FINANCIAL CONSTRAINTS AND INTANGIBLE INVESTMENT

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A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

Business Administration - Finance - Doctor of Philosophy

2015

ABSTRACT

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This dissertation is composed of three essays concerning the role of financial constraints in a firm's investment policy. In the first essay, I consider the role of the market for credit default swaps (CDS) in relaxing a firms financial constraints and spurring investment in intangible assets. I find that CDS trading is associated with an increase in research and development (R&D) spending and patenting activity. This is consistent with a channel in which CDS trading protects creditors when borrowers' assets have limited collateral value. In my second essay, I present evidence that the presence of financial constraints for a firms competitors tends to spur investment spending. I use a tax law change to identify exogenous changes in Financial Constraints (FC) to establish causality. This evidence suggests that product market considerations interact with financial constraints in the determination of both firm-level and industry-level innovation. My third essay is a methodological investigation pertaining to all studies that use panel data techniques, including studies of financial constraints and investment decisions. I investigate whether the Strict Exogeneity (SE) assumption, a necessary condition for consistent parameter estimates using common estimation techniques, holds in canonical finance panel data regressions. I find that this key assumption is violated quite frequently and can lead to substantive biases in parameter estimates. I suggest a general approach for identification in these situations that exploits industry-time variation in the variable of interest.

Dedicated to my wife Anne, and to my parents Robert and Kathy Grieser

ACKNOWLEDGEMENTS

I am grateful to Andres Almazan, Damien Brooks, Johnathan Cohn, Cesare Fracassi, Charlie Hadlock (Co-Chair), Zack Liu, Gonzalo Maturana, Jordan Nickerson, Joerg Picard, Josh Pollet, Avishai Schiff, Andrei Simonov (Co-Chair), Parth Venkat, Jeff Wooldridge, Hayong Yun (Committee Member), and Miriam Schwartz-Ziv (Committee Member) for useful comments and guidance.

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CHAPTER I

The Real Effects of Credit Default Swaps

1.1 Introduction

Do credit default swaps (CDS) have real economic effects? If financial markets are complete, then credit default swaps are redundant securities, making their existence inconsequential. In the presence of modest frictions however, CDS have been argued to reduce constraints for lenders and increase credit supply [*Hirtle* (2009), and *Jarrow* (2011)]. Furthermore, evidence suggests that these relaxed credit constraints can lead to changes in the capital structure of firms with a CDS trading on their debt [*Saretto* (2013)]. While CDS markets may affect credit supply and, in turn, a firm's capital structure, it is unclear whether these effects ultimately influence corporate investment.

In this paper, I exploit cross-sectional variation in a firm's ability to pledge assets as collateral to examine the effect of CDS trading on firm borrowing and investment decisions. Specifically, I show that firms with an intangible asset base see improved access to unsecured financing after a CDS starts trading on their debt. Interestingly, this improved access to unsecured debt is followed by an increase in innovation and investment spending on intangible assets, measured via patenting activity and R&D spending.

I argue that the likely channel through which credit default swaps have an impact on corporate financing and investment policy is the alleviation of collateral constraints. Both leverage and investment are more responsive to CDS trading for firms investing in intangible assets. These results are consistent with the theoretical work of *Bolton and Oehmke* (2011), who argue that CDS can relax financing frictions when firms have imperfect access to capital due to a limited commitment problem. While previous literature has identified channels through which CDS can impact financing frictions on the supply side of credit markets, I provide the first evidence that these effects interact with demand side financial constraints to affect corporate investment. This evidence suggests that the presence of an active CDS market mitigates collateral constraints and has a real economic impact by facilitating innovation and investment for companies with a limited ability to pledge future cash flows.

A large body of literature has argued that collateral is an important determinant of debt financing, because it provides creditors the right to liquidate assets in the event of default [e.g. *Bolton and Scharfstein* (1990); *Holmstrom and Tirole* (1997)]. It is generally perceived that the function of transferring liquidation rights is to protect lenders when informational asymmetries prevent observation (verification) of a borrowers performance [*Hart and Moore* (1994)]. Collateral constraints can occur if a firm has already pledged all of its assets on prior loans or if a firm's assets have limited collateral value. Asset intangibility is believed to negatively affect a firm's capacity to collateralize [e.g. *Almeida and Campello* (2007)]. Intangible assets may require special management, be costly to evaluate, and be difficult to redeploy when compared to tangible assets. These characteristics result in less value for creditors in default states.[1] If collateral circumvents informational asymmetries that restrict a firm's access to capital, collateral constraints could lead to underfinancing, and thus underinvestment, for firms investing in intangible assets.

Unlike collateral, CDS can increase the range of projects that receive financing, even for firms with a limited ability to collateralize assets. Credit default swaps can serve a function similar to that of collateral by transferring bargaining power to creditors during debt renegotiations, raising a borrowers pledgeable income [*Bolton and*

[1] see *Parsons and Titman* (2009) for an in depth summary

Oehmke (2011)]. The ability of creditors to initiate (purchase) a CDS contract on a firm's debt is dependent on the depth of the CDS market for that firm and is not directly related to the underlying firm's ability to pledge cash flows.[2] Consequently, credit default swaps may partially alleviate the underfinancing and underinvestment problem that is due to collateral constraints. Cross-sectional variation in the pledgeability of a firm's assets is then suggestive of variation in the impact that CDS trading should have on relaxing a firm's financing constraints.

Although relaxing collateral constraints is a clear channel through which CDS can impact corporate investment, the empirical literature has yet to document this effect to the best of my knowledge. Filling this void is the primary objective of my analysis. I follow the related work of *Ashcraft and Santos* (2009) and *Saretto* (2013), using the introduction of CDS markets as a treatment effect to study the impact of CDS on firm level outcomes.

I analyze private loans and find that after the inception of CDS trading, firms are 7.62% more likely to obtain a loan. Interestingly, this corresponds to a 9.08% increase in the use of unsecured debt and a 2.51% decrease in the use of secured debt for firms with an active CDS market. These results are consistent with prior evidence by *Hirtle* (2009) and *Saretto* (2013) that CDS can improve access to debt financing, but it highlights that the effect may be particularly relevant for firms with an inability to post collateral. To verify that this is the case, I show that the magnitudes of the effects are increasing in measures of inability to collateralize, such as asset intangibility, R&D intensity, and Tobins Q. As an additional measure, I estimate the unused pledgeable asset base of a firm and show that the improved access to unsecured debt is more pronounced for firms that have exhausted their pledgeable assets on previous loans.

One might be concerned that these effects are driven by the potential that CDS trade on firms with less information asymmetry, since such firms are likely to obtain

[2] The inability of a firm to collateralize assets can influence the depth of the CDS market to the extent that it encourages speculation.

unsecured financing despite having the ability to pledge collateral. I would argue that this is in contrast with the effects being larger for firms with a less tangible asset base and for R&D intensive firms. Nonetheless, to alleviate the concern I control for measures of information asymmetry such as analyst forecast dispersion, PIN, implied volatility, S&P500 inclusion, and credit rating. Furthermore, the results hold for within firm estimates, suggesting that within the same firm access to unsecured debt increases after the inception of CDS trading.

Most importantly, I find that firms respond to relaxed financial constraints by increasing R&D spending (innovation input) and patenting activity (innovation output). Specifically, the presence of an active CDS market leads to a 24.94% increase in R&D spending as a percentage of sales (in thousands), corresponding to a 1.3% increase in R&D as a percentage of assets. In addition, a firm generates roughly 2 additional patents per employee and 1.4 additional patent citations per employee after the introduction of a CDS on their debt. The results are obtained for both within-firm estimation (i.e. firm fixed effects) and between-firm estimation within an industry (CDS firm-years to non-CDS firm-years within an industry).

Another potential concern is that the emergence of a CDS market is simultaneously determined with a firm's leverage and investment decisions. While credit default swaps trade over-the-counter (OTC), and firms have essentially no control whether a CDS is trading on their debt, a CDS market could materialize in response to changes in a firm's risk profile. Any changes in capital structure or investment policy could ultimately change the risk of a firm and simultaneously encourage speculation in the CDS market for that firm.[3] I employ three methods to alleviate this concern. First, following the approach used by *Ashcraft and Santos* (2009) and *Saretto* (2013), I exploit the timing of the introduction of CDS by examining access to unsecured debt in the year after a CDS begins to trade. For R&D spending and patenting activity I

[3]Oehmke and Zawadowski (2014) document that speculation is one of the two primary function of CDS markets.

use a 1-5 year window since R&D and patenting are inherently slower processes than the process of increasing debt. It might still be the case that CDS markets are forward looking and begin trading well in advance of a firm's shifts in leverage or investment decisions. Using the findings of *Hilscher* (2014), who show that information in equity markets leads information in CDS markets, I include forward looking implied volatility as a control variable in all regressions.

As an added measure, I use a difference in difference analysis with the 2003 and 2009 ISDA provisions as a treatment effect. These provisions drastically enhanced the liquidity of a CDS contract, and therefore likelihood that a CDS would trade on a given firm. Additionally, it is hard to imagine that these provisions are related to a firms R&D spending or patenting activity, other than through the relaxation of financing frictions by making CDS contracts more standardized and more liquid.

My analysis draws on two distinct and growing literatures, the literature examining the impact of frictions on financing innovation, and the literature on understanding the impact of credit derivatives.

In the literature on financing innovation, *Manso* (2011) argues that innovation requires tolerance (or even reward) for short term failure and an emphasis on long term growth. Along these lines, *Holmstrom* (1989) suggests that public financing may stifle innovation by over-emphasizing short term gains at the expense of long run development. However, *Rajan and Zingales* (2003) argue that adverse selection, moral hazard, and banks' inability to understand innovative firms make financing innovation through private debt more problematic than financing innovation through public markets [see *Atanassov* (2014)]. My paper adds to these arguments by providing empirical evidence that an active CDS market can mitigate frictions due to asymmetric information and allow banks to offer financing to innovative firms. My findings that CDS result in improved access to private debt for innovative firms, which is followed by increased patenting activity and R&D spending, lends supporting evidence to the arguments of *Holmstrom* (1989) and *Manso* (2011), that short term oriented financing is less conducive to innovation.

The literature aimed at understanding the impact of credit derivatives has seen tremendous growth in recent years. Jarrow, 2013 describes conditions in which credit derivatives are welfare improving. *Hilscher* (2014) show that information in equity markets leads that of CDS markets. *Ashcraft and Santos* (2009) find that CDS have little effect on the cost of borrowing for firms with an active CDS market on their debt. Two articles in this strand of literature are particularly related to my analysis, an empirical article by Saretto and Tookes and a theory article by Bolton and Oehmke.

Saretto (2013) provide empirical evidence that firms with a CDS trading on their debt are able to increase leverage and lengthen their debt maturity. I build on their findings in three ways. First, while their analysis emphasizes supply side effects of CDS such as risk sharing, credit risk transfer, and capital requirement reductions to study firms' capital structure decisions, I focus on demand side constraints to see if CDS markets have implications for firm investment decisions. Specifically, I show that CDS can have implications on firm innovation. Second, I exploit cross-sectional variation in firms' ability to pledge collateral to test predictions on access to unsecured financing. By examining access to unsecured debt in addition to total leverage I can provide more detailed analysis as to which firms are affected by CDS trading and how. My evidence that CDS improve access to unsecured debt and the subsequent increase in spending on intangible investment suggests that CDS relax financing frictions for constrained firms, rather than simply changing the relative costs of financing sources for firms already at optimal investment levels. To the best of my knowledge, my results provide the first empirical evidence that CDS markets can have real effects on firm investment. Finally, I find corroborating evidence for Saretto's and Tookes' results on total leverage using a larger sample.

Bolton and Oehmke (2011) develop a theoretical model in which credit default swaps transfer bargaining power to creditors. Their model generates empirically testable implications. On one hand, the transfer of bargaining power to creditors can increase the pledgeable income of borrowers and increase the range the projects that receive financing. On the other hand, this can lead to an empty creditor problem in which creditors over insure, making them excessively tough during debt renegotiations. In the latter case firms may choose to decrease risk, rather than increase investment, in order to avoid excessively harsh creditors in default states. The empty creditor problem is the focus of Bolton's and Oehmke's paper.

My analysis can largely be viewed as an empirical test of the first implication of Bolton's and Oehmke's model, but with a few additional features. First, I argue that the commitment channel of CDS is particularly relevant for firms with an inability to pledge collateral. Furthermore, I bring together the literatures on the effects of credit derivatives and financing innovation. Since innovative firms often face the strictest collateral constraints, CDS can partially mitigate problems in financing innovation. This of course has real economic consequences since financing innovation can be quite inefficient [see *Hall and Lerner* (2010) for an overview].

1.2 Data and Sample Construction

I use Markitgroup data for credit default swap spreads and trading dates for the period 2001-2012. Markitgroup provides a unique identifier for the reference entities of a CDS contract which can then be mapped to the CRSP and Compustat merged database.[4] Data for implied volatility, historical volatility, and put option open interest come from OptionMetrics. Data on credit ratings come from the Compustat ratings file, marginal tax rates come from the Compustat tax rate file, information on a firm's lenders and covenant data come from Reuter's Dealscan, and information

[4]I merge by 6 digit cusip number and then verify that the company names match. If a cusip matches, but the company name doesnt I hand check the firm and discard the ones for which I am unsure if the match is valid.

on bond issuances and underwriters come from Mergent FISD.

I drop all firms in finance and utility sectors (i.e. sic codes 6000-6999 and 4900). I also drop firms that are not in the sample for at least 2 years, firms that have negative book equity or market to book ratio, firms with fic code other than "USA", and firms that have missing values for the main variables of interest (with the exception of the patent variables described below). I also drop a firm if I observe CDS trading on its debt within the first two months of 2001 since I cannot accurately identify the introduction of CDS for such firms.

For a given firm, I define CDS introduction as the first year that a quote for a CDS with 5 year maturity is observed in the data. TRADING is a dummy variable equal to one for all firm-years in which quotes on a 5yr CDS is observed. TRADED is a dummy variable equal to one if a 5yr CDS quote is ever observed during the sample period for a given firm. These variable definitions follow the those of *Ashcraft and Santos* (2009), and *Saretto* (2013).

In addition to financial data, I use patent data from the National Bureau of Economic Research (NBER) patent data project. In a joint effort on the project, the United States Patent and Trademark Office (USPTO) and *Hall et al.* (2001) (henceforth HJT) have collected data on over 3 million patents and 16 million citations. The project has recently been updated to include data from 1976-2006. For each patent I observe the patents technological category, application date, grant date, the list of cited patents, and information about the patents assignees (inventors).

Hall et al. (2001) have also graciously provided a match between the patent assignees and COMPUSTAT that dynamically tracks the ownership of each patent. For this study I am mostly concerned with patent application dates, but I do make use of the dynamic match for some of the variables that I detail below. With the match provided I am able to link firm's financial information to the patents using the CRSP-COMPUSTAT merged link table.

It is well documented that patenting (citing) propensities exhibit tremendous heterogeneity across patent technology classes and through time. To compare the value of patents (citations) in different technology classes at different points in time, HJT develop a reduced form approach and a structural approach to adjust patent and citation counts. In this paper I follow related finance literature and employ the reduced form approach, which involves sorting patents into 6 technological classes with 36 total subcategories. Each patent (citation) is then scaled by the total number of patents (average citations per patent) in each subcategory-year. These adjusted patents (citations) are then aggregated at the firm-year level, creating a weighted sum of each firm's patents. In addition, I include industry and time controls in regressions to mitigate any remaining heterogeneity problems.

The dependent variables that include patents are based on patent *applications* that are eventually granted. The important thing to note is that the patents show up in the year of application however, and not the year that the patent is granted. This is consistent with prior literature and highlights the firm *decision* to increase innovation that is not dependent on the patent granting process.

Control variables include Market-to-Book (MTB), log of total assets (LNTA), log of total market capitalization (size), firm leverage (lev), research spending divided by sales (RDS), cash holding (cashholdings), profitability (profit), tangibility (tangibility), a dummy for whether a firm has a public debt rating (rated), and implied volatility (impvol). Market to book is used to control for changes in a firm's investment opportunities. Leverage, cash holdings, profitability, and tangibility are standard firm level controls (for a detailed discussion see Seru). Implied volatility is used to control for CDS trading that might occur for speculative reasons. For industry classifications I use the Fama and French 48 industry portfolios. Detailed descriptions of variable construction are provided in the appendix.

The final sample runs from 2001 to 2012 and consists of 4,904 firms or 16,151 firm-years. 613 firms have a TRADED CDS at some point during the sample period. This includes 5,924 TRADED firm-years and 4,357 TRADING firm-years.[5] Once a CDS market emerges on a firm's debt it tends to remain for the duration of the sample period. However, 111 of the 613 CDS-firms have a CDS market that becomes inactive.[6]

1.3 Empirical Motivation and Discussion of Results

1.3.1 Credit Default Swaps and Firm Innovation

It is a commonly held view that in the presence of incomplete contracting and uncertainty, financing innovative projects through external sources can be prohibitively more difficult than financing routine projects that have more certain cash flows. For innovative projects, moral hazard and the inability to collateralize human capital can make debt a costly source of financing [e.g.*Rajan and Zingales* (2003); *Atanassov* (2014)]. Furthermore, recent work suggests that equity markets may not be a viable alternative because of 1) a short term emphasis that disincentivizes managers from exploring long-term, innovative projects and 2) an inability to value research and development (R&D) at the beginning of a project, despite a strong persistence in R&D success [e.g. *Cohen et al.* (2013); *Chava and Roberts* (2008)].

These financing frictions can result in a substantial underinvestment in innovation if firms with innovative opportunities have exhausted internal funds. That is, when additional investment requires external financing, firms may choose to forgo new investments altogether or choose to substitute innovative projects for less novel projects

[5] The data used in this article are not quite as comprehensive as those of (Tang, et al., forthcoming) who supplement Markit data with data from Credit Trade and GFI group to identify 901 firms with a traded CDS. I implicitly assuming that the firms that we cannot match but that have a traded CDS will not systematically bias our results. Results obtain both within the set of firms identified as CDS firms and in the full set of firms, thus this does not appear to be a tenuous assumption.

[6] The patent sample ends in 2005 because the data stop in 2006 and there is a truncation bias since we only observe patent applications that are eventually granted.

at the expense of long term growth. Understanding whether financial innovations can alleviate these constraints in an important issue.

In this section I investigate the potential role of credit default swaps (CDSs) as a shock to the supply of external financing for firm innovation. Building on the theoretical work of *Bolton and Oehmke* (2011), I claim that a likely channel through which CDSs can relax financing frictions is a transfer of bargaining power to creditors when firms have imperfect access to capital due to a limited commitment problem. If true, the introduction of CDS would particularly important for innovative firms, since innovation typically relies on intangible assets that are not pledgeable, such as highly specialized human capital.

Specifically, financing innovation is difficult because innovative firms rely on soft assets that are not pledgeable as collateral. As such, creditors are weary of innovative firms for fear of default states where the creditor has no bargaining power because the borrowers assets are of little value to the creditor.

Table A.2 presents OLS estimates of the impact of CDS trading on firm innovation. I follow Cohen et. al., 2012 and define R&D scaled by sales as my primary innovation input and patents scaled by the number of employees as my primary measure of innovation output. The results are obtained from the following regression:

$$Y_{i,t} = \alpha_{ind/firm} + \theta_t + \gamma_1 TRADING_{i,t-1} + \gamma_2 TRADED_i + \beta Controls_{i,t} + \epsilon_{i,t}$$

Where TRADING is a dummy variable equal to one in firm years with an actively traded CDS on a firm's debt and TRADED is a dummy variable equal to one if a CDS ever trades on a firm's debt during the sample period. The hypothesis is that γ_1 ¿ 0 in all specifications. Note that the TRADING variable is lagged one period, so we are looking R&D and patent applications in the year following the introduction of a CDS. Dependent variables of interest include the adjusted number of patent applications (npatents), the adjusted number of patent applications scaled by the number of employees in thousands (patents/emp), and research and development expenses scaled by sales (R&D/sales).

Controls include size, MTB, implied volatility, leverage, CF, and a dummy variable for whether the firm has a credit rating. Standard errors are clustered at the firm level. Columns 1, 2, and 4 include industry level intercepts and columns 3 and 4 include firm level intercepts. All columns include year dummies. The sample only includes firms that have positive R&D in at least one firm-year during the sample period. The patent data only covers 2001-2005 and is therefore has a more limited sample period than the analysis of R&D.

We can see that firms increase R&D/sales by (.13%-.25%) when a CDS is actively traded on their debt, corresponding to a 1.4% increase in R&D as a percentage of assets. These estimates are in line with the results found in [**?**] who look at financing constraints on innovative efficiency. We also see that innovation output increases by roughly 1.42-2.21 patent applications per thousand employees.[7] In column 1 the results on the raw number of patents is insignificant. Notice however that when we scale patents by number of employees the results become significant and the sign on firm size changes sign. This would suggest that raw patent count is picking up some non-linearity in size that scaling my the number of employees fixes.

1.3.2 Access to Unsecured Debt

If credit default swaps serve as a substitute for collateral, then we should see greater access to unsecured debt for firms after the inception of CDS trading. In table A.3 I present the estimates from a multinomial logit estimation (columns 1 and 2) on the decision of firms to obtain an unsecured or secured loan with no loan considered as the base case. Column 3 presents results from a binomial logit estimation for a simple binary choice dependent variable: loan vs. no loan, where loan includes both unsecured loans and secured loans. Column three is presented in

[7] Recall, these are patent applications that are eventually granted.

conjunction with columns 1 and 2 to illustrate that the extensive margin for obtaining a loan in general is also increasing.

As we can see in column 3, firms are 7.62% more likely to obtain a loan when a CDS is trading on their debt. Interestingly, this corresponds to a 9.08% increase in the use of unsecured debt and a 2.51% decrease in the use of secured debt for firms with an active CDS market. In unreported analyses I perform the same tests under probit and linear probability model specifications and find similar results. For brevity I only include the results for the multinomial logit estimation.

One might be concerned that these effects are driven by the potential that CDS trade on firms with less information asymmetry, since such firms are likely to obtain unsecured financing despite having the ability to pledge collateral. If CDS serve as a conduit for firms with limited pledgeability to obtain external financing, then we should see larger shocks to unsecured financing for firms with less pledgeable assets. In table A.4 I use a simple sorting procedure to illustrate which firms are impacted the most by the introduction of CDSs. In the full sample, I sort firms into quintiles based on tangibility, MTB, and R&D intensity. I then compare the percent of unsecured loans by quintile for firm-years with a traded CDS and firm-years without an actively traded CDS.

Both the relative and absolute changes in access to unsecured debt are increasing in pledgeability according to the R&D and tangibility proxies. According to MTB, the absolute change is increasing, but the relative change is slightly decreasing. In non CDS firm years, the lowest quantile of tangible firms' secures 82% of loans while the lowest quantile secures only 36% after the introduction of a CDS. However, firms in the highest tangibility quantile only decrease from securing 71% in non CDS firm-years to securing 40% in years after introduction.

The results in table A.4 do not take into account other variables that determine the demand or access to unsecured financing. In table A.5 I continue the analysis by looking at interactions in a linear probability model. I create two additional variables, *patentfirm* and *R&Dfirm*. These are dummy variables equal to one if a firm belongs in the top 40% of firms ranked on patenting intensity (R&D intensity), the number of patents (amount of R&D) scaled by the number of employees (total assets) in a given year. As described in *Titman and Wessels* (1988), R&D intensity provides a rough proxy for the uniqueness of a firms projects. Firms that invest heavily in R&D usually invest in highly specialized projects that rely more heavily on inalienable human capital which is not highly pledgeable. Thus, firms with high R&D intensity are probably more limited in their ability to pledge collateral. I take patenting intensity to have a similar interpretation.

The interaction of interest is TRADING×patentfirm (TRADING×R&Dfirm) where we can see that the impact of TRADING on firm access to unsecured debt is increasing (.0844) for firms with a limited commitment problem. As one would expect, the estimates are remarkably similar (.0844 vs. .0909) for the two classifications of limited pledgeability. In unreported results I check the estimates with probit and logit specifications, but only present the linear probability estimates because of the ability to include firm fixed effects in a linear probability model. The incidental parameter problem and non-convergent estimates prevent me from using firm-fixed effects in the probit and logit specifications.

To alleviate concerns that CDS are merely correlated with a firm's propensity to increase leverage, I run sub-sample analysis in tables A.7 and A.8 on the sub sample of firm-years in which a new loan is originated. I again separate the sample even further and consider only firm-years in which a new loan is originated on the sub sample of TRADED firms. The table presents probit, logit, and linear probability model specifications that include control variables to isolate the effect of an actively traded CDS market on unsecured financing. Table A.7 presents the parameter estimates, and table A.8 presents the more meaningful average marginal effects. All specifications include industry fixed effects, year fixed effects, and credit rating fixed effects. According to table A.8, the marginal effect of a CDS on access to unsecured capital ranges from 3.25% (column 3) to 6.26% (column5). The unconditional probability of obtaining unsecured financing in the sample is .48. So these estimates translate into a 6.7%-13% increase in access to unsecured debt relative to the mean.

Table A.9 provides a simplified linear probability model with an interaction effect for an alternative definition of firm pledgeability. The first interaction includes a dummy variable, *tang*, which is equal to 1 if a firm is in the top two quintiles of the full sample when sorted on tangibility. Similarly, *intang* is a dummy variable equal to 1 if a firm is in the top two quintiles of the full sample when sorted on intangibility.

Tangibility is a firm-level measure of expected asset liquidation values that borrows from Berger et al. (1996).[8] Firms with greater expected liquidation values will be able to obtain greater access to financing since creditors can recover a greater amount in bankruptcy. In determining whether investors rationally value their firms' abandonment option, Berger et al. gather data on the proceeds from discontinued operations reported by a sample of COMPUSTAT firms over the 1984-1993 period. The authors find that a dollar of book value yields, on average, 72 cents in exit value for total receivables, 55 cents for inventory, and 54 cents for fixed assets. Following their study, we estimate liquidation values for the firm-years in our sample via the computation:

$$tangibility = .715 * receivables + .547 * inventory + .535 * ppegt^{9}$$

Intangibility is defined as one minus property plant and equipment as a percentage of assets. Property plant and equipment are assets that can be pledged as collateral. The greater percentage of a firm's assets that consist of PP&E, the more tangible

[8] *Almeida and Campello* (2007) use this measure.

[9] This definition of tangibility is used in unreported results, but generates results that are not materially different from the alternative definitions listed below.

asset base they have. To convert the measure to a measure of intangibility I simply take one minus the ratio.

Column one (two) presents results from a LP model based on the sample of firms with *tang* = 0 (1). The interest is on the differences between the coefficients on TRADING between the two subsamples. Column three presents results on the full sample with an interaction term between TRADING and *tang*. Column four (five) presents results from a linear probability regression on the subsample of firms with *intang* = 0 (1), and column six presents the results from the full sample with an interaction term between TRADING and *intang*. The results suggest that CDS TRADING has a greater benefit on access to unsecured debt for intangible firms. The effect ranges from .0561% to 10.36%. These effects are similar in magnitude to the *patentfirm* and *R&Dfirm* classifications above.

1.3.3 Endogeneity and Potential Solutions

The existing empirical literature on credit default swaps in corporate finances is focused on the supply side of debt financing. The primary endogeneity concern in CDS analysis is whether the emergence of CDS markets is simultaneously determined with a firm's leverage and maturity decisions. For the supply side, one can instrument with bank's use in foreign exchange derivatives. This instrument was first used in *Saretto* (2013) and is meant to control for the propensity of certain banks to hedge in a way that is not not jointly determined by the borrowing firms' choice variables. This instrument works well for the supply side since banks typically have many borrowers and a banks general hedging propensity should not be heavily dependent on any single borrower. However, on the demand side, if selection of lenders is determined by factors correlated with a firm's investment decisions, then any instrument related to a bank's type could also be picking up differences in the type of a firm's investments. As such, I explore an alternative approach to identify a causal link.

I use a difference-in-difference analysis where the treatment effect is the 2003 enforcement of the ISDA provisions that greatly enhanced the liquidity of CDS contracts. The International Swaps and Derivatives Association (ISDA) is a trade organization of participants in the market for credit default swaps and publishes the definitions of credit events for CDS contracts. In 2001 the ISDA adopted a new set of provisions meant to address restructuring and the range of deliverable obligations that could be included in a credit default swap's master agreement. Restructuring was redefined to disallow the applicability of restructuring as a credit event where restructuring is limited to bilateral loans. The provisions also limited the range of deliverable securities that could be included in a contract, reducing uncertainty and ambiguity in pricing the derivatives. Both provisions were not put into effect until 2003, but they greatly enhanced the standardization of CDS contracts, making them much more liquid and easy to trade.

The enforcement of the ISDA provisions that greatly enhanced the liquidity of CDS contracts, representing a positive shock to the demand for CDS instruments. Thus there is intuitive reason to suspect the ISDA provision is relevant for affecting the propensity of credit default swaps to rade on a firm's debt. Futhermore, the 2003 ISDA provisions should be unrelated to a borrowing firm's choice variables other than through a potential impact on financing frictions.

Table A.6 presents the results of a difference-in-difference analysis. The treatment is the 2003 ISDA revision, and the treatment group is the set of firms that had a CDS market initiated on its debt during 2003. The control group includes all firms that had a CDS trading on their debt prior to 2003. All firms that either do not have a CDS trading at any point during the sample period, or no CDS trading until after 2003 are dropped from this analysis. In unreported analysis I include all firms that do not have a CDS begin to trade in 2003 as the control group with firm fixed effects. However, I believe the approach used in the presented analysis offers a more conservative approach to handling the selection problem. I use the set of firms with CDS trading prior to 2003 as the control group to control for any differences between firms that ultimately have a CDS trading and those that do not.

The important aspect of this analysis is that the introduction of a CDS in 2003 is not likely to be associated with the selection problems discussed above, but instead a result of the enhancement of the CDS market as a whole. Of course, the ISDA provisions also affect the liquidity of CDS contracts for firm's with existing CDS markets prior to 2003. However, I cannot rule out the selection affect for the existence of a CDS market on such firms. Furthermore, firms with CDS trading prior to 2003 are already likely to have benefited from the effects of CDS trading if such effects exist. It is also important to note that any positive benefits of CDS trading that stem from increased liquidity for firms that already have active CDS markets on there debt will only diminish the power of this test and not the validity.

I restrict the sample to two years before and after the event (i.e. 2001-2005). I use the treatment to test the effect of CDS on all of the primary variables of interest in the previous tables: namely, the raw number of adjusted patent applications (npatents), number of patents per employee (pat/emp), research and development expenses as a percentage of sales, (R&D/sale), leverage, and whether a new loan is unsecured (unsecured). The results are all consistent with the previous tests.

As an additional measure, I include a placebo (falsification) test where all firms with a CDS trading prior to 2005 are dropped from the sample and firms with a CDS market initiated after 2005 are artificially used as the treatment group (TREATED). In this setting I have identified a set of firms that have characteristics associated with CDS markets trading on their debt, but that do not experience any effect during 2003. As we should expect, the coefficients are all insignificant on the effect of TRADING on the variables of interest. I interpret this as corroborating evidence for 1) the timing of CDS trading apparently has an effect on firm financing and investment decisions and 2) even when the introduction of CDS markets is exogenous to firm investment decisions, there is still a significant effect on both financing and intangible investment.

1.3.4 Lender Analysis

If credit default swaps alleviate supply side frictions for debt financing, then there should be a larger effect for small banks who do not have as much bargaining power in debt renegotiations as larger, diversified banks. Smaller banks are less diversified than large banks and tend to be heavily concentrated in a particular region or industry. Because of their regional concentration, small banks are usually better suited to monitor nearby firms with highly specialized assets. Thus, on average, firms that rely on financing from relatively small banks are more likely to be firms that invest in less pledgeable assets.(insert citation)

Table A.10 presents results for the impact of CDS trading on access to unsecured debt as it relates to lender size. To define lender size, banks in the Dealscan database are merged to the federal reserve call reports on WRDS. A bank is classified as large if it is one of the 15 largest banks in the Dealscan database sorted on total grossloans outstanding for the bank holding company. The variable *biglender* is a dummy variable equal to 1 if a large bank is included on the loan facility of a firm according to Dealscan.

In the sample of firms with a traded CDS at some point during the sample period, there are only 203 loan facilities originated without a large lender as a syndicate member. Yet the impact of CDSs on access to unsecured capital is both statistically and economically more pronounced for these firms. Column one (two) of table A.10 includes the regression run only on firms without (with) a large lender on the loan syndicate $biglender$=0 (1). Columns three and four present results on the full sample with an interaction term $TRADING \times biglender$. Column three includes industry fixed effects and column four includes firm fixed effects.

It is important to note that all of the identification in table A.10 comes from within the sample of firms that have a CDS trading at some point during the sample period. The benefit of this approach is that we do not have to be concerned with any non-static, systematic differences between CDS and non-CDS firms that cannot be controlled for with dummy variables. One concern is that firms with the most limited asset pledgeability tend to be young, small firms while credit default swaps tend to exist mostly for more mature, large firms. While this is a concern, there is enough variation in the TRADED sample to test the relationship between asset pledgeability and access to unsecured debt. Furthermore, this may suggest that expanding the set of firms for which a CDS is traded would lead to greater economic growth.

1.4 Conclusion

Does the presence of a market for credit default swaps (CDS) have an effect on corporate financing and/or investment decisions? In a world with complete financial markets, CDS are redundant securities, in which case we would expect their presence to be inconsequential to the underlying firms. However, as illustrated theoretically by the *Bolton and Oehmke* (2011), in a world characterized by incomplete markets and financial frictions, it is possible that the presence of CDS securities can ease financial constraints for the underlying firm along some dimensions.

Recent empirical work has provided some interesting evidence with regard to this issue. In particular, Hirtle (2009) reports evidence indicating banks' willingness to extend credit increases when they are able to use credit derivatives to hedge risk. It appears that this shift in credit supply is exploited by client firms, as *Saretto* (2013) report that the presence of active CDS trading on a firms debt is associated with an increase in leverage and a longer average debt maturity. While this evidence on financing is interesting and important, the related question of whether the relaxation of financing constraints arising from CDS trading is ultimately transmitted to firms investment decisions has not yet been fully answered. I provide the first evidence that the supply side effects documented in previous literature interact with demand side financial constraints to affect corporate investment.

I claim that a likely channel through which CDSs relax financing frictions is a transfer of bargaining power to creditors when firms have imperfect access to capital due to a limited commitment problem. I argue that that any such role is most likely to manifest itself in innovative firms, since innovation typically relies on investment in intangible assets that are not pledgeable. I demonstrate that patenting activity and R&D spending increase at both the intensive and extensive margin following the introduction of CDS markets on a firm's debt. I show that firms see improved access to unsecured financing, highlighting that this effect may be particularly relevant for financing projects with limited collateral value. To substantiate this claim, I exploit cross sectional-variation in a firm's ability to pledge collateral, demonstrating the effect is stronger for innovative firms with an intangible asset base. To establish a causal link between credit default swaps and alleviation of financial constraints, I find corroborating evidence by exploiting the 2003 ISDA provisions as an exogenous shock to the likelihood that CDS markets are initiated on a firm's debt.

Collectively, the evidence provided in this paper suggests that the presence of an active CDS market mitigates collateral constraints and has a real economic impact by facilitating innovation and investment for companies with a limited ability to pledge future cash flows. More generally, my findings lend support the argument that financial innovation is important for economic growth.

CHAPTER II

Peer Effects of Financial Constraints on Innovation

2.1 Introduction

A large and growing literature in finance aims to understand how financial constraints affect corporate investment decisions. Most existing research in this area implicitly assumes that a firm's decisions depend on its own constraints but are independent of the constraints of competitors or peers. In contrast, other literatures have reasoned that firm actions are not always taken independently. For example, empirical evidence suggests that peer-firm leverage influences a firm's capital structure, pricing, and location decisions [*Leary and Roberts* (2014), *Shleifer and Vishny* (1992), and **?**]. In a similar fashion, we argue that that peer firm financial constraints have a direct impact on the investment opportunities and decisions of competitors.

The goal of this paper is to provide empirical evidence that firms alter their investment decisions according to peers' financial constraints. Financial constraints cause firms to forgo positive Net Present Value (NPV) investments. This can manifest in decreased competition for rivals, which increase projected cash flows on substitute goods and services. Furthermore, constrained firms are less able to adapt to new products and services offered by competitors. This opens up the potential for unconstrained competitors to establish significant barriers for future product development. All else equal, it follows that a firm's investment opportunity set increases as its peers become financially constrained.

We use patent citations of public corporations to identify firm innovation networks and study firms competing over similar technologies. We then create a measure of competitor financial constraints by taking the average financial constraints across a firm's technological peers. Using this measure, we find that firms increase their patenting activity in response to peer firms becoming financially constrained.

To further examine the strategic implications we create a measure of patent portfolio similarity between firms. We find that firms shift their patenting portfolios in the direction of constrained peers and away from less constrained peers. This movement turns out to be short lived and we find that new patents generated while moving closer to constrained peers receive fewer self citations when compared to a firm's previous patents. We interpret this as evidence that firms use patents to produce barriers for future innovation while peers are financially constrained.

Empirically testing hypotheses about interactions between peer-firm constraints and investment decisions is challenging for several reasons. First, limited data makes identifying inter-firm relationships difficult. The usual response to this problem is to control for unobserved heterogeneity with industry or firm fixed effects, but these approaches often ignore relevant information in cross-firm relationships that can help us understand the true impact of financial constraints on firm investment.

Second, information is usually aggregated at the firm level which limits the ability to study specific investment decisions. Finally, it is difficult to identify changes in financial constraints that are not related to simultaneous changes in firm investment spending.

Patenting is a type of firm investment that is particularly well-suited for studying strategic responses to peer-firm investment constraints. Patents are required to include detailed self-descriptions and to cite patents on related work (which are verified by a reviewer). This means that patent citations can be used to identify links between firms that have the ability to apply specialized knowledge unique to a particular technology space or product mix. The ability to identify firm meaningful firm connections allows us to study interactions between investment decisions and peer-firm constraints.

The patent market also provides benefits over the product market when studying financial constraints. As constrained firms are forced to cut projects we might naturally expect them to abandon less developed work rather than more stable projects that are likely easier to finance. While most firms' product mix includes both young and old investments, patents by definition represent new ideas. Furthermore, a firm forced to cut projects in the development stage may not have secured the necessary patents to protect a project from being taken by competitors.

There are several ways that a firm's patenting behavior might change as peers become constrained. While competitors are financially exposed they may not have the resources necessary to fight patent infringements or to continue publishing patents vital to continuing a particular project. A firm may "fill in the gap" of a competitor's unprotected intellectual property by publishing patents critical to the production process. An unconstrained firm can then steal the project by gaining monopoly rights to claim a production technology.

Alternatively, a firm may move closer into a competitor's technological niche and produce patents never intended for direct use, but for the sole purpose of blocking a competitor's future ability to patent in the area. This can prove effective because the constrained peer will have to maneuver around these patents, or avoid investing in similar technologies altogether.

It is not trivial that a competitor's financial constraints should have a positive effect on firm innovation. A large portion of innovation occurs collectively when competing firms share knowledge through mimicking and shared labor pools. A vast literature in economics, both theoretical and empirical, claim that knowledge spillovers are a crucial component of successful innovation (*Bloom et al.*, 2013). As constrained firms are forced to cut innovative projects the potential for knowledge spillovers is diminished. If the reduction in the knowledge pool is significant enough it may hinder a firm's innovation.

One may be concerned that a firm's innovation (quality) and their propensity to become financially constrained are jointly determined. For example, good management may successfully motivate R&D creativity and secure strong financing relationships. Alternatively, one might believe that innovation quality is persistent, leading firms with high patent quality to drive their peers into greater financial constraints.

To alleviate these concerns, we exploit two shocks to financial constraints that are unrelated to patenting activity other than through changes in a firm's cash flow. First, we use the AJCA repatriation tax holiday in 2004 as a positive cash shock to firms with an international presence. This law change provides a relaxation of financial constraints for firms with significant international cash flows, but it does not impact the cash flow of purely domestic firms. We find a significant decrease the strategic movement for firms whose peers benefited from the AJCA tax holiday. Furthermore, the effect is isolated to firms who's peers were constrained before the tax holiday.

As a second treatment we use the junk bond crisis of 1989 as a negative shock to fundraising for firms with a junk bond credit rating. We find strong evidence of strategic patenting activity for firms with competitors that were adversely affected by the junk bond crisis. Specifically, firms that have peers with junk bond ratings exhibit a significant increase in patenting activity and move patent portfolios in the direction of their junk rated peers after the crisis.

Our study draws on three distinct and growing literatures. First, we contribute to the literature on the impact of financial constraints on firm investment. *Fazzari et al.* (1988) wrote one of the first and most influential papers on this topic, highlighting that financing constraints can have a profoundly stifling effect on investment. A host of literature followed finding similar results, but highlighting many difficulties in estimating financial constraint and investment interactions.[1]

However, the empirical literature on financial constraints is quite scarce when it comes to inter-firm relationships. This contrasts the voluminous corporate finance and industrial organization literatures on financial distress, which directly acknowledge that peer firm financial distress can incentivize predatory behavior for cash-rich firms [cites].

These literatures typically rely on the presence of "prey" firms near bankruptcy and on driving the prey firm to exit as the primary objective for the predator [Tirole 2006]. This setting may not be appropriate when studying the strategic interaction of public corporations that may be financially constrained - unable to invest in all positive NPV projects because of financing frictions - but not financially distressed (near bankruptcy). If competitor bankruptcy is unlikely, then typical predation strategies such as price cutting and overproduction may not be relevant and firms may need to focus strategies on more incremental goals for capturing market share.

We also draw on recent literature on innovation in finance by examining how peers' financial constraints affects patenting decisions. Our paper is closely related to the work of *Almedia et al.* (2014) who find that financial distress can force managers to weed out inefficient projects. *Manso* (2011) shows that long-term contracts are optimal to incentivize managers to undertake innovative projects instead of short-term, safer projects. *Brown et al.* (2012) finds that R&D investment is exceptionally sensitive to financial constraints, leading to greater underinvestment. We add to this literature by showing that firms innovate strategically when competitors are financially constrained. This can have welfare implications as financial constraints can ultimately lead to fewer firms holding larger pieces of a smaller innovation pie. (i know this sounds terrible, but its a good point we need to make more clearly).

Finally, we contribute to the literature on peer firm effects in finance. One of the

[1] For example, Whited 92, Almeida Campello 2007, Alti 2003

problems in studying peer effects is the identification of peer firms. When simple classifications are used, such as industry categories or sorting on firm characteristics, the reflection problem (see *Manski* (1993)) can make identification nearly impossible. Furthermore, links according to these methods may not always represent meaningful relationships for the questions being studied. Our paper provides a novel approach to circumventing these problems.

2.2 Data and Summary Statistics

We use patent data from the National Bureau of Economic Research (NBER) patent data project. In a joint effort on the project, the United States Patent and Trademark Office (USPTO) and *Hall et al.* (2001) have collected data on over three million patents and 16 million citations. The project has recently been updated to include data from 1976-2006. For each patent, we observe the patent's technological category, application date, grant date, the list of cited patents, and information about the patent's assignees (inventors).

Hall et al. (2001) (henceforth HJT) have graciously provided a match between the patent assignees and COMPUSTAT that dynamically tracks the ownership of each patent. With the match provided we are able to link firm's financial information to the patents using the CRSP-COMPUSTAT merged database.

It is well-documented that patenting (citing) propensities exhibit tremendous heterogeneity across patent technology classes and through time. HJT develop a structural approach and a reduced form approach to adjust patent and citation counts. In this paper we follow related finance literature and employ the reduced form approach [e.g. *Seru* (2012)]. The procedure involves sorting patents into six technological classes with 36 total subcategories. Each patent (citation) is then scaled by the total number of patents (average citations per patent) in each class-year.[2] These adjusted

[2]We ran our results with both the 6 category and the 36 category adjustments and find qual-

patents (citations) are then aggregated at the firm-year level, creating a weighted sum of each firm's patents.

For measures of financial constraints, we use the *Whited and Wu* (2006) financial distress index (WW) and the size-age distress index (SA) developed by *Hadlock and Pierce* (2010). These measures serve two purposes in our regression specifications. First, we use these variables to build financial constraint indices of a firm's competitors. We provide a detailed description of this process below. Second, we use SA and WW as a control variables for a firm's own financial constraints. These control variables are important because firm performance is likely to be highly correlated for firms innovating in the same technology space, and we do not want our competitor constraint indices to be capturing this correlation.

Other control variables include Market-to-Book (MTB), research and development spending divided by sales (RDS), cash holding (cashholdings), profitability (profit), and tangibility (tangibility). MTB is used to control for changes in a firm's investment opportunities that are unrelated to peer-firms' financial constraints. Cash holdings, profitability, and tangibility are additional controls related to a firm's financial slack and investment opportunities and are commonly used controls for regressions on patent variables (for a detailed discussion see *Seru* (2012)). For industry classifications we use the Fama and French 17 industry portfolios.

In our final sample, we have 18,232 unique firm-year observations. The summary statistics are presented in Table 1.

[Insert Table 1 HERE]

All variables are winsorized at the 5% level (2.5% level in each tail). This ensures that extreme observations do not drive the results. Indeed, our results are not sensitive to the level of winsorization. Additionally, the financial constraint variables WW, SA, WW_cited, and SA_cited are normalized to have zero mean and a standard

itatively similar results. Results presented in this manuscript are obtained with the 36 category adjustment as we believe it to be the more accurate approach.

deviation of one. This simplifies the interpretation of the beta coefficients and allows for easier comparison across the specifications.

2.2.1 Defining Firm Relationships with Patent Citations

The NBER data set includes detailed information on patent-to-patent citations. Patents are required by law to cite influential and related work. Thus, patent citations represent a link between closely related patents and directly track the evolution of innovation both within and between firms. Both the patent applicant and patent reviewer, assigned by the USPTO, are permitted to add to the list of patent citations.[3] This helps to ensure that all relevant patents are cited. [cite] and [Li et. al., 2015] are among other studies to use patent classes to categorize firm relationships.

In our sample, firm A is defined as a peer to firm B if firm B cites firm A during the previous five years. In determining the length of time that firms remain linked, we face a tradeoff between having more links in our sample and having the link represent a meaningful connection. For example, two firms that cited each other on patents developed 20 years prior without any subsequent citations might not represent a meaningful relationship because the firms might have drastically changed their research and development focus. On the other hand, shortening the window too much would unnecessarily rule out meaningful firm relationships and decrease the power of our tests. We believe using a five-year window as our baseline provides a good, although admittedly subjective balance between the two tradeoffs. We check the robustness of our results using two-year and seven-year links and find similar results.

The firm links are then used to construct our main independent variables of interest. We define competitor's financial distress as the simple average of the WW index (SA index) for each of a firm's linked competitors by firm-year.

[3] Patents. Simplified.: Entrepreneur's Guide To US Patents And Patent Applications. Ozluturk, Kimmelblatt, and Patel (2013)

$$FC\_cited_{i,t} = \frac{\sum\limits_{j \in C_t,} FC_{j,t}}{num(C_t)},$$

where C_t is the list of firms cited by firm i and $num(C_t)$ is the number of firms in C_t. We normalize these financial constraint variables to have mean zero and standard deviation one to aid in interpretation of the regression results. Although the variables are normalized, they exhibit significant variation in both the cross-section and the time-series.

In a similar fashion we construct peer-firm variables to control for average research spending (RDS_cited) and market-to-book (MTB_cited). We control for these variables because we want to isolate the effect on peer-firm financial constraints holding constant the level of peers' investment opportunities and R&D spending. This aids in the cross-sectional interpretation of our results....why?

2.2.2 Patenting Responses to Financially Constrained Peers

In Table 2, we estimate our most basic firm-level regressions of patent outcomes on peer firm financial constraints (FC_cited) lagged by one period. Panel A presents results for regressions with firm fixed effect transformations and year dummy variables. Panel B repeats the analysis including industry-times-year dummy variables and no year dummies. In columns 1 and 3 of panel A we estimate the following regression:

$$adjpatents = \alpha_i + \gamma FCcited_{i,t-1} + \beta controls_{i,t-1} + \theta_t + \epsilon_{i,t}$$

and in columns 2 and 4 we estimate the following regression:

$$adjcitations = \alpha_i + \gamma FCcited_{i,t-1} + \beta controls_{i,t-1} + \theta_t + \epsilon_{i,t}$$

Where FCcited is the financial constraint measure. In columns 1 and 2 we use the Whited Wu index to build FCcited and in columns 3 and 4 we use the SA index. A detailed discription for how we calculate this measure can be found in section!! The vector of controls includes a firm's own market to book ratio (MTB), research and development expenditures scaled by sales (RDS), profit, cash holdings, and tangibility as well as peer firm market to book ratios and research and development expenditures. All dependent variables are lagged by one period. We expect γ to have a positive sign since we are controlling for a firm's own financial constraints, which should mitigate any spillover effects due to industry specific collateral shocks or other industry shocks to fundraising.

It is not trivial that γ should be positive. Knowledge spillovers are considered an important determinant of patent activity in the economics literature on innovation [citations]. Once a firm is forced to cut projects because of financial constraints there is less potential for knowledge spillovers to related firms. If knowledge spillovers dominate in our setting we would expect peer firm financial constraints to have a negative effect on patenting activity ($\gamma < 0$).

On the other hand, if competition dominates in our setting, then firm innovation should benefit when peers become financially constrained. There are two primary ways that a firm can take advantage of constrained rivals by using patents. First, a firm can directly take on positive NPV projects that constrained competitors are forced to cut. If constrained firms choose to cut longer term, less developed projects first then we should expect to see a lot of this activity take place in the technology space rather than in the product market. After a constrained firm stops developing a positive NPV project, a competitor could begin patenting technology critical to the production process and steal the project.

Second, firms can take advantage of constrained peers' inability to fight patents in the legal system. They can do this by flooding the market with patents to prevent future development by rivals in targeted technological areas. We cannot distinguish between these two alternatives in the regressions reported in table 2, but it is important to note that both scenarios would lead to a positive sign on γ.

Turning to the magnitude of the results in columns 1 and 2, a one standard deviation increase in the rivals' financial constraint index leads to 0.388 (7.359) additional adjusted patents (adjusted citations). The constraint variables in columns 1 and 2 are constructed using the Whited Wu index. In columns 3 and 4 we use the SA index as an alternative input to construct FCcited. Using the coefficient estimates from columns 3 and 4, a one standard deviation increase in rivals' financial constraints leads to 0.305 (4.397) additional patents. These estimates represent 20-30% increases relative to the respective means of the dependent variables. The positive sign on the adjusted patent coefficients suggest that firms increase patenting activity as competitors become more constrained and the positive sign on the adjusted citation coefficients suggest that citations also increase.

Patenting activity tends to be clustered in time due to technological breakthroughs and the compounding effects of knowledge spillovers. Although we adjust our patent measures to account for time and industry varying patent propensities, these adjustments aren't perfect and don't account for time and industry varying shocks to financial constraints. For this reason, we include year dummies in the regressions represented in panel A to control for any aggregate shocks that are potentially related to firm financial constraints and patenting activity. We also replace year dummies with industry-times-year dummy variables in panel be to allow for aggregate time shocks to be heterogenous across industries.

2.2.3 Identification

There are some potential endogeneity concerns with our baseline regressions. First, one could be concerned about reverse causality - successful firm innovation could be driving up rivals' financial constraints rather than rivals' financial constraints influencing patenting activity. If innovative firms capture future market share by securing crucial patents in a technological area, then creditors could be weary to lend more to rivals innovating in the same technology space.

Second, our financial constraint measures could potentially exhibit significant measurement error.[4] Innovative firms tend to hold less cash and have lower book values because they have a less tangible asset base and.... It might be that innovative firms appear constrained in our sample while they simply have different firm characteristics.

To address these concerns we exploit two exogenous shocks that are only related to patenting activity through their effect on peer firm's financial constraints. Specifically, we use the AJCA tax holiday as a positive shock to the cash holdings of financially constrained firms with significant international operations. As a second, negative shock to fundraising, we look at firms with junk bond status before and after the junk bond crisis in 1989.

In Table X, we report the results of the difference in difference regressions using the AJCA tax shock as a treatment variable. We compare the difference in patenting activity of firms who's rivals have significant overseas revenue to those that do not before and after the passage of the AJCA. This allows us to identify how a loosening of rival firms financial constraints affects strategic patenting behavior. The main variable of interest in the indicator for treated interacted with the indicator for post. The controls in this regression are the same as those used in Table 2.

Loosening of peer-firm financial constraints results in 0.813 (12.89) fewer patent applications (citations). This is a significant effect representing approximately 35-50% relative to mean change in patenting and citation activity. We see the largest impact for firms with rivals that were financially constrained before the event and that received a loosening of their constraints after the tax holiday.

To see this, look at the interaction effect (treated$\times$ post) in table 3. We include the same control variables as before. The variable of interest is FC_cited $\times \nVdash[Treat \times Post]$. This coefficient tells us the change in patenting (citation) behavior for an 1

[4]Several papers we should cite on FC measurement error

standard deviation in rivals financial constraint who [Need more here]

In columns 1 and 2, the financial constraint variable we focus on is the Whited Wu Index. The coefficient for patents (citations) is -0.916 (-17.50), significant at the 1% level. In columns 3 and 4, the financial constraint variable we focus on is the Size and Age Index. The coefficient for (citations) increases by -0.508 (6.818), significant at the 1% level.

In this table, we look at the results related to the junk bond crash of 1989. Firms who issued cheap junk bond debt prior to 1989 were not able to roll over their debt in 1989 and early 1990. This caused their cost of capital to increase drastically. This negative financial shock was harmful to their business and their competitors could take advantage of the weakened financial state of these firms.

In Table Junk , we report the results of the difference in difference regressions of this junk bond shock on patenting activity. To be specific, we compare the difference in patenting activity of firms whos rivals issued different amounts of junk bond before and after the junk bond market crash in 1989. This allows us to identify how a tightening of rival firms financial constraints affects strategic patenting behavior. The main variable of interest in the indicator for treated interacted with the indicator for post. The controls in this regression are the same as those used in Table 2.

2.2.4 Patent Portfolio Similarity

Measuring the similarity between firms' patent portfolios allows us to test whether firms shift their patenting portfolio in response to competitors' financial constraints. As mentioned in the data section, the USPTO classifies patents into one of 36 technology categories. It is rare for public corporations to patent entirely within a particular category.[5] By measuring the overlap between two firms' patenting portfolios we can get a sense of their technological similarity. The Mahalanobis Distance between firms

[5]fewer than 27% of firms in our sample patent exclusively in one technology class.

i and j at time t is defined as:

$$MD_{ij,t} = \sqrt{(\mathbf{P_{i,t}} - \mathbf{P_{j,t-1}})COV^{-1}(\mathbf{P_{i,t}} - \mathbf{P_{j,t-1}})}$$

where $\mathbf{P_i}$ is a 36×1 vector representing firm i's successful patent applications in year t. Each element of $\mathbf{P_i}$ contains the percentage of a firms total patents produced within an indexed technology class. COV^{-1} is the variance-covariance matrix of year-level patent portfolios aggregated across firms. The more firms tend to patent in the same technology class the smaller their Mahalonobis distance will be (the more similar their portfolios will be). Firms that patent in the exact same proportions in each technology class will have a distance of zero.

There are many potential measures for estimating patent portfolio similarity. However, Mahalanobis distance has the added advantage that it does not treat technology classes as orthogonal. The variance-covariance matrix explicitly accounts for the fact that some technology classes are more related than others by weighting observations according to cross-category patenting propensities. Under the MD metric, a firm patenting entirely in computer hardware is closer to a firm patenting entirely in computer software than it is to a firm patenting entirely in automobiles.[6] This beneficial characteristic of the MD metric is discussed in detail in [Bloom et. al., 2013].[7]

2.2.5 Self Citations

Firms may shift their patent portfolios in the direction of constrained peers for two reasons. First, the firm could be picking up projects cut by competitors. If this is the case, then we should expect the firm to continue producing follow up patents in the same technological area to maintain monopoly rights as the project develops. These

[6]Under a metric that treats technology classes as orthogonal, a firm patenting solely in computer hardware would be completely unrelated to a firm patenting entirely in computer software. While this is possible, it is unlikely that there is little overlap between the two classes.

[7]Note that if we calculated MD using the same time period for each firm's patent portfolio the measure would be symmetric, which means that firm A would be the same distance from firm B as firm B would be from A. In our setting this will not be the case because of the one year lag between the patent portfolios being measured.

follow-up patents will cite preceding patents on the project. Therefore, we should expect new patents, meant to help capture projects cut by competitors, to continue to receive self citations at a similar rate to other projects developed by a firm. (what if the new projects are significantly better or worse than a firm's existing projects? Can we claim that, on average, new projects should be similar to old projects? - Since we are comparing firms in the cross-section, I think so, but we need a better justification)

Second, a firm may choose to patent in the same technology space as a constrained peer in order to make it more difficult for the peer to continue developing a particular technology. A firm will face less opposition when publishing patents while peers are too constrained to fight patent infringement or to continue the pace of innovation required to maintain the appropriate legal rights on a particular technology. If a firm is publishing patents for the sole purpose of blocking peer-firm innovation, or forcing peer firms to maneuver around patents, then we have no reason to believe self citations will keep pace with a firms previous patents.

We build two measures of self citations: self citations as a percentage of total citations on new patents, and self citations per new patent application. If firms are capturing projects cut by competitors, we should expect both measures to stay the same or increase as peer firms become constrained. If firms are issuing patents to block peer-firm innovation, then the effect should be negative.

2.3 Conclusion

A large literature in corporate finance considers the relation between a firms financial constraints and its investment behavior. If financial constraints limit a firms ability to invest, this could have important implications for other market participants who are affected by the firms actions. In particular, a firms competitors may seize upon the opportunity offered by the presence of the firms constraints and alter their own investment behavior. Thus, constraints could have a substantive effect on industry competitive dynamics, industry level investment, and also the composition of that investment. While this feedback from a firms finances to competitor decisions has been recognized in the context of capital structure, pricing, and location decisions [Leary and Roberts (2014), Shliefer and Vishny (1992), and Chevalier (2015)], the presence of a relation between a firms financial constraints and competitor investment decisions has not been widely explored.

In this paper we directly examine the relation between financial constraints and competitor investment decisions. To conduct powerful tests, we focus on a specific type of investment, namely investment in intellectual property in the form of patented innovations. There are two principle advantages to focusing on this type of investment. First, it provides us with very micro-level information on the nature of a firms investment decisions in terms of the type of investment made and the ultimate success of the investment as indicated by subsequent patent citations. Second, by looking at cross-referencing of patents and patent portfolio similarity, we can carefully measure the degree of product-market overlap of the other entities that compete with any given firm.

Using a large sample of Compustat-listed firms from 1976-2006, we find strong initial evidence of a positive relation between a firms financial constraints and competitor patent-related investment decisions. In particular, using the WW and SA indexes of financial constraints, we find that an increase in constraints is associated with increases in patents by competitors. This result appears robust to a variety of different model specifications and choices. This initial evidence suggests that constraints not only limit a firms own investment decisions, but also they spur others in the same competitive space to invest more heavily. If this is indeed the case, the consequences of being financially constrained may be even larger than is commonly recognized.

While our initial evidence is compelling, there is the usual vexing issue of causality. In particular, one may be concerned that competitor constraints and the technology governing a firms patent-related investment opportunities could be positively correlated due to omitted factors that our empirical models cannot perfectly control for or, perhaps, because of reverse causality. To account for these possibilities, we identify two different exogenous shocks that should affect competitor constraints while not affecting a firms own patenting opportunities independent of the competitive feedback effects that we seek to identify. The first of these is the AJCA repatriation act of 2004 that effectively relaxed constraints for firms with overseas operations by lowering the effective tax rate on cash harvested from overseas operations. The second shock is the 1989 junk bond market collapse that effectively increased constraints sharply for firms that relied on this market as a major source of finance.

When we use the time variation in constraints driven by these exogenous events, our results are very similar to our initial evidence. In particular, we find compelling evidence that increases (decreases) in competitor constraints are associated with more (less) patent-related investment activity. These results are significant in both an economic and statistical sense, and they increase our confidence that we have identified an underlying positive causal relation between competitor constraints and patent-related investment spending.

To augment this evidence on the level of patenting activity, we also consider shifts in the type of activity firms pursue when a competitor becomes more constrained. Interestingly, we find evidence that a relative shift in constraints across a firms portfolio of competitors results in a shift of a firms spending towards the more/increased constrained firms and away from the less/decreased constrained firms. This is an interesting finding as it suggests that both the level and type of investment are affected in substantive ways by interplay between competitive dynamics and financial constraints.

Taken as a whole, our findings provide interesting new evidence that financial constraints not only limit a firms own spending, but also invite aggressive competition in the form of increased spending by financially stronger firms in the same competitive space. For the types of firms we study, in which intellectual property is a key ingredient for long-run success, the damage done by this competitive feedback effect could be quite severe. If this is indeed the case, our results suggest that firms in these environments should have a natural tendency to make financial and organizational choices that relax financial constraints to the highest degree possible.

CHAPTER III

Panel Data Estimation in Finance

3.1 Introduction

The use of panel data is extremely common in finance research. An important benefit of the panel structure is that it allows researchers to control for omitted unit-level factors that do not vary over time but may be arbitrarily correlated with explanatory variables of interest. In a common finance panel setting, the firm-year is the unit of observation and panel data estimation techniques are intended to control for the presence of time-invariant firm fixed effects. The most common panel data estimator in the recent finance literature is the fixed-effects estimator. However, other cousins (both close and distant) of this estimator are also occasionally used.

An important stream of recent research highlights some of the errors that appear in the finance literature in studies that rely on the fixed-effects estimator applied to panel data. First, as has been highlighted by *Gormley and Matsa* (2014), many researchers often do not calculate the fixed-effects estimator correctly because of errors in the process of transforming the variables that enter the regression equation. Second, as highlighted by *Petersen* (2009) and *Thompson* (2011), researchers often use standard errors that do not adequately adjust for the types of error correlation structures and heteroscedasticity that are ubiquitous in finance settings. There are standard solutions/approaches to these problems that are well known in the econometrics literature (see *Wooldridge* (2010). These solutions can be implemented by transforming the data in a way that is consistent with the underlying model of interest and by using the appropriate estimation commands and options in standard microeoconometric software packages such as Stata (see *Cameron and Trivedi* (2010)). In some cases, for example fixed effects along multiple dimensions, directly programming to create an estimator may be necessary (see *Gormley and Matsa* (2014)).

While this recent literature makes numerous useful and important points, it does not emphasize a fundamental assumption that must be true for the Fixed Effects estimator, or its cousin, the First Difference estimator, to have any hope of consistently estimating the coefficients of interest. The use of these estimators to derive consistent estimates requires reliance on a Strict Exogeneity assumption. This is a much stronger requirement than the typical notion of contemporaneous exogeneity, which (loosely) only requires a lack of contemporaneous correlation between the error term and the explanatory variables. In particular, as articulated by Wooldridge (2010), strict exogeneity effectively requires there to be no feedback from the dependent variable to future values of the independent variable. Even a cursory look at the variables used in finance research suggests that this assumption is usually violated. Many of the dependent variables of interest to financial economists, for example firm performance/returns, leverage, and compensation are almost surely related to the subsequent evolution of the explanatory variables of interest such as firm size, risk, or governance characteristics. In fact, many dynamic theoretical models posit exactly this type of feedback process.

In this paper we examine the strict exogeneity assumption in a set of canonical panel-data regression models selected from the existing finance literature. For each of these models we: (a) formally test whether the strict exogeneity assumption holds, and (b) explore whether failures in the strict exogeneity assumption are likely to lead to substantive inconsistencies in common estimators. We present overwhelming evidence that the strict exogeneity assumption is, in fact, quite commonly violated. In fact, when we use large samples, we can reject the validity of the strict exogeneity assumption in virtually all of the canonical regression models we consider. Thus, there is little hope that the common FE (or FD) estimates that appear in much of the finance literature are consistently estimating the parameter of interest. If the estimates cannot be expected to converge to their true values when the number of cross-sectional units (N) grows without bound, any concerns about the nuances of the standard error calculation would appear to be a relative side-show.

With regard to whether failure of the strict exogeneity assumption leads to substantive estimation errors with meaningful economic content, it is difficult to make strong statements without knowing more about the underlying structural dynamics. However, do uncover several facts that suggest that this problem can have a meaningful effect on economic inferences in finance settings. First, we note that the problem of inconsistency in the FE estimator is known to be on the order of 1/T, where T is the number of time periods, suggesting that the problem may become small if T is large (see *Nickell* (1981)). Unfortunately, this result depends on the presence of stable (i.e., time-invariant) fixed effects. As we show below, there is substantial evidence that in typical finance settings unit-level fixed effects do appear to change over time, perhaps because of occasional discrete changes to the management, ownership, or governance of firms. Thus, in the minority of settings in which a large number of time periods are even available, it appears unlikely that the 1/T result will solve the problem.

To gauge the possible magnitude of inference errors when a maintained assumption of strict exogeneity is violated, we consider the relative variation in the FE and FD estimates when applied to common datasets. Under strict exogeneity, these two estimates asymptotically converge to the same true underlying parameter value. If strict exogeneity is violated, as frequently appears to be the case, these estimators have different probability limits, neither of which is the true parameter value of interest. In the settings we examine, we find that the difference between the FE and FD estimators can be quite large, with differences on the order of 100% being relatively common. Moreover, there are some cases in which these estimators are significant and of opposite sign. These pathological cases are on the order of 20 times more prevalent than would be suggested by chance under some conservative assumptions. Taken as a whole, our evidence suggests that a large portion of prior research uses an estimation method that leads to inconsistent estimates and this inconsistency can be substantive. Thus, even if a researcher carefully follows the recommendation of the related recent literature, in many cases they will likely estimate something that differs non-trivially from the parameter of interest.

Our findings lead to a challenge as they suggest that simple FE or FD panel data estimators are in many cases not the correct tools to use in finance research in settings that include the presence of unit-level fixed effects. At the very least, our evidence suggests that one should test the strict exogeneity assumption in all settings before proceeding with these estimators. If, these tests reject, as appears to commonly be the case, one can either settle for an inconsistent estimator, an unappealing option, or turn to alternative estimators using either the same data at hand (the internal option) and/or techniques that exploit additional outside information (the external option).

With regard to the internal option, there are a variety of different estimators, mostly of the GMM variety. While it is beyond the scope of our paper to comment on these specific estimators, in the spirit of our analysis it is worth noting that common GMM estimators in finance (i.e., *Arellano and Bond* (1991),*Blundell and Bond* (1998)) also rely on testable assumptions related to the suitability of those methods. In particular, those methods usually maintain an assumption of no serial correlation, an assumption that can be tested. A recent paper by *Dang and Shin* (2015) demonstrates that these tests quite frequently reject, at least in the context of dynamic capital structure research. This suggests that the internal option may unfortunately at times offer no improvement over the more traditional alternatives.

The external option where new information is brought into a panel to identify the effect of interest is of course usually the most desired course of action. The challenge is in identifying good instruments that both satisfy the exclusionary restriction and the relevancy condition. While some notable successes along these lines have appeared in the finance literature, there is little of generality that can be said about this approach as it usually relies on special (often one-time) events such as exogenous shocks to a firms economic, legal, regulatory, or tax environment. In many contexts, shocks/instruments of this type are not readily apparent.

In an effort to offer some constructive guidance to finance researchers given the econometric challenges we highlight, we consider a systematic (quasi) external approach to identification that exploits industry-year variation in the explanatory variable of interest as in instrument for firm-level variation. Recent research has emphasized industry-year variation as either a potential nuisance factor to be controlled for (*Gormley and Matsa* (2014)), or as a source of variation to test whether theoretically irrelevant factors may affect economic decisions (e.g., *Jenter and Kanaan* (2014)). We suggest that in some cases industry-year variation is not a nuisance, but rather can be viewed as useful and theoretically relevant if exploited as an instrument for the underlying firm-level explanatory variable of interest. Of course the usefulness of this variation as a potential instrument will depend on the context, an issue we discuss at length below.

To illustrate the potential of this approach, we consider the role of firm risk in determining a firms level of managerial ownership. This is a context in which the data clearly reject the strict exogeneity assumption. Moreover, even the weaker requirement of contemporaneous exogeneity is highly questionable given the possible contemporaneous feedback effect from ownership to risk taking (see *Tufano* (1996)). As we would expect if firm risk is partially driven by industry shocks, we find a strong positive relation between innovations in industry risk and firm-risk, where the firm is excluded in the industry calculation. Thus, industry-risk innovations certainly appear to satisfy the relevancy condition. We argue that the exclusionary restriction is also very likely to hold in this context, as feedback from innovations in ownership and/or firm risk to industry risk would appear to be negligible, at least when we exclude large firms in concentrated industries.

When we proceed to instrument for firm-risk innovations with industry-risk innovations, our preliminary evidence indicates a significant positive role for risk in the determination of managerial ownership levels. While this result is interesting in its own right, for our purposes the more important point is that it suggests that exploiting industry-level innovations in explanatory variables of interest to achieve convincing identification in panel data contexts in finance may be a productive strategy. Given our evidence that many of the widely used panel-data approaches rely on assumptions that are rejected by the data, this would appear to be a particularly useful strategy to exploit in some settings, particularly when the other identification approaches discussed by *Roberts and WHited* (2012) are not feasible.

3.2 Prior literature and empirical strategy

3.2.1 Outlining the problem

While the strict (also called strong) exogeneity assumption is discussed in textbook treatments of panel data, with a couple of notable exceptions, this assumption is almost never acknowledged or addressed in finance panel-data applications. Given this lack of familiarity to finance audiences, we briefly outline the issue here with a specific eye towards finance applications. The reader is referred to textbook treatments for more of the technical details (e.g.,*Cameron and Trivedi* (2005), *Wooldridge* (2010)).

We consider a simple regression model with a dependent variable y, a single independent variable of interest x, and an assumed model in which $y_{it} = \alpha_i + \beta x_{it} + \epsilon_{it}$, where i denotes an arbitrary cross-sectional unit (from 1 to N) and t denotes an arbitrary time period (from 1 to T). Since in finance applications N is almost always much larger than T, all asymptotics will be for N approaching infinity.

Following *Wooldridge* (2010), we will refer to the assumption $E(\epsilon_{it}|x_{it}, \alpha_i) = 0$ as the contemporaneous exogeneity assumption and $E(\epsilon_{it}|x_{is}, \alpha_i) = 0$ for all t and s as the strict exogeneity assumption. Assuming contemporaneous exogeneity holds, and recognizing that lagged control variables can always be added to the model, we are concerned primarily with violations of strict exogeneity in which $E(\epsilon_{it}|x_{is}, \alpha_i) \forall s > t$. To see how this assumption may be violated, consider the case in which higher realizations of the dependent variable at time t (say firm performance) have a positive effect on subsequent levels of the explanatory variable (say managerial ownership). In this case, strict exogeneity would be violated because $E(\epsilon_{it} x_{i(t+1)}|\alpha_i) > 0$ higher value of this years performance are associated with higher levels of next year ownership.

To understand the resulting bias in the FE and FD estimator, consider the simple case in which we have two time periods (call them 1 and 2), perhaps several years apart, so that the FE and FD estimators are numerically identical and the resulting parameter estimate for β is derived from a simple linear regression of changes in y (in our example performance) on changes in x (in our example ownership). Suppose also that the true β is 0 in which case there is no causal effect of x on y. If we regress $(\Delta y = y_2 - y_1)$ on $(\Delta x = x_2 - x_1)$, the only systematic variation in the data will arise from high (low) y_1 values tending to feed back to high (low) x_2 values. This will result in an apparent negative correlation between Δy and Δx and will yield (asymptotically with probability 1) a spurious negative estimated coefficient. Extensions of this argument apply to upward and downward inconsistencies in parameter estimates that depend on the sign of the actual coefficient (when $\beta \neq 0$) and the sign of the dynamic feedback effect. Clearly multivariate models and multiple time periods make it more difficult to sign and understand the resulting bias.

Given the potential seriousness of this issue, it is not surprising that *Wooldridge* (2010) and other econometric treatments emphasize the importance of testing for strict exogeneity before relying of FE or FD estimation procedures. However, with the exception of models that include lagged dependent variables, and a couple of other rare exceptions discussed below, researchers in finance relying on FE or FD estimation never test for strict exogeneity. Unfortunately, we show that these tests will very commonly reject the strict exogeneity assumption, in which case the reported estimates will be inconsistent and should therefore be viewed with suspicion.

3.2.2 Recent/current practice

To characterize current practice, we search through every issue of the Journal of Finance, Journal of Financial Economics, and Review of Financial Studies from 2006 to 2013 for the mention of the word fixed effect (or a synonym). We quickly scan each flagged article to determine whether the paper features an empirical model with unit-level (e.g., firm, bank, person) fixed effects rather than solely time (e.g., year, quarter, etc.) effects. We placed each paper into non-mutually exclusive categories based on whether the authors report (a) traditional FE estimates, (b) traditional FD estimates, and/or (c) some version of a dynamic panel GMM estimate. We do not categorize models that rely on external instruments or natural experiments, as our focus is on evaluating models in which this type of external information is not exploited.

Our procedure flags 251 articles that report unit-level FE (222) and FD (47) estimates, and 17 report GMM estimates. If we exclude models that include lagged dependent variables, the corresponding numbers are 240, 216, 44, and 6 respectively. Clearly these figures indicate that FE is the most popular estimation procedure. In all of the papers that report solely FE estimates, only 3 mention the word strict or strong exogeneity, and of these only 1 actually test the strict exogeneity assumption. Clearly the field has either not widely recognized this issue, or perhaps the field is collectively hopeful that any resulting inconsistencies are not large enough in magnitude to substantively change the economic inferences of interest.

3.2.3 Prior work in finance that accounts for violations of strict exogeneity

As is well known, any panel data analysis that includes a lagged dependent variable as a control variable must violate the strict exogeneity assumption (i.e., there is no need to test for strict exogeneity, it is assumed to be violated in the underlying the model). The most prominent area in finance in which this is recognized is in the dynamic capital structure literature, as current leverage is usually assumed to be partially governed by past leverage. Since conventional FE and FD estimators are inconsistent in this context, a large literature has appeared using variants of the GMM approach and different identifying assumptions to estimate the parameters of interest, with much debate on the merits of different estimation approaches. We have little to offer here, except to note that testing the underlying assumptions in a GMM estimation is also called for whenever feasible. Recent evidence by *Dang and Shin* (2015) suggest that these assumptions are often rejected in the dynamic capital structure framework.

As we discuss earlier, the strict exogeneity issue is almost entirely unacknowledged in panel data finance models that do not include a lagged dependent variable. The one notable exception is the important work of *Wintoki and Netter* (2012). Those authors highlight the importance of the strict exogeneity issue in one specific setting, namely the effect of board structure on firm performance. They test for strict exogeneity in this context and reject the validity of this assumption, leading them to question prior work on this issue that relies on (inconsistent) FE or FD estimators. They proceed to use a GMM framework and show that the assumptions underlying the GMM estimation are not rejected in standard tests, although they do caution the reader with regard to testing power and other potential limitations.

Our paper is most similar to *Wintoki and Netter* (2012) who explore this issue in an important specific finance context. The distinguishing feature of our study is that we highlight that this issue applies to a large set of empirical models in finance and show that the strict exogeneity assumption is routinely rejected in finance panel data models even when there is no lagged dependent variable. Thus, the concern raised by *Wintoki and Netter* (2012) turns out to be only the tip in the iceberg. We also offer evidence on the potential magnitudes of the inconsistencies in FE and FD estimators when the strict exogeneity issue is ignored. Finally, we suggest an alternative systematic approach to external identification in large panels which has the potential in some cases to be more convincing than internal identification via GMM, and more widely applicable than the magic bullet strategy of hoping to find a unique economic, tax, legal, or regulatory event that perturbs the explanatory variable of interest.

3.3 Testing for strict exogeneity

3.3.1 Identifying a set of canonical regression

In order to explore these issues in a manageable set of well-known contexts, we identify a set of canonical panel-data regression models from the recent finance literature. To do this, we assign each fixed-effects-panel-data study flagged in our earlier literature search into one of a broad set of mutually exclusive categories based on the main dependent variable of interest. The six largest categories have dependent variables of the following type: (a) a firms investment level, (b) a firms leverage/capital structure, (c) a CEOs compensation level or ownership position, (d) a firms cash holdings, (e) a firms annual fundraising choice, and (f) a firms performance including stock returns, accounting returns, or sales growth. In total, 59% of the published panel studies with unit-level fixed effects in the elite set of three journals searched can be placed (using some subjective judgment) into one of these categories.

For each of these six dependent variable categories, we identify a small set of specific variables (both dependent and independent) that are used most frequently in the regression models identified in our literature search, subject to the constraint that the variables can be constructed from standard data sets. Our first choice is to construct variables and models that correspond to the choices by *Gormley and Matsa* (2014), as they offer some thoughtful off-the-shelf specifications that are also informed by the literature. For variables/models not included in their study, we use variable definitions that appear to be most common in the literature, with some subjective judgement on our part in grouping similar variables together. For all six dependent variable categories we identify one (or more) common dependent variable constructions. We then model these dependent variables as a function of all independent variables that (a) either appear in the corresponding *Gormley and Matsa* (2014) model, or (b) appear in at least 20% of all associated published studies flagged in our literature search.

For ease of exposition, we will refer to the selected dependent variables as Depvar1 through Depvar6. In some cases we use multiple constructs of a given type of dependent variable, for example both market and accounting measures of performance. In these cases we add letters to the end of the Depvar notation. Thus, for example, Deprvar1a and Depvar1b would refer to two different measures of investment spending (say capital spending/assets and R&D/assets).

Rather than discuss each of the independent variables in detail, we report in Table 1 a summary of the dependent variables and associated independent variables in models predicting each dependent variable. The actual construction procedure/technical definition of each of these variables is relegated to an online appendix. Our hope is that none of our choices are controversial. We are simply trying to collect and characterize a large and varied literature in a reasonable and succinct way. The number of independent variables varies depending on the dependent variable, with a maximum number of six. At times we refer to these as Indvar1 through Indvar8, where the mapping in Table 1 can be used to recover the actual economic variable in question.

3.3.2 Testing for strict exogeneity

For each dependent variable and a single associated explanatory variable, we conduct the strict exogeneity tests outlined by *Wooldridge* (2010), one based on the FE transformation and the other based on the FD transformation. Each test is for a model in which the dependent variable is a linear function of the unit-level (i.e., firm) fixed effect, year dummies, and the selected explanatory variable. We also report tests for a model in which all independent variables are included together. Test statistics are calculated with standard errors clustered at the unit-level to allow for arbitrary heteroskedasticity and serial correlation. These tests essentially include future values of the independent variable into the regression. Under strict exogeneity, the coefficients on these future values should be 0. Thus, evidence of a non-zero coefficient (or a set of one or more non-zero coefficients in the case of multiple explanatory variables) is taken as evidence against the strict exogeneity assumption.

The p-values for these tests using the entire universe of available Compustat data from 1950 to 2012 are reported in Table 2. The dependent variable for each model is listed in the left column, and the other column headings indicate the test that is conducted, with FE1 for example indicating the fixed effects version of the *Wooldridge* (2010) test for strict exogeneity in a model with Indvar1 as the sole independent variable (in addition to the year and firm effects). The joint test designations are for the pooled version of the FE and FD tests from models that include all of the explanatory variables together.

As the figures in table 2 indicate, the vast majority of the p-values are below .01, indicating that in most cases strict exogeneity can be rejected with a high degree of confidence. In fact, of the 86 tests conducted on individual explanatory variables, we can reject the strict exogeneity assumption at the 1% level in more than 3/4 of the reported models. Moreover, in the joint tests that include all explanatory variables, the strict exogeneity assumption is rejected at the 1% level in all but one model and at the 10% level in all models. Clearly this indicates that violations of strict exogeneity are extremely common, and the findings of **?** would appear to extend much more broadly to finance panel data studies. This is not surprising if one believes that financial choices/outcomes, performance, and incentives (the dependent variables) often have an effect on the future determinants of these choices (the explanatory variables), for example a firms asset, growth, or governance characteristics.

To further explore these results in more homogeneous settings, we break the sample into shorter time periods, or, alternatively, into industry-based subsamples. In particular, we first conduct the preceding analysis for the entire sample of firms restricted to 10 year sample sub-periods, and then we conduct an analysis for the sample of all years but restricted to industry-based subsamples based on a firms 1-digit SIC code. Since we now have multiple p-values on test statistics for each dependent variable-independent variable combination (one for each subsample), we tabulate the median p-value for the associate set of test statistics. These figures are reported in Table 3, with panel A reporting year subsample results and panel B reporting industry subsample results.

As we would expect given the smaller sample sizes involved in these test, the p-values in both panels of Table 3 are generally somewhat higher than the larger sample pooled tests in Table 2. However, it is quite remarkable that the median p-values are frequently below .10 and .05, suggesting that in more than half of all of these subsamples there is substantial evidence of a rejection of strict exogeneity. If we couple these observations with strong a priori theoretical reasons to believe that strict exogeneity will be violated, the general case for suspecting that most FE and FD estimates in these types of canonical models are inconsistent seems compelling.

3.3.3 Insights on magnitudes

The fact that FE and FD estimates will generally be inconsistent in many or most panel data finance settings is concerning. However, if the inconsistency is small, it is possible that conclusions regarding the magnitude of a coefficient of interest or a test of whether the coefficient is different from zero may be at least approximately valid. It is difficult to make more precise statements without specifying the possible dynamic structure of the underlying model. However, some potentially useful information can be inferred by comparing the FE and FD estimates, as large differences between these estimates would suggest a problem of substantial magnitude.

To investigate, we compare the magnitudes and signs of the corresponding FE and FD coefficient estimates for the models in Tables 2 and 3. We collect all of coefficient pairs and calculate the percentage of pairs in which the FE and FD coefficient estimates are of opposite sign, and also the percentage of cases in which both coefficients are significant at the 10% level or higher and are also of opposite sign. We also calculate the median ratio of the larger of the two coefficients in magnitude to the smaller coefficient in the subset of cases in which both coefficients have the same sign. These figures are reported in Table 4 (Panels A, B, and C). The final column of the table indicates the number of pairs used to make the calculations, which in all cases is equal to 32 x (# of independent variables), as there are 16 different samples/subsamples (all observations, 10 industry subsamples, 5 ten year subsamples) and 2 ways to estimate an FE or FD coefficient on a given explanatory variable (as the sole independent variable in the model or with all of the independent variables in the model).

The figures in column 1 indicate that FE and FD estimates are not infrequently of opposite sign. This is concerning, since we would expect two estimators that are cousins of one another and are applied to the same data to usually be of the same sign. As *Wooldridge* (2010) notes, substantial differences between FE and FD estimators are often an indication of a violation of strict exogeneity. It would be particularly concerning if these two estimators yield significant coefficients of opposite sign. If a given coefficient is 0, the likelihood of observing two coefficients that are significant at the 10% level and of opposite sign is .5% if we assume independence of the two estimators. In general we would expect the estimators to be positively correlated, and also the true coefficient will often not be 0. Both of these considerations will tend to lower the likelihood of observing this significant and of opposite sign phenomenon under standard distributional assumptions. The figures in Table 4, panels A and B, indicate that this behavior is not nearly as rare as would be expected, with rates in all cases far above the .5% threshold. We interpret this as additional evidence of potentially misleading inferences being drawn from FE and FD estimates, either because of a failure of strict exogeneity or other model misspecification.

If we restrict ourselves to cases in which the FE and FD estimates are at least of the same sign, we report in panel C that the median ratio of the larger magnitude estimate to the smaller magnitude estimate is frequently quite large. Pooling across all dependent variables, the median of these medians is 1.53, indicating that differences of magnitude on the order of 53% are quite common. This is of course after already excluding the substantial number of cases where the point estimates have opposite signs. Certainly this does not inspire confidence in the implicit assumptions underlying the FE/FD estimation approach in these canonical finance panel data models.

3.3.4 Hoping for a 1/T save

While the FE and FD estimators are both inconsistent, the degree of inconsistency of the FE estimator may be smaller in a long panel because the FE estimator uses differences of variables from their means while the FD estimator use differences from the adjacent periods. Intuition suggests that feedback effects will be more influential when directly comparing adjacent periods, and this notion is formally captured by the fact that the inconsistency of the FE estimator is on the order of 1/T while the inconsistency of the FD estimator is independent of T (see *Nickell* (1981)). Thus, one might hope that a long panel, when it is available, would render the FE coefficients to be relatively informative. For this 1/T result to be potentially useful, a firms fixed effect for the dependent variable of interest needs to be stable over an entire sample period. Unfortunately, given the occasional discrete changes that occur over time to a typical firms management, ownership, and asset base (via mergers/acquisitions/divestitures), the assumption of a stable unit-level fixed effect over a long sample period may not be valid.

To investigate, we calculate correlations in firms estimated fixed effect coefficients derived from models using different subperiods. In particular, for each dependent variable we estimate a FE model using the entire set of associated independent variables for non-overlapping 5-year and 10 year subperiods starting with the most recent observation year and rolling backwards. We then collect the estimated fixed effects and correlate these estimates for different lags.

In Table 5 we report the pairwise correlations of the fixed effects across 10-year periods for each of the canonical models. As the figure indicates, for all of the models there is strong evidence of a monotonic decline in correlation of the fixed effects estimates as the estimation time periods get further apart. The most distant lags (i.e. $\text{corr}(\alpha_1, \alpha_5)$) , most of the correlations are below .50. Certainly this preliminary evidence does not offer strong support for stability of the underlying unit-level fixed effects.

In Table 6 we present the same correlations, but for 5-year estimation periods. Again, as the time lag gets further these correlations drop fairly sharply, with correlations falling steadily as we move from adjacent periods to periods 15-30 years apart. In untabulated results, we find that these correlations die out further when we consider even more distant lags. To assure that these results are not specific to inclusion of all of the independent variables, we replicate the column 6 correlation using models that only include a single explanatory variable for each dependent variable. These figures, reported in table 7 tell the same basic story. As panels get longer, the implicit assumption of a constant unit-level fixed-effect becomes quite questionable. Thus, unfortunately, the 1/T save to the possible inconsistency of FE estimates in the presence of a violation of strict exogeneity may not be very useful.

3.3.5 Robustness and extensions

The models we present in the tables are intended to be boilerplate and uncontroversial. The basic point that emerges is that: (a) there are good theoretical reasons to expect that future values of many common finance independent variables are correlated with the dependent variable even after partialing out contemporaneous control variables, (b) formal tests confirm this with a very high degree of confidence, and (c) standard FE and FD estimates are inconsistent when this strict exogeneity assumption is violated, and (d) there are reasons to suspect that this inconsistency can at times be large in magnitude.

While the evidence we present seems quite strong, one may be concerned that some peculiarity in our modeling or sampling choices may be driving the results. To investigate, we have experimented with (a) a more aggressive winsorization at the 5% tails rather than 1%, (b) trimming (dropping) the 1% and 5% tail observations rather than winsorizing, and (c) completely eliminating all winsorization of the data.

We have also experimented with restricting the sample to the post 1970, post 1980, and post 1990 time periods, and we have also estimated the models on only the industries that were excluded from the initial sample (utilities and financials). Finally, in all cases in which there is an alternative popular definition/construction of an independent variable, we have substituted the most popular alternative in place of the variable that we use. In all cases the results with these model or sample alterations are substantively unchanged from what we report in Table 2. Thus, it seems that the evidence against strict exogeneity is quite robust.

The evidence against strict exogeneity seems so strong that one might question whether a test on a large sample would ever not reject. To investigate, we select as an independent variable a measure that is very hard to predict a firms stock return and as a dependent variable, a seemingly innocuous construct that is likely to depend on the independent variable and also to have a firm-specific component the ratio of a firms receivables to payables. If we conduct the Table 2 tests in a model using this dependent variable/independent variable combination, the p-value of the FE (FD) test for strict exogeneity is .77 (.91). Thus, it is not the case that the strict exogeneity test always rejects. It would thus appear that it rejects in most of the models we consider because of the economic feedback between all of the variables that enter many canonical finance panel data regressions. If this is the case, traditional FE and FD estimates are not consistent.

3.4 Using industry-year variation for identification in certain settings

If conventional FE and FD estimators are inconsistent in a given setting, a researcher left with a substantial challenge in their goal of identifying a parameter of interest. As discussed earlier, an internal identification approach using a variant of

GMM may be suitable in some, but certainly not all, settings. External identification exploiting exogenous changes in various economic parameters of interest is of course always desirable, but in many (perhaps most) settings, is not feasible.

In some cases, we suggest that time varying industry shocks may be suitable as instruments for firm-level innovations in the explanatory variable of interest. It will almost always be the case that industry innovations in a variable of interest will be correlated with firm-level innovations, so we suspect the relevancy condition using this strategy will generally be easy to establish. If industry shocks capture inputs into firm shocks that are not driven by firm decisions, the exclusionary restriction will also in many cases be defensible. In measuring industry shocks, it will be important to exclude the firm in any calculation so as to purge any endogeneity arising from purely firm-level variation. In addition, if a firm is large or an industry is concentrated, the exclusionary restriction will be less likely to hold, as there may be contemporaneous feedback from a firms choices to other industry participants.

We explore the potential usefulness of this approach for understanding the relation between a firms risk and inside ownership. Since ownership can affect either current or future risk taking, both the contemporaneous and strict exogeneity assumptions are concerning in this context. In untabulated results we have tested the strict exogeneity assumption using the tests we recommend above, and the assumption is soundly rejected. If we regress firm risk on industry measures of risk (along with year dummies and firm fixed effects), we detect the expected significant positive relation between industry risk and firm risk indicating that the relevancy condition is satisfied. Since industry-risk should be unrelated to a firms ownership or individual risk taking decisions, at least in cases in which the industry is not concentrated, the exclusionary restriction would also seem to be quite reasonable in this setting. When we undertake a panel **2SLS!** (**2SLS!**) analysis using industry risk as an instrument for firm risk, we detect a positive relation. If we exclude 4-digit industries with a Hefindahl index in the top quartile, or alternatively firms with sales that are more than 10% of the 4-digit industry total, these results are substantively unchanged. While this initial evidence is preliminary in nature, it does suggest substantial promise for this general approach to identification in some panel finance settings.

3.5 Conclusion

Several recent articles in the finance literature have investigated issues related to properly constructing conventional panel-data estimators and their associated standard errors given the types of panel databases that are commonly used in finance. In this study we ask the preliminary question of whether these conventional estimators (i.e., fixed effects (FE) or first difference (FD) estimates) are likely to be informative in the sense that they will yield consistent estimates. We highlight the fact that consistency of FE and FD estimates relies on a strict exogeneity assumption that is both much stronger than the typical notion of contemporaneous exogeneity and is also testable.

We proceed to conduct these tests in a set of standard panel-data finance models identified from the recent literature to. Perhaps not surprisingly given various dynamic theories of financial choices, we show that strict exogeneity can be rejected in essentially all of these models. In our view this evidence indicates that conventional FE and FD estimates, which we show are quite common in finance research are, in most cases, inconsistent estimators of the parameter of interest. At the very least, our evidence suggests that researchers should address the strict exogeneity assumption from a theoretical perspective, and also test this assumption, before they even consider using conventional estimators.

In an effort to gauge how misleading conventional panel data estimators may be in typical finance research settings, we examine differences between FE and FD estimates derived for the same model over the same sample. We show that these estimates frequently diverge substantially, suggesting that the magnitudes of inconsistencies arising from these estimation procedures can be, at least in some cases, substantial. We also caution that reliance on a long panel to minimize the inconsistency in the FE estimator may not be particularly useful, as unit level fixed-effects in typical finance research settings do not appear to be stable over long sample periods.

Our results are challenging as they suggest that finance researchers frequently must turn to less conventional approaches to estimate parameters of interest. GMM estimators, which have been increasing in popularity, are one such approach. However, in cases in which (a) theory suggests that the assumptions underlying GMM are violated, (b) the testable assumptions underlying GMM are rejected, or (c) the finite sample properties of GMM are poorly behaved, this is unlikely to be a suitable alternative.

Given these limitations to GMM, we suggest the possibility that in some settings the presence of industry-year shocks to explanatory variable may be a useful identification strategy. In particular, if there is an time-varying industry component to an explanatory variable of interest that satisfies the exclusionary restriction conditional on unit-level fixed effects and sample-wide year effects, this variation could be quite informative and could lead to a systematic approach to (quasi) external identification. We demonstrate the potential usefulness of this approach by using industry innovations in risk as an instrument for firm risk in an examination of the effect of risk on inside ownership. In this specific context our identification approach leads to fairly precise estimates that differ substantively from nave estimates that ignore endogeneity in the panel. While exploratory in nature, this preliminary evidence appears quite promising in terms of recommending this as a strategy to be considered in other contexts.

APPENDIX

APPENDIX

Table A.1 Summary Statistics

This table provides summary statistics for both CDS firms (Panel A) and non-CDS firms (Panel B). *leverage* is book leverage defined as long term debt plus short term debt over total book assets (dltt + dlc)/at, *mktlev* is market leverage defined as long term debt plus short term debt divided by total assets less common equity plus market value of common stock plus deffered taxes (dltt+dlc)/(at-ceq+mktcap+txditc), *MTB* is market to book ratio defined as total assets plus market value of common equity minus book equity all over total assets (at+mktcap-ceq)/at, *FixedAssets* is defines as gross property plant and equipment scaled by total assets (ppegt/at), *profit* is defined as operating income divided by lagged assets (oibdp/L.at), *lnsales* is the natural log of sales log(sale + 1), *lnsize* is the natural log of the market value of common stock, *hvol* is historical volatility (5yr) from OptionMetrics, *mtaxrate* is the marginal tax rate before interest deductions, *R&D* is research and development expenses scaled by total assets (xrd/at), *capex* is capital expenditures scaled by total assets (capx/at), WW is the Whited and Wu (2006) financial constraint index. All variables are winsorized at the 1% level.

	CDS FIRMS					NON CDS FIRMS				
	mean	sd	min	p50	max	mean	sd	min	p50	max
leverage	0.30	0.16	0.00	0.29	0.76	0.16	0.19	0.00	0.09	0.76
mktlev	0.22	0.14	0.00	0.19	0.65	0.11	0.14	0.00	0.05	0.65
MTB	1.67	0.89	0.55	1.39	8.39	2.16	1.48	0.55	1.66	8.39
FixedAssets	0.64	0.40	0.01	0.62	2.84	0.45	0.40	0.00	0.32	6.24
profit	0.15	0.09	-0.34	0.14	0.50	0.08	0.23	-1.00	0.12	0.50
size	8.35	1.14	0.00	8.32	10.39	5.91	1.80	0.00	6.17	10.39
lnsize	8.37	1.32	2.29	8.31	10.71	6.48	1.29	1.38	6.48	10.71
mtaxrate	0.33	0.05	0.00	0.34	0.38	0.27	0.11	0.00	0.33	0.39
hvol	0.43	0.18	0.14	0.40	1.61	0.63	0.24	0.15	0.59	3.83
RND	0.02	0.05	0.00	0.00	0.68	0.08	0.13	0.00	0.02	0.68
INV	0.06	0.06	0.00	0.04	0.41	0.06	0.07	0.00	0.03	0.41
WW	-0.36	0.07	-0.48	-0.36	-0.00	-0.23	0.09	-0.48	-0.23	0.08
	N = 4,496					N = 11,505				

Table A.2 Credit Default Swaps and Innovation

This table presents OLS estimates for the effects of credit default swaps on innovation input (R&D/sales) and innovation output (patent variables). Variable definitions are listed in the appendix and described in the empirical motivation section. The sample includes years 2001-2005 in columns 1-3, and includes years 2001-2012 in columns 4-5. Columns 1-3 only include firms that patent at least once from 2001-2005 and columns 4-5 only include firms that have positive R&D expenses in at least one year from 2001-2012. Columns 1, 2 and 4 include Fama-French 48 industry fixed effects and columns 3 and 5 include firm fixed effects. All columns include time dummies. Standard errors are clustered at the firm level in all columns.

	npatents	Patents/emp	Patents/emp	R&D/sales	R&D/sales
TRADING	2.1019	1.4212**	2.2185***	0.2494***	0.1316***
	(1.33)	(2.34)	(4.18)	(5.45)	(2.95)
TRADED	1.2975	0.0114		0.2544***	
	(1.10)	(0.02)		(4.68)	
size	2.2765***	-1.0815***	-2.7146*	-0.4274***	-1.2127***
	(8.78)	(-3.47)	(-1.88)	(-10.90)	(-8.68)
rated	0.3846	0.4057	1.3119	0.3489***	0.3735***
	(0.52)	(0.81)	(1.06)	(6.68)	(4.12)
impvol	6.8358***	3.1008**	7.2214	-0.8689***	-0.8911***
	5.47	(2.43)	(1.24)	(-6.61)	(-4.53)
DE	-0.9529***	-0.9083***	-1.2104***	-0.0383**	-0.0067
	(-3.08)	(-4.47)	(-3.51)	(-2.34)	(-0.29)
MTB	0.8056***	0.5169**	-0.0339	0.0780***	0.0082
	(4.71)	(2.29)	(-0.08)	(3.06)	(0.28)
lnkl	0.8082***	2.1784***	2.9316**	0.0236	0.1033
	(3.24)	(6.17)	(2.31)	(1.17)	(1.37)
RDS	0.1944**	0.4497	-1.2667*		
	(1.99)	(1.65)	(-1.74)		
CF	-2.5768***	-2.8789	-0.1632	-2.8287***	-1.0330***
	(-2.91)	(-1.49)	(-0.05)	(-12.16)	(-4.94)
Fixed Effects	Industry	Industry	Firm	Industry	Firm
Year FE	YES	YES	YES	YES	YES
nobs	5153	5153	5153	16151	16151
R2	0.2725	0.1830	0.7922	0.4053	0.8225

t statistics in parentheses $p < .01$, * $p < .10$, ** $p < .05$, ***

Table A.3 Unsecured and Secured Loans: Multinomial Logit Estimates

This table presents multinomial logit estimates (columns 1-2) for the effect of CDS trading on the use of secured and unsecured debt with no-loan as the base case. Column 3 presents the estimates from a binomial logit regression where the dependent variable is a dummy variable equal to one if the firm obtained a new loan, secured or unsecured, and zero if the firm did not initiate a new loan. All columns include year dummies and Fama-French 48 industry fixed effects. Control variables include total leverage, market to book ratio, size, a dummy for whether the firm is rated, research and development spending scaled by sales, Cash flow scaled by assets, implied volatility, and debt rating category dummy. All controls except for the rating dummies are lagged one period. standard errors are calculated using the Huber/White method.

	Multinomial Logit		Binary Logit
	Secured	Unsecured	Loan
NOT TRADING	.1344***	.3191***	.4528***
	(37.42)	(70.13)	(96.39)
TRADING	.1092***	0.4099***	.5291***
	(14.23)	(34.00)	(37.21)
Difference	-.0251***	.0908***	.0762***
	(-2.58)	(6.15)	(4.33)
Fixed Effects	Industry	Industry	Industry
Year FE	YES	YES	YES
nobs	16151	16151	16151

Table A.4 CDS and Secured vs. Unsecured Financing

This table presents a more detailed breakdown of firms' use of secured vs. unsecured financing for firm-years with and without an actively trading CDS. Firms are sorted into quintiles based on the variable *tangibility* in panel A, *MTB* in panel B, and *R&D* in panel C. Sorts are based on the full sample of firms, but this table only includes firm-years in which a new loan facility was originated from 2001-2012.

	CDS Not Trading				CDS Trading			
Panel A: Tangibility Quantiles								
Quantile	Unsecured		Secured		Unsecured		Secured	
Low	125	17.63%	584	82.37%	71	63.96%	40	36.04%
2	190	21.25%	704	78.75%	116	69.46%	51	30.54%
3	218	22.78%	739	77.22%	127	64.80%	69	35.20%
4	271	27.26%	723	72.74%	196	57.82%	143	42.18%
High	321	28.08%	822	71.92%	261	59.32%	179	40.68%
Panel B: MTB Quantiles								
Quantile	Unsecured		Secured		Unsecured		Secured	
Low	124	12.06%	904	87.94%	113	48.09%	122	51.91%
2	251	22.21%	879	77.79%	210	53.71%	181	46.29%
3	268	25.35%	789	74.65%	198	67.81%	94	32.19%
4	279	29.49%	667	70.51%	174	71.90%	68	28.10%
High	203	37.87%	333	62.13%	76	81.72%	17	18.28%
Panel C: R&D Quantiles								
Quantile	Unsecured		Secured		Unsecured		Secured	
Low	427	22.72%	1452	77.28%	327	62.52%	196	37.48%
2	139	25.50%	406	74.50%	99	53.23%	87	46.77%
3	284	27.00%	768	73.00%	200	60.98%	128	39.02%
4	200	25.94%	571	74.06%	125	70.22%	53	29.78%
High	75	16.67%	375	83.33%	28	73.68%	10	26.31%
Total	1,125	23.95%	3,572	76.05%	771	61.53%	482	38.47%

Full Sample

	CDS Not Trading				CDS Trading	
	All		Traded			
Unsecured	1,125	23.95%	160	34.33%	771	61.53%
Secured	3,572	76.05%	306	65.67%	482	38.47%
Total	4,697		466		1,253	

Table A.5 CDS and Unsecured Financing: Linear Probability Estimates

This table presents linear probability model estimates for the breakdown of firms' access to unsecured financing for firm-years with and without an actively trading CDS. The dependent variable, *unsecured*, is a dummy variable equal to one for firms that originated an unsecured loan facility according to Dealscan from 2001-2012. Patentfirm (R&Dfirm) is a dummy equal to for firm years with patenting activity (R&D spending) that is greater than the median firm for that year. The third column is limited by the availability of patent data which stops in 2006 and is truncated to 2005. Control variables (defined on page one of the results section) are CF, MTB, Profit, size, rated, and impvol. Industry, year, and rating category dummies are also included. *t-statistics* are computed using Huber-White standard errors

	unsecured	unsecured	unsecured	unsecured
TRADING	0.1944***	0.1422***	0.1659***	0.2010***
	(4.49)	(3.40)	(4.78)	(3.38)
TRADED	0.3286***	0.0697***	0.0603**	0.0048
	(7.94)	(4.09)	(2.14)	(0.18)
patentfirm			0.0104	
			(0.63)	
TRADED×patentfirm			-0.1584***	
			(-3.41)	
TRADING×patentfirm			0.0844**	
			(2.33)	
R&Dfirm				0.0120
				(0.65)
TRADED×R&Dfirm				-0.1539***
				(-3.41)
TRADING×R&Dfirm				0.0909**
				(2.11)
N	16151	16151	3842	16151
R^2	0.161	0.287	0.339	0.263
Fixed Effects	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES
Rating Effects	NO	YES	YES	YES
Controls	NO	YES	YES	YES

t statistics in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A.6 Difference in Differences for 2003 ISDA provisions

Panel A presents the results of a DID analysis using the 2003 ISDA provisions as a treatment effect. The sample includes years 2001-2005. The treatment group includes all firms that had a CDS *begin* trading during 2003. *TREATED* is a dummy variable equal to 1 if a firm belongs to this group. If a firm had a CDS start trading after 2005 or is a non-CDS firm, then the firm is dropped from the sample in Panel A. The control group includes firms that had a CDS trading prior to 2003. *PT* is a dummy variable equal to 1 during 2003-2005. *TRADING* is a dummy = 1 in firm-years for which a CDS is actively trading for firms in the treatment group. For the control group however, this variable is artificially set to 0. In this analysis *TRADING* is equivalent to interaction term of interest (posttreatment×treatment). Panel B presents results from a falsification test in which all firms with a CDS trading prior to 2005 are dropped and all firms with a CDS that started trading after 2005 are used as the treatment group (TRADED) and the control group becomes all non-CDS firms. The interaction effect (TRADING) is artificially set to 1 during 2003-2005 for this group. All specifications include controls, industry fixed effects, and standard errors clustered at the firm level.

Specification for columns 1-3:

$$y_{i,t} = \alpha_{ind} + \gamma_1 pt + \gamma_2 treated + \gamma_3 pt \times treated + \beta_1 Q_{i,t-1} + \beta_2 lnsize_{i,t-1} \\ +\beta_3 ROA_{i,t-1} + \beta_4 Profit_{i,t-1} + \beta_5 CF_{i,t-1} + \beta_6 rated_{i,t} + \beta_8 hvol_{i,t-1} + \varepsilon_{i,t}$$

Specification for columns 4-5:

$$y_{i,t} = \alpha_{ind} + \gamma_1 pt + \gamma_2 treated + \gamma_3 pt \times treated + \beta_1 Q_{i,t} + \beta_2 lnsize_{i,t} + \beta_3 ROA_{i,t} \\ +\beta_4 Profit_{i,t} + \beta_5 CF_{i,t} + \beta_6 rated_{i,t} + \beta_7 mtaxrate_{i,t-1} + \beta_8 hvol_{i,t-1} + \varepsilon_{i,t}$$

Panel A: Difference in Differences

	npatents	pat/emp	R&D/sale	Leverage	Unsecured
TRADING	2.6364**	0.5848*	0.05598*	0.0302***	0.047**
(PT×TREATED)	(2.09)	(1.66)	(1.76)	(3.12)	(2.04)
Post ISDA Revision	-6.64320***	-0.0262	-0.038279**	-0.0183***	0.00333
(PT)	(-3.62)	(-0.72)	(-2.07)	(-2.9)	(1.01)
TREATED	-4.902***	0.08313	-0.1185*	0.02221**	-0.00942
	(-3.71)	(0.25)	(-1.88)	(2.05)	(-0.65)
N	1827	1827	2373	2373	2373

Panel B: Falsification Test

	npatents	pat/emp	R&D/sale	Leverage	Unsecured
TRADING	0.817	-0.05746	0.0042	0.0039	0.0057
(PT×TREATED)	(0.99)	(-0.22)	(1.21)	(1.01)	(1.39)
Post ISDA Revision	-2.64320**	-0.589**	-0.0149***	0.0005	-0.00798
(PT)	(-2.12)	(2.36)	(-2.65)	(1.43)	(-1.19)
TREATED	-1.902*	-0.0262	-0.0222**	0.0146***	0.0003
	(-1.91)	(-0.06)	(-2.08)	(2.81)	(0.59)
N	3427	3427	3688	3688	3688

Table A.7 Impact of CDS on Unsecured Debt: Parameter Estimates

This table presents probit, logit, and linear probability (LP) estimates for the impact of credit default swaps on access to unsecured financing. The sample consists of loan-level observations for firm-years in which a new loan was originated according to Dealscan. Independent variables are defined in table 1 and in the appendix. The independent variable is a dummy variable equal to one if a firm obtains a new, unsecured loan in a given year. The unconditional probability of obtaining unsecured financing in the sample is .37.

	Probit	Logit	LP	Probit	Logit	LP
TRADING	0.1792***	0.2907***	0.0325**	0.2308***	0.4009***	0.0433*
	(2.71)	(2.52)	(2.03)	(3.28)	(3.28)	(1.81)
MTB	0.1440***	0.2388***	0.0414***	0.0129	0.0001	0.0045
	(5.85)	(5.55)	(4.39)	(0.27)	(0.00)	(0.29)
FixedAssets	0.1586***	0.2380**	0.0263	-0.0810	-0.1643	-0.0341
	(2.44)	(2.09)	(1.05)	(-0.84)	(-0.99)	(-0.73)
profit	0.4537*	0.9278**	0.0784	0.7067*	1.3707*	0.1765
	(1.96)	(2.21)	(1.00)	(1.79)	(1.95)	(1.41)
size	0.0464**	0.0765**	0.0128	-0.1198***	-0.2017***	-0.0279
	(2.26)	(2.13)	(1.03)	(-3.81)	(-3.70)	(-1.58)
rated	-0.6394	-1.2909	0.9113***	-0.8794*	-1.7335	0.6610***
	(-1.36)	(-1.25)	(27.44)	(-1.66)	(-1.54)	(8.31)
mtaxrate	2.3382***	4.5415***	0.3438***	-0.7156	-1.0644	-0.1162
	(4.31)	(4.29)	(2.72)	(-0.77)	(-0.64)	(-0.37)
hvol	-1.4610***	-2.6135***	-0.2221***	-0.6437***	-1.1676***	-0.0672***
	(-9.18)	(-9.20)	(-6.40)	(-2.60)	(-2.59)	(-2.29)
TRADED	-0.1621**	-0.2693**	-0.0321*			
	(-2.23)	(-2.10)	(-1.72)			
nobs	7792	7792	7822	3798	3798	3861
R2/PR2	0.2042	0.2050	0.1925	0.1997	0.2007	0.2052
Industry FE	YES	YES	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster	NO	NO	YES	NO	NO	YES
Sample	FULL	FULL	FULL	TRADED	TRADED	TRADED

t statistics in parentheses

All variables winsorized at the 1% level

Table A.8 Impact of CDS on Unsecured Debt: Marginal Effects

This table presents probit, logit, and linear probability (LP) *average marginal effects* estimates for the impact of credit default swaps on access to unsecured financing estimated in the previous table. Independent variables are defined in table 1 and in the appendix. The *dependent* variable is a dummy variable equal to one if a firm obtains a new, unsecured loan in a given year. The sample in this table only includes firm-years in which a new loans have been originated according to Dealscan. The unconditional probability of a loan being unsecured in this sample is .37.

	Probit	Logit	LP	Probit	Logit	LP
TRADING	0.0441***	0.0415***	0.0325**	0.0611***	0.0626***	0.0433*
	(2.66)	(2.48)	(2.03)	(3.36)	(3.38)	(1.81)
MTB	0.0347***	0.0335***	0.0414***	0.00349	0.0000230	0.0045
	(5.89)	(5.59)	(4.39)	(0.27)	(0.00)	(0.29)
FixedAssets	0.0382***	0.0333**	0.0263	-0.0219	-0.0262	-0.0341
	(2.44)	(2.08)	(1.05)	(-0.84)	(-1.00)	(-0.73)
profit	0.109*	0.130**	0.0784	0.191	0.219*	0.1765
	(1.96)	(2.21)	(1.00)	(1.79)	(1.96)	(1.41)
size	0.0112**	0.0107*	0.0128	-0.0324***	-0.0322***	-0.0279
	(2.26)	(2.13)	(1.03)	(-3.83)	(-3.72)	(-1.58)
rated	-0.154	-0.181	0.9113***	-0.237	-0.277	0.6610***
	(-1.36)	(-1.25)	(7.44)	(-1.66)	(-1.55)	(8.31)
mtaxrate	0.564***	0.636***	0.3438***	-0.193	-0.170	-0.1162
	(4.33)	(4.30)	(2.72)	(-0.77)	(-0.64)	(-0.37)
hvol	-0.352***	-0.366***	-0.2221***	-0.174***	-0.186***	-0.0672**
	(-9.29)	(-9.31)	(-6.40)	(-2.60)	(-2.60)	(-2.29)
TRADED	-0.0391**	-0.0377**	-0.0321*			
	(-2.23)	(-2.11)	(-1.72)			
N	7792	7792	7822	3798	3798	3861
R2/PR2	0.2042	0.2050	0.1925	0.1997	0.2007	0.2052
Industry FE	YES	YES	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster	NO	NO	YES	NO	NO	YES
Sample	FULL	FULL	FULL	TRADED	TRADED	TRADED

t statistics in parentheses

All variables winsorized at the 1% level

Table A.9 Unsecured Financing and Firm Tangibility

This table presents estimates of the impact of credit default swaps on access to unsecured financing as it relates to firm tangibility. *tang* is a dummy variable = 1 (0) if a firm is in the top (bottom) 2 quintiles of firms ranked on tangibility in the full sample. Similarly, *intang* is a dummy = 1 if a firm is in the top 2 quintiles of firms ranked on intangibility (measured as R&D + advertising expense scaled by total assets). All other variables are as defined in table 1. The first (second) column contains the estimates of a linear probability model for only those firm years in which tang == 0 (1). Column three contains estimates for all firms. Similarly, The fourth (fifth) column contains the estimates of a linear probability model for only those firm years in which intang == 0 (1). Finally, column six also contains estimates for all firms.

	Full Sample			TRADED sample		
	tang=0	tang=1	interaction	intang=0	intang=1	interaction
TRADING	0.1092***	0.0850**	0.1607***	0.0833***	0.1101***	0.0783***
	(4.33)	(2.13)	(4.57)	(3.31)	(3.01)	(5.05)
tang			0.0347*			
			(1.70)			
TRADING ×tang			-0.1036**			
			(-2.41)			
intang						0.0092
						(0.93)
TRADING ×intang						0.0561***
						(2.90)
MTB	0.0431***	0.0396***	0.0497***	0.0729***	0.0283***	0.0508***
	(2.42)	(3.15)	(5.10)	(4.86)	(2.51)	(5.18)
profit	0.1836***	-0.0543	0.0601	0.0101	0.1533	0.0178
	(2.49)	(-0.45)	(0.63)	(0.10)	(1.39)	(0.70)
size	0.0042	0.0593***	0.0285	0.0228	0.0274	0.0392***
	(0.25)	(3.57)	(1.54)	(1.28)	(1.65)	(6.04)
rated	-0.0057	-0.1394***	-0.0636	-0.0393	-0.1113***	-0.0647***
	(-0.13)	(-6.00)	(-1.67)	(-0.89)	(-3.77)	(-4.02)
mtaxrate	0.2382*	0.0600	0.1129	0.4033***	-0.0894	0.4594***
	(1.90)	(0.35)	(0.79)	(2.79)	(-0.38)	(5.71)
hvol	-0.4826***	-0.3293***	-0.4336***	-0.4134***	-0.4666***	-0.4564***
	(-12.10)	(-4.20)	(-8.42)	(-8.55)	(-5.61)	(-8.24)
TRADED	-0.0665***	0.0426	-0.0281	-0.0376**	0.0031	-0.0048
	(-2.53)	(1.24)	(-1.18)	(-1.98)	(0.06)	(-0.34)
nobs	3995	2468	7822	4205	1648	7822
R2	0.1214	0.1664	0.1240	0.1258	0.1486	0.1168
Fixed Effects	Industry	Industry	Industry	Industry	Industry	Industry
Year	YES	YES	YES	YES	YES	YES
Cluster	YES	YES	YES	YES	YES	YES
Sample	FULL	FULL	FULL	FULL	FULL	FULL

Table A.10 Lender Size and the Impact of CDS on Access to Capital

This table presents the results of a difference-in-differences-in-differences analysis of the impact of a CDS on leverage as it relates to lender size. The dependent variable in the regressions is book leverage. *biglender* is a dummy variable = 1 if a firm has a bank that is one of the 15 largest capital providers as a syndicate member on a loan. *biglender* = 0 if all of a firm's syndicate members lie oustside the the 15 largest capital providers. All other variables are defined in table 1. The first column presents regression results obtained only on the set of firms without a biglender on its loan facility. Columns three and four present results from the full set of firms with the interaction term.

	biglender=0	biglender=1	All Firms	All Firms
TRADING	0.0821**	0.0344***	0.0481***	0.0252***
	(2.12)	(3.61)	(4.32)	(3.63)
biglender			0.0499***	0.0171***
			(3.75)	(2.91)
TRADING×biglender			-0.0186***	-0.0062
			(-2.47)	(-1.00)
MTB	-0.0331	-0.0356***	-0.0318***	0.0018
	(-1.14)	(-3.90)	(-3.40)	(0.31)
FixedAssets	0.1297**	0.0365	0.0368	-0.0056
	(2.34)	(1.65)	(1.62)	(-0.23)
profit	-0.1129	-0.0012	-0.0143	-0.0180
	(-0.31)	(-0.01)	(-0.14)	(-0.54)
lnsize	-0.0613***	-0.0477***	-0.0491***	-0.0474***
	(-4.75)	(-6.96)	(-7.36)	(-8.15)
SPrating	-0.0010	-0.0003	0.0001	-0.0054***
	(-0.23)	(-0.20)	(0.07)	(-5.87)
mtaxrate	-0.1376	-0.1063	-0.0990	-0.1518**
	(-0.24)	(-0.89)	(-0.81)	(-2.06)
nobs	203	6983	7186	7186
R2	0.6310	0.3354	0.3505	0.8281
FE	Industry	Industry	Industry	Firm
Year FE	YES	YES	YES	YES
Cluster	YES	YES	YES	YES
Sample	TRADED	TRADED	TRADED	TRADED

t statistics in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A.11 Summary Statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) p10	(5) p25	(6) p50	(7) p75	(8) p90
adj_patent	19,882	2.039	5.834	0	0	0.215	0.974	4.738
adj_cites	19,882	35.01	108.9	0	0	2.443	15.95	74.74
lnSize	19,875	5.693	2.140	3.011	4.165	5.585	7.101	8.524
MTB	19,882	2.289	2.111	0.942	1.108	1.543	2.522	4.430
RDS	19,882	0.447	1.833	0.00803	0.0205	0.0583	0.147	0.441
SA	19,882	-1.31e-07	1.000	-1.430	-0.658	0.0438	0.693	1.227
WW	19,882	-2.16e-08	1.000	-1.374	-0.736	0.0477	0.733	1.247
SA_cited	19,882	4.30e-08	1.000	-1.295	-0.690	0.0115	0.672	1.269
WW_cited	19,882	1.17e-07	1.000	-1.239	-0.674	-0.0317	0.621	1.285
RDS_cited	19,882	0.134	0.295	0.0333	0.0439	0.0636	0.0981	0.187
MTB_cited	19,882	1.929	0.948	1.226	1.425	1.759	2.210	2.838
tangibility	19,882	0.484	0.211	0.189	0.339	0.499	0.630	0.727
cashholdings	19,882	0.215	0.224	0.0130	0.0385	0.128	0.328	0.570
profit	19,882	0.0712	0.283	-0.194	0.0300	0.125	0.198	0.280

Table A.12 Financial Constraints OLS Regressions

	WW		SA	
Panel A				
	patents	*cites*	*patents*	*cites*
FC_ cited	0.593***	13.27***	0.749***	15.54***
	(0.0684)	(2.002)	(0.101)	(3.077)
MTB	0.0452***	1.247***	0.0639***	1.486***
	(0.0132)	(0.308)	(0.0135)	(0.311)
RDS	0.0341***	0.676***	-0.0101	-0.124
	(0.0112)	(0.237)	(0.00668)	(0.147)
FC	-0.623***	-11.30***	-0.906***	-12.70***
	(0.0777)	(1.744)	(0.201)	(4.85)
MTB_cited	0.0379	0.948	0.0435	1.098
	(0.0426)	(0.908)	(0.0438)	(0.941)
RDS_cited	0.431**	9.997***	0.404**	9.880***
	(0.175)	(3.244)	(0.175)	(3.211)
profit	-0.0759	-1.099	-0.037	0.156
	(0.0886)	(1.999)	(0.091)	(2.078)
cashholdings	-0.338**	-10.79***	-0.312*	-11.28***
	(0.167)	(3.628)	(0.166)	(3.395)
Observations	19882	19882	19882	19882
R-squared	0.852	0.789	0.852	0.789
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Panel B				
	WW		SA	
	patents	*cites*	*patents*	*cites*
FC_ cited	0.568***	12.71***	0.708***	15.09***
	(0.0675)	(1.926)	(0.0982)	(2.978)
MTB	0.0372***	1.135***	0.0520***	1.348***
	(0.0126)	(0.307)	(0.013)	(0.314)
RDS	0.0305***	0.607**	-0.00894	-0.131
	(0.0114)	(0.249)	(0.00754)	(0.174)
FC	-0.551***	-10.33***	-0.692***	-10.54**
	(0.071)	(1.695)	(0.208)	(5.213)
MTB_cited	0.0297	1.361	0.035	1.494
	(0.0399)	(0.994)	(0.0412)	(1.035)
RDS_cited	0.385**	10.03***	0.370**	9.963***
	(0.151)	(3.057)	(0.152)	(3.009)
profit	-0.0456	-0.191	0.00739	1.236
	(0.0829)	(1.882)	(0.0883)	(2.097)
cashholdings	-0.430***	-12.64***	-0.439***	-13.50***
	(0.163)	(3.452)	(0.168)	(3.467)
Observations	19882	19882	19882	19882
R-squared	0.86	0.797	0.86	0.796
Firm FE	YES	YES	YES	YES
Ind x Year FE	YES	YES	YES	YES

Table A.13 AJCA Tax Holiday

	WW		SA	
	cites	*patents*	*cites*	*patents*
Treated x Post	-0.670***	-11.55***	-0.656***	-11.39***
	(0.148)	(3.432)	(0.142)	(3.419)
FC	0.0517	1.458**	-0.817***	-9.141**
	(0.034)	(0.665)	-0.269	-4.07
MTB	0.0324**	0.641*	0.0442**	0.764**
	(0.0145)	(0.327)	(0.0173)	(0.338)
RDS	-0.0207**	-0.385**	-0.0208**	-0.339*
	(0.00858)	(0.19)	(0.00969)	(0.174)
MTB_cited	-0.436*	-11.52*	-0.402*	-11.13*
	(0.238)	(5.846)	(0.236)	(5.909)
RDS_cited	0.273	6.371	0.27	6.309
	(0.215)	(4.931)	(0.219)	(5.011)
profit	0.0488	-1.142	-0.0908	-2.841
	(0.124)	(3.111)	(0.113)	(2.801)
cashholdings	-0.518*	-11.96	-0.329	-9.56
	(0.269)	(7.703)	(0.275)	(8.017)
tangibility	-0.622	-15.22	-0.222	-10.08
	(0.48)	(13.45)	(0.485)	(14.06)
Observations	5047	5047	5047	5047
R-squared	0.673	0.505	0.674	0.506
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A.14 AJCA 2

	WW		SA	
	patents	*cites*	*patents*	*cites*
FC_cited	-0.0000593	-0.486	-0.081	-3.190*
	(0.0664)	(1.283)	(0.0734)	(1.621)
FC_cited $\times \mathbb{1}\,[Post]$	-0.129***	-1.687**	-0.222***	-3.283***
	(0.0445)	(0.745)	(0.0614)	(1.154)
FC_cited $\times \mathbb{1}\,[Treated]$	0.346**	8.917**	0.0865	2.502
	(0.169)	(3.703)	(0.127)	(2.329)
FC_cited $\times \mathbb{1}\,[Treated \times Post]$	-1.005***	-19.34***	-0.728***	-14.87***
	(0.274)	(6.968)	(0.196)	(5.362)
$\mathbb{1}[Treated \times Post]$	-0.772***	-13.43***	-0.883***	-16.07***
	(0.191)	(4.332)	(0.245)	(5.591)
MTB	0.0106	0.248	0.0305	0.493*
	(0.0166)	(0.247)	(0.0185)	(0.262)
RDS	-0.00158	-0.0252	-0.0146	-0.213
	(0.00882)	(0.174)	(0.00963)	(0.166)
FC	0.0357	1.204**	-0.692***	-6.941*
	(0.0296)	(0.559)	(0.236)	(3.768)
MTB_cited	-0.193	-7.091	-0.266	-8.752
	(0.181)	(4.849)	(0.187)	(5.23)
RDS_cited	0.115	3.003	0.173	4.671
	(0.173)	(3.272)	(0.176)	(3.562)
profit	0.0551	-1.141	-0.0595	-2.311
	(0.104)	(2.61)	(0.0968)	(2.295)
cashholdings	-0.588*	-13.27	-0.531*	-13.37
	(0.303)	(8.547)	(0.274)	(8.54)
tangibility	-0.34	-10.18	0.0418	-5.002
	(0.44)	(12.38)	(0.408)	(11.94)
Observations	5047	5047	5047	5047
R-squared	0.712	0.552	0.704	0.543
Firm FE	YES	YES	YES	YES
Ind x Year FE	YES	YES	YES	YES

Table A.15 JunkBond

	WW		SA	
	patents	*cites*	*patents*	*cites*
JunkBondPct $\times \mathbb{1}\,[Post]$	1.218**	37.96***	1.215**	36.70***
	(0.532)	(9.746)	(0.522)	(9.574)
MTB	0.0102	-0.538	0.0137	-0.523
	(0.0285)	(0.557)	(0.0269)	(0.557)
RDS	0.0133	0.457	-0.0031	0.531
	(0.0298)	(0.472)	(0.0381)	(0.476)
FC	-0.161	-3.294**	-0.179	2.766
	(0.0957)	(1.438)	(0.164)	(2.597)
MTB_cited	0.205*	-0.101	0.188	-0.195
	(0.112)	(1.613)	(0.118)	(1.655)
RDS_cited	-0.565	-1.638	-0.551	-0.866
	(0.416)	(8.895)	(0.431)	(9.012)
profit	0.153	-7.792**	0.174	-7.928**
	(0.272)	(3.777)	(0.275)	(3.709)
cashholdings	-1.132***	-18.37***	-1.159***	-19.84***
	(0.393)	(6.084)	(0.403)	(6.427)
tangibility	-0.834	-24.08**	-0.871	-27.38**
	(0.584)	(11.45)	(0.643)	(12.3)
Observations	1856	1856	1856	1856
R-squared	0.98	0.977	0.98	0.977
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A.16 JunkBond2

	WW		SA	
	patents	*cites*	*patents*	*cites*
$\mathbb{1}\left[Treated \times Post\right]$	0.080	5.570***	0.081	5.464***
	(0.0648)	(1.11)	(0.0649)	(1.102)
MTB	0.0136	-0.304	0.0163	-0.294
	(0.0242)	(0.481)	(0.023)	(0.489)
RDS	0.0259	0.696	0.0168	0.847**
	(0.0309)	(0.425)	(0.0379)	(0.42)
FC	-0.112	-2.297	-0.0931	3.327
	(0.0961)	(1.501)	(0.148)	(2.388)
MTB_cited	0.125	-0.493	0.114	-0.505
	(0.101)	(1.433)	(0.105)	(1.438)
RDS_cited	-0.336	-0.171	-0.324	0.504
	(0.42)	(8.974)	(0.431)	(9.062)
profit	0.111	-8.424**	0.123	-8.629**
	(0.293)	(3.8)	(0.296)	(3.769)
cashholdings	-1.214***	-20.57***	-1.234***	-21.84***
	(0.366)	(5.744)	(0.375)	(6.008)
tangibility	-1.104*	-26.08**	-1.144*	-29.05**
	(0.56)	(11.31)	(0.615)	(12.14)
Observations	1856	1856	1856	1856
R-squared	0.981	0.978	0.981	0.978
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A.17 Self Citation Percentage

	WW		SA	
FC_ cited	-0.00509***	-0.00480***	-0.00778***	-0.00753***
	(0.00113)	(0.00112)	(0.00144)	(0.00142)
FC	0.00563***	0.00625***	0.0105***	0.0140***
	(0.00181)	(0.00189)	(0.00391)	(0.00398)
MTB	0.000	0.001	0.000147	0.000237
	(0.000377)	(0.000378)	(0.00039)	(0.000384)
RDS	0.001	0.001	0.00164**	0.00146*
	(0.000808)	(0.00076)	(0.000808)	(0.000752)
MTB_cited	-0.001	-0.001	-0.000859*	-0.000692
	(0.000517)	(0.000499)	(0.000516)	(0.000495)
RDS_cited	-0.00831***	-0.00692***	-0.00742***	-0.00612***
	(0.0018)	(0.00192)	(0.00186)	(0.00198)
profit	0.001	0.002	0.00085	0.00179
	(0.00307)	(0.00316)	(0.00314)	(0.00321)
cashholdings	0.0156***	0.0164***	0.0147***	0.0146***
	(0.00522)	(0.00525)	(0.00514)	(0.00512)
tangibility	-0.00948*	-0.00911*	-0.0109*	-0.0117**
	(0.00555)	(0.00546)	(0.00559)	(0.00549)
$\mathbb{1}[Patents > 0]$	-0.0242***	-0.0241***	-0.0244***	-0.0243***
	(0.00231)	(0.00228)	(0.00229)	(0.00228)
citations	4.99e-05***	4.82e-05***	5.07e-05***	4.91e-05***
	(0.0000135)	(0.0000139)	(0.0000135)	(0.0000141)
Observations	19882	19882	19882	19882
R-squared	0.476	0.49	0.476	0.491
Firm FE	YES	YES	YES	YES
Year FE	YES	NO	YES	NO
Ind x Year FE	NO	YES	NO	YES

Table A.18 Self Citation Per Patent

	WW		SA	
FC_ cited	-0.130**	-0.118*	-0.289***	-0.280***
	(0.064)	(0.0669)	(0.0569)	(0.0573)
FC	0.432***	0.426***	1.405***	1.389***
	(0.101)	(0.102)	(0.38)	(0.392)
MTB	0.000	0.002	-0.0326***	-0.0308***
	(0.0104)	(0.0107)	(0.0103)	(0.0106)
RDS	-0.010	-0.012	0.0206	0.0182
	(0.016)	(0.0153)	(0.0128)	(.0003)
MTB_cited	-0.0369**	-0.0399**	-0.0382**	-0.0404**
	(0.0171)	(0.0179)	(0.0172)	(0.0177)
RDS_cited	-0.192***	-0.177***	-0.137**	-0.126**
	(0.057)	(0.0615)	(0.0569)	(0.0601)
profit	0.099	0.095	0.187**	0.190**
	(0.118)	(0.12)	(0.0926)	(0.0924)
cashholdings	0.378*	0.464**	0.14	0.207
	(0.209)	(0.213)	(0.214)	(0.217)
tangibility	-0.336**	-0.284*	-0.656***	-0.619***
	(0.151)	(0.148)	(0.211)	(0.214)
$\not\Vdash[Patents > 0]$	-0.162***	-0.174***	-0.186***	-0.196***
	(0.0447)	(0.0444)	(0.0462)	(0.0463)
citations	-0.00121**	-0.00115*	-0.00110*	-0.00105*
	(0.000579)	(0.000586)	(0.000594)	(0.000607)
Observations	19882	19882	19882	19882
R-squared	0.378	0.385	0.388	0.393
Firm FE	YES	YES	YES	YES
Year FE	YES	NO	YES	NO
Ind x Year FE	NO	YES	NO	YES

Table A.19 Mahalanobis Distance

	WW		SA	
	Md	Md	Md	Md
FC_citedfirm	-0.0901***	-0.118***	-0.0336***	-0.0712***
	(0.0076)	(0.004)	(0.00827)	(0.00234)
RDS_citedfirm	8.68e-05**	0.000108***	-0.0000193	1.26e-05*
	(0.0000347)	(0.0000229)	(0.0000165)	(0.00000725)
FC	-0.0940***		-0.0469***	
	(0.0128)		(0.0136)	
RDS	0.00853***		-0.00107	
	(0.00181)		(0.00137)	
MTB	0.00828**		0.00361	
	(0.0035)		(0.00398)	
junkpost				
junk				
pifopct				
pifopost				
Observations	839068	1049472	856971	1056654
R-squared	0.146	0.585	0.032	0.499
Firm-Year FE	NO	YES	NO	YES

Table A.20 Market Value

	WW		SA	
	$ln(Size)$	$ln(Size)$	$ln(Size)$	$ln(Size)$
FC_ cited	0.012	0.003	0.037	0.030
	(0.494)	(0.127)	(1.327)	(1.126)
FC_cited $\times \mathbb{1}\left[HighCites\right]$	0.0151		0.0437**	
	(0.726)		(2.177)	
$\mathbb{1}\left[HighCites\right]$	0.119***		0.118***	
	(6.227)		(6.053)	
FC_cited $\times \mathbb{1}\left[HighPatents\right]$		0.0245		0.0494***
		(1.3)		(2.609)
$\mathbb{1}\left[HighPatents\right]$		0.140***		0.140***
		(7.99)		(7.793)
RDS	0.0422***	0.0419***	0.0131	0.0132*
	(4.909)	(0.975)	(0.986)	(1.001)
FC	-0.414***	-0.407***	-0.745***	-0.729***
	(-11.99)	(7.23)	(6.696)	(6.71)
RDS_cited	0.0431	0.0426	0.0434	0.0431
	(0.97)	(2.439)	(2.983)	(3.018)
profit	0.813***	0.814***	0.796***	0.797***
	(7.218)	(-15.05)	(-14.91)	(-14.95)
cashholdings	0.234**	0.239**	0.298***	0.300***
	(2.377)	(7.99)		(7.793)
tangibility	-1.831***	-1.827***	-1.831***	-1.828***
	(-15.01)	(90.59)	(71.43)	(71.64)
Observations	19875	19875	19875	19875
R-squared	0.922	0.922	0.921	0.921
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A.21 Large Moves in FC

	cites	*patents*
FC_ cited_move	139.6***	5.527***
	(5.936)	(5.698)
FC	-52.53***	-2.622***
	(-9.267)	(-10.15)
MTB	4.585***	0.196***
	(3.103)	(2.952)
RDS	2.115***	0.0839***
	(2.926)	(2.613)
MTB_cited	13.26***	0.740***
	(3.2)	(4.334)
RDS_cited	0.223	0.0205
	(0.575)	(1.164)
profit	-25.78***	-1.632***
	(-3.863)	(-5.493)
cashholdings	51.39***	2.077***
	(4.215)	(3.753)
tangibility	42.01***	2.074***
	(3.749)	(4.035)
Constant	-117.1***	-3.439***
	(-4.348)	(-3.071)
Observations	19882	19882
R-squared	0.194	0.237
Firm FE	YES	YES
Year FE	YES	YES

Table A.22 Model Specifications

DepVar	X1	X2	X3	X4	X5	X6
book leverage	Q	logSale	ROA	z-score	mcap	tangibility
debt iss	Q	logSale	ROA	z-score	mcap	tangibility
equity iss	Q	logSale	ROA	z-score	mcap	tangibility
capex/at	Q	logSale	z-score	CF	cash	
R&D/at	Q	logSale	z-score	CF	cash	
ROA	Q	logSale	R&D/at			
ActRec/ActPay	Q	ROA	Stock Return			
ownership	Q	ROA	Stock Return			
compensation	Q	ROA	Stock Return			
Q	R&D/at	logSale	ROA			

Table A.23 Full Sample Tests

DepVar	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	Joint
leverage	.00/.20	.00/.00	.02/.00	.00/.00	.31/.00	.00/.14	.00/.00
debt iss	.00/.00	.00/.00	.00/.00	.00/.00	.00/.00	.72/.53	.00/.00
equity iss	.00/.00	.00/.35	.00/.00	.00/.00	.00/.48	.00/.00	.00/.00
capex/at	.00/.00	.00/.00	.00/.34	.00/.00	.00/.00		.00/.00
R&D/at	.00/.00	.00/.00	.42/.11	.13/.00	.00/.00		.00/.00
ROA	.91/.59	.00/.00	.22/.17				.00/.00
Rec/Pay	.00/.00	.00/.02	.77/.90				.12/.04
ownership	.28/.00	.07/.05	.00/.00				.01/.00
comp	.00/.00	.00/.31	.00/.00				.00/.00
Q	.23/.96	.00/.00	.00/.01				.00/.00

Table A.24 Sub-sample Tests

Panel A: Median p-value by 10-year sub-periods							
DepVar	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	Joint
leverage	.127/.525	.007/.000	.566/.000	.000/.001	.184/.035	.277/.258	.000/.000
debt iss	.006/.053	.000/.001	.051/.000	.000/.000	.404/.000	.000/.045	.000/.000
equity iss	.000/.000	.000/.117	.316/.001	.002/.056	.000/.298	.000/.155	.000/.000
capex/at	.000/.493	.001/.000	.219/.058	.016/.000	.000/.000		.000/.000
R&D/at	.014/.120	.037/.362	.422/.335	.187/.363	.000/.199		.000/.074
ROA	.286/.007	.000/.012	.685/.289				.000/.000
Rec/Act	.312/.000	.075/.172	.485/.917				.020/.024
ownership	.188/.113	.135/.259	.229/.079				.000/.010
comp	.524/.280	.007/.340	.033/.000				.000/.000
Q	.187/.277	.000/.002	.000/.065				.000/.000
Panel B: Median p-value by 1 digit sic industry sub-samples							
DepVar	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	Joint
leverage	.086/.512	.086/.000	.253/.000	.000/.339	.271/.118	.008/.069	.006/.000
debt iss	.001/.017	.000/.009	.406/.002	.000/.000	.109/.023	.121/.053	.000/.001
equity iss	.000/.000	.000/.340	.242/.129	.011/.129	.000/.090	.000/.001	.000/.016
capex/at	.001/.301	.039/.000	.313/.060	.312/.000	.000/.000		.000/.001
R&D/at	.063/.387	.196/.191	.428/.191	.304/.279	.331/.279		.257/.372
ROA	.047/.020	.000/.044	.108/.274				.000/.274
Rec/Pay	.579/.225	.045/.043	.298/.651				.548/.206
ownership	.346/.090	.284/.472	.303/.041				.210/.232
comp	.214/.236	.177/.493	.071/.000				.244/.036
Q	.409/.399	.000/.045	.006/.405				.000/.039

Table A.25 Sign Difference Between FD and FE

% regressions with sign difference						
Dependent Variable	x1	x2	x3	x4	x5	x6
book leverage	.062	.500	.000	.000	.000	.005
debt iss	.000	.222	.171	.000	.500	.280
equity iss	.000	.500	.112	.390	.000	.171
capex/at	.000	.222	.444	.000	.000	
R&D	.567	.112	.000	.444	.171	
ROA	.000	.000	.000			
ActRec/ActPay	.281	.000	.062			
ownership	.062	.281	.171			
compensation	.062	.000	.000			
Q	.333	.062	.171			
% regressions with sign difference significant at 10% level						
Dependent Variable	x1	x2	x3	x4	x5	x6
book leverage	.000	.171	.000	.000	.000	.005
debt iss	.000	.171	.062	.000	.390	.171
equity iss	.000	.390	.112	.112	.000	.062
capex/at	.000	.112	.390	.000	.000	
R&D	.281	.062	.000	.000	.062	
ROA	.000	.000	.000			
ActRec/ActPay	.000	.000	.000			
ownership	.000	.000	.062			
compensation	.062	.000	.000			
Q	.110	.005	.062			
median ratio of coefficients : $\max(\beta_{fe}, \beta_{fd})/\min(\beta_{fe}, \beta_{fd})$						
Dependent Variable	x1	x2	x3	x4	x5	x6
book leverage	1.58	2.19	1.28	1.51	1.52	1.18
debt iss	1.16	1.99	1.28	1.57	1.39	2.15
equity iss	1.13	2.96	1.73	3.41	4.42	2.50
capex/at	1.46	1.95	2.13	1.34	1.52	
R&D	2.20	1.21	1.24	1.43	1.56	
ROA	1.24	1.70	1.11			
ActRec/ActPay	1.53	1.44	1.74			
ownership	2.24	2.43	2.85			
compensation	1.36	1.34	1.20			
Q	1.76	1.41	1.74			

Table A.26 FE α_i correlations 10 year sub-periods

	alpha1	alpha2	alpha3	alpha4	alpha5
Model 1					
alpha1	1.0000	0.7006	0.3380	0.2339	0.0995
alpha2	0.7006	1.0000	0.5394	0.3103	0.1066
alpha3	0.3380	0.5394	1.0000	0.5990	0.2479
alpha4	0.2339	0.3103	0.5990	1.0000	0.5656
alpha5	0.0995	0.1066	0.2479	0.5656	1.0000
Model 2					
alpha1	1.0000	0.6959	0.2961	0.3145	0.1201
alpha2	0.6959	1.0000	0.4319	0.3340	0.1004
alpha3	0.2961	0.4319	1.0000	0.3169	0.1368
alpha4	0.3145	0.3340	0.3169	1.0000	0.2035
alpha5	0.1201	0.1004	0.1368	0.2035	1.0000
Model 3					
alpha1	1.0000	0.7250	-0.4637	-0.2369	-0.4584
alpha2	0.7250	1.0000	-0.1509	-0.0583	-0.3113
alpha3	-0.4637	-0.1509	1.0000	0.3675	0.3652
alpha4	-0.2369	-0.0583	0.3675	1.0000	0.3909
alpha5	-0.4584	-0.3113	0.3652	0.3909	1.0000
Model 4					
alpha1	1.0000	0.6101	0.3832	0.4474	0.3231
alpha2	0.6101	1.0000	0.5770	0.5341	0.5205
alpha3	0.3832	0.5770	1.0000	0.6399	0.4704
alpha4	0.4474	0.5341	0.6399	1.0000	0.6985
alpha5	0.3231	0.5205	0.4704	0.6985	1.0000
Model 5					
alpha1	1.0000	0.8265	0.6898	0.6816	0.6110
alpha2	0.8265	1.0000	0.8478	0.7282	0.6921
alpha3	0.6898	0.8478	1.0000	0.8079	0.7077
alpha4	0.6816	0.7282	0.8079	1.0000	0.8636
alpha5	0.6110	0.6921	0.7077	0.8636	1.0000
Model 6					
alpha1	1.0000	0.7829	0.7216	0.5947	0.5896
alpha2	0.7829	1.0000	0.7158	0.6347	0.6514
alpha3	0.7216	0.7158	1.0000	0.6533	0.6526
alpha4	0.5947	0.6347	0.6533	1.0000	0.6121
alpha5	0.5896	0.6514	0.6526	0.6121	1.0000
Model 7					
alpha1	1.0000	0.6189	0.2902	0.3113	0.3609
alpha2	0.6189	1.0000	0.4411	0.2557	0.2798
alpha3	0.2902	0.4411	1.0000	0.5699	0.4191
alpha4	0.3113	0.2557	0.5699	1.0000	0.5088
alpha5	0.3609	0.2798	0.4191	0.5088	1.0000

Table A.27 FE α_i correlations 5 year sub-periods

	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
$leverage = Q + logSale + ROA + zscore + mcap + tangibility + i.year$									
alpha1	1.000	0.774	0.505	0.376	0.266	0.219	0.224	0.097	0.072
alpha2	0.774	1.000	0.737	0.468	0.288	0.293	0.178	0.137	0.095
alpha3	0.505	0.737	1.000	0.667	0.380	0.360	0.197	0.102	0.029
alpha4	0.376	0.468	0.667	1.000	0.611	0.454	0.287	0.186	0.099
alpha5	0.266	0.288	0.380	0.611	1.000	0.629	0.393	0.261	0.233
alpha6	0.219	0.293	0.360	0.454	0.629	1.000	0.627	0.361	0.312
alpha7	0.224	0.178	0.197	0.287	0.393	0.627	1.000	0.641	0.419
alpha8	0.097	0.137	0.102	0.186	0.261	0.361	0.641	1.000	0.720
alpha9	0.072	0.095	0.029	0.099	0.233	0.312	0.419	0.720	1.000
$debtiss = l.Q + logSale + ROA + l.zscore + l.mcap + l.tangibility + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.734	0.548	0.558	0.203	0.543	0.139	0.336	0.268
alpha2	0.734	1.000	0.694	0.579	0.285	0.533	0.070	0.372	0.248
alpha3	0.548	0.694	1.000	0.548	0.353	0.463	0.053	0.330	0.153
alpha4	0.558	0.579	0.548	1.000	0.386	0.543	0.143	0.359	0.319
alpha5	0.203	0.285	0.353	0.386	1.000	0.386	0.203	0.268	0.219
alpha6	0.543	0.533	0.463	0.543	0.386	1.000	0.276	0.405	0.379
alpha7	0.139	0.070	0.053	0.143	0.203	0.276	1.000	0.198	0.189
alpha8	0.336	0.372	0.330	0.359	0.268	0.405	0.198	1.000	0.295
alpha9	0.268	0.248	0.153	0.319	0.219	0.379	0.189	0.295	1.000
$equityiss = l.Q + logSale + ROA + l.zscore + l.mcap + l.tangibility + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.536	0.432	-0.521	-0.113	0.054	-0.201	-0.288	-0.433
alpha2	0.536	1.000	0.323	-0.107	0.124	0.184	0.023	-0.018	-0.231
alpha3	0.432	0.323	1.000	-0.154	0.064	0.083	-0.007	-0.116	-0.154
alpha4	-0.521	-0.107	-0.154	1.000	0.297	0.214	0.444	0.452	0.577
alpha5	-0.113	0.124	0.064	0.297	1.000	0.440	0.394	0.323	0.224
alpha6	0.054	0.184	0.083	0.214	0.440	1.000	0.481	0.375	0.153
alpha7	-0.201	0.023	-0.007	0.444	0.394	0.481	1.000	0.566	0.400
alpha8	-0.288	-0.018	-0.116	0.452	0.323	0.375	0.566	1.000	0.488
alpha9	-0.433	-0.231	-0.154	0.577	0.224	0.153	0.400	0.488	1.000

Table A.28 FE α_i correlations 5 year sub-periods

$capex/at = \alpha_i + l.Q + logSale + cf + l.zscore + l.cash + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.559	0.540	0.273	0.363	0.444	0.364	0.322	0.238
alpha2	0.559	1.000	0.697	0.631	0.477	0.504	0.549	0.550	0.508
alpha3	0.540	0.697	1.000	0.574	0.491	0.499	0.488	0.491	0.461
alpha4	0.273	0.631	0.574	1.000	0.582	0.501	0.475	0.420	0.456
alpha5	0.363	0.477	0.491	0.582	1.000	0.668	0.543	0.532	0.467
alpha6	0.444	0.504	0.499	0.501	0.668	1.000	0.701	0.609	0.562
alpha7	0.364	0.549	0.488	0.475	0.543	0.701	1.000	0.727	0.658
alpha8	0.322	0.550	0.491	0.420	0.532	0.609	0.727	1.000	0.793
alpha9	0.238	0.508	0.461	0.456	0.467	0.562	0.658	0.793	1.000
$R\&D/at = \alpha_i + l.Q + logSale + cf + l.zscore + l.cash + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.854	0.783	0.711	0.678	0.691	0.650	0.595	0.559
alpha2	0.854	1.000	0.913	0.817	0.784	0.749	0.691	0.692	0.681
alpha3	0.783	0.913	1.000	0.884	0.803	0.767	0.715	0.669	0.643
alpha4	0.711	0.817	0.884	1.000	0.893	0.793	0.646	0.676	0.681
alpha5	0.678	0.784	0.803	0.893	1.000	0.875	0.735	0.744	0.721
alpha6	0.691	0.749	0.767	0.793	0.875	1.000	0.875	0.815	0.797
alpha7	0.650	0.691	0.715	0.646	0.735	0.875	1.000	0.870	0.808
alpha8	0.595	0.692	0.669	0.676	0.744	0.815	0.870	1.000	0.919
alpha9	0.559	0.681	0.643	0.681	0.721	0.797	0.808	0.919	1.000
$ROA = \alpha_i + l.Q + logSale + R\&D + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.828	0.777	0.714	0.703	0.626	0.633	0.645	0.531
alpha2	0.828	1.000	0.855	0.708	0.755	0.673	0.661	0.684	0.602
alpha3	0.777	0.855	1.000	0.735	0.765	0.714	0.714	0.722	0.678
alpha4	0.714	0.708	0.735	1.000	0.694	0.660	0.667	0.699	0.721
alpha5	0.703	0.755	0.765	0.694	1.000	0.729	0.705	0.682	0.674
alpha6	0.626	0.673	0.714	0.660	0.729	1.000	0.715	0.686	0.643
alpha7	0.633	0.661	0.714	0.667	0.705	0.715	1.000	0.695	0.642
alpha8	0.645	0.684	0.722	0.699	0.682	0.686	0.695	1.000	0.746
alpha9	0.531	0.602	0.678	0.721	0.674	0.643	0.642	0.746	1.000
$Q = \alpha_i + R\&D + logSale + ROA + i.year$									
	alpha1	alpha2	alpha3	alpha4	alpha5	alpha6	alpha7	alpha8	alpha9
alpha1	1.000	0.425	0.464	0.220	0.328	0.331	0.296	0.400	0.316
alpha2	0.425	1.000	0.548	0.511	0.440	0.427	0.472	0.370	0.350
alpha3	0.464	0.548	1.000	0.425	0.310	0.277	0.132	0.360	0.173
alpha4	0.220	0.511	0.425	1.000	0.597	0.437	0.438	0.362	0.280
alpha5	0.328	0.440	0.310	0.597	1.000	0.617	0.381	0.416	0.349
alpha6	0.331	0.427	0.277	0.437	0.617	1.000	0.585	0.399	0.358
alpha7	0.296	0.472	0.132	0.438	0.381	0.585	1.000	0.505	0.375
alpha8	0.400	0.370	0.360	0.362	0.416	0.399	0.505	1.000	0.594
alpha9	0.316	0.350	0.173	0.280	0.349	0.358	0.375	0.594	1.000

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