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Informativeness of Insider Trades**

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Political Connections and the Informativeness of Insider Trades

Abstract:

This paper examines the relation between political connections and informed trading by corporate insiders in the context of the Financial Crisis. The unprecedented magnitude of government intervention, the substantial impact of this intervention on firm value, and the political nature of the intervention provide a powerful setting to examine the relation between political connections and informed trading. Consistent with political connections providing corporate insiders with an information advantage, we find strong evidence of a relation between political connections and the informativeness of their trades. Consistent with this relation stemming from private information related to government intervention, we find the relation is strongest during the period in which TARP funds were dispersed, and strongest among politically connected insiders at banks that received TARP funds. Examining insider trades around the announcements of TARP infusions, we find evidence of significant trading thirty days in advance of the announcement, and that these trades predict the market reaction to the announcement. Notably, we find these relations are present only for the trades of politically connected insiders. Overall, our results suggest that politically connected insiders had an information advantage during the Crisis and traded 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 channel through which politically connected insiders might extract rents from shareholders—informed trading.

We examine the relation between political connections and informed trading by corporate insiders within the context of the 2007-2009 Financial Crisis. The unprecedented magnitude of government intervention during the Financial Crisis, the substantial impact of the intervention on firm value, and the political nature of the intervention provide a powerful setting to examine the relation between political connections and informed trading. 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 trade on private information and that their trades predict long-run future performance (e.g., Aboody and Lev, 2000; Piotroski and Roulstone, 2005; Cohen, Malloy, and Pomorski, 2012). Within the context of financial institutions, prior research suggests insider trades are related to the bank's corporate culture (e.g., Bushman, Davidson, Dey, and Smith, 2015) and predict future writedowns (e.g., Ryan, Tucker, and Wu, 2015). 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 research does not examine either the role of political connections in conveying an information advantage, or the informativeness of insider trades as it relates to government intervention. As a result, little is presently known about the relation between political connections and insider trading.

We examine the relation between political connections and trading of corporate insiders using a comprehensive sample of all open market purchases and sales of Section 16 officers and directors (hereafter "insiders") at 497 publicly traded financial institutions ("banks") between 2005 and 2011.¹ One advantage of focusing on the trading of corporate insiders (as opposed to the trading of other parties) is that it is relatively easy to obtain biographical data and work history from firms' proxy statements. Following prior research, we measure an insiders' political connections based on whether a board member has current or previous work experience at the Federal Reserve, a bank regulator (e.g., FDIC, OTS, or OCC), Treasury, or Congress, and measure the informativeness of insider trades based on their predictive ability for future performance.

¹ 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. It is likely to be the case that the trades we study fall into a growing legal gray area. We discuss the legality of the trades and the evolving legal landscape around insider trading in Section 5.4.3.

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 dispersed, 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 dispersed, 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 increase in the informativeness of insider trades relating to private information gleaned from political connections, we find that the increase is concentrated entirely among 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 politically connected insiders is economically and statistically insignificant, −0.37%. However, during the period TARP funds were dispersed, the difference in one-month-ahead future returns between purchases and sales of politically connected insiders is both economically and statistically significant, 8.89%. These results are robust to a battery of sensitivity analyses including controlling for time-invariant, firm-specific and insider-specific characteristics, changes in market conditions, a differential effect of market conditions on firms with and without politically connected insiders, and measuring returns over longer horizons.

Next, we use two distinct sets of tests to investigate whether the information advantage of politically connected insiders relates specifically to TARP capital infusions. In the first set of tests, we repeat our analyses after partitioning the sample based on whether the firm received TARP funds. We find that 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, and we find no evidence of an increase in informativeness of insider trades among the trades of politically connected insiders at firms that did not receive TARP funds. This suggests the information advantage is conditional on the bank receiving TARP funds.

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. Moreover, we find that trades over the thirty days prior to the announcement predict both the amount of the infusion and the market reaction to the announcement—and that the predictive ability of these trades is concentrated among the trades of politically connected insiders. 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%). 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. The results from our event study analysis are consistent with politically connected insiders trading in anticipation of the surprise, and are robust to a variety of sensitivity tests: similar results are not observed among insiders without political connections, outside of the announcement period, in non-recipients during the

announcement period, or around other corporate events that are not directly related to TARP infusions (e.g., earnings announcements).

Collectively, our findings suggest that political connections provided corporate insiders with an important information advantage during the Financial Crisis, that a significant portion of this advantage related to knowledge about government intervention, and that some insiders traded to exploit this advantage. We contribute to the literature by documenting one channel through which politically connected insiders can extract rents from shareholders; by documenting the relation between insiders' political connections and information leakage; and by documenting an important characteristic of directors' social network that influences their trading decisions.

The remainder of the paper proceeds as follows. We discuss prior literature and the institutional setting in Section 2. We describe the sample and measurement of key variables in Section 3. We report the results from our cross-sectional tests in Section 4. We report the results from our tests relating to TARP infusions in Section 5. Concluding remarks are provided in Section 6.

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 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.²

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), can favorably influence tax policy (e.g., Brown, Drake, and Wellman, 2014), and can reduce the negative effects of government policy uncertainty on corporate investment (e.g., Wellman, 2015).

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 (2014) 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.

² 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.

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 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.³

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

³ While Angelo Mozilo is a prominent example of an insider trading case prosecuted in connection with the Financial Crisis, we are unaware of any cases against corporate insiders that allege trading on private information in conjunction with government bailouts.

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.⁴ 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 2009.⁵

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

⁴ The nine banks initially receiving TARP were coaxed or “forced” to take the infusion in order to mitigate concerns about adverse selection with respect to funding (e.g., Cho, Irwin, and Whoriskey, 2008; 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.

⁵ 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.

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 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.⁶

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; Bushman, Davidson, Dey, and Smith, 2015). Corporate insiders are also known to trade in anticipation of future corporate events and mispricing (e.g., Ke, Huddart, and Petroni, 2003; Jenter, 2005; Piotroski and Roulstone, 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

⁶ See Seyhun (1998) and Cohen, Malloy, and Pomorski (2012) for reviews of the insider trading literature.

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.

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, 2016). 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 dispersed.⁷

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,

⁷ 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; Mishkin, 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.

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 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.⁸

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.”⁹

⁸ 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, and 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.

⁹ We thank Ran Duchin and Denis Sosyura for providing these data.

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 members (e.g., Cao, Dhaliwal, Li, and Yang, 2015), 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, 2016). 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 that interact 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).¹⁰

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 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.¹¹

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.

¹⁰ 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 transactions occurred outside the Crisis period, and thus do not affect our inferences.

¹¹ 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.

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

Table 2 presents average values of our measures of insider trading activity in each period. 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 (38% 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. Regression Tests: Crisis Period

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 arbitrary time-series correlation and arbitrary correlation across insiders within a given firm.¹²

Table 4 reports results from estimating equations (1) and (2). On average, across the entire sample, results in columns (1) and (3) suggest insider trades are related to future returns at the one month horizon (*t*-stat of 2.04), but not the six month horizon (*t*-stat of 1.46). Columns (2) and (4) allow this relation to vary over time. We find a positive relation between insider trades and future returns over both horizons during non-Crisis periods (*Buyer t*-stats of 1.66 and 3.53); some evidence of a reduction in the informative of insider trades in the Pre-Bailout period

¹² 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, or two-way cluster standard errors by insider and month, (iv) 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).

(*Buyer*Crisis_PreBailout* t -stats of -0.07 and -1.99), and a marked increase in the informativeness of insider trades over both horizons in the Bailout period (*Buyer*Crisis_Bailout* t -stats of 2.49 and 1.87). Results in column (4) suggest that the difference in six-month returns between purchases and sales increases from 2.92% in the non-Crisis period to 7.75% during the Bailout period (a difference of 4.83%).

Notably, the observation that the coefficient on *Buyer* varies over time, and is particularly heightened during the Bailout period, suggests that any time-invariant firm characteristic is unlikely to explain our results. To explain our results, the omitted variable needs to explain not only why buys (sells) are followed by positive (negative) returns, but also why the difference in returns following buys and sells expands during the Bailout period.

4.2. Regression Tests: Crisis Period and Political Connections

Panel A of Table 5 reports results from estimating equation (2) separately for the trades of insiders with and without political connections. Column (3) and (6) test for differences in coefficients between the two samples. Over both horizons, the coefficient on *Buyer*Crisis_Bailout* is economically and statistically significant *only* for the trades of politically connected insiders. For example, column (4) suggests that among 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). Columns (3) and (6) suggest that at both horizons, 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 (differences of 5.81 and 20.17 , p -values <0.01).

This result is unique to the Bailout period. The difference in the coefficients on *Buyer*Crisis_PreBailout* is not statistically significant, regardless of the horizon; and the difference in the coefficients on *Buyer* is only statistically significant when future returns are measured over a six-month horizon, and of a much smaller magnitude (difference of 2.60, *p*-value of 0.10 in column (6)). We also note that the differences in the coefficients on the control variables are generally not statistically significant—suggesting the determinants of the cross-section of returns do not vary with insiders’ political connections.

Panel B of Table 5 reports results from estimating equation (2) separately for the trades of insiders with and without political connections, after including both insider fixed effects and month fixed effects. This is an important feature of our design. Insider-level fixed effects control for heterogeneity in insider-specific characteristics that might influence trading activity (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 (e.g., corporate governance).¹³ In this regard, our fixed effect structure controls for any cross-sectional differences between firms with and without political connections.

Month-level fixed effects control for changes in market conditions that affect all firms within a given period. Note that 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

¹³ We find there are only six insiders that 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 equivalent to insider-firm fixed effects.

firms with and without politically connected insiders. In this regard, each regression can be thought of as within-insider and within-month.¹⁴

Panel B suggests inferences are robust to this alternative research design. 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 time-invariant characteristics of the insider, temporal changes in market conditions, or a differential effect of market conditions on firms with and without politically connected insiders. Collectively, we interpret the evidence as suggesting that the informativeness of insider trades increases dramatically during the Crisis—but only after large scale government intervention—and that the increase is concentrated almost entirely among the trades of politically connected insiders.

5. TARP Infusions

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

5.1. Cross-Sectional Tests

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. If the information advantage of politically connected insiders relates to private information about the probability their bank receives TARP funds, we expect to find politically connected insiders have an information advantage at banks that did, and did not, receive TARP funds. In contrast, if

¹⁴ To estimate insider and month fixed effects, we require multiple trades per insider and multiple trades per month, which slightly reduces the respective samples. Month fixed effects subsume the main effects of *Crisis_PreBailout* and *Crisis_Bailout* respectively.

insiders' information advantage relates to details of the funding itself (i.e., is conditional on funding), we expect to observe the information advantage of politically connected insiders is concentrated in banks that received TARP funds, and minimal to no evidence that politically connected insiders have an information advantage in banks that did not receive TARP funds.

Table 6 presents results from estimating equation (2) within the sample of politically connected insiders, at banks that received TARP funds, columns (1) and (4), and banks that did not receive TARP funds, columns (2) and (5). For parsimony, we report results only for specifications where returns are measured over a six-month horizon. 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 (1) suggests that the coefficient on *Buyer*Crisis_Bailout* is 26.04 for TARP recipients compared to 3.81 for non-recipients (difference of 22.23, p -value <0.01).¹⁵

Collectively the results in Table 6 suggest that the increase in the informativeness of insider trades during the Bailout period is concentrated among politically connected insiders at firms that received TARP funds. We find no evidence of an increase in informativeness of insider trades either among insiders without political connections, or 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. Additionally, finding that our results *obtain within the sample of banks that received TARP funds*, suggests that our results are not attributable to cross-sectional differences in the type of firms that received TARP money.

5.2. Event Study: Insider Trading around TARP Infusions

¹⁵ At the one month horizon this difference is 10.75 (p -value <0.01).

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

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

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

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.

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).

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 seller) in advance of the announcement, the subsequent announcement was 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 where 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 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),

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.

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 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. Additional Considerations

5.4.1 Distinguishing TARP Infusions from Other Information Events

In this section, we compare the informativeness of insider trades as it relates to TARP infusions with the informativeness of insider trades as it relates to another corporate information event not directly related to TARP infusions—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.

5.4.2 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.¹⁹ 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

¹⁹ 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.

ownership requirements that might be expected to mechanically increase insider purchases 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 using exclusively the trades of non-officer directors. We find no difference between the informativeness of politically connected, non-officer directors trades' and politically connected officers' trades.²⁰ The results suggest that executive compensation restrictions related to TARP, which only apply only to officers, cannot explain our results.

5.4.3. *Legality*

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 (this literature finds consistent evidence that insider trades predict stock returns and anticipate corporate events). 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 assessment is based on private information, it seems inappropriate for the firm to disclose such

²⁰ For the sample of trades by politically connected non-officer (officer) directors, we find that the coefficient on *Buyer*Crisis_Bailout* at the one-month horizon is 6.10 (7.59) with a *t*-statistic of 2.54 (2.89) a difference of -1.49 (*p*-value 0.61).

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.²¹ 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.

5.4.4. Information Sharing

A key assumption of our measure of political connections is that privileged information is shared among board members. While this is consistent with anecdotal and empirical evidence (e.g., Sorkin, 2009; Cao, Dhaliwal, Li, and Yang, 2014), information sharing is difficult to pin down in the absence of direct observation. Nevertheless, we conduct a supplemental analysis to try and validate this assumption. We examine how the information advantage of insiders on politically connected boards varies with the number of insiders trading within the same month. We find that the information advantage of insiders on politically connected boards (over those on unconnected boards) is largest when three or more insiders on the politically connected board trade in the same month.²² Given a politically connected board has on average one individual with a direct connection (50th percentile = 1 directors, 75th percentile = 2 directors), this suggests a shared information advantage that extends beyond just the directors with direct political and regulatory ties. These findings are consistent with networking theory which suggests information

²¹ 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) and the discussion surrounding disclose-or-abstain in Li, Wasley, and Zimmerman (2016).

²² For the sample of trades by insiders with (without) political connections where less than three insiders trade within the same month we find that the coefficient on *Buyer*Crisis_Bailout* at the one-month horizon is 3.61 (2.09) with a *t*-statistic of 2.17 (1.59) at the one-month horizon. For the sample of trades by insiders with (without) political connections where more than three insiders trade within the same month we find that the coefficient on *Buyer*Crisis_Bailout* is 7.81 (−0.30) with a *t*-statistic of 2.39 (−0.19).

diffuses over an individuals' professional network (e.g., Cohen, Frazzini, and Malloy, 2008, 2010) and is consistent with Ahern (2016) who suggests informed trading is more likely the greater degree of separation from the information source.

5.4.5. *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 which case the bank's stock price should already reflect skilled forecasts of outsiders with political connections, and in the absence of private information, trades of politically connected insiders' should not predict future returns.

Second, if politically connected insiders are simply better at forecasting government actions, then regardless of the actual TARP funding decision, 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.

6. Conclusion

In this paper, we examine the relation between political connections and the trading of corporate insiders in the context of the Financial Crisis. The unprecedented magnitude of government intervention during the Financial Crisis, the substantial impact of the intervention on share prices, and the political nature of the intervention provide a powerful setting to examine the relation between political connections and the trading of corporate insiders. Although we do not directly observe the information transfer, we conjecture that connections between insiders and governmental officials with knowledge about the details of government intervention may be a plausible conduit for the transfer of valuable private information.

We employ a large 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 dispersed, 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 the increase in the informativeness of insider trades during the Bailout period relating to private information gleaned from political connections, 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 longer horizons.

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 subsequently received TARP funds and (ii) that 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, in banks 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 be informed about 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.

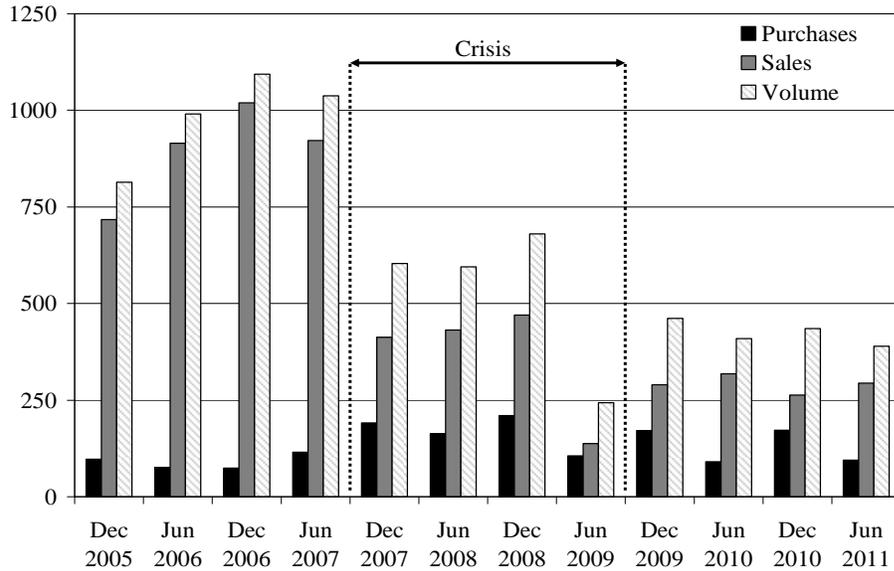
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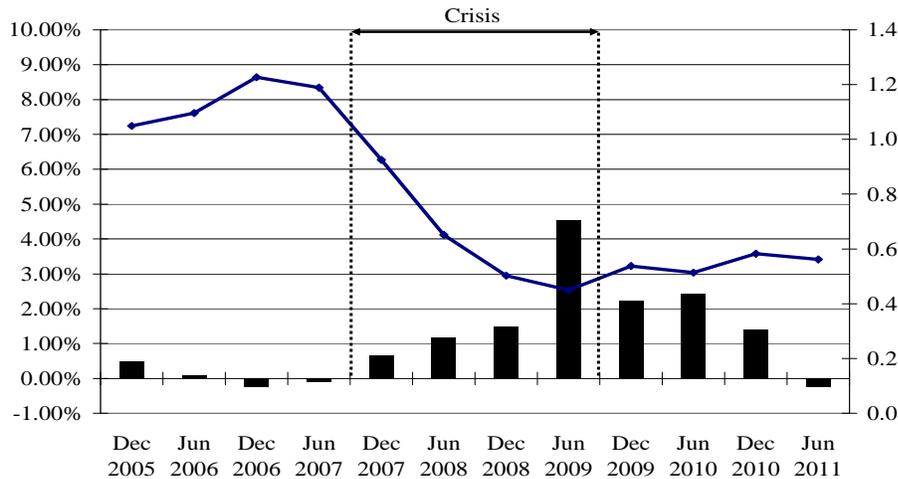
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Figure 1. Dollar value of purchases and sales by time period



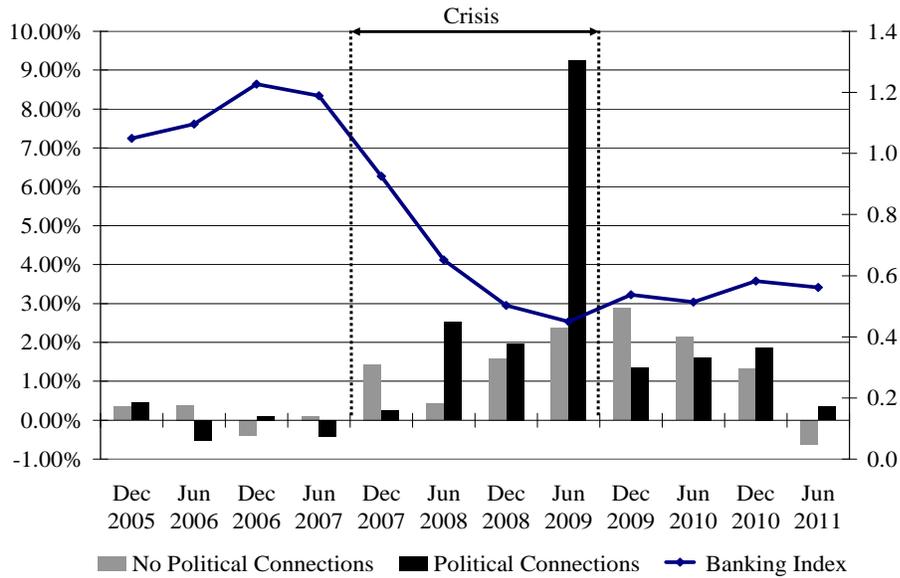
This figure plots the total dollar value of insider trades for all firms in our sample over the respective six month interval. Total dollar value of insider trades (in millions) appears on the y-axis, and the respective six month window appears on the x-axis. Black bars represent the total dollar value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



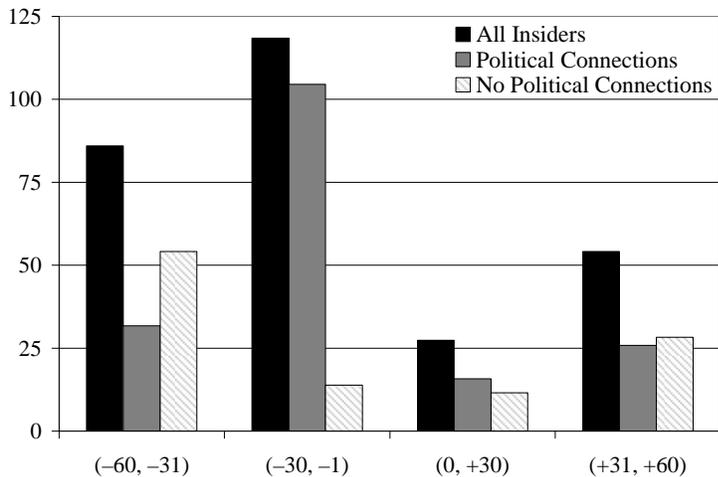
This figure plots the difference in one-month-ahead future returns between net purchase and net sales (black bars), averaged over the respective six month interval. We net all trades to the insider-month level and group observations based on whether the net trade in month t was a purchase or a sale. 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.

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_i* 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		2,776		4,546	
<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. 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). All trades are aggregated to the insider-month level. 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	Insiders with Political Connections				Insiders without Political Connections			
		Num. Obs.	Avg. <i>Buyer</i>	%Total Purchases	%Total Sales	Num. Obs.	Avg. <i>Buyer</i>	%Total Purchases	%Total Sales
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. 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 differences 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) separately for future returns over the subsequent month ($s = 1$) and subsequent six months ($s = 6$). All variables are defined in Table 1. t -statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Variables	Dependent variable: $BHR_{i,t+s}$			
	s = 1	s = 1	s = 6	s = 6
	(1)	(2)	(3)	(4)
<i>Buyer</i>	0.51** (2.04)	0.39* (1.66)	1.32 (1.46)	2.92*** (3.53)
<i>Buyer*Crisis_PreBailout</i>	.	-0.05 (-0.07)		-3.53** (-1.99)
<i>Buyer*Crisis_Bailout</i>	.	2.34** (2.49)		4.83* (1.87)
<i>Controls</i>				
<i>Size</i>	0.32*** (2.83)	0.32*** (2.80)	0.97*** (2.75)	1.07*** (3.25)
<i>BM</i>	2.57*** (4.61)	2.33*** (4.05)	6.91*** (4.86)	3.92*** (2.89)
<i>PastMoRet</i>	-0.11*** (-3.87)	-0.12*** (-4.11)	-0.08* (-1.66)	-0.17*** (-3.38)
<i>PastYrRet</i>	0.04*** (3.67)	0.02* (1.79)	0.17*** (7.30)	0.08*** (3.18)
<i>Crisis_PreBailout</i>		-2.10*** (-4.28)	.	-14.35*** (-11.47)
<i>Crisis_Bailout</i>		-3.34*** (-4.48)	.	-2.25 (-1.17)
F	13.24	12.33	17.38	41.19
N(insider-months)	29,777	29,777	29,777	29,777
N(firms)	497	497	497	497

Table 5. Political Connections and the Informativeness of Insider Trades During the Crisis

This table reports results from estimating the regression specification in Table 4 after partitioning the sample based on insiders' political connections. Panel A reports results from estimating equation (2) separately for trades placed by insiders with and without political connections, and testing for a difference in coefficients between the two samples. Panel B reports results after including insider fixed effects to control for time-invariant, insider-specific characteristics and month fixed effects to control for changes in market conditions. Month-level fixed effects subsume the main effects on *Crisis_PreBailout* and *Crisis_Bailout*. 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. 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.

Panel A. Pooled Regression

Variables	Dependent variable: $BHR_{i,t+s}$					
	<i>s</i> = 1			<i>s</i> = 6		
	Political Connections:			Political Connections:		
	Yes	No	Diff	Yes	No	Diff
(1)	(2)	(3)	(4)	(5)	(6)	
<i>Buyer</i>	-0.20 (-0.45)	0.55* (1.94)	-0.75 [0.15]	4.30*** (3.27)	1.70* (1.86)	2.60* [0.10]
<i>Buyer*Crisis_PreBailout</i>	0.40 (0.34)	0.14 (0.19)	0.27 [0.85]	-5.94** (-1.98)	-3.59* (-1.70)	-2.35 [0.52]
<i>Buyer*Crisis_Bailout</i>	6.43*** (3.35)	0.63 (0.62)	5.81*** [<0.01]	18.78*** (3.60)	-1.39 (-0.46)	20.17*** [<0.01]
Controls						
<i>Size</i>	0.18 (1.16)	0.48** (2.53)	-0.30 [0.23]	1.65*** (3.57)	0.44 (0.90)	1.21** [0.07]
<i>BM</i>	2.77** (2.48)	2.13*** (3.40)	0.64 [0.62]	5.50** (2.04)	2.72** (2.03)	2.78 [0.36]
<i>PastMoRet</i>	-0.12*** (-3.34)	-0.12*** (-2.97)	0.00 [0.98]	-0.24** (-2.56)	-0.13** (-2.38)	-0.11 [0.31]
<i>PastYrRet</i>	0.02 (1.35)	0.02 (1.41)	0.00 [0.91]	0.03 (0.84)	0.10*** (3.44)	-0.07 [0.17]
<i>Crisis_PreBailout</i>	-1.55** (-2.21)	-2.66*** (-4.20)	1.11 [0.24]	-15.85*** (-8.29)	-12.66*** (-7.64)	-3.19 [0.21]
<i>Crisis_Bailout</i>	-5.00*** (-3.93)	-2.54*** (-2.95)	-2.46 [0.11]	-9.47*** (-2.88)	1.51 (0.67)	-10.98*** [<0.01]
F	4.71	10.57		24.66	20.89	
N(insider-months)	10,204	19,572		10,204	19,572	
N(firms)	159	338		159	338	

Table 5. Political Connections and the Informativeness of Insider Trades During the Crisis (cont'd)

Panel B. Within-Insider and Within-Month Regressions

Variables	Dependent variable: $BHR_{i,t+s}$					
	s = 1			s = 6		
	Political Connections:			Political Connections:		
	Yes	No	Diff	Yes	No	Diff
(1)	(2)	(3)	(4)	(5)	(6)	
<i>Buyer</i>	1.26 (1.58)	0.70 (1.23)	0.56 [0.22]	4.54** (2.32)	0.85 (0.59)	3.69 [0.12]
<i>Buyer*Crisis_PreBailout</i>	-0.36 (-0.36)	-0.98 (-1.19)	0.62 [0.63]	-3.91 (-1.50)	-0.08 (-0.03)	-3.83 [0.29]
<i>Buyer*Crisis_Bailout</i>	2.83*** (2.69)	-0.96 (-0.82)	3.79*** [0.05]	8.81** (2.10)	-3.07 (-0.90)	11.88** [0.02]
Controls						
<i>Size</i>	0.25 (0.16)	-4.57** (-2.18)	4.82 [0.07]	0.28 (0.04)	-10.00*** (-2.99)	-10.28 [0.19]
<i>BM</i>	4.03*** (2.67)	1.79 (1.12)	2.24 [0.31]	11.49* (1.82)	6.75*** (2.68)	4.74 [0.48]
<i>PastMoRet</i>	-0.03 (-0.94)	-0.07* (-1.70)	0.04 [0.52]	-0.09 (-1.26)	-0.01 (-0.20)	-0.08 [0.41]
<i>PastYrRet</i>	0.02* (1.66)	0.03 (1.59)	-0.01 [0.75]	-0.03 (-0.95)	0.07 (1.35)	-0.10* [0.10]
Insider Fixed Effects	Yes	Yes		Yes	Yes	
Month Fixed Effects	Yes	Yes		Yes	Yes	
F	3.05	6.45		4.21	10.82	
N(insider-months)	9,308	18,126		9,308	18,126	
N(firms)	159	338		159	338	

Table 6. Role of TARP Within the Sample of Politically Connected Insiders

This table reports results from estimating equation (2) within the sample of politically connected insiders. Column (1) presents results for the trades of politically connected insiders at firms that received TARP funds. Column (2) presents results for the trades of politically connected insiders at firms that did not receive TARP funds. Columns (4) and (5) present analogous results after including insider and month fixed effects. All variables are defined in Table 1. For parsimony, coefficients on control variables are not tabulated. *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	Dependent variable: $BHR_{i,t+6}$					
	TARP Recipient			TARP Recipient		
	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)
<i>Buyer</i>	4.25** (2.46)	5.14*** (2.79)	-0.89 [0.73]	3.69 (1.40)	4.19* (1.86)	-0.50 [0.89]
<i>Buyer*Crisis_PreBailout</i>	-2.08 (-0.57)	-11.61*** (-2.76)	9.53* [0.09]	-1.35 (-0.43)	-8.25** (-2.26)	6.90 [0.15]
<i>Buyer*Crisis_Bailout</i>	26.04*** (3.75)	3.81 (0.79)	22.23*** [<0.01]	13.58** (2.52)	1.70 (0.37)	11.88* [0.09]
Control variables	Yes	Yes		Yes	Yes	
Insider Fixed Effects	No	No		Yes	Yes	
Month Fixed Effects	No	No		Yes	Yes	
F	26.16	9.29		4.58	1.28	
N(insider-months)	6,709	3,495		6,167	3,139	
N(firms)	92	67		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). Panels 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 that 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		