0.239	-1.894
	(0.556)	(0.465)	(0.260)	(0.105)
Log (Employment)	0.103	-1.922***	-0.026**	-1.339*
	(0.164)	(0.005)	(0.015)	(0.068)
Passenger / Total Revenue	-0.113	0.085	-0.097	-0.094
	(0.265)	(0.423)	(0.151)	(0.781)
Close to default	-0.030	-0.035	0.011	0.049
	(0.581)	(0.428)	(0.421)	(0.663)
Loan size / Total Assets			0.105	-0.036
			(0.514)	(0.537)
Region FE	Yes	Yes	Yes	Yes
Freight composition	Yes	Yes	Yes	Yes
Observations	313	141	411	169
R^2	0.009	0.062	0.057	0.053

Panel B: First vs. subsequent bailouts								
	Application				*Approval*			
	First Bailout		*Subsequent Bailouts*		*First Bailout*		*Subsequent Bailouts*	
Security	Bond	Equity	Bond	Equity	Bond	Equity	Bond	Equity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Connections	-0.194	0.031	-0.000	0.018	0.094*	1.166*	0.012	0.006
	(0.162)	(0.456)	(0.975)	(0.295)	(0.100)	(0.054)	(0.153)	(0.634)
Log (Total Assets)	-0.829	0.223	-0.133**	0.238	-0.867**	-6.513	-0.054	0.416***
	(0.217)	(0.816)	(0.034)	(0.380)	(0.002)	(0.171)	(0.650)	(0.001)
Net income / T.A.	-1.484	6.533	-0.439	1.116*	-5.976*	-	0.249	0.738
	(0.788)	(0.453)	(0.615)	(0.090)	(0.058)	106.682**	(0.673)	(0.579)
						(0.042)		
Leverage	-1.017*	-0.479	-0.139	-0.361	-0.003	-1.294	0.277	-0.009
	(0.056)	(0.592)	(0.352)	(0.215)	(0.993)	(0.793)	(0.175)	(0.979)
Cash / T.A.	-13.719	-22.862	-1.999	7.736	-14.464	-107.918	-3.821	5.933
	(0.503)	(0.246)	(0.426)	(0.173)	(0.109)	(0.420)	(0.126)	(0.125)
Log (Age, years)	0.000	0.002	0.001***	-0.001**	0.030	-0.848	0.103	0.145**
	(0.895)	(0.635)	(0.009)	(0.043)	(0.656)	(0.411)	(0.240)	(0.012)
Volatility	0.069	-0.029	0.076*	0.074	0.652**	5.702	-0.014	-0.34***
	(0.310)	(0.847)	(0.078)	(0.668)	(0.016)	(0.148)	(0.897)	(0.004)
Log (Employment)	1.549*	-0.298	0.141**	-0.316	1.711	22.009**	-0.04***	0.161
	(0.092)	(0.731)	(0.043)	(0.175)	(0.130)	(0.015)	(0.000)	(0.714)
Passenger / Total	-1.798	-0.470	0.022	-0.070	-0.756	-3.290	-0.13***	0.062
Revenue	(0.182)	(0.349)	(0.748)	(0.493)	(0.152)	(0.645)	(0.007)	(0.463)
Close to default	-0.142	0.118	0.007	0.008	-0.057	-1.981*	0.010	0.007
	(0.329)	(0.528)	(0.747)	(0.848)	(0.245)	(0.073)	(0.545)	(0.849)
Loan size / Total					-0.071	-0.298	-0.014*	-0.009
Assets					(0.166)	(0.388)	(0.089)	(0.490)
Firm FE	No	No	Yes	Yes	No	No	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Freight Composition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	72	31	241	102	67	28	337	141
R^2	-0.012	0.154	0.059	0.058	0.333	0.151	0.002	0.081

Table 6: Determinants of bond defaults

We regress bond defaults on lagged railroad characteristics. *Default* equals one if the railroad failed to meet a coupon or principal repayment, or in any way changed the terms of the issue in the current year. We drop all observations of the respective railroads the year after *Default* equals one. *Approval* equals one if the railroad obtained an RFC or PWA loan in the previous year. In columns 1 and 4, we run a probit regression model. In column 2 and 5, we present our first-stage regression for the instrumental-variable (IV) approach. We regress an indicator variable equal to one in the year the railroad received an *Approval*, and zero otherwise, on railroad controls. In columns 3 and 6, we present the second-stage instrumental variable (IV) regression. *p*-values, in parentheses, are adjusted for heteroskedasticity and clustered at the railroad-level. We include region, year and (in columns 4-6) bond rating fixed effects. For a railroad with multiple outstanding bonds, we use the rating of the bond closest to maturity. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Bond default (this year)			Bond default (this year or next two years)		
		First Stage	Second Stage		First Stage	Second Stage
	(1)	(2)	(3)	(4)	(5)	(6)
Approval	0.247		0.893	0.694**		1.904
	(0.531)		(0.697)	(0.029)		(0.291)
Log (Total Assets)	0.288	0.028*	0.274	0.343	0.016	0.316
	(0.370)	(0.068)	(0.412)	(0.185)	(0.181)	(0.258)
Net income / T.A.	-9.198**	-0.258	-8.857*	-6.259	-0.177	-5.764
	(0.034)	(0.108)	(0.053)	(0.133)	(0.202)	(0.197)
Leverage	0.399	0.050	0.364	0.589	0.053	0.513
	(0.426)	(0.161)	(0.471)	(0.251)	(0.129)	(0.271)
Cash / T.A.	-88.092***	0.351	-87.829***	-45.114***	0.183	-44.265***
	(0.001)	(0.216)	(0.001)	(0.005)	(0.354)	(0.007)
Log (Age, years)	-0.480**	-0.051***	-0.447**	-0.397**	-0.024**	-0.362**
	(0.015)	(0.002)	(0.041)	(0.018)	(0.032)	(0.041
Volatility	0.191	-0.055**	0.206	0.214	-0.003	0.205
	(0.525)	(0.015)	(0.511)	(0.281)	(0.943)	(0.328)
Log (Employment)	-0.319	0.027*	-0.351	-0.407	0.021*	-0.437*
	(0.314)	(0.072)	(0.254)	(0.108)	(0.088)	(0.064)
Bonds Due$_{1930\text{-}1934}$ / T.A.	0.415**	-0.003	0.456**	0.323**	-0.000	0.317**
	(0.021)	(0.711)	(0.018)	(0.039)	(0.984)	(0.027)
Passenger / Total Revenue	-3.143	-0.044	-2.935	-1.554	-0.088	-1.283
	(0.251)	(0.541)	(0.328)	(0.439)	(0.193)	(0.508)
Cum. Loan Size / Total Assets	-0.968	8.462***	-6.831	-1.261	8.611***	-12.401
	(0.827)	(0.000)	(0.762)	(0.702)	(0.000)	(0.440)
Connections		0.139***			0.147***	
		(0.000)			(0.000)	

Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Freight Composition	Yes	Yes	Yes	Yes	Yes	Yes
Log Likelihood	-64.834	-91.317	-91.317	-185.491	-137.569	-137.569
Observations	769	769	769	1,114	1,114	1,114
F-Statistic	n.a.	69.758	n.a.	n.a.	69.758	n.a
Specification	Probit	IV-Probit	IV-Probit	Probit	IV-Probit	IV-Probit

Table 7: Rating changes

We regress bond rating changes, for the nearest-to-maturity bond, on lagged railroad and bond characteristics. Columns 1 and 4 contain probit regressions. The dependent variable equals one if there was a Moody's rating downgrade in the current year, and zero otherwise (Column 1) or if there was more than one rating downgrade from the current year to year t+2 (Column 4). Variables are as defined in Tables 2 and 6. We include *Time to maturity*, the log of the number of years to maturity for the respective bond, and the *Nominal outstanding amount of the bond to total long-term debt*. In columns 2 and 5, we report first-stage regressions. We regress an indicator variable equal to one the year the railroad received an *Approval*, and zero otherwise, on railroad and bond controls. We present the second-stage instrumental variable (IV) probit regressions for one downgrade (column 3) and multiple downgrades (column 6). *p*-values, in parentheses, are adjusted for heteroskedasticity and clustered at the railroad-level. We include region, firm, and rating fixed effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Single Rating Downgrade (this year)			Multiple Rating Downgrades (this year or next two years)		
		First Stage	Second Stage		First Stage	Second Stage
	(1)	(2)	(3)	(4)	(5)	(6)
Approval	-0.416***		-1.434***	-0.305***		-2.633***
	(0.000)		(0.000)	(0.004)		(0.000)
Connections		0.096***			0.093***	
		(0.000)			(0.000)	
Firm control variables						
Log (Total Assets)	-0.139	0.262	0.268	-5.536	0.209	-1.386
	(0.936)	(0.102)	(0.806)	(0.119)	(0.130)	(0.420)
Net income / T.A.	6.355**	-2.139***	3.110	1.349	0.034	-0.067
	(0.012)	(0.001)	(0.177)	(0.695)	(0.875)	(0.952)
Leverage	0.288	0.769***	1.103	1.556	0.657***	2.435***
	(0.789)	(0.000)	(0.269)	(0.169)	(0.000)	(0.000)
Cash / T.A.	-26.299***	2.899**	-20.698**	-22.809**	-0.275	-10.675*
	(0.006)	(0.022)	(0.017)	(0.045)	(0.751)	(0.062)
Log (Age, years)	-1.190***	0.253***	-0.628	-5.495***	0.194**	-1.704***
	(0.006)	(0.010)	(0.181)	(0.000)	(0.037)	(0.001)
Volatility	-0.256*	0.073***	-0.163	-0.519**	0.061***	-0.147
	(0.066)	(0.000)	(0.213)	(0.018)	(0.001)	(0.325)
Log (Employment)	2.122***	-0.253**	1.595***	3.309***	-0.231**	0.831*
	(0.000)	(0.014)	(0.003)	(0.000)	(0.016)	(0.052)
Passenger / Total Revenue	6.327**	-17.797	2.928	14.387***	-1.901	-0.225
	(0.046)	(0.273)	(0.179)	(0.000)	(0.207)	(0.954)
Cum. Loan Size / T.A.	-0.567	1.259***	1.074	-7.288***	1.185***	0.284
	(0.562)	(0.003)	(0.306)	(0.000)	(0.004)	(0.848)
Bond control variables						
Time to Maturity	-0.068	0.013*	-0.054	-0.057	0.011	-0.009
	(0.104)	(0.085)	(0.167)	(0.348)	(0.119)	(0.781)
Nominal Outstanding / L.T.D.	0.127**	-0.011	0.103**	0.128***	0.013**	0.086***
	(0.023)	(0.204)	(0.038)	(0.000)	(0.002)	(0.000)

Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Freight Composition	Yes	Yes	Yes	Yes	Yes	Yes
Log likelihood	-950.939	-1,546.654	-1,546.654	-757,378	-1,356.539	-1,356.539
Observations	1,779	1,779	1,779	1,992	1,992	1,992
F-Statistic	n.a.	118.461	n.a.	n.a.	118.461	n.a.
Specification	Probit	IV-Probit	IV-Probit	Probit	IV-Probit	IV-Probit

Table 8: Difference-in-difference: Profitability, leverage, employment, and wage bill

We present difference-in-difference regressions for profitability, leverage, and employment with variation in treatment timing and multiple time periods following Callaway and Sant'Anna (2020). In Panel A, *ATT* is defined as the average treatment effect for the treated subpopulation. Panel B presents results for an event study analysis. Panel C presents results if we assume that bailed-out railroads (counterfactually) received an RFC or PWA loan in 1929, and the post-bailout-period was 1929-32. Columns 1, 4, 7, and 10 include all bailed-out railroads. Columns 2, 5, 8, and 11 include only those railroads that received a single bailout, whereas columns 3, 6, 9, and 12 include only those railroads that received multiple bailouts. The *p*-values in parentheses use doubly robust standard errors, following Sant'Anna and Zhao (2020). All regressions use year and region fixed effects. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Panel A: Average treatment											
	Profitability			*Leverage*			*Employment*			*Wage Bill*		
	All Bailouts	*1 Bailout*	*Multiple*	*All Bailouts*	*1 Bailout*	*Multiple*	*All Bailouts*	*1 Bailout*	*Multiple*	*All Bailouts*	*1 Bailout*	*Multiple*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ATT	-0.002	-0.006	-0.002	-0.033**	-0.050	-0.032*	0.019	0.102	0.010	0.045*	0.011	0.049*
	(0.640)	(0.241)	(0.723)	(0.032)	(0.227)	(0.053)	(0.597)	(0.614)	(0.764)	(0.100)	(0.712)	(0.098)
Obs.	1,838	1,346	1,747	1,838	1,346	1,747	1,838	1,346	1,747	1,921	1,429	1,830
	Panel B: Event study											
Year - 4	-0.002	-0.000	-0.003	0.007	0.012	0.006	0.073	-0.014	0.089	0.060	-0.101	0.089
	(0.479)	(0.923)	(0.481)	(0.295)	(0.556)	(0.399)	(0.193)	(0.483)	(0.176)	(0.771)	(0.353)	(0.712)
Year - 3	0.000	-0.006*	0.001	0.006	0.008	0.006	-0.004	-0.023	-0.001	-0.185	-0.015	-0.221
	(0.923)	(0.100)	(0.666)	(0.367)	(0.518)	(0.427)	(0.703)	(0.476)	(0.949)	(0.395)	(0.405)	(0.388)
Year -2	-0.001	-0.003	-0.000	0.004	-0.019	0.008	0.001	0.007	-0.000	0.123	-0.002	0.146
	(0.766)	(0.447)	(0.902)	(0.564)	(0.209)	(0.261)	(0.959)	(0.765)	(0.981)	(0.325)	(0.813)	(0.323)
Year -1	-0.001	0.010*	-0.004	-0.021**	-0.008	-0.024**	-0.003	-0.023	0.001	-0.022	-0.005	-0.025
	(0.494)	(0.098)	(0.175)	(0.017)	(0.235)	(0.022)	(0.783)	(0.450)	(0.968)	(0.276)	(0.436)	(0.291)
Year 0	-0.004	-0.006	-0.004	0.000	0.006	-0.001	0.012	0.006	0.013	0.036*	0.018	0.039
	(0.156)	(0.194)	(0.247)	(0.987)	(0.781)	(0.859)	(0.386)	(0.789)	(0.406)	(0.085)	(0.152)	(0.110)
Year +1	0.010	-0.010	0.003	-0.021*	-0.003	-0.0237*	0.031	0.012	0.034	0.044**	0.034	0.046*
	(0.748	(0.357)	(0.531)	(0.057)	(0.755)	(0.046)	(0.134)	(0.853)	(0.122)	(0.048)	(0.146)	(0.070)
Year +2	-0.005	-0.010	-0.004	-0.029*	-0.045	-0.027	0.042	0.177	0.027	0.049*	0.042	0.051*
	(0.260)	(0.321)	(0.378)	(0.086)	(0.464)	(0.110)	(0.202)	(0.258)	(0.363)	(0.061)	(0.143)	(0.083)

Year +3	-0.015	-0.001	-0.016	-0.050**	-0.084*	-0.046**	0.014	0.106	0.003	0.051*	0.031	0.053*
	(0.379)	(0.912)	(0.381)	(0.014)	(0.075)	(0.027)	(0.728)	(0.568)	(0.936)	(0.058)	(0.255)	(0.073)
Year +4	0.001	-0.001	0.001	-0.037*	-0.094	-0.032	0.022	0.075	0.017	0.043	-0.016	0.048
	(0.848)	(0.950)	(0.840)	(0.081)	(0.132)	(0.138)	(0.632)	(0.837)	(0.0650)	(0.136)	(0.808)	(0.115)
Obs.	1,928	1,346	1,747	1,838	1,346	1,747	1,930	1,346	1,747	1,921	1,429	1,830
						Panel C: Placebo						
ATT		-0.008			0.006			0.027			-0.001	
		(0.194)			(0.671)			(0.625)			(0.827)	
Obs.		913			913			913			913	

Table 9: Instrumental variable regressions: Profitability, leverage, and employment

In the first stage, we regress *Approval* on *Connections* and lagged railroad characteristics. In the second stage, we regress contemporaneous railroad profitability, leverage, employment, and the wage bill on the fitted level of lagged *Approval* and lagged characteristics. Variables are as defined in Table 2 and 6. *p*-values are adjusted for heteroskedasticity and clustered at the firm-level, in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	First-Stage	Second-Stage			
		Profitability	*Leverage*	*Employment*	*Wage Bill*
	(1)	(2)	(3)	(4)	(5)
Approval		0.008	-0.088*	-0.008	-0.966
		(0.648)	(0.053)	(0.940)	(0.210)
Log (Total Assets)	0.095	0.007	0.046*	0.026	0.011
	(0.106)	(0.248)	(0.052)	(0.357)	(0.864)
Cash / T. A.	0.121	-0.009	0.068	0.222	0.589
	(0.751)	(0.877)	(0.593)	(0.476)	(0.520)
Log (Age, years)	-0.006	0.003	-0.007	0.107***	0.368
	(0.937)	(0.465)	(0.575)	(0.005)	(0.243)
Volatility	0.017	-0.004*	-0.003	-0.011	0.062
	(0.604)	(0.072)	(0.521)	(0.476)	(0.279)
Net income / T.A.	0.233	0.407***	0.006	-0.182	1.379
	(0.154)	(0.002)	(0.936)	(0.357)	(0.342)
Leverage	0.007	0.021**	0.627***	0.089	-0.042
	(0.905)	(0.045)	(0.000)	(0.357)	(0.734)
Log (Employment)	-0.002	-0.001	-0.008	0.434***	0.097
	(0.963)	(0.778)	(0.674)	(0.000)	(0.103)
Passenger / Total Revenue	0.22	0.008	-0.016	-0.012	-4.822
	(0.961)	(0.458)	(0.821)	(0.887)	(0.311)
Connections	0.066***				
	(0.000)				
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Freight Composition	Yes	Yes	Yes	Yes	Yes
R^2	0.133	0.318	0.343	0.661	0.299
F statistic	76.140	n.a.	n.a.	n.a.	n.a.
Observations	1,568	1,515	1,515	1,515	1,517

Table 10: Railroad bailouts and building approvals

We regress the logarithm of building permits per city on *City RFC Approvals* (the fraction of all railroads that pass through the city that received an RFC/PWA railroad loan approval the previous year). We condition on state-level bank characteristics: the logarithm of bank loans per capita; the logarithm of bank deposits per capita; the logarithm of the number of all banks; and the capital of nationally chartered banks that operated in the state that were liquidated in year *t* divided by the capital of all nationally-chartered banks in that state in year *t*. We instrument *City RFC Approvals* with *Connections*. *p*-values are adjusted for heteroskedasticity and clustered at the city-level, in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
	(1)	(2)	(3)	(4)	(5)	(6)
City RFC Approvals		-0.881***		-0.884***		-0.156
		(0.000)		(0.000		(0.160)
Log (Loans per Capita)	-0.297***	0.188***	-0.314***	0.229***	-0.215	-0.262*
	(0.000)	(0.003)	(0.000)	(0.006)	(0.225)	(0.057)
Log (Deposits per Capita)	-1.099***	0.908***	-2.386***	1.187***	0.557**	0.398*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.016)	(0.100)
Log (Number of Banks)	0.560***	-0.449***	1.501***	-0.748***	0.124	0.169
	(0.000)	(0.000)	(0.000)	(0.007)	(0.524)	(0.373)
Capital of Suspended Banks	1.609**	1.979**	2.599***	1.851**	3.068***	1.118*
	(0.036)	(0.018)	(0.008)	(0.037)	(0.002)	(0.057)
Connections	0.452***		0.412***		0.239***	
	(0.000)		(0.000)		(0.000)	
Year FE	No	No	No	No	Yes	Yes
City FE	No	No	Yes	Yes	Yes	Yes
R^2	0.098	0.218	0.162	0.218	0.378	0.649
Observations	2,232	2,232	2,232	2,232	2,232	2,232
F-statistic	136.0	n.a.	94.9	n.a.	39.1	n.a.

Table 11: Announcement effects for related firms

We calculate the abnormal return (AR) and cumulative abnormal return (CAR) for related firms' equity after the announcement of a railroad's bailout application (Panel A) and approval (Panel B). We measure the AR as a firm's equity return less the CRSP market index. An overlap of *Yes* indicates the railroad/manufacturing firm operates in at least one city with the bailed-out railroad. An overlap of *No* indicates the railroad/manufacturing firm does not operate in any cities in which the bailed-out railroad operates. *High* indicates that the percentage overlap is above the mean level across all firms. *Low* indicates that the percentage overlap is non-zero and below the mean overlap across all firms. For railroads, the percentage overlap is defined as the number of cities that both railroads serve divided by the total number of cities of the bailed-out railroad. For manufacturing firms, the percentage overlap is defined as the number of cities that the railroad and manufacturing firm both operate in divided by the total number of cities the manufacturer operates in. *p*-values appear in parentheses. We report the *p*-values of t-test differences between the groups (*Yes – No* or *High - Low*) in Diff. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively. The returns are winsorized at the 2.5% level.

	Railroads				**Manufacturing**			
Overlap	Yes	No	High	Low	Yes	No	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Application				
Day -1	0.003**	0.002**	0.004**	0.002**	0.003***	0.002***	0.004***	0.002***
	(0.046)	(0.048)	(0.043)	(0.040)	(0.000)	(0.000)	(0.001)	(0.000)
Day 0	0.005***	0.003**	0.006**	0.003***	0.000	-0.004**	0.000	0.000
	(0.004)	(0.027)	(0.016)	(0.009)	(0.745)	(0.024)	(0.781)	(0.854)
Day +1	0.006***	0.008***	0.006***	0.008***	0.003***	0.001***	0.005***	0.002***
	(0.000)	(0.000)	(0.009)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
CAR	0.015***	0.013***	0.016***	0.013***	0.006***	0.005***	0.009***	0.004***
(t-1, t+1)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Diff	0.002		0.003		0.001*		0.006***	
(t-1, t+1)	[0.389]		[0.797]		[1.769]		[3.804]	
Obs.	2,208	4,861	1,442	756	12,694	68,865	4,936	7,759
				Panel B: Approval				
Day -1	-0.001	-0.000	-0.001	-0.001	0.003***	0.001***	0.003***	0.002*
	(0.427)	(0.497)	(0.949)	(0.308)	(0.000)	(0.000)	(0.000)	(0.000)
Day 0	0.002	0.004***	0.002	0.003***	0.001***	0.002***	0.002**	0.001
	(0.266)	(0.003)	(0.408)	(0.003)	(0.007)	(0.000)	(0.029)	(0.101)
Day +1	0.000	0.003***	-0.002	0.003***	0.001***	-	0.002**	0.007
	(0.756)	(0.009)	(0.174)	(0.002)	(0.005)	0.001***	(0.010)	(0.168)
						(0.008)		
CAR	0.001	0.006***	-0.001	0.005***	0.005***	0.004***	0.007***	0.004***
(t-1, t+1)	(0.545)	(0.005)	(0.782)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Diff	-0.005*		-0.006**		0.002**		0.003**	
(t-1, t+1)	[-1.688]		[-2.053]		[2.413]		[2.436]	
Obs.	2,905	6,146	1,889	1,001	17,475	70,600	6,705	10,770

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Appendix

Table A.1 Cross-sectional regression

We regress the change in profitability, leverage, and (log) employment between 1929 and 1939 on characteristics fixed in 1929. We include a dummy that yields one if the railroad has received (at least) one bailout between 1932 and 1939. We add the average annual number of connections between 1932 and 1939. *p*-values are adjusted for heteroskedasticity and clustered at the railroad-level. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Profitability	*Leverage*	*Employment*	*Wage Bill*
	(1)	(2)	(3)	(4)
$Approval_{1932-1939}$	0.007	-0.045	0.025	0.006
	(0.326)	(0.292)	(0.753)	(0.605)
$Average\ Connections_{1932-1939}$	0.010	0.002	0.031	0.017*
	(0.124)	(0.962)	(0.646)	(0.078)
Log(Total Assets)	-0.006	-0.028	0.109*	0.061***
	(0.282)	(0.414)	(0.085)	(0.000)
Net Income / T.A.	-0.905***	0.705	1.277	-0.249
	(0.000)	(0.294)	(0.307)	(0.162)
Leverage	-0.014	-0.202*	0.039	-0.002
	(0.483)	(0.078)	(0.852)	(0.954)
Cash	0.367**	-2.119**	-1.328	-0.220
	(0.031)	(0.031)	(0.461)	(0.388)
Log (Age, years)	0.001	0.014	-0.051	0.001
	(0.729)	(0.551)	(0.242)	(0.817)
Volatility	0.018	0.204	-0.160	-0.073
	(0.677)	(0.403)	(0.724)	(0.256)
Passenger / Total Revenue	-0.040	0.067	-0.091	-0.036
	(0.198)	(0.706)	(0.785)	(0.440)
Employment	0.002	0.020	-0.117*	-0.067***
	(0.716)	(0.576)	(0.084)	(0.000)
$BondsDue_{1930-1934}$	0.042	0.093	-0.078	0.006
	(0.292)	(0.682)	(0.854)	(0.915)
Region FE	Yes	Yes	Yes	Yes
Freight Composition	Yes	Yes	Yes	Yes
R squared	0.429	0.111	-0.061	0.352
Observations	115	115	115	115