

Political Connections and the Informativeness of Insider Trades

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Abstract:

We examine the relation between political connections and informed trading by corporate insiders at leading financial institutions during the Financial Crisis. We find strong evidence of a relation between political connections and informed trading, that the relation is strongest during the period in which TARP funds were disbursed, and strongest among TARP recipients. Notably, we find evidence of abnormal trading by politically connected insiders 30 days in advance of TARP infusions, and that these trades predict the market reaction to the infusion. Our results suggest that politically connected insiders had a significant information advantage during the Crisis and opportunistically timed their trades to exploit this advantage.

Keywords: Political Connections; Insider Trading; Financial Crisis; Troubled Asset Relief Program; Capital Purchase Program

JEL Classification: G14; G20; G28; G30; K2

1. Introduction

There is an extensive empirical literature that examines the relation between managers' political connections and firm value. Most of this research suggests these connections are associated with a wide range of benefits, including preferential access to capital and increased likelihood of winning government procurement contracts, and thus are generally valuable to shareholders (e.g., Faccio, 2006; Goldman, Rocholl, and So, 2009; Cooper, Gulen, and Ovtchinnikov, 2010). However, one missing aspect of this literature is whether (and how) corporate insiders use political connections to extract rents from shareholders. In this paper, we examine one mechanism politically connected insiders might use to extract rents from shareholders—informed trading.¹

There are two non-mutually exclusive channels through which political connections might manifest in informed trading by corporate insiders. First, while there are significant penalties to white collar crime (e.g., Karpoff et al., 2008), political connections could shield insiders from enforcement, allowing them to trade more aggressively on any existing private information. This is consistent with prior work that suggests political lobbying reduces and delays the likelihood and penalties of enforcement actions by the United States' Securities and Exchange Commission (e.g., Yu and Yu, 2011; Correia, 2014). Second, political connections could endow managers with private information about forthcoming government action that might favorably or unfavorably affect the firm. This is consistent with prior research that social connections generally, and political connections specifically, serve as a conduit for information transfer (e.g., Cohen, Frazzini, and Malloy, 2008, 2010; Gao and Huang, 2016; Ahern, 2017;

¹ Throughout the paper we use the term “insider” or “corporate insider” to refer to officers and directors covered by Section 16(b) of the Securities and Exchange Act of 1934 which requires such insiders to disclose their trades on Form 4 filings with the Securities and Exchange Commission, and “private information” to refer to information not fully impounded in market prices.

Christensen, Mikhail, Walther, and Wellman, 2017); with recent Securities and Exchange Commission investigations of the “political intelligence” industry; and with information leakage in conjunction with Congressional deliberations on Medicare reimbursement rates and Federal Reserve deliberations on monetary policy.²

We examine the relation between political connections and informed trading by corporate insiders within the context of government intervention during the 2007-2009 Financial Crisis. The unprecedented magnitude of the intervention, the substantial impact of the intervention on firm value, and the political nature of this intervention provides a powerful setting to examine our research question. For example, it is now well known that deliberations on government intervention largely took place in private meetings between government officials and insiders at leading financial institutions; details regarding the application and qualification process for funds from the Troubled Asset Relief Program (TARP) were not publicly disclosed; and political connections appear to have played a role in the allocation of these funds (e.g., Sorkin, 2009; Duchin and Sosyura, 2012). Thus, politically connected insiders at leading financial institutions were in a position to be disproportionately privately informed about the scope of government intervention, how this intervention would affect their firm, and details of any forthcoming TARP monies.

Considerable prior research suggests that corporate insiders are able to time the market and that their trades predict long-run future performance (e.g., Karpoff and Lee, 2001; Jeng, Metrick, and Zeckhauser, 2003; Jenter, 2005; Cohen, Malloy, and Pomorski, 2012). However, prior research reports mixed evidence that insiders traded in *anticipation* of the Crisis (e.g., Bebchuk, Cohen, and Spamann, 2010; Fahlenbrach and Stulz, 2011). Importantly, this prior

² With respect to Medicare, see Hamburger (2013), Henning (2013), McCoy (2014), and Mullins and Viswanatha (2016). With respect to the Federal Reserve see for example Viswantha, Davidson, Mullins, and Matthews (2015); Cox (2017).

research does not examine either the role of political connections in conveying an information advantage, or market timing ability as it relates to government intervention.

We examine the relation between political connections and the trading of corporate insiders using a comprehensive sample of all open market purchases and sales of Section 16 officers and directors (“insiders”) at 497 publicly traded financial institutions (“banks”) between 2005 and 2011. Following prior research, we measure political connections based on whether a board member has current or previous work experience at the Federal Reserve, Treasury Department, Congress, or a bank regulator (e.g., Office of Comptroller of Currency (OCC), Office of Thrift Supervision (OTS), or Federal Deposit Insurance Corporation (FDIC)); and we measure the “informativeness” of insider trades based on their predictive ability for future performance.

Consistent with the notion that managers were unable to predict the effect of the forthcoming Crisis on their firm, we find no evidence that insider trades predict future performance over the 24 months leading up to the Crisis, or during the Crisis before the creation of TARP (i.e., prior to October 2008). In contrast, over the nine months *after* the creation of TARP, i.e., the period in which TARP funds were disbursed, we find that the predictive ability of insider trades for future performance is greater than during any other period in our sample. Both the predictive ability of insider purchases for *positive* future performance and the predictive ability of insider sales for *negative* future performance increase during this period. Over the entire sample period, we find that the average one-month-ahead future return following purchases (sales) is 0.23% (−0.82%), a difference of 1.05%. However, during the period TARP funds were disbursed, we find that the average one-month-ahead future return following purchases (sales) is 1.84% (−2.87%), a difference of 4.71%.

Consistent with the sharp increase in the informativeness of insider trades relating to private information gleaned from political connections, we find that the increase is concentrated almost entirely in the trades of politically connected insiders. Prior to the Crisis, we find the difference in one-month-ahead future returns between purchases and sales of insiders with (without) political connections is economically and statistically insignificant, -0.37% (-0.05%). However, within the period TARP funds were disbursed, the difference in one-month-ahead future returns between purchases and sales of insiders with (without) political connections is both economically and statistically significant, 8.89% (2.81%).

These results are robust to a battery of sensitivity analyses including using various fixed effect structures (e.g., time, firm, and insider) to control for firm-specific and insider-specific characteristics, changes in market conditions, and a differential effect of market conditions on firms with and without politically connected insiders. Moreover, for a subset of individuals where we are able to construct a network map, we find our results vary with the strength of the connection (i.e., direct versus indirect; recent versus stale). While we cannot completely dismiss the possibility of correlated omitted variables, these tests help to mitigate concerns about omitted macro-level and firm-level characteristics.

Next, we use two distinct sets of tests to further sharpen identification, and investigate whether the information advantage of politically connected insiders relates specifically to details of TARP capital infusions. In the first set of tests, we repeat our analyses after partitioning the sample of politically connected insiders based on whether their firm received TARP funds. We find that the increase in the informativeness of insider trades during the Crisis is concentrated among politically connected insiders at firms that received TARP funds, no evidence of an increase in informativeness of insider trades among politically connected insiders at firms that

did not receive TARP funds. This suggests the information advantage of politically connected insiders is conditional on the firm receiving TARP funds—and suggests the advantage relates to the details of the infusion, or its importance for firm value.

In the second set of tests, we use an event study to identify the relation between insider trades and the timing, amount, and market reaction to TARP infusions. Measuring trading by corporate insiders over the thirty days prior to the announcement of TARP capital infusions, we find that insiders are net buyers (sellers) before 34.8% (20.3%) of infusions in our sample. We find a pronounced increase in the trading volume of politically connected insiders 30 days prior to the announcement, and that these trades predict both the amount of the infusion and the market reaction to the announcement. For infusions where politically connected insiders were net buyers (sellers) over the prior thirty days, the average three-day announcement period return is 4.39% (−5.13%). Notably, similar results are not observed among insiders without political connections—insiders without political connections do not appear to time their trades to coincide with TARP infusions.

These results suggest that when politically connected insiders were net buyers (sellers) prior to the announcement, the infusion was a large positive (negative) surprise to the market. These results are consistent with politically connected insiders trading in anticipation of the surprise, and are robust to a variety of sensitivity tests. For example, similar results are not observed on non- announcement dates, among firms that did not receive TARP funds, or around other corporate events that are not directly related to TARP infusions (e.g., earnings announcements).³

³ An interesting question raised by our analysis is whether the behavior we document is evidence of illegal insider trading. From correlations alone, it is difficult to say whether a given trade or series of trades were illegal or not. This limitation is not unique to our study, and applies broadly to the academic literature on option backdating and

Collectively, our findings suggest that politically connected insiders had an information advantage during the Crisis and traded to exploit this advantage. We contribute to the prior literature by documenting a novel channel through which politically connected insiders can extract rents from shareholders, and by documenting the relation between political connections and information leakage during the Financial Crisis.

The remainder of the paper proceeds as follows. We discuss prior literature and the institutional setting in Section 2, the sample and measurement of key variables are described in Section 3, the results from our pooled regression and within-firm and within-insider tests are presented in Section 4, and the results from our tests relating to TARP infusions are reported in Section 5. We discuss several alternative explanations, robustness tests, and the legality of the trades in Section 6, and provide concluding remarks in Section 7.

2. Related Literature

Our study relates to three distinct literatures: political connections, government intervention during the Financial Crisis, and insider trading.

2.1. Political Connections

A large and growing literature examines the effect of political connections on firm outcomes. A substantial part of this literature suggests that political connections are beneficial to shareholders. In particular, several papers provide evidence of a positive association between political connections and firm value. For example, Cooper, Gulen, and Ovtchinnikov (2010) find that political contributions are positively correlated with future firm performance; Goldman, Rocholl, and So (2009) find positive abnormal returns to adding a politically connected

“forensic finance” (e.g., Ritter, 2008). We discuss the legality of the trades and the evolving legal landscape around insider trading in Section 6.2.

individual to the firm's board of directors; and within the context of the Crisis, Acemoglu et al. (2016) report that financial institutions connected to Timothy Geithner experienced an abnormal return of 6% on the day he was announced as nominee for Treasury Secretary.⁴

Beyond the relation between political connections and firm value, several studies examine specific benefits of political connections. These studies generally find that political connections facilitate access to capital (e.g., Khwaja and Mian, 2005; Faccio, Masulis, and McConnell, 2006; Leuz and Oberholzer, 2006; Claessens, Feijen, and Laeven, 2008), can be instrumental in winning government procurement contracts (e.g., Goldman, Rocholl, and So, 2013; Tahoun, 2014), and can favorably influence tax policy (e.g., Brown, Drake, and Wellman, 2014).

However, more recent studies suggest that political connections can also facilitate managerial opportunism by shielding managers from prosecution. For example, Yu and Yu (2011) and Correia (2014) suggest political lobbying reduces and delays the likelihood of prosecution by the United States' Securities and Exchange Commission, and Bourveau, Coulomb, and Sangnier (2016) suggests that managers connected to Nicolas Sarkozy were shielded from prosecution for violations of French insider trading laws after Sarkozy won the 2007 French presidential election.

We contribute to this literature in two regards. First, while benefits to political connections are relatively well understood, the costs to shareholders are less well understood. One missing aspect of this literature is evidence that managers are able to extract personal benefits from their political connections. We contribute to this literature by examining a specific

⁴ Outside the U.S., Fisman (2001) finds that politically connected Indonesian firms experience a negative stock market reaction to rumors of President Suharto's health. Bunkanwanicha and Wiwantanakangtan (2009) find that the market valuation of Thai firms increases when those firms' owners are elected for government positions. Amore and Bonnedsen (2013) find that future performance of family run firms in Denmark is increasing in the family's political influence.

channel through which managers can exploit their political connections to extract rents from shareholders—insider trading. We examine this question using large sample evidence from the U.S. during the 2007-2009 Financial Crisis.

Second, we contribute to this literature by suggesting an additional channel—other than a reduction in prosecution—through which managers might extract personal benefits from political connections. Lower levels of expected prosecution are unlikely to explain the relation between political connections and the informativeness of insider trades in our setting. For example, lower levels of expected prosecution cannot explain why the relation between political connections and informativeness of insider trades is limited to the period of government intervention in capital markets. If anything, the regime of strong legal enforcement in the U.S. implies that the probability of prosecution for violation of insider trading laws is non-trivial and is likely to be *higher* during these periods. Rather, we interpret the temporal and cross-sectional variation in the relation between political connections and the informativeness of insider trades during the Crisis as consistent with political connections conveying private information to insiders about forthcoming government intervention.

2.2. Government Intervention During the Financial Crisis

On September 18, 2008, congressional leaders met with Treasury Secretary Henry Paulson and Chairman Ben S. Bernanke and were briefed on a plan for massive government intervention in the financial system on a scale not seen since the Great Depression (Appelbaum and Montgomery, 2008). On September 22nd, a draft TARP bill (the Emergency Economic Stabilization Act of 2008) was circulated on Capitol Hill. The bill was eventually defeated in the House of Representatives on September 29th over concerns about inadequate transparency and the staggering size of funds requested (Hulse and Herszenhorn, 2008). A few days later, on

October 1st, the Senate considered and passed a revised TARP bill, which was subsequently passed by the House of Representatives and signed into law by the President on October 3rd.

As part of TARP-implementation, in mid-October 2008, the Treasury Department announced its intent to use TARP funds to purchase \$250 billion in equity in the form of preferred stock from “*a broad array of financial institutions.*” While participation was mandatory for the nine largest “too big to fail” banks, subsequent participation was voluntary.⁵ A total of 707 financial institutions received injections: 350 were publicly traded banks, 296 were private banks, 57 were thrifts, and 4 were non-bank financial institutions. Ultimately, TARP provided approximately \$205 billion in capital infusions and was effectively concluded by June 2009.⁶

Prior research suggests these “TARP infusions” resulted in significant changes in firm value. Bayazitova and Shivdasani (2012) document returns of approximately 15% for the October 14th announcement date for the initial nine recipients and approximately 4% for subsequent recipients (see also, Farruggio, Michalak, and Uhde, 2013). Ng, Vasvari, and Wittenberg-Moerman (2012) report that after the conclusion of the program, the portfolio of recipients outperformed non-recipients by 10.3% through December 2010. In total, Veronesi and Zingales (2010, p. 340) estimate that TARP “created between \$86 and \$108 billion in value.”

The unprecedented magnitude of government intervention during the Financial Crisis and its political nature provide a powerful setting to examine the relation between political connections and insider trading. Prior research suggests a considerable political dimension to

⁵ The nine banks initially receiving TARP were “forced” to take the infusion in order to mitigate concerns about adverse selection with respect to funding (e.g., Landler and Dash, 2008). These banks include: Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, Merrill Lynch, Morgan Stanley, State Street, and Wells Fargo.

⁶ 99.17% of funds were allocated prior to the end of June 2009. See Bayazitova and Shivdasani (2012) and Calomiris and Khan (2015) for more institutional details on TARP.

TARP funding. Mian, Sufi, and Trebbi (2010) find that finance industry campaign contributions were associated with politicians' votes on the bill that created TARP; Duchin and Sosyura (2012) find that political connections influenced the probability that a bank receives TARP funds; and Tahoun and Van Lent (2013) show that the financial interests of ranking members of finance-related Congressional subcommittees influenced the provision of TARP funds.

The collective evidence from prior studies points to substantial changes in shareholder wealth associated with government intervention during the Financial Crisis and that political connections influenced the probability, amount, and timing of TARP infusions. However, the extent of information leakage and insider trading around government intervention has not been previously examined. Given the nature of the intervention, it is plausible that politically connected corporate insiders had private information about the scope of government intervention, how this intervention would affect their firm, and details of any forthcoming bailout monies. We contribute to this literature by examining how the informativeness of insiders' trades during the Financial Crisis varies with political connections and government intervention, and by examining whether politically connected insiders appear to trade on private information about forthcoming TARP infusions.

2.3. Insider Trading Literature

It is illegal for insiders to trade while in possession of material non-public information (Securities and Exchange Acts of 1933 and 1934; Insider Trading Sanctions Act of 1984; Insider Trading and Securities Fraud Enforcement Act of 1988). However, a large body of prior research finds that corporate insiders appear to place, and profit from, trades based on superior information. Studies in this literature use the predictive ability of insider trades for future returns to judge the level of informativeness of the trade (e.g., Aboody and Lev, 2000; Jeng, Metrick,

and Zeckhauser, 2003; Cohen, Malloy, and Pomorski, 2012). In this regard, insiders' "information advantage" is defined relative to the information that has been priced by the market.⁷

Prior work in this literature suggests that the informativeness of insider trades varies with corporate governance (e.g., Bettis, Coles, and Lemmon, 2001; Ravina and Sapienza, 2010; Jagolinzer, Larcker, and Taylor, 2011), an insider's position on the board (e.g., Ravina and Sapienza, 2010; Cao, Dhaliwal, Li, and Yang, 2015), personal attributes of the insider (e.g., Davidson, Dey, Smith, 2014) and corporate culture (e.g., Gao, Lisic, and Zhang, 2014). Corporate insiders are also known to trade in a contrarian fashion, and in anticipation of future corporate events (e.g., Karpoff and Lee, 1991; Jenter, 2005).

Within the context of the Financial Crisis, prior research finds (at best) mixed evidence that insiders traded in anticipation of the Crisis. On the one hand, Bebchuk, Cohen, and Spamann (2010) report that top executives at Bear Stearns and Lehman Brothers "cashed out" \$1 billion in performance-based compensation between 2000 and 2008, and Bhagat and Bolton (2013) report that over the same period the dollar value of insider sales at the fourteen largest banks was 100 times the dollar value of insider purchases. Ryan, Tucker, and Wu (2016) find that insider sales prior to the Crisis predict write-downs on securitized loans during the Crisis.

On the other hand, Fahlenbrach and Stulz (2011) report that the CEOs at the top eighty banks *did not* significantly reduce their ownership between 2007 and 2008. Consistent with the notion that managers did not anticipate the Crisis, Cheng, Raina, and Xiong (2013) examine public real-estate transactions and find that managers of financial institutions aggressively increased their personal investments in the housing market leading up to the Crisis.

⁷ See Seyhun (1998) and Cohen, Malloy, and Pomorski (2012) for reviews of the insider trading literature.

Our paper aims to extend this literature in two ways. First, none of the prior studies explore the role of political connections in conveying an information advantage. As a result, little is presently known about the relation between political connections and insider trading—either generally, or during the Financial Crisis. Second, while prior literature finds insiders’ trades reflect private information, identifying the source of this information advantage is challenging (e.g., Ahern, 2017). We contribute to this literature by examining whether political connections are a potential external source of insiders’ private information—and specifically whether the information gleaned from political connections during the Financial Crisis relates to TARP capital infusions.

3. Data and Measurement

3.1 Sample

We collect data on insider trades from the Thomson Reuters Insider Filings (Form 4) database. Consistent with prior work, we restrict our analyses to open market purchases and sales of common equity and exclude option exercises, option grants, and gifts. We require the trade price, the number of shares transacted, and the date of the transaction for each trade. We restrict attention to trades by individuals classified as a Section 16 officer or director at a publicly traded financial institution and aggregate insider trades to the insider-month level.

The Crisis is generally thought to have started in July 2007 and concluded two years later, in June 2009. Accordingly, we restrict attention to insider trades between July 2005 and June 2011, inclusive. This provides a symmetric two year window both before and after the Crisis, and ensures all trades in our sample occur after the effective date of Sarbanes-Oxley Act of 2002, which required corporate insiders to report their trades electronically to the SEC within

two business days. We refer to the two year period, July 2005 to June 2007, as the “Pre-Crisis period,” the two year period, July 2007 to June 2009, as the “Crisis period,” and the two year period, July 2009 to June 2011, as the “Post-Crisis period.” We further divide the Crisis period into two periods: the “Pre-Bailout period” from July 2007 to September 2008, and the “Bailout period” from October 2008 to June 2009—the months in which 99% of TARP funds were disbursed.⁸

We merge the Thomson Reuters Insider Filings database with CRSP/Compustat to obtain data on stock returns, market value, book-to-market ratios, and earnings. To appear in the sample, we require market value at the end of the month, non-missing returns in the prior month ($t-1$) and prior year ($t-2$ to $t-12$), and book value of equity at the end of the prior fiscal quarter. Finally, we require data on insiders’ political connections. Our measure of political connections is taken from Duchin and Sosyura (2012), and covers all publicly traded financial institutions that were eligible for TARP funds (i.e., domestically controlled banks, bank holding companies, and saving and loan associations). After requiring data on political connections, the final sample for our cross-sectional tests consists of trades by 7,301 corporate insiders at 497 firms from July 2005 to June 2011, for a total of 29,777 insider-months.

For our subsequent event study tests, we collect data from U.S. Treasury Department TARP transaction reports. Among other details, TARP transaction reports contain the date the Treasury provided the capital infusion, the name of the institution receiving the infusion, and the amount of the infusion. After imposing the above data requirements and excluding the nine

⁸ NBER business cycle dates indicate that a recession starts in the fourth quarter of 2007 and continues through the end of the second quarter of 2009, but prior work generally considers the crisis to start at the beginning of the third quarter of 2007 (e.g., Acharya and Richardson, 2009; Brunnermeier, 2009; Fahlenbrach and Stulz, 2011). Inferences throughout the paper are robust, and are generally strengthened, if we define the Crisis as ending in March 2009 or December 2009, rather than June 2009. Inferences are also robust to use non-symmetric windows of longer length before and after the Crisis (e.g., beginning the sample in 2003). Figures 2 and 3 provide a sense of the robustness of our results to the definitions of various sample periods.

initial participants forced to take TARP funds, the resulting sample used in our event study tests consists of 256 capital infusions to 249 unique firms (“TARP recipients”) across 31 different calendar dates.⁹

3.2 Measure of Political Connections

Following Duchin and Sosyura (2012), we measure political connections based on whether the bank’s board includes at least one member with current or previous work experience at the Federal Reserve, a bank regulator, i.e., the Federal Deposit Insurance Corporation (FDIC), Office of Thrift Supervision (OTS), or Office of the Comptroller of the Currency (OCC), Treasury, or Congress. Work experience is determined by analyzing each director’s biographical data as provided in the BoardEx database and the firm’s proxy statements. Officer and directors at banks in which one or more members of the board list such work experience are deemed “politically connected insiders.”¹⁰

In the context of our research question, this measure of political connections has two desirable properties. First, unlike other measures such as campaign contributions or lobbying expenditures, directors’ work experience allows us to detect direct connections to bank regulators—a connection that would be more difficult to infer from contributions to political campaigns. Second, our measure focuses broadly on whether an insider’s close professional network within the firm, i.e., the board of directors, contains someone with direct political or regulatory ties rather than focusing narrowly on the individual with the tie. Anecdotal and empirical evidence suggests that there is significant sharing of private information among board

⁹ Our sample of TARP capital infusions excludes Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, Merrill Lynch, Morgan Stanley, State Street, and Wells Fargo, as these banks were “forced” to take the infusion and had no basis on which to anticipate TARP funding (inclusion of these banks does not affect our results). Our sample is similar in size to prior research. For example, after imposing data requirements, Bayazitova and Shivdasani (2011) and Duchin and Sosyura (2012) examine a sample of 286 TARP recipients, Ng, Vasvari, and Wittenberg-Moerman (2011) examine a sample of 186 recipients, and Farruggio, Michalak, and Uhde (2013) examine a sample of 125 recipients.

¹⁰ We thank Ran Duchin and Denis Sosyura for providing these data.

members (e.g., Cao, Dhaliwal, Li, and Yang, 2015; Kim, 2016), and an analysis of prosecuted cases suggests that trading on private information typically occurs more than two degrees of separation from the source of private information (e.g., Ahern, 2017). It seems plausible that information about pending regulatory action and TARP infusions would be shared either formally or informally within the firm's board of directors during the Crisis (see the first-hand accounts detailed in Sorkin, 2009). Hence, we include a broad set of corporate insiders who interact both professionally and socially on a routine basis. If anything, misclassifying individuals as connected when no such connection exists, biases against finding a relation between connections and their trades.

3.3. Descriptive statistics

Table 1 presents descriptive statistics for our sample. Panel A suggests that the typical firm in our sample has a market capitalization of roughly \$250 million (mean natural logarithm of market value, *Size*, of 5.59), a book-to-market ratio of about 1.02, and negative returns in both the prior month (mean *PastMoRet* of -1.78%) and the prior year (mean *PastYrRet* of -5.11%). Panel A also suggests that the Board of the average firm in our sample has 0.58 directors with political connections (mean *NumPolConn* of 0.58); and that slightly more than half of the sample received TARP funds (mean *TARPRecipient* of 0.56).¹¹

Panel B suggests that 34% of all trades are made by politically connected insiders (mean *PoliticalConn* of 0.34); that the majority of all trades in the sample are net purchases (mean *Buyer* of 0.68); and that the total dollar volume of insider purchases (sales) is just over \$1.5

¹¹ We winsorize all continuous variables at the 1st and 99th percentile and exclude all insider trades (aggregated by month) of \$100 million or more. This results in the exclusion of 11 transactions: a \$737 million dollar open market sale by Citigroup director Roberto Hernandez Ramirez on November 9, 2006 (shares were acquired in connection with Citigroup's purchase of Banamex), a \$288 million dollar open market sale by a Etrade director Kenneth Griffin on April 29, 2010 (in his capacity as CEO of Citadel LLC), and nine purchases of \$100 million or more in connection with MatlinPatterson Global Advisers' investment in FlagStar bank in January, March, and November of 2010. All of these 11 transaction occurred outside the Crisis period, and thus do not affect our inferences.

billion (\$6.1 billion). The observation that the dollar value of insider sales is so much larger than that of purchases is consistent with prior research on insider trading outside the context of the Crisis (e.g., Ravina and Sapienza, 2010). Thus, while the number of purchase transactions is greater than the number of sales transactions, the dollar volume of sales is substantially larger.¹²

Panel B also reports descriptive statistics after partitioning the sample based on insiders' political connections. There are 2,776 (4,546) distinct insiders with (without) political connections in our sample, covering 10,204 (19,573) trades at 159 (338) distinct firms. Panel B reports the total value of purchases by insiders with (without) political connections is \$0.77 billion (\$0.79 billion); the total value of sales by insiders with (without) political connections is \$4.5 billion (\$1.6 billion); and 54% (74%) of trades by insiders with (without) political connections are net purchases. The statistics suggest insiders with political connections are both more likely to sell shares, and sell shares in greater volume.

Panel C presents a correlation matrix of firm and insider trade characteristics. Consistent with prior research that suggests insiders tend to purchase more in small firms, are value investors, and are contrarians (e.g., Lakonishok, and Lee, 2001), we find *Buyer* is negatively correlated with firm size (*Size*), positively correlated with the book-to-market ratio (*BM*), and negatively correlated with returns over the past month (*PastMoRet*) and past year (*PastYrRet*). These correlations are statistically significant at the 0.10 level or less (two-tailed).

3.4. Insider Trading During the Crisis

¹² The observation that purchases occur more frequently than sales, contrasts with evidence in prior studies that examines the universe of industries and stocks and finds the opposite (e.g., Piotroski and Roulstone, 2005; Ravina and Sapienza, 2010). In untabulated analysis, we find this phenomenon is driven by our focus on insider trades at banks. Expanding the sample to include insider trades in all firms, i.e., including non-banks over the same period, we find only 29% of trades are purchases—consistent with prior work that finds sales are more prevalent than purchases in the broader cross-section of firms.

Table 2 presents average values of our measures of insider trading activity in each period. All trades are aggregated to the insider-month level. Panel A suggests that 55% of trades are buys prior to the Crisis, 76% during the Pre-Bailout period, and 78% during the Bailout period. Figure 1 plots the total dollar value of insider purchases and sales over time. Figure 1 suggests the dollar volume of insider sales dropped dramatically at the start of the Crisis, whereas the dollar volume of insider purchases increases at the start of the Crisis and is the highest during the start of the Bailout period (i.e., the six month period ended December 2008).

Panel B of Table 2 present average values for our measures of insider trading activity after partitioning the sample based on insiders' political connections. Panel B suggests that insiders with political connections tend to be net *sellers* before the Crisis (39% of trades in the Pre-Crisis period are purchases), net *buyers* during the Crisis (68% of trades in the Crisis period are purchases), and that their buying is most intense in the Bailout period (74% of trades in the Bailout period are purchases). Panel B also suggests that, while insiders with political connections tend to purchase less frequently than insiders without political connections (mean *Buyers* of 0.54 and 0.74 respectively), the difference in buying activity narrows significantly during the Bailout period (mean *Buyers* of 0.74 and 0.80 respectively).

Panel B reports that insiders with political connections typically account for around 73% of sales volume, and their share of sales volume decreases sharply to 43% during the Bailout period. Conversely, insiders with political connections typically account for around 49% of purchase volume, and their share of purchase volume increases sharply to 71% during the Bailout period.

Collectively, the evidence suggests a relative increase in buying among insiders with political connections during the Crisis. The percentage of trades and percentage of dollar volume

that are purchases increases during the Bailout period, and this increase appears concentrated among insiders with political connections. However, there is significant cross-sectional variation in insider trading activity within each period. This within-period variation is the basis for our subsequent tests regarding the predictive ability of insider trades for the cross-section of future performance.

4. Predictive Ability of Insider Trades for Future Performance

4.1. Difference in Future Returns between Purchases and Sales

The evidence thus far speaks to patterns in the trading behavior of corporate insiders, but does not speak to the extent to which those trades anticipate future performance. Our primary tests examine the informativeness of insider trades, and how it relates to political connections and government intervention during the Financial Crisis. Following a large insider trading literature, we measure the informativeness of insider trades based on the predictive ability of the trades for future returns. If trades are based on private information, future returns should be higher (lower) among firms where insiders buy (sell) (e.g., Aboody and Lev, 2000; Lakonishok and Lee, 2001; Jeng, Metrick, and Zeckhauser, 2003; Cohen, Malloy, and Pomorski, 2012). In this regard, we measure insiders' information advantage, or the informativeness of their trades, relative to the information already impounded in prices.

Panel A of Table 3 presents average one-month-ahead future returns separately following purchases and sales. Consistent with findings in prior literature, we find the direction of insider trades is associated with the sign of subsequent stock returns. Over the full sample period, purchases foreshadow positive future returns (0.23%) and sales foreshadow negative future returns (−0.82%). Consistent with prior work, the difference in returns following purchases and

sales is both economically and statistically significant, 1.05% *per month* over the entire sample (*t*-stat. of 6.76). Panel A of Table 3 also reveals that the predictive ability of insider trades for future performance is greater during the Bailout period than any other period in our sample. Both the predictive ability of insider *purchases* for positive future performance and the predictive ability of insider *sales* for negative future performance increase during this period. During this period, the average one-month-ahead future return following purchases (sales) is 1.84% (–2.87%), a difference of 4.71%.

Panel B of Table 3 presents results after partitioning the sample based on insiders' political connections. Panel B reveals that the difference in one month ahead returns between purchases and sales during the Bailout period is 8.89% (2.81%) for insiders with (without) political connections—larger than during any other period in the sample. The difference in these differences, 6.08%, is both economically and statistically significant (*t*-stat of 3.81). Notably, in all periods except the Bailout period, the difference in returns between purchases and sales appears unrelated to insiders' political connections (difference-in-differences *t*-stats of –1.50, 0.89, and –0.10 during the Pre-Crisis, Pre-Bailout, and Post-Crisis periods, respectively).

Figure 2 plots the difference in one-month-ahead future returns between purchases and sales over time. Figure 2 suggests the difference in future returns is negligible prior to the Crisis, is increasing as the Crisis unfolds, and remains elevated until the end of 2010. This suggests the finding that the predictive ability of insider trades increases during the Crisis is not sensitive to lengthening the definition of the Crisis period to include all of 2009. Figure 3 plots the difference in one-month-ahead future returns between purchases and sales separately for insiders with and without political connections. Figure 3 shows a dramatic spike in the predictive ability

of politically connected insiders' trades for future returns during the period in which TARP funds were distributed (October 2008 – June 2009),

We next test whether the univariate patterns in future returns following insider trades are robust to: (i) controlling for cross-sectional determinants of returns, (ii) controlling for characteristics of the insider and the firm, (iii) controlling for contemporaneous changes in market conditions, (iv) controlling for differences in market conditions between firms with and without politically connected insiders, and (v) measuring returns over longer horizons.

4.2. Pooled Regression Tests

Following Cohen, Malloy, and Pomorski (2012) we estimate regressions of the form:

$$BHR_{t+s} = \delta_1 Buyer_t + \theta Controls_t + \varepsilon_{t+1}, \quad (1)$$

where BHR_{t+s} is either returns over the next month ($s = 1$) or over the next six months ($s = 6$), $Buyer$ is an indicator variable equal to one if the number of shares bought exceeds the number of shares sold in month t , $Controls$ is the vector of control variables that includes firm size ($Size$), the book-to-market ratio (BM), returns in the past month ($PastMoRet$), and returns in the past year ($PastYrRet$). All variables are defined in Table 1. In this specification, δ_1 represent the difference in future returns between buys and sells, after controlling for size, book-to-market, and past returns.

To examine whether the predictive ability of insider trading activity increases during the Crisis, before or after government intervention, we include separate indicator variables for the Pre-Bailout period ($Crisis_PreBailout$) and the Bailout period ($Crisis_Bailout$), and interact these variables with $Buyer$.

$$BHR_{t+s} = \delta_1 Buyer_t + \delta_2 Buyer_t * Crisis_PreBailout + \delta_3 Buyer_t * Crisis_Bailout + \beta_1 Crisis_PreBailout + \beta_2 Crisis_Bailout + \theta Controls_t + \varepsilon_{t+1}, \quad (2)$$

In this specification, δ_2 and δ_3 measure the *incremental* informativeness of insider trades during the Crisis, prior to and following government intervention, respectively. Throughout our analysis, we aggregate insider trades to the insider-month level and base inferences on standard errors clustered by firm. Clustering by firm allows for both arbitrary time-series correlation within a firm and arbitrary correlation across insiders within a given firm.¹³

Table 4 reports results from estimating equations (1) and (2). Panel A presents results from measuring future returns at the one month horizon. On average, across the entire sample period, results in columns (1) suggest insider trades are related to future returns (*Buyer*, *t*-stat of 2.04). Column (2) allows the relation between insider trades and future returns to vary over time. We find no evidence of a change in the informativeness of insider trades during the Crisis prior to the creation of TARP (*Buyer*Crisis_PreBailout* *t*-stat of -0.07), and a marked increase in the informativeness of insider trades during the Crisis following the creation of TARP (*Buyer*Crisis_Bailout* *t*-stat of 2.49).

To examine the relation between political connections and the informativeness of insider trades, we estimate equation (2) separately for the sample of insiders with and without political connections. Column (3) presents results from estimating equation (2) on the sample of insiders with political connections, column (4) presents results from estimating equation (2) on the sample of insiders without political connection, and column (5) tests for differences in coefficients between the two samples.

¹³ In untabulated analyses, we find inferences are unaffected by the following changes to our analysis: (i) aggregating insider-month observations to the firm-month level; (ii) disaggregating insider-month observations to the individual transaction level; (iii) clustering standard errors by insider, (iv) two-way clustering standard errors by insider and month, (v) using the algorithm in Cohen, Malloy, and Pomorski (2012) to classify trades into “opportunistic” and “routine” trades and excluding routine trades from our sample; (vi) replacing *Buyer* with the insider buy-sell imbalance, defined as the number of shares purchased less number of shares sold, scaled by the number purchased plus the number of sold (e.g., Jagolinzer, Larcker, and Taylor, 2011); and (vii) replacing *Buyer* with the purchase ratio, defined as the dollar value of purchases scaled by dollar value of purchases and sales (e.g., Piotroski and Roulstone, 2005).

Interestingly, the coefficient on *Buyer*Crisis_Bailout* is economically and statistically significant *only* for the trades of politically connected insiders (*Buyer*Crisis_Bailout* *t*-stat of 3.35 in column (3) and *t*-stat of 0.62 in column (4)). Column (3) suggests that among the trades of politically connected insiders, the difference in one-month returns between purchases and sales increases from an economically and statistically insignificant -0.20% in the non-Crisis period to 6.23% during the Bailout period (a difference of 6.43% , *t*-stat of 3.35). Additionally, column (5) suggests that the coefficient on *Buyer*Crisis_Bailout* estimated on the sample of trades by insiders *with* political connections, is economically and statistically larger than the same coefficient estimated on the sample of trades by insiders *without* political connections (difference of 5.81, *p*-value <0.01).

Notably the difference in informativeness of insider trades between individuals with and without political connection is unique to the Bailout period. In particular, the difference in the coefficients on both *Buyer* and *Buyer*Crisis_PreBailout* are not statistically significant and of a much smaller magnitude (differences of -0.75 and 0.27 , *p*-value of 0.15 and 0.85 respectively). We also note that the differences in the coefficients on the control variables are generally not statistically significant, which suggests that the determinants of the cross-section of returns do not vary with insiders' political connections.

Panel B presents similar results measuring future returns over the six month horizon. For example, column (3) suggests that among the trades of politically connected insiders, the difference in six-month returns between purchases and sales increases from 4.30% in the non-Crisis period to 23.08% during the Bailout period (a difference of 18.78% , *t*-stat of 3.60). Similar to Panel A, column (5) of Panel B suggests that the coefficient on *Buyer*Crisis_Bailout* estimated on the sample of trades by insiders *with* political connections, is economically and

statistically larger than the same coefficient estimated on the sample of trades by insiders *without* political connections (difference of 20.17, p -value <0.01).

The observation that the coefficient on *Buyer* varies over time is particularly heightened during the Bailout period, and is only among sample of politically connected insiders, suggests that any time-invariant firm characteristic is unlikely to explain our results. To explain our results, an omitted variable would need to explain not only why buys (sells) are followed by positive (negative) returns, but also why the difference in returns following buys and sells increases during the Bailout period, and why the increase is concentrated among the trades of insiders with political connection.

4.3. *Within-Firm and Within-Insider Tests*

Table 5 reports results from estimating equation (2) separately for the trades of insiders with and without political connections, after including month and *firm* fixed effects and alternatively, month and *insider* fixed effects. This fixed effect structure is an important feature of our design and it is instructive to highlight three aspects of this feature.

First, month-level fixed effects control for changes in market conditions that affect all firms within a given period. Moreover, we include separate month fixed effects in each sample of insider trades. By doing so, we explicitly account for the possibility of different time period effects (and different time trends) between the two samples. Including separate month fixed effects in each sample allows for the possibility that market conditions might differentially affect firms with and without politically connected insiders. In this regard, each regression can be thought of as a *within-month* analysis.¹⁴

Second, in the presence of firm-level fixed effects, our design relies on *within-firm* variation. As such, the inclusion of firm-fixed effects helps mitigate concerns about omitted

¹⁴ Month fixed effects subsume the main effects of *Crisis_PreBailout* and *Crisis_Bailout* respectively.

firm-level characteristics that might be correlated with future performance, insider trading activity, and political connections. In this regard, our firm fixed effects control for any time-invariant, cross-sectional differences between firms with and without political connections (e.g., corporate governance, TARP funding decision, pre-Crisis exposure to housing market).

Third, in the presence of insider-level fixed effects, our design relies on time-series variation in trading activity *within-insider*. In this regard, insider-level fixed effects control for heterogeneity in time-invariant insider-specific characteristics that might influence trading activity, for example a time-invariant information advantage (e.g., Davidson, Dey, and Smith, 2014). In our sample, insider fixed effects subsume firm fixed effects, and thus, also serve to control for any time-invariant firm-level characteristics.¹⁵ Tests that include insider fixed effects are based on a slightly smaller sample that requires at least two trades per individual.

Columns (1) through (3) of Table 5 present results from including month and firm fixed effects, and columns (4) through (5) of Table 5 present results from including month and insider fixed effects. For parsimony, coefficients on control variables are not tabulated and we present results only for future returns over the subsequent six months. The results in Table 5 suggest that our inferences are robust to these alternative fixed effect designs. We continue to find strong evidence of an increase in the informativeness of insider trades during the Bailout period, and only among politically connected insiders. This suggests our results are not driven by temporal changes in market conditions, a differential effect of market conditions on firms with and without politically connected insiders, or time-invariant characteristics of the firm or the insider.

5. TARP Infusions

¹⁵ We find there are only 21 insiders who overlap across multiple firms in our sample, and thus we cannot jointly estimate both firm and insider fixed effects. In our sample, insider fixed effects are effectively equivalent to insider-firm fixed effects.

We next use two distinct sets of tests to sharpen identification and investigate whether the information advantage of politically connected insiders during the Crisis relates specifically to TARP capital infusions.

5.1. TARP Funding Status and the Increase in the Information Advantage of Politically Connected Insiders

In the first set of tests, we focus specifically on the trades of politically connected insiders. Within the sample of trades by politically connected insiders, we partition the sample based on whether the politically connected insider is at a firm that received TARP funds. If the information advantage of politically connected insiders relates to private information about the probability their firm receives TARP funds, we expect to find politically connected insiders at both types of firms—firms that did, and did not, receive TARP funds—have an information advantage. In contrast, if the information advantage relates to details of the funding itself, that is, if the information advantage is conditional on being selected to receive funding, we expect to observe the information advantage of politically connected insiders is concentrated in firms that received TARP funds, and minimal to no evidence that politically connected insiders have an information advantage in firms that did not receive TARP funds.

Table 6 presents results from repeating the analysis in Table 5 after partitioning the sample of politically connected insiders based on whether their firm received TARP funds. Columns (1) through (4) of Table 6 present results from including month and firm fixed effects, and columns (5) through (8) present results from including month and insider fixed effects. For parsimony, coefficients on control variables are not tabulated and we present results only for future returns over the subsequent six months. Results for the trades of politically connected

insiders at TARP recipients are considerably stronger than those of politically connected insiders at non-recipients. For example, column (2) suggests that the coefficient on *Buyer*Crisis_Bailout* is 13.65 for TARP recipients compared to 3.65 for non-recipients (difference of 10.00, *p*-value 0.09).

Collectively the results in Tables 4 and 5 suggest a pronounced increase in the informativeness of trades by politically connected insiders during the Bailout period, and no such increase for the trades of non-connected insiders. Table 6 suggests that the increase in the informativeness of trades during the Bailout period is concentrated specifically among politically connected insiders *at firms that received TARP funds*. We find no evidence of an increase in informativeness of insider trades among politically connected insiders at firms that did not receive TARP funds. The combination of these results is consistent with the notion that the information advantage of politically connected insiders during the Crisis relates to the details of their firms' TARP capital infusions and their implications for firm value.

5.2. Event Study: Insider Trading around TARP Infusions

In the second set of tests, we use an event study to more closely identify the relation between insider trades and the timing, amount, and market reaction to TARP infusions. Specifically, we analyze both the timing and information content of insider trades around the announcement of TARP infusions. We use the short-window market reaction to the announcement of TARP infusions as a proxy for the extent to which the infusion surprised the market. If insiders traded on private information about the infusion (information that was not priced by the market), then we expect to observe a relation between insider trades before the announcement and the short-window market reaction to the announcement.¹⁶

¹⁶ Jagolinzer, Larcker, and Taylor (2011) use a similar design to examine the relation between insider trades and earnings news.

For this analysis, we focus only on firms in our sample that received TARP capital infusions. Our sample for these tests consists of 256 capital infusions corresponding to 249 unique firms and 31 unique calendar dates (i.e., dates of TARP capital infusions vary by firm). Our search of firm disclosures on Factiva and Lexis-Nexus suggests the vast majority of announcements occur within one trading day of the infusion. We refer to the first full trading day after the infusion as day 0, and use a three-day window $[-1,+1]$ to measure the market reaction. We find this window encompasses 95% of announcements.¹⁷

Figure 4 presents aggregate dollar volume of trading by corporate insiders at TARP recipients over the 60 days prior to the announcement, and the 60 days after the announcement. Figure 4 shows a clear increase in volume 30-days prior to the announcement, and that this increase is concentrated entirely among insiders with political connections. For politically connected insiders, volume increases from \$32 million over the $[-60, -31]$ to \$105 million during the $[-30, -1]$ window, and then falls to \$16 million during the $[0, +30]$ window, and is similar to pre-TARP levels, \$26 million, during the $[+31, +60]$ window

Panel A of Table 7 presents descriptive statistics for variables used in our event study tests. Focusing on the thirty trading days prior to the announcement, i.e., the $[-30, -1]$ window, we find that insiders traded during this window in 55% of infusions ($89 + 52 / 256$), that a total of 383 unique insiders traded during this period, and that the total dollar volume of their trades was \$118 million. Panel A also shows that the amount of the average infusion is substantial. For the average firm in our sample, the infusion represents 42.48% (2.28%) of prior quarter market value (book value of assets).

¹⁷ Section 114(a) of Emergency Economic Stabilization Act of 2008 stipulates that the Treasury must publicly disclose recipients within 48 hours of the infusion. Recall that our sample of TARP infusions excludes the nine initial, and largest, banks.

Panel A also presents descriptive statistics separately for the sample of infusions where insiders at the firm are net buyers (shares bought exceeds shares sold) and net sellers (shares sold exceeds shares bought) over the $[-30,-1]$ window. We find that insiders are net buyers before 34.8% of infusions in our sample and net sellers before 20.3% of infusions in our sample. Infusions where insiders are net buyers (sellers) are much larger (smaller) as a percentage of prior quarter market value and are associated with more of a positive (negative) announcement returns.

Panel B of Table 7 presents descriptive statistics for each of the four groups of infusions. Panel B suggests that, for infusions where politically connected insiders were net buyers (sellers) over the prior thirty trading days, the average infusion is 30.71% (21.80%) of prior quarter market value and the three-day announcement period return is 4.39% (-5.13%). These results suggest that when politically connected insiders were net buyers (net sellers) in advance of the announcement, the subsequent announcements were a large positive (negative) surprise to the market. Similar results are not observed for insiders without political connections.

Panel C of Table 7 presents results from regressing three-day announcement period returns on control variables from equation (1) and an indicator for whether insiders at the firm were net buyers over the prior thirty trading days, i.e., $Buyer(-30,-1)$. The coefficient on $Buyer(-30,-1)$ is positive and statistically significant *only* in the subsample of firms where insiders are political connected (sample of 94 infusions, t -stat of 2.24). We find no evidence of a relation between insider trades and announcement period returns among the subsample of firms where insiders are not politically connected (sample of 162 infusions, t -stat of -0.95).

Collectively, the results from our event study analysis are consistent with politically connected insiders trading in anticipation of the infusion. They suggest not only that the

information content of the trades is related to TARP infusions, but that insiders timed their trades in relation to the infusion announcement. This is difficult to reconcile with alternative explanations. At the very least, it suggests insiders opportunistically timed their trades relative to the infusion announcement.¹⁸ The timing of these trades cannot be easily explained by omitted firm characteristics. We next conduct a battery of sensitivity analyses to assess the robustness of our results.

5.3. Event study: Falsification Tests

To examine whether our event study results are an artifact of test misspecification, we employ two falsification tests. If our tests are well-specified, and the relation between insider trades and announcement period returns is due to insiders trading on private information in anticipation of TARP infusions, then we should not expect to observe similar results among TARP recipient on non-announcement dates (i.e., holding the firms fixed and altering the dates), and should not expect to observe similar results for non-recipients on the announcement dates (i.e., holding the dates fixed but altering the sample of firms).

Panel A of Table 8 presents results from holding the set of TARP recipients fixed, and estimating the regression specifications in Panel C of Table 7 for the same firms on all non-announcement dates from October 2008 to June 2009 (i.e., the Bailout period). We then test whether the difference in the estimated regression coefficient for insiders with and without political connections during the announcement period is different from that during the non-announcement period. Comparing the results between announcement and non-announcement periods enables us to rule out the possibility that what we are documenting is a general

¹⁸ While firms often use “trading blackout windows” to limit insider trading in the months before material corporate events like earnings announcements, these windows are crafted and enforced by the firm, not by the SEC (e.g., Bettis, Coles, and Lemmon, 2001; Jagolinzer, Larcker, and Taylor, 2011). Given the number of infusions with insider trades thirty days before the infusion and the volume of trade, it is clear that blackout windows did not apply to TARP infusions, or were not enforced, at the majority of firms in our sample.

phenomenon among firms that receive TARP funds, and not a specific effect related to the announcement of a TARP infusion.

Panel A of Table 8 reports a statistically significant relation between the trades of politically connected insiders and returns on non-announcement dates (t -stats of 5.83 and 3.79 for the trades of insiders with and without political connections respectively). Given prior results that insider trades predict returns even in the absence of TARP infusions, finding evidence of a positive relation between insider trades and returns during non-announcement dates should not be surprising. The important result is that the relation between political connections and the informativeness of insider trades is statistically and economically much larger during the announcement dates than during the non-announcement dates (diff-in-diff p -value = 0.03).

Panel B of Table 8 presents results from holding announcement dates fixed, and estimating the regression specifications in Panel C of Table 7 for firms that did not receive TARP infusions. We then test whether the difference in the estimated regression coefficients between insiders with and without political connections on the announcement date is different from that of non-recipients on the announcement date. For firms that did not receive TARP, we find no evidence of a relation between political connections and the informativeness of insider trades. We find that the relation between political connections and the informativeness of insider trades is statistically and economically much larger for TARP recipients during the announcement than for non-recipients during the announcement (diff-in-diff p -value = 0.01). Collectively, we interpret the results in Table 8 as suggesting that the relations we document are not observed in the absence of the announcement of TARP infusions, and are unique to such events.

5.4 Event study: Distinguishing TARP Infusions from Other Information Events

Next, we compare the informativeness of politically connected insider trades' around TARP infusions with the informativeness of their trades around other corporate information events not directly related to TARP infusions—specifically, earnings announcements. Since we conjecture that political connections provide insiders with private information specific to TARP funding, we expect to find the informativeness of politically connected insiders' trades for TARP infusions exceeds that of earnings announcements.

To conduct this test, we begin with the sample of 249 firms in our sample that received TARP infusions (recall that we exclude the initial nine recipients). We then gather data on quarterly earnings announcements during the Crisis period for these firms, three-day returns centered on the announcement date, and our control variables. This results in a sample of 1,619 earnings announcements for TARP recipients during the Crisis. We then repeat our earlier tests in Panel C of Table 7, measuring insider trading over the thirty days prior to the earnings announcement.

Columns (1) through (3) of Table 9 report results for estimating our tests around TARP infusion announcements, and columns (4) through (6) report results for estimating the same tests around earnings announcements. For TARP recipients, we find no evidence that insider trades thirty days before earnings announcements predict the market reaction to the announcement during the Financial Crisis—regardless of political connections (t -stats of 0.62 and -0.14 for trades of insiders with and without political connections respectively). The relation between political connections and the informativeness of insider trades is statistically and economically much larger around the announcement of TARP infusions than it is around earnings announcements (diff-in-diff p -value = 0.04). Collectively, we interpret the results in Table 9 as

suggesting that the associations we document are unique to TARP infusions. These associations are not observed around information events not directly related to TARP infusions.

6. Additional Considerations and Alternative Explanations

6.1. Information Sharing

Our primary tests use data on firm-level political connections collected by Duchin and Sosyura (2012). In our setting, a key assumption of a firm-level measure of political connections is that privileged information is shared among board members. While this assumption is consistent with anecdotal and empirical evidence (e.g., Sorkin, 2009; Cao, Dhaliwal, Li, and Yang, 2014; Kim, 2016), information sharing among board members is difficult to verify in the absence of direct observation. Nevertheless, we conduct several supplemental analyses in an attempt to validate this assumption.

Specifically, we use the BoardEx database to construct a (partial) network map for 123 politically connected firms with available data. Figure 5 provides an example. We distinguish insiders with direct connections to government agencies from insiders who are connected “through” another insider’s connection (an indirect connection), and distinguish recent connections from stale connections. If insiders share information with other members, both directly connected individuals and indirectly connected individuals should hold an information advantage over unconnected individuals. Moreover, the strength of the direct connection—measured using its recency—should manifest in the information advantage of both directly and indirectly connected individuals.

We identify 196 individual board members (at 123 firms) who trade during our sample and who had current or prior work experience at the Federal Reserve, Treasury, Congress, or a bank regulator (i.e., OCC, OTS, or FDIC) at the time of the Crisis. We refer to these individuals

as “directly connected,” and all other officers and directors *at the same firm* as “indirectly connected” (2,056 individuals). Within the 196 individuals with direct connections, we can identify the end date of the work experience for 92 individuals.¹⁹ We then calculate the recency of the direct connection, *Recency*, as the last year of the relevant work experience relative to 2008 (the year of TARP). For example, if a director stepped down from the Federal Reserve Board in 2005, then *Recency* is -3 . We set *Recency* to zero for individuals where the relevant position with the government agency ends on/after 2008).²⁰ In cases where an individual has multiple direct connections (e.g., to both Treasury and the Federal Reserve), we use the most recent connection to calculate *Recency*. Average *Recency* among the 92 individuals with direct connections is -8 , and the 25th, 50th, and 75th percentiles are -12 , -3 , and 0 . To facilitate exposition, we refer to connections with above (below) median recency as “recent” (“stale”) connections, where the median of -3 corresponds to a position that ended in 2005.

Panel A of Table 10 presents results from repeating the analysis in Table 4 after partitioning politically connected insiders based on whether they have a direct connections or an indirect connection. For reference, columns (1) and (2) present results from using the firm-level measure of political connections in Duchin and Sosyura (2012) to distinguish connected and unconnected individuals (i.e., Panel B of Table 4). Columns (3) and (4) present results after distinguishing direct and indirect connections; these regressions are conducted within the set of politically connected firms with data on BoardEx. Consistent with information sharing, we find a marked increase in the informativeness of trades following the creation of TARP for both

¹⁹ Calculating end dates is not always possible, even when hand collecting data from proxy statements; it is rare for executive biographies to provide the dates of prior government employment.

²⁰ For example, there is no rule against sitting board members of financial institutions serving on a Federal Reserve board, and the practice is quite common. For such individuals, *Recency* is coded as zero.

directly *and* indirectly connected insiders (but no evidence of an increase among unconnected insiders).

Panel B of Table 10 presents results from partitioning insiders both by the type of connection (direct/indirect) and the recency of the associated direct connection (recent/stale). Columns (1) and (2) report results for all recent and stale connections, respectively. Columns (3) and (4) report results for recent and stale *direct* connections. Columns (5) and (6) report results for recent and stale *indirect* connections. We find results are generally concentrated among insiders with recent, direct connections, and that insiders who are *indirectly* connected through an insider with a *stale* direction connection (i.e., column (5)) do not experience a significant increase in the informativeness of their trades in connection with TARP. Collectively, the evidence in Table 10 supports the notion of information sharing among board members.²¹

6.2. Legal Considerations

An interesting question raised by our analysis is whether the behavior we document is evidence of illegal insider trading. Like many empirical analyses, we rely on large-sample correlations. From correlations alone, it is difficult to say whether a given trade or series of trades were illegal or not. This limitation is not unique to our study, and applies broadly to the academic literature on insider trading and option backdating.²² Nevertheless, our analysis casts suspicion on trades of politically connected insiders during the Crisis, especially those trades that occurred in close proximity to TARP infusion announcements.

It is likely to be the case that the trades we study fall into a legal gray area. For example, suppose a director hears from his regulatory contact in the Treasury that there is a very high probability (but not certainty) that the bank will receive a TARP infusion. While the probabilistic

²¹ We caveat that, after requiring BoardEx data and partitioning observations into a 2 x 2 matrix (direct/indirect x recent/stale), the sample is drastically smaller and these tests potentially suffer from low power.

²² See Ritter (2008, p. 135) for a related discussion in the context of option backdating.

assessment is based on private information, it seems inappropriate for the firm to disclose such information (which some might call “rumor” or “scuttlebutt”). Given the private information is not hard fact, but an informed probabilistic assessment and disclosure is inappropriate, it is an open question whether existing disclose-or-abstain rules (i.e., Rule 10b-5) require the director to abstain from trading until a public announcement of the infusion.²³ Consciously, or unconsciously, the director may very well take the probabilistic assessment—one based on private information—into account when trading the firms’ shares, resulting in trades that (on average) are more predictive of future performance.

6.3 Regulatory Considerations

It is well known that TARP infusions were conditional on the acceptance of certain limits on the amount and forms of executive compensation.²⁴ Specifically, the Treasury Department imposed i) a \$500,000 tax deduction limit, ii) a \$500,000 compensation cap, iii) bonus clawbacks, iv) bonus prohibitions, v) an initial cap and subsequent prohibition of golden parachutes, and vi) a requirement that the compensation committee ensures that top executives’ compensation contracts do not introduce incentives to take excessive risk (see, for example, Cadman, Carter, and Lynch (2010) and Bayazitova and Shivdasani (2012) for more details on these restrictions).

Our results are unlikely to be driven by these temporary restrictions on executive compensation for several reasons. First, the regulation did *not* introduce any minimum stock ownership requirements that might be expected to mechanically increase insider purchases

²³ See, for example, the evolving debate on what constitutes insider trading in the wake of *United States v. Newman and Chiasson* (e.g., Henning, 2015; Wieczner, 2016) and the discussion surrounding disclose-or-abstain in Li, Wasley, and Zimmerman (2016).

²⁴ These restrictions were initially introduced under the Emergency Economic Stabilization Act of 2008 and subsequently modified by the Treasury Department in February of 2009, and extended by the American Recovery and Investment Act of 2009, also in February of 2009.

among TARP recipients. Second, these restrictions would not be expected to systematically trigger both insider purchases in advance of positive future performance *and* insider sales in advance of negative future performance. Finally, these requirements were imposed on executive officers, not on corporate directors. In untabulated analyses, we repeat our primary tests distinguishing politically connected officers from politically connected non-officer directors. We find no statistical difference between the increase in informed trading by politically connected, non-officer directors and politically connected officers.²⁵ These results suggest that executive compensation restrictions related to TARP, which only apply only to officers, cannot explain our results.

6.4. Political Connections as “Skill”

One potential interpretation of our results is that politically connected insiders do not have any private information per se, but are simply more skilled at forecasting government actions because they understand “how governmental regulators work.” This interpretation is consistent with some, but not all, of our results. There are three issues that put this interpretation into perspective. First, presumably there are politically connected *outsiders* who have just as much government expertise, if not more, than the politically connected insiders. If so, such outsiders should be able to forecast government actions with the same level of skill (or higher). In this case, the bank’s stock price should already reflect skilled forecasts of individuals with political connections that are not insiders of the firm (e.g., hedge funds); in the absence of private information, trades of politically connected insiders should not predict future returns. For example, Gao and Huang (2017) provide evidence of such a market mechanism with respect to politically connected hedge funds.

²⁵ For the sample of trades by politically connected non-officer (officer) directors, we find that the coefficient on *Buyer*Crisis_Bailout* at the six-month horizon is 22.25 (15.38) with a *t*-statistic of 3.33 (2.29) a difference of 6.87 (*p*-value 0.36).

Second, if politically connected insiders are simply better at forecasting government actions, then regardless of the actual TARP funding decision (i.e., yes/no/amount), politically connected insiders should have better anticipated the decision. In which case, we expect to find politically connected insiders have an information advantage at banks that did *and did not* receive TARP funds. However, findings in Table 6 suggest that the information advantage of politically connected insiders is conditional on receiving TARP money, and is concentrated entirely among politically connected insiders in banks that received TARP. This is consistent with the information advantage relating to details of the funding itself (i.e., is conditional on funding).

Third, the results from our event study suggest not only that the information advantage is related to the details of TARP funding, but that insiders timed their trades in relation to the infusion announcement. While it is true that insiders with political connections might skillfully anticipate the details of TARP funding absent any private information, at the very least, our results suggest they opportunistically timed their trades to precede the announcement.

7. Conclusion

We examine the relation between political connections and the trading of corporate insiders in the context of the Financial Crisis. We employ a comprehensive sample of trades by 7,301 corporate officers and directors across 497 publicly traded, TARP-eligible financial institutions. We measure insiders' political connections based on whether a board member has current or previous work experience at the Federal Reserve, a bank regulator (FDIC, OTS, or OCC), Treasury, or Congress, and we measure the informativeness of insider trades based on their predictive ability for future performance.

We find no evidence that insider trades predict future performance over the twelve months leading up to the Crisis, or during the Crisis prior to the creation of TARP. These results are consistent with the notion that corporate insiders were unable to predict the effect of the forthcoming Crisis on their firm. However, during the period TARP funds were disbursed (i.e., the “Bailout period”), we find the predictive ability of insider trades for future performance is greater than during any other period in our sample. Both the predictive ability of insider purchases for positive future performance and the predictive ability of insider sales for negative future performance increase during this period.

Consistent with politically connected insiders having an information advantage during the Crisis and trading to exploit this advantage, we find that the increase in informativeness is concentrated entirely among the trades of politically connected insiders. These results are robust to a battery of sensitivity analyses including controlling for time-invariant, firm-specific and insider-specific characteristics, controlling for contemporaneous changes in market conditions, controlling for a differential effect of market conditions on firms with and without politically connected insiders, and measuring returns over different horizons. Moreover, for a subset of individuals where we are able to construct a network map, we find our results vary predictably with the strength of the connection (i.e., direct versus indirect; recent versus stale).

Consistent with the notion that a significant portion of politically connected insiders’ information advantage relates specifically to government intervention and TARP capital infusions, we find: (i) the increase in the informativeness of insider trades during the Crisis is concentrated among the trades of politically connected insiders at firms that received TARP funds, (ii) an increase in trading volume among politically connected insiders 30-days prior to the announcement of TARP capital infusions, and (iii) politically connected insiders’ trades

thirty days prior to the announcement of TARP capital infusions predict the market reaction to the infusion. Similar results are not observed for insiders without political connections, on non-announcement dates, among firms that did not receive TARP funds, or around other corporate information events not directly related to TARP infusions.

Collectively, these results strongly suggest that political connections provided corporate insiders with an important information advantage during the Financial Crisis, and that a significant portion of this advantage related to knowledge about government intervention. Once the government announced it would intervene during the Crisis, our results suggest politically connected insiders appear to anticipate the effect of this intervention on their firms' share prices, and traded to exploit their information advantage. Our results provide evidence of one channel through which politically connected insiders can extract rents from shareholders and suggest political connections are an important characteristic of directors' and officers' social network that influences their trading decisions.

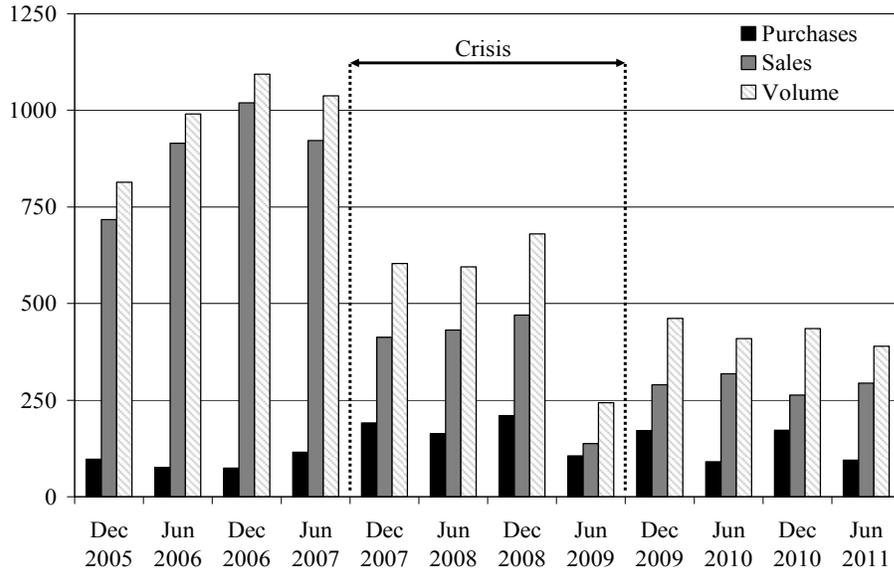
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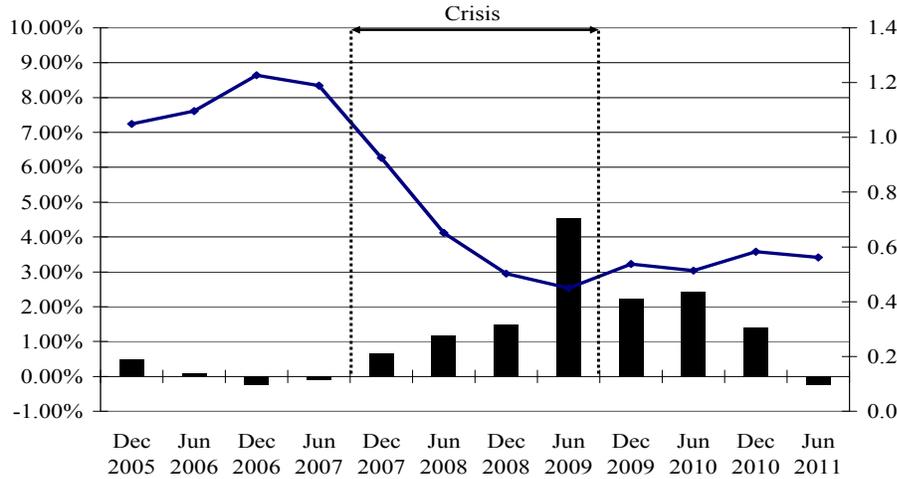
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Figure 1. Dollar value of purchases and sales by time period



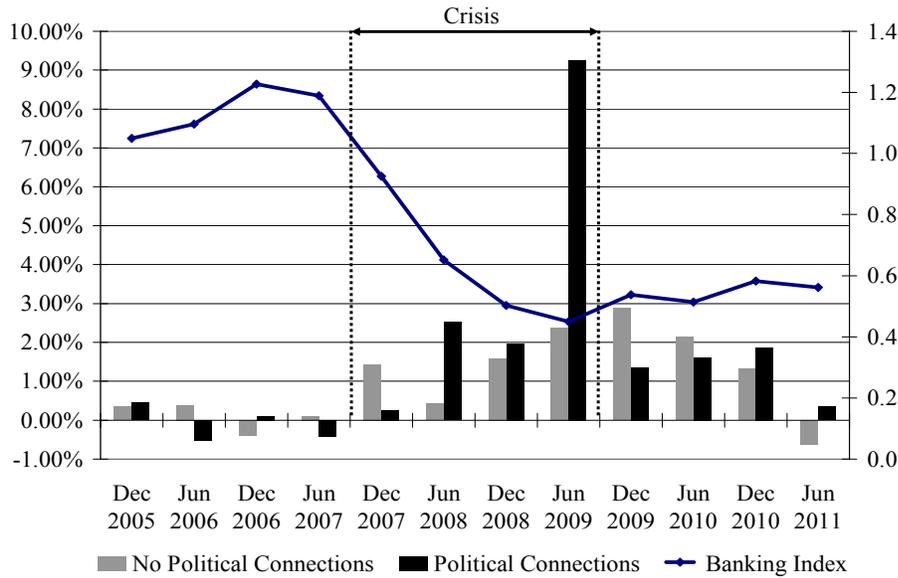
This figure plots the total dollar value of insider trades for all firms in our sample over the respective six month interval. Total dollar value of insider trades (in millions) appears on the y-axis, and the respective six month window appears on the x-axis. Black bars represent the total dollar value of purchases, gray bars represent the total dollar values of sales, and white bars represent the total dollar value of purchases plus sales, i.e., insider volume.

Figure 2. Difference in Future Returns Between Purchases and Sales



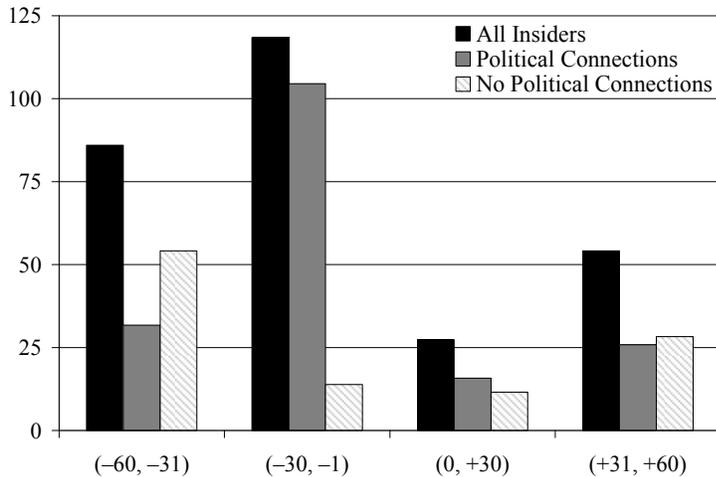
This figure plots the difference in one-month-ahead future returns between net purchase and net sales (black bars), averaged over the respective six month interval. We net all trades to the insider-month level and group observations based on whether the net trade in month t was a purchase or a sale. We report the difference in one-month-ahead future returns (i.e., returns in month $t+1$) between net purchases and net sales. We overlay the value of a \$1 investment in the Fama-French banking portfolio invested at the end of June 2005 (solid line).

Figure 3. Political Connections and the Difference in Future Returns



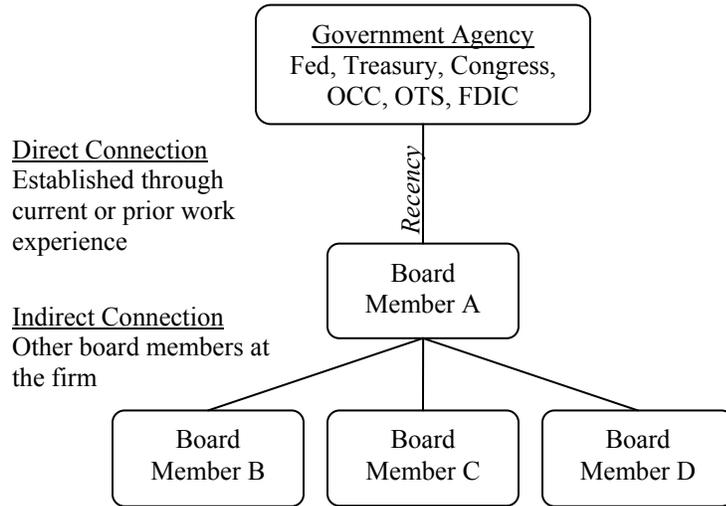
This figure plots the difference in one-month-ahead future returns between net purchase and net sales (black bars), averaged over the respective six month interval. We net all trades to the insider-month level and group observations based on whether the insider is politically connected and whether the net trade in month t was a purchase or a sale (2×2). We report the difference in one-month-ahead future returns (i.e., returns in month $t+1$) between net purchases and net sales separately for insiders with and without political connections. We overlay the value of a \$1 investment in the Fama-French banking portfolio invested at the end of June 2005 (solid line).

Figure 4. Insider Trades Around Infusion Announcements



This figures show the total dollar value of insider trades over the 60 trading days before and after the announcement of a TARP infusion, i.e., $-60 \dots +60$. Total dollar value of insider trades (in millions) appears on the y-axis, and the respective 30 day window appears on the x-axis. Black bars represent the total dollar value of trades, gray (white) bars represent the total dollar values of trades by insiders with (without) political connections. Sample of 256 infusions.

Figure 5. Example of a Partial Network Map



This figure presents an example of a “partial network map” used to test for information sharing among board members. We use BoardEx to determine whether a particular board member has work experience at either the Federal Reserve, Treasury Department, Congress, Office of Comptroller of Currency (OCC), Office of Thrift Supervision (OTS), or Federal Deposit Insurance Corporation (FDIC). Those board members with work experience at any of these agencies beginning prior to 2008 are classified as “directly connected.” All other officers and directors at these same firms are classified as “indirectly connected.” *Recency* is the ending year of the work experience relative to 2008. For example, if a director stepped down from the Federal Reserve Board in 2005, then *Recency* is -3 . If the work experience continues through 2008, *Recency* is coded as zero. We consider a direct connection “recent” if $Recency \geq -3$ and “stale” if $Recency < -3$, where -3 is the median value.

Table 1. Descriptive Statistics

This table presents descriptive statistics for variables used in our tests. The sample is constructed from the intersection of Thomson Insider (insider trades), Compustat (financial data), and CRSP (stock return data) for the time period July 2005 to June 2011. We focus on open market purchases and sales of common stock by directors and offices at publicly traded, TARP-eligible financial institutions and aggregate insider trades to the insider-month level. Panel A present descriptive statistics for firm characteristics. Panel B presents descriptive statistics for insider trade characteristics separately for all observations, for insiders with political connections (*PoliticalConn* = 1), and for insiders without political connections (*PoliticalConn* = 0). Panel C present the correlation matrix of firm and insider trade characteristics. Spearman (Pearson) correlations appear above (below) the diagonal. *Size* is the natural log of market value at the end of month *t*. *BM* is the book-to-market ratio at the end of month *t*, where book value is from the prior quarter-end. *PastMoRet* is the return in month *t*-1. *PastYrRet* is the buy-and-hold return over the past year from month *t*-2 to *t*-12. *NumPolConn* is the number of members on the firm's board of directors have current or previous work experience at the Federal Reserve, a bank regulator (FDIC, OTS, or OCC), Treasury, or Congress. *TARPRecipient* is an indicator variable equal to one if the firm received TARP funds. *PoliticalConn* is an indicator variable equal to one if *NumPolConn* is greater than zero, and zero otherwise. *Buyer_t* is an indicator variable equal to one if the number of shares bought exceeds the number of shares sold by insider *i* in firm *j* in month *t*. *BHR_{t+1}* (*BHR_{t+6}*) is the buy-and-hold return over the month (six months) subsequent to the trade. *Crisis* is an indicator variable equal to one for months between July 2007 and June 2009, and zero otherwise. *Crisis_PreBailout* (*Crisis_Bailout*) is an indicator variable equal to one for months between July 2007 and September 2008 (October 2008 and June 2009), and zero otherwise. *#Unique Firms* (*#Unique Insiders*) is the number of unique firms (insiders) in the respective sample. *Total Purchases* (*Total Sales*) is the total dollar value of insider purchases (sales) in the respective sample.

Panel A. Firm-level characteristics

Variable	Mean	Std	P25	P50	P75
<i>Size</i>	5.59	1.85	4.25	5.33	6.53
<i>BM</i>	1.02	0.85	0.53	0.75	1.17
<i>PastMoRet</i>	-1.78	11.58	-6.28	-0.68	3.78
<i>PastYrRet</i>	-5.11	28.71	-21.05	-2.55	10.98
<i>NumPolConn</i>	0.58	1.13	0.00	0.00	1.00
<i>TARPRecipient</i>	0.56	0.50	0.00	1.00	1.00

Panel B. Trade-level characteristics

	All observations N = 29,777		Insiders with political connections N = 10,204		Insiders without political connections N = 19,573	
	Mean	Median	Mean	Median	Mean	Median
<i>PoliticalConn</i>	0.34	0.00	1.00	1.00	0.00	0.00
<i>Buyer</i>	0.68	1.00	0.54	1.00	0.74	1.00
<i>BHR_{t+1}</i>	-0.11	-0.32	0.20	-0.06	-0.27	-0.41
<i>BHR_{t+6}</i>	-4.79	-3.90	-4.04	-3.41	-5.18	-4.28
<i>Crisis</i>	0.40	0.00	0.39	0.00	0.41	0.00
<i>Crisis_PreBailout</i>	0.26	0.00	0.26	0.00	0.26	0.00
<i>Crisis_Bailout</i>	0.14	0.00	0.13	0.00	0.15	0.00
<i>#Unique Firms</i>	497		159		338	
<i>#Unique Insiders</i>	7,301		2,767		4,534	
<i>Total Purchases</i> (\$ millions)	1,562.22		769.24		792.98	
<i>Total Sales</i> (\$ millions)	6,190.63		4,547.03		1,643.60	

Table 1. Descriptive Statistics (cont'd)

Panel C. Correlation Matrix

	<i>Size</i>	<i>BM</i>	<i>PastMoRet</i>	<i>PastYrRet</i>	<i>ConnDirectors</i>	<i>TARPPRecipient</i>	<i>PoliticalConn</i>	<i>Buyer</i>	<i>BHR_{t+1}</i>	<i>BHR_{t+6}</i>	<i>Crisis</i>	<i>Crisis_PreBailout</i>	<i>Crisis_Bailout</i>
<i>Size</i>		-0.51	0.14	0.31	0.43	0.01	0.40	-0.42	0.02	0.04	-0.11	0.00	-0.16
<i>BM</i>	-0.39		-0.26	-0.54	-0.13	0.15	-0.12	0.31	0.01	-0.07	0.28	0.07	0.30
<i>PastMoRet</i>	0.11	-0.32		0.17	0.02	-0.05	0.02	-0.21	-0.05	0.02	-0.23	-0.14	-0.14
<i>PastYrRet</i>	0.28	-0.55	0.17		0.06	-0.12	0.05	-0.26	0.05	0.14	-0.46	-0.28	-0.29
<i>ConnDirectors</i>	0.57	-0.05	0.01	0.06		0.13	0.98	-0.22	0.02	0.02	-0.02	0.00	-0.03
<i>TARPPRecipient</i>	0.02	0.13	-0.04	-0.11	0.09		0.14	-0.01	-0.01	-0.06	0.01	0.01	0.00
<i>PoliticalConn</i>	0.43	-0.06	0.00	0.04	0.71	0.14		-0.20	0.02	0.01	-0.02	0.00	-0.03
<i>Buyer</i>	-0.43	0.25	-0.18	-0.25	-0.21	-0.01	-0.20		0.02	-0.01	0.16	0.11	0.09
<i>BHR_{t+1}</i>	-0.01	0.14	-0.14	-0.02	0.02	0.02	0.02	0.04		0.37	-0.07	-0.08	0.00
<i>BHR_{t+6}</i>	0.02	0.11	-0.07	0.06	0.05	-0.01	0.02	0.01	0.43		-0.24	-0.32	0.06
<i>Crisis</i>	-0.11	0.21	-0.22	-0.43	-0.03	0.01	-0.02	0.16	-0.04	-0.18		0.72	0.50
<i>Crisis_PreBailout</i>	-0.01	-0.05	-0.11	-0.23	-0.02	0.01	0.00	0.11	-0.07	-0.28	0.72		-0.24
<i>Crisis_Bailout</i>	-0.15	0.36	-0.17	-0.32	-0.02	0.00	-0.03	0.09	0.03	0.11	0.50	-0.24	

Table 2. Insider Trades During the Financial Crisis

This table presents descriptive statistics for insider trading variables over time. All trades are aggregated to the insider-month level. Panel A presents number of trades, average values of *Buyer*, i.e., the probability the trade is a net purchase, the total dollar value of purchases and sales (in millions), and the ratio of the value of purchases to sales. Panel B partitions the sample based on political connections of the insider. For each sample partition, Panel B reports the number of trades, average values of *Buyer*, and the fraction of total dollar value of purchases and sales (in percent). An insider is coded as having political connections if a member of the firm's board of directors has current or previous work experience at the Federal Reserve, a bank regulator (FDIC, OTS, or OCC), Treasury, or Congress.

Panel A. Insider Trading Activity by Period

Time period	Date Range	Num. Obs.	Avg. <i>Buyer</i>	Total Purchases (\$ millions)	Total Sales (\$ millions)	Total Purchases / Total Sales
Full Sample	Jul 05 – Jun 11	29,777	0.68	1562.22	6190.63	0.25
Pre-Crisis period	Jul 05 – Jun 07	10,278	0.55	362.22	3573.32	0.10
Crisis period	Jul 07 – Jun 09	11,923	0.77	670.82	1450.90	0.46
Pre-Bailout period	Jul 07 – Sep 08	7,625	0.76	443.15	1078.84	0.41
Bailout period	Oct 08 – Jun 09	4,298	0.78	227.67	372.06	0.61
Post-Crisis period	Jul 09 – Jun 11	7,576	0.70	529.18	1166.41	0.45

Panel B. Political Connections and Insider Trading Activity by Period

Time period	Date Range	Num. Obs.	Insiders with Political Connections			Insiders without Political Connections			
			Avg. <i>Buyer</i>	%Total Purchases in Panel A	%Total Sales in Panel A	Num. Obs.	Avg. <i>Buyer</i>	%Total Purchases in Panel A	%Total Sales in Panel A
Full Sample	Jul 05 – Jun 11	10,204	0.54	49.24	73.45	19,573	0.74	50.76	26.55
24 mos. before the Crisis	Jul 05 – Jun 07	3,744	0.39	42.22	73.23	6,534	0.64	57.78	26.77
Crisis	Jul 07 – Jun 09	3,961	0.68	52.48	71.38	7,962	0.82	47.52	28.62
Crisis: Pre-Bailout	Jul 07 – Sep 08	2,632	0.64	42.92	80.90	4,993	0.83	57.08	19.10
Crisis: Bailout	Oct 08 – Jun 09	1,329	0.74	71.08	43.78	2,969	0.80	28.92	56.22
24 mos. after the Crisis	Jul 09 – Jun 11	2,499	0.57	49.94	76.74	5,077	0.77	50.06	23.26

Table 3. Average Future Returns

This table presents average future returns following insider trades over time. All trades are aggregated to the insider-month level. Panel A reports average one-month-ahead future returns separately for net purchases ($Buyer = 1$) and net sales ($Buyer = 0$). Panel B presents average one-month-ahead future returns separately for each of four groups (2x2): whether the insider is politically connected and whether the trade was a net purchase or a net sale. An insider is coded as having political connections if a member of the firm's board of directors has current or previous work experience at the Federal Reserve, a bank regulator (FDIC, OTS, or OCC), Treasury, or Congress. t -statistics appear in parentheses and test for a difference in means. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. Average Future Returns by Period

Time period	Date Range	BHR_{t+1} following Purchases	BHR_{t+1} following Sales	Difference in BHR_{t+1}
Full Sample	Jul 05 – Jun 11	0.23	-0.82	1.05 (6.76)
Pre-Crisis period	Jul 05 – Jun 07	-0.24	-0.10	-0.14 (-1.44)
Crisis period	Jul 07 – Jun 09	-0.18	-2.62	2.44 (6.78)
Pre-Bailout period	Jul 07 – Sep 08	-1.35	-2.49	1.14 (3.12)
Bailout period	Oct 08 – Jun 09	1.84	-2.87	4.71 (6.18)
Post-Crisis period	Jul 09 – Jun 11	1.44	-0.14	1.58 (4.99)

Panel B. Political Connections and Average Future Returns by Period

Label	Date Range	Insiders with Political Connections			Insiders without Political Connections			Difference -in- Differences
		BHR_{t+1} following Purchases	BHR_{t+1} following Sales	Difference in BHR_{t+1}	BHR_{t+1} following Purchases	BHR_{t+1} following Sales	Difference in BHR_{t+1}	
Full Sample	Jul 05 – Jun 11	1.03	-0.79	1.81 (6.92)	-0.07	-0.86	0.79 (3.90)	1.02 (3.16)
24 mos. before the Crisis	Jul 05 – Jun 07	-0.44	-0.07	-0.37 (-2.34)	-0.17	-0.12	-0.05 (-0.37)	-0.32 (-1.50)
Crisis	Jul 07 – Jun 09	1.66	-2.56	4.22 (6.56)	-0.94	-2.67	1.73 (3.95)	2.49 (3.40)
Crisis: Pre-Bailout	Jul 07 – Sep 08	-0.27	-2.07	1.80 (2.82)	-1.79	-2.93	1.14 (2.49)	0.66 (0.89)
Crisis: Bailout	Oct 08 – Jun 09	5.00	-3.89	8.89 (5.96)	0.53	-2.28	2.81 (3.23)	6.08 (3.81)
24 mos. after the Crisis	Jul 09 – Jun 11	1.31	-0.19	1.50 (3.72)	1.48	-0.09	1.57 (3.46)	-0.07 (-0.10)

Table 4. Informativeness of Insider Trades During the Crisis

This table presents results from estimating equations (1) and (2). Panel A presents results measuring future returns over the subsequent month ($s = 1$) and Panel B presents results measuring future returns over the subsequent six months ($s = 6$). Specifications (1) and (2) estimate the regression specification pooling over all observations, specifications (3) and (4) estimate the regression specification separately for insiders with and without political connections. Column (5) presents the difference in coefficients between (3) and (4). The coefficient on *Buyer* represents the informativeness of the net insider trade for future performance and the coefficient on *Buyer*Crisis_Bailout* represents the change in the informativeness of the trade during the “Bailout period.” All variables are defined in Table 1. *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. One-month ahead future returns

Variables	Dependent variable: $BHR_{i,t+1}$				
	All Obs		Political Connections		
	(1)	(2)	Yes	No	Diff.
<i>Buyer</i>	0.51** (2.04)	0.39* (1.66)	-0.20 (-0.45)	0.55* (1.94)	-0.75 [0.15]
<i>Buyer*Crisis_PreBailout</i>	.	-0.05 (-0.07)	0.40 (0.34)	0.14 (0.19)	0.27 [0.85]
<i>Buyer*Crisis_Bailout</i>	.	2.34** (2.49)	6.43*** (3.35)	0.63 (0.62)	5.81*** [<0.01]
<i>Controls</i>					
<i>Size</i>	0.32*** (2.83)	0.32*** (2.80)	0.18 (1.16)	0.48** (2.53)	-0.30 [0.23]
<i>BM</i>	2.57*** (4.61)	2.33*** (4.05)	2.77** (2.48)	2.13*** (3.40)	0.64 [0.62]
<i>PastMoRet</i>	-0.11*** (-3.87)	-0.12*** (-4.11)	-0.12*** (-3.34)	-0.12*** (-2.97)	0.00 [0.98]
<i>PastYrRet</i>	0.04*** (3.67)	0.02* (1.79)	0.02 (1.35)	0.02 (1.41)	0.00 [0.91]
<i>Crisis_PreBailout</i>		-2.10*** (-4.28)	-1.55** (-2.21)	-2.66*** (-4.20)	1.11 [0.24]
<i>Crisis_Bailout</i>		-3.34*** (-4.48)	-5.00*** (-3.93)	-2.54*** (-2.95)	-2.46 [0.11]
F	13.24	12.33	4.71	10.57	
N(insider-months)	29,777	29,777	10,204	19,573	
N(firms)	497	497	159	338	

Table 4. Informativeness of Insider Trades During the Crisis (cont'd)

Panel B. Six-month ahead future returns

Variables	Dependent variable: $BHR_{i,t+6}$				
	All Obs		Political Connections		
	(1)	(2)	Yes	No	Diff.
<i>Buyer</i>	1.32 (1.46)	2.92^{***} (3.53)	4.30^{***} (3.27)	1.70[*] (1.86)	2.60[*] [0.10]
<i>Buyer*Crisis_PreBailout</i>		-3.53^{**} (-1.99)	-5.94^{**} (-1.98)	-3.59[*] (-1.70)	-2.35 [0.52]
<i>Buyer*Crisis_Bailout</i>		4.83[*] (1.87)	18.78^{***} (3.60)	-1.39 (-0.46)	20.17^{***} [<0.01]
<i>Controls</i>					
<i>Size</i>	0.97^{***} (2.75)	1.07^{***} (3.25)	1.65^{***} (3.57)	0.44 (0.90)	1.21^{**} [0.07]
<i>BM</i>	6.91^{***} (4.86)	3.92^{***} (2.89)	5.50^{**} (2.04)	2.72^{**} (2.03)	2.78 [0.36]
<i>PastMoRet</i>	-0.08[*] (-1.66)	-0.17^{***} (-3.38)	-0.24^{**} (-2.56)	-0.13^{**} (-2.38)	-0.11 [0.31]
<i>PastYrRet</i>	0.17^{***} (7.30)	0.08^{***} (3.18)	0.03 (0.84)	0.10^{***} (3.44)	-0.07 [0.17]
<i>Crisis_PreBailout</i>	.	-14.35^{***} (-11.47)	-15.85^{***} (-8.29)	-12.66^{***} (-7.64)	-3.19 [0.21]
<i>Crisis_Bailout</i>	.	-2.25 (-1.17)	-9.47^{***} (-2.88)	1.51 (0.67)	-10.98^{***} [<0.01]
F	17.38	41.19	24.66	20.89	
N(insider-months)	29,777	29,777	10,204	19,573	
N(firms)	497	497	159	338	

Table 5. Within-Firm and Within-Insider Analysis

This table reports results from estimating the regression specification in Table 4 after including month fixed effects, firm fixed effects, and insider fixed effects. All fixed effects are allowed to vary with political connections. For example, there are two vectors of month fixed effects, one for the sample of insiders with political connections and one for the sample of insiders without political connections. Month fixed effects subsume the main effects on *Crisis_PreBailout* and *Crisis_Bailout*, and insider fixed effects subsume firm fixed effects. All variables are defined in Table 1. For parsimony, coefficients on control variables are not tabulated and we present results only for future returns over the subsequent six months (dependent variable: $BHR_{i,t+6}$). *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Variables	Month + Firm Fixed Effects			Month + Insider Fixed Effects		
	Political Connections			Political Connections		
	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)
<i>Buyer</i>	2.94** (2.02)	1.48* (1.67)	1.46 (0.86)	4.54** (2.32)	0.85 (0.59)	3.69 [0.12]
<i>Buyer*Crisis_PreBailout</i>	-3.12 (-1.32)	-0.46 (-0.23)	-2.66 (-0.86)	-3.91 (-1.50)	-0.08 (-0.03)	-3.83 [0.29]
<i>Buyer*Crisis_Bailout</i>	9.97*** (2.71)	-3.02 (-1.08)	12.99*** (2.82)	8.81** (2.10)	-3.07 (-0.90)	11.88** [0.02]
Control Variables	Yes	Yes		Yes	Yes	
Month Fixed Effects	Yes	Yes		Yes	Yes	
Firm Fixed Effects	Yes	Yes		No	No	
Insider Fixed Effects	No	No		Yes	Yes	
F	3.85	9.89		4.21	10.82	
N(insider-months)	10,204	19,573		9,308	18,126	
N(firms)	159	338		159	338	

Table 6. TARP Funding Status and the Increase in the Information Advantage of Politically Connected Insiders

This table reports results from estimating the regression specifications in Table 5 after partitioning on TARP funding status. Column (1) presents results for the trades of politically connected insiders at all firms in our sample. Columns (2) and (3) presents results after dividing the sample of politically connected insiders into two groups based on whether their firm received TARP funds. Columns (1), (2), and (3) include month and firm fixed effects. Columns (4), (5), and (6) presents results from analogous specifications that include month and insider fixed effects. All fixed effects are allowed to vary with TARP funding status. Month fixed effects subsume the main effects on *Crisis_PreBailout* and *Crisis_Bailout*, and insider fixed effects subsume firm fixed effects. All variables are defined in Table 1. For parsimony, coefficients on control variables are not tabulated and we present results only for future returns over the subsequent six months (dependent variable: $BHR_{i,t+6}$). *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Variables	Month + Firm Fixed Effects				Month + Insider Fixed Effects			
	Political Connected Insiders	Political Connected Insiders	Political Connected Insiders	Diff.	Political Connected Insiders	Political Connected Insiders	Political Connected Insiders	Diff.
	Table 5 Col (1)	<i>TARP Recipients</i>	<i>Non-TARP Recipients</i>		Table 5 Col (4)	<i>TARP Recipients</i>	<i>Non-TARP Recipients</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Buyer</i>	2.94** (2.02)	3.79* (1.80)	0.79 (0.59)	3.00 [0.23]	4.54** (2.32)	3.69 (1.40)	4.19* (1.86)	-0.50 [0.89]
<i>Buyer*Crisis_PreBailout</i>	-3.12 (-1.32)	-0.71 (-0.24)	-7.88* (-2.53)	7.17* [0.09]	-3.91 (-1.50)	-1.35 (-0.43)	-8.25** (-2.26)	6.90 [0.15]
<i>Buyer*Crisis_Bailout</i>	9.97*** (2.71)	13.65*** (2.86)	3.65 (1.04)	10.00* [0.09]	8.81** (2.10)	13.58** (2.52)	1.70 (0.37)	11.88* [0.09]
Control Variables	Yes	Yes	Yes		Yes	Yes	Yes	
Month Fixed Effects	Yes	Yes	Yes		Yes	Yes	Yes	
Firm Fixed Effects	Yes	Yes	Yes		No	No	No	
Insider Fixed Effects	No	No	No		Yes	Yes	Yes	
F	3.85	5.25	1.34		4.21	4.58	1.28	
N(insider-months)	10,204	6,709	3,495		9,308	6,169	3,139	
N(firms)	159	92	67		159	92	67	

Table 7. Anticipation of TARP Infusions—Event Study

This table presents results for an event study of the market reaction to TARP infusions. Panel A presents descriptive statistics for key variables related to the infusion and insider trading over the thirty trading days prior to the announcement of the infusion, i.e., the $[-30, -1]$ window. We report statistics pooling across all infusions (column (1)), and separately for those infusions where shares bought by insiders exceeds shares sold by insiders (column (2)) and infusions where shares sold by insiders exceeds shares bought by insiders (column (3)). Panel B presents descriptive statistics for key variables related to the infusion and insider trading after partitioning the sample based on political connections and direction of insider trade (2×2). Panel C present results from a regression of three-day announcement period returns on measures of insider trading over the prior thirty trading days and control variables. *Total # Insiders Trading* is the total number of insiders who trade over the respective window. *Total Insider Volume* is the total dollar value of insider trades over the respective window aggregated across all infusions. *EventRet* is the buy-and-hold return over a three-day announcement period return centered on the infusion announcement (i.e., $t = -1 \dots +1$) expressed in percent. *Infusion%MV* (*Infusion%Assets*) is the amount of the infusion as a percentage of prior quarter market value (book value of total assets). *Buyer(-30,-1)* is an indicator variable equal to one if the net trade by insiders over the thirty trading days prior to the event is a purchase. All other variables are defined in Table 1. The sample consists of 256 capital infusions across 249 firms and 31 dates, excluding the nine initial recipients. *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Infusion Characteristics Partitioned by Insider Trade

Variable	All Infusions [-30, -1] N = 256	Infusions where insiders are <i>Net Buyers</i> [-30, -1] N = 89	Infusions where insiders are <i>Net Sellers</i> [-30, -1] N = 52
<i>Total # Insiders Trading</i>	383.00	269.00	114.00
<i>Total Insider Volume</i> (\$ millions)	118.43	63.08	55.35
<i>Avg. EventRet</i>	-0.40	0.35	-1.28
<i>Avg. Infusion%MV</i>	42.48	43.11	26.86
<i>Avg. Infusion%TA</i>	2.28	2.13	2.52

Panel B. Infusion Characteristics Partitioned by Political Connections and Insider Trade

Variable	Political Connections: Yes		Political Connections: No	
	<i>Net Buyers</i> [-30, -1] N = 32	<i>Net Sellers</i> [-30, -1] N = 27	<i>Net Buyers</i> [-30, -1] N = 57	<i>Net Sellers</i> [-30, -1] N = 25
<i>Total # Insiders Trading</i>	87.00	63.00	182.00	51.00
<i>Total Insider Volume</i> (\$ millions)	55.31	49.24	7.77	6.11
<i>Avg. EventRet</i>	4.39	-5.13	-1.92	2.87
<i>Avg. Infusion%MV</i>	30.71	21.80	50.08	32.32
<i>Avg. Infusion%TA</i>	2.10	2.26	2.15	2.80

Table 7. Anticipation of TARP Infusions—Event Study (cont'd)*Panel C. Cross-Sectional Regressions*

Variables	All Infusions (1)	Political Connections		
		Yes (2)	No (3)	Diff (4)
<i>Buyer(-30,-1)</i>	1.11 (0.76)	5.41** (2.23)	-1.67 (-0.95)	7.08** [0.02]
Controls				
<i>Size</i>	-0.25 (-0.61)	-0.78 (-1.46)	0.50 (0.76)	-1.28 [0.13]
<i>BM</i>	-1.26 (-1.33)	-2.00 (-1.04)	-0.21 (-0.19)	-1.79 [0.41]
<i>PastMoRet</i>	0.01 (0.15)	-0.10 (-1.36)	0.07 (1.23)	-0.17* [0.07]
<i>PastYrRet</i>	-0.01 (-0.42)	-0.01 (-0.27)	-0.01 (-0.13)	0.00 [0.91]
F	0.56	2.42	1.05	
N(firm-days)	256	94	162	
N(firms)	249	92	157	

Table 8. Anticipation of TARP Infusions—Event Study Falsification Tests

This table presents results from using two falsification tests to estimate the cross-sectional variation in event returns under the null hypothesis that it is unrelated to TARP infusions. Panel A presents results from the first falsification test. In the first falsification test, we hold the firms in the sample fixed (i.e., the set of TARP recipients), and estimate the regression specifications in Panel C of Table 7 for the same firms on all non-announcement days from October 2008 to June 2009. We test whether the estimated coefficients from TARP recipients on the announcement day are different from those estimated from TARP recipients on non-announcement days. Sample of 249 unique firms across 188 non-event days. Panel B presents results from the second falsification test. In the second falsification test, we hold the event dates in the sample fixed, and estimate the regression specifications in Panel C of Table 7 for the same dates for firms that did not receive TARP infusions. We test whether the estimated coefficients from TARP recipients on the announcement day are different from those estimated from non-recipients on the announcement day. Sample of 242 unique non-recipients across 31 announcement days. For parsimony, we do not tabulate coefficients on control variables. *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Falsification Test #1: TARP Recipients on Non-Announcement days

Variables	TARP recipients on the announcement day (Table 7, Panel C)			TARP recipients on the non-announcement dates			Diff -in- Diff (7)
	Political Connections		Diff (3)	Political Connections		Diff (6)	
	Yes (1)	No (2)		Yes (4)	No (5)		
<i>Buyer(-30,-1)</i>	5.41** (2.23)	-1.67 (-0.95)	7.08** [0.02]	1.32*** (5.83)	0.67*** (3.79)	0.65** [0.03]	6.43** [0.03]
Controls	Yes	Yes		Yes	Yes		
F	2.42	1.05		25.00	14.34		
N(firm-days)	94	162		17,201	29,242		
N(firms)	92	157		92	157		

Panel B. Falsification Test #2: Non-Recipients on Announcement days

Variables	TARP recipients on the announcement day (Table 7, Panel C)			Non-recipients on the announcement dates			Diff -in- Diff (7)
	Political Connections		Diff (3)	Political Connections		Diff (6)	
	Yes (1)	No (2)		Yes (4)	No (5)		
<i>Buyer(-30,-1)</i>	5.41** (2.23)	-1.67 (-0.95)	7.08** [0.02]	0.14 (0.28)	0.65** (2.14)	-0.51 (-0.88)	7.59*** [0.01]
Controls	Yes	Yes		Yes	Yes		
F	2.42	1.05		4.46	3.29		
N(firm-days)	94	162		1,958	5,224		
N(firms)	92	157		66	176		

Table 9. Comparison to Other Information Events: Earnings Announcements

This table presents results from repeating our event study tests on TARP recipients' earnings announcements during the Crisis. We estimate the regression specifications in Panel C of Table 7 for the same firms (i.e., TARP recipients) measuring returns over the three-day window centered on the earnings announcement, and insider trades over the thirty trading days prior to the announcement. We test whether the estimated coefficients around the announcement of TARP infusions is different from the estimated coefficients around earnings announcements. Sample of 1,619 earnings announcements between July 2007 and June 2009 for 249 TARP recipients. For parsimony, we do not tabulate coefficients on control variables. *t*-statistics (two-tailed *p*-values) based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Variables	TARP Recipients' infusions announcements (Table 7, Panel C)			TARP Recipients' earnings announcements			Diff -in- Diff (7)
	Political Connections		Diff (3)	Political Connections		Diff (6)	
	Yes (1)	No (2)		Yes (4)	No (5)		
<i>Buyer(-30,-1)</i>	5.41** (2.23)	-1.67 (-0.95)	7.08** [0.02]	0.65 (0.62)	-0.09 (-0.14)	0.74 [0.60]	6.34** [0.04]
Controls	Yes	Yes		Yes	Yes		
F	2.42	1.05		5.74	4.18		
N(firm-days)	94	162		604	1015		
N(firms)	92	157		92	157		

Table 10. Information Sharing

This table presents results from use the BoardEx database to construct a partial network map (see Figure 5), and testing whether attributes of insiders' connections affect the informativeness of their trades. Panel A reports results from estimating the regression specification in Table 4 after partitioning trades placed by insiders with "direct" and "indirect" political connections. Panel B reports results from estimating the regression specification in Table 4 after partitioning trades placed by insiders with "recent" or "stale" political connections. We refer to individuals as "directly connected" if they have prior work experience at the Federal Reserve, Treasury, OTS, OCC, FDIC, or Congress, and all other officers and directors at the same firm as "indirectly connected." For each individual with a direct connection, we consider the connection to be "recent" if the relevant work experience ended in 2005 or later, and "stale" if it ended before 2005 (2005 is the median value). See Section 6.1 and Figure 5 for more details. All other variables are defined in Table 1. For parsimony, coefficients on control variables are not tabulated and we present results only for future returns over the subsequent six months (dependent variable: $BHR_{i,t+6}$). *t*-statistics based on standard errors clustered by firm appear in parentheses (brackets). ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Distinguishing Direct and Indirect Connections

Variables	Main Sample (Table 4 Panel B)		BoardEx Sample	
	Firm-Level Political Connection		Individual-Level Political Connection	
	No (1)	Yes (2)	Direct (3)	Indirect (4)
<i>Buyer</i>	1.70* (1.86)	4.30*** (3.27)	1.08 (0.39)	5.49*** (3.47)
<i>Buyer*Crisis_PreBailout</i>	-3.59* (-1.70)	-5.94** (-1.98)	-12.52* (-1.83)	-6.85* (-1.89)
<i>Buyer*Crisis_Bailout</i>	-1.39 (-0.46)	18.78*** (3.60)	40.14*** (3.15)	17.44*** (2.73)
Control Variables	Yes	Yes	Yes	Yes
F	20.89	24.66	7.89	20.05
N(insider-months)	19,573	10,204	812	7,555
N(firms)	338	159	123	123

Panel B. Recency of the Connection

Variables	Direct & Indirect Individual Connections		Only Direct Individual Connections		Only Indirect Individual Connections	
	Recent (1)	Stale (2)	Recent (3)	Stale (4)	Recent (5)	Stale (6)
	<i>Buyer</i>	3.14 (1.03)	6.63*** (2.59)	-3.37 (-0.74)	4.44 (0.93)	4.06 (1.29)
<i>Buyer*Crisis_PreBailout</i>	-10.82 (-1.63)	-3.94 (-0.72)	-11.85 (-1.07)	-13.72** (-2.03)	-11.38* (-1.67)	-3.07 (-0.54)
<i>Buyer*Crisis_Bailout</i>	32.98** (2.30)	11.38 (1.28)	84.11*** (3.25)	37.35*** (2.67)	28.23** (1.99)	6.24 (0.72)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
F	16.20	10.90	7.50	2.16	15.50	11.69
N(insider-months)	2,566	1,912	245	128	2,321	1,784
N(firms)	41	39	41	39	41	28