The Conditional Nature of Embeddedness: A Study of Borrowing by Large U.S. Firms, 1973-1994^{*}

Mark S. Mizruchi

University of Michigan

Linda Brewster Stearns

Southern Methodist University

Christopher Marquis

University of Michigan

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ABSTRACT

Economic and organizational sociologists have increasingly demonstrated that the actions of individuals and firms are affected by the social networks within which they are embedded. In recent years scholars have begun to recognize that the effects of these social networks may vary across populations or types of relations. In this paper we examine the extent to which the effects of interfirm networks on the behavior of firms are historically contingent. Focusing on the level of debt financing among approximately 140 large U.S. corporations over a 22-year period, we show that the extent to which the firms' use of debt was influenced by those with which they were tied through director interlocks declined over time. We argue that this decline in the network effect reflected a shift in the institutional environment within which the firms operated and that it was driven by three processes: the professionalization of the finance function within the firm, the internalization of financial decision making, and the increased volatility of the environment. We conclude that corporate financing is socially embedded, but this embeddedness is historically contingent.

The idea that economic action can only be fully understood by an examination of the social relations within which actors are embedded has become a widely accepted staple of sociological thought (Granovetter 1985). In the past two decades, sociologists and organizational theorists have provided a broad range of support for this formulation. At the firm level, researchers have demonstrated the effects of interfirm ties on a range of firm strategies and outcomes, including mergers and acquisitions (Haunschild 1993; Palmer and Barber 2001), adoption of the multidivisional form (Palmer, Jennings, and Zhou 1993), takeover defense strategies (Davis 1991), and firm survival (Uzzi 1996). At the individual level, researchers have shown that the structure of an actor's personal network can affect his or her ability to achieve rapid promotion (Burt 1992; Podolny and Baron 1997) as well as success in task performance (Mizruchi and Stearns 2001).

These studies have gone far in demonstrating that networks matter, but they have contained the seeds of something more: that the extent to which networks matter varies across actors and situations. Burt (1997), for example, showed that the sparse personal networks that facilitated the rapid promotion of male managers had the opposite effect for women. Women experienced greater upward mobility when they attached themselves to an older male sponsor who could confer legitimacy on them. Podolny and Baron (1997) suggested that whether a sparse or dense personal network was helpful to a manager varied depending on the type of network. For ties based on the exchange of information and resources, sparse networks yielded superior mobility outcomes but for ties based on normative expectations and social support, dense networks produced greater success. At the firm level, Uzzi (1996) showed that the effect of strong social ties with one's business partners was associated with firm survival, but only up to a point. Beyond a certain threshold, social ties became a deterrent to survival. And Haunschild and Beckman (1998) showed that the extent to which director ties affected corporate acquisitions varied depending on the presence of alternate sources of information.

The above studies have shown that network effects differ for different groups of actors, and that the effects of network structures vary depending on the content and/or nature of the tie. What has not been established is that the effects of network structures on members of a group may vary *over time*. That is, it is possible that even within a

particular type of network and a particular population of actors, the effect of the actors' networks may differ across periods, and under different institutional environments. In this paper we argue that the effect of interfirm social networks on firm behavior is historically contingent. Our site for this test is the use of debt financing by large American corporations over a 22-year period, 1973 through 1994. As we show, the effects of social network ties on firm financing differ across different periods. We argue that the variations in these effects over time were a consequence of three significant changes in the environment within which the firms operated: the professionalization of the finance function within the firm, the internalization of financial decision making, and the increased volatility of the environment. We then test hypotheses designed to identify the effects of these changes.

THE HISTORICAL CONTINGENCY OF ORGANIZATIONAL ACTION

Although the issue has generated controversy dating back to the days of Durkheim and Weber, the goal of most sociological theorizing and research has been to develop explanations for social behavior that transcend the particular case under investigation. Few sociologists believe that their findings reflect universal properties that apply regardless of time or place, but most hope that they have at least some degree of generalizability. In recent years, however, a number of sociologists have issued warnings against approaches that, in their view, attempt to develop universal laws based on a comparison of historical cases (a partial list includes Isaac and Griffin 1989; Sewell 1996; Steinmetz 1998; and Paige 1999). They argue that causal forces are historically contingent—operative under some conditions but absent under others. One need not agree with the idea of abandoning general theories to acknowledge that the effects of particular variables may vary across time. A focus on the contingent nature of causality is compatible with both general and more idiographic forms of theorizing.

Within the study of organizations, the acknowledgement of historical contingency has been most prominent within the neo-institutional perspective (for a review of this literature, see Schneiberg and Clemens 2005). An important early example of this was a

study by Tolbert and Zucker (1983). Examining the adoption of civil service reform by city governments in the late nineteenth and early twentieth centuries, Tolbert and Zucker found that in the early years, from 1885 to 1904, adoption was predicted by factors related to reducing conflict and increasing the efficiency of city government. In later years, especially after 1925, however, these factors no longer had an effect on adoption. This suggested that civil service reform had become institutionalized and that its adoption had become a taken for granted mode of operation.

In recent years the idea that the effects of organizational variables vary over time has received increasing attention. In particular, researchers have focused on the adoption of alternative models, or worldviews, in different periods, and the ways in which these worldviews have altered the determinants of firm behavior. In a study of railroad mergers in Massachusetts, for example, Dobbin and Dowd (2000) suggested that prior to 1897, when the U.S. government began to enforce antitrust legislation, railroads behaved according to a "cooperative model," in which cartels were the preferred market strategy. After 1897, the focus shifted to a "finance model," in which financiers recommended a strategy of friendly mergers. Dobbin and Dowd showed that the predictors of both the purchase and sale of railroads varied across time, after the shift from the cooperative to the finance model. In a study of acquisitions by large firms in the 1980s, Davis, Diekmann and Tinsley (1994) showed how a shift in accepted interpretations of the nature of firm boundaries influenced the types of acquisitions in which firms engaged. As business rhetoric focused increasingly on the importance of organizations' core competencies, firms became less likely to acquire companies in unrelated industries. Thornton and Ocasio (1999) documented a shift in the academic publishing industry from what they termed an "editorial logic," prevalent in the 1950s and 1960s, to a "market logic," beginning in the mid-1970s. This shift resulted in changing patterns of executive succession. And Scott, Ruef, Mendel, and Caronna (2000) showed how governance patterns changed to correspond with three institutional eras that characterized the healthcare industry between 1945 and 1995.

Especially relevant for our study is a recent article by Zajac and Westphal (2004), who argue that the American business environment underwent a shift during the 1980s. In the 1970s, they suggest, firms operated according to a "corporate logic," with a focus

on autonomous professional managers oriented toward the stability and growth of their firms. In the 1980s, Zajac and Westphal argue, there was a turn toward an "agency logic," in which managers were viewed as opportunists with no particular expertise, and the focus of managerial actions shifted toward increasing the firm's stock price. This corresponded with the rise and proliferation of agency theory, which, according to Zajac and Westphal, had a significant influence on corporate behavior during this period. Consistent with this argument, Zajac and Westphal note that during the 1970s, corporate proxy statements submitted to the Securities and Exchange Commission focused primarily on identifying and recruiting top executive talent, while the focus during the 1980s shifted toward the alignment of stockholder and management interests. The authors show that investors' reactions to stock repurchase announcements moved from significantly negative in the early 1980s to significantly positive by the late 1980s and early 1990s. Although this analysis does not identify changes in causal effects over time, it does provide evidence of a changing institutional environment, and shows that this change in the environment led to a shift in the behavior of investors.

The historical argument advanced by Zajac and Westphal serves as a backdrop to our own model. Examining the financing behavior of approximately 140 American corporations over a 22-year period, we argue that the environment within which firms made their decisions shifted over time. As we suggest, and demonstrate, the changes in this environment affected the degree to which interfirm network ties influenced firm financing behavior. In the following section we discuss our research site—firm financial strategies—and the role of interfirm social networks in these strategies. Following that, we develop a model in which we predict, and attempt to explain, the historical contingency of these network effects on firm financing behavior.

CORPORATE FINANCING AS AN ORGANIZATIONAL STRATEGY

Although economic sociologists and organizational researchers have become increasingly bold in terms of the firm strategies they have studied, there are some issues that are assumed to remain the purview of economists, and have therefore attracted little

attention. One of these issues involves the ways in which firms manage their capital, that is, the basis on which firms determine their financing strategies. A small but growing literature on financing has recently emerged in sociology. Most of this work involves the analysis of credit and the effectiveness of credit-rating systems (Carruthers and Cohen 2001; Guseva and Rona-Tas 2001) or the acquisition of venture capital (Podolny 2001; Sorenson and Stuart 2001, although see Keister 2001). Uzzi (1999) has examined the determinants of whether "mid-market" firms gain access to capital, as well as the interest rate on the funds they borrow. He has shown that the social relations between firms and their banks have significant effects on both of these variables. While Uzzi's concern is with whether middle-sized firms are able to acquire capital and if so, the price they pay for it, our study examines the largest American corporations, for whom access to capital is less problematic. We focus on firms that are able to borrow, and for whom the level of external financing is a strategic decision.

Why Study Financing?

All firms, regardless of industry, require capital. If firms had sufficient levels of cash generated from retained earnings, there might be no need to raise external funds. Firms could borrow when interest rates were favorable, while investing their cash in alternative outlets, or they could use their cash for expansion and eschew external financing altogether. The extent to which American corporations have depended on external financing was the subject of debate for much of the twentieth century (Berle and Means, [1932] 1968; Lintner 1959; Mintz and Schwartz 1985). Most observers now acknowledge that this dependence has fluctuated over time (Stearns 1986; Stearns and Mizruchi 1993a). Regardless of how much external financing firms require, it is clear that they engage in a substantial amount of it. Between 1984 and 1990, for example, U.S. nonfinancial corporations issued a net total of \$1.2 trillion worth of debt (Remolona et al. 1992).

Corporations can raise external capital in a number of ways, and the types and complexity of financing have increased significantly in recent years. Traditionally, three mechanisms have pervaded: equity, short-term notes, and long-term bonds. In more

recent years, alternatives such as commercial paper have become increasingly common. Although different forms of external financing may have different purposes and different sources, each is ultimately the result of a decision by the firm's managers. Just as managers make decisions on whether to acquire another firm, relocate a production facility, or adopt an alternative organizational structure, they also make decisions on how they will finance their activities. If adoption of the multidivisional form is a strategy, so is the use of debt as opposed to retained earnings. As Barton and Gordon noted, "the question of how to finance the firm... represents a fundamental functional (financial) decision which should support and be consistent with the long-term strategy of the firm" (1987:67).

In a series of articles based on an analysis of 22 firms from 1955 through 1983, Stearns and Mizruchi (1993a; 1993b; Mizruchi and Stearns 1994) showed that firms' use of debt was strongly affected by a series of financial variables, including retained earnings and the firms' anticipated return on future investment. At the same time, they found that the level of debt was also accounted for in part by the presence of representatives of financial institutions on the firms' boards of directors. Mizruchi and Stearns (1994) showed that firms that had representatives of financial institutions on their boards used higher levels of external financing than did firms without financial representation on their boards. Similarly, Stearns and Mizruchi (1993a; 1993b) showed that the specific type of external financing a firm used, whether short-term debt, longterm public borrowing, or long-term private borrowing, could be accounted for by the specific type of financial representative who sat on the firm's board. These findings were consistent with the view that firms' social network ties within the business community can have an independent effect on their economic behavior.

The Stearns-Mizruchi studies contained two important problems, however: First, the social network effect, which the authors sought to demonstrate as a means of showing that financing decisions were socially embedded, was tested in an indirect manner. The authors assumed that the presence of a financial representative on a firm's board of directors conveyed information that led to a single, specific strategy: the use of higher levels of financing. It is certainly possible that the bankers on a firm's board will advise the firm to borrow, in the same way that surgeons have a tendency to recommend

surgery. On the other hand, there is no assurance that bankers will recommend higher levels of debt.¹ Moreover, focusing only on the presence of financial representatives on a firm's board does not address the issue of whether a firm's borrowing, high or low, is affected directly by the behavior of the firms to which it is socially tied. Two bankers, each of whom sits on the board of a different firm, might suggest very different strategies for the two firms. Firm financial decisions could also be affected by the behavior of the nonfinancial peers to which they are tied, a variable the authors did not examine.

The second problem with the Stearns-Mizruchi studies was a theoretical and epistemological one: Although the authors had time-series data covering a 28-year period (1956 through 1983) and although they examined the effects of several timespecific variables (such as interest rates and the stage of the business cycle), they assumed that the effects of both their financial and social embeddedness variables were constant over time, that is, invariant to changes in the historical context. Yet the period between the 1950s and the 1990s, on which the earlier and current studies were based, saw a number of changes in the environments within which American firms operated. As we saw in our earlier discussion of the neo-institutional literature, there might be reason to question whether the determinants of external financing remained constant over time.

In this paper we address both of the above-mentioned limitations of the earlier sociological work on corporate financing. We use a network autocorrelation model to allow us to directly examine the effects of interfirm social networks on corporations' financing behavior. And we develop hypotheses that allow us to test for historical contingency in the effects of social network ties on corporations' use of debt over a 22-year period, from 1973 through 1994. This approach allows us to combine the recent focus on the contingent effect of social networks with the neo-institutional emphasis on the historical bases of this contingency.

THE HISTORICAL CONTINGENCY OF NETWORK EFFECTS

¹ One recent study suggests that for poorly performing firms, the presence of lending bankers on firms'

The period of our study, from the early 1970s through the early 1990s, witnessed a number of changes in the environment within which large corporations operated. American corporations' use of external financing increased significantly beginning in the mid 1960s, and remained high into the early 1980s (Stearns 1986). Beginning around 1983, firms' dependence on banks for their financing began to decline as firms found alternative sources of capital, and banks began to shift their focus away from lending (Davis and Mizruchi 1999). It is not clear if, or if so, how, these changes would affect the determinants of borrowing. There is preliminary evidence, however, that changes in the effect of network ties on borrowing may have changed over time. In a dyadic analysis of financing behavior in five years—1973, 1978, 1983, 1988, and 1993—Mizruchi and Stearns (2003) found that in the years through 1983, pairs of firms that shared directors had more similar levels of borrowing than did pairs of firms that did not share directors. By 1988 this effect had disappeared, and it remained nonexistent in 1993.

The fact that this study focused on only five years—in five-year intervals—means that it is difficult to tell if Mizruchi and Stearns identified an actual trend, or just random yearly fluctuations. It does suggest the possibility that a decline in the network effect actually occurred, however. And to the extent that this decline was real, it raises the question of why it occurred. We argue that the institutional context within which large American firms operated shifted as we moved from the 1970s into the 1980s, and that this led social networks across firms to have less of an effect on firm financing decisions as time progressed. Three changes in particular occurred during this period that, we suggest, had the consequence of reducing the effect of interfirm networks on financing: First, the finance function within the firm was professionalized. Second, in part as a consequence of the first, firm financial decision making was internalized. And third, the external environment within which firms operated became increasingly volatile. We discuss each of these in turn.

Professionalization. From the period after World War II to about 1975, corporate finance, if not simple, was a relatively straightforward operation. As Stearns (1986) has shown, firm dependence on external capital fluctuated during this period, but

boards is associated with relatively low levels of debt (Byrd and Mizruchi 2005).

the alternative ways in which firms could raise their capital remained generally constant during the entire period. There were three primary ways in which firms raised debt: through short-term debt (handled primarily by commercial banks), long-term private bonds (handled primarily by life insurance companies), and long-term public bonds (handled primarily by investment banks).²

Decisions about major debt financing programs were generally made at the top levels of the firm, usually by the president/CEO, often in consultation with the board. Financial officers had titles such as "treasurer," "comptroller," or, in some cases, "vice president for finance," and were typically not highly placed within the firm hierarchy. As Zorn (2004) notes, these officers were involved primarily with bookkeeping and budget monitoring, but they were rarely involved in high level decision making. Although they might be consulted about various financing strategies, the actual decisions tended to be made at the top. As for the nature of financial decision making itself, compared to what occurred in later years, these decisions were made with considerable deliberation, as, for example, General Motors' decision to raise debt to finance its expansion in the late 1940s (Freeland 2001).

As we moved into the late 1970s, however, two related developments occurred. The first was the continued development of what Fligstein (1990) called the "finance conception of control." According to Fligstein, the twentieth century witnessed a series of shifts in the predominant view of the most efficient way to organize and operate a firm. A focus on the productive process predominated in the early part of the century, followed by an emphasis on sales and marketing through the 1950s. By the 1960s, firms began to focus less on what they produced or how they distributed it, and more on accumulating profits by whatever means possible, without regard to industry or product. Firms were now viewed as a "bundle of assets," and the focus was on the financial

² Equity, the issue of stock, is another possible means of raising capital. Among the largest American corporations, equity has not been a dominant form of financing, accounting for no more than 15 percent of long-term financing in the United States between 1945 and 1980 (Stearns 1986) and for no more than 17 percent during the 1980s. Even with stocks at historically high prices, equity equaled less than 18 percent of corporate long-term financing between 1990 and 1999. Mizruchi and Stearns (1994) found a marginally significant positive association between the issue of new equity and the issue of new debt. Whether equity was included or excluded from the measure of external financing had no effect on the outcome of the

balance sheet. As evidence of this phenomenon, Fligstein shows that the proportion of CEOs with backgrounds in the finance and accounting wing of the firm increased sharply after 1960 and continued through the 1970s. Only 10 percent of the large firms in Fligstein's sample had finance CEOs during the 1950s. This figure rose to 21 percent during the 1960s and 28 percent in the 1970s.

A development that corresponded with the increased emphasis on finance within the firm was the rise of a new functionary, the chief financial officer (CFO). As Zorn (2004) shows, the title, first introduced in the 1960s, was rare into the mid 1970s but began to diffuse rapidly after 1979. By the mid 1990s, more than 80 percent of the large firms in Zorn's sample had adopted a CFO. Our data revealed a similar pattern. As shown in Figure 1, fewer than two percent of the firms in our sample had CFOs in 1973. By 1994 more than 65 percent of our firms had CFOs.

FIGURE 1 ABOUT HERE

Unlike the treasurer/comptroller described above, the CFO was a significant player in firm decision making. As Zorn puts it (2004:347), "CFOs gained critical say in key strategic and operational decisions, from evaluating business unit performance, inventing new ways to leverage capital, managing acquisitions and divestitures, and fending off hostile takeover attempts, to serving as the company's primary ambassador to investors and financial analysts." The elevation of the CFO signaled the professionalization of the finance functionary as a key player in the firm. Rather than merely keeping track of the financial consequences of decisions that had been made by others, the new finance executive was a central participant in those decisions.

Internalization. Because financing decisions in the post World War II period were typically made at the top levels of the firm, by generalists, large corporations frequently invited officers of major financial institutions, especially commercial banks, to sit on their boards of directors. In the early twentieth century these bank representatives often played a control function, as when J.P. Morgan placed several officers of his

analysis, however, nor did its inclusion in equations predicting the use of debt have an effect on the coefficients of the other variables. We focus on debt financing in the current paper.

investment bank on the boards of U.S. Steel and International Harvester. Even in more recent years these bankers were occasionally driven to play a monitoring role during periods of crisis (Mintz and Schwartz 1985). Their primary role in the postwar period was to lend financial advice to the firm's managers, however, as well as to provide prestige for the firm.

Bankers on a firm's board represented a class known as outside directors, those whose primary affiliations were with organizations other than the firm in question. These directors were often appointed as a means of providing legitimacy, by signaling to both the investment community and the larger public that the firm was a responsible social actor (Parsons 1960; Selznick 1957). During the 1950s and 1960s, internal corporate crises were relatively rare in the United States, and boards, although occasionally stepping in to oust and replace a firm president, were relatively inactive, except as advisors. In the wake of the 1970 bankruptcy of the Penn Central Railroad, as well as the public suspicion of major American institutions that accompanied the Watergate Scandal of the early 1970s, many firms began to appoint increasing numbers of outside directors to their boards. Between the early 1970s and the mid 1990s, the average proportion of outside directors on the boards of the large American corporations in our data rose steadily and consistently, from 53.2 percent in 1973, to 64.9 percent in 1983, to 72.2 percent in 1994. Despite the rise of outside directors in general, however, the presence of representatives of financial institutions experienced an equally steady and consistent decline during the same period. Among the outside directors in the data cited above, 27.6 percent were principally affiliated with a financial institution in 1973, 18.6 percent in 1983, and only 12.7 percent in 1994. As shown in Figure 2, the proportion of financial representatives among all board members dropped continuously, from 14.7 percent in 1973 to 9.2 percent in 1994. This decline occurred simultaneously with the ascendance of the CFO.

FIGURE 2 ABOUT HERE

The question for our purposes is what relevance do the rise of the CFO and the decline of bankers on the board have for the effects of interfirm network ties on firm

financing behavior? The answer has to do with the nature of these network effects, that is, the process by which they occurred. The argument made by Stearns and Mizruchi in their studies of network effects on financing (Stearns and Mizruchi 1993a; Mizruchi and Stearns 1994; 2003) is that firm financial strategies often diffused through interlocks among the firms' boards of directors. As board members discuss issues of relevance to the firm, those who sit on the boards of other firms have experiences and insights from which they can draw. These board members might discuss financial strategies, either at board meetings or in informal settings, both concurrent with and apart from the board. As Haunschild (1993) has shown, firms whose CEOs sat on the boards of firms that had recently engaged in acquisitions were disproportionately likely to engage in acquisitions themselves, presumably in part because such acquisitions were discussed at board meetings. Mizruchi and Stearns suggested that firm financial strategies might have diffused through a similar process.

The rise of the CFO, coupled with the decline of bankers on the board, suggested a shift in this process, however. The increased prominence of the CFO meant that the CEO now had a financial specialist to consult on a regular basis. Bankers on the board as advisors were no longer as essential, which may account for their decline. Moreover, the financial tools available to the firm were becoming increasingly complex, and reliance on traditional sources of funding from banks and insurance companies declined. As we entered the 1980s, a range of new financial sources emerged, the most prominent of which was commercial paper, in which firms borrowed directly from one another (Davis and Mizruchi 1999). Meanwhile, financial decision making became increasingly complex, both a consequence of the professionalization of the financial function and a cause of its further ascendance. As a National Research Council official noted in 1995, "Corporate finance used to be glorified accounting, but now it's applied mathematics" (Jackson 1995).

The preceding discussion suggests that as we moved from the 1970s into the 1980s, financial decisions were increasingly made by highly placed specialists within the firm. It simultaneously suggests that the role of financial representatives on firm boards was reduced over time, and that these board members therefore had an increasingly smaller role in the determination of firm financial strategies. To the extent that the

diffusion of behaviors across firms occurred through director interlocks, it follows that these ties would have a declining impact on firm financing decisions as a result of the professionalization and internalization processes.

The preceding discussion suggests the following hypotheses:

H1: The effect of interfirm network ties on firms' use of debt declined over time.

H2: The effect of interfirm network ties on firms' use of debt is negatively associated with the prevalence of chief financial officers.

H3: The effect of interfirm network ties on firms' use of debt is positively associated with the prevalence of representatives of financial institutions on the firms' boards.

Volatility. While the company finance function was becoming professionalized and internalized, another key event occurred that also affected the nature of financing: the merger/takeover wave of the 1980s. The number of mergers among private, for-profit firms in the American economy increased steadily from the end of World War II into the mid 1960s, then increased sharply in the late 1960s before declining between 1970 and 1974. After 1974 the number again increased steadily, before exploding during the mid 1980s and leveling off around 1990. The trends during the period of our study are illustrated in Figure 3. The 1980s merger wave was far from the first (see Stearns and Allan 1996; Golbe and White 1988, for discussions of the four twentieth century waves), but it was unprecedented in the extent to which corporate managers faced the risk of losing control of their firms. As evidence of this, nearly one-third of the Fortune 500 largest manufacturing corporations disappeared during the decade. Not only did managers experience genuine vulnerability during this period, but the enormous publicity given to the wave of takeovers, hostile and otherwise, ensured that managers faced greater perceived vulnerability as well.

FIGURE 3 ABOUT HERE

In the context of our discussion of firm financing, two aspects of the 1980s merger wave are relevant. First, in keeping with the rise of agency logic described by Zajac and Westphal (2004), the idea that takeovers were both warranted and economically beneficial became increasingly popular within the business community (Schneper and Guillen 2004). In this view, firms whose stock was defined as "undervalued" were seen as inefficient, and thus ripe for acquisition. Corporate raiders used this narrative to justify their claims to their target firms (Hirsch 1986), and they found a receptive audience among the stockholders who held the firms' low-priced shares. At the same time, large institutional investors became considerably more active in monitoring companies than they had been during the 1970s. In particular, as Useem (1996) notes, these investors pressured managers to adopt reforms aimed at increasing stockholder returns.

Contributing to the siege mentality experienced by corporate managers was the fact that a target firm's stockholders benefit from the firm's acquisition. Historical estimates of the stock price increases of target firms suggest a gain of about 20 percent in mergers and 30 percent in tender offers (Jensen and Ruback 1983). Firm managers adopted a two-pronged strategy in response to these conditions. On one hand, they instituted takeover defense policies (Davis 1991). On the other hand, they accepted the emergence of agency logic. Managers' primary orientation now became the maintenance of the firm's stock price (Zajac and Westphal 2004), what Fligstein (2001) has referred to as the "shareholder value" conception of the firm. This new approach demanded that managers be able to justify their decisions, including financial ones, according to objective, formulaic criteria. No longer could managers base significant decisions about how and how much to finance their investments on the informal social relations in the ties among their firms' directors. Instead, every decision had to be justified, to both the stockholders and the investment community, in terms of clear, systematic, financial criteria. Financing decisions were now made inside the firm, but they were made under increasing pressure from external forces.

The second important consequence of the merger wave for our argument is what it did to the nature of financing itself. In calmer times, financing decisions were often made deliberately, with consultation among various parties not only inside the firm, but on the board as well. The 1980s merger wave greatly increased the speed with which financing decisions had to be made. A prototypical example of this is described in the book Barbarians at the Gate (Burrough and Helyar 1990), which recounts the \$25 billion contest for control of RJR Nabisco that occurred in the fall of 1988. The three parties to the quest worked furiously with financers, often on a moment's notice, over a six-week period, to assemble sufficient levels of capital as conditions of the bid changed daily. As Henry Kravis (of Kohlberg Kravis Roberts), one of the parties in the conflict put it, "We were charging through the rice paddies, not stopping for anything, and taking no prisoners" (quoted in Burrough and Helvar 1990:253). Under conditions such as these, firm managers had no time to think in terms of long-term strategies, nor could they wait until their board was assembled to solicit advice. Instead, these situations required quick decision making, and the process was typically handled by insiders, without influence from the social networks that linked the firm's board to others.³

The increased volatility of the environment within which firms operated during the 1980s thus led to a situation, we argue, in which interfirm networks played a reduced role in the determination of firm financial strategies. This suggests the following hypothesis:

H4: The effect of interfirm network ties on firms' use of debt is negatively associated with the volatility of the environment in which firms are operating.

The institutionalization of volatility. We have suggested that when firms are operating in a volatile external environment, their financing behavior is less likely to be influenced by their interfirm network ties. Our discussion suggests an additional factor

³ We are not suggesting that social networks were unimportant in this process. Those competing for control of a firm often made use of contacts within the financial community, especially when funds had to

that might have played a role in reducing the effect of interfirm networks: the possibility that the 1980s represented a period with a distinct institutional form; that is, during the decade, volatility became a taken for granted characteristic of the corporate environment.

The shift from a corporate to an agency logic (Zajac and Westphal 2004) and the rise of the shareholder value conception of the firm (Fligstein (2001) suggest that corporate managers faced a qualitatively different external environment, dating from the mid-1980s, from what they had faced in prior years. We have hypothesized that a high level of volatility in the corporate environment would reduce the effect of interfirm networks on firm financing decisions. If the managerial environment was actually subject to a distinct set of cultural dictates in this later period, then the effect of environmental volatility should be greater in those years. In other words, if our argument about the changing nature of the corporate environment is correct, then the effect of environmental pressures on depressing the network effect should be increasingly negative over time, as the perception of firms as potentially vulnerable to acquisition became institutionalized. This suggests the following hypothesis:

H5: The effect of volatility in the environment on the effect of interfirm network ties on firms' use of debt will be increasingly negative over time.

DATA, RESEARCH DESIGN AND VARIABLES

The data for our analysis are derived from a 40-year time series, collected as part of a larger project on the determinants of corporate financing among large American corporations. Our concern was with the behavior of the largest corporations, those that have the most influence on economic activity and are likely to have the highest level of discretion in their financial strategies. We began with the 200 largest manufacturing firms in the United States in 1955, the first year in which *Fortune* compiled its list of the 500 largest corporations. These 200 firms were followed yearly, through 1994. During

be raised quickly. Our point is that the financing decisions by the firms themselves were typically made internally, and quickly, with little opportunity to consult with board members.

the 40-year period, 80 of the 200 original firms disappeared, either through bankruptcy or, more often, through acquisition by other firms. Most of these disappearances occurred after 1980. Had every firm survived for the entire period, and had we complete data on every firm for every year, the data set would have included 8,000 company-year observations. The disappearance of firms and other missing data left us with a final total of 6,088 company-years.

Because we have a sample of firms repeated yearly, the logical unit of analysis for our study is the company-year. Because of the estimation technique that we used to examine the effects of our social networks, it is not possible to pool the data across years. Instead, we examined a series of cross-sections, one for each year of our study. The units of analysis are thus firms within a particular year, and we conduct separate analyses for each year. We explain our reasons for this below. The tests of our hypotheses are based on a time-series analysis in which our units are individual years, from which we compare the effects of particular variables over time. We also explain this below.

Although our data originated in 1955, we have missing information on several key variables prior to 1970. Because one of our key control variables requires the examination of data three years prior to the year in question, we begin our analysis in 1973. The analysis involves data from every year consecutively, through 1994, for a total of 22 individual time points. The number of firms in our analysis was 137 in the first year, 1973, and rose as high as 145 (in 1975).⁴ By 1994, the number of firms in our analysis had declined to 85. We address the consequences of this issue in more detail below.

To test our hypotheses it is necessary to proceed in two steps. First we have to identify the network effects on financing. Then we have to account for variation in these effects over time. The first component of our analysis therefore uses firms' level of debt as the dependent variable. Once we have identified the network effects on debt, we then

⁴ The number of firms in the analysis occasionally increased from one year to the next. The reason for this is that the algorithm that we used to compute our network effects required the inclusion of only firms that had at least one direct interlock with another firm in the sample. A previously isolated firm that had been excluded from the analysis in a given year would enter (or re-enter) the analysis in a subsequent year when it established a new interlock. The small number of cases in which this occurred ensured that this had virtually no effect on our results.

turn to the second step, in which we test the above hypotheses using the size of the network effect as the dependent variable.

The identification of network effects on firms' use of debt requires an approach in which we consider, and control for, a broad range of additional factors. To identify these additional factors, we draw on a model of the determinants of debt financing advanced by Mizruchi and Stearns (1994). In the current paper we are less concerned with testing this model than with identifying network effects. At the same time, we include the variables posited by Mizruchi and Stearns, as well as a series of control variables that they included in their analyses, to ensure that our network effects are not spurious consequences of alternative variables.

Drawing on both neo-institutional and network perspectives as well as the financial economics literature, Mizruchi and Stearns identified four variables as key sources of borrowing: the availability of internal funds; the anticipated return on borrowing; the strategic orientation of the firm; and the embeddedness of the firm's decision making apparatus.

The availability of funds was operationalized in terms of retained earnings. A number of theorists, ranging from organizational and political sociologists (Pfeffer and Salancik 1978; Mintz and Schwartz 1985) to transaction cost and finance economists (Myers 1984; Williamson 1988) have suggested that ceteris paribus, firms would prefer to finance their investments with retained earnings rather than debt. This suggested that there would be a negative association between a firm's retained earnings and its use of new debt. The anticipated return on borrowing was operationalized as the expected future return on investment minus the cost of capital for that firm. Mizruchi and Stearns hypothesized that the greater the anticipated return, the higher the level of borrowing. The third factor, the strategic orientation of the firm, was based on Fligstein's (1990) conception of control argument discussed earlier. In Fligstein's view (1990:15), firms whose CEOs originated in the finance or accounting wing of the firm would be more likely to engage in acquisitions, and would therefore be likely to use higher levels of debt. And finally, Mizruchi and Stearns operationalized the embeddedness of the firm's decision making apparatus in terms of the number of representatives of financial institutions who sat on the firm's board. They hypothesized that this number would be

positively associated with the firm's use of debt, both because financial representatives encouraged the use of debt and because they provided access to it. In an analysis of the borrowing behavior of 22 large American firms from 1955 through 1983, Mizruchi and Stearns found support for three of the four hypotheses. Retained earnings were negatively associated with borrowing, and the presence of a CEO from a finance background and the presence of financial representatives on the firm's board were positively associated with the level of borrowing. Only the anticipated return variable behaved contrary to expectations—negatively, as opposed to the predicted positive effect. This negative effect may have been due to the fact that the measure of anticipated return contained (and was almost perfectly correlated with) the firm's recent profitability and growth. Well performing firms in general tended to use lower levels of debt.

In the present study we include all four of these variables, although we modify the anticipated return variable to measure the firm's recent performance, without regard to the cost of capital. The firm's prior profitability and growth were correlated almost perfectly with the measure of anticipated return, and yielded identical substantive conclusions. In addition to these four variables, we include the control variables examined by Mizruchi and Stearns. These include firm size, the primary industry within which the firm operates, the firm's prior debt ratio (roughly corresponding to a lagged dependent variable), and the extent to which a firm issued stock for the purpose of an acquisition.

Our dependent variable, firm borrowing, is the firm's new long-term debt and notes payable acquired in a given year. We standardized the variable by the firm's total assets. Retained earnings were computed as the sum of the firm's net income minus the sum of preferred and common dividends. This variable was also standardized by total assets. CEO background was treated as a dummy variable, coded 1 for background in finance or accounting and 0 for background in other areas. Financial representation on the board was coded as the number of individuals on a firm's board whose primary affiliations were with financial institutions. Our network variable, described in the following section, was based on board of director overlaps between the firms in our data set.

Firm size was computed as total assets. Debt ratio was computed as the firm's long term debt plus current liabilities, divided by total assets. Because both of these variables were highly right-skewed, we converted their values to logarithms (base e). The firm's recent performance was computed, following Mizruchi and Stearns (1994), as the product of the firm's mean profitability (return on assets) and growth (change in assets over prior assets) over the three years prior to the year in question. Industry dummy variables were created based on the firms' primary (two-digit) industries, as defined by Standard and Poor. The vast majority of firms in our data were clustered in five primary industries: food; printing, publishing, and allied industries; petroleum refining; primary metals; and transportation equipment. The remaining industries did not have enough cases to warrant the creation of separate industry variables. We therefore created dummy variables for the five most common industries and treated the remaining firms as members of the reference category. The industries that predominated in our data set reflect the heavy manufacturing economy that was prevalent in the United States during the 1950s. A sampling of the largest firms under current conditions would undoubtedly yield a different distribution. Any attempt to sample from more recent lists of the largest companies would have created a significant survivor bias, involving the omission of all firms that disappeared prior to the year in question (which could have created significant, and difficult to resolve, sample selection problems). The fact that we are dealing with "old economy" firms may render our findings more conservative, in that it may reduce the variation for several of our exogenous variables.

Given the time-series nature of our data, we considered the use of lags for all of our predictors. Recent performance is based on profit and growth rates for the previous three years so is lagged by definition. Although our dependent variable is not identical to the firm's debt ratio (because it is based on the level of new borrowing in a given year), the control for the previous year's debt ratio serves in part as a lagged endogenous variable. We use contemporaneous measures for retained earnings, the presence of a CEO from a finance background, and the number of financial representatives on the firm's board. Published data on board membership and firm management are often a year old at press time, and these variables tend to be highly stable across individual years. For retained earnings we determined that the amount of cash available to firms can change quickly enough that lagging the variable may leave too much lead time.

Our data came from three primary sources. All data on board members and CEOs were entered directly from back issues of *Standard & Poor's Directory of Corporations* and were cross-checked with comparable issues of *Moody's Industrial Manual*. All instances of discrepancies were looked up and resolved individually by research assistants. Financial data were derived from a data set, commissioned by the authors, assembled by Standard and Poor's Compustat service.

ESTIMATING THE NETWORK EFFECT

Our data are organized in a pooled cross-sectional time-series format common in econometric analyses. Individual companies appear in consecutive years in the data, and the resulting units of analysis are company-years. The standard way to handle such a data set, when the dependent variable is continuous, is to use either a fixed effects model, with dummy variables for individual firms and years, or a random effects model that uses a generalized least squares estimator to purge autocorrelation from the error term. The latter is the approach used by Stearns and Mizruchi (1993a; 1993b; Mizruchi and Stearns 1994).

A problem presents itself in the current analysis, however. In the earlier study by Mizruchi and Stearns (1994), the authors identified a network effect by charting the number of representatives of financial institutions on the firm's board. As we have seen, this approach was inadequate for two reasons: first, because it failed to account for the behavior of firms' peers, focusing instead only on financial representation; and second, because it ignored the possibility that interfirm influence could result in low as well as high levels of financing. In a subsequent study, Mizruchi and Stearns (2003) addressed this problem by examining interfirm dyads. This allowed them to directly examine the effects of network ties—they hypothesized that interlocked pairs of firms would behave more similarly—but it also made the examination of firm level effects extremely cumbersome—controlling for debt ratio, for example, meant examining whether pairs of

firms with similar debt ratios would engage in similar levels of borrowing. The high number of observations that result from dyadic analyses, approximately three million were we to pool the current data into a single data set, also make a dyadic analysis less feasible for our study.

An alternative to both of these approaches is to use a network autocorrelation model. This model allows for the incorporation of network effects into a standard individual-level regression format. The model has a number of variants, but the classic approach was developed by Cliff and Ord (1981) to handle spatial proximity in geographic analyses. Unequally distributed spatial proximity across units creates problems for the independence assumption that underlies ordinary least squares (OLS) regression. The Cliff-Ord model begins with the standard matrix equation for OLS, $Y = X\beta + \varepsilon$, but then estimates the error vector ε as a function of a square matrix of proximities among the observations, W, such that $\varepsilon = \lambda W\varepsilon + \mu$, where λ is the estimate of the level of spatial autocorrelation and μ represents a new vector of randomly distributed error terms (Anselin and Hudak 1992).

The prior approach to spatial autocorrelation treats the autocorrelation between observations as a nuisance that requires removal so that the substantive predictors in the regression model will produce correctly estimated standard errors. The W matrix can also be treated as a substantively interesting variable, however, especially if one views the effects of relations among observations as worthy of estimation. In this model, W can be treated as a matrix of social relations among the actors, and a parameter, ρ , can be treated as an estimate of the effects of an actor's social ties on an outcome variable. The model that emerges from this approach is $Y = \rho WY + X\beta + \varepsilon$, where ρ is a substantively interesting coefficient, along with the elements of β (Anselin and Hudak 1992; Doreian 1990). What ρ represents is the effect of the behavior of the alters that are proximate to ego on ego's behavior. In our case, it represents the effect of the level of borrowing by the firms that are socially proximate to the focal firm on the focal firm's level of borrowing. There are a number of ways to estimate ρ . We use a maximum likelihood approach, available in version 8 of Stata (Pisati 2001).

How the W matrix is defined has been a source of some controversy among network theorists (Leenders 2002). The most important criterion is that the definition have a clear substantive basis. The W matrix in our study is based on board of director interlocks among the firms. Interlocks have been the most widely-used indicator of interfirm ties over the past two decades. They have been shown to affect a wide range of corporate behaviors, including mergers and acquisitions, adoption of takeover defense policies, adoption of the multidivisional form, and political contributions (see Mizruchi 1996 for a review of this literature). In recent years organizational researchers have examined several other types of ties, including interindustry and interfirm business transactions (Burt 1983; Mizruchi 1992; Uzzi 1996; Baker, Faulkner, and Fisher 1998), common social club memberships (Kono et al. 1998; Marquis 2003), and strategic alliances (Gulati 1995; Powell, Koput, and Smith-Doerr 1996; Stuart 1998; Ahuja 2000). Given their role in facilitating communication across firms, as well as the wide availability of data, director interlocks represent the most appropriate measure for our purposes.

In our study, W represents a firm by firm matrix, the cells of which contain the "distances," that is, the number of steps, between each pair of actors. Two firms, i and j, that are directly interlocked receive a 1 in their ij and ji cells. Two firms that are not directly interlocked but are connected through a common tie to a third firm (that is, are indirectly interlocked) receive a 2 in their cells. Firms that are separated by no fewer than two intermediate links receive a 3. Because the likelihood of actors having an influence over one another tends to decline sharply after two steps (Granovetter, [1974]1995), we coded all separations of three or more steps as 3. There were few ties with distances of greater than three, but the distribution was sharply skewed at that point, so by capping the distances at 3 we also guard against individual observations having undue influence on our results. Because our input into the W matrix was the distance between firms, a positive network effect would lead to a negative coefficient, since existing theory suggests that firms will be influenced by those that are relatively "close" to them in the network. To make the direction of the effect more consistent with this expectation about the role of network ties, we reversed the signs of the network coefficients so that the effects represent closeness rather than distance. A positive

coefficient is therefore consistent with the idea that firms' behavior is influenced by those with whom they are closely linked.

Although our use of the network autocorrelation model represents a significant improvement over the methods used in the earlier Mizruchi and Stearns studies, it does create one problem: Because each network must be examined only in the year to which it applies, it is not possible for us to pool our data across years. Doing so would require us to create a W matrix that included firm-year to firm-year ties across all observations, including, for example, company A in 1973 with company B in 1994. Such an approach would be substantively meaningless as well as computationally demanding, assuming it were possible at all with existing software. The consequence of this problem is that our units of analysis must be firms, rather than firm-years, examined individually at each time point. We therefore compute separate regression models for each year of our data. In addition to sharply reducing the number of observations in each of our analyses, this approach also prevents us from statistically testing for over time interaction (and thus historical contingency) in a pooled regression model. We can, however, take the results from our individual year regressions and run statistical analyses on the time-series data that result. We do this in our tests of Hypotheses 1 through 5.

Before proceeding to our analyses, we must address one additional issue. The substantial attrition of firms over time raises the question of whether we are dealing with the same population of firms in the later years of our analysis as in the earlier years. This is especially significant given our hypotheses about the changing strength of the effects over time. The standard way to address this issue is with the use of a sample selection model (Heckman 1979; Berk 1983). In the standard Heckman model, one estimates a probit regression model, regressing the probability that the firm survives in a given year on a series of variables. From this equation one identifies a hazard rate of survival for each firm, which is then inserted into the substantive regression equation as an exogenous variable. If inclusion of the hazard rate does not affect the strength of the remaining predictors, selection bias is assumed to be unproblematic. In our data, the number of firms leaving the sample is relatively small in each year. This means that there are too few non-survivors in any given year to estimate a year-to-year selection equation. The alternative we chose was to estimate a selection equation for the first year of our data,

1973, with the probability of survival in 1994 (our last year of data) as our dependent variable. We then inserted the hazard rates from that model into our substantive equation for 1994. Because the Heckman model requires a probit equation, for which the network autocorrelation model is not available, it was necessary to remove the network variable from both our selection and substantive equations. Computation of the selection model is facilitated by including in the selection equation at least one variable that does not appear in the substantive equation. We used the firm's issue of new equity in the given year as our instrument. Our model (available on request) revealed that the hazard rate in the 1994 substantive equation was insignificant, and that its inclusion had no effect on the coefficients of the remaining variables.

RESULTS

Step 1: Models of the Determinants of Debt

Table 1 presents 22 regression equations, one for each year of our data, containing the network autocorrelation models, with borrowing as the dependent variable and our firm level and network predictors as the independent variables. Because we are examining equations from 22 separate data sets, it is not feasible to present the summary statistics and correlations for each year. These data are available on request. Due to space considerations, we also do not include the coefficients for the industry dummy variables. These are also available on request.

Although we did not offer specific hypotheses about the variables in these models, three findings involving the control variables warrant comment. Consistent with Mizruchi and Stearns (1994), retained earnings had a negative effect on borrowing in all 22 years. The coefficient was statistically significant in only 15 of the 22 years. The random probability that the effect would be negative in all 22 equations is less than .001 ($\chi^2 = 22.0$ with 1 d.f.), however, suggesting that a negative association between retained earnings and the use of new debt is operative across the entire term of our study. Contrary to Mizruchi and Stearns' findings, the effects of the presence of a finance CEO and financial representatives on the firm's board are in virtually all cases null. In only one year for each variable does the coefficient have a probability of even less than .10 in the expected direction—1978 for financial directors and 1986 for the presence of a finance CEO. Given that we examined 22 individual years, one would expect approximately one significant result purely by chance.

TABLE 1 ABOUT HERE

One possible reason for the null effect of finance CEO is that by the 1970s, the finance conception of control had become institutionalized, such that firm CEOs operated according to its principles even if they did not hail from finance backgrounds. Zorn (2004), who finds no effect of finance CEOs on firms' propensity to appoint chief financial officers, makes a similar argument for his null finding. Regarding the null effect of financial directors, one possible reason for its lack of significance is that in this study we include the additional effect for network ties, which does a better job of tapping the characteristics that the financial board representation variable was originally designed to identify. Removing the network variable from the model has virtually no effect on the strength of the financial board representation played a role in firm borrowing, it was as likely to lead to a reduction in debt as it did to an increase. This is consistent with the findings of Byrd and Mizruchi (2005), who found that the presence of bankers on firm boards increased debt under some circumstances and reduced it under others.

The network effect reveals an interesting pattern. From 1973 through 1985, we observe a significant positive effect seven out of 13 times. From 1986 through 1994 we observe a significant positive effect only three times out of nine. This pattern is consistent with the prediction in Hypothesis 1, although the pattern is not unambiguous. As we shall see below, a casual "eyeballing" of the network effects gives a misleading picture of their actual trajectory over time. We turn to this issue in the following section.

Step 2: Historically Contingent Effects

In Hypotheses 1 through 5 we suggested a number of factors that would account for changes in the effect of social networks on firm financing over time. The question is how to test these hypotheses. One approach would be to pool the data across years and compute interaction effects between the network effect and the variables described in our hypotheses. As we noted above, this approach is not feasible because it is not appropriate to pool network data across years. An alternative is to create a new data set in which our observations represent the 22 years from which our data were culled. Recall that the dependent variable in our hypotheses is the size of the network effect in a given year. By treating these effects as data, we can examine their determinants using variables that capture the processes described in our hypotheses. The use of coefficients from one model as data in a second-stage model has a history in the economics literature dating back at least to the 1970s (see Saxonhouse 1976 for a classic discussion; Waring 1996 and Hornstein 2004 for applications). We discuss below some of the computational issues involved in these models.

Our dependent variable, then, is the coefficient for the network effect for each of the 22 years. Our hypotheses suggest four predictor variables. For H1, which suggests that the size of the network effect declined over time, we use the year. We expect the effect of year on the network effect to be negative. For H2, which suggests that the professionalization of the finance function inside the firm led to a decline in the network effect, we use the proportion of firms in our data set that had CFOs in a given year. For each firm for each year we coded whether the firm had a CFO. We then computed the proportion of firms in a given year that had CFOs.⁵ H2 suggests that we should observe a negative coefficient for the proportion of firms with CFOs. For H3, which suggests that the internalization of financial decision making led to a decline in the network effect, we examined the prevalence of representatives of financial institutions on the boards of the firms in our sample. For each year we computed the average, for the firms in our sample,

⁵ We would like to thank Frank Dobbin and Dirk Zorn for sharing their CFO data with us. Because their data included only about one-third of our firms and because we were collecting additional variables, we ultimately collected all CFO data ourselves. We were able to use the Dobbin-Zorn data as a check on our accuracy, however. We found discrepancies in only two cases.

of the proportion of board members whose principal affiliations were with financial institutions. H3 leads to the prediction that the effect of the prevalence of financial board representatives on the network effect will be positive. For H4, which suggests that the volatility in the external environment led to a reduction in the network effect, we coded data on the number of mergers that occurred in the American economy in a given year.⁶ The prediction from H4 is that the effect of the number of mergers on the size of the network effect will be negative. Finally, for H5, which suggests that the effect of mergers on the network effect will be increasingly negative over time, we computed an interaction term involving the product of the number of mergers and the year.

In addition to the above variables, we also controlled for the density of the network in the particular year, where density is defined as the number of existing direct interfirm ties in the network (coded in binary fashion, as 1 for existing and 0 for non-existing) divided by the number of possible ties, the latter computed as $(N^2 - N)/2$. As is evident from this formula, there is a tendency for density to decline with the increasing size of a network. Because the number of firms in our data declines consistently over time, the density tends to increase over time. We know of no existing analysis that examines the effect of network density on the effect of particular network ties on an outcome variable, and we have no reason to believe that there is an intrinsic association between the two.⁷

There is reason on substantive grounds, however, to believe that there might be a positive association between network density and the size of the network effect. In their classic study of social influence, Festinger, Schachter, and Back (1950) showed that the more cohesive the social group, the greater the pressures toward uniformity of behavior. This suggests the possibility that social ties among the firms will have a stronger effect on similarity of behavior to the extent that the overall level of cohesion in the group is high. It follows that we would expect to observe a positive effect of the density of the network in a particular year on the strength of the network effect on firm borrowing.

⁶ Our data on mergers came from the *Statistical Abstract of the United States* from 1994 back to 1980 and, prior to 1980, the trade journal *Mergers and Acquisitions*.

Although we include density primarily as a control, we shall also take note of whether its coefficient exhibits the predicted positive effect.⁸

Because our 22 observations represent a time-series, it is necessary to check, and if necessary adjust, the data for autocorrelation. We did this using the Prais-Winsten GLS estimation technique (Ostrom 1990). The Prais-Winsten approach is identical to the widely used Cochrane-Orcutt technique, except that it provides an estimate for the first year of data, which would otherwise be lost because the previous year's observation is necessary to adjust the data for serial correlation of the residuals. In this approach, each Y and X observation (including the constant) is transformed through the formulas $Y^* =$ $Y_t - \rho Y_{t-1}$ for Y, $X^* = X_t - \rho X_{t-1}$ for the Xs, and $a^* = a(1-\rho)$ for the constant, where ρ is the autocorrelation estimate. For the first observation, the Prais-Winsten technique transforms Y to $Y^* = Y \sqrt{(1-\rho^2)}$ and X to $X^* = X \sqrt{(1-\rho^2)}$ (Ostrom 1990:31).⁹

In his discussion of the use of coefficients from one model as dependent variables in a second-stage model, Saxonhouse (1976) notes the possible presence of heteroskedasticity, since each observation is a coefficient with a unique sampling variance. To correct for this, Saxonhouse recommends a weighted least squares approach, in which one multiplies both sides of the second-stage equation by the inverse of the standard error associated with each coefficient (in our case the standard error of each ρ from the network autocorrelation model). An alternative means of correcting for heteroskedasticity is with the use of robust standard errors (Huber 1967; White 1980).

⁷ The absence of evidence for this connection was confirmed for us by three leading network methodologists: Patrick Doreian, Noah Friedkin, and David Krackhardt. We thank them for their feedback on this issue.

⁸ In their time-series analyses of firm financing, Stearns and Mizruchi (1993a; 1993b; Mizruchi and Stearns 1994) found that macro-economic indicators such as interest rates and the stage of the business cycle affected the level of debt that firms used. Although it is reasonable to expect that firms' use of debt will be affected by these macro-economic variables, it is less clear why such indicators would affect the extent to which firms' use of debt is affected by their interfirm network ties. In models not shown here but available on request, we found no effect of either interest rates or the stage of the business cycle on the size of the network effect over time.

⁹ Note that this ρ , which represents the year to year correlation of the residuals from our time-series analysis, is distinct from the ρ from our network autocorrelation model, which in these regressions serves as our substantive dependent variable.

We ran our analyses with both the WLS and robust standard error (hereafter "RSE") approaches. Both yielded identical substantive conclusions.

We have chosen to report results using robust standard errors because unlike in weighted least squares, the RSE approach allows us to preserve the variables in their original form. Results from the WLS models are available on request. We examined two sets of results using the RSE models. As a conservative approach, we began by including the standard error of ρ on the right side of the equation, as a control. This should not be necessary given that the RSE adjustment corrects for heteroskedasticity, but we included it as a special precaution due to our small sample size. In a second set of equations we removed the standard error from the analysis. The results in those equations were slightly stronger in two cases and slightly weaker in two others, but they led to identical substantive conclusions. We present only the results that include the standard error as a control. The equations without the standard error are available on request.

The classic RSE model takes the generalized least squares formula for the variance-covariance matrix of the regression coefficients $[(X'X)^{-1} X'\Omega X (X'X)^{-1}]$ and inserts e_i^2 (the squares of the residuals from the computed regression) into the diagonal of Ω . MacKinnon and White (1985) raised questions about the viability of this approach for small samples and suggested a series of alternatives. Long and Ervin (2000) showed that one of these alternatives, $[e_i^2 / (1-h_{ii})^2]$, where h_{ii} is the leverage of observation i [the diagonal element of the matrix X (X'X)⁻¹ X'], provided the most accurate standard errors when heteroskedasticity was present. Because significant heteroskedasticity was present in all of our equations that used ρ as the dependent variable, we used this modified RSE calculation (option "hc3" in Stata) in the models presented here. The results using this approach yielded larger standard errors, and thus more conservative inferences, than the alternative weights discussed by MacKinnon and White and Long and Ervin.¹⁰

Time-Series Results

¹⁰ One possible source of heteroskedasticity is the presence of outliers in the data. We examined our data, both visually and with the calculation of leverage values and studentized residuals, and found no significant outliers.

Table 2 presents the summary statistics and correlations among the variables. Because time-series data are prone to autocorrelation, significance tests should not be applied to the individual correlations. It is worth noting, however, that several of our variables are highly correlated. Our three key substantive predictors—proportion of firms with CFOs, average proportion of financial representatives on the board, and mergers—all have absolute correlations with year that exceed .9, and all three are correlated at least .9 with one another. The three variables plus year have correlations with density that range from .75 to .85. The standard error of ρ and network density are also correlated .57, probably because the high density networks occurred in years with fewer firms. Given the greater than .9 correlations among our substantive predictors and the fact that we have only 22 observations, multicollinearity is virtually certain to be a problem. We therefore examine each of our hypothesized predictors in separate equations. We do maintain the controls for density and the standard error in every equation, however, and we also report below a series of equations in which we examine the simultaneous effects of each hypothesized substantive variable and year.

TABLE 2 ABOUT HERE

Table 3 presents a series of regression equations. In each equation, the coefficient for the network effect from Table 1 serves as the dependent variable. Because we have predicted directions for all of our effects, we use one-tailed statistical tests for all of our variables in these equations. We expect to observe negative effects for year, proportion of firms with CFOs, mergers, and the merger*year interaction term and positive effects for the proportion of financial representatives on the firms' boards, density, and the standard error. As is conventional for one-tailed tests, we shall treat any coefficient with a low probability value but in the opposite-from-predicted direction as non-significant.

Equation 1 in Table 3 includes the year as our predictor, which we hypothesize to have a negative effect, along with network density and the coefficient's standard error as controls. The results are consistent with Hypothesis 1. The T-statistic for year based on the robust standard error is -2.34 (p=.016), and this occurs in an equation with only 22

cases and three exogenous variables, all of which are highly correlated (the tolerance for year is .407).¹¹ Notice also that although the simple correlation between year and the network effect is close to zero and actually positive (.001), we achieve the predicted negative effect when we control for network density and the coefficient's standard error. If we remove the standard error from the equation, the significant negative effect of year remains—the coefficient increases from -.362 to -.598, while the robust T-statistic remains exactly the same, -2.34. The standard error, not surprisingly, has a positive association with the coefficient, although the T-statistic based on the robust standard error (2.05) is considerably lower than that based on the unadjusted standard error (3.74). Network density also has the predicted positive effect, but the coefficient is not statistically significant.

TABLE 3 ABOUT HERE

In Equation 2 we replace the year variable with the proportion of firms in our sample that had CFOs. Consistent with Hypothesis 2, the effect of the CFO variable is negative. The inclusion of this variable produces a coefficient of determination of .630, slightly higher than the .608 in Equation 1 (based on the year variable). The effects of density and the standard error are virtually identical in Equation 2 to those in Equation 1. In Equation 3 we replace the CFO variable with the average proportion of representatives of financial institutions on the firms' boards. As predicted by Hypothesis 3, the effect of this variable is significantly positive. Again, the coefficients and T-statistics of density and the standard error remain basically the same as in the previous equations. The T-statistic for the financials on the board variable is slightly lower than those of year and CFOs, but it remains strongly significant in the expected direction. The R² for the model in Equation 3, .604, is virtually the same as that in Equation 1. In Equation 4 we replace the financial board variable with the number of mergers in the economy as a whole.

¹¹ There is no established standard for identifying the presence of severe multicollinearity. Common rules of thumb that we have encountered range from a tolerance .4 or below as point of concern, as suggested by Allison (1999), to a value of below .1. The tolerance, the inverse of the variance inflation factor, is the

Based on Hypothesis 4 we expect mergers to be negatively associated with size of the network effect. As predicted, the coefficient for mergers is significantly negative. Its T-statistic (-3.93) and the model R² (.717) are the highest of the equations we have examined thus far. Finally, in Equation 5 we test Hypothesis 5, in which we suggested that there would be a negative interaction effect between the number of mergers and year. As the equation indicates, our findings are again consistent with the hypothesis. The statistically significant negative interaction term shows that the effect of the number of mergers on the size of the network effect becomes increasingly negative over time. Despite enormous multicollinearity due in part to the presence of five exogenous variables in a model with only 22 observations, the interaction effect of mergers and year has a T-statistic of -1.94 (p=.035). If we remove the standard error from Equation 5, the T statistic for the interaction increases to -3.24. These results support our suggestion that the extent to which environmental pressures on managers depressed the level to which they relied on their interfirm social networks became more pronounced over time. The equations in Table 3 thus provide support for all five of our hypotheses.

Before concluding the discussion of our results, we must address one issue regarding the nature of our models in Table 3. In our first hypothesis we predicted that the effect of interfirm network ties on firm financing would decline over time. We discussed a series of processes by which we believe that this hypothesized decline took place. The three processes, the professionalization of the finance function within the firm, the internalization of financial decision making, and the increased volatility of the environment, were all time-dependent, however. That is, all three exhibited strong over time trends that were highly correlated with the variable year. Given the high correlations of our three substantive variables with year—in all three cases well over .9—it raises the question of whether our three variables are simple artifacts of time, as opposed to causal mechanisms.

On one hand we believe that this concern is unnecessary. The variable year may allow us to predict the patterns of the network effect, but time by itself has no theoretical content. Only by specifying the mechanisms by which the network effect shifted over

proportion of the total sum of squares remaining in the particular exogenous variable after the effects of the other exogenous variables have been taken into account.

time can we develop a theoretically meaningful account. By showing that the proliferation of chief financial officers, the decline in the number of financial representatives on firm boards, and an increase in the number of mergers were all associated with a decline in the effect of interfirm networks, we have specified the processes by which this over-time decline occurred. Beyond our substantive explanation, however, there is evidence that even statistically, our processes played a role, independent of time.

Consider the variable mergers, which has a correlation of .918 with year. If we include both mergers and year in an equation simultaneously (along with density and the standard error), the tolerance for mergers is only .094, meaning that less than 10 percent of the variance in mergers remains after controlling for the other three variables, and the tolerance for year is only .154. Yet despite the almost certain existence of severe multicollinearity (enough to render the effect of year non-significant), the effect of the number of mergers remains significantly negative (T=-2.30, p=.017). In other words, the negative effect of mergers predicted by Hypothesis 4 is strong enough that the effect maintains itself even with the inclusion of a variable with a .92 correlation. Moreover, it is the mergers variable that holds its significance while the effect of year disappears.

A similar, albeit not as powerful, result occurs when we compute an equation that simultaneously includes year and the proportion of firms with CFOs. These two variables have a correlation of .989, meaning that it is nearly impossible to compute partial coefficients for both of them in the same equation. The tolerance for the CFO variable is .021 (a whopping variance inflation factor of 47.4), meaning that only two percent of the variation in this variable remains once we include year in the equation. The variance inflation factor for year is 49.7. Yet even under these conditions, the T-statistic for CFOs remains negative, and is nearly statistically significant (T=-1.29, p=.107), while the coefficient for year is null (and even slightly positive). Again, the effect of our substantive variable exceeds that of year when the two are included in the same equation. The variable for financial board representation, which is correlated -.976 with year, does not approach significance when the two variables are included simultaneously (the tolerance for the financial board variable is .038 and the variance inflation factor is 26.2). In this case, both variables exhibit clearly null effects. Taken as

a whole, however, the fact that two of our three substantive variables largely hold their effects even when controlling for year, and that they do this despite the small number of observations and the relatively large number of variables in the equation, suggests that these measures are more than mere artifacts of time.

The results from our time-series analysis thus suggest strong overall support for our hypotheses. The effect of interfirm network ties on firms' use of debt declined over time. And the historical processes that we identified—the professionalization of the finance function within the firm, the internalization of financial decision making, and the increased volatility of the environment—are all significantly associated with this decline. All three factors account for the changes that we observe in the effect of interfirm networks on firm financing behavior.

DISCUSSION AND CONCLUSION

Economic and organizational sociologists have increasingly demonstrated that the effects of social network ties on the behavior of individuals and firms varies—across both types of actors (such as men versus women) and types of networks (such as those based on instrumental versus expressive ties). We have argued that the effects of a particular type of network—the ties created by director interlocks among corporations—varied over time. In particular, we suggested that between the 1970s and the 1990s, corporate managers in the United States experienced a series of changes in the nature of firm decision making and in the environment within which their firms operated. The result was a decline in the extent to which interfirm network ties affected firms' use of debt financing.

Three changes in particular, we argued, affected the extent to which firm financing behavior was affected by interfirm network ties. First, the financial function within the firm took on increasing importance during this period, symbolized by the ascendance of a new top management official, the chief financial officer. Beginning as a rarity in the early years of our study (only 1.8 percent of our firms had CFOs in 1973), by 1994 this title was present in nearly two-thirds of our firms. Second, the presence of

representatives of financial institutions on the firms' boards of directors declined over time. The proportion of financial representatives on firm boards declined by 37 percent between 1973 and 1994, and the proportion of outside directors whose primary affiliations were with financial institutions dropped by 54 percent. The concurrent rise of the CFO and decline of financial representatives on boards suggested that firm financing decisions were increasingly made by specialists inside the firm, without reliance on board members and the interfirm networks their presence both created and reflected. Third, the merger wave of the 1980s greatly increased the volatility of the environment within which firm managers operated. Among other consequences, this phenomenon altered the nature of financial strategies, from a relatively deliberate, long-run, focus to one of rapid, short-term, decision making. Finally, following several other thinkers, we argued that the merger wave of the 1980s was accompanied by the development of a new worldview, variously termed the "agency" (Zajac and Westphal 2004) or "shareholder value" (Useem 1993; Fligstein 2001) model, in which the most important goal of managers was to increase the firm's stock price and the vulnerability of managers, as well as the pressure to "eat or be eaten," became a taken-for-granted characteristic of the environment.

Our argument about the changing nature of firms' internal and external environments suggested that the effects of social network ties on managerial decisionmaking would change over time. Following from this suggestion, we hypothesized that the effects of social network ties on firms' use of debt would tend to decline over time, and that this decline corresponded with the rise in the prevalence of chief financial officers, the decline in the level of financial representation on boards of directors, and increases in the level of merger activity. Using data on approximately 140 large U.S. corporations over a 22-year time-series, we examined individual regression models for each year using a network autocorrelation model, in which we simultaneously measured the effects of firm-level variables and the effect of the behavior of the firms with which the focal firm was socially connected. We then examined the trajectory of the resulting network effects across the 22 years. Consistent with our hypotheses, the network effect declined as the prevalence of CFOs increased, as the level of financial board representation declined, and as the level of merger activity increased. We also found, consistent with our prediction, that the effect of the level of merger activity on the size of

the network effect became increasingly negative over time, as we moved into an era with an alternative conception of business behavior. The results of all of our analyses are therefore strongly supportive of our contingency hypothesis: the strength of the network effect on firm financing behavior declined systematically over time, and across changing levels of the factors described in our model.

Our model and findings have implications for an important debate within economic and organizational sociology. Two major approaches have dominated the recent literature on the diffusion of organizational strategies and structures: a neoinstitutional model that focuses on the symbolic and cultural underpinnings of organizational behavior, and a social network model that emphasizes the social structural constraints and opportunities that shape organizational action. Proponents of these approaches have often invoked concepts and predictions derived from the other, but the two models have tended to appear in separate, and distinct, literatures. Yet just as neoinstitutional ideas about the socially constructed nature of firm strategies can benefit from the use of network concepts—these socially constructed notions often diffuse through social networks—network models of the social structural determinants of behavior can benefit from neo-institutional ideas about the historical context within which social networks are situated. We are not the first to recommend a synthesis of the two approaches—similar ideas are evident in classic works by Pfeffer and Salancik (1978) and DiMaggio and Powell (1983), as well as in empirical applications (such as Palmer, Jennings, and Zhou 1993). We are among the first to build an institutional account into a network model, however.

A relevant prior work in this regard is a study by Davis and Greve (1997), who compared the diffusion of two firm practices—takeover defense policies known as "poison pills" and CEO severance packages known as "golden parachutes"—through interlock networks. Although poison pills diffused through interlock ties, golden parachutes did not. Davis and Greve argued that the reason for these divergent effects could be found in the fact that poison pills were viewed as legitimate within the community of directors, while golden parachutes were viewed as illegitimate. The authors thus used the neo-institutional concept of legitimacy to account for differences in the effects of social network ties.

Similar to the Davis-Greve approach, we use neo-institutional theory as a context for the application of network models. Where our account differs is in our focus on the ways in which a changing historical context leads to changes in the network effect. Whereas Davis and Greve suggest that the network effects driving two different firm behaviors can vary depending on extent to which a given practice is viewed as legitimate, we have shown that the network effects driving a single firm behavior, even one whose legitimacy is not in question, can vary across time, depending on the institutional environment within which the practice takes place. Both studies indicate that the extent to which interfirm networks drive firm practices must be viewed as contingent—in Davis and Greve's case on the nature of the practice itself and in our case on the historical period in which it occurs. Our findings raise the additional question of whether the many findings in the literature on network effects on firm behavior might have been specific to the time in which the study took place. They suggest that we should pay increasing attention in the future to the historical context of network effects.

Given our findings, it is crucial to emphasize that we are not saying that social networks are no longer important in understanding economic action. Our paper deals with one specific area of firm behavior, one that many observers would consider less susceptible to social influences than the variables typically studied by economic sociologists and organizational theorists. We are dealing with a specific sample, consisting largely of "old economy" firms that were dominant in the mid-1950s. Our own work (xxxx), based on data more recent than those used in this paper, indicates that social networks play a role in bankers' success in closing deals, and studies by several other researchers, including those cited in our introductory sections, have revealed similarly strong effects of social networks on behavior in the economic arena. Moreover, because the data we have presented end in 1994, we cannot speak to any changes that might have occurred in the late 1990s and the early 2000s. This is especially significant given the wave of corporate scandals at the turn of the century, and the calls for the increased monitoring of management that followed. We make no assumption that the changes that we posited during the period of our study represent a linear, or even monotonic, historical trend. Indeed, some authors have argued that managerial autonomy has fluctuated in a cyclical fashion over the past century (Stearns 1986; Useem 1993).

Our key finding, and conclusion, remains, however: Just as network effects may vary by demographic groups or the type of network examined, they may also vary, within the same group and the same type of network, across historical time.

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Table 1: Spatial Regression Estimates of Corporate Borrowing, 1973-1994

Year	<u>1973</u>	<u>1974</u>	<u>1975</u>	<u>1976</u>	<u>1977</u>	<u>1978</u>	<u>1979</u>	<u>1980</u>	<u>1981</u>	<u>1982</u>	<u>1983</u>
Network Effect	0.003	-0.003	-0.000	-0.000	0.004	-0.000	0.016	0.001	0.014	0.017	0.001
	(2.15)***	(-1.94)	(-0.02)	(-0.11)	(1.43)+	(-0.14)	(3.24)***	(0.42)	(3.56)***	(2.31)**	(0.16)
Log Total Assets	0.006	0.007	0.017	0.005	0.012	0.007	0.000	0.007	0.003	0.016	0.017
	(1.05)	(0.92)	(2.57)*	(0.79)	(1.98)*	(1.56)	(0.03)	(1.31)	(0.50)	(1.70)+	(3.04)**
Recent Performance	1.138	-0.203	-0.032	0.310	4.386	-0.098	1.207	0.807	0.923	-1.005	-1.278
	(2.07)*	(-0.41)	(-0.06)	(0.42)	(7.89)***	(-0.18)	(2.87)**	(2.09)*	(2.17)*	(-1.39)	(-2.72)**
Financial Directors	0.003	-0.002	0.000	-0.001	0.000	0.003	-0.003	-0.001	-0.001	-0.001	-0.005
	(1.28)	(-0.50)	(0.12)	(-0.19)	(0.30)	(1.49)+	(-1.18)	(-0.47)	(-0.37)	(-0.26)	(-1.53)
Finance CEO	0.009	0.014	-0.014	-0.003	0.013	0.002	0.007	0.012	0.006	0.008	0.010
	(0.80)	(1.08)	(-1.09)	(-0.23)	(1.07)	(0.29)	(0.79)	(1.22)	(0.62)	(0.51)	(0.81)
Log Debt Ratio (t-1)	-0.030	-0.050	-0.039	-0.005	-0.024	-0.010	-0.001	-0.040	-0.051	-0.031	-0.040
	(-1.69)+	(-2.15)*	(-1.43)	(-0.22)	(-1.19)	(-0.63)	(-0.07)	(-2.27)*	(-2.67)**	(-0.99)	(-1.97)+
Retained Earnings	-0.103	-0.059	-0.083	-0.067	-0.101	-0.096	-0.140	-0.111	-0.127	-0.003	-0.064
	(-2.39)**	(-1.16)	(-1.65)+	(-1.24)	(-2.10)*	(-2.23)*	(-3.94)***	(-3.15)**	(-3.47)***	(-0.55)	(-1.67)*
Log Stock for Acq.	0.009	0.012	0.007	0.007	-0.004	0.013	0.003	0.007	0.006	0.005	0.006
	(2.53)*	(2.59)*	(1.64)	(1.95)+	(-1.23)	(5.83)***	(1.83)+	(2.76)**	(2.74)**	(1.49)	(1.91)+
Constant	0.106	0.040	-0.003	0.017	0.035	0.016	0.303	0.094	0.360	0.152	-0.035
	(2.13)*	(0.56)	(-0.05)	(0.25)	(0.57)	(0.34)	(3.09)**	(1.72)+	(3.33)***	(1.02)	(0.59)
Observations	137	140	145	142	139	133	135	139	131	130	126

z statistics in parentheses; +p < .10, *p < .05, **p < .01, ***p < .001; probabilities involving the network effect, retained earnings, financial directors, and finance CEO are one-tailed; all others are two-tailed; industry dummy variables are omitted to conserve space.

Table 1 (continued)

Year	<u>1984</u>	<u>1985</u>	<u>1986</u>	<u>1987</u>	<u>1988</u>	<u>1989</u>	<u>1990</u>	<u>1991</u>	<u>1992</u>	<u>1993</u>	<u>1994</u>
Network Effect	0.024	0.009	0.003	-0.001	0.002	0.005	0.005	0.010	0.004	-0.001	-0.003
	(2.79)**	(2.06)*	(1.04)	(-0.33)	(0.48)	(1.75)*	(1.69)*	(2.27)*	(0.97)	(-0.34)	(-0.92)
Total Assets	-0.004	0.000	0.020	0.032	0.066	0.039	0.034	0.062	0.055	0.028	0.008
	(-0.46)	(0.03)	(2.01)*	(2.51)*	(5.89)***	(3.26)**	(2.28)*	(3.10)**	(2.56)*	(2.91)**	(0.87)
Anticipated Return	1.908	1.128	-1.893	-1.498	0.049	1.638	-1.240	-2.954	2.014	-0.195	-0.586
	(2.51)*	(1.37)	(-1.89)+	(-1.13)	(0.06)	(1.32)	(-0.52)	(-1.14)	(0.78)	(-0.27)	(-0.80)
Financial Directors	-0.008	-0.004	-0.015	-0.007	0.002	-0.004	-0.001	-0.017	-0.021	-0.004	-0.003
	(-1.47)	(-0.56)	(-2.28)	(-0.69)	(0.20)	(-0.56)	(-0.08)	(-1.28)	(-1.21)	(-0.47)	(-0.42)
Finance CEO	-0.039	-0.007	0.035	-0.032	-0.007	-0.026	-0.009	-0.037	-0.068	0.014	-0.025
	(-1.88)	(-0.26)	(1.48)+	(-1.09)	(-0.24)	(-0.95)	(-0.23)	(-0.73)	(-1.20)	(0.56)	(-1.08)
Debt Ratio (t-1)	-0.045	-0.082	0.007	-0.073	-0.048	0.000	0.081	0.164	0.131	0.022	0.017
	(-1.53)	(-2.71)**	(0.24)	(-2.24)*	(-1.21)	(0.01)	(2.12)*	(4.00)***	(2.55)*	(1.08)	(0.91)
Retained Earnings	-0.085	-0.022	-0.035	-0.110	-0.307	-0.012	-0.219	-0.254	-0.302	-0.082	-0.082
	(-1.67)*	(-0.47)	(-0.86)	(-1.93)*	(-5.24)***	(-0.22)	(-2.99)**	(-3.25)***	(-3.12)**	(-1.84)*	(-1.95)*
Log Stock for Acq.	0.007	0.009	0.006	0.014	0.004	0.009	-0.004	0.001	-0.009	0.002	0.010
	(2.30)*	(2.16)*	(1.73)+	(2.85)**	(0.90)	(1.98)*	(0.54)	(0.12)	(-0.89)	(0.35)	(2.86)**
Constant	0.516	0.293	-0.049	-0.046	-0.213	-0.133	-0.047	-0.320	-0.340	-0.187	-0.024
	(2.95)**	(2.82)**	(-0.46)	(-0.36)	(-1.76)+	(-1.05)	(-0.30)	(-1.40)	(-1.49)	(-1.82)+	(-0.27)
Observations	119	116	111	108	99	98	95	90	89	90	84

z statistics in parentheses; +p < .10, *p < .05, **p < .01, ***p < .001; probabilities involving the network effect, retained earnings, financial directors, and finance CEO are one-tailed; all others are two-tailed; industry dummy variables are omitted to conserve space.

	Means, S Among	Standard D g Variables	eviations, s in Time-S (n = 22)	and Corre Series Ana	lations lysis			
	Mean	SD	2	3	4	5	6	7
1. Network Effect (closeness)	5.02	7.163	0.009	0.319	0.701	-0.01	-0.071	0.044
2. Year	83.5	6.494		0.756	0.31	0.989	-0.976	0.918
3. Network Density	0.032	0.004			0.57	0.748	-0.786	0.851
4. Standard Error	3.48	2.243				0.327	-0.391	0.345
5. Firms with CFOs	0.304	0.215					-0.954	0.906
6. Pct financials on board	11.95	1.651						-0.919
7. Mergers	2663.32	1248.7						

The network effect is the ρ coefficient from the network autocorrelation regressions in Table 1. The standard error is that associated with each ρ .

Table 2

Table 3: GLS Estimates of Changes in the Effect of Network Closeness on Borrowing Across Time (n = 22)

Variable	(1)	(2)	(3)	(4)	(5)
Year	-0.362 (-2.34)*				1.247 (2.20)
Network Density	336.86 (0.86)	395.19 (0.99)	349.84 (1.06)	1051.63 (3.10)**	736.79 (1.73)
Standard Error	2.093 (2.05)*	2.105 (2.10)*	2.196 (2.11)*	1.662 (1.92)*	1.106 (1.24)
Firms with CFOs		-12.494 (-2.99)**			
Pct Financials			1.508 (2.20)*		
Mergers				-0.004 (-3.93)***	0.028 (1.60)
Mergers*Year					-0.0004 (-1.94)*
Constant	17.364 (1.66)	-10.993 (-1.14)	-31.687 (-2.17)*	-24.011 (-3.01)**	-112.68 (-2.83)*
ρ	23	24	22	45	57
R^2	.608	.630	.604	.717	.776

p<.05, p<.01, p<.01, p<.001; all probabilities, except for those involving the constant, are one-tailed. The values in the table represent unstandardized coefficients, with T-statistics, based on the Prais-Winsten EGLS estimation technique and robust standard errors, in parentheses.





