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ABSTRACT

We use the unique circumstances that led to the Panic of 1907 to analyze its impact on economic activity. The panic was fuelled by runs on the 'shadow banks' of the time, New York's trust companies. But the shock that triggered the runs was unrelated to the nonfinancial corporations affiliated with those institutions. Using newly collected data, we find that small corporations with close ties to the trust companies that lost the most deposits experienced an immediate decline in their stock price of 10.4 percentage points, and performed worse in the years following the panic across a range of outcomes, including their return on equity, which fell 13.1 percent, their dividend rate, which fell 22 percent, and their average interest costs, which rose 8.3 percent, relative to mean pre-panic levels. The effect on their investment rate was much greater: it fell by nearly 50 percent. The relative decline in investment induced by affiliations with the worst-affected trust companies alone accounted for at least 18.4 percent of the total decline in corporate investment in the United States in 1908. This effect diminished in magnitude over time but persisted for at least five years following the panic.

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1. Introduction

The recent economic crisis has rekindled interest in financial history, in recognition of the insights it offers for the analysis of modern financial markets. Among the many crises in America's past, the Panic of 1907 resembles the recent panic perhaps most closely, in that it originated in runs on the 'shadow banks' of the time, New York's trust companies. At the time of the panic, those institutions had grown so rapidly that they rivaled the city's national banks in scale: New York's shadow banking system was nearly as large as its traditional banking system. Less regulated than commercial banks, trust companies held low cash reserve balances and did not belong to the city's private clearinghouse, which could act as a lender of last resort. Early in the crisis, Knickerbocker Trust, one of the largest institutions of its kind, was denied assistance and closed. As with Lehman Brothers' bankruptcy in 2008, Knickerbocker's closure triggered more runs and spread turmoil throughout the financial system. And as with the extraordinary lending facilities created by the Federal Reserve to bring the 2008 panic under control, the runs on trust companies were ultimately arrested by emergency loans organized by J.P. Morgan, and partly funded by the U.S. Treasury.

We study the effects of the contraction in financial intermediation during the Panic of 1907 on the American economy and, in particular, on aggregate investment. A robust statistical relationship between historical crises and the depth of recessions has been established (e.g., Reinhart and Rogoff 2009), but that relationship may not reflect causation: financial crises may be triggered by severe recessions or by the conditions that precipitate such recessions. In addition, since financial crises may influence credit demand as well as its supply, it is particularly difficult to isolate effects arising from the contraction in intermediation. The Panic of 1907 presents these challenges: the economic contraction was

already underway when the runs on trust companies began, and the fall in lending was likely influenced by the ongoing recession.[1]

But the unique circumstances that led to the onset of the panic present an opportunity to isolate the effects of the disruption in financial intermediation provided by early shadow banks. More than commercial banks, trust companies were heavily involved in long-term corporate finance, and established relationships with large numbers of major nonfinancial corporations. But the runs that broke out in October 1907 were triggered by fears that a few trust company directors were involved in a scandal, and had no connection to any of the corporate clients of those institutions.

To establish the impact of the contraction in intermediation, we analyze the relative performance of the nonfinancial corporations affiliated with the trust companies that were most severely affected by the runs. Our sample includes all industrial and railroad corporations listed on the New York Stock Exchange (NYSE), which accounted for around 19 percent of total corporate investment in the United States prior to the panic.[2] The affiliations between trust companies and their clients are observed through the presence of board interlocks, which were a common part of lending and underwriting relationships in the early twentieth century.[3] At the firm level, we analyze the impact of the shock on a range of outcomes, including share prices, profitability, borrowing costs, dividend rates, and investment rates. An important contribution of our study is that we are then able to determine the impact of the financial contraction at the aggregate level. Specifically, we use the total decline in firm investment that can be attributed to connections to the worst-affected trust companies to assess the contribution of the financial shock to the overall

[1] The NBER dates a peak in May of 1907; the panic began in October. Over the following year, real GNP declined by 11 percent, industrial production contracted by 16 percent, and the unemployment rate nearly doubled (Balke and Gordon 1986; Davis 2004; Romer 1983).
[2] Our measure of investment for our sample firms totals $534 million in 1907; aggregate investment in the United States by nonfinancial corporations was $2.8 billion in that year (see the Data Appendix for sources).
[3] On the significance of the roles of financiers on nonfinancial firms' boards in the early twentieth century, see DeLong (1991), Ramirez (1995), and Cantillo Simon (1998).

contraction in corporate investment in the economy following the panic.

Our analysis proceeds in three steps. First, in Section 2 we establish that much of the variation in deposit losses among New York's trust companies was due to their associations with a handful of men involved in a failed attempt to corner the shares of a mining company. Since those men were directors of trust companies, households feared that their deposits were threatened, and initiated the runs. But the event that triggered the runs was unrelated to the corporate clients of the trust companies. Thus, the problems that often confound efforts to determine the effects of financial contractions—that lenders and borrowers may be subject to correlated shocks, or that shocks to borrowers may actually cause shocks to lenders—were unlikely to have been important.

Our second step in Section 4 assesses the impact of the shock at the firm level. At the onset of the runs, the decline in the stock returns of firms connected to a severely affected trust company was 60 percent greater than those of similar, non-connected nonfinancial firms. This indicates that investors immediately perceived that the runs would harm affiliated corporations. We then analyze the medium-run impact of the shock on a variety of firm outcomes. To facilitate comparisons across outcomes, we gauge the magnitude of the effects by relating our estimates of the impact of connections to affected trust companies to the mean level of each outcome among sample firms prior to the panic. The results indicate that for small firms—defined as those at the 25th percentile of assets—the return on equity fell 13.1 percent, the dividend rate fell 22 percent, and the average interest costs rose 8.3 percent, relative to mean 1906 levels.[4] These impacts were smaller by about one-fourth to one-half for medium-sized firms and negligible for the large firms in the sample, which suggests that information asymmetries aggravated the consequences of the financial contraction. However, the effect on investment was considerably greater. Among

[4] The mean 1906 levels for these variables were 0.084, 0.032, and 0.048, respectively. The mean 1906 level of investment was 0.036.

small and medium-sized firms, investment rates fell by 49.7 and 39.2 percent, respectively, relative to the 1906 mean.

Finally in Section 5 we analyze the macroeconomic impact of the financial contraction among the New York trust companies by calculating its effect on aggregate investment. Among all firms we can identify with ties to the worst-affected trust companies, we find that the total decline in investment attributable to the crisis was $128.5 million, equivalent to 18.4 percent of the $700 million contraction in investment among all nonfinancial corporations in the economy in 1908. The reduction in investment among these firms persisted for at least five years but diminished in magnitude over time; in 1912 their investment was reduced by only $49.6 million, equivalent to 39 percent of the 1908 effect. It should be emphasized that this is only the differential effect from exposure to the hardest-hit trust companies; the total impact resulting from the contraction in lending among all New York trust companies was likely larger.

We consider two possible mechanisms by which the shock from trust companies may have been transmitted to their nonfinancial clients. First, trust companies may have curtailed their provision of financial services such as securities underwriting or lending—this is the 'lending channel,' broadly defined (Bernanke and Gertler 1995). Alternatively, there could have been a 'reputation channel' if customers and lenders were made uneasy about the quality of the firms' assets, due to their associations with troubled financial institutions. But we find no effects from connections to the commercial banks that were associated with the men involved in the scandal. Those banks suffered from tarnished reputations, but did not experience a severe contraction in deposits. This suggests that the reputation channel alone was unlikely to have been important, and the lending channel was therefore responsible for the effects on firms affiliated with the hardest-hit trust companies.

A potential source of concern is that our findings may reflect the selection of

particular types of firms into relationships with particular trust companies. Our estimation framework controls for time-invariant unobserved characteristics such as firm 'quality,' and we use a variety of strategies to deal with selection on observables. A more difficult problem to address is selection based on unobservable characteristics that manifested only in response to a panic, for example if firms more sensitive to a downturn were matched to trust companies more affected by the panic. Yet in a "placebo test" we find that firms with ties to the most severely affected trust companies in 1907 did not perform worse during an earlier crisis, the recession and financial panic of 1903-04.

Our paper connects two distinct and influential literatures analyzing the effects of financial crises. Several prominent studies document severe and protracted contractions in the aftermath of historical crises (Reinhart and Rogoff 2009; Bordo and Haubrich 2010; Jordà, Schularik and Taylor 2011). These works, however, cannot isolate the contribution of financial contractions to the aggregate downturn. A separate and rapidly expanding literature exploits natural experiments and firm-level data to provide convincing evidence of the effects of a contraction in credit intermediation on borrowers' outcomes.[5] Most of these studies, however, do not measure the domestic economic impact of a financial crisis, and instead focus on documenting the credit-channel effects of a shock transmitted internationally or an isolated shock to particular institutions. We use a natural experiment within the context of a financial crisis to analyze its impact, and quantify the effects of the lending channel on aggregate investment.[6] Our results complement those of Chodorow-Reich (2014), who studies the effects of the lending channel on employment following the 2008 panic. Our paper also adds to this literature by exploiting a new source of

[5] Recent contributions to this literature include Khwaja and Mian (2008), Puri, Rocholl and Steffen (2011), and Schnabl (2012), who focus on the borrowers' access to financing; Slovin et al. (1993), Chava and Purnanandam (2011) and Lin and Paravisini (2013), who study market values and financial outcomes; Amiti and Weinstein (2011) and Paravisini et al. (2011), who analyze the effects on exports.

[6] Gan (2007) relates the contraction in bank liquidity to cross-sectional differences in firm investment post-crisis in the context of the Japanese crisis in the 1990s. In contrast, we use a differences-in-differences specification that allows us to control for unobserved firm heterogeneity.

identification, focused on the personal connections between institutions that faced an exogenous shock, and affiliated firms.[7]

Another novel contribution of our paper is that it analyzes the economic consequences of runs on shadow banks, rather than a crisis within the traditional banking sector.[8] Our results suggest that the impact of a shadow banking panic may be somewhat different, particularly if those institutions serve a unique role in the financial system. Our understanding of the consequences of crises may therefore be enhanced by focusing not only on the way in which pre-crisis booms are financed (Schularik and Taylor 2012), but also on the roles of particular types of intermediaries.

We also contribute to the extensive literature on historical banking crises in the United States.[9] The causes and macroeconomic context of the Panic of 1907 in particular have received considerable attention, both in the years that immediately followed its onset (Sprague 1910; Barnett 1910) and more recently (Moen and Tallman 2000; Odell and Weidenmier 2004; Hansen 2011; and Rogers and Wilson 2011). We provide new insights into this crisis by documenting the central role of trust companies in the contraction of credit intermediation, as suggested by Moen and Tallman (1992), and by quantifying the resulting impacts on economic activity.

[7] Some contributions to this literature study the within-firm effects of connections to multiple lenders. However, Khwaja and Mian (2008) note that the use of within-firm estimators is not feasible when the outcome variable of interest is aggregated at the firm-year level, as is the case for investment.
[8] Chernenko and Sunderam (2012) study the effects of recent shocks to money market funds.
[9] Much of this work, including Bernanke (1983), Carlson, Mitchener and Richardson (2011), Calomiris and Mason (2003), Mladjan (2012), Richardson and Troost, (2009), and Ziebarth (2013), is focused on the Great Depression. Calomiris and Gorton (2000), Wicker (2000), Hanes and Rhode (2013), Kupiec and Ramirez (2012) analyze multiple historical crises.

2. Origins and Resolution of the Panic

In this section we present a brief sketch of the events of the Panic of 1907, to establish both that the runs on trust companies were unrelated to their corporate clients, and that some commercial banks associated with the men at the center of the panic can be used to study the mechanisms by which the financial contraction was transmitted to nonfinancial firms.[10] The Panic of 1907 offers valuable lessons for the study of modern financial crises; we present the relevant institutional details in this section, and discuss the parallels with the recent crisis in the conclusion of the paper.

The panic began at a time of instability within American financial markets (Odell and Weidenmier 2004). In addition, New York's banking system had recently experienced a significant change, in the form of the rapid growth of trust companies. Incorporated under permissive state laws, they were not subject to the stricter regulations of the National Banking Act, and often specialized in financing corporate investments, acquisitions and reorganizations. Whereas in 1890, New York's trust companies' lending totaled about half of the city's national banks', by 1906, lending by trust companies was roughly equal to that of national banks.

Trust companies enjoyed broad powers, including the ability to hold corporate equity and debt, and to underwrite and distribute securities (Smith 1928; Neal 1971). But these institutions did not facilitate a large volume of payments, and did not hold substantial deposits from out-of-town banks, which made them less subject to the seasonal demands for cash from the interior than New York's national banks. Their managers thus argued that they could maintain relatively low cash reserve balances (Judd 1907), and did not need to become members of the New York Clearing House (NYCH), the private organization that

[10] Strouse (1999), Carosso (1987) and Bruner and Carr (2007) present engaging histories of the panic.

could act as a lender of last resort for its members (Gorton 1985).[11] Trust companies could gain access to the NYCH by clearing through a member bank, but only if they maintained a minimum level of cash reserves, which most found too high. In part because their status as important institutions was new, and in part because they did not perceive themselves to be vulnerable to a shock, trust companies failed to establish any mechanisms that could facilitate coordinated action or provide emergency assistance in response to a liquidity shock.

Onset of the Panic

The panic was precipitated by events that had no direct connection to any trust company. Instead, it was triggered by a failed attempt in September 1907 to corner the shares of United Copper Company, a mining concern, which resulted in significant losses for the speculators involved, and the banks and stockbrokers that financed them. The principal perpetrators of the speculation were mining entrepreneur Augustus Heinze and financier Charles W. Morse who, together with some associates, had collectively gained control of a series of relatively small commercial banks and used some of their resources to finance their ventures. Most of those banks were members of the NYCH.

On October 16, a run began on Mercantile National, the bank most prominently associated with Morse, which quickly appealed to the NYCH for aid. The NYCH provided a loan to Mercantile, and publicly pledged to support the other member banks connected to Morse and his associates. On October 20, the NYCH announced that all member banks associated with the speculators were solvent, and that a fund had been raised to provide aid to these institutions. As a condition for this aid, the NYCH required the resignation of the entire board of directors of Mercantile, and demanded that the men at the center of the

[11] New York City's national banks were required to hold reserves equal to 25% of deposits; the city's trust companies faced no reserve requirement until 1906 when a 15% reserve requirement was imposed, but only one third of it was required to be held in cash.

scandal resign from the boards of all other clearing banks (Bayles 1919: 178). The support from the NYCH and the change in management ended the runs on these commercial banks. However, the expulsions from clearinghouse banks contributed to the perception that the directors had committed fraud. As one observer noted, "the ousting of the adventurers from the banks…had left the community so keenly apprehensive that almost anything would have stampeded it" (Lefevre 1908:12).

A stampede of trust company depositors indeed began on October 21, when Knickerbocker Trust, one of the largest in the city, made two announcements: first, it had dismissed its president, Charles T. Barney, because of his "connection with Mr. Morse" and his directorships with the banks involved in the copper company speculation.[12] Second, the National Bank of Commerce, a prominent member of the NYCH, would no longer act as Knickerbocker's clearing agent.

These announcements came as a shock to Knickerbocker's depositors. The end of the clearing relationship meant that the NYCH was unlikely to support Knickerbocker; in fact the NYCH denied Knickerbocker's request for a loan on the night of October 21 (Wicker 2000: 91). The dismissal of Barney, even though it was accompanied by assurances that the firm was in sound condition, created the impression that he had behaved improperly or used the funds of the trust company to help finance the speculative schemes of Morse. A run on Knickerbocker ensued, and the firm could not withstand the heavy withdrawals without external assistance. None was provided, and on October 22, it was forced to close its doors.

This caused "panic—sheer blind, unreasoning fear" among depositors as they realized that trust companies would be denied emergency assistance from the NYCH, and "wild rumors circulated" regarding their possible ties to the men behind the copper

[12] *The Sun* [NY], 22 October 1907, p. 1.

speculation.[13] By October 23, runs had spread to several other trust companies. The Trust Company of America, which also had Barney as a director, went so far as to issue a public statement that his "company had no business relations, directly or indirectly, with Charles W. Morse, as the rumors had intimated."[14]

The deposit losses of the 38 trust companies in New York City between August 22 and December 19 of 1907 were strongly influenced by any observable connection to Charles Morse or the other speculators involved in the failed corner. We define a trust company as having a *direct connection* to those men if they had one of them on their board of directors—this was true of five. Three trust companies had what we term a *board interlock connection*—they had at least two directors in common with a trust company with a direct connection. And finally three trust companies had a *deposit connection:* they had deposited part of their capital with an institution with a direct connection, or held some of the capital of an institution with a direct connection. Deposit connections may not have been directly observable to depositors, but likely signified a business relationship that would have been known.[15]

Panel A of Table 1 shows that the extent of the deposit loss varied substantially across firms with and without a connection to the perpetrators of the United Copper corner. The deposits of the trust companies with a connection to those men declined by an average of 53 percent, whereas the trust companies with no such connection saw their deposits decline only 23 percent. We analyze the determinants of the change in each New York trust company's deposits between August and December of 1907 in Panel B of Table 1. In column (1), we regress the change in deposits on separate indicator variables for the three types of connections to the men at the center of the scandal. The estimated parameters

[13] Lefevre (1908, p. 13) and *Evening World* [NY], 23 October 1907. See also Moen and Tallman (2000).
[14] *New York Times*, 23 October 1907.
[15] Detailed information on these connections and the sources used to observe them are presented in the Data Appendix.

range from -36 to -25 percent, and they collectively account for more than 50 percent of the variation in the dependent variable. In column (2), we add several balance sheet ratios as well as the log of their ages, and the size of the estimated effect of the indicator variables for associations with the tainted financiers remains unchanged.

Measures of financial health do not capture differences in depositor clienteles. The trust companies located in uptown Manhattan likely received a greater share of their deposits from individuals, who may have been more likely to participate in runs than the institutions that placed deposits in the trust companies located downtown (Hansen 2011). In column (3) we find that the uptown firms indeed suffered greater deposit losses, but controlling for an uptown location does not substantially alter our estimates of the effect of associations with the men associated with the scandal. These results indicate that the deposit losses of trust companies can be regarded as a response to personal connections to these men, rather than the product of concerns regarding the corporate clients of the trust companies.

Resolution and Consequences of the Panic

During the panic, J. P. Morgan famously led a series of rescues of trust companies, partly funded by the U.S. Treasury, which were instrumental in containing the crisis. Although Knickerbocker Trust was refused aid by Morgan, emergency assistance was provided to other trust companies beginning on the day following Knickerbocker's closure. This provision of liquidity prevented additional trust companies from closing, but it did not bring a quick halt to the runs. Several trust companies faced heavy losses for weeks, and one of the largest, the Trust Company of America, endured the heaviest bank run in American history up to that time. The runs on trust companies ultimately ended with the creation of a final syndicated lending fund on November 6.

In contrast to the trust companies, commercial banks quickly made use of the mechanisms they had developed to manage crises. On October 26, in the face of heavy withdrawals from out-of-town banks, the NYCH issued "clearing house loan certificates" in order to provide liquidity to its members, and New York's commercial banks suspended the convertibility of their deposits into cash. Commercial banks throughout much of the country followed suit, and full convertibility of deposits by the nation's banks was not restored until January 1908. The suspension made transactions more difficult (James, McAndrews and Weiman 2011), but likely halted the spread of the panic and prevented widespread failures, as in 1930-33 (Friedman and Schwartz 1963).

Trust companies did not have access to similar mechanisms. As shadow banks, they lacked the legitimacy and institutional clout that enabled commercial banks to restrict payments to depositors, and also lacked any institutional framework for mutual assistance.[16] That trust companies had no history of coordination became painfully evident during the panic; as he attempted to assess their situation, J.P. Morgan famously remarked: "the presidents of the trust companies had to be introduced to each other, and I don't think much can be expected of them."[17]

The impact of the panic on financial intermediation, and the stark contrast between its effects on trust companies and national banks, is illustrated in Figure 1. Trust companies contracted their lending by 40 percent following the panic, whereas New York's national banks actually increased their lending. The president of one trust company noted the consequences: "As to the general effect upon credit of manufacturing or industrial companies, it very considerably restricted their operations. They were unable to borrow money and […] trade was very much depressed" (E. R. Chapman 1910: 332-33).

[16] When Knickerbocker Trust closed its doors to depositors and effectively 'suspended,' the Attorney General of New York immediately filed suit to force them into receivership (Hanna 1931).
[17] Letter from Benjamin Strong to Thomas W. Lamont, 1924, p. 8. Benjamin Strong Papers, Federal Reserve Bank of New York.

3. Data on Nonfinancial Companies and their ties to Financial Intermediaries

All of the data utilized in the empirical analysis were hand-collected for this paper. In this section we present a brief description of the sources and methods used in the creation of the dataset, while the Data Appendix provides more complete details.

We identify connections between trust companies and nonfinancial firms by the presence of board interlocks. To observe interlocks, we collect the names of all directors and officers of all NYSE-listed industrials and railroads, and cross-reference them with lists of all directors of commercial banks and trust companies.

Board interlocks between financial and nonfinancial firms were widespread in the early twentieth century, and they signified close relationships. Although no comprehensive data on lending or underwriting exists for this period, the historical literature highlights many examples of trust companies lending to or underwriting for firms represented on their boards (Dewing 1914; Lamoreaux 1994). Moreover, newspaper accounts during the crisis detailed significant lending to trust company directors and their affiliated companies.[18] Finally, archival records, which survive for only a small number of New York trust companies for this time period, suggest that lending to affiliated corporations could be quite significant, and occurred at non-negligible levels even among the oldest and most conservative institutions (see the Data Appendix, Table A2).

We construct a new panel dataset of accounting information for all NYSE-listed industrial companies and railroads from *Moody's Manuals*, which provide firm-level data obtained from annual reports. The sample consists of the 125 firms that reported an income statement and balance sheet in 1906, and the analysis is restricted to the period 1903-1912. In order to reduce the influence of outliers, we trim all accounting variables at the top and

[18] See, for example, *New York Sun*, 22 October 1907, p. 2; *New York World*, 30 November 1907, p. 2; *New York Times*, 1 December 1907; and *New York Tribune*, 3 December 1907.

bottom one percent.

Tables 2 and 3 present summary statistics.[19] Column (1) of Table 2 reveals that in 1907 NYSE-listed nonfinancial companies had extensive connections to financial intermediaries; the average firm in the sample had nearly 4 trust company directors on its board, as well as 1.5 directors from New York's six dominant commercial banks.[20] Railroads and other transportation enterprises made up 56 percent of the sample, with the others mostly engaged in manufacturing.

Throughout the analysis that follows, we designate as the differentially "affected trust companies" those that were in the top quartile of deposit losses from August to December, 1907. Among the 125 nonfinancial firms in our sample, 61 had a director in common with one of those trust companies. Column (2) of Table 2 reports the difference between firms that were affiliated with at least one the worst-affected trust companies, and firms that had no such affiliation. Affiliated firms had larger boards, were more likely to be railroads, and had more extensive connections to financial institutions, with 2.7 more trust company directors on their boards, and nearly one more director from a major New York commercial bank. These firms were also older, but they did not differ in the ratio of their retained earnings to total common equity, which is an alternative measure of the stage of a firm's life cycle (DeAngelo, DeAngelo and Stulz 2006). Thus, it does not appear that weaker or less "established" firms were more likely to have relationships with the financial institutions that came under the greatest pressure during the panic.

Another potential concern is that firms affiliated with affected trusts might have disclosed less accurate or detailed financial information, or suffered from weaker corporate

[19] Definitions of all variables are presented in the Data Appendix. Since firms did not produce statements of cash flows in our period, we do not observe capital expenditures directly. We follow Ramirez (1995) in calculating investment as the change in the property account plus depreciation, and convert it into a rate by dividing by lagged total assets. As some of our firms disclosed depreciation expense only in later years, in all investment regressions we include an indicator variable for whether or not depreciation was reported.

[20] These six banks held about three-fourths of all net deposits of out-of-town banks in the city's national banks (Sprague 1910).

governance. As a proxy for the quality of disclosure, we calculate the number of lines of items on the firms' balance sheets (McCartney and Arnold 2002). Firms connected to the hardest-hit trusts published more detailed financial statements (nearly 4 additional lines, relative to a mean of 21). We also measure the presence of insiders (defined as current and past officers) among directors, which is generally interpreted as evidence of weak monitoring by the board (Adams, Hermalin and Weisbach 2010). Firms affiliated with affected trust companies actually had slightly fewer insiders on their boards.

Table 3 summarizes the financial characteristics of the NYSE-listed companies in 1906. Since the fiscal year end date varied across firms, throughout the analysis we assign to year *t* the financial information corresponding to fiscal years ending from July of *t* to June of year *t+1*. Column (1) reveals that the firms in our sample were very large enterprises, with an average value of log total assets of about 18.1 ($74.3 million).[21] The average leverage ratio, defined as bonded debt divided by total assets, was 0.29, and the average firm held about three percent of its assets in cash. We calculate two measures of profitability, Return on Assets (ROA), defined as net income divided by total assets, and Return on Equity (ROE), defined as net income divided by common shareholders' equity, which were 0.04 and 0.08 on average. The average firm paid out about 3 percent of the book value of its common shares in dividends, and faced an average interest rate, measured as interest expense divided by long-term debt, of 4.8 percent. The average investment rate, relative to lagged total assets, was 3.6 percent.

Table 3 also investigates whether these financial characteristics differed by the firms' connections to affected trusts. Column (2) shows that although profitability, dividends and interest rates were similar across groups, the affiliated firms were larger, more levered, had lower cash holdings, and invested at slightly higher rates. However, the

[21] In dollars of 1906. As most of the accounting data we analyze are ratios, we utilize nominal values.

two groups of firms did not evolve along different trends in the years preceding the panic, which is crucial for our estimation strategy. Column (3) presents the estimated coefficient from separate regressions of each outcome variable on an indicator variable for an affiliation with an affected trust company interacted with a time trend using data from 1903-1906. None of the differential trends are large in magnitude or statistically significant.

4. Firm-Level Effects of the Financial Contraction

The effects of the credit supply shock on nonfinancial firms were likely to be heterogeneous (Bernanke 1983). Firms with substantial collateral, or with well-established reputations, should have suffered less from problems related to asymmetric information, and had an easier time obtaining alternative sources of finance.[22] As there was considerable variation in the size and reputation of the firms listed on the NYSE during our period, we expect the effects of the contraction in financial intermediation to differ among the firms in our sample. Our empirical strategy accounts for this heterogeneity by allowing the effect of ties to affected trusts to vary by firm size. Following similar approaches, recent work on modern firms has established a robust relationship between firm size and susceptibility to financial shocks (Khwaja and Mian 2008; Chodorow-Reich 2014).

Results: Stock Market Event Study

We begin by analyzing changes in stock returns of NYSE-listed firms around the onset of the panic. The stock market suffered significantly during the crisis, losing about 25 percent of its value from October to November. If investors perceived that the losses of trust company deposits would adversely impact their nonfinancial clients, we should observe a

[22] This argument suggests that the firms in our sample, some of the largest and best-established enterprises in the United States, were among those firms *least* likely to suffer as a result of the shock to the trust companies.

larger decline in the stock prices of firms connected to those trust companies relative to other firms. We collect all necessary information from *The New York Times* (see the Data Appendix for data sources, variable definitions, and discussion of methodological issues). Securities markets were relatively illiquid early in the twentieth century; our data contain cumulative returns for about 68 percent of the firms in the sample, perfectly split between firms with and without connections to affected trusts.

As a starting point, we create equal-weighted portfolios of weekly industry-adjusted common stock returns.[23] We index the portfolios to their initial value about a month prior to the panic, and we cumulate them until the end of the year. Figure 2 shows that all portfolios followed a similar trend until October 18, around the time the run on Knickerbocker started. In the following week, the returns for small firms connected to affected trusts plummeted, and had not recovered by the end of the year.[24] In contrast, the other three portfolios did not suffer significantly during the runs, and returned to their pre-panic level by the end of December.

To perform a more rigorous analysis of the market reaction in the weeks surrounding the run on Knickerbocker, we employ an event study methodology. For each firm in the sample, we calculate cumulative industry-adjusted weekly returns over a window of weeks [-*k*, *k*] centered at the onset of the panic. Our empirical strategy consists of estimating:

$$Ind.-Adj.Cum.Return_i = \alpha + \beta_0 Affectedtrust_i + \beta_1 Affectedtrust_i \times logassets06_i + \beta_2 logassets06_i + \delta X_i + \varepsilon_i , \quad (1)$$

[23] We define firms as small if their log level of assets was below the median for the firms with non-missing returns on 25 October 1907, the sample that is the main focus in the event study analysis. Returns are equal-weighted due to the small number of firms in each portfolio.

[24] Contemporary observers noted the differential negative performance of the shares of small firms affiliated with the affected trusts. One journalist described October 22, the day of Knickerbocker's suspension, as a day of "severe liquidation" on the stock market, and among the "heaviest sufferers" were small firms that had been associated with institutions connected to Charles W. Morse. *New York Times*, 23 October 1907.

where $Affectedtrust_i$ is an indicator equal to one for firms with at least one director from one of the trusts among the top 25 percent in deposit losses; $logassets06_i$ is the log level of assets in 1906; and X_i includes firm characteristics prior to the panic that may affect returns. The differential effect for firms affiliated with affected trusts is equal to $\beta_0 + \beta_1 logassets06_i$. If the market perceived that firms affiliated with an affected trust company would suffer, the effect on the firm's return should be negative $(\beta_0 < 0)$. The interaction term between affected trust and log assets captures the differential effects for firms of different sizes; we expect larger firms connected to affected trusts to have higher returns during the panic than smaller firms with similar connections $(\beta_1 > 0)$.

Using end-of-week prices, we center the event on the closest date available after Knickerbocker's October 22 failure—Friday October 25. Panel A of Table 4 presents differences in means for industry-adjusted returns cumulated from one week prior to one week after the run on Knickerbocker. Firms with connections to affected trust companies saw a decline in returns of about three percent relative to their industry peers, while market values increased by about two percent for non-connected firms. The difference is driven entirely by small firms.

Panel B of Table 4 analyzes the cross-sectional variation in market valuations as the runs unfolded using the model specified in equation (1). Column (1) presents regression estimates for returns that are not industry-adjusted, for a one-week window around October 25. The runs had a large negative effect on firms affiliated with affected trusts; both β_0 and β_1 are statistically significant and have the expected signs. For a firm of median size affiliated with an affected trust company, cumulative returns were reduced by 6.9 percentage points (= -1.046 + [0.0534×18.29]). This effect is generally consistent with the results of Chava and Purnanandam (2011), who estimate a differential decline of 3.54 percentage points in the returns of U.S. firms connected to banks exposed to the Russian

crisis in 1998. In our sample, the effect is greater for smaller firms; at the 25th percentile of the assets, the decline in returns was 10.4 percentage points (=-1.046 + [0.0534 ×17.64]). To better gauge the magnitude of our estimates, we compare the predicted returns based on all of the estimated coefficients in column 1 of Table 4. Our estimates indicate that a median-sized industrial firm, for example, would have had 60 percent lower returns relative to an identical firm purely due to its connection to an affected trust company (based on predicted returns of -0.185 for the connected firm and -0.116 for the non-connected firm).

We next analyze industry-adjusted returns to address the concern that firms within a given industry may have faced common shocks. The results are unchanged by this transformation (column 2). In column (3) we include many of the characteristics that differed across firms with and without connections to affected trusts prior to the panic, including the degree of connections to financial firms, the leverage ratio, and the cash holdings ratio. Adding these controls does not affect the estimated difference in cumulative returns relative to industry peers. The results are also robust to using a two-week event window (column 4).

Since our strategy compares the cross-sectional differences in returns, one concern could be that these results may not be specific to the financial crisis—perhaps firms affiliated with affected trust companies performed worse generally. As a "placebo" test, we center our analysis at a fictitious event date as early in advance of the panic as our price data allows, September 13, 1907. As shown in column (5), we find no statistical differences in returns, and the magnitudes of the estimated values of β_0 and β_1 are much smaller and have the opposite sign than during the panic.

Results: Firm Performance

We now turn to the medium-run effects of the panic, and analyze the accounting performance of nonfinancial firms using our panel dataset. Since firms have many margins of adjustment to changes in financing costs, we study the effects of affiliations with the affected trust companies on five outcomes: ROA, ROE, dividend rates, average interest rates, and investment rates. Our preferred empirical specification is:

$$\pi_{it} = \alpha_i + \gamma_t + \lambda_1 Affectedtrust_i \times postpanic_t + \lambda_2 Affectedtrust_i \times postpanic_t \times logassets06_i + \delta \boldsymbol{X}_{it} + \varepsilon_{it}, \quad (2)$$

where π_{it} is one of our measures of performance for firm *i* during year *t*; α_i and γ_t are firm and year fixed effects; $\boldsymbol{X}_{it}$ is a vector of time-varying firm characteristics, such as industry-specific trends or pre-panic characteristics interacted with trends; $Affectedtrust_i \times postpanic_t$ is an indicator for firms affiliated with a differentially affected trust in 1907 multiplied by an indicator for the years 1907 and later; and the $Affectedtrust_i \times postpanic_t \times logassets06_i$ is the same indicator multiplied by the log value of the firm's assets in 1906. In this framework λ_1 and λ_2 are the time-averaged effects for the post-panic years, relative to the years up to 1906, and we again expect $\lambda_1 < 0$, and $\lambda_2 > 0$ (and the reverse effects for the interest rate). In order to estimate the variation in the strength of the effects over time, we also modify the interactions in equation (2) by replacing the $postpanic_t$ indicator with indicators for individual years:

$$\pi_{it} = \alpha_i + \gamma_t + \sum \theta_{1t} Affectedtrust_i \times year_t + \sum \theta_{2t} Affectedtrust_i \times year_t \times logassets06_i + \delta \boldsymbol{X}_{it} + \varepsilon_{it}. \quad (3)$$

In this specification, θ_{1t} and θ_{2t} are the differences in differences for firms affiliated with affected trusts for a given year *t*, relative to an excluded period.

It is important to note that, by design, our strategy will understate the full effect of the disruption to financial intermediation. Firms in the control group may have been affected

by the financial shock—for example, through connections to some of the less hard-hit trust companies—and some firms in the treatment group may have been able to find other sources of financing—for example, through connections to prominent financiers. Our categorization of the treated and control groups based solely on the presence of an affiliation with a severely affected trust company should therefore make it more difficult for us to distinguish empirically between the two groups.

Before presenting our main results, we examine the differences in trends between treated and control firms in the years surrounding the panic. Figure 3 plots each year's estimates of $\theta_{1t} + \theta_{2t}logassets06$ for our five outcome variables from specification (3) for firms at the 25th and 50th percentiles of the size distribution, relative to the excluded year of 1906. Although the magnitude and timing of the effects vary across the outcomes, they are consistently negative: profitability, dividends, and investment contract, and average interest rates rise after 1907. Consistent with our expectations of heterogeneous effects, in each case, the outcome is more severe for the smallest firms than for the firms of median size. Reassuringly, the lines do not display any obvious trends prior to the panic, and they generally change slope at the time of the panic, or in the following year. Thus, it is unlikely that any estimated post-panic effect is the result of preexisting differential trends in the dependent variables between firms with and without affiliations with affected trusts.

In order to more precisely assess the magnitude of the effects, Table 5 presents estimates of specification (2). In all regressions, standard errors are clustered by firm. We begin with a basic specification that includes industry-specific trends, to account for the fact that firms affiliated with affected trust companies tended to operate in somewhat different industries (Panel A). The credit supply shock adversely impacted firms' ability to finance their operations and investments, particularly for small firms. To help interpret the economic magnitude of these estimates, Panel A also reports the implied effects for firms of

different sizes. For firms at the 25th percentile of assets, we find a moderate increase of 40 basis points for the interest rate, which is equivalent to 8.3 percent of the mean 1906 rate. This estimated impact likely understates the true effect on borrowing costs because our measure reflects the average interest rate on the debt stock, rather than the marginal borrowing costs for new financing. Small firms also experienced declines of 0.4 and 1.1 percentage points for ROA and ROE (10.5 and 13.5 percent of the 1906 means, respectively), and of 0.7 percentage points for the dividend rate (about 22 percent relative to the 1906 mean).[25]

Financial constraints appear to have had a particularly important impact on capital expenditures: the effects on investment for small firms were much larger—1.8 percentage points, equivalent to a 49.7 percent contraction relative to the mean 1906 investment rate, which was 3.6 percent. The 90 percent confidence interval for this estimate is (-0.029, -0.006), encompassing effects ranging from 80 percent to 16 percent of the 1906 mean. This is generally comparable with the findings of Almeida et al (2012), who attribute a decline in investment of one-third relative to the pre-crisis investment level in 2007 to frictions introduced by debt maturity for a small sample of U.S. publicly traded companies.

The effects on these outcomes were much weaker for the large firms in the sample, which is consistent with the notion that information asymmetries aggravated the consequences of the contraction in credit intermediation. The size of the estimated effects falls in absolute magnitude by one-fourth to one-half for a median-sized firm, and none of the outcomes, except investment, show economically meaningful effects for firms at or above the 75th percentile.

The summary statistics presented in Tables 2 and 3 suggest that the firms affiliated with the affected trusts had more extensive connections to financial institutions, were more

[25] For the 125 firms in the sample, the median value of log assets in 1906 is 18.025, the 25th percentile is 17.302, and the 75th percentile is 18.865. The 1906 mean values of the average interest rate, ROA, ROE, and the dividend rate were 0.048, 0.038, 0.084, and 0.032, respectively.

levered and held lower cash balances prior to the crisis. To rule out that our estimates are driven by differential trends among firms with these characteristics, Panel B of Table 5 includes their 1906 levels interacted with time trends. The results indicate that differential trends were not responsible for the estimated effects of affiliations with affected trusts.

The relatively large effect on investment implies that our sample firms responded to the shock primarily by cutting their capital expenditures; they cut their dividends as well, but to a lower extent. The lack of alternative sources of financing may be particularly severe during a financial crisis, leading hard-hit firms to drastically reduce their investments. A contraction in fixed assets induced even by a short-run but deep shock to credit availability may have longer-run consequences for the growth path of these firms, and for the aggregate economy. We next study the macroeconomic impact of the crisis, and the duration of its effects.

5. Aggregate Effects of the Financial Contraction

In the aftermath of the Panic of 1907, the American economy suffered one of its deepest recessions prior to the Great Depression. To what extent was the shock to financial intermediation induced by the runs on trust companies responsible for this decline?

Unfortunately, no firm-level data exist for output or employment during this period, so we cannot use our framework to analyze the effect of the financial shock on those variables. Instead, we focus on aggregate investment. A unique aspect of our data is that we observe firms for several years after the shock. We therefore calculate the magnitude and duration of the aggregate effect on investment, by estimating the total change in capital spending accounted for by differential exposure to the hardest-hit trust companies in each year following the panic. The decline in investment likely depressed contemporaneous and

future output through its effects on spending, hours demanded, and capital accumulation (e.g., Gilchrist and Zakrajsek 2012; Hall 2011); we leave the calculation of those magnitudes for future work.

Aggregate Impact on Investment

In order to analyze the annual effect on investment of affiliations with the severely affected trust companies, Panel A of Table 6 presents estimates of specification (3), where the effect is estimated separately in each post-panic year, relative to the years 1903-06. The total change in investment in year *t* that was caused by affiliations with affected trust companies can be calculated using the estimated parameters from equation (3), as follows:

$$\sum_{i \in M} (\hat{\theta}_{1t} + \hat{\theta}_{2t} logassets_{06i}) Assets_{(t-1)i}. \quad (4)$$

For each firm *i* in the set *M* of affiliated with affected trusts, the term in parentheses is the estimated annual change in its post-panic investment rate that is solely attributable to the financial shock. Recall that our investment measure is calculated in proportion to the firm's lagged assets. To translate the estimated change in the investment rate into dollar values of investment in year *t*, we multiply firm *i*'s rate by its level of assets in year *t-1*. Finally, we aggregate over all firms in *M*, but we include only those firms at points in the size distribution where $\hat{\theta}_{1t} + \hat{\theta}_{2t} logassets_{06i}$ is statistically distinguishable from zero.

In order to capture the change in investment induced by the financial shock, it is necessary to apply (4) not just to the sample firms, but to all firms with affiliations with the worst-affected New York trust companies. The sample NYSE-listed companies represent just a small subset of such firms; at least 540 additional nonfinancial corporations maintained such affiliations. These additional firms generally disclosed very little information, but 274 of them reported the book value of their equity, which can be used to estimate the value of their assets (see the Data Appendix for details.) Under the assumption

that the financial shock had the same impact on these additional firms as it did for sample firms of equivalent sizes, we use the method described above to calculate the total change in their investment, based on their estimated levels of assets.

We first calculate the impact for the year 1908, when economy-wide corporate investment fell by $700 million, a 25 percent decline relative to its 1907 level. Columns (1) and (2) of Table 7 present the results for the small and medium-sized, and large firms in the sample, whose investment fell by $80.9 and $221.3 million, respectively. Among the small and medium-sized firms, the estimated effect of affiliations with the hardest-hit New York trusts was -39.8 million—that is, fully 49.2 percent of the total decline can be attributed to the contraction in intermediation. In contrast, for the large firms in the sample, only $7.9 million, or 3.6 percent, of their decline in investment was accounted for by affiliations with the worst-affected trust companies. Column (3) of the table presents the results of the same calculation for the 274 additional firms affiliated with the affected trusts, for which we do not observe actual investment data. The estimated effect among those firms totaled -$80.8 million.

As column (4) shows, by adding the effects from the sample firms to those from the additional firms, the decline in investment accounted for by affiliations with the affected trusts is $128.5 million, or 18.1 percent of the decline in private investment in the economy in 1908.[26] This number likely understates the true effect, since the parameters $\hat{\theta}_1$ and $\hat{\theta}_2$ are estimated from a framework that captures only the relative impact of differential exposure to the financial shock, and since the 266 additional firms for which we have no data are excluded from the calculation.

These magnitudes seem generally consistent with those available from studies that calculate aggregate magnitudes from firm-level estimates of credit supply shocks. For

[26] When we use instead the estimates of the time-averaged effect of connections to affected trust companies for the entire post-panic period presented in Panel A of Table 5, we can attribute 15.1 percent of the aggregate change in investment in 1908 to the financial contraction.

example, Paravisini et al. (2011) find that credit shortages account for 15 percent of the total decline in Peruvian exports following the recent crisis, and Chodorow-Reich (2014) attributes between 35 and 50 percent of the reduction in employment among the small and medium size firms in his sample during 2008-09 to credit frictions. Complementing these findings, our results indicate that the lending channel can account for a substantial fraction of the contraction in aggregate investment during financial crises.

Duration of the Effect on Investment

The macroeconomic literature generally finds a protracted contraction in real economic activity following financial crises (Reinhart and Rogoff 2009; Jordà, Schularik and Taylor 2011). Yet we know relatively little about the extent to which the persistence of recessions is related to the distortions in credit markets. We utilize our estimation strategy to study the effects of the Panic of 1907 on firm and aggregate investment in the years following the crisis.

The impact of the financial contraction on investment was sizable in 1908, but as the annual estimates in Panel A of Table 6 make clear, its effect decreased in magnitude over time.[27] For the small and medium-sized firms, the estimates remain statistically significant for three to five years after the shock, whereas for the large firms, the effect is only significant in 1908. Panel B of Table 6 presents the total dollar value of the decline in investment accounted for by affiliations with the worst-affected trusts for each year, computed using the same methods as those of column (4) of Table 7. Over time the effect generally declines in magnitude, and by 1912 the total estimated impact is equivalent to only 38.6 percent of the 1908 impact. Our sample ends in 1912, and we therefore cannot measure the full duration of the effect. But these results are consistent with Jordà,

[27] The effects of the financial shock on the other outcome variables included in our firm-level analysis exhibited similar reductions in magnitudes over time; Table A6 of the Results Appendix presents those results.

Schularick and Taylor (2011: p. 32), who find that recessions following financial crises produce a decline in real investment per capita lasting at least five years.

The persistence of the effect suggests that firms affected by the financial shock may have had difficulties accessing credit markets for several years, either because lending relationships were important or because other financial institutions were imperfect substitutes for trust companies. But the shock may have induced firms to cut their investments well after the credit constraints were lifted. For example, firms may have chosen to reduce investment and maintain higher liquidity as a precaution against future liquidity shocks (Aghion, Fahri and Kharroubi 2012). Alternatively, the customers of constrained borrowers may have switched to other suppliers, leading to a lower growth and investment path for these firms.

6. Mechanisms of Effects and Robustness of Results

Mechanisms: Lending Channel, or Reputation?

Our results are consistent with two possible channels through which affiliations with trust companies may have had negative consequences for nonfinancial firms. First, if ties through the board of directors facilitated lending or underwriting, then nonfinancial firms may have experienced a negative shock to their supply of external financing. This is the 'lending channel.' But a second possibility is that a relationship with a troubled financial institution may have made suppliers or other lenders uneasy about its quality. That is, the affiliation with a trust company associated with a scandal may have tarnished the firm's reputation. This 'reputation channel' could be thought of as a special case of what has been termed the 'balance sheet channel' (Bernanke and Gertler 1995).

The events that triggered the Panic of 1907 present an opportunity to distinguish between these two channels. As discussed in Section 2, the commercial banks controlled by Charles W. Morse and his associates came under pressure early in the panic, but the clearinghouse quickly provided them with aid, and halted the runs. They were also unlikely to have been heavily involved in long-term corporate finance (Carosso 1970). Nonfinancial corporations affiliated with those banks were therefore unlikely to have suffered a severe contraction in available financial services—the lending channel should not have been significant. But because those banks were strongly identified with the tainted speculators, their affiliated corporations would have been stigmatized by the scandal—the reputation channel should have been operative.

Morse and his associates held directorships with commercial banks that had board interlocks with 65 of the 125 companies in our NYSE sample. To assess the relative importance of the reputation channel, we estimate a version of specification (2) where the trust company interactions are replaced with interactions for affiliations with those commercial banks. To bias the estimates towards finding an effect, these specifications include only a minimal set of covariates, and do not control for connections to affected trust companies.[28] As shown in Table 8, we find no evidence that nonfinancial corporations affiliated with those banks suffered differentially in the years following the panic. The point estimates on investment and interest rates are essentially zero, and the estimates for the other outcomes are considerably smaller than their counterparts in Panel A of Table 5. These results suggest that the reputation channel was not responsible for our findings.

[28] There were 39 companies linked both to the affected trusts and commercial banks, 26 linked only to the affected commercial banks, and 22 linked only to affected trusts. As we show in the Results Appendix (Table A4), when we also control for affiliations with trust companies, most of the parameters associated with affected commercial banks switch signs, implying differential *positive* effects for small firms. In contrast, the effects for affiliations with trust companies remain essentially the same as those of Table 5.

Robustness of the Effects on Firm Performance

In this section, we address the most significant sources of concern regarding our results. We provide further robustness analysis in the Results Appendix.

The observed differences in a number of characteristics raise the possibility that the firms that had no affiliation with an affected trust may not be an appropriate control group for those that did. This concern does not seem severe because the estimates in Table 3 indicate that there were no differential pre-panic trends along these (or other) characteristics. Still, to address this problem more thoroughly, we use estimated propensity scores to restrict the sample to the common support in the propensity to have an affiliation with an affected trust, and also weight the observations by their inverse propensity scores.[29] The results of these regressions are presented in Panel C of Table 5. The estimated effects remain generally unchanged, indicating that it is unlikely that a lack of common support creates bias in the results presented in Tables 5 to 7.

Another issue of concern is that the results may be driven by selection on time-varying unobservable firm characteristics. If firms that were more sensitive to an economic downturn were matched to trust companies that were adversely affected by the panic, then this selection mechanism could account for the results that we attribute to the connections to affected financial intermediaries.

To test for the vulnerability of our affiliated firms directly, we use the experience of an earlier recession and financial crisis. In 1903 and 1904, the United States suffered a prolonged recession that included a financial panic during 1903. This so-called "Rich Man's Panic" saw a sustained decline in securities prices, and led to a significant

[29] Specifically, we estimate a firm-level probit regression of an indicator for an affiliation with an affected trust with 1906 measures of log assets, leverage, cash holdings, the number of board seats held by major NY commercial banks, and industry indicators. We then restrict the sample to the common support in the propensity to have such an affiliation; that is, we eliminate firms without an affiliation with a propensity score below the minimum for the firms with an affiliation (and above the maximum for those with an affiliation). We weight the remaining observations by the inverse of the propensity score to eliminate all significant differences in the chosen firm characteristics.

contraction in credit markets. Although the recession was not as severe as the one that followed the Panic of 1907, and did not result in widespread bank failures, it did produce substantial numbers of commercial failures and a significant contraction in demand (Romer 1999). If the firms affiliated with affected trust companies in 1907 were fundamentally similar four years prior to that shock, then their experience during 1903 and 1904 should provide a test of whether they are indeed a differentially vulnerable group of firms.

Using data from 1900-06, we investigate whether the firms that had affiliations with affected trusts in 1907 performed differentially worse in 1903-04. To conduct this analysis, we add the available accounting data from 1900 to 1902 in order to create a pre-panic period, and designate 1905 and 1906 a post-panic period. We estimate a regression similar to specification (2), comparing the difference in the years 1903-04 relative to the pre- and post-period, for firms that were affiliated in 1907 with the hardest-hit trust companies. As in our earlier analysis, we interact the trust company affiliation indicator with the level of assets in 1904 to allow for heterogeneous effects.[30] To bias our estimates in favor of finding a result, we designate only 1903 and 1904 as the 'treated' period, and include minimal controls.

The results, reported in Table 9, indicate that the firms affiliated with affected trusts in 1907 did not perform differentially worse in the 1903-04 recession. All of the estimates are substantially smaller than the comparable estimates in Panel A of Table 5, and most have the opposite sign; for dividends, the parameters even become statistically significant with the signs reversed. These results contradict the hypothesis that the firms affiliated with the worst-affected trust companies in 1907 were differentially vulnerable to the effects of a shock.[31]

[30] Ideally we would like to use the value of assets in 1902, before the recession started. However, we observe the value of assets for only 38 firms in 1902, whereas we observe them for 90 firms in 1904.

[31] The validity of this test hinges on whether or not the economic fundamentals of the firms affiliated with affected trusts in 1907 were substantially the same in 1903. Two potential sources of concern could be that

7. Conclusion: Lessons of the Panic

The Panic of 1907 shares many important features with the recent financial crisis. But in the century that passed between these two events, the structure of the American economy in general and its financial markets in particular have transformed dramatically. One must therefore be cautious generalizing from historical experience. With that caution in mind, what lessons can be gleaned from the experience of the 1907 panic?

First, the Panic of 1907 offers insights into the consequences of financial regulations, and suggests that strict rules intended to limit risk-taking may sometimes promote the growth of riskier institutions outside of the purview of those rules. Similar to the rise of modern shadow banks, the growth of New York's trust companies in the years prior to the panic was fostered by the relatively stringent regulations imposed on national banks. But trust companies grew so rapidly that their capacity to generate systemic risk was not well understood by bank regulators or, more importantly, *by bankers themselves*. And whereas the traditional banking system developed institutions for cooperation and for controlling panics, New York's trust companies did not. Their leaders argued that since trust companies did not facilitate a large volume of payments, they would not be subject to the seasonal shocks that often buffeted the banking system, and they therefore did not need to join the clearinghouse, or hold substantial cash reserves. When an entirely different shock engendered distrust among their depositors, the trust companies nearly collapsed.

A second and related point is that the panic illustrates the problems posed by an institution acting as a lender of last resort only for its members. The panic originated among a few commercial banks, and the runs among those institutions were quickly contained by

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some firms were subject to federal antitrust investigations, or were involved in mergers that may have weakened their balance sheets, after 1904. In the Results Appendix, we show that our estimated effects for 1907 are comparable or, for some outcomes, stronger when we exclude firms that were involved in antitrust cases or major mergers from the analysis.

substantial support from the clearinghouse (NYCH). But the clearinghouse's decision to expel the individuals whose actions initiated the crisis triggered runs on the trust companies also associated with those men. When Knickerbocker Trust appealed for aid, the NYCH denied their request, and Knickerbocker's closure spread havoc among New York's trust companies. The runs on trust companies could likely have been prevented if the clearinghouse had chosen to support them as strongly as it had supported its own member banks; instead its actions aggravated the crisis among non-member institutions.

The runs were ultimately arrested by a series of privately organized lending arrangements. But this suggests a third lesson of the Panic of 1907, which is that once a significant bank panic gets underway, emergency lending measures to bring it under control may not be sufficient to prevent it from contracting economic activity. Evidence on the effectiveness of bailouts in containing the macroeconomic impact of financial crises is scarce. The Federal Reserve remained mostly inactive as banks failed in the early 1930s, which helped turn a severe recession into the Great Depression. In contrast, the Fed provided extraordinary lending facilities during the recent crisis. That lending helped to halt the panic, but was insufficient to prevent the crisis from contracting economic activity. The experience of the 1907 panic further validates the limited impact of emergency loans or bailouts. Corporations affiliated with the hardest-hit trust companies suffered differentially following the panic, even though those trust companies were rescued.

A fourth lesson is that the nature and severity of the economic consequences of runs on shadow banks may be different from the effects of a crisis within the traditional banking system. Trust companies did what commercial banks could not; even though New York's commercial banks withstood the panic reasonably well, their operations were likely imperfect substitutes for the set of financial services provided by trust companies. The significant cuts in investment made by clients of trust companies following the panic are

consistent with the unique role of those institutions in facilitating access to long-term finance. One might speculate that the impact of the recent financial crisis was felt differentially within those sectors most heavily reliant on the modern shadow banking system for funding.

Finally, the experience of the Panic of 1907 suggests that the distortions to economic activity resulting from disruptions to financial intermediation may persist well beyond the time when normalcy is restored to financial markets. The runs on trust companies ended in November 1907, the resumption of cash payments to depositors of commercial banks occurred in January 1908, and the economy reached a 'trough' and began growing in June 1908. Yet some of the smaller firms affiliated with the most severely affected trust companies continued to invest at lower rates for three to five years after the panic. A promising subject for further theoretical and empirical inquiry is the mechanism by which financial frictions might produce such persistent effects.

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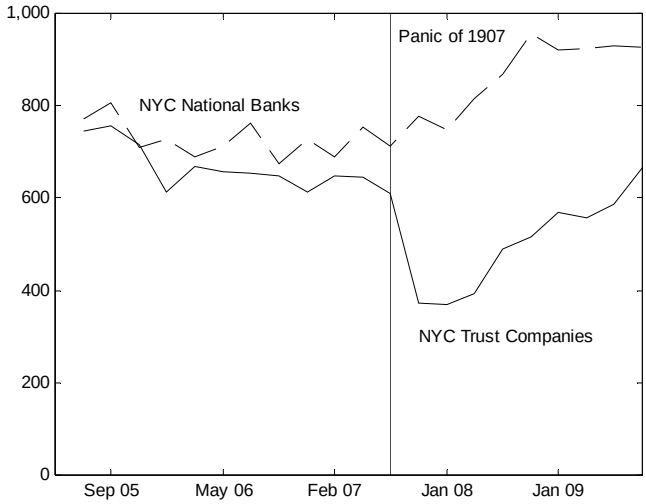

Figure 1: Total Lending, New York City National Banks and Trust Companies, in Millions
For national banks, total loans and discounts plus overdrafts; for trust companies, total lending on collateral plus bills purchased. Sources: see Data Appendix.

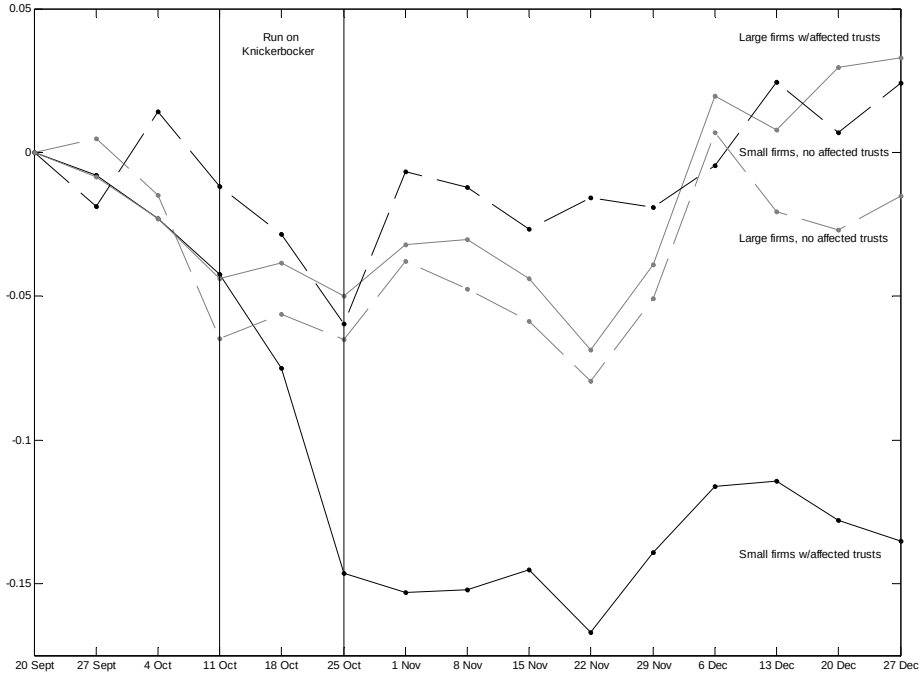

Figure 2: Stock market reaction, by affiliation and by size
The lines in the figure present the cumulative, industry-adjusted weekly returns from 20 September through 27 December, 1907. Small firms are defined as those with below-median assets. The run on Knickerbocker began silently around 16 October, and ended with the closure of the firm on 22 October.

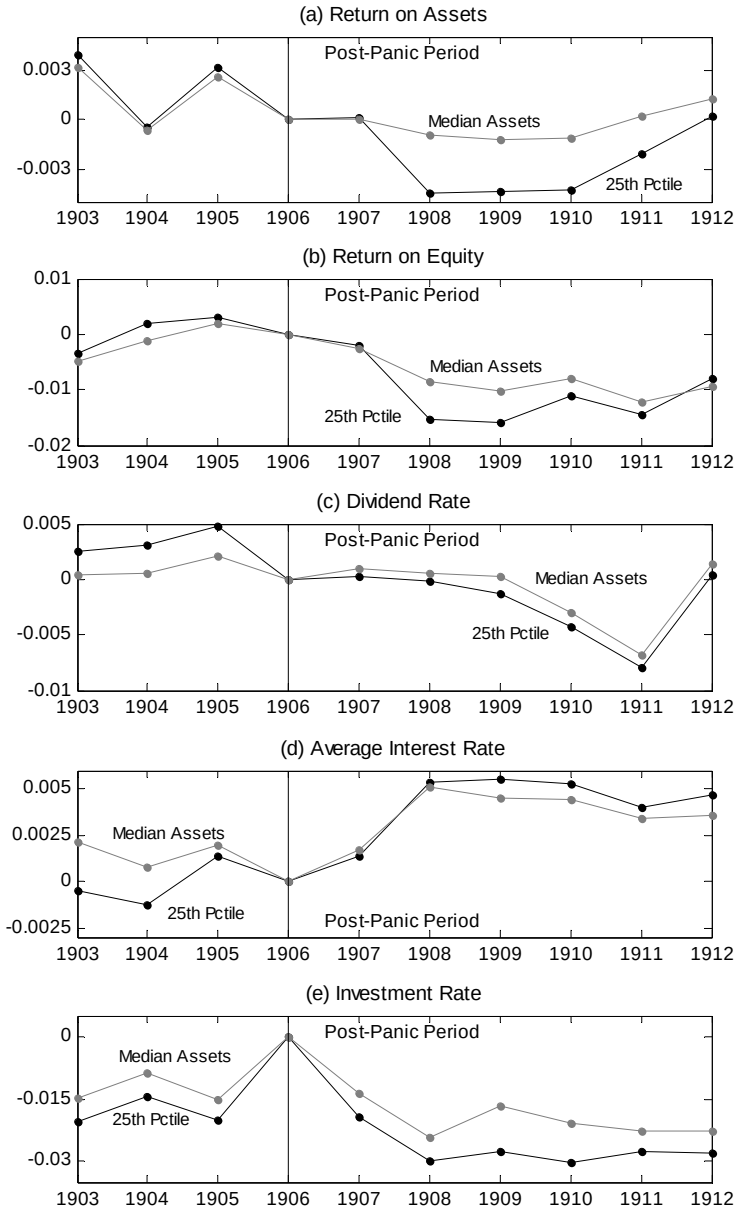

Figure 3: Annual estimated differences between firms with and without affected trusts
The lines plot the annual difference between firms with and without affected trusts on their board in 1907, as estimated in specification (2), relative to the excluded year of 1906. The darker lines are the estimates for firms at the 25th percentile of assets; the lighter lines are for firms with the median level of assets.

Table 1
Percent Change in Deposits, August-December 1907,
New York City Trust Companies

A. Change in Deposits				
	All Trust Companies	Direct, Interlock or Deposit Conn.	No Connection	Difference
Mean	-0.321	-0.534	-0.234	-0.300**
	[0.195]	[0.204]	[0.106]	(0.050)
Observations	38	11	27	

B. OLS Regressions on Change in Deposits			
	(1)	(2)	(3)
Direct Connection	-0.362**	-0.380**	-0.363**
	(0.113)	(0.0963)	(0.0721)
Board Interlock Connection	-0.248*	-0.244*	-0.226**
	(0.0931)	(0.107)	(0.0633)
Deposit Connection	-0.248**	-0.251**	-0.191*
	(0.0707)	(0.0834)	(0.0866)
Net Worth / Assets		0.104	0.0249
		(0.186)	(0.163)
Cash / Assets		1.414	3.879
		(3.710)	(3.041)
Stock and Bond Investments / Assets		0.0763	-0.0339
		(0.155)	(0.146)
Log(Total Assets)		-0.00576	-0.0585+
		(0.0335)	(0.0317)
Log(Firm Age)		-0.0226	-0.0126
		(0.0299)	(0.0252)
Uptown Headquarters			-0.200**
			(0.0554)
Constant	-0.234**	-0.186	0.655
	(0.0211)	(0.550)	(0.517)
Observations	38	38	38
R-squared	0.524	0.590	0.725

Notes: Standard deviation in brackets and standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1. Robust standard errors presented in Panel B.

Table 2
Summary Statistics, Firm characteristics

		Mean [Std Dev] 1907	Difference: Firms with Affected trust Interlock, 1907
		(1)	(2)
Directors:	Board size	12.608	1.353+
		[3.705]	(0.701)
	Seats of trust company directors	3.696	2.740**
		[2.935]	(0.503)
	Number of trust co's on board	4.752	3.931**
		[4.039]	(0.645)
	Seats of major NY commercial banks	1.504	0.944**
		[1.639]	(0.327)
	Seats of insiders	2.967	-0.656*
		[1.669]	(0.273)
Disclosure:	Balance sheet lines	20.544	3.813**
		[8.204]	(1.308)
Life cycle:	Firm age (years)	19.384	5.892+
		[19.305]	(3.0309)
	Retained earnings / total common equity	0.149	-0.005
		[0.123]	(0.024)
Industry:	SIC 1 (Mining)	0.048	-0.0297
		[0.215]	(0.0382)
	SIC 2 (Light mfg)	0.128	-0.122*
		[0.335]	(0.059)
	SIC 3 (Heavy mfg)	0.248	-0.100
		[0.434]	(0.077)
	SIC 4 (Transportation)	0.560	0.251**
		[0.498]	(0.087)
	SIC 5 (Distribution)	0.016	0.001
		[0.126]	(0.023)

Notes: Standard deviations in brackets and standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. Column (1) reports means and standard deviations for 1907. Column (2) reports 1907 differences in means for firms with affiliations with affected trust companies relative to those without, from a regression with industry fixed effects (except for those where the industry is the dependent variable), and presents robust standard errors in parentheses. For firm age and retained earnings/total common equity 1906 values are used.

Table 3
Summary Statistics, Financial Variables

	Mean [Std Dev] 1906	Difference: Firms with Affected trust Interlock, 1906	Differential trend: Firms with affected Trust interlock, 1903-1906
	(1)	(2)	(3)
Log(Assets)	18.121	0.418*	0.007
	[1.125]	(0.194)	(0.013)
Cash/Assets	0.028	-0.008+	-0.002
	[0.030]	(0.005)	(0.002)
Leverage Ratio	0.294	0.075**	0.009
	[0.204]	(0.025)	(0.006)
ROA	0.038	-0.005	-0.0007
	[0.034]	(0.005)	(0.0014)
ROE	0.084	0.010	0.002
	[0.056]	(0.011)	(0.003)
Dividend Rate	0.032	0.003	-0.0002
	[0.040]	(0.008)	(0.002)
Interest Rate	0.048	-0.001	-0.0013
	[0.010]	(0.002)	(0.0009)
Investment Rate	0.036	0.015+	-0.00009
	[0.041]	(0.009)	(0.005)

Notes: Standard deviations in brackets and standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. Column (1) reports means and standard deviations for 1906. Column (2) reports 1906 differences in means for firms with affiliations with affected trust companies relative to those without, from a regression with industry fixed effects, and presents robust standard errors in parentheses. Column (3) presents the differential trends estimated from regressions with year fixed effects and firm fixed effects for 1903-1906, and presents standard errors adjusted for clustering by firm.

Table 4
Stock Returns at the Onset of the Panic

A. One-week Industry-adjusted Cumulative Returns (%), by connections and size

		All (n=85)	Small (n=42)	Large (n=43)
Connected to Affected Trusts	Mean	-0.029	-0.088	0.014
		[0.133]	[0.165]	[0.086]
Not connected to Affected Trusts	Mean	0.023	0.018	0.030
		[0.121]	[0.135]	[0.103]
Not connected versus connected	Difference	0.051+	0.106*	0.015
		(0.028)	(0.046)	(0.029)

B. OLS Regressions on Cumulative Returns (%)

	Cumulative returns	Cumulative industry-adjusted returns			
Event date:	October 25th, 1907				September 13th, 1907
Event window:	[-1,1] (Mean -.095, SD .130)	[-1,1] (Mean -.003, SD .129)		[-2,2] (Mean -.028, SD .127)	[-1,1] (Mean .009, SD .080)
	(1)	(2)	(3)	(4)	(5)
Affected trust	-1.046*	-1.009*	-1.009*	-1.501**	0.293
	(0.435)	(0.450)	(0.485)	(0.463)	(0.368)
Affected trust × log $assets_{06}$	0.0534*	0.0512*	0.0512*	0.0787**	-0.0157
	(0.0229)	(0.0237)	(0.0254)	(0.0243)	(0.0195)
Log $assets_{06}$	-0.0225	-0.0202	-0.0160	-0.0339+	-0.00166
	(0.0198)	(0.0204)	(0.0239)	(0.0192)	(0.0167)
Railroad	0.0809*	0.0661+	0.0544	0.0335	-0.0127
	(0.0351)	(0.0353)	(0.0417)	(0.0335)	(0.0214)
Number of trusts on $board_{06}$			-0.000263		
			(0.00696)		
Seats of major $banks_{06}$			-0.00558		
			(0.0130)		
$Leverage_{06}$			0.0380		
			(0.0724)		
$Cash/assets_{06}$			-0.0371		
			(0.534)		
Constant	0.296	0.357	0.287	0.593+	0.0470
	(0.357)	(0.367)	(0.430)	(0.346)	(0.304)
Observations	85	85	85	84	81
R-squared	0.175	0.153	0.160	0.151	0.042

Notes: Standard deviations in brackets and robust standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. Returns adjusted by value-weighted industry returns on each date. Returns are cumulated over a window of one week around October 25th, 1907 in Panel A and columns (1)-(3) in Panel B. Column (4) cumulates returns over two weeks around this date, while column (5) cumulates returns over 1 week around September 13th, 1907. Small (large) firms are defined as below (above) median log assets in 1906 for the 78 firms with non-missing cumulative returns.

Table 5
Time-Averaged Effects of Connections to Affected Trusts in Post-Panic Years

	ROA (Mean, .04; SD, 0.03)	ROE (Mean, .08; SD, 0.06)	Dividend Rate (Mean, .03; SD, 0.04)	Average Interest Rate (Mean, .05; SD, 0.01)	Investment Rate (Mean, .04; SD, 0.03)
A. Industry-Specific Trends					
Affected trust × post-panic	-0.0525*	-0.0985*	-0.0787*	0.0299*	-0.109*
	(0.0219)	(0.0478)	(0.0329)	(0.0141)	(0.0533)
Affected trust × post-panic × log assets$_{06}$	0.00278*	0.00501+	0.00416*	-0.00148+	0.00525+
	(0.00117)	(0.00256)	(0.00180)	(0.000767)	(0.00278)
Implied effects:					
25th pctile of assets	-0.004	-0.011*	-0.007+	0.004*	-0.018*
median assets	-0.002	-0.008+	-0.004	0.003*	-0.014*
75th pctile of assets	0.00006	-0.004	-0.0002	0.002	-0.010+
Observations	1,066	1,051	1,068	738	879
R-squared	0.870	0.798	0.885	0.724	0.336
B. Controls of A + 1906 Values of Leverage, Cash/Assets, Board Seats of Major Comm Banks, and Number of Trust Companies on Board interacted with Trends					
Affected trust × post-panic	-0.0603*	-0.119*	-0.0701*	0.0351*	-0.124*
	(0.0232)	(0.0509)	(0.0350)	(0.0157)	(0.0558)
Affected trust × post-panic × log assets$_{06}$	0.00326**	0.00620*	0.00381*	-0.00181*	0.00617*
	(0.00124)	(0.00273)	(0.00192)	(0.000864)	(0.00294)
Observations	1,066	1,051	1,068	738	879
R-squared	0.872	0.799	0.887	0.728	0.342
C. Controls of B + Sample Restricted to Common Support in Propensity to have an Affiliation With an Affected Trust Company and Weighted by Inverse Propensity Scores					
Affected trust × post-panic	-0.0625**	-0.106+	-0.0609+	0.0309+	-0.0932+
	(0.0238)	(0.0548)	(0.0335)	(0.0162)	(0.0557)
Affected trust × post-panic × log assets$_{06}$	0.00340**	0.00559+	0.00346+	-0.00167+	0.00431
	(0.00129)	(0.00300)	(0.00189)	(0.000908)	(0.00290)
Observations	958	944	971	710	786
R-squared	0.836	0.791	0.891	0.789	0.342
All Specifications:					
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 6
Annual Post-Panic Effects of Connections To Affected Trusts: Investment

	1907	1908	1909	1910	1911	1912
A. Annual Estimates						
Affected trust	-0.0756	-0.0961	-0.210*	-0.180+	-0.0714	-0.0778
	(0.0720)	(0.0801)	(0.0951)	(0.0959)	(0.0926)	(0.0974)
Affected trust × log $assets_{06}$	0.00381	0.00435	0.0111*	0.00919+	0.00311	0.00342
	(0.00366)	(0.00430)	(0.00512)	(0.00520)	(0.00494)	(0.00522)
Implied effects:						
25^{th} pctile of assets:	-0.0096	-0.0208*	-0.0185*	-0.0209*	-0.0175	-0.0187
50^{th} pctile of assets:	-0.0069	-0.0176*	-0.0105	-0.0143+	-0.0153+	-0.0162+
75^{th} pctile of assets:	-0.0037	-0.0140+	-0.0012	-0.0066	-0.0127	-0.0133
B. Aggregate Impacts						
Change in investment due to affiliations w/ affected trusts (mill.)		-128.5	-74.8	-91.2	-51.0	-49.6

Notes: Standard errors adjusted for clustering by firm in parentheses. ** p<0.01, * p<0.05, + p<0.1. The regression in Panel A includes the same firm characteristics interacted with trends as those of Panel B in Table 5. The bottom row presents the total estimated decline in investment due to affiliations with affected trusts, as estimated using the same procedure as the corresponding number for 1908 in column 4 of Table 7.

Table 7
Change in Investment, 1908

	Sample Firms		Additional Affiliated Firms	All nonfinan. Corporations in U.S.
	Small & medium	Large		
	(1)	(2)	(3)	(4)
Actual change in investment, 1908 (mill.)	-80.9	-221.3	--	-700.0
Estimated change in investment due to affiliations w/ affected trusts (mill.)	-39.8	-7.9	-80.8	-128.5
Percent of change in investment explained by affiliations w/ affected trusts	49.2%	3.6%	--	18.4%

Notes: Small and medium sample firms are defined as those at or below the 75^{th} percentile of assets; large firms are those above the 75^{th} percentile. The additional affiliated firms include all out-of-sample firms with affiliations with the worst-affected trusts for which capitalization data could be found. No investment data is available for those firms.

Table 8
Assessing the Channels by which the Panic was Transmitted:
Effects on Connections to Affected Commercial Banks

	ROA	ROE	Dividend Rate	Average Interest Rate	Investment Rate
Commercial bank × post-panic	-0.0274	-0.0312	-0.0548	-0.00894	0.00578
	(0.0222)	(0.0437)	(0.0479)	(0.0170)	(0.0462)
Comm. bank × post-panic × $logassets_{06}$	0.00157	0.00192	0.00304	0.000577	-0.000306
	(0.00119)	(0.00237)	(0.00260)	(0.000902)	(0.00248)
Observations	1,066	1,051	1,068	738	879
R-Squared	0.870	0.797	0.884	0.721	0.330
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. ** p<0.01, * p<0.05, + p<0.1. The equations include the same controls as those of Panel A of Table 5.

Table 9
Assessing the effect of unobservable characteristics:
The 1903-04 Recession

	ROA	ROE	Dividend Rate	Average Interest Rate	Investment Rate
Aff. trust in '07 × 1903-04	0.0188	0.0525	0.0492*	-0.0384	-0.0526
	(0.0423)	(0.116)	(0.0245)	(0.0274)	(0.0830)
Aff. trust in '07 × 1903-04 × $logassets_{04}$	-0.00110	-0.00317	-0.00280*	0.00214	0.00279
	(0.00224)	(0.00608)	(0.00127)	(0.00148)	(0.00441)
Observations	373	371	370	246	253
R-Squared	0.918	0.860	0.933	0.884	0.527
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Appendix: For Online Publication Only

A1. Data Appendix

Total Lending: New York City National Banks and Trust Companies

Quarterly data for national banks located in New York City were collected from *Annual Report of the Comptroller of the Currency*, various years. The amount reported as total lending in Figure 1 is "Loans and Discounts" plus "Overdrafts." Quarterly data for all trust companies located within New York City were obtained from the *Annual Report of the Superintendent of Banks Relative to Savings Banks, Trust Companies, Safe Deposit Companies, and Miscellaneous Corporations*, various years. The amount reported as total lending for these institutions is "Amount Loaned on Collaterals" plus "Amount of Other Loans Including Bills Purchased" plus "Overdrafts." The exact dates for the data points in each series are the call dates chosen by the national regulators (for national banks) and state regulators (for trust companies). Although the dates are not exactly the same, they generally fall within the same quarter.

Trust Companies: Connections to Speculators

We designate the men at the center of the copper speculation as Augustus Heinze, Arthur P. Heinze, Charles W. Morse, Charles T. Barney, and Orlando F. Thomas. The relationships between these men and various banking institutions in New York are presented below, in figure A1.

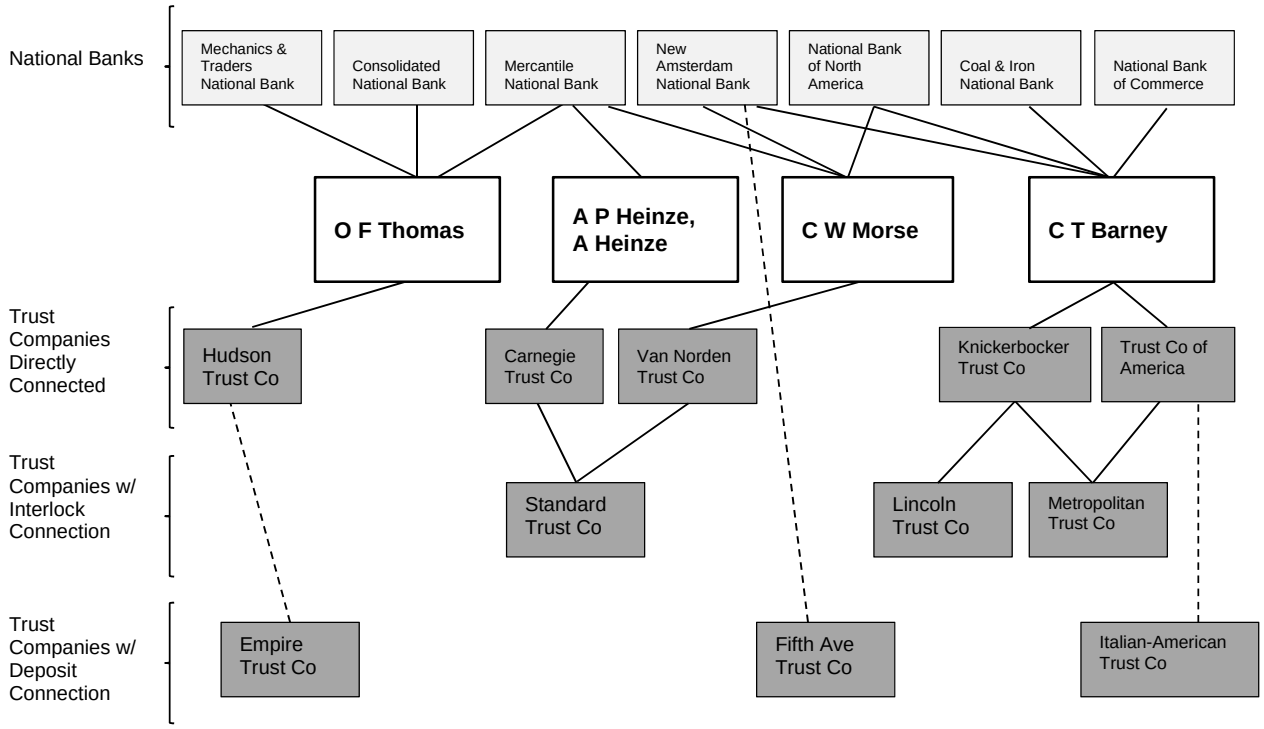

Figure A1: Connections Between Speculators and Financial Institutions
In the figure, solid lines denote board seats; the dashed lines represent deposit relationships.

The solid lines in the figure denote board interlocks; the dashed lines denote deposit connections. The Appendix subsection *"Board Data, Director Names & Matching Procedure"* describes in detail how we determine these connections. As discussed in the paper, the men involved in the copper scandal had board seats in five New York Trust Companies, which we define as "direct connections." Three other trust companies were indirectly connected to these men, in the sense that

they shared at least two directors in common with any of the five trust companies directly connected to the scandal—we label these as "interlock connections."

The figure also identifies seven national banks that had connections to the men at the center of the scandal; three of these, Mercantile National, National Bank of North America, and New Amsterdam National, faced runs and received aid from the NYCHA. In addition to those seven national banks, the men depicted in the figure held board seats on one additional national bank (Garfield National) and three state banks (Fourteenth Street, New York Produce Exchange, and the Bank for Savings).

The figure only presents deposit connections between institutions that had no direct or indirect connection through board interlocks, and only to the national banks and trust companies that were directly connected to the men at the center of the panic. Some of the trust companies in the figure with a direct connection to the men in the scandal also maintained deposit connections with these seven national banks.

In addition to the five men depicted in the figure, several additional men were connected to the speculation, and their presence on bank boards may also have contributed to depositor fears. These included brokers Otto Heinze, P Heinze, and Max H Schultze; Orlando's brother E.R. Thomas, a broker and banker; and E.R. Thomas' partners Robert Maclay and Harold H Weeks. Many of these men had connections to financial institutions, although their connections mostly duplicated those presented in the figure. We focus on the narrowly defined group of five men as those responsible for the scandal because in contemporary accounts they seem most strongly identified both with the scandal and with the institutions where they held board seats.

The boards of trust companies included many prominent names. For example, Treasury Secretary Leslie M. Shaw resigned from office in March of 1907 and became the President of the relatively new Carnegie Trust Company; former Secretary Lyman J. Gage was also a director of a trust company; former Vice President of the United States Levi P. Morton was president of Morton Trust Company. Partners of investment banks such as Kuhn Loeb & Company, and J.P. Morgan & Company, sat on the boards of several New York trust companies, as did senior executives of U.S. Steel and General Electric. The names of trust company directors were advertised widely, and were therefore known by investors and depositors.

Trust Companies – Deposit and Balance Sheet Data and Sources

The deposit losses of each of the 38 trust companies in New York City during the panic, along with the sizes of their balance sheets prior to the panic, are presented in figure A2 below.

It is worth noting that each of the trust companies with a connection to the men involved in the copper company speculation lost a substantial fraction of their deposits during the panic, and that firm size does not appear to be strongly correlated with deposit losses.

Summary statistics for the variables used in the regression presented in Table 1 are shown in Table A1 below. Means are presented for all trust companies, and then separately for those with and without a connection to the scandal (whether direct, through interlocks, or through deposit relationships). Standard deviations are given in brackets.

Trust companies with a connection to the scandal were somewhat younger, had a lower proportion of their assets invested in stocks and bonds, had lower levels of net worth relative to their assets, and held higher cash reserves, relative to trust companies with no such connections. They were also more likely to be located uptown.

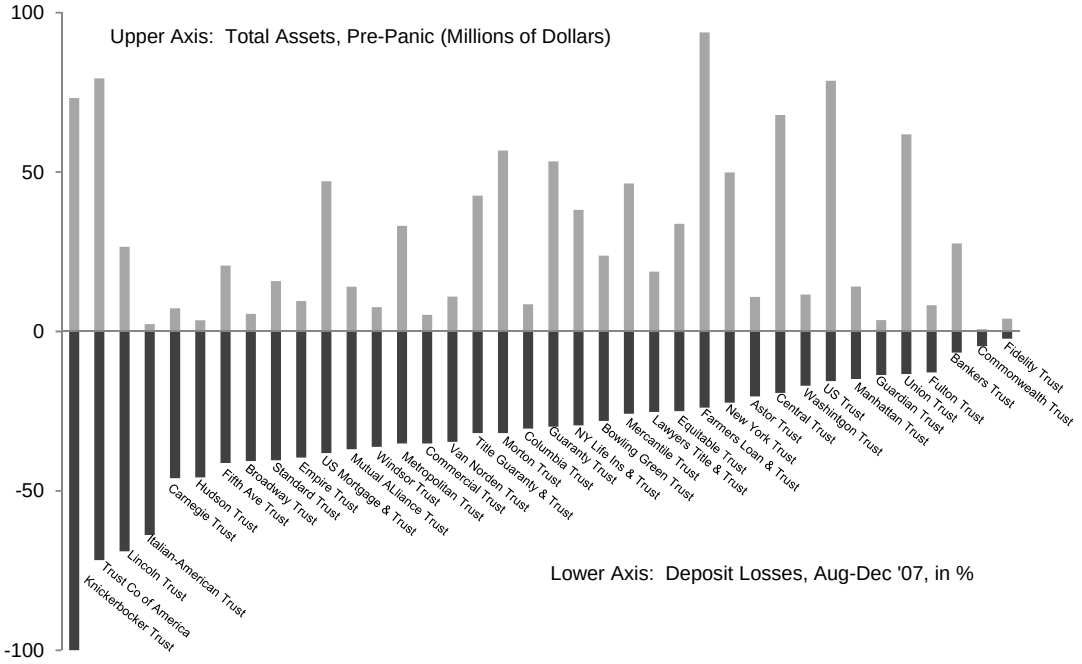

Figure A2: Assets and deposit losses at New York City trust companies

Upper axis: total assets, June 1907, in millions. Lower axis: percentage change in deposits between August 22, 1907 and December 19, 1907, in percent.

Table A1
Summary Statistics, New York Trust Companies

	All Trust Co's (n = 38)	Trust Co's w/ No Connection to Scandal (n=27)	Trust Co's w/ Connection to Scandal (n=11)
Net Worth / Assets	0.232	0.249	0.184
	[0.199]	[0.229]	[0.080]
Cash / Assets	0.043	0.041	0.045
	[0.012]	[0.009]	[0.017]
Stock and Bond Investments / Assets	0.257	0.276	0.210
	[0.257]	[0.298]	[0.102]
Log(Total Assets)	16.676	16.727	16.549
	[1.164]	[1.200]	[1.117]
Log(Firm Age)	2.333	2.500	1.926
	[1.155]	[1.176]	[1.041]
Uptown Headquarters	0.263	0.185	0.455
	[0.446]	[0.396]	[0.522]

Data on trust company balance sheets, deposits and ages was collected from quarterly reports submitted to the New York Superintendent of Banks, as reported in the *Annual Report of the Superintendent of Banks Relative to Savings Banks, Trust Companies, Safe Deposit Companies, and Miscellaneous Corporations* (1908). We obtain headquarter locations from Hansen (2011, Figure 1).

The identity of the directors of trust companies and commercial banks was obtained from the *Rand McNally Bankers' Directory* of 1907. Data on interbank deposits of firm capital was found in

Inactive Bank Files, New York State Banking Department, Series 14272-85, New York State Archives, Albany NY. These data were found for only 27 of the 38 trusts; those with missing data were assumed not to have a deposit relationship with the institutions with a direct connection to the men in the figure.

Trust Companies – Lending Data

No systematic data is available regarding the identity of lenders and underwriters to nonfinancial corporations, or of the recipients of loans from banks or trust companies, during our period. However, the historical literature implies that trust companies often provided a substantial part of the external financing required by nonfinancial corporations with which they had board interlocks (Dewing, 1914).

In an effort to obtain data on trust company lending, a thorough search was conducted through publicly accessible archives. This produced lending and/or securities holdings data for three trust companies around the time of the sample period. These data are somewhat fragmentary – in some cases, only part of the company's lending could be observed. Table A2 shows that trust companies often provided a large fraction of their total lending to affiliated companies. Moreover, a considerable portion of their security holdings was comprised of stocks and bonds of corporate borrowers connected through board interlocks.

Table A2
Lending by Trust Companies to Affiliated Companies

Trust Company	Date	Loans & Discounts To Affiliated Co's As % of Total	Securities of Affiliated Co's Owned as % of Total
Trust Company of the Republic	March, 1903	58.16	--
Knickerbocker Trust	Nov., 1907	--	41.6
Farmers Loan & Trust	April, 1916	4.4	15.3
Farmers Loan & Trust	April, 1918	3.5	11.4
Farmers Loan & Trust	October, 1919	9.8	4.9

Data sources: Inactive Bank Files, New York State Banking Department, Series 14272-85, Box 15, Folder 213, New York State Archives, Albany NY; "Appraisers Memorandum: Schedule of Investment Securities Owned by Knickerbocker Trust Company, New York City," November 4, 1907. Box 183, Knickerbocker Trust Folder, Marvin Scudder Collection, Columbia University; Report of Examination Records, Farmer's Loan and Trust, Series 07, Vault Item 754 00, Shelf 4A, Citigroup Archives.

Stock Price Data

To calculate weekly returns at the onset of the Panic of 1907, we collect stock price and dividend data for the 18 weeks from the last week in August to the last week in December of 1907. For each firm in our sample, we search for the Friday closing price of common shares in *The New York Times* Stock Quote tables published in Saturday's newspaper. Although most of the information corresponds to prices on the NYSE, we expand the sample by collecting transaction prices from the

Boston, Chicago, Philadelphia, and Pittsburg Stock Exchanges. Since the market was fairly illiquid at that time, we also search for Saturday's closing price in the Stock Quote table published in Sunday's newspaper. We obtain a total of 1,170 non-missing Friday prices, and we increase the number of observations to 1,523 by filling in missing Friday data with Saturday's prices. Since this procedure does not alter our results but increases the sample size, we use the filled in data.

To calculate returns, we adjust stock prices by the dividend payout when shares go ex-dividend. We obtain information on announced dividend payouts (both for regular and extra cash dividends) and ex-dividend dates from the Declared Dividends table published each Sunday in *The New York Times*. Because the dividend payouts were mostly quoted as a percentage of the book value of shares, we use the par value of common shares, collected from the *Moody's Manuals* for the year 1907, to determine the dividend dollar amount.[32] We also obtain from Moody's the number of shares outstanding for each company, calculated as the book value of common shares dividend by their par value, which allows calculating value-weighted returns. We are more likely to observe stock returns for large firms, but no other observable firm characteristic is a strong predictor of availability of stock price information.

Correctly measured returns should also adjust stock prices in the event of a stock split. Identifying stock splits during the last four months of 1907 is difficult within our data because we only obtain the number of shares outstanding on an annual basis. To gauge whether ignoring stock splits introduces a large bias in the returns, we collect information on the number of shares outstanding for the weeks ended on September 7th and December 28th from week-end summary Stock Quotes tables published in *The New York Times* on the following Mondays. The tables report the value of the capital stock outstanding and the par value of shares, from which we calculate the number of common shares for the 56 firms included on both dates. Of these firms, 86 percent had no change in the absolute number of shares. For the other firms, the change was never higher than 0.74 percent of the shares outstanding in September. Thus, there is no evidence that the firms in our sample experienced a stock split during the period under study.

The industry-adjusted return on firm *i* from week *t*-1 to week *t* is computed as:

$$R_{i,t}^{adj} = R_{i,t} - \sum_{j=1}^{J} w_{j,t-1} R_{j,t}$$

where $R_{i,t}$ is the total return on firm *i* from week *t*-1 to *t* calculated as $R_{i,t} = [(P_{i,t} + d_{i,t}) - P_{i,t-1}]/P_{i,t-1}$, the *J* firms in the same industry classification as firm *i* are used to calculate the industry return, and $w_{j,t-1}$ is the market capitalization weight of firm *j* within the industry portfolio at the end of the previous period. Given the small sample size of our data, we can only use one-digit SIC to obtain enough observations within each industry. We restrict the empirical analysis on SIC 2 to SIC 4 because we only have price data for at most two firms in other industries. To calculate the industry return, we use all firms in the industry in a given week, regardless of whether the same firm also has available returns in surrounding weeks. Using an equal-weighted industry index rather than a value-weighted measure does not affect our findings.

Cumulative returns for a window of weeks [-*k*, *k*] centered on the onset of the panic are calculated as:

$$Industry - Adjusted\ Cum.\ Return_{\,i} = \sum_{t=-k}^{t=k} R_{i,t}^{adj}$$

We also present results with unadjusted returns, which are simply based on the cross-sectional variation in returns. This is equivalent to using to a market-adjusted-return model, which assumes that α=0 and β=1 for every share (Campbell, Lo and MacKinlay, 1997). Because the event date is

[32] We verify the par values using the weekly summary Stock Quotes tables published in *The New York Times*.

the same for all firms in the sample, subtracting the overall market return from the weekly individual stock return for all firms would only affect the constant term of the regression.

Our strategy diverges from the standard event study methodology because we cannot calculate abnormal returns as the difference between the actual return and the return predicted from a market model. Estimating such a model would require collecting weekly price data and dividend information from primary sources for about four years prior to the panic, an exceedingly large data collection effort. Our strategy is therefore close to the market-adjusted-return model. The lack of information on stock returns prior to the panic also limits our ability to adjust the standard errors using the pre-period variance in returns.

Another limitation of historical stock market data is that securities markets were fairly illiquid. Some companies rarely traded during the four months in our sample, and are therefore not part of our analysis. Others traded frequently, but may not have had a transaction at the end of every week. To be able to cumulate returns when some returns are missing within the event window, we assume no price changes in weeks for which prices are missing, under the restriction that at least one non-missing return was observed during the event window [$-k$, k]. For example, for the week centered on October 25, we observe returns for all three Fridays in the event window for 77 firms. For another 14 firms we observe either one or two returns. We assume that the missing percentage change in price was zero in these 14 cases. However, we restrict the data further to the firms for which there is at least one non-missing return at either end of the event window, which drops four firms from the analysis.[33] Restricting the sample to industries with cumulative returns for more than nine firms eliminates another two firms. In this manner, our final sample contains a total of 85 firms with cumulative returns for the period [-1, 1]. Results are overall robust to using only observations with non-missing returns in any week within the window.

Accounting Data – Nonfinancial Companies

All accounting data were collected from *Moody's Manuals of Railroads and Corporation Securities.* Financial statements varied considerably across firms and over time. The accounting data are quite noisy, and in order to eliminate the potential for outliers to exert a significant influence on the estimation, all variables are trimmed at the top and bottom one percent.

Unfortunately, the quality of financial reports varied considerably across firms. Beginning around 1905, the financial statements of railroads were of relatively high quality and reasonably consistent across firms.[34] The industrials, however, were altogether a different matter. Although the NYSE required listed firms to publish financial statements, it did not specify the contents of the required statements, and many firms took great license in their interpretation of the requirement. The contents of balance sheets varied considerably, and relatively few industrial firms presented much detail in their income statements.[35] We therefore focus the analysis of firms' financial policies on profitability ratios and dividend rates, which can be calculated for nearly all NYSE firms, and average interest rates on bonded debt, for which sufficient data is available for a smaller but still substantial number of firms.

One important aspect in which companies' statements differed was in the dates of their fiscal year end. For example, among the industrial firms in 1907, 25 percent had a fiscal year end of June, 35 percent had a fiscal year end of December, and the remaining firms were roughly evenly divided among the other months of the year. We designated the accounting data for a fiscal year as being for

[33] To be precise, consider the case of cumulative returns over [-4, 4]. If we observed no non-missing return from week 3 onwards, for example, we would not include this firm in the sample.
[34] The 1906 Hepburn Act required railroads to submit detailed financial reports to the Interstate Commerce Commission; the initial 1906 reports presented data for 1905.
[35] Sivakumar and Waymire (1993), and Barton and Waymire (2004) analyze the content of early financial statements.

that same year in our dataset if the fiscal year end was in July or later. If the fiscal year ended in June or earlier, we designated it as being from the previous year.

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

Return on Assets (ROA): net income/total assets. The definition of net income varied somewhat across firms; most railroads reported no depreciation expense and may not have recognized depreciation in their accounting.

Return on Equity (ROE): net income/common shareholders' equity, where shareholders' equity includes the book value of the common shares as well as the firm's 'surplus' (retained earnings). In some cases common and preferred shares were reported as a single item on the balance sheet, and the exact amounts of each had to be computed from disclosures of the number of shares outstanding and the par value of the shares. This is not available for a few firms for which ROA is available (accounting for the difference in observations in regressions for the two variables), because they did not disclose their surplus separately from other accounts.

Dividend rate: dividends paid out on common stock/book value of common stock. In some cases only total dividends (common plus preferred) were disclosed and the amount of common dividends had to be calculated based on separate disclosures of the dividend rate and of the number of shares outstanding and the par value of the shares.

Interest rate: total interest payments/bonded debt. This variable is not available for many company-years, first, because many companies do not report debt in their balance sheet; second, because some companies do not disclose interest expense as a separate item in their income statement (instead including it with many other expenses); and third, in a very small number of cases firms in default show low "interest rates" by our measure as they fail to pay some or all of the interest they owe, and we delete those firm-years from the sample.

Investment rate: the accounting statements produced in the early twentieth century generally do not disclose capital expenditures. We infer investment as (change in property account + depreciation)/lagged assets. All of the railroads treated improvements in their track and equipment as cash expenses and did not account for depreciation. Many of the industrial firms did not disclose depreciation for some or all years; we include an indicator variable for whether or not a firm disclosed depreciation in all regressions with this variable. The investment variable is not available for many firm-years, for the following reasons: some of the firms are holding companies that hold only the securities of subsidiaries, rather than fixed physical assets, on their balance sheets; some firms radically change their property accounts over time, for example by adding their subsidiaries' property to their own, in which case we eliminate those firms' property accounts from the sample; and the fact that we use lagged assets as a denominator means we cannot calculate investment for the first year for each company.

Leverage ratio: long-term debt/total assets. For most firms, long-term debt was simply 'bonded debt.' With regard to long-term debt, many firms reported liabilities of a somewhat ambiguous character; these included obligations to parent or subsidiary companies. In addition, some firms disclosed liabilities that could have represented long-term debt, such as loans, together with current liabilities, such as accounts payable, as a single balance sheet item.

Cash to assets ratio: cash/total assets. Although many firms reported securities on their balance sheet, few of these seemed to constitute 'cash equivalents' and instead were likely securities held for the purposes of controlling other enterprises. The variable therefore includes only

cash in its numerator.

Firm age: current year minus the year of the oldest date of incorporation for the firm found in *Moody's*.

Industry codes: obtained from Chandler's (1990) designations of his sample of firms. For those firms not included in Chandler's sample, SIC codes were assigned based on the descriptions of their operations in *Moody's*.

Board Data, Director Names & Matching Procedure

We obtain information on the names of officers and directors of all railroads and industrial firms from the *Moody's Manual of Railroads and Corporation Securities* of 1907. We start with a sample of all NYSE-listed firms in that year, a total of 115 industrials and 66 railroads. When we restrict the sample to the 125 firms for which we have accounting data in 1906, the dataset contains a total of 2,236 observations, of which 70.5 percent are directors, and the balance are officers and other executives. Thus, our final sample has 783 names of directors of industrial firms and 793 names of directors of railroads. To be able to identify bankers, we obtain the names of 4,266 directors of 274 commercial banks and trust companies in the three major financial centers, New York, Boston, and Chicago, from the *Rand McNally Bankers' Directory* of 1907.

We match on names across these samples to identify connections between nonfinancial firms and banks, as well as the connections amongst these two types of institutions. We follow a thorough procedure to clean the collected names and ensure the accuracy of the matches. First, we ensure that matching is not hampered by transcription errors or inconsistencies in the source material. Since the management of financial and nonfinancial firms was relatively stable over time, management data collected from the 1905 and 1909 *Moody's Manuals* and *McNally's Directories* aided the cleaning process.[36] We start by verifying the transcription of a given full name (defined by first and middle name, last name, and suffix) by finding its presence in the same company in the years surrounding 1907. For names that do not match perfectly, we use an algorithm to find approximate matches in names in surrounding years. This procedure identifies cases in which only one letter of the entire name differs across sources. In this manner, we are able to identify transcription errors and inconsistencies in the source materials. Finally, unusual first and middle names, defined as those not found in the top 500 names of males born in the United States in 1880 as reported by the Social Security Administration, as well as unique names that were not found in the adjacent years, are also re-checked against the source materials for 1907 in order to identify and correct transcription errors.

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

For the purpose of determining interlocks between the boards of financial and nonfinancial firms, we would ideally use each director's full name. However, the Moody's Manuals report only initials for

[36] To provide a longer run view of the connections between financial and nonfinancial firms, as in Figure 3 in the paper, we collect similar management data in 1911 and 1913. We use exactly the same procedure described in this section to clean and accurately match the names of directors across companies and years.

first and middle name for 37.2 percent of the recorded directors. Thus, we are constrained to matching on names using only first initial, middle initial, last name, and suffix. This data restriction is problematic since it will lead to overestimating interlocks across boards whenever two individuals who share a last name have different first *and* middle names with the same initials. To address this potential source of overmatching, we use data on names across firms, banks, and years to identify cases where two or more individuals share the same first initial, middle initial, last name, and suffix, but where there is variation in their full names for at least one of all their observations. We then use information from *Directories of Directors*, newspaper articles, annual reports, biographies, and other sources to provide first and middle names for these cases. Using this information we develop a new full name variable, *fullname_d*, which separately identifies individuals that would otherwise collapse to one person if we were to use the uncorrected first and middle initials. For example, Walter H Taylor from Norfolk & Western, William H Taylor from Bowling Green Trust Company and W H Taylor from American Writing Paper would all be incorrectly identified as the same person using only first and middle name initials. The use of additional sources allows determining whether W H Taylor was Walter, William, or a third person altogether (he was William). As we discuss below, this corrected name variable consistently identifies individuals across samples and years.

Interlocks between nonfinancial firms and commercial banks, as well as across firms within each of these samples, are found by identifying exact matches in the variable *fullname_d*. Out of 1,576 directors of the 125 nonfinancial firms with complete accounting data, 648 were found also to be directors of commercial banks. Since a person can sit on several boards, these directors form 1,341 interlocks between nonfinancial firms and commercial banks. On average, each nonfinancial firm was connected through directors to 10.7 commercial banks and trust companies, and to 9.4 New York City commercial banks and trust companies in 1907.

Even after carefully cleaning the data, it is possible that our sample could suffer from some degree of overmatching. For example, two individuals may have shared the same full name. A potentially more common problem is that we may only observe initials for first and middle name across all samples and years in our date. Conditional on the same last name and suffix, we would not be able to identify these cases as potentially problematic, in the way described for the example for W H Taylor above. Algorithms based on the "sound" of names that are commonly used to determine the likelihood of a correct match when researchers match on names would not be of use in our case. Instead, we assess the reliability of our matching procedure by using an external source. During the first few decades of the twentieth century, it is possible to obtain *Directories of Directors* for various cities. These directories are compendia of businessmen and their board affiliations. For example, the New York City directory claims to contain "a complete alphabetical list of Directors or Trustees having New York City addresses, followed by the names of Companies with which each is connected." The publishers and coverage do change somewhat across cities and over time, but the presence of most of the individuals in our sample in these volumes suggests that their reporting is quite reliable, at least for prominent individuals.

To give a sense of the validity of our procedure, we look for external verification using the entire sample of 181 railroad and industrial firms, which includes those for which no accounting data is available. Using our variable *fullname_d*, we match the 2,119 directors of these nonfinancial firms to the 4,264 directors of commercial banks and trust companies in 1907. This results in 381 individuals creating an interlock between at least one nonfinancial and one financial firm. Of these, we were not able to find 6.6 percent of the observations in the *Directory of Directors* volumes. For the 353 directors that we were able to locate in those volumes, there were only three false matches, all due to different individuals with identical names. That is, for only 0.85 percent of the interlocks, our matching mechanism incorrectly identified different individuals as the same person.[37] For the 350 individuals correctly identified as creating an interlock, on average the *Directories of Directors* list

[37] Of course, we correct the *fullname_d* variable for these three individuals based on this additional information. Thus, the data we use to determine connections between financial and nonfinancial firms in the paper is highly reliable.

them on 88 percent of all the boards that are identified in our data. Inspection of the few cases that did match suggests that this was mostly due to the difference in the timing and coverage between the *Moody's Manuals* and the *Directories of Directors*. Thus, we conclude that our matching procedure is highly accurate.

Trust Company Board Interlocks with Firms Not Included in the Sample

In order to identify non-NYSE listed corporations that were affiliated with the affected New York trust companies, the names of every director of each of those trust companies was checked in the *Directory of Directors of the City of New York*, and all of the names of the nonfinancial corporations in which they held directorships were recorded. (Partnerships and financial corporations were excluded.) This produced a list of 540 companies that were not included in the sample. Of those 540 companies, 274 reported at least the book value of their equity in *Moody's.*

The 274 firms were substantially smaller, on average, than the firms in the original sample. Their reported book value of equity was on average $9.29 million, whereas the mean value for the sample firms was $71.5 million.

Estimating Total Assets for Additional Firms (Tables 6 and 7)

We use the relationship between log equity and log assets among the sample firms to construct estimates of log assets for the additional affiliated firms, for which we do not observe assets, and we only observe 1907 equity. We first estimate the relationship between log assets and 1907 log equity for every year beginning in 1907 among the sample firms. For example, for 1908, the estimated relationship is: $\widehat{logassets}_{08} = -0.492 + 1.062 logequity_{07}$, with an R-squared of 0.86. The log level of shareholders' equity in 1907 predicts log total assets in subsequent years relatively well; the R-squared of the relationship was never less than 0.82. We then used these parameters to construct estimates of the value of log assets for the additional firms for each year. With these estimated values of log assets, which we exponentiated to obtain the value of assets ($\widehat{Assets}_{(t)i}$), we used the same approach as for the within-sample firms to calculate the total estimated change in investment resulting from affiliations with affected trusts, namely:

$$\sum_i (\hat{\theta}_{1t} + \hat{\theta}_{2t} \widehat{logassets}_{07i}) \widehat{Assets}_{(t)i}.$$

Note we use estimated 1907 assets rather than 1906 assets in the interaction term, because it is the most precise estimate available; the results do not change substantially if estimated 1906 assets are used instead. Finally, as with the calculations for the total effects on firms within the sample, we aggregate over all firms affiliated with affected trusts, but we include only those firms at points in the size distribution where $\hat{\theta}_{1t} + \hat{\theta}_{2t} \widehat{logassets}_{07i}$ is statistically distinguishable from zero.

Investment Data

The aggregate data presented in Table 6 is calculated from the BEA's *Fixed Asset Account Tables*, in particular, Table 6.7, "Investment in Private Fixed Assets by Industry Group and Legal Form of Organization." We utilize line 4, nonfinancial corporations.

A2. Results Appendix

Main Specifications

In this section, we address additional sources of concern regarding our results by presenting alternative robustness checks to the main specifications in the paper. The results of these specifications are presented in Table A2.

One potential source of concern regarding our results could be that some firms were growing rapidly and perhaps unsustainably in the years prior to the panic, which led them to be particularly vulnerable when the panic began. Although our earlier results demonstrated that there was no large or statistically significant difference in the trend in log total assets between firms with and without affiliations with affected trust companies (see Table 3 above), in Panel A of Table A3 we explicitly include the average growth rate of assets in the years 1903-06 in the regression, interacted with a time trend. The results remain substantially the same.

Another potential concern is that the smaller firms in our sample may have been subject to shocks unrelated to the trust companies with which they were affiliated—for example, an exogenous decline in the demand for their products during the crisis could have reduced their profitability. In order to address this issue, we look for additional evidence that the hypothesized mechanism of a contraction in financial intermediation worsened firm performance. If the loss of an affiliated provider of financial services forced some firms to seek alternative sources of finance, firms with collateral whose value was easily verified should have been best able to weather the panic. In the early twentieth century, the firms with the best collateral, and as a result those with the highest leverage ratios, were the railroads. With their extensive land holdings, track, and rolling stock, railroads had collateralizable assets whose value was relatively easy to establish. In contrast, the assets of many industrial firms were likely to include intangibles such as patents, and the physical capital of firms in some industries such as electrical supplies was much harder to value. We therefore test whether the railroads affiliated with affected trust companies suffered less than industrials in the years following the panic, relative to other firms.

Panel B of Table A3 presents results from regressions that relate each of our outcome variables to separate interactions between the usual *Affectedtrust×post-panic* variable and indicator variables for railroads and for industrials. Consistent with the notion that more opaque firms, or firms with collateral that was more difficult to value should have suffered more following the panic, we find that industrials performed worse according to every measure of performance, whereas the railroads were barely affected.[38]

[38] Within industrials, the largest effects are generally found among retailers and mining companies. The former had few fixed assets, whereas the latter had assets regarded as risky and difficult to value.

Table A3
Further Robustness Checks

	ROA	ROE	Dividend Rate	Average Interest Rate	Investment Rate
A. Controlling for firm growth, 1903-06					
Affected trust × post-panic	-0.0423+	-0.0786+	-0.0763*	0.0379*	-0.106*
	(0.0219)	(0.0466)	(0.0336)	(0.0149)	(0.0527)
Affected trust × post-panic × log $assets_{06}$	0.00229+	0.00396	0.00402*	-0.00195*	0.00515+
	(0.00116)	(0.00248)	(0.00184)	(0.000808)	(0.00276)
B. Use of interactions with industrials, railroads					
Affected trust × post-panic × industrial	-0.00594	-0.023**	-0.00593	0.00494*	-0.0164+
	(0.00403)	(0.00732)	(0.00450)	(0.00212)	(0.00831)
Affected trust × post-panic × railroad	0.00214	0.00436	0.000780	0.00158	-0.00654
	(0.00234)	(0.00535)	(0.00398)	(0.00177)	(0.00566)
All Specifications:					
Firm Fixed Effects	YES	YES	YES	YES	YES
Industry Fixed Effects	YES	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES	YES

Affiliations with Commercial Banks

The men involved in the United Copper speculation held board seats with eight national banks and three state banks (see Figure A2 and the related discussion above.) One issue suggested by the discussion in the text is that many of the companies with interlocks with those commercial banks also had interlocks with affected trust companies. To further isolate the effect of an affiliation with an affected commercial bank, we control for affiliations with affected trust companies, using the same specification as reported in Table 8 in the text. The results are reported in Panel A Table A4.

For all of the outcome variables, the estimated effect of an affiliation with an affected commercial bank, controlling for any affiliation with an affected trust company, is essentially zero, although there is a mildly negative (beneficial) effect for interest rates for small firms. In contrast, when one controls for an affiliation with an affected commercial bank, the effect of an affiliation with an affected trust company remains generally unchanged, relative to estimates reported in Panel A of Table 5. We interpret these results as evidence that the pure 'reputation' effect was unlikely to have been important.

One concern regarding these results could be that the commercial banks designated as 'affected' because they had one of the men involved in the scandal on their boards included at least a few institutions that were unlikely to have been strongly associated with those men. The inclusion of these institutions among the 'treated' group may have diluted any estimated effect.

Table A4
Effect of Affiliations with Affected Commercial Banks and Trust Companies

	ROA	ROE	Dividend Rate	Average Interest Rate	Investment Rate
A. Controlling for Trust Company and Commercial Bank Affiliations					
Affected comm bank × post-panic	0.0166	0.0579	-0.00266	-0.0438+	0.0592
	(0.0330)	(0.0665)	(0.0796)	(0.0260)	(0.0753)
Affected comm bank × post-panic × lassets$_{06}$	-0.000802	-0.00281	0.000232	0.00243+	-0.00310
	(0.00178)	(0.00360)	(0.00434)	(0.00140)	(0.00406)
Affected trust × post-panic	-0.0625+	-0.135+	-0.0761	0.0559*	-0.144+
	(0.0327)	(0.0725)	(0.0620)	(0.0215)	(0.0792)
Affected trust × post-panic × log assets$_{06}$	0.00330+	0.00692+	0.00400	-0.00289*	0.00713+
	(0.00176)	(0.00391)	(0.00341)	(0.00116)	(0.00422)
B. Analyzing Affiliations with Strongly Affected Commercial Banks					
Affected comm bank × post-panic	0.0588	0.00613	-0.0469	-0.00566	0.0134
	(0.0390)	(0.106)	(0.0437)	(0.0388)	(0.447)
Affected comm bank × post-panic × lassets$_{06}$	-0.0034+	-0.000371	0.00213	0.000556	-0.000240
	(0.00206)	(0.00554)	(0.00232)	(0.00217)	(0.0241)
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES	YES

In Panel B of Table A4, we address this concern by limiting the 'treated group' of commercial banks to those that actually had Charles W. Morse himself on the board. These were among the commercial banks that actually faced runs, and were most strongly identified with the copper company speculation. (These included the National Bank of North America, Garfield National Bank, New Amsterdam National Bank, Mercantile National Bank, and two state banks.) These banks were the most strongly associated with the scandal of any institutions in New York. Again, there is no evidence of any negative effect on firm performance in the years following the panic.

1903-04 Recession

One source of concern regarding the placebo test in the 1903-04 recession could be that in the years between 1904 and 1907, some NYSE-listed firms may have experienced events that made them more vulnerable to the shock than they had been earlier. Our placebo test using the earlier recession would be invalidated if the fundamental characteristics of nonfinancial firms changed substantially in the four-year period between the two crises. Two potential sources of change are antitrust prosecutions by the federal government, and mergers and acquisitions.

Beginning with the Northern Securities case, which was initiated in 1902 and decided in 1904, the

Roosevelt Administration initiated a number of antitrust cases that may have weakened some NYSE firms. In order to identify the firms that were subject to antitrust prosecutions, we compiled a list of all federal court decisions citing the Sherman Antitrust Act issued between 1904 and 1912, and searched for all corporate defendants named in the decisions. Many of these cases were initiated against firms that were not listed on the NYSE, such as Standard Oil and its various subsidiaries. In total, 23 NYSE firms were subject to federal antitrust cases decided during those years.

Mergers or major acquisitions are a related issue. The wave of mergers that swept through the U.S. economy in the late nineteenth century was underway until around 1904, and firms may have undertaken mergers that weakened their balance sheets prior to 1907. Only 3 firms within our sample were part of mergers that resulted in an increase in their total assets of more than 30%. It is likely that firms first appeared within our sample as the outcome of a merger, so that no pre-merger data are generally available.

We address the possibility that antitrust cases and mergers made firms more vulnerable to the 1907 panic by deleting all observations from firms that were subject to an antitrust case (irrespective of the outcome) or were part of a merger and re-estimating the specifications for 1907 and 1903-04. The results are presented in Table A5.

Table A5
Firms involved in Mergers and Antitrust Cases Deleted

	ROA	ROE	Dividend Rate	Average Interest Rate	Investment Rate
Affected trust × post-panic	-0.0620*	-0.143*	-0.108*	0.0299+	-0.177*
	(0.0257)	(0.0582)	(0.0422)	(0.0159)	(0.0704)
Affected trust × post-panic × log assets$_{06}$	0.00332*	0.00752*	0.00589*	-0.00148+	0.00915*
	(0.00139)	(0.00319)	(0.00236)	(0.000871)	(0.00378)
Observations	841	827	847	585	705
R-Squared	0.870	0.784	0.880	0.746	0.321
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES	YES

Standard errors adjusted for clustering by firm in parentheses. ** p<0.01, * p<0.05, + p<0.1. Regressions also include contemporaneous log assets and a constant term.

The results clearly show that deleting these firms strengthens, rather than reduces, the estimated effects of the panic. The reasons for this are intuitive: the most profitable, successful and robust firms were likely the subject of antitrust cases, and even successful prosecutions of these firms was not enough to radically change their dominant market positions. These firms were also less sensitive to the loss of a lender.

Duration of Effects

Table 6 in the paper presents a detailed analysis of the duration of the effects of the financial contraction on firm investment. In this section we present an analysis of the duration of the effects on our other firm-level outcome variables.

Table A6
Annual Post-Panic Effects of Connections To Affected Trusts

	ROA	ROE	Dividend Rate	Average Interest Rate
Affected trust × 1907	-0.00579	-0.0247	-0.0614*	0.0148
	(0.0317)	(0.0812)	(0.0283)	(0.0130)
Affected trust × 1907 × log assets$_{06}$	0.000270	0.00124	0.00342*	-0.000796
	(0.00172)	(0.00444)	(0.00155)	(0.000696)
Affected trust × 1908	-0.0957*	-0.213*	-0.0577*	0.0305+
	(0.0402)	(0.0901)	(0.0285)	(0.0160)
Affected trust × 1908 × log assets$_{06}$	0.00520*	0.0113*	0.00318*	-0.00148+
	(0.00212)	(0.00477)	(0.00157)	(0.000855)
Affected trust × 1909	-0.0866**	-0.183**	-0.0805*	0.0507**
	(0.0319)	(0.0663)	(0.0382)	(0.0190)
Affected trust × 1909 × log assets$_{06}$	0.00468**	0.00960**	0.00444*	-0.00263*
	(0.00167)	(0.00354)	(0.00211)	(0.00104)
Affected trust × 1910	-0.0864**	-0.120	-0.0770	0.0460+
	(0.0321)	(0.0780)	(0.0518)	(0.0245)
Affected trust × 1910 × log assets$_{06}$	0.00467**	0.00622	0.00406	-0.00237+
	(0.00175)	(0.00417)	(0.00282)	(0.00138)
Affected trust × 1911	-0.0635+	-0.104	-0.0757	0.0399
	(0.0352)	(0.0794)	(0.0546)	(0.0245)
Affected trust × 1911 × log assets$_{06}$	0.00348+	0.00510	0.00377	-0.00209
	(0.00187)	(0.00430)	(0.00299)	(0.00137)
Affected trust × 1912	-0.0310	-0.00833	-0.0631	0.0510+
	(0.0410)	(0.0908)	(0.0637)	(0.0273)
Affected trust × 1912 × log assets$_{06}$	0.00173	-6.82e-05	0.00353	-0.00268+
	(0.00219)	(0.00501)	(0.00346)	(0.00154)
Observations	1,066	1,051	1,068	738
R-Squared	0.874	0.802	0.888	0.731
Firm Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Industry Trends	YES	YES	YES	YES
Firm 1906 Characteristics×Trends	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses; ** p<0.01, * p<0.05, + p<0.1. Each specification includes the same firm characteristics interacted with trends as those of Panel B in Table 5.

In general, the differential effects of the panic on firms affiliated with affected trust companies were not reflected in the data until 1908. This is actually quite reasonable; due to the variation in the end of the firms' fiscal years, the data for the year 1907 often include accounting information from before late October, 1907, when the panic began. Moreover, 1908 was the worst year of the economic contraction in the economy. The one exception is the dividend rate, which had fallen by 1907. The impact on most of the firm outcomes was largest in 1908 and 1909, and had dissipated significantly by 1911. The only firm outcome for which the estimated effect remained substantial in 1912 was the interest rate. This result is not surprising because we measure the average, not the marginal, interest rate. The higher-cost debts incurred following the panic likely remained on many of the firms' balance sheets for several years, thus driving up the average interest rates they paid.