# Entrepreneurial Wealth and Employment: Tracing Out the Effects of a Stock Market Crash

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April 6, 2021

**Abstract**

I provide evidence that adverse shocks to the wealth of business owners during the Financial Crisis had large effects on their firms' financing, employment, and investment. I use individual-level portfolio data from Norway to exploit the dispersion in stock returns during 2008–09 as a source of exogenous variation in entrepreneurs' wealth. I then trace out the effects of these shocks to the entrepreneurs' privately-held firms. I find that the adverse employment and investment effects are primarily driven by young firms who—relative to mature firms—obtain considerably less bank financing following an owner wealth shock. Firms adjust employment primarily through hiring less, rather than firing, consistent with firms providing extensive-margin insurance for existing workers. These findings provide a causal link between asset price shocks and the real economy; and document that equity-financing frictions and the procyclicality of entrepreneurial wealth are important channels through which economic shocks amplify.

*Keywords:* Financial Crisis, Employment, Entrepreneurs, Equity Financing
*JEL codes:* G01, G32, G50, E24, J23

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# 1 Introduction

Recent research has shown that financial shocks have large effects on employment during economic crises. This literature has underlined the importance of securing bank financing to firms, which has had extensive and persistent policy implications (Crouzet and Tourre, 2020). However, this literature is largely mute on the effects of adverse shocks to the financial health of business owners who serve as the primary source of funding for many small firms.[1] While economic crises see disruptions to bank lending, they also see large drops in the wealth and liquidity of business owners. In the presence of financial frictions, this procyclicality of entrepreneurial wealth may work to amplify the consequences of adverse economic shocks. This idea lies at the heart of the financial accelerator framework by Bernanke, Gertler, and Gilchrist (1999), an important underpinning of modern macroeconomics. Adverse shocks to entrepreneurial wealth are likely to come at the worst of times: when credit supply is weak, revenues are low, and business uncertainty is high. Yet evidence of how shocks to entrepreneurs' wealth affect the real economy, through the employment and investment of their firms, is lacking. This presents a limitation to our understanding of how economic shocks amplify and thereby which policies should be pursued to curtail unemployment during a crisis.

The scarcity of empirical research on this subject is rooted in long-standing data limitations and identification challenges. Data that links information on business owners' wealth with the characteristics of their firms is limited. To the extent that it is obtainable, we face the issue that the level of (or any changes to) the owners' wealth is likely highly correlated with, or even endogenous to, their firms' investment opportunities. I overcome these challenges by using administrative tax data from Norway that contains detailed financial information for all Norwegian residents and firms. The central new ingredient is a stockholder registry. This allows me to link detailed data on firms and entrepreneurs through private-firm ownership records. In addition, the security-level data on ownership in listed stocks allows me to quantify idiosyncratic wealth shocks, which are based on the exogenous forward returns of the stocks in the entrepreneur's pre-crisis portfolio. These data, together with educational records and employment registers, allow me to perform a detailed investigation of the real effects of shocks to entrepreneurial wealth. Using stock market losses as the identifying variation in wealth allows me to examine the real effects of stock market crashes, by tracing out the effect of the stock market crash of 2008–09 to investors, the private firms they own, and their employees.

I first exploit the fact that the wealth shocks are idiosyncratic—rather than driven by local conditions, such as house prices. This allows me to provide a novel test of the hypothesis that the firms of adversely-shocked entrepreneurs can substitute toward equity financing from other, unshocked, investors.[2] I find that a moderate financial wealth loss of 10% reduces the probability of an eq-

[1] The 2016 Small Business Credit Survey finds that the most common source of financing for the 61% of employer firms facing financial challenges was the personal funds of the owners.

[2] When identifying from shocks that operate through house prices, alternative local investors may have a decreased ability to invest in new or other firms through, e.g., the collateral channel (Chaney, Sraer, and Thesmar, 2012)

uity injection by a sizable 22%. The finding that idiosyncratic shocks to the wealth of an existing owner substantially lowers the probability of equity injections is consistent with firms facing important frictions in establishing new equity-financing relationships. I find that these effects are significantly stronger for more levered firms. I then study the effect on equity extraction in the form of dividends. While I find little effect for the average firm, this masks significant heterogeneity. Less shocked, more levered firms pay out less dividends. This suggests that wealthier entrepreneurs forego dividends from levered firms during the crisis. More liquid firms, however, pay out substantially more dividends to owners who experience adverse wealth shocks.

I also consider the effect on loans provided by the owner to the firm. I find that more levered firms, who see more equity injections and less extractions following an adverse shock, receive less debt financing from the owner. This is consistent with equity, rather than debt, being the preferred instrument to finance more levered firms in a recessionary environment.

Using the employer-employee register, I continue by exploring the real economic effects in terms of employment outcomes. I find that wealth shocks only affect employment growth when the ex-ante wealth of the owner is similar in magnitude to the potential financing needs of firm. These are the cases where the owner would be a viable source of financing absent a wealth shock, but not necessarily resilient to sizable portfolio losses. In these cases, I find economically large effects: A loss of 10% of the owner's financial wealth reduces employment growth from 2007 to 2010 by almost 5 percentage points. A central new finding is that these effects are almost entirely driven by younger firms, whose employment shows no sign of recovery even five years after the onset of the crisis.

I provide a novel decomposition of this change in employment growth into firing and hiring. This shows that firms provide extensive-margin insurance to existing workers against tightened financial constraints—by passing the shocks on to potential new workers. Rather than displacing existing employees, adversely-shocked firms respond by reducing their hiring. Conditional on hiring, I find that shocked younger firms hire differentially fewer college-educated workers. This is consistent with more educated workers being costlier to finance or more reluctant to work for firms in financial distress.

To obtain a broader picture of the real effects of entrepreneurial wealth shocks, I employ supplementary capital expenditure data to study the effects on investments. On the extensive margin, I find that adverse wealth shocks significantly lower the propensity of net investment in plants or property. This effect is significantly higher for younger firms. On the intensive margin, I find that effects are modest for the average firm, but strong for younger firms. A loss of 10% of financial wealth lowers their two-year investment to asset ratio by 4.2 percentage points—a vigorous 84% decline relative to the mean investment rate during 2008–09. These effects complement the employment analyses in underlining the scope of the real effects caused by harshened financial frictions for adversely shocked firms.

The finding that young firms are the most affected is consistent with Petersen and Rajan (1994)

who suggest that firms may follow a pecking-order of borrowing over time with an initial high reliance on funding from the owners. Other financial multipliers, such as leverage, do not interact with wealth shocks with similar force. To explore the firm-age heterogeneity further, I examine the differential ability of young versus old firms to substitute towards other sources of financing. These analyses reveal that older firms are able to substitute towards bank-provided financing, while younger firms see a reduction in credit issued by banks. This firm-age heterogeneity in the degree of substitution between bank and equity financing is consistent with the notion that younger firms face more expensive external financing due to having had less time to develop creditor relationships or a financial track record, and are therefore more vulnerable to financing disruptions. I further find no evidence of shocked owners in either young or old firms reducing their ownership share. This would occur if adversely affected owners gave way to new investors. This finding is further evidence of material frictions in equity-financing relationships.

This paper faces two broadly-defined sets of identification challenges, which are related to the selection of listed stocks. First, investors may select into listed stocks that operate in the same or related industries (see, e.g., Døskeland and Hvide 2011). This may cause stock portfolios and private firms to be subject to common shocks. Second, unobservable traits, such as ability or risk aversion, of the investors may lead to correlated outcomes between the listed stocks in his or her portfolio and the private firm he or she owns. The data allows me to address these concerns in multiple ways. Firstly, I show that the results are robust to controlling for a wide range of firm and investor characteristics, such as past portfolio returns or even realized future profitability. I also show that my identifying variation in wealth is uncorrelated with pre-crisis observables. Second, I perform placebo tests by examining the correlation between portfolio losses and firm outcomes when the initial stock market exposure is too low to allow adverse portfolio returns to cause meaningful financing effects. These tests reveal a precisely estimated correlation of zero. Beyond these tests, the absence of pre-trends, the effects on financing, and the observed heterogeneity in the treatment effects support the notion that the observed outcomes are causally driven by harshened financial frictions.

The Financial Crisis of 2007–08 provides a natural laboratory to study how the procyclicality of entrepreneurial wealth may amplify business cycles. However, it is useful to benchmark the key findings against the adverse effects of owner wealth shocks during non-recessionary periods. I extend my analyses in this direction by exploiting the dispersion in portfolio returns experienced both before (2006–07) and after (2010–11) the crisis, and do not find any statistically significant employment effects. While I cannot rule out effects that are of some economic importance, comparing the results suggests that wealth shocks largely work to amplify the adverse economic shocks experienced during an economic crisis, such as reduced availability of credit and lower cash flows.

The analyses in this paper may be viewed as a comprehensive test of the null hypothesis that adverse shocks to business owner wealth do not affect real outcomes in firms. This strong null hypothesis can be decomposed into less trivial layers, which my empirical analyses shed new light on. (**i**) Business

owners do not expose themselves to risk that can lead to adverse liquidity shocks occurring when their firms need financing. In the early 2000s, stock markets plummeted and unemployment rose following the burst of the IT bubble. The possibility that such an event could reoccur should certainly be salient among entrepreneurs prior to the Financial Crisis. (**ii**) To the extent that such events do occur, firms are able to substitute towards other sources of financing, e.g., by obtaining equity injections from other investors or individuals in the entrepreneur's network, leaving financing outcomes unaffected. (**iii**) To the extent that financing outcomes are affected, real outcomes are not, as firms can substitute toward bank financing. (**iv**) If they cannot, their investments and employment outcomes are not dependent on owner or bank-provided financing, for other reasons, such as the availability or trade credit or the willingness of workers to delay wage payments. While combinations of (iii) and (iv) have seen considerable empirical attention, the extent of equity-related financial frictions, their interaction with bank lending, and their real economic effects have not, and are particularly novel to this paper.

My paper contributes to multiple related literatures. By showing how shocks to business owners' wealth affect firm-level employment, through increasing the severity of financial frictions, I am adding to a growing literature on financial frictions and employment (see, e.g., Bentolila, Jansen, and Jiménez 2018, Berton, Mocetti, Presbitero, and Richiardi 2018, Juelsrud and Wold 2020, Chodorow-Reich 2014, Benmelech, Frydman, and Papanikolaou 2019, Greenstone, Mas, and Nguyen 2020, Duygan-Bump, Levkov, and Montoriol-Garriga 2015). This literature presents a near consensus that adverse shocks to bank lending reduce employment growth. However, focusing on smaller firms, Greenstone, Mas, and Nguyen (2020) find statistically insignificant and economically small effects of bank-lending contractions on employment growth, even during the Financial Crisis. They argue that small firms may plausibly have access to other sources of credit when bank lending supply contracts. This would be consistent with the hypothesis that small firms do not face substantial procyclical financing constraints—a hypothesis that I can reject in my empirical setting.

The core contribution of my paper is to focus on a different, highly procyclical, and underresearched source of financial frictions that is particularly important for smaller firms. This contribution is facilitated by merging detailed financial data on both incorporated firms and their equity holders. I make additional contributions as well: First, I provide a detailed examination of employment outcomes through (a) distinguishing between the effects on firing and hiring and (b) considering the effect on the educational composition on workers. This exercise presents new evidence that it is the pool of potential new employees—rather than the existing ones—who are affected on the extensive margin. Second, I provide a more complete picture of the real effects of financing disruptions by also studying the effects on capital investments. Third, I provide evidence of a link between equity- and bank-financing frictions by showing that the ones most affected by owner wealth shocks (i.e., young firms) are also those who are less able to substitute towards bank-provided financing.

My results that highlight the importance of business owner wealth for the outcomes of young firms contributes to a large literature on financial frictions and entrepreneurship (see, e.g., Hvide and Møen

2010, Fracassi, Garmaise, Kogan, and Natividad 2016, Cespedes, Huang, and Parra 2019, Andersen and Nielsen 2012, Adelino, Schoar, and Severino 2015, Schmalz, Sraer, and Thesmar 2017, Hurst and Lusardi 2004, Kerr, Kerr, and Nanda 2015, Corradin and Popov 2015) that has largely focused on entrepreneurial entry rather than the effects of shocks to existing firms. While there is growing evidence that local house price appreciation facilitates business growth, there is still no consensus regarding the extent to which the personal wealth facilitates entrepreneurship through alleviating financial frictions (see, e.g., Hurst and Lusardi 2004, Andersen and Nielsen 2012, and Kerr, Kerr, and Nanda 2015). In the broader entrepreneurship literature, this paper complements research on angel financing (Lindsey and Stein 2019, Kerr, Lerner, and Schoar 2014, Denes, Howell, Mezzanotti, Wang, and Xu 2020) by highlighting the importance of the financial circumstances of investors on firm outcomes.[3] It is also consistent with the findings of Townsend (2015) that financial shocks may propagate through common ownership and the findings of Hanspal (2018) that the loss of wealth for sole-proprietors may inhibit business growth. My analyses of age-heterogeneity in the responses to economic shocks contribute to recent studies of differential effects of economic shocks on firm employment (see, e.g., Adelino, Ma, and Robinson 2017, Brown, Earle, and Morgulis 2015). By showing that firms largely insure *existing* workers against financial shocks on the extensive margin, I contribute to the literature on the pass through of firm shocks to workers (see, e.g., Guiso, Pistaferri, and Schivardi 2005, Ellul, Pagano, and Schivardi 2018, Friedrich, Laun, Meghir, and Pistaferri 2019, and Sockin and Sockin 2021). This paper also contributes to a new literature establishing a causal link from stock market fluctuations to the real economy (Di Maggio, Kermani, and Majlesi 2020, Chodorow-Reich, Nenov, and Simsek 2019, Crane, Koch, and Lin 2019).[4]

Finally, this paper contributes to research on financial frictions and wealth inequality (Cagetti and De Nardi 2006 and Quadrini 2000). In frictionless financial markets, financing for profitable projects should be readily available, despite any adverse effects of wealth shocks on the liquidity or risk-tolerance of entrepreneurs. Whether firm financing and real outcomes materially depend on the personal wealth of owners therefore provides a test of the severity of financial frictions in equity markets. These frictions—rather than those in debt markets—are found by Peter (2020) to be key in determining of the level of wealth inequality. Furthermore, my findings indicate that less-wealthy entrepreneurs may be at a significant disadvantage in pursuing growth opportunities during a financial crisis. My findings thus highlight financial frictions as a channel through which economic crises may amplify wealth inequality.

[3] See also the concurrent paper by Berzins, Bøhren, and Stacescu (2020) who study the effect of wealth-tax-induced shareholder illiquidity on firm outcomes; Bahaj, Foulis, and Pinter (2020) who document important (collateral) effects of directors' home values on firms' financing and investment outcomes; Bjørneby, Markussen, and Røed (2020) who find a positive effect of wealth taxes on firm employment growth; and Becker and Hvide 2019 who find large detrimental effects of the entrepreneur's departure through death.

[4] Hanspal (2018) makes a similar concurrent contribution by using stock holdings in failing banks as one of his main explanatory variables.

The paper proceeds as follows: Section 2 describes the data. Section 3 provides a discussion of the empirical strategy. Section 4 considers owner-provided financing results. Section 5 considers firm-level employment outcomes. Section 6 investigates investment outcomes. Section 7 shows additional financing results, and Section 8 concludes.

# 2 Data

## 2.1 Data sources

This paper uses Norwegian administrative data maintained by Statistics Norway, and largely collected by the Norwegian Tax Authorities. Data collection primarily occurs through third-party reporting, where government agencies, financial institutions, and employers report data directly to the tax authorities. The different data sources are linked by using (de-identified) person and firm identification numbers that are consistent across datasets. It covers the universe of Norwegian residents and incorporated domestic firms.

A particularly attractive feature of this data is that it offers the ability to link households and incorporated firms through the Stockholder Register. This register, which is unique in an international setting, was established in 2004 to streamline the reporting of private equity wealth for wealth tax purposes. It allows me to focus on *incorporated* entrepreneurs. These are the type of entrepreneurs who Levine and Rubinstein, 2017) find to "engage in activities and open businesses that are more closely aligned with core conceptions of entrepreneurship"; consistent with the earlier findings of Berglann, Moen, Røed, and Skogstrøm (2011) who initially noted the importance of studying incorporated entrepreneurs in a Norwegian context. The presence of limited liability for incorporated firms implies that the entrepreneurs and their firms firm have (legally) distinct balance sheets. This allows me to study financing outcomes and effects on ownership structure.

I provide an overview of the different data sources below.

**Income Register** (1993–2012): These records contain detailed information about individuals' income, broken down into various sources such as wage income, capital income and government transfers. For the purpose of annual wealth taxation, the data includes information on individuals' wealth, broken down into different asset classes, such as bonds, mutual funds, listed stocks, non-listed stocks, primary and secondary real estate, debt, and deposits.

**Stockholder Register** (2004–2013): This database contains yearly snapshots of ownership positions in all listed and non-listed limited liability companies. It contains security (share type and firm) and owner (firm or person) identifiers, and provides the number of shares held at the end of the year and the amount of dividends that were paid to the stockholder.

**Employer-Employee Register** (1995–2011): This register contains data on each employment relationship between individuals and firms. A record in this dataset is on the firm-plant-employee-year

level. For each record, I observe firm, plant, and employee identifiers, the start and stop dates of the employment relationship, and total monetary compensation.

**Firm Tax Returns** (2004–2013): These records contain detailed data on firms' income and assets in the form of approximately 400 accounting variables. It contains similar variables as those found in common datasets such as BvD Amadeus, but contains a large set of additional variables collected for tax purposes. This database covers the universe of incorporated firms.

Supplementary data includes: Records of family links, addresses, gender and marital status from the **Central Population Register** (1993–2010); Educational attainment for all residents from the **National Education Database** (1993–2012).

I obtain industry identifiers for firms from the tax returns, employer-employee register and the stockholder register, in the form of NACE codes. NACE is the standard industry classification in the European Union and are based on the 4th revision of the U.S. ISIC classification system.

## 2.2 Sample construction

I describe the construction of my dataset below.

**Holdings of listed stocks.** I obtain data on stock portfolios and returns from the Stockholder Register. For listed stocks that are owned by firms, I iterate once on ownership links in order to attribute these shares to private individuals.[5] I use this data to calculate the two-year forward return, $R_{v,t,t+2}$ of an owner's portfolio, based on the owner's portfolio composition at time $t$ (December 31st of that year). At the portfolio-year (equivalently, owner-year) level, I also compute portfolio HHI as the sum of squared portfolio weights within the portfolio and the average of the natural log of the total market cap of the stocks in the portfolio. I drop a very small number of investors who own a large share ($\geq$0.5%) of a listed stock. Some securities have missing price data; I omit these, and owners who on average hold more than 3 such stocks.

**Private firm ownership.** I start with the Stockholder Register for limited liability companies, which is on the owner-firm-year level. I exclude all firms that have been or become publicly listed. The owner can be a firm, thus I iterate once on the ownership links to uncover individuals who own firms indirectly. I exclude firms for which I cannot attribute ownership to at least 75% of the shares in a company after this procedure.

**Firm employment.** I link all employees to firms using the Employer-Employee Register, which is on the firm-plant-employee-year level. I first aggregate all variables to the firm-employee-year level. I then merge this data with the Central Population Register and the National Education database, both of which are on the individual level. I merge this data again, on the firm-individual-year level with the stockholder register to distinguish between regular employees and owner-employees. I then aggregate

[5] Investors may choose to own stocks and other financial assets through LLCs for tax smoothing reasons.

this data to the firm-year level. I create means of education variables by weighting individuals by the duration of their within-year employment.

**Firm-owner-year-level dataset.** My main analysis dataset is then created by merging the private firm ownership dataset (firm-owner-year level) with the the employment data in order to establish whether the owner is employed (on the firm-employee-year level); I then obtain firm-level employment characteristics by merging with the firm-year level employment data (described above); I merge this with firm tax records (firm-year level); owner tax returns (individual-year level); owner education (individual level); owner gender and other demographics (individual level); and finally with the dataset on holdings of listed stocks (owner-year level), which is described above.

I then only keep firm-investor observations that satisfy the following criteria in 2007: (1) Firm profitability (profits/revenue) must be between -75% and 75%; (2) The investor must own at least 1% of the firm; the firm must employ at least 1 non-owner. This leaves me with 67,000 firm-investor level observations in 2007, and approximately 32,000 firms employing around 300,000 non-owner employees.

For my analyses, I focus on the subset of firm-investor observations where I observe that the investor owns listed stocks. This reduces my sample to 20,000 firm-investor observations. This reduction is due to the fact that many investors primarily hold mutual funds, for which I do not observe the exact portfolio composition—and I am therefore unable to exploit cross-sectional variation in returns across funds. I make the additional restriction that the owner must have at least 1% of his or her Gross Financial Wealth (GFW) in listed stocks, and that the stock market holdings make up at least 1% of the average of the past 2 years' operating expenditures of the firm he or she owns, since these observations do not provide much additional identifying variation. This reduces the number of firm-investor observations in 2007 to approximately 4,700.

### 2.2.1 Definitions of Key Variables

**Gross Financial Wealth (GFW).** This is the sum of domestically held financial assets: deposits, bonds, mutual fund holdings, listed stocks, taxable value of private equity holdings, and outstanding claims. Deposits, bonds, mutual funds, and listed stocks are third-party reported (e.g., banks or other financial intermediaries) and are marked to market. Private equity holdings are provided in the firm's tax returns to the tax authorities and is then pre-filled onto investors' tax returns. It is roughly computed as the owner's ownership share * (historical cost of firm assets + market value of financial assets - firm debt).

**Stock Market Exposure.** This is the fraction of the owner's Gross Financial Wealth that is invested in listed stocks.

$$\frac{Stocks_{v,t}}{GFW_{v,t}} \tag{1}$$

**Returns**. For brevity, I will refer to "intended returns" as just returns. (Intended) returns are the returns that an investor, $v$, would experience from time $t$ to $t+j$, assuming that the portfolio composition does not change after time $t$, i.e. the weight, $w_{v,s,t}$, of investor, $v$'s, holdings of stock, $s$, does not change between time $t$ and $t+j$. In subsection A.8 in the Appendix, I present evidence consistent with a strong relationship between intended and experienced returns.

$$R_{v,t,t+j} = \sum_s R_{v,s,t,t+j} \cdot w_{v,s,t} \tag{2}$$

**Stock Market Wealth Shock.** My main explanatory variable is the product of the owner's intended returns (equation 2) and the owner's stock market exposure (equation 1). This product gives us the fraction of the owner's wealth that he or she is "intended" to lose.[6]

$$\frac{Gains_{v,t+1,t+2}}{GFW_{v,t}} = R_{v,t,t+2} \cdot \frac{Stocks_{v,t}}{GFW_{v,t}} \tag{3}$$

This empirical design only uses variation coming from ownership in listed stocks. While mutual fund ownership is extensive, I do not observe the ownership composition in terms of funds or underlying securities. I only observe the yearly amount held in mutual funds, which precludes me me from using ex-ante portfolio composition as an instrument.

### 2.3 Summary Statistics

The average firm in my sample has NOK 5.6 MNOK (USD 930,000) in assets, and employs 8.54 individuals, out of which 6.96 are regular employees and not owners in the firm. This dataset is thus well suited to analyze the behavior of smaller firms who account for a sizable fraction of employment in most countries.[7]

My sample is limited to incorporated firms. This is a necessary feature of studying the link between household and corporate finance, since unincorporated firms, i.e., sole proprietors, do not have balance sheets that are legally separated from their sole owner. It is also a necessary feature of studying employment effects, as unincorporated Norwegian sole proprietors rarely have employees.[8]

In Table A.3 in the Appendix, I explore how firms whose owners own listed stocks differ from other firms. I find that they are generally somewhat older (13.8 versus 12.4 years), have higher profitability (10% versus 7%), and are less levered (50% versus 56%). The differences are not dramatic, but suggest

[6]Since I do not observe within-year transactions, I cannot use this as an instrument in a traditional IV setting. If, for example, I use this as an instrument for log-differenced GFW, the first stage could be (endogenously) biased downward if entrepreneurs reduce their consumption to offset losses. However, I discuss how one might want to scale the reduced-form estimates in subsection A.8.

[7]In 2017, 28% of U.S. private sector employees worked at firms employing less than 50 people. In Norway this fraction is closer to 50%.

[8]As of 2008, there were 146,903 sole proprietorships (enkeltpersonforetak) in Norway. Only 15% of these had any employees, and only 2% had more than four employees. Source: Statistics Norway (statistics series 09107)

that the firms that are in my sample were somewhat less financially constrained *prior to* the financial crisis. It would therefore be likely that the elasticities that I uncover would understate those found in a broader sample of firms, which would include firms that were more constrained ex-ante.

In Figure A.1 in the Appendix, I compare the industry composition of the firms in my analysis sample with a broader sample of Norwegian firms. The main sample restriction is that the firm must have one or more owners who own listed stocks.

I observe that oil and gas companies do not have a sizable presence. This is connected to the fact that oil and gas companies tend to be very large and are often listed on the stock exchange or have more complicated ownership structures with a large presence of foreign ownership. Firms for whom I cannot link a majority of ownership shares to private, domestic individuals through at most one holding company are are excluded from my data.

The largest industry group in my sample is professional services. This group consists mostly of accounting firms (30%), engineering consulting services (25%), and auditing firms (10%). Due to the sizable overrepresentation (by about 100%) of professional services (PS) in my analysis sample, caused by restricting the sample to owners with a meaningful listed stock portfolio, I (down-)weight these firms by a factor of 0.5 in my analyses on employment and investment effects. This re-weighting, however, does not matter: My main results on employment are highly robust to different weighting schemes, which include not (down-)weighting PS firms as well as completely omitting them. The results of these robustness tests are reported in Table A.11 in the Appendix.

Table 1: Firm characteristics

| | N | mean | sd | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|---|
| log(Assets) | 4051 | 14.89 | 1.14 | 13.54 | 14.12 | 14.84 | 15.57 | 16.38 |
| Firm Age | 4051 | 13.77 | 11.56 | 2.00 | 6.00 | 11.00 | 19.00 | 26.00 |
| Leverage (LT) | 4026 | 0.09 | 0.17 | 0.00 | 0.00 | 0.00 | 0.10 | 0.34 |
| Leverage (ST) | 4026 | 0.41 | 0.21 | 0.15 | 0.25 | 0.39 | 0.54 | 0.70 |
| Profitability | 4026 | 0.10 | 0.15 | -0.02 | 0.02 | 0.08 | 0.17 | 0.28 |
| Cash/OpEx | 4026 | 0.24 | 0.26 | 0.02 | 0.06 | 0.15 | 0.31 | 0.61 |
| # Owners | 4026 | 2.76 | 1.81 | 1.00 | 1.00 | 2.00 | 4.00 | 5.00 |

All variables are measured in December 2007. LT refers to long-term, and ST refers to short-term. Cash/OpEx is the sum of bank deposits and cash divided by the average of the firm's past two years operating expenditures. # Owners counts the number of owners in the firm, including owners that are not in my analysis sample.

Table 2 provides summary statistics on the firm-owner level. On the firm-owner level, the mean ownership share, which includes spousal co-ownership, is 55%. I find that most owners were also present in previous years: Conditional on the firm existing in 2004, 92% of owners were present also in 2004. I find that 16% (11%) of owners have increased (decreased) the number of shares they own

since 2004. I observe that 71% of owners live in the same city as their firm and 85% live in the same county. There is also a large presence of family businesses. Conditional on not being a sole-owner, 21% of investors co-own with a sibling and 17% are parents of other owners. These (and additional) statistics can be found in Table A.2 in the Appendix.

A majority of owners are also employed in the firm. On average, they have worked in the firm for 11.71 years, and their labor earnings from the firm make up 96% of their total labor earnings, implying that very few owner-employees are also employed elsewhere. 30% of the firms pay dividends, and on average (including those who do not pay dividends) these dividends are equal to 5% of the investors Gross Financial Wealth in 2007, and the sum of dividends in 2008 and 2009 made up, on average, 17% of their 2007 Gross Financial Wealth, implying that the ratio of dividends to financial wealth rose during the financial crisis.

Table 2: Firm-Owner Pair Statistics

| | N | mean | sd | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|---|
| Ownership (Share) | 4783 | 0.55 | 0.34 | 0.10 | 0.25 | 0.50 | 1.00 | 1.00 |
| Employed | 4783 | 0.60 | | | | | | |
| Employment Tenure (Years) | 2669 | 11.75 | 8.47 | 2.00 | 5.00 | 10.00 | 18.00 | 24.00 |
| GFW/OpEx | 4783 | 0.75 | 0.77 | 0.08 | 0.18 | 0.44 | 1.05 | 2.45 |
| Stocks/GFW | 4783 | 0.20 | 0.22 | 0.02 | 0.05 | 0.11 | 0.27 | 0.53 |
| $\text{Gains}_{08-09}$/GFW | 4783 | -0.06 | 0.11 | -0.16 | -0.07 | -0.02 | -0.01 | -0.00 |

All variables are measured as of end-of-year 2007, except $\text{Gains}_{08-09}$, which uses data on stock returns from 2007 to 2009. The unit of observation is the firm-investor level, and each observation is weighted by the owner's stock market exposure such that the weights sum to 1 for each firm.

I provide summary statistics for the characteristics of owners in Table A.1 in the Appendix. They are are primarily men (85%) and are 52 years old on average. The median owner has MNOK 4.6 ($\approx$ USD 770,000) in GFW. The average number of firms owned by a given investor is 1.26. The dataset is therefore unlikely to include many professional investors. I further observe that 91% of owners have a high school degree and 45% have a college degree, which is considerably more than in Schmalz, Sraer, and Thesmar (2017)—who consider a mix of incorporated entrepreneurs and sole-proprietors—but similar to Levine and Rubinstein (2017) who study incorporated entrepreneurs.[9]

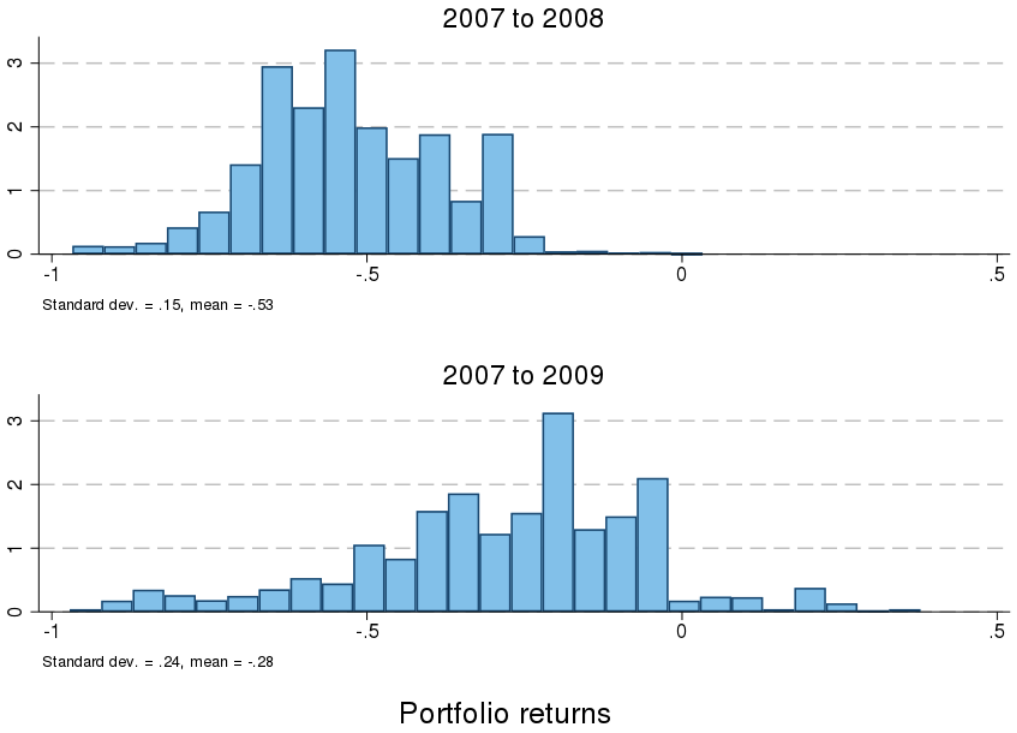
[9] In the sample of French entrepreneurs in Schmalz, Sraer, and Thesmar (2017), only 23% have a college diploma. Levine and Rubinstein (2017) find that 46% of incorporated entrepreneurs in the Current Population Survey (1996-2012) have a college degree, and using the NLSY (1982-2012), they find that 36% of incorporated entrepreneurs have a college degree.

### 2.3.1 Owners' Stock Portfolios

Figure 1 keeps portfolios fixed at the (end-of-year) 2007 composition and provides the distribution of owners' stock returns during 2008 and 2008–09. I see that nearly all investors experienced negative returns from during 2008, and that there is only moderate dispersion in returns (sd=15%). During the two-year period of 2008–09, on the other hand, there is a much larger dispersion (sd=25%)—with almost 5% of investors experiencing positive returns, and 5% of investors experiencing returns south of -74%.

Figure 1: Stock Portfolio Returns

This figure provides histograms of the 1-year (2008) and 2-year (2008–09) returns for the stock portfolios of the business owners in the sample. These returns are calculated by applying realized returns to 2007 end-of-year portfolio weights; following the definition in in equation 2, using $t = 2007$ and $j = 1, 2$.




Beyond providing more cross-sectional variation in wealth shocks, using two-year returns addresses the issue that one-year returns provide highly transitory wealth shocks: The correlation between 2008 and 2009 portfolio returns is approximately $-1$, which is driven by a similar autocorrelation in the underlying stocks on the Oslo Stock Exchange. Thus, the portfolios that performed the worst during 2008 performed fairly well during 2009, making wealth shocks based on 2008 returns alone very short lived. While the effects of transitory shocks are interesting, as they can inform us of potential long-lived effects of only temporarily worsened financial conditions, the limited variation in 1-year returns lower our ability to perform statistical inference. This is clear from supplementary analyses provided

in Figure A.5 in the Appendix, which exhibits confidence intervals of the effects on wealth shocks on employment growth that are twice as large as when using 2-year returns.

### 2.4 Discussion of External Validity

The data sources introduced above offer a unique opportunity to study the interplay between financial markets and the real economy. However, it is worth discussing how Norwegian institutional features may affect the applicability of my findings to other countries.

With respect to the financial crisis, participation in global markets led Norwegian financial markets to see a similarly dramatic decline during 2008–09 as the rest of the world (Bernhardsen et al., 2009). However, the impact of the financial crisis on the real economy was weaker than in many other countries. This feature is typically attributed to the Norwegian government's strong fiscal response (OECD, 2014). Therefore, it seems likely that Norwegian firms faced a smaller reduction in demand, and were therefore were *less* reliant on financing from their owners than firms in worse-hit countries. This is useful to consider given the economic effects of even temporary demand shocks on firms (Hvide and Meling, 2020). It suggests that firms' reliance on owner financing and the real effects of adverse shocks to business owner wealth may be even more important in other settings.

More generally, the Norwegian business environment is comparable to, if not better than, that of most other developed countries (See, e.g., a series of reports by the World Bank that emphasize different features of the economic environment that firms face; World Bank 2006, World Bank 2008, and World Bank 2014). In 2006, for example, Norway placed 9th in the World Bank's overall "Ease of Doing Business" ranking, not far below the United States. In terms of ownership structure, there is a significant presence of family-owned firms, comparable to most other countries (Bøhren, Stacescu, Almli, and Søndergaard, 2019).

While employee turnover rates in Norway are relatively high and comparable to the U.S. (Bhuller, Kostol, and Vigtel 2021), a notable feature is the presence of mandatory notice periods of 1 to 3 months. This implies that workers are entitled to 1 to 3 months of salary after notification of dismissal is given. This lowers the net cost-saving benefit of layoffs relative to at-will employment countries, such as the U.S, and implies a potentially lower pass-through of economic shocks to existing workers in Norway. However, Norway is by no means an outlier in an international context. Firing costs, in terms of weeks of salary that must be provided after a layoff notice, is lower than, e.g., Germany, the Netherlands, and the United Kingdom (World Bank, 2008).

Finally, the strong social safety net in Norway is unlikely to drive any of our findings. As in other countries, workers face considerable uninsured income risk (see, e.g., Fagereng, Guiso, and Pistaferri 2017 for a discussion). This is particularly true for entrepreneurs, who receive relatively more income from uninsured sources, such as dividends.

# 3 Empirical Strategy

## 3.1 Empirical Specification

My main regression specification to estimate the effect of owner wealth shocks on firm outcomes, $Y_{f,v}$, is the following:[10]

$$Y_{f,v,t} = \alpha_{r,t} + \alpha_{n,t} + \beta_t \frac{Gains_{v,08,09}}{GFW_{v,07}} + \Gamma_t \left( \frac{Stocks_{v,07}}{GFW_{v,07}} \right) + \rho_t' P_{v,07} + \eta_t' V_{v,07} + \zeta_t' F_{f,07} + \varepsilon_{f,v,t}, \quad (4)$$

where the $v$ subscript indicates the owner/investor and the $f$ subscript indicates the firm. Outcome variables, $Y$, are primarily measured at the firm level, but some outcomes, such as dividend flows, are measured at the firm-owner (f,v) level. Equation 4 is estimated separately for different $t$ whenever event-plots are provided. Similar to Chodorow-Reich (2014), I typically measure firm-level outcomes as (cumulative) growth rates relative to 2007, which removes firm-level fixed effects in the level of the variable of interest.[11] $\alpha_{r,t}$ and $\alpha_{n,t}$ are region and industry fixed effects, respectively. I primarily use 3-digit NACE codes, but show that the main results are unchanged when using 3- or 4-digit codes. Regions are the 19 Norwegian counties. The main coefficient of interest is $\beta$, which measures the effect of wealth shocks on $Y_{f,v}$. The shock variable $Gains_{v,08,09}/GFW_{v,07}$ is the fraction of the owner's financial wealth to be gained (or mostly lost) in the stock market during 2008 and 2009 when the portfolio composition is kept constant at its 2007 configuration.

My main control variable is the fraction of wealth invested in the stock market per December 31st 2007, $Stocks_{v,07}/GFW_{v,07}$, and its square: $\Gamma_t(x) = \gamma_{1,t}x + \gamma_{2,t}x^2$. These terms are intended to flexibly account for the fact that more exposed owners may have firms with different economic trajectories. For example, owners may optimally choose to allocate more of their assets to the stock market, rather than to their own firm, when their firm's growth opportunities are limited.

$P$ is a vector of portfolio controls, including the portfolio HHI and the average log size, as defined by stock market capitalization, of the firms in the portfolio. It also includes a variable indicating the fraction of listed stocks held through a holding company (a separate LLC). I perform robustness tests where I also condition on past returns by including 1st through 4th order polynomials in demeaned portfolio returns between 2005 and 2007.[12]

$V$ is a vector of investor controls, including age bins (20-35; 36-45 ;46-55 ;55-67 ;67+), log(GFW), log(debt), log(labor earnings), dummies for educational attainment (compulsory, high-school, and

[10] $Y_{f,v}$ includes a subscript $v$, since for some outcomes the variable is measured on the firm-investor level, e.g., dividend flows.

[11] This is also the approach used by Benmelech, Frydman, and Papanikolaou (2019) who center event time around 1928, one year prior to the onset of the Great Depression.

[12] I do not do this in my main specifications as it reduces my sample size by about 12%. However, I find that my results are unaffected by including these controls.

college), and the share of financial wealth invested in mutual funds.

$F$ is a vector of firm controls including long-term (LT) and short-term (ST) leverage, log(Assets), log(OpEx), log(total payroll), Profitability (profits/revenue), and liquidity, as measured by Cash/OpEx, which is the sum of cash and bank deposits divided by the firm's average operating expenditures over the past two years. In specifications including both $F$ and $V$ controls, I also include the relative size of the investor, as measured by the ratio of the owner's GFW to the firm's OpEx.

In my regressions on firm-level outcomes, I apply weights to ensure that firms are equally weighted in the presence of multiple investors. While the average firm has 2.75 owners, most of these owners will not appear in the regression at the same time, since I only include owners with a stock market exposure (Stocks/GFW) exceeding 0.01. Only a handful of firms have multiple business-owners satisfying this requirement, and for those firms I weight the investors according to the size of their stock market portfolio. In robustness checks, I show that my main results are virtually unchanged when instead weighting by ownership share or applying equal weights for all observations.

In my baseline approach, I two-way cluster standard errors on the firm and investor level. I find that my estimated standard errors shrink slightly when instead two-way cluster on the level of the largest firm in the owner's stock portfolio and the firm's 3-digit NACE code.

### 3.2 Discussion of Internal Validity

A strong version of the identifying assumption is that portfolio-induced wealth shocks $\left(\frac{Gains_{v,08,09}}{GFW_{v,07}}\right)$ are orthogonal to changes in the firm's investment opportunity once controlling for over-all stock market exposure $\left(\frac{Stocks_{v,07}}{GFW_{v,07}}\right)$. This essentially says that the composition of the stocks in an entrepreneur's portfolio is uncorrelated with the economic outlooks of the firm. This assumption may seem overly strong, since investors may have a bias towards investing in firms that are close in both a geographic and industry sense (Døskeland and Hvide, 2011). A weaker version of the identifying assumption is that this orthogonality holds once I condition on geographic and industry fixed effects. While this is the primary identifying assumption that I rely on, I explore how portfolio returns covary with industry characteristics in Figure A.4 in the Appendix. This reveals no material (unconditional) correlation between the forward returns of the 2007 portfolio and characteristics such as industry concentration, average crisis-period employment growth, or pre-crisis profitability. I explore conditional (on, e.g., region and industry fixed effects) correlations in more detail in the next subsections.

The strength of the identification in this paper is that—conditional on portfolio selection—the exact returns that the entrepreneur's portfolio will experience is fully exogenous to the entrepreneur. This overcomes the issue that changes to the entrepreneur's wealth may be correlated with—or endogenous to—their investment opportunities. For example, entrepreneurs may save more to offset lower expected cash-flows in the future, or strategically save in assets whose returns are affected by the firm's actions (Decaire and Sosyura, 2020). They may also save more if their expected return on capital goes

down, and income effects dominate intertemporal substitution effects as in Ring (2020).

Since individual returns are exogenous, but portfolio stocks are selected, the key question for assessing the validity of the empirical design is thus the following. To which extent did entrepreneurs, *prior to* the crisis, select stocks whose *subsequent* returns were correlated with other factors affecting firm performance during the crisis? In the two subsections below, I discuss these potential selection problems and present tests that address the potential severity of these issues. In addition, throughout the paper, I present several robustness exercises and results on heterogeneous responses that are inconsistent with sizable confounding due to selection.

### 3.2.1 Assessing Internal Validity: Correlation Between Portfolio Returns and Firm Outcomes

If investors are biased towards selecting listed stocks that are geographically close or operate the same or related industries, correlations between stock market wealth shocks and firm outcomes may be confounded if my region and industry fixed effects do not fully capture this selection. For example, an investor may own an engineering consulting firm that specializes in the oil industry, but the industry code only specifies his or her firm as a structural engineering consultancy. This investor further owns a significant amount of oil-related stocks, which means that their stock portfolio and their firm may be subject to common shocks.

Changes in firm outcomes may be driven by either common shocks or by tightened financial constraints. It is thus challenging to appropriately test for the severity of common shocks. To circumvent this, I design the following placebo test. I focus on a sample of business owners whose stock market exposure is low. The purpose is to consider a sample of business owners who may own listed and private firms subject to common shocks, but where experiencing very negative stock returns have only an immaterial effect on their ability to provide financing. These individual investors may have a low stock market exposure due to having allocated more of their wealth to other sources, such as mutual funds, bonds, or deposits, rather than to listed stocks. Since I do not observe *which* mutual funds they hold, mutual fund returns are not included in their forward returns, $R_{v,08,09}$. This provides a meaningfully large set of investors, $v$, whose returns, $R_{v,08,09}$, can be used to construct a placebo test.

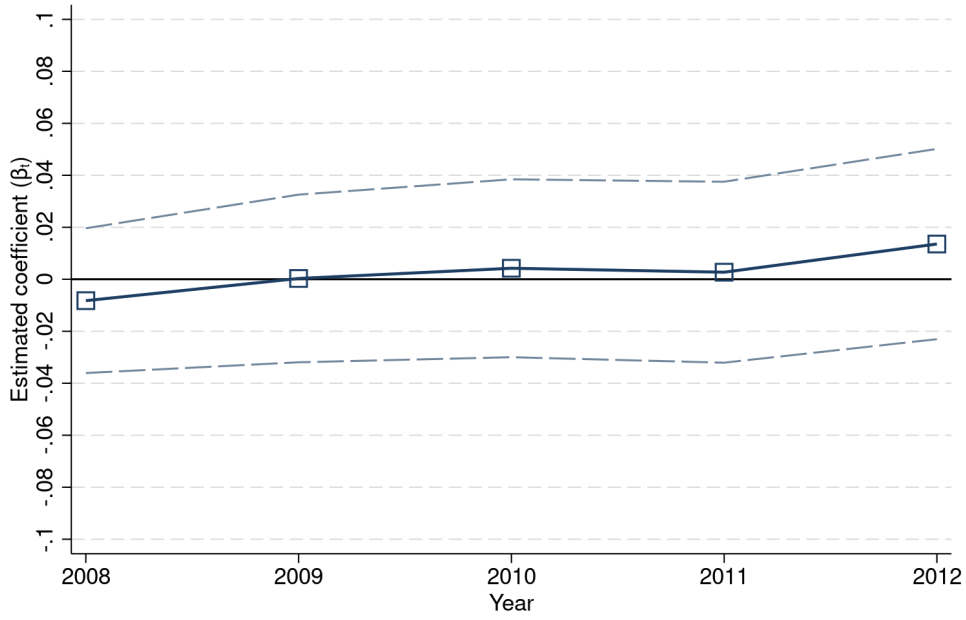
More specifically, I restrict the sample to owners with stock market exposures between 0.25% and 10% of their total financial wealth. I require at least 0.25% exposure to limit the number of investors with trivially small portfolios. This yields a sample of 5,648 firm-owner pairs.[13] I report the results in Figure 2, where I find no significant correlation between stock returns and either concurrent or future revenue growth. A one standard deviation increase in 2008–09 returns (0.24) is associated with

[13] In my analysis samples I only include investors with at least 1% exposure, here I include less exposed investors to increase the sample size and precision. Excluding the the investors with $< 1\%$ exposure yields the same results, but larger standard errors.

excess revenue growth inside a 95% confidence interval of about ±1%. I perform the same exercise for changes in profitability, and similarly find no economically meaningful or statistically significant correlations. These results are presented in the Appendix in Figure A.2.

Figure 2: Placebo Test: Correlation Between Portfolio Returns And Revenue Growth when Stock Market Exposure is Low

Results are obtained from regressing year-on-year revenue growth on portfolio returns. I provide the estimated coefficient on portfolio returns ($R_{v,08,09}$) from estimating the following equation: $(Revenue_{f,t} - Revenue_{f,t-1})/(0.5 \cdot Revenue_{f,t} + 0.5 \cdot Revenue_{f,t-1}) = \alpha_{n,t} + \alpha_{r,t} + \alpha_{n,t} + \beta_t R_{v,07,09} + \rho_t' P_{v,07} + \eta_t' V_{v,07} + \zeta_t' F_{f,07} + \varepsilon_{f,v,t}$, for $t = 2008, ..., 2012$. The analysis is limited to investors with a stock market exposure between 0.25% and 10% in 2007. The blue dashed lines provide the 95% confidence interval.




While these tests do not allow me to reject the presence of any selection issues, they do suggest that the severity of these issues is modest in this empirical setting.[14]

### 3.2.2 Assessing Internal Validity: Investor Characteristics and Portfolio Returns

Investors who lost more during the 2008–09 may have picked more procyclical listed stocks *and* private firms. However, I show in my results on employment that this does not seem to be the case. Wealth losses during 2008–09 predict lower employment growth during 2008–10, but not before. My results are also robust to controlling for past returns and their higher order moments. One may also worry that effects are confounded by ability. However, my analyses do not support this notion. For example, Table A.8 in the Appendix reveals no statistically significant correlation between profitability, educational attainment, or portfolio diversification with future stock returns. This results still hold

[14] This is not inconsistent with (Døskeland and Hvide, 2011) since I condition on county and industry fixed effects and consider a specific sample of investors (i.e., entrepreneurs).

when I condition on the owner having been present in the firm for multiple years, thus giving the owners ability time to materialize as higher profitability for the firm. My main employment results are also robust to controlling for both past and future profitability. Finally, the heterogeneity of my results does not seem consistent with these confounders driving my results. Again, I cannot reject that there is, for example, some measure of ability that is correlated with both stock returns and the firm's performance during the crisis. However, my analyses suggest that the importance of this mechanism is limited relative to the effects that portfolio losses have on increasing the severity of financial frictions.

### 3.2.3 Differences on Observables: Whether Wealth Shocks are Correlated with Pre-period Observables

I now investigate whether firms whose owners were more exposed to the stock market or experienced larger wealth losses were different on observable characteristics. I regress firm characteristics on the wealth shock and stock market wealth exposure variables as well as industry and region fixed effects. Taking out these fixed effects allow us to interpret the coefficients as indicators of how these firms differ from other firms in the same region or area. It also allows us to to partially test the main identifying assumption that wealth shocks, once I condition on overall exposure as well as industry and region fixed effects, are uncorrelated with other confounding factors that may affect employment growth.

Table 3: Wealth Shocks and Lagged Observables

| | (1) Profitability | (2) log(Assets) | (3) Leverage | (4) Cash/OpEx | (5) log(Firm Age) | (6) Frac. College |
|---|---|---|---|---|---|---|
| $\text{Gains}_{08-09}/\text{GFW}_{07}$ | -0.032 | -0.339 | -0.049 | 0.016 | 0.121 | 0.044 |
| | (0.024) | (0.218) | (0.049) | (0.041) | (0.157) | (0.060) |
| $\text{Stocks}_{07}/\text{GFW}_{07}$ | -0.143*** | -0.850*** | 0.351*** | -0.418*** | -0.455** | 0.042 |
| | (0.031) | (0.261) | (0.053) | (0.050) | (0.181) | (0.060) |
| $(\text{Stocks}_{07}/\text{GFW}_{07})^2$ | 0.090** | 0.595** | -0.240*** | 0.304*** | 0.310 | -0.006 |
| | (0.036) | (0.300) | (0.061) | (0.057) | (0.212) | (0.070) |
| $E[\Delta Y \mid \Delta Stocks/GFW = 1 \text{ sdev}]$ | -.02 | -.107 | .044 | -.051 | -.059 | .008 |
| mean(Y) | .096 | 14.927 | .498 | .228 | 2.424 | .195 |
| sdev(Y) | .146 | 1.15 | .234 | .252 | .777 | .304 |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.154 | 0.200 | 0.124 | 0.192 | 0.114 | 0.272 |
| N | 4750 | 4750 | 4750 | 4750 | 4750 | 4750 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $Gains_{08-09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. The difference in Y based on changing $Stocks/GFW$ by one standard deviation evaluates $Stocks/GFW$ at the mean. Frac. College refers to the fraction of employees with a college degree.

Reassuringly, I find that my shock variable is not predictive of pre-shock observable characteristics of the firm. However, I do find, perhaps unsurprisingly, that firms whose investors had allocated a larger share of their financial wealth to the stock market (Stocks/GFW) are different. I find that these differences are best characterized by a second order polynomial in Stocks/GFW. To ease readability, I provide the predicted differences in my Y-variables based on increasing Stocks/GFW by one standard deviation (0.22) from the mean (0.20).

Firms with more exposed owners (one standard deviation from the mean) are less profitable (2 percentage points or 21%), smaller (0.11 log points), more levered (4.4 percentage points, or 10%), less liquid (5.1 percentage points or 22%), and slightly younger (0.06 log points). I do not find that that these firms differ on the fraction of workers with a college degree. I note that this is conditional on a sample of firms whose owners had a stock market exposure $\geq 1\%$. These differences serve to reduce the gap between the firms in my analysis sample who hold a non-trivial amount of listed stocks and the superset of firms whose owners may or may not hold listed stocks. This can be seen in Table A.3 in the Appendix. Here, I also show that more exposed firms had higher past employment growth than less exposed firms, but in the fourth quartile of exposure past employment growth is identical as to the over-all population of firms. Conclusions are similar for leverage and profitability.

# 4 Results on Owner-provided Financing

I now examine whether and how wealth shocks affect the owner-provided financing of firms. This offers a test of a two-layered hypothesis. Firstly, we may expect that entrepreneurs do not allocate a significant portion of their liquid financial wealth to risky assets outside of the firm if it is conceivable that such assets may be vital for the firm in the near future. Less than ten years prior to the Financial Crisis, the IT bubble burst, stock markets crashed, and Norway experienced unemployment levels exceeding that of the Financial Crisis.[15] That such an event might reoccur should thus have been a salient potential outcome for Norwegian entrepreneurs. Therefore, we might expect that adverse shocks to business owner wealth to realize only when there is no need for financing.

The second layer of the hypothesis is that if there is demand for financing, then financing outcomes will still not be affected, as firms can substitute towards other equity investors. Thus, an idiosyncratic shock to the wealth of an existing owner would not affect, e.g., the probability of the firm receiving an equity injection. Relatedly, we would also not expect to find an effect on dividend payments, as owners could forego these by, e.g., selling an equity stake in the firm to salvage personal liquidity.

Testing this hypothesis is particularly informative in my empirical setting, where I obtain identifying variation in wealth, from households' *idiosyncratic* exposure to exogenous variation in stock returns during 2008–09. That the shocks are idiosyncratic is important—because a shocked entrepreneur could, in theory, approach unshocked local investors for financing. This would not be the case if I

[15] See https://fred.stlouisfed.org/series/LMUNRRTTNOM156S.

had used local or regional shocks, such as house price shocks, which is the dominating supplier of identifying variation in the entrepreneurial finance literature (see, e.g., Schmalz, Sraer, and Thesmar 2017, Adelino, Schoar, and Severino 2015, Kerr, Kerr, and Nanda 2015, Corradin and Popov 2015), since alternative local providers of equity-financing, e.g., angel investors or other firms, may have a reduced investment ability through the collateral channel (Chaney, Sraer, and Thesmar, 2012).

I report the results on the related analyses in Table 4. I focus on three outcome variables. Columns (1)-(2) consider the effects on Paid-in-Capital (PIC). The outcome variable is a dummy variable for whether the firm saw an increase in PIC (i.e., an equity injection) during 2008–09.[16] In column (1), I find that a moderate financial wealth loss of 10% reduces the probability of an equity injection by 1.37 percentage points. This is a fairly large effect of 22% when we scale by the unconditional mean probability of an injection (6.1%). This variable is measured at the firm level. This implies that if there are no frictions to raising equity from other (unshocked) investors, and the firm's liquidity needs are unaffected, we should not see any effect. This finding is thus indicative of stickiness in equity-financing relationships. This line of reasoning is employed in the literature on the transmission of bank shocks: If an adverse shock to a bank with which the bank has a relationship translates into reduced borrowing, then this implies frictions in bank-lending relationships (see, e.g., Chodorow-Reich 2014).

In column (2), I show that the effect on PIC is stronger for more levered firms. This is consistent with more levered firms having a higher demand for equity-financing during the crisis.

In columns (3)-(4), I report results on how wealth shocks affect dividend payments at the firm-investor level. I scale dividend payments by the owner's GFW, which facilitates a NOK-for-NOK interpretation. While wealth shocks do not affect dividend payments on average, there is sizable heterogeneity along the dimensions of leverage and liquidity. A wealth loss of NOK 1 increases dividend payments by NOK 0.156 (NOK 0.325) for firms whose leverage (liquidity) is one standard deviation above the mean. The heterogeneity along leverage is consistent with the hypothesis that more-levered firms have a stronger need to retain equity and investors who lost less are more able to allow for equity retention through foregoing dividends. The heterogeneity along liquidity suggests that stock market losers are more likely to offset losses in the stock market by increasing dividend payments when the firm has ample liquidity.

Columns (5-6) consider the effects on owner-provided investor loans. This type of owner-provided financing is similar to equity in that it has very low seniority, but may be a preferable method of liquidity injection since it does not require changing the ownership structure of the firm. The dependent variable that I examine is the change in investor loans on the firm's balance sheet from

[16] Since many firms are close to the regulatory minimum PIC, there are very few events where PIC decreases. The unconditional probability of a PIC increase is 6.1%, there is therefore little room to statistically uncover intensive-margin effects.

2007 to 2009, scaled by the owner's GFW, which also facilitates a NOK-for-NOK interpretation. While I find no effects on average (column 5), this masks significant heterogeneity with respect to liquidity and leverage. A wealth loss of NOK 1 is associated with a 0.33 NOK decrease in investor loans for a firm with $Cash/OpEx$ one standard deviation below the mean. However, this effect is cancelled out if I also increase leverage by one standard deviation. This lowered sensitivity of funding for more levered firms suggests that loans may be an undesirable source of financing for more levered firms. This may be caused by a preference for lower *observable* leverage among distressed firms. In Norway, and most other European countries, private firm accounting data is public information. Increasing leverage might thus send an observable, and potentially negative, signal about firm bankruptcy risk to suppliers and customers.

Table 4: The Effect of Wealth Shocks on Firm Financing: Equity-Injections, Dividend flows and Owner-Provided Loans

| | Financing Outcomes During 2008–09 | | | | | |
|---|---|---|---|---|---|---|
| | ΔPaid-in-Capital > 0 | | Dividends/GFW | | ΔInvestor Loans/GFW | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Gains_{08-09}/GFW_{07}$ | 0.137* | 0.103* | -0.071 | -0.125 | -0.055 | -0.054 |
| | (0.070) | (0.058) | (0.098) | (0.104) | (0.108) | (0.096) |
| $Gains_{08-09}/GFW_{07}$ * Leverage | | 0.146** | | -0.156* | | -0.341*** |
| | | (0.074) | | (0.086) | | (0.126) |
| $Gains_{08-09}/GFW_{07}$ * Cash/OpEx | | 0.023 | | -0.325** | | -0.331** |
| | | (0.073) | | (0.141) | | (0.129) |
| $Gains_{08-09}/GFW_{07}$ * Profitability | | -0.141 | | -0.043 | | 0.168 |
| | | (0.089) | | (0.136) | | (0.145) |
| mean(Y) | .061 | .061 | .148 | .148 | -.001 | -.001 |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1303 | 0.1376 | 0.4721 | 0.4860 | 0.1976 | 0.2080 |
| N | 3722 | 3722 | 3408 | 3408 | 3722 | 3722 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $Gains_{08-09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. Interaction variables are normalized to have a zero mean and a standard deviation of one, and are included as controls, as well as interactions with $Stocks/GFW$. Controls include $Dividends_t/GFW_t$, a $Dividends_t > 0$ dummy, an $InvestorLoans_t > 0$ dummy, $InvestorLoans_t/GFW_t$. ΔPaid-in-Capital $> 0$ is equal to one if $PIC_{09} > PIC_{07}$. Dividends refer to dividends paid during 08 and 09. ΔInvestorLoans = $InvestorLoans_{09}$-$InvestorLoans_{07}$. Paid-in-Capital and Investor loans are measured at the firm level (from firms' tax returns), while dividends are on the firm-investor level (from the Stockholder Register).

In these analyses on firm financing, the sample is limited to firms that do not own any listed stocks. For tax smoothing reasons, business-owners may choose to invest in the stock market and attribute the ownership of these stocks to the firm. Thus, from a tax accounting perspective, the

financial shock would appear within the firm, without any effect on the flow of financing from the owner's outside wealth and the firm. Approximately 14% of firms in my sample are recorded as owning listed stocks.

These analyses cover the core, observable sources of owner-provided financing. However, there may be other ways owners for owners to finance their firms, for example, through delaying the payment of their own wages. Unfortunately, this is hard to distinguish from other loans, as it may appear as both (or either) investor loans or liabilities to employees on the firm's balance sheet.

To examine the presence of pre-trends in financing outcomes, I repeat my analysis while considering outcome variables during 2005–06 (and keeping all the right-hand-side variables the same). Reassuringly, this does not reveal any statistically significant correlations. These results are provided in Table A.6 in the Appendix.

# 5 Results on Employment Growth

## 5.1 Definition of Employment Growth

My sample primarily consists of businesses with fewer than 20 employees. Since many employees are also owners, I omit them when measuring differences in employment levels. This addresses the concern that the employment situation of owners may be affected through other channels, such as the owner responding to adverse wealth shocks by increasing labor supply.[17]

My main measure of employment growth is defined as follows:

$$EG^{D}_{f,t,t+j} \equiv \frac{\#\text{Non-owner Employment Days}_{f,t+j} - \#\text{Non-owner Employment Days}_{f,t}}{\#\text{Employment Days}_{f,t}}, \quad (5)$$

where firms that exit at time $t$ are defined to have zero employment days at time $t$.

This measure is based on days of employment rather than number of employees during the year. This addresses an important potential issue related to using growth rates in end-of-year employment levels. Many firms, such as retailers, may have highly seasonal employment, with peak seasons falling during Christmas. This may lead to a downward bias in the effect of financial shocks on employment if peak-season employment is less sensitive to financial frictions due to, e.g., high cash flow.

My employment growth measure further offers potential advantages relative to using changes in the total of number of employees employed in the course of a given year. While such a growth measure would address the cyclicality issue, it would also count employee turnover as growth. By effectively weighting the number of employees by their days of employment, my measure in equation 5 avoids

[17] These effects have been found to be strong in many settings. See, e.g., Bø, Halvorsen, and Thoresen (2019), Ring (2020), Zator (2020), and Hanspal, Weber, and Wohlfart (2020).

this issue. It further avoids the issue of overweighting high-income workers, which would be the result of weighting employees by their salaries. Of course, salary-weighted employment could be the relevant variable in some settings. I explore this as an outcome in Subsection 5.8 by considering total (non-owner) pay as the outcome variable. In Table A.10 in the Appendix, I show how my main results are affected when using these different measures of employment growth. This comparison suggests that both year-on-year growth and number-of-employee growth may understate the effect of financing shocks on labor demand.

Table 5: Employment Summary Statistics

| | N | mean | sd | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|---|
| 2007 Employment | | | | | | | | |
| # Owner-employees | 4051 | 1.58 | 1.30 | 0.00 | 1.00 | 1.00 | 2.00 | 3.00 |
| # Reg. employees | 4051 | 6.96 | 11.99 | 1.00 | 1.00 | 3.00 | 7.00 | 15.00 |
| Avg. Age | 4051 | 40.53 | 10.71 | 27.00 | 33.00 | 40.00 | 47.40 | 55.50 |
| Avg. Years of Edu. | 4051 | 13.11 | 2.06 | 11.00 | 11.86 | 12.75 | 14.00 | 16.75 |
| Employment Growth | | | | | | | | |
| $EG^D$ 05-07 | 3713 | 0.116 | 0.481 | -0.314 | -0.117 | 0.000 | 0.208 | 0.610 |
| $EG^D$ 07–08 | 4051 | 0.015 | 0.369 | -0.332 | -0.100 | 0.001 | 0.106 | 0.335 |
| $EG^D$ 07–09 | 4051 | 0.001 | 0.458 | -0.500 | -0.199 | 0.000 | 0.154 | 0.457 |
| $EG^D$ 07–10 | 4051 | -0.028 | 0.498 | -0.643 | -0.281 | -0.002 | 0.167 | 0.499 |
| $EG^D$ 07–10, Symmetric | 4051 | -0.161 | 0.593 | -0.947 | -0.328 | -0.002 | 0.154 | 0.399 |
| Decomposing $EG^D$ 07–10 | | | | | | | | |
| New hires | 4051 | 0.260 | 0.388 | 0.000 | 0.000 | 0.086 | 0.392 | 0.723 |
| Separations | 4051 | -0.291 | 0.321 | -0.750 | -0.500 | -0.250 | -0.020 | 0.000 |

$EG^D$ is days-of-employment-weighted employment growth, as defined in equation 5. The main employment growth measure considers growth from end-of-year 2007 to end-of-year 2010, and is denoted $EG^D$ 07–10.

I choose to use the standard formula for percentage change, rather than the symmetric growth rate or log differences for two main reasons. First, using symmetric-growth rates for very small firms that move between 0, 1 and 2 employees can vigorously overstate employment decline. In Table 5, I find that the average employment growth rate from 2007 to 2010 in my sample increases in magnitude from -2.8% to -16.1% when using the symmetric growth rate.[18] Measuring growth rate using log differences has similar issues. Log differences are beneficial to reduce the impact of outliers in the presence of positive growth. However, when there is a *decline* in employment growth, it will increase magnitudes. My sample contains very few firms experiencing sizable positive employment growth,

[18] Other studies using the symmetric growth rate (e.g., Chodorow-Reich 2014) or log differences in employment (e.g., Benmelech, Frydman, and Papanikolaou 2019) are likely much less affected by these issues due to mostly having large firms in their samples.

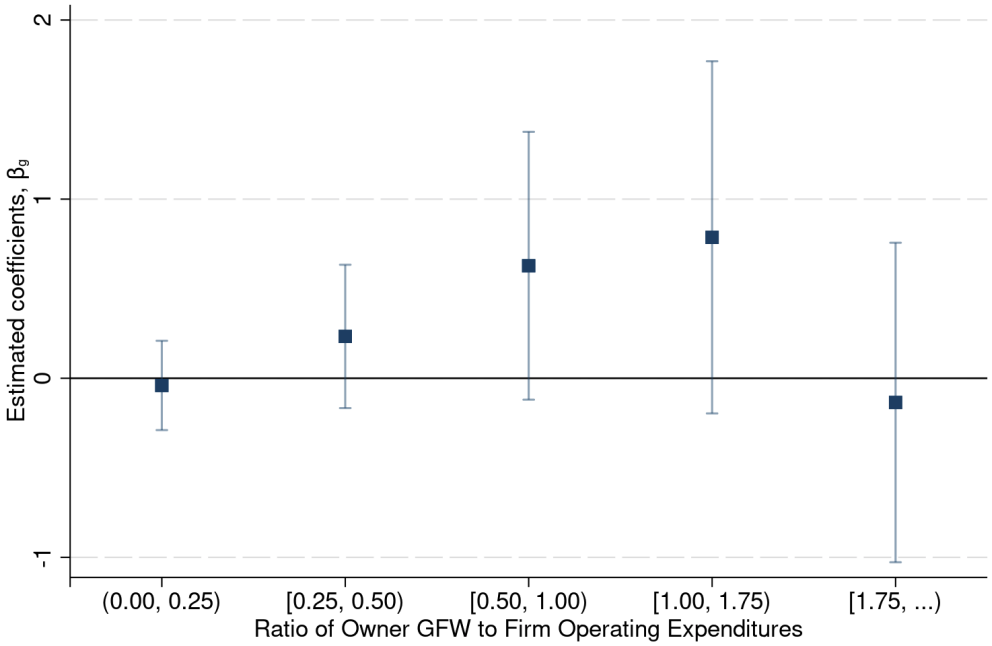
however, to minimize their impact, I bind employment growth to be $\leq 200\%$.

### 5.2 When Do Wealth Shocks Matter for Firm Employment?

If the firm owner has substantial private wealth relative to the financing needs of the firm, he or she would be able to provide financing even after suffering a large negative wealth shock. Consider a firm that has financing needs of \$100 and the owner holds \$300 in wealth. Even after suffering a 50% wealth loss, the owner still has \$150 > \$100 to cover the firm's financing needs. On the other hand, consider an owner who only has \$10 in private wealth. Regardless of whether he or she experiences a 50% wealth loss, he or she would only be able to cover a small fraction of the firm's financing needs.

Figure 3: Differential Effects of Wealth Shocks on Employment Growth Based on Relative Investor-Firm Size

This figure shows the differential effects of wealth shocks on firm employment growth for different bins of the ratio of an owner's Gross Financial Wealth (GFW) to the firm's operating expenditures (OpEx). Point estimates come from estimating equation 6. Vertical lines provide 95% confidence intervals.




If an owner of a firm has little wealth relative to the potential financing needs of the firm ex-ante, any shocks to his or her wealth is unlikely to have impact on the financing available to the firm, ex-post. Similarly, if the owner has substantial wealth relative to the firm, any shocks to the ability, or perhaps willingness, to provide financing may not be large enough to affect the flow of funds to the firm. Thus, I only expect meaningful effects when the owner's status as a viable financier changes. This is exactly what I uncover: I create bins for different ratios of owner financial wealth to firm operating expenditures, and estimate the effect on wealth shocks, as a fraction of the owner's financial

wealth, separately within these bins. These results are reported in Figure 3 below, which are based on estimating equation 6 below.

$$\frac{E_{f,2010} - E_{f,2007}}{E_{f,2007}} = \alpha_n + \alpha_r + \alpha_g + \beta_g \frac{Gains_{v,08,09}}{GFW_{v,07}} + \Gamma_t \left( \frac{Stocks_{v,07}}{GFW_{v,07}} \right) + \rho' P_{v,07} + \eta' V_{v,07} + \zeta' F_{f,07} + \varepsilon_{f,v}. \tag{6}$$

I find that wealth shocks have no effect on firm employment growth when the ratio is less than 25% or above 175%. While a 100% loss for owners with large wealth relative to the firms financing needs would likely constrain them, the owners in my sample rarely experience stock market losses exceeding 50% of financial wealth. This heterogeneity is similar to that reported by Bahaj, Foulis, and Pinter (2020). They find that housing wealth shocks experienced by a firm's directors only affect investments when housing wealth accounts for a meaningful share of the firm's potential financing needs.[19]

In the remainder of my analysis, where I consider the effects on employment and investment outcomes, I restrict my sample to firm-owner pairs where the financial wealth of the owner is between 25% and 175% (100% ± 75%) of the firm's average operating expenditures in 2006 and 2007.

### 5.3 Main Employment Regressions

In Table 6, I report the main results on the employment effects of owner wealth shocks. This baseline analysis considers the effect on employment growth during 2008–10. I show the effect of varying this time horizon in Section 5.5.

The results of the preferred empirical specification are presented in column (6). This demonstrates a substantial elasticity of employment growth to the owner's Gross Financial Wealth (GFW) of 0.49. I find that the estimated coefficient, $\hat{\beta}$, is robust to changes in the set of controls. In column (7), I find that that standard errors shrink when I two-way cluster on firm's 3 digit industry (114 clusters) code and the largest firm in the owner's portfolio (88 clusters), as opposed to on the firm (F) and investor (V) level in columns (1) through (6). My regression sample consists of approximately 2,500 firm-owner observations, including approximately 2,250 firms and 2,320 owners.

One important take-away from traveling from column (1) to column (8) is that my estimated coefficient grows consistently, albeit only slightly, when adding more fine-grained industry and geographic controls. If my results were confounded by industry, regional or industry-region shocks, I would expect to see a noticeable *decrease* rather than increase in estimated coefficient on Gains/GFW when adding fixed effects to account for these confounders. The largest change is observed when I initially introduce 2-digit industry fixed effects in column (3), which suggests that correlations between stock returns and industry shocks are slightly negative rather than positive. Table A.9 in the Appendix reveals virtually

[19] Bahaj, Foulis, and Pinter (2020) employ the cut off that home values must exceed 15% of the firm's assets.

no change in the estimated coefficient on wealth shocks when introducing even more fine-grained (4- and 5-digit NACE codes) industry classification fixed effects.

Table 6: Main Employment Regressions:
The Effects of Wealth Shocks on Employment Growth From 2007 to 2010

| $EG^{D}_{07,10}$ | (1) | (2) | (3) | (4) | (5) | **(6)** | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| $\text{Gains}_{08-09}$/GFW | 0.323* | 0.332* | 0.435** | 0.452** | 0.458** | 0.488** | 0.488*** | 0.522*** |
| | (0.188) | (0.181) | (0.182) | (0.184) | (0.187) | (0.192) | (0.179) | (0.200) |
| Stocks/GFW | 0.027 | -0.015 | 0.049 | 0.048 | 0.012 | -0.075 | -0.075 | 0.042 |
| | (0.086) | (0.185) | (0.181) | (0.180) | (0.181) | (0.189) | (0.210) | (0.250) |
| $(\text{Stocks/GFW})^2$ | | 0.064 | 0.042 | 0.037 | 0.102 | 0.229 | 0.229 | 0.070 |
| | | (0.232) | (0.221) | (0.221) | (0.217) | (0.223) | (0.314) | (0.305) |
| Profitability | | | | | | 0.311*** | 0.311*** | 0.210 |
| | | | | | | (0.118) | (0.107) | (0.150) |
| Leverage (ST) | | | | | | 0.183** | 0.183** | 0.177* |
| | | | | | | (0.083) | (0.085) | (0.101) |
| Leverage (LT) | | | | | | -0.080 | -0.080 | -0.070 |
| | | | | | | (0.089) | (0.077) | (0.112) |
| log(Assets) | | | | | | 0.075*** | 0.075** | 0.069** |
| | | | | | | (0.028) | (0.031) | (0.035) |
| Cash/OpEx | | | | | | -0.176*** | -0.176** | -0.078 |
| | | | | | | (0.064) | (0.078) | (0.079) |
| Lagged 1-Yr E.G. | | | | | | -0.099** | -0.099** | -0.105** |
| | | | | | | (0.041) | (0.038) | (0.047) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y | Y | Y |
| FE | - | - | NACE2 | NACE2,R | NACE3,R | NACE3,R | NACE3,R | NACE3×R |
| Cluster | F,V | F,V | F,V | F,V | F,V | F,V | NACE3,LS | F,V |
| R2 | 0.0023 | 0.0023 | 0.0376 | 0.0490 | 0.0852 | 0.1340 | 0.1340 | 0.3118 |
| N | 2521 | 2521 | 2520 | 2520 | 2496 | 2496 | 2496 | 2099 |

The main coefficient of interest is that on $Gains_{08-09}/GFW_{07}$, which is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. Stocks/GFW is over-all stock market exposure. Columns (1) through (6) and (8) gradually introduce more control variables and fixed effects. ST and LT refer to short-term and long-term liabilities, respectively. Cash/OpEx is the sum of bank deposits and cash scaled by the average of the firm's past 2-year operating expenditures. Standard errors in all columns except (7) are two-way clustered at the firm and investor level. In column (7), standard errors are two-way clustered at the firm's 3 digit industry (114 clusters) and the largest firm in the owner's portfolio (88 clusters). Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

In Table A.9 in the Appendix, I explore robustness to controlling for *future* Profitability (measured in 2010), and find that my results are robust: The coefficient is reduced from 0.488 to 0.412, or by approximately 1/7th, and remains significant at the 5% level. However, this result should be interpreted with caution since profitability may be endogenous to the wealth shock. However, the fact that the coefficient remains large and significant is reassuring. It also indicates that wealth shocks

reduce employment growth through channels not closely linked to profitability. I also control for past, demeaned, returns from 2005 to 2007, including 2nd through 4th order polynomials, and find that the estimated coefficient on the wealth shock variable is unaffected.

### 5.4 Heterogeneity in Employment Effects

In order to better understand the mechanisms at work, I estimate the heterogeneous effects of wealth shocks along the dimensions of profitability, liquidity (as measured by Cash/OpEx), and leverage. In these analyses on real outcomes, I also consider heterogeneity with respect to firm age. The motivation for this is that a given owner-financing shock may have stronger effects on younger firms, since they may find it harder or costlier to substitute towards other sources of financing. This suggests that a given owner-provided financing shock has a larger over-all effect on financing for younger firms, which may translate into stronger effects on employment growth and investment.

In order to estimate the heterogeneous effects, I run the following regressions, interacting my shock and exposure measure with one or multiple variables, $x$:[20]

$$\begin{aligned} EG^{D}_{07,10} &= \alpha_n + \alpha_r + \beta \frac{Gains_{v,08,09}}{GFW_{v,07}} + \Gamma \left( \frac{Stocks_{v,07}}{GFW_{v,07}} \right) \\ &+ \sum_x \left( \xi_x \cdot x + \beta_x \cdot x \cdot \frac{Gains_{v,08,09}}{GFW_{v,07}} + \Gamma_x \left( \frac{Stocks_{v,07}}{GFW_{v,07}} \right) x \right) \\ &+ \rho' P_{v,07} + \eta' V_{v,07} + \zeta' F_{f,07} + \varepsilon_{f,v}. \end{aligned} \tag{7}$$

I report the results of these regressions in Table 7. I employ dummy variables that indicate whether these characteristics are below or above some rounded cutoff near the sample median. I find no statistically significant differences when considering profitability, liquidity, or leverage. This lack of heterogeneity is not too surprising. Firstly, while profitability may imply higher ability to self-finance out of retained earnings, profitability may also proxy for investment opportunities and thus make it more sensitive to changes in the supply of finance from owners. Secondly, the same argument may apply to liquidity, to the extent that the firm is hoarding cash in expectation of upcoming investment opportunities. Thirdly, while more levered firms may be more adversely affected by a contraction in credit supply if lenders discriminate against more levered borrowers—more levered firms may also have superior creditor relationships, and thus be better positioned to substitute toward bank financing.

The theoretical predictions on how wealth shocks interact with firm age are perhaps less ambiguous. Younger firms have had less time to signal their quality and less time to develop relationships with

[20] $\Gamma_x(a)x \equiv \gamma_{1,x} a \cdot x + \gamma_{2,x} a^2 \cdot x$. I thus estimate the slopes on the interaction between each $x$ and stock market exposure and squared stock market exposure.

external financiers. To the extent that firms grow toward their long-run optimal size over time, there is also reason to suspect younger firms still face positive-NPV investment opportunities on average. Consistent with this, I find that the effect of wealth shocks interacts strongly with firm age, and that the average effect on employment growth is almost entirely driven by firms $< 10$ years old.[21]

Table 7: Heterogeneous effects of wealth shocks on employment growth

| $EG^{D}_{f,07,10}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $\text{Gains}_{08-09}$/GFW | 0.488** | 0.131 | 0.567* | 0.238 | 0.472 | 0.076 |
| | (0.192) | (0.192) | (0.333) | (0.279) | (0.325) | (0.402) |
| * Firm Age $< 10$ | | 0.990** | | | | 0.808** |
| | | (0.398) | | | | (0.412) |
| * Profitability $< 10\%$ | | | -0.103 | | | -0.371 |
| | | | (0.396) | | | (0.460) |
| * Cash/OpEx $< 10\%$ | | | | 0.496 | | 0.669 |
| | | | | (0.366) | | (0.451) |
| * Leverage $> 50\%$ | | | | | 0.035 | 0.102 |
| | | | | | (0.399) | (0.419) |
| P, V, and F | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1340 | 0.1368 | 0.1345 | 0.1432 | 0.1343 | 0.1471 |
| N | 2496 | 2496 | 2496 | 2496 | 2496 | 2496 |

Coefficients estimated using equation 7. These specifications also include a (dummy) control variable for whether the firm had any bank loans/financing in 2007. Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

### 5.5 Cumulative Employment Effects

In this subsection, I explore the effects of wealth shocks on cumulative employment growth. It is not obvious whether employment effects are persistent. Improvements in demand for goods and credit supply may allow firms to substitute towards other sources of financing and grow back on track. However, some profitable projects might not reappear. If shocked firms had to permanently let go of these, they might remain below their pre-crisis trajectory indefinitely. Some employment opportunities may also not reappear. Firms may therefore have had to pass on hiring individuals with skills that are in scarce supply.

My analyses reveal rather persistent effects, particularly for younger firms who account for most of the employment effect. Panel (B) of Figure 4 reports the coefficients on the interaction between *Gains/GFW* and *Firm Age* $< 10$. This effect does not appear to subside. The point estimates for cumulative employment growth from 2007 to 2010 are the same as those for 2007 to 2012, with the

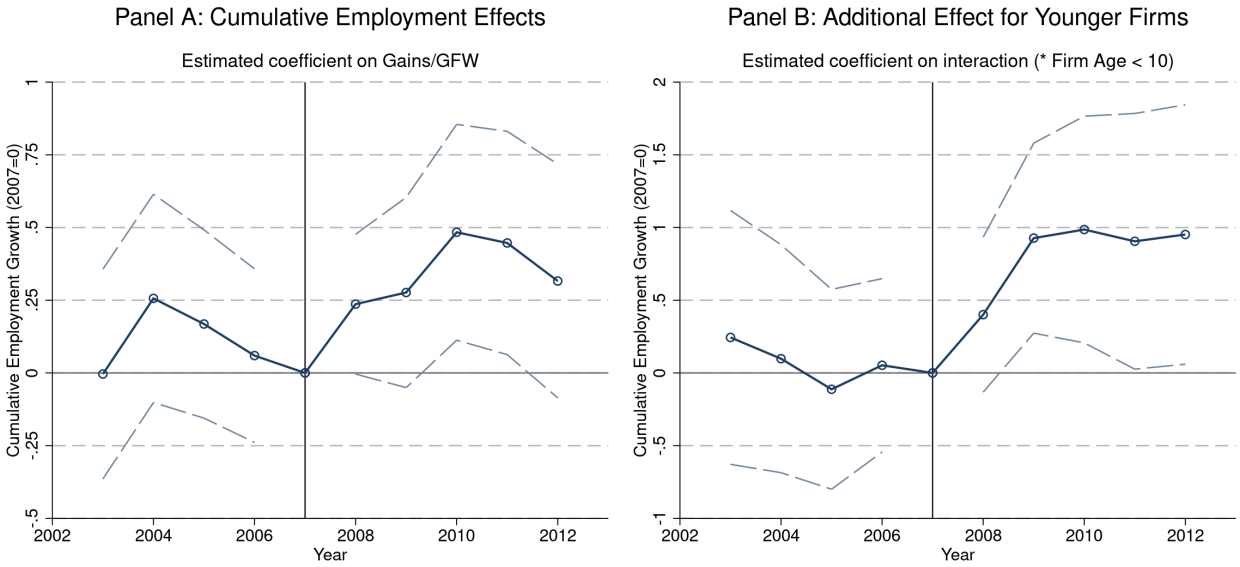
[21] 10 years is the same threshold as used by Rajan and Zingales (1998) to distinguish the external financing needs of young and mature firms. In their context 10 years is the cutoff for time since IPO.

latter remaining statistically significant at the 5% level.

The finding that the (differentially) adverse effects on employment growth last at least 5 years is consistent with Schmalz, Sraer, and Thesmar (2017) who find persistent (8-year) intensive margin effects of housing wealth on employment outcomes for newly created French firms. In the literature on credit market contractions and employment growth, there are varying results on the duration of bank-lending induced financial shocks on employment. Popov and Rocholl (2018) find effects that dissipate within 3 years among German firms connected to less healthy banks during the Financial Crisis. Chodorow-Reich (2014) find that 3-year effects are similar to 2-year effects among mostly larger U.S. firms whose previous lenders reduced credit supply during the Financial Crisis.

Figure 4: Cumulative Effects of Owner Wealth Shocks on Employment Growth

Panel (A) plots the coefficients on $Gains_{08\text{–}09}/GFW$ in regressions of cumulative employment from year $t$ to 2007, $EG^{D}_{07,t}$. The point estimate for 2010 equals the main estimate, in column (6) in Table 6. Panel (B) similarly plots the coefficient on the interaction term Firm Age $< 10$. The point estimate for 2012 equals the estimate provided in column (2), Table 7. Dashed blue lines provide 95% confidence intervals.




These analyses also allow me to address potential identification concerns. For example, investors who lost a large amount of their wealth during the crisis may have taken on significant risks, both in public and private equity. This might suggest that these investors expanded more rapidly during the crisis, and thus had to reduce their employment growth more following the onset of the crisis. An additional, but related, concern is that these investors had invested in both private and listed firms that were highly procyclical. Both of these concerns would suggest that more shocked firms had higher employment growth prior to experiencing wealth losses during 2008–09. I do not find this to be the case. In Panel (A) of Figure 4 below, I do not find that more shocked firms experienced higher cumulative growth rates between 2003 and 2007. Importantly, Panel (B) shows that younger shocked

firms, who produce most of the effect on employment, do not display any pre-trends in employment growth.

In studying cumulative responses, a natural question that arises is how they vary with the persistence of the wealth shock. A regression of 1-year forward returns as of $t+1$ on time 1-year forward returns as of $t+1$, allowing coefficients to vary by year, reveals a strong negative correlation of -1.13 for $t = 2008$. In other words, 2008 returns were extraordinarily transitory. On average, a 2008-portfolio return that was 25 percentage points below the market return would yield a 2009 return that was 28 percentage points above the market return. In Figure A.5 in the Appendix, I examine how these largely-transitory 1-year wealth shocks affected employment growth. We immediately see that the effects are much less precisely estimated. This relates to the significantly smaller variance in 2008 versus 2008–09 returns. Interestingly, we see that the cumulative effects for the average firm are quite similar. However, given the width of the confidence bands, it is not possible to statistically infer whether effects are similar.

### 5.6 Decomposing Employment Growth into Firing and Hiring

In this subsection, I exploit the detailed nature of the Employer-Employee register to decompose employment growth into separations and new hires. To my knowledge, the literature on financial frictions and employment has provided little evidence on how financial frictions may affect these two components of employment growth differently.[22] While separating workers from the firm may mitigate financing short-falls by reducing payroll costs, and have longer-term positive effects on average productivity, it may have significant short-term costs due to potential legal costs and notice periods.[23] Additional costs arise if workers walk away with the firm's intangible capital (Sun and Xiaolan, 2019). It is therefore unclear how attractive job separations are for financially constrained firms. A more attractive option is likely to curtail the hiring of new workers, which is typically associated with an initial training period and little immediate value added. This further allows firms to offer extensive-margin income insurance for their existing employees.

My results are consistent with this intuition. I find that approximately three-quarters of the effect on employment growth is through hiring, and that I cannot reject the null of no effect on job separations. I decompose the numerator in the employment growth variable ($\Delta$# Non-owner Employment Days) into new hires ($\Delta$# Non-owner Employment Days for workers not present at time $t$) and existing workers ($\Delta$# Non-owner Employment Days for workers present at time $t$). I keep the denominator the same as before, containing the total number of employment days. Columns (1)-(2) show the baseline (reference) results that have employment growth variable as the dependent variable. Columns (3)-(4) focus on new hires and columns (5)-(6) consider existing workers.

[22] A concurrent paper by Barbosa, Bilan, and Célérier (2019) also uses this decomposition to look at the effects of credit supply on human capital retention and acquisition.

[23] Notice periods are 1 to 3 months in Norway.

Comparing columns (1), (3) and (5), I find that most of the effect is driven by new hires. The coefficient on growth attributable to new hires makes up 74% of the coefficient on over-all employment growth. While I cannot rule out an economically meaningful effect on job separations in younger firms in column (6), the results in column (4) indicate that the reduction in hiring among younger firms is the primary driver of the average elasticity uncovered in column (1).

Table 8: The Effect of Wealth Shocks on Employment Growth during 2008–10 Decomposed into Changes Among Existing and New Workers

| | | | Change in denominator limited to | | | |
|---|---|---|---|---|---|---|
| | $EG^{D}_{07,10}$ | | New Hires | | Existing Workers | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{Gains}_{08-09}$/GFW | 0.488** | 0.131 | 0.359*** | 0.113 | 0.107 | 0.010 |
| | (0.192) | (0.192) | (0.133) | (0.128) | (0.127) | (0.164) |
| * Firm Age < 10 | | 0.990** | | 0.720*** | | 0.226 |
| | | (0.398) | | (0.254) | | (0.258) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, and V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1340 | 0.1368 | 0.1856 | 0.1896 | 0.2029 | 0.2053 |
| N | 2496 | 2496 | 2496 | 2496 | 2496 | 2496 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Columns (1)-(2) correspond to my main employment specification and are provided as a reference. In columns (3)-(4) I only use the change in employment arising from new hires, i.e. employees that were not present at time $t = 2007$. In columns (5)-6) I only consider changes in employment arising from workers who were present at time $t = 2007$. The dependent variables in columns (3)-(4) and (5)-(6) sum up to the dependent variable in columns (1)-(2) for approximately 99% of the observations, some deviations occurs due to separate winsorization.

These findings suggest that the adverse effects of shocks to business owner wealth do not pass through to pre-existing workers along the extensive margin. The employment summary statistics in Table 5 show that while the average employment growth is -2.8% from 2007 to 2010, this is (essentially) the sum of considerable hiring (26%) and separations (-29%). This high degree of turnover presents an opportunity for firms to reduce the strain of employment costs by curtailing hiring and thereby insure existing workers on the extensive margin. These findings are consistent with Risch (2019) who finds that adverse shocks to business owners, operating through their personal income taxes, lead to intensive but not extensive margin effects on existing workers' compensation.

### 5.7 Effect on the Educational Composition of Workers

I now explore how wealth shocks affect the composition of the firm's work force in terms of educational attainment. A recent empirical literature documents that workers, especially highly-educated ones, are less willing to work for financially distressed firms. Brown and Matsa (2016) find that workers from ZIP codes with higher (average) educational attainment are differentially less

likely to apply to financially distressed firms. Baghai, Silva, Thell, and Vig (forthcoming) find that higher-ability workers are more likely to leave firms in financial distress. These findings of meaningful effects on the educational composition among both existing and incoming workers motivate use of the hiring versus firing decomposition when considering the effects of the educational composition of workers.

I report the results in Table 9, where column (1) reveals that there is no effect on average. However, when considering differential effects for young firms in column (2), I find that adversely affected small firms reduce the fraction of college-educated workers in their firm. Comparing columns (2), (4), and (6) show that the effect is driven by differences in educational attainment among new hires. A potential explanation for this is that more educated workers demand higher wages and offer delayed returns in terms of their contribution to firms' revenues and profits. This may be an investment constrained younger firms are unwilling to make. Also, perhaps surprisingly, I find that adversely affected mature firms *increase* the education level of new hires. This essentially serves to limit (or reverse) the educational gap between young and old firms, as young firms, on average, had 24% college educated workers, while older firms had 19%.

Table 9: The Effects of Wealth Shocks on the Educational Composition of All, New, and Existing Workers.

| | All workers 2010 | | New Hires | | Existing Workers | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Gains_{08-09}$/GFW | 0.058 | -0.030 | 0.022 | -0.462*** | -0.068 | -0.024 |
| | (0.069) | (0.070) | (0.184) | (0.149) | (0.072) | (0.067) |
| * Firm Age < 10 | | 0.252* | | 1.163*** | | -0.164 |
| | | (0.132) | | (0.236) | | (0.171) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| N | 2036 | 2036 | 1274 | 1274 | 2036 | 2036 |

The dependent variable is the share of workers with a college degree. This is measured using the 2010 employee pool; considering all workers, workers who were present in 2007, and workers who were not present in 2007, in columns (1)-(2), (3)-(4), and (5)-(6), respectively. I include the 2007-valued fraction of workers with a college degree as a control. Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

The finding of a reduction in the educational attainment among new hires is consistent with concurrent work by Barbosa, Bilan, and Célérier (2019). However, they also find that financial constraints also lead to an exit of more skilled workers, which does not appear to be the case in my setting.

### 5.8 Effects on Total Payroll and Subcontracting Expenditures

In this section, I present results on changes in firm-level total (non-owner) pay and subcontracting expenditures.

My main measure of employment growth considers the growth in number of employment days. This does not account for potential intensive-margin effects on wages. To address this, I consider the effect of wealth shocks on the cumulative payroll growth in column (1) of Table 10. I find that a 10% wealth shock reduces payroll expenditures by 3 percentage points ($t$-stat = 1.63). This reveals a weaker effect than my baseline estimates (an effect of almost 5 percentage points). Rather then intensive-margin effects on wages, Compositional effects, as those we found in subsection 5.7, may thus play an important role. This becomes clearer when considering heterogeneity with respect to firm age in column (2). This reveals a forceful effect on the payroll expenditures for younger firms. The negative point estimate of -0.134 ($t$-stat=-0.72) for older firms, although statistically insignificant, is consistent with the finding that older firms increase their hiring of college-educated—and likely more expensive—workers. Relatedly, the point estimate for the young-firm interaction effect of 1.158 is larger than (although statistically similar to) the point estimate on employment growth. This is consistent with the result of a negative effect on the hiring of college-educated workers for younger firms.

Table 10: The Effects of Wealth Shocks on Total Payroll and Subcontracting Expenditures

| | ΔTotalPay | | ΔSubContracting | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\text{Gains}_{08-09}$/GFW | 0.298 | -0.134 | -0.261** | -0.266* |
| | (0.183) | (0.186) | (0.122) | (0.149) |
| * Firm Age < 10 | | 1.158*** | | 0.031 |
| | | (0.387) | | (0.246) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1435 | 0.1478 | 0.1517 | 0.1525 |
| N | 2496 | 2496 | 2271 | 2271 |

Total pay excludes salary or wage earnings for owners. ΔTotalPay and ΔSubContracting are scaled by 2007 total payroll. In column (3)-(4), I control for payroll-scaled subcontracting as of 2007. Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Since firms facing difficulties in financing labor may find it easier to subcontract, over-all employment effects may be partially offset. I therefore consider the effects on subcontracting expenditures in columns (3)-(4). In column (3), I see that for the average firm, decreases in payroll are almost entirely offset by increases in subcontracting expenditures. Column (4) reveals that this is not the case for younger firms. While they too appear to increase subcontracting, increased subcontracting

expenditures only offset payroll decreases by approximately 23%=(0.266-0.031)/(-0.134+1.158).

### 5.9 Effects of Owner Wealth Shock Outside the Financial Crisis

The goal of this paper is to further our understanding of the economic challenges facing private firms during a crisis. Smaller firms often rely heavily on their owners for financing, which may prove particularly problematic when the ability of owners to provide financing is shocked at the same time as internal cash-flows dissipate and other sources of financing contract. The financial crisis of 2008–09 provides a natural laboratory for such a study. In particular, it allows us to provide empirical evidence useful for guiding policies aimed at reducing the severity of widespread economic shocks.

However, the presence of financial frictions can lead firms to be reliant on the entrepreneur's personal financial resources even in normal times. While this has been studied extensively in the context of entrepreneurial entry, there exists much less evidence on the effects of adverse shocks to owner's financial situation and the economic outcomes of existing firms. To further our understanding of the passthrough of owner wealth shocks in normal times as well, a natural extension of this paper's analysis is to use the empirical framework to study the effects of stock-portfolio induced wealth shocks outside of the financial crisis. The natural hypothesis is that the effects will be muted relative to the financial crisis. Firstly, more stable cash flows may lower the need for outside financing. Secondly, alternative sources of finance may be more readily available. There may be more willingness to provide equity or loans among outside investors and banks, and suppliers may be more flexible in their payment schedules.

I extend my analysis in this direction by rerunning the main regressions during two alternative time periods.[24] The results are reported in Table 11. Columns (1)-(2) consider the pre-crisis period, and provide the estimated effect of wealth shocks experienced during 2006-07 on concurrent outcomes. Columns (3)-(4) provide the results for the financial crisis as a benchmark. Columns (5)-(6) consider the post-crisis period.

Interestingly, I find no statistically significant effects outside of the financial crisis. This can either be due to differences in the economic environment or differences in the distribution of wealth shocks across these periods, as 05–07 stock returns were largely positive, 07–09 mostly negative, while 09–11 were mixed. To examine potential asymmetric effects, I estimate a third order polynomial in the wealth shock variable, Gains/GFW, for the analysis covering 09–11. This reveals no evidence of any particular asymmetries.[25]

The finding of a null effect outside the financial crisis does not imply that the personal financial

[24] Since security-level data on listed stocks is only available starting in 2004, I am unable to consider other economic crises, such as that following the burst of the IT bubble in the early 2000s.

[25] The estimated coefficients (standard errors) on the first through third-order polynomial in $Gains/GFW$ are 0.1359 (0.2404), 0.1064 (0.2566), and -0.2422 (0.3598), respectively. Plotting the treatment effects for different values of $Gains/GFW$ does not reveal any significant asymmetries.

resources of an entrepreneur is generally immaterial for firm outcomes. The point estimates for the effects for younger firms are positive for both the pre- and post-crisis periods, and economically important effects may be hidden inside the confidence intervals. I can only conclude that the pass-through of economic shocks is likely considerably smaller in normal times.

Table 11: The Effects of Wealth Shocks on Subsequent Employment Growth during Different Time Periods

| | $EG^{D}_{05,08}$ | | $EG^{D}_{07,10}$ | | $EG^{D}_{09,12}$ | |
|---|---|---|---|---|---|---|
| | t=2005 | | t = 2007 (baseline) | | t=2009 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Gains_{t+1-t+2}/GFW_t$ | -0.014 | -0.042 | 0.488** | 0.131 | 0.076 | -0.004 |
| | (0.106) | (0.135) | (0.192) | (0.192) | (0.156) | (0.185) |
| * Firm Age < 10 | | 0.072 | | 0.990** | | 0.186 |
| | | (0.232) | | (0.398) | | (0.327) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1432 | 0.1449 | 0.1340 | 0.1368 | 0.1136 | 0.1139 |
| N | 2212 | 2212 | 2496 | 2496 | 2721 | 2721 |

Coefficients are estimated using equation 4. $Gains_{t+1-t+2}/GFW_t$ is amount of stock market gains the investor experienced during t+1 and t+2, fixing portfolio weights at the end of year t, scaled by year t GFW. In columns (1)-(2) $t = 2005$, in columns (3)-(4) $t = 2007$, and in columns (5)-(6) $t = 2009$. Firm age, and all other controls measured at time $t$. Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. * , ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

## 6 The Effect of Wealth Shocks on Capital Expenditures

In this section, I analyze the effects of wealth shocks on firm investment. While Section 5 documents sizable real effects in terms of employment, the economic effects of wealth-shock induced financial constraints may go well beyond labor demand. If firms are severely constrained, we would also expect an adverse effect on capital expenditures. However, documenting this effect is interesting because it sheds light on the hypothesis that financial frictions may be more severe for labor. There are at least two reasons why this is a plausible hypothesis. First, capital—as opposed to labor—may serve as collateral. Second, capital—as opposed to labor—may not decide to leave or refuse to join the firm (Baghai, Silva, Thell, and Vig forthcoming, Brown and Matsa 2016, Babina 2020). Workers may be less willing to join due to an increased probability of unemployment or, for example, reduced investments in workplace safety (Cohn and Wardlaw, 2016).

The literature on credit market contractions and employment focuses almost exclusively on employment outcomes. This literature therefore does not shed much light on whether both labor and capital expenditures are materially affected in the same sample of firms, which is necessary to evaluate

the above hypothesis.

Table 12: Firm Investments during 2008–09

| | N | mean | sd | p1 | p5 | p10 | p25 | p50 | p75 | p90 | p95 | p99 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Plant /Assets | 3671 | 0.01 | 0.05 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.23 |
| Property /Assets | 3671 | 0.00 | 0.03 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.04 |
| Total Investments /Assets | 3671 | 0.05 | 0.13 | -0.28 | -0.04 | 0.00 | 0.00 | 0.01 | 0.07 | 0.18 | 0.27 | 0.57 |

Total investments include net investments in R&D (e.g., acquisitions of intangibles), vehicles (cars, planes, ships, etc.), inventory and machinery, plant, and property. Only firms remaining in the sample until 2009 are included. Investment ratios are defined as the sum of net investments during 2008 and 2009, scaled by 2007 assets. These ratios are censored to be between -1 and 1.

The empirical challenge is that data on capital expenditures is not always readily available in contexts where employment outcomes are. This challenge may be overcome in Norwegian register data, as all incorporated firms must report each year the transaction value of all investments and disinvestments in fixed assets, broken down by asset class. The purpose of these reporting requirements is to facilitate the calculation of tax deductions based on asset depreciation. A positive cash flow inducing sale of a fully depreciated asset triggers taxes, and future depreciation allowances depend on the historical (transaction) cost of the asset. Up until 2011 these yearly figures were reported in the firm's main tax return form.[26] This allows me to analyze the effects on firm-level investments with likely minimal measurement error. In Table 12, I provide summary statistics for the firms' net investment in 2008 and 2009 scaled by assets in 2007.

I first examine the effect on the over-all investment ratio. I report my estimates in Table 13. Results are based on estimating equation 7, using investment outcomes as left-hand-side variables. In column (1), I find that the effect of a wealth shock on the average firm's overall investment ratio is statistically insignificant. However, this masks statistically significant heterogeneity with respect to firm age, which is apparent in column (2): A one standard deviation adverse wealth shock (-10%) reduces the 2-year investment ratio by 10%*(-0.027+0.447) = 4.2 percentage points or 32% of its standard deviation (0.042/0.13).

I also consider the effect on major capital expenditures events, such as investments in plants or new property.[27] In column (4), I find that a wealth loss of 10% lowers the probability of investments in Plant and Property by 2.5 percentage points, or by 35.7% relative to the mean probability of 0.07. In column (3), I find that this effect is again largely driven by younger firms.

[26] After 2011 these numbers enter a separate form that is filed an appendix to the firm's tax return. This appendix is not part of my dataset.

[27] There are fairly few such events, with very large variation in the expenditures. This causes me to restrict my focus to investments on the extensive rather than intensive margin.

Table 13: The Effects of Wealth Shocks on Firm Investments during 2008–09

| | Total Inv/Assets | | | Plant&Prop > 0 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Gains_{08-09}$/GFW | 0.121 | -0.027 | -0.124 | 0.250** | 0.140 | 0.247 |
| | (0.096) | (0.094) | (0.183) | (0.116) | (0.098) | (0.191) |
| * Firm Age < 10 | | 0.447** | 0.422* | | 0.388* | 0.251 |
| | | (0.200) | (0.221) | | (0.216) | (0.206) |
| * Profitability < 10% | | | 0.605*** | | | 0.041 |
| | | | (0.222) | | | (0.269) |
| * Leverage > 50% | | | -0.389* | | | -0.335 |
| | | | (0.216) | | | (0.209) |
| * Cash/OpEx < 10% | | | -0.207 | | | 0.210 |
| | | | (0.237) | | | (0.231) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| Lagged Outcome | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1655 | 0.1706 | 0.1791 | 0.3385 | 0.3428 | 0.3458 |
| N | 2129 | 2129 | 2129 | 2129 | 2129 | 2129 |

Columns (1) through (3) consider the effect on total investments during 2008 and 2009 scaled by 2007 assets. Columns (3) through (6) use a dummy variable for whether there were net positive investments in plant or property during 2008 or 2009. Coefficients are estimated using equation 7, using investment outcomes as left-hand-side variables. Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

# 7 Additional Financing Outcomes

## 7.1 Bank Financing

My results show that the real effects of owner wealth shocks are driven by younger firms. A potential explanation for this is that younger firms have had less time to develop relationships with potential external financiers, e.g., banks, and are thus less able to substitute to external sources of financing. In Table 14, I present results consistent with this intuition. For affected mature firms, bank loans appear to be a partial substitute for owner-provided financing, while for affected young firms, they appear to be complementary.

In columns (1)-(2), I consider extensive-margin effects on long-term (LT) bank financing. This reveals significant substitution for the average firm (column 1), but much less so for younger firms (column 2). In columns (3)-(4), I consider intensive-margin effects. This reveals a more striking heterogeneity with respect to firm age. While mature firms are able to substitute toward long-term bank financing, younger firms are not—point estimates indicate a reduction in borrowing. This heterogeneity becomes even more clear in columns (5)-(6) where short-term bank financing is included.

Column (6) says that a mature firm whose owner loses 10% of his or her wealth sees an increase in the bank loan to asset ratio of 0.02, or approximately 40% relative to the the mean (conditional on a non-negative change) of 0.051.[28] Young firms, on the other hand, decrease this ratio by approximately 52%.[29]

Table 14: The Effects of Wealth Shocks on Bank-provided Financing during 2008–09
Extensive and Intensive Margin Effects

| | Δ LT Bank Loans > 0 | | ΔLT Bank Loans/Assets$_{07}$ | | ΔAll Bank Loans/Assets$_{07}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Gains$_{08-09}$/GFW | -0.249** | -0.255* | -0.101 | -0.206* | -0.061 | -0.203* |
| | (0.115) | (0.130) | (0.095) | (0.117) | (0.100) | (0.116) |
| * Firm Age < 10 | | 0.122 | | 0.368 | | 0.468* |
| | | (0.268) | | (0.249) | | (0.261) |
| mean(Y) | .116 | .116 | .004 | .004 | .008 | .008 |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1544 | 0.1583 | 0.1362 | 0.1432 | 0.1473 | 0.1534 |
| N | 2349 | 2349 | 2349 | 2349 | 2349 | 2349 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. LT refers to long-term liabilities. The differences refer to changes in bank loans over the two-year period between Dec 2007 and Dec 2009. Controls include stock market exposure (linear and squared), the FirmAge < 10 dummy, their interactions, A dummy for having no bank loans, and bank loans divided by assets, all measured in 2007. Only firms who have non-missing accounting data during both 2007 and 2009 are present. For columns (5)-(6): Conditional on having a non-negative change in over-all loans from 2007 to 2009, the mean change, scaled by 2007 assets is 0.051. Conditional on being strictly positive, the mean is 0.234.

## 7.2 Investor Exit and Ownership Structure

In this subsection, I first examine whether shocked owners liquidate their holdings in the stock market. This is useful to inform the possible extent of alternative mechanisms, such as a diminished willingness to grow the stock of labor or capital in response to negatively affected beliefs about the future. These results are reported in columns (1)-(2) of Table 15. Interestingly, there is no evidence that wealth shocks induce an exit from the stock market. In column (3)-(4), I examine whether they exit their equity position in the private firm they own, and find no evidence of this either.

In columns (5)-(6), I examine whether adversely-affected firms are able to engage new equity investors. Empirically, I consider the effect on the conditional ownership share of the affected owner. This reveals no effect, which is consistent with an inability of these firms to substitute toward external equity financing— as this would reduce the ownership share of the existing owner. I also find no

[28] Conditional on having a non-negative change in over-all loans from 2007 to 2009, the mean change, scaled by 2007 assets is 0.051. Conditional on being strictly positive, the mean is 0.234.

[29] -(0.203-0.468)*0.1/0.051

heterogeneity with respect to firm age.

This heterogeneity analysis is important, because younger firms are the ones found to be the the least able to substitute toward bank-provided financing. A possible hypothesis would thus be that younger firms are differentially more likely to substitute toward equity investors, while older firms are more likely to substitute toward bank financing. However, I find that younger firms substitute toward neither of these financing sources. This is consistent with my results that younger firms experience the most severe real effects in terms of employment growth and investment.

Table 15: Stock Market Participation, Entrepreneurial Exit, and Firm Ownership Structure

| | Portfolio Stocks$_{2010} > 0$ | | Firm Ownership Share$_{2010} > 0$ | | Firm Ownership Share$_{2010}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Gains$_{08–09}$/GFW | -0.104 | -0.080 | -0.101 | -0.003 | 0.014 | 0.008 |
| | (0.121) | (0.139) | (0.161) | (0.150) | (0.051) | (0.065) |
| * Firm Age < 10 | | 0.042 | | -0.300 | | 0.018 |
| | | (0.279) | | (0.344) | | (0.107) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1247 | 0.1285 | 0.1410 | 0.1445 | 0.9032 | 0.9032 |
| N | 2496 | 2496 | 2285 | 2285 | 1970 | 1970 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $Gains_{08–09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. Columns (4)-(5) show results on firm ownership share, conditional on still being an owner.

# 8 Conclusion

Entrepreneurs and equity investors' wealth are subject to a wide range of cash-flow and asset-price shocks that can inhibit their ability to provide financing. Such shocks include the value and cash-flows of their investments in (other) firms, the stock market, real estate, and other financial instruments, as well as taxation. Many of these shocks will be tightly correlated to the investment opportunities of their private firms. Adverse wealth shocks are particularly likely to occur during economic crises, and therefore coincide with reductions in demand and credit supply from financial institutions. In this paper, I use variation in stock market wealth

In this paper, I exploit differences in pre-crisis portfolio compositions and the large dispersion in 2008–09 stock returns, in an attempt to keep firms' investment opportunities constant while still obtaining meaningful variation in wealth and liquidity. This allows me to present new evidence on how small, and especially young, firms are affected by shocks to their owners' wealth during an economic crisis. I show that these shocks have economically large effects on employment and investment. A multitude of tests are inconsistent with these effects being driven by confounding due to, e.g., selection

on investor characteristics or common shocks.

The fact that wealth shocks affect firms' financing and that the effects on employment and investment are primarily driven by younger firms suggest that the main mechanism through which the owner's wealth affects the firm is by increasing financial constraints. There are at least three alternative stories. First, wealth losses could affect the owner's consumption demand. For example, lowered wealth may induce lower consumption of leisure. The owner may therefore wish to work more himself which can crowd out the firm's demand for labor. Second, updated beliefs about the future may limit the owner's willingness to grow the stock of capital or labor. If either of these two mechanisms were dominating, I would not expect to uncover the heterogeneous results with respect to firm age or the wealth of the owner relative to the potential financing needs of the firm. A potential third mechanism is that wealth losses may induce owner-managers to take less risk due to a wealth effect. This is hard to distinguish from a pure liquidity effect. Any reduction in liquidity provision from the owners, either because they have no liquid assets or because they're unwilling to move their liquidity into the firm, contributes to harsher financial constraints from the perspective of the firm. It is therefore unclear how one could distinguish this channel from a pure liquidity effect and in which scenarios this distinction matters.

My findings underline the importance of owners in financing investment and employment in small firms in a recessionary economic environment. Greenstone, Mas, and Nguyen (2020) find that small firms are largely unaffected by contractions in small business lending, suggesting that small firms may have access to other sources of credit when bank lending contracts. This may be consistent with an absence of procyclical financial constraints for smaller firms; a hypothesis which my findings reject. My analyses further reveal sizable effects that are rather persistent for younger firms. This suggests that younger firms who lose out on investment or employment opportunities due to financial frictions do not catch up in the short to medium term. Given the importance of small businesses in most economies and the fact that they often rely heavily on owners (rather than banks) for financing, these findings stress that policymakers should consider policy responses to adverse economic shocks that go beyond securing bank credit. Examples include policies that lead to (i) increased provision of loans to small, young businesses or to their owners, or (ii) increased incentives for investors to provide equity financing to these firms.

The findings further stress that, to the extent that financial shocks pass through to workers, those affected may primarily be potential new employees. The provision of standard unemployment insurance, which typically requires pre-existing employment, may therefore not be the right medicine.

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# A Appendix

## A.1 Business Owner Summary Statistics

Table A.1: Business owner characteristics

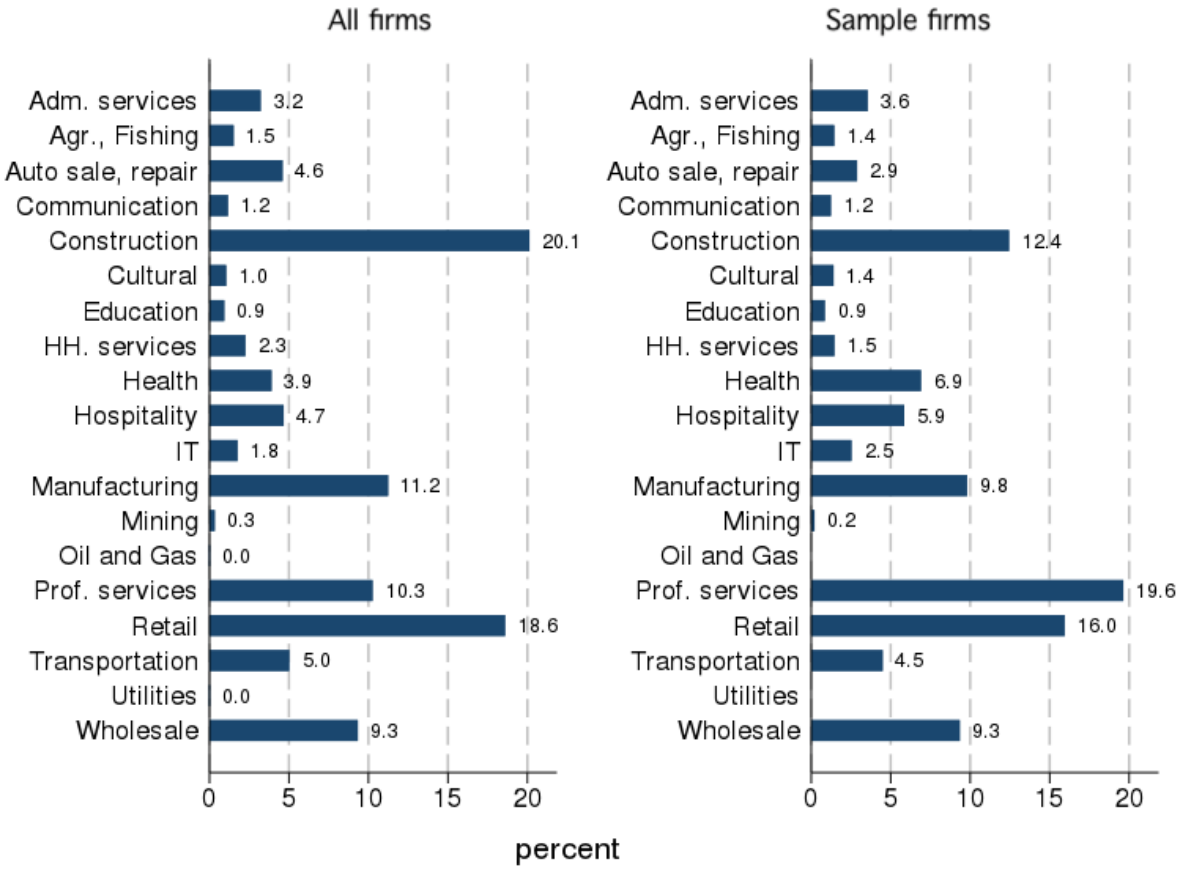
| | count | mean | sd | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|---|
| log(GFW) | 4150 | 14.42 | 1.36 | 12.69 | 13.56 | 14.41 | 15.30 | 16.11 |
| log(Earnings) | 4150 | 12.88 | 1.00 | 11.86 | 12.73 | 13.04 | 13.37 | 13.70 |
| Age | 4150 | 52.38 | 10.94 | 37.00 | 45.00 | 53.00 | 60.00 | 65.00 |
| Male | 4150 | 0.85 | 0.36 | | | | | |
| HighSchool | 4150 | 0.91 | 0.28 | | | | | |
| College | 4150 | 0.45 | 0.50 | | | | | |
| Owns $>$ 50% of a firm | 4150 | 0.49 | 0.50 | | | | | |
| # Firms owned | 4150 | 1.26 | 0.70 | 1.00 | 1.00 | 1.00 | 1.00 | 2.00 |

All variables are measured as of end-of-year 2007. The unit of observation is the investor level. # Firms owned counts the number of firms where the owner owns $\geq$ 1% of the shares, including firms that may not be in the analysis sample. College indicates completed higher education. Earnings consist of wage and salary earnings plus any self-employment income.

## A.2 Industry composition of firms

Figure A.1: Industry Composition of Firms

The left hand side chart shows the distribution of firms in my data prior to implementing the restriction of a non-trivial stock market exposure. The right-hand-side chart shows the distribution of firms in my analysis sample. These are firms whose owners owned a non-trivial amount of listed stocks as of 2007.



## A.3 Firm-Owner Pair Statistics, Long version

Table A.2: Firm-Owner Pair Statistics, long version

| | N | mean | sd | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|---|
| Ownership(%) | 4747 | 0.55 | 0.34 | 0.10 | 0.25 | 0.50 | 1.00 | 1.00 |
| Ownership(%), excl. spouse | 4747 | 0.51 | 0.33 | 0.10 | 0.25 | 0.50 | 0.90 | 1.00 |
| WasOwner2004 | 4306 | 0.92 | | | | | | |
| WasOwner2006 | 4783 | 0.97 | | | | | | |
| OwnViaHolding | 4747 | 0.35 | | | | | | |
| IncrOwnershipSince2004 | 3946 | 0.16 | | | | | | |
| DecrOwnershipSince2004 | 3946 | 0.11 | | | | | | |
| SameCity | 4783 | 0.71 | | | | | | |
| SameCounty | 4783 | 0.85 | | | | | | |
| SiblingOwners | 3393 | 0.21 | | | | | | |
| ParentOwner | 3393 | 0.17 | | | | | | |
| ChildOwner | 3393 | 0.12 | | | | | | |
| Employed | 4783 | 0.60 | | | | | | |
| EmploymentTenure | 2669 | 11.75 | 8.47 | 2.00 | 5.00 | 10.00 | 18.00 | 24.00 |
| Owner's pay (NOK) | 2848 | 462214 | 256918 | 138860 | 304723 | 435033 | 596156 | 781988 |
| % of Owner's Earnings | 2848 | 0.96 | 0.16 | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 |
| $\text{Dividends}_t > 0$ | 4783 | 0.29 | | | | | | |
| $\text{Dividends}_t/\text{GFW}_t$ | 4162 | 0.05 | 0.12 | 0.00 | 0.00 | 0.00 | 0.02 | 0.15 |
| $\text{Dividends}_{t+1,t+2}/\text{GFW}_t$ | 4162 | 0.17 | 0.47 | 0.00 | 0.00 | 0.00 | 0.14 | 0.47 |

OwnViaHolding is a dummy for whether any of the owner's shares were held through another LLC. Dummies for increases and decreases in ownership share are only defined for those who were owners in both 2004 and 2007. For non-100% owners, ParentOwner=1 if the owner is the parent of another shareholder. ChildOwner is for children of another owner. SiblingOwner is similarly defined if one of your siblings is present as an owner. Dividends are recorded as they appear on the owner's tax records. The USD/NOK exchange rate was approximately 6 in 2007.

### A.3.1 Owner Stock Market Exposure and Firm Characteristics

Most of the identifying variation in my analyses will come from firms whose investors have larger stock market exposures. In order to inform my later discussion of external validity, I provide summary statistics of all the firms in my sample (first column), all firms in the analysis sample firms (second column), and by quartiles of stock market exposure firms (4 last columns) in Table A.3.

I find that sample firms (those whose owners have a meaningful exposure to the stock market in the form of holding listed stocks) are fairly similar to other firms. They tend to be slightly older, have a larger number of owners, fewer employees, and lower past employment growth. These differences in employment growth, however, are decreasing in the owner's stock market exposure.

The lower part of Table A.3 provides the distribution of my continuous treatment variable, $\frac{Gains_{t+1,t+2}}{GFW_t}$ for my entire analysis sample, as well as by quartiles of stock market exposure.

Table A.3: Stock Market Exposure and Firm Characteristics

| | All firms | Exposed | By Exposure (Stocks/GFW) Quartile | | | |
|---|---|---|---|---|---|---|
| Means | | | 1 | 2 | 3 | 4 |
| log(Assets) | 14.96 | 14.89 | 14.98 | 14.89 | 14.89 | 14.81 |
| Leverage (ST) | 0.45 | 0.41 | 0.37 | 0.39 | 0.41 | 0.45 |
| Leverage (LT) | 0.11 | 0.09 | 0.07 | 0.08 | 0.09 | 0.11 |
| Profitability | 0.07 | 0.10 | 0.12 | 0.11 | 0.10 | 0.08 |
| Firm Age | 12.38 | 13.75 | 14.68 | 14.35 | 13.34 | 12.60 |
| # Owners | 2.22 | 2.76 | 2.57 | 2.71 | 2.84 | 2.90 |
| # Owner-Employees | 1.59 | 1.57 | 1.29 | 1.55 | 1.76 | 1.71 |
| # Regular Employees | 9.29 | 6.93 | 7.09 | 6.55 | 6.36 | 7.74 |
| Empl. $\text{Growth}_{05,07}$ | 0.17 | 0.11 | 0.07 | 0.10 | 0.12 | 0.17 |
| $\text{Investments}_{06\text{–}07}/\text{Assets}_{05}$ | 0.11 | 0.09 | 0.08 | 0.09 | 0.09 | 0.10 |

Employment Growth is measured as growth in number of days of within-year employment at the firm. I discuss this measure in greater detail in the Employment section of the paper. Investments include investments in vehicles, plant, property, and other fixed assets. Summary statistics are based on one observation per firm, and stock market exposure is assigned based on the owner with the largest ownership share.

### A.3.2 Stock Market Exposure and Investor Characteristics

The main identifying variation in wealth comes from investors with greater exposure to the stock market. While I control for this exposure in my regression specifications, knowledge of how these investors differ from the less exposed may guide the interpretation of the results.

I find that business owners with positive stock market exposure are wealthier, older, more educated and have lower personal leverage. Once conditioning on positive exposure, I find that these differences are decreasing, except for education, which does not vary with stock market exposure. The observation that stock market investors are wealthier and less leveraged points in the direction that these investors, and likely also their firms, are less ex-ante financially constrained than the over-all population of firms.

Table A.4: Stock Market Exposure and Investor Characteristics

| | All owners | Exposed | By Exposure (Stocks/GFW) Quartile | | | |
|---|---|---|---|---|---|---|
| Means | | | 1 | 2 | 3 | 4 |
| Stocks/GFW | 0.02 | 0.21 | 0.03 | 0.08 | 0.18 | 0.53 |
| GFW, log | 13.01 | 14.42 | 15.17 | 14.61 | 14.28 | 13.60 |
| PersonalLeverage | 0.44 | 0.29 | 0.20 | 0.26 | 0.30 | 0.41 |
| Earnings, log | 12.89 | 12.88 | 12.84 | 12.91 | 12.89 | 12.87 |
| Age | 46.90 | 52.38 | 54.87 | 52.86 | 51.58 | 50.21 |
| Norwayborn | 0.96 | 0.96 | 0.96 | 0.97 | 0.95 | 0.96 |
| Male | 0.77 | 0.85 | 0.87 | 0.86 | 0.84 | 0.84 |
| HighSchool | 0.86 | 0.91 | 0.92 | 0.93 | 0.91 | 0.89 |
| College | 0.29 | 0.45 | 0.46 | 0.45 | 0.43 | 0.45 |
| st.dev($R_{t,t+2}$) | | 0.24 | 0.25 | 0.24 | 0.24 | 0.24 |
| $Gains_{t,t+2}/GFW_t$ | | | | | | |
| sd | | 0.11 | 0.01 | 0.02 | 0.05 | 0.17 |
| p1 | | -0.50 | -0.04 | -0.08 | -0.19 | -0.76 |
| p5 | | -0.28 | -0.02 | -0.06 | -0.14 | -0.49 |
| p10 | | -0.17 | -0.02 | -0.05 | -0.12 | -0.40 |
| p25 | | -0.07 | -0.01 | -0.03 | -0.07 | -0.24 |
| p50 | | -0.02 | -0.01 | -0.02 | -0.04 | -0.13 |
| p75 | | -0.01 | -0.00 | -0.01 | -0.02 | -0.06 |
| p90 | | -0.00 | -0.00 | -0.00 | -0.01 | -0.02 |
| p95 | | 0.01 | 0.00 | 0.01 | 0.01 | 0.04 |
| p99 | | 0.08 | 0.01 | 0.03 | 0.06 | 0.15 |

All variables are measured as of 2007. Personal Leverage is the ratio of debt to the sum of financial wealth and tax-implied real-estate wealth. Education dummies (HighSchool and College) are cumulative.

## A.4 Breakdown of firm liabilities

Table A.5: Breakdown of firm liabilities

| | Firm Age $\geq 10$ | | Firm Age $< 10$ | |
|---|---|---|---|---|
| % of Assets | mean | median | mean | median |
| *Equity* | | | | |
| PIC | 14.24 | 8.86 | 14.09 | 8.46 |
| RetainedEarnings | 3.09 | 3.84 | 3.57 | 4.22 |
| *Long-term liabilities* | | | | |
| BankDebt | 10.40 | 0.00 | 9.98 | 0.00 |
| Owners | 4.08 | 0.00 | 4.35 | 0.00 |
| Other | 3.61 | 0.00 | 4.71 | 0.00 |
| *Short-term liabilities* | | | | |
| BankDebt | 2.64 | 0.00 | 2.48 | 0.00 |
| Owners | 7.01 | 0.00 | 6.42 | 0.00 |
| Suppliers | 13.55 | 7.71 | 11.99 | 6.11 |
| Wages | 8.15 | 6.31 | 7.66 | 6.20 |
| Other | 33.21 | 25.89 | 34.74 | 29.07 |

All variables measured in 2007. Sample is the superset of the analysis sample: It also includes firms with owner who do not hold listed stocks. PIC is Paid-in Capital. Other short-term debt includes payroll and value-added taxes.

## A.5 Financing, Placebo Regressions

I repeat the analysis on financing outcomes, keeping all the right-hand-side variables the same, but considering lagged financing outcomes (2005–06).

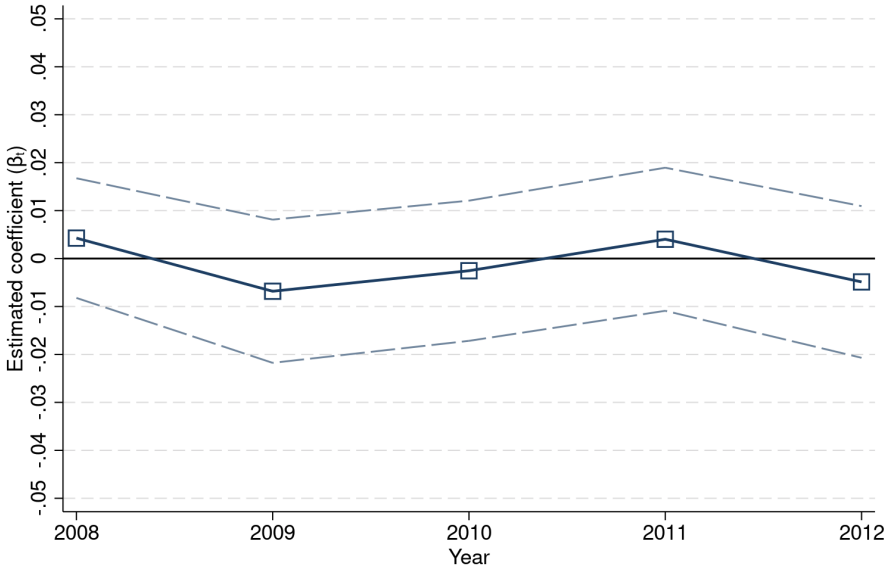
Table A.6: Financing, Placebo Regressions

| | Financing Outcomes During 2005–06 | | | | | |
|---|---|---|---|---|---|---|
| | ΔPaid-in-Capital > 0 | | Dividends/GFW | | ΔInvestor Loans/GFW | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Gains_{08–09}/GFW_{07}$ | -0.044 | -0.074 | 0.099 | 0.089 | 0.184 | 0.197 |
| | (0.099) | (0.097) | (0.098) | (0.122) | (0.136) | (0.174) |
| $Gains_{08–09}/GFW_{07}$ * Leverage | | 0.143 | | -0.072 | | -0.247 |
| | | (0.105) | | (0.091) | | (0.156) |
| $Gains_{08–09}/GFW_{07}$ * Cash/OpEx | | -0.047 | | -0.037 | | -0.094 |
| | | (0.120) | | (0.198) | | (0.290) |
| $Gains_{08–09}/GFW_{07}$ * Profitability | | 0.110 | | -0.034 | | -0.137 |
| | | (0.118) | | (0.188) | | (0.187) |
| mean(Y) | .13 | .13 | .156 | .156 | .005 | .005 |
| P, V, F controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| R2 | 0.1400 | 0.1423 | 0.2021 | 0.2085 | 0.1442 | 0.1479 |
| N | 2956 | 2956 | 3557 | 3557 | 2953 | 2953 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, * and *** indicate significance at the 10%, 5% and 1% levels, respectively. $Gains_{08–09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. Interaction variables are scaled by their standard deviation, and are included as controls, as well as interactions with $Stocks/GFW$. Controls for lagged $Dividend/GFW$ ratio, and a dividend dummy, are included.

## A.6 Placebo Test: Returns and Profitability

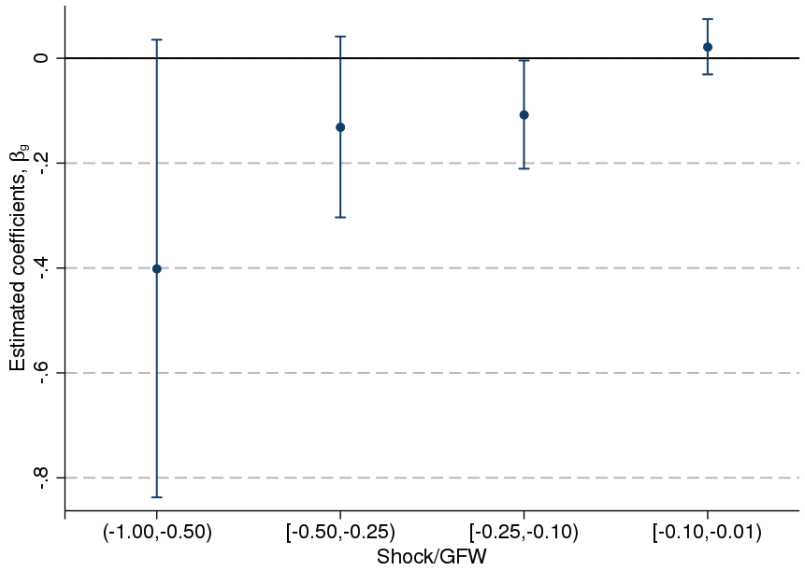
Figure A.2: Placebo Test: Correlation Between Portfolio Returns and Changes to Profitability for Owners with Low Stock Market Exposure




Results are obtained by estimating the following equation for each year $t$: $\Delta Profitability_{f,t} = \alpha_{n,t} + \alpha_{r,t} + \alpha_{n,t} + \beta_t R_{v,07,09} + \rho'_t P_{v,07} + \eta'_t V_{v,07} + \zeta'_t F_{f,07} + \varepsilon_{f,v,t}$. The plot shows the coefficient on the portfolio return from 2007 to 2009. The analysis is limited to investors with a stock market exposure between 0.25% and 10%.

## A.7 Effects on Employment Growth by Wealth Shock Bins

Figure A.3: Estimating effects of wealth shocks by bins




Results are obtained by performing the following regression for shock bin, $g \in G = \{(-1.00,-0.50),[-0.50,-0.25),[-0.25,-0.10),[-0.10,-0.01)\}$ : $EG^{D}_{07,10} = \alpha_n + \alpha_r + \sum_g \beta_g \mathbb{1}\left[\frac{Gains_{v,08,09}}{GFW_{v,07}} \in g\right] + \gamma_1 \frac{Stocks_{v,07}}{GFW_{v,07}} + \gamma_2 \left(\frac{Stocks_{v,07}}{GFW_{v,07}}\right)^2 + \rho' P_{v,07} + \eta' V_{v,07} + \zeta' F_{f,07} + \varepsilon_{f,v}$, where the excluded category consists of firm-investor observation where the owner lost less than 1% of GFW.

## A.8 Quasi First Stage: Portfolio stickiness

My empirical approach assume that that investors experience returns from $t$ to $t+j$ that depend on their portfolio composition at time $t$. If investors immediately sold off or reshuffled their portfolios right after time $t$, then the investors would not be affected by these intended returns. Since I do not observe within-year transactions of securities, only the yearly portfolio compositions, I construct the following test.

I regress $R_{v,08,09}$, which is the owner's portfolio returns from 2008 to 2009, based on her 2008 portfolio composition, on $\tilde{R}_{v,08,09}$, which are the returns based on her 2007 portfolio composition.

$$R_{v,08,09} = \pi_0 + \pi_1 \tilde{R}_{v,08,09} + \varepsilon_v \tag{8}$$

The "worst case scenario" would be if investors randomly reshuffle their portfolios with frequent intervals. This would imply that $\hat{\pi}_1 = 0$. If there were no transactions, I would find that $\hat{\pi}_1 = 1$. In Table A.7 below I report these estimates. In columns (1)-(3), I omit observations where $R_{v,08,09}$ is missing. These would be missing if the investors had exited the stock market. In columns (5)-(6),

I replace missing portfolio returns with zeros. In order to inform us of whether portfolio stickiness varies with the experienced returns from 2007 to 2008, I also estimate $\hat{\pi}_1$ separately for investors who lost more than 60% or less than 40%. These returns are close to the 25th and 75th percentiles, respectively.

Table A.7: Testing Portfolio Stickiness

| | Only non-missing $R_{08,09}$ | | | Missing $R_{08,09} \equiv 0$ | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\tilde{R}_{08,09}$ | 0.649*** | 0.600*** | 0.648*** | 0.584*** | 0.532*** | 0.586*** |
| | (0.011) | (0.020) | (0.040) | (0.011) | (0.020) | (0.040) |
| Intercept | 0.226*** | 0.291*** | 0.193*** | 0.231*** | 0.301*** | 0.188*** |
| | (0.009) | (0.023) | (0.016) | (0.009) | (0.023) | (0.016) |
| $R_{07,08}$ | All | <-60% | >-40% | All | <-60% | >-40% |
| F | 3306.39 | 905.20 | 260.17 | 2590.61 | 700.84 | 218.08 |
| R2 | 0.4572 | 0.3986 | 0.2272 | 0.3844 | 0.3263 | 0.1873 |
| N | 3928 | 1368 | 887 | 4150 | 1449 | 948 |

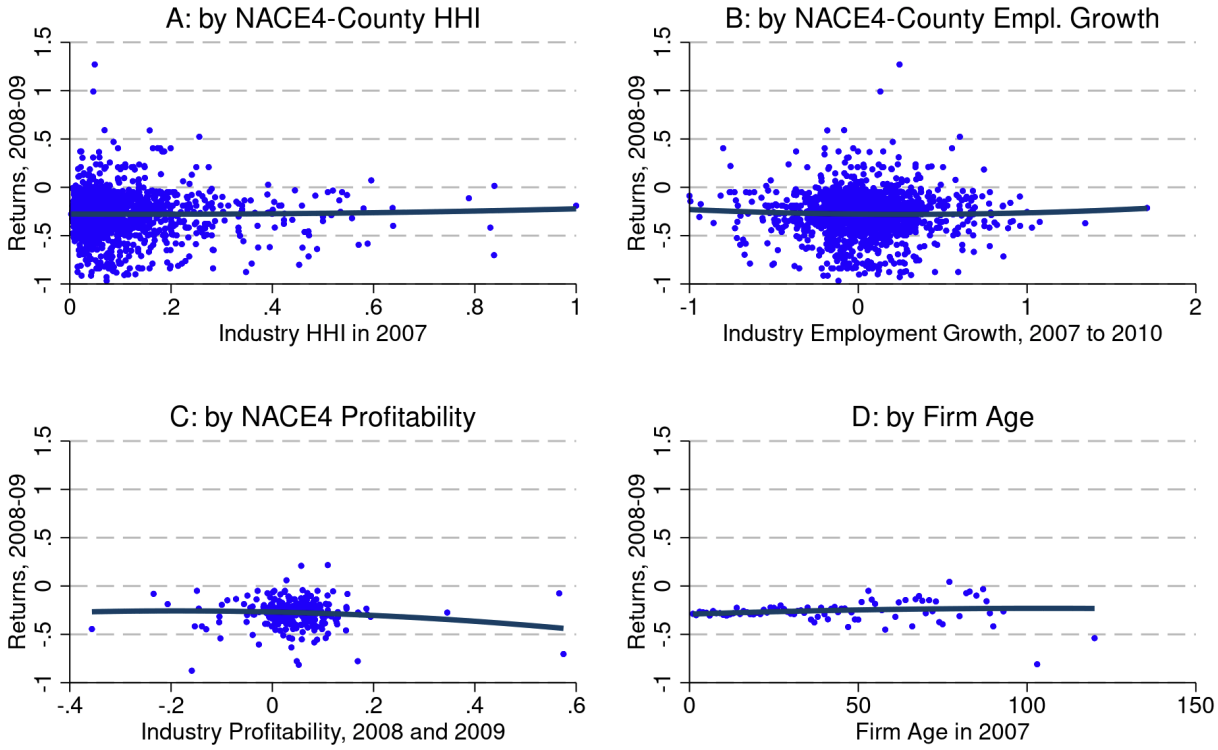
Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $\tilde{R}_{v,08,09}$ is the return of the portfolio based on the owner's 2007 portfolio composition.

How do I interpret the coefficients? A coefficient of $\pi_1$ would suggest that the investor had reshuffled a fraction $1-\pi_1$ of her portfolio sometime during the year, and on average (assuming the average trade occurs July 1st) would not have experienced $0.5 \cdot (1-\pi_1)$ of the predicted returns during that period. If I extend this to a two-year horizon, as in my empirical setting, I would expect the "non-compliance" to be approximately $2 \cdot 0.5 \cdot (1-\pi_1) = 1-\pi_1$. Alternatively phrased, I would expect a 2-year compliance of approximately $\pi_1$. Table A.7 suggests that this would be approximately 60%. I find a strongly positive intercept, $\hat{\pi}_0 > 0$, due to the fact that stock markets rose dramatically from 2008 and 2009. Thus, any investors who bought new stocks that were positively correlated with the market, but not perfectly correlated with her existing portfolio, would contribute to the estimated positive intercept.

### A.8.1 Future Portfolio Returns by Firm and Industry Characteristics

Figure A.4: Portfolio Returns during 2008–09 and Industry and Firm Characteristics

Panels (A) through (D) plot portfolio returns, based on 2007 portfolio composition, against firm and industry characteristics. Panel (A) considers the HHI industry concentration measure at the within-county 4-digit NACE code level. Panel (B) considers the average employment growth rate during 2008–10. Panel (C) considers 4-digit NACE code level profitability (profits/revenues) during 2008–09. Panel (D) considers the age of the firm. In Panels (A) through (C) each scatter point indicates the mean returns within each county-industry cell. In Panel (D), each scatter point indicates the mean returns within a 1-year age bin. The solid navy-blue line is a quadratic fit, estimated by regressing portfolio returns on the x-axis characteristic for all owner-firm observations in the sample.

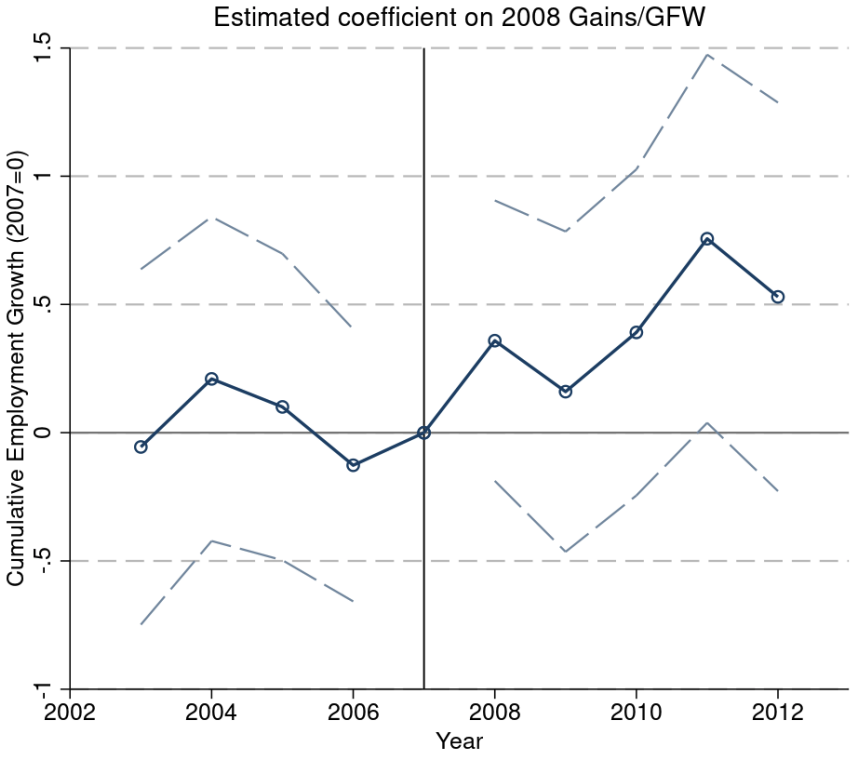



### A.8.2 Cumulative Employment Effects using 1-year (rather than 2-year) portfolio returns

In this subsection, I explore the effects of wealth shocks driven by 1-year portfolio returns. As discussed in the main text, 1-year returns are transitory. A return of $-e$ percentage points in excess of the market during 2008 is associated with a return of $+1.17e$ in excess of the market during 2009.

Figure A.5: Cumulative Effects of Using (Largely-Transitory) 1-Year Portfolio Return Shocks

This figure shows the main results on employment growth when using 1-year forward returns to provide variation in wealth, rather than 2-year returns as in the main specification. The figure plots the coefficients on $Gains_{08}/GFW$ in regressions of cumulative employment from year $t$ to 2007, $EG^{D}_{07,t}$. The point estimate for 2010 equals that in column (6) in Table 6. Dashed blue lines provide 95% confidence intervals.




### A.8.3 Investor Characteristics and Future Portfolio Returns

Table A.8 examines whether portfolio diversification (measured as portfolio HHI), profitability, or educational attainment can predict (future) portfolio returns. The sample size reduction is caused by only keeping one firm per owner (highest ownership share) and running regressions on the business-owner level. In Column (1), I consider the HHI of the portfolio. This is the sum of squared portfolio weights. This reveals an insignificant relationship between diversification and the returns on the stocks in the portfolio. Columns (4)-(5) further shows an insignificant relationship between educational attainment and portfolio returns.

In columns (2) through (6), I find that firm profitability does not predict stock market returns, either measured in 2007 or in 2009. If there is some individual fixed effect that affects both ability and firm performance, I would expect that stock returns correlated with ability, especially if the investor has been invested in the firm for a longer time period. In column (6), I limit to firm-owner pairs where the investor had already entered the firm during or before 2004, and still find that firm

profitability as of 2007 does not predict superior stock market performance. Finally, if stock market returns are correlated with ability in my sample, I would expect a positive relationship between the owner's individual earnings or wealth level, and their stock market returns. However, I do not find any evidence of this.

Table A.8: Portfolio Returns and Portfolio Diversification, Profitability, and Educational Attainment

| $R_{v,2007,2009}$ | Owner's first year in firm $\leq$ 2007 | | | | | $\leq$ 2004 |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Portfolio HHI | 0.0043 | 0.0039 | -0.0017 | -0.0011 | -0.0016 | 0.0148 |
| | (0.0122) | (0.0122) | (0.0125) | (0.0125) | (0.0127) | (0.0136) |
| Profitability | | -0.0274 | | -0.0486 | -0.0481 | -0.0092 |
| | | (0.0255) | | (0.0302) | (0.0312) | (0.0302) |
| $\text{Profitability}_{t+2}$ | | | -0.0072 | 0.0036 | 0.0178 | |
| | | | (0.0179) | (0.0197) | (0.0202) | |
| Educ = Highschool | | | | -0.0010 | -0.0022 | -0.0137 |
| | | | | (0.0141) | (0.0143) | (0.0154) |
| Educ = College | | | | 0.0101 | 0.0112 | 0.0008 |
| | | | | (0.0142) | (0.0152) | (0.0164) |
| IndustryFE | - | - | - | - | Yes | Yes |
| RegionFE | - | - | - | - | Yes | Yes |
| r2 | 0.0078 | 0.0081 | 0.0076 | 0.0086 | 0.0458 | 0.0445 |
| N | 4150 | 4150 | 3866 | 3866 | 3862 | 3476 |

Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. If a business-owner owns multiple firms, the Profitability variable is associated with the firm in which he or she has the highest ownership share. All RHS variables, except $\text{Profitability}_{09}$, are measured in 2007. The omitted education category consists of owners with educational attainment below highschool. All regressions include a linear control for stock market exposure, $\text{Stocks}_{07}/\text{GFW}_{07}$.

## A.9 Robustness

## A.10 Additional controls for main employment growth regressions

Table A.9: Additional Controls for Main Employment Growth Regressions

| $EG^D_{07,10}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $\text{Gains}_{08-09}/\text{GFW}_{07}$ | 0.488** | 0.413** | 0.483** | 0.500** | 0.527** | 0.465** |
| | (0.192) | (0.186) | (0.206) | (0.210) | (0.207) | (0.212) |
| Profitability | 0.311*** | 0.072 | 0.324** | 0.325** | 0.258** | 0.251** |
| | (0.118) | (0.143) | (0.130) | (0.130) | (0.122) | (0.125) |
| $\text{Profitability}_{10}$ | | 0.191** | | | | |
| | | (0.080) | | | | |
| $r_{v,05,07} - \bar{r}_{05,07}$ | | | -0.038* | -0.046 | | |
| | | | (0.022) | (0.049) | | |
| $(r_{v,05,07} - \bar{r}_{05,07})^2$ | | | | 0.079 | | |
| | | | | (0.129) | | |
| $(r_{v,05,07} - \bar{r}_{05,07})^3$ | | | | 0.017 | | |
| | | | | (0.110) | | |
| $(r_{v,05,07} - \bar{r}_{05,07})^4$ | | | | -0.071 | | |
| | | | | (0.157) | | |
| $\text{GFW}_t/\text{OpEx}_t$ | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P,F,V controls | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE**4**,R | NACE**5**,R |
| Cluster | F,V | F,V | F,V | F,V | F,V | F,V |
| r2 | 0.1340 | 0.1333 | 0.1457 | 0.1459 | 0.1744 | 0.1942 |
| N | 2496 | 2254 | 2187 | 2187 | 2455 | 2430 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $Gains_{08-09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. $\bar{r}_{05,07}$ is the mean portfolio returns from 2005 to 2007 for investors in the sample.

### A.10.1 Different measures of employment growth

I now explore whether my results are robust to changing the definition of employment growth. $EG^N$ is defined similarly as $EG^D$, but does not account for the duration of employment within the year. $EG^Y$ is year-on-year employment growth, considering the changes in the number of employees from December to December. In Table A.10, I find that my preferred measure of employment growth is the measure most sensitive to wealth shocks. Disregarding the employment duration ($EG^N$) lowers my coefficient by 24%, but reveals very similar heterogeneity with respect to firm age. Year-on-year employment growth is the measure least sensitive to wealth shocks. I provide intuition for these differences in the main text, and suggest that a reasonable preference ordering includes $EG^D \gtrsim EG^N \gtrsim EG^Y$ when the objective is to understand the effect of financing shocks on employment.

Table A.10: Different Measures of Employment Growth

| | $EG^D 07,10$ | | $EG^N 07,10$ | | $EG^Y 07,10$ | | Total Pay Growth 07-10 | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Gains_{08-09}$/GFW | 0.488** | 0.131 | 0.371* | 0.054 | 0.170 | -0.072 | 0.298 | -0.136 |
| | (0.192) | (0.192) | (0.210) | (0.199) | (0.198) | (0.193) | (0.183) | (0.187) |
| * Firm Age < 10 | | 0.990** | | 0.952** | | 0.751* | | 1.161*** |
| | | (0.398) | | (0.381) | | (0.447) | | (0.386) |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| P, F, V controls | Y | Y | Y | Y | | | | |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| r2 | 0.1340 | 0.1368 | 0.1398 | 0.1432 | 0.1331 | 0.1351 | 0.1424 | 0.1467 |
| N | 2496 | 2496 | 2496 | 2496 | 2496 | 2496 | 2496 | 2496 |

Standard errors in parentheses. GFW/OpEx $\in [0.25, 1.75]$ .

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Columns (1)-(2) uses the preferred employment growth measure, $EG^D$. These results are provided as a reference point. $EG^N$ is defined similarly as my main employment growth measure, $EG^D$, but does not account for the duration of employment within the year. $EG^Y$ is year-on-year employment growth, which considers growth from Dec-to-Dec in the number of employees.

### A.10.2 Robustness to Weighting

I show that the main results are highly robust to different weighting schemes in Table A.11. In particular, I show that results are somewhat *stronger* when completely omitting professional services (PS) firms that are overrepresented in my sample.

First, consider Table A.11. Column (1) provides the baseline specification for comparison. Column (2) weights by ownership share. If a firm only appears in the sample with one investor owning $<$ 100%, then this firm will be down-weighted relative to other firms. Column (3) does not downweight professional services. Column (4) excludes professional services. Columns (5)-(6) do not use weighting: Instead I select only one investor per firm. The selection criteria is that this weight, when not downweighting professional services, was $>$ 50%. Columns (5) and (6) differ in that column (6) excludes professional services.

Table A.11: Robustness to Weighting Scheme

| $EG^{D}_{07,10}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $\text{Gains}_{08-09}$/GFW | 0.488** | 0.436** | 0.430** | 0.561*** | 0.458** | 0.603*** |
| | (0.192) | (0.195) | (0.186) | (0.213) | (0.193) | (0.225) |
| Weighting | Stocks | Own % | Stocks | Stocks | - | - |
| DownweightPS | Y | - | - | - | - | - |
| OnlyOneInv | - | - | - | - | Y | Y |
| IncludePS | Y | Y | Y | - | Y | - |
| GFW/OpEx | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] | [0.25,1.75] |
| Pcontrols | Y | Y | Y | Y | Y | Y |
| Fcontrols | Y | Y | Y | Y | Y | Y |
| Vcontrols | Y | Y | Y | Y | Y | Y |
| FE | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R | NACE3,R |
| r2 | 0.1340 | 0.1446 | 0.1285 | 0.1434 | 0.1303 | 0.1468 |
| N | 2496 | 2479 | 2496 | 1981 | 2085 | 1655 |

Standard errors are two-way clustered at the firm and investor level and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. $Gains_{08-09}/GFW_{07}$ is amount of stock market gains the investor experienced during 2008 and 2009, fixing portfolio weights in 2007, scaled by 2007 Gross Financial Wealth. Column (1) represents the main specification and is provided as a reference. Here, I weight by stocks which implies weighting by the amount of stock wealth the investor has relative to the total stock wealth of all investors who also own shares in the firm and are in the regression sample. In column (2), I instead weight by ownership share. In column (3), I again weight by owners by their stock holdings, but omit the down weighting of Professional Services (PS) firms. In column (4), I still weight owners by stock holdings, but completely drop PS firms. In column (5), I include PS firms, do not down weight them, but only keep one investor per firm. OnlyOneInv implies that per firm I only select investors whose non-downweighted weight was $> 50\%$, leaving me with one observation per firm. IncludePS indicates whether I included Professional Services firms (NACE2 codes 69-75).