Essays in Corporate Finance

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Anna Milanez

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Abstract

Written in the wake of the 2007-08 financial crisis, the following essays explore the nature and implications of firm-level financial distress. The first essay examines the external effects of financial distress, while the second and third essays examine its internal consequences. The first essay investigates the potential contagion effects of financial distress among retail firms using a novel measure of retailers' geographic exposure to one another and, in particular, to liquidated chain stores. The second essay draws on new, hand-collected data on firm-level layoff instances to look into the ways in which financial distress impinges on firms' employment behavior. Building on the second essay, the third essay considers financial market reactions to layoff decisions, particularly those resulting from financial strain. Each essay sheds additional light on the ways in which financial distress propagates through to affect the economy at large. Overall, the picture that emerges is one in which firm-level financial distress appears to be an important factor behind the long and protracted nature of the current economic recovery.

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Chapter 1: Bankruptcy Externalities in Retail

Introduction

How does bankruptcy spread? While research on bankruptcy and financial distress has documented how bankruptcy reorganizations affect firms that go through Chapter-11 reorganizations, there is limited evidence on the effect of bankruptcies and financial distress on competitors and industry peers.[1] In this paper, we identify a new channel through which bankrupt firms impose negative externalities on their non-bankrupt competitors, namely, through their impact on peer firm sales and on the propensity to close stores.

Research in industrial organization has argued that the geographic concentration of stores can be explained by consumers' imperfect information and their need to search the market (Wolinsky (1983)). Consistent with theoretical predictions, empirical studies show that sales of neighboring stores are correlated in a manner that is consistent with the existence of positive externalities among them. Such externalities exist since some stores – those of national name-brands or anchor department stores, in particular – draw customer traffic not only to their own stores but also to nearby stores. As a result, store level sales may depend on the sales of neighboring stores for reasons that are unrelated to local economic conditions (Gould and Pashigan (1998) and Gould, Pashigan and Prendergast (2005)).

We conjecture that the externalities that exist between neighboring stores, and the economies of agglomeration they create, can be detrimental during downturns, propagating and amplifying the negative effects of financial distress and bankruptcies among firms in the same locality. Our main prediction is that, due to economics of agglomeration, retail stores in distress impose negative externalities on their neighboring peers: store sales tend to decrease with the reduction in sales, and ultimately the closure, of stores nearby. If such negative externalities are sufficiently strong,

[1] For papers that study the effect of bankruptcy on firm outcomes, see Asquith, Gertner, and Scharfstein (1994), Hotchkiss (1995), and Stromberg (2000).

bankruptcies, and the store closure they involve, will lead to additional bankruptcies, propagating within a given area.

Identifying a causal link, however, from the bankruptcy and financial distress of one retailer to the sales and closure decisions of its neighboring retailers is made difficult by the fact that bankruptcy filings and financial distress are correlated with local economic conditions. Correlation in sales among stores in the same vicinity may therefore simply reflect weak demand in an area. Similarly, the fact that store closures tend to cluster locally may often be the outcome of underlying difficulties in the local economy, rather than the effect of negative externalities among stores. Local economic conditions will naturally drive a correlation in outcomes among stores located in the same area.

Using a novel and detailed dataset of all national chain store locations, openings, and closures across the United States from 2005 to 2010, we provide empirical evidence that supports the view that bankruptcies of retail companies impose negative externalities on neighboring stores owned by solvent companies. Our identification strategy consists of analyzing the effect of Chapter 7 bankruptcies of large national retailers, such as Circuit City and Linens 'n Things, who liquidated their entire store chain during the sample period. Using Chapter 7 bankruptcies of national retailers alleviates the concern that local economic conditions led to the demise of the company: it is unlikely that a large retail chain will suffer major financial difficulties because of a localized economic downturn in one of its many locations. Supporting this identification assumption, we show that stores of retail chains that eventually end up in Chapter 7 bankruptcy are not located in areas that are worse than the location of stores operated by chains that do not end up in bankruptcy, along a host of economic characteristics.

Using detailed data on store locations, we show that stores located in proximity to stores of national chains that are liquidated are more likely to close, and, further, that new stores are unlikely to open in these areas. We also study the interaction between the geographical effect of store closures and the financial health of solvent owners of neighboring stores. We hypothesize that the impact of national chain store liquidations will be stronger on firms in weaker financial health, as these stores are expected to suffer more from the reduction in customer traffic. Focusing on stores owned by a parent company, and measuring financial health using the profitability of the parent, we find consistent with our hypothesis that the geographical effect of store closures on neighboring stores is indeed more pronounced in financially weaker firms.

Next, we turn to analyze the aggregated firm-level effects of bankrupt store closures. While the fine resolution of store-level analysis enables us to better identify localized effects and control for unobserved localized geographic heterogeneity, it is unclear whether the localized effects aggregate up in a meaningful way to firm-level outcomes. To this end, we run firm-level regressions examining the effect of the level of firm exposure to neighboring national-chain store closures on various firm-level outcomes. We find that the impact of store closures does indeed aggregate to the firm level. Increases in the exposure to neighboring store closures is associated with reduced firm-level sales as well as a reduction in the number of stores under operation.

Related Literature

This paper is related to a broad literature on the competitive behavior of firms, including product market competition, price setting behavior, firms' location preferences and entry and exit decisions. We focus on entry and exit decisions, in particular – the strategic openings and closures of individual retail stores in response to neighboring competitors.

The literature on spatial competition extends back to Hotelling (1929). According to Hotelling's model, firms co-locate in order to attract consumers who travel to the nearest firm. The classic example is ice cream vendors locating near one another on a beach, which extends to the tendency for retailers to co-locate in shopping centers and malls. There are other explanations for co-location.

From the supply side, firms' location choices may create competitive advantages by improving access to key resources (such as skilled labor or suppliers), reducing input costs or benefiting from knowledge spill-over (Marshall (1920)). From the demand side, firms co-locate to attract consumers, who are often concentrated.

Our analysis relies on the existence of economies of agglomeration. Evidence for this is provided in Gould and Pashigan (1998) and Gould, Pashigan and Prendergast (2005) who show that anchor stores in malls create positive externalities on other non-anchor stores by attracting customer traffic. Mall owners internalize this externality by providing rent subsidies to anchor stores. Indeed, the rent subsidy provided to anchor stores as compared to non-anchor stores – estimated at no less than 72 percent – suggests that these positive externalities are economically large.

Previous studies have related entry and exit behavior to the intensity of product market competition. Chevalier (1995a) establishes an empirical link between firm capital structure and product-market competition using data from local supermarket competition and, more specifically, the entry and exit behavior of chains surrounding leveraged buyouts (LBOs). An event-study analysis suggests that an LBO announcement increases the market value of the LBO chain's local competitors. In addition, supermarket chains were more likely to enter and expand in local markets in which a large share of the incumbent firms in the local market undertook leveraged buyouts. Overall, the study suggests that leverage increases in the late 1980s led to softer product market competition, which in turn encouraged the entry of competitors.

Kovenock and Phillips (1995) present evidence on the link between capital structure and product market competition that is consistent with the LBO analysis in Chevalier (1995a). They study the relationship between product market rivalry and capital structure using data on capital structure decisions and product market behavior from the U.S. Census. They find that firms are more likely to recapitalize when they have individual plants of low productivity and operate in a highly concentrated industry. Kovenock and Phillips (1997) add to Kovenock and Phillips (1995) by considering how leverage recapitalization affects individual firm investment and plant closure decisions across ten different industries. They find that, in highly concentrated markets, high leverage appears to decrease firm-level investment and increase plant closures.

According to theoretical predictions, an increase in leverage creates incentives to raise product prices, which will in turn affect entry and exit behavior. While Chevalier (1995a) looks at local product market competition through entry and exit following LBO announcements, the study does not contain evidence on price changes. Chevalier (1995b) studies prices changes within supermarkets across a variety of local markets, using firm-level prices to study price differences between LBO and non-LBO firms. The study finds evidence that prices rise following LBOs in local markets in which the LBO firm's competitors are also highly leveraged. In these contexts, the LBO firms tend to have higher prices than non-LBO firms, suggesting that LBOs tend to induce price increases. However, the study finds that prices tend to fall following LBOs in local markets in which the LBO firm's competitors have relatively low leverage, where these price drops are associated with the LBO firm leaving the market.

There have been several other empirical studies on price competition. Chevalier and Scharfstein (1995) find that industries in which a relatively large fraction of output is produced by small firms tend to have more counter-cyclical price markups (after controlling for total market concentration). The underlying idea is that small firms should be more strapped for cash during recessions, since smaller firms have more restricted access to capital markets in general. As a consequence, they are predicted to forgo investment in customer loyalty in these periods and raise prices.

Chevalier and Scharfstein (1996) consider pricing within supermarkets along another angle. They focus on price changes in states hit hard by the oil-price decline of 1986. They ask whether, within these oil states, supermarkets belonging to national chains (and thus able rely to a greater extent on external financing) lowered their prices relative to local supermarkets, who were presumably more strapped for cash. They find this to be the case, suggesting that national supermarkets were more willing to invest in customers because they had lower discount rates as a result of easier access to external financing.

There have been few studies analyzing how firms in bankruptcy or financial distress affect their industry peers. One exception is Benmelech and Bergman (2011) who use data from the airline industry to examine how firms in financial distress impose negative externalities on their industry peers. This negative externality arises in the form of an increase in the cost of capital of peer firms using the same type of collateral as those firms entering distress. This collateral channel thus provides a different mechanism than that studied in this paper through which financial distress can propagate and be amplified through the economy.

Identification Strategy

Our main prediction is that, due to economics of agglomeration, the closure of retail stores imposes negative externalities on their neighbors – that is, store sales tend to decrease with a decline in customer traffic in their area. If this effect is sufficiently large, store closures will tend to propagate geographically. However, identifying a causal link from the financial distress or bankruptcy of retailers to the decision of a neighboring solvent retailer to close its stores is difficult because financial distress is potentially correlated with underlying local economic conditions. For example, the fact that local retailers are in financial distress can convey information about weak local demand. Similarly, the fact that store closures tend to cluster locally does not imply in and of itself a causal link but rather may simply reflect difficulties in the local economy.

Our identification strategy consists of analyzing the effect of Chapter 7 bankruptcies of large national retailers, such as Circuit City and Linens 'n Things, who liquidate their entire store chain during the sample period. Using Chapter 7 bankruptcies of national retailers alleviates the concern that local economic conditions led to the demise of the company: it is unlikely that a large retail chain will suffer major financial difficulties because of a localized economic downturn in one of its many locations. Still, it is likely that national chains experiencing financial distress will restructure their operations and cherry-pick those stores they would like to remain open. According to this, financially distressed retailers will shut down their worst performing stores while keeping their best stores open, implying that a correlation between closures of stores of bankrupt chains may merely reflect poor local demand rather than negative externalities driven by financial distress. We address this concern directly by only utilizing variation driven by bankruptcy cases that result in the liquidation of the *entire* chain. In these cases, there is clearly no concern of cherry-picking of the more successful stores; all stores are closed regardless of local demand conditions.

In examining national chain liquidations, one concern that remains is that the stores of the liquidating chain were located in areas that experienced negative economic shocks – for example, because of poor store placement decisions made on the part of headquarters – and that it was these shocks that eventually drove the chain into bankruptcy. In fact, this turns out not to be the case: we show empirically that stores of chains that eventually file for Chapter 7 bankruptcy are not located in areas that are worse than the location of stores operated by chains that do not end up in bankruptcy, along a host of economic characteristics such as median household income, house value, and the percent of population in poverty.

Data and Summary Statistics

Sample Construction and Data Sources

Our dataset is composed of several sources, each described in turn in this section. The main source is Chain Store Guide (CSG) a dataset of retail chain stores across the United States, in which each individual store is described by the name of the chain that it belongs to and by its street address. The data is organized in annual snapshots from 2005 through 2010. We link store chain names to their parent company owners and rely on the parent company names to incorporate other sources of firm-level data, such as SDC and Compustat. Lastly, we gather demographic data from a variety of sources, including the Census, the BLS, the IRS and Zillow.

Chain Store Guide

Chain Store Guide (CSG) is a trade publication devoted to trends facing retail and wholesale chain stores. It provides store location data on major retailers, restaurants, distributors and wholesalers in the Unites States and Canada.[2] We obtained access from CSG on apparel and general merchandise chain stores between 2005 and 2010. The variables included in the data are; (1) company name; (2) store phone number, and (3) store address (street number, street name, city, state and zip code). In its raw form, CSG contained 829,747 observations spanning the years 2005-2010, spread across 51 unique states (including Washington D.C.).

The unique company identifier in the CSG data is company name, which we clean and organize for consistency, resulting in 10,370 unique company names, with large chain stores accounting for the bulk of the data. For example, in 2010, the 75 largest chain stores accounted for 108,099 of the 166,032 stores in the dataset, representing 65.1 of the stores in the data for that year.

SDC Platinum

We use SDC Platinum to identify retail bankruptcies since January 2000 within the following SIC retail trade categories: general merchandise (SIC 4-digit codes 5311, 5331 and 5399), apparel (5600, 5621 and 5651), home furnishings (5700, 5712, 5731, 5734 and 5735) and miscellaneous (5900, 5912, 5940, 5944, 5945, 5960, 5961 and 5990). There are 93 cases of retail bankruptcy between 2000 and 2011. The largest bankruptcies in recent years include Anchor Blue Retail Group, Blockbuster, Borders Group, Boscov's Department Store, Circuit City, Filene's Basement,

[2] The data is typically used by manufacturers, suppliers, service providers, real estate professionals, retailers, analysts and consultants in the retail and foodservice markets.

Gottschalks, Hancock Fabrics, Jennifer Convertibles, Linens ‘n Things, Mervyn’s and the Movie Gallery. Table 1 summarizes Chapter 7 bankruptcies from 2003 through 2011.

Table 1: Retail Bankruptcies from 2003-2011

This table summarizes bankruptcies within the following SIC retail trade categories: general merchandise (SIC 4-digit codes 5311, 5331 and 5399), apparel (5600, 5621 and 5651), home furnishings (5700, 5712, 5731, 5734 and 5735) and miscellaneous (5900, 5912, 5940, 5944, 5945, 5960, 5961 and 5990). Dollar figures are in millions.

Year	Number of Bankruptcies	Sum of Assets at Initial Filings (USD $mm)	Average Assets at Initial Filings (USD $mm)	Sum of Liabilities at Initial Filings (USD $mm)	Average Liabilities at Initial Filings (USD $mm)
2011	2	$ 216.96	$ 108.48	$ 229.34	$ 114.67
2010	2	$ 51.38	$ 25.69	$ 77.77	$ 38.89
2009	12	$ 2473.7	$ 206.14	$ 2472.32	$ 206.03
2008	22	$ 6906.81	$ 313.95	$ 7073.88	$ 321.54
2007	5	$ 679.96	$ 135.99	$ 499.58	$ 99.92
2006	3	$ 48.88	$ 16.29	$ 218.71	$ 72.9
2005	3	$ 460.75	$ 153.58	$ 302.92	$ 100.97
2004	7	$ 1404.46	$ 200.64	$ 917.69	$ 131.1
2003	7	$ 1898.52	$ 271.22	$ 2005.1	$ 286.44

Compustat Fundamental and Industry Data

Next, we match the CSG data to Compustat Fundamental and Industry Data. We use the Compustat North America Fundamentals Annual and Quarterly databases to construct variables that are based on operational and financial data. These include the number of employees in the firm, size (defined as the natural log of total assets), the level of investment (defined as capital expenditures divided by the lagged value of property, plant and equipment), the level of inventories, the ratio of investment to capital, the market-to-book ratio (defined as the market value of equity and book value of assets less the book value of equity, divided by the book value of assets), profitability (defined as earnings over total assets), leverage (defined as total current liabilities plus long-term debt, divided by the book value of assets), liquidity (defined as net income plus depreciation and amortization, divided by the lagged value of property, plant and equipment) and sales revenue (defined as total sales). Appendix A provides a complete description of the variables used in the paper and their construction.

We supplement the financial information with retail industry specific variables from the Compustat North America Industry Specific Annual and Quarterly databases. These include the net sales per retail square foot, the minimum rental expense, the number of stores opened during the period, the number of stores at the period's end, the number of stores closed during the period, the percentage change in comparable sales, other rental expenses and the total retail square footage. Next we supplement the data with information pertaining to the local economies from the Census, the IRS, Zillow and the BLS.

Census Data

We rely on the Census 2000 survey for a host of demographic variables available by zip code. These include population and population density, gender, age, race, household size and number of households, marital status, educational attainment, employment status, median and average household income, number of housing units, occupancy and vacancy rates, median house value, median rent, portion of housing units financed with a mortgage, second mortgage or home equity loan, and portion of the population living in poverty.

IRS

Since income data is unavailable in the Census 2010 survey, we turn instead to the Internal Revenue Service (IRS). The IRS provides the number of filed tax returns (a proxy for the number of households), the number of exemptions (a proxy for the population), adjusted gross income (which includes taxable income from all sources less adjustments such as IRA deductions, self-employment taxes, health insurance, alimony paid, etc.), wage and salary income, dividend income and interest income.

Zillow

We use data on house prices from Zillow, an online real estate database that tracks valuations throughout the United States. We collect estimated house value and address including city, state and zip code. We then map zip codes to counties to obtain county-level median house values.

State GDP and Unemployment Data

Lastly, we collect GDP and unemployment data from 2000 to 2009 for all states from the Bureau of Labor Statistics (BLS).

Summary Statistics

Table 2 provides summary statistics of zip code characteristics for the 816,648 store-year observations in our final dataset. Summary statistics are calculated over the entire sample and are therefore weighted by the number of store-year observation in each zip code. As the table shows, total population in zip codes that correspond to store locations was 27,864.5 in the year 2000, with a population density of 2,617 residents per square mile. Population between the ages of 18 and 55 accounted for 53.6 of the total population with a standard deviation of 0.063, while the mean population under 18, and over 55 account for 0.248 and 0.217, respectively. According to the Census, 0.784 of the population was classified as white and 0.111 as black. The number of house holds within a zip code was on average 10,624.7 (median 10,165) and the average household size was 2.55 with a standard deviation of 0.359.

Turning to information about the education characteristics of the zip codes in which the stores are located – 27.8 percent of the residents had at least a high school education, while 28.1 percent had some college education, 16.9 percent had bachelor degrees, and the fractions of residents with masters or professional degrees were 6.3 and 2.1 percent, respectively. Finally, the median household income in 2000 was $49,016 and about 10 percent of the population was living in poverty in the year 2000 according to the Census classification. The median house price was $257,514 but ranged from a 25th percentile of \$138,536 to a 75th percentile of \$317,273 with a standard deviation of \$184,613. Finally, the median house price in the counties where stores are located appreciated by 62.0 percent from 2002 to 2006, ranging from a 25th percentile of 26.8% to the 75th percentile of median house price growth of 90.9%.

Table 2: Summary Statistics

This table provides summary statistics for the demongraphic variables used in the empirical analysis.

Panel A: Zip-code characteristics: 2005-2010 (816,748 store-year observations)					
	Mean	25th percentile	Median	75th percentile	Standard deviation
Population					
Total population	27,864.5	16,292.0	26,142.0	37,097.0	15,944.6
Population density	2,617.0	276.2	1,238.8	3,000.4	6,007.9
Population under 18	0.248	0.222	0.250	0.277	0.051
Population between 18 and 55	0.536	0.500	0.530	0.564	0.063
Population over 55	0.217	0.172	0.214	0.254	0.071
Population white	0.784	0.699	0.841	0.925	0.190
Population black	0.111	0.014	0.044	0.130	0.163
Average household size	2.55	2.35	2.52	2.71	0.36
Number of households	10,624.7	6,384.0	10,165.0	14,268.0	5,816.8
Education					
Less than high school	0.177	0.101	0.157	0.234	0.103
High school	0.279	0.218	0.283	0.339	0.087
Some college	0.281	0.242	0.282	0.322	0.060
Bachelor degree	0.169	0.100	0.149	0.225	0.087
Master degree	0.063	0.033	0.052	0.083	0.041
Professional degree	0.021	0.010	0.015	0.025	0.020
Income and house prices					
Living in poverty, 2000	0.103	0.051	0.086	0.136	0.072
Median household income, 2000	49,016.4	37,141.0	46,038.0	58,298.0	16,654.7
Median house price	257,513.5	138,536.0	205,703.0	317,273.0	184,613.3
Median house price growth, 2002-2006	62.04%	26.76%	57.18%	90.86%	42.22%

Store Opening and Closures

A comparison of the data from one year to the next enables us to infer store openings and closings, summarized by year in Table 3. We define a store opening if an entry appears in a given year but not in the preceding one. Similarly, we define a store closure if an entry appears in a given year but not in the subsequent one. From 2006 to 2010, we observe 121,261 chain store openings, the peak year being 2006. From 2005-2009, we observe 440315 closures, the peak year being 2008.[3]

Table 3: Store Openings and Closures Over Time

This table summarizes the numbers of individual store openings and closures between 2005 and 2010. We define a store opening if an entry appears in a given year but not in the preceding one. Similarly, we define a store closure if an entry appears in a given year but not in the subsequent one. Given the nature of the data, we can define store openings starting in 2006 and store closures up to 2009.

Year	Number of store openings	Number of store closures	Total number of stores (at year end)
2005	--	4,013	84,388
2006	45,582	10,673	125,896
2007	32,326	1,994	147,550
2008	2,146	16,365	148,432
2009	23,297	7,270	155,113
2010	17,865	--	165,770
	121,216	40,315	742,761

Empirical Analysis

The Initial Locations of Liquidated Chain Stores

The main idea of our identification strategy is that large bankruptcies of national retail chains are less likely to be driven by local economic conditions if their stores are diversified geographically but are rather driven by bad strategy or business plan that is unrelated to the location of their stores. For our identification strategy to be valid, we first need to show that the initial locations of stores of retail chains that will eventually end-up bankrupt are not in zip codes that are worse in terms of their economic characteristics than those locations of stores operated by chains that do not end up in bankruptcy. In order to estimate the relation between local economic conditions and the location of

[3] Note that given the nature of the data, we can define store openings starting in 2006 and store closings up to 2009.

stores of national retail chains that end up in liquidation, we run a probit model where the dependent variable is a dummy variable equal to one if a store operated by a national retail chain that will end up in liquidation by the end of the year, and zero otherwise. As explanatory variables we include the 2000 Census socio-demographic controls, 2-digit retail industry fixed-effects and state fixed-effects. Table 4 reports the coefficients on four of the leading economic indicators that are based on the income and housing data; the log of the median household income in 2000; the log of the median house value in 2000; the fraction of the population living in poverty; and median house price growth during the period 2002-2006. We run the regression separately for stores that are located in shopping malls and "stand-alone" stores not in shopping malls. Table 4 reports the results from estimating different variants of the model. The table displays marginal effects computed based on the probit estimation and standard errors (in parentheses) that are clustered at the zip code level throughout the paper.

As the first column of Table 4 demonstrates, "stand-alone" stores of national retail chains that end-up in liquidation after the year 2006 are located in zip-codes with socio-demographic characteristics that are not statistically different from zip codes in which other stores are located. None of the four variables reported in the first column of Table 4 are significant statistically or have meaningful economic magnitudes. The second column of the table compares liquidated chain stores that are located in shopping malls to other stores located in shopping malls. The results indicate that, if anything, stores of liquidated chains are located in zip codes with slightly higher median household income that stores of chains that do end up in liquidation. Columns 3 and 4 repeat the analysis in Columns 1 and 2 for new store openings during the year 2005. Consistent with our previous findings, stores of retail chains that will end up in liquidation are located in zip codes that are similar to the locations of other stores in terms of median house value, poverty rate and house price appreciation. As in Column 2, the only difference between the locations of liquidated chain stores and the location of other stores is that stores of liquidated chains are located in zip codes with slightly higher median household income. As the coefficients on log(median household income) imply, liquidated chain stores are located in zip codes with median household income that is between 0.2% and 1.3% higher than in the location of other stores. Finally, in the last two columns of Table 4 we study the location of stores of liquidated chain stores by including zip-code fixed-effects. We pool together all stores in the data for both years 2005 and 2006 and study separately the location decisions of "stand alone" liquidated chain stores (Column 5) and liquidated chain stores that are located in shopping malls (Column 6). The inclusion of zip-code level fixed-effects difference out the time-invariant zip-code level socio-demographic variables. The only zip-code characteristic that is time-varying is log(median house value) which we obtain from Zillow. As Table 4 demonstrates, there is no statistically significant difference between house values in zip codes where liquidated chain stores are located compared to other stores.

In summary, Table 4 demonstrates that there are no significant differences between the location of liquidated chain stores and other stores. Moreover, the only slight differences in terms of location is that liquidated chain stores are more likely to be located in zip codes with slightly higher median household income. These results confirm that the initial location of stores of national chains that end up in liquidation is not a likely cause of their failure and thus closure of these stores is unlikely to be driven by worse local economic conditions.

Table 4: Big Bankruptcies, Store Locations and Openings

	Location	Location	Opening	Opening	Location	Location
Log(median household income)	0.001	0.011 *	0.013 ***	0.002 *		
	0.003	0.007	0.005	0.001		
Log(median house value)	-0.002	-0.003	-0.002	0.001	-0.003	-0.011
	0.001	0.003	0.003	0.001	0.004	0.010
Living in poverty	-0.001	-0.022	0.010	-0.004		
	0.013	0.025	0.022	0.005		
Median house price growth, 2002-2006	0.000	0.004	0.002	0.001		
	0.002	0.003	0.003	0.001		
Year	2006	2006	2006	2006	2005-2006	2005-2006
2000 Census zip code controls	Yes	Yes	Yes	Yes	No	No
State fixed-effects	Yes	Yes	Yes	Yes	No	No
Zip code fixed-effects	No	No	No	No	Yes	Yes
Industry fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Mall?	No	Yes	No	Yes	No	Yes
Number of zip codes	5,896	2,446	1,810	1,465	6,555	2,551
Observations	28,040	18,658	4,692	7,657	73,347	31,542
Estimation	Probit	Probit	Probit	Probit	OLS	OLS
Adjusted R^2	0.16	0.08	0.21	0.36	0.04	0.03

Store-level Analysis: The Effect of Bankruptcy on Store Closures

We begin with a simple test of the negative externalities hypothesis by estimating a linear probability model of store closures conditional on the liquidation of local stores that result from a national retailer bankruptcy. We define local stores as stores that are: (1) located in the same address; (2) stores that are located in a different address and are located with a 50 meters radius; (3) stores that are located in a different address and are located in a radius of more than 50 meters but less or equal than 100 meters; and (4) stores that are located in a different address but within a radius of more than 100 meters but less than 250 meters. The number of stores in proximity to bankrupt chains is described by year in Table 5. The greatest exposure to bankrupt chains occurs in 2008, with 9,155 stores sharing the addresses of bankrupt chains. In the same year, 5,921 stores had locations within 50 meters of bankrupt chain stores, 4,111 stores had locations within 50 to 100 meters and 8,700 stores had locations within 100 to 250 meters.

Table 5: Number of Stores in Proximity to Big Bankruptcies

This table summarizes the number of stores in proximity to big bankruptcies using four distance measures: (1) located in the same address (0m), (2) located at a different address and within 50 meter radius, (3) located at a different address and within a radius of more than 50 meters but less than or equal to 100 meters, and (4) located at a different address and within a radius of more than 100 meters but less than or equal to 250 meters.

Number of stores within range of a big bankruptcy

Year	0m	50m	50m - 100m	100m - 250m
2005	211	310	117	330
2006	3,876	1,096	656	1,511
2007	2,142	579	349	860
2008	9,155	5,921	4,111	8,700
2009	1,869	1,623	1,162	2,823

Table 6 presents the results from estimating a linear probability model in which the dependent variable equals one if a store is closed, and zero otherwise.[4] As explanatory variables we use a dummy for whether the store belongs to a bankrupt company, a dummy for whether the store is located in a shopping mall, time-variant zip-code characteristics that include the log of median house value, and the annual change in the median house value, state-level time-variant economic variables including log(income per capita) and annual income growth and firm-level characteristics that include size (defined as the natural log of total assets), leverage (defined as total current liabilities plus long-term debt, divided by the book value of assets) and profitability (defined as earnings over total assets).

We investigate the transmission of the negative externalities that are imposed by bankruptcies of neighboring store further by studying the joint impact of the firm financial health and neighboring store closures on the likelihood that a firm will close its own store. We hypothesize that the effect of neighboring store closures on the likelihood that a store will close should be larger for firms with low profitability. Less profitable firms are more likely to be in financial distress, making them more

[4] We employ a liner probability model instead of probit because of the incidental parameters problem that results from the saturation of the model with many fixed-effects.

vulnerable to a decline in demand that is driven by other stores closing down. We therefore introduce an interaction variable between profitability and each of the local store closures into the specification estimated in the regressions that are reported in Table 6. We run the analysis separately with different fixed-effects to control for geographic heterogeneity. Column 1 includes the zip code fixed-effects while Column 2 include instead county fixed-effects, while Columns 3 and 4 each control for state, and Census division fixed-effects. All the specifications include year fixed-effects and standard errors are clustered at the zip-code level.

As Table 6 demonstrates, bankrupt retailers are between 2.7 and 3.5 percentage points more likely to close their stores, representing an increase of approximately 44 to 57 percent relative to the mean. Moreover, during the period studied stores that are located in a mall were more likely to close down than "stand alone" stores. Larger retailers are less likely to close their stores, while more leveraged and less profitable retailers are more likely to close their stores. The evidence that is based on these firm characteristic is consistent with the conjecture that the financial health of the firm is an important determinant of whether stores stay open or are closed down.

Moving to the effect of local store closures on the likelihood that a store will be closed down we find evidence that supports our hypothesis that local store closures impose negative externalities on other retailers in the area. We find that local store closures in the same address increase the likelihood that a store will close down by 2.1 percentage points, representing an increase of 34 percent relative to the mean. Likewise, store closing in a different address but within a 50 meters radius further increase the likelihood of store closure by 2.5 percentage points, representing an increase of 41 percent relative to the unconditional mean. Finally, we do not find a significant effect of stores closures that are more than 50 meters away.

Consistent with the prediction of the joint effect of financial distress and store closures, we find that the effect of local store closing is amplified when the retailer operating the store under investigation is in financial distress. As can be seen in Table 6, the coefficient on the interaction term between *same address* and *profitability* is negative and statistically significant at the one percent level (the effect ranges from 0.099 to 0.100 with a standard error of 0.024). Local store closures increase the likelihood that a store with a low profitability parent will close by 9.9 to 10.0 percentage points, which represents an increase of 162 to 164 percent relative to the unconditional mean. Likewise, the coefficient on the interaction tern between *distance <= 50 meters* and *profitability* is negative and statistically significant at the one percent level (the effect ranges from 0.157 to 0.160 with standard errors between 0.039 and 0.04). These magnitudes indicate that local store closures increase the likelihood that a store with a low profitability parent will close by 15.7 to 16.0 percentage points, which represents an increase of 257 to 262 percent relative to the unconditional mean.

To summarize so far, our results are consistent with the notion that the effect of the externalities imposed by local store closures exist and hat they are more pronounced for firms that less profitable and more leveraged.

Table 6: The Effect of Bankruptcy on Store Closures

***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Bankrupt stores				
Same address	0.021 ***	0.020 ***	0.018 ***	0.018 ***
	(0.006)	(0.006)	(0.006)	(0.006)
x Profitability	-0.099 ***	-0.100 ***	-0.099 ***	-0.100 ***
	(0.024)	(0.024)	(0.024)	(0.024)
Distance ≤ 50 meters	0.025 ***	0.023 ***	0.023 ***	0.022 ***
	(0.008)	(0.008)	(0.008)	(0.009)
x Profitability	-0.160 ***	-0.157 ***	-0.158 ***	-0.158 ***
	(0.039)	(0.040)	(0.040)	(0.040)
50 meters < distance ≤ 100 meters	-0.003	-0.006	-0.006	-0.006
	(0.009)	(0.009)	(0.009)	(0.009)
x Profitability	0.015	0.014	0.014	0.012
	(0.045)	(0.044)	(0.044)	(0.044)
100 meters < distance ≤ 250 meters	0.002	0.001	0.000	0.000
	(0.004)	(0.006)	(0.006)	(0.008)
x Profitability	-0.004	-0.006	-0.007	-0.008
	(0.026)	(0.026)	(0.026)	(0.026)
Bankruptcy	-0.027 **	-0.034 **	-0.034 **	-0.035 **
	(0.011)	(0.010)	(0.010)	(0.010)
Mall	0.007 ***	0.009 ***	0.009 ***	0.009 ***
	(0.002)	(0.001)	(0.001)	(0.002)
Size	-0.005 ***	-0.005 ***	-0.006 ***	-0.006 ***
	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.049 ***	0.047 ***	0.047 ***	0.047 ***
	(0.003)	(0.002)	(0.002)	(0.002)
Profitability	-0.021 ***	-0.019 ***	-0.019 ***	-0.018 ***
	(0.006)	(0.006)	(0.006)	(0.006)
Log(median house value)	-0.006	-0.007 *	0.004 *	0.004 *
	(0.007)	(0.004)	(0.002)	(0.002)
Median house value change	-0.004	-0.003	-0.013	-0.008
	(0.008)	(0.008)	(0.008)	(0.008)
Log(income per capita)	-0.072 ***	0.017 *	0.006	0.005
	(0.023)	(0.009)	(0.005)	(0.050)
Income growth	0.079	0.009	0.000	-0.008
	(0.027)	(0.024)	(0.024)	(0.025)
Year fixed-effects	Yes	Yes	Yes	Yes
Zip code fixed-effects	Yes	No	No	No
County fixed-effects	No	Yes	No	No
State fixed-effects	No	No	Yes	No
Census division fixed-effects	No	No	No	Yes
Observations	305,394	305,394	305,394	305,394
Adjusted R^2	0.03	0.05	0.02	0.02

Store-level Analysis: The Effect of Bankruptcy on Store Openings

We now turn to test the effect of the negative externalities hypothesis by estimating the effect of closures of local stores of bankrupt national chains on the likelihood that new stores will open in their vicinity. As before we employ a linear probability model and use the same control variables as in the regressions reported in the previous section.

Table 7 presents the results from estimating a linear probability model in which the dependent variable equals one if a new store is opened, and zero otherwise. Similar to our previous results, we investigate the transmission of the negative externalities that are imposed by bankruptcies of neighboring stores further by studying the joint impact of the firm's financial health and neighboring store closures on the likelihood that a firm will open a new store. The results reported in Table 7 show that bankrupt retailers are between 14.9 and 16.3 percentage points less likely to open new stores stores, representing a decrease of 90 to 100 percent relative to the mean. Moreover, new stores are more likely to be opened in shopping malls. Turning to firm characteristics, we find that larger retailers are more likely to open news stores, while more profitable retailers are less likely to open new stores.

We find that local store closures in the same address decrease the likelihood that a store will be opened by between 1.3 and 2.0 percentage points, representing an increase of 21 to 33 percent relative to the mean. Likewise, a store closure in a different address but within a 50-meter radius further decreases the likelihood of a store opening by between 2.3 and 2.7 percentage points. Unlike in Table 6, we find that local store closures have an effect on the likelihood of new stores opening even for closures that are further away – between 50 and 100 meters and between 100 and 250 meters from the location of the store. Finally, as in our analysis in Table 6, we find that more profitable firms are less affected in their decisions to open new stores by local store closures.

Table 7: The Effect of Bankruptcy on Store Openings

***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Bankrupt stores				
Same address	-0.020 ***	-0.013 ***	-0.015 ***	-0.015 ***
	(0.006)	(0.006)	(0.006)	(0.006)
x Profitability	0.226 ***	0.241 ***	0.244 ***	0.242 ***
	(0.031)	(0.030)	(0.030)	(0.030)
Distance ≤ 50 meters	-0.027 ***	-0.024 ***	-0.023 ***	-0.023 ***
	(0.007)	(0.006)	(0.006)	(0.006)
x Profitability	0.148 ***	0.165 ***	0.163 ***	0.161 ***
	(0.034)	(0.034)	(0.034)	(0.034)
50 meters < distance ≤ 100 meters	-0.023 ***	-0.020 ***	-0.018 ***	-0.018 ***
	(0.008)	(0.007)	(0.007)	(0.007)
x Profitability	0.083 ***	0.094 ***	0.094 ***	0.091 ***
	(0.035)	(0.034)	(0.033)	(0.033)
100 meters < distance ≤ 250 meters	-0.036 ***	-0.030 ***	-0.029 ***	-0.029 ***
	(0.005)	(0.005)	(0.005)	(0.005)
x Profitability	0.146 ***	0.155 ***	0.153 ***	0.153 ***
	(0.025)	(0.026)	(0.026)	(0.026)
Bankruptcy	-0.149 ***	-0.163 ***	-0.162 ***	-0.163 ***
	(0.010)	(0.009)	(0.008)	(0.008)
Mall	0.060 ***	0.069 ***	0.067 ***	0.066 ***
	(0.003)	(0.002)	(0.002)	(0.002)
Size	-0.013 ***	-0.014 ***	-0.014 ***	-0.014 ***
	(0.001)	(0.001)	(0.005)	(0.001)
Leverage	0.165 ***	0.148 ***	0.145 ***	0.145 ***
	(0.005)	(0.005)	(0.005)	(0.005)
Profitability	-0.145 ***	-0.169 ***	-0.170 ***	-0.169 ***
	(0.008)	(0.007)	(0.007)	(0.007)
Log(median house value)	0.014	0.009	0.029 ***	0.025 ***
	(0.016)	(0.009)	(0.004)	(0.004)
Median house value change	-0.061 ***	-0.060 ***	-0.077 ***	-0.068 ***
	(0.021)	(0.018)	(0.015)	(0.014)
Log(income per capita)	-0.008	0.018	0.003	0.004
	(0.048)	(0.016)	(0.008)	(0.007)
Income growth	0.105 *	0.086	0.146 ***	0.123 ***
	(0.062)	(0.053)	(0.043)	(0.042)
Year fixed-effects	Yes	Yes	Yes	Yes
Zip code fixed-effects	Yes	No	No	No
County fixed-effects	No	Yes	No	No
State fixed-effects	No	No	Yes	No
Census division fixed-effects	No	No	No	Yes
Observations	262,395	262,395	262,395	262,395
Adjusted R^2	0.11	0.09	0.08	0.08

Firm-level Analysis: The Effect of Local Store Closures on Firm Performance

Our analysis so far has focused on store-level outcomes such as store closures or openings. While the fine resolution of store-level analysis enables us to better identify localized effects and control for unobserved localized geographic heterogeneity, it is not clear whether the localized effect aggregates up in a meaningful way to have an effect on firm-level outcomes. In this part of the paper, we investigate the overall effect of firm-level exposure to neighboring stores closing on firm-level measures of store performance and profitability.

We begin by aggregating-up store "exposure" by summing-up the overall number of store closures by liquidated national chains to which the individual stores of specific retailers are exposed. Table 8 presents summary statistics of store closures among the liquidated chains.

Table 8: Summary Statistics of Bankrupt Chain Store Closures

This table presents summary statistics of firm-level store closures by chains that went bankrupt the period.

Year	Mean	25th percentile	Median	75th percentile	Standard deviation
2005	48.0	21.0	33.0	90.0	36.0
2006	84.6	18.0	26.0	88.0	117.6
2007	40.0	3.5	26.0	76.5	48.9
2008	102.0	1.0	25.0	125.0	169.7
2009	498.0	44.0	316.0	1156.0	541.0

Using the Compustat Fundamental and Industry data, we construct two dependent variables: (1) the total number of store closures during the year; and (2) the annual percentage change in firm revenue. Table 9 reports the results from estimating the effect of exposure to a local store closure on total store closures by the firm. The sample includes 96 individual retailers over the years 2005-2010 with a total of 401 firm-year observations. All regressions are estimated with year fixed-effects and standard errors clustered at the firm level. As Table 9 demonstrates and consistent with the results in Table 6, exposure to local store closing aggregates up into a meaningful effect on store closures.

Table 9: Firm-Level OLS Regressions, Store Closures

***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Bankrupt stores					
Same address	14.170 ***				
	(3.516)				
Distance ≤ 50 meters		16.576 ***			
		(3.787)			
50 meters < distance ≤ 100 meters			21.427 ***		
			(5.326)		
100 meters < distance ≤ 250 meters				17.259 ***	
				(4.429)	
0 meters < distance ≤ 250 meters					13.749 ***
					(3.380)
Bankruptcy	72.667 ***	70.742 ***	60.589 ***	54.511 ***	59.887 ***
	(10.541)	(11.217)	(12.083)	(13.685)	(12.374)
Market-to-Book	-10.208 **	-10.988 **	-11.347 **	-12.256 **	-11.996 **
	-(2.250)	(4.714)	(4.788)	(4.976)	(4.771)
Profitability	-77.609 *	-68.192	-77.076 *	-67.544	-75.357 *
	(43.218)	(44.633)	(42.659)	(43.739)	(44.037)
Leverage	-7.378	-7.240	-11.769	-8.110	-8.536
	(27.600)	(28.529)	(27.082)	(28.667)	(27.674)
Liquidity	14.353	41.094	37.342	40.549	31.148
	(27.247)	(31.313)	(29.228)	(32.074)	(29.693)
Size	4.803	3.416	3.000	2.189	3.129
	(3.258)	(3.509)	(3.509)	(3.802)	(3.529)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Observations	401	401	401	401	401
Number of Firms	96	96	96	96	96
Adjusted R^2	0.20	0.18	0.19	0.17	0.18

Table 10 further investigates the aggregate effect of local store closures by conditioning the effect on the financial health of the firm. As demonstrated in the interaction term between the measures of store closures and firm profitability, the effect of local store closures is stronger for less profitable firms.

Table 10: Firm-Level OLS Regressions, Store Closures and Profitability

***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Bankrupt stores					
Same address	23.212 ***				
	(5.959)				
x Profitability	-50.693 ***				
	(19.240)				
Distance ≤ 50 meters		25.484 ***			
		(6.900)			
x Profitability		-51.449 ***			
		(23.039)			
50 meters < distance ≤ 100 meters			30.959 ***		
			(8.542)		
x Profitability			-58.499 **		
			(26.289)		
100 meters < distance ≤ 250 meters				22.958 ***	
				(6.569)	
x Profitability				36.617 *	
				(18.586)	
0 meters < distance ≤ 250 meters					18.979 ***
					(5.105)
x Profitability					-31.251 **
					14.611
Bankruptcy	82.392 ***	77.847 ***	61.184 ***	53.507 ***	59.319 ***
	(8.237)	(9.251)	(11.389)	(13.897)	(12.496)
Market-to-Book	-5.849	-8.673 *	-9.538 **	-10.636 **	-9.328 **
	(4.323)	(4.447)	(4.487)	(4.790)	(4.606)
Profitability	-14.646	-11.197	-13.507	-13.116	-14.384
	(41.906)	(46.455)	(46.040)	(47.276)	(47.490)
Leverage	-5.692	6.786	-11.327	-8.533	-8.416
	(26.434)	(27.652)	(26.236)	(28.105)	(26.983)
Liquidity	10.171	33.961	32.125	35.590	26.750
	(28.062)	(32.138)	(30.201)	(33.040)	(30.657)
Size	3.685	2.766	2.454	1.911	2.544
	(3.403)	(3.610)	(3.592)	(3.842)	(3.632)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Observations	401	401	401	401	401
Number of Firms	96	96	96	96	96
Adjusted R^2	0.23	0.20	0.21	0.19	0.20

Next, we analyze the effect of store closures on the annual percentage change in firm revenue. We regress the change in firm revenue on each of the measures of exposure to local store closures as well as on a dummy indicating whether the firm is in bankruptcy proceedings, and controls for market-to-book, profitability, leverage, liquidity and firm size. As Table 11 demonstrates, exposure to local store closures is linked to a decline in firm revenue, with coefficients that are between -0.037 (statistically significant at the two percent level) and -0.060 (statistically significant at the one percent level).

Table 11: Firm Revenue and Bankrupt Store Closures, Reduced Form OLS

***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Bankrupt stores					
Same address	-0.037 ***				
	(0.015)				
Distance ≤ 50 meters		-0.058 ***			
		(0.022)			
50 meters < distance ≤ 100 meters			-0.045 ***		
			(0.026)		
100 meters < distance ≤ 250 meters				-0.060 ***	
				(0.023)	
0 meters < distance ≤ 250 meters					-0.051 ***
					(0.018)
Bankruptcy	-0.053	-0.049	-0.015	0.008	-0.011
	(0.075)	(0.074)	(0.074)	(0.075)	(0.073)
Market-to-Book	0.148 *	0.147 *	0.148 *	0.147 *	0.149 *
	(0.089)	(0.088)	(0.090)	(0.089)	(0.089)
Profitability	0.039	0.087	0.073	0.099	0.107
	(0.386)	(0.365)	(0.379)	(0.371)	(0.374)
Leverage	-0.118	-0.11	-0.097	-0.098	-0.112
	(0.309)	(0.305)	(0.309)	(0.304)	(0.304)
Liquidity	0.309	0.259	0.246	0.278	0.305
	(0.256)	(0.249)	(0.251)	(0.249)	(0.259)
Size	0.063 **	0.068 **	0.064 **	0.071 **	0.070 **
	(0.028)	(0.029)	(0.029)	(0.030)	(0.029)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Observations	424	424	424	424	424
Number of Firms	107	107	107	107	107
Adjusted R^2	0.10	0.11	0.10	0.11	0.11

Conclusion

Our analysis shows that bankrupt firms impose negative externalities on non-bankrupt firms by weakening of the economics of agglomeration in retail centers. Store closures by national retailers, which are often anchor tenants in malls and shopping centers, lead to the reduced attractiveness of retail areas as customers prefer to shop in areas with full vacancy. This, in turn, leads to declines in demand for retail services in the vicinity of bankrupt stores, causing contagion from financially distressed firms to non-bankrupt firms.

Chapter 2: The Human Capital Costs of Financial Distress

Introduction

Do financial frictions affect firm-level employment decisions? This question has been asked with renewed interest and urgency following the 2007-2009 financial crisis. The crisis led to a sharp contraction in non-financial corporate lending, placing potentially severe external financing constraints on firms. At the same time, the labor market in the United States witnessed significant increases in unemployment: from a low of 4.4% in May 2007 to a high of 10.0% in October 2009. How are these outcomes related? A negative productivity shock in the real economy causes financial markets to weaken and employment to decline broadly. This may reflect an important link between financial markets and firm-level employment or it may not. In an attempt to understand this channel, I use the financial crisis as a shock to the supply of external financing and focus on financial frictions leading to unemployment.

This paper examines how external financing constraints affect both the quantity of labor a firm chooses to employ as well as the quality. I introduce new, hand-collected data on the occupations of workers affected in mass layoff instances in California between 2006 and 2011, which allows me to assess the quality of dismissed workers using proxy measures of human capital. An exploration of these relationships is key aspect of an understanding of firm behavior as well as variation in employment over the business cycles.

Theoretically, the availability of external financing should affect employment decisions for several reasons. I first examine the impact on the quantity of labor. External financing constraints may affect employment indirectly through an impact on the level of investment (labor and capital being complements in the production function). In the face of high external finance premiums, employment will shrink alongside reductions in capital expenditures. Alternatively, in the context of liquidity constraints, payments to labor may exceed cash flow generation. Firms that finance labor activity using working capital will be forced to reduce payroll costs as working capital deteriorates.

Finally, particularly in crisis environments, firms may reduce employment as a means to preemptively reduce their dependence on external financing from unstable or weak banks.

Reducing labor may be particularly attractive to firms if capital is fixed or if adjustment costs are large. But labor theory tells us that layoffs may not be costless, as they may destroy worker-firm match-specific capital. I next examine the impact of external financing constraints on the quality of labor (i.e. the degree of human capital) affected in mass layoff instances. Theoretical predictions from labor economics hold that, given a layoff decision, firms will sort workers in inverse order of firm-specific human capital and begin dismissing workers at the bottom. This is driven by the result that workers with more firm-specific skills contribute more to a firm's profitability. Though a more cursory explanation, this behavior is also consistent with a "last in, first out" pecking order. I explore how this theoretical pecking order is affected by financing constraints.

These questions have remained unanswered, as we have lacked data on firm-level employment outcomes that includes occupational detail. I introduce new, hand-collected data on mass layoffs in California between 2006 and 2011, which includes detail on the occupations affected in each layoff instance. This allows me to investigate firms' propensity for mass layoffs during the financial crisis as well as their human capital choices in the face of a mass layoff.

Testing for a causal effect of financial constraints on firm employment decisions is complicated by identification concerns. In particular, variables measuring firms' financial health are also correlated with their demand for labor. To address this, I use the onset of the 2007-2009 financial crisis provides an identification tool. The crisis led to a significant contraction in non-financial corporate lending, representing potentially severe external financing constraints for firms. Firms that faced the need to rollover existing long-term corporate debt obligations at the onset of the crisis encountered sudden and unexpected difficulty. This contraction in lending arguably provides a shock to the supply of external financing that is unrelated to the strength of corporate business fundamentals. To isolate the effect of financial constraints on employment decisions, I exploit firm-level variation in the amount of debt coming due at the onset of the 2007-2009 financial crisis, following Almeida et al. (2012). I examine whether firms with large fractions of long-term debt maturing at the onset of the crisis adjust their employment behavior in ways that are more pronounced than otherwise similar firms that did not face a need to refinance their long-term debt at that time. To the extent that these refinancing effects are large, they imply that the terms of financial contracting (i.e. contract maturity) affect employment outcomes.

Long-term debt is typically publicly-held and difficult to renegotiate on short notice (Bolton and Scharfstein (1996)). Because cumulative, hard-to-reverse decisions made several years in the past affect current long-term debt maturity structures, it is hard to argue that firms are at their optimal debt maturities at all times. Therefore, whether a firm had to refinance a significant portion of its long-term debt right after August 2007 is plausibly unrelated to the firm's operating performance. I exploit this maturity-structure discontinuity, using the portion of long-term debt pre-set to mature right after fall of 2007 to gauge how firms' employment decisions are affected by financing constraints. While my analysis treats variation in the fraction of long-term debt that comes due right after August 2007 as exogenous to firm outcomes, it is plausible that other sources of firm heterogeneity could underlie these relationships. To alleviate this concern, I use a difference-in-difference matching estimation approach that incorporates observable firm characteristics and accounts for unobservable, idiosyncratic firm effects. The tests match firms that should be more susceptible to the negative effects of refinancing constraints (firms that had a large fraction of their long-term debt coming due when the crisis hit) with firms that did not face a need to rollover their debt, allowing me to compare otherwise similar firms that differ only in their profiles of long-term debt maturity. The tests account for time-invariant heterogeneity by comparing within-firm changes in outcome variables from the period that precedes the 2007 credit shock to the period that follows.

My findings are as follows. I first verify pronounced cross-firm variation in long-term debt maturity structure at the onset of the 2007 crisis. Cross-sectional variation in long-term debt maturity is persistent over time, with similar dispersion patterns observed in the years preceding the crisis. I isolate a sizable pool of firms with a large fraction of long-term debt maturing right after the crisis (financially constrained firms) that are virtually identical to other firms whose debt happens to mature in later years (financially unconstrained firms). I show that these two groups of firms are similar across all characteristics except for the share of long-term debt due at the onset of the crisis.

I then show that whether a firm faced financing constraints due to impending debt maturity has important consequences for post-crisis employment outcomes. While the growth rate of total employment declined for both financially constrained and unconstrained firms, it declined 5.07% more among the financially constrained. In order to verify that the employment behavior differences between the two groups are particular to an environment in which credit is tight, I replicate my experiment over a number of non-crisis years. In non-crisis years, debt coming due is unlikely to induce financial constraint; consistent with this, debt maturity leads to layoffs only for firms whose debt comes due in the 2007 environment of tight credit.

Having shown that financial constraints cause firms to reduce employment, I then turn to understand how adjustments are made. In particular, do firms lay off workers or simply slow hiring? Using data on layoff instances, I repeat the analysis on changes in total employment using an indicator variable of firm-level mass layoffs. I find that the likelihood of a mass layoff increased 6.77% between 2007 and 2008 among financially constrained firms, though it barely changed at all among unconstrained firms. Overall, firms facing external financing constraints were 6.89% more likely to make a mass layoff than otherwise similar but unconstrained firms. The effect of financing constraints on the likelihood of mass layoff instances is insignificant in subsequent years.

Conditional on a mass layoff, do constrained firms also lay off more workers? Do financing constraints influence the degree of human capital of workers affected in a layoff event? I restrict my attention to the subset of firms that made a mass layoff and examine the quality of layoff instances using proxies for human capital by occupation. I find that external financing constraints have important consequences for the degree of human capital laid off. Financially constrained firms laid off workers with higher average annual salaries following the onset of the crisis. The difference-in-difference estimate implies that financially constrained firms laid off workers earning $12,617 more relative to financially unconstrained firms and relative to the pre-crisis period. I do not find a significant difference in the average annual salary of workers laid off by financially unconstrained firms and the difference between salaries of laid off workers across constrained and unconstrained firms does not persist in later years. These results are robust to alternative measures of human capital (educational attainment, work experience and on-the-job training).

From measures of salary, educational attainment, work experience and on-the-job training, it appears that financially constrained firms laid off higher human capital workers following the onset of the crisis relative to unconstrained firms. The fact that the outcomes tend in the same direction adds credibility to the result. Overall, the results point to destruction of the firm-worker match in a sort of human capital fire sale brought on by financial constraint. This may reflect a strategic decision to lay off relatively more expensive employees in a struggle to conserve cash holdings.

In a follow-up exercise, I note that many firms in my dataset laid off workers in multiple instances. I look into how the average level of human capital of laid off workers varies within firms by layoff instance. Financially healthy firms laid off workers in order of human capital, letting go of low human capital workers first and high human capital workers later. However, financially distressed firms behave in the opposite manner, laying off high human capital workers in early layoff instances and deescalating to low human capital workers later on. This result contradicts theoretical predictions concerning firms' investment in human capital.

Finally, I consider stock market reactions to mass layoff announcements. A valuation-based understanding of layoff announcements should provide additional context in which to interpret the results described above. I find 3-day cumulative abnormal returns to be slightly negative following a mass layoff announcement. This is consistent with Farber and Hallock (2008), who document negative returns among firms with layoff announcements reported in the Wall Street Journal between 1970 and 1999. In addition, I find a negative relationship between 3-day cumulative abnormal returns and the degree of human capital laid off. These results indicate that valuations decline upon destruction of the value created in worker-employer relationships. Moreover, the decline is particularly pronounced for high human capital worker-employer relationships, suggesting that the market understands the cost of these types of layoffs.

Theoretical Framework

The link between financial constraint and firm employment decisions is analogous to the link between financial constraint and firm expenditures, a well-examined question in corporate finance. I begin by describing that literature in order to highlight useful parallels and distinctions for thinking about employment. Modigliani and Miller (1958) predict that, in perfect markets, a firm's financial structure will not affect its market value. Thus, real firm decisions, motivated by the maximization of shareholders' claims, are independent of financial factors. Applied to firms' capital investment, this prediction provided a foundation for the neoclassical theory of investment (Hall and Jorgenson (1967), Jorgenson and Siebert (1968) and Elliot (1973)), in which a firm's inter-temporal optimization problem could be solved without reference to a firm's financial condition.

Stepping away from a Modigliani-Miller world, the cost of external finance may exceed the cost of internal finance (due to information asymmetries, agency costs, incomplete contracting or the tax system), in which case the two are not perfect substitutes. A central prediction is that, where the cost of external finance exceeds the cost of internal finance (also known as a positive external finance premium), firms will respond by decreasing capital expenditures (Fazzari, Hubbard, and Petersen (1988, 2000), Kaplan and Zingales (1997, 2000), Lamont (1997) and Rauh (2006)). While this literature is interested the impact of financial constraints on real outcomes in general, it has little to say about labor outcomes. Yet labor is a very large share of firm expenditure. This paper fits into a burgeoning literature that is concerned instead with the impact of financial constraints on firm employment.

Theoretically, the cost and availability of external debt financing should affect employment decisions for several reasons. Labor and capital being complements in the production function, the availability of external finance may affect employment indirectly through its impact on the level of investment. In the face of high external finance premiums, employment will shrink naturally alongside reductions in capital expenditures. Alternatively, in the context of liquidity constraints, payments to labor may exceed cash flow generation. Firms that finance labor activity using working capital will be forced to reduce payroll costs as working capital deteriorates (Greenwald and Stiglitz (1988)). Finally, firms may also reduce employment in a push to preemptively reduce their dependence on debt financing from unstable or weak banks. The link between financial constraints and employment is also explored in Benmelech, Bergman and Seru (2011), which uses a set of quasi-experiments to suggest that financial constraints and the availability of credit play an important role in determining firm employment levels as well as aggregate unemployment. This paper uses observations on layoff instances in addition to data on total employment by firm, as well as information on the human capital of affected workers.

Concerning the human capital of affected workers, I am interested in whether financial constraints also impact employment quality, i.e. the degree of human capital that a firm chooses to employ. Economic theory on firm investment in human capital is rooted in the classic papers of Becker (1962) and Oi (1962). They wrote about the distinction between general and firm-specific training of workers. By definition, firm-specific knowledge is useful only in the firms providing it, whereas general knowledge is translatable to other firms. Accordingly, firms are predicted to pay for specific knowledge but leave the costs of general training to be borne by the workers. This helps explain why workers with highly firm-specific skills are less likely to quit their jobs. It also suggests that they are the last to be laid off during business downturns; we should expect layoffs to affect workers with high degrees of firm-specific human capital disproportionately less and workers with low degrees of firm-specific human capital disproportionately more. What about general human capital? More recent papers have broadened the theories laid down by Becker (1962) and Oi (1962) by weakening the certain assumptions, such as that of perfectly competitive labor markets. Acemoglu and Pischke (1998, 1999)[5] and Kessler and Lülfesmann (2006) suggest that firms have an interest in general human capital in addition to specific and are indeed willing to pay for it. This may be due to labor market imperfections, to firms' desires to gather superior information on workers' abilities, or complementarities between specific and general training. While the explanations are varied, there is strong evidence that firms are invested in levels of general human capital in addition to levels of specific human capital. This leads to a view that layoffs affect workers with high degrees of general human capital disproportionately less as well, and workers with low degrees of general human capital disproportionately more.

The layoff data that I rely on reflects mass layoff instances in particular. Employers use mass layoffs for a host of strategic reasons: change of location, outsourcing of labor, productivity gains that render some functions superfluous or the elimination of an unviable business line. In these examples, mass layoffs are a planned, strategic management choice and may be unrelated to

[5] Acemoglu and Pischke (1998) note the familiar example of employers sending workers to college, certificate or MBA programs offering general skills.

financing constraints. Yet they are also commonly related to financial distress. Abowd, McKinney and Vilhuber (2005) relate mass layoff events to firm closures, finding that mass layoffs increase the probability of a closure. They also find that layoffs occur disproportionately more often in firms that employ workers in the lowest quartile of the human capital distribution and disproportionately less often in firms that employ workers in the highest quartile of the human capital distribution. This makes sense: firms are more willing to lay off employees that can be easily trained. Conditioning their analysis the level of human capital within each firm, they find that firms that employ a disproportionate fraction of workers in the highest quartile of the human capital distribution are less likely to close even given a layoff event. High human capital appears to protect the firm from closure.

Research on the effects of layoffs on short-run stock prices is extensive. The key paper on the topic is Farber and Hallock (2008), which uses an event study methodology to analyze the stock price reactions to 5,353 Fortune 500 company layoff announcements collected from the *Wall Street Journal* from 1970-2007. The paper finds three-day cumulative abnormal returns surrounding the publication of layoff events to be negative (and gradually less negative over time). The authors also analyze the stock price reaction conditional on the reported reasons for the layoffs. They find positive cumulative abnormal returns for reasons such as "reorganization" and "plant closing" but negative cumulative abnormal returns for "demand slump" and "cost." The signs of these reactions make good sense. Reorganizations and plant closings are byproducts of strategic change within the company. Layoffs attributed to these reasons are more likely to be seen as management's good stewardship, causing stock prices to rise. The opposite is true for layoffs attributed to a demand slump or the need for cost cutting: as symptoms of poor stewardship, it is intuitive that these layoff events would prompt a fall in stock prices. They find negative returns to be largely associated with demand slumps yet, the financial crisis having induced an economy-wide demand slump, layoffs motivated by reduced demand may have been assessed differently between 2007 and 2009. It may also have been the case that firms had excess labor leading up to the crisis, in which case mass layoffs may have enhanced value.

My results complement research documenting that layoffs are more prevalent among financially constrained firms, whose management faces greater pressure to reorganize (Denis and Kruse (2000), Kahl (2002) and Powell and Yawson (2009)). The paper also adds to a large literature documenting layoff characteristics (Itkin and Salmon (2011), Guthrie and Datta (2008), Pagano and Volpin (2005), and Cappelli (2000)).

Data

This paper introduces a new, hand-collected data set on firm-level mass layoffs in California from 2006-2011. The dataset is built around firm-level mass layoff instances. Since my analysis also requires additional worker and firm characteristics, I combine the following four datasets into one: (1) firm-level mass layoff data available as a result of the WARN Act; (2) proxies of human capital by occupation (salary, educational attainment, work experience, and on-the-job training) from the Bureau of Labor Statistics (BLS); (3) quarterly and annual firm fundamentals as well as credit ratings from Compustat; and (4) stock price and market return data from CRSP. The final, combine dataset consists of 412 unique, public firms having made 824 mass layoffs in California between 2006 and 2011. This section describes each source, data selection and variable construction.

Firm-Level Mass Layoff Data

Firm-level data on mass layoffs is available as a result of the Worker Adjustment and Retraining Notification (WARN) Act,[6] passed federally in 1989. The WARN Act requires firms with more than 100 full-time employees to provide 60-day advanced notice of impending mass layoff events, defined by the BLS as affecting 50 or more employees of a single company in a given location. Notice must

[6] I compiled the data from over 3,000 PDF pages of notices available to the public from the State of California's Employment Development Department: http://www.edd.ca.gov/jobs_and_training/layoff_services_warn.htm.

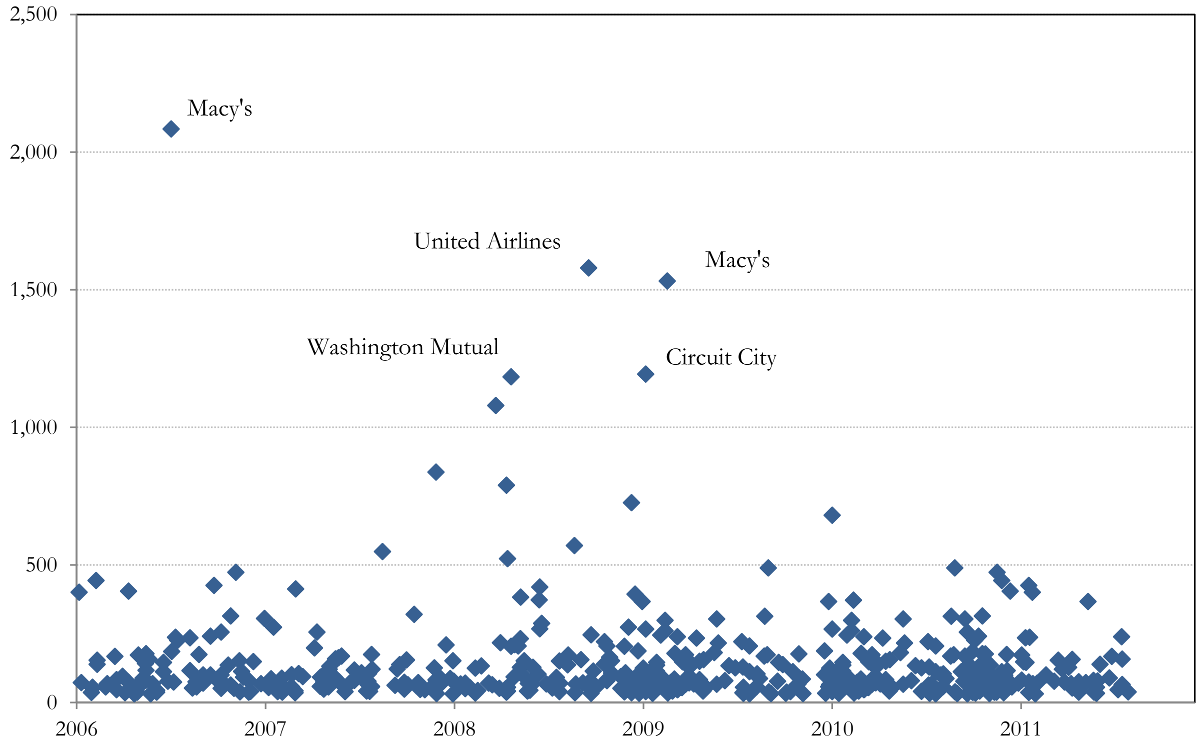
be given in writing to: (1) the employees' representative or, if there is no representative, to each affected employee; (2) the state dislocated worker unit; and (3) the local government where the plant is located.

Implementation of the WARN Act having been left to states, the availability of WARN data varies widely. Compliance with the Act, the variables collected, the time span over which they have been collected, as well as public access to the records, vary by state. The non-standard nature of the reporting makes it difficult to imagine a national dataset. Many states would be missing, there would be few data fields in common, and the time series would be short. As a result, I have chosen to focus on a single state, California. In addition to being a large economy, California has enforced thorough WARN reporting and has made the records relatively easily accessible. It is also the only state to require firms to report the occupations affected in a mass layoff, which is important to my analysis. Californian WARN notices require the following information: company name; address of layoff location; layoff date; date notice received; number of employees affected; layoff or closure; severance; union representation; bumping rights; and occupations of affected employees. In addition, California defines a mass layoff more narrowly, as affected 35 or more employees. To my knowledge, this research is the first to describe and analyze firm-level layoffs beyond a case study of a single firm.

Figure 1 presents a geographic scatter plot of all mass layoff instances in California between 2006 and 2011. Mass layoff instances are largely clustered in the urban areas surrounding San Francisco and Los Angeles, corresponding to the locations of most large firms, retail stores, and production facilities. Figure 2 presents a scatter plot of layoff instances over this period. The raw data consists of 4,335 layoff events among 1,274 unique public and private firms, affecting a total of 260,100 workers. The average layoff event in this period affected 110 workers. Several major layoff events stand out. The largest and third largest layoff events belong to Macy's, which laid off 2,053 workers on September 1st, 2006 and 1,501 workers on May 1st, 2009.

Figure 1: Geographic Dispersion of Mass Layoffs in California, 2006-2011

Figure 2: Number of Workers Affected by Mass Layoffs in California



The second largest layoff event belongs to United Airlines, which laid off 1,549 workers on October 5th, 2008. The fourth largest layoff belongs to Circuit City, which laid off 1,163 workers on March 21st, 2009. The fifth largest layoff belongs to Washington Mutual, which laid off 1,153 workers on June 30th, 2008.

I compared the WARN series to both initial unemployment claims from the BLS and an estimate of mass layoffs derived from BLS data in order to get a sense for the completeness of the WARN data. Initial claims are only a partial description of layoffs in California, as not all those laid off apply for unemployment assistance. Nevertheless, initial claims offer a more complete picture of layoffs than the WARN data, as an unemployment assistance claim can be initiated by any laid off worker, not just those affected by a mass layoff. I find that the WARN data represents approximately 20% of initial claims. In another attempt to assess the completeness of the WARN data, I estimate the minimum amount of mass layoffs in California using Mass Layoff Statistics

(MLS) from the BLS. The MLS program does not report the number of employees affected by mass layoffs, but it does report the number of monthly mass layoff incidents in the state. California defines a mass layoff as a layoff incident affecting at least 35 workers. Thus, I assume that a minimum of 35 workers are affected in each mass layoff incident and simply multiply the number of mass layoff events by 35 in order to arrive at a minimum estimate. I find that the WARN data represents 60% of estimated mass layoffs. This leads me to believe that some firms are simply not reporting mass layoff events as they are required to by state and federal law. This is unsurprising, as there is slight or no enforcement of the WARN Act in California. Non-reporting firms are likely to be less well-run administratively rather than intentionally flouting the state disclosure requirement; I do not believe that the omission of these firms biases the data in a predictable direction.

Table 12 presents a tabulation of mass layoff events by industry and Table 13 tabulates the employers having fired the greatest numbers of workers. Financial firms (including Wells Fargo, Washington Mutual, Fleetwood, Indymac, Citigroup) made a large number of mass layoffs, as did major retail firms (including Macy's, Mervyn's, Circuit City, Target, JC Penney's). The airline and aerospace industry (including United, American, ATA, Boeing), persistently beleaguered, cut many jobs as well. The mix of occupations affected in each mass layoff depends to some extent on the firm. For example, the WARN data reveals that aerospace engineers and flight attendants were laid off by United Airlines, whereas marketing managers and sales personnel were laid off by Macy's. However, each mass layoff notice pertains to a variety of occupations and those occupations tend to be repeated among firms within the same industry.

Table 12: Layoff Firms by Industry

Fama-French 12 Industry Classifications	# Firms
Finance	357
Business Equipment	165
Other	145
Healthcare, Medical Equipment, and Drugs	136
Wholesale, Retail	111
Manufacturing	107
Utilities	60
Consumer Non-Durables	58
Telephone and Television	45
Oil, Gas, and Coal Extraction and Products	36
Consumer Durables	29
Chemicals and Allied Products	26
	1,274

Table 13: Layoffs by Employers Having Laid Off the Most Workers

Company	# of Employees Affected	Company	# of Employees Affected
Macy's	3,554	JC Penney	337
United Airlines	2,095	Owens Corning	328
Circuit City	1,526	Medtronic	323
General Electric	1,319	Conagra Foods	322
Intel	1,292	Marriot	311
Target	1,161	Siemens	311
Washington Mutual	1,153	Cardinal Health	291
Boeing	1,069	TTM Technologies	283
Applebee's	1,049	Cisco Systems	275
American Airlines	971	Zebra Technologies	268
AT&T	949	Adobe Systems	263
Abbott Vascular	867	Smurfit Stone	261
KLA Tencor	770	Electronic Arts	255
Fleetwood	729	Quiksilver	244
Citigroup	678	Xyratex International	243
Intuit	638	Callaway Golf	240
Lockheed Martin	489	Hewlett Packard	237
Oracle	413	Albertson's	231
Technicolor Home Entertainment	402	Northrop Grunman	230
Wells Fargo	376	JP Morgan Chase	230

It is common for firms to engage in multiple rounds of mass layoffs as opposed to all at once; sometimes the layoff events are separated by years, sometimes merely by several weeks. Thus, firms behave as though WARN notice filings carry either no market signal or at least not a negative market signal. It may be that press releases lead the WARN announcements, in effect nullifying the information that they contain for financial markets. Multiple mass layoffs may be a sign of ongoing financial constraint.

Constructing Proxies for Human Capital By Occupation From the BLS

In order to construct measured of human capital by occupation, the job titles reported in the WARN data had to be unified by occupation. I unified WARN job titles by hand using the standard occupation classification (SOC) system available from the BLS. Once WARN job titles were matched to SOC occupations they were also linked to SOC codes, which allowed me to connect to other occupational data tracked by the BLS. This data is the basis of four human capital proxies: annual salary, educational attainment, work experience and on-the-job training.

Annual salary is available by occupation (840 unique occupation classifications) and by metropolitan statistical area (24 unique areas within California). To take an example, this allows me to estimate that a typical chemical engineer in the San Diego-Carlsbad-San Marcos area earns an annual salary of $86,490. In the absence of a direct measure of worker skill, the literature has commonly used wages as a proxy. Examples include Bernard and Jensen (2002) and Dunne and Roberts (1990), who consider the determinants of wages and the effects of wages on plant closures and Carneiro and Portugal (2003), who consider the link between wages and displacement events.

Next, SOC codes link to estimates of educational attainment, work experience and on-the-job training for each occupation. Occupations receive designations in three categories:

1) Entry-level education: doctoral/professional degree; master's; bachelor's; associate's; postsecondary; some college; high school; less than high school

2) Related work experience: > 5 years; 1-5 years; < 1 year; none

3) On-the-job training: internship; apprenticeship; long-term (> 1 year); moderate (1-12 months); short-term (< 1 month); none

To take an example, a typical judge has a doctoral or professional degree, more than 5 years of work experience and short-term on-the-job training. Layoffs in California between 2006 and 2011 are summarized according to these four human capital proxies in Table 14.

A worker's human capital can be thought of in two pieces: firm-specific and general. The proxies described above are each indicators of a worker's general human capital. Ideally, I would have data on firm-level investment in specific skills, i.e. those that do not easily translate to other firms or context, by occupation. Tenure at a firm would be a rough but reasonable proxy, as firm-specific knowledge naturally increases with tenure. However, lacking data on tenure or potential other proxies, I use the four general human capital proxies as though they are representative of firm-specific human capital and interpret the results with this caveat.

Table 14: Measures of the Human Capital of Laid Off Workers

This table summarizes the human capital of workers laid off in mass layoff instances in California between 2006 and 2011 using the following four proxies available from the BLS: annual average salary by occupation, estimated educational attainment by occupation, recommended related work experience and estimated on-the-job training. A total of 260,100 workers were affected in mass layoffs over this period. The panels of the table shows the distribution of layoffs by proxy.

Average Annual Salary	# Laid Off	% of Layoffs
> $150,000	24,267	9%
$125,000 - $150,000	19,097	7%
$100,000 - $125,000	19,305	7%
$75,000 - $100,000	63,956	25%
$50,000 - $75,000	54,959	21%
$25,000 - $50,000	29,557	11%
< $25,000	48,959	19%
Total	260,100	

Educational Attainment	# Laid Off	% of Total Laid Off
Doctoral/Professional Degree	4,446	2%
Master's Degree	16,065	6%
Bachelor's Degree	50,124	19%
Associate's Degree	24,650	9%
Post-Secondary Vocational Award	13,305	5%
Some College	41,810	16%
High School	52,628	20%
Less than High School	57,072	22%
Total	260,100	

Related Work Experience	# Laid Off	% of Total Laid Off
> 5 Years	66,843	26%
1-5 Years	103,550	40%
< 1 Year	35,067	13%
None	54,640	21%
Total	260,100	

On-the-Job Training	# Laid Off	% of Total Laid Off
> 1 year	61,126	24%
1-12 Months	101,179	39%
< 1 Month	42,193	16%
None	55,601	21%
Total	260,100	

Compustat Annual Fundamental Data

I consider the entire universe of firms from the Compustat Annual and Quarterly Fundamental Files between 2000 and 2011. In addition to balance sheet and income statement information, Compustat also reports the number of workers employed by a firm. The main independent variables are size (represented as the log value of total assets), Tobin's Q (proxied by the market-to-book ratio), cash flow, cash balance, and long-term debt normalized by total assets. Variable definitions and constructions are detailed in the Appendix. Finally, I use four-digit SIC codes in order to map each firm's industry into Fama-French 12 and Fama-French 48 industry classifications.

CRSP Stock Price and Market Return Data

I use value-weighted return and market return data from the Center for Research in Security Prices (CRSP) to calculate 3-day cumulative abnormal returns for each firm surrounding each layoff event. The CRSP data was merged using CUSIP and stock tickers.

The 2007 Credit Shock and the Maturity Structure of Corporate Debt

I begin my overview of the credit crisis by describing changes in 3-month LIBOR and commercial paper rates – both common sources of short-term financing – in August 2007.[7] Spreads between LIBOR and commercial paper and comparable-maturity Treasuries were low in the period between 2001 and the early part of 2007 (around 0.5%) but spiked in August 2007 (around 1.5%). The re-pricing of credit instruments spread from short-term bank financing to longer-term instruments quickly, highlighting the interdependence of financial market segments. Current research on the crisis suggests that spreads on long-term corporate bonds increased sharply. Adrian, Colla and Shin (2012) find that spreads relative to comparable-maturity Treasuries tripled during the financial crisis, from 156 basis points in the second quarter of 2007 to 436 basis points in the second quarter of 2009. This evidence supports the conjecture that there was a substantial increase in the

[7] I defer to Gorton (2008) and Brunnermeir (2009) for broader summaries of the roots of the crisis.

cost of short- and long-term bond financing. This environment of tight corporate credit provides a unique opportunity to identify the effects of supply contractions on corporate policies.

My identification strategy also requires variation in long-term debt maturity across firms. In particular, it relies on an adequate group of firms with long-term debt maturing right after the onset of the crisis. One might expect firms to have well-diversified maturity structures, protecting against the need to repay or refinance significant amounts of debt in any particular year; if true, this would limit the effectiveness of the proposed strategy. Fortunately, a literature on capital market frictions outlines evidence that it is difficult for firms to maintain their optimal capital structures.[8]

Almeida et al. (2012) investigated the distribution of debt maturities for their Compustat sample of firms. For each firm in the third quarter of 2007, they collected information on the amount of long-term debt maturing in the subsequent five years and report these amounts as a fraction of total long-term debt (between 0% and 100%). They find that while a significant number of firms have long-term debt maturing largely in 2008 (some firms with nearly 100% of their long-term debt maturing that year), many firms do not have any significant amount of long-term debt maturing in 2008. Other years exhibit similar variation. Debt maturity commonly concentrates in a particular year, but not necessarily in 2008. Further, the distributions of long-term debt maturing in the individual years beyond 2008 (2009 through 2012) look fairly similar to the distribution of long-term debt maturing in 2008. This suggests that firms may not always try to renegotiate in advance to prolong debt maturities. They also examine the distributions of debt maturities in years prior to 2007 and find that they look very similar to years following 2008.

Empirical Design

This section describes the basic empirical design, including the matching methodology to construct a comparison group and difference-in-difference regression specifications. My empirical

[8] This can be thought of as due to transaction costs, as in Fischer, Heinkel and Zechner (1989), or due to market-timing strategies, as in Baker and Wurgler (2002) and Welch (2004).

strategy uses variation in long-term debt maturity at the onset of the 2007 crisis as a tool to identify the effect of credit supply shocks on corporate policies. In a frictionless capital markets, debt maturity is irrelevant. Firms can always refinance and re-contract their way around the potential effects of a balloon debt payment. The 2007 crisis is a unique context because financial markets contain more friction in a crisis environment. Maturing debt was not as easy to rollover and, at the same time, firms found it difficult to substitute across alternative funding sources. As a result, firms that had large portions of debt maturing at the onset of the 2007 crisis may be expected to face tighter financing constraints than firms that did not have a large portion of debt coming due.

Matching Methodology

I want to test whether the employment decisions of firms needing to rollover their long-term debt obligations at the onset of the credit crisis differed from those of firms that did not face such a need. My identification strategy resembles an experiment: the firm's long-term debt maturity structure and developments in the financial markets coincide such that the firm needs to refinance a large fraction of its debt in the midst of a credit contraction. If debt maturity was randomly assigned across firms, then it would suffice to compare the outcomes of firms that had significant debt maturing around the time of the crisis with firms whose debt happened to mature at a later date. However, the data in this study is non-experimental. The challenge is to gauge firms' outcomes had they not been caught between a credit crisis and the need to refinance their debt. One way to tackle this issue is to estimate differences between plausibly counterfactual outcomes and those that are observed in the data. Under this approach, a standard method is to use a parametric regression where the group of interest is identified by a dummy variable. Outcome differences are then estimated by the coefficient on the group dummy.

This strategy is closely related to the design-based test described by Angrist and Pischke (2010). Within the natural experiment framework, I add the use of matching estimators, which aim to isolate "treated" observations (firms with debt maturing during the crisis). Next, from the population of "non-treated" observations, I look for control observations that best match the treated ones according to a set of firm characteristics. In this framework, the set of counterfactuals are restricted to the matched controls. In other words, it is assumed that in the absence of the treatment, the treated group would have behaved similarly to the control group. The matches are made so as to ensure that treated and control observations have identical distributions along each and every one of the firm characteristic covariates chosen (firm size, profitability, leverage and credit rating). Inferences about the treatment of interest (refinancing constraints) are based on differences in the post-treatment outcomes of treated and control groups. I rely on the Abadie and Imbens (2006) estimator, as implemented by Abadie, Drukker, Herr, and Imbens (2004). The Abadie-Imbens matching estimator minimizes the distance (i.e., the Mahalanobis distance) between a vector of observed covariates across treated and non-treated firms, finding controls based on matches for which the distance between vectors is smallest. I select one matched control for each treated firm. The estimator produces heteroskedastic-robust standard errors.

Matching aims to account for variables that may influence the selection into treatment and observed outcomes. The outcome variables here relate to employment. It is important to include only covariates for which one could make a reasonable case for simultaneity in the treatment—outcome relation. Categorical variables include firms' industrial classification codes (Fama-French SIC-12 and SIC-48 classification codes) and the credit rating of public bonds. Non-categorical variables include size (the log of total assets), Q, cash flow, cash balance and the ratio of long-term debt to total assets. It is commonly accepted that these covariates capture much of the otherwise unobserved firm heterogeneity. The estimations implicitly account for all possible interactions between the included covariates. I estimate Abadie-Imbens' average effect of the treatment on the treated (ATT) and then model the outcomes in differenced form using difference-in-differences estimations.

Difference-in-Difference Specifications

I compare changes in employment behavior between financially constrained and unconstrained firms (denoted $Constrained$ and $Unconstrained$) and before and after the onset of the financial crisis (denoted Pre and $Post$). The logic is that employment decisions may be different preceding and following the crisis, in which case the inferences may be biased by uncontrolled firm-specific differences. I estimate the following difference-in-difference specification for each outcome variable:

$$y = \beta_0 + \beta_1 * Constrained + \delta_0 * Post + \delta_1 * Constrained \times Post + \varepsilon$$

The coefficient of interest is δ_1, the coefficient on the interaction term $Constrained \times Post$. In this equation, β_0 is the baseline average, β_1 represents the time trend in the financially constrained group, δ_0 represents the difference between financially constrained and unconstrained groups in the period before the crisis and δ_1 represents the difference in the changes over time. Assuming that both groups face the same credit conditions over time, this specification controls for a possible time trend, allowing me to isolate the impact of financial constraints on employment outcomes.

Some of the outcome variables that I consider, such as the mass layoff indicator as well as educational attainment, work experience and on-the-job training indicators, are binary. I estimate these outcome variables using a linear probability model rather than logit or probit regressions due to the difficulty comparing outcomes among groups in these models (see Norton and Ai (2003) and Norton, Wang, Ai (2004)). Angrist and Pischke (2009) show linear probability models to be good options for certain dependent variables. Given an interest in the average effect of some variable upon some outcome, Hellevik (2009) also makes a compelling case for choosing a linear probability model of over logit.

Results

This section first presents a comparison of financially constrained and unconstrained firms using summary statistics of main variables. I then present evidence on the employment effects of the 2007 credit crisis and evidence on the human capital effects of the 2007 credit crisis. Last, I present results on the stock market reaction following a mass layoff announcement.

Summary Statistics

Summary statistics of the main variables are presented in Table 15 for financially constrained and financially unconstrained firms (both full and matched samples) at the end of 2007. Recall that financially constrained firms are defined as those for which the percentage of long-term debt maturing within one year is greater than 20 percent, while unconstrained firms are those for which this percentage is less than or equal to 20 percent. The overall sample consists of 844 firms. The treated sample consists of 119, the non-treated of 725, and the control sample of 119 firms using one-to-one matching. Looking at differences between financially constrained firms and financially unconstrained firms in the full sample, I observe that financially constrained firms are smaller in size, have lower long-term leverage, high interest coverage ratios, higher KZ Index values, slightly higher Q values, as well as higher cash flow and higher cash balances.

These sample differences are not unexpected. The goal of matching techniques is to control for these distributional differences, as they may affect whether a firm becomes financially constrained as well as post-crisis outcomes. The set of unconstrained firms in the matched sample is a subset of unconstrained firms in the full sample, where matching is based on the following set of firm characteristics: size (log of total assets), market-to-book, cash flow, cash balance, long-term debt normalized by assets and Fama-French 12 industry indicators. This approach allows me to compare otherwise similar firms, with the only difference being the profile of their long-term debt maturity. Upon matching, I have 119 firms in the financially constrained group and 119 firms in the matched, financially unconstrained group. Importantly, I find no statistical differences between the main variables across the two groups after matching.

Table 15: A Comparison of Financially Constrained and Unconstrained Firm Characteristics

This table compares financially constrained and unconstrained groups (in the full sample and in the matched sample) across several dimensions at the end of 2007. Financially constrained firms are defined as those for which the percentage of long-term debt maturing within one year (DD1/DLTT) is greater than 20 percent; unconstrained firms are those for which this percentage is less than or equal to 20 percent. The set of unconstrained firms in the matched sample is a subset of unconstrained firms in the full sample. To construct it, I match unconstrained firms to constrained firms using the following set of firm characteristics: size (log of total assets), market-to-book, cash flow, cash balance, long-term debt normalized by assets and Fama-French 12 industry indicators. The overall sample consists of 844 firms. The treated sample consists of 119, the non-treated of 725, and the control sample of 119 firms using one-to-one matching. See Appendix for variable definitions.

	Financially	Full Sample		Matched Sample	
	Constrained	Unconstrained	Difference	Unconstrained	Difference
Total Assets ($mm)	$2,694	$10,438	-$7,744 ***	$2,577	$117
Total Revenue ($mm)	$251	$1,156	-$905 ***	$272	-$21
Total Employees	1,363	4,805	-3,442 ***	1,442	-79
Long-Term Leverage	$774	$3,712	-$2,938 ***	$548	$226
Profitability	0.029	0.026	0.003	0.024	0.005
Interest Coverage	21.257	5.000	4.421 ***	4.842	16.415
Kaplan-Zingales Index	1.277	0.877	0.400 **	1.540	-0.264
Market-to-Book	2.694	2.499	0.195 **	2.587	0.107
Cash Flow	0.128	0.088	0.039 ***	0.120	0.008
Cash Balance	0.150	0.103	0.047 *	0.132	0.019
Dividends	0.008	0.011	-0.003	0.013	-0.004
Debt Portion	0.370	0.484	-0.114 ***	0.320	0.050
% Changes					
Investment	-16.55%	5.59%	-22.14%	21.51%	-38.06%
Employment	7.36%	5.36%	2.00%	2.73%	4.64%
Capital Expenditure	27.76%	14.27%	13.485% ***	19.77%	7.98%
Number of Firms	119	725		119	

Employment Effects of the 2007 Credit Crisis

I examine the employment behavior of financially constrained and matched, financially unconstrained firms around the 2007 credit crisis. I first consider changes in total firm employment, seeking to understand whether reductions in employment during the crisis were more pronounced for financially constrained firms. In Table 16, the first row of Panel A shows that both financially constrained and unconstrained firms were growing total employment (7.03% for financially constrained firms versus 6.45% for financially unconstrained firms) between 2006 and 2007. The difference is economically and statistically insignificant after matching. Examining the differences in total employment changes between 2007 and 2008, I find that employment decisions of financially constrained firms differed from those of unconstrained firms. While average annual employment among financially constrained firms fell by 1.23%, average annual employment among financially unconstrained firms continued to grow by 3.08%. My estimates imply that annual changes in employment among financially constrained firms were reduced by -4.90% relative to financially unconstrained firms following the onset of the crisis. The Abadie-Imbens estimate of the difference-in-difference coefficient is -5.07%.

Panel B presents the difference-in-difference coefficient estimates and Abadie-Imbens estimates across non-crisis years (total employment changes from 2000-2001 through 2005-2006). My identification strategy argues that financial constraint is brought on by the perfect storm of debt coming due in a credit crisis. In non-crisis years, debt coming due is less likely to induce financial constraint. Consistent with this, I find that the effects of financing constraints due to impending debt maturity hold only for the 2007 environment of tight credit. Difference-in-difference estimates in non-crisis years are economically and statistically insignificant.

Table 16: Difference-in-Difference Comparisons of Total Employment Before and After the Onset of the 2007 Credit Crisis

This table reports evidence on how total firm employment was affected by the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) reduced annual employment by 5.07% more than otherwise similar firms whose debt was scheduled to mature after 2008 (unconstrained firms). Panel A compares changes in total employment between 2006 and 2007 to changes in total employment between 2007 and 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturing in that period. There are 119 financially constrained firms and 119 unconstrained firms. The set of unconstrained firms is constructed by matching on Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. Panel B compares annual changes in total employment between non-crisis years. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Percentage Changes in Total Annual Employment Before and After the Fall 2007 Credit Crisis			
	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007-2006	7.03% ***	6.45% ***	0.49%
	(0.03)	(0.02)	(0.04)
2008-2007	-1.23% ***	3.08% ***	-4.31% ***
	(0.00)	(0.00)	(0.01)
Difference (2008/07 - 2007/06)	-8.27% ***	-3.37%	-4.90% ***
	(0.02)	(0.03)	(0.02)
Matching Estimator (ATT)			-5.07% ***
			(0.03)

Panel B: Placebo Tests	Difference in Employment Changes Between Financially Constrained and	Matching Estimator (ATT)
2002/01 - 2001/00	0.28%	0.33%
	(0.03)	(0.03)
2003/02 - 2002/01	0.21%	0.30%
	(0.02)	(0.02)
2004/03 - 2003/02	0.28%	0.30%
	(0.03)	(0.04)
2005/04 - 2004/03	0.61%	0.60%
	(0.04)	(0.03)
2006/05 - 2005/04	0.38%	0.39%
	(0.02)	(0.03)
2007/06 - 2006/05	0.45%	0.48%
	(0.04)	(0.04)
2008/07 - 2007/06	-3.80% ***	-4.07% ***
	(0.04)	(0.04)

Having shown that financial constraints cause firms to reduce employment, I next turn to understand how adjustments are made. This analysis draws on data on mass layoff instances in California between 2006 and 2011, represented by an indicator variable equal to one in the event of a mass layoff and zero otherwise. I estimate a difference-in-difference regression of a mass layoff indicator variable using a linear probability model, which yields coefficients that describe a firm's mass layoff propensity. Panel A of Table 17 presents the main results. The set of financially constrained firms and the matched set of unconstrained firms both exhibited a mass layoff propensity of 1.69% preceding the crisis. The likelihood of a mass layoff among financially constrained firms increased dramatically between 2007 and 2008, reaching 8.46% following the onset of the crisis but barely changing for unconstrained firms. Looking at the difference-in-difference estimate, I find that firms facing external financing constraints were 6.50% more likely to make a mass layoff compared to otherwise similar but unconstrained firms. The Abadie-Imbens estimate of the difference-in-difference mass layoff likelihood is 6.89%.

Panel B of Table 17 contains difference-in-difference coefficient estimates and Abadie-Imbens estimates for subsequent years (from 2008-2009 through 2010-2011). I consider subsequent years rather than preceding non-crisis years for these placebo tests because my layoff indicator variable is available beginning in 2006. I find the effect of financing constraints on the likelihood of mass layoff instances to be insignificant in subsequent years, holding only for the 2007 period. It is somewhat surprising that the difference between mass layoff propensities does not last between 2008 and 2009. One might have expected mass layoffs to be prevalent among financially constrained firms in across these years as well, perhaps tapering off between 2009 and 2010. This indicates that the propensity for mass layoffs was concentrated early on in the crisis. Evidence that firms prefer to cluster the timing of layoff instances with firms in their industries seems to support this result (see Agarwal and Kolev (2012)).

Table 17: Difference-in-Difference Comparisons of Mass Layoff Propensity Before and After the Onset of the 2007 Credit Crisis

This table reports evidence on the likelihood of a mass layoff among financially constrained and unconstrained firms in the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) were 6.9% more likely to make a mass layoff than otherwise similar firms whose debt was scheduled to mature after 2008 (financially unconstrained firms). There is no significant difference in the mass layoff propensities of financially constrained and unconstrained firms in the years following 2008. The table presents the likelihood of a mass layoff in percentage points. Panel A compares the likelihood between 2006 and 2007 to the likelihood between 2007 and 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturiting. There are 119 financially constrained firms and 119 financially unconstrained firms. The set of unconstrained firms is constructed by propensity score matching on the following firm characteristics: Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. As a placebo test, Panel B compares the mass layoff propensities across years following 2008. The only significant difference in mass layoff propensities among financially constrained and unconstrained firms occured between 2007 and 2008. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Propensity for Mass Layoffs Before and After the Fall 2007 Credit Crisis

	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007	1.69% ***	1.69% ***	0.00%
	(0.003)	(0.002)	(0.002)
2008	8.46% ***	1.97% ***	6.49% ***
	(0.006)	(0.002)	(0.007)
Difference (2008 - 2007)	6.77% ***	0.27%	6.50% ***
	(0.005)	(0.002)	(0.006)
Matching Estimator (ATT)			6.89% ***
			(0.007)

Panel B: Placebo Tests	Difference in the Propensity for Mass Layoffs Between Financially	Matching Estimator (ATT)
2008 - 2007	6.50% ***	6.89% ***
	(0.006)	(0.007)
2009 - 2008	0.34%	0.36%
	(0.002)	(0.002)
2010 - 2009	0.27%	0.31%
	(0.003)	(0.004)
2011 - 2010	0.11%	0.13%
	(0.004)	(0.004)

Human Capital Effects of the 2007 Credit Crisis

Following evidence that external financing constraints affect the level of total employment, do financing constraints influence the degree of human capital of workers affected in a layoff event? This analysis draws on the subset of firms that made a mass layoff between 2006 and 2011, i.e. the intensive margin. I compare the degree of human capital of workers laid off among financially constrained firms to the degree of human capital of workers laid off among unconstrained firms, using annual average salary, educational attainment, work experience and on-the-job training as proxies.

Results for average annual salary are presented in Table 18. I find that financially constrained firms laid off workers with higher average annual salaries following the onset of the crisis. The first row of Panel A indicates that financially constrained firms laid off workers with an average annual salary of $66,151 preceding the crisis, while unconstrained firms laid off workers with an average annual salary of $63,357. Following the onset of the crisis, the average annual salary of workers affected in a mass layoff rose $13,175 to $77,326 (or nearly 20%). In contrast, the average annual salary of workers affected in a mass layoff by unconstrained firms rose less than 2%, from $63,357 to $64,488. The difference-in-difference estimate implies that financially constrained firms laid off workers earning $12,044 more relative to financially unconstrained firms and relative to the pre-crisis period. The Abadie-Imbens estimate of the difference-in-difference coefficient is $12,617. Panel B indicates that the difference between salaries of laid off workers across constrained and unconstrained firms does not persist in later years but is specific to the 2007-2008 period. Following the result that the propensity for mass layoffs was much reduced in subsequent periods, this result is unsurprising.

Table 18: Difference-in-Difference Comparisons of the Annual Salaries of Workers Affected by Mass Layoffs

This table reports evidence on the salaries of workers affected by mass layoff events in the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) laid off workers with higher average annual salaries of $12,617 compared to otherwise similar firms whose debt was scheduled to mature after 2008 (financially unconstrained firms). There is no significant difference in the salaries of laid off workers between financially constrained and unconstrained firms across non-crisis years. The table presents the average annual salaries of laid off workers in dollars. Panel A compares the salaries in 2007 to the salaries in 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturiting in that period. There are 68 financially constrained firms and 68 financially unconstrained firms. The set of unconstrained firms is constructed by matching on the following set of firm characteristics: Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. As a placebo test, Panel B compares annual average salaries of laid off workers for the years that follow. The only significant salary difference of laid off workers between financially constrained firms above financially unconstrained firms occured between 2007 and 2008. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Propensity for Mass Layoffs Before and After the Fall 2007 Credit Crisis

	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007	$64,151 ***	$63,357 ***	$794
	($3,705)	($3,354)	($487)
2008	$77,326 ***	$64,488 ***	$12,838 ***
	($3,893)	($3,875)	($572)
Difference (2008 - 2007)	$13,175 ***	$1,131	$12,044 ***
	($878)	($675)	($633)
Matching Estimator (ATT)			$12,617 ***
			($714)

Panel B: Placebo Tests	Difference in Salaries of Laid Off Workers Between Financially Constrained and Unconstrained Firms	Matching Estimator (ATT)
2008 - 2007	$12,044 ***	$12,617 ***
	($633)	($714)
2009 - 2008	$1,094	$1,102
	($669)	($714)
2010 - 2009	$955	$967
	($713)	($722)
2011 - 2010	$728	$773
	($624)	($660)

Similar results also hold for the other three proxies of human capital: educational attainment, work experience and on-the-job training. The results for educational attainment by occupation are presented in Table 19. I consider employment decisions affecting workers with high levels of educational attainment (having a bachelor's degree or greater). Looking at the first row of Panel A, I find that financially constrained firms laid off 10.15% of their share of highly educated workers in the pre-crisis period, compared to 9.82% for unconstrained firms. Following the onset of the crisis, this portion increases to 38.27% for financially constrained firms, or 28.12%. The increase in the portion of highly educated workers fired is economically and statistically insignificant among unconstrained firms. The difference-in-difference estimate implies that financially constrained firms laid off 27.90% more highly educated workers relative to financially unconstrained firms and relative to the pre-crisis period. The Abadie-Imbens estimate of the difference-in-difference coefficient is 28.25%. Panel B reports that this difference is significant for the comparison between 2007 and 2008, but not in subsequent years. These agree with the results on the average salary discussed above, as we expect salary and educational attainment to be highly correlated.

Table 19: Difference-in-Difference Comparisons of the Education Levels of Workers Affected by Mass Layoffs

This table reports evidence on the education levels of workers affected by mass layoff events in the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) laid off a greater portion of highly educated workers compared to otherwise similar firms whose debt was scheduled to mature after 2008 (financially unconstrained firms). The table summarizes indicator variables representing the fraction of mass layoffs that affected workers with higher education. Panel A compares the fraction of highly educated workers affected by mass layoffs in 2007 to that in 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturiting in that period. There are 68 financially constrained firms and 68 financially unconstrained firms. The set of unconstrained firms is constructed by matching on Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. As a placebo test, Panel B compares the fraction of highly educated workers affected by mass layoffs for the years that follow. The only significant difference between financially constrained firms above financially unconstrained firms occured between 2007 and 2008. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Fraction of Employees with Higher Education Affected in Mass Layoffs

	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007	10.15% ***	9.82% ***	0.33%
	(0.034)	(0.032)	(0.003)
2008	38.27% ***	10.04% ***	28.23% ***
	(0.032)	(0.020)	(0.027)
Difference (2008 - 2007)	28.12% ***	0.22%	27.90% ***
	(0.025)	(0.002)	(0.076)
Matching Estimator (ATT)			28.25% ***
			(0.060)

Panel B: Placebo Tests	Difference in the Fraction of Higher Education Mass Layoffs Between Financially Constrained and Unconstrained Firms	Matching Estimator (ATT)
2008 - 2007	27.90% ***	28.25% ***
	(0.076)	(0.060)
2009 - 2008	3.99%	4.05%
	(0.069)	(0.072)
2010 - 2009	3.46%	3.58%
	(0.062)	(0.065)
2011 - 2010	5.62%	4.77%
	(0.066)	(0.075)

The results for work experience by occupation are presented in Table 20. As with educational attainment, I consider employment decisions affecting workers with high levels of work experience (at least five years). Looking at the first row of Panel A, I find that financially constrained firms laid off 7.25% of their share of workers with substantial work experience in the pre-crisis period, compared to 7.03% for unconstrained firms. Following the onset of the crisis, this portion increases to 35.61% for financially constrained firms, or 28.36%. The increase in the portion of workers with substantial work experience is economically and statistically insignificant among unconstrained firms. The difference-in-difference estimate implies that financially constrained firms laid off 26.67% more highly experienced workers relative to financially unconstrained firms and relative to the pre-crisis period. The Abadie-Imbens estimate of the difference-in-difference coefficient is 26.14%. We see from Panel B that this difference is significant for the comparison between 2007 and 2008 but is insignificant in subsequent years.

Table 20: Difference-in-Difference Comparisons of Work Experience of Workers Affected by Mass Layoffs

This table reports evidence on the work experience of workers affected by mass layoff events in the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) laid off workers with greater work experience compared to otherwise similar firms whose debt was scheduled to mature after 2008 (financially unconstrained firms). The table summarizes indicator variables representing the fraction of mass layoffs that affected workers with at least five years of work experience. Panel A compares the fraction of workers with high work experience affected by mass layoffs in 2007 to that in 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturiting in that period. There are 68 financially constrained firms and 68 financially unconstrained firms. The set of unconstrained firms is constructed by matching on Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. As a placebo test, Panel B compares the fraction of high work experience mass layoffs for the years that follow. The only significant difference between financially constrained firms above financially unconstrained firms occured between 2007 and 2008. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Fraction of High Work Experience Employees Affected in Mass Layoffs

	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007	7.25% ***	7.03% ***	0.22%
	(0.024)	(0.023)	(0.002)
2008	35.61% ***	8.72% ***	26.89% ***
	(0.030)	(0.017)	(0.026)
Difference (2008 - 2007)	28.36% ***	1.69%	26.67% ***
	(0.025)	(0.015)	(0.073)
Matching Estimator (ATT)			26.14% ***
			(0.049)

Panel B: Placebo Tests	Difference in the Fraction of High Work Experience Mass Layoffs Between Financially Constrained and Unconstrained Firms	Matching Estimator (ATT)
2008 - 2007	26.67% ***	26.14% ***
	(0.073)	(0.049)
2009 - 2008	5.21%	4.94%
	(0.068)	(0.065)
2010 - 2009	5.09%	4.88%
	(0.065)	(0.055)
2011 - 2010	5.13%	4.89%
	(0.052)	(0.052)

Finally, I consider the differences in layoff decisions concerning workers' levels of on-the-job training. The results are presented in Table 21. I consider employment decisions affecting workers with high levels of on-the-training (at least one year). I find that, in comparison to unconstrained firms, financially constrained firms laid off a greater fraction of workers with high levels of on-the-job training. Looking at the first row of Panel A, I find that financially constrained firms laid off 22.45% of their share of highly trained workers in the pre-crisis period, compared to 20.74% for unconstrained firms. Following the onset of the crisis, this portion increased to 45.80% for financially constrained firms, or 23.35%. The increase in the portion of highly trained workers fired is economically and statistically insignificant among unconstrained firms. The difference-in-difference estimate implies that financially constrained firms laid off 19.54% more highly trained workers relative to financially unconstrained firms and relative to the pre-crisis period. The Abadie-Imbens estimate of the difference-in-difference coefficient is 21.17%.

Table 21: Difference-in-Difference Comparisons of On-the-Job Training of Workers Affected by Mass Layoffs

This table reports evidence on the levels of on-the-job training of workers affected by mass layoff events in the fall 2007 credit crisis. Firms with a large portion of long-term debt maturing right after the third quarter of 2007 (financially constrained firms) laid off workers with greater levels of on-the-job training compared to otherwise similar firms whose debt was scheduled to mature after 2008 (unconstrained firms). The table summarizes indicator variables representing the fraction of mass layoffs that affected workers with at least one year of on-the-job training. Panel A compares the fraction of workers with high on-the-job training affected by mass layoffs in 2007 to that in 2008. The financially constrained set is defined as firms with at least 20% of long-term debt maturing in 2008 (with the onset of the financial crisis), while the unconstrained set is defined as those with less than or equal to 20% of long-term debt maturiting in that period. There are 68 financially constrained firms and 68 financially unconstrained firms. The set of unconstrained firms is constructed by matching on Q, cash flow, size, cash holdings, long-term debt normalized by assets, SIC 12 industry classifications and credit rating category. As a placebo test, Panel B compares the fraction of high on-the-job training mass layoffs for the years that follow. The only significant difference between financially constrained firms above financially unconstrained firms occured between 2007 and 2008. Heteroskedasticity-consistent standard errors are in parentheses.

Panel A: Fraction of High On-the-Job Training Employees Affected in Mass Layoffs

	Constrained	Unconstrained	Difference (Constrained - Unconstrained)
2007	22.45% ***	20.74% ***	1.71%
	(0.076)	(0.068)	(0.017)
2008	45.80% ***	24.55% ***	21.25% ***
	(0.038)	(0.049)	(0.020)
Difference (2008 - 2007)	23.35% ***	3.81%	19.54% ***
	(0.021)	(0.033)	(0.054)
Matching Estimator (ATT)			21.17% ***
			(0.054)

Panel B: Placebo Tests	Difference in the Fraction of High On-the-Job Mass Layoffs Between Financially Constrained and Unconstrained Firms	Matching Estimator (ATT)
2008 - 2007	19.54% ***	21.17% ***
	(0.054)	(0.054)
2009 - 2008	3.36%	3.87%
	(0.058)	(0.063)
2010 - 2009	4.13%	4.39%
	(0.067)	(0.066)
2011 - 2010	4.22%	4.40%
	(0.075)	(0.077)

From measures of salary, educational attainment, work experience and on-the-job training, it appears that financially constrained firms laid off higher human capital workers following the onset of the crisis relative to unconstrained firms. Though these measures are approximate, it is encouraging that the outcomes tend in the same direction. What can we infer from the differing magnitudes? The greatest gap between the human capital of workers laid off by financially constrained firms relative to unconstrained firms shows up for educational attainment (28.25%), followed by work experience (26.14%) and then followed by on-the-job training (21.17%). On-the job training seems to be the best proxy for firm-specific human capital as opposed to general human capital, as it reflects training specific to a firm. However, because this variable is an estimate by occupation and not actual on-the-job training reported by firms in WARN filings, there is no way of knowing whether the amount of on-the-job training was acquired at the firm that made the mass layoff. Thus, it makes little sense to read to closely into the magnitudes of each estimate.

Next, I am interested in the evolution of human capital affected across multiple mass layoff instances. I split the subset of firms that have made a mass layoff into two sub-samples: firms with KZ Index values in bottom quartile (which I designate as financially healthy) and firms with KZ Index values in the top quartile (which I designate as financially distressed). I then estimate a regression relating the number of layoff instances within a firm to the degree of human capital laid off. I find the degree of human capital laid off to be positively related to the number of layoff instances among financially health firms, but negatively related to the number of layoff instances among financially distressed firms.

Stock Market Reactions to Mass Layoffs Announcements

Finally, I consider stock market reactions to mass layoff announcements in Table 23. A valuation-based understanding of layoff announcements should provide additional context in which to interpret the results described above. I find 3-day cumulative abnormal returns to be slightly negative following a mass layoff announcement. This is consistent with Farber and Hallock (2008), who document negative returns among firms with layoff announcements reported in the *Wall Street Journal* between 1970 and 1999. In addition, I find a negative relationship between 3-day cumulative abnormal returns and the degree of human capital laid off. These results indicate that valuations decline upon destruction of the value created in worker-employer relationships, and that the decline is particularly pronounced for high human capital worker-employer relationships.

Table 22: The Number of Layoff Instances and the Degree of Human Capital Laid Off

This tables presents evidence on the relationship between the number of layoff instances within firms and the degree of human capital laid off. I split the set of firms having made at least one mass layoff into two groups: firms with KZ Index values in the bottom quartile (financially healthy) and firms with KZ Index values in the top quartile (financially distressed firms). The number of layoff instances varies from one to eight in the period between 2006 and 2011. The average annual salary is the weighted average salary of workers laid off in a given layoff instance. I find the degree of human capital laid off to be positively related to the number of layoff instances among financially health firms, but negatively related to the number of layoff instances among financially distressed firms. I include industry and firm fixed effects.

	Average Annual Salary of Workers Laid Off By Financially Healthy Firms	Average Annual Salary of Workers Laid Off By Distressed Firms
Total Employment	-0.104	0.167
	(0.100)	(0.191)
Q	-0.158 **	-0.097 **
	(0.061)	(0.048)
# of Layoff Instances	0.660 ***	-0.823 ***
	(0.233)	(0.243)
Constant	0.474 ***	-0.542 ***
	(0.104)	(0.163)
R-Squared	0.17	0.21
Adjusted R-Squared	0.11	0.16
Fixed Effects		
Fama-French SIC-12 Indust	Yes	Yes
Year	Yes	Yes
Number of Observations	206	206

Table 23: Stock Market Reactions Following Mass Layoff Announcements

This table reports coefficient estimates for regressions on cumulative abnormal returns in the [-3, +3] day window surrounding mass layoff announcements. Announcemnt dates are the dates which firms reported as layoff notice dates in the WARN data and then adjusted by hand after looking up the first occurence of the layoff news in Factset media sources. Robust standard errors in parentheses.

	3-day Cumulative Abnormal Return	3-day Cumulative Abnormal Return
Log of Average Annual Salary	-5.38% ***	-5.23% ***
	(0.004)	(0.004)
Higher Education Dummy	-1.24% **	-1.11% **
	(0.007)	(0.005)
Work Experience Dummy	-1.94% **	-1.75% ***
	(0.004)	(0.003)
On-the-Job Training Dummy	-1.74% ***	-1.75% ***
	(0.003)	(0.003)
Fraction of Total Employment Affected		-2.18% ***
		(0.004)
R-Squared	0.090	0.137
Year Fixed Effects	Yes	Yes
Number of Observations	802	802

One concern is how market participants observe the degree of human capital affected. WARN notifications containing the job titles of affected workers are made public with a lag. It is more likely that market participants are simply responding to their knowledge of whether a given firm is a low or high capital employer, for example, a manufacturer or a biotech firm.

Conclusion

I use the August 2007 credit panic to assess the effect of financial contracting on employment outcomes. In particular, I consider whether firms with a significant portion of long-term debt maturing at the onset of the crisis experienced more pronounced outcomes than otherwise similar firms that did not face a need to rollover a significant portion of debt during the crisis. I use plausibly exogenous variation to control for observed and time-invariant unobserved firm heterogeneity using a difference-in-difference matching estimator.

My results indicate that debt maturity structure can have significant implications for firms' employment decisions when they face a credit shock. Firms whose long-term debt was largely maturing right after the third quarter of 2007 reduced their total employment by 5.07% more than otherwise similar firms whose debt was due following the onset of the crisis and were 6.89% more likely to make a mass layoff.

Examining the quality of layoffs, I find that financially constrained firms lay off greater portions of high human capital workers relative to financially unconstrained firms. These specifications rely on a variety of human capital proxies – salary, educational attainment, work experience and on-the-job training. Given multiple layoff instances, I find that financially healthy firms began laying off low human capital workers, escalating to high human capital workers in later layoff instances. In contrast, financially distressed firms laid off high human capital workers in early layoff instances, deescalating to low human capital workers later on. The pecking order of layoffs that I observe among financially distressed firms contradicts labor economics theory predicting that, given a layoff event, firms will sort workers in inverse order of firm-specific human capital and begin laying off at the low end.

Finally, I consider stock market reactions to mass layoff announcements. I find 3-day cumulative abnormal returns to be slightly negative following a mass layoff announcement. In addition, I find a negative relationship between 3-day cumulative abnormal returns and the degree of human capital laid off. These results indicate that valuations decline upon destruction of the value created in worker-employer relationships, and that the decline is particularly pronounced for high human capital relationships.

My results contribute to the literature in a number of ways. My results point to the importance of maturity structure for maintenance of labor. This highlights the extra attention firm managers should pay to the maturity profile of their firms' debt. Second, my results provide evidence that the 2007 credit crisis had significant real effects on labor decisions in 2008. Third, I present new evidence on human capital choices within layoff instances, which underscores the attention that firm managers should pay to the cost and contribution of each occupation, both in the near and long terms. Broadly, my findings suggest that financing constraints have a significant impact on firm-level employment outcomes and, in particular, on the type of human capital dismissed in layoffs induced by financial constraint.

Chapter 3: Layoff Announcements, Human Capital and Stock Price Reactions

Introduction

Given the economic climate of the last few years, many firms have undergone downsizing in an attempt to cut costs. As these downsizing efforts (or layoffs) are announced, the market reaction is often mixed. For some firms, there can be a significant negative stock price reaction on the date that a firm announces a layoff, yet for other firms, the market reaction can be significantly positive. This raises the question as to how the market perceives layoffs. Does a layoff announcement provide a signal about the present or future financial distress of the company, with the market reaction being negative? Or is the layoff viewed as a solution to an existing problem that may benefit the company, in which case the market reaction should be positive? These two alternatives form the basis for two main hypotheses that have been presented in prior research regarding the stock market reaction to layoff announcements.

The financial distress hypothesis, advanced by Worrell, Davidson and Sharma (1991), is based on the premise that the signal provided by the layoff announcement tends to reinforce knowledge about the current negative financial condition of the firm. Layoffs confirm management's view that the current financial problems are real and long-lasting. The financial distress hypothesis predicts a negative stock price reaction. An alternative hypothesis is the potential benefit hypothesis, advanced by Iqbal and Shetty (1995), which is based on the premise that to some extent all layoffs are an attempt to cut costs and improve earnings. Firms that engage in a layoff do so in an attempt to achieve a future benefit, including the potential for a larger increase in future profits. The layoff may even help the firm avoid bankruptcy. Stock price reactions are expected to be positive under the potential benefit hypothesis.

This chapter addresses the conflicting results that have emerged in prior studies to address the financial distress and potential benefit hypotheses. It also recognizes that these two hypotheses are not necessarily in competition; they may explain concurrent and additive effects of the stock price reactions to layoff announcements. Price reactions will be a function of the economic impact of the layoff, where the economic impact is closely linked to the types of workers laid off, i.e. their occupations and degrees of human capital.

Related Literature

The empirical literature that has considered stock price reactions surrounding layoff announcements has, by and large, found the relationship to be negative. One straightforward interpretation of this is that layoffs signal a reduction in product demand relative to existing production capacity (the "reduced demand" hypothesis). Clearly, reduced demand is not the only factor driving layoff events. To name a few alternative scenarios, layoffs may come about as a means to cut costs, following a productivity gain or following a restructuring event such as a merger. In each case, the stock price may respond differently. Thus, studies examining stock price reactions in greater detail have focused on the stock price reaction relative to (1) the stated reason for the layoff and (2) the financial condition of the firm. These two branches are described in turn below.

Focusing on firms' stated reasons for the layoff events, Worrell, Davidson and Sharma (1991) examined 197 layoff announcements that appeared in the Wall Street Journal between 1979 and 1987. Using mean cumulative prediction errors, they found a significant negative overall stock price reaction over an eleven-day period surrounding the date of the announcement. In addition, they found that firms that stated a layoff was due to "financial reasons" experienced significantly more negative returns than those that stated a layoff was due to "restructure or consolidation." The authors conclude that the layoff announcement was viewed as a signal that the firm's problems were serious, and was thus perceived negatively by the market. These results support the financial distress hypothesis.

In a related study, Iqbal and Shetty (1995) examined 187 layoff announcements that appeared in the Wall Street Journal between 1986 and 1989. Using cumulative average prediction errors, they found a significant negative overall stock price reaction over the two-day event window surrounding the layoff announcement date. These results are consistent with Worrell, Davidson and Sharma (1991). Iqbal and Shetty also examined differences in stock price reactions to the layoff announcements of "financially weak" versus "financially healthy" firms, and found that financially weak firms had a significantly more positive stock price reactions than financially healthy firms. These results contradict the results of Worrell, Davidson and Sharma, who found the opposite: financially weak firms had more negative stock price reactions. Iqbal and Shetty attribute their findings to the potential benefit hypothesis. They pointed out, however, that the measure they used to identify financially weak firms is different than the measure used by Worrell, Davidson and Sharma, which could account for the difference. In addition, their sample size of financially weak firms (17) was small, which may contribute to the difference between the two studies.

Consistent with the potential benefit hypothesis, in a case study of restructuring events at General Dynamics in the early 1990s, Dial and Murphy (1995) found that layoff events resulted in efficiency improvements and value creation. While it is of course difficult to draw generalizable conclusions from a case study, the study fits the broad interpretation that layoffs are a signal of a productivity gain – management has found more efficient ways to produce using less labor.

Under the financial distress hypothesis, a layoff announcement for financial reasons is confirmation of bad news: a firm laying off workers for financial reasons is in a tenuous position that is bound to grow worse. Thus, the stock price reacts negatively. The potential benefit hypothesis, on the other hand, views a layoff announcement as potential good news: a financially weak firm that chooses to lay off workers is likely to do so strategically and to its benefit. Thus, the stock price reacts positively. An ability to distinguish between the two hypotheses would seem to require an additional clue: an understanding of the characteristics of the workers the firm has laid off. In particular, it seems important to account for these workers' contributions to the firm and, thus, whether the firm can expect to be helped or hurt by their departure. Ideally, each worker's impact might be understood through a measure of productivity per worker, such as sales per worker. Lacking that degree of detail, I instead rely on several proxies of human capital by occupation. The proxies are salary, level of educational attainment, level of work experience and level of on-the-job training, and are specific to occupation. Presumably, earlier studies did not make use of such information because due to a lack of data. Detail on the human capital of workers laid off should add to our ability to meaningfully distinguish between the financial distress and potential benefit hypotheses.

Data

This paper relies on a new, hand-collected data set on firm-level mass layoffs in California from 2006-2011. The dataset is built around firm-level mass layoff instances. Since my analysis also requires additional worker and firm characteristics, I combine the following four datasets into one: (1) firm-level mass layoff data available as a result of the WARN Act; (2) proxies of human capital by occupation (salary, educational attainment, work experience, and on-the-job training) from the Bureau of Labor Statistics (BLS); (3) quarterly and annual firm fundamentals as well as credit ratings from Compustat; and (4) stock price and market return data from CRSP. The final, combine dataset consists of 412 unique, public firms having made 824 mass layoffs in California between 2006 and 2011. This section describes each source, data selection and variable construction.

Firm-Level Mass Layoff Data

Firm-level data on mass layoffs is available as a result of the Worker Adjustment and Retraining Notification (WARN) Act,[9] passed federally in 1989. The WARN Act requires firms with more than 100 full-time employees to provide 60-day advanced notice of impending mass layoff events, defined by the BLS as affecting 50 or more employees of a single company in a given location. Notice must be given in writing to: (1) the employees' representative or, if there is no representative, to each

[9] I compiled the data from over 3,000 PDF pages of notices available to the public from the State of California's Employment Development Department: http://www.edd.ca.gov/jobs_and_training/layoff_services_warn.htm.

affected employee; (2) the state dislocated worker unit; and (3) the local government where the plant is located.

Implementation of the WARN Act having been left to states, the availability of WARN data varies widely. Compliance with the Act, the variables collected, the time span over which they have been collected, as well as public access to the records, vary by state. The non-standard nature of the reporting makes it difficult to imagine a national dataset. Many states would be missing, there would be few data fields in common, and the time series would be short. As a result, I have chosen to focus on a single state, California. In addition to being a large economy, California has enforced thorough WARN reporting and has made the records relatively easily accessible. It is also the only state to require firms to report the occupations affected in a mass layoff, which is important to my analysis. Californian WARN notices require the following information: company name; address of layoff location; layoff date; date notice received; number of employees affected; layoff or closure; severance; union representation; bumping rights; and occupations of affected employees. In addition, California defines a mass layoff more narrowly, as affected 35 or more employees. To my knowledge, this research is the first to describe and analyze firm-level layoffs beyond a case study of a single firm.

Figure 1 presents a geographic scatter plot of all mass layoff instances in California between 2006 and 2011. Mass layoff instances are largely clustered in the urban areas surrounding San Francisco and Los Angeles, corresponding to the locations of most large firms, retail stores, and production facilities. Figure 2 presents a scatter plot of layoff instances over this period. The raw data consists of 4,335 layoff events among 1,274 unique public and private firms, affecting a total of 260,100 workers. The average layoff event in this period affected 110 workers. Several major layoff events stand out. The largest and third largest layoff events belong to Macy's, which laid off 2,053 workers on September 1st, 2006 and 1,501 workers on May 1st, 2009. The second largest layoff event belongs to United Airlines, which laid off 1,549 workers on October 5th, 2008. The fourth largest layoff belongs to Circuit City, which laid off 1,163 workers on March 21st, 2009. The fifth largest layoff belongs to Washington Mutual, which laid off 1,153 workers on June 30th, 2008.

I compared the WARN series to both initial unemployment claims from the BLS and an estimate of mass layoffs derived from BLS data in order to get a sense for the completeness of the WARN data. Initial claims are only a partial description of layoffs in California, as not all those laid off apply for unemployment assistance. Nevertheless, initial claims offer a more complete picture of layoffs than the WARN data, as an unemployment assistance claim can be initiated by any laid off worker, not just those affected by a mass layoff. I find that the WARN data represents approximately 20% of initial claims. In another attempt to assess the completeness of the WARN data, I estimate the minimum amount of mass layoffs in California using Mass Layoff Statistics (MLS) from the BLS. The MLS program does not report the number of employees affected by mass layoffs, but it does report the number of monthly mass layoff incidents in the state. California defines a mass layoff as a layoff incident affecting at least 35 workers. Thus, I assume that a minimum of 35 workers are affected in each mass layoff incident and simply multiply the number of mass layoff events by 35 in order to arrive at a minimum estimate. I find that the WARN data represents 60% of estimated mass layoffs. This leads me to believe that some firms are simply not reporting mass layoff events as they are required to by state and federal law. This is unsurprising, as there is slight or no enforcement of the WARN Act in California. Non-reporting firms are likely to be less well-run administratively rather than intentionally flouting the state disclosure requirement; I do not believe that the omission of these firms biases the data in a predictable direction.

Table 12 presents a tabulation of mass layoff events by industry and Table 13 tabulates the employers having fired the greatest numbers of workers. Financial firms (including Wells Fargo, Washington Mutual, Fleetwood, Indymac, Citigroup) made a large number of mass layoffs, as did major retail firms (including Macy's, Mervyn's, Circuit City, Target, JC Penney's). The airline and aerospace industry (including United, American, ATA, Boeing), persistently beleaguered, cut many jobs as well. The mix of occupations affected in each mass layoff depends to some extent on the firm. For example, the WARN data reveals that aerospace engineers and flight attendants were laid off by United Airlines, whereas marketing managers and sales personnel were laid off by Macy's.

However, each mass layoff notice pertains to a variety of occupations and those occupations tend to be repeated among firms within the same industry.

It is common for firms to engage in multiple rounds of mass layoffs as opposed to all at once; sometimes the layoff events are separated by years, sometimes merely by several weeks. Thus, firms behave as though WARN notice filings carry either no market signal or at least not a negative market signal. It may be that press releases lead the WARN announcements, in effect nullifying the information that they contain for financial markets. Multiple mass layoffs may be a sign of ongoing financial constraint.

Constructing Proxies for Human Capital By Occupation From the BLS

In order to construct measured of human capital by occupation, the job titles reported in the WARN data had to be unified by occupation. I unified WARN job titles by hand using the standard occupation classification (SOC) system available from the BLS. Once WARN job titles were matched to SOC occupations they were also linked to SOC codes, which allowed me to connect to other occupational data tracked by the BLS. This data is the basis of four human capital proxies: annual salary, educational attainment, work experience and on-the-job training.

Annual salary is available by occupation (840 unique occupation classifications) and by metropolitan statistical area (24 unique areas within California). To take an example, this allows me to estimate that a typical chemical engineer in the San Diego-Carlsbad-San Marcos area earns an annual salary of $86,490. In the absence of a direct measure of worker skill, the literature has commonly used wages as a proxy. Examples include Bernard and Jensen (2002) and Dunne and Roberts (1990), who consider the determinants of wages and the effects of wages on plant closures and Carneiro and Portugal (2003), who consider the link between wages and displacement events.

Next, SOC codes link to estimates of educational attainment, work experience and on-the-job training for each occupation. Occupations receive designations in three categories:

1) Entry-level education: doctoral/professional degree; master's; bachelor's; associate's; postsecondary; some college; high school; less than high school

2) Related work experience: > 5 years; 1-5 years; < 1 year; none

3) On-the-job training: internship; apprenticeship; long-term (> 1 year); moderate (1-12 months); short-term (< 1 month); none

To take an example, a typical judge has a doctoral or professional degree, more than 5 years of work experience and short-term on-the-job training. Layoffs in California between 2006 and 2011 are summarized according to these four human capital proxies in Table 14.

A worker's human capital can be thought of in two pieces: firm-specific and general. The proxies described above are each indicators of a worker's general human capital. Ideally, I would have data on firm-level investment in specific skills, i.e. those that do not easily translate to other firms or context, by occupation. Tenure at a firm would be a rough but reasonable proxy, as firm-specific knowledge naturally increases with tenure. However, lacking data on tenure or potential other proxies, I use the four general human capital proxies as though they are representative of firm-specific human capital and interpret the results with this caveat.

Compustat Annual Fundamental Data

I consider the entire universe of firms from the Compustat Annual and Quarterly Fundamental Files between 2000 and 2011. In addition to balance sheet and income statement information, Compustat also reports the number of workers employed by a firm. The main independent variables are size (represented as the log value of total assets), Tobin's Q (proxied by the market-to-book ratio), cash flow, cash balance, and long-term debt normalized by total assets. Variable definitions and constructions are detailed in Appendix A. Finally, I use four-digit SIC codes in order to map each firm's industry into Fama-French 12 and Fama-French 48 industry classifications.

CRSP Stock Price and Market Return Data

I use value-weighted return and market return data from the Center for Research in Security Prices (CRSP) to calculate 3-day cumulative abnormal returns for each firm surrounding each layoff event. The CRSP data was merged using CUSIP and stock tickers.

Adjustments to Layoff Announcement Dates

The layoff announcement dates reported in WARN announcements required verification. I looked up layoff announcements using Factiva and Lexus Nexus and recorded the earliest surrounding date on which news was reported in a major news publication, such as The Wall Street Journal. I relied on these verified and adjusted dates in the event study. The date adjustments are important to my results.

Event Study Methodology

The event study methodology that I rely on is widely used in the empirical corporate finance literature (Brown and Warner (1985), Campbell, Lo and MacKinlay (1997), Fama, Fisher, Jensen and Roll (1969), and MacKinlay (1997)). I will, therefore, only provide a broad overview here. As many of these papers note, clearly defining the event date is critical and often difficult. I assume that the market become aware of a layoff announcement on the day it was reported in an Associated Press or Wall Street Journal article. It may be that the market knew of what I identify as the announced event at some time prior. To the extent that this is the case, my analysis will not capture the full effect of layoff announcements on stock prices.

Using value-weighted return data from CRSP, cumulative average excess returns are calculated using a market model that regresses security returns against the overall market return to generate a series of abnormal returns. Let t index time in trading dates, let $s$indicate the "event date" (the date of the layoff announcement) and let i index firms. The firm daily return, R_{it}, is regressed on R_{mt}, the value-weighted market index for date t, which is available from CRSP. This regression,

$$R_{it} = \alpha_i + \beta_i R_{mt} + \eta_{it},$$

is estimated for a period from day $s - 60$ to day $s - 30$.[10] Next, for days around the event date, I calculate the daily abnormal, or excess, return, as

$$ER_{it} = R_{it} - (a_i + b_i R_{mt}),$$

where R_{it} is the actual return on security i at time t, a_i and b_i are the estimated regression coefficients resulting from the equation above, and $(a_i + b_i R_{mt})$ is the predicted return on security i at time t. The excess return represents the component of the stock return of firm i that is not correlated with overall market movement in the stock returns and presumably reflects unexpected firm-specific factors.

The excess returns calculated for each date around a layoff announcement are used to calculate the cumulative excess return for each announcement. These are computed by added up the daily excess returns over various event windows around the date of the announcement:

$$ER_t = \sum_{i=1}^{N} \frac{ER_{it}}{N}.$$

The mean cumulative excess returns over various intervals, T_1 to T_2, is then computed as:

$$MCER_{T_1T_2} = \sum_{t=T_1}^{T_2} ER_t,$$

where the interval T_1 to T_2 represents different lengths. I report analysis based on cumulative excess returns computed using the one-, three- and five-day windows. In an efficient market, ER_t and $MCER_{T_1T_2}$ will be random across time except when news affects the intrinsic value of a firm's stock. If the market is efficient (Fama (1970)), its reaction to news will be immediate. To test whether news like a layoff announcement affects firm value, I compute a test statistic by standardizing the ER_{it} by its estimated standard deviation, s_{it}:

$$SER_{it} = \frac{ER_{it}}{s_{it}}.$$

[10] I experimented with other estimation periods, such as -100 to -70 and -100 to -50, with no meaningful effect on the results.

The s_{it} is computed as the typical regression forecast error. This method specifically adjusts the standard deviation for the distance of the independent variable in the test period from the mean value in the estimation period, ensuring that same-sized prediction errors to have differing levels of significance for different firms due to individual variation. SER_{it} is the test statistic for an individual company's abnormal returns.

The standardized mean cumulative prediction error over the interval, $SCER_i$, over the interval $t = T_{1i}, \ldots, T_{2i}$is then

$$SCER_i = \sum_{T_{1i}}^{T_{2i}} i * SER_{it} * \sqrt{T_{2i} - T_{1i} + 1}$$

where $T_{2i} - T_{1i} + 1$ is the number of days spanning the test interval. The test statistic for N securities is

$$Z = \sum_{i=1}^{N} \frac{SCER_i}{\sqrt{N}}.$$

Each SER_{it} is assumed to be normally distributed in the absence of abnormal performance. Under this assumption, Z is also normal.

If the market views a layoff announcement positively, negatively or neutrally, the values of ER_t and $MCER_{T_1T_2}$ for the intervals surrounding day zero will be significant and positive, significant and negative, and insignificant, respectively.

Results

Stock Returns For All Layoff Announcements

Table 24 summarizes mean cumulative excess returns for all layoff announcements over varying time intervals: -90 to +90, -30 to +30, -5 to +5, -3 to +3 and -1 to +1 day(s). The first row explores all layoff announcements. In agreement with the results in related studies, I find the mean cumulative excess returns to be negative following layoff announcements. The results are highly significant in the -5 to +5 day and -3 to +3 day intervals, mildly significant in the -1 to +1 day interval and insignificant in the -90 to +90 day and -30 to +30 day intervals. This is evidence that a significant negative stock market reaction is associated with layoff announcements and confined to the days immediately surrounding the layoff announcements.

Table 24: Mean Cumulative Excess Returns

This table presents mean cumulative excess returns over various time intervals. I first calculate excess returns for each date around a layoff announcement and next calculate the cumulative excess returns for each announcement by adding up the daily excess returns over various event windows. Standard errors are in parentheses.

	Days				
Variables	-90 to +90	-30 to +30	-5 to +5	-3 to +3	-1 to +1
All layoff announcements	-0.0199	-0.0215	-0.0142	-0.0045	-0.0041
	-(0.49)	-(0.96)	-(3.51)	-(2.83)	-(1.70)
Degree of human capital					
Salary					
High	-0.0563	-0.0481	-0.0246	-0.0081	-0.0079
	-(0.74)	-(0.24)	-(3.85)	-(3.51)	-(1.66)
Low	-0.0034	-0.0025	-0.0002	-0.0006	-0.0004
	-(0.72)	-(0.81)	-(0.12)	-(0.45)	-(0.33)
Education					
High	-0.0617	-0.0298	-0.0093	-0.0117	-0.0083
	-(0.33)	-(0.62)	-(3.30)	-(3.24)	-(1.89)
Low	-0.0112	-0.0082	-0.0018	-0.0009	-0.0007
	-(0.94)	-(0.59)	-(0.22)	-(0.75)	-(0.50)
Work experience					
High	-0.0334	-0.0197	-0.0136	-0.0227	-0.0119
	-(0.55)	-(0.40)	-(1.88)	-(1.92)	-(1.42)
Low	-0.0018	-0.0027	-0.0026	-0.0002	-0.0001
	-(0.63)	-(0.55)	-(0.29)	-(0.15)	-(0.36)
On-the-job training					
High	-0.046	-0.0177	-0.0148	-0.0083	-0.0066
	-(0.19)	-(0.15)	-(1.99)	-(1.75)	-(1.30)
Low	-0.0024	-0.0019	-0.0011	-0.0005	-0.0006
	-(0.25)	-(0.34)	-(0.17)	-(0.13)	-(0.09)
Percentage of employees affected					
More than 5	-0.0066	-0.0066	-0.021	-0.0077	-0.0026
	-(0.85)	-(0.85)	-(2.27)	-(2.78)	-(0.78)
Less than 5	-0.0007	-0.0005	-0.0034	-0.0014	-0.0012
	-(0.88)	-(0.67)	-(2.33)	-(2.24)	-(0.25)

Stock Returns By Degree of Human Capital

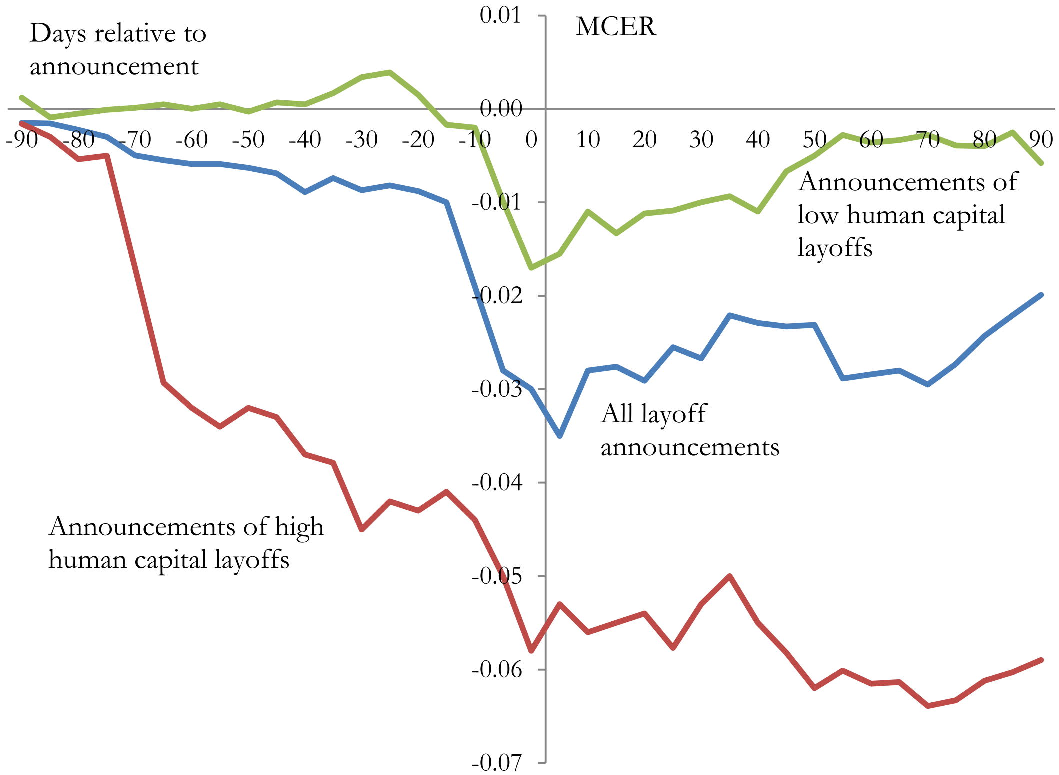
The four middle rows of Table 24 examine stock price reactions to layoff announcements by the degree of human capital of the employees affected. I rely on the four proxies of human capital described above: salary, educational attainment, work experience and on-the-job training. For each proxy, I designate the top quartile the high human capital segment and the bottom quartile the low human capital segment and calculate mean cumulative excess returns over the chosen time intervals.

Whereas mean cumulative excess returns were found to be negative when examined across all layoff announcements, I find that negative excess returns appear to be driven by high human capital layoffs. That is, mean cumulative excess returns are significantly negative over the -5 to +5 day, -3 to +3 day and -1 to +1 day intervals for high human capital layoffs but not for low human capital layoffs. For low human capital layoffs, mean cumulative excess returns are very slightly negative but insignificant. The results are strongest using salary as a proxy for human capital but hold with mild significance for the other human capital proxies as well. The returns tend to be negative and highly significant a for the -5 to +5 day and -3 to +3 day intervals and mildly significant for the -1 to +1 day intervals, indicating that a three-day window may be too narrow to pick up the effects of the layoff announcements. Figure 3 presents a plot of cumulative excess returns relative to layoff announcement dates (measured as WARN layoff notice dates).

Stock Returns By Layoff Size

To study the effects of the size of a mass layoff announcement, I categorized layoffs into two sizes: layoffs of more than 5 percent of the firm's total labor and layoff of less than 5 percent of the firm's total labor. For layoff announcements of 5 percent or more, the mean cumulative excess return is -0.021 from day -5 to day +5 and -0.0077 from day -3 to day +3. For layoff announcements of 5 percent or less, the mean cumulative excess return is -0.0034 from day -5 to day +5 and -0.0014 from day -3 to day +3. While the mean cumulative excess returns are negative in all cases, the magnitudes are substantially less for smaller layoff announcements.

Figure 3: Mean Cumulative Excess Returns Relative to Layoff Announcement Dates



Conclusion

This exploratory event study has found evidence in agreement with previous studies: investors tend to respond negatively to firm-level layoff announcements. In addition, it goes a step further by making use of new data on the occupations of workers affected in a given layoff instance. The occupations of workers affected turns out to matter crucially. I find that investors tend to respond negatively to firm-level layoff announcements when I consider all announcements at once but, when layoffs are separated into high human capital versus low human capital categories, the negative relationship is only present among high human capital layoffs. The reactions continue to be negative among low human capital layoff announcements but these results are insignificant.

This nuance goes some way in helping to understand the two main hypotheses regard stock price reactions to layoff announcements: the financial distress hypothesis and the potential benefit hypothesis. Under the financial distress hypothesis, a layoff announcement for financial reasons is confirmation of bad news: a firm laying off workers for financial reasons is in a tenuous position that is bound to grow worse. Thus, the stock price reacts negatively. The potential benefit hypothesis, on the other hand, views a layoff announcement as potential good news: a financially weak firm that chooses to lay off workers is likely to do so strategically and to its benefit. Thus, the stock price reacts positively.

Considering the full set of layoff announcements, the stock price reaction is negative, which would appear to support the financial distress narrative. However, conditioning on the degree of human capital, the negative stock price reaction holds in the event of high human capital layoffs but not in the event of low human capital layoffs. Is this consistent, instead, with the potential benefit hypothesis? Perhaps another way of considering the potential benefit hypothesis is the notion that some layoffs are more likely to imply a potential benefit whereas others are more likely to imply potential harm. High human capital layoffs are more likely to be harmful, as it will be more difficult for a firm to re-hire and re-train higher human capital workers. A negative stock price reaction given high human capital layoffs seems to support a version of the potential benefit hypothesis or, more aptly, a hypothesis that might more aptly be labeled the "potential harm" hypothesis. The potential benefit and potential harm hypotheses are not contradictory. It is most likely the case that the elimination of low human capital workers, as opposed to high human capital workers, will benefit a firm.

While previous studies have found clear evidence that the distribution of stock market reactions is negative, I find evidence that the relationship between announcements and stock price reactions depends on the occupations of the workers laid off. In particular, the negative relationship is particular to layoffs of high human capital workers.

Appendix

This section documents the definitions of the variables used in the empirical analysis. Variable names in parentheses are from the Compustat Annual Fundamental files, unless noted.

i. Market-to-book: total book value of assets (AT) plus the market value of equity (AT+CSHO*PRCC_F) minus the book value of equity deferred taxes (CEQ+TXDB), all over total assets (AT*0.9) plus the market value of assets (MKVALT*0.1)

ii. Long-term leverage: total debt (DLTT+DLC+DCLO) divided by total assets (AT)

iii. Profitability: EBITDA (OIBDP) divided by beginning-of-period total assets (AT)

iv. Interest coverage: operating income before depreciation (OIBDP) divided by interest and related expenses (XINT)

v. Liquidity: net income (IB) plus depreciation and amortization (DP) over the lag of property, plant and equipment (PPENT)

vi. Investment-to-capital: capital expenditures (CAPX) divided by the lag of property, plant and equipment (PPENT)

vii. Size: natural log of total assets (AT)

viii. Sales: net sales (SALE)

ix. Cash flow: net income (IB) plus depreciation and amortization (DP) over the lag of property, plant and equipment (PPENT)

x. Cash balance: the ratio of cash and short-term investments (CHE) to total assets (AT)

xi. Dividends: common dividend (DVC) plus preferred dividend (DVP) over lagged total assets (AT)

xii. KZ Index: index of financial constraint, calculated following Lamont, Polk, and Saa-Requejo (2001) as follows:

$$KZ\ Index(t) = -1.002 * Cash\ Flow(t) + 0.283 * Market-to-Book(t) + 3.139 * Debt\ Portion(t) - 39.368 * Dividends(t) - 1.315 * Cash\ Balance(t)$$

xiii. Investment: capital expenditures (CAPX) divided by the lag of property, plant and equipment (PPENT)

xiv. % Δ investment: percentage change in investment from $t-1$ to t

xv. % Δ employment: percentage change in the number of employees (EMP) from $t-1$ to t

xvi. % Δ capital expenditure: percentage change in capital expenditure (CAPX) from $t-1$ to t

The following variables were pulled from the Compustat Industry file:

xvii. RTCRENT: Contingent rental expense

xviii. RTLCS: Comparable sales (%)

xix. RTLNSC: Number of stores closed during period

xx. RTLNSE: Numbers of stores at period end

xxi. RTLNSO: Number of stores opened during period

xxii. RTMRENT: Minimum rental expense

xxiii. RTNSSF: Net sales per retail square foot

xxiv. RTORENT: Other rental expense

xxv. RTTSF: Total retail square footage

Table A1: Compustat Sample Selection

The sample consists of all firms in the Compustat Annual and Quarterly Fundamentals Files in 2007. Following Almeida et al. (2012) as well as Almeida, Compello and Weisback (2004) and Frank and Goyal (2003) before them, I apply the following screens. Additionally, I remove small firms (those with fewer than 500 employees), following Benmelech, Bergman and Seru (2011). This reduces an inital sample of 9,395 firms with Compustat data to a final sample of 844 firms with Compustat data.

Number of firms in fiscal year 2007	9,395
Drop firms with:	
SIC 6000s	-2,008
SIC 8000s	-191
SIC 9000s	-28
Total employees < 500	-3,456
Total assets < $100 million	-90
Negative sales	-1
Missing sales	-410
Cash greater than assets	0
PPE greater than assets	-1
Total debt (DD1+DLTT) greater than assets	-95
Missing DD1 or missing DLTT	-1
Notes payable over assets > 1%	-842
DLTT < (DD2+DD3+DD4+DD5)	-524
Fiscal year end months 2, 3, 4, 5, 6, 7, 8	-278
DLTT / assets > 5%	-469
Missing outcome and control variables	-157
Number of firms after sample selection screens	844

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