The Information Content of Insider Trades around Government Intervention during the Financial Crisis

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Abstract: This paper examines whether insiders at leading financial institutions anticipated the effect of government intervention during the Financial Crisis on their firms' share prices. While we find no evidence that insiders anticipated the Crisis, we find considerable evidence that insiders anticipated the recovery and bank bailouts. Specifically we find: (i) that the predictive ability of insider trades for future firm performance is higher during the nine months following the announcement of the Troubled Asset Relief Program (TARP) than at any other time from 2002 to 2010, (ii) that the increase in predictive ability associated with the announcement of TARP is concentrated in firms that previously performed poorly, and (iii) that insider trades at banks five days before the announcement of TARP capital infusions predict the market reaction to the infusion. Overall, our results suggest that once the government announced it would intervene during the Crisis, insiders of financial institutions anticipated the effect of this intervention on share prices.

Keywords: Insider Trading; Troubled Asset Relief Program; Capital Purchase Program; Bank Bailout; Financial Crisis

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

1. Introduction

One intriguing question about the 2007-2009 Financial Crisis is whether managers of financial institutions anticipated the Crisis and/or the eventual recovery. Given their access to information regarding mortgage activities and the valuation of asset-backed securities and associated derivatives, managers in the financial sector were in a position to be privately informed about the weakening prospects of their firms prior to the outset of the Crisis. Similarly, during the Crisis, managers in the financial sector were in a position to be privately informed not only about the creation of bailout programs (e.g., the Troubled Asset Relief Program or TARP), but also about the bailout money their firm would receive and the importance of this money for continuing operations. For example, it is well known that the U.S. government's deliberations on whether to bailout the banking system largely took place in private meetings between government officials and managers of leading financial institutions (e.g., Sorkin, 2009), and that the application process for bailout funds was shrouded in secrecy for fear that public knowledge of a rejection of an application would trigger a bank run (e.g., Bayazitova and Shivdasani, 2012).

Considerable prior evidence suggests that managers trade on private information and that insider trades predict long-run future performance (e.g., Jeng, Metrick, and Zeckhauser, 2003; Piotroski and Roulstone, 2005; Cohen, Malloy, and Pomorski, 2012).[1] Yet, in the context of the Financial Crisis, prior research finds (at best) mixed evidence that insiders anticipated the Crisis. For example, Bebchuk, Cohen, and Spamann (2010) report that top executives at Bear Stearns and Lehman Brothers "cashed out" $1 billion in performance-based compensation between 2000 and 2008, and Bhagat and Bolton (2013) report that over the same period the dollar value of insider sales at the fourteen largest banks was 100 times the dollar value of insider purchases (see

[1] There is a large empirical literature documenting the predictive ability of insider trades for future performance, and factors affecting the predictive ability of these trades. See Seyhun (1998) for a review of the early literature.

also, Cziraki 2011). In contrast, however, Fahlenbrach and Stulz (2011) report that the CEOs at eighty banks did not significantly reduce their equity holdings between 2007 and 2008. While these studies provide mixed evidence regarding the volume of trades prior to the Crisis, they do not examine the relation between such trades and subsequent firm performance, insider trades during the Crisis, or insider trades in relation to government bailouts. Another related stream of research focuses on the valuation implications of government bailouts and TARP infusions (e.g., Veronesi and Zingales, 2010; Bayazitova and Shivdasani, 2012), but does not focus on the extent of information leakage and insider trading surrounding these bailouts. Despite the large corporate finance literature on insider trading, and the growing literature on the Financial Crisis and bank bailouts, little is presently known about whether insiders' trades anticipated government intervention, the subsequent recovery, and whether they traded on information about TARP capital infusions.

We conduct two sets of analyses in this paper. First, we use standard portfolio sorts and cross-sectional regressions to examine the relation between insider trades and future firm performance before, during, and shortly after the Financial Crisis. If managers anticipated the effect of the Crisis and subsequent recovery on shares prices, we expect (i) cross-sectional variation in their trading activity predicts cross-sectional variation in future performance, and more importantly, (ii) temporal variation in their information advantage (and hence the predictive ability of their trades) related to the Crisis and bank bailout. Second, we use standard event-study methodologies to examine whether managers anticipated the announcement of government bailout programs and TARP capital infusions. If managers traded in anticipation of these events, we expect cross-sectional variation in their trades predicts the market reaction to these events.

Our analysis is based on a sample of all open market purchases and sales of top executives (Section 16 officers) at publicly-traded financial institutions between 2002 and 2010. We find no evidence that insider trades predict future performance over the twelve months leading up to the Crisis, or during the Crisis *prior* to the creation of TARP. This suggests that managers were *unable* to predict the effect of the forthcoming Crisis on their firm.[2] However, we find strong evidence that insiders anticipated the economic impact of government intervention. In the nine months *after* the creation of TARP (October 2008 to June 2009), the predictive ability of insider trades for future performance is greater than at any other point during the 2002-2010 time period. 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. A hedge portfolio that mimics insider trades earns higher monthly returns over the nine months after the announcement of TARP than at any other period between 2002 and 2010. The average hedge portfolio return from October 2008 to June 2009 is 3.18% *per month*, and these hedge returns accrue even while the overall market is declining.

We consider three potential sources of private information that could give rise to the observed increase in the predictive ability of these trades: (i) conditional on government intervention, insiders had a better appreciation of the extent to which this intervention would affect the financial system and firm valuation, (ii) insiders were privately informed about various government bailout programs in advance of the public, and (iii) insiders had private information about the capital infusion their firm would receive in connection with the government bailout.

[2] These results are largely consistent with statements made by senior executives of major financial institutions during various Congressional inquiries. Citigroup CEO Charles Prince, for example stated that "(n)obody could have predicted that the bank's highest-rated collateralized debt obligations…would lose so much money" (http://www.bloomberg.com/news/2010-04-08/prince-rubin-say-they-didn-t-recognize-citigroup-s-cdo-risk-before-crisis.html). Lehman Brothers CEO Richard Fuld similarly testified that "(n)o one realized the extent and magnitude of these problems, nor how the deterioration of mortgage-backed assets would infect other types of assets and threaten our entire system" (http://money.cnn.com/2008/10/06/news/companies/lehman_hearing/).

With regard to (i), we find that the increase in predictive ability of insider trades after the announcement of TARP is concentrated in financial institutions that stood to benefit the most from an increase in the overall health of the financial system: those institutions with poor past performance. In contrast, prior to TARP, we find no evidence that the predictive ability of insider trades is concentrated in firms with poor past performance. The observation that the increase in predictive ability of insider trades in connection with government intervention is concentrated among poorly performing firms suggests that the market did not fully appreciate the valuation implications of government intervention for these firms, and that insiders traded to exploit this misvaluation.

With regard to (ii), we examine the association between insider trading and abnormal returns for seven key events relating to the creation of TARP studied in prior research; ranging from the first public proposal of TARP to the formal announcement of the TARP-Capital Purchase Program (CPP). We find little evidence that insider trading five days before these events predict the market reaction to these events. We interpret these results as suggesting that insiders generally did not trade in anticipation of regulatory events.

With regard to (iii), we find strong evidence that insider trades at TARP recipients five days before the announcement of TARP capital infusions predict announcement day returns. We interpret these findings as suggesting that insiders had private information about the TARP capital infusion and the associated valuation implications of this infusion. These results are robust to a variety of rigorous sensitivity tests designed to sharpen identification. Notably, we find the relation between insider trades and daily returns on the announcement day is unique to trades placed five days before the announcement, and that the relations are unique to TARP

infusion dates. However, we find these results are attributable to insider trades at a small number of banks that receive TARP funds (<10% of TARP recipients in our sample).

Overall, our analysis suggests that insiders were unable to predict either the Financial Crisis or the events surrounding the creation of TARP. However, conditional on the creation and implementation of TARP, our analysis suggests insiders had a greater appreciation (than the market) of the extent to which this intervention would affect the financial system and firm valuation, and traded to take advantage of overly pessimistic market valuations.

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

2. Institutional Setting

In this section we describe the events surrounding the creation and implementation of TARP and discuss the broader insider trading literature.[3]

2.1 Events Surrounding TARP

In 2008, with the massive scope of the global financial crisis becoming apparent, governments around the world began to intervene in financial markets and recapitalize financial institutions. Within the U.S., preparations for a bailout of the financial sector were hastened when Lehman Brothers filed for bankruptcy on September 15. Just two days later, on September 17th, the U.S government announced the collapse of AIG. Shortly thereafter, on the evening of September 18, 2008, congressional leaders met with Treasury Secretary Henry Paulson and

[3] For reviews of the Crisis and government policy responses see among others Acharya, Cooley, Richardson, and Walter (2009), Brunnermeier (2009), Diamond and Rajan (2009), and Mishkin, (2011).

Chairman Ben S. Bernanke and were briefed on a plan for government intervention in the financial system on a massive scale not seen since the Great Depression (Appelbaum and Montgomery, 2008). On September 19th, Treasury Secretary Henry Paulson publically announced plans for an unparalleled $700 billion bailout of financial firms (Cho and Appelbaum, 2008). This program would later become known as the Troubled Asset Relief Program (TARP). Notably, the return to the CRSP value-weighted market index that day was 4.59%.

By September 22nd, a draft TARP bill (entitled the Emergency Economic Stabilization Act of 2008) was circulated on Capitol Hill. The bill was eventually defeated in the House of Representatives on September 29th over concerns about inadequate transparency and the staggering size of funds requested (Hulse and Herszenhorn, 2008). The market sold off in response to the vote: the return to the CRSP value-weighted market index that day was –8.28%. A few days later, on October 1st, the Senate considered and passed a revised TARP bill, which was subsequently passed by the House of Representatives and signed into law by the President on October 3rd. While Section 101 of the act provided the Treasury Secretary the authority "to purchase, and to make and fund commitments to purchase, troubled assets from any financial institution," this framework was eventually abandoned in favor of direct capital injections into troubled banks.

As part of TARP implementation, the Treasury Department announced, on October 13th, its intent to purchase equity in the form of preferred stock from "a broad array of financial institutions," what would later become known as the Capital Purchase Program of TARP (CPP).[4] The next day, the Treasury Department announced more specifics about the program to purchase up to $250 billion of senior preferred shares from qualifying U.S. financial institutions, and

[4] http://www.treasury.gov/press-center/press-releases/Pages/hp1199.aspx

announced that nine of the largest financial institutions had agreed to accept funding.[5] This shift in the focus of TARP from purchasing underwater mortgages and troubled assets to direct capital injections in financial institutions was very controversial; participation was mandatory for the nine largest "too big to fail" banks and effectively represented a partial nationalization of the financial system.[6] Subsequent participation was voluntary, and a total of 707 financial institutions received injections: 350 of were publicly traded banks, 296 were private banks, 57 were thrifts, and 4 were non-bank financial institutions (e.g., Bayazitova and Shivdasani, 2012). Ultimately, the Capital Purchase Program provided approximately $205 billion in capital infusions and concluded December 2009 (see also Veronesi and Zingales, 2010; Ng, Vasvari, and Wittenberg-Moerman, 2011; Duchin and Sosyura, 2012; Farrugio, Michalak, and Uhde, 2013).

With regard to determinants of participation in the CPP program, Bayazitova and Shivdasani (2012) examine the decision for banks to apply to the CPP program and the Treasury Department's decision to approve CPP funding. They find that only weaker banks tended to opt into the program, but that conditional on opting into the program, approval was more likely at banks with stronger asset quality. Duchin and Sosyura (2012) examine the political dimension to CPP funding, and show that political influence measured by lobbying, political contributions, and director connections also affected the provision of CPP capital.

Bayazitova and Shivdasani (2012) find that government equity infusions under the Capital Purchase Program (CPP) produced very positive stock returns for financial institutions. They document returns of approximately 15% for the October 14th announcement date for the

[5] http://www.treasury.gov/press-center/press-releases/Pages/hp1207.aspx.
[6] The nine banks initially receiving TARP were coaxed or "forced" to take the infusion in order to mitigate concerns about adverse selection with respect to CPP funding (e.g., Cho, Irwin, and Whoriskey, 2008; Landler and Dash, 2008). These banks include: Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, Merrill Lynch, Morgan Stanley, State Street, and Wells Fargo.

initial nine recipients and approximately 4% for subsequent recipients. They also find that increases in recipient bank value were recognized when the program was announced by the Treasury (October 14th), but *not* when the specific capital infusions were subsequently announced. Farruggio, Michalak, and Uhde (2013) similarly report that the announcement of the CPP program and CPP capital infusions increased shareholder value and decreased systematic risk, but that these effects were concentrated around the announcement of the CPP program rather than the infusion dates themselves. With regard to subsequent performance, Ng, Vasvari, and Wittenberg-Moerman (2011) report that banks that received CPP funding underperformed non-CPP banks by 5.6% during the CPP initiation period (October 2008-March 2009), but that following the conclusion of the CPP, the portfolio of CPP recipients outperformed non-CPP recipients by 10.3% (April 2009 - December 2010).[7]

While the collective evidence from prior studies points to substantial changes in shareholder wealth associated with announcements of and participation in TARP programs, the extent of information leakage and insider trading in connection with the bailout has not been previously addressed.

2.2 Insider Trades

There is a large empirical literature in corporate finance that examines the information content of insider trades. Prior papers document that insider trades measured over various intervals predict future stock returns and future cash flows (e.g., Jeng, Metrick, and Zeckhauser,

[7] In related work, Veronesi and Zingales (2010) calculate the cost and benefits of the CPP program and find that the program produced net benefits of between $86 and $109 billion. Kim and Stock (2012) examine the impact of TARP financing on the value of existing preferred stock. They find that existing preferred shareholders of banks that voluntarily participated in TARP likely benefited more than shareholders of banks that involuntarily participated (i.e. the initial nine institutions).

2003; Piotroski and Roulstone, 2005; Cohen, Malloy, and Pomorski, 2012).[8] Regarding the underlying information source, there is a general consensus that insiders trade both on private information about misvaluation (e.g., private information about the pricing of public information) and private information about future events. Rozeff and Zaman (1998), Jenter (2005), Sias and Whidbee (2010), and others suggest insiders take contrarian positions relative to past performance and buy (sell) undervalued (overvalued) stocks. Ke, Huddart, and Petroni (2003) suggest that insiders trade on information about pending quarterly earnings increases. Piotroski and Roulstone (2005) suggest that insiders trade on both misvaluation of current earnings and private information about future earnings. Still other studies suggest that managers trade before and concurrent to corporate events as if timing the market (e.g., Karpoff and Lee, 1991; Lee, 1997; Bettis, Coles, and Lemmon 2000; Jenter 2005). Finally, recent studies find the predictive ability of insider trades for future performance has declined following the Sarbanes-Oxley Act of 2002 (e.g., Cheng, Nagar, and Rajan 2007; Brochet, 2010) which required that open market trades by corporate insiders be reported electronically to the SEC within two business days.[9]

One study of particular relevance is Seyhun (1990), which examines whether insiders anticipated the October 1987 Crash and the subsequent recovery. Seyhun (1990) finds the 1987 Crash was a surprise to insiders, they tended to purchase stocks after the crash, and that stocks purchased had larger returns in the subsequent year than stocks that were sold. Seyhun (1990) interprets the evidence as suggesting that investor overreaction was an important element of the

[8] Evidence that insider trades can predict future cash flows implies that the relation between insider trades and returns is not driven by a reaction to the trades themselves, but that the trades contain information. The broad consensus is that insider trades contain information and the market underreacts to the disclosure of these trades (see Seyhun, 1998).

[9] Consistent with prior work on the effect of Sarbanes-Oxley on insider trading, we find a drop in the predictive ability of insider trades for future performance between 2002 and 2004.

October 1987 Crash. Despite the large corporate finance literature on insider trading, and the growing literature on the Financial Crisis and bank bailouts, little is presently known about whether insiders' trades anticipated government intervention, the subsequent recovery, and whether they traded on information about TARP capital infusions.

3. Data and Measurement

3.1 Sample

To construct our sample, we first collect data on insider trades from the Thomson Reuters Insider Filings (Form 4) database. Consistent with prior work, we restrict our analyses to open market purchases and sales of common equity and exclude option exercises, option grants, and gifts. We require the trade price, the number of shares transacted, and the date of the transaction for each trade, and restrict attention to trades by individuals classified as an officer of a financial institution (SIC code 6000 to 6999). We aggregate all insider trades to the firm-month level of analysis (see Section 3.2), and merge the Thomson Reuters insider filings database with CRSP/Compustat for the period between 2002 and 2010 to obtain data on firm stock returns, market value, book-to-market ratios, and earnings. To appear in the sample, we require market value at the end of the month, non-missing returns in the prior month (t–1) and prior year (t–2 to t–12), and book value of equity at the end of the prior fiscal quarter. After applying these criteria, the final sample for our cross-sectional tests consists of 26,333 firm-months.

We begin the sample in 2002 in order to have a sufficiently long time-series of insider trading activity prior to the Crisis with which to compare insider activity immediately prior to and during the Crisis.[10] The Crisis is generally thought to have started in July 2007, continue through the October 3rd creation of TARP, and conclude by June 2009. We refer to the twelve

[10] Inferences are robust to beginning the sample in 2004, the first effective year of the Sarbanes-Oxley Act of 2002.

months immediately prior to the Crisis, July 2006 to June 2007, as the "Pre-Crisis period," the twenty-four months between July 2007 and June 2009 as the "Crisis period," and the nine months after TARP, October 2008 to June 2009, as the "Bailout period." [11]

Finally, we collect data from U.S. Treasury Department TARP transaction reports for our subsequent event study tests that analyze insider trading at recipients of TARP funds.[12] 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 260 capital infusions to 251 unique firms ("TARP banks") across 31 different calendar dates.[13]

3.2 Measuring Insider Trading Activity

We calculate three traditional measures of insider trading activity for each firm-month in our sample. The first measure is the insider buy-sell imbalance at the firm (*BSI*). *BSI* is calculated as insider net purchases (number of shares bought less number of shares sold) in month *t* scaled by insider volume (the number of shares bought plus the number of shares sold) in month *t*. The second measure is the insider net purchase ratio (*NPR*). *NPR* is calculated as insider net purchases in month *t* as a percentage of shares outstanding. The difference between

[11] NBER business cycle dates indicate that a recession starts in the fourth quarter of 2007 and continues through the end of the second quarter of 2009, but prior work generally considers the crisis to start at the beginning of the third quarter of 2007 (e.g., Acharya, Cooley, Richardson, and Walter, 2009; Brunnermeier, 2009; Fahlenbrach and Stulz, 2011; Mishkin, 2011). Inferences throughout the paper are robust, and are in many cases strengthened, if we define the "Crisis period" and "Bailout period" as ending in March 2009 or December 2009, rather than June 2009.

[12] http://www.treasury.gov/initiatives/financial-stability/reports/Pages/TARP-Investment-Program-Transaction-Reports.aspx

[13] Results are robust to including the nine initial participants forced to take TARP funds (see footnote 7 for a list of these institutions). Our sample of TARP recipients is similar in size to prior research. For example, after imposing data requirements, Bayazitova and Shivdasani (2011) and Duchin and Sosyura (2012) examine a sample of 286 TARP recipients, Ng, Vasvari, and Wittenberg-Moerman (2011) examine a sample of 186 recipients, and Farruggio, Michalak, and Uhde (2013) examine a sample of 125 recipients.

BSI and *NPR* is that the buy-sell imbalance measures the commonality in the direction of insider trading activity, but does not contain information about the magnitude of the activity (e.g., *BSI* equals 1 if all trades are purchases, and –1 if all trades are sales); whereas the net purchase ratio measures the magnitude of the net trade but does not contain information about the commonality of trading within the firm. The third measure (*Buyer*) is an indicator variable equal to one if the number of shares bought by insiders in month *t* exceeds the number of shares sold by insiders in month *t*, and zero otherwise. Examining the relation between several measures of insider trading activity and future performance strengthens the credibility of our inferences and helps to ensure that our results are not an artifact of specific measurement choices.

3.3 Descriptive Statistics

Table 1 presents descriptive statistics for our sample. Panel A presents descriptive statistics for various firm characteristics and measures of insider trading activity used in our cross-sectional tests. All variables are calculated at the monthly level, conditional on at least one insider trade during the month. Panel A suggests the average market-adjusted buy-and-hold return in the month following the trade (mean *AbRet*) is 0.31%. Panel A also suggests that the typical financial institution in our sample has a market capitalization of roughly $500 million (mean log of market value, *Size*, of 6.28), book-to-market ratio of about 0.75, and positive market-adjusted buy-and-hold returns in the prior month (mean *PastMoRet* of 0.23%) and prior year (mean *PastYrRet* of 6.91%). The mean and median values of all insider trading measures suggest that insiders at financial institutions are generally engaged in selling transactions over the time period between January 2002 and December 2010.

Panel B presents insider trading activity by time period during our sample. We divide the sample from January 2002 to December 2010 into three periods related to the Crisis as described above (i.e., the Pre-Crisis, Crisis, and Bailout periods) and two "Baseline periods" January 2002 to June 2006, and July 2009 to December 2010. Across all measures of insider trading activity, we see clear evidence that, on average, insiders generally move from being net sellers prior to the Crisis to net purchasers in the Crisis and Bailout periods.

Prior to the Crisis, average *BSI* is negative and significant. Only 34% and 31% of trades for January 2002 to June 2006, and July 2006 to June 2007 are net purchases (i.e., mean values of *Buyer* of 0.34 and 0.31 respectively). In contrast, during the Crisis, average *BSI* is positive and significant. In fact, the majority of trades during the Crisis are net purchases: 55% of trades from July 2007 to September 2008 (i.e. the Crisis period) and 61% of trades from October 2008 to June 2009 (i.e., the Bailout period). Results from testing for a difference in means among the periods indicate that the average values of *BSI*, *NPR*, and *Buyer* are significantly higher during the Crisis, but following the bailout, than during any other period in the sample. Importantly, while the average net trade changes from a sale to a purchase during the Crisis and Bailout periods, there is significant cross-sectional variation in insider trading activity within each period. This within-period cross-sectional variation in insider trading is the basis for our subsequent tests regarding the predictive ability of insider trades for the cross-section of future firm performance.

A simple univariate test of the predictive ability of insider trades for future firm performance is presented in Panel C. For each period, Panel C reports the cross-sectional correlation between the respective measure of insider trading and abnormal returns in the subsequent month. Each cross-sectional correlation represents the slope coefficient from a

regression of abnormal returns in month *t+1* (*AbRet*) on the respective measure of insider trading in month *t,* estimated during the respective period. We find a statistically positive cross-sectional correlation between insider trades and future abnormal returns during the Baseline period from January 2002 to July 2006 (i.e., more than one year before the Crisis) and during the Bailout period from October 2008 to June 2009. Over the entire sample, predictive ability is lowest during the Crisis immediately prior to the bailout (i.e., July 2007 to September 2008), and highest during the Crisis immediately following the bailout (i.e., October 2008 to June 2009). Results from testing for differences in correlations between periods indicate that the correlations during the Bailout period are significantly higher than any other period in the sample. Thus, the univariate descriptive statistics suggest not only that the intensity of insider purchases dramatically increases following government intervention in October 2008 (i.e., the creation of TARP), but also that the correlation between those trades and future returns increases as well.

4. Predictive Ability of Insider Trades

Two approaches are commonly used in the literature to study the predictive ability, or information content, of insider trades for future performance. The first approach, which we label "mimicking portfolios," entails sorting firms into a set of calendar-time portfolios based on insider trades and calculating risk-adjusted returns at the portfolio level relative to a set of common factors. The primary advantage of this approach is that it provides an estimate of returns an investor could have realized by mimicking insider trades. This approach also collapses the cross-section of returns (on a given date) into a single time-series observation, thereby alleviating econometric concerns regarding cross-sectional dependence. However, a key assumption of this approach is that returns are generated by a set of common risk factors (e.g.,

Fama and French, 1993). The second approach, which we label "characteristic regressions," entails computing buy-and-hold returns over a specified window and then estimating a regression of these returns on various measures of insider trading activity and multiple control variables designed to capture various firm-specific characteristics associated with the cross-section of returns (e.g., Daniel and Titman 1997). The primary advantage of this approach is the ability to precisely estimate marginal effects of insider trading while controlling for a large number of confounding variables. The disadvantage is that it assumes a linear relation between the sort variable (insider trading activity) and future returns. As each method is known to have its own unique advantages and disadvantages, we report results using both methods for the sake of completeness.

4.1. Mimicking portfolios

Our first set of tests examines the returns to portfolios formed according to whether insiders are net buyer or net sellers. Specifically, we form portfolios based on whether the net trade in month *t* was a purchase (i.e., *Buyer* equals one, in which case *BSI* and *NPR* are positive) or a sale (i.e., *Buyer* equals zero, in which case *BSI* and *NPR* are negative). That is, each month stocks are grouped into one of two portfolios based on whether insiders' net purchases in month *t* are positive ("Net Buy" portfolio) or negative ("Net Sell" portfolio). Stocks are then held over the following month (month *t*+1). Portfolios are rebalanced at the end of each month based on new insider trades that month, and portfolio returns are calculated separately assuming equal-weights and value-weights.[14] This procedure results in a time-series of 108 monthly observations

[14] Value-weights are based on market value at the time of portfolio formation (end of month *t*). To ensure that value-weighted portfolios are not dominated by a very few, very large banks, we value-weight individual stocks using the logarithm of market value rather than the dollar value of market value.

for each portfolio between 2002 and 2010. We then construct the hedge portfolio by going long in the Net Buy portfolio and short in the Net Sell portfolio.

Figure 1 presents average *monthly* hedge portfolio returns by calendar quarter between January 2002 and December 2010 along with the closing price of the CRSP market portfolio (normalized to 1 at the beginning of 2002). We note four distinct patterns that warrant special attention. (1) Consistent with Cheng, Nagar, and Rajan (2007) and Brochet (2010), hedge portfolio returns decline after the Sarbanes-Oxley Act goes into effect in 2004, and are relatively flat prior to the start of the Crisis. (2) Returns to the hedge portfolio at the outset of the Crisis (i.e., prior to the introduction of TARP between July 2007 and September 2008) are substantially lower than prior to the Crisis. This suggests that firms in which insiders are buying are actually fairing worse than firms in which insiders are selling. (3) There is a dramatic increase in hedge returns in the fourth quarter of 2008, after TARP was announced (i.e., October 3, 2008) and this increase persists for the remainder of the Crisis. (4) The dramatic increase in hedge portfolio returns is observed immediately *prior* to the market bottom and is greatest over a six-month period in which *the overall market continues to decline* (i.e, the 4th quarter of 2008 through the 1st quarter of 2009). This suggests that the hedge portfolio is not simply tracking contemporaneous market movements. In other words, while the overall market was declining, stocks where insiders were net purchasers were experiencing dramatic *increases* in price. This illustrates that our results are not an artifact of insiders purchasing while the broader market was rebounding. Instead, the results are consistent with the evidence in Panel B of Table 1 that insiders began purchasing in *advance* of the market bottom, and that prices of firms where insiders were net purchasers in advance of the market bottom, led the recovery (i.e., recovered before the overall market).

To assess the statistical and economic significance of the temporal changes in hedge portfolio returns, and to examine whether the temporal change is attributable to insider purchases or insider sales, we estimate time-series regressions of monthly portfolio returns on the three Fama-French factors, the momentum factor, and three indicator variables for whether the month corresponds to the Pre-Crisis period, the Crisis period, or the Bailout period.

$$R^p{}_t - R^f_t = \alpha_0 + \beta\ Controls + \alpha_1 PreCrisisPd_t + \alpha_2 CrisisPd + \alpha_3 BailoutPd + \varepsilon_{p,t} \quad (1)$$

where R^p (R^f) is the portfolio return (risk-free rate) in month t expressed in percent, *Controls* is vector of control variables including the three Fama-French factors and momentum factor expressed in percent (*MKTRF, SMB, HML, UMD*), *PreCrisisPd* is an indicator variable equal to one during the 12 months leading up to the Crisis (July 2006 and June 2007) and zero otherwise, *CrisisPd* is an indicator variable equal to one during the 24-month Crisis period (July 2007 to June 2009) and zero otherwise, and *BailoutPd* is an indicator variable equal to one during the Crisis period following the announcement of TARP (October 2008 to June 2009) and zero otherwise.

The intercept from this regression represents average monthly risk-adjusted returns to the portfolio over the sample period (2002 to 2010). The coefficient on *PreCrisisPd* represents the change in average risk-adjusted returns immediately prior to the Crisis, the coefficient on *CrisisPd* represents the change in average risk-adjusted returns during the Crisis, and the coefficient on *BailoutPd* represents the change in average monthly risk-adjusted returns during the Crisis, between the period prior to TARP and the period subsequent to TARP. We adjust for autocorrelation in the time-series of regression residuals, and calculate *t*-statistics based on Newey-West corrected standard errors.

Panel A of Table 2 presents results from estimating equation (1) for equal-weighted portfolio returns and separately excluding the Fama-French factors (i.e., raw portfolio returns) and including the Fama-French factors (i.e., risk-adjusted portfolio returns). We find similar results regardless of risk-adjustment. For parsimony, we discuss results pertaining to risk-adjusted portfolios. Consistent with prior literature that finds insider trades predict future returns, we find that the hedge portfolio earns statistically significant risk-adjusted returns *excluding* the three periods related to the Crisis (i.e. the circumstance where *PreCrisisPd* = 0, *CrisisPd* = 0, and *BailoutPd* = 0). In particular, we find the intercept, α_0, is economically and statistically significant (coefficient 0.69, *t*-stat of 3.35). Excluding these periods, the hedge portfolio earns 0.69% per month, and most of this return is driven by the returns of the Net Buy portfolio which earns 0.65% per month (*t*-stat of 1.99).

Examining time-series variation in hedge portfolio returns, we find statistically negative coefficients on *PreCrisisPd* and *CrisisPd* (coefficients –0.62 and –1.33, *t*-stat of –2.55 and –3.58, respectively). We also find a large positive coefficient on the *BailoutPd* (coefficient 2.94, *t*-stat of 5.29). During the Crisis *prior to TARP*, the hedge portfolio earned risk-adjusted returns of –0.64% (*Intercept* + *CrisisPd*). However, during the Crisis *after TARP*, the hedge portfolio earned risk-adjusted returns of 2.30% (*Intercept* + *CrisisPd* +*BailoutPd*), a statistically and economically significant difference of 2.94% (coefficient on *BailoutPd*).

Examining whether these results are driven by purchases or sales, we find a large positive coefficient on *BailoutPd* for the Net Buy portfolio (coefficient 5.03, *t*-stat of 2.20) and a large negative coefficient on *BailoutPd* for the Net Sell portfolio (coefficient –1.31, *t*-stat of –2.17). This suggests that both the predictive ability of insider purchases for *positive* future risk-adjusted

returns *and* the predictive ability of insider sales for *negative* future risk-adjusted returns increased subsequent to government intervention.

Panel B presents results using value-weighted portfolio returns. We use value-weighted portfolio returns to assess whether the results are attributable to trading at large or small financial institutions. Value-weighted portfolio will place more (less) weight on trades of insiders at large (small) financial institutions. Accordingly, if our results are attributable to the predictive ability of insider trades at large firms, the use of value-weighted portfolios should increase the significance of our results; whereas if our results are attributable to the predictive ability of insider trades at small firms, the use of value-weighted portfolios should decrease the significance of our results. Results in Panel B are very similar in magnitude to those in Panel A, suggesting hedge returns are not sensitive to value-weighting. Collectively, the results from our portfolio tests suggest a pronounced increase in insiders' information advantage following government intervention, but no evidence of an increase in information advantage during the Crisis prior to government intervention, or prior to the Crisis.

4.2. Characteristic Regressions

Our second set of tests estimates the relation between various measures of insider trading activity and future returns after controlling for various firm characteristics. Following Cohen, Malloy, and Pomorski (2012), we regress market-adjusted buy-and-hold returns over the future *s* months ($AbRet_{t+s}$) on a vector of control variables which includes the natural log of market value at the end of month *t* (*Size*), the book-to-market ratio at the end of month *t* (*BM*), the market-adjusted return in month *t*–1 (*PastMoRet*), and the market-adjusted buy-and-hold return over the past year from month *t*–2 to *t*–12 (*PastYrRet*), and one of three measures of insider trading

activity (*IT*) at the firm in month *t* (BSI_t, NPR_t, or $Buyer_t$). The coefficient on the measure of insider trading activity represents the predictive ability of insider trading activity for future returns.

To examine whether the predictive ability of insider trading activity increases before or during the Crisis, we include *PreCrisisPd, CrisisPd,* and *BailoutPd* in the regression and interact them with the respective measure of insider trading activity. Specifically we estimate,

$$AbRet_{i,t+s} = \theta\ Controls_{i,t} + \delta_1 IT_{i,t} + \delta_2 IT_{i,t} * PreCrisisPd_t + \delta_3 IT_{i,t} * CrisisPd_t + \delta_4 IT_{i,t} * BailoutPd_t + \varepsilon_{i,t+1,} \quad (2)$$

where the vector of control variables includes all of the controls variables described above as well as the *PreCrisisPd, CrisisPd,* and *BailoutPd* main effects, and all variables are as previously defined. We estimate equation (2) separately measuring future returns over the subsequent 1, 3, 6, or 12 month periods (i.e., s = 1, 3, 6, or 12 respectively), and base inferences on standard errors clustered by firm (which allows for arbitrary time-series correlation). For ease of interpretation, we also rank control variables into quintiles each month and scale the quintile ranks to range from 0 to 1. As a result of using scaled quintile ranks, the coefficient represents the difference in abnormal returns between the top and bottom quintile of the respective variable, *ceteris paribus*. For example the coefficient on *BM* indicates the difference in future market-adjusted returns when moving from the bottom quintile of *BM* to the top quintile of *BM*.[15]

Table 3 reports results from estimating equation (2) using one-month ahead future returns as the dependent variable (i.e., s = 1). Consistent with the prior literature (e.g., Cohen, Malloy, Pomorski, 2012), we find *BM* is positively associated with future returns (coefficients range

[15] Inferences throughout the paper are unchanged if (i) we use the raw values of the control variables rather than their quintile ranks, (ii) we use raw returns, industry-adjusted returns, or abnormal returns relative to the size and book-to-market matched portfolios as the dependent variable, or (iii) we cluster standard errors by both firm and date.

between 0.54 and 0.60, *t*-stat between 2.61 and 2.85), *PastMoRet* is negatively associated with future returns (coefficients range between –0.52 and –0.55, *t*-stat between –2.64 and –2.80), and *PastYrRet* is positively associated with future returns (coefficients range between 0.84 and 0.87, *t*-stats between 4.18 and 4.31). Also consistent with prior research, columns (1), (3), and (5) of Table 3 report a statistically significant positive relation between each of the three measures of insider trading activity and one-month ahead returns (coefficients 0.26, 1.29, and 0.50 and *t*-stats 3.45, 2.54, and 3.35 for *BSI*, *NPR*, and *Buyer* respectively).

Columns (2), (4), and (6) of Table 3 report the results after including the various Crisis period indicator variables and interaction terms. Consistent with the earlier portfolio analysis, across all three measures of insider trading activity, we find that the relation between insider trading and future returns weakens considerably prior to the Crisis ($IT_{i,t}$ * $PreCrisisPd_t$, *t*-stats –2.90, –2.65, and –2.67 respectively) and during the Crisis prior to government intervention ($IT_{i,t}$ * $CrisisPd_t$, *t*-stats –3.34, –3.05, and –3.31 respectively). In contrast, across all three measures of insider trading activity, we find that the relation between insider trading and future returns strengthens considerably during the Crisis subsequent to government intervention ($IT_{i,t}$ * $BailoutPd_t$, *t*-stats 4.95, 3.72, and 4.96 respectively).

One potential concern about measuring future returns over the subsequent month is that insider trading, even absent any information content, could trigger a short-term investor (over)reaction that is corrected over a longer time horizon. Further, insiders might be trading based on private information not impounded in price in the next month. To explore this issue, Table 4 reports results from estimating equation (2) but measuring future performance over the subsequent three, six, and twelve month periods (i.e., s = 3, 6, and 12 respectively). Across all measures of insider trading activity, and all horizons of future returns, the evidence in Table 4 is

consistent with that in Table 3. For example, across all measurement horizons, the predictive ability of insider trades for long-run future performance declines significantly both prior to the Crisis ($IT_{i,t}$ * $PreCrisisPd_t$, t-stats between –3.17 and –4.78 for s = 3, between –3.26 and –4.96 for s = 6, and between –1.95 and –2.65 for s = 12) and during the Crisis prior to government intervention ($IT_{i,t}$ * $CrisisPd_t$, t-stats between –2.36 and –4.16 for s = 3, between –3.03 and –3.68 for s = 6, and between –2.62 and –3.89 for s = 12). In contrast, across all measurement horizons, the predictive ability of insider trades for long-run future performance strengthens considerably during the Crisis subsequent to government intervention ($IT_{i,t}$ * $BailoutPd_t$, t-stats between 3.25 and 5.08 for s = 3, between 3.90 and 4.52 for s = 6 and between 3.11 and 3.49 for s = 12).[16]

Notably, the predictive ability of insider trades for future returns increases with the measurement horizon (e.g., coefficients of 1.75, 2.93, and 3.46 on *Buyer* and 9.08, 13.44, and 15.22 on *Buyer * BailoutPd* for 3-, 6-, and 12-month horizons respectively). These results are inconsistent with what one would expect if the information content of insider trades is revealed (and priced) shortly after the trade. Rather, the evidence suggests insider trades contain information about long run future performance, and that there is a pronounced increase in the information content of these trades for long-run performance subsequent to government intervention during the Financial Crisis.

In Table 5 we examine the robustness of our results to selected changes in model specification.[17] For example, one concern is that our results are driven by the confounding effect of unobserved firm characteristics. Columns (1), (2), and (3) present the results from estimating

[16] By virtue of the fact that insider trades predict long horizon future returns, our results suggest the market underreacts to the disclosure of these trades. This is not surprising, and is consistent with a large body of prior research on insider trading (e.g., Lakonishok and Lee, 2001).

[17] For parsimony we tabulate results from estimating alternative specifications of equation (2) using one-month ahead returns (i.e., s = 1). Inferences are affected if we use longer horizons.

equation (2) after including both firm-fixed effects and date-fixed effects.[18] Even holding the firm fixed (i.e., the variation is now "within firm"), we find that variation in insider trading generally predicts variation in future returns and that this predictive ability increases subsequent to government intervention ($IT_{i,t}$ * $BailoutPd_t$, *t*-stats of 3.33, 3.07, and 3.40 respectively). These results are consistent with earlier results and provide evidence that even the within-firm predictive ability of insider trades increases following government invention.

Similar to the extant insider trading literature, our tests are conditional on observing an insider trade, and thus can be viewed as tests of whether firms with insider purchases outperform firms with insider sales. To mitigate concerns that our results are somehow attributable to focusing only on firm-months in which we observe insider trades, columns (4), (5) and (6) report results from estimating a modified version of equation (2) in which we include all firm-month observations over the entire sample period for which we have data (i.e., we include months *without insider trade*). In this specification, we set all measures of insider trading activity equal to zero during months without any insider trading. The resulting sample consists of 118,254 firm-month observations. The results suggest that the increase in predictive ability of insider trades following government intervention is robust to including firm-months in which there is no insider trading ($IT_{i,t}$ * $BailoutPd_t$, *t*-stats of 5.08, 3.76, and 4.05 respectively).

Finally, to control for the potential confounding effect of unobserved executive characteristics, columns (7), (8) and (9) report results from estimating a modified version of equation (2) in which we disaggregate firm-month observations to the executive-month level and include executive-fixed effects. In this specification, the unit of observation is the executive-month rather than the firm-month. The resulting sample consists of 45,403 executive-month

[18] The main effects, *PreCrisisPd*, *CrisisPd*, and *BailoutPd* are excluded in columns (1), (2) and (3) because they are collinear with the date-fixed effects.

observations. Even holding the executive fixed (i.e., the variation is now "within executive"), we continue to find that the predictive ability of insider trades increases after government intervention ($IT_{i,t}$ * $BailoutPd_t$, *t*-stats of 3.35, 2.23, 3.39 respectively).

Collectively, the results in Tables 2 through 5 indicate that the information content of insider trades did not increase in anticipation of the Crisis, but did increase substantially during the Crisis following government intervention. These results are inconsistent with the notion that top executives at financial institutions anticipated the Crisis. However, they strongly suggest that—conditional on government intervention—top executives at financial firms anticipated the recovery.

We next explore three non-mutually exclusive explanations for the pronounced increase in insider's information advantage: (i) conditional on government intervention, insiders had (and traded on) superior information about the extent to which the intervention would affect firm valuation, (ii) insiders had (and traded on) private information about various government bailout programs in advance of the public, and/or (iii) insiders had (and traded on) private information about their firm's forthcoming capital infusion or bailout.

4.3. Information content of insider trades conditional on prior firm performance

One explanation for the increase in insiders' information advantage during the bailout period is that insiders had a better appreciation than the general public of the extent to which government intervention would affect firm valuation. If insiders exploited this information advantage, then we should observe that the increase in predictive ability of insider trades is concentrated in firms that will benefit more from an increase in the health of the financial

system, i.e., those firms that previously performed poorly. That is, we should observe a pattern similar to what Seyhun (1990) found during the Market Crash of 1987.

Table 6 presents results from testing whether the increase in predictive ability of insider trades in connection with government intervention varies with prior firm performance. Specifically, we partition the sample of firm-months into quintiles based on two measures of past performance: the prior quarter's earnings (Panel A) and prior month's market-adjusted return (Panel B).[19] We then estimate equation (2) separately for the bottom and top quintiles (i.e., the top and bottom 20%) and pooling the intermediate quintiles (i.e., the middle 60%). We refer to the bottom, intermediate, and top quintiles as Q1, Q3, and Q5, respectively.

The results reported in Panel A of Table 6 indicate that the increase in information content of insider trades in connection with government intervention declines with the prior quarter's earnings surprise. For all three measures of insider trading activity, the coefficient on *IT*BailoutPd* declines monotonically across the quintiles: from 3.37, to 2.54, to 0.62 for *BSI*; 14.06, to 10.29, to 10.27 for *NPR*; 6.66, to 5.00, to 1.79 for *Buyer*. While the coefficient on *IT*BailoutPd* is reliably positive and statistically significant for all measures of insider trading in the *lowest* quintile (*t*-stats of 2.98, 2.95, and 2.94 respectively), it is not statistically distinguishable from zero for two of the three measures in the *highest* quintile (*t*-stats of 0.86, 2.87, and 1.35 respectively) The difference in the coefficient on *IT*BailoutPd* between the bottom and top quintile is 2.75, 3.79, and 4.87 (*t*-stats of 2.69, 1.93, and 2.47) for each of the respective measures of insider trading. This differential relation appears unique to the Bailout period because there is no evidence of statistical differences in the coefficients on *IT*, *IT*PreCrisisPd*, and *IT*CrisisPd* between the extreme quintiles.

[19] Earnings surprise is calculated as the forecast error from a seasonal random walk model of quarterly earnings scaled by total assets. The sample in Table 6 is reduced to 24,701 by the requirement of earnings surprise in the prior quarter. Results are robust to measuring past performance over longer horizons.

The results reported in Panel B of Table 6 reveal that the monotonic decline in the coefficient on *IT*BailoutPd* is even more pronounced when prior performance is measured using market-adjusted returns. The coefficient on *IT*BailoutPd* is reliably positive and is statistically significant in the *lowest* quintile (*t*-stats of 2.13, 2.79, 2.33), however it is not statistically distinguishable from zero in the *highest* quintile (*t*-stats of –0.12, –1.23, –0.19). Similar to Panel A, the difference in the coefficient on *IT*BailoutPd* between the bottom and top quintile in Panel B is 4.71, 40.91, and 10.33 (*t*-stats 2.85, 3.01, and 2.08) for each of the respective measures of insider trading whereas there is no evidence of a statistical difference in the coefficients on *IT*, *IT*PreCrisisPd*, and *IT*CrisisPd* between the bottom and top quintile.

Collectively, we interpret the evidence in Table 6 as suggesting that the information content of insider trades in poorly performing firms did not increase prior to the Crisis but did increase subsequent to government invention, whereas the information content of insider trades in well performing firms neither increased prior to Crisis nor subsequent to government intervention. Thus, the evidence in Table 6 suggests that the increases in information content of insider trades in connection with government intervention is concentrated in firms with the worst prior performance. This is consistent with the notion that insiders in these firms had a better appreciation than the general public of the extent to which government intervention would affect firm valuation, and traded on this knowledge. Of course, it can also be the case that insiders were privately informed about various government bailout programs in advance of the public, and/or insiders had private information about the extent to which their firm would receive a capital infusion from the government and its valuation implications. We examine these additional possibilities using event study methods in Section 5.

5. Event study analyses

5.1. Insider trading around the announcement of government intervention

An additional potential source of private information that insiders could have exploited in connection with government intervention is advanced knowledge of the creation of various government bailout programs (relative to the general public). For example, Sorkin (2009) suggests that the U.S. government's deliberations surrounding TARP and other government programs entailed extensive consultations with executives of leading financial institutions. For the purposes of our analysis, we focus on TARP because it provided the greatest amount of bailout monies and represented direct investments in recipient firms.

To assess whether insiders traded strategically in anticipation of the creation of TARP, we follow Bayazitova and Shivdasani (2012) and focus on the following relevant dates: (1) the date of the first announcement of TARP on September 19, 2008; (2) the date of the Draft TARP bill being circulated on September 22, 2008; (3) the date the Bill fails in the House on September 29, 2008; (4) the date the Bill passes in the Senate on October 1, 2008; (5) the date the Bill passes the House and is signed by President Bush on October 3, 2008; (6) the date the Treasury announces a program to purchase equity in financial institutions on October 13, 2008 (what would become the CPP); and (7) the date Treasury Secretary Paulson formally announces the Capital Purchase Program and makes an investment in nine large financial institutions on October 14, 2008.

We first analyze the market reaction to these events to confirm that they were material. For each event, Panel A of Table 7 presents the CRSP market return on the event day and the average returns on that day for all banks in our sample. Consistent with Bayazitova and Shivdasani (2012), the pooled results in Panel A of Table 7 show that in our sample, banks

experienced positive returns over the seven selected event dates. Results are robust to increasing the measurement horizon of the insider trading variables to thirty days before the event, and to measuring event returns using a two-day window.

We next assess whether insiders' trades predict the stock market reaction to these events by estimating the following cross-sectional regression for each event,

$$AbRet_0 = \theta\ Controls + \delta_1\ IT_{(-5,-1)} + \varepsilon \quad (3)$$

where $AbRet_0$ is the market-adjusted return on the event day, *Controls* is vector of control variables including *Size*, *BM*, *PastMoRet*, and *PastYrRet* (all measured in the month prior to the event and as previously defined) and $IT_{(-5,-1)}$ is one of three directional measures of insider trading activity calculated over the prior five trading days. $BSI_{(-5,-1)}$ is calculated as insider net purchases over the prior five trading days prior to the event, scaled by the total number of shares bought plus the number of shares sold over the same period, $NPR_{(-5,-1)}$ is calculated as insider net purchases over the prior five trading days prior to the event as a percentage of shares outstanding. $Buyer_{(-5,-1)}$ is an indicator variable equal to 1 if the number of shares bought by insiders exceeds the number of shares sold by insiders over the five trading days prior to the event, and 0 otherwise. As before, we use the scaled quintile ranks of the control variables and cluster standard errors by firm.

Panel B of Table 7 reports results from estimating equation (3) separately for each measure of insider trading activity, for each event, and pooling across all events. We focus our attention on the estimate of δ_1, the partial correlation between insider trade five days before the event ($t = -5 \ldots -1$) and event day returns ($t = 0$).[20] For parsimony we do not tabulate control variables. Pooling observations across all seven events, we find no evidence of a correlation

[20] Focusing on a narrow window sharpens identification and increase the chance that the trades are directly linked to the event in question. Inferences are robust to increasing the measurement horizon of the insider trading variables to thirty days before the event.

between insider trading in advance of the event and the market's reaction. However, if we drop Event #3 (Bill fails in the House) which is the only event associated with a decrease in the likelihood of government intervention, we find coefficients of 1.45 (*t*-stat of 1.88) and 2.19 (*t*-stat of 1.71) in pooled event regressions when insider trading is measured using *BSI* and *Buyer*, respectively. Examining the events individually, there is little evidence of a relation between insider trading shortly before the event and the market reaction to the event. The absence of such a relation could indicate either insiders do not have private information about the forthcoming event, or have such information but are reluctant to trade in short proximity before major events due to heightened litigation risk. Collectively, we interpret the results in Panel B of Table 7 as (at best) weak evidence that insider trades shortly before the legislative and regulatory events related to TARP are correlated with the subsequent market reaction to the event.

5.2. Insider trading around capital infusions

The previous tests find weak evidence that bank insiders anticipated events surrounding the creation of TARP. However, it is plausible that while insiders did not trade on private information about the creation of TARP, they subsequently had (and traded on) private information about the TARP monies their firm received. Thus, an additional potential source of private information that insiders could have exploited in connection with government intervention is advanced knowledge about the bailout funds their firm would receive.

We examine this possibility by analyzing the market reaction to TARP capital infusions. Specifically, we use the market reaction to the announcement of TARP capital 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 and the announcement returns. For this analysis, we focus on 251 financial institutions in our sample that received capital infusions under the Capital Purchase Program of TARP, which we refer to as "TARP banks" (see Section 3.3 for details on sample construction). Unlike the legislative and regulatory events detailed in Table 8 that occur on common dates for all firms, dates of TARP capital infusions vary by firm. Capital infusions had to be publically disclosed (by the Treasury) within 48 hours of the completion of the infusion, however our search of firm disclosures on Factiva and Lexis-Nexus suggests the vast majority of infusion announcements occur within one trading day of the infusion.[21] Accordingly, we focus our analysis on returns the first trading day following the completion of the infusion (i.e. the announcement day).

Panel A of Table 8 presents the average return on the announcement day for our sample of TARP banks. Consistent with prior research, the average return for TARP banks is not different from zero (e.g., Bayazitova and Shivdasani, 2012).[22] However, the lack of an "on average" positive response ignores significant cross-sectional variation in the response. While shareholders in some banks clearly view the infusion as unexpected negative news (i.e., the 25th percentile of the announcement day return is –4.62%), shareholders in other banks clearly view the infusion as unexpected positive news (i.e., the 75th percentile of announcement day return is 2.70%). This cross-sectional variation provides the opportunity to develop more insightful tests of insider anticipation of TARP infusions.

We assess whether insiders traded on private information about the infusion by examining whether insiders' trades predict announcement day returns. In particular, we estimate

[21] Section 114(a) of Emergency Economic Stabilization Act of 2008 stipulates that the Treasury must publicly disclose recipients within 48 hours of the infusion.
[22] Bayazitova and Shivdasani (2012) find that increases in recipient bank value were recognized when the program was announced by the Treasury (October 14th), but *not* when the specific capital infusions were subsequently announced.

equation (3) for all banks receiving an infusion, and focus our attention on the estimate of δ_1, the partial correlation between insider trade five days before the announcement and announcement day returns. Panel B of Table 8 presents strong evidence of a positive relation between insider trading activity five-days before the announcement of a TARP capital infusion and announcement day returns. Across all three measures of insider trading activity (i.e., $BSI_{(-5,-1)}$, $NPR_{(-5,-1)}$, and $Buyer_{(-5,-1)}$), columns (1), (3) and (5) of Panel B suggest the coefficient on $IT_{(-5,-1)}$ is highly positive and is statistically significant. The observation that insider trades *five days before* the announcement of a TARP capital infusion predict the market reaction to the infusion is particular striking. These results suggest that insiders traded on private information regarding TARP capital infusions and the associated valuation implications of the infusion.

To investigate the robustness of this relation, we include returns over the five days before the event ($Ret_{(-5,-1)}$) and insider trading over the 25-day window ending five days before the event ($IT_{(-30,-6)}$) as additional controls. $Ret_{(-5,-1)}$ controls for any confounding effects of short-run return continuation or reversal, and $IT_{(-30,-6)}$ controls for the possibility that the associations we observe are not unique to trades placed five-days before the announcement day. For example, if an omitted variable is correlated with the market reaction to the infusion and with the general direction/intensity of insider trading at the firm, we would expect to observe a similar relation for those trades occurring more than five days in advance of the infusion. Column (2), (4), and (6) of Panel B present results after including these additional controls. Across all specifications, we find evidence of a short-run return reversal (i.e., a negative coefficient on $Ret_{(-5,-1)}$), but no evidence of a relation between insider trades more than five-days in advance of the announcement, i.e., $IT_{(-30,-5)}$, and announcement returns. In all cases, insider trading over the prior five-days continues to predict announcement day returns.

5.3. Insider trading around capital infusions: Placebo tests

The tests in Table 8 are premised on the notion that absent TARP infusions, insider trading activity over the prior five days is unrelated to daily returns. That is, we are operating under the null hypothesis that, absent a TARP infusion, the coefficient on $IT_{(-5,-1)}$ in equation (3) is zero. However, there are two important alternative considerations. First, there is the possibility that the relation between insider trades and announcement day returns is attributable to the market reacting to the disclosure *of insider trades* rather than to the TARP infusion.[23] Second, there is always the possibility that the relation between insider trades and announcement day returns is the result of test misspecification or some omitted determinant of the cross-section of returns that is correlated with insider trading. If either of these alternatives is the case, we would expect to observe a similar relation between insider trades and daily returns in the absence of TARP infusions.

We provide insight into these concerns by using a placebo or "falsification" test to assess the relation between insider trades and daily returns in the absence of a TARP infusion. Specifically, rather than test whether the coefficients from estimating in equation (3) on the announcement day are different from zero, we test whether they are different from similar coefficients on non-announcement days. This test amounts to a difference-in-difference estimator. That is, we assess whether abnormal returns on a given event day vary cross-sectionally with insider trading activity over the prior five days (i.e., the first difference), and then assess whether this variation is significantly different between announcement and non-

[23] While entirely possible, it is important to bear in mind that prior research finds the market generally does not react to insider trades either at the time of the trade, or the time the trade is publicly reported (e.g., Lakonishok and Lee, 2001).

announcement days (i.e. the second difference). Comparing the results between announcement and non-announcement days enables us to rule out the possibility that what we are documenting is a general phenomenon among TARP banks (and not a specific effect related to the announcement of TARP infusions).

The results for the placebo tests are presented in Table 9. Columns (1), (4), and (7) report the respective coefficients and *t*-statistics from Panel B of Table 8 (denoted as β). Columns (2), (5), and (8) report results from estimating the same regression for the sample of TARP recipients, but pooling across all non-announcement days during the Bailout period (denoted as $E[\beta]$). Regardless of the measure of insider trading, we find a positive relation between insider trading activity over the prior five days and the return on *non-announcement* days ($IT_{(-5,-1)}$, *t*-stats of 3.79, 1.88, and 3.27, respectively). However, we also find that the coefficients on the insider trading variables on the *announcement* day are more than 10x greater than their counterparts on non-announcement days, and this difference is statistically significant (*p*-values for difference in coefficients of 0.04, <0.01, and 0.06 respectively). Notably, the coefficients on the control variables are not statistically different between announcement and non-announcement days. We interpret these results as suggesting that the relations we document are not observed in the absence of TARP infusions and are unique to such events.[24]

Finally, because the notion that insiders trade on knowledge of TARP infusions shortly before the publicly announcement of these infusions is provocative, we examine the incidence of this behavior. We find that insider trades at a small set of banks that receive TARP funds are primarily responsible for the result in Tables 8 and 9 (i.e., insider trades at <10% of TARP

[24] In untabulated analysis, we conduct an additional sensitivity test where we examine whether the relations we document on the announcement day vary between TARP recipients and non-recipients. We find no evidence of a statistically significant relation between insider trading activity and announcement day returns for non-recipients, and we find that the difference in coefficients between recipients and non-recipients is statistically significant at the 5% level or less. We interpret this as suggesting the relations we document are unique to TARP recipients.

recipients, or 16 banks in total). Interpreting this in the context of our earlier findings, while the empirical results in Tables 8 and 9 clearly suggest insiders at *some* banks appear to trade on private information about TARP infusions, the incidence of this behavior is not sufficiently widespread that it could account for the general increase in predictive ability of insider trades documented in Tables 2 through 6.

6. Conclusion

This paper examines whether managers at leading financial institutions anticipated the effect of the Financial Crisis, subsequent recovery, and bank bailout on their firms' share prices. If managers anticipated the effect of the Crisis and subsequent recovery on shares prices, we expect that managers' information advantage, and hence the predictive ability of their equity trades for future performance, will be heightened before and during the Crisis.

Analyzing a sample of all open market purchases and sales of top executives (Section 16 officers) at publicly-traded financial institutions between 2002 and 2010, we find no evidence that insider trades over the twelve months leading up to the Crisis or during the Crisis prior to the creation of TARP predict future performance. This suggests that insiders were *unable* to predict the effect of the forthcoming Crisis on their firm.

However, we find strong evidence that insiders anticipated the subsequent recovery. Specifically, we find that the predictive ability of insider trades for future performance is greater during the nine months following the announcement of the Troubled Asset Relief Program (TARP) than at any other point during the 2002-2010 time period. 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. A hedge portfolio that mimics insider

trades earns substantial positive risk-adjusted returns even while the overall market continues to decline. In additional analysis, we find that the increase in the predictive ability of insider trades is concentrated among financial institutions that would benefit the most from an increase in the health of the financial system, and that it is not attributable to insiders trading in advance of key events relating to TARP or TARP capital infusion.

Overall, we interpret the evidence as suggesting that insiders were unable to predict the Financial Crisis. However, once the government announced it would intervene, insiders of financial institutions appear to have anticipated the effect of this intervention on share prices. Specifically, we find compelling evidence that insiders had a greater appreciation (than the market) of the extent to which government intervention would affect the financial system and firm valuation, and traded to exploit this information advantage.

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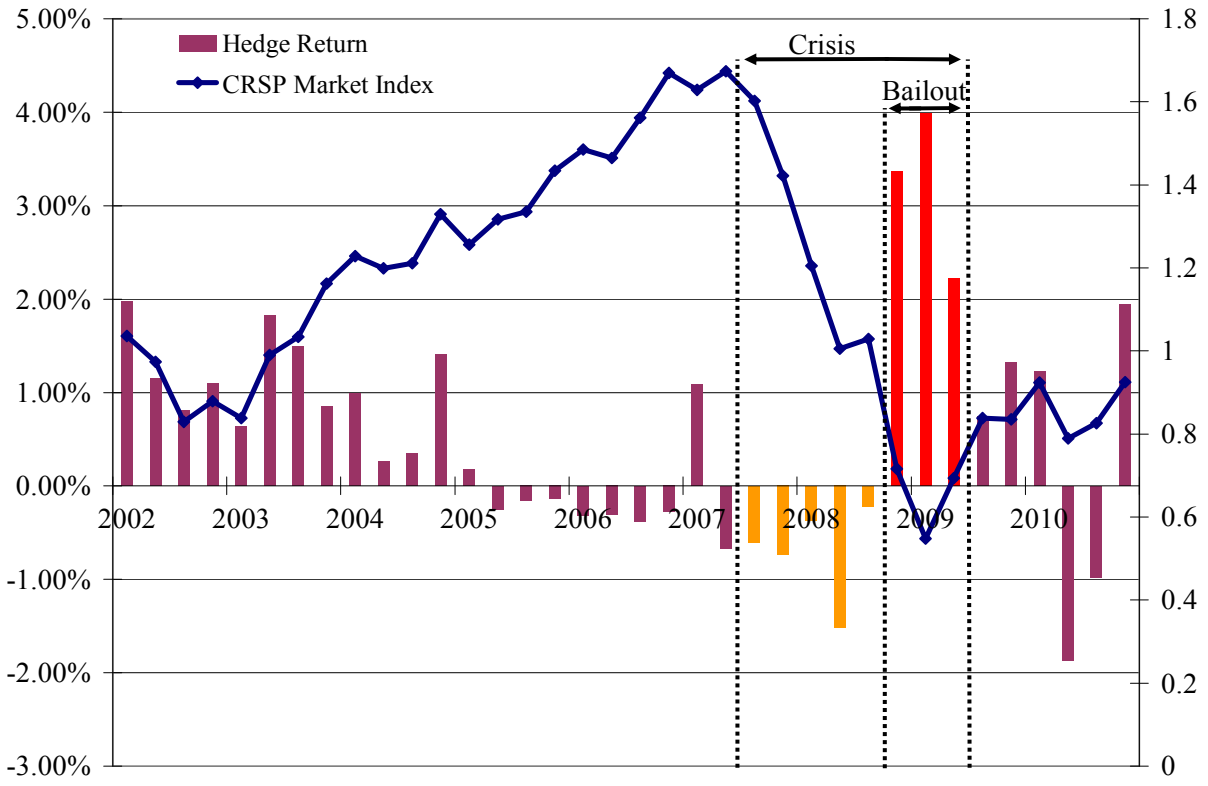
Figure 1. Time-series of Monthly Hedge Portfolio Returns



This figure shows average *monthly* hedge portfolio returns to mimicking insider trades for each calendar quarter from January 2002 through December 2010. Hedge portfolio returns for month *t+1* are calculated purchasing (shorting) stocks where the net purchase of insiders in month *t* is positive (negative). Portfolio returns are calculated assuming equal-weights and rebalancing at the end of each month. The level of the CRSP Market Index is the value of a $1 investment in the CRSP market portfolio at the beginning of January, 2002. We refer to the 12 months prior to the Crisis, July 2006 to June 2007, as the "Pre-crisis period", the 24 months from July 2007 to June 2009 as the "Crisis period," and the 9 months following the announcement of TARP, October 2008 to June 2009, as the "Bailout period."

Table 1. Descriptive Statistics

This table presents descriptive statistics for variables used in our cross-sectional tests. The sample is constructed from the intersection of Thomson Insider (insider trades), Compustat (financial data), and CRSP (stock return data) for the time period January 2002 to December 2010. The sample covers a total of 26,333 firm-months at financial institutions (SIC codes 6000-6999). Panel A presents descriptive statistics for each firm-month, pooling across all observations in the samples. *AbRet* is the market-adjusted buy and hold return in month *t*+1 (in percent). *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 most recent previous quarterly financial statement. *PastMoRet* is the market-adjusted return in month *t*–1 (in percent). *PastYrRet* is the market-adjusted buy-and-hold return over the past year from month *t*–2 to *t*–12 (in percent). BSI_t is the buy-sell imbalance, measured as insider net purchases in month *t* scaled by the number of shares bought plus the number of shares sold in month *t*. NPR_t is the net purchase ratio, measured as insider net purchases in month *t* as a percentage of shares outstanding. $Buyer_t$ is an indicator variable equal to one if the number of shares bought by insiders exceeds the number of shares sold by insiders in month *t* and zero otherwise. Panel B presents the mean level of the insider trading variables by time period. *PreCrisisPd* is an indicator variable equal to one during the period July 2006 to June 2007. *CrisisPd* is an indicator variable equal to one during the period July 2007 to June 2009, and *BailoutPd* is an indicator variable equal to one during the period October 2008 to June 2009. Panel C presents cross-sectional correlations between insider trading in month *t* and abnormal returns in month *t*+1 by time period ($\rho(AbRet, BSI)$, $\rho(AbRet, NPR)$, $\rho(AbRet, Buyer)$). Each cross-sectional correlation represents the slope coefficient from a regression of abnormal returns in month *t*+1 on the respective measures of insider trading in month *t* estimated during the respective period.

Panel A. Pooled Sample

Variable	Mean	Std	P25	P50	P75
AbRet	0.31	10.36	–4.36	–0.12	4.32
Size	6.28	1.98	4.78	6.18	7.60
BM	0.75	1.33	0.43	0.60	0.87
PastMoRet	0.23	10.52	–4.36	0.08	4.67
PastYrRet	6.91	43.67	–13.55	2.35	22.15
BSI	–0.22	0.96	–1.00	–1.00	1.00
NPR	–0.04	0.16	–0.05	–0.01	0.01
Buyer	0.39	0.49	0.00	0.00	1.00

Table 1 (cont'd)

Panel B. Insider Trading by Period

	Period									
Label	Baseline		Pre-Crisis		Crisis: Pre-Bailout		Crisis: Post-Bailout		Baseline	
Calendar Period	Jan 2002 to June 2006		July 2006 to June 2007		July 2007 to Sept 2008		Oct 2008 to June 2009		July 2009 to December 2010	
Variable	Mean	p–value	Mean	p–value	Mean	p–value	Mean	p–value	Mean	p–value
BSI	–0.33	[<0.01]	–0.39	[<0.01]	0.10	[<0.01]	0.23	[<0.01]	–0.19	[<0.01]
NPR	–0.05	[<0.01]	–0.05	[<0.01]	–0.01	[0.04]	0.003	[0.43]	–0.02	[<0.01]
Buyer	0.34	[<0.01]	0.31	[<0.01]	0.55	[<0.01]	0.61	[<0.01]	0.40	[<0.01]

Panel C. Cross–Sectional Correlations by Period

	Period									
Label	Baseline		Pre-Crisis		Crisis: Pre-Bailout		Crisis: Bailout		Baseline	
Calendar Period	Jan 2002 to June 2006		July 2006 to June 2007		July 2007 to Sept 2008		Oct 2008 to June 2009		July 2009 to December 2010	
Variable	Mean	p–value	Mean	p–value	Mean	p–value	Mean	p–value	Mean	p–value
ρ(AbRet, BSI)	0.38	[<0.01]	–0.12	[0.30]	–0.47	[0.04]	1.86	[<0.01]	0.18	[0.39]
ρ(AbRet, NPR)	1.71	[<0.01]	–0.68	[0.37]	–3.57	[0.02]	8.50	[<0.01]	4.00	[<0.01]
ρ(AbRet, Buyer)	0.74	[<0.01]	–0.20	[0.40]	–0.94	[0.04]	3.74	[<0.01]	0.31	[0.77]

Table 2. Portfolio Returns

This table presents average returns and average risk-adjusted returns on portfolios that mimic insider trades. Stocks are grouped into one of two portfolios each month based on whether net purchase of firm insiders in month *t* is positive (*Net Buy*) or negative (*Net Sell*). Stocks are then held over the following month (month *t*+1). Portfolios are rebalanced at the end of each month based on new insider trading data. The hedge portfolio is constructed by purchasing the *Net Buy* portfolio and shorting the *Net Sell* portfolio. Portfolio returns are calculated separately assuming equal-weights (Panel A) and value-weights (Panel B). We report results from estimating the following time-series regressions for each portfolio:

$$R^p_t - R^f_t = \alpha_0 + \beta\ Controls + \alpha_1 PreCrisisPd_t + \alpha_2 CrisisPd + \alpha_3 BailoutPd + \varepsilon_{p,t}$$

where R^p (R^f) is the portfolio return (risk-free rate) in month *t* expressed in percent, *Controls* is vector of control variables including the three Fama-French factors and momentum factor expressed in percent (*MKTRF, SMB, HML, UMD*), *PreCrisisPd* is an indicator variable equal one during the 12 months leading up to the Crisis (July 2006 and June 2007) and zero otherwise, *CrisisPd* is an indicator variable equal one during the 24 month Crisis period (July 2007 to June 2009) and zero otherwise, and *BailoutPd* is an indicator variable equal one during the Crisis period following the announcement of TARP (October 2008 to June 2009) and zero otherwise. For parsimony we do not tabulate coefficients on controls. *t*-statistics based on Newey-West standard errors appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. Equal-weighted portfolio returns

	Raw returns			Risk-adjusted Returns		
Variables	Net Buy	Net Sell	Hedge: Buy–Sell	Net Buy	Net Sell	Hedge: Buy–Sell
Intercept	**1.71**	**1.11**	**0.60**	**0.65**	–0.04	**0.69**
	(4.00)	(3.60)	(2.76)	(1.99)	(–0.17)	(3.35)
PreCrisisPd	**–2.40**	**–1.75**	**–0.65**	**–1.62**	**–1.00**	**–0.62**
	(–2.26)	(–1.82)	(–2.65)	(–3.47)	(–2.55)	(–2.55)
CrisisPd	**–4.79**	**–3.50**	**–1.29**	**–1.90**	–0.57	**–1.33**
	(–5.04)	(–4.14)	(–3.92)	(–2.56)	(–1.12)	(–3.58)
BailoutPd	**5.03**	1.15	**3.88**	**1.63**	**–1.31**	**2.94**
	(2.20)	(0.49)	(7.57)	(1.74)	(–2.17)	(5.29)
Controls included	No	No	No	Yes	Yes	Yes
N	108	108	108	108	108	108

Panel B. Value-weighted portfolio returns

	Raw returns			Risk-adjusted Returns		
Variables	Net Buy	Net Sell	Hedge: Buy–Sell	Net Buy	Net Sell	Hedge: Buy–Sell
Intercept	**1.62**	**1.09**	**0.53**	0.47	–0.06	**0.53**
	(4.21)	(3.49)	(2.74)	(1.49)	(–0.28)	(3.21)
PreCrisisPd	**–2.24**	**–1.65**	**–0.59**	**–1.41**	**–0.96**	**–0.45**
	(–1.97)	(–1.68)	(–2.24)	(–2.76)	(–2.42)	(–2.02)
CrisisPd	**–4.60**	**–3.68**	**–0.92**	**–1.51**	–0.62	**–0.89**
	(–4.81)	(–4.08)	(–2.57)	(–2.08)	(–1.29)	(–2.38)
BailoutPd	**4.78**	1.65	**3.13**	1.36	**–0.93**	**2.29**
	(2.10)	(0.69)	(6.76)	(1.36)	(–1.73)	(3.45)
Controls included	No	No	No	Yes	Yes	Yes
N	108	108	108	108	108	108

Table 3. Future Return Regressions

This table presents results from regressions of future abnormal returns on control variables and directional measures of insider trading in the current month.

$$AbRet_{i,t+1} = \delta_0 + \theta\ Controls_{i,t} + \delta_1 IT_{i,t} + \delta_2 IT_{i,t}*PreCrisisPd_t + \delta_3 IT_{i,t}*CrisisPd_t + \delta_4 IT_{i,t}*BailoutPd_t + \varepsilon_{i,t+1}$$

where the dependent variable ($AbRet_{t+1}$) is the market-adjusted return in month t+1 expressed in percent. *Controls* is vector of control variables including the natural log of market value (*Size*), the book-to-market ratio (*BM*), the market-adjusted return in month t-1 (*PastMoRet*), and the market-adjusted buy-and-hold return over the past year from month t–2 to t–12 (*PastYrRet*). *PreCrisis*, *Crisis*, and *BailoutPd* are main effects, and IT_t is one of three directional measures of insider trading activity in month t, either *BSI*, *NPR*, or *Buyer*. BSI_t is the buy-sell imbalance, measured as insider net purchases in month t scaled by the number of shares bought plus the number of shares sold in month t. NPR_t is the net purchase ratio, measured as insider net purchases in month t as a percentage of shares outstanding. $Buyer_t$ is an indicator variable equal to one if the number of shares bought by insiders exceeds the number of shares sold by insiders in month t and zero otherwise. All other variables are as previously defined. For ease of interpretation, control variables are ranked into quintiles and scaled to range from 0 to 1. t-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

	Directional Measure of Insider Trading (*IT*)					
	IT = BSI		*IT = NPR*		*IT = Buyer*	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Size	0.20 (0.97)	0.22 (1.05)	–0.01 (–0.01)	0.01 (0.07)	0.20 (0.93)	0.21 (1.01)
BM	**0.54** (2.61)	**0.56** (2.70)	**0.59** (2.77)	**0.60** (2.85)	**0.55** (2.62)	**0.56** (2.71)
PastMoRet	**–0.51** (–2.64)	**–0.55** (–2.80)	**–0.53** (–2.73)	**–0.54** (–2.78)	**–0.52** (–2.66)	**–0.55** (–2.80)
PastYrRet	**0.86** (4.23)	**0.84** (4.17)	**0.86** (4.25)	**0.87** (4.31)	**0.86** (4.22)	**0.84** (4.18)
PreCrisisPd	. .	**–2.40** (–16.26)	. .	**–2.38** (–16.77)	. .	**–1.99** (–12.78)
CrisisPd	. .	**–0.85** (–3.58)	. .	**–0.91** (–3.79)	. .	**–0.07** (–0.22)
BailoutPd	. .	0.10 (0.21)	. .	0.56 (1.12)	. .	**–2.26** (–3.58)
IT	**0.26** (3.45)	**0.32** (4.12)	**1.29** (2.54)	**1.82** (3.10)	**0.50** (3.35)	**0.60** (3.95)
*IT*PreCrisisPd*	. .	**–0.44** (–2.90)	. .	**–2.56** (–2.65)	. .	**–0.80** (–2.67)
*IT*CrisisPd*	. .	**–0.79** (–3.34)	. .	**–5.55** (–3.05)	. .	**–1.55** (–3.31)
*IT*BailoutPd*	. .	**2.35** (4.95)	. .	**12.17** (3.72)	. .	**4.72** (4.96)
F	9.14	30.99	8.15	29.12	8.93	30.93
N	26,333	26,333	26,333	26,333	26,333	26,333

Table 4. Future Return Regressions: Multi-Month Evidence

This table reports regressions of future abnormal returns, measured over different horizons, on directional measures of insider trading in the current month and control variables. The regression specification follows Table 2, except that the dependent variable is market-adjusted returns cumulated over the subsequent *s* months. Specifically the dependent variable is either market-adjusted buy-and-hold returns over the next three months (s = 3), the next six months (s = 6) or the next twelve months (s = 12), all measured in percent. All other variables are as previously defined. *t*-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

	Directional Measure of Insider Trading (*IT*)								
Measurement Interval for Future Returns:	*IT = BSI*			*IT = NPR*			*IT = Buyer*		
	3 mos.	*6 mos.*	*12 mos.*	*3 mos.*	*6 mos.*	*12 mos.*	*3 mos.*	*6 mos.*	*12 mos.*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Size	0.33	1.12	**2.61**	–0.11	0.38	1.86	0.32	1.13	**2.62**
	(0.80)	(1.51)	(1.86)	(–0.27)	(0.51)	(1.29)	(0.79)	(1.50)	(1.85)
BM	–0.15	0.05	1.02	–0.11	0.20	1.02	–0.16	0.04	0.99
	(–0.35)	(0.07)	(0.63)	(–0.25)	(0.25)	(0.63)	(–0.36)	(0.05)	(0.61)
PastMoRet	0.13	0.14	0.64	0.18	0.17	0.75	0.14	0.15	0.65
	(0.43)	(0.32)	(0.91)	(0.59)	(0.38)	(1.10)	(0.44)	(0.34)	(0.93)
PastYrRet	**1.29**	**3.02**	**5.26**	**1.42**	**3.18**	**5.57**	**1.30**	**3.05**	**5.29**
	(2.97)	(4.10)	(3.79)	(3.25)	(4.28)	(4.12)	(3.00)	(4.14)	(3.83)
PreCrisisPd	**–6.82**	**–14.62**	**–24.04**	**–6.62**	**–14.15**	**–23.73**	**–5.29**	**–11.75**	**–21.33**
	(–20.65)	(–23.56)	(–21.16)	(–21.39)	(–24.27)	(–21.21)	(–15.23)	(–17.26)	(–16.66)
CrisisPd	**–2.80**	**–6.15**	**–10.19**	**–2.87**	**–6.16**	**–10.31**	–0.91	**–3.56**	**–6.14**
	(–5.68)	(–7.67)	(–8.69)	(–5.76)	(–7.65)	(–8.62)	(–1.36)	(–3.45)	(–4.35)
BailoutPd	**–1.87**	**–4.28**	–4.16	–1.04	–3.04	–2.95	**–6.43**	**–11.07**	**–11.90**
	(–1.88)	(–2.38)	(–1.48)	(–1.00)	(–1.61)	(–1.01)	(–5.37)	(–5.81)	(–4.15)
IT	**0.90**	**1.51**	**1.79**	**4.50**	**6.58**	**8.03**	**1.75**	**2.93**	**3.46**
	(5.58)	(5.24)	(3.26)	(4.22)	(3.61)	(1.86)	(5.50)	(5.16)	(3.20)
*IT*PreCrisisPd*	**–1.56**	**–2.93**	**–2.79**	**–6.82**	**–11.33**	**–13.11**	**–3.06**	**–5.72**	**–5.37**
	(–4.78)	(–4.96)	(–2.65)	(–3.17)	(–3.26)	(–1.95)	(–4.77)	(–4.94)	(–2.60)
*IT*CrisisPd*	**–1.97**	**–2.72**	**–4.17**	**–8.13**	**–14.09**	**–17.80**	**–3.81**	**–5.19**	**–8.09**
	(–4.16)	(–3.68)	(–3.89)	(–2.36)	(–3.03)	(–2.62)	(–4.07)	(–3.58)	(–3.84)
*IT*BailoutPd*	**4.48**	**6.45**	**7.03**	**22.49**	**38.97**	**50.95**	**9.08**	**13.44**	**15.22**
	(4.97)	(4.25)	(3.11)	(3.25)	(3.90)	(3.40)	(5.08)	(4.52)	(3.49)
F	46.47	61.48	54.01	44.90	61.35	53.36	47.02	62.69	55.24
N	26,333	26,333	26,333	26,333	26,333	26,333	26,333	26,333	26,333

Table 5. Future Return Regressions: Alternative Specifications

This table reports regressions of future abnormal returns on directional measures of insider trading in the current month and control variables. The regression specification follows Table 3, except that firm-fixed effects and date-fixed effects are included in columns (1) though (3), firm-month observations without insider trade are included in columns (4) through (6), and firm-month observations are disaggregated to the executive-month level and regressions are estimated using executive-month observations with executive-fixed effects in columns (7) through (9). All variables are as previously defined. *t*-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

	Alternative specifications								
	Including firm-fixed effects and date-fixed effects			*Including observations with no insider trading*			*Disaggregating to the executive-month level and including executive-fixed effects*		
Directional Measure of Insider Trading (*IT*):	*BSI*	*NPR*	*Buyer*	*BSI*	*NPR*	*Buyer*	*BSI*	*NPR*	*Buyer*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Size	**–7.32** (–6.36)	**–7.44** (–6.53)	**–7.32** (–6.37)	0.00 (0.02)	–0.03 (–0.28)	–0.03 (–0.25)	**–9.53** (–5.98)	**–9.96** (–6.25)	**–9.53** (–5.98)
BM	**2.82** (5.94)	**2.89** (6.14)	**2.83** (5.97)	**0.49** (3.91)	**0.50** (4.00)	**0.50** (3.99)	**3.06** (4.05)	**3.15** (4.14)	**3.06** (4.05)
PastMoRet	**–0.53** (–2.66)	**–0.52** (–2.61)	**–0.53** (–2.67)	**–0.52** (–4.42)	**–0.52** (–4.43)	**–0.51** (–4.39)	**–0.58** (–1.82)	**–0.60** (–1.91)	**–0.58** (–1.82)
PastYrRet	**0.62** (2.82)	**0.65** (2.97)	**0.62** (2.82)	**0.58** (5.25)	**0.58** (5.32)	**0.57** (5.23)	0.16 (0.42)	0.17 (0.44)	0.16 (0.42)
PreCrisisPd	. .	. .	. .	**–2.18** (–26.03)	**–2.18** (–26.42)	**–2.10** (–24.38)	**–1.85** (–7.27)	**–1.65** (–6.44)	**–1.32** (–4.31)
CrisisPd	. .	. .	. .	**–1.11** (–8.37)	**–1.12** (–8.49)	**–1.04** (–7.18)	–0.21 (–0.52)	–0.15 (–0.37)	0.89 (1.55)
BailoutPd	. .	. .	. .	**–0.45** (–1.91)	–0.34 (–1.46)	**–0.75** (–2.87)	0.48 (0.65)	0.86 (1.15)	**–1.89** (–1.93)
IT	**0.50** (4.47)	**3.33** (4.84)	**0.94** (4.35)	**0.25** (3.67)	**1.59** (2.90)	**0.40** (3.15)	**0.97** (4.91)	**3.75** (3.72)	**1.91** (4.85)
*IT*PreCrisisPd*	–0.02 (–0.11)	–1.60 (–1.40)	0.02 (0.07)	**–0.37** (–2.65)	**–2.18** (–2.39)	**–0.72** (–2.69)	**–0.52** (–2.05)	0.70 (0.46)	**–1.04** (–2.03)
*IT*CrisisPd*	**–0.76** (–3.04)	**–5.84** (–2.73)	**–1.46** (–2.97)	**–0.75** (–3.13)	**–5.48** (–3.05)	–0.61 (–1.63)	**–1.12** (–2.99)	–5.97 (–1.58)	**–2.19** (–2.94)
*IT*BailoutPd*	**1.64** (3.33)	**10.35** (3.07)	**3.31** (3.40)	**2.52** (5.08)	**12.35** (3.76)	**3.21** (4.05)	**2.35** (3.35)	**11.90** (2.23)	**4.70** (3.39)
F	17.08	16.92	17.10	70.03	69.02	71.81	14.03	13.24	14.01
N	26,333	26,333	26,333	118,254	118,254	118,254	45,403	45,403	45,403

Table 6. Future Return Regressions: Partitioning by Past Performance

This table reports results from estimating the regression specification in Table 3 after partitioning the sample based on recent past performance. Each month firms are ranked into quintiles based on a measure of past performance. We estimate separate regressions for the each of the extreme quintiles (i.e., the top and bottom 20%) and pooling across the intermediate quintiles (i.e., the middle 60%). The bottom, intermediate, and top quintiles are referred to as Q1, Q3, and Q5 respectively. In Panel A, firms are ranked based on the most recent, previously announced, quarterly earnings surprise, where earnings surprise is calculated as the forecast error from a seasonal random walk model of quarterly earnings scaled by total assets. In Panel B, firms are ranked based on the prior month's abnormal return. For parsimony, coefficients on control variables are not tabulated. All variables are as previously defined. The sample is reduced to 24,701 by the requirement of earnings surprise in the prior quarter. *t*-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. Sort on Prior Quarter Earnings Surprise

	Directional Measure of Insider Trading (*IT*)											
	IT = BSI				*IT = NPR*				*IT = Buyer*			
Sample	Q1	Q3	Q5	Diff Q1 – Q5	Q1	Q3	Q5	Diff Q1 – Q5	Q1	Q3	Q5	Diff Q1 – Q5
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IT	**0.42**	**0.29**	**0.48**	–0.06	**4.31**	**1.74**	1.74	2.57	0.70	**0.57**	**0.95**	–0.25
	(1.75)	(2.38)	(1.77)	(–0.19)	(2.72)	(2.01)	(1.33)	(1.20)	(1.42)	(2.36)	(1.80)	(–0.40)
*IT*PreCrisisPd*	–0.23	–0.30	–0.56	0.33	**–4.82**	–1.85	–0.93	–3.89	–0.26	–0.53	–1.09	0.83
	(–0.59)	(–1.40)	(–0.77)	(0.41)	(–2.37)	(–1.44)	(–0.30)	(–1.06)	(–0.32)	(–1.29)	(–0.76)	(0.52)
*IT*CrisisPd*	–0.90	–0.75	**–0.93**	0.03	**–11.70**	**–5.68**	–1.91	**–9.79**	–1.74	–1.49	**–1.84**	0.10
	(–1.20)	(–1.50)	(–1.95)	(0.03)	(–2.22)	(–2.17)	(–0.74)	(–1.93)	(–1.24)	(–1.51)	(–1.99)	(0.07)
*IT*BailoutPd*	**3.37**	**2.54**	0.62	**2.75**	**14.06**	**10.29**	**10.27**	**3.79**	**6.66**	**5.00**	1.79	**4.87**
	(2.98)	(2.30)	(0.86)	(2.69)	(2.95)	(1.78)	(2.87)	(1.93)	(2.94)	(2.33)	(1.35)	(2.47)
Controls included	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
F	5.93	31.65	2.56		5.97	29.16	2.67		5.86	31.64	2.59	
N	4,891	14,886	4,924		4,891	14,886	4,924		4,891	14,886	4,924	

Table 6. Future Return Regressions: Private Information about the Pricing of Public Information (cont'd)

Panel B. Sort on Prior Month Abnormal Return

	Directional Measure of Insider Trading (*IT*)											
	IT = BSI				*IT = NPR*				*IT = Buyer*			
Sample	Q1	Q3	Q5	Diff Q1 – Q5	Q1	Q3	Q5	Diff Q1 – Q5	Q1	Q3	Q5	Diff Q1 – Q5
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IT	0.29	**0.29**	**0.57**	–0.28	2.82	1.14	**4.07**	–1.25	0.52	0.55	**1.06**	–0.54
	(1.37)	(2.18)	(2.15)	(–0.84)	(1.57)	(1.36)	(2.55)	(–0.51)	(1.20)	(2.13)	(2.02)	(–0.83)
*IT*PreCrisisPd*	**–0.88**	–0.21	–0.32	–0.56	**–5.72**	0.26	**–5.79**	0.07	**–1.55**	–0.37	–0.59	–0.96
	(–2.39)	(–0.88)	(–0.49)	(–0.92)	(–2.27)	(0.20)	(–1.73)	(0.02)	(–2.16)	(–0.77)	(–0.47)	(–0.83)
*IT*CrisisPd*	–0.65	**–0.80**	–0.78	0.13	**–12.63**	**–4.05**	–3.68	–8.95	–1.23	–1.56	–1.64	0.41
	(–0.43)	(–2.00)	(–1.35)	(0.08)	(–2.11)	(–1.66)	(–1.23)	(–1.31)	(–0.42)	(–1.94)	(–1.50)	(0.12)
*IT*BailoutPd*	**4.59**	**2.04**	–0.12	**4.71**	**35.37**	**11.78**	–5.54	**40.91**	**9.97**	**4.02**	–0.36	**10.33**
	(2.13)	(2.66)	(–0.12)	(2.85)	(2.79)	(2.73)	(–1.23)	(3.01)	(2.33)	(2.63)	(–0.19)	(2.08)
Controls included	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
F	8.54	22.08	5.16		8.43	20.85	5.17		8.38	22.14	5.12	
N	4,834	15,024	4,843		4,834	15,024	4,843		4,834	15,024	4,843	

Table 7. Event Study–Anticipation of Government Intervention

This table presents results for an event study of the market reaction to various legislative events related to the Troubled Asset Relief Program (TARP). Sample of seven events. Panel A presents the average return on the respective day for the CRSP market portfolio and the average return on our sample of banks. Panel B presents results from a regression of market-adjusted returns on the event day (*AbRet*) on control variables (*Controls*) and directional measures of insider trading over the prior five trading days ($IT_{(-5,-1)}$). *Controls* is vector of control variables including *Size*, *BM*, *PastMoRet*, and *PastYrRet*. $IT_{(-5,-1)}$ is one of three directional measures of insider trading activity calculated over the prior five trading days, either $BSI_{(-5,-1)}$, $NPR_{(-5,-1)}$, or $Buyer_{(-5,-1)}$. $BSI_{(-5,-1)}$ is the buy-sell imbalance over the five trading days prior to the event, measured as insider net purchases over the prior five days scaled by the number of shares bought plus the number of shares sold over the prior five days, $NPR_{(-5,-1)}$ is the net purchase ratio, measured as insider net purchases over the prior five days as a percentage of shares outstanding. $Buyer_{(-5,-1)}$ is an indicator variable equal to one if the number of shares bought by insiders over the prior five days exceeds the number of shares sold by insiders over the prior five days, and zero otherwise, and all other variables are as previously defined. We estimate the regressions separately for each event and pooling across all events. For parsimony, only coefficients on $IT_{(-5,-1)}$ are tabulated. *t*-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. Average Returns

Event	Date	CRSP Market Return (1)	Banks (2)
Pooled	All	0.18 (0.07)	**0.85** (5.63)
Pooled	All excluding Event #3	1.59 (0.70)	**1.89** (11.84)
#1: First announcement of TARP	9/19/2008	4.59	**5.82** (12.66)
#2: Draft Bill circulated	9/22/2008	–3.69	**–4.12** (–10.97)
#3: Bill fails in the House	9/29/2008	–8.28	**–5.43** (–9.99)
#4: Bill passes the Senate	10/1/2008	–0.57	**1.90** (4.94)
#5: Bill passes the House and is signed by President Bush	10/3/2008	–1.44	**–0.76** (–2.67)
#6: Treasury announces a program to purchase equity in financial institutions	10/13/2008	11.52	**5.72** (13.24)
#7: Paulson formally announces Capital Purchase Program (CPP) and investment in nine large financial institutions	10/14/2008	–0.90	**2.79** (6.18)

Table 7. Event Study–Anticipation of Government Intervention (cont'd)

Panel B. Cross-Sectional Regression

	Directional Measure of Insider Trading		
		Banks	
	BSI Coeff.	*NPR* Coeff.	*Buyer* Coeff.
Event	(1)	(2)	(4)
Pooled (all events)	0.77 (1.08)	8.12 (0.80)	1.41 (1.17)
Pooled (all events, excluding Event #3)	**1.45** (1.88)	14.29 (1.17)	**2.19** (1.71)
#1: First announcement of TARP	0.11 (0.10)	4.05 (0.34)	–0.24 (–0.14)
#2: Draft Bill circulated	0.88 (0.65)	**28.36** (1.98)	2.04 (0.82)
#3: Bill fails in the House	**–3.74** (–2.71)	**–13.99** (–1.67)	**–6.77** (–2.92)
#4: Bill passes the Senate	3.79 (1.32)	13.43 (0.85)	**8.34** (1.74)
#5: Bill passes the House and is signed by President Bush	1.99 (1.09)	8.46 (0.70)	2.55 (0.79)
#6: Treasury announces a program to purchase equity in financial institutions	–1.19 (–0.54)	–5.09 (–0.65)	–0.46 (–0.17)
#7: Paulson formally announces Capital Purchase Program (CPP) and investment in nine large financial institutions	2.50 (0.85)	0.38 (0.19)	3.88 (0.95)

Table 8. Event Study–Anticipation of TARP infusions

This table presents results for an event study of the market reaction to TARP infusions. The sample consists of 260 capital infusions to 251 unique banks across 31 different calendar dates. Panel A presents the average return on the announcement day for the CRSP market portfolio and the average return for the respective banks receiving a TARP infusion. Panels B present results from a cross-sectional regression of market-adjusted returns on the announcement day ($AbRet_0$) on control variables (*Controls*) and one of three directional measures of insider trading over the prior five trading days ($BSI_{(-5,-1)}$, $NPR_{(-5,-1)}$, or $Buyer_{(-5,-1)}$). In additional specifications, we also include the market-adjusted return over the prior five days ($AbRet_{(-5,-1)}$) and the respective measure of insider trading activity calculated over the twenty five day period ending five days prior to the event day ($IT_{(-30,-6)}$) as controls. All variables are as previously defined. *t*-statistics based on standard errors clustered by firm appear in parentheses. Bold denotes statistical significance at the 0.10 level or less (two-tail).

Panel A. Average Returns

	CRSP Market Return	CPP Banks
Event	(1)	(2)
Announcement day (t = 0)	–0.24	–0.51
	(–1.10)	(–1.07)

Panel B. Cross-Sectional Regression

Dependent variable: $AbRet_0$

	Directional Measure of Insider Trading (IT)					
	BSI		*NPR*		*Buyer*	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Size	–1.48	–2.24	–1.47	–1.78	–1.59	–2.10
	(–1.13)	(–1.62)	(–1.12)	(–1.36)	(–1.22)	(–1.60)
BM	–1.85	–0.41	–0.90	–0.72	–0.92	–0.59
	(–0.55)	(–0.27)	(–0.60)	(–0.49)	(–0.60)	(–0.38)
PastMoRet	0.21	0.01	0.24	0.19	0.22	0.14
	(0.15)	(0.01)	(0.18)	(0.14)	(0.16)	(0.10)
PastYrRet	–2.52	–2.64	–2.15	–2.40	–2.64	–2.73
	(–1.35)	(–1.45)	(–1.17)	(–1.33)	(–1.42)	(–1.50)
$AbRet_{(-5,-1)}$	.	**–3.86**	.	**–3.53**	.	**–3.84**
	.	(–2.83)	.	(–2.64)	.	(–2.82)
$IT_{(-30,-6)}$	.	–0.76	.	–1.50	.	–0.50
	.	(–0.92)	.	(–0.99)	.	(–0.41)
$IT_{(-5,-1)}$	**4.23**	**4.31**	**52.31**	**48.36**	**4.75**	**4.81**
	(2.15)	(2.25)	(11.05)	(9.66)	(2.01)	(2.06)
F	1.52	2.12	33.99	25.22	1.45	2.00
N	260	260	260	260	260	260

Table 9. Anticipation of CPP infusions: Placebo Test

This table presents results from using a placebo test to assess the relation between insider trades and daily returns in the absence of a TARP infusion. For all TARP recipients in our sample, we estimate the relation between insider trades and market-adjusted returns on the announcement day and compare these results to those obtained from estimation the same relation on non-announcement days. Columns (1), (4), and (7) report the respective coefficients and *t*-statistics from a regression of market-adjusted returns on the announcement day (i.e., *Day t* = 0) on insider trading variables and control variables (see Panel B of Table 8). Columns (2), (5), and (8) report results from repeating the same regression, on the same sample of TARP recipients, but pooling across all non-announcement days (i.e., *Day t* ≠ 0) during the Bailout period between October 2008 and June 2009. Columns (3), (6), and (9) report the difference between announcement day and non-announcement day coefficients. Sample consists of 260 capital infusions to 251 unique banks across 31 unique calendar dates and 192 unique non-announcement dates. *t*-statistics appear in parentheses and are based on standard errors clustered by firm. *p*-values appear in brackets and test for a difference in coefficients. Bold denotes statistical significance at the 0.10 level or less (two-tail).

	Directional Measure of Insider Trading (*IT*)								
	IT = BSI			*IT = NPR*			*IT = Buyer*		
	Day $t = 0$	Day $t \neq 0$	Difference	Day $t = 0$	Day $t \neq 0$	Difference	Day $t = 0$	Day $t \neq 0$	Difference
	β	$E[\beta]$	$\beta - E[\beta]$	β	$E[\beta]$	$\beta - E[\beta]$	β	$E[\beta]$	$\beta - E[\beta]$
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Size	–1.48	**–0.21**	–1.27	–1.47	**–0.23**	–1.24	–1.59	**–0.22**	–1.37
	(–1.13)	(–3.83)	[0.33]	(–1.12)	(–4.23)	[0.35]	(–1.22)	(–4.00)	[0.30]
BM	–1.85	**–0.14**	–0.71	–0.90	–0.13	–0.77	–0.92	–0.12	–0.80
	(–0.55)	(–1.71)	[–0.64]	(–0.60)	(–1.60)	[0.61]	(–0.60)	(–1.55)	[0.60]
PastMoRet	0.21	–0.08	0.29	0.24	–0.09	0.33	0.22	–0.08	0.30
	(0.15)	(–1.09)	[0.84]	(0.18)	(–1.25)	[0.81]	(0.16)	(–1.13)	[0.83]
PastYrRet	–2.52	**–0.35**	–2.17	–2.15	**–0.36**	–1.79	–2.64	**–0.35**	–2.29
	(–1.35)	(–4.00)	[0.24]	(–1.17)	(–4.05)	[0.33]	(–1.42)	(–4.06)	[0.22]
$IT_{(-5,-1)}$	**4.23**	**0.35**	**3.88**	**52.31**	**1.84**	**50.47**	**4.75**	**0.42**	**4.04**
	(2.15)	(3.79)	[0.04]	(11.05)	(1.88)	[<0.01]	(2.01)	(3.27)	[0.06]
F	1.52	16.57		33.99	13.30		1.45	16.08	
N-unique firms	251	251		251	251		251	251	
N-unique days	31	192		31	192		31	192	