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Political Connections and the Informativeness of Insider Trades

ALAN D. JAGOLINZER, DAVID F. LARCKER, GAIZKA ORMAZABAL, and DANIEL J. TAYLOR*

ABSTRACT

We analyze the trading of corporate insiders at leading financial institutions during the 2007 to 2009 financial crisis. We find strong evidence of a relation between political connections and informed trading during the period in which Troubled Asset Relief Program (TARP) funds were disbursed, and that the relation is most pronounced among corporate insiders with recent direct connections. Notably, we find evidence of abnormal trading by politically connected insiders 30 days in advance of TARP infusions, and that these trades anticipate the market reaction to the infusion. Our results suggest that political connections can facilitate opportunistic behavior by corporate insiders.

AN EXTENSIVE EMPIRICAL LITERATURE EXAMINES the relation between managers' political connections and firm value. Most of this research suggests that political connections are associated with a wide range of shareholder benefits, including preferential access to capital and an increased likelihood of winning government procurement contracts (e.g., Faccio (2006), Goldman, Rocholl,

* Alan D. Jagolinzer is with the Judge Business School at University of Cambridge. David F. Larcker is with the Graduate School of Business at Stanford University. Gaizka Ormazabal is with the IESE Business School at University of Navarra. Daniel J. Taylor is with the Wharton School at University of Pennsylvania. We thank Stefan Nagel (editor), an anonymous associate editor, two anonymous reviewers, and seminar participants at Cornell, Northwestern, SUNY-Binghamton, Texas A&M, UC-Davis, University of Chicago, University of Minnesota, University of Southern California, University of Utah, and the Securities and Exchange Commission. We thank Joseph Grundfest and several officials at the U.S. Treasury, Federal Bureau of Investigation, and Department of Justice for helpful comments and suggestions. We thank Michelle Gutman, Adam Bordeman, Vikram Dhawam, John Kepler, Richard Knapp, Alex Yuan, Jessica Weber, and Ruizhong Zhang for outstanding research assistance, and Ran Duchin and Denis Sosyura for sharing their data on political connections. We thank our respective schools for financial support, and acknowledge funding from the Winnick Family Faculty Fellowship at Stanford University, the Marie Curie and Ramon y Cajal Fellowships at IESE, and the Dean's Research fund at The Wharton School. We have read *The Journal of Finance*'s disclosure policy and have no conflicts of interest to disclose. Data on TARP application amounts and dates used in the paper were obtained from the U.S. Treasury and are provided in the Internet Appendix that is available in the online version of the article on *The Journal of Finance* website.

Correspondence: Daniel J. Taylor, Wharton School, University of Pennsylvania, Philadelphia, PA 19105; e-mail: dtayl@wharton.upenn.edu.

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and So (2009), Cooper, Gulen, and Ovtchinnikov (2010)). However, while the shareholder benefits of political connections are well documented, evidence on whether political connections facilitate opportunistic behavior by managers and directors (hereafter "corporate insiders") is scarce. In this paper, we examine whether political connections facilitate one particular form of opportunism—informed trading.[1]

We examine the relation between political connections and informed trading by corporate insiders within the context of government intervention during the 2007 to 2009 financial crisis. The unprecedented magnitude of the intervention, the substantial impact of the intervention on firm value, and the political nature of the intervention provide a powerful setting in which to examine our research question. It is now well known that deliberations on government intervention took place largely in private meetings between government officials and corporate insiders of leading financial institutions. Moreover, 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 and how this intervention would affect their firms, for example, through the receipt of any forthcoming TARP monies.

While there are good reasons to suspect that politically connected insiders had private information in this setting, it is not obvious that insiders would trade based on this information. Opportunistic use of political connections for personal gain carries significant risks. For example, unlike other market participants, corporate officers and directors have a fiduciary duty to shareholders to "disclose or abstain (from trading)," that is, if officers or directors had private information that their firm would receive government monies, their fiduciary duty requires that they either disclose that information or abstain from trading until such time as the information becomes public. In this regard, our tests are joint tests that political connections provide an information advantage and that insiders trade based on this advantage. We expect the former—the opportunity for informed trading—to be more pronounced during the crisis than during other periods.

We examine the relation between political connections and the trading of corporate insiders using a comprehensive sample of all open market purchases and sales of Section 16 officers and directors ("insiders") at 497 publicly traded financial institutions ("banks") between 2005 and 2011. Following prior research, we identify political connections based on whether a board member has current or previous work experience at the Federal Reserve, Treasury

[1] Throughout the paper, we use the term "insider" or "corporate insider" to refer to officers and directors covered by Section 16(b) of the Securities and Exchange Act of 1934, which requires that such insiders disclose their trades on Form 4 filings with the Securities and Exchange Commission (SEC).

Department, Congress, or a bank regulator (e.g., Office of Comptroller of Currency [OCC], Office of Thrift Supervision [OTS], or Federal Deposit Insurance Corporation [FDIC]). We then measure the "informativeness" of insider trades based on their predictive ability for future performance.

We begin our analysis by examining trades prior to the crisis. Consistent with the notion that managers were unable to predict the effect of the forthcoming crisis on their firm (e.g., Fahlenbrach and Stulz (2011)), we find no evidence that insider trades predict future performance over the 24 months leading up to the crisis or during the crisis period before the creation of TARP (i.e., prior to October 2008). Moreover, we find no evidence of a relation between political connections and informed trading during either period. These findings are consistent with the notion that when political connections play less of a role in influencing firm-level outcomes (e.g., prior to large-scale government intervention), they also play less of role in influencing trading decisions.

In contrast, over the nine months *after* the creation of TARP (i.e., the period over which TARP funds were disbursed), we find a sharp increase in the predictive ability of insider trades for future performance. During this period, the average one-month-ahead future return following purchases (sales) is 1.84% (−2.87%), a difference of 4.71%. Strikingly, we find that the increase in predictive ability is concentrated almost entirely in the trades of politically connected insiders. During the period over which TARP funds were disbursed, the difference in one-month-ahead future returns between purchases and sales of insiders with (without) political connections is both economically and statistically significant, at 8.89% (2.81%).[2]

These findings are robust to a battery of sensitivity analyses, for instance, using various fixed effect structures to control for firm-specific characteristics, changes in market conditions, and a differential effect of market conditions on firms with and without politically connected insiders. Our analyses suggest that *within a given firm*, those officers and directors with political connections significantly outperformed their peers during the crisis. These results are consistent with political connections providing insiders with an information advantage, and with insiders opportunistically trading to exploit that advantage. The results indicate that political connections are more likely to influence trading decisions during periods in which the government plays an active role in capital markets (i.e., during periods of large-scale government intervention).

While our findings are consistent with politically connected insiders having a significant information advantage during the crisis and opportunistically trading to exploit this advantage, they do not speak to the source of this information

[2] During this period, politically connected insiders made 3,058 trades that amounted to over $324 million in trading volume. Average trade size was $105,987 and the average trade earned market-adjusted profits of $22,251; aggregating across all trades yields $68 million in profits. Market-adjusted profits are calculated as the signed dollar value of the trade multiplied by the market-adjusted buy-and-hold return over the subsequent 180 days (Ravina and Sapienza (2010), Jagolinzer, Larcker, and Taylor (2011)).

advantage. We consider two potential sources of this advantage. First, politically connected insiders might have been "tipped" about forthcoming TARP infusions.[3] Second, even in the absence of a tip, politically connected insiders might be more skilled at forecasting government actions, in which case these insiders would have an advantage over both politically connected outsiders and unconnected insiders. This distinction is subtle. From an economic standpoint, both cases entail trading of private information—information that is not common knowledge and not impounded into price.

Admittedly, in the absence of directly observing a "tip," it is difficult to empirically distinguish between these two potential sources of private information. Indeed, this is a general limitation of prior academic research examining the trading patterns of corporate insiders—their trades are known to predict future performance, but it is difficult to pin down the source of the information or the channel through which insiders learn that information (e.g., Cohen, Malloy, Pomorski (2012)). Nonetheless, we conduct two sets of analyses to strengthen our empirical identification and to speak to potential sources of insiders' information advantage.

First, we use detailed data on officers' and directors' work history disclosed in proxy statement biographies to construct a network map. While undoubtedly incomplete, this map enables us to trace the nature of the political connection, the degrees of separation, and the recency of the connection, as well as to identify several relevant individual traits. We then correlate these attributes of the connection with the informativeness of insider trades during the crisis. Within the sample of politically connected insiders who trade during the crisis, we find that the information advantage is most pronounced among insiders with recent, direct connections, and that this advantage is partially (but incompletely) transferred to other board members of the firm.

Second, we use an event study design to examine insider trades in a short window prior to TARP infusions. Evidence of elevated trading among politically connected insiders prior to TARP infusions, and that these trades predict the market reaction to the infusion, would suggest that insiders are trading based on superior information about TARP infusions. Measuring trading by corporate insiders over the 30 days prior to the announcement of their firm's TARP infusion, we find that insiders are net buyers (sellers) before 34.8% (20.3%) of

[3] This channel is consistent with prior research, suggesting that social connections, in general, and political connections, in particular, serve as a conduit for information transfer (e.g., Cohen, Frazzini, and Malloy (2008, 2010), Gao and Huang (2016), Ahern (2017), Christensen etal. (2017)). It is also consistent with prosecuted cases of information leakage by politically connected individuals. For anecdotes, see Hamburger, Tom, "Political intelligence firms set up investor meetings at White House," *Washington Post*, May 26, 2013; Henning, Peter J., "The fine line between political intelligence and insider trading," *New York Times*, April 8, 2013; McCoy, Kevin, "Court fight bares SEC insider-trading probe," *USA Today*, June 26, 2014; Viswanatha, Aruna, Kate Davidson, Brody Mullins, and Christopher M. Matthews, "Questions about leak at Federal Reserve escalate to insider-trading probe," *Wall Street Journal*, September 30, 2015; Mullins, Brody, and Aruna Viswanatha, "The king of political intelligence faces a reckoning," *Wall Street Journal*, August 18, 2016; and Cox, Jeff, "Richmond Fed President Lacker says he was involved with Medley leak, announces immediate resignation," *CNBC*, April 4, 2017.

infusions in our sample. We find a pronounced increase in the trading activity of politically connected insiders 30 days prior to the announcement, and that these trades predict the market reaction to the infusion. Notably, similar results do not obtain among insiders without political connections: insiders without political connections do not appear to time their trades to coincide with TARP infusions.

The staggered nature of TARP infusions provides several potential avenues to control for confounding effects. The results are robust to a variety of such tests. For example, similar results do not obtain among politically connected insiders on nonannouncement dates, among politically connected insiders around other corporate events not directly related to TARP infusions (e.g., earnings announcements), or among politically connected insiders in firms that did not receive TARP funds. Taken together, the results of our event study suggest that politically connected insiders traded to front-run TARP infusions.

Finally, to identify a potential source of information, we obtain data on TARP applications through a Freedom of Information Act (FOIA) request to the U.S. Treasury. While the Treasury did not furnish the actual TARP application or dates of associated correspondence, they did provide us with a list of bank names, application dates, and application amounts.[4] We find that the trades of politically connected insiders who front-run the infusion vary with the wedge between the infusion amount and the application amount. The greater the infusion amount as a percentage of the application amount, the more likely politically connected insiders were to purchase shares 30 days prior to the infusion. These results do not hold among insiders without connections. These findings suggest that private information about TARP infusions is an important (but not the exclusive) source of politically connected insiders' information advantage.

Overall, our findings suggest that political connections provided corporate insiders with an important information advantage during the financial crisis and that insiders traded to exploit their advantage. This conclusion is subject to two important caveats. First, from correlations alone, it is difficult to speak to the legality of a given trade or series of trades. This limitation is not unique to our study and applies broadly to the academic literature on insider trading and option backdating (see Ritter (2008)). Nevertheless, our analysis casts suspicion on trades of politically connected insiders during the crisis, especially those trades that occurred in close proximity to TARP infusions. At a minimum, the behavior we document is evidence of opportunism on the part of politically connected insiders. Uncovering such opportunism furthers our understanding of the consequences of opacity in government decision-making, and highlights a novel channel through which insiders can benefit from crony capitalism.

Second, our focus on financial institutions during the crisis potentially limits the generalizability of our findings—our findings do not generalize to the "average politically connected insider" or the "average period." However, generalizability is less of a concern given our research question. We do not seek to

[4] The raw data are provided in Table IA.VIII of the Internet Appendix.

examine whether the average politically connected insider trades opportunistically, but rather whether a detectable set of politically connected insiders engage in such behavior when given the opportunity. Our findings are particularly salient in the current economic climate, where the government is playing a more active role in U.S. capital markets. As the government becomes more active in influencing firm-level outcomes, we expect politically connected insiders to be at an information advantage. We therefore encourage investors and market regulators to monitor the trades of politically connected insiders.

The remainder of the paper proceeds as follows. Section I discusses related literature and the institutional setting. Section II describes our sample and the measurement of key variables. Section III presents results from our primary regression tests. Section IV discusses the construction and analysis of the map of insiders' networks. Section V presents our event-study tests relating to TARP infusions. Section VI discusses several alternative explanations, robustness tests, and additional analyses. Section VII provides concluding remarks.

I. Related Literature

Our study bridges three distinct literatures: that on government intervention during the financial crisis, that on political connections, and that on insider trading.

A. Government Intervention during the Financial Crisis

On September 18, 2008, congressional leaders met with U.S. Treasury Secretary Henry Paulson and Federal Reserve Chairman Ben S. Bernanke and were briefed on a plan for massive government intervention in the financial system on a scale not seen since the Great Depression.[5] On September 22, 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 29 due to concerns about inadequate transparency and the staggering size of funds requested.[6] A few days later, on October 1, the Senate considered and passed a revised TARP bill that was subsequently passed by the House of Representatives and signed into law by the President on October 3.

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 range of financial institutions. While participation for the nine largest banks was mandatory, subsequent

[5] Appelbaum, Binyamin, and Lori Montgomery, "Citing grave financial threats, officials ready massive rescue," *Washington Post*, September 19, 2008.

[6] Hulse, Carl, and David M. Herszenhorn, "Defiant House rejects huge bailout; next step is uncertain," *New York Times*, Sep 30, 2008.

participation was voluntary.[7] Prior research generally suggests that these "TARP infusions" resulted in significant changes in firm value. For example, Veronesi and Zingales (2010, p. 340) estimate that TARP "created between $86 and $108 billion in value."[8] Importantly, prior research also suggests a considerable "political dimension" to the creation and allocation of TARP funds. Mian, Sufi, and Trebbi (2010), for instance, 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 of a bank receiving 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 unprecedented magnitude of government intervention during the financial crisis and its political nature provide a powerful setting in which to examine whether political connections facilitate opportunistic behavior.

B. Political Connections

A large literature suggests that political connections increase access to capital (e.g., Khwaja and Mian (2005), Faccio, Masulis, and McConnel (2006)). Leuz and Oberholzer-Gee (2006) and Claessens, Feijen, and Laeven (2008) can be instrumental in winning government procurement contracts (e.g., Goldman, Rocholl, and So (2013), Tahoun (2014), and can favorably influence tax policy (e.g., Brown, Drake, and Wellman (2015)). Studies in this literature generally conclude that political connections are value-increasing (e.g., Goldman, Rocholl, and So (2009), Cooper, Gulen, and Ovtchinnikov (2010)). For example, 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 U.S. Treasury Secretary.

Valuation implications notwithstanding, evidence on whether insiders' political connections facilitate opportunistic behavior is scarce. For example, several recent studies examine the relation between political connections and prosecutions for white-collar crime (e.g., Yu and Yu (2011), Correia (2014), Bourveau, Coulomb, and Sangnier (2018)). We contribute to this literature by examining opportunistic use of political connections, and by documenting that such

[7] The nine banks initially receiving TARP funds were "forced" to take the infusion to mitigate concerns about adverse selection with respect to funding (e.g., Landler, Mark, and Eric Dash, "Drama behind a $250 billion banking deal," *New York Times*, October 14, 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. A total of 707 financial institutions received capital injections: 350 publicly traded banks, 296 private banks, 57 thrifts, and four nonbank financial institutions. Ultimately, TARP provided approximately $205 billion in capital infusions and was effectively concluded by June 2009.

[8] Bayazitova and Shivdasani (2012) document returns of approximately 15% around the October 14 announcement for the initial nine recipients and approximately 4% for subsequent recipients (see also Farruggio, Michalak, and Uhde (2013)). Ng, Vasvari, and Wittenberg-Moerman (2011) report that after the conclusion of the program, the portfolio of recipients outperformed that of nonrecipients by 10.3% through December 2010.

connections facilitate one particular form of opportunism—informed trading. Broadly speaking, the view that political connections can facilitate informed trading is in line with prior work that suggests politicians exploit their information advantage to earn abnormal returns (e.g., Ziobrowski et al. (2004)).

C. Insider Trading Literature

It is illegal for insiders to trade while in possession of material nonpublic information (Securities and Exchange Acts of 1933 and 1934, Insider Trading Sanctions Act of 1984, Insider Trading and Securities Fraud Enforcement Act of 1988). Moreover, unlike other market participants, corporate officers and directors have a fiduciary duty to shareholders, a duty that requires them to "disclose or abstain (from trading)." Thus, while there are strong reasons to suspect that politically connected insiders had an information advantage during the crisis, and thus had the opportunity to trade, it is not obvious that insiders would have exploited this advantage.

Prior research finds 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 (2014) report that over the same period, the dollar value of insider sales at the 14 largest banks was 100 times the dollar value of insider purchases. Similarly, Ryan, Tucker, and Zhou (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 of the top 80 banks *did not* significantly reduce their ownership between 2007 and 2008, and Cheng, Raina, and Xiong (2013) find that managers of financial institutions aggressively increased their personal investments in the housing market leading up to the crisis.

Our paper is the first to examine whether politically connected insiders traded to exploit their information advantage during the crisis. Similar to our analysis, in a subsequent working paper, Akin et al. (2018, ACFP) show that the cumulative trades of politically connected insiders during the crisis predict the market reaction to TARP infusions. While this finding is similar in spirit to one aspect of our analysis, ACFP do not examine trades in *short windows* (i.e., one-month or less) before TARP infusions, do not conduct a broad panel data analysis of the relation between political connections and informed trading pre-, during, and postcrisis, and do not build a network map of political connections to analyze individual attributes of the connection.

Our paper extends this literature by offering novel evidence on the relation between political connections and informed trading pre-, during, and postcrisis. While prior literature finds that insiders' trades reflect private information, identifying the source of insiders' information advantage is challenging. Our results suggest that insiders' political connections are one source of private information and that this information is being shared within the firm; political

connections provide an information advantage that is partially (but incompletely) transferred to other board members of the firm.

II. Data and Measurement

A. Sample

We collect data on officers' and directors' trades from the Thomson Reuters Insider Filings (Form 4) database. This database contains detailed information on the trades of corporate insiders in their firm's equity.[9] 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 range provides a symmetric two-year window both before and after the crisis, and ensures that all trades in our sample occur after the effective date of the Sarbanes-Oxley Act of 2002, which requires that corporate insiders report their trades electronically to the SEC within two business days. We refer to the two-year period from July 2005 to June 2007 as the "precrisis period," the two-year period from July 2007 to June 2009 as the "crisis period," and the two-year period from July 2009 to June 2011 as the "postcrisis period." We further divide the crisis period into the "prebailout period" from July 2007 to September 2008 and the "bailout period" from October 2008 to June 2009, where the latter corresponds to the period over which 99% of TARP funds were disbursed.[10]

We merge the Thomson Reuters Insider Filings database with CRSP/Compustat to obtain data on stock returns, market value, book-to-market, and earnings. To appear in the sample, we require market value at the end of the month, returns in both 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 comes from Duchin and Sosyura (2012) and covers all publicly traded financial institutions that were eligible for TARP funds (i.e., domestically controlled banks, bank holding companies, and saving and loan associations). After requiring data on political connections, the final sample for our cross-sectional

[9] We do not observe officers' and directors' trades in other firms or trades of lower level employees.

[10] NBER business cycle dates indicate that a recession started in the fourth quarter of 2007 and continued through the end of the second quarter of 2009, but prior work generally considers the crisis as starting at the beginning of the third quarter of 2007 (e.g., Acharya and Richardson (2009), Brunnermeier (2009), Fahlenbrach and Stulz (2011)). Inferences throughout the paper are robust, and are generally strengthened, if we define the crisis as ending in March 2009 or December 2009 rather than June 2009. Figures2 and3 present results in six-month intervals from 2005 through 2011 and provide a sense of the robustness of our results to alternative sample period definitions.

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.

B. 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 (such as the FDIC, OTS, or OCC), the 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. All officers and directors at banks in which one or more members of the board list such work experience are deemed "politically connected insiders."[11]

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, 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 more 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, et al. (2015), Kim (2016)), and an analysis of prosecuted cases suggests that trading on private information typically occurs more than one degree of separation from the source of private information (e.g., Ahern (2017)). It seems plausible that private information 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)). Accordingly, we include a broad set of corporate insiders who interact professionally (and likely socially) on a routine basis. If anything, misclassifying individuals as connected when no such connection exists biases against finding a relation between connections and their trades. In subsequent analysis, we focus on specific individuals with connections and attributes of the connection (e.g., recency of the connection, degrees of separation, etc.).

C. Descriptive Statistics

Table I presents descriptive statistics for our sample. Panel A shows that the typical firm in our sample has 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 shows that the board of the average firm in our sample has 0.58 directors with political connections (mean *NumPolConn* of 0.58), 34% of firms have at least one director

[11] We thank Ran Duchin and Denis Sosyura for providing these data.

Table I
Descriptive Statistics

This table presents descriptive statistics for the variables used in our tests. Panel A presents descriptive statistics for firm characteristics. Panel B presents descriptive statistics for insider trades. *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 with current or previous work experience at the Federal Reserve, a bank regulator (FDIC, OTS, or OCC), the U.S. Treasury, or Congress, and *Connected* is an indicator equal to 1 if at least one member has such experience. *TARPRecipient* is an indicator variable equal to 1 if the firm received TARP funds. $Buyer_t$ is an indicator variable equal to 1 if the number of shares bought by insider i in firm j and month t exceeds the number of shares sold by insider i in firm j and month t. BHR_{t+1} (BHR_{t+6}) is the buy-and-hold return over the one month (six months) subsequent to the trade. #*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
Size	5.59	1.85	4.25	5.33	6.53
BM	1.02	0.85	0.53	0.75	1.17
PastMoRet	−1.78	11.58	−6.28	−0.68	3.78
PastYrRet	−5.11	28.71	−21.05	−2.55	10.98
NumPolConn	0.58	1.13	0.00	0.00	1.00
Connected	0.34	0.47	0.00	0.00	1.00
TARPRecipient	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 $N = 19{,}573$	
Variable	Mean	Median	Mean	Median	Mean	Median
Buyer	0.68	1.00	0.54	1.00	0.74	1.00
BHR_{t+1}	−0.11	−0.32	0.20	−0.06	−0.27	−0.41
BHR_{t+6}	−4.79	−3.90	−4.04	−3.41	−5.18	−4.28
#*Unique Firms*	497		159		338	
#*Unique Insiders*	7,301		2,767		4,534	
Total Purchases ($ millions)	1,562.22		769.24		792.98	
Total Sales ($ millions)	6,190.63		4,547.03		1,643.60	

with political connections (mean *Connected* of 0.34), and slightly more than half of the sample firms received TARP funds (mean *TARPRecipient* of 0.56).[12]

[12] We winsorize all continuous variables at the 1st and 99th percentiles and exclude all insider trades (aggregated by month) of $100 million or more. This results in the exclusion of 11 transactions: a $737 million open market sale by Citigroup director Roberto Hernandez Ramirez on November 9, 2006 (shares were acquired in connection with Citigroup's purchase of Banamex),

Panel B shows 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.

Panel B reports descriptive statistics after partitioning the sample based on insiders' political connections. There are 2,767 (4,534) distinct insiders with (without) political connections in our sample of 159 (338) firms. Panel B shows that 54% (74%) of trades by insiders with (without) political connections are net purchases, the total value of purchases by insiders with (without) political connections is $769 million ($793 million), and the total value of sales by insiders with (without) political connections is $4.5 billion ($1.6 billion). These statistics suggest that insiders with political connections are more likely to sell shares and sell shares in greater volume.

D. Insider Trading during the Crisis

Figure 1 plots the total dollar value of insider purchases and sales over time. Figure 1 suggests that the dollar volume of insider sales drops dramatically at the start of the crisis, whereas the dollar volume of insider purchases increases at the start of the crisis and peaks during the bailout period.

Table II presents average values of our measures of insider trading activity in each period. Panel A shows that 55% of trades prior to the crisis, 76% during the prebailout period, and 78% during the bailout period are purchases. Panel B of Table II presents average values for our measures of insider trading activity after partitioning the sample based on insiders' political connections. Panel B shows that insiders with political connections tend to be net *sellers* before the crisis (39% of trades in the precrisis period are purchases) and net *buyers* during the crisis (68% of trades in the crisis period are purchases) with their buying most intense in the bailout period (74% of trades in the bailout period are purchases). Panel B also shows 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). Insiders with political connections typically account for around 73% of sales volume, but their share of sales volume decreases sharply to 43% during the bailout period. Conversely, insiders with political connections typically

a $288 million open market sale by 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 11 of these transactions occurred outside the crisis period and thus do not affect our inferences.

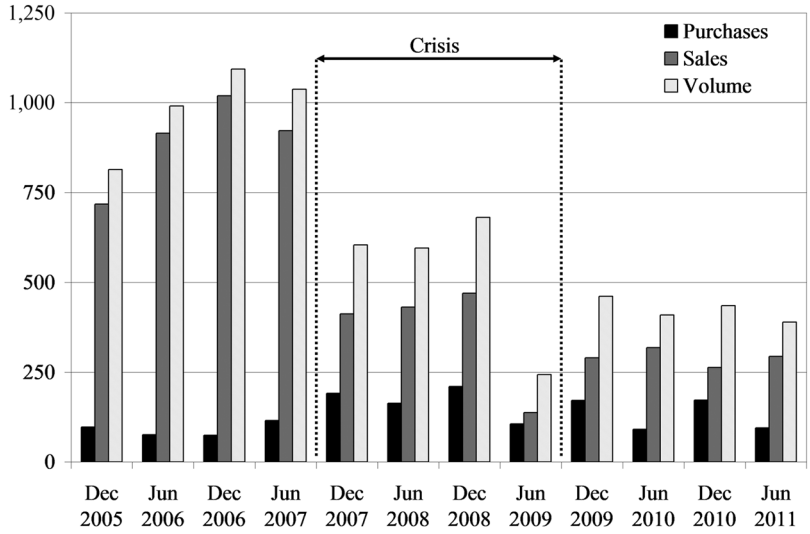


Figure 1. Dollar value of purchases and sales by time period. This figure plots the total dollar value of insider trades for all firms in our sample over the respective six-month interval. The total dollar value of insider trades (in millions) appears on the *y*-axis, and the respective six-month window 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.

account for around 49% of purchase volume, but their share of purchase volume increases sharply to 71% during the bailout period.

Taken together, the evidence in Figure 1 and Table II suggests a relative increase in buying among insiders with political connections during the crisis. The percentage of trades (dollar volume) that are purchases increases during the bailout period, with this increase 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 on the predictive ability of insider trades for the cross-section of future performance.

III. Predictive Ability of Insider Trades for Future Performance

A. 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 are informed, that is, anticipate future performance. Our primary tests examine the informativeness of insider trades and how this relates to political connections. Following a large literature on insider trading, we capture 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

Table II

Trading Activity during the Financial Crisis

This table presents descriptive statistics for insider trading variables over time. Panel A presents the number of trades, the average values of *Buyer*, 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, the panel reports the number of trades, average value of *Buyer*, and the fraction of the 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), the U.S. Treasury, or Congress.

Panel A. Insider Trading Activity by Period

Time Period	Date Range	Num. Obs.	Avg. *Buyer*	*Total Purchases* ($ millions)	*Total Sales* ($ millions)	*Purchases/Sales*
Full Sample	July 5 to Jun 11	29,777	0.68	1,562.22	6,190.63	0.25
24 mos. before the Crisis	July 5 to June 7	10,278	0.55	362.22	3,573.32	0.10
Crisis: Prebailout	July 7 to September 8	7,625	0.76	443.15	1,078.84	0.41
Crisis: Bailout	October 8 to June 9	4,298	0.78	227.67	372.06	0.61
24 mos. after the Crisis	July 9 to June 11	7,576	0.70	529.18	1,166.41	0.45

Panel B. Political Connections and Insider Trading Activity by Period

		Insiders with Political Connections			Insiders without Political Connections		
Time Period	Date Range	Avg. *Buyer*	*%Total Purchases in Panel A*	*%Total Sales in Panel A*	Avg. *Buyer*	*%Total Purchases in Panel A*	*%Total Sales in Panel A*
Full Sample	July 5 to June 11	0.54	49.24	73.45	0.74	50.76	26.55
24 mos. before the Crisis	July 5 to June 7	0.39	42.22	73.23	0.64	57.78	26.77
Crisis: Prebailout	July 7 to September 08	0.64	42.92	80.90	0.83	57.08	19.10
Crisis: Bailout	October 8 to June 9	0.74	71.08	43.78	0.80	28.92	56.22
24 mos. after the Crisis	July 9 to June 11	0.57	49.94	76.74	0.77	50.06	23.26

(lower) among firms in which insiders buy (sell) (e.g., Aboody and Lev (2000), Lakonishok and Lee (2001), Jeng, Metrick, and Zeckhauser (2003), Cohen, Malloy, and Pomorski (2012)). We thus measure insiders' information advantage relative to the information already impounded in prices.

Panel A of Table III presents average one-month-ahead future returns following purchases and sales separately. Consistent with prior literature, we find that 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, at 1.05% *per month* over the entire sample (t-statistic of 6.76). Notably, we find the predictive ability of insider trades for future performance is greater during the bailout period 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. The average one-month-ahead future return following purchases (sales) is 1.84% (−2.87%), a difference of 4.71%.

Panel B of Table III 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-statistic 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-statistic of −1.50, 0.89, and −0.10 during the precrisis, prebailout, and postcrisis period, respectively).

Figure 2 plots the difference in one-month-ahead future returns between purchases and sales over time. The figure shows that 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. These results suggest that the findings above are 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. 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) measuring returns over longer horizons, (iii) conducting a within-firm analysis, (iii) controlling for contemporaneous changes in market conditions, and (iv) controlling for differences in market conditions between firms with and without politically connected insiders.

Table III

Average Future Returns

This table presents average future returns following insider trades. 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 (2×2) according to whether the insider is politically connected and whether the trade was a net purchase or net sale. 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), the U.S. Treasury, or Congress. *t*-Statistics appear in parentheses and test for the difference in means. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 level (two-tail), respectively.

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	July 5 to June 11	0.23	−0.82	1.05^{***}	(6.76)
24 mos. before the Crisis	July 5 to June 7	−0.24	−0.10	−0.14	(−1.44)
Crisis: Prebailout	July 7 to September 8	−1.35	−2.49	1.14^{***}	(3.12)
Crisis: Bailout	October 8 to June 9	1.84	−2.87	4.71^{***}	(6.18)
24 mos. after the Crisis	July 9 to June 11	1.44	−0.14	1.58^{***}	(4.99)

Panel B. Political Connections and Average Future Returns by Period

		Insiders with Political Connections				Insiders without Political Connections					
Label	Date Range	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}		Difference-in-Differences	
Full Sample	July 5 to June 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	July 5 to June 7	−0.44	−0.07	−0.37	(−2.34)	−0.17	−0.12	−0.05	(−0.37)	−0.32	(−1.50)
Crisis: Prebailout	July 7 to September 8	−0.27	−2.07	1.80^{***}	(2.82)	−1.79	−2.93	1.14^{***}	(2.49)	0.66	(0.89)
Crisis: Bailout	October 8 to June 9	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	July 9 to June 11	1.31	−0.19	1.50^{***}	(3.72)	1.48	−0.09	1.57^{***}	(3.46)	−0.07	(−0.10)

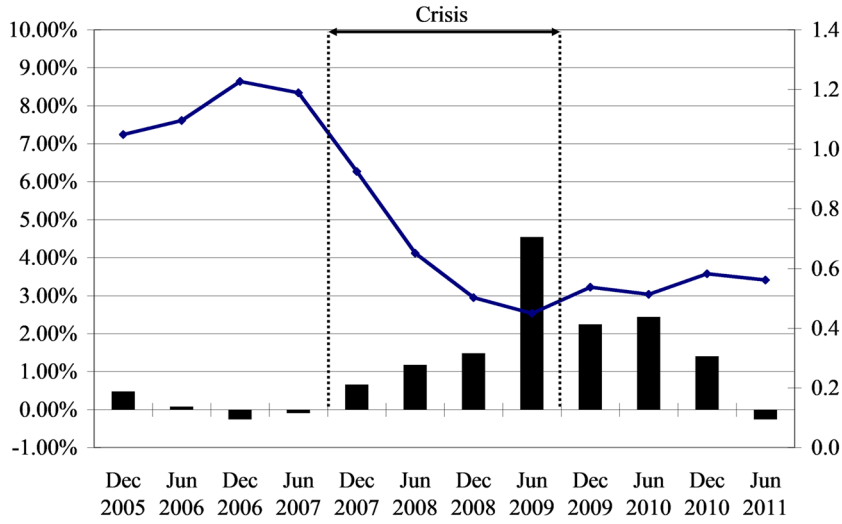


Figure 2. Difference in future returns between purchases and sales. This figure plots the difference in one-month-ahead future returns between net purchases and net sales, averaged over the respective six-month interval (black bars). We net all trades to the insider-month level and group observations based on whether the net trade in month t was a purchase or 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).
(Color figure can be viewed at wileyonlinelibrary.com)

B. Pooled Regression Tests

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

$$BHR_{t+s} = \delta_1 Buyer_t + \vartheta\ Controls_t + \varepsilon_{t+1}, \tag{1}$$

where BHR_{t+s} is either returns over the next month ($s = 1$) or over the next six months ($s = 6$), $Buyer_t$ is an indicator variable equal to 1 if the number of shares bought by insider i in firm j and month t exceeds the number of shares sold by insider i in firm j and month t (all trades are aggregated to the insider-firm-month level), and $Controls_t$ is a vector of control variables that includes firm size ($Size_t$), book-to-market ratio (BM_t), returns over the past month ($PastMoRet_t$), and returns over the past year ($PastYrRet_t$). For parsimony, we omit subscripts for insider i at firm j.[13] All variables are defined in Table I. In this specification, δ_1 represents 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 the

[13] We find that only 21 insiders overlap across multiple firms in our sample, and thus, the unit of analysis is effectively an insider-month.

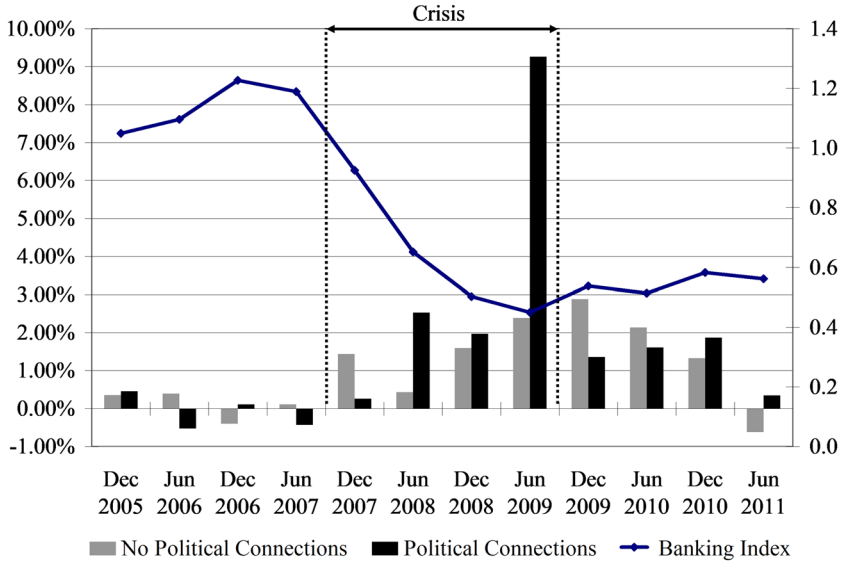


Figure 3. Political connections and the difference in future returns. This figure plots the difference in one-month-ahead future returns between net purchases and net sales, averaged over the respective six-month interval (black bars). We net all trades to the insider-month level and group observations based on whether the insider is politically connected and whether the net trade in month t was a purchase or 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).
(Color figure can be viewed at wileyonlinelibrary.com)

indicator variables $Crisiss\_PreBailout_t$ and $Crisiss\_Bailout_t$ for the "prebailout period" (July 2007 to September 2008) and the bailout period (October 2008 to June 2009), respectively, and interact these variables with $Buyer_t$:

$$\begin{aligned} BHR_{t+s} = amp;\delta_1 Buyer_t + \delta_2 Buyer_t * \ Crisis\_PreBailout_t \\ + \delta_3 Buyer_t * \ Crisis\_Bailout_t + \beta_1 \, Crisis\_PreBailout_t \\ + \beta_2 \, Crisis\_Bailout_t + \vartheta \ Controls_t + \varepsilon_{t+1,} \end{aligned} \tag{2}$$

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 base inferences on standard errors clustered by firm. Clustering by firm allows for both arbitrary time-series correlation within a firm and arbitrary cross-sectional correlation across insiders within a given firm.

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

To examine the relation between political connections and the informativeness of insider trades, we estimate equation (2) separately for the sample of insiders with political connections (*Connected* $= 1$) and without political connections (*Connected* $= 0$). Column (3) presents results for the sample of insiders with political connections, column (4) presents results for the sample of insiders without political connections, and column (5) tests for differences between the coefficients for the two samples.

Interestingly, the coefficient on *Buyer*Crisis_Bailout* is economically and statistically significant *only* for the trades of politically connected insiders. Column (3) suggests that among the trades of politically connected insiders, the difference in one-month returns between purchases and sales increases from an economically and statistically insignificant -0.20% in the noncrisis period to 6.23% during the bailout period (a difference of 6.43%, t-statistic of 3.35). Additionally, column (5) suggests that the coefficient on *Buyer*Crisis_Bailout* estimated on the sample of insiders *with* political connections is economically and statistically larger than the same coefficient estimated on the sample of insiders *without* political connections (difference of 5.80, p-value < 0.01).

Notably, the difference in informativeness of insider trades between insiders with and without political connections is unique to the bailout period: the difference between the coefficients on *Buyer* and the difference between the coefficients on *Buyer*Crisis_PreBailout* are not statistically significant (p-values of 0.15 and 0.85, respectively). We also note that the differences in the coefficients on the control variables are generally not statistically significant, which suggests that the determinants of the cross-section of returns do not vary with insiders' political connections.

Panel B of Table IV presents similar results when future returns are measured over a six-month horizon. Measuring future returns of a six-month horizon allows for the possibility that it takes longer than one month for insiders' private information to be impounded into prices. Column (5) of Panel B shows that the coefficient on *Buyer*Crisis_Bailout* estimated on the sample of trades by insiders *with* political connections is economically and statistically larger than that estimated on the sample of trades by insiders *without* political connections (difference of 20.17, p-value < 0.01).

Table V reports results after adding firm and date fixed effects to the prior specification. This fixed effect structure is an important feature of our design. First, firm fixed effects control for any time-invariant, cross-sectional

Table IV

Political Connections and the Informativeness of Insider Trades: Between-Group Analysis

This table presents results from estimating equations (1) and (2). In Panel A, future returns are measured over the subsequent month ($s = 1$), and in Panel B, future returns are measured over the subsequent six months ($s = 6$). Columns (1) and (2) estimate the regression specification pooling over all observations, and columns (3) and (4) estimate the regression separately for insiders with and without political connections. Column (5) presents the difference in coefficients between columns (3) and (4). The coefficient on *Buyer* represents the informativeness of net insider trade for future performance and the coefficient on *Buyer*Crisis_Bailout* represents the change in the informativeness of the trade during the bailout period. *Crisis_PreBailout* is an indicator variable equal to 1 for the period from July 2007 to September 2008. *Crisis_Bailout* is an indicator variable equal to 1 for the period from October 2008 to June 2009. All other variables are defined in Table I. 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 level (two-tail), respectively.

Panel A. One-Month-Ahead Future Returns

	Dependent Variable: $BHR_{i,t+1}$				
			Political Connections		
Variables	All Obs (1)	All Obs (2)	Yes (3)	No (4)	Diff. (5)
Buyer	0.51**	0.39*	−0.20	0.55*	[0.15]
	(2.04)	(1.66)	(−0.45)	(1.94)	
*Buyer*Crisis_PreBailout*		−0.05	0.40	0.14	[0.85]
		(−0.07)	(0.34)	(0.19)	
*Buyer*Crisis_Bailout*		2.34**	6.43***	0.63	[<0.01]
		(2.49)	(3.35)	(0.62)	
Controls					
Size	0.32***	0.32***	0.18	0.48**	[0.23]
	(2.83)	(2.80)	(1.16)	(2.53)	
BM	2.57***	2.33***	2.77**	2.13***	[0.62]
	(4.61)	(4.05)	(2.48)	(3.40)	
PastMoRet	−0.11***	−0.12***	−0.12***	−0.12***	[0.98]
	(−3.87)	(−4.11)	(−3.34)	(−2.97)	
PastYrRet	0.04***	0.02*	0.02	0.02	[0.91]
	(3.67)	(1.79)	(1.35)	(1.41)	
Crisis_PreBailout		−2.10***	−1.55**	−2.66***	[0.24]
		(−4.28)	(−2.21)	(−4.20)	
Crisis_Bailout		−3.34***	−5.00***	−2.54***	[0.11]
		(−4.48)	(−3.93)	(−2.95)	
F	13.24	12.33	4.71	10.57	
N (insider-months)	29,777	29,777	10,204	19,573	
N (firms)	497	497	159	338	

(*Continued*)

Table IV—*Continued*

Panel B. Six-Month-Ahead Future Returns

	Dependent Variable: $BHR_{i,t+6}$				
			Political Connections		
Variables	All Obs (1)	All Obs (2)	Yes (3)	No (4)	Diff. (5)
Buyer	1.32	2.92***	4.30***	1.70*	[0.10]
	(1.46)	(3.53)	(3.27)	(1.86)	
*Buyer*Crisis_PreBailout*		−3.53**	−5.94**	−3.59*	[0.52]
		(−1.99)	(−1.98)	(−1.70)	
*Buyer*Crisis_Bailout*		4.83*	18.78***	−1.39	[<0.01]
		(1.87)	(3.60)	(−0.46)	
Controls					
Size	0.97***	1.07***	1.65***	0.44	[0.07]
	(2.75)	(3.25)	(3.57)	(0.90)	
BM	6.91***	3.92***	5.50**	2.72**	[0.36]
	(4.86)	(2.89)	(2.04)	(2.03)	
PastMoRet	−0.08*	−0.17***	−0.24**	−0.13**	[0.31]
	(−1.66)	(−3.38)	(−2.56)	(−2.38)	
PastYrRet	0.17***	0.08***	0.03	0.10***	[0.17]
	(7.30)	(3.18)	(0.84)	(3.44)	
Crisis_PreBailout		−14.35***	−15.85***	−12.66***	[0.21]
		(−11.47)	(−8.29)	(−7.64)	
Crisis_Bailout		−2.25	−9.47***	1.51	[<0.01]
		(−1.17)	(−2.88)	(0.67)	
F	17.38	41.19	24.66	20.89	
N (insider-months)	29,777	29,777	10,204	19,573	
N (firms)	497	497	159	338	

differences between firms with and without political connections (e.g., corporate governance, precrisis exposure to housing market). Inclusion of these fixed effects helps mitigate concerns that omitted firm-level characteristics might be correlated with future performance, insider trading activity, and political connections. Second, date fixed effects control for changes in market conditions that affect all firms in a given period. We include separate date fixed effects for each sample, to allow the time period effects (and time trends) to differ between the two samples. This design controls for the possibility that market conditions differentially affect firms with and without politically connected insiders. Date fixed effects subsume the time period indicator variables *Crisis_PreBailout* and *Crisis_Bailout*.

Columns (1) through (3) of Table V present results after adding firm fixed effects. Columns (4) through (6) present results after adding both firm and date fixed effects. The results in Table V suggest that our inferences are robust to these alternative fixed effect designs. In particularly, we continue to find strong evidence of an increase in the informativeness of insider trades during the bailout period, with this result concentrating among politically connected

Table V

Political Connections and the Informativeness of Insider Trades: Within-Firm Analysis

This table reports results from estimating equation (2) after including firm and date fixed effects. The dependent variable is six-month-ahead future returns ($BHR_{i,t+6}$) and all fixed effects are allowed to vary with political connections. For example, there are two vectors of date fixed effects, one for the sample of insiders with political connections and one for the sample of insiders without political connections. Because *Crisis_PreBailout* and *Crisis_Bailout* are time period indicator variables, date fixed effects subsume the main effects on *Crisis_PreBailout* and *Crisis_Bailout*. All variables are defined in Table I. *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.

	Firm Fixed Effects			Firm + Date Fixed Effects		
	Political Connections			Political Connections		
Variables	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)
Buyer	2.74*	0.71	[0.28]	2.94**	1.48*	[0.39]
	(1.70)	(0.76)		(2.02)	(1.67)	
*Buyer*Crisis_PreBailout*	−0.29	1.77	[0.55]	−3.12	−0.46	[0.39]
	(−0.11)	(0.83)		(−1.32)	(−0.23)	
*Buyer*Crisis_Bailout*	18.29***	0.69	[<0.01]	9.97***	−3.02	[<0.01]
	(3.99)	(0.25)		(2.71)	(−1.08)	
Control Variables	Yes	Yes		Yes	Yes	
Firm Fixed Effects	Yes	Yes		Yes	Yes	
Date Fixed Effects	No	No		Yes	Yes	
F	25.88	22.99		3.85	9.89	
N (insider-months)	10,204	19,573		10,204	19,573	
N (firms)	159	338		159	338	

insiders. Thus, our main results are not driven by temporal changes in market conditions, a differential effect of market conditions on firms with and without politically connected insiders, or time-invariant firm characteristics. To explain our results, an omitted variable would need to explain not only why, within a given firm, buys (sells) are followed by positive (negative) returns, but also why the difference in returns following buys and sells increases during the bailout period and why this increase is observed only among the trades of insiders with political connections.[14]

[14] Tables IA.I and IA.II of the Internet Appendix show that inferences are unchanged if we replace *Buyer* with three alternative measures of insider trading activity (e.g., Lakonishok and Lee (2001), Piotroski and Roulstone (2005)), namely (i) the purchase ratio, defined as the number of shares purchased scaled by number of shares purchased plus the number of shares sold, (ii) the net purchase ratio, defined as the dollar value of net purchases (purchases less sales) scaled by the dollar volume of purchases plus the dollar value of sales, and (iii) the dollar value of net purchases. Table IA.III shows that inferences are unchanged when using the algorithm in Cohen, Malloy, and Pomorski (2012) to classify traders as "nonroutine" versus "routine" and focusing on the former. Table IA.IV shows that inferences are robust to aggregating insider-month observations

While the results above suggest that politically connected insiders had a significant information advantage during the crisis and opportunistically traded to exploit this advantage, they do not speak to the source of this information advantage. We next conduct two sets of tests that strengthen empirical identification and speak to potential sources of insiders' information advantage. First, we use detailed data on officers' and directors' work history disclosed in proxy statement biographies to construct a network map, that while undoubtedly incomplete, enables us to trace the nature of the political connection, the degrees of separation, and the recency of the connection. The network map also allows us to identify several relevant individual attributes. We then correlate attributes of the connection and the individual with the informativeness of their trades during the crisis. Second, we implement an event study to examine whether insiders front-run TARP infusion announcements. Evidence of elevated trading among politically connected insiders in a short window prior to the TARP infusion, and that these trades predict the market reaction to the infusion, would suggest that politically connected insiders are trading based on superior information about TARP infusions.

IV. Network Map and Attributes of the Connection

Our primary tests use data on firm-level political connections collected by Duchin and Sosyura (2012). A key assumption of a firm-level measure of political connections is that privileged information is shared among board members. While this assumption is consistent with anecdotal and empirical evidence (e.g., Sorkin (2009), Cao et al. (2015), Kim (2016)), information sharing among board members is difficult to verify in the absence of direct observation. In this section, we deviate from Duchin and Sosyura (2012) and measure political connections at the individual level. Specifically, we use the BoardEx database to construct a (partial) network map between government regulatory agencies and individual officers and directors. This network map allows us to (i) distinguish individuals with direct connections to government agencies from individuals who are connected indirectly through another insider's connection, and (ii) distinguish individuals with recent connections from individuals with stale connections. Figure 4 illustrates how we map these relationships.

To drill down on individual insiders and individual connections, we conduct this analysis within the sample of firms with politically connected insiders who traded during the bailout period (i.e., those observations for which *Connected* = 1 and *Crisis_Bailout* = 1). Panel A of Table VI reports the number of individuals in the sample with various attributes. There are 740 individuals in this sample, of which 319 are officers (*Officer* = 1) and 138 are financial experts according to the definition provided in the Sarbanes-Oxley Act of 2002 (*Financial Expert* = 1). Notably, 72 of the 740 individuals are directly connected through current

to the firm-month level, as well as to disaggregating insider-month observations to the individual transaction level.

Table VI

Network Analysis: Attributes of the Connected Individual

The table presents results from constructing a partial network map and analyzing the attributes of individual insiders and their connections. See Figure 4 for details. Panel A presents sample requirements for this analysis. Board members with work experience at the Federal Reserve, the U.S. Treasury, Congress, Office of Comptroller of Currency (OCC), Office of Thrift Supervision (OTS), or Federal Deposit Insurance Corporation (FDIC) beginning prior to 2008 are classified as "directly connected" (*Connected* = 1, *Direct Connection* = 1). All other officers and directors at the same firms are classified as "indirectly connected" (*Connected* = 1, *Direct Connection* = 0). Among those with direct connections, we measure the age or "recency" of the connection relative to 2008. For example, if a director stepped down from the Federal Reserve Board in 2005, then *Recency* = 3. If the work experience continues through 2008, the connection is current, and *Recency* = 0. We consider a direct connection "recent" (*Direct Recent Connection* = 1) if *Recency* ≤ 3 and "stale" (*Direct Stale Connection* = 1) if *Recency* > 3. The sample contains 740 politically connected insiders who trade during the bailout period, 72 of whom have direct connections. Panel B reports the number of insiders with direct connections who are officers, and the number of insiders with direct connections who are financial experts. *Officer* is an indicator equal to 1 if the individual is an officer of the firm, and *Financial Expert* is an indicator equal to 1 if the individual satisfies the financial expert requirement of SOX. Panel C reports results from estimating equation (3) on the sample of trades placed during the bailout period by insiders with political connections (*Connected* = 1 and *BailoutPd* = 1), distinguishing trades of indirect and direct connections, trade of recent and stale connections, trades of officers and directors, and trades of financial experts. *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 level (two-tail), respectively.

Panel A. Sample of Politically Connected Insiders Trading during the Bailout

Number of Firms with ≥1 politically connected insider trading during the bailout period	146 firms
After requiring BoardEx data	114 firms
Number of connected insiders trading during the bailout period	740 insiders
Number of officers (within connected sample)	319 of 740
Number of financial experts (within connected sample)	138 of 740
Number of Insiders with Direct Connections (1 degree of separation)	72 of 740
Number of insiders with *recent* direct connections (*Recency* ≤ 3 years)	20 of 72
Number of insiders with *stale* direct connections (*Recency* > 3 years)	52 of 72

Panel B. Separating Connections from Board Position

		Officer			Financial Expert		
		No	Yes	Total	No	Yes	Total
Direct Connection	No	388	280	668	547	121	668
	Yes	34	38	72	55	17	72

(*Continued*)

Table VI—*Continued*

Panel C. Within-Political-Connected Insider Analysis

	Dependent Variable: $BHR_{i,t+6}$			
Variables	Base Model (1)	Direct vs. Indirect Connections (2)	Recent vs. Stale Direct Connections (3)	Controlling for Board Position (4)
Buyer	8.44**	7.66**	7.83**	8.64**
	(2.56)	(2.30)	(2.36)	(2.35)
*Buyer*Direct Connection*		9.56*		
		(1.71)		
*Buyer*Recent Direct Connection*			35.41***	36.65***
			(3.02)	(3.08)
*Buyer*Stale Direct Connection*			4.67	4.56
			(0.87)	(0.83)
*Buyer*Officer*				−1.37
				(−0.28)
*Buyer*Financial Expert*				0.79
				(0.12)
Controls				
Size	−46.15**	−45.85**	−46.03***	−46.44***
	(−2.69)	(−2.67)	(−2.68)	(−2.71)
BM	25.86*	26.02*	26.06**	25.99*
	(1.95)	(1.96)	(1.97)	(1.94)
PastMoRet	−0.01	−0.01	−0.01	−0.01
	(−0.08)	(−0.10)	(−0.08)	(−0.07)
PastYrRet	−0.43***	−0.43***	−0.44***	−0.44***
	(−3.35)	(−3.36)	(−3.45)	(−3.48)
Direct Connection		−5.01		
		(−1.08)		

(*Continued*)

Table VI—*Continued*

Panel C. Within-Political-Connected Insider Analysis

	Dependent Variable: $BHR_{i,t+6}$			
Variables	Base Model (1)	Direct vs. Indirect Connections (2)	Recent vs. Stale Direct Connections (3)	Controlling for Board Position (4)
Recent Direct Connection			−28.24***	−28.40***
			(−2.63)	(−2.64)
Stale Direct Connection			−0.71	−0.45
			(−0.17)	(−0.10)
Officer				−0.26
				(−0.07)
Financial Expert				−3.80
				(−0.66)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes
F	26.92	21.10	20.35	15.28
N(insider-months)	1,055	1,055	1,055	1,055
N(firms)	109	109	109	109
test: *Buyer*Recent Direct Connection – Buyer*Stale Direct Connection = 0*			[<0.01]	[<0.01]

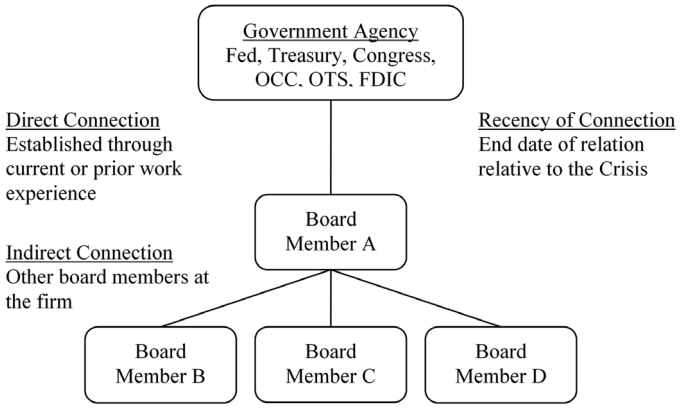


Figure 4. Example of a partial network map. This figure presents an example of a "network map" used to test for information sharing among board members. We use BoardEx to determine whether a particular board member has work experience at the Federal Reserve, U.S. Treasury Department, Congress, Office of Comptroller of Currency (OCC), Office of Thrift Supervision (OTS), or Federal Deposit Insurance Corporation (FDIC). Board members with work experience at any of these agencies beginning prior to 2008 are classified as "directly connected." All other officers and directors at those firms are classified as "indirectly connected." *Recency* is the ending year of the work experience relative to 2008. For example, if a director stepped down from the Federal Reserve Board in 2005, then *Recency* = 3. If the work experience continues through 2008, the connection is considered current and *Recency* = 0. We consider a connection "recent" if it is less than or equal to three years old (where three is the median).

or prior work experience at the Federal Reserve, the U.S. Treasury, Congress, or a bank regulator (i.e., OCC, OTS, or FDIC) at the time of the crisis (*Direct Connection* = 1), with the remaining 668 connected through such individuals, that is, they sit on the same boards (*Direct Connection* = 0).

Focusing on the 72 directly connected individuals, we identify the end date of the work experience and use the end date of the work experience relative to 2008 as a rough proxy for the recency of the connection. For example, if a director stepped down from the Federal Reserve Board in 2005, *Recency* = 3. If the work experience continues through 2008, the connection is considered current and *Recency* is coded as zero.[15] We consider a connection "recent" (*Recent Direct Connection* = 1) if *Recency* $\leq$ 3 and "stale" (*Stale Direct Connection* = 1) if *Recency* $>$ 3. Panel A shows that 20 of the 72 directly connected individuals have recent connections.

Next, we examine the overlap between connections and other attributes. Panel B shows that of the 72 directly connected individuals, 38 are officers and 17 satisfy the financial expertise requirements of the Sarbanes-Oxley Act. We

[15] There is no rule against board members of financial institutions simultaneously serving on a Federal Reserve board. For such individuals, *Recency* is coded as zero. In cases in which an individual has multiple direct connections (e.g., to both the U.S. Treasury and the Federal Reserve), we use the most recent connection in calculating *Recency*.

caution, however, that as we continue to drill down on specific attributes of the connection and individual, sample sizes become considerably smaller.

To examine how these attributes relate to the informativeness of insider trades, we estimate

$$\begin{aligned} BHR_{t+6} = amp;\, & \delta_1 Buyer_t + \delta_2 Buyer_t * Recent\ Direct\ Connection \\ & + \delta_3 Buyer_t * Stale\ Direct\ Connection + \delta_4 Buyer_t * Officer \\ & + \delta_5 Buyer_t * Financial\ Expert + \vartheta\ Controls_t \\ & + Firm\ Effects + Date\ Effects_t + \varepsilon_{t+1}, \end{aligned} \tag{3}$$

where *Recent Direct Connection*, *Recent Stale Connection, Officer*, and *Financial Expert* are insider attributes defined above, *Controls* is a vector of control variables that includes all of the main effects of the interaction terms in addition to the control variables from equation (2), *Firm Effects* is a vector of firm fixed effects, and $Date\ Effects_t$ is a vector of date fixed effects.

This analysis is effectively the regression specification in Table V, augmented with variables that capture connection and insider attributes, and estimated on the subsample of trades by politically connected insiders during the bailout period (i.e., within the sample of observations where for which $Connected = 1$ and $Crisis\_Bailout = 1$). Because all individuals in this sample have either a direct or an indirect connection, this specification effectively compares the informativeness of trades of insiders with *indirect* connections (measured by δ_1) to those of insiders with *recent direct* connections (measured by $\delta_1 + \delta_2$) and those of insiders with *stale direct* connections (measured by $\delta_1 + \delta_3$). The parameter δ_2 represents the difference in the informativeness of trades between insiders with recent direct connections ($N = 20$) and insiders with indirect connections ($N = 668$), while δ_3 represents the difference in the informativeness of trades between insiders with stale direct connections ($N = 52$) and insiders with indirect connections ($N = 668$).

Panel C of Table VI presents the results from estimating equation (3). Focusing on the results in column (5), we find that recent direct connections have the most informed trades ($\delta_1 + \delta_2 = 45.29$), followed by direct stale connections ($\delta_1 + \delta_3 = 13.20$), and then indirect connections ($\delta_1 = 8.64$). The difference between trades of recent direct connections and those of indirect connections is statistically significant ($\delta_2 = 36.64$, t-statistic 3.08). The difference between trades of recent direct connections and those of stale direct connections is also statistically significant ($\delta_2 - \delta_3 = 32.09$, two-tailed p-value < 0.01). However, the difference between trades of stale direct connections and indirect connections is insignificant ($\delta_3 = 4.56$, t-statistic 0.83).

In addition, results in Panel C show that connection attributes appear to be more salient in determining the informativeness of trades than other insider attributes such as officer/director or whether the insider is a financial expert (t-statistics of -0.28 and 0.12 on *Buyer*Officer* and *Buyer*Financial Expert*, respectively). These findings suggest that the information advantage and associated opportunistic trading are most pronounced among insiders with

recent, direct connections, and that this advantage is partially but incompletely transferred to other board members at the firm (i.e., those with "indirect" connections). Recall that our tests are joint tests of an information advantage and insiders trading on this advantage. Consequently, an alternative interpretation is that insiders with indirect connections and insiders with direct connections have a similar information advantage, but the latter group trades more aggressively.[16]

V. Political Connections and Potential Front-Running of TARP Infusions

A. Event Study

We next conduct an event-study analysis to further sharpen identification and investigate whether political connections facilitate front-running of TARP capital infusions. Specifically, we examine both the timing and the 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.

For this analysis, we focus on those firms in our sample that received TARP capital infusions. We collect data on TARP infusions 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") on 31 calendar dates.[17] Our search of firm disclosures on Factiva and Lexis-Nexus suggests that 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

[16] Consistent with incomplete information transfer between insiders with recent direct connections and other insiders on the same board, Table IA.V of the Internet Appendix reports that the trades of insiders with recent direct connections (*Recent Direct Connection* = 1, *Connected* = 1) are more informative than the trades of connected insiders without direct connections (*Recent Direct Connection* = 0, *Connected* = 1), which, in turn, are more informative than the trades of unconnected insiders (*Recent Direct Connection* = 0, *Connected* = 0).

[17] Our sample of TARP capital infusions excludes Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, Merrill Lynch, Morgan Stanley, State Street, and Wells Fargo, as these banks were "forced" to take the infusion (inclusion of these banks does not affect our results). Our sample is similar in size to prior research. For example, after imposing data requirements, Bayazitova and Shivdasani (2012) 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.

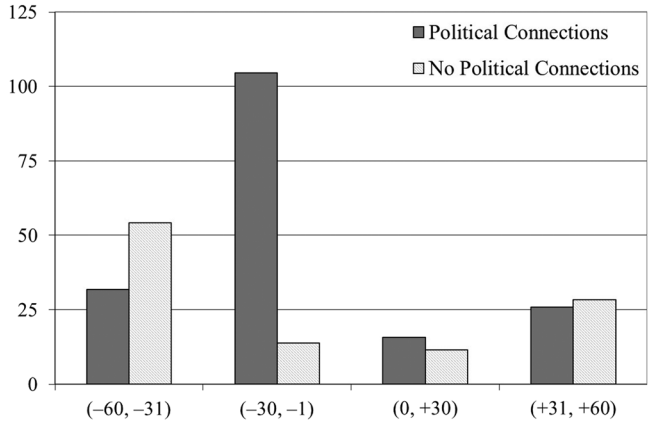


Figure 5. Unsigned volume of insider trades around infusion announcements. This figure shows the dollar value of insider trading volume over the 60 trading days before and after infusion announcement. Dollar volume (in millions) appears on the *y*-axis, and the respective 30-day window appears on the *x*-axis. Gray (white) bars represent dollar volume by insiders with (without) political connections. The sample comprises 256 infusions.

0, and we use a three-day window [−1, +1] to capture the market reaction. We find that this window contains 95% of announcements.[18]

Figure 5 presents aggregate dollar volume of trading by corporate insiders at TARP recipients over the 60 days prior to the announcement and over the 60 days after the announcement. Figure 5 shows a clear increase in volume 30 days prior to the announcement, with this increase concentrated among insiders with political connections. For politically connected insiders, volume increases from $32 million over the [−60, −31] window to $105 million during the [−30, −1] window, and then falls to $16 million during the [0, +30] window before returning closer to pre-TARP levels, $26 million, during the [+31, +60] window. The surge in trading by politically connected insiders immediately prior to the infusion is consistent with politically connected insiders timing there trades to coincide with TARP infusions.

Panel A of Table VII presents descriptive statistics for the variables used in our event-study tests. Focusing on the 30 trading days prior to the announcement, that is, the [−30, −1] window, we find that insiders traded during this window in 55% of infusions (89 + 52/256), a total of 383 unique insiders traded during this period, and the total dollar volume of their trades was $118 million. The panel also shows that the average infusion amount 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).

[18] Section 114(a) of the Emergency Economic Stabilization Act of 2008 stipulates that the U.S. Treasury must publicly disclose recipients within 48 hours of the infusion.

Table VII
Front-Running TARP Infusions

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 after partitioning by the direction of insider trade 30 trading days prior to the infusion announcement. Panel B presents descriptive statistics after partitioning based on the direction of insider trade and political connections (2×2). Panel C presents results from a cross-sectional regression of the three-day announcement-period return on the direction of insider trading over the 30 trading days prior to the infusion and control variables. *Total # Insiders Trading* is the total number of unique insiders trading over the 30-day window prior to the infusion. *Total Insider Volume* is the total dollar value of insider trades over the 30-day window prior to the infusion. *AnncRet* is the three-day buy-and-hold return centered on the infusion and 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 equal to 1 if the net insider trade 30 days prior to the infusion is a purchase. All other variables are as previously defined. 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 level (two-tail), respectively.

Panel A. Infusion Characteristics Partitioned by Insider Trade

Variable	All Infusions [−30, −1] $N = 256$	Infusions Where Insiders Are *Net Buyers* [−30, −1] $N = 89$		Infusions Where Insiders Are *Net Sellers* [−30, −1] $N = 52$
Total # Insiders Trading	383.00	269.00		114.00
Total Insider Volume ($ millions)	118.43	63.08		55.35
Avg. AnncRet	−0.40	0.35		−1.28
p-Value for test of differences			[0.38]	
Avg. Infusion%MV	42.48	43.11		26.86
p-Value for test of differences			[0.02]	
Avg. Infusion%TA	2.28	2.13		2.52
p-Value for test of differences			[0.06]	

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

	Political Connections: Yes		Political Connections: No	
Variable	*Net Buyers* [−30, −1] $N = 32$	*Net Sellers* [−30, −1] $N = 27$	*Net Buyers* [−30, −1] $N = 57$	*Net Sellers* [−30, −1] $N = 25$
Total # Insiders Trading	87.00	63.00	182.00	51.00
Total Insider Volume ($ millions)	55.31	49.24	7.77	6.11
Avg. AnncRet	4.39	−5.13	−1.92	2.87
p-Value for test of differences	[<0.01]		[0.05]	
Avg. Infusion%MV	30.71	21.80	50.08	32.32
p-Value for test of differences	[0.03]		[0.16]	
Avg. Infusion%TA	2.10	2.26	2.15	2.80
p-Value for test of differences	[0.22]		[0.06]	

(*Continued*)

Table VII—*Continued*

Panel C. Cross-Sectional Regressions

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

Panel A also presents descriptive statistics separately for the subsamples of infusions in which corporate insiders are net buyers (shares bought exceeds shares sold) versus net sellers (shares sold exceed shares bought) over the [−30,−1] window. We find that insiders are net buyers before 34.8% of the infusions in our sample and net sellers before 20.3% of the infusions in our sample. Infusions in which insiders are net buyers are a much larger percentage of prior-quarter market value.

Panel B of Table VII presents descriptive statistics for each of the four groups of infusions. We find that for infusions in which politically connected insiders were net buyers (sellers) over the prior 30 trading days, the average three-day announcement-period return is 4.39% (−5.13%). These results suggest that when politically connected insiders were net buyers (net sellers) prior to the announcement, the subsequent announcement was a large positive (negative) surprise to the market. Similar results do not obtain for insiders without political connections. These results suggest that politically connected insiders appear to front-run TARP infusions.

In terms of magnitudes of gains and losses, Panel B reports $55.31 million in purchases by 87 politically connected insiders over the 30 days before TARP infusions. These results suggest that the average purchase per insider is ∼$635,000 (the average sale per insider is ∼$780,000). Even if we assume that political connections allow an insider to reap a generous additional 10% abnormal profit (or avoid 10% in losses) from front-running the infusion, the dollar value comes out to less than $100,000. Gains (or loss-avoidance) of this

magnitude are quite common among insider trading cases pursed by the SEC and Department of Justice.[19]

As an alternative to partitioning by trading behavior and then examining the difference in announcement returns, in Figure 6, we partition by the sign of the announcement return and then examine the differences in trading behavior. Panel A plots the aggregate dollar value of net purchases over the [−60, +60] window around those announcements with positive announcement-period returns. We find a significant spike in buying among politically connected insiders that anticipates positive announcement returns. Panel B plots the aggregate dollar value of net purchases over the [−60, +60] window around those announcements with negative announcement period returns. We find that politically connected insiders sell in much higher volume over both the 30- and 60-day windows in anticipation of negative announcement-period returns. In both cases, similar patterns do not obtain among insiders without political connections.[20]

To test the hypothesis that trades shortly before the infusion predicted the market reaction, we regress the three-day announcement-period returns on the control variables from equation (1) and an indicator for whether insiders at the firm were net buyers over the prior 30 trading days (i.e., *Buyer*(–30,–1)) and estimate this regression separately for trades of connected and unconnected insiders. Panel C of Table VII reports the results. The coefficient on *Buyer*(–30,–1) is positive and statistically significant *only* for the subsample of firms in which insiders are politically connected (sample of 94 infusions, t-statistic of 2.24). We find no evidence of a relation between insider trades and announcement-period returns among the subsample of firms in which insiders are not politically connected (sample of 162 infusions, t-statistic of −0.95).[21]

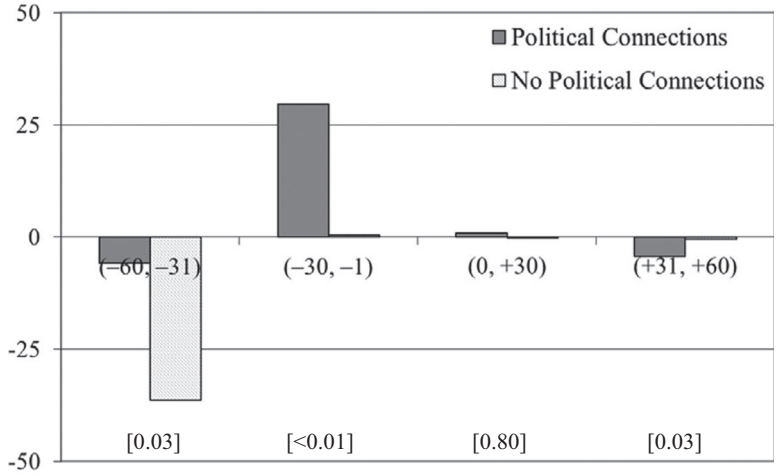
The results from our event-study analysis are consistent with politically connected insiders trading in anticipation of the infusion. In particular, the results suggest that insiders timed their trades in relation to the infusion announcement and that the information content of the trades was related to TARP infusions. This is difficult to reconcile with alternative explanations, and the strategic timing of these trades (relative to the infusion) cannot be easily explained by omitted variables. At the very least, the evidence strongly

[19] For example, several pending cases at the time of this paper include allegations of gains (or loss-avoidance) of less than $100,000. See https://www.justice.gov/criminal-fraud/insider-trading-fraud for examples.

[20] The sample sizes for this analysis (i.e., the number of trades in the respective window) are presented in Figure IA.1 of the Internet Appendix.

[21] Panel A of Table IA.VI in the Internet Appendix repeats the analysis in Panel C of TableVII focusing on trading activity over the [−60,–30] window. We find no evidence that trades over this alternative window predict announcement returns. Panel B of Table IA.VI repeats the analysis in Panel C of TableVII after decomposing the indicator variable for whether insiders at the firm were net buyers over the [−30, −1] window into separate indicators for (i) whether insiders were net buyers over the [−5, −1] window, *Buyer*(–5,–1), and (ii) whether insiders were net buyers over the [−30,–6] window, *Buyer*(–30,–6). Only the coefficient on the latter variable is significant, which suggests that trades that anticipate the infusion are placed more than five days prior to the infusion.

Panel A. Net Purchases Before Infusions with Positive Announcement Returns



Panel B. Net Purchases Before Infusions with Negative Announcement Returns

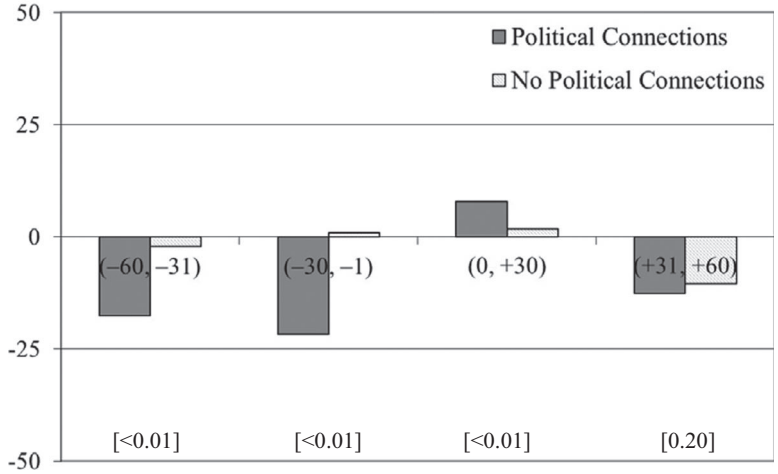


Figure 6. Net purchases around infusion announcements. This figure shows the dollar value of net purchases (dollar value of buys less that of sales) over the 60 trading days before and after the infusion announcement. Positive (negative) values indicate a net buy (net sell). Panel A presents the dollar value of net purchases for the 122 infusions with positive announcement returns. Panel B presents the dollar value of net purchases for the 134 infusions with negative announcement returns. The dollar value of insider trades (in millions) appears on the *y*-axis, and the respective 30-day window appears on the *x*-axis. Gray (white) bars represent the dollar values of net purchases by insiders with (without) political connections. *p*-Values appear in brackets and test for the difference in average trade size between insiders with and without political connections.

suggests that politically connected insiders opportunistically timed their trades to front-run the infusion.[22] We next conduct a battery of sensitivity analyses to assess the robustness of our results.

B. Event Study: Falsification Tests

The staggered nature of TARP infusions provides several potential avenues to control for confounding effects. 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 do not expect to observe similar results among TARP recipients on nonannouncement dates (i.e., holding the firms fixed but altering the dates) or among nonrecipients on announcement dates (i.e., holding the dates fixed but altering the sample firms).

Panel A of Table VIII presents results from holding the set of TARP recipients fixed and estimating the regression specifications in Panel C of Table VII for the same firms on all nonannouncement dates from October 2008 to June 2009 (i.e., the bailout period). We 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 nonannouncement period. Comparing the results between announcement and nonannouncement 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 VIII reports a statistically significant relation between the trades of politically connected insiders and returns on nonannouncement dates (t-statistic 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, evidence of a positive relation between insider trades and returns during nonannouncement 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 nonannouncement dates (difference-in-differences, p-value $= 0.03$).

Panel B of Table VIII presents results from holding announcement dates fixed and estimating the regression specifications in Panel C of Table VII for firms that did not receive TARP infusions. Here, we 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 nonrecipients

[22] While firms often use "trading blackout windows" to limit insider trading in the months before material corporate events like earnings announcements, these windows are created and enforced by the firm, not by the SEC (e.g., Bettis, Coles, and Lemmon (2000), Jagolinzer, Larcker, and Taylor (2011)). Given the number of infusions with insider trades 30 days before the infusion and the volume of trade, it is clear that blackout windows either did not apply to TARP infusions or were not enforced at the majority of firms in our sample.

Table VIII

Front-Running TARP Infusions—Falsification Tests

This table presents results from using two falsification tests to estimate the cross-sectional variation in announcement 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 specification in Table VII, Panel C for the same firms on all nonannouncement 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 nonannouncement days. The sample comprises 249 unique firms across 188 nonevent 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 specification in Table VII, Panel C 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 nonrecipients on the announcement day. The sample comprises 242 unique nonrecipients across 31 announcement days. *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 level (two-tail), respectively.

Panel A. Falsification Test #1: TARP Recipients on Nonannouncement days

	TARP Recipients on the Announcement Day (Table VII, Panel C)			FALSIFICATION: TARP Recipients on Nonannouncement Dates			
	Political Connections			Political Connections			
Variables	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)	Diff-in-Diff (7)
Buyer(−30,−1)	5.41**	−1.67	7.08**	1.32***	0.67***	0.65**	6.43**
	(2.23)	(−0.95)	[0.02]	(5.83)	(3.79)	[0.03]	[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: Nonrecipients on Announcement Days

	TARP Recipients on the Announcement Day (Table VII, Panel C)			FALSIFICATION: Nonrecipients on the Announcement Dates			
	Political Connections			Political Connections			
Variables	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)	Diff-in-Diff (7)
Buyer(−30,−1)	5.41**	−1.67	7.08**	0.14	0.65**	−0.51	7.59***
	(2.23)	(−0.95)	[0.02]	(0.28)	(2.14)	[0.38]	[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		

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 nonrecipients during the announcement (difference-in-differences p-value = 0.01).

Next, we compare front-running of TARP infusions with that of other events not directly related to TARP infusions. Panel A of Table IX compares the predictive ability of insider trades 30 days before TARP infusions to that of insider trades 30 days before earnings announcements. To conduct this test, we begin with the sample of 249 firms in our sample that received TARP infusions. 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 VII, measuring insider trading over the 30 days prior to the earnings announcement. Panel A of Table IX shows that the predictive ability of politically connected insiders' trades for TARP infusion announcements vastly exceeds that for earnings announcements.

Next, we compare front-running of TARP infusions with that on extreme return days. To conduct this test, we begin with the sample of 249 firms in our sample that received TARP infusions. For each firm, we then select the day during the crisis with the most extreme price change (i.e., the largest unsigned return). We then repeat our tests in Panel C of Table VII, measuring insider trading over the 30 days prior to the extreme return. Panel B of Table IX shows that the predictive ability of politically connected insiders' trades for TARP infusion announcements vastly exceeds that for extreme return days.

Taken together, the results in Tables VIII and IX suggest that the relations we document above do not hold in the absence of a TARP infusion announcement and appear to be a unique feature of such events.

C. Event Study: FOIA Analysis

One limitation of using the market reaction to infusion announcements as a measure of "surprise" is that it comingles the wedge between anticipated and received funds and the valuation consequences of those funds. To disentangle the two, and to help pin down a potential source of information, we obtain data on TARP applications through an FOIA request to the U.S. Treasury. Treasury provided us with a partially redacted list of bank names, application dates, and application amounts.

Using these FOIA data, we measure the gap between the infusion amount and the application amount, and drill down on those infusions in which insiders traded in the 30-day window prior to the infusion. The sample comprises 105 infusions with the requisite data. Panel A of Table X presents descriptive statistics for the application and infusions amounts, *Application* and *Infusion*, respectively. The average application is $374 million and the average infusion

Table IX

Front-Running TARP Infusions—Comparison to Other Information Events

This table presents results from repeating our event study tests on other news events during the crisis. Panel A presents results from estimating the specification of Table VII, Panel C, after replacing the infusion date with the earnings announcement date. The sample comprises 1,619 earnings announcements between July 2007 and June 2009 for 249 TARP recipients. Panel B presents results from estimating the specification of Table VII, Panel C, after replacing the infusion date with the date during the crisis that has the largest absolute return. The sample comprises 249 extreme returns between July 2007 and June 2009. *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 level (two-tail), respectively.

Panel A. Front-Running Earnings Announcement

	TARP Recipients' Infusions Announcements (Table VII, Panel C)			TARP Recipients' Earnings Announcements			
	Political Connections			Political Connections			Diff-in-Diff
Variables	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)	(7)
Buyer(−30,−1)	5.41**	−1.67	7.08**	0.65	−0.09	0.74	6.34**
	(2.23)	(−0.95)	[0.02]	(0.62)	(−0.14)	[0.60]	[0.04]
Controls	Yes	Yes		Yes	Yes		
F	2.42	1.05		5.74	4.18		
N (firm-days)	94	162		604	1,015		
N (firms)	92	157		92	157		

Panel B. Front-Running Extreme Return Days

	TARP Recipients' Infusions Announcements (Table VII, Panel C)			TARP Recipients' Most Extreme News Day			
	Political Connections			Political Connections			Diff-in- Diff
Variables	Yes (1)	No (2)	Diff (3)	Yes (4)	No (5)	Diff (6)	(7)
Buyer(−30,−1)	5.41**	−1.67	7.08**	−13.94**	−4.77	−9.17	16.25*
	(2.23)	(−0.95)	[0.02]	(−2.34)	(−0.79)	[0.28]	[1.80]
Controls	Yes	Yes		Yes	Yes		
F	2.42	1.05		8.56	3.73		
N (firm-days)	94	162		92	157		
N (firms)	92	157		92	157		

is $372 million. For each bank, we measure the ratio of the infusion amount to the application amount (*Ratio* = *Infusion/Application*). For nearly all banks, this ratio is one. Panel A shows that percentiles of *Application* and *Infusion* are identical, and that the 1st percentile of *Ratio* is 71% and the 99th percentile

Table X

Front-Running TARP Infusions—Evidence from FOIA Data

This table presents descriptive statistics on the amount of funds requested by the firm and its relation to insider trading activity 30 days prior to the infusion. Data on requested funds were obtained through a Freedom of Information Act (FOIA) request to the U.S. Treasury Department. Panel A presents distributional statistics on the amount of funds requested (*Application*), the amount of funds received (*Infusion*), and ratio of funds received to requested (*Ratio* = *Infusion*/*Application*). *Application* and *Infusion* are expressed in millions. Panel B presents descriptive statistics for each of three terciles of *Ratio*: infusions where *Ratio* < 1, *Ratio* = 1, and *Ratio* > 1. Panel C presents results from estimating the direction of insider trade 30 days prior to the infusion (*Buyer*(*–30,–1*)) as a function of the subsequent market reaction to the infusion (*AnncRet*), and the tercile rank of the wedge between infusion and application amounts (*Ratio*). The sample comprises 105 infusions in which insiders trade more than $10,000 over the 30 days before the infusion. *t*-Statistics based on standard errors clustered by firm appear in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 level (two-tail), respectively.

Panel A. Descriptive Statistics on Application and Infusion Amounts

			Percentile						
Variable	Mean	Std	1st	5th	25th	50th	75th	95th	99th
Application	374.19	1,121.00	7.00	9.00	23.18	48.20	135.00	2,250.00	6,599.00
Infusion	372.46	1,114.00	7.00	9.00	23.18	48.20	135.00	2,250.00	6,599.00
Ratio	0.99	0.05	0.71	0.97	1.00	1.00	1.00	1.00	1.02

Panel B. Insider Trading and Political Connections

Variable	*Ratio* < 1 *N* = 11 Infusions	*Ratio* = 1 *N* = 91 Infusions	*Ratio* > 1 *N* = 3 Infusions
Average *Ratio*	0.91	1.00	1.03
Average *Connected*	0.73	0.40	0.33
Average *Buyer(−30,−1)*	0.45	0.60	1.00

Panel C. Determinants of Front-Running

	Dependent Variable: *Buyer*(*–30,–1*)		
Variables	All observations (1)	Political Connections: Yes (2)	Political Connections: No (3)
AnncRet	0.01*	0.01***	0.002
	(1.84)	(2.61)	(0.40)
Ratio	0.16**	0.26***	0.17
	(2.22)	(2.96)	(1.36)
Controls			
Buyer(−60,–31)	0.20**	0.13	0.21*
	(2.44)	(1.02)	(1.91)
Size	−0.11***	−0.12***	−0.12**
	(−4.58)	(−4.34)	(−2.14)

(*Continued*)

Table X—*Continued*

Panel C. Determinants of Front-Running

	Dependent Variable: *Buyer*(–30,–1)		
Variables	All observations (1)	Political Connections: Yes (2)	Political Connections: No (3)
BM	0.04	−0.38***	0.13
	(0.58)	(−2.81)	(1.36)
PastMoRet	−0.01***	−0.003	−0.01
	(−2.66)	(−0.96)	(−1.38)
PastYrRet	−0.01**	−0.01***	−0.001
	(−2.11)	(−4.33)	(−0.33)
F	29.04	30.77	8.13
N (firm-days)	105	45	60
N (firms)	105	45	60

is 102%. With only rare exceptions, banks appear to receive all of the money requested.

Panel B of Table X sorts infusions into three groups: infusions with *Ratio* < 1 ($N = 11$ infusions), infusions with *Ratio* = 1 ($N = 91$ infusions), and infusions with *Ratio* > 1 ($N = 3$ infusions). Counterintuitively, the results suggest that firms with politically connected insiders did not receive larger infusions when measured relative to their application (*Connected* decreases from 0.73 to 0.40 to 0.33 across the three groups).[23] The results also suggest that insider buying prior to the infusion anticipates the gap between the infusion amount and the application amount. The probability that insiders buy 30 days before the infusion, *Buyer*(–30,–1) increases monotonically from 0.45 to 0.60 to 1.00 across the three groups. We caution, however, that the data are based on very small samples and that regression results are identified by only a handful of observations (e.g., 14 infusions with insider trades and *Ratio* ≠ 1).

Panel C presents results from estimating the determinants of front-running activity, *Buyer*(–30,–1), as a function of subsequent announcement-period returns, *AnncRet*, and the wedge between the infusion and application amounts, *Ratio*. This test is motivated by extant noisy rational expectations models that suggest that informed traders' demand orders in period t are a function of private information that will be realized in period $t+1$ (e.g., Kyle (1985)). Column (1) of Panel C shows that front-running activity of politically connected insiders not only anticipates the subsequent three-day market reaction to the infusion (t-statistic on *AnncRet* of 1.84), but also anticipates the wedge between the infusion and application amounts (t-statistic on *Ratio* of 2.22). Columns (2)

[23] Duchin and Sosyura (2012) examine whether the likelihood of participation in TARP varies with political connections, but not—conditional on participation—whether the amount of infusion varies with connections.

and (3) show that this phenomenon is unique to the trades of politically connected insiders—we find no evidence that the trades among insiders without connections anticipate either the reaction to the infusion or the amount of the infusion.[24] Taken together, the findings suggest that political connections provided insiders with the ability to front-run TARP infusions.

VI. Conclusion

In this paper, we examine whether political connections facilitate opportunistic behavior by corporate insiders. We focus our analysis on one particular form of opportunism—informed trading. We find no evidence of a relation between political connections and informed trading over the 24 months leading up to the crisis or during the crisis but 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 over which TARP funds were disbursed), we find strong evidence of a relation between political connections and informed trading by officers and directors at leading financial institutions. During the period over which TARP funds were disbursed, the difference in one-month-ahead future returns between the purchases and sales of insiders with (without) political connections is both economically and statistically significant at 8.89% (2.81%). These findings are robust to a battery of sensitivity analyses such as using various fixed effect structures to control for firm-specific characteristics, changes in market conditions, and a differential effect of market conditions on firms with and without politically connected insiders.

We conduct two additional sets of analyses to strengthen empirical identification and speak to potential sources of politically connected insiders' information advantage. First, we use detailed data on officers' and directors' work history disclosed in proxy statement biographies to construct a partial network map. While undoubtedly incomplete, this map enables us to trace the nature of the political connection, the degrees of separation, and the recency of the connection. We then correlate attributes of insiders' connections with their trading behavior. Within the subset of insiders with political connections, we find that the information advantage is most pronounced among insiders with recent, direct connections. We further find that individuals who sit on the same board as these directly connected insiders experience a modest advantage over individuals on unconnected boards.

Second, we examine insider trades 30 days prior to TARP infusions. Notably, we find evidence of abnormal trading by politically connected insiders 30 days in advance of TARP infusions, with these trades anticipating both the market reaction to the infusion and the wedge between the amount of the infusion and the application amount. We find no evidence of abnormal trading or predictive

[24] Table IA.VII of the Internet Appendix reports results from estimating the regressions in Panel C of TableVII after including *Ratio* as an additional explanatory variable. The inferences are unaffected. After controlling for trades that front-run the infusion, we find no evidence of an incremental relation between *Ratio* and announcement returns.

ability among unconnected insiders—insiders without political connections do not appear to time their trades to coincide with TARP infusions. These findings suggest that insiders' political connections provide them with the ability to front-run TARP infusions.

Taken together, our results suggest that officers' and directors' political connections can facilitate opportunism—politically connected insiders had a significant information advantage during the crisis and traded to exploit this advantage.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.