Essays in Corporate Finance

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ESSAYS IN CORPORATE FINANCE

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by

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The first essay of this dissertation measures the real effect of increases in local deposit supply on local economic outcomes. To identify this effect, I use exogenous variation in local deposit supply from oil and natural gas shale discoveries. A change in deposit supply should have its largest effect on areas where credit supply frictions are the strongest. I find that the effect is strongest in areas dominated by small banks.

The second essay analyzes the investment policies of public and private natural gas firms, and is joint work with Jérôme Taillard. We find that privately held firms are 60% less responsive to natural gas price changes than publicly traded firms. Additionally, we find that private firms do not respond to new shale investment opportunities, whereas public firms do. We believe these results are consistent with private firms having a higher cost of external capital.

The third essay empirically tests whether firms increase risk taking activity when they are close to distress due to the risk taking incentives of equity-holders. I find that firms actually reduce risk taking when they are close to distress, and in the years prior to bankruptcy. This evidence suggests that risk reduction incentives may be more important for the average firm as it gets close to distress.

This dissertation is the product of my work at Boston College, and I benefited significantly from the help of my advisor, Phil Strahan, and my dissertation committee: Edie Hotchkiss, Darren Kisgen, and Jérôme Taillard. I also benefited from the help of the broader finance Faculty at Boston College as well.

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ESSAY 1

Does Local Access to Finance Matter?

Evidence from U.S. Oil and Natural Gas Shale Booms*

Erik Gilje†

Abstract

I use oil and natural gas shale discoveries as a natural experiment to identify where and when local access to finance is economically important for firms. Shale discoveries lead to large unexpected personal wealth windfalls, which cause an exogenous increase in local bank deposits and a positive local credit supply shock. After a credit supply shock, business establishments increase in industries with high external finance requirements relative to industries with low external finance requirements, but only in lending markets dominated by small banks. The relative increase is 7.1% in lending markets dominated by small banks, while there is no change in other lending markets. These results indicate that economically important frictions related to local credit supply have the largest impact on areas dominated by small banks, while these frictions are mitigated in other lending markets.

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1 Introduction

In frictionless financial markets, entrepreneurs and firms should be able to obtain funding for all positive net present value projects. In such a world, changes in local credit supply would have no effect on real outcomes. However, if information or agency frictions interfere with capital mobility then suboptimal outcomes can occur. Existing empirical literature has focused on the real effects of these financing frictions.[1] Understanding exactly when and where these frictions are most important, however, has received much less attention.

There are reasons to believe that the importance of lending market frictions may vary, due to the substantial variation that exists across local lending markets. For example, some lending markets have large multi-market banks that can redeploy capital geographically (Gilje et al. (2013)), while other markets are dominated by small banks that rely on local sources of capital for lending (Houston et al. (1997), Kashyap and Stein (2000), Campello (2002)). Do these differences result in different exposures to lending market frictions? Do these differences have real effects? These questions have direct implications for our understanding of how real outcomes are affected by lending market frictions.

The goal of this study is to identify where and when lending market frictions have the largest influence on real outcomes by measuring the effect of *similar* changes in local credit supply on real outcomes in *different* lending markets. I use a novel source of exogenous variation in local credit supply from oil and natural gas shale discoveries to examine the effect of changes in credit supply on real outcomes. I identify shale discoveries ("booms") at the county level in the seven major shale producing U.S. states between 2003 and 2009 using a unique dataset of 16,731 individual shale wells. Unexpected technological breakthroughs in shale development have caused energy companies to make high payments to individual mineral owners for the right to develop shale discoveries. I find that the increase in individual mineral wealth associated with shale booms raises local bank deposits by 9.3%. These deposits from newly wealthy mineral owners enhance a bank's ability to make new loans,

[1] This literature includes Peek and Rosengren (2000), Petersen and Rajan (2002), Ashcraft (2005), Becker (2007), Khwaja and Mian (2008), Paravisini (2008), Agarwal and Hauswald (2010), Butler and Cornaggia (2011), Chava and Purnanandam (2011), Iyer and Peydro (2011), Schnabl (2011)

resulting in a positive local credit supply shock.

To measure how a shale boom credit supply shock affects real outcomes in a lending market I use a difference-in-differences empirical specification to compare the number of business establishments, my outcome measure, before a boom to after a boom across industries with different external financing requirements.[2] Because both credit supply and credit demand may be changing in a shale boom I focus on within county-year comparisons. Specifically, to identify the causal effect of changes in credit supply I include county-year fixed effects, so that any demand effect which impacts industries similarly in a given county in a given year is controlled for.

I find that after a shale boom, the number of business establishments in industries with high external finance requirements increases 4.6% relative to industries with low external finance requirements.[3] More importantly, for the purposes of this study, this figure varies across different lending markets. I find that the effect of changes in credit supply on local firms is strongly linked with local banking market structure, with areas dominated by small banks benefiting the most from an expansion in local credit supply. Specifically, after a boom the number of business establishments in industries with high external finance requirements increases 7.1% relative to the number with low external finance requirements in counties dominated by small banks, whereas there is no change in other lending markets. This result indicates that cross sectional variation in the impact of credit supply frictions on real outcomes is linked with a lending market's banking structure.

Why might local credit supply be particularly important in counties dominated by small banks? If local banks are large, capital can be redeployed geographically to fund projects. However, if local banks are small it could be more difficult for capital to be redeployed from other areas to be lent locally.[4] Furthermore, small banks are typically more reliant on deposit

[2]A business establishment is an operating address of a firm; a single firm may have multiple business establishments. I use this as my primary outcome measure as it is among the most granular economic data available at the county-year-industry level during the sample period.

[3]I have excluded all economic outcome measures directly related to oil and gas extraction, construction, real estate, and financial services, because economic outcomes for these industries potentially improve due to reasons unrelated to better local credit supply.

[4]Prior research discussing this issue includes Houston et al. (1997) and Jayaratne and Morgan (2000)

funding than large banks, which suggests they may have more challenges in obtaining alternative external capital due to information and agency concerns. Prior research also suggests that small banks may be more adept at lending to "soft" information borrowers (Stein (2002), Berger et al. (2005)). If areas with more small banks have more "soft" information borrowers, the inability of a small bank to obtain outside funding for these types of borrowers would also lead to worse economic outcomes. The results of this paper indicate that the ultimate set of information and agency frictions influencing outcomes are both frictions between borrowers and banks as well as frictions between banks and funding sources.

Non-credit based interpretations of my results may be a concern.[5] For example, some industries could benefit differentially from a shale discovery due to consumer demand shocks, wealth shocks, or other non-credit based shocks associated with a shale discovery. If any of these shocks are correlated with external financing requirements, then a credit supply based interpretation of the results could be problematic. However, for these alternative shocks to alter the interpretation of my empirical design, they would also need to be correlated with the size of a county's local banks. I find no evidence that after booms demand shocks differ across counties with different bank sizes. Specifically, retail sales, a proxy for local demand, increase by similar amounts after booms in counties dominated by small banks as they do in other counties. Additionally, there is no evidence that deposits increase more after booms in counties dominated by small banks than in other counties, as one might expect if demand shocks affected counties differently. More broadly, the empirical design of this paper requires an alternative, non-banking based, interpretation of results to reconcile why outcomes for industries with distinct external financing requirements respond differently after a shale boom, and why these different responses are larger in counties dominated by small banks.

In placebo tests I show that the results of this study are not driven by pre-existing growth

[5] I follow the approach of other studies and focus on economic outcome variables, because detailed bank level loan data is typically unavailable in the United States. Among banks which have all of their branches in a shale boom county, which plausibly suggests that a significant portion of the lending activity reported in Call Report disclosures occurs in a shale county, I do confirm that Commercial and Industrial loans increase after a shale discovery.

trends. I also demonstrate that the main results of this study are not driven by any single industry or industry exposure to economic fluctuations as proxied by industry asset beta. Additionally, I conduct robustness tests related to local banking structure and find that my main results are not driven by changes to local banking markets after a boom, different small bank size definitions, or banks that are part of holding companies.

How are shale booms different than other types of economic growth? I argue that the key differentiator of shale booms is the significant relative increase in local credit supply in shale counties, relative to other types of growth shocks. Because county banking market structure is not randomly assigned, a concern may be that the real outcomes I observe are not driven by a deposit effect, but instead, an omitted variable which affects how certain counties or certain industries respond to economic growth (e.g. rural and underdeveloped areas may respond differently when there is growth). To attempt to identify how this might be influencing my tests, I examine whether non-shale growth shocks affect counties dominated by small banks differently or firms with greater external financing requirements differently. I find no evidence of differential affects linked to county banking market composition or industry external financing requirements in response to non-shale growth shocks. This result is consistent with the credit supply component of shale booms being a key factor for real outcomes, relative to other types of economic growth.

Are banks using shale deposit windfalls to fund positive net present value projects? While difficult to test empirically, there are at least two pieces of suggestive evidence which indicate that banks are not making bad loans. First, an analysis of banks which have all of their operations in shale counties, for which Call Report data may be considered plausibly representative of the loans a bank may be making in a shale county, I find no evidence that a bank's non-performing loan ratio increases after a shale boom. Second, establishments in industries with high external finance requirements represent a smaller portion of the economy in lending markets dominated by small banks. Specifically, in non-shale counties dominated by small banks they comprise 37.8% of all establishments in 2009. In lending markets dominated by small banks that have benefited from a shale boom, this figure is 40.8%. This amount is nearly equal to the 40.7% they comprise in lending markets with a greater presence of large banks. Thus, these additional establishments increase only to an amount similar to their proportion in counties with a greater presence of large banks, the control group, they are not increasing to a level significantly higher than the control group, which might be a cause for concern.

One should be cautioned against interpreting the results of this study as suggesting that the existence of small banks is suboptimal. Due to the type of borrowers small banks may serve, and the potential difference in borrowers in counties dominated by small banks relative to other counties, it is not clear that more big banks would improve outcomes. Alternatively, this study does suggest that improved access to funding in areas dominated by small banks does lead to improved outcomes. The results would suggest that additional tools or innovations which could mitigate information or agency frictions for small banks in obtaining funding, may improve outcomes in areas dominated by small banks.

This study also highlights a bright side, linked to the limited impact of frictions in some lending markets, as areas with a significant presence of large banks are largely unaffected by changes in local credit supply. This suggests that some economically important lending frictions in some places have been mitigated, relative to what prior studies have found (Becker (2007), Peek and Rosengren (2000)).

In Section 2 I provide an overview of the hypothesis tested in this study and the related literature. Section 3 provides detail on my identification strategy and background on my natural experiment. Section 4 discusses data and variable definitions. Section 5 discusses my results, and Section 6 concludes the paper.

2 Hypothesis Development and Related Literature

The underlying research question in this paper: "Does local access to finance matter?" is a dual hypothesis test of two sets of frictions 1) frictions between borrowers and banks 2) frictions between banks and access to funds for lending. Both sets of frictions have to be present for the observed results.

If firms could seamlessly access capital regardless of location, then neither local credit supply, local banking characteristics, nor a local bank's ability to obtain external funds for lending would matter for local economic outcomes. Any local negative credit shock would be counteracted by distant lenders stepping in to fund positive net present value projects. Recent research suggests that geography and distance currently play less of a role in enhancing informational frictions between borrowers and banks due to improved use of information technology. Berger (2003) documents the rise of internet banking, electronic payment technologies, and credit scoring, while Loutskina and Strahan (2009) document the importance of securitization. These advances would suggest a reduced importance of local access to finance, because borrowers can more easily convey information about themselves to banks that are farther away.

Regulatory based frictions in the U.S. have also eroded over time, reducing the importance of distance in lending relationships. Banking deregulation in U.S. states has affected output growth rates (Jayaratne and Strahan (1996)), the rate of new incorporations (Black and Strahan (2002)), the number of firms and firm-size distribution (Cetorelli and Strahan (2006)), and entrepreneurship (Kerr and Nanda (2009)). Additionally, Bertrand et al. (2007) document that banking deregulation in France leads to better allocation of bank loans to firms and more restructuring activity.

If distance does aggravate information based frictions between borrowers and lenders, then local credit supply may matter. In particular, if the cost to overcoming distance related frictions is prohibitive as could be the case with "soft" information borrowers[6], then local credit supply could be important. In this setting, the frictions that a bank faces in obtaining external funding become important for local economic outcomes. Existing literature suggests that bank size is a key characteristic along which frictions in obtaining external capital may vary. Kashyap and Stein (2000) document that monetary policy influences lending for small banks more than for large banks, while Bassett and Brady (2002) document that small banks rely more on deposit funding. Smaller banks also have fewer sources of funding outside a

[6]Small banks may focus more on relationship lending based on "soft" information relative to transaction lending (Berger and Udell (2006)). Sufi (2007) documents that borrowers and lenders are geographically close when information asymmetry is severe.

local area (Houston et al. (1997), Jayaratne and Morgan (2000), Campello (2002)). If small banks need to raise capital externally, while large banks can redeploy capital internally across different geographic regions, then areas with more small banks may have more agency and informational frictions related to obtaining external funding. These bank funding frictions may mean that areas with a higher proportion of small banks could be less likely to have access to funding beyond local deposits.

This paper is also more broadly related to other papers which use natural experiments to document the importance of access to finance for economic outcomes in different settings earlier in the United States (Peek and Rosengren (2000), Ashcraft (2005), Chava and Purnanandam (2011)) and internationally (Khwaja and Mian (2008), Iyer and Peydro (2011), Schnabl (2011), Paravisini (2008)). In other related work, Guiso et al. (2004) use Italian data to document the importance of financial development on new firm entry, competition, and growth. Recent literature has also used natural experiments in the U.S. to document the importance of local access to finance for productivity (Butler and Cornaggia (2011)) and risk-management (Cornaggia (2012)). Additionally, Plosser (2011) uses shale discoveries as an instrument for bank deposits, but focuses on bank capital allocation decisions during financial crises. My contribution differs from these papers in that I identify significant cross-sectional variation in the effect of changes in local credit supply on firms. Characterizing this variation provides insight as to where and when information and agency frictions affect the flow of capital in the banking system and have the largest impact on firms.

3 Identification Strategy: Shale Discoveries

3.1 Natural Gas Shale Industry Background

The advent of natural gas shale development is one of the single biggest changes in the U.S. energy landscape in the last 20 years. According to the U.S. Energy Information Agency, in its 2011 Annual Energy Outlook, there are 827 Trillion Cubic Feet (Tcf) of technically recoverable unproved shale gas reserves in the United States, this estimate is a 72% upward revision from the previous year. 827 Tcf of natural gas is enough to fulfill all of the United

States' natural gas consumption for 36 years. On an energy equivalent basis 827 Tcf represents 20 years of total U.S. oil consumption or 42 years of U.S. motor gasoline consumption. As recently as the late 1990s, these reserves were not thought to be economically profitable to develop, and represented less than 1% of U.S. natural gas production. However, the development of the first major natural gas shale "play" in the United States, the Barnett Shale in and around Fort Worth, TX, changed industry notions on the viability of natural gas shale.

In the early 1980s Mitchell Energy drilled the first well in the Barnett Shale (Yergin (2011)). However, rather than encountering the typical, highly porous, rock of conventional formations, Mitchell encountered natural gas shale. Shale has the potential to hold vast amounts of gas, however, it is highly non-porous which causes the gas to be trapped in the rock. Over a period of 20 years Mitchell Energy experimented with different techniques, and found that by using hydraulic fracturing (commonly referred to as "fracking") it was able to break apart the rock to free natural gas. With higher natural gas prices and the combination of horizontal drilling with "fracking" in 2002, large new reserves from shale became economically profitable to produce. Continued development of drilling and hydraulic fracturing techniques have enabled even more production efficiencies, and today shale wells have an extremely low risk of being unproductive (unproductive wells are commonly referred to as "dry-holes").

The low risk of dry-holes and high production rates have led to a land grab for mineral leases which were previously passed over. Prior to initiating drilling activities a firm must first negotiate with a mineral owner to lease the right to develop minerals. Typically these contracts are comprised of a large upfront "bonus" payment, which is paid whether the well is productive or not, and a royalty percentage based on the value of the gas produced over time. Across the U.S., communities have experienced significant fast-paced mineral booms. For example, the New Orleans' *Times-Picayune (2008)* reports the rise of bonus payments in the Haynesville Shale, which increased from a few hundred dollars an acre to $10,000 to $30,000 an acre plus 25% royalty in a matter of a year. An individual who owns one square mile of land (640 acres) and leases out his minerals at $30,000/acre would receive an upfront one-time payment of \$19.2 million plus a monthly payment equal to 25% of the value of all the gas produced on his lease. The media has dubbed those lucky enough to have been sitting on shale mineral leases as "shalionaires." The significant personal windfalls people have experienced in natural gas shale booms has led to increases in bank deposits in the communities that they live in. Since the first major shale boom in the Barnett (TX), additional booms have occurred in the Woodford (OK), Fayetteville (AR), Haynesville (LA + TX), Marcellus (PA + WV), Bakken (Oil ND), and Eagle Ford (TX).

3.2 Identification Strategy

The booms experienced by communities across the U.S. due to shale discoveries are exogenous to the underlying characteristics of the affected communities (health, education, demographics etc). The exogenous factors driving shale development include technological breakthroughs (horizontal drilling/hydraulic fracturing) and larger macroeconomic forces (demand for natural gas and natural gas prices). Acknowledging the unexpected nature of shale gas development John Watson, CEO of Chevron, stated in a *Wall Street Journal (2011)* interview, that the technological advances associated with "fracking" took the industry "by surprise." The development of shale discoveries is typically undertaken by large publicly traded exploration and production companies that obtain financing from financial markets outside of the local area of the discovery. To track shale development I use a unique data set which has detailed information on the time and place (county-year) of drilling activity associated with shale booms.[7] The exogenous nature of a shale boom and the effect it has on local deposit supply creates an attractive setting for a natural experiment, which I use to identify the importance of local credit supply and local banking market structure.

[7] I use horizontal wells as my key measure of shale development activity. Horizontal drilling is a component of the key technological breakthrough that enables the production of shale resources to be economically profitable. Nearly all horizontal wells in the U.S. are drilled to develop shale or other unconventional oil and gas resources.

3.2.1 Effect of Boom on Deposits

The first step in my analysis is to quantify the deposit shock in shale boom counties. Specifically what is the impact of a shale boom on local deposit supply? In order to do this I estimate the following regression model

$$Deposit_{i,t} = \alpha + \beta_1 Boom_{i,t} + Year\ FE_t + County\ FE_i + \varepsilon_{i,t}$$

$Boom_{i,t}$ is a measure of shale activity, in my tests I use both logarithm of total shale wells, and a binary dummy boom variable to measure the shale boom. $Deposit_{i,t}$ is either the logarithm of deposits summed across all branches in county i at time t or the logarithm of deposits per capita summed across all branches in county i at time t. County fixed effects are included to control for time invariant county effects and year effects are included to account for time-varying effects, these enter the specification in the form of $Year\ FE_t$ (year fixed effect) and $County FE_i$ (county fixed effect). The key variable of interest in this specification is the coefficient β_1, which indicates the change in $Deposit_{i,t}$ attributable to the $Boom_{i,t}$ variable.

A primary concern in my empirical setting may be whether counties with different bank size characteristics experience similar shocks. If a deposit shock were correlated with the underlying banking structure in a county it could suggest problems for my broader empirical tests. To test whether counties with different banking characteristics are affected differently by the deposit shock, I estimate the following regression:

$$\begin{gathered} Deposit_{i,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 Small\ Bank_{i,t} \\ +\beta_3 Small\ Bank_{i,t} * Boom_{i,t} + Small\ Bank_{i,t} * Year\ FE_t + County\ FE_i + \varepsilon_{i,t} \end{gathered}$$

The key coefficient of interest in measuring whether counties with different bank size characteristics experience different deposit shocks is the interaction coefficient (β_3). This specification includes both $Small\ Bank_{i,t} * Year\ FE_t$ to control for differing deposit trends across counties with different banking structures and $County\ FE_i$ to control for time invariant county effects on deposit levels.

3.2.2 Effect of a Change in Credit Supply on Firms: Difference-in-Differences

To identify the economic outcomes related to the local credit supply shock, I use a regression specification which distinguishes between economic outcomes for industries with high external financing requirements relative to those with low external financing requirements. To achieve this aim, I use a regression form of difference-in-differences, where the first difference (β_1) can be thought of as the difference in economic outcomes between boom county-years and non-boom county-years. To identify the effect of the credit component of a boom I incorporate a second difference (β_3), the difference in economic outcomes for industries with high external finance requirements and industries with low external finance requirements.

$$\begin{gathered} Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Boom_{i,t} * High_j \\ + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t} \end{gathered}$$

Where $Establishment_{i,j,t}$ is either the logarithm of the number of establishments in county i and industry group j at time t or the establishments per capita in county i and industry group j at time t. I have grouped establishments into two industry types: one industry group which has high requirements for external finance, for which $High_j = 1$ and one industry group with low requirements for external finance $High_j = 0$.[8] Thus, for every county I have two industry groups, which are delineated by requirements for external finance. I also include three sets of fixed effects. $IndustryYear\,FE_{j,t}$ control for time-varying differences in industry growth, $CountyIndustry\ FE_{i,j}$ control for county specific differences in industry make-up, while $CountyYear\,FE_{i,t}$ absorbs any county-year specific effects (e.g. demand effects) which

[8] $High_j$ is not reported in the regression results because this variable is subsumed by the county-industry fixed effects, $CountyIndustry\ FE_{i,j}$, while $Boom_{i,t}$ is not reported because it is absorbed by $CountyYear\ FE_{i,t}$. The high dimensional fixed effects used for this study are based off of the techniques outlined in Gormley and Matsa (Forthcoming)

might effect firms in both industry groups similarly.

This specification is a regression form of difference-in-differences, with the key variable of interest being the coefficient on the interaction term, β_3. If industries with a high dependence on external finance benefit more from shale booms, β_3 would be positive, which would indicate the importance of the credit supply component of a boom. Alternatively, if local credit supply does not influence local economic outcomes, β_3 would be zero. That is, while the boom may benefit all industries through the coefficient β_1 (overall increased demand for goods and services), there would be no evidence that the credit supply component of a boom enhances local economic outcomes.

3.2.3 Effect of Bank Size and Credit Supply on Firms: Triple Differencing

To estimate the importance of local bank size for local credit supply I use a triple differencing specification. The first two differences are: non-boom county-years vs. boom county-years, high requirements for external finance vs. low requirements for external finance. The third difference tests whether the effect from the first two differences is bigger in areas dominated by small banks: high small bank market share vs. low small bank market share. $Small\,Bank_{i,t}$ is a variable representing small bank market share in county i at time t. To measure small bank market share, $Small\,Bank_{i,t}$, I use both the proportion of branches in a county which belong to small banks as well as a dummy variable for the counties which are above median in small bank branch market share in any given year. The interaction of $Small\,Bank_{i,t}$ with the other terms in the specification yields a regression form of difference-in-difference-in-differences.[9]

[9] $High_j$ is not reported in the regression results because this variable is subsumed by the county-industry fixed effects, $CountyIndustry\ FE_{i,j}$, while $Boom_{i,t}$, $Small\ Bank_{i,t}$, and $Boom_{i,t} * Small\ Bank_{i,t}$ are not reported because they are absorbed by $CountyYear\ FE_{i,t}$

$$Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Small\ Bank_{i,t}$$
$$+\beta_4 Boom_{i,t} * High_j + \beta_5 Boom_{i,t} * Small\ Bank_{i,t} + \beta_6 High_j * Small\ Bank_{i,t}$$
$$+\beta_7 Boom_{i,t} * Small\ Bank_{i,t} * High_j + IndustryTrends\ FE_{j,t}$$
$$+CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

In this regression the key variable of interest is β_7. If industries with higher requirements for external finance benefit more from a local credit supply shock in counties dominated by small banks this coefficient would be positive.

4 Data and Variable Definition

For my panel data set I include the seven states that have experienced shale development activity from 2000 through 2009. These are Arkansas, Louisiana, North Dakota, Oklahoma, Pennsylvania, Texas, and West Virginia. There are 639 counties in these states with at least one bank branch over the sample period. This sample includes counties that have experienced shale booms, as well as counties which have not, and it is these non-boom county-years which serve as a control group in empirical tests. The data is constructed on an annual frequency and compiled from four different sources:

- Well Data (From Smith International Inc.)
- Deposit and Bank Data (From FDIC Summary of Deposits Reports)
- County Level Economic Outcome Data by Industry (Census Bureau, Establishment Data)
- External Finance Requirement Measures (From Compustat)

4.1 Well Data

Well data is used to calculate the $Boom_{i,t}$ variables in the regressions. The well data is obtained from Smith International Inc. which provides detailed information on the time (year), place (county), and type (horizontal or vertical) of well drilling activity. I use horizontal wells as the key measure of shale development activity, as the majority of horizontal wells in the U.S. drilled after 2002 target shale or other unconventional formations. In order to best measure the influence of shale development activity I focus on two different measures.

- $Boom_{i,t} = Dummy_{i,t}$: A dummy variable set to 1 if county i at time t is in the top quartile of all county-years with shale well activity (total shale wells $>$ 17) in the panel dataset. Once the variable is set to 1, all subsequent years in the panel for the county are set to 1. Based on this definition 88.1% of all shale wells are drilled in boom county-years.
- $Boom_{i,t} = Log\,Total\,Shale\,Wells_{i,t}$: The logarithm of the total number of shale wells drilled in county i from 2003 to time t.

Regressions are based on the total shale wells drilled for the year leading up through March. This corresponds to when the County Business Pattern Data are tabulated. Summary statistics on sample states, counties, and well data are presented in Table 1 as well as a detailed list of the shale boom counties used in this study. Figure 1 presents a map of the intensity and location of shale development activity.

4.2 Deposit and Bank Data

Deposit and bank data are obtained from the Federal Deposit Insurance Corporation (FDIC) Summary of Deposit data, which is reported on June 30 of each year and provides bank data for all FDIC-insured institutions. I use the Summary of Deposit data as opposed to data from the Reports of Condition and Income (Call Reports) because Summary of Deposit data provides deposit data at the branch level, while Call Reports only provide data at the bank level. Additionally, Summary of Deposit data provides detailed information on the geographic location of each branch that a bank has, so I can directly observe the branches in boom counties and the banks they belong to. To obtain county level deposit data I sum deposits across all branches in a county. To calculate small bank market share in a county I calculate the proportion of branches in a county which belong to small banks. I define small banks to be banks with assets below a threshold which could cause a bank to be funding constrained. For the results in this paper I use $500 million (year 2003 dollars) as the asset threshold for small banks.[10] Prior literature (Black and Strahan (2002), Jayaratne and Morgan (2000), Strahan and Weston (1998)), has suggested that banks with assets in the $100 million to $500 million range may be funding constrained. In my empirical tests I use two measures of small bank market share. Specifically, I use dummy variables set to 1 for the counties with high small bank branch market share (above median) in each year, and 0 otherwise. Additionally, I also use the ratio of small bank branches to total branches in a county. Summary data for bank and branch variables are provided in Table 2.

4.3 County Level Economic Outcome Data by Industry

Economic outcome variable data by industry was obtained from the County Business Patterns survey, which is released annually by the Census Bureau. It is worth noting, that the survey provides data only at the establishment level, not the firm level, for example, a firm may have many establishments. The survey provides detailed data on establishments and employment in each county, by North American Industry Classification System (NAICS) code as of the

[10] I document that the main results remain statistically significant when using $200 million or $1 billion in assets as the definition of a small bank. The results are also robust to basing this definition on bank holding company assets.

week of March 12 every year. My main results are based on economic outcomes grouped at the two digit NAICS code level, which I match with corresponding Compustat two digit NAICS code external finance requirement measures. More disaggregated NAICS codes (six digit NAICS as opposed to two digit NAICS) provide fewer NAICS code matches to Compustat, which I rely on for external finance requirement measures. I exclude codes 21 (Oil and Gas Extraction), 23 (Construction), 52 (Financials), 53 (Real Estate) because they may be directly influenced by booms. I exclude 99 (Other) due to lack of comparability with Compustat firms.[11]

After matching County Business Pattern data with Compustat external finance requirement measures, I aggregate all industry codes into two industry groups, one with above median requirements for external finance (high) and one with below median requirements for external finance (low). The two digit NAICS code from the County Business Patterns data is used to obtain an external finance requirement measure from Compustat, which is described in more detail in the next subsection. The objective of the matching is to have the cleanest sorting of NAICS codes into high external finance requirement and low external finance requirement bins. Details on the industries in these bins are provided in Table 3.

While the County Business Patterns Survey provides detailed data on establishment counts by industry, employment data may be suppressed, for privacy reasons, if there are too few establishments in a particular industry. Employment data suppression is a particular problem for counties with smaller populations, for this reason the number of observations in employment regressions is reduced. Furthermore, this suppression of employment data makes including employment in the regressions related to small bank market share problematic, as 62% of establishments in high small bank market share counties have employment reporting suppressed, therefore I do not include employment as an outcome variable in my study.

[11] Using three digit NAICS code industries poses two problems 1) There are 71 industries as opposed to 14, so there are far fewer comparable Compustat firms for some industries 2) There was a change in industry categorization that occurred in 2002-2003, which creates problems when constructing a pre-boom control period for booms that occur in 2003 and 2004.

4.4 External Finance Requirement Measures

I use an external finance requirement measure similar to the measure used by Rajan and Zingales (1998). The main difference is that while they use this measure only for manufacturing firms, I use it for all industry groups similar to Becker (2007). Specifically, over the 1999 to 2008 time period for each firm in Compustat I sum the difference between capital expenditures and operating cash flow. I use the time period 1999 to 2008 because these fiscal years, which end in December for most public firms, correspond most closely to March of the following year (2000 to 2009), which is when the county business patterns survey is conducted. By summing over several years the measure is less susceptible to being driven by short term economic fluctuations. I then divide this sum by the sum of capital expenditures. Specifically, for firm n, the measure is calculated as:

$$ExtFinRequirement_n = \frac{\sum_{1999}^{2008}(CapitalExpenditures_{n,t} - OperatingCashFlow_{n,t})}{\sum_{1999}^{2008} CapitalExpenditures_{n,t}}$$

I take the median of this measure to get an industry's external finance requirement. The calculation of this measure for each industry is displayed in Table 3. The underlying assumption in the Rajan and Zingales (1998) measure is that some industries, for technological reasons, have greater requirements for external financing than others. As Cetorelli and Strahan (2006) highlight, using a measure based on Compustat firms may be considered a cleaner measure, relative to the actual loan amounts small private firms may issue, of the true demand for financing of the firms in the sample. The measure is based on public firms in the United States which have among the best access to capital of any firms in the world, therefore the amount of capital used by these firms is likely to be a good measure of an industry's true demand for external financing. Cetorelli and Strahan (2006) further document a correlation between external finance requirement measures constructed from Compustat and those constructed from the Survey of Small Business Finance, providing further support for the use of this measure.

5 Results

5.1 Effect of Shale Booms on Deposit Levels

Table 4 provides regression results of log deposits and log deposits per capita on different shale boom variables. The evidence suggests a causal relationship between shale booms and bank deposits, specifically, that the individual mineral wealth generated by shale booms translates into more bank deposits. In Panel A of Table 4 columns (1) and (2) provide results on different measures of the $Boom_{i,t}$ variable. In each case, the $Boom_{i,t}$ variable is found to have both economic and statistical significance. For example, the dummy variable measure of $Boom_{i,t}$ can be interpreted as a boom increasing local deposits by 9.3%. To put this in context, the average annual growth rate in deposits across all counties from 2000 to 2009 was 4.6%, so a boom county would experience an additional increase of 9.3% (4.6% + 9.3% = 13.9% total increase), or a total increase in deposits roughly triple its average annual increase.

Further tests will focus on comparisons between counties with high small bank market share and low small bank market share. An assumption in this comparison is that both types of counties experience similar deposit shocks. To directly test this assumption I estimate interactions of county bank size characteristics interacted with the shale boom variables. Panel B reports the results of this specification. The key coefficient of interest in assessing whether counties experience different shocks based on their banking structure is the coefficient on the interaction term (β_3). This coefficient is neither economically nor statistically significant, suggesting that counties with different banking structures receive similar deposit shocks.

An additional concern may be that deposits could be rising in anticipation of a boom, or that there could be some spurious correlation in a county during part of the boom period which is causing the result in Table 4. To test the precise timing of the boom relative to deposit growth I replace the boom dummy variable used in Table 4 with dummy variables based on the position of an observation relative to a boom. So, for example, if a boom occurs in 2006 in county i, then the observation in county i in 2003 would receive a t-3 boom dummy, county i observation in 2004 would receive the t-2 boom dummy and so on. I include a set of dummies for each year relative to a boom from t-3 to t+3. Due to limited observations beyond t+3, I group any observations after t+3 with the t+3 dummy (3+). Figure 2 is a graph of the coefficients from this regression, and provides visual evidence that the deposit level does not change substantially until time 0, the first year of the boom. This serves to alleviate concerns regarding whether deposits rise in anticipation of a boom, as well as concerns about possible spurious correlations during part of the boom period.

5.2 Effect of Credit Supply Shock on Firms

In order to estimate the effect of the credit supply shock associated with a shale boom on firms, it is necessary to look at the difference between outcomes for firms in industries with a high requirement for external finance compared to those with a low requirement for external finance. To measure the credit supply effect of a boom, I not only compare firms in different industries, but also include county-year fixed effects in regression specifications, therefore any direct demand effect that both industry groups experience is fully absorbed. Table 5 provides a direct estimate of the effect of the credit supply shock on firms using a regression form of difference-in-differences. The coefficient of interest for assessing whether improved local credit supply plays a role in local economic outcomes is the interaction term $Boom_{i,t} * High_j$. The sign and magnitude of this term indicates whether one industry group is affected disproportionately when there is a credit supply shock. The coefficient on the interaction term is positive and statistically significant in all specifications, suggesting that firms in industries with high external finance requirements benefit more than firms in industries with low external finance requirements. The outcome measures used in the regressions are logarithm of the number of establishments and establishments per capita in each industry group. The economic interpretation of the interaction coefficient in (1) of Table 5 is that, when there is a boom, establishments in industries with high requirements for external finance increase 4.6% relative to establishments in industries with low requirements for external finance. To put this number in context, the average annual increase in establishments of firms in industries with high external finance requirements from 2000 to 2009 is 0.9%. The interpretation of (3) in Table 5 is that there are 3.6 additional establishments per 10,000 people after the credit supply shock in industries with high external finance requirements relative to industries with low external finance requirements. [12]

There may be some concern as to the timing of the boom and changes in local economic outcomes. If establishment levels of low external finance requirement industries and high external finance requirement industries trend differently prior to the boom, they may be poor control/treatment groups. Additionally, if high external finance requirement establishments trend higher well before the boom, it would suggest a problem with my empirical design, as the deposit levels in Figure 2 do not increase until time 0. To directly assess the validity of these concerns I construct a graph similar to Figure 2, but for establishments. Specifically, for each of the industry groups I estimate a regression, but replace the $Boom_{i,t}$ variable with a set of dummy variables based on the time period of an observation relative to a boom for any given county i (similar to what is done in Figure 2). The coefficients from this regression are graphed for each industry group in Figure 3. As can be seen, from time t-3 to t-1, each industry group tracks relatively closely, then at time 0, the first year of a boom, there is a divergence in trends, which increases through t+3. This indicates that when the boom occurs, establishments in high external finance requirement industries benefit disproportionately more compared to low external finance requirement industries. The evidence presented in Figure 3 should serve to address concerns regarding the change in establishment levels relative to the precise timing of a boom.

5.3 Effect of Bank Size and Credit Supply on Firms

As previously discussed, local bank size composition could play a role in the importance of improved local credit supply for economic outcomes. Specifically, counties dominated by small

[12] I document in Appendix A that for banks that have all branches in a single county, both deposits and Commercial & Industrial loans increase after a boom. Overall interest income and interest paid on deposits are unchanged after a boom. Lending driven purely by demand would be more likely to result in higher interest rates and interest income.

banks may benefit more from a credit supply shock due to information and agency frictions in the banking system. To test this in a difference-in-differences framework, I subdivide counties into high small bank market share and low small bank market share counties, based on whether a county is above median in small bank market share in a given year. I estimate the specification presented in Table 5 for each of these subgroups, and report the results in Table 6.

In every specification the counties dominated by small banks have a higher coefficient for the interaction term $Boom_{i,t} * High_j$. The magnitude of the difference is often quite large, with high small bank market share counties (Bank = High Small Bank Mkt Share) having coefficients four to five times higher than the coefficients of low small bank market share counties (Bank = Low Small Bank Mkt Share), depending on the specification. The interaction coefficient for lending markets with low small bank market share is often not statistically significant. The economic interpretation of (1) is that establishments in industries with high requirements for external finance increase 7.1% relative to establishments in industries with low requirements for external finance after a shale boom. While the economic interpretation of (2) is that establishments in industries with high requirements for external finance increase 1.2% relative to establishments in industries with low requirements for external finance, though this difference is not statistically significant. These results indicate that there is significant cross-sectional variation in the effect of changes in credit supply linked to banking market structure. In the absence of frictions changes in local credit supply should not affect local firms, because there is a larger effect of changes in credit supply in counties dominated by small banks, it suggests that these lending markets are where frictions in the banking system are most problematic. Alternatively, in other lending markets, with a greater presence of large banks, there is an economically negligible effect on local firms, which is often not statistically significant. This indicates that the impact of some economically important frictions in the banking system has been reduced in these areas.

In order to address concerns regarding anticipation and spurious correlations, I graph coefficients as in Figure 3, but further subdivide high external finance and low external finance industries by bank size characteristics to form four separate subgroups in Figure 4.

As can be seen, all subgroups trend similarly until time 0, when the subgroup that comprises high external finance requirement industries in high small bank market share counties trends higher.

To formally test the difference in coefficients across specifications in Table 6 and Figure 4, I estimate a regression form of difference-in-difference-in-differences, with the results shown in Table 7. This is done by adding additional interactions with small bank market share variables. The coefficient of interest in these tests is the triple interaction term $Boom_{i,t} * High_j * Small\ Bank_{i,t}$. A positive coefficient on the triple interaction term indicates that industries with high external finance requirements benefit more relative to industries low external finance requirements when there is a boom in an area with high small bank market share compared to other lending markets. Specifically, the interpretation of (1) in Table 7 is that high external finance requirement establishments increase by 6.2% relative to establishments in industries with low requirements for external finance in boom counties dominated by small banks relative to the difference between these industry groups in other boom counties.[13] Across all specifications the coefficient on $Boom_{i,t} * High_j * Small\ Bank_{i,t}$ is positive and statistically significant, providing evidence suggesting that higher small bank market share counties were more affected by economically important frictions in the banking system which may have disrupted the flow of capital. Specifically, if there were no frictions in the banking system to impede the flow of capital, additional deposits from the boom should not disproportionately affect high external finance requirement industries in high small bank market share counties.[14]

The results in Table 7 also address concerns regarding alternative explanations from the prior difference-in-differences tests conducted. An important concern is whether industries with high external finance requirements disproportionately benefit from a boom for a reason other than the credit supply component of a boom. For example, it could be the case

[13] Appendix B documents that similar and statistically significant results are obtained when different bank size and holding company definitions are used. Appendix C documents that similar and statistically significant results are obtained when holding the banking structure constant as of the year prior to the shale discovery.

[14] Appendix D documents that the largest increase in establishments is among establishments with fewer than 10 people, while establishment counts with 10 people or more are unaffected.

that high external finance requirement industries benefit more in general when there is an economic boom (high asset beta). However, this explanation would not account for the differential impact experienced in high small bank market share counties relative to other lending markets. An additional concern may be that there could be more demand for goods and services for industries in the high external finance dependence industry group. However, in order for this explanation to be consistent with the results in Table 7, there would also need to be a rationale for why this demand differential is relatively higher in counties with high small bank market share.

5.4 Validity of Experimental Design

5.4.1 Sensitivity of Results to Industry Classifications

A potential concern with my empirical design is whether local economic outcomes for industries with higher requirements for external finance improve relative to outcomes for industries with low requirements for external finance for some reason other than improved local credit supply. The difference-in-difference-in-differences tests help rule out several alternative explanations, however, an additional test of this assumption is included in Table 8. Specifically, for each industry group I calculate a measure of exposure to underlying economic fluctuations, asset beta, using two different asset beta methodologies.

$$\beta_{Asset1} = \frac{\beta_{Equity}}{1 + (1 - Tax\ Rate) * \frac{Debt}{Equity}}$$

$$\beta_{Asset2} = \frac{\beta_{Equity}}{1 + \frac{Debt}{Equity}}$$

The asset betas used are industry median asset betas. If it is the case that the asset betas for each industry group are different it could be cause for concern, as this would suggest that one industry group would be more sensitive to overall fluctuations in an economy. The results in Panel A of Table 8 provide evidence that the high external finance requirement industry group does have a higher asset beta. However, when the two highest asset beta industry groups are dropped from the regressions causing both industry groups to have similar asset betas, as in Panel B of Table 8, the interaction and triple interaction coefficients from the difference-in-differences regression and difference-in-difference-in-differences regression are still positive and statistically significant. This suggests that the difference in underlying asset betas between the groups is not driving my main results. Additionally Table 8 provides evidence that the regression results presented in Table 5 and Table 7 are not being driven by any single industry group in the study.

5.4.2 Non-Shale Growth Shock

Banking market structure is not randomly assigned, therefore, one concern may be that there are omitted factors which affect both a county's banking market structure as well as how certain industries (e.g. those with high external finance requirements) are affected by growth shocks. To attempt to assess whether such omitted factors may be affecting my estimates I conduct a test to assess whether non-shale growth shocks affect one industry group compared to another or one industry group relatively more in counties dominated by small banks. Specifically, in Table 9 I use data from the states immediately adjacent to the seven shale states to test whether non-shale growth shocks or "booms" affect the number of establishments in industries with high external finance requirements differently or the number of establishments in high external finance requirement industries in counties dominated by small banks differently. $Growth\ Shock_{i,t}$ dummy variables are inserted after high growth county-years so that the number of growth shock county years is approximately the same proportion as the number of shale boom county years obtained in the main sample (5% of all county-years). I obtain growth shock years by identifying years which experience a large increase in the number of business establishments, on average these growth shocks result in a 17.6% increase in establishments across all industries, a figure significantly higher than shale booms. The key coefficient of interest to test whether industries with high external finance requirements are affected differentially by these growth shocks is on the interaction term $Growth\ Shock_{i,t} * High_j$, this coefficient is not statistically significant. Additionally, the triple interaction term $Growth\ Shock_{i,t} * High_j * Small\ Bank_{i,t}$ is neither positive nor statistically significant. These results indicate that industries with high external finance requirements and industries with high external finance requirements in counties dominated by small banks are not differentially affected by general economic growth.

The primary difference between the growth shocks identified in Table 9, and the shale growth shocks used in this study is the relative importance of the credit supply component of the growth shock. Specifically, in a shale boom, overall establishments increase by 2.2% with significant variation linked to external finance requirements (documented in Table 5 and Table 7), bank deposits increase by 9.3%, more than four times the overall establishment increase. Alternatively, in the non-shale growth shocks establishments increase overall by 17.6%, while deposits increase by slightly less than half this amount, a deposit change of less than half the establishment increase compared to the more than four times relative increase in shale booms. These results suggest that the credit component of shale booms make shale growth shocks unique from general localized growth shocks.

5.4.3 Pre-existing Trends Placebo Test

An identifying assumption of a natural experiment is whether treatment and control groups would have behaved similarly in the absence of treatment. One way to provide evidence in support of this assumption is to test whether there are differential trends prior to treatment. To directly test whether any of the local economic outcome changes begin prior to a boom, I include dummy variables for the two years prior to the first shale development. These enter the regressions in the form of the $False\ Boom_{i,t}$ variable. As can be seen in the results in Table 10, neither the $False\ Boom_{i,t}$ variable, nor any of the interaction variables are statistically significant. This result provides direct evidence that the changes in economic outcome variables documented in this paper do not occur prior to the onset of shale development activity, and that there are no statistically significant pre-existing trends. Furthermore, because shale discoveries occur in different years in different counties (not just a single event in all counties at the same time), alternative interpretations of results would need to address changes in economic outcomes that happen to coincide with boom events in different locations at different points in time.

5.4.4 Are Demand Shocks from Shale Booms Correlated with Bank Size?

A potential concern for the validity of my empirical design is whether real shocks associated with a shale boom are larger in counties dominated by small banks relative to other counties. If this is the case, my interpretation of my empirical tests may be problematic. To provide evidence to alleviate this concern, I use retail sales data from the Economic Census conducted by the U.S. Census Bureau every 5 years. For this test, I use data on retail sales to proxy for demand in an area. The specific comparison I make is based on the 2002 and 2007 Economic Census data. Using this data I can test whether retail sales increase more in counties dominated by small banks after a boom relative to other counties after a boom. The key coefficient of interest in this test, is the interaction term $Boom_{i,t} * SmallBank_{i,t}$. If this coefficient is greater than 0, it would suggest that retail sales increase more in a county with a particular type of bank structure, and therefore indicate that demand shocks may be different across different counties. As can be seen in the specifications in Table 11, the coefficients on the interaction term $Boom_{i,t} * SmallBank_{i,t}$ are not statistically different from 0, suggesting that demand shocks are not correlated with bank size.

6 Conclusions

The United States has one of the most developed banking systems in the world. Prior research has demonstrated that deregulation, the adoption of lending technology and securitization, have led to improved economic outcomes. However, this paper provides new evidence that, after these improvements, there is significant cross sectional variation in the effect of information and agency frictions in the banking system. To identify this variation I use oil and gas shale discoveries to obtain exogenous variation in local credit supply to document where and when changes in local credit supply have the largest effect on local firms. If capital were able to flow, absent frictions, to fund positive net present value projects, changes in local credit supply would not affect local firms. Given that changes in local credit supply do affect local firms, it suggests that economically important frictions adversely affect the flow of capital in the banking system.

I find that cross sectional variation in the effect of changes in credit supply is strongly linked to local bank size. Areas dominated by small banks experience the biggest benefit, in the form of more business establishments in industries with greater external financing requirements, indicating that these lending markets suffer the most from information and agency frictions in the banking system. However, this paper also highlights an important bright side, as other lending markets with a greater presence of large banks do not experience changes in economic activity linked to changes in credit supply. This indicates that many of the advances in financial innovation, such as securitization and credit score models, may have served to mitigate economically important frictions in lending in these markets.

The evidence presented in this paper suggests that information and agency frictions in lending affect economic outcomes along two dimensions. In particular, the greater importance of local credit supply in areas dominated by small banks suggests that the combination of small banks facing frictions in obtaining external capital and borrowers in areas dominated by small banks facing frictions in obtaining loans has the biggest overall adverse impact on economic outcomes. These results would suggest that additional tools or innovations which could mitigate information or agency frictions for small banks in obtaining funding, may improve outcomes in areas dominated by small banks.

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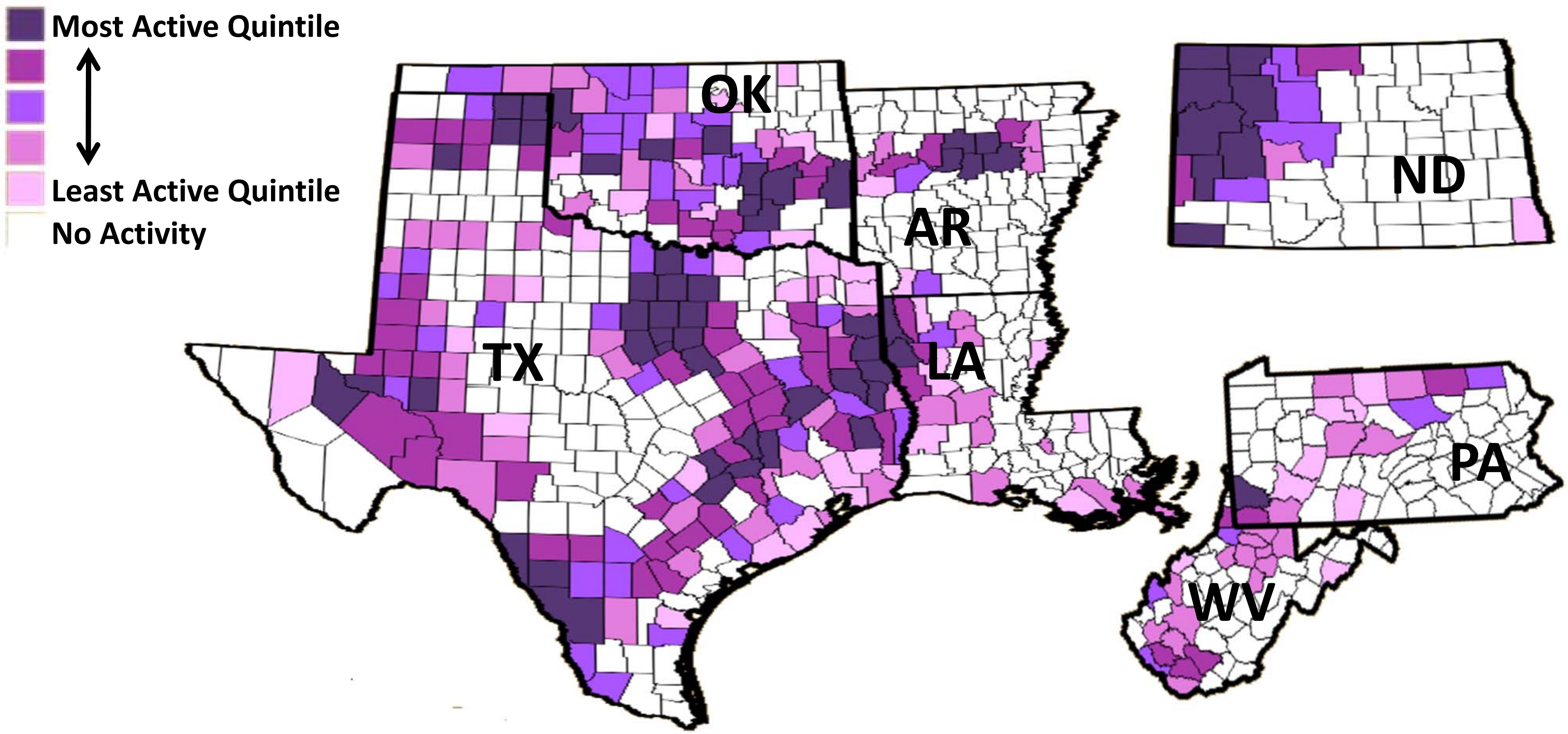

Figure 1: Location and Intensity of Shale Activity
The figure maps the counties of the 7 shale boom states included in this study: OK, TX, LA, WV, PA, ND and AR. White counties are counties with no shale development activity. The remaining counties are shaded based on intensity of activity related to the total number of shale wells drilled through 2009.

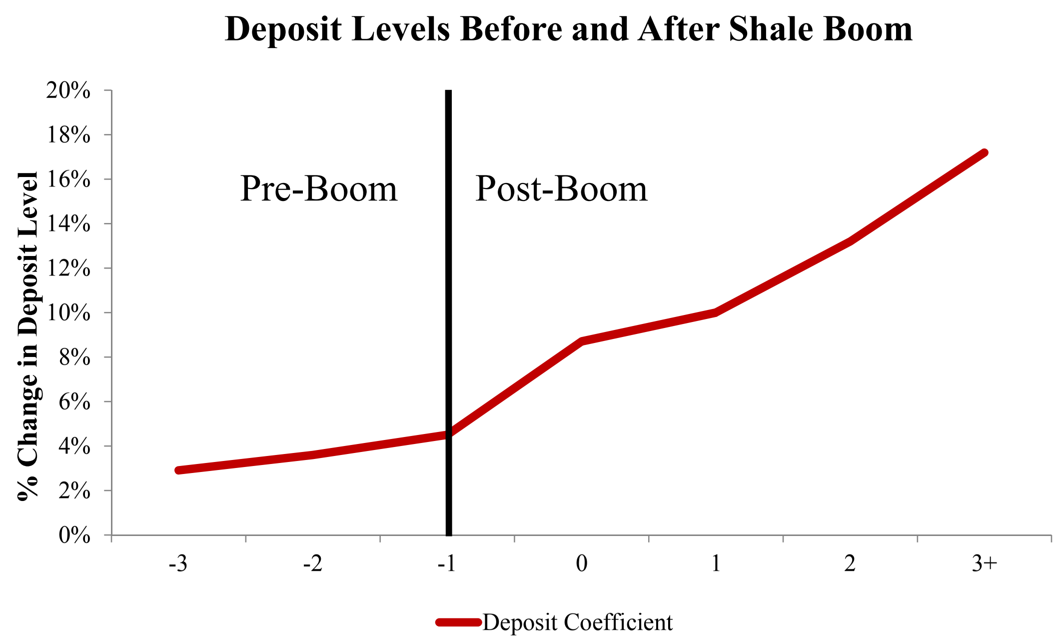

Figure 2: Deposit Levels Before and After Shale Boom

This figure plots the regression of dummy variables based on the year relative to a boom. The first year of a boom is year 0, and the definition of boom that is used is Boom Dummy (previously defined). For example, the first point is the plot of a dummy variable for time t-3 relative to the boom. Due to limited observations for times greater than t+3, all observations after time t+3 are grouped with the t+3 dummy (3+). The dependent variable is the logarithm of total deposits in the county, so the coefficients can be interpreted as the percentage change in the level of deposits at different points in time relative to the boom. Year fixed effects, and county fixed effects were included in the regression as well.

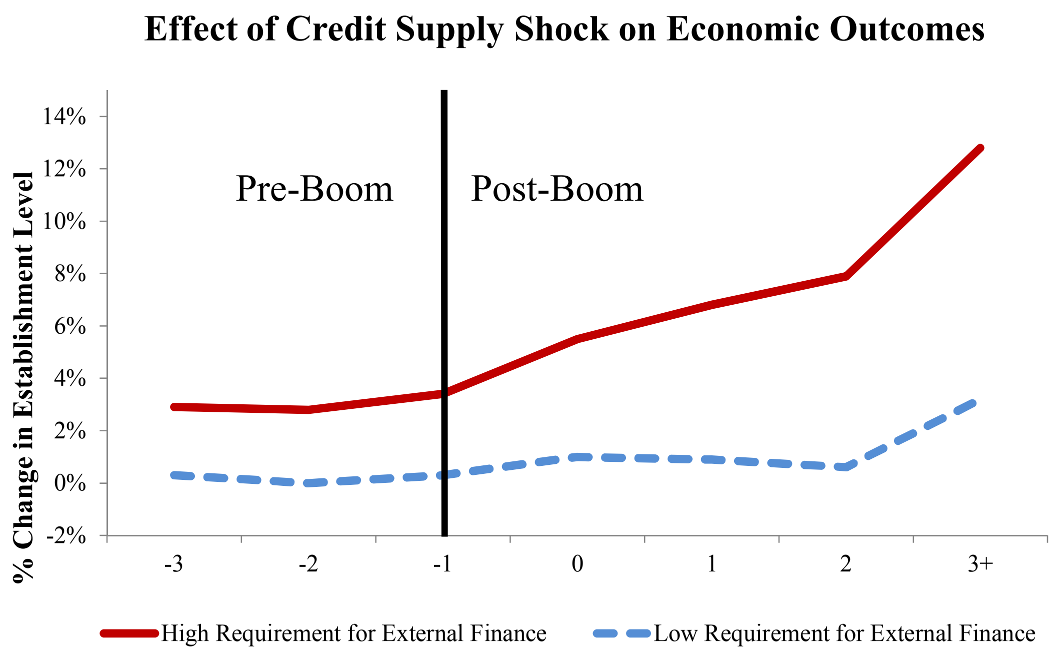

Figure 3: Establishment Levels Before and After Credit Supply Shock

This figure plots separately the regression coefficients of dummy variables of the year relative to a boom for industries with high requirements for external finance and low requirements for external finance. The first year of a boom is year 0, and the definition of boom that is used is Boom Dummy (previously defined). For example, the first point is the plot of a dummy variable for time t-3 relative to the boom. Due to limited observations for times greater than t+3, all observations after time t+3 are grouped with the t+3 dummy (3+). The dependent variable is logarithm of establishments in an industry in a county, so the coefficients can be interpreted as the percentage change in establishment levels at different points in time relative to the boom. Year fixed effects, and county fixed effects were included in the regression as well.

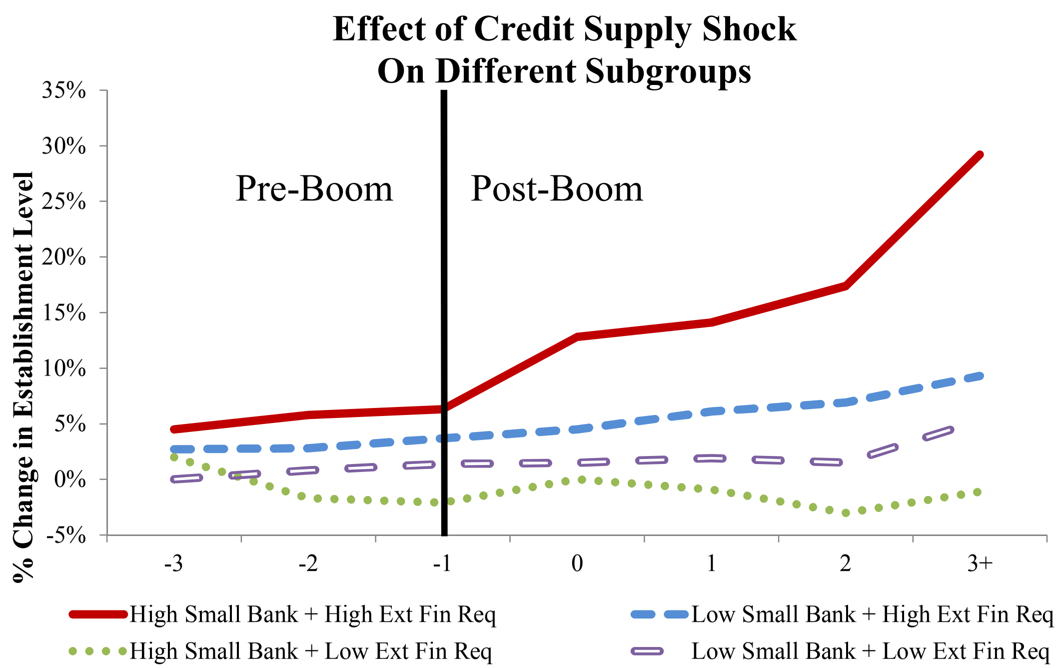

Figure 4: Effect of Credit Supply Shock on Counties with Different Bank Sizes

This figure plots separately the regression coefficients of dummy variables of the year relative to a boom for different subgroups. Specifically four different group designations are used based on whether an establishment has high or low requirements for external finance and whether it is in a county with high or low small bank market share. The first year of a boom is year 0, and the definition of boom that is used is Boom Dummy (previously defined). For example, the first point is the plot of a dummy variable for time t-3 relative to the boom. Due to limited observations for times greater than t+3, all observations after time t+3 are grouped with the t+3 dummy (3+). The dependent variable is logarithm of establishments in an industry in a county, so the coefficients can be interpreted as the percentage change in establishment levels at different points in time relative to the boom. Year fixed effects, and county fixed effects were included in the regression as well.

Table 1: Summary Statistics of States and Counties With Shale Booms

This table contains summary statistics for the well data used in this study. Development of shale and other unconventional formations is done using horizontal drilling, so I use horizontal well activity as the primary method of measuring when and where booms occur. The states in the sample are states situated in the primary shale development areas: Barnett (TX), Woodford (OK), Haynesville (LA + TX), Fayetteville (AR), Marcellus (PA + WV), Eagle Ford (TX), Bakken (ND). Well data was obtained from Smith International Inc.

Panel A: States, Counties, Shale Well Activity

Number of States	7
Number of Counties	639
Number of Boom Counties	104
Total Number of Shale Wells	16,731
Time Period	2000 - 2009

Panel B: Shale Discoveries ("Booms")

	County	Boom Year		County	Boom Year
1	Bowman County, North Dakota	2003	53	Bosque County , Texas	2007
2	Brazos County, Texas	2003	54	Ector County , Texas	2007
3	Moore County, Texas	2003	55	Erath County , Texas	2007
4	Potter County, Texas	2003	56	Hill County , Texas	2007
5	Upton County, Texas	2003	57	Jack County , Texas	2007
6	Washington County, Texas	2003	58	Jasper County , Texas	2007
7	Haskell County, Oklahoma	2004	59	Madison County , Texas	2007
8	Pittsburg County, Oklahoma	2004	60	Midland County , Texas	2007
9	Denton County, Texas	2004	61	Panola County , Texas	2007
10	Fayette County, Texas	2004	62	Somervell County , Texas	2007
11	Grimes County, Texas	2004	63	Webb County , Texas	2007
12	Johnson County, Texas	2004	64	Zavala County , Texas	2007
13	Lipscomb County, Texas	2004	65	Cleburne County , Arkansas	2008
14	Maverick County, Texas	2004	66	Atoka County , Oklahoma	2008
15	Shelby County, Texas	2004	67	Latimer County , Oklahoma	2008
16	Terrell County, Texas	2004	68	Lincoln County , Oklahoma	2008
17	Wise County, Texas	2004	69	Roger Mills County , Oklahoma	2008
18	De Soto County, Louisiana	2005	70	Washita County , Oklahoma	2008
19	Billings County, North Dakota	2005	71	Andrews County , Texas	2008
20	McKenzie County, North Dakota	2005	72	De Witt County , Texas	2008
21	Williams County, North Dakota	2005	73	Edwards County , Texas	2008
22	Le Flore County, Oklahoma	2005	74	Ellis County , Texas	2008
23	Gaines County, Texas	2005	75	Freestone County , Texas	2008
24	Hardeman County, Texas	2005	76	Harrison County , Texas	2008
25	Lee County, Texas	2005	77	Hemphill County , Texas	2008
26	Nacogdoches County, Texas	2005	78	Hutchinson County , Texas	2008
27	Parker County, Texas	2005	79	Karnes County , Texas	2008
28	Pecos County, Texas	2005	80	Lavaca County , Texas	2008
29	Reeves County, Texas	2005	81	Live Oak County , Texas	2008
30	Tarrant County, Texas	2005	82	Montague County , Texas	2008
31	Tyler County, Texas	2005	83	Palo Pinto County , Texas	2008
32	Divide County, North Dakota	2006	84	Polk County , Texas	2008
33	Golden Valley County, North Dakota	2006	85	Robertson County , Texas	2008
34	Coal County, Oklahoma	2006	86	Winkler County , Texas	2008
35	Bee County, Texas	2006	87	Logan County , Arkansas	2009
36	Burleson County, Texas	2006	88	Bossier County , Louisiana	2009
37	Dimmit County, Texas	2006	89	Caddo County , Louisiana	2009
38	Hood County, Texas	2006	90	Red River County , Louisiana	2009
39	Houston County, Texas	2006	91	Sabine County , Louisiana	2009
40	Ochiltree County, Texas	2006	92	Bottineau County , North Dakota	2009
41	Roberts County, Texas	2006	93	Canadian County , Oklahoma	2009
42	Ward County, Texas	2006	94	Carter County , Oklahoma	2009
43	Conway County, Arkansas	2007	95	Johnston County , Oklahoma	2009
44	Faulkner County, Arkansas	2007	96	Marshall County , Oklahoma	2009
45	Van Buren County, Arkansas	2007	97	Greene County , Pennsylvania	2009
46	White County, Arkansas	2007	98	Washington County , Pennsylvania	2009
47	Burke County, North Dakota	2007	99	Cherokee County , Texas	2009
48	Dunn County, North Dakota	2007	100	Dallas County , Texas	2009
49	Mountrail County, North Dakota	2007	101	Leon County , Texas	2009
50	Ellis County, Oklahoma	2007	102	San Augustine County , Texas	2009
51	Hughes County, Oklahoma	2007	103	Wheeler County , Texas	2009
52	Oklahoma County, Oklahoma	2007	104	Wood County , Texas	2009

Table 2: Panel Regression Summary Statistics

This table contains summary statistics for the data used in the panel regressions. The unit of observation for economic outcome variables is business establishment counts in a panel data set at the county-year-industry (external finance industry group) level, while the unit of observation for bank deposits is at the county-year level. Data on establishments is from the County Business Patterns survey. Economic outcome variables are summed across all industries into two groups based on an industry's requirements for external finance. Hence for each county-year there are two industry groups, one with high requirements for external finance and one with low requirements for external finance. Data on annual population levels are from the Census Bureau. Small Banks are categorized as banks with less than $500 million in assets, adjusted for inflation (year 2003 dollars). Bank data was compiled from the FDIC Summary of Deposit reports. Shale well information is based on well data obtained from Smith International Inc.

	Obs	Mean	Std Dev
Deposits			
Log Deposits	6,382	12.60	1.41
Deposits ($ in thousands)	6,382	1,195,253	5,228,131
Economic Outcomes			
Log Establishments	12,764	5.24	1.44
Establishments	12,764	680	2,193
Establishments per Capita (per 10,000 people)	12,764	84.81	34.14
Control/Explanatory Variables			
Log Total Shale Wells	12,764	0.45	1.07
Small Bank Branch Market Share	12,764	0.63	0.30

Table 3: External Finance Requirements of Industries

This table reports the industry groups used in this study. The industry groups are based on the two digit North American Industry Classification System used in the reporting of the County Business Patterns survey, which is reported annually by the Census Bureau. For each industry a measure of dependence on external finance is calculated, based on the method used by Rajan and Zingales (1998). The external finance requirement measure reported for each industry is the industry median requirement for external finance. The data used to calculate the external finance dependence measure is from Compustat for the period from 1999 to 2008 (the fiscal years that are closest to the March data collection of the County Business Patterns survey from 2000 to 2009). The economic outcome measures used are aggregated into two separate industry groups in each county, one with above median dependence on external finance (External Dependence Flag = 1), and one with below median dependence on external finance (External Dependence Flag = 0).

Two Digit NAICS	Two Digit NAICS Name	External Requirement Measure	External Dependence Flag
81	Other Services (except Public Administration)	-0.505	0
42	Wholesale Trade	-0.360	0
62	Health Care and Social Assistance	-0.175	0
44	Retail Trade	-0.127	0
11	Agriculture, Forestry, Fishing and Hunting	-0.079	0
61	Educational Services	-0.012	0
22	Utilities	-0.004	0
48	Transportation and Warehousing	0.071	1
56	Administrative and Support and Waste Management and Remediation Services	0.105	1
72	Accommodation and Food Services	0.183	1
71	Arts, Entertainment, and Recreation	0.418	1
31	Manufacturing	0.475	1
54	Professional, Scientific, and Technical Services	1.023	1
51	Information	1.097	1

Table 4: Effect of Shale Booms on Bank Deposits

This table reports the results of regressions which measure the effect of different boom variables on deposits. The dependent variables in these regressions is the log of total deposits in county i in year t and the log of total deposits per capita in county i year t. The explanatory variables are different shale boom variables, which have previously been defined. County and year fixed effects are included to control for time effects and time invariant county effects. Panel A documents the effect of a shale boom on county bank deposits, while Panel B tests whether there is a differential effect on bank deposits based on local bank size. The Small Bank Dummy variable used in Panel B is equal to 1 if a county has above median small bank branch market share in a given year, and 0 otherwise. Panel B includes the interacted fixed effects of Small $\text{Bank}_{i,t}$ * Year FE_t to control for differing trends in deposits across counties with different banking market structures. The definition of small bank in these regressions is any bank with less than $500 million in assets adjusted for inflation. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Deposits_{i,t} = \alpha + \beta_1 Boom_{i,t} + Year\ FE_t + County\ FE_i + \varepsilon_{i,t}$$

Panel A: Effect of Shale Booms on Bank Deposits

	Dependent Variable = Log Deposits		Dependent Variable = Log Deposits per Capita	
	Boom = Dummy	Boom = Log Total Shale Wells	Boom = Dummy	Boom = Log Total Shale Wells
	(1)	(2)	(3)	(4)
$\text{Boom}_{i,t}$	0.093***	0.026***	0.073***	0.018***
	(4.49)	(4.42)	(3.70)	(3.24)
Year FE_t	Yes	Yes	Yes	Yes
County FE_i	Yes	Yes	Yes	Yes
R^2 - Within	0.631	0.634	0.604	0.604
N	6,382	6,382	6,382	6,382

$$Deposits_{i,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 Small\ Bank_{i,t} + \beta_3 Boom_{i,t} * Small\ Bank_{i,t} + Small\ Bank_{i,t} * Year\ FE_t + County\ FE_i + \varepsilon_{i,t}$$

Panel B: Effect of Shale Boom on Bank Deposits: Counties With Different Bank Sizes

	Dependent Variable = Log Deposits		Dependent Variable = Log Deposits per Capita	
	Small Bank = Dummy		Small Bank = Dummy	
	Boom = Dummy	Boom = Log Total Shale Wells	Boom = Dummy	Boom = Log Total Shale Wells
	(1)	(2)	(3)	(4)
$\text{Boom}_{i,t}$	0.127***	0.029***	0.097**	0.020**
	(3.16)	(2.76)	(2.53)	(2.01)
Small $\text{Bank}_{i,t}$	Absorbed by Small $\text{Bank}_{i,t}$ x Year FE_t			
$\text{Boom}_{i,t}$ * Small $\text{Bank}_{i,t}$	-0.047	-0.002	-0.037	-0.002
	(-1.13)	(-0.17)	(-0.94)	(-0.20)
Small $\text{Bank}_{i,t}$ x Year FE_t	Yes	Yes	Yes	Yes
County FE_i	Yes	Yes	Yes	Yes
R^2 - Within	0.641	0.643	0.608	0.607
N	6,382	6,382	6,382	6,382

Table 5: Effect of Credit Supply Shock on Firms: Difference-in-Differences
(Boom vs. Non-Boom, High Ext Finance Requirements vs. Low Ext Finance Requirements)

This table reports the results of regressions measuring the effect of a credit supply shock on local firms by comparing the change in establishments between two industry groups, one which has high requirements for external finance (Ind = High) and one which has low requirements for external finance (Ind = Low). The dependent variables in these regressions are log establishments and establishments per capita (per 10,000 people). The regression estimates are a regression form of difference-in-differences and test change in establishments before the boom versus after the boom across industries groups with different external finance requirements. County-industry and industry-year (industry trends) fixed effects are included, as well as county-year fixed effects. Note, reported R squared is close to one, due to the inclusion of these high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Boom_{i,t} * High_j + IndustryYearFE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYearFE_{i,t} + \varepsilon_{i,j,t}$$

	Dependent Variable = Log Establishments		Dependent Variable = Establishments per Capita	
	Boom = Dummy	Boom = Log Total Shale Wells	Boom = Dummy	Boom = Log Total Shale Wells
	(1)	(2)	(3)	(4)
$Boom_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$High_j$	Absorbed by County x Industry $FE_{i,j}$			
$Boom_{i,t}$ * $High_j$	0.046***	0.012***	3.629***	0.974***
	(2.67)	(2.61)	(2.71)	(2.78)
Industry x Year $FE_{j,t}$	Yes	Yes	Yes	Yes
County x Industry $FE_{i,j}$	Yes	Yes	Yes	Yes
County x Year $FE_{i,t}$	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.990	0.990
N	12,764	12,764	12,764	12,764

Table 6: Effect of Credit Supply Shock on Firms: Subdivided by Small Bank Market Share
(Boom vs. Non-Boom, High External Finance Requirements vs. Low External Finance Requirements)

This table reports a regression form of difference-in-differences for two different county groups, one county group which has high small bank market share (Bank = High Small Bank Mkt Share), and one county group with low small bank market share (Bank = Low Small Bank Mkt Share). The definition of small bank in these regressions is any bank with less than $500 million in assets adjusted for inflation. For this regression high small bank market share counties are defined to be counties with above median small bank branch market share. The dependent variables in these regressions are log of establishments and establishments per capita (per 10,000 people). The explanatory variables are different shale boom variables, which have previously been defined. Additionally, an interaction between boom variables and the "High" external finance dependence dummy is included, this is the difference-in-differences coefficient of interest (β_3). County-industry and industry-year (industry trends) fixed effects are included, as well as county-year fixed effects. Note, reported R squared is close to one, due to the inclusion of these high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Boom_{i,t} * High_j + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

	Dependent Variable = Log Establishments				Dependent Variable = Establishments per Capita			
	Boom = Dummy		Boom = Log Total Shale Wells		Boom = Dummy		Boom = Log Total Shale Wells	
	High Small Bank Market Share	Low Small Bank Market Share	High Small Bank Market Share	Low Small Bank Market Share	High Small Bank Market Share	Low Small Bank Market Share	High Small Bank Market Share	Low Small Bank Market Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Boom_{i,t}$	Absorbed by County x Year $FE_{i,t}$							
$High_j$	Absorbed by County x Industry $FE_{i,j}$							
$Boom_{i,t}$ * $High_j$	0.071***	0.012	0.019***	0.004*	4.905***	1.443	1.453***	0.329
	(3.10)	(1.09)	(2.86)	(1.73)	(3.03)	(1.35)	(3.16)	(1.46)
Industry x Year $FE_{j,t}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Industry $FE_{i,j}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Year $FE_{i,t}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.997	0.999	0.997	0.999	0.988	0.995	0.988	0.995
N	6,502	6,262	6,502	6,262	6,502	6,262	6,502	6,262

Table 7: Effect of Bank Size and Credit Supply on Firms: Differences-in-Differences-in-Differences Regression

(Boom vs. Non-Boom, High Ext Finance Requirements vs. Low Ext Finance Requirements, High Small Bank Market Share vs. Low Small Bank Market Share)

This table reports results for a regression form of difference-in-difference-in-differences, where the coefficient of interest is the triple interaction term. The dependent variables in these regressions are log establishments or establishments per capita (per 10,000 people) in county *i* , year *t* , industry group *j* . The explanatory variables are different boom variables, which have previously been defined. The definition of small bank in these regressions is any bank with less than \$500 million in assets adjusted for inflation. These specifications provide results for two different measures of Small $\text{Bank}_{i,t}$. One measure is a dummy variable, set to 1 if a county has above median small bank branch market share in any given year and 0 otherwise (Small Bank = Dummy), while the other measure is the ratio of branches which belong to small banks relative to the total number of bank branches in a county (Small Bank = Ratio). Additionally, a set of fully saturated interactions between Boom variables, Small Bank variables, and the High external finance dependence dummy are included. The key coefficient of interest for the difference-in-difference-in-differences regression is the triple interaction term β_7. County-industry and industry-year (industry trends) fixed effects are included, as well as county-year fixed effects. Note, reported R squared is close to one, due to the inclusion of these high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Small\ Bank_{i,t} + \beta_4 Boom_{i,t} * High_j + \beta_5 Boom_{i,t} * Small\ Bank_{i,t}$$
$$+ \beta_6 High_j * Small\ Bank_{i,t} + \beta_7 Boom_{i,t} * Small\ Bank_{i,t} * High_j + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

	Dependent = Log Establishments				Dependent = Establishments per Capita			
	Boom = Dummy		Boom = Log Shale Wells		Boom = Dummy		Boom = Log Shale Wells	
	Bank = Dummy	Bank = Ratio	Bank = Dummy	Bank = Ratio	Bank = Dummy	Bank = Ratio	Bank = Dummy	Bank = Ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Boom}_{i,t}$	Absorbed by County x Year $\text{FE}_{i,t}$							
High_j	Absorbed by County x Industry $\text{FE}_{i,j}$							
Small $\text{Bank}_{i,t}$	Absorbed by County x Year $\text{FE}_{i,t}$							
$\text{Boom}_{i,t}$ * High_j	0.008	-0.065*	0.003	-0.015*	0.742	-3.079	0.207	-0.937
	(0.49)	(-1.78)	(0.84)	(-1.80)	(0.48)	(-1.05)	(0.59)	(-1.49)
$\text{Boom}_{i,t}$ * Small $\text{Bank}_{i,t}$	Absorbed by County x Year $\text{FE}_{i,t}$							
Small $\text{Bank}_{i,t}$ * High_j	-0.010	0.023	-0.013	0.011	-0.319	3.803	-0.656	2.767
	(-1.20)	(0.91)	(-1.49)	(0.44)	(-0.37)	(1.32)	(-0.76)	(0.97)
$\text{Boom}_{i,t}$ * Small $\text{Bank}_{i,t}$ * High_j	0.062**	0.172**	0.015**	0.043***	4.715**	10.378**	1.371***	3.127***
	(2.14)	(2.54)	(2.24)	(2.66)	(2.09)	(2.11)	(2.70)	(2.81)
Industry x Year $\text{FE}_{j,t}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Industry $\text{FE}_{i,j}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Year $\text{FE}_{i,t}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.999	0.999	0.990	0.990	0.990	0.990
N	12,764	12,764	12,764	12,764	12,764	12,764	12,764	12,764

Table 8: Sensitivity of Results to Different Industries

Panel A: This panel reports regression results of the key interaction coefficients of interest when excluding specific industries from the regression results originally reported in Table 5 (column (2) below) and Table 7 (column (3) below). The definition of Boom variable used in these regressions is log total shale wells, and the definition of Small Bank Share is a dummy variable for counties with above median small bank branch market share. Additionally this table reports the asset beta for each industry, using two different methodologies.

Panel B: This panel reports regression results of the key interaction coefficients of interest when excluding the two highest asset beta industries from the regression results originally reported in Table 5 (column (2) below) and Table 7 (column (3) below). The definition of Boom variable used in these regressions is log total shale wells, and the definition of Small Bank Share is a dummy variable for counties with above median small bank branch market share. Columns (6) and (7) report the average asset beta for each industry group when the two highest asset beta industry groups are excluded.

Panel A

Industries		Excluding Industry			Asset Beta By Industry	
(1) Two Digit NAICS	(2) Two Digit NAICS Name	(3) $Boom_{i,t}$ * $High_j$	(4) $Boom_{i,t}$ * Small $Bank_{i,t}$ * $High_j$	(5) Ext Finance Dependence Flag	(6) β_{Asset1}	(7) β_{Asset2}
62	Health Care and Social Assistance	0.011***	0.012*	0	0.64	0.61
42	Wholesale Trade	0.012***	0.015**	0	0.79	0.77
11	Agriculture, Forestry, Fishing and Hunting	0.011**	0.015**	0	0.40	0.42
61	Educational Services	0.012***	0.015**	0	0.86	0.83
81	Other Services (except Public Administration)	0.011**	0.016**	0	0.51	0.49
44	Retail Trade	0.013***	0.019***	0	0.82	0.77
22	Utilities	0.010**	0.014**	0	0.21	0.21
56	Administrative and Support and Waste Management and Remediation Services	0.011***	0.013**	1	0.82	0.80
48	Transportation and Warehousing	0.009**	0.011*	1	0.60	0.58
31	Manufacturing	0.012***	0.012*	1	0.51	0.50
72	Accommodation and Food Services	0.013**	0.018**	1	0.61	0.59
71	Arts, Entertainment, and Recreation	0.013***	0.015**	1	0.69	0.64
54	Professional, Scientific, and Technical Services	0.012***	0.019***	1	1.18	1.19
51	Information	0.012**	0.016**	1	1.39	1.38
	Average for Low Dependence on External Finance				0.60	0.59
	Average for High Dependence on External Finance				0.83	0.81

Panel B

Industries		Excluding Industry			Asset Beta By Industry	
(1) Two Digit NAICS	(2) Two Digit NAICS Name	(3) $Boom_{i,t}$ * $High_j$	(4) $Boom_{i,t}$ * Small $Bank_{i,t}$ * $High_j$	(5) Ext Finance Dependence Flag	(6) β_{Asset1}	(7) β_{Asset2}
51-54	Two Highest Beta Industries (Codes 51 and 54)	0.012**	0.020***	1		
	Average Asset Beta of Low Dependence (Exclude Codes 51 and 54)				0.60	0.59
	Average Asset Beta of High Dependence (Exclude Codes 51 and 54)				0.65	0.62

Table 9: Effect of Non-Shale Growth Shocks on Firms

This table reports estimates of regressions similar to those presented in Tables 5 and 7, except the shocks used are non-shale growth shocks in states adjacent to the seven shale states in this study. Specifically, dummy "Growth Shock" variables are inserted after high growth county-years such that the number of Growth Shock county years is approximately the same proportion of Shale Boom county years obtained in the main sample (roughly 5% of all county-years). The objective of this specification is to test whether general growth shocks differentially affect a particular industry group or a particular set of industries in counties dominated by small banks. The dependent variables in these regressions are log establishments or establishments per capita (per 10,000 people) in county i, year t, industry group j. Note, reported R squared is close to one, due to the inclusion of multiple high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

	Dependent Variable = Log Establishments		Dependent Variable = Establishments per Capita	
	Growth Shock = Dummy		Growth Shock = Dummy	
	Small Bank Share = Dummy		Small Bank Share = Dummy	
	(1)	(2)	(3)	(4)
Growth Shock$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
High$_j$	Absorbed by County x Industry FE$_{i,j}$			
Small Bank$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
Growth Shock$_{i,t}$ * High$_j$	0.002	0.011	-3.591	-3.480
	(0.07)	(0.52)	(-1.54)	(-1.29)
Growth Shock$_{i,t}$ * Small Bank$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
Small Bank$_{i,t}$ * High$_j$		-0.002		-0.468
		(-0.25)		(-0.59)
Growth Shock$_{i,t}$ * Small Bank$_{i,t}$ * High$_j$		-0.027		-0.338
		(-0.83)		(-0.10)
Industry x Year FE$_{j,t}$	Yes	Yes	Yes	Yes
County x Industry FE$_{i,j}$	Yes	Yes	Yes	Yes
County x Year FE$_{i,t}$	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.993	0.993
N	22,932	22,932	22,932	22,932

Table 10: Placebo Test of Pre-Boom Trends

This table reports results of falsification tests for the regressions in Tables 5 and 7. Specifically, dummy variables are inserted for the two years prior to the beginning of shale well activity. The dependent variables in these regressions are log establishments or establishments per capita (per 10,000 people) in county i, year t, industry group j. Note, reported R squared is close to one, due to the inclusion of multiple high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$(1)\ Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_{1F} False\ Boom_{i,t} + \beta_2 High_j + \beta_3 Boom_{i,t} * High_j + \beta_{3F} False\ Boom_{i,t} * High_j + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

$$(2)\ Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_{1F} False\ Boom_{i,t} + \beta_2 High_j + \beta_3 Small\ Bank_{i,t} + \beta_4 Boom_{i,t} * High_j + \beta_{4F} False\ Boom_{i,t} * High_j + \beta_5 Boom_{i,t} * Small\ Bank_{i,t} + \beta_{5F} False\ Boom_{i,t} * Small\ Bank_{i,t} + \beta_6 High_j * Small\ Bank_{i,t} + \beta_7 Boom_{i,t} * Small\ Bank_{i,t} * High_j + \beta_{7F} False\ Boom_{i,t} * Small\ Bank_{i,t} * High_j + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

	Dependent Variable = Log Establishments		Dependent Variable = Establishments per Capita	
	False Boom = Dummy, Boom = Log Total Shale Wells		False Boom = Dummy, Boom = Log Total Shale Wells	
	Small Bank = Dummy		Small Bank = Dummy	
	(1)	(2)	(3)	(4)
$Boom_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$False\ Boom_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$High_j$	Absorbed by County x Industry $FE_{i,j}$			
$Small\ Bank_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$Boom_{i,t} * High_j$	0.012**	0.004	1.046***	0.236
	(2.52)	(0.83)	(2.72)	(0.58)
$False\ Boom_{i,t} * High_j$	0.002	0.001	0.289	0.000
	(0.35)	(0.15)	(0.64)	(0.00)
$Boom_{i,t} * Small\ Bank_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$False\ Boom_{i,t} * Small\ Bank_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$Small\ Bank\ Share_{i,t} * High_j$		-0.013		-0.809
		(-1.60)		(-0.97)
$Boom_{i,t} * Small\ Bank_{i,t} * High_j$		0.016**		1.477**
		(2.19)		(2.54)
$False\ Boom_{i,t} * Small\ Bank_{i,t} * High_j$		0.003		0.624
		(0.28)		(0.58)
Industry x Year $FE_{j,t}$	Yes	Yes	Yes	Yes
County x Industry $FE_{i,j}$	Yes	Yes	Yes	Yes
County x Year $FE_{i,t}$	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.990	0.990
N	12,764	12,764	12,764	12,764

Table 11: Retail Sales Changes in Boom Counties with Different Local Bank Sizes

This table reports the results of regressions which estimate the effect of different boom variables on retail sales. The dependent variable in these regressions is the log of total retail sales by establishments in county i in year t. The regressions test whether there is a differential effect on retail sales based on local bank size. The Small Bank Dummy variable used is equal to 1 if a county has above median small bank branch market share in a given year, and 0 otherwise. The definition of small bank in these regressions is any bank with less than $500 million in assets adjusted for inflation. Retail sales data is from the U.S. Bureau of the Census Economic Census in 2002 and 2007 (conducted every 5 years). Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$\textit{Log Retail Sales}_{i,t} = \alpha + \beta_1 \textit{Log Population}_{i,t} + \beta_2 \textit{Boom}_{i,t} + \beta_3 \textit{Small Bank}_{i,t} + \beta_4 \textit{Boom}_{i,t} * \textit{Small Bank}_{i,t} + \text{Year } \textit{FE}_t + \textit{County FE}_i + \varepsilon_{i,t}$$

Effect of Shale Boom on Retail Sales: Counties With Different Bank Sizes

	Small Bank = Dummy	
	Boom = Dummy	Boom = Log Total Shale Wells
	(1)	(2)
$Boom_{i,t}$	0.048*	0.016***
	(1.65)	(2.67)
$Small\ Bank_{i,t}$	0.021	0.015
	(0.90)	(0.57)
$Boom_{i,t}$ * $Small\ Bank_{i,t}$	-0.064	0.003
	(-0.76)	(0.16)
$Log\ Population_{i,t}$	0.669***	0.661***
	(8.34)	(8.22)
Year FE_t	Yes	Yes
County FE_i	Yes	Yes
R^2	0.670	0.673
N	1,263	1,263

Appendix A: Regressions for Banks with All Branches in One County

This table reports the results of regressions which estimate the effect of different shale boom variables on bank outcomes for banks that have all of their branches in a single county. The unit of observation in this panel is county *i*, bank *j*, year *t*. Data on banks was compiled from Call Reports and Summary of Deposit reports. A bank is in the sample if all of its branches are in a single county in given year, treatment banks are those banks which are in shale boom county-years, while control banks are single county banks in non-shale boom county-years. C&I loans are the total amount of commercial and industrial loans a bank reports on its Call Report. Interest income is the total interest income a bank generates in a year, divided by its average total loans. Deposit Interest Rate is the interest paid on all deposits divided by the average amount of deposits a bank has in a given year. Both interest rate and deposit interest rate variables were winsorized at 1% and 99%. Year fixed effects and bank fixed effects are included. Standard errors are clustered by bank, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$\textit{Bank Outcome}_{i,j,t} = \alpha + \beta_1 \textit{Boom}_{i,t} + \textit{Year FE}_t + \textit{Bank FE}_j + \varepsilon_{i,j,t}$$

Panel A

	Boom = Dummy				
	Outcome = Log Deposits	Outcome = Log C&I Loans	Outcome = Interest Rate	Outcome = Deposit Interest Rate	Outcome = Non-Performing Loan Ratio
	(1)	(2)	(3)	(4)	(5)
$Boom_{i,t}$	0.0960***	0.1072**	0.0004	0.0004	-0.0008
	(4.24)	(2.45)	(0.12)	(1.16)	(-0.91)
Year FE_t	Yes	Yes	Yes	Yes	Yes
Bank FE_j	Yes	Yes	Yes	Yes	Yes
R^2	0.374	0.102	0.399	0.871	0.024
N	8,176	8,176	8,176	8,176	8,176

Panel B

	Boom = Log Total Shale Wells				
	Outcome = Log Deposits	Outcome = Log C&I Loans	Outcome = Interest Rate	Outcome = Deposit Interest Rate	Outcome = Non-Performing Loan Ratio
	(1)	(2)	(3)	(4)	
$Boom_{i,t}$	0.0277***	0.0316***	-0.0007	0.0000	-0.0004**
	(4.37)	(2.69)	(-0.88)	(0.36)	(-2.03)
Year FE_t	Yes	Yes	Yes	Yes	Yes
Bank FE_j	Yes	Yes	Yes	Yes	Yes
R^2	0.376	0.103	0.399	0.871	0.025
N	8,176	8,176	8,176	8,176	8,176

Appendix B: Alternative Thresholds for Small Bank Size

(Boom vs. Non-Boom, High Ext Finance Requirements vs. Low Ext Finance Requirements High Small Bank Market Share vs. Low Small Bank Market Share)

This table reports results for a regression form of difference-in-difference-in-differences, where the coefficient of interest is the triple interaction term. The dependent variable in these regressions is log establishments in county *i* , year *t* , industry group *j* . This table reports specifications using different small bank definitions. The regressions in this table report results for small bank cutoffs at $200 million, $500 million, and $1 billion in assets adjusted for inflation. Additionally, the specifications report results for assets measured both at the bank level and at the bank holding company level. Small $\text{Bank}_{i,t}$ is a dummy variable, set to 1 if a county has above median small bank branch market share in any given year and 0 otherwise (Small Bank = Dummy). A set of fully saturated interactions between Boom variables, Small Bank variables, and the High external finance requirement dummy are included. The key coefficient of interest for the difference-in-difference-in-differences regression is the triple interaction term β_7. Note, reported R squared is close to one, due to the inclusion of multiple high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Establishments_{i,j,t} = \alpha + \beta_1 Boom_{i,t} + \beta_2 High_j + \beta_3 Small\ Bank_{i,t} + \beta_4 Boom_{i,t} * High_j + \beta_5 Boom_{i,t} * Small\ Bank_{i,t}$$
$$+ \beta_6 High_j * Small\ Bank_{i,t} + \beta_7 Boom_{i,t} * Small\ Bank_{i,t} * High_j + IndustryYear\ FE_{j,t} + CountyIndustry\ FE_{i,j} + CountyYear\ FE_{i,t} + \varepsilon_{i,j,t}$$

	Boom = Log Shale Wells				
	Small Bank = $500M Dummy	Small Bank = $200M Dummy		Small Bank = $1B Dummy	
	Holding Company = Yes	Holding Company = No	Holding Company = Yes	Holding Company = No	Holding Company = Yes
	(1)	(2)	(3)	(4)	(5)
$\text{Boom}_{i,t}$			Absorbed by County x Year $\text{FE}_{i,t}$		
High_j			Absorbed by County x Industry $\text{FE}_{i,j}$		
Small $\text{Bank}_{i,t}$			Absorbed by County x Year $\text{FE}_{i,t}$		
$\text{Boom}_{i,t}$ * High_j	0.001	0.004	0.005	0.003	0.001
	(0.29)	(1.22)	(1.45)	(0.84)	(0.29)
$\text{Boom}_{i,t}$ * Small $\text{Bank}_{i,t}$			Absorbed by County x Year $\text{FE}_{i,t}$		
Small $\text{Bank}_{i,t}$ * High_j	-0.008	0.001	-0.004	-0.013	-0.008
	(-1.03)	(0.11)	(-0.56)	(-1.49)	(-1.03)
$\text{Boom}_{i,t}$ * Small $\text{Bank}_{i,t}$ * High_j	0.019***	0.014**	0.012*	0.015**	0.019***
	(2.92)	(2.13)	(1.72)	(2.24)	(2.92)
Industry x Year $\text{FE}_{j,t}$	Yes	Yes	Yes	Yes	Yes
County x Industry $\text{FE}_{i,j}$	Yes	Yes	Yes	Yes	Yes
County x Year $\text{FE}_{i,t}$	Yes	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.999	0.999	0.999
N	12,764	12,764	12,764	12,764	12,764

Appendix C: Pre-Boom Banking Structure Robustness

This table reports results for a regression form of difference-in-difference-in-differences, where the coefficient of interest is the triple interaction term. This regression is similar to the regression results reported in Table 7, however, the bank size structure of a county is kept the same as the year prior to experiencing a boom. The dependent variable in these regressions is log establishments in county i, year t, industry group j. These specifications provide results for two different measures of Small Bank$_{i,t}$. One measure is a dummy variable, set to 1 if a county has above median small bank branch market share in any given year and 0 otherwise (Small Bank = Dummy), while the other measure is the ratio of branches which belong to small banks relative to the total number of bank branches in a county (Small Bank = Ratio). Additionally, a set of fully saturated interactions between Boom variables, Small Bank variables, and the High external finance dependence dummy are included. The key coefficient of interest for the difference-in-difference-in-differences regression is the triple interaction term β_7. County-industry and industry-year (industry trends) fixed effects are included, as well as county-year fixed effects. Note, reported R squared is close to one, due to the inclusion of multiple high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$\textit{Log Establishments}_{i,j,t} = \alpha + \beta_1 \textit{Boom}_{i,t} + \beta_2 \textit{High}_j + \beta_3 \textit{Small Bank}_{i,t} + \beta_4 \textit{Boom}_{i,t} * \textit{High}_j + \beta_5 \textit{Boom}_{i,t} * \textit{Small Bank}_{i,t}$$
$$+ \beta_6 \textit{High}_j * \textit{Small Bank}_{i,t} + \beta_7 \textit{Boom}_{i,t} * \textit{Small Bank}_{i,t} * \textit{High}_j + \textit{IndustryYear FE}_{j,t} + \textit{CountyIndustry FE}_{i,j} + \textit{CountyYear FE}_{i,t} + \varepsilon_{i,j,t}$$

	Boom = Dummy		Boom = Log Shale Wells	
	Small Bank = Dummy	Small Bank = Ratio	Small Bank = Dummy	Small Bank = Ratio
	(1)	(2)	(3)	(4)
Boom$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
High$_j$	Absorbed by County x Industry FE$_{i,j}$			
Small Bank$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
Boom$_{i,t}$ * High$_j$	0.013	-0.061	0.004	-0.015
	(0.76)	(-1.43)	(1.02)	(-1.55)
Boom$_{i,t}$ * Small Bank$_{i,t}$	Absorbed by County x Year FE$_{i,t}$			
Small Bank$_{i,t}$ * High$_j$	-0.010	0.020	-0.012	0.008
	(-1.18)	(0.77)	(-1.46)	(0.31)
Boom$_{i,t}$ * Small Bank$_{i,t}$ * High$_j$	0.054*	0.154**	0.014*	0.040**
	(1.80)	(2.12)	(1.91)	(2.31)
Industry x Year FE$_{j,t}$	Yes	Yes	Yes	Yes
County x Industry FE$_{i,j}$	Yes	Yes	Yes	Yes
County x Year FE$_{i,t}$	Yes	Yes	Yes	Yes
R^2	0.999	0.999	0.999	0.999
N	12,764	12,764	12,764	12,764

Appendix D: Effect of Bank Size and Credit Supply on Establishments of Different Size

(Boom vs. Non-Boom, High Ext Finance Requirements vs. Low Ext Finance Requirements High Small Bank Market Share vs. Low Small Bank Market Share)

This table reports results for the regressions estimated in Table 5 (Difference-in-Differences) and Table 7 (Difference-in-Difference-in-Differences) with outcome measures based on the per capita number of establishments (per 10,000 people) in different size categories. Specifications (1) and (2) are for changes in the number of small establishments (fewer than 10 people). While specifications (3) and (4) are for establishments with more than 10 people. Each outcome variable is scaled by the number of people in a county, and so can be interpreted as establishments per capita in a given size category. Note, reported R squared is close to one, due to the inclusion of multiple high dimensional fixed effects. Standard errors are clustered by county, with t-statistics reported in parentheses below coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$\textit{Establishments Per Capita}_{i,j,t} = \alpha + \beta_1 \textit{Boom}_{i,t} + \beta_2 \textit{High}_j + \beta_3 \textit{Small Bank}_{i,t} + \beta_4 \textit{Boom}_{i,t} * \textit{High}_j + \beta_5 \textit{Boom}_{i,t} * \textit{Small Bank}_{i,t}$$

$$+ \beta_6 \textit{High}_j * \textit{Small Bank}_{i,t} + \beta_7 \textit{Boom}_{i,t} * \textit{Small Bank}_{i,t} * \textit{High}_j + \textit{IndustryYear FE}_{j,t} + \textit{CountyIndustry FE}_{i,j} + \textit{CountyYear FE}_{i,t} + \varepsilon_{i,j,t}$$

	Dependent = Small Est Per Capita		Dependent = Large Est Per Capita	
	Boom = Log Shale Wells		Boom = Log Shale Wells	
	Small Bank = Dummy		Small Bank = Dummy	
	(1)	(2)	(3)	(4)
$Boom_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$High_j$	Absorbed by County x Industry $FE_{i,j}$			
$Small\ Bank_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$Boom_{i,t}$ * $High_j$	0.942***	0.145	-0.250	0.028
	(2.76)	(0.44)	(-0.61)	(0.08)
$Boom_{i,t}$ * $Small\ Bank_{i,t}$	Absorbed by County x Year $FE_{i,t}$			
$Small\ Bank_{i,t}$ * $High_j$		-0.339		-0.188
		(-0.39)		(-0.18)
$Boom_{i,t}$ * $Small\ Bank_{i,t}$ * $High_j$		1.421***		-0.493
		(2.92)		(-0.95)
Industry x Year $FE_{j,t}$	Yes	Yes	Yes	Yes
County x Industry $FE_{i,j}$	Yes	Yes	Yes	Yes
County x Year $FE_{i,t}$	Yes	Yes	Yes	Yes
R^2	0.985	0.986	0.956	0.956
N	12,764	12,764	12,764	12,764

ESSAY 2

Do Private Firms Invest Differently than Public Firms? Taking Cues from the Natural Gas Industry*

Erik Gilje† Jérôme P. Taillard‡

Abstract

We study the investment behavior of private and public firms using a unique dataset of onshore U.S. natural gas producers. In firm-level regressions we find that investments by private firms are 60% less responsive to changes in natural gas prices, a measure that captures changes in marginal q. Exploiting county-specific shale gas discoveries as a natural experiment, we show that public firms increase investment in response to new growth opportunities with large capital requirements while private firms do not. We observe that private firms sell these capital intensive growth opportunities to public firms. These findings are not driven by heterogeneity in firm size, product markets, pricing or costs. Our evidence is consistent with the higher cost of external capital of private firms being of first order importance for their investment policies.

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1 Introduction

Due to the large role of private firms in the U.S. economy, understanding how and why listing status influences investment decisions is important.[1] Privately-held firms have more concentrated ownership, which makes them less vulnerable to shareholder-manager agency conflicts than publicly-traded firms. As such, private firms are less prone to the investment distortions created by shareholder-manager agency conflicts (e.g. Stein (1989) and Jensen (1986)). However, private firms are also more opaque than publicly-traded firms. This greater information asymmetry results in greater agency conflicts between existing shareholders and potential new investors, which in turn raise the cost of external capital for private firms. The goal of this study is to investigate how these listing-related frictions affect investment.

Analyzing the investment behavior of private and public firms presents many challenges. First, data on private firms are typically unavailable. Second, accurately measuring firms' investment opportunities is a source of contention in the literature (e.g., Erickson and Whited (2000) and Alti (2003)). Third, listing status is an endogenously determined variable.

Our study contributes to the literature by focusing on a setting where (1) detailed investment data is available for both public and private firms; (2) accurate measures of marginal q exist for both public and private firms; and (3) exogenous shocks affect investment opportunities for public and private firms that have similar cost structures, pricing, and technology.

We use a unique dataset to study the investment activity of *all* public and private firms in the onshore U.S. natural gas industry between 1997 and 2012. With detailed data on 74,670 individual projects, we are able to precisely observe the investment behavior of 380 private firms and 92 public firms. We find that private firms' investment policies are less sensitive to changes in investment opportunities. This difference in investment response is strongly related to a project's capital requirements; we find that private firms are significantly less likely to pursue investment opportunities that require large capital outlays. We observe that private firms sell these capital intensive growth opportunities to publicly-traded firms. This suggests that the redeployment of projects with large capital requirements from private firms to public firms can serve to mitigate potential underinvestment concerns.

[1] In 2008 we estimate that in the U.S. at least 94.3% of business entities were privately-held and 53.8% of aggregate business net income was from privately-held firms. These calculations are based on data reported by the Internal Revenue Service in its Integrated Business Dataset.

The U.S. onshore natural gas industry offers several advantages when studying corporate investment policies. First, capital expenditures in this industry correspond to drilling new wells, and we are able to observe where and when each new well is drilled over a 16 year period. Second, all firms produce natural gas as their main output, therefore the profitability of each new well is directly tied to the price of natural gas, which is observable and exogenously given. Third, we show that our sample firms have homogenous cost structures that exhibit minimal returns to scale. Given these revenue and cost characteristics, changes in marginal q are proportional to changes in natural gas prices for both public and private firms. Moreover, technological breakthroughs in hydraulic fracturing ("fracking") and horizontal drilling in 2003 create significant new investment opportunities over our sample period, which allow us to compare investment responses of private and public firms in a natural experiment setting.

Using two distinct identification strategies we find that private firms' investment policies are less responsive to changes in investment opportunities than those of public firms. The first strategy is based on an investment to q panel regression where changes in investment opportunities are measured using changes in the price of natural gas. We find that private firms are 60% less responsive to changes in natural gas prices than their publicly-traded counterparts. While differences in firm size exist across public and private firms, we obtain very similar results when we match public and private firms on size, and when we add size controls to our specifications.

Our second identification strategy uses shale discoveries as a natural experiment; these discoveries provide localized positive exogenous shocks to investment opportunities.[2] These new investment opportunities have the characteristic of requiring significantly more capital than non-shale projects. We apply a difference-in-differences approach to shale discoveries that occur between 2003 and 2010 in 102 separate counties. Specifically, we analyze *county*-level investment decisions made by private and public firms both before and after a discovery. We focus only on firms active in areas *prior* to a shale discovery, and find that public firms respond significantly to this positive shock with a 40% increase in drilling activity, while private firms do not increase their investment activity.

We undertake several tests of the internal validity of our natural experiment following

[2]Section 2 outlines evidence that these discoveries provide positive shocks to the investment opportunity set of firms operating in the area of a discovery.

Roberts and Whited (2012). A valid difference-in-differences empirical design requires that the "parallel trends" assumption be satisfied. In our setting, this corresponds to public and private firms having similar investment trends in the absence of a shale discovery. Using falsification tests, we gauge whether pre-discovery trends differ between public and private firms prior to a shale discovery and find no differences between the two groups of firms. We also graphically show that the timing of changes in investment response is closely linked with the timing of shale discoveries. Importantly, our natural experiment is comprised of multiple events staggered over time and across different geographies; this empirical design limits the potential impact of confounding variables driving investment behavior.

Given our empirical setting, there are two main competing explanations for the differences in investment behavior we observe. First, in a traditional manager-shareholder agency cost framework, managerial actions induced by the separation of ownership and control could cause public firms to overinvest or "empire build" (e.g., Jensen (1986), Stulz (1990)). Alternatively, private firms may have lower investment responses because they face a higher cost of external capital (Pagano, Panetta, and Zingales (1998), Brav (2009), Schenone (2010), Saunders and Steffen (2011), Maksimovic, Phillips, and Yang (Forthcoming)).

To assess the overinvestment hypothesis empirically we compare public and private firm investment levels in bad states of the world (low natural gas prices), and find similar investment intensity levels across the two groups. This is the case even late in our sample when significant new shale drilling opportunities have become available. These results are not consistent with public firms overinvesting when faced with unattractive investment opportunities.

Second, we test whether our results are driven by public firms that are more susceptible to manager-shareholder agency conflicts. Given that the potential for these conflicts is the greatest for firms with low insider ownership, we estimate our main investment sensitivity specifications excluding firms with below median insider ownership. We find that private firms react 73% less to changes in the price of natural gas relative to the subset of public firms with high insider ownership, i.e. public firms with arguably fewer manager-shareholder conflicts. This result is also not supportive of the overinvestment hypothesis.

Overinvestment is often linked to settings in which firms have positive free cash flow (Jensen (1986)). In our setting, public firms do not have positive free cash flow on average, and are heavily reliant on external capital markets to fund capital expenditures. Using the Rajan and Zingales (1998) measure of external capital, the average public natural gas producer raises 34% of its capital expenditures from external capital markets during our sample period. This suggests the free cash flow-based argument for overinvestment may be less applicable in our setting.

Differences in cost of external capital could also explain differences in investment behavior between public and private firms. Public firms have market prices readily available for their equity and publicly available financial statements; both of which provide important information for potential investors (Michaely and Roberts (2012)). Conversely, private firms do not have the same disclosure requirements and do not face the same level of scrutiny by the markets. These differences lead to greater information asymmetries for private firms between existing shareholders and potential investors, which in turn lead private firms to face a higher cost of external capital relative to public firms. Consistent with this theoretical argument, the existing literature has documented that both equity (Brav (2009)) and debt (Pagano et al. (1998), Schenone (2010), Saunders and Steffen (2011)) are more costly for private firms to raise.

Our empirical setting offers evidence that is largely consistent with the greater cost of external capital hypothesis. First, we find that public firms only invest more than private firms when opportunities are the most attractive, namely in high natural gas price environments. Moreover, public firm investment in high natural gas price environments is facilitated by access to external capital markets given that the average public firm in our sample raises external capital equal to 15% of its assets in high natural gas price years, compared to just 6% in low natural gas price environments.

Second, while we observe that private firms do respond to changes in natural gas prices at the county level for less capital intensive non-shale wells, we find that they do not adjust their investment behavior when new investment opportunities linked to more capital intensive shale projects become available. Under the cost of external capital hypothesis, we would expect to find this differential in reaction given that shale projects require significantly greater external capital than the development of non-shale wells.

Differences in investment policies between private and public firms do not necessarily lead to aggregate underinvestment. If private firms are relatively more capital constrained, assets may be redeployed to relatively unconstrained firms in order to generate a better allocation of resources (Shleifer and Vishny (1992)). Therefore, one mechanism that would allow attractive capital intensive projects to be pursued are asset sales of capital intensive projects from private firms to public firms. We find direct evidence of this transfer of capital intensive projects in our data. Using detailed data on two shale discoveries, we find that private firms sell their shale drilling tracts to publicly-traded firms 63% of the time. This compares to public firms selling drilling tracts 21% of the time, and only to other public firms. This result provides a deeper understanding of how frictions related to listing status can be mitigated.

An ideal empirical strategy would not only have exogenous changes in investment opportunities, but also random assignment of a firm's listing status. We do not have random assignment of listing status in our setting. However, we show that private and public firms share many similar characteristics in terms of cost structure, technology, output and profitability. Therefore, our setting allows us to make some progress towards reducing potential endogeneity issues. Furthermore, the cost of external capital interpretation of our results relies on different investment responses to projects based on capital requirements. Thus any alternative interpretation would need to explain both the differences in investment responses between public and private firms, and the differential response for projects which require more capital versus less capital. We evaluate the plausibility of several alternative explanations based on unobserved differences in risk aversion, risk management practices and manager-shareholder agency conflicts. While we do not rule out that these alternative factors can influence investment behavior, our evidence suggests that differences in the cost of external capital are of first order importance in explaining the different investment policies of public and private firms.

Sheen (2009) and Asker, Farre-Mensa, and Ljungqvist (2011) also compare the investment behavior of public and private firms, albeit in different empirical settings than ours. Sheen (2009) analyzes multi-year plant expansion decisions in the chemical industry and shows that private firms *anticipate* future demand better than their public counterparts, whereas we focus on investment *responses* to changes in marginal q. Asker et al. (2011) make use of a large dataset on private firms to show that public firms are less responsive to changes in their investment opportunities than private firms. However, they measure investment opportunities for a given firm by its sales growth and its industry Q, both of which have the potential for mismeasurement error. These papers rely on shareholder-manager agency-based "short-termism" theories to explain their results (e.g., Stein (1989)). In particular, Asker et al. (2011) show that their underinvestment result is driven by industries that have high stock price sensitivity to earnings news, and therefore are more prone to myopic behavior. We do not find evidence of underinvestment by public firms. However, based on Asker et al. (2011)'s measure, natural gas producers have stock price sensitivity to earnings news that is below the Compustat median, therefore myopic behavior should be less prevalent in our setting.[3]

We show that public firms increase investments significantly more than private firms during periods of high natural gas prices. This increase in investment by public firms is facilitated by greater access to capital markets. We also observe that public firms increase investment when new projects with large capital requirements become available, while private firms do not. These results imply that listing related frictions can have an economically important influence on the investment behavior of private and public firms. We also show that the impact of these frictions can be mitigated by the transfer of capital intensive projects from private firms to public firms.

The paper proceeds as follows. In Section 2, we provide details on our measures of investment opportunities. In Section 3, we discuss our unique dataset. In Section 4, we present our methodology and results. Section 5 provides a discussion of our results. Section 6 concludes.

2 Measures of Investment Opportunities

> "*The Company can adjust quickly to the changes in commodity prices if necessary. Equal has an extensive multiple year drilling inventory so it can increase capital*

[3] Asker et al. (2011) are able to replicate our firm-level results with their data by restricting their sample to natural gas producers (NAICS 211111).

spending in a higher commodity price environment."

- Equal Energy, publicly-traded natural gas producer

The onshore U.S. natural gas industry has several characteristics which make it an attractive setting to test how public and private firms respond to changes in investment opportunities. First, changes in investment opportunities for both public and private firms can be measured using commodity prices. Second, we can precisely measure capital expenditures for every public and private firm in this industry as capital expenditures correspond to the number of new wells being drilled. Moreover, all new wells drilled are directly observable in our dataset for both public and private firms. Lastly, the natural gas industry has experienced a technological shock in the last decade ("fracking") which has made the development of new reserves (shale) economically viable. We justify the use of this unexpected technological shock as a natural experiment in this section.

2.1 Q theory, Marginal q and Natural Gas Prices

Neoclassical models show that the optimal investment intensity level of a firm is a function of marginal q, whereby marginal q is equal to the expected present value of the profits generated from investing one additional unit of capital (e.g. Hayashi (1982)). Typically, researchers can only observe average q which is the ratio of the market value of existing capital to its replacement costs. Hayashi (1982) demonstrates that the marginal product of capital is equal to the average product of capital only under perfect competition and when there are no returns to scale. When these conditions are not satisfied, using average q leads to well-known mismeasurement errors (e.g. Erickson and Whited (2000)). In this study, we can circumvent this issue as we do not rely on average q. We use natural gas prices, which are directly related to marginal q in our setting. Specifically, the expected present value of the profits generated from investing one additional unit of capital, marginal q, is proportional to the natural gas prices a producer can obtain for new production.

Empirically, a key advantage of the natural gas industry is the high degree of commonality between public and private firms in terms of the marginal returns to one extra unit of capital invested. Specifically, in order to make valid inferences within our investment regression framework, we need changes in natural gas prices to affect the marginal q of private and public firms similarly. We offer both theoretical arguments and empirical evidence in support of this assumption.

First, in terms of output, all projects yield the same fungible good and because natural gas is provided by a competitive market of suppliers, all firms are price takers and thus obtain similar prices for their product. While geographical differences may yield different output prices, we show in Appendix A that there is a very high correlation in natural gas prices obtained across different regions of the U.S. Second, the amount of gas produced from one extra unit of capital needs to be the same across private and public firms. While geographical differences can lead to discrepancies in terms of well productivity, the regressions with firm-county-level fixed-effects control for potential discrepancies in project output linked to differences in geography. Using two shale discoveries where we have detailed production data, we show in Appendix B (Panel B) that the output for shale wells is not statistically or economically different across private and public firms operating in the same geography.

While gross profits expand similarly for both private and public firms when natural gas prices increase, one may be concerned that drilling costs could vary systematically in the cross-section. In particular, some industries exhibit returns to scale on the cost side whereby large companies can extract discounts from suppliers and contractors on investments due to their scale. To test whether scale is a factor in per well costs, we hand-collect data on capital expenditures and wells drilled from 10-K filings of publicly-traded firms in SIC 1311 from 2006 to 2009. We then compute the average well cost for each firm and analyze how it varies within the universe of publicly-traded natural gas producers in our sample. The results from this analysis are displayed in Appendix C and indicate that there is almost no discernible difference between the median per well cost of large and small publicly-traded firms in our sample, despite the fact that large firms are on average five to six times the size of small firms. This evidence serves to alleviate concerns that cost heterogeneity in the cross-section is driving our results.

Overall, the economics of this industry are such that all firms produce an exogenously priced commodity and have a relatively homogeneous cost structure. Hence the net benefits of one extra unit of capital are similar across private and public firms. This feature creates an attractive setting to compare and contrast the investment responses of private and public firms to changes in natural gas prices.

2.2 Natural Experiment: Shale Gas Discoveries

This section explains the key features of shale gas discoveries, which provides justification for their use in the context of a natural experiment setting.

2.2.1 Unexpected Development of Natural Gas Shale

> "*Today's tight natural gas markets have been a long time in coming, and distant futures prices suggest that we are not apt to return to earlier periods of relative abundance and low prices anytime soon.*"
>
> - Alan Greenspan, July 2003, Senate Energy Committee Testimony

Prior to the development of natural gas shale, the consensus view was that low supply levels of natural gas would persist for the foreseeable future. As recently as the year 2000, natural gas produced from shale comprised only 1% of natural gas production in the United States. The technological breakthroughs occurring in 2003 that combined hydraulic fracturing ("fracking") with horizontal drilling enabled the economically profitable development of shale (Yergin (2011)). As a consequence, natural gas produced from shale today comprises 25% of all U.S. natural gas production and new natural gas reserves from shale are now equivalent to a 100 year supply of U.S. natural gas consumption (Yergin (2011)). These advancements have resulted in new investment opportunities for the development and production of natural gas in the major natural gas shale fields that have been discovered to date. Many shale discoveries have been made across the United States since 2003. In Panel A of Appendix B we document the number of shale discoveries that occur each year.[4]

In our study, we compare investment decisions for shale projects versus non-shale projects. An important feature of this comparison is the difference in capital requirements to drill shale wells. While shale wells produce significantly more than non-shale wells they are more

[4]For our study we focus on shale discoveries in the six states with major natural gas shale discoveries: Arkansas, Louisiana, Oklahoma, Pennsylvania, Texas, and West Virginia.

expensive than non-shale wells. Lake et al. (2012) state that shale wells cost between $6.7M and $9.5M, while non-shale wells usually cost less than $1M.

The development of shale uses drilling technology that is provided by third party contractors (e.g. Halliburton). Therefore the technology for shale development is available to all operators in our sample. This fact mitigates concerns regarding potential differences in access to patents or technological know-how that could be problematic for tests using technological breakthroughs in other settings.

2.2.2 Profitability of Natural Gas Shale Drilling

We offer three pieces of evidence which suggest that shale development is profitable during our sample period. The first piece of evidence is based on a detailed evaluation of the cash flows associated with shale discoveries by Lake, Martin, Ramsey, and Titman (2012). Using data from a Haynesville shale well and extensive simulations, they find that most shale gas wells are profitable under their modeling assumptions. In particular, they find that the key driver of a well's NPV is the price of natural gas with a breakeven point of $3.80 per Mcf. Over the time period of shale production in our study from 2003 to 2012 natural gas prices averaged $5.30 on an annual basis, and only dipped below $3.80 in two out of ten years. Additionally, to mitigate the risk of price fluctuations after a well has been drilled, Lake et al. (2012) point out that it is common for producers to hedge price risk for up to five years using derivatives. There is a high correlation between the spot price of natural gas and futures prices up to 36 months out (see Appendix Figure A). This feature of our setting combined with the front loading of project cash flows suggests that if a firm was concerned about price fluctuations, it could "lock-in" current prices for the most productive period of a well.[5]

The second piece of evidence is based on market measures of project profitability. If a firm has positive NPV projects, we would expect it to have a market-to-book ratio (average q) above one. This is because the numerator (market value) includes the net present value of a firm's future investments or growth opportunities (Lindenberg and Ross (1981)). If shale development were not profitable we would expect the negative cash flows from these projects

[5]An example of a typical well's production decline over time is depicted in Appendix Figure B. For example, Lake et al. (2012) assume that 70% of available reserves are extracted in the first year.

to be recognized by the market and observe market-to-book values significantly below one. During the time period of significant shale discoveries in our study (2003 to 2012), the average market-to-book ratio for public firms in our sample was 1.52. This evidence is inconsistent with shale projects being unprofitable. Furthermore, over this ten year time period, market participants have had significant time to analyze detailed data on the profitability of these projects, making it less likely that markets are misinformed about the profitability of these projects.

The third piece of evidence, which suggests that shale resources are profitable to develop, is the frequent need to access external capital markets by natural gas producers to finance their capital expenditure programs. Natural gas producers raise 34% of their capital expenditures from external sources during our sample period. This capital raising activity means that over a sustained period of time investors have provided public firms with significant funding for shale investments. If shale investments were unprofitable, it is unlikely that capital markets would continue to provide funding for them over a prolonged period of time. Taken together, the evidence presented above implies that, during our sample period, shale discoveries provide positive shocks to the investment opportunity set of firms active in the area of a discovery.

2.2.3 Characteristics of Shale Discoveries

In this subsection, we highlight two features of shale discoveries that make their use particularly well-suited in the context of a difference-in-differences approach. First, Panel A of Appendix B shows the number of shale discoveries that occur each year in different counties. One advantage of our setup highlighted in this panel is the fact that we have 102 different shocks to county-level investment opportunities over eight years. As suggested in Roberts and Whited (2012), the fact that we have multiple staggered events alleviates the risk that other confounding events could be driving the difference-in-differences results.

Second, shale projects offer very similar investment opportunities for both public and private firms already operating in an area of a shale discovery. In particular, by making use of a unique dataset from the Oklahoma Corporation Commission, we show that, in the Woodford shale and Cana shale, private and public firms face similar development costs and also obtain similar production levels. In specification (1) of Panel B in Appendix B we regress production volumes from a well's first year of production on a private indicator variable. The economic interpretation of the coefficient on the private indicator variable is that the first year of production from wells drilled by private firms is 4.4% less than wells drilled by public firms, however, this difference is not statistically significant. Specification (2) of Panel B in Appendix B shows that well costs for wells drilled by private firms are 3.7% lower. This difference is neither economically nor statistically significant. Taken together, this evidence suggests that there are no economically significant differences in production or costs for shale projects related to a firm's listing status.[6]

3 Data

Data on investment activity for private and public firms is obtained from Schlumberger Corporation's Smith International Rig Count, henceforth referred to as our "drilling" dataset. Schlumberger reports information on every rig in the United States that is actively drilling a natural gas well. This dataset provides detailed information on where a natural gas well is being drilled, who is drilling it, and when it is being drilled, at a weekly frequency over the period 1997 to 2012. Appendix D provides an example of the raw drilling data. The raw dataset is comprised of rig-week data points, whereby a rig-week is defined as the week that a drilling rig is actively drilling a well. The number of rig-week observations corresponds to the number of weeks it takes to drill a given well. Our study captures drilling activity not through the rig-week measure but through the number of wells being drilled by a given firm. Each well being drilled has a state/county/well name/well number identifier in our rig-week dataset. We describe in more detail below how we use the raw data on each individual well to construct our firm-level and county-level variables.

We conduct Lexis Nexis and Internet searches to determine whether natural gas producers in the drilling database are publicly-traded, a subsidiary of a publicly-traded firm or a private firm. We only include firms in this study that could be conclusively validated as public or private. Drilling activity of a subsidiary is combined with the drilling activity of its parent. All publicly-traded firms not within SIC 1311 (Crude Oil & Natural Gas) are excluded from our sample for firm-level regressions. In particular, this restriction eliminates all the vertically

[6] Our main shale tests are based on a much broader dataset of discoveries. The purpose of this detailed analysis of two shale discoveries in Oklahoma is to document the homogeneity in shale projects across public and private firms using unique production and cost data made available by the Oklahoma Corporation Commission.

integrated oil and gas companies, such as ExxonMobil, whose investment opportunity set is not well captured by changes in the price of natural gas due to their diversified lines of business (e.g. refining). Lastly, we exclude the twelve firms that switch from private to public or public to private during our sample period.

3.1 Firm-level Data

We make use of a unique dataset of all drilling activity conducted by onshore U.S. natural gas producers to proxy for capital expenditures and net PP&E for each firm in our sample. We aggregate the Smith International weekly Rig Count data into firm-year observations to construct a panel that makes our estimations comparable to the existing literature. Our measures of capital expenditures (CAPEX) and capital stock (PP&E) are derived from this drilling dataset.

Having measures which are reasonable proxies of accounting-based capital stock and investment for both private and public firms is one of the main advantages of our empirical framework. Because drilling is the primary investment activity of natural gas producers, we use the number of wells for which drilling operations have been initiated in a given year as our proxy for the amount of investment (I) a firm makes. The second metric we proxy for is a firm's capital stock (K). Net Property, Plant, and Equipment (PP&E) is typically used as a proxy for the capital stock of a firm in large panel studies (e.g., Cleary (1999)). In the natural gas industry, net PP&E predominantly consists of proven reserves, i.e. reserves that are meant to be recoverable with reasonable certainty under the current geopolitical, economic and technological conditions (FASB 19). Hence, in order to increase its productive capital, a natural gas producer must drill additional wells thereby increasing the amount of natural gas it can book as reserves. We compute a proxy for capital stock from the drilling data as the number of wells for which drilling operations have been completed in the prior three years. We use three years to achieve a balance between having a reasonably sized sample and having a good proxy for capital stock. Using the prior three years for our estimate of capital stock requires that the sample for our regressions starts in the year 2000 rather than 1997, which means we have 13 years of data for our firm-level panel regressions. By computing the ratio of these two measures (I/K), we derive a measure of investment intensity that is often used in the literature as the main dependent variable of interest for investment sensitivity regressions (e.g. Kaplan and Zingales (1997)).

To reduce the effect of outliers and ensure we have reasonable estimates of a firm's investment and capital stock we apply a number of screens to the raw drilling data. Specifically, we require that a firm must drill at least one well to have a firm-year observation in the sample. This restriction ensures that only firms with active investment programs are included. We also require that a firm have a minimum capital stock of at least 10 wells in the prior three years and we exclude observations with an I/K ratio above the 99th percentile. Table 1 outlines the main sample used for the firm level panel regressions. Our sample contains 380 unique private firms and 92 unique public firms, which have 1,813 and 569 firm-year observations respectively over the 2000-2012 time period. Using the subset of Compustat firms in our sample for which we have both drilling and accounting-based data, we show in Appendix Figure C that our proxies for investment and capital stock enable us to construct I/K measures that are comparable across the two datasets.

Lastly, we compute an annual measure of natural gas prices by computing the annual average of the daily wellhead gas prices obtained by natural gas producers, as reported by the U.S. Energy Information Administration.[7] One significant advantage of this measure is that we smooth out some transient jumps in the daily wellhead prices linked to two "January cold snaps" in 2001 and 2003 and Hurricane Katrina in 2005.

One issue highlighted in Table 1 is that private firms are on average smaller than their publicly-traded counterparts. To assess whether differences in size between public and private firms are responsible for how firms respond to changes in natural gas prices we undertake several exercises. First we increase the minimum size requirement for inclusion in the sample. Specifically, we require that both public and private firms have capital stock levels above different minimum threshold levels. Table 1 Panel B documents how the firm-size distribution changes for both public and private firms when different size cutoffs are used. While size differences are reduced when we increase the size cutoffs, there remain significant disparities across the two types of firms.

To further address this size issue, we devise a second approach using a size-based matched

[7] We document in Appendix A that the wellhead price of natural gas is highly correlated with natural gas "strip" futures prices and with the price of natural gas in different regions in the United States. This suggests that the wellhead price of natural gas is a reasonable proxy for the price a firm could obtain for its production, as well as its investment opportunities regardless of a firm's specific region of operation.

sample. We follow the same nearest-neighbor matching methodology as in Asker et al. (2011). In particular, as soon as a private firm enters our sample we match it to a public firm based on its capital stock value in the year it enters the sample. We keep the same match every year until the private firm or the matched public firm drops out of the sample. If the matched public firm drops from the sample, then we find a new match for the private firm in that year which is then kept going forward. Similar to Asker et al. (2011), we match with replacement to ensure that we get the best match possible. After conducting this procedure, we end up with a public-private sample matched on size, with 67 unique public firms and 354 unique private firms, and a total of 3,176 firm-years. As Panel B of Table 1 documents, our size matching generates remarkably comparable firm-sizes across public and private firms in the year of the match, with a mean capital stock of public firms of 22.07 wells compared to a mean capital stock of private firms of 22.15 wells.

Relative to Asker et al. (2011), we further impose a 10% discrepancy tolerance threshold for each matched pair in the year of the match. It is important to note that our procedure does not over-sample from a subset of small public firms. We find that the top decile of the most sampled public firms is matched to 23.6% of all private firm-year observations.[8]

3.2 Natural Experiment: County-level Data

Our dataset contains specific information on the location of wells and well characteristics that allows us to observe where and when a shale discovery occurs. We use the same definition as Gilje (2011), which relies on the number of horizontal wells drilled in a given county.[9] Specifically, we define a shale discovery to have occurred when there have been more than 20 horizontal wells drilled in the county. This threshold is set such that counties in the top quartile of county-years with horizontal drilling activity are considered shale discovery county-years. Using this definition implies that more than 90% of all horizontal wells in our sample are drilled in county-years that are shale discovery county-years.

We focus only on the subset of firms that are active in a county prior to a shale discovery; this guarantees that they have the right to drill new wells in a shale discovery county through their existing leases. Specifically, if a firm was drilling non-shale wells in a county *prior* to

[8]Our main results are similar when we exclude these oversampled firms and their matches.

[9]Horizontal wells are the primary type of well used to develop shale gas.

the discovery of shale, it now has valuable acreage that can be further developed to extract the new shale resource by drilling horizontal wells in the shale rock below its existing wells. Additionally, we require that a firm has some investment activity after the discovery of shale in a county, which insures that they did not exit an area prior to the shale discovery. For our shale discovery test, we use discoveries that are staggered across several years between 2003 and 2010 (see Appendix B). The end date for our shale discoveries is 2010 although we have data until 2012. This restriction ensures that we have a three year pre and post-period window for each shale discovery in our sample.

4 Methodology and Results

4.1 Investment Policies and Natural Gas Prices

In this section, we first compare the investment levels of both public and private firms during different price regimes over our sample period. We then compare in a panel regression framework the sensitivity of investment for both public and private firms to changes in natural gas prices, our proxy for marginal q. Figures 1 and 2 provide suggestive evidence at the aggregate level as to how firms in this industry react to changes in natural gas prices. Figure 1 highlights a strong relationship at the industry level between aggregate investment and changes in natural gas prices. However, Figure 2 shows that when this aggregate drilling activity is broken down between public and privately-held firms, public firms appear to be more sensitive to changes in natural gas prices than private firms. The difference is particularly visible during the 2003-2008 run-up in natural gas prices whereby the drilling activity of public firms follows the upward trend in natural gas prices while the drilling activity of private firms remains relatively flat over that time period.

Table 2 presents the results of univariate tests which compare investment intensity levels of public and private firms at different natural gas price levels. We split natural gas price levels into terciles, and compare year-by-year investment intensity levels in the different price environments. In low price environments, public and private firms do not have statistically different investment intensity levels. Low price environments appear both at the beginning and at the end of our sample; this is important because it suggests that even in the presence of significant shale-related drilling opportunities, both public and private firms reduce their investments in the face of adverse natural gas prices. In medium level price environments, public firms' investment intensity is statistically significantly greater than private firms' in two of four years, while public firms invest significantly more than private firms in all high price years.

A second observation can be made from Table 2 regarding the investment sensitivity to natural gas prices. Namely, public firms increase their investment activity significantly more than private firms when going from a low to a high price environment. For example, when comparing the investment mean values from the low price environment to the highest, public firms increase I/K from an average of 0.34 to 0.59, while private firms increase I/K from an average of 0.29 to 0.40. In terms of percentage change, public firm investment increases 74% from the low price environments to the high price environments compared to a 38% increase from low to high price environments for private firms. These initial univariate tests provide evidence that public firm investment is more sensitive than private firm investment to natural gas prices.

We more formally test these univariate results in a regression framework. To do so, we estimate a panel regression with firm fixed effects, controlling for any time-invariant unobserved differences across firms. We also cluster the error terms by firm. Specifically, we run panel regressions for two measures of investments (I/K and $log(I)$) regressed on indicator variables, $High_t$ and Low_t, which are based on the natural gas price terciles during the sample period from 2000 to 2012, respectively the highest and the lowest price terciles. These price environment indicators are interacted with a private dummy also ($High_t * Private_i$ and $Low_t * Private_i$):

$$\begin{aligned} Investment_{i,t} &= \alpha + \beta_1 Low_t + \beta_2 Low_t * Private_i + \\ &\quad \beta_3 High_t + \beta_4 High_t * Private_i + \beta_5 Private_i + FirmFE_i + \varepsilon_{i,t} \end{aligned}$$

The key coefficient of interest in determining whether private firms' investment levels are significantly different from those of public firms in high natural gas price environments is β_4, the coefficient on the interaction term $High_t * Private_i$. Similarly, the magnitude and sign of β_2, the coefficient on the interaction term $Low_t * Private_i$, provide an indication of how private firms respond relative to public firms in low natural gas price environments. The private dummy, $Private_i$, is absorbed by the firm fixed effects in our regressions. Results are shown in Table 3. We implement the test with a minimum size cutoff in columns (1)-(2) and (5)-(6) and on our size matched sample in columns (3)-(4) and (7)-(8).

We find that the coefficient on the interaction term $High_t * Private_i$ is negative and statistically significant in all our specifications. These results indicate that private firms invest significantly less in high natural gas price regimes than public firms. Conversely, the coefficient on the interaction term $Low_t * Private_i$ is positive but not statistically significant in all specifications. This result confirms the asymmetry documented earlier in Table 2. The differences in investment behavior between private and public firms occur in high price environments. When prices are high, public firms invest significantly more than their privately-held counterparts.

After analyzing investment levels across different price regimes, we now turn to measuring firm-level investment sensitivities to changes in natural gas prices, our proxy for marginal q. We use a panel regression framework with firm fixed effects, controlling for any time-invariant unobserved differences across firms. We also cluster the error terms by firm. Specifically, we run panel regressions for two measures of investments (I/K and $log(I)$) regressed on natural gas prices (NG_t) and natural gas prices interacted with a private dummy ($NG_t * Private_i$):

$$Investment_{i,t} = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$$

The key coefficient of interest in determining whether private firms respond differently to changes in the price of natural gas is β_2, the coefficient on the interaction term $NG_t * Private_i$. The magnitude and sign on the coefficient of this term is an indication of how private firms respond relative to public firms for a given change in natural gas prices.[10]

Results are shown in Table 4 Panel A for I/K and Panel B for the $log(I)$ specification. To address concerns regarding differences in size, we implement several minimum size cutoffs in specifications (1)-(6). Additionally, we also run our tests on our size matched sample in specifications (7)-(8).

We find that the coefficient on the interaction term $NG_t * Private_i$ is negative and statistically significant in all our specifications, including the matched sample. Specifically, the

[10] The private dummy, $Private_i$, is absorbed by the firm fixed effects in our regressions.

magnitude of the coefficient on the interaction term $NG_t * Private_i$ is equal to 60% of the magnitude of the coefficient on NG_t, which indicates that private firms are significantly less responsive to changes in natural gas prices than their publicly-traded counterparts.

Do private firms respond at all to changes in the price of natural gas? To assess the effect of changes in the price of natural gas on private firms we need to test whether the combination of the coefficients on NG_t and $NG_t * Private_i$ is significantly greater than zero (H_0: $\beta_1 + \beta_2 = 0$ vs. H_a: $\beta_1 + \beta_2 > 0$). The results for this test are shown below the main regressions in both Panel A and Panel B of Table 4. For example, in specification (2) of Panel A we find that the sum of the two coefficients is equal to 0.026 (= 0.065 − 0.039), a figure that is both positive and statistically significant. This difference is positive and statistically significant in all specifications found in Panel A and Panel B of Table 4. This result means that private firms react to changes in natural gas prices, albeit at a significantly lower degree than their publicly-traded counterparts.

Relating the coefficients in specification (2) of Panel A to the median investment intensity of each firm type implies that a one standard deviation increase in natural gas prices leads public firms to increase their investment intensity ratio by 29% while the investment intensity ratio of private firms only increases by 15%. Similarly, in specification (2) of Panel B, with log of investments as the dependent variable, we find that a one standard deviation increase in the price of natural gas leads public firms to increase investment by 30% while private firms increase investments by only 17%.

The sign and significance of our results remain unchanged in most specifications, and the magnitude of our coefficient remains nearly the same throughout. When firms are matched on size in specifications (7)-(8), we find very similar and statistically significant results. This finding suggests that differences in size do not account for the observed differences in investment behavior. To further investigate how firm size, as opposed to listing status, affects our results, we augment our baseline specification by adding size controls in our regressions in Table 5. To do so, we include an indicator variable for whether a firm is above the median firm in terms of size in a given year, and when the left-hand side variable is the logarithm of investments (log(I)), we include the logarithm of capital stock ($log(K)$) as a control variable for size ($Size_{i,t}$). Moreover, we include an interaction term between these measures of size and our investment opportunity measures $NG_t * Size_{i,t}$ to test whether being private proxies for a size effect. When we add both the interaction of price with the private dummy and price with size, we observe that the interaction with the private dummy remains statistically significant throughout all specifications in Table 5. This result provides further evidence that differences in size between private and public firms are not driving our results.

In Table 6 we perform a variety of robustness tests on our baseline specifications. We first replace in columns (1) and (2) the spot price of natural gas with the futures price of natural gas as our proxy for marginal q. We use 12 month futures "strip" prices as our measure for futures prices. The "strip" price is the industry standard measure of futures prices and corresponds to an arithmetic average of the natural gas futures prices with delivery dates over next 12 months from a given point in time. Historically the "strip" price has been highly correlated with spot prices but we test that the sensitivities estimated are robust to this alternative specification. Results in columns (1) and (2) show that we observe quantitatively very similar results to our main regression when we use futures prices instead of spot prices.

We also test our main specification in columns (3) and (4) at the quarterly frequency. We observe coefficients that are roughly a quarter in magnitude relative to those in our main specification in Table 4 Panel A. More importantly, the differences in sensitivities between public and private firms at the quarterly frequency are of similar economic magnitudes as those at the annual frequency.

In order to ensure that our results are not driven by the behavior of the largest firms in our sample, we exclude firms with more than 500 wells drilled over the past three years in columns (5) and (6) and observe similar results as Table 4. Lastly, when we include time-fixed effects in columns (7) and (8), NG_t is no longer identified, but $NG_t * Private_i$ still is and we observe an interaction coefficient similar to our main specification. Overall, our panel regression results are robust to many alternative specifications. We find that private firms are significantly less sensitive to changes in investment opportunities than their publicly-traded counterparts.

The evidence provided in this section is both economically and statistically robust. The univariate tests and regression results provided in Table 2 and 3 focus on the relative levels of investment across public and private firms. We observe that both public and private firms invest at similar levels in low natural gas price environments, which occur both at the beginning and at the end of the sample period. However, in high price environments, public firms invest significantly more than their privately-held counterparts. These results translate directly into our firm-level investment sensitivity results. When prices increase, public firms increase their investment intensity level more than private firms. And when prices drop towards the end of the sample period, the reduction in investments is more pronounced among public firms given that they invest at higher levels in the high price environment. This pattern of behavior corresponds to the higher sensitivities of public firms to natural gas prices observed in our firm-level sensitivity regressions.

4.2 Natural Experiment: Shale Gas Discoveries

In this section, we test how private and public firms respond to county-level shale discoveries. We use the unexpected investment opportunities created by shale gas discoveries as a natural experiment. Specifically, we use data on firm investment activity at the county level in a difference-in-differences framework to see how public and private firms with pre-existing operations in counties with shale discoveries respond to the new investment opportunity. The first difference can be viewed as comparing investment pre-discovery versus post-discovery, while the second difference can be thought of as the difference in how public and private firms respond to the shale gas discovery.

By testing how private and public firms respond to shale discoveries, we can rule out several firm-level differences as potential explanations for the differences in investment behavior we observe in the previous section. For instance, it could be the case that the results of our firm-level specifications are driven by some unobserved heterogeneity between public and private firms such as geographic differences in natural gas development opportunities, which could then lead to transportation cost differences. Alternatively, it could be the case that one set of firms is better at searching for new areas to drill. Our shale discovery natural experiment design helps alleviate many of these concerns. In particular, because we require all firms to be drilling in a shale county prior to a discovery, any differences in investment activity between public and private firms cannot be explained by one set of firms always having superior abilities to search and seek out new drilling opportunities. Furthermore, the evidence presented in Section 2 suggests that private and public firm face similar costs and generate similar production volumes when developing shale discoveries. Ultimately, shale discoveries provide new growth opportunities at the same time and location, with similar costs and production volumes; providing for a comparison of similar investment opportunities across public and private firms.

The dependent variable in our natural experiment is $Investment_{i,j,t}$ which corresponds to investments made by firm i in county j at time t. To ensure that we have consistent standard errors in our estimation we follow the approach recommended by Bertrand et al. (2004) and collapse time periods for each discovery into two periods; one pre-period and one post-period. Specifically, in a given county, the investment activity of a given firm is averaged across the three years prior to the discovery and the three years after the discovery. Thus, for each firm in each shale county we have two observations: One pre-discovery and one post-discovery. The time period of shale discoveries in our sample spans from 2003 to 2010, this ensures that we have a three year pre and post-event window for each discovery. For example, a discovery occurring in 2010 will have a pre-period of 2007, 2008, 2009 and a post period of 2010, 2011, and 2012.

In our baseline difference-in-differences regressions, we explain $Investment_{i,j,t}$ with a post-discovery dummy variable ($PostDiscovery_{j,t}$) and post-discovery dummy interacted with a private dummy ($PostDiscovery_{j,t} * Private_i$):[11]

$$\begin{aligned} Investment_{i,j,t} &= \alpha + \beta_1 NG_t + \beta_2 PostDiscovery_{j,t} + \beta_3 PostDiscovery_{j,t} * Private_i \\ &\quad + \beta_4 Private_i + FirmCountyFE_{i,j} + \varepsilon_{i,j,t} \end{aligned}$$

The key coefficient of interest in determining whether private firms respond differently to shale discoveries is β_3, the coefficient on the interaction term $PostDiscovery_{j,t} * Private_i$. The magnitude and sign on the coefficient of this term is an indication of how private firms respond relative to public firms to a shale discovery in a given county. We also include firm-county fixed effects to account for time invariant heterogeneity of firm investment policies in different counties.

Table 7 documents that county-level investment of public firms increases significantly after a shale discovery. Specifically, the coefficient on $PostDiscovery_{j,t}$ in specification (3) indicates that public firms increase investment by 39.9% after a shale discovery. The interaction coefficient $PostDiscovery_{j,t} * Private_i$ is negative and statistically significant, which indicates

[11] The direct effect of being private ($Private_i$) is subsumed by the firm-county fixed effects.

that private firm investment responds significantly less than public firm investment to a shale discovery. Furthermore, when testing whether private firms respond to a shale discovery with any increased investment, we cannot reject the null that the increase in investment is not statistically different from zero, meaning that private firms do not show any statistically significant increases in their investment in shale discovery counties in the three years following a discovery.[12] We obtain similar results when using number of wells, instead of logarithm of wells as the dependent variable in specification (6). Changing the functional form of the specification and finding similar results provides a useful confirmation of our main results.

In Table 8, we augment our baseline specification to test whether size could be driving differences in the responsiveness to shale discoveries. This new specification is important to test as size is a variable that affects a firm's access to external financing and hence its ability to undertake shale drilling. Specifically, we include both a size indicator variable ($SizeDummy_{i,t}$) and the size indicator variable interacted with the post-discovery dummy ($PostDiscovery_{j,t} * SizeDummy_{i,t}$). We use our proxy for capital stock at the firm-level as our size variable; the indicator variable takes the value of one for firms with above median size for the given three year period, and zero otherwise. The key coefficient of interest when testing whether larger firms (as opposed to public firms) are able to respond better to shale discoveries is the coefficient on the interaction term: $PostDiscovery_{j,t} * Size_{i,t}$. If it is the case that larger firms respond more to shale discoveries, then we would expect this interaction term to be positive, yet it is close to zero, and not statistically significant. Given that the coefficient on $PostDiscovery_{j,t} * Private_i$ remains negative and significant even after the inclusion of these size controls, we infer that size differences are not driving the observed disparities in investment responsiveness between public and private firms. It is important to note, that we do not include a matched sample in our natural experiment due to the limited number of potential matches available among the public firms also operating within the county prior to the shale discovery. At the firm level private firms have the full universe of public firms to obtain a match from. However, at the firm-county level, a given private firm has on average only 2.97 public firms to obtain a match from.

[12] To formally test this hypothesis, we test whether the linear combination of the coefficient on the post-discovery dummy and the coefficient on the interaction term of private and post-discovery dummy is significantly greater from zero (H_0:$\beta_2 + \beta_3 = 0$ vs. H_a: $\beta_2 + \beta_3 > 0$).

Table 9 provides evidence as to the internal validity of our natural experiment in the form of a falsification test. The main assumption of a difference-in-differences empirical design is the "parallel trends" assumption. In our setting this assumption corresponds to whether, in the absence of a shale discovery, the investment patterns of public and private firms would have had similar trends. We empirically test whether trends were different for these two sets of firms prior to a discovery by artificially moving the time of the shale discovery to be three years earlier for every shale discovery county in our sample. The results on the interaction term $PlaceboDiscovery_{j,t} * Private_i$, as well as the direct effect, $PlaceboDiscovery_{j,t}$, are not statistically significant, which suggests that there were no differences in investment trends between public and private firms prior to a shale discovery. It also suggests that there was no drilling made in anticipation of the shale discoveries from either public or private firms.

In Figure 3 we provide graphical evidence in support of the "parallel trends" assumption. For both Figure 3.1 and Figure 3.2, we run a regression of investments made at the county level on yearly indicator variables in event time. For Figure 3.2, the event time is artificially moved forward by three years prior to shale discoveries, as in the falsification test. For Figure 3.1, the event time is centered around the county-level shale discoveries. The figures plot the yearly coefficients around the event window from $t-3$ to $t+2$ relative to the baseline effect at $t-3$ set at zero. Figure 3.2 shows no difference in trends in the pre-discovery time period, which is consistent with the falsification results shown in Table 9. In addition, Figure 3.1 sheds light as to the precise timing of the response to shale discoveries shown in Table 7. It shows that both public and private firms invest similarly prior to a shale discovery, and then public firms respond with a sharp increase in investment at the time of the discovery while private firms do not. This result provides a graphical confirmation as to the timing and reaction to shale discoveries documented in Table 7.

Lastly, in order to shed additional light on the interpretation of our natural experiment results, we compare the county-level shale discovery responses of our main diff-in-diff test to non-shale county-level investment responses to changes in natural gas prices in Table 10. This allows us to compare two sets of investment responses at the county level, one for capital intensive shale discoveries and one for less capital intensive traditional non-shale wells. As Table 10 shows, the sum of $\beta_1 + \beta_2$ is statistically significantly greater than zero in both specification (2) and specification (4); therefore private firms do respond to changes in natural gas prices at the county level, though less than public firms. Relating the coefficients in specification (4) of Table 10 to the average county-level well investment of each firm type implies that a one standard deviation increase in natural gas prices leads public firms to increase their investment by 27% while the investment of private firms only increases by 9%. Alternatively, in specification (2) of Table 10, with log of investments as the dependent variable, we find that a one standard deviation increase in the price of natural gas leads public firms to increase investment by 16% while private firms increase investments by only 5%. Ultimately, we have investment responses for two different project types, one with higher capital needs (shale in Table 7) and one with lower capital needs (non-shale in Table 10), and we observe a larger difference in response for the project type with high capital needs. An interpretation of these investment responses needs to reconcile why two different project types, which differ primarily on capital needs, experience such differential responses based on a firm's listing status.

4.3 Corporate Activity and Shale Discoveries

In this section we provide details on corporate activity related to shale discoveries. First, we focus on asset sales by providing a detailed analysis of asset sales after the discovery of two shale plays. Second, we provide evidence on IPO and M&A activity during our sample period.

4.3.1 Asset Sales

In this section, we study asset sales patterns around shale discoveries. Obtaining detailed data on drilling tracts is challenging. Therefore, we focus our analysis on two shale discoveries where data is made available by the Oklahoma Corporation Commission, and land and regulatory rules make asset sales straightforward to infer. Specifically, using production data we can observe who owns the drilling tracts with existing producing wells prior to a shale discovery. If subsequent shale development is performed on a tract by a firm other than the firm with pre-existing producing wells, it means that the firm with pre-existing wells has sold the asset (the right to drill shale) to a new firm. We use data covering more than 66,560 acres in two shale discoveries in Oklahoma over the period 2003 to 2010: Specifically the Woodford shale and Cana shale across four counties: (1) Canadian county (discovery in 2008); (2) Coal county (discovery in 2006); (3) Pittsburg county (discovery in 2006), and (4) Hughes county (discovery in 2006). We test whether drilling tracts are being transferred from private to public firms in a significant manner after these shale discoveries are made.

Across those two shale discoveries, we show in Table 11 that 63% of acreage tracts held by private firms prior to a shale discovery are sold to public firms. In contrast, among public firms, only 21% sell their drilling tracts, with all tracts being sold to other public firms and none to private firms. The differences are statistically and economically significant. The evidence shown in this section is suggestive of a significant transfer of capital intensive projects from private to public firms.

4.3.2 IPO and M&A activity

In terms of IPO and M&A activity, we can only provide anecdotal evidence given the scarcity of IPOs and takeovers in our dataset. Changing listing status is major corporate decision and there are significant costs associated with an IPO (Ritter (1987)). We find only 12 IPOs occur after the advent of shale drilling. While we do not have enough IPOs for statistical analysis, the qualitative evidence we collect in Appendix E documents that 11 out of the 12 IPOs after 2003 use proceeds from the IPO to fund costly capital expenditure programs linked to shale-related opportunities.

During our time period, there are also only a dozen cases of public firms acquiring the full operations of private firms. We believe that this result can be directly related to the fungible quality of assets in this industry. Given the ease of transferring assets from one operator to another, a firm will incur fewer transaction costs with an asset sale and this would explain the prevalence of asset sales documented in the previous subsection. There is some anecdotal evidence that financing constraints are a significant driver of full acquisitions of private firms by public firms. For example, after the sale of privately held Stroud Energy to publicly traded Range Resources Corp, Stroud's CEO Patrick J. Noyes stated that the acquisition would "allow for the accelerated development of our properties."

5 Interpretation and Discussion

Using two distinct methodologies, we have shown that private firms react less to changes in investment opportunities than their publicly-traded counterparts. The unique features of our empirical setting allow us to rule out many explanations based on differences in firm characteristics between private and public firms. In particular, they produce the same good that is sold at a given market price and face similar cost structures. We further refine our comparison by using very granular data at the county-level and highlight differences in responses to new investment opportunities made available to both public and private firms already operating in the area. Hence, our natural experiment further controls for potential geographical and technological differences between private and public firms. Additionally, we show that private firms sell significant amounts of their drilling tracts to public firms after a shale discovery.

In this section we explore several potential alternative explanations for our results. The two main explanations rely on agency conflicts. The first conflict is between managers and shareholders and affects predominantly public firms, which have a more dispersed ownership structure than private firms. This agency conflict can push public firms to overinvest relative to private firms. The second agency conflict is between existing shareholders and potential new investors; the greater the information asymmetry between insiders and outside investors, the greater the cost of external capital. This conflict affects private firms more given their greater opacity. The higher cost of external capital faced by private firms may lead them to underreact to investment opportunities.

We do not rule out that additional factors can influence investment decisions. In particular, we assess other channels at the end of this section that could help us understand the observed differences in investment behavior between private and public firms. Specifically, we discuss the potential differences in hedging behavior and risk aversion across public and private firms.

5.1 Shareholder-Manager Agency Costs

The fact that public firms invest more and have greater investment sensitivities than private firms could be consistent with public firms overinvesting (e.g. Jensen (1986)). However, the results presented in Table 2 showed that (1) public firms invest more than private firms when investment opportunities are the most profitable, i.e. when natural gas prices are high, and (2) public firms invest similarly to private firms in low price environments. These facts are not consistent with the overinvestment theory.

We undertake two additional tests in this section to assess whether overinvestment, caused by shareholder-manager agency conflicts at public companies, can explain the investment responses we observe. To do this, we compare both investment levels and investment sensitivities for public firms that are more susceptible to shareholder-manager agency costs (low insider ownership) and public firms that are less susceptible to agency costs (high insider ownership). To the extent shareholder-manager agency costs are greater for firms in which management has lower insider ownership; this proxy should capture a relative measure of the importance of this agency cost in explaining why public firms react more than private firms to changes in investment opportunities.

In Table 12, we provide an analysis similar to Table 2, except that we subdivide our public sample into firms with low insider ownership and high insider ownership, defined as being below or above the median insider ownership in a given year. As can be seen from Table 12, firms with high insider ownership (less susceptible to manager-shareholder agency costs) invest similarly to firms with low insider ownership (more susceptible to manager-shareholder agency costs). If anything the firms less prone to agency costs invest at higher levels, though not statistically significant in most years, and have greater sensitivity to changes in natural gas prices. For example, firms with high insider ownership have investment intensity 103% higher in high price environments relative to low price environments, while firms with low insider ownership have investment intensity by 61% in high price environments relative to low price environments.

The differences in sensitivities observed in Table 12 are formally tested in a regression framework in Table 13. To evaluate the overinvestment hypothesis further, we test whether the difference in investment behavior observed between public and private firms is driven by firms that are more prone to agency conflicts. Given that the potential for manager-shareholder conflicts is the greatest for firms with low insider ownership, we estimate firm-level investment regressions excluding the subset of public firms with the lowest insider ownership (below median). We show in Table 13 that public firms with higher insider ownership (lower agency conflicts) are still more reactive to changes in their investment opportunity set than private firms. Specifically, in specification (2) of Panel A in Table 13 we find that private firms are 73% less reactive to changes in investment opportunities than public firms. This result provides further evidence against the overinvestment hypothesis.

5.2 Cost of External Capital

In this section, we first present evidence from the literature that private firms face greater costs of external finance. In particular, it is well established that private firms face both greater costs of debt and equity. We then assess whether the evidence on investment patterns made by private and public firms provided in Section 4.1 and 4.2 as well as the evidence on corporate activity provided in Section 4.3 are consistent with financing constraints being of first-order importance in understanding why private firms have different investment policies relative to their publicly-traded counterparts.

5.2.1 Cost of Debt

Several studies have documented that private firms have a higher cost of debt. For example, Saunders and Steffen (2011) document that private firms have loan spreads that are on average 27 basis points higher, as compared to publicly traded firms, even after controlling for loan and borrower characteristics. Schenone (2010) finds that loan spreads are 21 basis points higher before an IPO versus after an IPO, and Pagano et al. (1998) find that for IPOs in Italy this figure is 40 to 70 basis points. While these magnitudes are economically meaningful, it is unlikely that loan spread differentials of 21 to 70 basis points alone would be driving differences in investment responses as large as we observe in our setting.

However, there are other aspects of a borrower-lender relationship to consider beyond the interest rates charged on existing outstanding debt. For example, Schenone (2010) suggests that one of the reasons firms have a higher interest rate pre-IPO is that banks exploit an information based monopoly from a "locked-in client firm." This view is empirically supported by Saunders and Steffen (2011) and theoretically supported by Sharpe (1990) and Rajan (1992), who suggest that lending relationships could be problematic for firms if they become informationally locked in.

Being locked in a lending relationship also imposes a borrowing constraint. Specifically, banks have regulatory limits on the amount they can lend to any single borrower, and often have additional limits based on internal risk controls. If a borrower is locked into a lending relationship, a critical issue for its cost of obtaining external capital is the upper bound in lending limits it faces from its bank. Switching or adding relationships creates further uncertainty and associated costs. For the average U.S. bank the maximum regulatory limit that can be lent to a given borrower is $26 million.[13] The impact of lending limits is particularly important for private firms which may have developed long standing lending relationships prior to unanticipated large growth opportunities such as shale projects.

5.2.2 Cost of Equity

There is both empirical and theoretical evidence which suggests that the cost of equity for private firms is greater than for public firms. Brav (2009) documents that private firms have leverage that is 50% higher than public firms, and attributes this to private equity being more costly than public equity. Additionally, when private firms raise external capital they favor debt more than equity. Specifically, Brav (2009) finds that when external capital is needed, private firms raise equity only 10% of the time, while public firms raise equity 40% of the time. The economic magnitudes of these differences are large, and indicate significant differences in the cost of equity between private firms and public firms.

Brav (2009) suggests that the higher cost of equity for private firms is driven by concerns regarding information asymmetry and control. Private firms are more informationally opaque than public firms, which makes agency costs between existing shareholders and potential new investors more acute for private firms. Since equity is a more junior security than debt in the capital structure, equity is likely to be more sensitive to information asymmetry issues (Myers and Majluf (1984)). This information asymmetry will mean that the cost of equity for private firms will be greater because private firms do not offer minority shareholders the same disclosure and protection a public firm does.

Brav (2009) suggests that maintaining control is one of the main advantages of being privately-held. Closely held private firms are not subject to the same agency conflicts as public firms. As such, surrendering a significant amount of control to pursue new growth opportunities may offset the benefit these growth opportunities would provide to the firm's owners. Of significance, this suggests that greater cost of external capital need not be externally imposed by markets, but may be self-imposed by a firm's owner who is unwilling to

[13] FDIC Part 32.3 and Office of Thrift Supervision (OTS) Section 211 limit lending to any single borrower to 15% of a bank's unimpaired capital and unimpaired surplus. We computed the unimpaired capital and unimpaired surplus for each bank using Call Report data. The average unimpaired capital and unimpaired surplus was $173 million; of which 15% corresponds to $26M.

dilute control. The cost of relinquishing control will be greater when more external capital is needed. Given the significant capital needs for shale development, the associated dilution in control could lead to significant differences in the cost of external capital across private and public firms.

The mechanisms discussed in this section can have a large effect on a private firm's ability and willingness to raise external equity capital. The net effect is that private firms face greater costs of external equity capital; furthermore the leverage choice and equity raising activity documented by Brav (2009) indicate that the differences in cost of equity capital between public and private firms are economically large.

5.2.3 Cost of Capital, Investments and Asset Sales

Our first result shows that private and public firms invest at similar levels in low price environments, while public firms invest significantly more than private firms in response to higher natural gas prices. In our second empirical approach, we find that only public firms increase drilling activity in response to the improvement in capital intensive investment opportunities provided by shale discoveries. While private firms respond to increases in natural gas prices for low capital intensity non-shale wells, they do not respond to more capital intensive investment opportunities provided by shale. Both the firm-level and county-level evidence can be understood in the context of private firms facing a higher cost of external capital. If private firms are more capital constrained, they will not be able to pursue all projects, in particular those that are more capital intensive.

A financially constrained firm can undertake several actions to alleviate the impact of higher cost of external capital. Going public is one way to obtain access to more external financing. We observe a dozen firms going public in the period of shale drilling and all but one mention access to capital in order to pursue shale drilling as a reason for the IPO. A constrained firm can also sell a portion or the entirety of its operations to a firm with better access to capital markets. The unconstrained firm creates value by providing the funds to pursue all the positive net present value projects of the constrained target. Erel et al. (2012) find evidence consistent with the view that full acquisitions can ease financial constraints faced by target firms. Each of these mechanisms provides a channel for private firms to alleviate the effects associated with having a higher cost of external capital.

Beyond the anecdotal evidence provided by IPOs and takeovers, we find significant evidence that private firms active in areas with shale discoveries sell their rights to develop shale acreage to public firms. The corporate activity observed during the period of shale discoveries is consistent with a rational response by private firms to defer capital intensive projects to public firms, which have a lower cost of external capital (see Shleifer and Vishny (1992)). These transactions suggest that even if private firms do not exploit their drilling rights in the wake of shale discoveries, profitable projects are still being undertaken with an efficient redeployment of assets towards the less financially constrained public firms.

5.3 Hedging

Hedging has two effects within the context of our study. The first, which we outlined previously, is that it enables firms to lock-in the profitability of a project using futures contracts. The second effect is that hedging undertaken in prior years may affect a firm's current internal cash flows positively or negatively. However, the only difference between a hedged and an unhedged firm will be in terms of internal cash flow generation, not in terms of changes in investment opportunities. An increase in natural gas prices provides the same improvement in marginal q, i.e. the profitability of drilling a *new* well, for a firm that has hedged its *existing* production relative to an unhedged firm. Furthermore, a more fully hedged firm has the same access to new shale discoveries from existing acreage as an unhedged firm.

Haushalter (2000) shows that firms more subject to financing constraints are more fully hedged. Given that private firms have a higher external cost of capital, they could be more hedged at any point in time than public firms. As such, hedging might adversely affect private firms' internal cash flows relative to than public firms, yet it is unlikely to be the main driver behind our results. The reason is that most firms in our sample are highly dependent on external capital. Using the measure developed by Rajan and Zingales (1998), we find that the median public natural gas producer raises an average of 34% of its annual capital expenditures from public equity and debt issuances in order to respond to changes in its investment opportunities. This significant need for external financing suggests that the effect of hedging on internal cash flow is unlikely to be the main driver behind the large observed differences in investment behavior.

5.4 Risk Aversion

Private firms might differ systematically from public firms in terms of risk aversion. In particular, one might argue that private firms are run more conservatively than public firms. First, private firms have more concentrated ownership, characterized by long-term investors with substantial wealth at risk. Second, the population of private firms may have a greater proportion of family firms, which tend to be relatively more concerned with long-term survival and reputation (Anderson et al. (2003)). These concerns could directly influence the investment decisions of private firms.

Risk aversion may play a role in explaining our results. For private firms, equity is provided by a limited number of shareholders who potentially have a significant portion of their wealth tied to the fortune of the firm. If that is the case, their risk aversion will factor directly into the cost of equity.

We would argue, however, that there are at least two reasons why it is less plausible that differences in risk aversion would be the first order explanation behind our results. First, we observe that private firms respond to projects differently based on a project's capital needs. Therefore, a risk aversion based explanation would need to reconcile a differential response for projects that require more capital relative to projects that require less capital. To the extent that higher risk aversion may be linked with the amount of capital outlay a project requires, it is likely due to an owner's inability to diversify the risk by issuing equity for the larger project or potentially having to face more adverse terms for a larger amount of debt necessary to finance the new project (e.g. personal guarantees). These explanations would both be linked to a private firm facing a higher cost of external capital. In such cases, risk aversion need not be a mutually exclusive explanation from a cost of external capital based interpretation.

Second, higher risk aversion would imply that for a given natural gas price environment private firms invest less than public firms in all states of the world. However, the results in Table 2 indicate that private firms only invest significantly less than public firms when natural gas prices are higher, precisely when investment opportunities are better. In low natural gas price environments when investment opportunities are less attractive, private and public firms invest at similar levels. These results suggest that potential variation in risk aversion across public and private firms is not the first order explanation for the observed differences in investment responses.

6 Conclusion

In this paper, we exploit a unique dataset of onshore U.S. natural gas producers to study how private and public firms differ in their investment behavior. We find that private firms respond less to changes in their investment opportunities than their publicly-traded counterparts. We reach this conclusion by analyzing the investment behavior of private and public firms using two different identification strategies. The first is based on firm-level investment to q regressions where changes in investment opportunities are measured by changes in natural gas prices. In this setting, private firms are 60% less responsive to changes in marginal q relative to public firms. The second approach implements a difference-in-differences methodology using county-level shale discoveries as a natural experiment to assess the responsiveness of private and public firms' investments to capital intensive growth opportunities. Following a shale discovery, we find that public firms increase their county-level investment activity by 40%, while private firms do not pursue these capital intensive shale projects.

Our empirical setting offers several advantages beyond studying the investment activity of a large sample of both public and private firms. First, due to the economics of our setting, changes in natural gas prices are exogenously given and directly related to changes in marginal q for both private and public firms. This fact offers an improvement on most of the literature using average q to proxy for marginal q. Second, we are also able to make use of significant shale gas discoveries in specific counties to design a difference-in-differences test that rules out potential alternative explanations for our findings. As such, our results are not driven by heterogeneity in firm size, product markets, technology, pricing, or costs.

We evaluate two main competing explanations for our results. First, public firms could be overinvesting due to manager-shareholder agency conflicts. However, we find little support for the overinvestment hypothesis. In particular, public firms do not invest more than private firms in low natural gas price environments and public firms that are more prone to manager-shareholder agency conflicts are not the ones driving the wedge observed between public and private firms' responses to changes in investment opportunities.

Alternatively, the relative lack of investment response by private firms to both high natural gas price environments and capital intensive shale projects could be consistent with private firms facing a higher cost of external capital. We show that the increased investment by public firms in high natural gas price environments is facilitated by access to external capital markets. Furthermore, our evidence suggests that differences in investment responses between private and public firms are more pronounced for projects that require large capital outlays. These results imply that listing related frictions have an economically important influence on the investment behavior of private and public firms. We also show that the impact of these frictions can be mitigated by the transfer of capital intensive projects from private firms to public firms.

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Aggregate Onshore U.S. Natural Gas Drilling Investment: All Firms

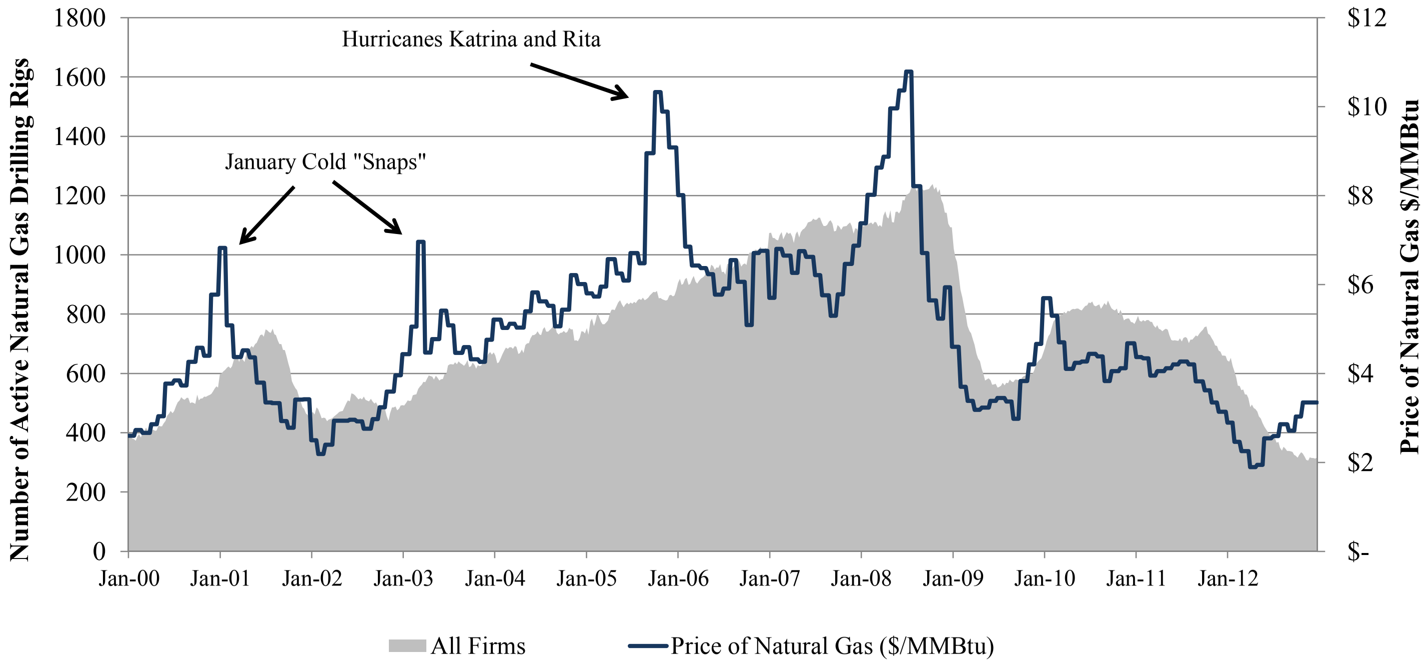


Figure 1: Onshore U.S. Natural Gas Drilling Investment: All Firms
This figure plots the weekly time-series of aggregate investment activity in the onshore U.S. natural gas industry, as proxied by the number of active drilling rigs. The figure also shows the weekly time-series of the wellhead price of natural gas. The time period ranges from 2000 to 2012.

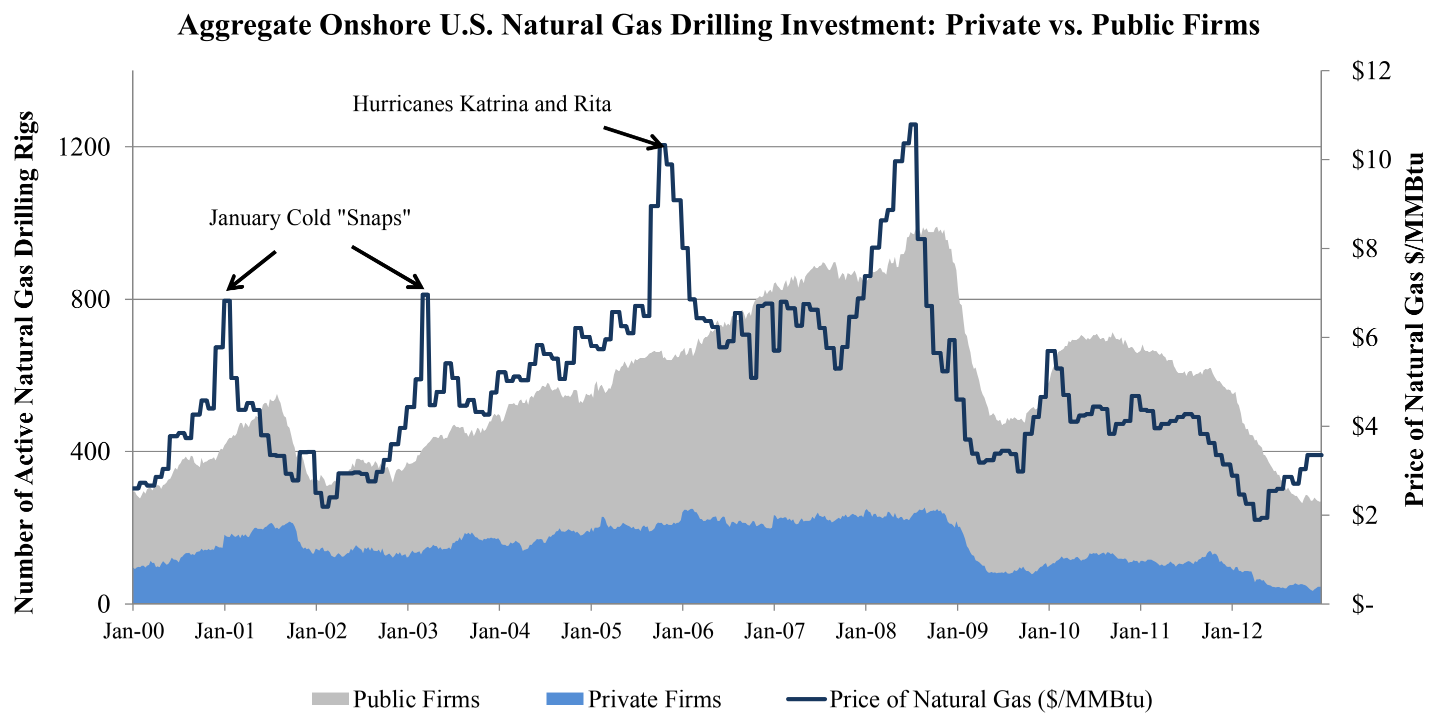


Figure 2: Onshore U.S. Natural Gas Drilling Investment: Private vs. Public Firms
This figure plots for public and private firms separately the weekly time-series of aggregate investment activity in the onshore U.S. natural gas industry, as proxied by the number of drilling rigs. The aggregate drilling activity of public firms is plotted as the upper boundary of the lighter shade, which is always greater than the aggregate drilling activity of private firms (darker shade) over the sample period. The figure also shows the weekly time-series of the wellhead price of natural gas. The time period ranges from 2000 to 2012.

Figure 3.1 Investment in Event Time: Shale Discovery

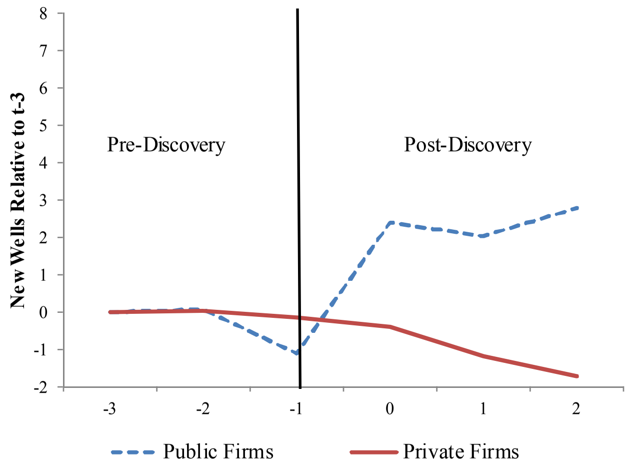


Figure 3.2 Investment in Event Time: Placebo Discovery

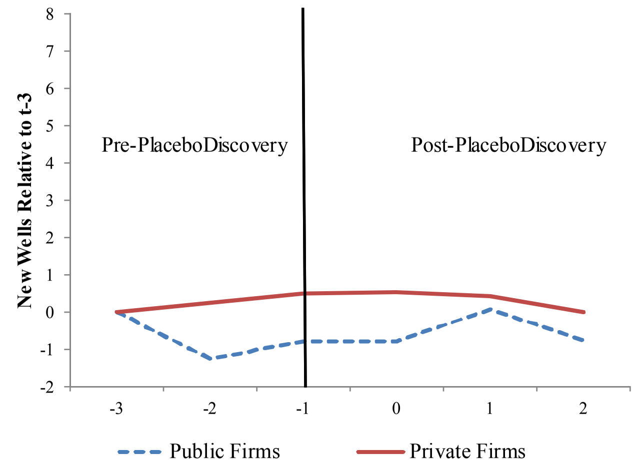


Figure 3.1: Investment in Event Time: Shale Discovery
This figure plots separately for private and public firms the regression coefficients of dummy variables for investment levels based on the year relative to a discovery. The first year of a discovery is year 0. The first point is the plot of a dummy variable for time t-3 relative to the discovery. The dependent variable is the number of wells for a given firm in a county in a given year, so the coefficients can be interpreted as the change in investment levels at different points in time relative to t-3. Natural gas prices and county-firm fixed effects are also included as controls in the regression.

Figure 3.2: Investment in Event Time: Placebo Discovery
This figure plots separately for private and public firms the regression coefficients of dummy variables for investment levels based on the year relative to a placebo discovery. Placebo discoveries are set to be three years prior to an actual shale discovery for a given county. The first year of a placebo discovery is year 0. The first point is the plot of a dummy variable for time t-3 relative to the placebo discovery. The dependent variable is the number of wells for a given firm in a county in a given year, so the coefficients can be interpreted as the change in investment levels at different points in time relative to t-3. Natural gas prices and county-firm fixed effects are also included as controls in the regression.

Table 1. Summary Statistics

This table contains summary statistics for all publicly-traded and privately-held firms in the onshore U.S. Natural Gas industry over the period 2000 to 2012. For a given firm, investments (I) is defined as the total number of wells drilled in a given year and capital stock (K) is defined as the total number of wells drilled in the previous three years. Capital intensity (I/K) is defined as the ratio of these two variables. A firm-year observation is included in our sample if it satisfies the following requirements: 1) non-missing capital stock; 2) drilled at least one well in current year and 3) have capital stock greater than or equal to 10 wells. To mitigate the effect of outliers we exclude observations above the 99th percentile for I/K. Panel A reports summary statistics for the key variables used in the firm-level regressions with $K \geq 10$ cutoff. Panel B highlights firm size differences between public and private firms for different capital stock (K) cutoffs and the matched sample. Firm size is proxied by capital stock (K). Similar to Asker et al. (2011), the matching is done on the capital stock (K) measure in the year that a private firm enters the sample. To be included, each matched pair is required to have no more than a 10% size difference in the year of the match. The matching technique is based on a nearest neighbor approach and the matched pair is kept until either the private firm is no longer in the sample or the matched public firm is no longer in the sample. If a public firm that has been matched leaves the sample, a new public firm is matched to the private firm based on the capital stock in that firm-year. The p-values are based on tests of differences in means (respectively medians). ***, **, * indicates statistical significance at the 1%, 5% and 10% level respectively.

Panel A: Descriptive Statistics for key variables

I/K:	Public	Private	Difference	p-value
Mean	0.45	0.35	0.10***	0.000
Median	0.37	0.28	0.09***	0.000
Standard Deviation	0.36	0.31		
Log(I):	Public	Private	Difference	p-value
Mean	3.58	2.00	1.59***	0.000
Median	3.61	1.95	1.67***	0.000
Standard Deviation	1.43	0.91		
Marginal q: Natural Gas Prices	$/MMBtu			
Mean	4.90			
Median	4.48			
Standard Deviation	1.67			
Number of Years in Sample	13			

Panel B: Firm Size Comparisons for Different Capital Stock Cutoffs and the Matched Sample

		Public	Private	Difference	p-value
	Mean	262.10	35.34	226.76***	0.000
$K \geq 10$	Median	104	21	83***	0.000
	N	569	1813		
	Mean	331.80	75.55	256.25***	0.000
$K \geq 30$	Median	145	48	97***	0.000
	N	441	567		
	Mean	383.21	116.41	266.79***	0.000
$K \geq 50$	Median	177	78	99***	0.000
	N	374	273		
	Mean	22.07	22.15	-0.08	0.95
Matched sample	Median	14	15	-1	0.86
	N	1588	1588		

Table 2. Firm-level Investments by Year: Private vs. Public Firms

This table compares the investment intensity (I/K) levels between private and public firms for every year during the sample period from 2000 to 2012. The investment (I) and capital stock (K) variables are defined in Table 1. The variable NG State Prices takes on three values: (1) Low; (2) Med and (3) High based on the natural gas price terciles (respectively lowest, middle, and highest third) during the sample period from 2000 to 2012. The differences in mean investment intensity levels between public and private firms are reported with statistical significance based on a t-test. ***, **, * indicates statistical significance at the 1%, 5% and 10% level respectively.

		Mean Comparison		
Year	NG State Prices	Public	Private	Difference
2000	Low	0.52	0.48	0.05
2001	Med	0.50	0.48	0.02
2002	Low	0.40	0.30	0.09
2003	Med	0.43	0.33	0.11**
2004	Med	0.51	0.36	0.15***
2005	High	0.59	0.43	0.15**
2006	High	0.61	0.41	0.19***
2007	High	0.63	0.39	0.24***
2008	High	0.51	0.36	0.15**
2009	Low	0.16	0.20	-0.04
2010	Med	0.29	0.24	0.05
2011	Low	0.34	0.27	0.07
2012	Low	0.29	0.21	0.08

Table 3. Firm-level Investments in Different Natural Gas Price Environments: Private vs. Public Firms

This table tests for differences in investment levels in both low and high price environments across public and private firms. The price environment indicator variables "Low" and "High" are based on the natural gas price terciles (respectively lowest and highest tercile) during the sample period from 2000 to 2012. The table reports results for investments divided by the beginning of year capital stock measure (I/K) in columns (1) to (4) and for the logarithm of investment levels (Log(I)) in columns (5) to (8). The investment (I) and capital stock (K) variables are defined in Table 1. The different columns report the results for different samples based on size requirements, specifically columns (1)-(2) and (5)-(6) impose minimum capital stock levels of $K \geq 10$, while columns (3)-(4) and (7)-(8) report results for our matched public-private sample, with matching based on capital stock (see Table 1 for details). The sample period is 2000 to 2012. All regressions include firm-level fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmFE_i$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$I_{i,t} = \alpha + \beta_1 Low_t + \beta_2 Low_t * Private_i + \beta_3 High_t + \beta_4 High_t * Private_i + \beta_5 Private_i + FirmFE_i + \varepsilon_{i,t}$$

	Dependent Variable = I/K				Dependent Variable = Log(I)			
	$K \geq 10$		Matched on K		$K \geq 10$		Matched on K	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) Low_t	-0.085***	-0.115***	-0.105***	-0.136***	-0.199***	-0.237***	-0.192***	-0.228***
	[-5.56]	[-4.14]	[-5.06]	[-3.79]	[-5.82]	[-4.16]	[-5.64]	[-4.83]
(β_2) Low_t * $Private_i$		0.043		0.062		0.056		0.072
		[1.29]		[1.52]		[0.79]		[1.06]
(β_3) $High_t$	0.074***	0.160***	0.135***	0.229***	0.299***	0.470***	0.440***	0.584***
	[3.48]	[3.30]	[4.94]	[4.92]	[6.61]	[5.05]	[9.25]	[8.46]
(β_4) $High_t$ * $Private_i$		-0.113**		-0.187***		-0.226**		-0.289***
		[-2.11]		[-3.45]		[-2.13]		[-3.13]
(β_5) $Private_i$	Absorbed by $FirmFE_i$							
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.042	0.050	0.042	0.054	0.084	0.089	0.114	0.124
N - Total Firm Years	2382	2382	3176	3176	2382	2382	3176	3176
Private Firm Years	1813	1813	1588	1588	1813	1813	1588	1588
Public Firm Years	569	569	1588	1588	569	569	1588	1588

Table 4. Firm-level Investment Sensitivities to Natural Gas Prices: Private vs. Public Firms

This table tests for differences in investment sensitivity to changes in marginal q across public and private firms. Marginal q is proxied by the price of natural gas (NG) in these regressions. The sample period is 2000 to 2012. Panel A reports results for investments divided by the beginning of year capital stock measure (I/K), while Panel B reports results for the logarithm of investment levels (Log(I)). The investment (I) and capital stock (K) variables are defined in Table 1. The different columns report the results for different samples based on size requirements, specifically columns (1) to (6) impose minimum capital stock levels (K ≥ 10, K≥ 30, and K≥ 50 respectively), while columns (7) and (8) report results for our matched public-private sample, with matching based on capital stock (see Table 1 for details). All regressions include firm-level fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmFE_i$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: $I/K_{i,t} = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

Panel B: $log(I_{i,t}) = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

Panel A: Dependent Variable = I/K

	K ≥ 10		K ≥ 30		K ≥ 50		Matched on K	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) NG_t	0.036***	0.065***	0.040***	0.059***	0.045***	0.059***	0.053***	0.083***
	[7.03]	[5.33]	[5.46]	[4.81]	[5.85]	[5.88]	[4.44]	[3.70]
(β_2) NG_t * $Private_i$		-0.039***		-0.035**		-0.037**		-0.059**
		[-2.93]		[-2.38]		[-2.37]		[-2.52]
(β_3) $Private_i$				Absorbed by $FirmFE_i$				
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.032	0.040	0.054	0.064	0.088	0.101	0.031	0.041
N - Total Firm Years	2382	2382	1008	1008	647	647	3176	3176
Private Firm Years	1813	1813	567	567	273	273	1588	1588
Public Firm Years	569	569	441	441	374	374	1588	1588
Effect of NG_t on Private Firms								
$\beta_1 + \beta_2 =$		0.026***		0.024***		0.022*		0.024***
		[4.94]		[2.87]		[1.88]		[3.90]

Panel B: Dependent Variable = Log(I)

	K ≥ 10		K ≥ 30		K ≥ 50		Matched on K	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) NG_t	0.123***	0.185***	0.156***	0.199***	0.189***	0.222***	0.151***	0.196***
	[9.24]	[7.22]	[7.55]	[7.90]	[7.50]	[8.52]	[6.79]	[5.13]
(β_2) NG_t * $Private_i$		-0.084***		-0.083**		-0.090		-0.091**
		[-2.80]		[-2.03]		[-1.52]		[-2.15]
(β_3) $Private_i$				Absorbed by $FirmFE_i$				
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.076	0.083	0.112	0.119	0.163	0.172	0.093	0.102
N - Total Firm Years	2382	2382	1008	1008	647	647	3176	3176
Private Firm Years	1813	1813	567	567	273	273	1588	1588
Public Firm Years	569	569	441	441	374	374	1588	1588
Effect of NG_t on Private Firms								
$\beta_1 + \beta_2 =$		0.102***		0.116***		0.132**		0.105***
		[6.63]		[3.62]		[2.48]		[5.82]

Table 5. Firm-level Investment: Size vs. Listing Status

This table compares the effects of differences in size and listing status on investment sensitivity to changes in marginal q across public and private firms. Marginal q is proxied by the price of natural gas (NG) in these regressions. The sample period is 2000 to 2012. The investment (I) and capital stock (K) variables are defined in Table 1. The size indicator variable takes the value of one for a firm that is above the median firm in terms of capital stock (K) in a given year. The sample has a minimum capital stock cutoff of K $\geq$ 10. All regressions include firm-level fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmFE_i$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$I_{i,t} = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + \beta_4 Size_{i,t} + \beta_5 NG_t * Size_{i,t} + FirmFE_i + \varepsilon_{i,t}$$

	I/K	**Log(I)**	
	(1)	(2)	(3)
NG_t	0.084***	0.181***	0.155*
	[5.34]	[5.04]	[1.89]
NG_t * $Private_i$	-0.042***	-0.087**	-0.080*
	[-2.90]	[-2.49]	[-1.79]
$Private_i$		Absorbed by $FirmFE_i$	
Size $Dummy_{i,t}$	-0.149***	0.193	
	[-2.60]	[1.15]	
NG_t * Size $Dummy_{i,t}$	-0.021**	0.005	
	[-2.08]	[0.16]	
Log($Size_{i,t}$)			0.236**
			[2.36]
NG_t * Log($Size_{i,t}$)			0.006
			[0.43]
$FirmFE_i$	Yes	Yes	Yes
R^2 Within	0.119	0.094	0.119
N - Total Firm Years	2382	2382	2382
Private Firm Years	1813	1813	1813
Public Firm Years	569	569	569

Table 6. Firm-level Investment Sensitivities: Alternate Specifications

This table reports firm-level regressions of investment sensitivity to natural gas prices for public and private firms using several alternative specifications relative to the main specification of Table 4. Investment levels (I) and capital stock (K) are defined in Table 1. Panel A reports results for investments divided by the beginning of year capital stock measure (I/K), while Panel B reports results for the logarithm of investment levels (Log(I)). The sample period is 2000 to 2012. The columns report the results for several alternative specifications. Specifically, columns (1) and (2) show results for the futures "strip" price of natural gas instead of the spot price. Columns (3) and (4) use observations based on quarterly data as opposed to annual data. Columns (5) and (6) exclude the largest firms (firms with K ≥ 500) from the sample. Columns (7) and (8) include time fixed effects in the specification. All regressions include firm-level fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmFE_i$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: $I/K_{i,t} = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

Panel B: $log(I_{i,t}) = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

Panel A: Dependent Variable = I/K

	Futures		Quarterly		Excluding Largest Firms		Time FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) NG_t	0.025***	0.049***	0.008***	0.013***	0.036***	0.069***	Absorbed by $TimeFE_t$	
	[5.54]	[4.60]	[7.40]	[5.16]	[6.69]	[4.84]		
(β_2) NG_t * $Private_i$		-0.032***		-0.007**		-0.042***		-0.030**
		[-2.78]		[-2.56]		[-2.80]		[-2.40]
(β_3) $Private_i$				Absorbed by $FirmFE_i$				
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.025	0.033	0.017	0.020	0.031	0.038	0.167	0.171
N - Total Firm Years	2382	2382	9525	9525	2302	2302	2382	2382
Private Firm Years	1813	1813	7244	7244	1806	1806	1813	1813
Public Firm Years	569	569	2281	2281	496	496	569	569
Effect of NG_t on Private Firms								
$\beta_1 + \beta_2$ =		0.017***		0.006***		0.026***		NM
		[3.58]		[5.54]		[4.93]		NM

Panel B: Dependent Variable = Log(I)

	Futures		Quarterly		Excluding Largest Firms		Time FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(β_1) NG_t	0.093***	0.145***	0.082***	0.145***	0.120***	0.179***	Absorbed by $TimeFE_t$	
	[7.95]	[6.48]	[8.86]	[7.42]	[8.93]	[5.89]		
(β_2) NG_t * $Private_i$		-0.071***		-0.085***		-0.077**		-0.065**
		[-2.71]		[-3.88]		[-2.29]		[-2.21]
(β_3) $Private_i$				Absorbed by $FirmFE_i$				
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.069	0.076	0.041	0.049	0.071	0.076	0.150	0.154
N - Total Firm Years	2382	2382	9525	9525	2302	2302	2382	2382
Private Firm Years	1813	1813	7244	7244	1806	1806	1813	1813
Public Firm Years	569	569	2281	2281	496	496	569	569
Effect of NG_t on Private Firms								
$\beta_1 + \beta_2$ =		0.075***		0.060***		0.102***		NM
		[5.56]		[6.10]		[6.96]		NM

Table 7. Shale Discoveries and Investment: Private vs. Public Firms

This table reports firm-county-level regressions which measure the responsiveness of investments from public and private firms to positive localized shocks to their investment opportunity set generated by shale gas discoveries. The dependent variable is a measure of investment by firm (i) in county (j) at time (t). The sample period spans from 2000 to 2012, and covers shale discoveries that occur between 2003 and 2010, in order to provide for a three year pre- and post-period window for each shale discovery. For a given shale discovery, firm-level investments in a county are aggregated into two separate time periods, one for the average number of wells drilled three years prior to a discovery and one for the average number of wells drilled three years after a discovery. For example, a shale discovery in 2005 would have a single pre-period covering investments for 2002-2004 and a single post period covering investments for 2005-2007. In order to be in our sample, a firm is required to have drilling activity in both the time period before and after the discovery. The price of natural gas is the average wellhead price over the three year period being aggregated. Investment is measured as either the logarithm of wells drilled or the number of new wells drilled by a firm in a county over the three year period being aggregated. The coefficient for $Private_i$ is not reported because it is not identified with $FirmCountyFE_{i,j}$ fixed effects. Standard errors are clustered by county, with t-statistics reported in brackets below the coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Investment_{i,j,t} = \alpha + \beta_1 NG_t + \beta_2 PostDiscovery_{j,t} + \beta_3 PostDiscovery_{j,t} * Private_i + \beta_4 Private_i + FirmCountyFE_{i,j} + \varepsilon_{i,j,t}$$

	Investment = Log(New Wells)		Investment = New Wells	
	(1)	(2)	(3)	(4)
(β_1) NG_t	0.064	0.072	0.626	0.740
	[1.40]	[1.64]	[1.11]	[1.35]
(β_2) $PostDiscovery_{j,t}$	0.201***	0.399***	2.785***	5.373***
	[3.00]	[3.98]	[3.08]	[3.51]
(β_3) $PostDiscovery_{j,t}$ * $Private_i$		-0.430***		-5.634***
		[-4.51]		[-3.87]
(β_4) $Private_i$		Absorbed by $Firm\text{-}CountyFE_{i,j}$		
$Firm\text{-}CountyFE_{i,j}$	Yes	Yes	Yes	Yes
R^2 Within	0.034	0.076	0.029	0.063
N	1106	1106	1106	1106
Public Firm-County-Years	612	612	612	612
Private Firm-County-Years	494	494	494	494
Effect of $PostDiscovery_{j,t}$ on Private Firms				
$\beta_2 + \beta_3 =$		-0.032		-0.261
		[-0.62]		[-0.75]

Table 8. Shale Discoveries and Investment: Size vs. Listing Status

This table reports firm-county-level regressions which measure the importance of size and listing status for investment responses by public and private firms to shale gas discoveries. The dependent variable is a measure of investment by firm (i) in county (j) at time (t). The sample period spans from 2000 to 2012, and covers shale discoveries that occur between 2003 and 2010, in order to provide for a three year pre- and post-period for each shale discovery. For a given shale discovery, firm-level investments in a county are aggregated into two separate time periods, one for the average number of wells drilled three years prior to a discovery and one for the average number of wells drilled three years after a discovery. In order to be in our sample, a firm is required to have drilling activity in both the time period before and after the discovery. The price of natural gas is the average wellhead price over the three year period being aggregated. Investment is measured as either the logarithm of wells drilled or the number of new wells drilled by a firm in a county over the three year period being aggregated. Capital stock at the firm-level is used as our proxy for firm size; the size indicator variable ($Size\ Dummy_{it}$) takes the value of one for firms with above median size during the aggregated three year period, and zero otherwise. The coefficient for $Private_i$ is not reported because it is not identified with $FirmCountyFE_{i,j}$ fixed effects. Standard errors are clustered by county, with t-statistics reported in brackets below the coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Investment_{i,j,t} = \alpha + \beta_1 NG_t + \beta_2 PostDiscovery_{j,t} + \beta_3 Size\ Dummy_{i,t} + \beta_4 PostDiscovery_{j,t} * Private_i + \beta_5 PostDiscovery_{j,t} * Size\ Dummy_{i,t} + \beta_6 NG_t * Private_i + \beta_7 Private_i + FirmCountyFE_{i,j} + \varepsilon_{i,j,t}$$

	Investment = Log(New Wells)			Investment = New Wells		
	(1)	(2)	(3)	(4)	(5)	(6)
(β_1) NG_t	0.072	0.073*	0.035	0.740	0.738	0.785
	[1.64]	[1.66]	[0.55]	[1.34]	[1.35]	[0.79]
(β_2) $PostDiscovery_{j,t}$	0.399***	0.422***	0.388***	5.442***	5.316***	5.359***
	[3.95]	[3.16]	[2.75]	[3.53]	[2.77]	[2.65]
(β_3) $Size\ Dummy_{i,t}$	-0.012	-0.000	-0.021	-1.765	-1.831	-1.804
	[-0.08]	[-0.00]	[-0.12]	[-1.14]	[-0.99]	[-0.99]
(β_4) $PostDiscovery_{j,t}$ * $Private_i$	-0.431***	-0.450***	-0.397***	-5.717***	-5.612***	-5.678***
	[-4.43]	[-3.77]	[-3.06]	[-3.89]	[-3.18]	[-2.94]
(β_5) $PostDiscovery_{j,t}$ * $Size\ Dummy_{i,t}$		-0.029	-0.019		0.156	0.144
		[-0.20]	[-0.13]		[0.08]	[0.07]
(β_6) NG_t * $Private_i$			0.079			-0.098
			[1.34]			[-0.10]
(β_7) $Private_i$	Absorbed by $Firm\text{-}CountyFE_{i,j}$					
$Firm\text{-}CountyFE_{i,j}$	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.076	0.076	0.080	0.064	0.064	0.064
N	1106	1106	1106	1106	1106	1106
Public Firm-County-Years	612	612	612	612	612	612
Private Firm-County-Years	494	494	494	494	494	494
Effect of $PostDiscovery_{j,t}$ on Private Firms						
$\beta_2 + \beta_4$ =	-0.032	-0.028	-0.009	-0.276	-0.295	-0.319
	[-0.62]	[-0.52]	[-0.17]	[-0.78]	[-0.76]	[-0.84]

Table 9. Pre-Discovery Parallel Trends

This table reports falsification regressions designed to test for "parallel trends" prior to shale discoveries. The dependent variable is the logarithm of wells or number of wells drilled by firm (i) in county (j) at time (t). The falsification tests in this table are based on moving a discovery in a given county three years earlier to create a $PlaceboDiscovery_{j,t}$ variable. Because this specification tests for trends during pre-discovery time periods, the sample time period is reduced by three years. Specifically, we move forward all discoveries occurring between 2006 and 2010 by three years so our "Placebo" discoveries range from 2003 to 2007. Firm level investments in a given county are aggregated into two separate time periods, one for the average number of wells three years prior to a placebo discovery and one for the average number of wells three years after a placebo discovery. In order to be in our sample, a firm is required to have drilling activity in both the time period before and after the placebo discovery. The resulting dataset has two time periods for a firm active in a given shale discovery county, one for the time period before the placebo discovery and one for the time period after a placebo discovery. The price of natural gas is the average wellhead price over the three year period being aggregated. All regressions include firm-county fixed effects (fixed effect for each firm in each county). The coefficient for $Private_i$ is not reported because it is not identified with $FirmCountyFE_{i,j}$ fixed effects. Standard errors are clustered by county, with t-statistics reported in brackets below the coefficient estimates, where * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Investment_{i,j,t} = \alpha + \beta_1 NG_t + \beta_2 PlaceboDiscovery_{j,t} + \beta_3 Private_i + \beta_4 Size\ Dummy_{i,t} + \beta_5 PlaceboDiscovery_{j,t} * Private_i + \beta_6 PlaceboDiscovery_{j,t} * Size\ Dummy_{i,t} + \beta_7 NG_t * Private_i + \beta_8 Private_i + FirmCountyFE_{i,j} + \varepsilon_{i,j,t}$$

	Investment = Log(New Wells)		Investment = New Wells	
	(1)	(2)	(3)	(4)
NG_t	0.117***	0.149***	0.922***	1.131**
	[5.16]	[3.48]	[4.22]	[2.64]
$PlaceboDiscovery_{j,t}$		-0.046		-0.324
		[-0.38]		[-0.30]
Size $Dummy_{i,t}$		0.056		-0.752
		[0.41]		[-0.59]
$PlaceboDiscovery_{j,t}$ * $Private_i$		0.016		0.454
		[0.15]		[0.46]
$PlaceboDiscovery_{j,t}$ * Size $Dummy_{i,t}$		0.053		1.056
		[0.48]		[1.06]
NG_t * $Private_i$		-0.060		-0.788*
		[-1.36]		[-1.90]
$Private_i$	Absorbed by Firm-$CountyFE_{i,j}$			
Firm-$CountyFE_{i,j}$	Yes	Yes	Yes	Yes
R^2 Within	0.082	0.091	0.064	0.083
N	994	994	994	994
Public Firm-County-Years	514	514	514	514
Private Firm-County-Years	480	480	480	480

Table 10. Firm-County-level Investment and Changes in Natural Gas Prices

This table tests for differences in investment sensitivity to changes in marginal q at the county level across public and private firms. The dependent variable is based on the number of non-shale wells by firm (i) in county (j) at time (t). To avoid any effect of shale discoveries, we focus only on non-shale wells and only in county-years that do not experience a shale discovery. Marginal q is proxied by the price of natural gas (NG) in these regressions. The sample period ranges from 2000 to 2012. The sample is based on firms with minimum capital stock levels of $K \geq 10$. All regressions include firm-county-level fixed effects. Standard errors are clustered by county, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmCountyFE_{i,j}$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Investment_{i,j,t} = \alpha + \beta_1 NG_t + \beta_2 NG_{j,t} * Private_i + \beta_3 Private_i + FirmCountyFE_{i,j} + \varepsilon_{i,j,t}$$

	Investment = Log(New Wells)		Investment = New Wells	
	(1)	(2)	(3)	(4)
(β_1) NG_t	0.071***	0.100***	0.528***	0.831***
	[7.49]	[7.52]	[4.31]	[3.79]
(β_2) NG_t * $Private_i$		-0.067***		-0.680***
		[-4.58]		[-2.96]
(β_3) $Private_i$		Absorbed by Firm-$CountyFE_{i,j}$		
Firm-$CountyFE_{i,j}$	Yes	Yes	Yes	Yes
R^2 Within	0.024	0.030	0.013	0.018
N	9816	9816	9816	9816
Effect of NG_t on Private Firms				
$\beta_1 + \beta_2 =$		0.034***		0.151***
		[3.62]		[3.20]

Table 11. Sales of Shale Acreage

This table reports shale acreage transactions between publicly-traded and privately-held firms in two shale discoveries. The analysis is based on detailed production level data of the Woodford and Cana shale in Oklahoma. Specifically, using detailed production data from the Oklahoma Corporation Commission over the 2003 to 2010 time period in four counties ((1): Canadian county (discovery in 2008), (2): Coal county (discovery in 2006), (3) Pittsburg county (discovery in 2006), and (4) Hughes county (discovery in 2006)), we first compute the exact acreage that has pre-existing non-shale production prior to shale discoveries. We then compute the proportion of this acreage that is sold off after the shale discoveries are made. We infer that a transfer of acreage from producer A to producer B has occurred when producer B is producing on the acreage that producer A was developing prior to the shale discovery. The proportion of acreage held by private firms sold off to other private (respectively public) firms is computed, as well as the proportion of public acreage sold off to private (respectively other public) firms. The differences in proportions sold off are reported with statistical significance based on a t-test. ***, **, * indicates statistical significance at the 1%, 5% and 10% level respectively.

Panel A: Acreage Overview

Number of Acres Held by Private Companies Prior to Shale Discovery	36,480
Number of Acres Held by Public Companies Prior to Shale Discovery	30,080
Total	**66,560**

Panel B: Drilling Right Sale Comparisons

Private to Public Sale	Public to Public Sale	Difference
63.2%	21.3%	41.9%***

Private to Public Sale	Private to Private Sale	Difference
63.2%	5.3%	57.9%***

Private to Public Sale	Public to Private Sale	Difference
63.2%	0.0%	63.2%***

Table 12. Shareholder-Manager Agency Costs: Investment Levels Comparison

This table compares the investment intensity (I/K) levels between private firms, public firms with above median insider ownership (High Insider Ownership), and public firms with below median insider ownership (Low Insider Ownership). The sample period is from 2000 to 2012. Annual data on insider ownership was hand-collected from proxy statements. The investment (I) and capital stock (K) variables are defined in Table 1. The variable NG State Prices takes on three values: (1) Low; (2) Med and (3) High based on the natural gas price terciles (respectively lowest, middle, and highest third). The differences in mean investment intensity levels between public firms with high insider ownership vs. public firms with low insider ownership, and respectively public firms with high insider ownership vs. private firms are reported with statistical significance based on a t-test. ***, **, * indicates statistical significance at the 1%, 5% and 10% level

		Mean I/K			Difference	
Year	NG State Prices	Public High Insider Ownership	Public Low Insider Ownership	Private	Public High Insider Ownership vs. Public Low Insider Ownership	Public High Insider Ownership vs. Private
2000	Low	0.62	0.50	0.48	0.13	0.15
2001	Med	0.55	0.44	0.48	0.11	0.07
2002	Low	0.35	0.34	0.30	0.01	0.05
2003	Med	0.41	0.43	0.33	-0.02	0.09
2004	Med	0.60	0.47	0.36	0.13	0.24**
2005	High	0.72	0.51	0.43	0.21	0.28*
2006	High	0.73	0.59	0.41	0.14	0.31**
2007	High	0.82	0.51	0.39	0.30**	0.43***
2008	High	0.58	0.49	0.36	0.09	0.22**
2009	Low	0.15	0.17	0.20	-0.02	-0.05**
2010	Med	0.25	0.28	0.24	-0.03	0.01
2011	Low	0.37	0.28	0.27	0.08	0.09
2012	Low	0.27	0.35	0.21	-0.08	0.06

Table 13. Shareholder-Manager Agency Costs: Investment Sensitivity Comparison

This table reports firm-level regressions of investment sensitivity to natural gas prices for public and private firms, excluding public firms with low insider ownership (below median insider ownership level). Annual data on insider ownership was hand-collected from proxy statements. The sample period is from 2000 to 2012. Investment levels (I) and capital stock (K) are defined in Table 1. The dependent variables in these regressions are different measures of investment activity. Panel A reports results for investments divided by the beginning of year capital stock measure (I/K), while Panel B reports results for the logarithm of investment levels (Log(I)). The columns report results for different subsamples based on size requirements, specifically columns (1) to (6) require different minimum levels of capital stock. All regressions include firm-level fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. The coefficient for $Private_i$ is not reported because it is not identified with $FirmFE_i$ fixed effects. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: $I/K_{i,t} = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

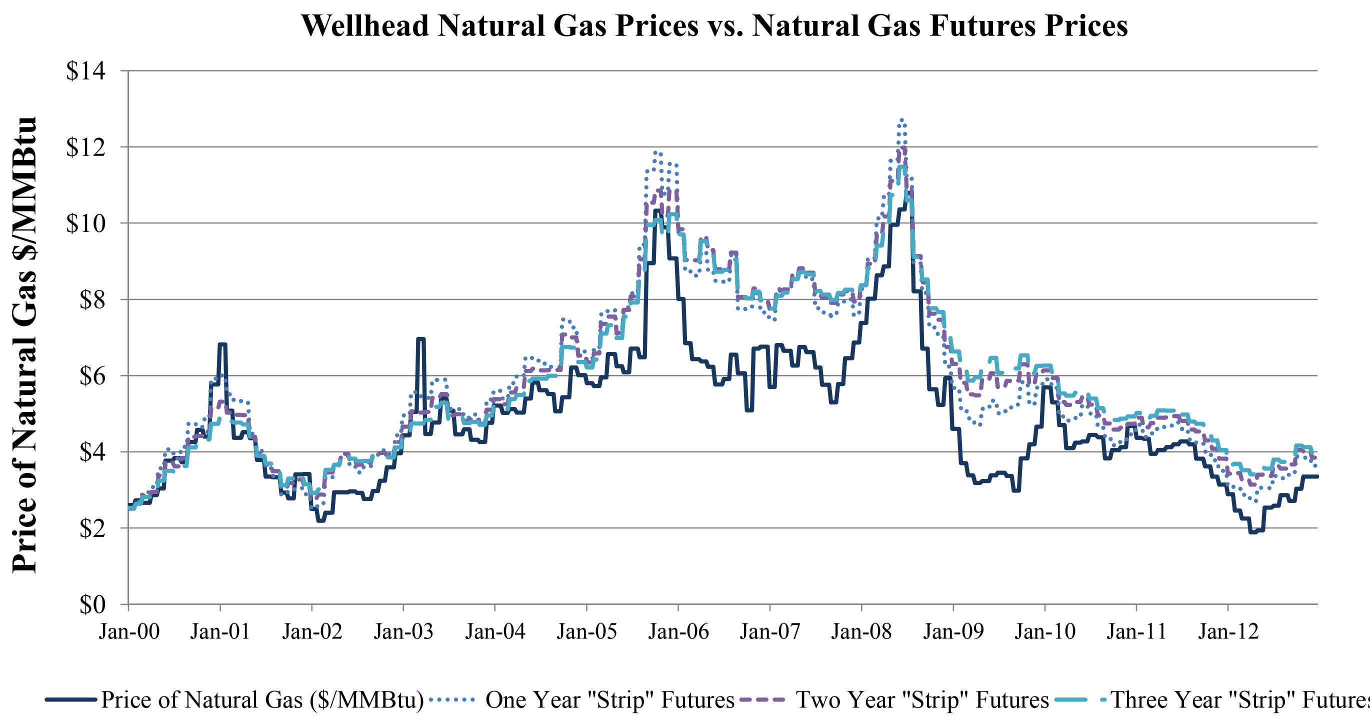
Panel B: $log(I_{i,t}) = \alpha + \beta_1 NG_t + \beta_2 NG_t * Private_i + \beta_3 Private_i + FirmFE_i + \varepsilon_{i,t}$

Panel A: Dependent Variable = I/K

	K ≥ 10		K ≥ 30		K ≥ 50	
	(1)	(2)	(3)	(4)	(5)	(6)
(β_1) NG_t	0.034***	0.099***	0.041***	0.088***	0.044***	0.075***
	[6.11]	[4.63]	[4.67]	[4.60]	[4.66]	[5.85]
(β_2) NG_t * $Private_i$		-0.072***		-0.065***		-0.053***
		[-3.30]		[-3.10]		[-3.03]
(β_3) $Private_i$			Absorbed by $FirmFE_i$			
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.028	0.040	0.052	0.078	0.076	0.103
N - Total Firm Years	2051	2051	761	761	442	442
Private Firm Years	1813	1813	567	567	273	273
Public Firm Years	238	238	194	194	169	169
Effect of NG_t on Private Firms						
$\beta_1 + \beta_2 =$		0.026***		0.024***		0.022*
		[4.94]		[2.86]		[1.88]

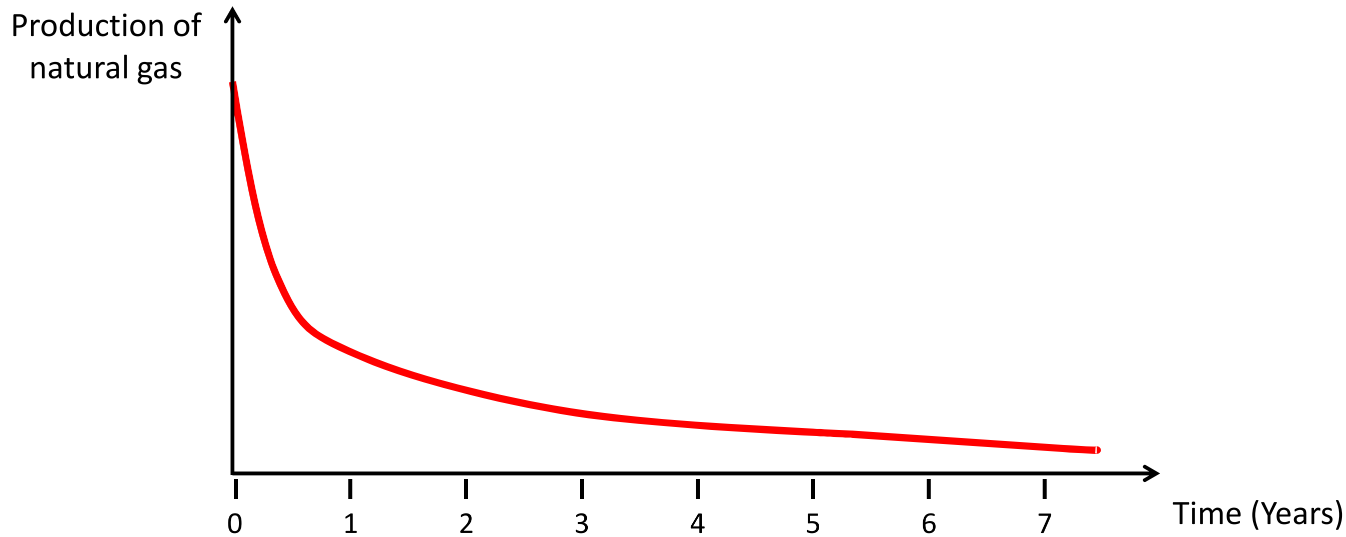
Panel B: Dependent Variable = Log(I)

	K ≥ 10		K ≥ 30		K ≥ 50	
	(1)	(2)	(3)	(4)	(5)	(6)
(β_1) NG_t	0.114***	0.216***	0.140***	0.205***	0.175***	0.238***
	[7.94]	[6.09]	[5.33]	[4.97]	[4.92]	[5.93]
(β_2) NG_t * $Private_i$		-0.114***		-0.089*		-0.107
		[-2.96]		[-1.71]		[-1.60]
(β_3) $Private_i$			Absorbed by $FirmFE_i$			
$FirmFE_i$	Yes	Yes	Yes	Yes	Yes	Yes
R^2 Within	0.065	0.071	0.083	0.090	0.122	0.133
N - Total Firm Years	2051	2051	761	761	442	442
Private Firm Years	1813	1813	567	567	273	273
Public Firm Years	238	238	194	194	169	169
Effect of NG_t on Private Firms						
$\beta_1 + \beta_2 =$		0.102***		0.116***		0.132**
		[6.63]		[3.61]		[2.47]

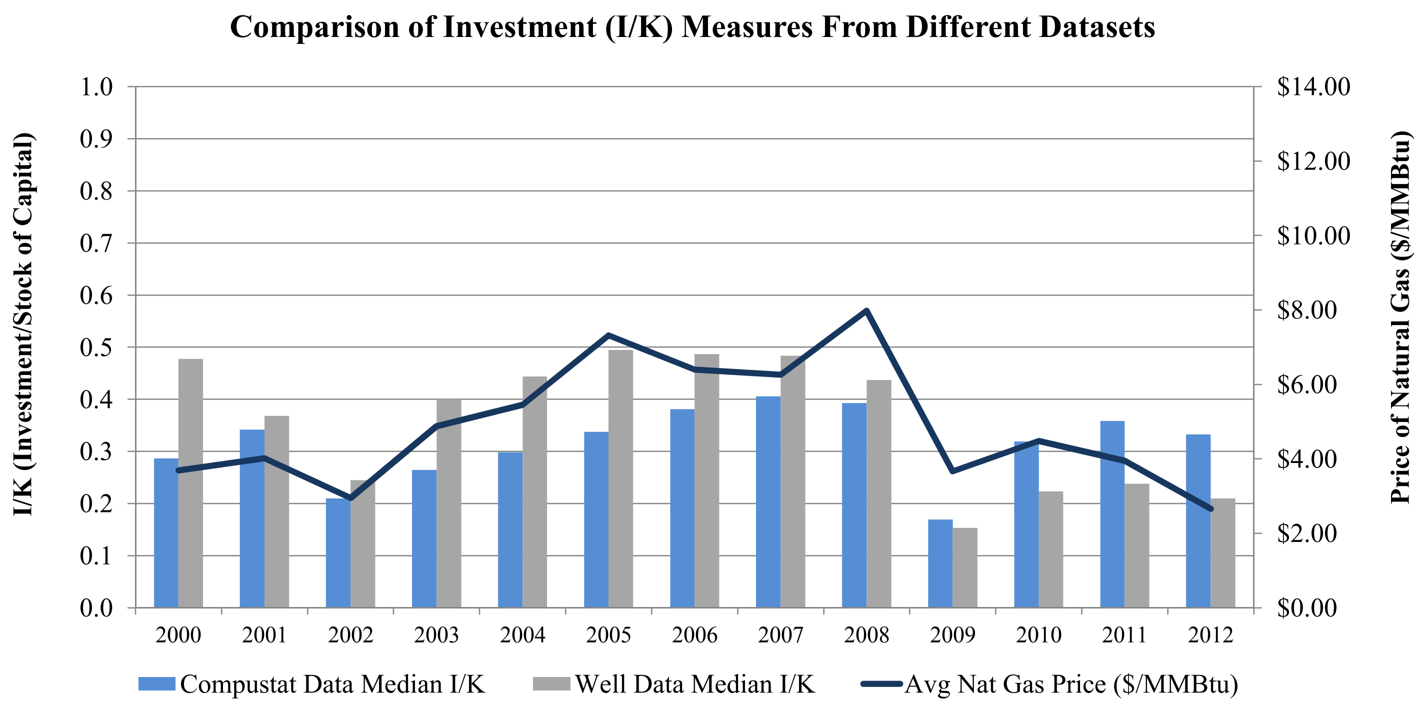


Appendix Figure A: U.S. Natural Gas Wellhead and Futures Prices

This figure plots wellhead natural gas prices and three different measures of natural gas futures prices. The wellhead natural gas price measure is the primary measure used in the study and is based on data from the U.S. Energy Information Administration (EIA). The natural gas price futures measures are calculated using the industry standard "strip" price convention which provides an average of futures prices of different maturities. Specifically, the "strip" price for any given time period to come is the average of all monthly natural gas futures prices over that time period to come. For instance, the one year strip is the average of all monthly futures prices with maturity date of twelve months or less. The futures prices used are NYMEX futures prices, which have a delivery point of a single location in Henry Hub, LA. This data is from Bloomberg.



Appendix Figure B: Example of Project Timeline
This figure plots a typical production curve over time for a natural gas well, once production begins. It is based on similar figures found in Lake, Martin, Ramsey, and Titman (2012) as well as company investor presentations.



Appendix Figure C: Comparison of Investment (I/K) Measures based on Accounting Data versus Drilling Data

This figure compares the medians of two different measures of Investment/Capital Stock (I/K), one based on Compustat accounting data and one based on our drilling data, plotted with the annual average wellhead price of natural gas. The comparison is performed on the same set of sample firms, meaning that a firm must have both Compustat accounting data and drilling activity data in a given year, and be classified in SIC 1311 (Crude Petroleum and Natural Gas). The Compustat measure of I/K is calculated as investment (code: capx) divided by beginning of period net property, plant, and equipment (code: ppent). The drilling data measure of I/K is calculated as investment (proxied by the number wells drilled) divided by capital stock (proxied by the number of wells completed in the previous three years).

Appendix A. Correlation of Wellhead Price with Futures Prices and Regional Prices

This table reports correlations of the monthly wellhead price of natural gas reported by the EIA (the price data used for our main tests) with natural gas futures prices and regional natural gas prices over the time period 2000 to 2012. Natural gas futures prices were collected from Bloomberg, and are calculated using the industry standard "strip" convention. The "strip" price of natural gas is the average price of natural gas futures prices with monthly expirations over a certain time period. For example, the 12-month strip price for January 2010 is comprised of the average of the futures price for delivery in February 2010, March 2010 and so on until delivery in January 2011. This smoothes out any seasonal fluctuations in futures prices. Regional hub prices are the average of spot prices at natural gas trading hubs in a given region; these were also obtained from Bloomberg.

Correlation With Futures Prices	Futures 12 Month "Strip"	Futures 24 Month "Strip"	Futures 36 Month "Strip"	
Wellhead Price of Natural Gas ($/MMBtu)	95%	91%	88%	
Correlation With Regional Prices	U.S. Gulf Coast	U.S. Midwest	U.S. Northeast	U.S. West
Wellhead Price of Natural Gas ($/MMBtu)	96%	95%	96%	91%

Appendix B. Shale Discovery Background

Shale discoveries at the county level represent local growth opportunity shocks for firms with existing operations in these areas. These shocks are used in the difference-in-differences approach shown in Table 7. Panel A reports the number of shale discoveries at the county level over time. Shale discoveries are defined as in Gilje (2011), which relies on the number of horizontal wells drilled in a given county. Data requirements limit the discoveries to those made in 2010 or before. Specifically, we require three years of data for the post-discovery window and our dataset ends in 2012 so the latest discoveries have to be in 2010 to have a valid post-discovery window (2010-2012). Panel B compares shale-related production and well costs for private and public firms. Data on well production and well costs is for the Woodford (OK) and Cana (OK) shale discoveries, where detailed data on production and well costs are available from the Oklahoma Corporation Commission. Specifically, the panel reports results for shale well production (respectively shale well costs) regressed on a private dummy variable and township (precise location) and year fixed effects. These regressions test whether privately-held firms have different production levels (respectively costs) from the shale wells they undertake relative to their publicly-traded counterparts. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: Shale Discoveries Over Time

Year	Discoveries	Cumulative Discoveries
2003	4	4
2004	10	14
2005	3	17
2006	13	30
2007	14	44
2008	25	69
2009	15	84
2010	18	102
Total		**102**

Panel B: Shale Well Production and Cost Comparison

	Dependent Variable	
	Log(Production)	Log(Cost)
	(1)	(2)
$Private_i$	-0.044	-0.037
	[-0.36]	[-0.83]
$YearFE_t$	Yes	Yes
$TownshipFE_j$	Yes	Yes
Wells Drilled by Private Firms	165	165
Wells Drilled by Public Firms	793	755
R^2	0.387	0.553
N	958	920

Appendix C. Well Cost Comparison Large Firms vs. Small Firms

This table reports the median drilling cost per well for publicly traded firms within the industry code SIC 1311 (Crude Petroleum and Natural Gas) that are also in the drilling data sample. Firms are divided into two groups: 1) Large firms, defined as firms with total assets above the median asset size for all public firms in a given year and 2) Small firms, defined as firms with total assets below the median asset size for all public firms in a given year. The well cost for a firm is based on capital expenditures divided by the total number of all wells drilled by that firm in a given year, which is hand-collected information from each firm's 10-K. The median asset values within small and large firms are also reported for each year. The differences in well cost and firm size (assets) across small and large firms are computed with statistical significance based on a median test (only p-values reported).

	Large Natural Gas Producers			Small Natural Gas Producers			Difference (only p-values reported)	
Year	Obs	Median Well Cost ($ Millions)	Median Assets ($ Millions)	Obs	Median Well Cost ($ Millions)	Median Assets ($ Millions)	Well Cost (p-value)	Assets (p-value)
2006	19	2.3	4829.8	19	2.8	893.0	0.330	0.000
2007	22	2.8	5529.2	23	2.2	861.0	0.460	0.000
2008	22	3.1	6234.9	22	3.7	1055.0	0.370	0.000
2009	18	4.5	6994.4	19	4.3	1435.0	0.870	0.000

Appendix D. Dataset Example

The drilling dataset used in this study is compiled from the Smith International Rig Count (now a subsidiary of Schlumberger). Below is a portion of the raw output of a download of this data. The unit of observation of the dataset is: Rig-Week, which is defined as the week that a drilling rig is actively drilling a well. The number of rig-week observations corresponds to the number of weeks it takes to drill a given well. Our firm-year and firm-county-year datasets are constructed by computing the number of wells a given firm starts in a year or county-year. The unique identifier used for a well is its state + county + well name + well number. This data spans from 1997 to 2012, however, we use the first 3 years of data to build our capital stock (K) measures. As such, our sample period with complete data runs from 2000 to 2012.

FRIDAY DATE	COUNTRY	STATE	COUNTY	TEXAS RR DISTRICT	PARENT COMPANY NAME	COMPANY NAME	CONTRACTOR	CONTRACT TYPE	RIG NAME NUMBER TXT	RIG TYPE	RIG DEPTH RATING (FT)	WELL NAME	WELL NUMBER	SPUD DATE	LEASE NAME	WELL STATUS	WELL TYPE	WELL DIRECTION	PROPOSED DEPTH	WELL TARGET	WELL LOCATION
4/13/2012	UNITED STATES	OK	CANADIAN		CIMAREX ENERGY	CIMAREX ENERGY	CACTUS DRILLING	Day Work	148	Land	10000	WARD	5-28H	4/8/2012	WARD	DRILLING	Dev	Horizontal	17350	Gas	OnShore
4/20/2012	UNITED STATES	OK	CANADIAN		CIMAREX ENERGY	CIMAREX ENERGY	CACTUS DRILLING	Day Work	148	Land	10000	WARD	5-28H	4/8/2012	WARD	DRILLING	Dev	Horizontal	17350	Gas	OnShore
4/27/2012	UNITED STATES	OK	CANADIAN		CIMAREX ENERGY	CIMAREX ENERGY	CACTUS DRILLING	Day Work	148	Land	10000	WARD	5-28H	4/8/2012	WARD	DRILLING	Dev	Horizontal	17350	Gas	OnShore
5/4/2012	UNITED STATES	OK	CANADIAN		CIMAREX ENERGY	CIMAREX ENERGY	CACTUS DRILLING	Day Work	148	Land	10000	WARD	5-28H	4/8/2012	WARD	DRILLING	Dev	Horizontal	17350	Gas	OnShore

Field Description	
Friday Date	Date of the survey on rig counts
Country	United States, all data for this rig count are from the U.S.
State	U.S. State of Drilling Rig Activity
Texas RR District	Texas Railroad Commission District (the governing body for oil and gas development in TX), if the well is in Texas
Parent Company Name	The parent company name of the firm drilling the well
Company Name	The company name of the firm drilling the well
Contractor	The 3rd party contractor that is hired to drill the well
Contract Type	Day work, turn key, or footage. This specifies whether firms pay by the day (Day Work), the foot (footage), or a fixed amount (Turn Key) for the well
Rig Name Number TXT	The rig number that the contracting firm has assigned to the rif
Rig Type	Land or Off-shore, all observations used in our study are for Land Rigs
Rig Depth Rating (FT)	The listed depth at which the rig is rated to drill
Well Name	The name of the well being drilled
Well Number	The number assigned to the well, the combination of well name with well number creates a unique identifier for the well
Spud Date	The date the drilling rig began drilling a well
Lease Name	The name of the lease that a well is on
Well Status	A field which indicates whether a well is drilling, rigging down, or rigging up.
Well Type	A field which indicates whether a well is Development or Exploratory
Well Direction	A field which indicates whether a well is being drilled vertically or horizontally
Proposed Depth	The proposed depth in feet of a well
Well Target	An indication of whether the well is targeting natural gas, oil, or water
Well Location	An indicator field for whether the well is onshore or offshore

Appendix E. Analysis of Company IPOs

This table reports data from the companies in SIC code 1311 that go public during the time period of shale discoveries in our study. It includes qualitative language on the use of proceeds, and classifies whether funding costly shale projects played a role in the decision to go public based on whether the proceeds were (1) going to be used for capital expenditures for drilling and development by a company with shale projects or (2) being used to repay prior indebtedness incurred to meet capital expenditure requirements. The source for the data is from the S-1 filings prior to when a company goes public.

Key	Firm	Fiscal Year	IPO related to funding shale projects?	Financing Language Details
1	Approach Resources Inc	2007	Yes	We intend to use the net proceeds of this offering to ***repay approximately $XX million outstanding under our revolving credit facility***, to repurchase shares of our common stock held by Neo Canyon Exploration, L.P. at a purchase price of $XX million and the remainder for general corporate purposes, ***including exploration and development activities, gas and oil reserves and leasehold acquisitions*** in the ordinary course of business and for working capital. At March 31, 2007, outstanding borrowings under our revolving credit facility totaled approximately $52.2 million with an interest rate of 7.02%. ***We incurred the debt under our revolving credit facility principally to meet our capital expenditure requirements*** and other working capital needs. We will have no outstanding borrowings under our revolving credit facility after the closing of this offering.
2	Bill Barrett Corp	2004	Yes	We intend to use the net proceeds of this offering to ***repay all of our outstanding indebtedness under our revolving credit facility*** and to ***fund exploration and development activities***, acquisitions, working capital and other general corporate purposes. Our revolving credit facility provides for commitments of $200 million with an initial borrowing base of $150 million. Currently, the borrowing base is divided into two parts, with the "Tranche A" portion making $100 million available for all corporate purposes and the "Tranche B" portion *making* ***$50 million available only to develop natural gas and oil properties***.
3	Concho Resources Inc	2007	Yes	We intend to use all of the net proceeds we receive from this offering to ***repay a portion of our outstanding indebtedness*** under our second lien term loan facility, our revolving credit facility or a combination of the foregoing. Under the terms of our second lien term loan facility, we are obligated to use not less than 50% of our net proceeds to repay our outstanding indebtedness under our second lien term loan facility. Our second lien term loan facility currently bears interest at 9.10% per annum and matures on March 27, 2012.
4	Continental Resources Inc	2007	No	The selling shareholder identified in this prospectus is offering shares of our common stock. We will not receive any proceeds from the sale of the shares by the selling shareholder.
5	Encore Energy Partners LP	2007	Yes	We intend to use the estimated net proceeds of approximately $190.1 million from this offering, after deducting underwriting discounts of $14.5 million and estimated offering expenses of approximately $2.5 million, to: • ***repay $64.9 million of indebtedness*** under our revolving credit facility; and • ***repay all $120 million of indebtedness***, together with accrued interest of $5.2 million, under a subordinated secured term loan agreement with one of EAC's subsidiaries. On March 7, 2007, we acquired oil and natural gas properties and related assets in the Elk Basin for approximately $328.7 million, including estimated transaction costs of approximately $0.3 million. We partially ***financed the acquisition*** and related costs with borrowings of $115 million ***under our revolving credit facility*** and proceeds from a $120 million subordinated secured term loan from EAP Operating, Inc., a wholly owned subsidiary of EAC. As of March 31, 2007, the interest rate was 7.1% under the revolving credit facility and 10.3% under the subordinated secured term loan.
6	Sandridge Energy Inc	2007	Yes	We intend to use these proceeds for general corporate purposes, including the ***acceleration of our drilling program*** in West Texas and the Piceance Basin.
7	Venoco Inc	2006	Yes	We anticipate that the net proceeds will be used as follows: 1. for potential acquisitions; 2. to ***supplement our existing sources of funding for our capital expenditure program*** *as needed*; and 3. for general corporate purpose
8	Pinnacle Gas Resources Inc	2007	Yes	We plan to use all of the net proceeds we receive from this offering for ***accelerated capital expenditures***, infrastructure development and general corporate purposes.
9	Matador Resources Co	2012	Yes	We intend to use the net proceeds we receive from this offering to ***repay in full the $25.0 million term loan*** that is due and payable on December 31, 2011 and to ***repay in full the outstanding indebtedness under our revolving credit agreement*** ($60.0 million at October 31, 2011). Following the application of the net proceeds we receive from this offering, we will not have any long-term indebtedness outstanding and will have $78.7 million available for potential future borrowings (after giving effect to outstanding letters of credit). We intend to use the remaining net proceeds from this offering to ***fund a portion of our 2012 capital expenditure requirements*** and for potential acquisitions of interests and acreage.
10	Bonanza Creek Oil Co	2011	Yes	We intend to use a portion of the net proceeds from this offering to ***repay all outstanding indebtedness under our credit facility***, which as of April 30, 2011, was approximately $68.4 million. The remaining proceeds will be used to ***fund our exploration and development program*** and for general corporate purposes.
11	Laredo Petroleum Holdings	2011	Yes	We intend to use a portion of the net proceeds from this offering to ***repay $XX million of our outstanding indebtedness under our senior secured credit facility***, approximately $500 million of which was outstanding on August 19, 2011. The remaining net proceeds of approximately $XX, including the net proceeds from any exercise of the underwriters' option to acquire additional shares of common stock, will be used to ***fund our future exploration, development and other capital expenditures***, as well as for general working capital purposes.
12	Midstates Petroleum Co	2012	Yes	We intend to use $XX million of the net proceeds from this offering to ***repay all outstanding indebtedness under our revolving credit facility***. The remaining proceeds of approximately $XX million will be used to ***fund a portion of our exploration and development program***.

ESSAY 3

Do Firms Engage in Risk Shifting? Empirical Evidence*

Erik Gilje†

Abstract

I empirically test whether firms engage in risk-shifting in a setting where corporate investment risk measures are available in SEC disclosures. Contrary to what risk-shifting theory predicts, I find that firms reduce investment risk both when leverage increases and when they approach distress. In firm-level panel regressions I find that firms reduce the riskiness of capital expenditures by 21.6% when leverage is high, relative to the average firm. In a second test, I use a natural experiment with exogenous shocks to leverage, and find that firms with exogenous increases in leverage reduce risk taking. This result suggests risk reducing incentives during distress, such as borrower reputation and managerial reputation concerns, outweigh risk-shifting incentives in investment decision making.

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1 Introduction

How does corporate investment risk taking change when a firm has high leverage or approaches distress? In high leverage states of the world equity holders benefit from successful outcomes of high risk projects, while losses from unsuccessful outcomes are borne by debtholders. This asymmetry between who receives the gains and losses from a project could make it optimal for equity holders to maximize the amount of risk a firm undertakes when leverage is high. This hypothesized increased risk taking in a firm's investments, referred to as risk-shifting or asset substitution, could result in an overall cost to the firm (Jensen and Meckling (1976)).

Concerns about the size, prevalence, and mitigation of these costs have been the focus of substantial theoretical work.[1] However, there is little empirical evidence on the size or pervasiveness of changes in investment risk taking when leverage is high or when a firm is in financial distress. The empirical challenges are two-fold. First obtaining a measure of the riskiness of a firm's overall capital expenditures is challenging in most settings. Second, distress and high leverage are not randomly assigned to firms. To the extent a corporate investment plan and high leverage/distress are jointly determined, or are caused by an omitted variable, obtaining clean identification of the effect of distress or leverage on risk taking is problematic. The contribution of this paper is to provide empirical advancements on both these fronts. First, I focus on a setting where a firm's investment risk taking is clearly defined by measures of investment risk from SEC disclosures. Second, I use quasi-random shocks to leverage to identify the effect of an increase in leverage and distress on investment risk taking.

I use a setting in which investments can be categorized into two different types of activities, one that is high risk and one that is low risk. To do this, I focus on the oil and gas industry, where exploratory projects (high risk) are nearly six times more likely to result in an unproductive project than development projects (low risk).[2] Moreover, these categories

[1]Existing theoretical work related to the size and mitigation of risk-shifting includes: Smith and Warner (1979) (covenants), Green (1984) (convertible debt), Barnea et al. (1980) (debt maturity), John and John (1993) (managerial compensation).

[2]The firms in my sample drilled a total of 12,574 exploratory wells of which 3,326 were unsuccessful (26.4%), and drilled 88,277 development wells of which 3,809 were unsuccessful (4.3%). Additionally, in

have clear definitions outlined by the Financial Accounting Standards Board (FASB) and are disclosed in SEC filings, so there is a standardization in these measures across firms and over time, which is typically unavailable in other settings. I construct a dataset from hand collected data on investment risks from the 10-Ks of 184 firms in the oil and gas industry. Using these risk disclosures, I test how the proportion of high risk investment to total investment changes as leverage increases as well as when firms approach distress.

Contrary to what risk-shifting theory would predict, in firm-level panel regressions I find that high leverage reduces the riskiness of a firm's investments. A one standard deviation increase in leverage reduces the proportion of a firm's high risk investments to total investment by 8.5% relative to the mean level of firm risk taking. I also find that the proportion of high risk investment to total investment is reduced by 21.6% for firm-years in which leverage is in the top quartile of the sample. Furthermore, this risk-reducing behavior also occurs in the years prior to declaring bankruptcy.

One concern with firm-level panel regression results could be reverse causality. For example, it could be that a firm increases its leverage because it is planning to reduce its investment risk in the future. Specifically, a firm, or its lender, may feel more comfortable with higher leverage if the firm has less cash flow uncertainty from its future investments. Such an argument would suggest that firms are not reducing the riskiness of their investments because they have high leverage, but that they increase their leverage because they are planning to reduce investment risk in the future.

To address the simultaneity and omitted variable endogeneity concerns and rule out other alternative explanations I use a natural experiment to test how risk taking changes with leverage during two significant commodity based negative leverage shocks in 1998 and 2008. I focus on firms with similar pre-event book leverage, but whose existing assets are differentially affected by the commodity price shocks due to different mixes of oil and gas assets or different geographic locations. Despite similar pre-event book-leverage, the differential effect of the commodity price shocks result in the Merton (1974) distance to default (DD)

comparing reserve additions from discoveries relative to exploration capital expenditures, in 27% of all firm years, firms failed to add reserves through discoveries that exceeded their exploration spending.

default probability increasing from 0.03 to 0.44 for treatment firms, but only from 0.04 to 0.18 for control firms. I use oil and gas reserve changes due to commodity prices to isolate the component of the leverage changes which are due to exogenous commodity price shocks. I designate treatment firms as firms with shocked book leverage in the top quartile of all firms, control firms are firms matched on pre-event book leverage to the treatment firms. I show that treatment and control firms have a number of similar observable characteristics. Using a differences-in-differences approach, I find that treatment firms reduce investment risk taking after exogenous shocks to book leverage relative to control firms.

A key potential concern related to using commodity price shocks as part of an identification strategy is whether firms whose existing assets are adversely affected by a commodity price shock, may choose to undertake a different investment program for a reason other than the effect of the commodity price shock on leverage. To mitigate this concern I test whether there is a direct effect of changes in prior period reserves on firms' investment programs. Thereby measuring how changes in reserves due to commodity prices affects investment, but without conditioning on ex ante leverage as in the natural experiment. I find in both the full sample and a limited sample (excluding the years of the natural experiment) that prior period changes in existing reserves due to commodity prices does not affect a firm's mix of exploratory versus development drilling. This is consistent with the view that the new projects a firm undertakes is likely going to be based on where the best new opportunities are, versus where it's existing assets are.

Existing empirical literature has studied the risk-shifting incentives of equity holders in a variety of ways. Initial work by Andrade and Kaplan (1998) studies 31 firms in financial distress and finds no evidence that distressed firms made large or unusually risky investments or acquisitions. Rauh (2009) studies how risk taking in pension funds change in relation to the financial condition of a firm. Consistent with the findings in this paper Rauh (2009) finds that risk taking in pension funds is reduced as the financial conditions of a firm deteriorate. Parrino and Weisbach (1999) utilize simulation and find that risk-shifting is not a primary driver of capital structure decisions. Gormley and Matsa (2011) find that firms respond to exogenous increases in liabilities by undertaking diversifying acquisitions. Furthermore, survey evidence from Graham and Harvey (2001) suggests that risk-shifting concerns are the least important factor for CFOs in determining the maturity of debt a firm issues as well as whether a firm issues convertible bonds. Alternatively, Eisdorfer (2008) studies risk-shifting within the context of a real options framework, and finds that, consistent with risk-shifting theory, volatility increases investment by distressed firms.[3] However, to my knowledge, my study is the first to use exogenous variation in leverage and ex-ante investment risk measures from SEC disclosures to directly test whether firms engage in risk-shifting behavior.

Because direct measures of risk are difficult to obtain, prior literature has often used different proxies for firm risk taking activities. For example, standard deviation of changes in quarterly ROA and equity price volatility have been used in the past. I document that the standard deviation of changes in ROA and equity price volatility have a low, but positive correlation with my measure of investment risk, suggesting that existing proxies do not capture the investment risk captured by my measure. Additionally, these measures likely capture many effects other than just the operating policies and risk-taking decisions that are made by management. For example, product market competition, financial market volatility, changes in government regulatory regimes as well as other factors could be affecting these measures.

Research & Development (R&D) spending has also been used as a proxy for risk taking, however, due to the multi-year life cycle of typical R&D projects it is difficult to envision that an increase in R&D in a year of financial distress would result in an outcome the following year which could save the firm from further distress or bankruptcy. Alternatively, the primary project type for oil and gas companies, the drilling of a well, typically has a very short project length, ranging from a month to a few months depending on where the well is being drilled. Thus, it is plausible that a successful major exploratory well could alter the fortunes of a company in a short period of time. There is a strong empirical relationship between exploration capital expenditures and reserve additions from discoveries in a given

[3]Additional work has focused on risk-shifting incentives of banks during the S&L crises and more recently in the sub-prime crisis (Landier et al. (2011)), however, the government role in financial institutions and the mortgage market makes it unclear whether these findings would be applicable to industrial firms (Almeida et al. (2011)).

year.[4] This short project time-line also would suggest that if risk-shifting were to occur, it would be more likely to occur in this setting than in others. Furthermore, the higher than average capital intensity of this industry suggests that current period investment can have a large effect on the overall riskiness of the firm, whereas year to year changes in R&D may be less likely to influence the overall risk level of a firm.

This paper provides evidence on how firms change their risk taking behavior as leverage changes and firms approach distress. In particular, the results highlight that firms reduce risk taking when leverage increases and when they approach distress. This suggests that while firms may have a risk-shifting incentive (Jensen and Meckling (1976)), other risk mitigating incentives may outweigh risk-shifting incentives in their decision making. For example, managers may have career reputation concerns which result in a reduction in investment risk taking (Hirshleifer and Thakor (1992)). Firms too, likely have incentives to ensure that they have a good reputation to ensure access to debt markets (Diamond (1989)), which can affect their ability to pursue future positive NPV projects (Almeida et al. (2011)).

This paper proceeds in the following order, Section 2 discusses motivation and related literature. Section 3 outlines the data that is used. Section 4 discusses identification and the empirical design. Section 5 reports the results of the empirical tests, and Section 6 concludes.

2 Motivation and Related Literature

Why might risk-shifting not be observed in empirical tests? A potential explanation that prior theoretical literature has focused on is the reliance of the Jensen and Meckling (1976) risk-shifting result on a single period framework; in other words, agents make decisions as if there is no tomorrow. Jensen and Meckling (1976) directly acknowledge that when their framework is applied to a multi-period setting different outcomes may occur:

> "*It seems clear for instance that the expectation of future sales of outside equity and debt will change the costs and benefits facing the manager in making decisions which benefit himself at the (short-run) expense of the current bondholders and*

[4] Evidence documenting this relationship is in Appendix A.

stockholders. If he develops a reputation for such dealings he can expect this to unfavorably influence the terms at which he can obtain future capital from outside sources. This will tend to increase the benefits associated with "sainthood" and will tend to reduce the size of the agency costs. "

- Jensen and Meckling (1976)

Existing theoretical literature using multi-period settings has suggested several possible explanations for why a firm may choose to not undertake risk-shifting. Diamond (1989) suggests that firms may avoid risk-shifting due to borrower reputational concerns, while Hirshleifer and Thakor (1992) suggest that manager reputational concerns leads managers to reduce risky investment. Almeida et al. (2011), suggest that concerns for the ability to fund future projects may cause firms to reduce risk, so that positive NPV projects can be funded in the future.

Covenants on loans and bonds may also play an important role in a firm's investment policies. While the clear accounting based definitions of investment risk used in this study enable tests on risk-shifting, they also would enable a financial covenant to be designed to limit the amount of capital being invested in high risk projects. However, in this setting, as with pension funds in Rauh (2009), I do not find any limitations on risk taking for investments in loan or bond covenants. However, this does not rule out the possibility of other covenants indirectly effecting a firm's risk taking. For example, conditional on being limited to a certain investment amount, a firm may elect to invest in lower risk projects, while if it were unconstrained in the amount it could invest it may have elected to pursue higher risk projects. It could very well be the case that the need for explicit limits on risk taking for a given level of investment are not needed as other covenants may make investing in low risk projects in high leverage states of the world the most attractive choice for a firm's managers/equity-holders.

3 Data Industry Background

I use hand collected data on investment risk from the 10-K disclosures of all publicly traded U.S. domiciled oil and gas firms (SIC 1311 Crude Oil & Natural Gas) from 1997 to 2010 for this study. The resulting data set is composed of 184 firms and 1,208 firm years. Standard accounting variables were obtained from Compustat, while the detailed hand collected 10-K data was used to develop investment risk measures.

3.1 Investment Risk Variable Definition

Each firm in the study provides disclosures for the "Costs Incurred in Natural Gas and Oil Exploration and Development, Acquisitions and Divestitures." These disclosures provide information on expenditures for high risk (exploratory) capital and low risk (development) capital. The Financial Accounting Standards Board (FASB) provides clear guidance for the definitions of exploratory and development activities which I outline below:

Exploratory well - An exploratory well is a well drilled to find a new field **or** to find a new reservoir in a field previously found to be productive of oil or gas in another reservoir.

Development well - A development well is a well drilled within the proved area of an oil or gas reservoir to the depth of a stratigraphic horizon known to be productive.

I categorize all activities associated with exploratory drilling as high risk, this includes both the capital to drill and the capital to acquire the unproved acreage to drill. All activities associated with development drilling, which include the drilling of development wells and the acquisition of proved/producing acreage for development drilling, I classify as low risk. Moreover, the total capital across all these categories is comparable to the figure reported in Compustat, although there are some slight differences due to the expensing of some types of capital expenditures for oil and gas companies. The unit of observation used in this study is firm-year, firm i in year t, so my primary measure of risk is calculated as the proportion of high risk projects to total costs incurred as shown below:

$$HighRiskCapex_{i,t} = ExploratoryDrilling_{i,t} + AcquisitionOfUnprovedAcreage_{i,t}$$

$$LowRiskCapex_{i,t} = DevelopmentDrilling_{i,t} + AcquisitionoOfProvedAcreage_{i,t}$$

$$RiskRatio_{i,t} = \frac{HighRiskCapex_{i,t}}{HighRiskCapex_{i,t} + LowRiskCapex_{i,t}}$$

The difference in risk between high risk and low risk activities is also documented in the success rate of each activity type. In additional disclosures, firms disclose the number of successful wells and number of unsuccessful wells for both exploratory and development wells. The firms in my sample drilled a total of 12,574 exploratory wells of which 3,326 were unsuccessful (26.4%), and drilled 88,277 development wells of which 3,809 were unsuccessful (4.3%). Thus on average an exploratory well was nearly six times more likely to be unsuccessful than a development well.

In order to assess how exploratory capital expenditures affect a firm's reserve additions (e.g. project profitability), I plot the distribution of reserve additions divided by exploratoty capital expenditures. A ratio above one indicates that a firm added more proved reserves from discoveries than it spent on exploration. As can be seen in Figure 1, there is significant variability in the payoff of exploratory drilling in a given year. For example, in 27% of firm years, companies do not recover drilling costs. Alternatively, in 13% of firm years, companies gain 10x their investment in exploratory wells in the form of proved reserves.

3.2 Leverage and Distress Definitions

Existing literature has used different definitions of leverage. In this study I use a market based definition of leverage from Welch (2004). The book leverage and market leverage definitions are outlined below:

$$MarketLeverage_{i,t} = \frac{D_{i,t}}{E_{i,t} + D_{i,t}}$$

$$BookLeverage_{i,t} = \frac{L_{i,t}}{A_{i,t}}$$

Where $E_{i,t}$ is the equity market capitalization for firm i in year t, and $D_{i,t}$ is the book value of total debt for firm i in year t. Similarly, $L_{i,t}$ is the total liabilities for firm i in year t, and $A_{i,t}$ is the book value of assets for firm i in year t. While the market leverage of a firm is bounded between 0 and 1 by construction, a firm could have a book leverage of greater than 1 if its liabilities exceed its assets. To ensure that coefficients retain an economically meaningful interpretation and minimize the amount of data that is excluded from the study I winsorize any values of book leverage greater than 1 to 1. Additionally, in all of my tests I use dummy variables for different leverage levels based on market leverage quartiles for the sample, this enables the measurement of any non-linear effects of leverage on investment risk taking. Several other controls are included in the main regressions, these include log of assets, market to book, profitability, and proportion of short term debt.

I follow the method of Bharath and Shumway (2008) in calculating the Merton (1974) distance to default (DD) model probability of default. The Merton DD model uses an option framework to calculate the probability of default. It does so by viewing the equity as a call option on the value of a firm, and using the strike price for the option as the value of a firm's debt. By using the equity and debt values of the firm and the volatility of a firm's equity, the overall value of the firm and volatility of firm value can be calculated, using the iterative procedure outlined in Bharath and Shumway (2008). The model provides a z-score which can be used to calculate a probability of default based on the normal cumulative density function.

3.3 Summary Statistics

Table 1 reports summary statistics for the firm-years of the sample used in this study. The key dependent variable of interest is the risk ratio (previously defined), the higher the risk ratio the more risky a firm's capital investment is in a given year. Across all firm-years the average value for the risk ratio is 32%, which can be interpreted as a firm spending 32% of its capital expenditures on high risk projects. The average market leverage for firm-years in the sample is 0.28, while the average book leverage is 0.52. The average Merton DD default probability is 0.08.

Panel B of Table 1 reports the correlation of the risk ratio constructed for this study with other proxies that other studies have used for risk taking. The correlation with my risk measure is low but positive. This suggests that the investment risk measure I use from SEC disclosures captures important risk taking activity not captured by the other measures.

4 Identification and Empirical Design

4.1 Firm-Level Panel Regressions

The first set of firm level panel regressions estimated in this study are designed to test whether there is a correlation between different measures of leverage and distress with the risk ratio (investment risk) of a firm. By including a number of controls, I can rule out some potential explanations. The main firm-level panel regressions estimated in this study are of a form similar to what is outlined below:

$$RiskRatio_{i,t} = \alpha + \beta_1 Leverage_{i,t-1} + Controls_{i,t-1} + FirmFE_i + YearFE_t + \varepsilon_{i,i}$$

$$RiskRatio_{i,t} = \alpha + \beta_1 Distress_{i,t-1} + Controls_{i,t-1} + FirmFE_i + YearFE_t + \varepsilon_{i,i}$$

The primary definitions of leverage used are the market leverage and book leverage variables defined in the data section. The main measure of distress used is Merton DD default probability, which takes a value between 0 and 1. Additionally, leverage dummy variables are used to allow for non-linearity in the relationship between leverage and investment risk. The timing convention of this specification tests the effect of the beginning of year leverage or distress (leverage and distress is measured at the end of year $t-1$) on the investment risks taken in year t. For example, the impact of December 31, 2009 leverage is being measured on the investment risks taken during the year in 2010. Thus, all leverage measures, distress measures, and controls are measured prior to when investment dollars are spent.

The $Controls_{i,t-1}$ are comprised of size, profitability, market to book, and proportion of short term debt. Size is proxied by the log of assets at time $t-1$, while profitability is measured as operating income before depreciation divided by assets at time $t-1$. Market to book is included as a proxy for investment opportunities, this is measured as the market value of assets divided by book value of assets at time $t-1$. Debt maturity could also have implications for investment risk, this is controlled for as the proportion of debt due in the next year divided by total debt at time $t-1$. As with the leverage variable, by using time $t-1$ for the control variables, the impact of variables measured at year-end are being compared to investment risks taken in the following year. For example, the influence of profitability during 2009 or market to book at December 31, 2009 is compared to investment risks in 2010.

Additional controls for firm fixed effects $FirmFE_{i,t}$ and time fixed effects $TimeFE_{i,t}$ are included. The inclusion of firm fixed effects controls for any time invariant heterogeneity (for example time invariant lending relationships, CEO characteristics etc.). Time fixed effects are included to control for any time period specific shocks, this is particularly important given that the firms in the sample all produce commodities. By including time fixed effects in the specification changes in investment opportunities due to changes in commodity prices are controlled for, to the extent these shocks affect all firms the same.

4.2 Natural Experiment: Commodity Based Leverage Shocks

While the firm-level regressions outlined above could allow me to establish a basic relationship between leverage and investment risk, with some observables and time invariant heterogeneity controlled for, better inference can be achieved by using a natural experiment framework. The natural experiment I use is two commodity driven leverage shocks. The commodity shocks I use in 1998 and 2008 were driven by unexpected economic collapses, which make them an attractive setting for a natural experiment. Specifically, the price collapse in 1998 was due to the Russian default and Asian financial crisis, these events were not anticipated. In January 1998 futures contracts indicated natural gas prices of \$2.46/mmbtu and oil prices of \$18.56/barrel for December 1998, while actual realized prices were \$1.95/mmbtu and \$11.35/barrel respectively. The price collapse in 2008 was due to the financial crisis in the fall of 2008, and also was not anticipated. In January 2008 futures contracts indicated natural gas prices of \$9.00/mmbtu and oil prices of \$94.05/barrel for December 2008, while actual realized prices were \$5.94/mmbtu and \$41.12/barrel respectively.

Commodity prices are exogenous, as no single firm can control prices for oil or natural gas. The price collapses experienced by commodities in 1998 and 2008 influenced the leverage levels of firms differently based on 1) the amount of leverage a firm had prior to the shock and 2) the precise exposure a firm's existing assets had to the commodity shock based on its mix of oil and natural gas reserves 3) The geographic location of a firm's existing assets.

The initial differences-in-differences framework can be thought of as 1) the difference between pre-shock and post-shock behavior 2) the difference in behavior of firms more affected by the shock (treatment) and firms less affected by the shock (control). As mentioned above whether a firm is considered treatment or control is a function of commodity prices on its leverage via the revaluation of its existing assets. Book leverage prior to the shock, can be calculated directly from Compustat data. To calculate the effect of the commodity price shock on a firm's leverage I can take advantage of additional unique disclosures in the oil and natural gas industry. Specifically, in every 10-K a firm has to report the different components of changes to the dollar value of its reserves (acquisitions, discoveries, commodity prices etc.), with this data I can isolate the precise effect of commodity prices on a firm's reserves, distinct from any management action to alter or improve dollar reserves. This enables me to calculate what a firm's book leverage would be if the only event that occurred was the commodity shock, the calculation is as follows:

$$BookLe\tilde{ve}rage_{i,Post} = \frac{TotalLiabilities_{i,Pre}}{TotalAssets_{i,Pre} + \$ChangeReservesPrices_{i,Post}}$$

For example, in the case of the shock that occurred in 2008, the total liabilities as of December

31, 2007 are used in conjunction with the change in reserves due to commodity prices during 2008 to calculate the market leverage as of December 31, 2008. The firms in the top quartile of leverage using the above calculation are used as treatment firms, while the control firms are obtained by matching on December 31, 2007 book leverage. To mitigate any issues with concurrent changes in investment policies, I exclude the year of a shock. So in the case of 2008, I compare investment risks taken in 2007 to investment risks taken in 2009. For the natural experiment I focus on book leverage as this is what is most closely related to the reserve changes a firm has on its balance sheet.

I use a regression form of differences-in-differences to test the effect of leverage on investment risk in a natural experiment framework. The specific regression I estimate is below:

$$RiskRatio_{i,t} = \alpha + \beta_1 Treatment_{i,t} + \beta_2 Post_{i,t} + \beta_3 Treatment_{i,t} * Post_{i,t}$$

$$+Controls_{i,t-1} + FirmFE_i + YearFE_t + \varepsilon_{i,i}$$

Where the $Treatment_{i,t}$ is a 0 or 1 dummy variable constructed from the reserve based book leverage calculation outlined above and $Controls_{i,t-1}$ are similar to the panel regression. The key coefficient of interest in this specification is β_3, which measures how the treatment group is differentially affected by the shock. For example, if firms whose leverage is more affected by a commodity shock reduce investment risk after the shock, then β_3 would be negative.

5 Results

5.1 Firm Level Panel Regressions

Table 2 reports results from firm-level panel regressions of different measures of investment risk on measures of leverage and distress. Every measure of leverage and distress has a negative and statistically significant effect on the investment risk taken by a firm. The coefficient on market leverage in specification (1) can be interpreted as a one standard deviation increase in leverage reducing the investment risk ratio by 8.5% relative to the mean firm-year investment risk ratio. Alternatively specification (5) can be interpreted as a firm reducing its risk taking by 21.6% relative to the mean firm-year, when it is in the top quartile of sample leverage. The coefficient on the Merton DD default probability can be interpreted as a one standard deviation increase in default probability reducing a firm's risk-taking by 6.8% relative to the mean firm-year investment risk-ratio.

A concern in the interpretation of the firm-level regression results reported in Table 2 is how reverse causality might explain the observed coefficient estimates. It could be the case that firms are increasing leverage because they are planning to reduce investment risk, and are more comfortable with a higher debt load as they reduce their investment risk. One test of the plausibility of the reverse causality argument is in Table 3, which reports how firms change their risk taking prior to bankruptcy. There are only 16 bankruptcies in the sample, yet the reduction in risk in the years prior to bankruptcy is large enough that there is statistical power even for this small number of observations. The economic interpretation of the coefficient in specification (1) is that in the year prior to bankruptcy firms reduce investment risk taking 23.8% relative to the investment risk taking of the mean firm. A result inconsistent with the reverse causality explanation above, as firms that are in distress and about to declare bankruptcy are less likely to be increasing their leverage deliberately.

5.2 Natural Experiment

A key assumption when using a natural experiment framework is the conditionally random assignment of treatment. Treatment in the setting of my natural experiment is based on the effect of a change in existing assets caused by commodity price shocks on leverage. Because leverage is a firm decision, pre-shock differences in leverage may be a cause for concern regarding the conditionally random assignment assumption. As Table 4 Panel A shows, there are economically significant and statistically significant differences in leverage between all firms and the treatment firms. This is not surprising given that pre-existing book leverage affects a firm's probability of being treated. Interestingly, treatment firms are very similar across other observable dimensions, when compared to the other firms in the sample.

To mitigate concerns regarding pre-shock differences in leverage I undertake two matching procedures in Panel B and Panel C. Specifically I match firms on pre-shock book leverage (book leverage as of Dec 31, 1997 or Dec 31, 2007), both with replacement (Panel B) and without replacement (Panel C). In both panels the matching procedure results in firms with similar pre-shock book leverage. Additionally, with the exception of log assets in Panel C, firms in the treatment and control groups match well across market to book, profitability, and market leverage.

Table 5 reports the effect of the commodity price shock on treatment and control firms. Specifically, the shocked book leverage reported is based on the book leverage prior to the shock (book leverage as of Dec 31, 1997 or Dec 31, 2007), adjusted only for the effect of the change in commodity prices on a firm's existing assets. This variable is unaffected by any management actions that occur during the period of the negative commodity price shock. Because shocked book leverage is the variable that determines treatment, it is not surprising to see large economically significant and statistically significant differences between the treatment firms and other firms. Additionally, there are also economically significant and statistically significant differences between treatment firms and control firms in Merton DD default probability and market leverage. Panel B and Panel C of Table 5 indicate that despite treatment and control firms having similar observable characteristics and similar pre-shock book leverage, the effect of the negative commodity shock on treatment firms, results in treatment firms being closer to distress than control firms. In essence, this framework relies on negative commodity shocks having quasi-random effects on firms with similar characteristics. Given that the differential effect of commodity price shocks on oil versus gas, or on one geography versus another geography was unpredictable, this framework yields quasi-random assignment of treatment and control.

Table 6 reports the results of a regression form of differences-in-differences. This specification uses exogenous variation in leverage caused by negative commodity shocks in 1998 and 2008 to identify the influence of leverage on investment risk taking. The key coefficient of interest is the coefficient for the interaction term $Treatment_{i,t} * Post_t$ which measures how firms with leverage that is more affected by the commodity shock change their investment risk relative to firms less affected by the commodity shock. This coefficient in specification (1) is negative and statistically significant across all specifications, and matching group methodologies. The economic interpretation of the interaction coefficient in (2) is that treatment firms reduce risk taking by 75.3% relative to the mean firm risk level in response to the leverage shock relative to the investment risk taking of control firms. The control variables used in Table 6 are based on t-1 variables, or variables prior to the commodity price shock, as many of the controls themselves are affected by the commodity price shock they could be considered bad controls. Specifically, investment risk taking in 2007 uses control variables from 2006, while investment risk taking in 2009 uses year end 2007 (pre-shock) control variables, as these are unaffected by the shock. The results in Table 6 further mitigate some of the reverse-causality and omitted variable endogeneity concerns in the panel regressions, as the leverage changes in the natural experiment are driven by the effect of commodity prices on a firm's existing assets, which is outside of a firm's control.

An important assumption when using a differences-in-differences approach in a natural experiment framework is the parallel trends assumption. That is, in the absence of treatment, would the treatment and control groups have behaved similarly. Table 4 provides evidence that the treatment and control groups used are similar across a number of observable dimensions, however, Table 7 takes an additional step in testing whether the treatment and control groups behave similarly in time periods that did not experience negative leverage shocks. In Table 7 I create placebo events in 2001 (three years after 1998) and in 2005 (three years before 2008), to see whether treatment and control firms behave similarly in these other time periods. I find that the interaction coefficient $Treatment_{i,t} * PlaceboPost_t$ is not statistically significant in any of the specifications. This suggests that in other time periods the investment risk trends across these firms was similar or "parallel."

One concern with using a differences-in-differences framework in a natural experiment setting is that many factors that influence investment decisions, in addition to leverage, could be changing. In particular if changes in the value of a firm's existing assets has an impact on its risk taking for a reason other than changes to leverage (e.g. worse investment opportunities), it could be a concern for my identification. To test whether this is the case I report results in Table 8 which measure the effect of prior period changes in reserves due to commodity prices on investment risk taking. The coefficients for this variable are not statistically significant and are close to zero, suggesting that prior period changes in commodity prices are not of first order concern in making decisions regarding investment risk taking. I report results for both the full sample as well as for the subsample where the years used in the natural experiment are excluded.

6 Conclusion

Whether firms engage in risk-shifting has been an open empirical question. Lack of data and adequate measures of risk, and the endogeneity of leverage and risk taking have meant this question has not been able to be addressed directly. I use a setting which has quasi-random shocks to leverage and objective measures of investment risk, from SEC disclosures, to test whether firms engage in risk-shifting. I find that firms reduce risk, rather than increase risk, when leverage is high and when they get close to distress.

Prior theoretical literature outlines several reasons for why firms may have incentives to reduce risk taking in distress. Managers may have career reputational concerns which result in a reduction in investment risk taking (Hirshleifer and Thakor (1992)). Firms too likely have incentives to ensure that they have a good reputation to ensure access to debt markets (Diamond (1989)), which can affect their ability to pursue future positive NPV projects Almeida et al. (2011). The evidence in this paper suggests that risk-mitigation incentives outweigh risk-shifting incentives in investment decision making for the average firm.

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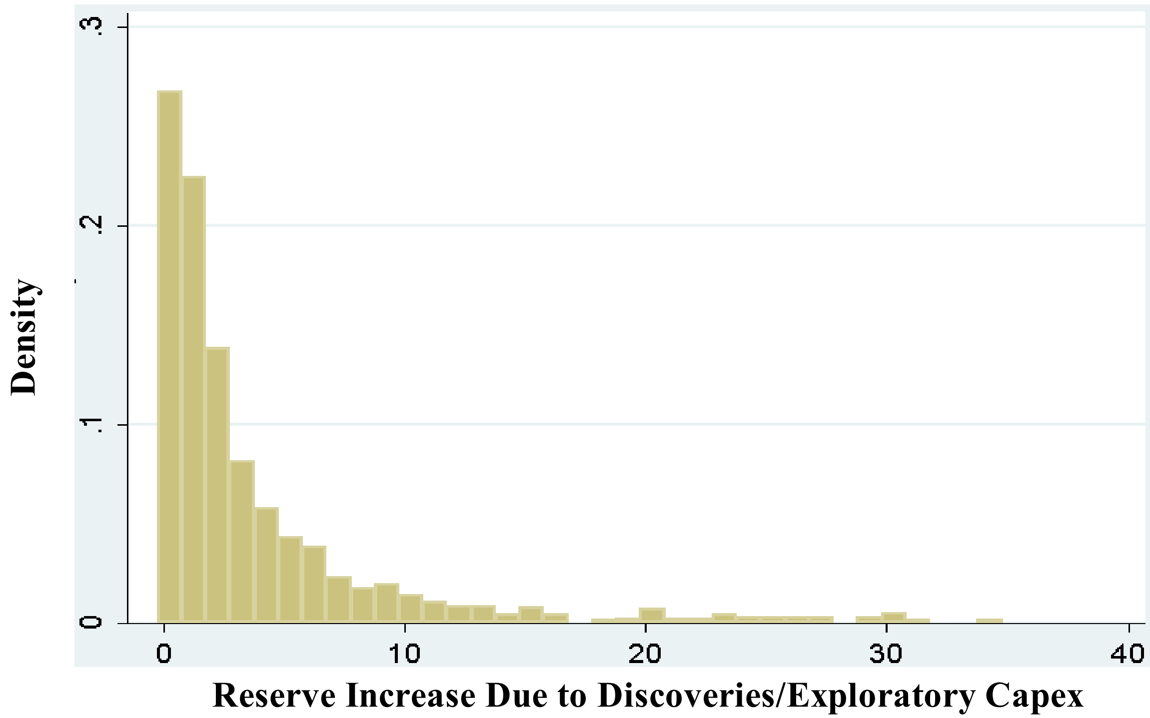

Figure 1: This figure plots the density function of proved reserve additions from discoveries, divided by Exploratory (High Risk Capex). The objective of the figure is to show the distribution of potential payoffs from exploratory capital expenditures for firms in the sample years. A number of 10 on the horizontal access can be interpreted as $10 of proved reserves being added in a year in which $1 of Exploratory Capital Expenditures was made.

Table 1: Summary Statistics and Correlations

This table contains summary statistics for the variables used in the firm level panel regressions used in this study. The risk ratio is defined as the proportion of capital invested in high risk projects (exploratory activities) to total capital expenditures. Market leverage is defined as the market value of equity divided by the market value of equity plus the book value of debt (Welch 2004). Book leverage is defined as total liabilities divided by total assets. In order to be included in the sample a firm-year must 1) be in SIC 1311 (Crude Oil and Natural Gas) 2) be U.S. domiciled and file a 10-K. To mitigate outliers book leverage values are winsorized at 1, and profitability and market to book variables are winsorized at 1% and 99%. The distance to default is based on the Merton (1974) bond pricing model, as implemented by Bharath and Shumway (2008). Firm-year observations span from 1997 through 2010.

Panel A: Summary Statistics

	N	Mean	Std Dev	p25	p50	p75
Dependent Variable						
Risk Ratio	1208	0.32	0.26	0.12	0.26	0.46
Control Variables						
Market Leverage	1208	0.28	0.23	0.10	0.23	0.41
Book Leverage	1208	0.52	0.23	0.36	0.52	0.66
Size (Assets in $Millions)	1208	2,102.46	5,875.16	52.55	283.10	1,185.76
Profitability	1208	0.17	0.24	0.08	0.19	0.30
Short Term Debt/Total Debt	1208	0.10	0.24	0.00	0.00	0.05
Market to Book	1208	1.49	1.07	0.93	1.22	1.68
Merton DD Default Probability	1073	0.08	0.21	0.000	0.000	0.008

Panel B: Correlations With Other Risk Proxies

	Risk Ratio	Std Dev of Quarterly Chg in ROA	Volatility of Monthly Equity Ret
Risk Ratio	1.00		
Std Dev of Quarterly Change in ROA	0.13	1.00	
Volatility of Monthly Equity Ret	0.10	0.24	1.00

Table 2: Investment Risk and Measures of Distress and Leverage - Panel Regression

This table reports firm-level regressions which document the effect of different leverage and financial distress measures on the riskiness of a firm's investments. The dependent variable in these regressions is the risk ratio for firm i in year t. A firm's risk ratio is calculated as the proportion of capital expenditures invested in high risk projects relative to all capital expenditures. All regressions include firm level fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 Distress_{i,t-1} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

$$RiskRatio_{i,t} = \alpha + \beta_1 LeverageD_{i,t-1} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Risk Ratio = High Risk Capex/Total Capex				
	(1)	(2)	(3)	(4)	(5)
Market Leverage$_{i,t-1}$	-0.118**				
	[-2.32]				
Book Leverage$_{i,t-1}$		-0.122***			
		[-2.65]			
Merton DD Default Probability$_{i,t-1}$			-0.103***		
			[-2.87]		
Q4 Mkt Lev Dummy$_{i,t-1}$				-0.045**	-0.069**
				[-2.26]	[-2.40]
Q3 Mkt Lev Dummy $_{i,t-1}$					-0.025
					[-1.06]
Q2 Mkt Lev Dummy$_{i,t-1}$					-0.025
					[-1.12]
Size$_{i,t-1}$	0.045**	0.042**	0.048**	0.041*	0.044**
	[2.10]	[2.03]	[2.06]	[1.96]	[2.15]
Market to Book$_{i,t-1}$	0.002	0.009	0.008	0.006	0.003
	[0.15]	[0.76]	[0.52]	[0.51]	[0.25]
Profitability$_{i,t-1}$	-0.120***	-0.119***	-0.149***	-0.115***	-0.118***
	[-3.11]	[-3.07]	[-2.99]	[-3.01]	[-3.08]
FirmFE$_i$	Yes	Yes	Yes	Yes	Yes
TimeFE$_t$	Yes	Yes	Yes	Yes	Yes
R^2	0.067	0.068	0.078	0.066	0.067
N	1208	1208	1073	1208	1208

Table 3: Investment Risk Prior to Bankruptcy (16 Bankruptcies in Sample)

This table reports firm-level regressions that document how investment risk changes for a firm in the years prior to bankruptcy. The dependent variable in these regressions is the risk ratio for firm i in year t. A firm's risk ratio is calculated as the proportion of capital expenditures spent on high risk projects relative to all capital expenditures. Dummy variables are inserted based on the number of years prior to bankruptcy, for example in the year immediately prior to declaring bankruptcy the variable "One Year Prior to Bankruptcy Dummy" is equal to 1, and equal to 0 for all other firm-years. All regressions include firm level fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 OneYearPriortoBankruptcyD_{i,t} + \beta_2 TwoYearsPriortoBankruptcyD_{i,t} + \beta_3 ThreeYearsPriortoBankruptch_{i,t}$$
$$+ Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Risk Ratio = High Risk Capex/Total Capex	
	(1)	(2)
One Year Prior to Bankruptcy Dummy$_{i,t}$	-0.076*	-0.118**
	[-1.92]	[-2.08]
Two Years Prior to Bankruptcy Dummy$_{i,t}$		-0.105
		[-1.51]
Three Years Prior to Bankruptcy Dummy$_{i,t}$		-0.034
		[-0.41]
Size$_{i,t-1}$	0.040*	0.042**
	[1.90]	[2.00]
Market to Book$_{i,t-1}$	0.010	0.010
	[0.82]	[0.81]
Profitability$_{i,t-1}$	-0.113***	-0.113***
	[-3.00]	[-2.99]
FirmFE$_i$	Yes	Yes
TimeFE$_t$	Yes	Yes
R^2	0.061	0.064
N	1208	1208

Table 4: Natural Experiment, Pre-Event Treatment vs. Control Comparison

This table reports univariate t-tests which compare observable variables of the treatment and control groups used in the natural experiment in the pre-event year. The event years are linked to years in which negative commodity price shocks occur, in this case 1998 and 2008. The set of panels below compare pre-event characteristics of the firms used (characteristics as of year end 1997 and 2007). Panel A is comprised of all firms in the pre-event years, while Panels B and C rely on different matching methodologies. Panel B and C are based on obtaining matching firms less affected by the negative commodity shock to those more affected by it based on pre-event book leverage. Panel B is comprised of control firms that are matched with replacement while Panel C is comprised of control firms which are matched without replacement. Differences in group means are reported along with p-values, * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: Full Sample

	Pre-Shock Comparison			
Total	All Firms	Treatment Firms	Difference	p value
Book Leverage	0.46	0.62	-0.16***	0.00
Log Assets	19.93	20.22	-0.29	0.48
Market to Book	1.58	1.61	-0.03	0.83
Profitability	0.14	0.15	-0.01	0.67
Market Leverage	0.22	0.30	-0.08**	0.04
Merton DD Default Probability	0.01	0.03	-0.02*	0.06
Number of Event Firms	105	36		

Panel B: Matching Sample With Replacement

	Pre-Shock Comparison			
Total	Control Firms	Treatment Firms	Difference	p value
Book Leverage	0.60	0.62	-0.02	0.76
Log Assets	20.96	20.22	0.74	0.11
Market to Book	1.48	1.61	-0.13	0.57
Profitability	0.11	0.15	-0.04	0.11
Market Leverage	0.34	0.30	0.04	0.43
Merton DD Default Probability	0.04	0.03	0.01	0.52
Number of Event Firms	36	36		
Number of Unique Event Firms	22	36		

Panel C: Matching Sample Without Replacement

	Pre-Shock Comparison			
Total	Control Firms	Treatment Firms	Difference	p value
Book Leverage	0.56	0.62	-0.06	0.15
Log Assets	21.01	20.22	0.79*	0.08
Market to Book	1.48	1.61	-0.13	0.53
Profitability	0.13	0.15	-0.02	0.47
Market Leverage	0.28	0.30	-0.02	0.64
Merton DD Default Probability	0.01	0.03	-0.02	0.40
Number of Firms	36	36		
Number of Unique Event Firms	36	36		

Table 5: Natural Experiment, Event Treatment vs. Control Comparison

This table reports univariate t-tests which compare observable variables of the treatment and control groups used in the natural experiment in the event year. The event years are linked to years in which negative commodity price shocks occur, in this case 1998 and 2008. The set of panels below compare event characteristics of the firms used (characteristics as of year end 1998 and 2008). The composition and matching procedures of Panel A, Panel B, and Panel C are the same as Table 4. Differences in group means are reported along with p-values, * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Panel A: Full Sample

	Post-Shock Comparison			
Total	All Firms	Treatment Firms	Difference	p value
Shocked Book Leverage	0.54	0.98	-0.44***	0.00
Merton DD Default Probability	0.24	0.44	-0.20***	0.00
Market to Leverage	0.40	0.54	-0.14***	0.01

Panel B: Matching Sample With Replacement

	Post-Shock Comparison			
Total	Control Firms	Treatment Firms	Difference	p value
Shocked Book Leverage	0.68	0.98	-0.30***	0.00
Merton DD Default Probability	0.18	0.44	-0.26***	0.00
Market to Leverage	0.37	0.54	-0.17***	0.00

Panel C: Matching Sample Without Replacement

	Post-Shock Comparison			
Total	Control Firms	Treatment Firms	Difference	p value
Shocked Book Leverage	0.62	0.98	-0.36***	0.00
Merton DD Default Probability	0.22	0.44	-0.22***	0.00
Market to Leverage	0.40	0.54	-0.14**	0.02

Table 6: Natural Experiment, Effect of Leverage Shock on Investment Risk: Difference-in-Differences

(Pre-Shock vs Post-Shock, Treatment = High Leverage Shock Firms vs. Control = Low Leverage Shock Firms)

This table reports results from a regression form of differences-in-differences. The first difference is pre-shock vs. post-shock, while the second difference is high leverage shock vs. low leverage shock. The dependent variable in these regressions is the risk ratio of firm *i* at time *t*. Firms are divided into treatment and control groups based on the effect of the commodity shock on leverage. For the firms that have implied book leverage in the top quartile due to the commodity shock, the variable Treatment is equal to 1 and 0 otherwise. The two leverage shocks used in this regression are in 1998 and 2008, the years of the shocks are excluded from the sample, therefore the pre-post comparisons compare 1997 (post = 0) to 1999 (post = 1) and 2007 (post = 0) to 2008 (post = 1). (1) and (2) report regression estimates for the matched sample with replacement, (3) and (4) report estimates for the matched sample with replacement, (5) and (6) report estimates for all firms in the sample during the event years. Control variables have been previously defined, after the shock occurs these variables are fixed at their last pre-event values, as they are affect by the shock (for example the size control used for the 1999 post period is based on year end 1997 size). The dependent variable in these regressions is the risk ratio for firm *i* in year *t*. All regressions include firm-event fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 Post_{i,t} + \beta_2 Treatment_{i,t} + \beta_3 Treatment_{i,t} * Post_{i,t} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Differences-in-Differences					
	Matched With Replacement		Matched Without Replacement		All Firms	
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,t}$	Absorbed by $TimeFE_t$		Absorbed by $TimeFE_t$		Absorbed by $TimeFE_t$	
$Treatment_{i,t}$	Absorbed by $FirmEventFE_i$		Absorbed by $FirmEventFE_i$		Absorbed by $FirmEventFE_i$	
$Treatment_{i,t}*Post_{i,t}$	-0.241*	-0.237**	-0.172***	-0.186***	-0.094*	-0.112**
	[-1.83]	[-2.25]	[-2.93]	[-3.08]	[-1.95]	[-2.34]
$Size_{i,t-1}$		-0.035		0.054		0.089
		[-0.38]		[0.78]		[1.36]
$Profitability_{i,t-1}$		-1.498**		-0.688		-0.277
		[-2.04]		[-1.66]		[-0.80]
Market to $Book_{i,t-1}$		0.053		0.035		-0.002
		[0.64]		[0.53]		[-0.06]
$FirmEventFE_i$	Yes	Yes	Yes	Yes	Yes	Yes
$TimeFE_t$	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.188	0.320	0.119	0.187	0.031	0.061
N	144	144	144	144	282	282

Table 7: Placebo Shock: Difference-in-Differences

(Pre-Placebo Shock vs Post-Placebo Shock, Treatment Firms vs. Control Firms)

This table reports estimations from a regression form of differences-in-differences. The first difference is the pre-placebo shock vs. post-placebo shock, while the second difference is the treatment firms used in Table 6 vs. the control firms used in Table 6. Placebo shocks are based on moving the 1998 shock three years forward to 2001 and the 2008 shock three years backward to 2005 (Note: data is not available for having a placebo shock in 1995, so I move the placebo event three years forward instead). The dependent variable in these regressions is the risk ratio of firm *i* at time *t*. The matching and sample composition for (1) to (6) is the same as what is described in Table 6. All regressions include firm-event fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 PostPlacebo_{i,t} + \beta_2 Treatment_{i,t} + \beta_3 Treatment_{i,t} * PostPlacebo_{i,t} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Differences-in-Differences					
	Matched With Replacement		Matched Without Replacement		All Firms	
	(1)	(2)	(3)	(4)	(5)	(6)
$PostPlacebo_{i,t}$	Absorbed by $TimeFE_t$		Absorbed by $TimeFE_t$		Absorbed by $TimeFE_t$	
$Treatment_{i,t}$	Absorbed by $FirmEventFE_i$		Absorbed by $FirmEventFE_i$		Absorbed by $FirmEventFE_i$	
$Treatment_{i,t}$*$PostPlacebo_{i,t}$	0.088	0.082	0.092	0.024	0.011	-0.034
	[0.82]	[0.79]	[1.37]	[0.37]	[0.21]	[-0.55]
$Size_{i,t-1}$		0.115		-0.126		0.142
		[0.74]		[-1.12]		[1.57]
$Profitability_{i,t-1}$		-0.476		-1.006**		-0.776*
		[-0.73]		[-2.69]		[-1.99]
Market to $Book_{i,t-1}$		-0.051		-0.006		0.012
		[-0.58]		[-0.18]		[0.30]
$FirmEventFE_i$	Yes	Yes	Yes	Yes	Yes	Yes
$TimeFE_t$	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.043	0.119	0.078	0.278	0.084	0.169
N	92	92	84	84	206	206

Table 8: Impact of Reserve Changes on Investment Risk - Panel Regression

This table reports firm-level regressions which document the effect of reserve changes on the riskiness of a firm's investments. Reserve changes are based on a firm's prior year change in reserves due to commodity prices scaled by assets. The dependent variable in these regressions is the risk ratio for firm i in year t. A firm's risk ratio is calculated as the proportion of capital expenditures invested in high risk projects relative to all capital expenditures. Specification (1) reports results for the full sample period, 1997 to 2010, while specification (2) excludes the sample years used in the natural experiment, 1997-1999 and 2007-2009. All regressions include firm level fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 ReserveChange_{i,t-1} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Risk Ratio = High Risk Capex/Total Capex	
	Full Sample	Excluding Natural Experiment Years
	(1)	(2)
Reserve Change/Assets$_{i,t-1}$	0.009	-0.002
	[0.42]	[-0.11]
Size$_{i,t-1}$	0.061***	0.079***
	[2.86]	[3.33]
Market to Book$_{i,t-1}$	0.008	-0.004
	[0.51]	[-0.20]
Profitability$_{i,t-1}$	-0.172***	-0.271***
	[-3.35]	[-3.71]
ShortTermDebt/TotalDebt$_{i,t-1}$	-0.065	-0.055
	[-1.44]	[-0.80]
FirmFE$_i$	Yes	Yes
TimeFE$_t$	Yes	Yes
R^2	0.082	0.107
N	1070	638

Table Appendix A: Effect of Exploratory Capex on Reserves Additions from Discoveries

This table reports firm-level regressions which document the effect of high risk capital expenditures of different lags on reserves added from discoveries. The objective is to measure which lag of capex is most important for proved reserves added in a given year. For example, in specification (1) reserves added in a given year are regressed on the high risk capital expenditures (exploratory capital expenditures) spent in that year. All regressions include firm level fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$Reserves\ Added\ From\ Discoveries_{i,t} = \alpha + \beta_1 High\ Risk\ Capex_{i,t} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Dependent Variable = Reserves Added From Discoveries/Assets				
	(1)	(2)	(3)	(4)	(5)
High Risk Capex/Assets$_{i,t}$	0.428***	0.489***	0.535***	0.544***	0.465***
	[4.02]	[3.70]	[3.54]	[3.57]	[2.79]
High Risk Capex/Assets$_{i,t-1}$		0.057	0.170*	0.199*	0.093
		[0.73]	[1.67]	[1.73]	[0.83]
High Risk Capex/Assets$_{i,t-2}$			0.167	0.181	-0.013
			[1.58]	[1.13]	[-0.11]
High Risk Capex/Assets$_{i,t-3}$				0.150	-0.023
				[1.08]	[-0.19]
High Risk Capex/Assets$_{i,t-4}$					0.014
					[0.15]
FirmFE$_i$	Yes	Yes	Yes	Yes	Yes
TimeFE$_t$	Yes	Yes	Yes	Yes	Yes
R^2	0.198	0.238	0.236	0.237	0.211
N	1127	999	837	696	573

Table Appendix B: Investment Risk and Composition of Leverage and Other Measures of Distress

This table reports firm-level regressions which document the effect of different components of leverage on a firm's investments (proportion of capitalization that is bank debt vs non bank-debt, secured vs unsecured, or short term vs long-term). The dependent variable in these regressions is the risk ratio for firm *i* in year *t*. A firm's risk ratio is calculated as the proportion of capital expenditures invested in high risk projects relative to all capital expenditures. Specification (4) reports changes in investment risk after a new covenant violation, while specification (5) reports changes in investment risk relative to a firm's Altman (1968) Z-score. All regressions include firm level fixed effects and time fixed effects. Standard errors are clustered by firm, with t-statistics reported in brackets below the coefficient estimates. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

$$RiskRatio_{i,t} = \alpha + \beta_1 Debt\ Variable_{i,t-1} + Controls_{i,t-1} + TimeFE_t + FirmFE_i + \varepsilon_{i,t}$$

	Risk Ratio = High Risk Capex/Total Capex				
	(1)	(2)	(3)	(4)	(5)
Secured Debt/(Total Debt + Total Equity)$_{i,t-1}$	-0.182**				
	[-2.54]				
Unsecured Debt/(Total Debt + Total Equity)$_{i,t-1}$	-0.112*				
	[-1.71]				
Bank Debt/(Total Debt + Total Equity)$_{i,t-1}$		-0.164**			
		[-2.38]			
Non Bank Debt/(Total Debt + Total Equity)$_{i,t-1}$		-0.126**			
		[-2.01]			
Short Term Debt/(Total Debt + Total Equity)$_{i,t-1}$			-0.109**		
			[-2.04]		
Long Term Debt/(Total Debt + Total Equity)$_{i,t-1}$			-0.146*		
			[-1.71]		
Covenant Violation$_{i,t-1}$				-0.025	
				[-1.22]	
Z-Score$_{i,t-1}$					0.004
					[0.60]
Size$_{i,t-1}$	0.049**	0.051**	0.044**	0.046*	0.035
	[2.06]	[2.21]	[2.09]	[1.95]	[1.65]
Market to Book$_{i,t-1}$	-0.001	0.002	0.002	0.006	0.011
	[-0.05]	[0.15]	[0.15]	[0.51]	[0.91]
Profitability$_{i,t-1}$	-0.135***	-0.129***	-0.119***	-0.101**	-0.119***
	[-2.94]	[-2.86]	[-3.10]	[-2.18]	[-2.94]
FirmFE$_i$	Yes	Yes	Yes	Yes	Yes
TimeFE$_t$	Yes	Yes	Yes	Yes	Yes
R^2	0.082	0.080	0.067	0.071	0.060
N	1056	1072	1208	907	1207