Economic Effects of Runs on Early 'Shadow Banks': Trust Companies and the Impact of the Panic of 1907

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Abstract: We use the unique circumstances that led to the Panic of 1907 to analyze its consequences for non-financial corporations. The onset of the panic occurred following a series of scandalous revelations about the investments of prominent financiers, which triggered widespread runs on trust companies associated with those men. Using newly collected data, we find that corporations with close ties to the trust companies that faced severe runs experienced an immediate decline in their stock price, and performed worse in the years following the panic: they earned fewer profits and paid fewer dividends, and faced higher interest rates on their debt. Consistent with the notion that information asymmetries aggravated the consequences of the contraction of credit intermediation, these effects were largest for smaller firms and for industrials, whose collateral was more difficult to value than that of railroads.

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1. Introduction

One of the most enduring and important questions in Economics is how financial crises, and the resulting contractions in financial intermediation, affect the real economy. In the presence of asymmetric information, a financial contraction may leave creditworthy borrowers unable to gain access to lending (see, for example, Bernanke, 1983, and Holmstrom and Tirole, 1997), and thus contribute to the decline in output and investment that is often associated with crises. Yet clear empirical evidence of this particular channel is difficult to obtain, because financial crises influence the demand for credit, as well as the institutions and relationships facilitating its supply. In this paper, we use the unique circumstances that led to the Panic of 1907 to analyze its consequences for non-financial corporations.

The Panic of 1907 was one of the most severe financial crises in the United States prior to the Great Depression. Over the following year, real GNP declined by 11 percent, industrial production contracted by 16 percent, and the unemployment rate almost doubled (Balke and Gordon, 1986; Davis 2004; Romer, 1983). The panic originated in New York with runs on financial intermediaries known as trust companies. Similar to modern "shadow" banks, in the years prior to the crisis trust companies had grown rapidly and became important sources of lending.[1] Less regulated than commercial banks, New York's trust companies were highly levered, held low cash reserve balances, and issued uninsured liabilities. In addition, they did not have direct access to a lender of last resort because they did not belong to the private clearinghouse association that facilitated partial co-insurance of commercial banks prior to the establishment of the Federal Reserve System. As the crisis unfolded, panic among trust company depositors spread turmoil throughout the financial system, which was ultimately contained only through a halting series of privately organized rescues and suspensions.[2]

The runs that precipitated the crisis were triggered by rumors that the directors of some trust companies were involved in a failed speculation that resulted in scandalous losses, and had imperiled

[1] On the rise of trust companies, see Neal (1971) and Moen and Tallman (1992). On the modern shadow banking system, see Gorton and Metrick (2010).
[2] The Federal Reserve was created in 1913, partly in response to the vulnerability of the financial system revealed by the Panic of 1907.

their institutions' deposits. These rumors were later revealed to be unfounded, in the sense that all of New York's trust companies were solvent, and emergency loans enabled all but one of those institutions to meet the demands of their depositors.[3] Nonetheless, the self-perpetuating nature of bank runs made them difficult to stop, and many trust companies faced enormous deposit losses, in spite of well-publicized evidence that their depositors' fears were unfounded.[4] More important for our purposes than the inaccuracy of the rumors was the fact that they had no connection to any of the major non-financial corporations that were clients of New York's trust companies. The rumors, and the consequent runs, produced a financial shock that originated within the trust companies and was transmitted to their borrowers, as the trust companies were forced to contract their lending in response to deposit losses.

Using new data on the identity and performance of the non-financial corporations affiliated with New York's trust companies, this paper analyzes the consequences of the Panic of 1907 at the firm level. In the early twentieth century, the directors of banks and trust companies often held seats on their clients' boards.[5] These affiliations likely served at least two purposes: they may have facilitated access to credit, by helping to resolve information problems between lenders and their borrowers, and they may have 'certified' the firm to investors or other lenders (DeLong, 1991).[6] By collecting a comprehensive dataset of the directors of trust companies, as well as directors of a large sample of non-financial corporations, we identify ties between trust companies and their clients through board interlocks. Using this measure of connections, we investigate whether ties to the trust companies that were most severely affected by the crisis, in the sense that they lost the most deposits, had negative consequences on the performance of non-financial firms.

The paucity of extant research on the impact of the panic, and on corporate finance in

[3] Knickerbocker Trust was denied emergency assistance and was forced to close its doors. However, it was in fact solvent at the time of its closure and its depositors were permitted access to their funds in 1908 (see Hanna, 1931 for details).

[4] The classic analysis of the self-fulfilling nature of bank runs is Diamond and Dybvig (1983). Gorton (1988) presents a complementary view that connects panics in the U.S. National Banking Era to increases in depositors' perceived risk.

[5] The close affiliations between banks and industrials of that era are no longer common in the U.S., and in some respects resembled bank-firm relationships in Germany at the time. Guinnane (2002) analyzes the German system.

[6] See Diamond (1984) for a model of banks as delegated monitors, and Kroszner and Strahan (2001) for the empirical determinants of bank-firm ties among modern American firms. Bank-firm ties may also have costs for the client firms (Weinstein and Yafeh, 1998; Guner, Malmendier and Tate, 2007).

general during this period, is due largely to the lack of data on individual firms. An important contribution of this paper is to construct a firm-level panel dataset with detailed financial information on all NYSE-listed industrials and railroads for the years 1900-1912. We focus on NYSE firms because they were required by the exchange to produce financial statements. But this relatively small number of corporations was responsible for a substantial fraction of economic activity: for 1909, the first year of the federal income tax on corporations, the sample firms accounted for 21.1 percent of total corporate income in the United States.[7]

We posit two channels through which a connection to a trust company that came under acute pressure during the crisis may have had negative consequences for non-financial firms, which correspond to the two main benefits of bank-firm ties. First, if ties through the board of directors facilitated lending or underwriting, then non-financial firms may have experienced a negative shock to the supply of external financing or the provision of other financial services.[8] A second possibility is that a relationship with a troubled financial institution may have made other lenders, suppliers, or customers of the firm uneasy about the quality of the firm's own assets or operations. That is, the 'certification' role of an affiliation with a financial institution would have been turned into its opposite, as the trust company director 'stigmatized' the firm. This mechanism may have been important because of the rumored associations with a scandal in the 1907 panic.[9]

In the absence of financial frictions or asymmetries of information, neither effect would have had serious consequences, since creditworthy borrowers would have been able to immediately find new lenders. Yet in an environment with relatively little financial disclosure and many new industries and enterprises emerging, asymmetries of information were likely significant. Building new relationships with alternative financial institutions would most likely have been difficult and taken a substantial amount of time, and the consequences of an association with a scandal, however

[7] Calculated as total reported net income of sample firms ($758.974 million) divided by total taxable income of all corporations, or $3.59 billion (Table Ea731-739, *Historical Statistics of the United States*.) This measure is conservative because the reported net income for many industrials in our sample was after-tax.

[8] Theoretical work by Bernanke and Blinder (1988), Bernanke and Gertler (1989), Holmstrom and Tirole (1997), and Stein (1998) show that a financial shock to banks may affect real firm outcomes in the presence of market imperfections for banks and firms.

[9] Although it would be interesting to separately identify these two channels, it is not possible to obtain information on the volume and characteristics of lending arrangements, as well as the provision of other financial services, at the bank-firm level early in the twentieth century.

indirect, may have been severe. The effects should have been worse for corporations that were more vulnerable to information problems, such as smaller or less "established" firms, with assets whose value was difficult to ascertain for use as collateral.

Our empirical analysis proceeds in three steps. First, we establish that much of the variation in deposit losses among the New York trust companies at the center of the panic was due to their associations with a handful of men involved in a scandal. The scandal did not impact any of the trust companies directly, but instead raised fears among households that their deposits *may* have been threatened. This unique characteristic of the Panic of 1907 helps rule out the possibility of reverse causation—that concerns regarding non-financial client firms led to runs. Second, we present an event study of the stock market's reaction to the onset of the runs, and show that the non-financial companies with ties to at least one of the trust companies most severely affected—defined as those that were among the top 25 percent in deposit losses—were discounted more heavily (by about 8.1 percentage points for a firm of median size) relative to other firms. Thus, investors perceived these connections to negatively affect non-financial firms when the runs started.

Finally, we find that deposit shocks to trust companies had a negative effect on the performance of affiliated non-financial firms in the years following the panic. A difference-in-differences estimation indicates that the profitability and dividend payout rates of firms with ties to the most severely affected trusts fell significantly, while the average interest rates paid by these firms rose, relative to other firms. Consistent with the notion that information asymmetries aggravated the consequences of the contraction of credit intermediation, these effects were largest for smaller firms and for industrials, whose collateral was more difficult to value than that of railroads. The magnitude of these effects was substantial: for small firms, defined as those at the 25^{th} percentile in assets, average interest rates rose by 36 basis points (equivalent to 8 percent of the mean interest rate in 1906), and their dividend rates fell by 0.7 percentage points (equivalent to 22 percent of the mean dividend rate in 1906).

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 includes firm fixed effects, and therefore controls for time-invariant unobserved characteristics such as firm

'quality.' We also control for selection on observables using pre-panic firm characteristics interacted with time trends, and by weighting observations using inverse propensity scores. However, selection would remain a problem if the differentially affected trust companies were represented on the boards of firms most vulnerable to a shock or recession, or possessing some other latent unobservable characteristic that manifested only in response to a financial panic. Yet in a "placebo test" we find that firms with ties to trust companies in 1907 did not perform worse during an earlier crisis, the recession and financial panic of 1903-04.

Our results offer new micro-level evidence on the consequences of crises for the real economy. The very nature of financial panics produces severe problems arising from asymmetries of information, as lenders and investors eliminate their exposure to particular institutions or markets; the collapse of the asset-backed commercial paper market in 2008 is one of many recent examples. Our analysis implies that borrowers strongly identified with the institutions or markets that come under such pressure in a panic may suffer significantly and persistently following a crisis, even if liquidity is restored relatively quickly. It is possible that a disruption of trust or reputation in financial markets, which is intrinsically difficult to rebuild, may contribute to the persistence of the effects (Sapienza and Zingales, 2012).

The paper contributes to the extensive literature on the channels through which financial crises and bank failures impact the real economy. Following the work of Bernanke (1983), recent works in this literature in the context of historical crises include Calomiris and Mason (1993), Kupiec and Ramirez (2012), Ziebarth (2012) and Mladjan (2012). Building on those contributions, this paper investigates a setting in which connections between specific financial institutions and firms can be identified, and the shocks to the financial intermediaries are likely uncorrelated with those faced by the non-financial firms. Works focusing on more recent crises include Kashyap, Lamont and Stein (1994), Khwaja and Mian (2008), Amiti and Weinstein (2009), Schnabl (2011) and Paravisini, Rappoport, Schnabl and Wolfenzon (2011), which exploit firm-level data on connections to financial institutions. Our work is similar to theirs in approach, but our dataset allows us to obtain market-based assessments of the consequences of the contraction of intermediation through a stock market event study, and to analyze firms' profitability and interest costs over the

medium run. Moreover, the characteristics of the Panic of 1907 provide a unique opportunity to analyze an exogenous shock to credit intermediation arising from widespread runs on an early version of a "shadow banking" sector. Finally, our findings are also closely related to Fernando, May, and Megginson (2012), who document a negative stock market reaction to the investment banking clients of Lehman Brothers when that firm went bankrupt in 2008.

We also contribute to the growing literature on the Panic of 1907. The causes and macroeconomic context of this crisis have been the focus of a substantial body of research in the years immediately following the crisis (Sprague, 1910; Barnett, 1910) and more recently (Moen and Tallman, 1992, 2000; Odell and Weidenmier, 2004; Hansen 2011; Haines and Rhode 2011; and Rogers and Wilson, 2011). This paper extends this literature by analyzing the microeconomic impact of the crisis, and the consequences of the disruption of the financial system for the real economy.

Finally, some of our findings relate to studies of the role of trust in financial markets (Guiso, Sapienza and Zingales, 2008) and, in particular, of the effects of impaired reputations of corporate directors. This literature mostly focuses on the consequences of a negative reputational shock on directors' future careers (Agrawal, Jaffe and Karpoff, 1999; Fich and Shivdasani, 2007). In contrast, our results suggest that a firm may suffer losses when its directors are perceived to be associated with a scandal not directly connected to the firm.

2. Historical Background: Anatomy of a Panic

The events that led to the Panic of 1907 are well chronicled.[10] Nonetheless, documenting the involvement of specific individuals and institutions helps establish the origins of the shock within the financial system and explains the variation in the intensity of the shock across institutions. Here we present a brief sketch of the onset of the panic, and the historical and institutional context within which it occurred. We then turn to an econometric analysis of the determinants of the trust companies' deposit losses during the panic to establish our claims regarding the causes of the deposit runs more rigorously.

[10] Strouse (1999), Carosso (1987) and Bruner and Carr (2007) present engaging histories of the panic, while Moen and Tallman (2000) present a chronology. For early detailed accounts, see Lefevre (1908) and Bayles (1919).

The Panic of 1907 began at a time of instability within American financial markets (Sprague, 1910; Odell and Weidenmier, 2004). In addition, the structure New York's banking system had recently undergone a significant change, in the form of the rapid proliferation of trust companies, whose total assets approached those of the national banks in size.[11] Originally created to serve as fiduciaries, trust companies enjoyed broad powers, including the ability to hold corporate equity and debt, to underwrite and distribute securities, and to act as financial agents for corporations (Smith, 1928; Neal 1971).[12] Incorporated under permissive state laws, they were not subject to the regulations of the National Banking Act, and often specialized in providing financing for corporate investments and acquisitions. One observer noted that the industry's profits were "derived largely from the skill of their officers in financing important combinations and aiding in the creation of new enterprises" (Conant 1904, p. 223).[13]

Trust companies also maintained relatively low cash reserve balances. Whereas the national banks located in New York City were required to hold reserves equivalent to 25 percent of their deposits in cash, New York's trust companies faced no minimum reserve requirement until 1906, when a relatively weak one was imposed.[14] Moreover, they were not members of the New York Clearing House Association (NYCHA), the private organization that facilitated clearing and that could provide emergency lending to its members in times of crisis (Gorton, 1985). Trust companies could gain access to the NYCHA by clearing through a member bank, but only if they maintained a minimum level of cash reserves, which most found unacceptably high.[15] When the panic arose, there

[11] In the ten years ending in 1907, trust company assets in New York State had grown 244 percent (from $396.7 million to $1.364 billion) in comparison to a 97 percent growth (from $915.2 million to $1.8 billion) in the assets of national banks (Barnett, 1910, p. 235).

[12] As financial agents, trust companies held deposits, negotiated loans, served as trustees for bonds, acted as registrar for securities, and facilitated dividend and interest payments. Contemporary observers noted that firms selected well-known New York trust companies as financial agents "because of the added confidence thus obtainable for the security issues of the corporation" (Smith, 1928, p. 355).

[13] In the parlance of the time, "combinations" denoted large industrial enterprises that had been formed through the merger of smaller firms that had likely been competitors. On the wave of mergers that ended around 1904, see Lamoreaux (1985).

[14] A 15% reserve requirement was imposed in 1906, but only one third of it was required to be held in cash. Advocates for New York's trust companies argued that their deposits were like investment accounts and therefore subject to much lower seasonal fluctuations than those of national banks—making a lower level of reserves appropriate (Judd, 1907).

[15] In 1903, the NYCHA adopted the rule that trust companies clearing through its members would have to maintain a cash reserve of 10% to 15% after 1 June 1904 (Judd, 1907). At the time trust companies faced no

was no established mechanism to facilitate cooperation among New York's trust companies, or to provide loans to a trust company that faced a liquidity problem.[16]

Trust company advertisements soliciting deposits prominently featured the names and affiliations of their directors. Many prestigious private bankers, two former U.S. Treasury Secretaries, and numerous executives of major non-financial corporations were among the directors of these enterprises, which enhanced their reputations.[17] But depositors' knowledge of the identity of trust company directors, and their reliance on the reputations of those men, contributed to the spread of panic when some of them were tainted by scandalous rumors.

Onset of the Panic

The panic was precipitated by events that had no direct connection to any trust company. Instead, they were 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 that financed them. At the center of the failed speculation were mining entrepreneur Augustus Heinze, along with financiers E. R. Thomas, O. F. Thomas and Charles W. Morse, who had collectively gained control of a series of relatively small banks and used some portion of their resources to finance their ventures.[18] On October 16, a run began on the Mercantile National Bank, which was under the control of those men, who appealed to the NYCHA for aid. The NYCHA provided a loan to Mercantile, and publicly pledged to support the other member banks connected to those men as well, which included the National Bank of North America.[19] However, as a condition for this aid, the NYCHA required the resignation of the entire board of directors of

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legal reserve requirement at all. Of the 26 trust companies that cleared through NYCHA members, all but two ended their clearing relationships when the rule was imposed (Kilburn, 1904).

[16] In the aftermath of the crisis, trust company insiders mentioned that there was intense "pressure for cooperative action" and that the "lack of organized cohesion" between banks and trusts made the situation much more difficult (*Trust Companies*, November 1907, p. 751.)

[17] 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.

[18] The story of Heinze's exploits in mining, his transition into banking, and the failed speculation in shares of United Copper is presented in McNelis (1968).

[19] On October 20, the NYCHA announced that all of the member banks involved in the copper speculation were solvent, and that a fund of $10 million had been raised to provide aid to these institutions (Bayles, 1919).

Mercantile, and demanded that Morse, the Thomases and Heinze resign from all other clearing banks where they held directorships, declaring that they "shall never again be allowed to do banking business under Clearing House auspices."[20] The very public support from the NYCHA and the change in management ended the runs on the Mercantile and other member banks connected to those men. However, it is likely that their expulsions from Clearing House banks severely tarnished their reputations and contributed to the perception that they had committed fraud. The public became concerned about the safety of deposits in other institutions to which these men had connections, however minor or insignificant.[21]

Several New York City trust companies had such connections. For example, Charles T. Barney, president of Knickerbocker Trust and director of Trust Company of America, two of the largest institutions of their kind in the city, was known to have been involved in earlier business dealings with Morse, and held a board seat with the National Bank of North America. Moreover, Morse, Thomas, and Augustus' brother Arthur Heinz held directorships with other trust companies. The business connections among these individuals, and the board seats they held, were widely reported in the press.[22]

The connections between the men at the center of the United Copper speculation and various financial institutions are illustrated in Figure 1. Morse, Thomas, Barney and the Heinzes held seats on the boards of five trust companies; we identify these institutions as having a *direct* connection to them. However, those five trust companies were, in turn, closely associated with three other trust companies, because they had at least two directors in common.[23] Those three trust companies are therefore identified as having an *indirect* connection to Morse, Thomas, Barney and the Heinzes. The degree to which trust companies were associated with those men may have influenced the intensity of the runs they faced during the panic, and we formally test this hypothesis below.

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[20] Bayles (1919: 178).

[21] Lefevre (1908:12) argues that "the ousting of the adventurers from the banks…had left the community so keenly apprehensive that almost anything would have stampeded it."

[22] See, for example, "New Banking Group Headed by the Heinzes," 21 January 1907, *New York Times*; and "C.W. Morse Quits the Banking Field," 20 October 1907 *New York Times*.

[23] In addition, the trust companies with Morse, Thomas, Barney and the Heinzes on their boards had only one director in common with an additional three trust companies. We focus on trust companies with at least two directors in common to capture stronger connections. The empirical analysis below is robust to the inclusion of a variable that separately identifies trust companies with only one link to these men.

The runs on trust companies began silently around October 16, when Knickerbocker Trust started to face heavy withdrawals. Knickerbocker was one of the few trust companies that chose to maintain sufficient reserves to gain access to the NYCHA through a member of the clearinghouse, National Bank of Commerce, where Barney was also a director. When Knickerbocker depositors began to withdraw their funds by depositing checks on their accounts in other banks, the National Bank of Commerce was responsible for those checks. Facing a debit balance at the NYCHA of $7 million and the prospect of even larger debits, on October 21 the National Bank of Commerce announced that it would no longer act as Knickerbocker's clearing agent.[24] On that same day, Knickerbocker Trust announced that it had dismissed Barney from the office of its Presidency, because of his "personal position in the directorate of certain institutions recently under criticism," and "in particular because of his connection with Mr. Morse."[25]

These events came as a shock to Knickerbocker's depositors. The end of the clearing relationship meant that other banks would no longer cash the trust company's checks and, more importantly, that the NYCHA would likely not aid Knickerbocker if the firm encountered liquidity problems.[26] The dismissal of Barney, even though it was accompanied by assurances that the firm was in sound condition, may have created the impression that Barney had done something improper or used the funds of Knickerbocker to help finance the speculative schemes of Morse. A severe run on the Knickerbocker ensued, and the firm could not withstand the heavy withdrawals without receiving 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 NYCHA, and "wild rumors circulated" regarding their financial condition.[27] These rumors often focused on possible

[24] These events are described in Senate Committee on Banking and Currency (1912, p. 1695). The National Bank of Commerce apparently appealed for aid for Knickerbocker from the NYCHA, but was denied (Wicker, 2000). Given Barney's association with National Bank of Commerce, it is possible that some of that bank's debit balance resulted from depositors moving their money to other clearing banks.
[25] *The Sun* [NY], 22 October 1907, p. 1.
[26] Moen and Tallman (2000) explore the significance of the trust companies' isolation from the NYCHA.
[27] Lefevre (1908, p. 13) and *Evening World* [NY], 23 October 1907. See also Moen and Tallman (2000: 150-52). Several studies have assessed the counterfactual history that would have followed if aid to Knickerbocker had prevented it from failing; see, for example, Sprague (1910) and Friedman and Schwartz (1963).

connections between trust companies and the men at the center of the failed corner scheme; the chairman of the Trust Company of America 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."[28] Within a few days, all of the trust companies where Thomas, the Henizes, or Morse held directorships announced their resignations.[29] By October 23, runs had spread to the Trust Company of America and Lincoln Trust, and several other trust companies also faced heavy deposit withdrawals.[30] To address the fears of depositors, some trust companies explicitly stated that they had no connection to the men associated with the scandal in their advertising.[31] All trust companies began to call in loans and liquidate assets to build up their cash reserves.

The losses of deposits of the 38 trust companies in New York City between August 22 and December 19 of 1907 are depicted in Figure 2, and detailed summary statistics of these institutions' balance sheet data are presented in Table A1 of the Data Appendix.[32] It is worth noting that the size of the trust companies prior to the crisis, measured as their total assets as of June 1907, was generally uncorrelated with the percentage decline in deposits. All of the trust companies associated with Morse, the Heinzes, Thomas, or Barney lost substantial amounts of deposits, although several others with no apparent direct or indirect connection to these men did as well.[33] Panel A of Table 1 shows that the average change in deposits for all trust companies was -32 percent, but there were large differences for firms with and without connections to the men involved in the scandal. The eight trust companies with a direct or indirect connection to Morse, the Heinzes, Thomas or Barney (as shown in Figure 1) had on average a decline in deposits of 55 percent, whereas the mean was

[28] John E. Borne, quoted in *New York Times*, 23 October 1907.
[29] *New York Times*, 20 October, 22 October, and 23 October, 1907.
[30] *Evening World* [NY], 23 October 1907, p. 1-2.
[31] For example, Bankers Trust's ad in the *New York Times* from 25 October 1907 lists its directors and their affiliations, and says "Particular attention is called to the personal character and strength of the company's directors."
[32] Since the Superintendent of Banks collected this data quarterly, we use the report dates closest to the panic.
[33] There is some suggestive evidence that financial relationships between the men at the center of the scandal, and trust companies other than those with whom they had a connection through board seats, may also have caused depositors to associate particular trusts with those men. For example, a 1908 criminal indictment claimed that E. R. Thomas had arranged a significant loan from the Italian-American Trust Company (*New York Times*, 28 February 1908). That firm had no connection to Thomas or his confederates through its board, but nonetheless lost 64 percent of its deposits. Unfortunately, it is impossible to observe such lending agreements systematically.

only -26 percent for those with no such connection, a very large difference that is also statistically significant.

We analyze the determinants of the percentage change in each New York trust company's deposits between August and December of 1907 more formally in Panel B of Table 1. In column (1), we regress the change in deposits on separate indicator variables for whether the trusts had direct or indirect connections to the men at the center of the scandal. The estimated parameters are -34 and -22 percent, respectively, and they jointly account for about 40 percent of the variation in the dependent variable. It is of course possible that these effects may have been driven by differences in the financial condition of the trust companies at the time of the crisis. In column (2), we add several balance sheet ratios calculated from the trust companies' financial statements of June 1907—measures of net worth, cash reserves relative to deposits, the percentage of assets invested in securities, and their overall size—as well as the log of their ages.[34] The estimated correlations generally have the expected signs, with the firms' net worth and cash holdings having particularly large and positive magnitudes, suggesting that more solvent and liquid trusts faced fewer withdrawals of deposits. Controlling for these characteristics, however, does not diminish the size of the estimated effect of the two indicator variables for association with the tainted bankers.

The available balance sheet information does not capture differences in the depositor clienteles of the trust companies. Hansen (2011) argues that New York's trust companies located in the vicinity of Wall Street generally received large deposits from corporations and institutions, whereas those located in uptown Manhattan solicited deposits more aggressively from individuals. He proposes that the uptown firms experienced greater deposit losses because small individual depositors were more likely to participate in runs. In order to address this possibility, column (3) includes an indicator variable for whether the trust company had an uptown headquarters.[35]

[34] Trust company advertisements nearly always listed the firms' paid-in capital and surplus (net worth), so this information was easily available to depositors. More detailed balance sheet information was collected by the New York Superintendent of Banks on a quarterly basis and republished in trade publications such as *Trust Companies* magazine or *Bankers Magazine*, making it generally accessible as well.

[35] We obtain headquarter locations from Hansen (2011, Figure 1). It should be noted that the distinction between uptown and downtown trusts is unclear in some cases because at least a few companies headquartered uptown such as Knickerbocker had downtown branches, and downtown trusts such as U.S. Mortgage and Trust had uptown branches.

Consistent with Hansen's argument, the estimated correlations indicate that the uptown firms did indeed lose a greater proportion of their deposits. But importantly, controlling for an uptown location does not alter the size of the estimated effect of the indicator variables for the strength of the connection with the men associated with the United Copper corner. Interestingly, the uptown variable does diminish the estimated effect of some of the balance sheet ratios, indicating that the estimated coefficients for these variables in column (2) may have resulted partly from correlations between the financial condition of trust companies and their type of depositor clientele. In sum, our results show that the deposit losses can be regarded in large measure as a response to an association with men involved in a scandal, and thus minimize concerns of reverse causality for the empirical analysis that follows.

Consequences of the Panic

During the Panic, J. P. Morgan and a few trusted associates coordinated a series of rescues of trust companies, securities dealers, and the City of New York that were instrumental in containing the financial panic.[36] Although Knickerbocker Trust was refused aid, emergency assistance was provided to other trust companies beginning on the day following Knickerbocker's closure, and continued well into November.[37] Ultimately these efforts to inject liquidity prevented additional trust companies from failing, and helped resolve the crisis.

On October 26, in the face of heavy withdrawals from out-of-town banks, the NYCHA issued "clearing house loan certificates" in order to provide liquidity to its members, and New York's commercial banks soon after suspended the convertibility of their deposits into currency.[38] Full convertibility of deposits by the nation's banks was not restored until January 1908. The suspension made important transactions more difficult (James, McAndrews and Weiman, 2011), but

[36] In late October, 1907, the U.S. Treasury made a series of deposits in the major national banks in New York, which helped fund some of this lending. Nonetheless, much of the lending to trust companies was financed by commitments from the stronger trust companies, and organized by Morgan.

[37] Knickerbocker's board claimed they had been promised aid (*New York Tribune*, 22 October 1907, pg. 1). However, Morgan decided that aid should be conditional on the solvency of the firm. Since his associates could not determine whether the firm was solvent in the few hours they had while the firm faced an intense run, aid to Knickerbocker was denied, and the firm was forced to close.

[38] There is some indication that by early November, the trust companies in New York began making payments via certified checks payable at the NYCHA, rather than in cash (*New York Tribune*, 1 November 1907, p. 1.)

likely halted the spread of the banking panic and averted a total collapse of the banking system, as in 1930-33 (Friedman and Schwartz, 1963).

The contraction of lending that occurred during the panic in New York was heavily concentrated within trust companies. Prior to the panic, the aggregate volume of New York trust company loans was similar to that of New York's national banks. However, total loans at trust companies contracted by $247.6 million, or 37 percent, between August and December (Moen & Tallman, 1992). In contrast, the loans of national banks in New York fell by only 2 percent during the same period. Contemporary observers noted the consequences: "It is obvious that every trust company is protecting itself to the full extent of its powers, and the small borrowers, however solvent, necessarily suffer at such a time."[39] The man brought in to serve as president of the Hudson Trust Company after E. R. Thomas resigned, testifying in a subsequent civil trial, was more specific: "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 were compelled to ask assistance from their creditors and trade was very much depressed."[40]

Did the corporations affiliated with trust companies lose access to lending during the crisis? Unfortunately, few if any records of the identities of specific borrowers from trust companies, or specific lenders to non-financial corporations, survive from this period. However, the records of one major New York trust company from a somewhat later period indicate that it did indeed lend substantial amounts to the companies represented on its board.[41] If that company's experience is at all representative of lending patterns in 1907, it is possible that many affiliated firms would have faced significant contractions in lending during the panic.

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[39] *Wall Street Journal*, 24 October 1907, p. 4.

[40] E. R. Chapman (1910: 332-33).

[41] Between 1916 and 1919, when it had become an enormous institution with total resources approaching $200 million, between 3.5 and 9.8 percent of the value of Farmer's Loan and Trust Company's loans were made to firms represented on their board, or to their directors. In addition, between 4.9 and 15.3 percent of the securities held by this trust company were issued by companies represented on their board. As the oldest and largest of the trust companies in the city, in a later era of more stringent regulations, these numbers are likely lower than they would have been for the younger and more aggressive trust companies back in 1907. (Report of Examination Records, Farmer's Loan and Trust, Series 07, Vault Item 754 00, Shelf 4A, Citigroup Archives).

3. Data on Non-financial Companies and their ties to Banks and Trust Companies

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 a trust company and a non-financial firm by the presence of a board interlock between the two. To observe board connections, we collected the names of all directors and officers of all NYSE-listed industrials and railroads as reported in *Moody's Manuals of Railroads and Corporation Securities*, and obtained lists of directors of commercial banks and trust companies from the *Rand McNally Bankers' Directory*. Cross-referencing the names of bankers with those of corporate directors enables us to identify the presence of trust company directors on boards of non-financial firms. We describe our matching procedure and the alternative sources we have used to verify its accuracy in the Data Appendix. While board interlocks signified close relationships between financial and non-financial firms at that time, one important question is whether the men generating these connections were primarily bankers or industrialists. To the extent that the principal affiliations of directors can be reliably classified, we found that the individuals creating these interlocks were primarily industrialists, railroad executives, or men best described as 'capitalists' sitting on trust company boards, rather than men strictly defined as bankers sitting on non-financial company boards.[42] The nature of these interlocks suggests that the affiliated financial institutions were indeed important for the provision of credit.

Throughout the analysis that follows, we designate as the differentially "affected trust companies" those that were among the top 25 percent in deposit losses.[43] Among the 125 non-financial firms in our sample with accounting data, 61 had a director in common with one of those

[42] Principal affiliations were recorded from the *Directory of Directors for the City of New York* for 1906 or, if the individual's affiliation was not identified in that volume, by using various biographical sources. Among the 88 unique interlocks between a trust company and a non-financial firm in 1907, only 9 were created by someone whose primary affiliation was with a commercial bank or trust company, and another 23 were created by partners of investment banks. Industrialists, railroad executives, and capitalists created most of the balance.

[43] The results presented below are robust to alternative definitions of the group of differentially affected trust companies. For example, focusing on the top 20 percent of deposit losses strengthens the findings, while results are slightly weaker when using the top 30 percent.

trust companies. For the accounting data on non-financial firms, we constructed a comprehensive dataset of all NYSE-traded industrial companies and railroads from various *Moody's Manuals*, which presents annual firm-level data obtained from annual reports. For each firm in our sample, we searched for financial information for each available year from 1900 to 1912. Except when explicitly stated, we restrict most of our analysis to the period 1903-1912, when accounting information is available for most of the firms in our sample.

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.[44] 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 reported balance sheets varied considerably, and relatively few industrials firms presented much detail in their income statements.[45] Still fewer firms reported any information at all regarding capital expenditures or investment. We therefore focus the analysis 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.

Our panel dataset contains annual observations of these measures, as well as data on the companies' boards and their connections to financial institutions obtained at two-year intervals. Tables 2 and 3 present summary statistics. Column (1) of Table 2 reveals that in 1907 publicly traded non-financial companies had extensive board interlocks with trust companies; the average NYSE-listed firm had nearly 4 trust company directors on its board, as well as 1.5 directors from New York's six major commercial banks.[46] Railroads and other transportation enterprises made up 56 percent of these firms, with the others mostly engaged in manufacturing.

Column (2) of Table 2 reports the difference between firms that were affiliated with at least

[44] The 1906 Hepburn Act required railroads to submit detailed financial reports to the Interstate Commerce Commission; the initial 1906 reports presented data for 1905.
[45] Sivakumar and Waymire (1993), and Barton and Waymire (2004) analyze the content of early financial statements.
[46] Sprague (1910) notes that these six banks alone accounted for about three fourths of all net deposits from other banks within New York City's national banks.

one of the trust companies that was most severely affected during the panic, and firms that had no such affiliation. Firms affiliated with affected trust companies had larger boards, were somewhat older, and were substantially more likely to be railroads. In addition, they 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.

Table 3 presents summary financial statistics for the NYSE-listed companies for 1906, the year before the panic. Column (1) reveals that these were very large enterprises, with a value of log total assets equal to 18.1.[47] The average leverage ratio, defined as bonded debt divided by total assets, was 0.29, and their cash holdings were about three percent of their assets. We calculated two measures of profitability from our data: 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 reflects the effects of firms' capital structure choices. These were 0.04 and 0.08 on average, and the average dividend rate on common stock was 0.03. The average interest rates paid by these firms, measured as interest expense divided by bonded debt, was 0.048.

Columns (2) and (3) of the table investigate differences between firms affiliated with the affected trusts and other firms. Column (2) shows that the affiliated firms were larger and more levered, which helps explain why they also had more extensive connections to financial institutions, as seen in Table 2. However, their profitability, dividends, and interest rates were quite similar to those of other firms. Column (3) analyzes whether there were any differential trends between the two groups of firms in the years preceding the panic. It presents the estimated coefficient from panel regressions with data from 1903-1906, in which each variable is regressed on year and firm fixed effects, and an indicator variable for an affiliation with an affected trust company interacted with a time trend. None of the estimated differential trends are large in magnitude or statistically significant, indicating that the two groups of firms were not evolving along different paths in the years prior to the panic.

4. The Effect of the Panic on Non-Financial Firms

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

A preliminary indication of the significance of the panic for firms affiliated with trust companies is found in Figure 3, which presents the share of all non-financial company board seats held by New York trust company directors, and by the directors of the most severely affected trust companies. In the years 1907 through 1913, the proportion of all board seats held by the directors of the differentially affected trust companies fell from 6.2 percent to 0.1 percent. In contrast, the board seats of all New York trust companies over the same period fell far less in relative terms, from 29.0 to 23.2 percent of board seats; the fall in seats held by the differentially affected trusts accounts for most of this decline.[48] If board interlocks with trust companies were formed at least in part to facilitate improved access to credit, the widespread severing of these relationships is consistent with the notion that firms had to turn elsewhere for lending or 'certification' following the panic.

Before proceeding with the empirical analysis, it should be noted that the effects of an affiliation with an affected trust during the panic were likely to be quite heterogeneous. Firms with substantial collateral whose value was easily verified, or with well-established reputations that were regarded as being of the "highest standing," should have suffered less than other firms, as they would have had an easier time obtaining alternative sources of lending.[49] As there was considerable variation in the size, age and reputation of the firms listed on the NYSE at the time of the panic, we expect the effects of the contraction in financial intermediation to be heterogeneous among the firms in our sample. In all of our empirical specifications below, we explicitly account for this heterogeneity using interaction terms with firm size, which likely captures the extent to which the firm's reputation was well known among lenders and investors, as they would have had to have raised substantial sums from the capital markets over the years.

Results: Stock Market Event Study

[48] Over this period, the total number of trust companies declined from 38 in 1907 to 33 in 1911 and 28 in 1913, due mostly to consolidation in the industry. The number of the affected trust companies still existing as independent entities fell from 8 in 1907 to 7 in 1911, and 4 in 1913.

[49] This implies that the sample of firms in this paper, those with shares traded on the NYSE, were among those firms *least* likely to suffer as a result of the shock to the trust companies, since they were among the largest and best-established enterprises in the United States.

We begin by analyzing changes in the common stock returns of NYSE-listed firms around the onset of the panic. Overall, the stock market suffered significantly during the crisis. From the beginning of October through late November, the market fell approximately 25 percent. However, the decline in stock market value varied considerably across firms. If investors perceived that the losses of deposits and reputations among the trust companies would adversely impact their non-financial clients, we would expect a decline in the stock prices of firms connected to those trust companies relative to other firms.

As a starting point, we create equal-weighted portfolios of weekly industry-adjusted returns by grouping firms according to their size and their connections to affected trusts.[50] 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. As Figure 4 shows, all portfolios followed a similar trend until October 18, around the time the run on Knickerbocker started. In the following week, the returns on small firms connected to affected trusts plummeted, and had not recovered by the end of the year. In contrast, the other three portfolios did not suffer significantly during the depositor runs, and returned to their pre-panic level by the end of December.

The portfolio analysis is suggestive of a large negative effect of the financial panic on the stock market outcomes for small firms with ties to affected trusts, relative to other firms. 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.[51] Since no high-frequency data on stock prices and dividend payouts are readily available for our period, we collect all necessary information from *The New York Times*. We discuss the data sources, variable definitions and methodology in detail in the Data Appendix. As securities markets were

[50] 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, particularly towards the end of the sample period.

[51] Our strategy diverges from the standard event study methodology because we do not calculate abnormal returns as the difference between the actual return and a predicted return obtained from a market model. Estimating such a model would require collecting weekly prices and dividend information from primary sources for an extended period of time prior to the panic, which would be a large data collection effort. Our strategy is closer to a market-adjusted-return model, which assumes that $\alpha=0$ and $\beta=1$ for every share (Campbell, Lo and MacKinlay, 1997). The lack of extensive information on stock returns prior to the panic also limits our ability to adjust the standard errors using the pre-period variance in returns.

relatively illiquid early in the twentieth century, our data contain cumulative returns for about 60 percent of the firms in the sample, almost perfectly split between firms with and without connections to affected trusts.[52]

Our empirical strategy consists of estimating:

$$\textit{Industry - Adjusted Cum. Return}_i = \alpha + \beta_0 \textit{Affectedtrust}_i + \beta_1 \textit{Affectedtrust}_i \times \log \textit{assets}06_i + \beta_2 \log \textit{assets}06_i + \delta X_i + \varepsilon_i$$

where $\textit{Affectedtrust}_i$ 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* is the log level of assets at the end of 1906, measuring the size of firms prior to the panic; and X_i captures firm characteristics prior to the panic that may affect the level of returns, such as being a railroad. If the market perceived that connections to an affected trust company through board interlocks were detrimental because of disruptions in access to credit or harm to the firm's reputation, then we would expect to find a negative overall effect on the returns paid by the firm's shares $(\beta_0 < 0)$. However, as the consequences of these connections were likely to be heterogeneous, the interaction term between affected trust and log assets captures the differential effects for firms of different sizes. We expect to find that larger firms connected to affected trusts had higher returns during the panic than smaller firms with similar connections $(\beta_1 > 0)$.

Ideally, we would like to study the stock market reaction on Tuesday October 22, 1907, the day on which Knickerbocker failed. Since our data contain only end-of-week prices, instead we center the event on the closest day available after the failure, Friday October 25. Panel A of Table 4 presents simple 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 four percentage points, significantly different from the increase in returns on about two percentage points for non-connected firms. Dividing the sample between large and small firms reveals that small firms account for this difference.

[52] We are more likely to observe stock returns for larger firms, but no other observable firm characteristic in our data is a strong predictor of availability of stock price information.

Panel B of Table 4 presents regressions of the effects on the stock market as the runs unfolded. In column (1), we analyze the cross-sectional variation in cumulative returns for a one-week period around this event date, beginning with returns that are not industry-adjusted. Cumulating returns over this window helps address the possibility of anticipation of the effect, particularly if non-financial firms began to experience a tightening of credit or loss of reputation around the run on the Mercantile National Bank. Our findings suggest that the panic had a significant differential negative effect on the returns of firms connected to affected trusts. The estimates of β_0 and β_1 are statistically significant and have the expected signs. For a firm of median size, the difference in cumulative returns on the week around the panic was -8.1 percentage points (= -1.206 + [0.0615 ×18.29]) lower than for a non-connected firm of similar size. This is quite a large magnitude given that the average cumulative return was -0.10, and it reflects the large dispersion in returns in our sample.[53] As expected, the magnitude of the effect was heterogeneous and related to the initial size of firms. For firms in the 75th percentile of the assets distribution, a connection to an affected trust led to a much smaller -0.030 (= -1.206 + [0.0615 ×19.12]) decline in cumulative returns, whereas a firm at the 25th percentile experienced very large negative returns of -0.114 (= -1.206 + [0.0615 ×17.75]) due to ties to affected trusts.

We next analyze industry-adjusted returns, to address the potential concern that firms within a given industry may have faced common shocks as the crisis evolved.[54] The results are unchanged by this transformation (column 2). In column (3) we include many of the firm characteristics that differed across firms with and without connections to affected trusts, including the total number of trust companies connected to the firm, the number of board seats held by directors of major commercial banks, the book level of leverage in 1906, and the cash to assets ratio in the same year. None of these characteristics affect the estimated difference in cumulative returns, and the coefficients of interest remain unchanged. Finally, column (4) shows that the results are robust to extending the event window to the two-week period surrounding the run on Knickerbocker.

[53] For example, the share price of International Steam Pump, an industrial firm connected to an affected trust, declined from $20.25 on October 11 to $9.75 on November 1.

[54] We adjust the individual firm returns by the industry return in each week during the event window, as described in the Data Appendix. For all specifications, we restrict the sample to industries defined at the one-digit level where there are at least nine firms with non-missing cumulative returns.

Whether because of reduced access to lending or a loss in reputation, these results indicate that smaller firms connected to affected trust companies saw their stock prices fall substantially during the panic. Since our strategy compares the cross-sectional differences in returns, one possible concern is that the results may not be specific to the financial crisis—perhaps these firms generally performed worse. As a "placebo" test, we replicate our analysis of one-week cumulative returns centered at a fictitious event date as early in advance of the panic as our price data allows. As shown in column (5), we find no statistical differences in the returns centered at September 13, 1907, and the magnitudes of the estimated coefficients, both for the "affected trust" dummy variable and for its interaction with initial firm size, are much smaller and have the opposite sign than during the panic. These results are quite reassuring as they suggest that firms connected to affected trusts did not have lower cumulative returns in general relative to the non-connected firms.

Results: Firm Performance

The event study reveals an immediate effect on firm valuations of the ties between financial and non-financial firms as the panic unfolded, but these results do not shed much light on the medium-run effects of an interruption of credit intermediation, beyond the fact that the market expected them to be substantial. We further study the transmission of the panic to the real economy by analyzing the performance of non-financial firms in the years following the panic. We use our panel dataset from 1903 to 1912, which contains 125 firms with accounting information both before and after the panic.[55]

Before proceeding to the regressions, Figure 5 presents the annual differences between firms with and without board interlocks with affected trusts in 1907 for return on assets and return on equity, in the upper panel, and firms' dividend rates and interest rates, in the lower panel, as estimated from regressions with firm fixed effects.[56] Quite reassuringly, in the years prior to the

[55] The actual number of firms varies slightly across specifications since the accounting information is missing for some firm-years for some variables of interest.

[56] The end of fiscal year varied across firms. To be consistent, we assigned accounting data to a given calendar year in our dataset if the fiscal year ended between July of that year and June of the following calendar year. Thus, the data represented in the points labeled 1907 in Figure 5 contain information from financial statements for fiscal years ended from July 1907 to June 1908, a blend of pre- and post-panic data.

panic the differences in profitability and dividend rates do not exhibit a negative trend, and the difference in interest rates does not show a positive trend. Instead, the lines change slope dramatically at the time of the panic.[57] Thus, it is unlikely that any estimated effect would be the result of preexisting differential trends in the dependent variables between firms with and without ties to affected trusts.

The upper panel shows that the profitability of firms affiliated with affected trusts was rising in the years prior to the panic relative to other firms, and then collapsed in 1907 and 1908, before recovering somewhat in 1909 and 1910. The lower panel of the figure provides some insight into how these firms responded to this fall in profitability. The dividend rate of firms affiliated with affected trusts had been rising relative to other firms, but was cut sharply in 1908 and remained low in 1909 before recovering in 1910. In addition, the interest rates paid by firms affiliated with affected trusts relative to other firms increased substantially between 1906 and 1908, a sign that connected firms resorted to more expensive sources of credit.

To more formally test the difference-in-differences in these series, and to investigate the potential sources of heterogeneity in the effects of the panic, we estimate:

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

where π_{it} is one of the measures of performance of interest for firm *i* during year *t*; α_i and γ_t are firm and year fixed effects that control for time-invariant firm characteristics and for overall macroeconomic conditions; $\boldsymbol{X}_{it}$ is a vector of time-varying firm characteristics, such as log assets; $Affectedtrust_i \times postpanic_t$ is an indicator equal to one for all years for firms with a director of a trust that was differentially affected during the panic on its board multiplied by an indicator for the years 1907 and later; and $Affectedtrust_i \times postpanic_t \times logassets06_i$ is that same indicator multiplied by the

[57] It should be noted that, as the values of the interest rate can only be calculated from actual indebtedness, they almost certainly understate the change in market interest rates faced by many firms, as those that found quoted rates unacceptable may have minimized or forgone new borrowing.

log value of the firm's assets in 1906. In this framework the differential effect on firm performance of having an affiliation with an affected trust in 1907 for the years during and after the financial crisis is ($\lambda_1+\lambda_2 logassets06_i$.). If the effect is indeed concentrated among smaller firms, which may have been perceived as risker, we would again expect $\lambda_1 < 0$, and $\lambda_2 > 0$.

Table 5 studies the effect of connections to affected trusts on firms' profitability, using return on assets and return on equity. As in the rest of our analysis, the standard errors are clustered by firm, in order to address potential problems of autocorrelation in residuals. Consistent with the notion that credit intermediation contracted in the wake of the panic, and that this contraction adversely impacted firms' ability to finance their operations and investments, all of the specifications indicate a negative effect on profitability that was greater for smaller firms. In columns (1) and (3), the estimated difference-in-differences for firms affiliated with affected trusts in the wake of the panic is about -0.002 and -0.008 for firms with the median level of log assets, and -0.005 and -0.013 for firms at the 25th percentile of assets.[58] The effects for the smaller firms are equivalent to around 13 and 15 percent of the mean values of ROA and ROE in 1906, respectively.

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 levered and held lower cash balances prior to the crisis. Thus, omitted variables are a source of concern about the results in columns (1) and (3) of Table 5 if firms with these characteristics performed differently in the years following the crisis, or evolved along a differential trend throughout the sample period. Therefore columns (2) and (4) include additional controls for firm characteristics that differed between connected and non-connected firms, as observed in 1906, interacted with a time trend.[59] These include the number of trust companies represented on the firm's board, the number of seats held by the six major New York commercial banks on the firm's board, the firm's leverage ratio, and the firm's cash to assets ratio. In addition, the firms affiliated with the affected trust companies tended to operate in somewhat different industries than other firms. In order to address the

[58] For the 125 firms in the sample, the median value of log assets in 1906 is 18.025, and the 25th percentile is 17.302.

[59] The results are essentially the same if instead of interacting these variables with a time trend, they are instead interacted with an indicator variable for the post-panic period.

possibility that these industry differences may have contributed to differences in performance following the panic, industry-specific trends are also added. The inclusion of all of these separate trends does not substantially alter the magnitude of the estimated effects.

We next use similar specifications to study the effect of connections to affected trusts for firms' dividend rates and the average interest rates on their debt. These results are reported in Table 6. All of the results are qualitatively similar to those presented above for firms' profitability. The estimated coefficients in columns (1) and (3) of the table imply that the difference-in-differences for firms affiliated with affected trusts was an increase in average interest rates of 0.0026 and a decrease in dividend rates of -0.0043, for firms with the median level of assets. For firms at the 25th percentile of assets, these values become 0.0036 (or 36 basis points) and -0.007, respectively, about 8 percent of the mean interest rate and 22 percent of the mean dividend rate in 1906. Columns (2) and (4) add the same trends as in Table 5, and once again the estimated effects are not substantially affected. Taken together, these results imply that firms with connections to the trust companies that came under acute pressure suffered a persistent and significant reduction in several measures of performance in the years following the panic.

Robustness of the Results

In this section, we address the most significant potential sources of concern regarding our results. In the Results Appendix, which follows the Data Appendix, we explore the robustness of the results further by estimating several alternative specifications of the regressions presented above.

One potential issue is that the firms that had no affiliation with an affected trust may not be an appropriate control group for those that did, since they were different by a number of observable characteristics. This concern does not seem so severe in our case because the estimates in Table 3 indicate that there were no differential pre-trends along these (or other) characteristics, and the results in Tables 6 and 7 were robust to the inclusion of controls for differential trends. Still, to address this problem more thoroughly, we first eliminate from the sample firms in the control group that had no affiliation with any trust company. This strategy drops firms that were likely to be the most 'different' from the firms with affiliations with affected trusts. Second, 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 the inverse propensity scores.[60] The results of these regressions, where the specifications chosen are the same as those with the full set of controls from Tables 5 and 6, are presented in Tables 7 and 8. In general, the estimated effects are not substantially different and retain their statistical significance. The one exception is the regression for return on equity in column (4) of Table 7, but the estimated effect is still around 80 percent of the size of the equivalent specification from above. These results indicate that control and treatment firms in our original sample were similar enough—that is, it is unlikely that a lack of common support creates bias in the results in Tables 5 and 6.

Another potential problem 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 leading to worsened firm performance. If the loss of an affiliated lender forced some firms to seek alternative sources of finance, firms with collateral whose value was easily verified should have been best able to do this. 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 with affiliations with affected trust companies suffered less than industrials in the years following the panic, relative to other firms.

Table 9 presents regressions for our standard outcome variables that estimate a specification

[60] 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, the cash to assets ratio, the number of 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, 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) are eliminated. We weight the resulting observations by the inverse of the propensity score. This weighting eliminates all significant differences in the chosen firm characteristics.

where the usual *Affectedtrust*×*post-panic* variable is interacted with an indicator for railroads, and an indicator for industrials. Consistent with the notion that firms with riskier collateral should suffer more following the panic, the results indicate that industrials performed worse according to every measure of performance, whereas the railroads were barely affected.[61]

A final and very important issue of concern is that the results may be driven by selection on time-varying unobservable firm characteristics, which is particularly difficult to address. If firms that were especially vulnerable to a crisis selected into relationships with the trust companies that were also adversely affected by the panic, then this selection process could account for the results that we attribute to the connections to affected financial intermediaries. For example, firms that pursued aggressive financing strategies or undertook extraordinarily risky investments, which would have made them more vulnerable to the effects of a financial crisis, may have formed affiliations with the affected trust companies. Our estimation framework does not address this type of selection.

To test for the vulnerability of our affiliated firms directly, we use the experience of an earlier recession and financial panic in which trust companies did not face runs. In 1903 and 1904, the United States experienced a prolonged recession that included a financial panic during 1903.[62] This crisis saw a sustained decline in securities prices, and led to a significant contraction in credit markets; "even the best railways and municipalities found it difficult to borrow on their accustomed terms" (Mitchell, 1913: p. 67). 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.[63] It certainly constituted a shock to securities markets and a significant decline in demand.[64] If the firms affiliated with affected trusts in 1907 were fundamentally similar four years prior to that shock, in the sense that the degree of their vulnerability to shocks was roughly the same, and if the panic and recession that occurred can be considered reasonably similar to the panic

[61] Within industrials, the largest effects are generally found among the retailers, and the second largest among the mining companies. The former had relatively few fixed assets, whereas the latter likely had assets regarded as particularly risky, or difficult to value.
[62] The NBER dates a peak in September 1902, and a trough in August 1904. The financial crisis in 1903 was given the moniker "The Rich Man's Panic."
[63] Friedman and Schwartz (1963: p. 151-52) note that Treasury Secretary Shaw made "unprecedented efforts" to bring relief to banks, significantly increasing government deposits and waiving reserve requirements.
[64] Romer (1999) presents evidence on the relative magnitude of the output loss associated with this recession.

of 1907, then the experience of firms that were affiliated with affected trusts in 1907 during the years 1903-04 should provide a test of whether they are indeed a differentially vulnerable group of firms.

We test this hypothesis using data from 1900-06. In particular, we investigate whether the firms that had affiliations with affected trusts in 1907 performed differentially worse in the years 1903 and 1904, and whether any such effects were heterogeneous and related to firm size. To perform this difference-in-difference analysis, we add the available accounting data from 1900 to 1902 in order to create a pre-period, and designate 1905 and 1906 a post-period. We estimate a regression of the same form as those above, focusing on the difference observed in the years 1903-04 relative to the pre- and post-period:[65]

$$\pi_{it} = \alpha_i + \gamma_t + \lambda_1 Affectedtrust_i \times 1903/04_t + \\ + \lambda_2 Affectedtrust_i \times 1903/04_t \times logassets04_i + \delta \boldsymbol{X}_{it} + \varepsilon_{it}$$

where now the differential effect of affiliation with an affected trust during the recession is equal to ($\lambda_1 + \lambda_2 logassets04_t$.)[66]

The results, reported in Table 10, indicate that the firms affiliated with affected trusts in 1907 did not perform differentially worse in the 1903-04 recession. The estimates for ROA and ROE in columns (1) and (2) are much smaller in absolute value than those reported in Table 5 and are not statistically distinguishable from zero. In addition, the point estimates for λ_1 and λ_2 are positive and negative, respectively – the opposite pattern found for the 1907 panic. Given that they are very imprecisely estimated, we are cautious about interpreting the precise magnitude of the estimated coefficients and comparing them with our earlier estimates. With this caveat in mind, the effect on ROA for a firm at the 25th percentile in log assets is positive, and the effect on ROE is negative but only about 23 percent of the size of the effect from 1907. In column (3), the

[65] We focus on the narrow window of the panic years relative to both the pre- and post-periods in order increase the number of firms for which we can estimate the effects. As the consequences of the panic were concentrated in 1903-04, this also makes it more likely to find an effect.

[66] Ideally we would like to use the value of assets in 1902, which would not reflect any changes that occurred during the recession. However, we only observe the value of assets for 38 firms in 1902, whereas we observe them for 90 firms in 1904, which creates a much larger sample. Results using the smaller sample with the 1902 log assets interaction are substantially unchanged.

coefficients on the dividend rate even become statistically significant with the opposite signs than in the 1907 crisis; the effect for a small firm is an *increase* in the dividend rate of 0.07 percent. Finally, again for a small firm, the interest rate on their debt actually fell by 0.13 percent (column 4).

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 and 1904. Two potential sources of concern about the consistency of our firms over time could be that some of them were subject to antitrust investigations after 1904—enforcement of the Sherman Act certainly became more aggressive around this time—or were involved in mergers that may have weakened their balance sheets. In the Results Appendix Table A3 we analyze this possibility by excluding from the analysis all firms that were involved in federal antitrust cases or major mergers. The effects of the Panic of 1907 actually become stronger for firms affiliated with affected trusts once those firms are eliminated from the sample.[67] The stronger effects for the Panic of 1907 relative to 1903-04 do not appear to be the result of antitrust enforcement or mergers. Any interpretation of the results of this paper based on differential vulnerability of the firms affiliated with affected trusts in 1907 through channels other than the shocks to their connected financial intermediaries must confront the fact that these firms did not perform differentially worse in the 1903-04 panic and recession.

5. Conclusion

The Panic of 1907 was one of the most severe financial crises prior to the Great Depression. This paper investigates a particular channel through which the financial crisis was likely transmitted to the real economy: the affiliations between the trust companies that came under acute pressure, and their client firms. In the early twentieth century, financial institutions such as trust companies commonly placed one of their directors on the boards of the firms with which they had strong ties. Trust companies were major lenders, holders of securities, and providers of fiduciary services for corporations. The corporations affiliated with trust companies that came under acute pressure likely

[67] As discussed in the Data Appendix, it is quite likely that the strongest firms were the subject of antitrust investigations, and even successful prosecutions did not appreciably harm many firms.

faced a differential shock during the panic. That the runs were triggered by rumors about trust company directors unrelated to any major nonfinancial companies is important evidence that this shock originated within the financial sector, and was likely uncorrelated with other shocks that the trust companies' client firms faced.

At the onset of the panic, traders sold off the shares of non-financial companies affiliated with the severely affected trust companies to a greater degree than they did other firms in the same industries. The performance of these firms also suffered for some years following the panic. Some evidence suggesting that the fall in profitability occurred due to financial constraints was found with the rise in interest rates these firms experienced.

The results highlight the important role of financial frictions in transmitting the effects of financial crises. In a frictionless world in which the creditworthiness of firms was common knowledge, the loss of a few lenders would not severely hurt affiliated borrowers, who would be able to easily obtain credit from alternative sources. Indeed, there would be no reason for borrowers and lenders to become 'affiliated.' Instead, we have found that small borrowers—those whose reputation was not yet well established, and those with collateral that was difficult to value—were severely affected by the near-collapse of their lenders. This effect could have been the result of the trust companies reducing the provision of financial services to their clients, but it may also have been caused by a stigma created by the clients' association with a troubled institution.

Although the contraction of financial intermediation resulting from the Panic of 1907 was severe, it was also relatively short-lived. In part this was due to the privately organized rescue efforts that prevented the panic from spreading further and enabled all but one of the New York trust companies to meet the demands of depositors. It was also partly due to the decision by commercial banks to suspend convertibility of their deposits into cash, which reduced the contraction of their balance sheets and likely prevented the spread of contagion (Friedman and Schwartz, 1963). Yet even though liquidity was restored to the financial system relatively quickly, the firms that were differentially exposed to the contraction suffered for several years. This is suggestive evidence that restoring liquidity in a financial panic alone may not prevent a relatively persistent economic downturn.

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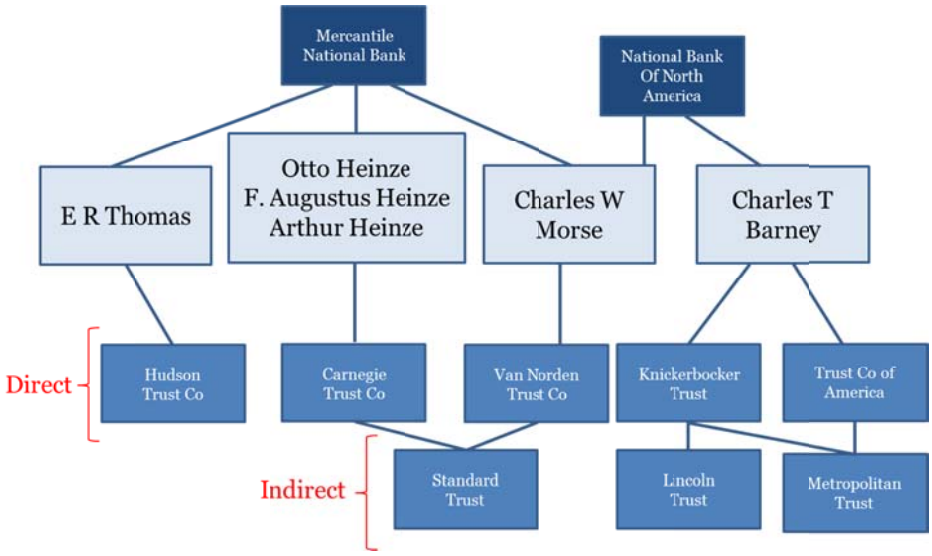

Figure 1: Connections between speculators and trust companies

Lines connecting individuals and institutions (or institutions to other institutions) indicate where directorships are held. The institutions at the top of the figure are the national banks that connect the speculators to one another and to Barney. Morse, Thomas and the Heinzes also controlled several other banks (not shown). The group of trust companies denoted as having an indirect connection to the speculation was defined as those with at least two directors in common with those that had a direct connection.

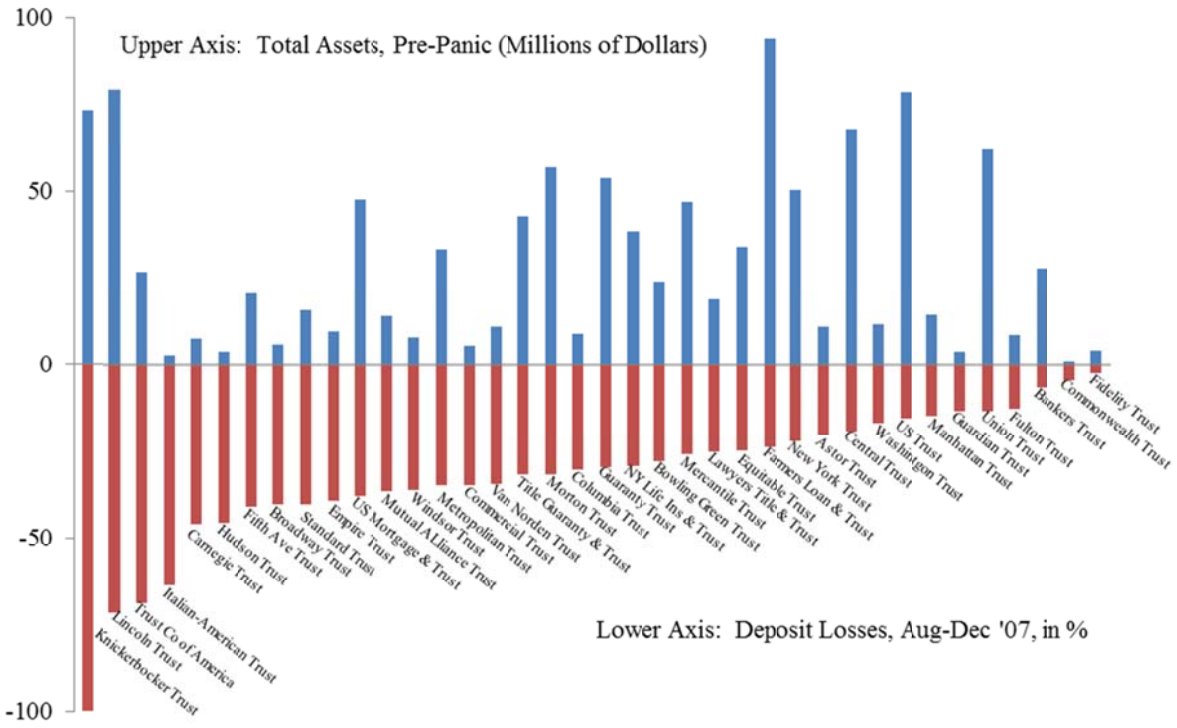

Figure 2: Assets and deposit losses at New York City trust companies

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

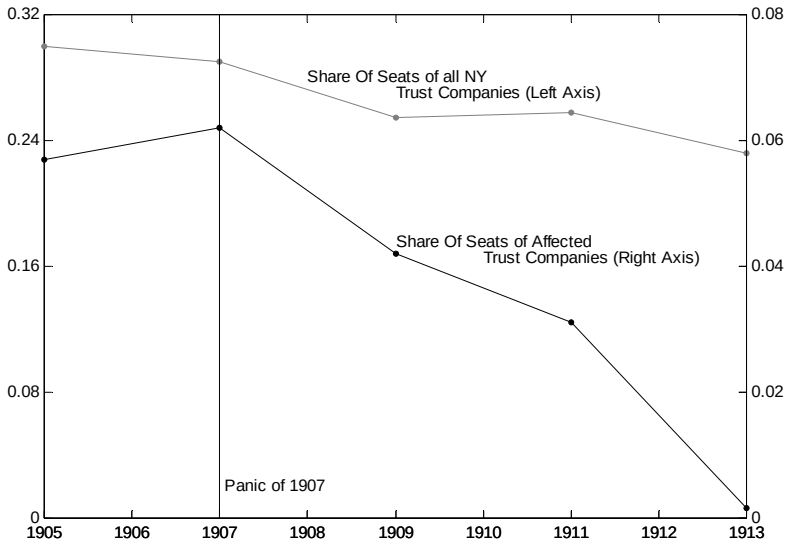

Figure 3: Board seats held by trust company directors, 1905-1913

Percent of all board seats of NYSE-listed railroads and industrials held by directors of New York City trust companies, and by directors of the differentially affected trust companies, defined as those among the top 25% in deposit losses during the Panic of 1907.

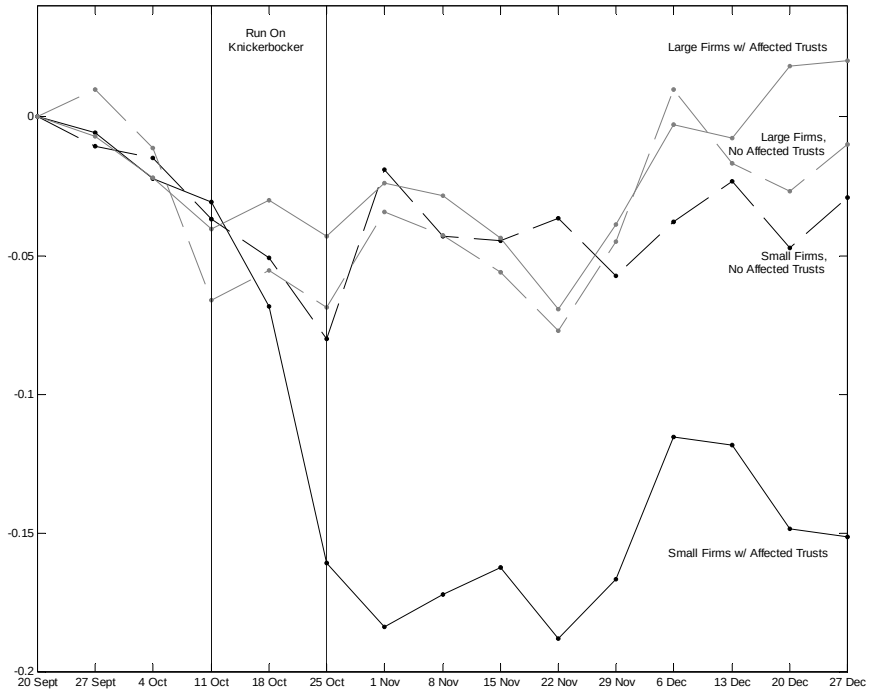

Figure 4: Stock market reaction, by affiliation and by size

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.

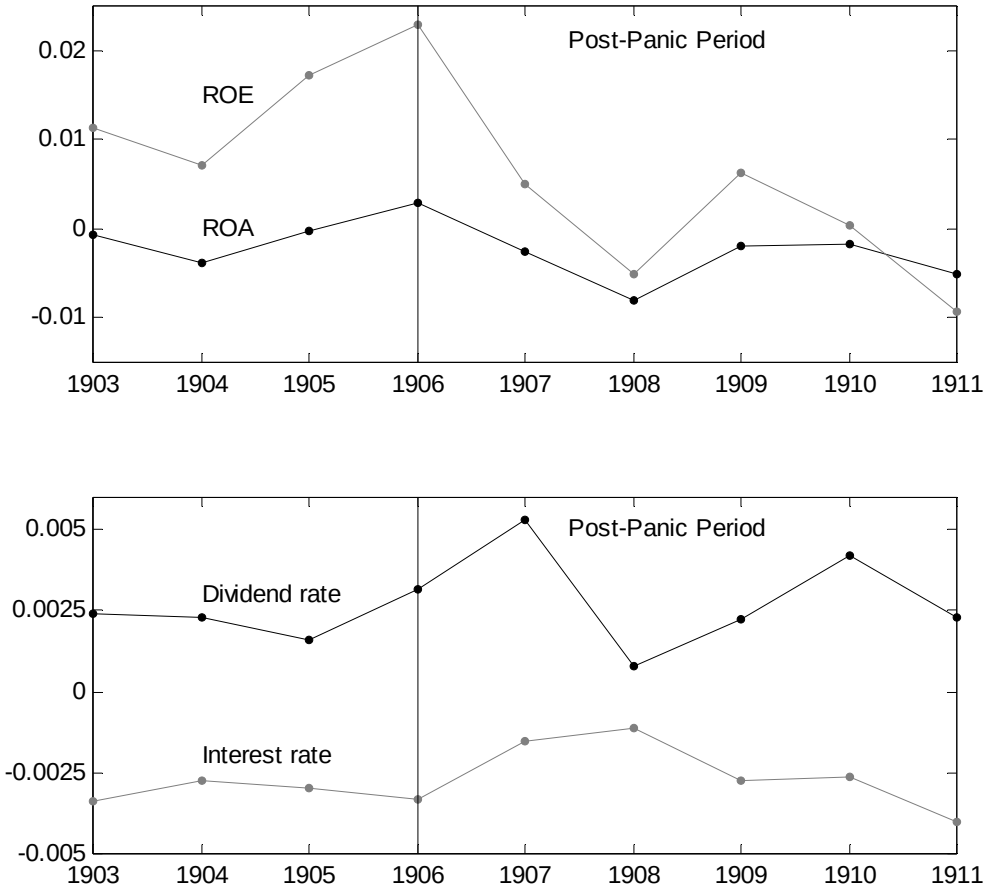

Figure 5: Annual estimated differences between firms with and without affected trusts
Each line plots the annual difference between firms with and without affected trusts on their board in 1907, as estimated in a regression that controls for firm fixed effects and firm assets. All reported differences are relative to 1912.

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

A. Change in Deposits				
	All Trust Companies	Direct or Indirect Connection	No Connection	Difference
Mean	-0.321	-0.553	-0.259	-0.295***
	[0.195]	[0.230]	[0.130]	(0.062)
Observations	38	8	30	
B. OLS Regressions on Change in Deposits				
		(1)	(2)	(3)
Direct Connection		-0.338***	-0.378***	-0.347***
		(0.113)	(0.104)	(0.0757)
Indirect Connection		-0.223**	-0.219**	-0.203***
		(0.0927)	(0.0877)	(0.0528)
Net Worth / Assets			0.475**	0.219
			(0.184)	(0.208)
Cash / Checkable Deposits			4.517*	3.991
			(2.217)	(2.612)
Stock and Bond Investments / Assets			0.270	0.139
			(0.166)	(0.134)
Log(Total Assets)			0.0154	-0.0261
			(0.0383)	(0.0342)
Log(Firm Age)			-0.0249	-0.0129
			(0.0318)	(0.0261)
Uptown Headquarters				-0.168**
				(0.0700)
Constant		-0.259***	-0.810	0.00322
		(0.0244)	(0.667)	(0.631)
Observations		38	38	38
R-squared		0.406	0.576	0.676

Notes: Standard deviation in brackets and standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors calculated in Panel B. The balance sheet variables used in the regressions in Panel B are as of June 1907.

Table 2
Summary Statistics, Firm characteristics

	Mean [Std Dev] 1907	Difference: Firms with Affected trust Interlock, 1907
	(1)	(2)
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)
Firm age	19.3840	5.8916+
	[19.3052]	(3.0309)
Industry: SIC 1 (Mining)	0.0480	-0.0297
	[0.2146]	(0.0084)
SIC 2 (Light mfg)	0.1280	-0.1219*
	[0.3354]	(0.0587)
SIC 3 (Heavy mfg)	0.2480	-0.1002
	[0.4336]	(0.0772)
SIC 4 (Transportation)	0.5600	0.2510**
	[0.4984]	(0.0865)
SIC 5 (Distribution)	0.016	0.0007
	[0.1260]	(0.0226)

Notes: Column (1) reports means for 1907, with standard deviations in brackets, based on 125 firms. Column (2) reports differences in means for firms with affiliations with affected trust companies (a total of 61 firms), relative to those without (a total of 64 firms), for 1907, from a regression with industry fixed effects, and presents robust standard errors in parentheses. (The regressions for the industry classifications do not contain industry fixed effects.)

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)	18.1021	0.4726*	0.0060
	[1.1412]	(0.1991)	(0.0133)
Cash/Assets	0.0282	-0.0095+	-0.0021
	[0.0306]	(0.0048)	(0.0025)
Leverage Ratio	0.2935	0.0753**	0.0091
	[0.2039]	(0.0253)	(0.0060)
ROA	0.0383	-0.0047	-0.0006
	[0.0342]	(0.0054)	(0.0014)
ROE	0.0835	0.0100	0.0023
	[0.0556]	(0.0108)	(0.0031)
Dividend Rate	0.0320	0.0028	-0.0002
	[0.0403]	(0.0084)	(0.0019)
Interest Rate	0.0481	-0.0020	-0.0013
	[0.0100]	(0.0022)	(0.0009)

Notes: Column (1) reports means for 1906, with the standard deviations in brackets. Column (2) reports differences in means for firms with affiliations with affected trust companies (a total of 61 firms), relative to those without (a total of 64 firms), for 1906, 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. **, * and + denote significance at 1%, 5% and 10%, respectively.

Table 4
Stock Returns at the Onset of the Panic

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

		All (n=78)	Small (n=39)	Large (n=39)
Connected to Affected Trusts	Mean	-0.042	-0.129	0.016
		[0.130]	[0.136]	[0.087]
Not connected to Affected Trusts	Mean	0.023	0.019	0.030
		[0.111]	[0.112]	[0.113]
Not connected versus connected	Difference	0.065*	0.148**	0.014
		(0.027)	(0.040)	(0.032)

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 -.099, SD .125)	[-1,1] (Mean -.010, SD .125)		[-2,2] (Mean -.042, SD .115)	[-1,1] (Mean .010, SD .067)
	(1)	(2)	(3)	(4)	(5)
Affected trust	-1.206**	-1.300**	-1.339**	-1.306**	0.220
	(0.437)	(0.429)	(0.448)	(0.451)	(0.303)
Affected trust × log assets	0.0615**	0.0661**	0.0678**	0.0684**	-0.0115
	(0.0230)	(0.0226)	(0.0236)	(0.0237)	(0.0161)
Log assets	-0.0256	-0.0216	-0.0192	-0.0153	-0.00598
	(0.0200)	(0.0203)	(0.0235)	(0.0179)	(0.0139)
Railroad	0.0998**	0.0709+	0.0677+	0.0343	-0.00791
	(0.0366)	(0.0361)	(0.0394)	(0.0335)	(0.0218)
Number of trusts on board			0.00390		
			(0.00693)		
Seats of major banks			-0.0128		
			(0.0137)		
Leverage			0.00776		
			(0.0766)		
Cash/assets			-0.0409		
			(0.409)		
Constant	0.337	0.377	0.336	0.233	0.123
	(0.353)	(0.360)	(0.410)	(0.315)	(0.249)
Observations	78	78	78	77	74
R-squared	0.268	0.253	0.266	0.197	0.057

Notes: Standard deviation in brackets and robust standard errors in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively. 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. The log assets interaction in the second row of Panel B uses the value of assets in 1906. In column (3) the added regressors are observed in 1907, in the case of the board variables, or 1906, in the case of the accounting variables.

Table 5
Effects of Connections to Affected Trusts on Firm Profitability

	ROA: (Mean .033, SD .033)		ROE: (Mean .071, SD .053)	
	(1)	(2)	(3)	(4)
Affected trust × post-panic	-0.0753**	-0.0616*	-0.126*	-0.112*
	(0.0262)	(0.0246)	(0.0543)	(0.0538)
Affected trust × post-panic × log assets	0.00406**	0.00324*	0.00654*	0.00564+
	(0.00138)	(0.00132)	(0.00289)	(0.00288)
Number of trusts on board × time trend		-0.00005		-0.000009
		(0.00005)		(0.000098)
Seats of major banks × time trend		-0.00009		-0.00001
		(0.0002)		(0.000387)
Leverage × time trend		0.00264		0.00218
		(0.00170)		(0.00388)
Cash/assets × time trend		-0.00692		-0.0223+
		(0.00623)		(0.0121)
Log assets	-0.00619	-0.00152	0.0296*	0.0386*
	(0.00643)	(0.00656)	(0.0145)	(0.0163)
Constant	0.147	0.0574	-0.470+	-0.662*
	(0.117)	(0.118)	(0.265)	(0.297)
Observations	994	994	981	981
R-squared	0.861	0.876	0.800	0.808
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry-specific trends	NO	YES	NO	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively. The log assets interaction in the second row uses the value of assets in 1906. In columns (2) and (4), the added regressors are observed in 1907, in the case of the board variables, or 1906, in the case of the accounting variables, and interacted with a time trend.

Table 6
Effects of Connections to Affected Trusts on Dividend and Interest Rates

	Dividend Rate: (Mean .033, SD .043)		Interest Rate (Mean .048, SD .010)	
	(1)	(2)	(3)	(4)
Affected trust × post-panic	-0.0724*	-0.0639+	0.0296*	0.0370*
	(0.0341)	(0.0336)	(0.0138)	(0.0170)
Affected trust × post-panic × log assets	0.00378*	0.00343+	-0.0015+	-0.00190*
	(0.00186)	(0.00184)	(0.00074)	(0.000924)
Number of trusts on board × time trend		-0.00004		-0.000002
		(0.00008)		(0.00003)
Seats of major banks × time trend		0.000205		-0.00005
		(0.000346)		(0.000129)
Leverage × time trend		-0.00369		0.00201
		(0.00293)		(0.00124)
Cash/assets × time trend		0.0129		0.0102
		(0.0198)		(0.00795)
Log assets	0.0105	0.00934	-0.00318	-0.00732
	(0.00988)	(0.0110)	(0.00434)	(0.00454)
Constant	-0.153	-0.155	0.105	0.179*
	(0.180)	(0.203)	(0.0804)	(0.0831)
Observations	996	996	687	687
R-squared	0.861	0.891	0.687	0.699
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry-specific trends	NO	YES	NO	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively. The log assets interaction in the second row uses the value of assets in 1906. In columns (2) and (4), the added regressors are observed in 1907, in the case of the board variables, or 1906, in the case of the accounting variables, and interacted with a time trend.

Table 7
Robustness checks on Firm Profitability

	ROA:		ROE:	
	Restricted To Firms w/ Trust Co Affiliations	Common Support; Inverse Propensity Score Weighted	Restricted To Firms w/ Trust Co Affiliations	Common Support; Inverse Propensity Score Weighted
	(1)	(2)	(3)	(4)
Affected trust × post-panic	-0.0627*	-0.0641*	-0.116*	-0.0985
	(0.0251)	(0.0266)	(0.0551)	(0.0607)
Affected trust × post-panic × log assets	0.00328*	0.00343*	0.00586*	0.00510
	(0.00134)	(0.00144)	(0.00295)	(0.00327)
Number of trusts on board × time trend	-0.000036	-0.000005	0.000013	0.00007
	(.000047)	(0.000058)	(0.000096)	(0.000123)
Seats of major banks × time trend	0.000097	-0.000064	0.00002	-0.000396
	(0.000208)	(0.000226)	(0.000383)	(0.000509)
Leverage × time trend	0.00249	0.00241	0.000427	-0.000013
	(0.00189)	(0.00221)	(0.00427)	(0.00434)
Cash/assets × time trend	-0.00517	-0.00208	-0.0211	-0.00745
	(0.00938)	(0.00930)	(0.0179)	(0.0241)
Log assets	0.00350**	0.00257*	0.00517*	0.000514
	(0.00101)	(0.00115)	(0.00200)	(0.00199)
Constant	4.90e-05	-0.000678	0.000786	-0.00113
	(0.001000)	(0.000778)	(0.00183)	(0.00161)
Observations	894	886	879	872
R-squared	0.870	0.837	0.806	0.800
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry-specific trends	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively. In columns (1) and (3), the sample is restricted to firms that have an interlock with at least one trust company. In columns (2) and (4), the sample is restricted to the common support in the propensity to have an affiliation with an affected trust, and the observations are weighted by the inverse of the firms' propensity scores. The log assets interaction in the second row uses the value of assets in 1906. In columns (2) and (4), the added regressors are observed in 1907, in the case of the board variables, or 1906, in the case of the accounting variables, and interacted with a time trend.

Table 8
Robustness checks on Dividend and Interest Rates

	Dividend Rate:		Interest Rate:	
	Restricted To Firms w/ Trust Co Affiliations	Common Support; Inverse Propensity Score Weighted	Restricted To Firms w/ Trust Co Affiliations	Common Support; Inverse Propensity Score Weighted
	(1)	(2)	(3)	(4)
Affected trust × post-panic	-0.0807*	-0.0575+	0.0365*	0.0314+
	(0.0312)	(0.0326)	(0.0174)	(0.0178)
Affected trust × post-panic × log assets	0.00430*	0.00320+	-0.00190*	-0.00174+
	(0.00172)	(0.00185)	(0.000948)	(0.000977)
Number of trusts on board × time trend	-0.00004	-0.000034	-0.000005	0.000033
	(0.00008)	(0.000115)	(0.000036)	(0.000038)
Seats of major banks × time trend	0.000177	0.0000008	-0.00004	-0.000188
	(0.000338)	(0.000431)	(0.000131)	(0.000160)
Leverage × time trend	-0.00583+	-0.00540	0.00330*	0.00330*
	(0.00315)	(0.00405)	(0.00135)	(0.00153)
Cash/assets × time trend	-0.0116	0.00471	0.0199*	0.0142
	(0.0115)	(0.0168)	(0.00799)	(0.01000)
Log assets	0.00337	0.00187	-0.00127+	-0.000933
	(0.00210)	(0.00211)	(0.000683)	(0.000753)
Constant	0.000651	0.00102	0.000391	0.000565
	(0.00194)	(0.00211)	(0.000385)	(0.000416)
Observations	896	899	631	649
R-squared	0.888	0.890	0.714	0.778
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry-specific trends	YES	YES	YES	YES

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively. In columns (1) and (3), the sample is restricted to firms that have an interlock with at least one trust company. In columns (2) and (4), the sample is restricted to the common support in the propensity to have an affiliation with an affected trust, and the observations are weighted by the inverse of the firms' propensity scores. The log assets interaction in the second row uses the value of assets in 1906. In columns (2) and (4), the added regressors are observed in 1907, in the case of the board variables, or 1906, in the case of the accounting variables, and interacted with a time trend.

Table 9
Comparison of Industrials vs. Railroads

	ROA	ROE	Interest rate	Dividend rate
	(1)	(2)	(3)	(4)
Affected trust×post-panic×Industrial	-0.00928*	-0.0262**	0.00526*	-0.00915*
	(0.00468)	(0.00784)	(0.00203)	(0.00425)
Affected trust×post-panic×Railroad	0.00412	0.00408	0.00126	0.000563
	(0.00250)	(0.00516)	(0.00183)	(0.00412)
Log(assets)	-0.00671	0.0275*	-0.00360	0.0108
	(0.00618)	(0.0136)	(0.00432)	(0.00970)
Constant	0.157	-0.430+	0.113	-0.159
	(0.113)	(0.249)	(0.0800)	(0.177)
Observations	994	981	687	996
R-squared	0.863	0.806	0.688	0.883
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively.

Table 10
Assessing the effect of unobservable characteristics: The 1903-04 Recession

	(1) ROA	(2) ROE	(3) Dividend rate	(4) Interest rate
Affected trust in 1907 × 1903-04	0.0343	0.0535	0.0493+	-0.0374
	(0.0425)	(0.115)	(0.0249)	(0.0272)
Affected trust in 1907 × 1903-04 × logassets'04	-0.00190	-0.00326	-0.00280*	0.00208
	(0.00223)	(0.00603)	(0.00130)	(0.00147)
logassets	-0.0183*	-0.0257	-0.000543	-0.00561+
	(0.00887)	(0.0340)	(0.0114)	(0.00309)
Constant	0.358*	0.535	0.0363	0.154**
	(0.161)	(0.618)	(0.207)	(0.0569)
Observations	372	370	370	244
R-squared	0.929	0.863	0.933	0.882
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively.

A1. Data Appendix

Trust Companies

Data on trust company balance sheets and deposits collected from quarterly reports submitted to the New York Superintendent of Banks, as reported in the trade publication *Trust Companies*. The dates chosen for the data presented in Figure 2 for the change in deposits—August 22 as a beginning and December 19, 1907 as the end—represent the two call dates closest to the panic. The data used in the regressions presented in Table 1 are collected from quarterly statements presented in Trust Companies, except for firm age, which was defined as 1907 minus the year of the company's incorporation, which was obtained from the *Annual Report of the Superintendent of Banks Relative to Savings Banks, Trust Companies, Safe Deposit Companies, and Miscellaneous Corporations* (1908).

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

Table A1
Summary Statistics, New York Trust Companies

	All Trust Co's (n = 38)	Trust Co's w/ No Connection to Scandal (n=30)	Trust Co's w/ Direct or Indirect Connection to Scandal (n=8)
Net Worth / Assets	0.215 [0.127]	0.224 [0.137]	0.185 [0.084]
Cash / Checkable Deposits	0.042 [0.013]	0.040 [0.012]	0.049 [0.016]
Stock and Bond Investments / Assets	0.237 [0.154]	0.242 [0.168]	0.218 [0.095]
Log(Total Assets)	16.692 [1.151]	16.664 [1.182]	16.797 [1.092]
Log(Firm Age)	2.233 [1.155]	2.419 [1.149]	2.014 [1.198]
Uptown Headquarters	0.263 [0.446]	0.233 [0.430]	0.375 [0.518]

Trust companies with a connection to the scandal were somewhat younger and had a lower proportion of their assets invested in stocks and bonds (usually representing customer assets in trust accounts). However, they were larger, and held somewhat higher cash reserves. They also had somewhat lower levels of capital relative to their assets.

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. 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 1,068 non-missing Friday prices, and we increase the number of observations to 1,394 by filling in missing Friday data with Saturday's prices. Since this procedure does not alter our results but increases our sample size, we use the filled in data.

To calculate returns, the stock price needs to be adjusted by the dividend payout when common shares go ex-dividend. In order to do so, 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, the par value of common shares is necessary to determine the dollar amount. We use the par values collected from the *Moody's Manuals* for the year 1907.[68] We also use this source to obtain information on 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.

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. We only observe shares outstanding on both dates for 56 out of the 78 firms included in the event study. 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

[68] The Stock Quotes tables reporting Saturday prices also list par values. All values match the ones we obtained from Moody's.

that $\alpha=0$ and $\beta=1$ for every share (Campbell, Lo and MacKinlay, 1997). Because the event date is 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.

A limitation of historical stock market data is that security 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. Thus, it is possible that we have missing returns for some weeks within the event window. To be able to cumulate returns, 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 72 firms. For another 10 firms we observe either one or two returns. We assume that the missing percentage change in price was zero in these 10 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 two firms from the analysis.[69] 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 78 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 – Non-financial 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.

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 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; for example some industrials, and 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. (None of the sample firms in this period had issued zero-par shares.) This is not available for one firm for which ROA is available (accounting for the difference in observations in regressions for the two variables), because it did not disclose its surplus separately from other accounts.

Dividend rate: dividends paid out on common stock/book value of common stock. In general this was quite reliably reported; however in some cases only total dividends (common plus preferred) were disclosed and the amount of common dividends had to be calculated based

[69] 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.

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/long-term debt. For most firms, long-term debt was simply 'bonded debt.' For many firms, this measure is not available because leverage is zero (140 firm-year observations) or the income statement reports it together with other expenses (e.g., "other expenses"). A relatively large fraction of firms disclosed "fixed charges" rather than interest, which in theory could include payments on leases and other related expenses. For those firms, the interest rate in our dataset is actually fixed charges/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. The results presented in the paper are robust to the use of an alternative measure of the interest rate where the denominator is defined more broadly to include those items; statistical precision, however, is reduced from the noise that is introduced into the variable.

Leverage ratio: long-term debt/total assets. See the discussion of the interest rate variable.

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 non-financial 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 non-financial 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.[70] We start by verifying the transcription of a given full name (defined by first and middle

[70] To provide a longer run view of the connections between financial and non-financial 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.

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 non-financial firms, we would ideally use each director's full name. However, the Moody's Manuals report only initials for 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 non-financial 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 non-financials 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 non-financials and commercial banks. On average, each non-financial 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 non-financials 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 non-financial 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.[71] For the 350 individuals correctly identified as creating an interlock, on average the *Directories of Directors* list 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*.[72] Thus, we conclude that our matching procedure is highly accurate.

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 issue might be that the balance sheet variables included as controls in the regressions, and interacted with time trends, were the values from 1906, perhaps too close to the onset of the panic. Instead, it may be preferable to use the earliest available values. Unfortunately balance sheet data are not available for many companies for the years 1903 and before. Therefore, we use the values from 1904 in the regressions, although data for even that year are not available for many companies. For example, using 1904 values would result in the loss of 244 observations in the profitability regressions. In order to keep the sample consistent, we substitute the value of assets from 1905 in cases in which the 1904 value is not available (this is available for 200 of the 244 observations with missing 1904 data) and, where necessary, the 1906 values (which are available for the final 44 observations). The first two rows of Table A2 present the results of specifications identical to those of columns (2) and (4) of Tables 6 and 7, with 1904 values (or the earliest available) for leverage and cash interacted with time trends, rather than the 1906 values.

Another potential source of concern 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 assets between firms with and without affiliations with affected

[71] 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 non-financial firms in the paper is highly reliable.

[72] For example, the 1906 *Directory of Directors* would not list a commercial bank established in 1907, but this firm would appear in the Rand McNally Banker's Directory for 1907.

trust companies, in part B of the table we explicitly include the average growth rate of assets in the years 1903-06 in the regression, interacted with a time trend. Our growth rate of assets variable is available (in the sense that there is at least one year of assets growth data) for companies accounting for all but 57 of the observations in the profitability regressions; thus the sample contracts by 57 observations in those regressions, and by 38 observations in the interest rate regression. Industry-specific trends are also included in the regression.

Finally, one might be concerned that the specification with the assets interaction we have relied upon thus far might not actually be the relevant characteristic for determining the degree to which firms would be exposed to financial frictions. Arguably, firm age would also capture the degree to which a firm was "known" to participants in financial markets, as older firms would likely have longer track records as issuers of securities or borrowers. The final two rows of the table present regressions in which the assets interaction is replace with an interaction with log age, in specifications identical to those of columns (1) and (3) of Tables 6 and 7.

In general, the results of each of these alternative specifications are similar to those of the baseline specification they correspond to. In each case, the estimates for one of the four dependent variables of interest become statistically insignificant. But in every case, the signs and the magnitudes remain relatively similar. We argue that our results are not the product of misspecification.

Table A2
Further Robustness Checks

	ROA	ROE	Dividend Rate	Interest Rate
A. 1904 balance sheet values interacted w/ trends				
Affected trust × post-panic	-0.0616*	-0.106+	-0.0654+	0.0264
	(0.0241)	(0.0541)	(0.0332)	(0.0175)
Affected trust × post-panic × log assets	0.00326*	0.00536+	0.00351+	-0.00130
	(0.00130)	(0.00291)	(0.00182)	(0.000965)
B. Growth of assets '03-'06 x trend, industry trends				
Affected trust × post-panic	-0.0457+	-0.0853	-0.0591+	0.0304*
	(0.0254)	(0.0559)	(0.0301)	(0.0151)
Affected trust × post-panic × log assets	0.00243+	0.00432	0.00308+	-0.00158+
	(0.00136)	(0.00300)	(0.00164)	(0.000812)
C. Age rather than assets interaction				
Affected trust × post-panic	-0.00845	-0.0293*	-0.0116+	0.00715*
	(0.00641)	(0.0113)	(0.00640)	(0.00278)
Affected trust × post-panic × log age	0.00306	0.00875*	0.00340+	-0.00164*
	(0.00189)	(0.00354)	(0.00201)	(0.000802)

1903-04 Recession

One potential source of concern regarding the test of firm performance 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 effects of a shock then they had been earlier. Our placebo test using the earlier recession would be invalidated if the fundamental characteristics of non-financial firms changed substantially in the four-year period between financial crises. Two important 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 vulnerable to the 1907 panic in a way that they were not during the 1904 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 A3.

Table A3
Firms involved in Mergers and Antitrust Cases Deleted

	(1) ROA	(2) ROE	(3) Dividend rate	(4) Interest rate
Affected trust × post-panic	-0.0851**	-0.200**	-0.108*	0.0347*
	(0.0316)	(0.0704)	(0.0508)	(0.0155)
Affected trust × post panic × logassets'06	0.00465**	0.0107**	0.00590*	-0.00174*
	(0.00171)	(0.00389)	(0.00285)	(0.000853)
Logassets	-0.000251	0.0328+	0.0114	-0.00196
	(0.00799)	(0.0173)	(0.0106)	(0.00459)
Constant	0.0398	-0.518	-0.171	0.0822
	(0.143)	(0.312)	(0.191)	(0.0836)
Observations	789	778	795	547
R-squared	0.860	0.779	0.880	0.695
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Standard errors adjusted for clustering by firm in parentheses. **, *, and + indicate significance at the 1, 5 and 10 percent level, respectively.

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 or the tarnished reputation of one of their directors.