Do entrepreneurs matter?*

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Abstract

Entrepreneurs strongly affect firm outcomes. We use deaths of several hundred entrepreneurs as a source of exogenous variation, and find large and sustained effects of entrepreneurs at all levels of the firm performance distribution. Entrepreneurs strongly affect performance of both very young firms and more mature firms, and more strongly in 'dynamic' industries with higher education level, larger R&D expenses and higher sales growth. The effects appear to be driven by entrepreneur specialness rather than leadership transition; the effects of death of entrepreneur-managers is economically and statistically much stronger than the death of managers that are not entrepreneurs. Overall, entrepreneurs play a large and unique role not previously documented.

Keywords: entrepreneurship, firm performance, human capital.

JEL Classification: D21, D24, J23, L11, L25, G39.

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I. Introduction

In the large literature on firm performance, spanning corporate finance, industrial economics and parts of labor economics, economists have paid little attention to entrepreneurs. The idea of entrepreneurs as movers and shakers is old (Schumpeter, 1934), but geographical, institutional, and industry characteristics (Syverson, 2011), managers and managerial practices (e.g., Bertrand and Schoar, 2003, Bloom and Van Reenen, 2007), and financial conditions (Guiso et al., 2004) have been the focus of empirical work. One objective of the paper is to ask how much individual entrepreneurs contribute to the performance of firms.

Little is known whether entrepreneurs have much of an effect. In addition to informing our understanding of firm outcomes, this question relates to an old debate stemming from Coase (1937) over what constitutes a firm and keeps it together. We study whether the entrepreneurs constitutes the 'core' of the firm, and for how long. We also study whether entrepreneur are 'special' compared to managers. We contribute to the debate on the foundation of the firm, which arguably lies at the root of economics, and also to which factors contribute to the performance of nascent and young firms.

To study the influence of entrepreneurs, we examine firms where the entrepreneur dies. In these firms, the entrepreneur's engagement was random, determined by the timing of the entrepreneur's death rather than underlying economic conditions. These deaths therefore provide an opportunity to quantify whether entrepreneurs have a causal effect on firm performance.[1] We analyze the impact of entrepreneur death on firm survival, growth, and profitability. For example, we compare survivorship rates of firms where the entrepreneur dies with survivorship rates of firms where the entrepreneur does not die. We interpret differences in survivorship rates between these two groups of firms as evidence for the personal importance of the entrepreneur.

Alternatively, a lack of difference between these two groups would provide evidence of the non-importance of the entrepreneur; once the firm has been formed the entrepreneur can literally vanish without harming the firm. Imagine a small restaurant. The entrepreneur decides which corner to locate on, the menu, and which individuals to hire. After these major initial

[1] Several recent papers use death as an exogenous event to study causal effects, for example Azoulay et al. (2010) on the spillover effects of research superstars, Jones and Olken (2005) on the influence of national leaders for economic growth, Nguyen and Nielsen (2010) on the value of independent directors at company boards, Bennedsen et al. (2007) on the value of CEOs, and Andersen and Nielsen (2012) on the effect of windfall gains through inheritance on entrepreneurial activity.

decisions have been made, the entrepreneur may not play a large, irreplaceable, role anymore, and the firm may be equally well led by other individuals.

We employ a large and unique database that contains longitudinal accounting and employment information on the universe of incorporated firms established in Norway between 1999 and 2007. Covering the population of new firms means that the vast majority of firms in the database are small. We focus on firms that employ at least one person at the end of the first year of operations. For each firm, the data identify the initial owners. We define an entrepreneur as an individual with a substantial ownership share in the firm when it is established.

We define an entrepreneur as an individual that owns more than 50 percent of the shares initially. The database contains 16,127 firms started up by such individuals, with at least one employee in the year of foundation, and 205 firms where the majority founder dies before the end of 2010. In separate analyses, we also look at 50 percent owners and at minority owners. We track firm performance with accounting data submitted yearly to the tax authorities. The accounting data runs until 2012, thus firms in the database are between zero and thirteen years old.

We ask whether entrepreneurs have a causal effect on firm performance. In order to accommodate that entrepreneurs that die within the sample period are on average older (and, as a consequence, wealthier) than entrepreneurs that do not, for each of the firms where the entrepreneur dies ('treated' firms) we use propensity score matching to identify a similar firm ('matched control' firm), restricted to be started up in the same calendar year. The matched control firms have similar characteristics at startup date, but do not experience subsequent entrepreneur death. We run difference-in-differences regressions, comparing the performance of treated firms and matched control firms before and after entrepreneur death. In a separate set of regressions, we utilize all firms in the database (i.e., do not use matching) and obtain very similar results. To accommodate time-invariant firm characteristics, we include firm fixed effects in both type of regressions.

The empirical analysis provides robust evidence that firm performance drops after the entrepreneur's death. Entrepreneur death leads to a stark reduction in sales and smaller but significant effect on employment, compared to the control group, during the first two years after entrepreneur's death. We expected the group of firms that experienced the death of the entrepreneur to have a dip in performance immediately after the death owing to the upheaval, but

we anticipated there would be a bounce back. However, even four or five years after the death, the treated group of firms shows no sign of recovering relative to the control group and the negative effect on performance appears to continue even further beyond that.

The results are partly driven by firms closing down. But quantile regressions suggests large negative effects of entrepreneur death also for firms that do not go out of business; the results are strong at all quantiles from the 50th to the 90th.

One would expect entrepreneurs to matter more in sectors of the economy that are more reliant on human capital relative to physical capital. The results suggest that this is indeed the case; the negative effects of entrepreneur death are larger in sectors with more highly educated entrepreneurs, and in sectors with larger R&D expenses and higher sales growth.

The results we are picking up may not be due to entrepreneur specialness, but detrimental effects of abrupt leadership transition in young firms. Indeed, it is possible for founders and successors to be of identical ability and yet to find an effect of founder death because it is costly for the firm to adapt to a new leader. In order to deal with this question, we employ data that provides the identity of the firm's chief executive officer (CEO) between 2002 and 2007. We split the CEOs into two groups: founders and non-founders, and compare the causal effect of CEOs that are founders with the causal effect of CEOs that are not founders. We find that the effects of death of CEO-founders are economically and statistically much stronger than for CEOs that are not founders. We conclude that the results appear driven by the specialness of the entrepreneur and not leadership transition of young firms.

One would expect that firms become less reliant on the entrepreneur as they mature. Johnson et al. (1985) examine the effect on share price of senior management deaths for a sample of 53 U.S. publicly traded firms. The effect of CEO death on share price is negative for the sample overall, but positive for the death of CEOs that were also founders of the company, a finding verified with more recent data by Pérez-González (2006). We analyze whether the magnitude of the drop in firm performance upon entrepreneur death depends on firm age. The very youngest companies suffered most after the founder's death, but sizable effects were still felt by companies that were up to ten years old.

An alternative explanation for our findings could be reverse causality: poor firm performance leads to entrepreneurs having a higher probability of dying. This could come in two forms. First, firms experiencing founder death might be weaker at birth because illness (or ex-

pectation of illness) of the founder leads the founder to start small. We do not really expect this to be the case given the undoubted strains of starting up a business, which might make firm foundation more of an 'all or nothing' choice. Indeed, we show that treated and control firms do not differ in their year zero employment and equity, suggesting that key observable indicators of firm size do not differ between the two groups.

Second, firms experiencing founder death might start to perform poorly after foundation but before founder death. We do not find evidence of pre-treatment differences in firm performance, which suggests that reverse causality is not a major concern.[2]

Part of the explanation for the strong effects of entrepreneur death could be that post-death, the control of the firm is transfered to less competent family members. Perez-Gonzales (2006), Bennedsen et al. (2007), and Bertrand et al. (2008) document negative effects on performance from family CEO appointments inside mature firms. To deal with this question, we examine whether entrepreneur death affects family firms (defined as a firm where another family member is a co-owner initially) differently than non-family firms. Although the sample of family firms is not large (less than 6% of firms are co-owned by family members), it appears that family firms are more resilient to the loss of the entrepreneur than non family firm. It does not appear, therefore, that value destruction by family members can explain the results.[3]

Firms that experience entrepreneur death have much lower survival rates in the years after the death event.[4] Perhaps heirs voluntarily close down firms that were largely motivated by providing private rather than economic benefits for the dead entrepreneur. To examine this question we analyze bankruptcies. The bankruptcy code in Norway is similar to Chapter 7 in the U.S. bankruptcy code, i.e., bankruptcy is associated with creditors taking control, and very unlikely to be 'voluntary' (as in Chapter 11 in the U.S bankruptcy code). We find that firms where the entrepreneur dies have twice the probability of going bankrupt. Thus there is little to support the notion that voluntary close-downs by heirs is driving the results.

The paper connects to several strands of literature. First, economists have shown that large

[2]We do not have access on data on health or on the cause of death. The absence of pre-treatment differences suggests that founder death comes unexpected, or that health issues associated with an expected death are not sufficiently large to deter the entrepreneur from actively engaging prior to death.

[3]In Section VI.A. we discuss whether aspects of Norwegian inheritance law or capital gains tax could influence our results.

[4]A firm is not active if it (a) has gone bankrupt, (b) closed down for other reasons, (c) has less than NOK 50,000 (ca. 6,500 Euros) in sales. So, bankruptcy is just one reason for non-survival.

and persistent differences in productivity across firms exist even after taking into account geographical, industry and firm age differences (see the surveys by Bartelsman and Doms, 2000 and Syverson, 2011). Much less is known about the importance of individuals. We point out that a factor missing in this literature – individual entrepreneurs and their engagement – can explain some of the heterogeneity for young firms.

Second, a growing literature demonstrates that professional managers and management practices can lead to improved outcomes for mature firms. [5] Bennedsen et al. (2007) show that within-family succession of chief executive officers (CEOs) harm firm performance relative to employing a manager from outside the family. Bennedsen et al. (2010) finds that the average operating returns on assets decreases by about 1 percentage point in a four-year window around CEO death. Bennedsen et al. (2012) find that CEO hospitalization leads to firm underperformance. For example, long hospital stays (more than 30 days within a year) implies about 1 percentage point drop in operating returns on assets of the firm. Our findings complement this literature by showing that for young firms, entrepreneurs play a large role. In addition, we show that entrepreneur-managers play a much larger role than non-entrepreneur managers.

Third, much work on entrepreneurship focuses on the antecedents of entrepreneurship (Evans and Jovanovic, 1979, Hurst and Lusardi, 2004, Kerr and Nanda, 2009) and the risk-return trade-off of the entry decision (Hamilton, 2000, Vereshchagina and Hopenhayn, 2009, Hall and Woodward, 2010). Less is known about which factors affect outcomes. Holtz-Eakin et al. (1994), Hvide and Moen (2011) and Andersen and Nielsen (2012) analyze the role of financial constraints. Lerner and Malmendier (2014) finds that learning about other individuals' entrepreneurial experiences decreases entry rates but improves performance, and Hvide and Panos (2014) finds that entrepreneurial risk aversion has a similar effect. We complement this literature by being the first to directly measure the impact of entrepreneurs, and to demonstrate that the impact is large.[6]

[5] Bertrand and Schoar (2003) documents differences in management styles between individuals, and find evidence consistent with CEOs of publicly listed companies affecting firm performance. Bloom and Van Reenen (2007) and Bloom et al., (2011) document that higher-quality management practices are correlated with several measures of productivity and firm performance in a sample of non-listed firms. They do not focus on the role of individual managers, but their results are consistent with individual managers playing a large role through affecting management practices. Kaplan et al., (2012) show that CEO characteristics matter for firm performance.

[6] Kaplan et al. (2009) study strategy and management changes in a sample of 156 fast-growing companies that eventually go public. Between receiving venture funding and the initial public offering, almost none of these companies change their line of business, while the management team changes quite frequently. Thus, for this

Fourth, governments try to boost competitiveness through a vast array of policies. Whether entrepreneurs matter relates to public policy measures to alleviate liquidity constraints. If entrepreneurs personally embed a major part of the value of the firm, it will be difficult to pledge the value of the firms to outside investors, which leads to lack of financing and underinvestment in entrepreneurial firms, as in Hart and Moore (1994). The extent to which entrepreneurs are non-substitutable is a largely unexplored question. We show, consistent with Hart and Moore (1994), that entrepreneurs personally do embed a major part of the value of the firm, and for surprisingly long.

The remainder of this paper is organized as follows. Section II discusses why entrepreneurs should matter in light of existing theory and evidence. Section III presents the data and descriptive statistics. Section IV discusses the empirical strategy. Section V presents the main results and specification checks, while Section VI explores heterogeneity by means of interaction effects. Section VII interprets the results and concludes.

II. Why should entrepreneurs matter?

Standard theoretical models tend to take a neoclassical view of the firm in which entrepreneurs are homogeneous inputs in the production process, and substitutable once a firm has been founded. For example, in Kihlstrom and Laffont (1979), the entrepreneur bears residual risk but does not contribute to firm performance. In sorting models (e.g., Lucas, 1978, Evans Jovanovic, 1989, Lazear, 2005), individuals with high entrepreneurial ability become entrepreneurs, while individuals with low entrepreneurial ability become workers. Although sorting models, or variations of such, are consistent with individual entrepreneurs being important to firm performance, a degree of smoothness in the distribution of entrepreneurial ability will tend to rule out individuals playing a large role. Of course, the neoclassical view does not exclude the possibility that there are transitional costs, such as search costs or turbulence costs, from replacing the

sample of companies, the idea seemed more stable than the management team. One takeaway from the present paper is that individuals are more important for newly established firms than the results from Kaplan et al. (2009) would suggest. Glaeser et al. (2014) use mines as an instrument for entrepreneurship and find a persistent link between entrepreneurship and city employment growth. We use random variation created by death and find evidence suggesting that entrepreneurs have a large and sustained impact on their firms.

entrepreneur.[7]

One theoretical tradition that justifies non-substitutability is critical resource theory (Wernerfelt, 1984, and Rajan and Zingales, 1998, 2001), where a firm is a set of specific investments built around a critical resource or resources. In the current context, the entrepreneur's human capital, personality, and ideas can be seen as the critical resource which the firm is initially organized around (this is a sense in which the entrepreneur shapes the production function of the firm). The entrepreneur then invests in physical and human assets that are complementary to himself, and may not be fully substitutable because other individuals lack his combination of traits. Under this view, the entrepreneur can have two effects on firm performance. The first is the direct effect through own productivity, and the second, which works via providing the critical asset, is positive spillover effects on the other assets of the firm. We find very large negative effects on sales after entrepreneur death but smaller negative effects on firm employment, consistent with spillovers from the entrepreneur to the productivity of the firm's employees.

Critical resource theory says less about for how long the entrepreneur is essential. One reason to be concerned about this question is that the duration of non-substitutability influences how long firms are financially constrained and subject to underinvestment. Critical resource theory also says less about which activities make entrepreneurs important. Leadership in mature firms is divided between managers and owners, where managers take care of daily operations and owners oversee managers and provide strategic direction. Entrepreneurs in young firms tends to be engaged in both processes and our empirical strategy allows us to study which role is more important.

III. Data and descriptive statistics

III.A. Norway

We start with a brief description of the Norwegian economy, the tax code, and the basis for the data collection.[8] Norway is an industrialized nation with a population of about 4.7 million. The

[7]Other theories of entrepreneurship such as Hellmann (2007) and Hvide (2009) emphasize how contractual frictions in established firms can induce entrepreneurship. These theories can explain productivity differences between entrepreneurs, but not why entrepreneurs become non-substitutable.

[8]The material is taken from the OECD Statistical Profile for Norway: 2010, available at OECD.org, and from Statistics Norway webpages.

GDP per capita in 2008 was about $58,717 when currencies are converted at purchasing power parity; this is higher than the EU average of $30,651. Norway is characterized by a large middle class, and a low inequality of disposable income. For labor income, the maximum marginal tax rate (for incomes above $75,000) is about 50%, which is fairly typical by European standards. The capital income tax is a flat 28% on net capital gains.

Similar to other industrialized countries, setting up an incorporated company in Norway carries tax benefits relative to being self-employed (e.g., more beneficial write-offs for expenses such as home office, company car, and computer equipment), and incorporation status will therefore be more tax-efficient than self-employment status except for the smallest projects. The formal capital requirement for registering an incorporated limited liability company was NOK 50,000 in equity until 1998 and NOK 100,000 thereafter (in 2008, $1 was equal to about 7 NOK).

In contrast to most OECD countries, Norwegian households are subject to a wealth tax every year throughout their lives.[9] The government's statistical agency, Statistics Norway (also known by its Norwegian acronym SSB) collects yearly data on wealth and income at the individual level from the Norwegian Tax Agency, and we obtain our data from Statistics Norway. Earnings and wealth figures for individuals are public information in Norway. This transparency is generally believed to make tax evasion more difficult and hence data more reliable.

The tax value of a firm, which is included in its owners' wealth statements, is calculated as sixty percent of assets subtracted debt, where debt is evaluated at face value while assets are at book value (typically lower than market value). Selling off a non-listed company therefore produces a tax liability if, which one can expect to commonly be the case, the transaction price exceeds the tax value of the company. This liability can be evaded by transferring the company to a holding company before selling off. We therefore do not expect the capital gains tax to bias the individuals that inherit a non-listed company towards keeping it or selling it off. In Norway there is also tax on inheritance. The inheritance tax on a non-listed company is based on the tax value of the firm on January 1 in the year of death. This means that the inheritance tax is effectively sunk once inheritance has taken place. We have therefore no reason to believe that

[9] In contrast, the U.S. tax system requires wealth reporting only in connection with estate tax, which is imposed only on the very rich at the time of death (Campbell, 2006). The wealth tax in Norway is 0% up to about $120,000 in net wealth, and about 1% for net wealth above $120,000.

the inheritance tax will bias our results in any particular direction.[10]

III.B. Data

We construct a database that consists of the universe of incorporated, limited liability, firms in Norway between 1999 and 2007, where one individual holds at least 50 percent of the initial shares.[11] The data include yearly detailed accounting and employment measures for each firm until the end of 2012, so that the firms in the database are between zero and thirteen years old. Covering the population of new firms means that the majority of firms in the database are small. In the analysis, we therefore confine attention to firms that have at least one employee (which may be the entrepreneur himself) in the first year of operations. Further, to avoid counting wealth management vehicles as start-ups, we eliminate finance and real estate firms (NACE 65-70). The inclusion of these firms gives similar results. The median book value of assets and number of employees in the first year of operations is about $160,000 and three, respectively.

Comparing our dataset with recent datasets used in the productivity literature, Foster et al. (2008) analyze the universe of manufacturing plants in the U.S. over a 20-year period. The firms are split into four age categories [age bracket in parentheses]: entrants [0,5], young [5,10], medium [10,15] and old [15 and older]. Thus our data cover more than two of the four firm age brackets considered by Foster et al. (2008). Compared to datasets of the productivity literature, a main novelty is that the data contain ownership shares in the incorporation year, broken down by each owner with at least a ten percent ownership share. We have a detailed panel on socio-demographic information on all owners, including year of death if applicable, ranging from 1993 to 2009.

[10] If a spouse inherits, no inheritance tax will be paid until the spouse dies or remarries. If children of the entrepreneur inherit, in the period we study there was a 20% inheritance tax on inheritances whose tax value exceeded NOK 550.000, 8% rate on inheritances between 250.000 and 550.000 and 0% below 250.000 (for unrelated beneficiaries, the rates were slightly higher). For example, if the firm has NOK 2.1 million in assets and NOK 1 million in debt, the tax value is NOK 1.1 million. If two children inherit, they receive NOK 550.000 each, and are taxed 8% on NOK 300.000, i.e., they pay NOK 24.000 in inheritance tax each. (NOK 24,000 is equivalent to about 3,200 Euro.) This is unlikely to be a challenge for most Norwegian households, so we do not expect liquidity constraints to be important, in contrast to in Tsoutsoura (2014). The approximate median tax value of the firms in our sample is NOK 71.000, the 75 percentile is NOK 154,000, and the 90 percentile is NOK 355.000. In 2008, $1 was equal to about NOK 7.

[11] For 1999, the data contain only a sample of the firms started. Diagnostic tests do not suggest any selection bias. We eliminated firms where the founder died after 2010 because we want to have at least two years of post-death information for any firm in our analysis. We also drop firms where the founder was older than 67, i.e. beyond retirement age, when founding the firm. Our results do not weaken if we include these firms.

The data are compiled from three different registers:

1. *Accounting information from Dun & Bradstreet's database of accounting figures based on the annual financial statements submitted to the tax authorities.* This data include variables such as 5-digit industry code, sales, assets, number of employees, and profits for the years 1999-2011. Note that the D&B data contain yearly information on *all* Norwegian incorporated limited liability companies, and not a sample as in the U.S. equivalent. Incorporated companies are required to have an external auditor certifying the accounting statements in the annual reports.

2. *Data on individuals from 1993 to 2010 prepared by Statistics Norway.* These records are based on government register data and tax statements, and include the anonymized personal identification number and yearly socio-demographic variables such as gender, age, education in years, taxable wealth, and income. The data identify the year of death, if applicable, and also identifies family relationships between individuals, which allows us to identify family firms. The data contain *all* Norwegian individuals, not a sample as in the Panel Study of Income Dynamics or the Survey of Consumer Finance. As with the PSID and the SCF, the data are anonymized (contains no names of individuals).

3. *Founding documents submitted by new firms to the government agency 'Brønnoysund-registeret'.* This register data include the start-up year, total capitalization, and the personal identification number and ownership share of all initial owners with at least 10 percent ownership stake.

For each new firm identified in 1), we create a list of owners identified through 3) and compile their associated socio-demographic information from 2). We define an entrepreneur as a person with more than 50 percent ownership of the total shares in a newly established limited liability firm. We interchangeably refer to this person as 'the entrepreneur' or 'the founder'. Restricting the sample to majority owners ensures that we are likely to include 'real' entrepreneurs in our sample. (In separate analysis below, we also look at owners with less than, and equal to, 50 percent ownership share.) For a small fraction of firms, the first year of financial reporting, defined through 1), is different than the year of incorporation defined by 3). For these firms, we define the first year as the first year of reporting.[12]

[12]In contrast to Hvide and Moen (2010), the current dataset contains the population of new firms. A large

III.C. Descriptives of original sample

Table 1 presents descriptive statistics of the firms and founders in the sample. Founder characteristics generally refer to the first year of operations, with the exception of log wealth and log earnings which are taken as the log of five-year averages prior to firm foundation. Firm characteristics refer to time of incorporation. Table 1 contrasts characteristics of 'treated' firms (i.e., where the founders die during our sample period) with 'control' firms (i.e., where the founders do not die during the sample period). In the initial sample of 16,127 firms, 205 experience founder death during our sampling period.[13] Founders who die are older and (likely as a consequence) wealthier and less educated. The sectoral composition is very similar. The only small differences are that firms where the founder dies are more likely to be in transportation, and less likely to be in other services. This might reflect the fact that the 'treated' founders are less educated and therefore more prone to be in more traditional industries.

INSERT TABLE 1 HERE

Table 2 shows the timing of entry and the timing of death for the treated firms. Firms where the founder dies enter in all years between 1999 and 2007 inclusive. Founders of these firms die in all years between 2001 and 2010 inclusive.[14] Another useful descriptive is firm age at founder death. Founder death occurs at any firm age, from year 1 through year 11 (the maximum firm age possible given our sample). In our analysis, amongst others, we will look into the question of whether founder death has different implications for younger versus older firms.

INSERT TABLE 2 HERE

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literature focuses on the self-employed (e.g., Hurst and Lusardi, 2004). By studying incorporations, we can meaningfully distinguish between the life-span of the entrepreneur and the life-span of the firm; for obvious reasons our empirical strategy would be impossible with data on the self-employed.

[13] About one-half of the firms in our database have an individual with at least 50% initial ownership. The remaining firms are either started up by a team of individuals or (more frequently) by a firm. The latter category is likely to be spin-offs of divisions of established firms, rather than start-ups proper. This is also reflected in the firm size distribution at firm foundation.

[14] Remember that we deliberately excluded observations where the founder dies after 2010 because we have no data for their firms after the year of death, so we cannot identify effects of founder death on firm survival and firm performance for them.

IV. Empirical strategy

IV.A. Estimation sample

It is natural *not* to compare the 205 firms with founder death to *all* 15,922 firms without founder death, but to limit the analysis to those firms (and their founders) in the control group who are most comparable in terms of their observable characteristics. We use propensity score matching to select the firms in the control group who are most similar to the firms in the treatment group. More specifically, we use nearest neighbor matching to select those firms in the control group whose *ex ante* probability of experiencing founder death is closest to that of the 205 firms where the founder dies.[15] Our further analysis then proceeds on this matched sample. For comparison, we also perform the analysis using OLS on all 16,127 firms in our database. Those results are presented in Table A.1.

The propensity score is the probability of treatment (i.e., founder death) conditional on pre-treatment characteristics. The idea of propensity score matching is to match treated and controls whose *ex ante* probability of receiving treatment (i.e., to experience founder death) – as predicted by their pre-treatment characteristics – is 'identical' (see Rosenbaum and Rubin, 1983). By 'pre-treatment characteristics' we mean characteristics at firm foundation, i.e., the variables shown in Table 1. Characteristics measured at a later point, e.g., in the year before founder death, might already be subject to endogeneity bias because of the foreshadowing of (later) founder death.

To estimate the propensity score, we run a probit model of founder death on the characteristics from Table 1. The results are reported in Table A.2. We obtain estimated propensity scores for all 205 'treated' founders and for 15,922 controls.[16] *Ex ante*, the treated make up just above 1 percent of our sample. Based on the estimated propensity score, we use nearest-neighbor matching (without replacement) to combine treated and control observations.[17] We impose a caliper (i.e., radius) of 0.05, i.e., treated firms that have no comparison unit whose estimated propensity score is within 0.05 of their own estimated propensity score are discarded to avoid

[15] In unreported analysis, we use two-nearest neighbor matching and obtain very similar results.

[16] Some control units are automatically dropped in the propensity score estimation because they have predicted probabilities of zero, i.e. their characteristics perfectly predict non-treatment.

[17] We use a version of Edwin Leuven and Barbara Sianesi's Stata module *psmatch2* (2010, version 4.0.4, *http://ideas.repec.org/c/boc/bocode/s432001.html*) to perform propensity-score matching and covariate balance testing.

bad matches. Imposing this caliper, it turns out, we lose no treated founders whatsoever.[18] Importantly, we impose exact matching on the year the firm starts activities. This is to make sure that we are comparing pairs of treated and control firms that are of the same age in the same calendar year.

In line with the differences detected in Table 1 between treatment and control group, the pre-treatment characteristics have substantial explanatory power in predicting founder death. Table A.2 shows that the pseudo-R^2 is 0.11. The variables entering the propensity score estimation are jointly significant at the 1%-level. Another indicator of differences between treatment and control group before matching is the so-called median absolute standardized bias, defined by Rosenbaum and Rubin (1985) as the comparison between (standardized) means of treated and control units, where the standardized differences (standardized biases) between the means for a covariate $\mathbf{x}_i$ are defined as:

$$B_{before}(\mathbf{x}_i) = 100 \cdot \frac{\bar{\mathbf{x}}_{i1} - \bar{\mathbf{x}}_{i0}}{\sqrt{\frac{1}{2}(V_1(\mathbf{x}_i) + V_0(\mathbf{x}_i))}}$$

where $\bar{\mathbf{x}}_{i1}$ denotes the treated unit mean and $\bar{\mathbf{x}}_{i0}$ the control unit mean for covariate $\mathbf{x}_i$ and where $V_1(\mathbf{x}_i)$ and $V_0(\mathbf{x}_i)$ are the sample variances in the treated group and control group, respectively. The median absolute standardized bias before matching is 16.95. Rosenbaum and Rubin (1985) suggest that a value of 20 is 'large', i.e., in line with the other two indicators above, treated and control groups do differ considerably *ex ante*.

On the basis of the estimated propensity score, for each treated firm we search for the control whose propensity score is closest to that of the treated firm ('nearest neighbor matching'). All control firms that do not qualify as a nearest neighbor are discarded from the further analysis.

Matching gives us a better control group and reduces the bias in comparing treated and control groups to the extent that it manages to largely remove the pre-treatment differences between the treatment and control group. We can formally test this, using the same three indicators of imbalance between the treatment and control group, but now using the matched sample. To do so, we re-run the same propensity score specification on the matched sample, i.e., on the sample of treated and *matched* controls. After matching, the pseudo-R^2 drops to 0.02 (from the 0.11 reported in Table A.2). Similarly, the variables entering the propensity score are no longer

[18] While imposing a caliper is inessential in our case, we follow common practice to impose it in the first place.

jointly significant, with a p-value of 0.995. The median absolute standardized bias drops from 16.95 before matching to 3.85 after matching.[19] Matching thus appears to be very successful at reducing (or even removing) differences in observable pre-treatment characteristics. In other words, our matched sample consists of firms where the founder dies and a set of 'twin firms' who are *ex ante* observationally identical, but where the founder does not die. We consider the matched control group as a useful comparison group that approximates the counterfactual outcome of the treated firms.

IV.B. Difference-in-differences setup

We ask whether individual entrepreneurs have a causal effect on firm performance. To answer this question, we examine whether firms where the founder dies perform differently from firms where the founder does not die. We are mainly interested in differences after founder death. However, we also look into performance differences before founder death. Differences in performance before founder death would indicate a deterioration in the condition of the founder and his firm before his death. As we will show, there are no differences between treated and control firms before founder death, which is consistent with two possible explanations. Either founder death comes as a surprise, in which case it is natural not to detect any pre-death differences in performance; alternatively, even if the founder already has health issues before his year of death, they do not seem to affect firm performance. When comparing firm performance measures in the year before founder death, we can again use the pseudo-R^2 of a regression of the treatment dummy on firm performance measures as indicators of differences between treated and control firms.[20] The pseudo-R^2 from a regression of the treatment dummy on these performance measures is 0.007, an indication that treated firms and controls do not differ in their performance in the year before founder death. In fact, when looking at t-tests for differences in means between treated firms and matched controls for each and every performance variable, we

[19]The median absolute standardized bias after matching is defined as

$$B_{after}(\mathbf{x}_i) \quad = \quad 100 \cdot \frac{\bar{\mathbf{x}}_{i1M} - \bar{\mathbf{x}}_{i0M}}{\sqrt{\frac{1}{2}(V_1(\mathbf{x}_i) + V_0(\mathbf{x}_i))}},$$

where $i1M$ and $i0M$ refer to the matched treated and control units.

[20]We use the same firm performance measures that we use later on in our main analysis: firm survival, (log) sales, (log) assets, (log) number of employees, and operating return on assets.

find no significant differences in the year before founder death. All t-statistics are below 1.5. We take this as clear evidence that treated and control founders/firms are not only comparable at firm foundation (see the results from propensity score estimation discussed above), but that matched pairs of treated firms and controls founded in the same year also develop similarly until the year right before founder death.

Our main focus from now on is on understanding whether founder death affects firm performance after founder death. Why do we not just perform a standard regression analysis using the whole sample? There are two reasons. First, as shown above, treated firms and controls are not necessarily comparable *ex ante*, and matching allows us select those controls that are best matches. Yet, Angrist (1998) shows that matching and regression analysis using a fully saturated (=interacted) model differ only in the (implicit) weighting attached to treatment effects within cells defined by combinations of X characteristics. So, matching is not fundamentally different from a fully saturated OLS model and this is not the main reason for using matching. In fact, in Table A.1, we also present OLS results, for comparison. Second, and most importantly, for control observations, the year of founder death is not defined. Matching is key to finding comparable controls who started business in the same year as individual observations of treated firms. We then use year of founder death at treated firms to impute the counterfactual year of founder death of the matched control.[21] Based on this, we can define 'before' and 'after' founder death for both treated firms and matched controls. Our estimation sample consists of the 205 treated firms and 205 matched controls.

We start by looking at very basic differences-in-differences panel regressions, where we compare treated and matched controls to assess how firm performance is affected by founder death:

$$Performance_{it} = \alpha + \beta_1 * treated_i + \beta_2 * after_{it} * treated_i + \beta_3 * after_{it} + \gamma * X_{it} + \delta_t + \epsilon \quad (1)$$

β_2 is our main coefficient of interest, measuring the difference between treated firms and control firms after founder death.[22] However, β_1 is also of interest because it provides for a test of (a

[21] The analysis described above, where we looked into the comparison of treated firms and controls in the year before founder death, is based on the actual (for the treated firms) and imputed (for the controls) year of founder death.

[22] Note that, in the basic differences-in-differences regressions, we exclude the year of founder death from the regressions because it cannot be clearly assigned to either before or after founder death. Later on, we take the

lack of) pre-treatment effects. We routinely control for all variables that entered the original matching procedure, i.e., founder and firm characteristics pertaining to the year in which the firm started operations, as well as year dummies. Adding control variables adjusts for any small residual bias and increases efficiency. This 'bias-corrected' matching has been found in Abadie and Imbens (2006) to work well in practice.

Later, we extend this analysis in various ways. First, we look in more detail at how performance varies year by year after founder death, i.e., we replace the simple 'after' dummies by indicators for 'one year after founder death', 'two years after founder death' etc. Second, we look into heterogeneity of the treatment effect by founder and firm characteristics. The idea is that, for instance, the death of a highly educated founder might be a bigger loss to the firm than the death of a less educated founder. Similarly, founder death may be more detrimental for young firms than for mature firms. We approach these questions by introducing interaction terms between the treatment dummies and certain binary characteristics, like whether the founder is highly educated or not. Likewise, we interact the before/after dummies and the difference-in-differences parameter β_2 with binary indicators of founder or firm characteristics. This informs us whether treatment affects some firms more than others, i.e., whether there is heterogeneity in treatment effects. Third, we look into quantile regressions to see whether the results are driven by things that happen at the lower, middle or upper end of the conditional performance distribution. We turn to these issues below.

Startup performance can be measured by survival, growth, and profitability. We analyze how entrepreneur death affects all these aspects of firm performance. Survival is assessed by whether a firm is active in given year or not.[23] To assess growth, we examine the effect of entrepreneur death on sales, on human assets as measured by employment, and on the (book) value of physical assets. For a firm that closes down, we set the relevant variables equal to zero to measure the effect on sales, employment and assets.[24] To assess profitability, we use operating return on assets (OROA). OROA is defined as the ratio of earnings before interest and taxes (EBIT) to the total asset base used to generate them, and is the standard performance measure

analysis one step further and estimate separate treatment effects for each year, including the year of founder death.

[23] A firm is not active if it (a) has gone bankrupt, (b) closed down for other reasons, (c) has less than NOK 50,000 in sales.

[24] One might be tempted to exclude firm-year observations after firm closure, but that would introduce a bias. A simple example illustrates this point: if founder death has a devastating effect so that only one of the highest-quality treated firm survive, our regression estimates for firm growth would be positive.

in a large accounting and financial economics literature (see e.g., Bennedsen et al. 2007 and references therein).[25] Firms that cease to exist have zero earnings, zero employees, and zero assets (see above), while OROA is undefined. We impute OROA equal to zero for these observations. In an alternative specification, we impute an OROA that is 'unreasonably' high, equal to the average OROA in our data (about 6.1 percent). Under this alternative imputation, we obtain no effects on OROA at the mean but obtain very similar results in the quantile regressions.

V. Do entrepreneurs matter?

V.A. Main results

Table 3, Panel A, presents the results from the difference-in-difference estimation described in Section IV. We consider a window from five years before to five years after founder death (including all years slightly strengthens the results).[26] The second row reports the estimated β_2 coefficient for the outcome variables.

INSERT TABLE 3 HERE

The results presented in Panel A of Table 3 show that entrepreneurs have significant effects on firm growth and survival. The effects, especially for firm growth, are large; for example, the mean effects on sales are about 53 percent, while the mean asset effects are about 60 percent.[27] The effect on employment is considerably smaller, with 19%. The large effect on sales relative to employment suggest that entrepreneurs contribute to the productivity of the firm's employees through spillover effects.[28] The estimated effects on profitability are quite modest compared to growth, which is likely due to asset sales after founder death.

[25] Unlike returns to equity or returns to capital employed, OROA compares firm profitability relative to total assets. In contrast to net income-based measures such as return on assets, OROA is not affected by capital structure or dividend policy differences across firms. The asset base we use to compute yearly OROA is the average of assets at the beginning and the end of the calendar year. To prevent outliers from driving our results, we winsorize the yearly profits and OROA values at the 5% level.

[26] Appendix Figure A.1 shows that the number of observations outside this time window rapidly declines.

[27] Remember that with log dependent variables, coefficients on dummy variables need to be transformed as $\exp(\text{coefficient}) - 1$ to yield percentage effects.

[28] The evidence is merely suggestive on this account: The drop in labor productivity may be partly accounted for by the large negative effect on firm assets after founder death.

The large drop in assets may to some extent be a voluntary reallocation of resources to other firms. The data do not allow us to determine whether the reduction in assets is 'forced', i.e., due to financial distress, or whether it is due to 'voluntary' reallocation of resources to more productive usage outside the firm. One way to analyze this question is to through bankruptcy rates: the bankruptcy code in Norway is similar to Chapter 7 in the U.S. bankruptcy code, i.e., bankruptcy is associated with creditors taking control and is not 'voluntary' as in Chapter 11 in the U.S bankruptcy code. In unreported regressions we find that 20 percent of the treated firms and 10 percent of the matched control firms go bankrupt before 2010 (the difference is significant at the 1 percent level).

For comparison, we also perform the analysis using OLS on all 16,127 firms in the database.[29] The estimated coefficients, reported in Table A.1, are of similar magnitude to that in the matching analysis.

The main reason for the negative effects on firm performance documented in Panel A could be turbulence created by entrepreneur death. If turbulence drives the results, we would expect entrepreneur death to have a large short-run effect on firm performance, and a partial or full reversal over time (for example, finding a substitute for the entrepreneur could be easier in the longer than in the shorter run). On the other hand, if the entrepreneur is a critical resource for the firm, in the sense outlined in Section II, we would expect the negative performance effects to be long-lasting. To examine this question, in Panel B of Table 3 we estimate separately the effect 1-2 years after founder death, and 3-5 years after founder death. The sample size is larger than in Panel A because we also include the year of founder death. The fourth and fifth rows of Panel B show that compared to the control group, the performance for the treatment group of firms, if anything, deteriorates over time; for firm survival, the effect is about 20 percentage points 1-2 years after founder death, and 19 percentage points 3-5 years after founder death. Also for sales, assets, and profitability, the point estimates suggest a further drop over time. What is clear is that entrepreneur death leads to large and sustained negative effects on firm performance and that there is no bounce-back.

The following figure plots the estimated difference between treated firms and control firms across all years of event time, summarizing the regression results.

[29] OLS estimates are based on the following regression: $Performance_{it} = \alpha + \beta_1 * treated_i + \beta_2 * after_{it} * treated_i + \gamma * X_{it} + \delta_t + \epsilon$, where $after_{it}$ is equal to 1 in the years after founder death in firms where the founder dies. Note that $after_{it}$ is set equal to zero in all periods for firms where the founder does not die.

INSERT FIGURE 1 HERE

The graphs illustrate that over time, the difference between the control and treated group is accentuated. We interpret this as evidence supporting the notion that the entrepreneur is a critical asset that is not easily substituted even in the longer run.

V.B. Are there pre-treatment differences?

We showed in Section IV that there are no differences between control and treated firms in the year of incorporation We want to highlight that the results reported in Table 3 also address the important issue whether indeed the post-treatment effect is causal in the sense that they are the result of an exogenously timed death. Similar to Jones and Olken (2005), we look at whether there are pre-treatment differences between treated and control firms. We have done so in the context of the regression estimates presented in Table 3 and in the context of the graphs presented in Figure 1. In all cases, there is no evidence of any pre-treatment differences between treated and control firms. The timing of founder death therefore seems to come as a surprise and we interpret differences after founder death as the result of (largely unexpected) founder death. We discussed above that the finding of no pre-treatment effects is consistent with the alternative interpretation that even if the founder was ill before his death, on average that illness does not seem to have affected firm performance.[30]

It is possible that unobserved factors in the years leading up to founder death affect both the founder's death probability and firm performance after founder death (for example, an ailing marriage). We should emphasize, therefore, that the performance regressions include only pre-determined controls, i.e., firm characteristics from the year of foundation. Our results should therefore be interpreted as the effects of founder death conditional on initial firm characteristics, not on any intermediate characteristics that might have been affected by illness. Our results below show that actual firm performance is not affected by such potential unobserved differences prior to death, because treated and control firms do not differ in their performance in any of the years before founder death. It is still of interest to consider the effects conditioning on control variables measured closer to founder death. An alternative estimation strategy is to match on

[30]Under the second interpretation, one can speculate that it is not essential that the founders spend a large number of hours on the firm (which illnesses such as cancer, and their treatment, would preclude) but rather a small, but sufficient number of hours to take care of the main strategic issues.

firm and founder characteristics in the year before founder death and to restrict attention to firms that are still active. This strategy has the added benefit of reducing measurement error since we capture firm characteristics closer to founder death. The results are reported in Appendix Table A.3, Panel A. As expected, the results are considerably larger in absolute value compared to the main analysis.

The coefficient β_1 estimated in the first row of Table 3, Panel A, shows that there are no overall pre-treatment effects. The interaction terms with pre-treatment dummies {-5,-4,-3} and {-2,-1}, reported in Panel B of Table 3, give further evidence that there are no pre-treatment effects, i.e., that founder death has no effect on firm performance in the years preceding death. This is an important 'placebo' test supporting our identification strategy.

V.C. Quantile effects

Firms that experience entrepreneur death have about 20 percentage points lower survival rates in the first years after the death event. It is possible that entrepreneur death speeds up evolution by weeding out firms that likely would not flourish even if the entrepreneur stayed alive. For example, the heir or creditors could voluntarily close down unprofitable firms. In that case entrepreneur death could simply mean a lower threshold for closing down firms rather than changing the underlying outcome distribution.

To address this important issue, in Table 4 we look at quantile regressions for the same type of specification as in Table 3, but where we compare the performance of treated and control firms above median of the conditional performance distribution. Below the median, there are smaller differences between treated and control firms, which is largely explained by the fact that both treated and control firms at the lower quantiles of the distribution are going out of business.

INSERT TABLE 4 HERE

There are negative effects of founder death on log(assets) and log(sales) at all deciles between the median the 90th percentile. At all quantiles, the effects are stronger 3, 4 and 5 years after founder death (not reported). At the 95th percentile (not reported), differences between treated and control firms seem to disappear. This result has to be taken with caution because Chernozhukov and Fernandez-Val (2011) suggest that, for data sets of a sample size like ours, a normal distribution approximation at the 95th percentile might not be appropriate. We conclude

that entrepreneur death appears to have a negative effect on growth and profitability across the firm performance distribution.

To deal with the issue that there are no treatment effects in the lower quartile due to attrition of both treated and control firms, an alternative estimation strategy is to match on firm and founder characteristics in the year before founder death and to restrict attention to firms that are still active. The results of this regression are reported in Appendix Table A.3, Panel A. As expected, the results are larger in absolute value compared to the main analysis.

V.D. Firm age

Does the importance of the entrepreneur diminish as the firm matures? We analyze whether the drop in firm performance depends on firm age when the entrepreneur dies. We depict these results graphically by showing the treatment effects as a function of firm age in the year when the founder dies. The plots are based on a second-order polynomial in firm age, interacted with the treatment effect. We depict the predicted effects up to firm age 8 years (recall that there are few firms at firm age larger than 8 years in our sample).

INSERT FIGURE 2 HERE

Figure 2 shows very strong negative effects of founder death on very young firms, and smaller negative effects on survival, sales, and assets for more mature firms. These results are particularly interesting against the background that the majority of firms reach a more mature phase with moderate growth after about five or six years of existence. Thus the founder also has a large effect when the firms have reached this more mature stage and beyond. There is a strong implication for the financing of young firms: it will be difficult even for quite mature firms to pledge the value of the firms to outside investors. This suggests that financing constraints and underinvestment of the type described by Hart and Moore (1994) may be present for a long time in a firm's life.

V.E. Firm size

One concern is that many firms in our sample could be vehicles for cutting the tax bill for essentially self-employed individuals, or firms started up as a 'consumption good' for the en-

trepreneur. In both these cases, it would be no surprise to see the firm to vanish with the founder. We therefore investigate whether the effect of entrepreneur death depends on startup size, the idea being that small startups are more likely to have founders with these types of motivations. We interact the treatment effect with a dummy for firms above/below the median in terms of initial assets. Initial assets are likely to be exogenous to the death of the founder and moreover predict firm size at the time of founder death.

In Table 5, we find large effects for startups both below and above the median, and no difference between them.

V.F. Other interactions

In Table 5, we also report the results from analyzing the differential effects of a number of different types of firms and founders. We are particularly interested in whether the entrepreneur is more important in sectors where human capital is more important relative to physical capital. Our results give support to this conjecture. First, we find stronger results in sectors where founder education is above that in the sector with median education. We also find stronger results in sectors with higher R&D expenses, and in sectors where sales growth is above median. These results suggest that founders are especially important in human capital intensive firms and sectors. Among several other dimensions we did not find differences in treatment effects. First, based on the endogenous growth literature (e.g., Glaeser et al., 1992), we ask whether the causal effect of individual entrepreneurs is lesser in urban areas, where the supply of entrepreneurs is denser. In unreported analysis, we find no difference in causal effect of entrepreneurs in rural and urban areas. This might indicate that, even if there is a larger supply of (potential) entrepreneurs in a city, there could be mitigating demand-side effects, such as the alternative entrepreneurs' opportunity cost of time being higher. We also split firms up depending on whether the entrepreneur was the sole owner at the incorporation date or not. The differences are minor. We also looked at whether founder death matters less for old founders (60 years or more in the startup year) because they might be less dynamic than younger founders and therefore potentially more easily replaceable. However, we find no differences in treatment effects by age (not reported). We also looked at the gender dimension, but find no heterogeneity of the treatment effect by gender (not reported).

VI. Mechanisms

The results of Section V are consistent with a simple mechanism: entrepreneurs personally embed a major part of the value of firms, and less entrepreneurial engagement harms firm performance. In this section we discuss this mechanism in further detail. We also discuss the role of two alternative mechanisms; within-family transfer of control and fragility of young firms to shocks. In the next section we discuss in more detail whether detrimental effects of abrupt leadership transition could explain our results.

VI.A. Entrepreneurial engagement

To explore the role of entrepreneurial engagement further, we analyze whether the effects of death depends on whether the entrepreneur works for the firm or not prior to death. We interact the treatment effect with a dummy for whether the entrepreneur had the firm as his main employer one year prior to death. The results are reported in Table 5 and show that the negative effects of founder death are predominantly driven by entrepreneurs that are employed by their firm prior to death. This result corroborates the idea that entrepreneur engagement is a critical factor to young firms. Furthermore, it suggests that entrepreneurs, in order to be important, need to actively engage in the day-to-day operations of the firm. In Panel B of Appendix Table A.3, we corroborate the finding that founder engagement by working in the firm is important, when matching on firm and founder characteristics in the year before founder death. In Section VII we investigate entrepreneur engagement further, by utilizing data for 2002 to 2007 on the identity of chief executive officers.

VI.B. The role of the family

Part of the explanation for the strong effects of founder death could be that post-death, the control of the firm is transfered to less competent family members.[31] Perez-Gonzales (2006), Bennedsen et al. (2007), and Bertrand et al. (2008) document negative effects on performance from family CEO appointments inside mature firms.

[31] Alternatively, family members might be subject to inheritance tax, which in turn might force them to sell off (parts of) the firm. As we discussed in section III.A., inheritance tax issues only play a minor role.

As a first step, we investigate ownership changes in the aftermath of entrepreneur death.[32] We classify ownership into two categories, the entrepreneur and family members, and outsiders. Outsiders could be either individuals that are non-family member, or other firms. In Figure 3 we graph how ownership evolves for these two categories in event time. We see that even four years after founder death, the family still owns half the company on average. So there is no doubt that the family plays an important role for the surviving firms.

Our next step is to regress post-death performance on a dummy of whether family keeps a majority-ownership or not, including death year variables as control. In unreported results, we find firms where the family keeps at least 50% in the first year after founder death are more likely to exit in the subsequent years. This could be due to two reasons (that our data do not allow us to differentiate); either a genuine negative causal effect of family ownership, or because better-performing firms are easier for the family to sell off. However, conditional on the firm staying active, there are no significant differences in firm performance between firms where the family does or does not sell off.

The second way to approach whether the family hurts the firm is to introduce interaction terms between the treatment dummies and a family firm dummy. Likewise, we interact the before/after dummies and the difference-in-differences parameter β_2 with a family firm dummy. This method is more indirect but gives us causal estimates. It informs us whether treatment affects family firms more than others. We define a family firm as a firm where at least one of the founding minority owners is a child, parent, sibling or spouse of the entrepreneur.

INSERT TABLE 5 HERE

In Table 5, we find no difference in results for family and non-family firms. As an alternative way to test whether the strong effects of founder death could be due to transfer of control to less competent family members, we split firms into two groups, those where the founder has children aged 16 or older and those where not. In unreported regressions, we do not find any difference in treatment effects for founders with and without children.[33] Thus here we do not find evidence

[32]Note that, while data on ownership is complete in the year of firm foundation (which is the basis for our definition of majority, 50% and minority ownership), ownership data has some missing values in later years. While this is unfortunate, we do not have reason to believe that it biases the findings below in a systematic way.

[33]To examine this question in more detail, we modified the matching function to match exactly on the number of children of the founder. Neither this approach gave differences in treatment effect. An alternative way to analyze the role played by within-family transitions is to link post-death performance to whether children of the founder are employed by the firm. This empirical strategy is problematic because the employment decision is endogenous

that the large effects of founder death is due to incompetence of family members.

VI.C. Fragility of young firms

Initial owners of start-ups are often family members, friends, former co-workers. Our results, therefore, may not show a special role of the entrepreneur but rather the fragility of young firms to circumstances, such as turbulence and emotional distress, created by death in a close-knit group. If so, one would expect the death of other individuals inside the circle to have a similar negative effect. We therefore analyze the impact of minority owner death (an ownership share of at least 10 percent and less than 50 percent, in all 343 death events) on firm performance using the same type of matching technology as in the main analysis. The results, reported in the first panel of Table 6, show that there are small or zero negative effects.

INSERT TABLE 6 HERE

We then analyze the effects of the death of key workers, defined as individuals that are both employed by the firm and hold an initial minority ownership share. For such key workers the effect of death, reported in the second panel of Table 6, is statistically insignificant. These results reinforce the idea that engagement in daily operations by the entrepreneur is critical; daily engagement even by presumably key workers is of much less importance.[34]

To summarize, the results of Section V and VI are consistent with the mechanism outlined in Section II: entrepreneurs are a core asset for young firms, and less entrepreneurial engagement harms firm performance. Other mechanisms, such as within-family transfer or the fragility of young firms to any shocks do not appear to explain the results.

VII. Leadership transition

Entrepreneurs typically have two roles: they found the company and provide its initial 'blueprint' and vision, but typically also manage the firm. It is possible that the results of Section V and Section VI are not due to entrepreneur specialness, but detrimental effects of abrupt leadership

to the performance of the firm.

[34]For completeness, the third and the fourth panel of Table 6 reports the results of the same type of analysis for individuals that own exactly 50 percent of the firm initially (129 death events).

transition and the costs of adapting to a new leader. In order to deal with this question, we employ data for that provides the end-of-year identity of the firm's chief executive officer (CEO) between 2002 and 2007, and analyze the effects of CEO death. We split the CEOs into two groups: founders and non-founders, and compare the causal effect of CEOs that are founders with the causal effect of CEOs that are not founders.

The idea behind this analysis is simple: if entrepreneur specialness is driving the results, we would expect the death of CEOs that are entrepreneurs to have a stronger effect than the death of CEOs that are not entrepreneurs. Conversely, if non-entrepreneur CEO death yields equally strong results, we would conclude that our results are predominantly due to leadership transition.

In this part of the analysis, we keep the firms where we know the identity of the CEO in at least one year between 2002 and 2007. As in the main analysis, CEOs that die are older, and it is not natural to compare firms with CEO deaths to *all* firms with no CEO death in our database, but limit the analysis to those firms (and CEOs) in the control group who are most comparable in terms of observable characteristics. Again we use propensity score matching to select the firms in the control group who are most similar to the firms in the treatment group.

We use nearest neighbor matching to select those firms in the control group whose *ex ante* probability of experiencing CEO death is closest to that of the firms where the CEO dies, and our further analysis proceeds on this matched sample. As in the main analysis, we also present OLS results, using all the firms in the database (with the restriction that we know the CEO identity in at least one year).

As CEO identity can change over time, we perform matching at the firm-year level, where we find comparable firms in the year before CEO death. For firm characteristics, we match on year zero characteristics, in order to avoid endogeneity. We require a matched control to be started up in the same year, and have the same firm age as the treated firm (this implies that treated and matched control will be measured in the same calendar year). We also require exact matching on CEO type, i.e. we match founder-CEOs to founder-CEOs and non-founder CEOs to non-founder CEOs. In the regressions, we include year, firm age, and year by sector fixed effects. Table A.4 presents descriptive statistics of the matched sample.

Table 7 presents the results.[35] The first three columns are based on the matched sample.

[35]Here we focus on firm survival, but Table A.5 shows results for other firm performance measures.

Overall, for all three groups of CEOs, death negatively impacts on firm survival. We see that the entrepreneur-CEO category yields economically large effects, statistically significant at the 1% level. The difference to non-entrepreneur CEOs is economically large and significant at the 7% level. In column, we perform the same analysis without matching, using all the firms in our sample, and using firm fixed effects, as in column 3. Now the entrepreneur-CEO effect is still large, but the non-entrepreneur CEO effect is small and statistically insignificant.

INSERT TABLE 7 HERE

In Panel B we investigate the effect for "smaller" shocks, the death of a person in the nuclear family (spouse or child). We obtain a matched sample in the same way as before and descriptive statistics are shown in Panel B of Table A.4. We find overall smaller effects on firm performance, as expected. Interestingly, the effect is significant only for CEOs that are entrepreneurs. Thus the finding that CEOs that are entrepreneurs are considerably more important than non-entrepreneur CEOs seems to be a recurring pattern.

To conclude, we find that the effects of death of CEO-founders are economically and statistically much stronger than for CEOs that are not founders. We conclude that the results appear driven by the specialness of the entrepreneur and not leadership transition of young firms.

VIII. Conclusion

In the large literature on firm performance, economists have given little attention to the founders of firms. While the idea of entrepreneurs being important is old, other factors have been the focus of most empirical work. This paper uses several hundred exogenously timed deaths as a natural experiment to identify the causal effect of entrepreneurs on firm performance. We find that entrepreneurs strongly affect performance of both very young firms and more mature firms, and more strongly in 'dynamic' industries with higher education level, larger R&D expenses and higher sales growth. The effects appear to be driven by entrepreneur specialness rather than leadership transition; the effects of death of entrepreneur-managers are economically and statistically much stronger than the death of managers that are not entrepreneurs.

These results point to entrepreneurs playing a large and unique role not previously documented. Much of the existing evidence in favor of the importance of entrepreneurs is based

on comparing environments with high versus low entrepreneurship rates (e.g., Acs et al., 2009, Glaeser et al., 2014). However, these findings are open to several interpretations. A key contribution of our analysis is to directly measure the impact of entrepreneurs, and to show that it is large even compared to managers.

We highlight one area of possible future research. Our empirical results are much in line with the model of entrepreneurship proposed by Hart & Moore (1994). One of the implications of this model is that founder non-substitutability leads to credit constraints and suboptimal investment levels. Our empirical results suggests a metric to predict what type of founders are less substitutable, and therefore less likely to be funded in the first place. One possible extension of the present work is to use this metric to predict liquidity constraints, and possibly suggest public policies to alleviate them.

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Table 1
Descriptive statistics in year of foundation

	Firms where founder dies (205 obs)				Firms where founder does not die (15,922 obs)			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Founder death	1.00	.00	1.00	1.00	.00	.00	.00	.00
Age	50.29	9.53	25.00	67.00	42.20	9.96	18.00	67.00
Female	.10	.30	.00	1.00	.18	.38	.00	1.00
Single	.15	.35	.00	1.00	.27	.44	.00	1.00
Years of education	11.68	2.82	5.00	20.00	12.25	2.57	5.00	21.00
Log wealth in year before firm foundation	13.58	1.36	9.21	16.38	13.11	1.34	9.21	20.47
Log earnings in year before firm foundation	12.71	.78	9.21	14.15	12.70	.77	9.21	17.78
Self-empl. experience over previous 10 years	.35	.41	.00	1.00	.25	.36	.00	1.00
Number of employees	3.81	4.65	1.00	33.00	3.62	5.50	1.00	139.00
Dummy: family firm	.15	.35	.00	1.00	.09	.28	.00	1.00
Dummy: Urban area	.41	.49	.00	1.00	.39	.49	.00	1.00
Log equity at firm foundation	11.72	.58	10.89	14.26	11.73	.57	10.89	18.49
Ownership share at firm foundation	.82	.19	.51	1.00	.88	.18	.50	1.00
Dummy: sole owner at firm foundation	.47	.50	.00	1.00	.64	.48	.00	1.00
Year of firm foundation	2001.79	1.94	1999.00	2007.00	2002.95	2.24	1999.00	2007.00
Agriculture and Fishery	.05	.22	.00	1.00	.02	.15	.00	1.00
Mining	.00	.00	.00	.00	.003	.06	.00	1.00
Manufacturing	.07	.26	.00	1.00	.06	.23	.00	1.00
Utilities	.00	.00	.00	.00	.0009	.03	.00	1.00
Construction	.13	.34	.00	1.00	.16	.36	.00	1.00
Commerce	.27	.44	.00	1.00	.29	.45	.00	1.00
Business Services	.24	.43	.00	1.00	.24	.43	.00	1.00
Other Services	.13	.33	.00	1.00	.17	.37	.00	1.00
Transport, storage and communication	.09	.28	.00	1.00	.05	.22	.00	1.00

Note: The table depicts summary statistics of majority founders and the firms they start up, broken down by whether the founder dies (d=1) or not (d=0).

Table 2
Firms where the founder dies

Year of foundation			Year of death			Firm age at founder death		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Freq.	Percent		Freq.	Percent		Freq.	Percent
1999	17	8.29						
2000	51	24.88				1	12	5.85
2001	35	17.07	2001	5	2.44	2	19	9.27
2002	31	15.12	2002	4	1.95	3	22	10.73
2003	30	14.63	2003	13	6.34	4	38	18.54
2004	23	11.22	2004	16	7.80	5	18	8.78
2005	5	2.44	2005	13	6.34	6	28	13.66
2006	12	5.85	2006	23	11.22	7	25	12.20
2007	1	0.49	2007	24	11.71	8	10	4.88
			2008	26	12.68	9	20	9.76
			2009	43	20.98	10	8	3.90
			2010	38	18.54	11	5	2.44
Total	205	100	Total	205	100	Total	205	100

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: The table depicts summary statistics of founders and the firms they start up, for firms where the founder dies (d=1).

Table 3
Effect of founder death on firm performance

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall effect of founder death on firm performance					
Treated	-.055	-.304	-.298	-.022	-.027
	(.034)	(.214)	(.168)*	(.066)	(.018)
After * Treated	-.164	-.761	-.905	-.215	-.022
	(.043)***	(.249)***	(.210)***	(.076)***	(.019)
Obs.	3274	3274	3274	3271	3274
Adjusted R^2	.228	.250	.276	.329	.085
Panel B: Effect of founder death on firm performance over time					
Treated * (3,4,5) years before founder death	-.046	-.285	-.300	-.019	-.023
	(.037)	(.230)	(.177)*	(.067)	(.023)
Treated * (1,2) years before founder death	-.064	-.324	-.299	-.025	-.031
	(.039)	(.238)	(.190)	(.074)	(.020)
Treated * year of founder death	-.150	-.787	-.785	-.106	-.066
	(.045)***	(.267)***	(.228)***	(.083)	(.022)***
Treated * (1,2) years after founder death	-.226	-1.135	-1.201	-.270	-.039
	(.043)***	(.260)***	(.226)***	(.082)***	(.015)***
Treated * (3,4,5) years after founder death	-.212	-.997	-1.211	-.201	-.060
	(.051)***	(.311)***	(.282)***	(.098)**	(.015)***
Obs.	3684	3684	3684	3681	3684
Adjusted R^2	.217	.244	.271	.323	.081

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: In Panel A, observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Additional control variables in Panel A: "After", i.e. dummy=1 for time after (imputed) year of founder death; further control variables in Panel A and B are all measured in startup year. In Panel A and B, sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Table 4
Effect of founder death on firm performance: quantile regressions

	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)
50% percentile				
Treated	-.036	-.009	.029	-.031
	(.098)	(.118)	(.023)	(.006)***
After * Treated	-2.828	-2.716	-.377	-.004
	(.140)***	(.168)***	(.033)***	(.008)
Obs.	3274	3274	3271	3274
R^2	.226	.218	.273	.069
60% percentile				
Treated	.020	-.025	.036	-.043
	(.074)	(.088)	(.022)	(.008)***
After * Treated	-2.976	-2.422	-.544	-.013
	(.105)***	(.126)***	(.032)***	(.012)
Obs.	3274	3274	3271	3274
R^2	.191	.180	.309	.102
70% percentile				
Treated	.028	-.046	.046	-.048
	(.069)	(.091)	(.030)	(.015)***
After * Treated	-.897	-.964	-.366	-.026
	(.098)***	(.130)***	(.042)***	(.021)
Obs.	3274	3274	3271	3274
R^2	.173	.162	.322	.109
80% percentile				
Treated	-.009	-.096	.050	-.036
	(.054)	(.087)	(.041)	(.015)**
After * Treated	-.487	-.567	-.302	-.037
	(.077)***	(.124)***	(.057)***	(.021)*
Obs.	3274	3274	3271	3274
R^2	.189	.168	.323	.121
90% percentile				
Treated	.040	-.080	.021	-.023
	(.070)	(.102)	(.058)	(.017)
After * Treated	-.124	-.427	-.198	-.037
	(.099)	(.146)***	(.082)**	(.024)
Obs.	3274	3274	3271	3274
R^2	.211	.192	.318	.139

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Additional control variables: "After", i.e. dummy=1 for time after (imputed) year of founder death; further control variables are all measured in startup year. Sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Table 5
Heterogeneity of the effect of founder death on firm performance

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Owner works at firm in $(t-1)$					
After * Treated	-.0006	.196	-.093	.059	.013
	(.055)	(.314)	(.280)	(.103)	(.025)
After * Treated * (Owner works at firm in $(t-1)$)	-.342	-2.020	-1.685	-.559	-.076
	(.083)***	(.484)***	(.416)***	(.150)***	(.039)**
Obs.	3274	3274	3274	3271	3274
Adjusted R^2	.308	.329	.333	.412	.099
Firm assets in year of foundation above/below median					
After * Treated	-.188	-.916	-.923	-.185	-.029
	(.048)***	(.275)***	(.236)***	(.086)**	(.021)
After * Treated * (Firm equity above median)	.123	.778	.106	-.141	.038
	(.104)	(.643)	(.508)	(.198)	(.050)
Obs.	3274	3274	3274	3271	3274
Adjusted R^2	.234	.252	.282	.331	.089
Firm with/without family members as co-owners					
After * Treated	-.193	-.929	-1.012	-.276	-.018
	(.045)***	(.268)***	(.226)***	(.081)***	(.021)
After * Treated * (Family firm)	.193	1.130	.734	.404	-.028
	(.130)	(.750)	(.626)	(.236)*	(.051)
Obs.	3274	3274	3274	3271	3274
Adjusted R^2	.229	.253	.281	.333	.087
Firm in sector with above/below median founder education					
After * Treated	-.106	-.430	-.651	-.130	-.024
	(.048)**	(.295)	(.245)***	(.091)	(.021)
After * Treated * (Sector with founder education above median)	-.237	-1.351	-1.015	-.354	.011
	(.098)**	(.524)***	(.464)**	(.156)**	(.051)
Obs.	3274	3274	3274	3271	3274
Adjusted R^2	.232	.252	.280	.330	.088

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Additional control variables: "After", i.e. dummy=1 for time after (imputed) year of founder death; further control variables are all measured in startup year. Sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Table 5
Heterogeneity of the effect of founder death on firm performance (cont'd)

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Firm in sector with above/below median sales growth					
After * Treated	-.068	-.185	-.356	-.178	-.016
	(.067)	(.417)	(.354)	(.118)	(.029)
After * Treated * (Sector sales growth above median)	-.205	-1.242	-1.061	-.098	-.009
	(.092)**	(.547)**	(.466)**	(.166)	(.040)
Obs.	2956	2956	2956	2953	2956
Adjusted R^2	.228	.254	.293	.349	.100
Firm in sector with above/below median R&D					
After * Treated	-.124	-.483	-.653	-.186	-.017
	(.057)**	(.347)	(.288)**	(.104)*	(.024)
After * Treated * (Sector R&D above median)	-.146	-.998	-.744	-.141	-.017
	(.099)	(.561)*	(.484)	(.165)	(.045)
Obs.	2956	2956	2956	2953	2956
Adjusted R^2	.229	.254	.288	.345	.091
Firm in sector with above/below median wages					
After * Treated	-.111	-.426	-.536	-.100	-.010
	(.058)*	(.347)	(.298)*	(.106)	(.024)
After * Treated * (Sector wages above median)	-.134	-.857	-.849	-.288	-.028
	(.094)	(.539)	(.454)*	(.157)*	(.043)
Obs.	2996	2996	2996	2993	2996
Adjusted R^2	.228	.256	.286	.346	.091
Firm in sector with above/below median sales volatility					
After * Treated	-.148	-.674	-.857	-.194	-.022
	(.050)***	(.293)**	(.252)***	(.086)**	(.022)
After * Treated * (Sector sales volatility above median)	-.289	-1.634	-.613	-.285	-.007
	(.140)**	(.833)*	(.601)	(.270)	(.063)
Obs.	2956	2956	2956	2953	2956
Adjusted R^2	.228	.256	.287	.346	.091

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Additional control variables: "After", i.e. dummy=1 for time after (imputed) year of founder death; further control variables are all measured in startup year. Sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Table 6
Effect of founder death on firm performance for minority owners and 50% owners

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Effect of founder death for minority owners					
Treated	-.001	.086	-.006	.155	-.011
	(.026)	(.179)	(.157)	(.057)***	(.017)
After * Treated	-.006	-.102	-.072	-.160	.010
	(.033)	(.203)	(.177)	(.066)**	(.016)
Obs.	5564	5564	5563	5564	5563
Adjusted R^2	.128	.130	.143	.264	.034
Effect of founder death for minority owners, interacted with dummy for whether owner works at firm in $(t-1)$					
After * Treated	-.002	-.072	-.090	-.140	.002
	(.037)	(.227)	(.202)	(.072)*	(.019)
After * Treated * (Owner works at firm in $(t-1)$)	-.015	-.106	.103	-.071	.040
	(.074)	(.476)	(.406)	(.163)	(.037)
Obs.	5564	5564	5563	5564	5563
Adjusted R^2	.157	.168	.179	.306	.045
Effect of founder death for 50-percent owners					
Treated	-.066	-.500	-.444	-.073	-.020
	(.046)	(.282)*	(.233)*	(.078)	(.023)
After * Treated	-.128	-.584	-.269	-.240	-.0006
	(.049)***	(.298)**	(.251)	(.091)***	(.025)
Obs.	2066	2066	2066	2066	2066
Adjusted R^2	.251	.291	.283	.360	.065
Effect of founder death for 50-percent owners, interacted with dummy for whether owner works at firm in $(t-1)$					
After * Treated	-.020	.050	.189	-.034	-.002
	(.060)	(.376)	(.320)	(.117)	(.031)
After * Treated * (Owner works at firm in $(t-1)$)	-.301	-1.774	-1.299	-.569	.005
	(.097)***	(.583)***	(.489)***	(.174)***	(.051)
Obs.	2066	2066	2066	2066	2066
Adjusted R^2	.335	.366	.371	.418	.08

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Additional control variables: "After", i.e. dummy=1 for time after (imputed) year of founder death; further control variables are all measured in startup year. Sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

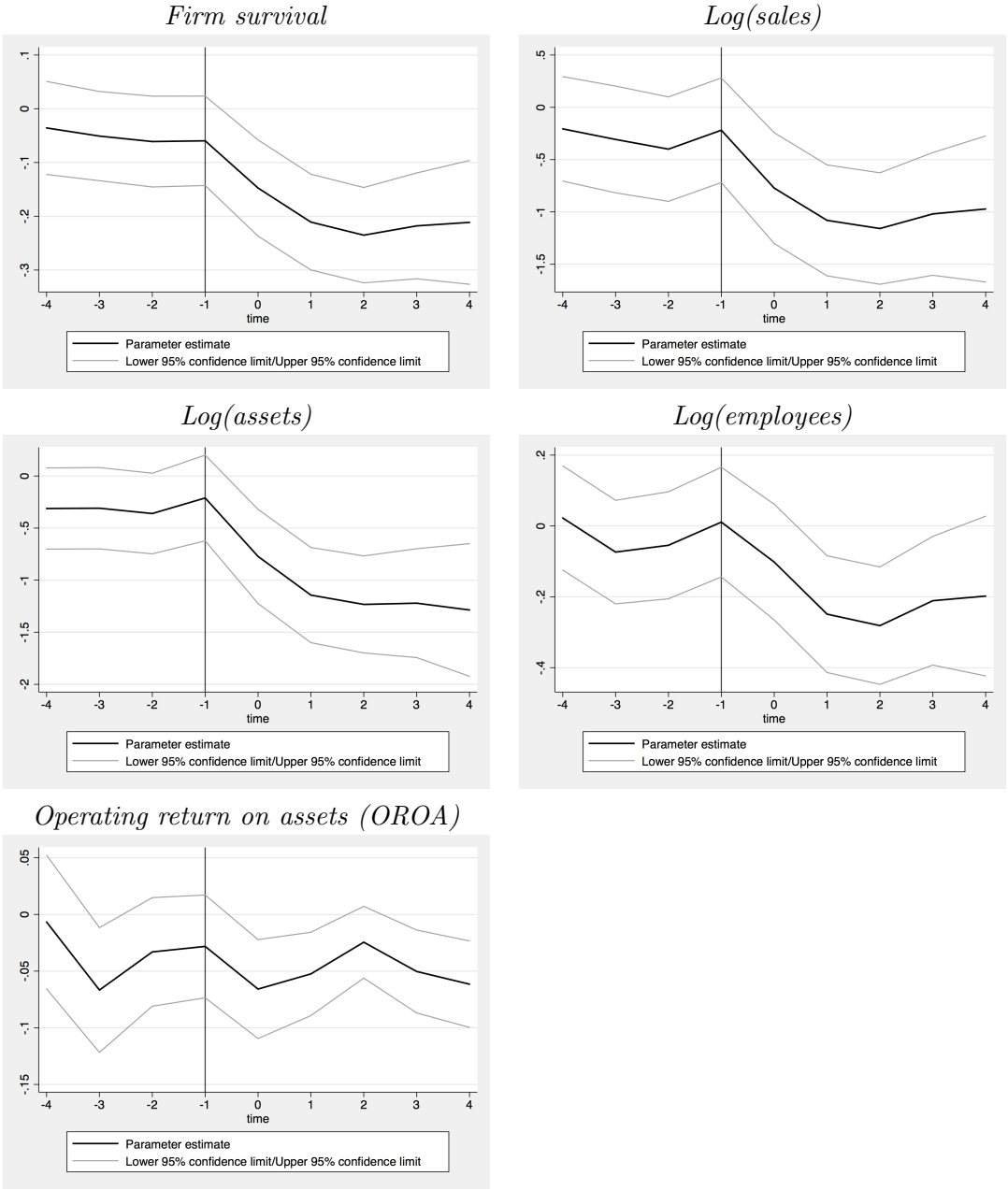
Table 7
Effect of CEO death and death of CEO family members on firm survival

	Matched sample			Unmatched sample
	No controls	With controls	Fixed effects	Fixed effects
	(1)	(2)	(3)	(4)
Panel A: Death of CEO				
After * Professional CEO	-.130	-.118	-.154	-.069
	(.077)*	(.077)	(.083)*	(.064)
After * Majority-founder CEO	-.323	-.317	-.302	-.246
	(.078)***	(.079)***	(.083)***	(.063)***
After * 50%-founder CEO	-.194	-.142	-.155	-.258
	(.153)	(.163)	(.179)	(.111)**
Obs.	2112	2112	2112	308672
Adjusted R2	.236	.262	.559	.584
Panel B: Death of close family member of CEO				
After * Professional CEO	.037	.015	.029	.044
	(.051)	(.050)	(.053)	(.031)
After * Majority-founder CEO	-.094	-.076	-.092	-.054
	(.045)**	(.045)*	(.049)*	(.033)
After * 50%-founder CEO	.030	.051	.077	.061
	(.072)	(.073)	(.087)	(.046)
Obs.	5152	5152	5152	308869
Adjusted R2	.115	.134	.478	.584

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Panel A and Panel B both use matched samples of firms who are subject to CEO death (panel A) or death of a close family member of the CEO (panel B). In Panel A and B, sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Figure 1

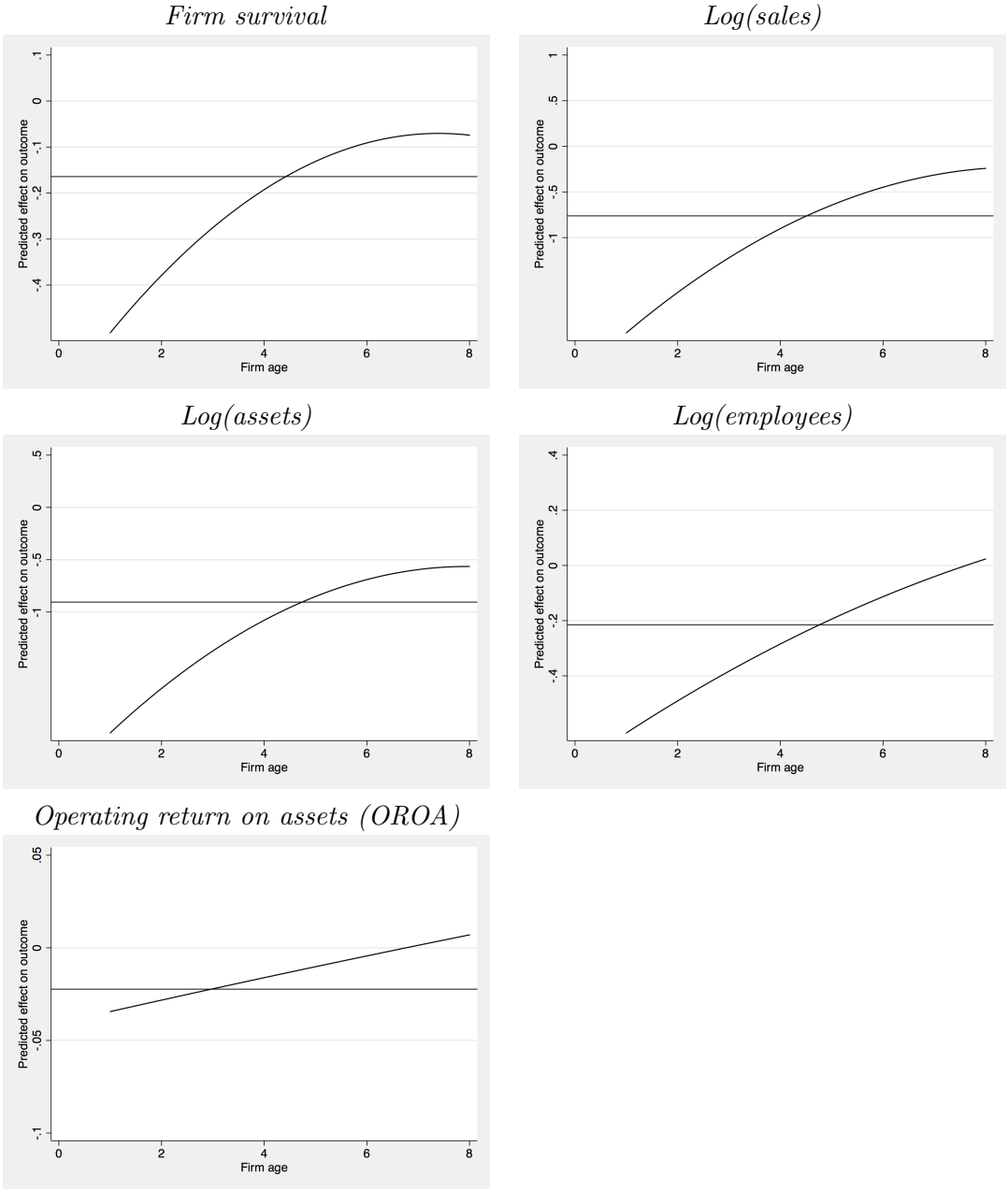
Treatment effects before and after founder death



Note: Year 0 refers to year of founder death for treated firms and imputed year of death for matched firms.

Figure 2

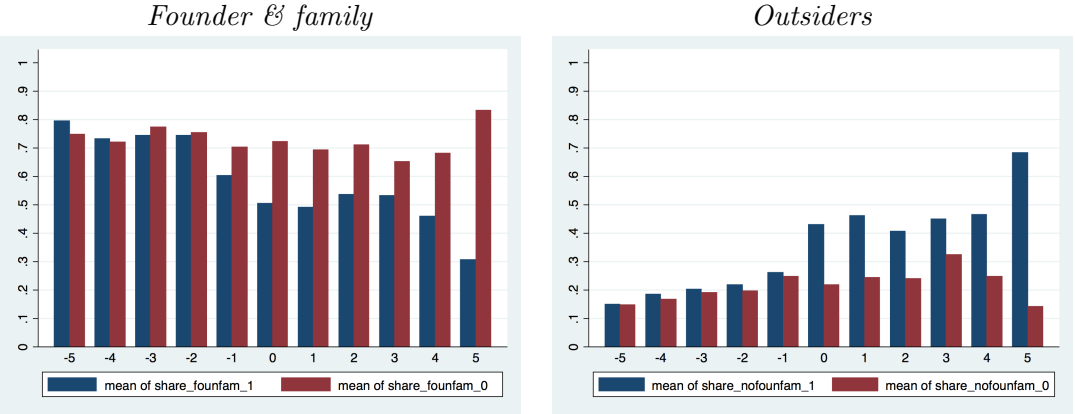
Treatment effect by firm age at founder death



Note: The figure plots the predicted outcome for treated firms in a regression where the treatment effect is interacted with firm age at founder death.

Figure 3

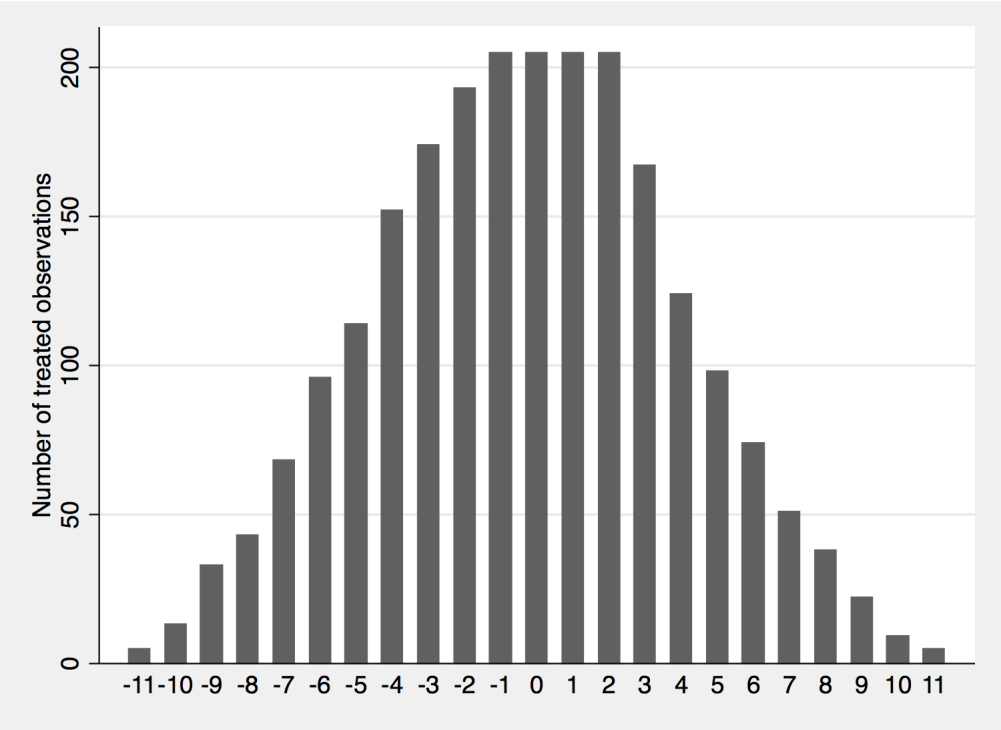
Ownership changes before and after founder death



Note: The figure plots the ownership shares of (a) founder and family and (b) outsiders, for treated and control groups.

Figure A.1

Number of treated observations before and after founder death



Note: Graph displays the number of treated observations before and after founder death.

Table A.1
Ordinary least squares regression estimates

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall effect of founder death on firm performance					
Founder death	-.048	-.205	-.172	.001	-.013
	(.027)*	(.167)	(.137)	(.047)	(.012)
After * Treated	-.172	-.846	-.973	-.259	-.038
	(.033)***	(.191)***	(.162)***	(.052)***	(.014)***
Obs.	161609	161543	161594	161499	161598
Number of firms	16127	16127	16127	16127	16127
Adjusted R^2	.159	.159	.168	.270	.058
Panel B: Effect of founder death on firm performance over time					
Treated * (3,4,5) years before founder death	-.036	-.173	-.143	-.005	-.003
	(.028)	(.177)	(.143)	(.049)	(.015)
Treated * (1,2) years before founder death	-.061	-.242	-.204	.008	-.023
	(.029)**	(.180)	(.148)	(.053)	(.014)*
Treated * year of founder death	-.144	-.710	-.735	-.127	-.046
	(.034)***	(.200)***	(.173)***	(.059)**	(.016)***
Treated * (1,2) years after founder death	-.214	-1.036	-1.024	-.239	-.043
	(.032)***	(.194)***	(.171)***	(.057)***	(.010)***
Treated * (3,4,5) years after founder death	-.227	-1.070	-1.275	-.277	-.058
	(.037)***	(.227)***	(.202)***	(.068)***	(.010)***
Obs.	161814	161748	161799	161704	161803
Number of firms	16127	16127	16127	16127	16127
Adjusted R^2	.159	.159	.168	.270	.058

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Estimates are based on the following regression: $Performance_{it} = \alpha + \beta_1 * treated_i + \beta_2 * after_{it} * treated_i + \gamma * X_{it} + \delta_t + \epsilon$, where $after_{it}$ is equal to 1 in the years after founder death in firms where the founder dies. Note that $after_{it}$ is set equal to zero in all periods for firms where the founder does not die. See main text for details.

Table A.2: Propensity score estimation

Age	.0007 (.0005)
Age squared	-7.12e-07 (5.40e-06)
Female	-.003 (.001)
Single	.002 (.002)
Years of education	-.0006 (.0002)
Dummy: Urban area	.001 (.001)
Log wealth in year before firm foundation	-.001 (.006)
Log wealth in year before firm foundation squared	.0002 (.0002)
Log earnings in year before firm foundation	.02 (.01)
Log earnings in year before firm foundation squared	-.0005 (.0005)
Interaction between log wealth and log earnings	-.0003 (.0005)
Self-empl. experience over previous 10 years	.001 (.002)
Number of employees	.0004 (.0003)
Number of employees squared	-.0000107 (9.90e-06)
Log equity at firm foundation	-.001 (.001)
Dummy: family firm	.0005 (.002)
Ownership share at firm foundation	.003 (.007)
Dummy: sole owner at firm foundation	-.005 (.003)

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Table A.2: continued from previous page

Agriculture and Fishery	.0004 (.005)
Manufacturing	-.003 (.003)
Construction	-.004 (.003)
Commerce	-.004 (.003)
Business Services	-.003 (.003)
Other Services	-.004 (.003)
Transport, storage and communication	-.0006 (.004)
Firm started in 2000	.0005 (.003)
Firm started in 2001	-.002 (.002)
Firm started in 2002	-.003 (.002)
Firm started in 2003	-.003 (.002)
Firm started in 2004	-.005 (.002)
Firm started in 2005	-.007 (.001)
Firm started in 2006	-.007 (.001)
Firm started in 2007	-.008 (.0009)
Obs.	16060
Pseudo-R^2	.11

Source: See main text for details.
Note: Standard errors in parentheses.

Table A.3
Effect of founder death on firm performance: matching on characteristics in (t-1)

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall effect of founder death on firm performance: matching on $(t-1)$ characteristics					
Treated	.006	.171	-.068	.070	.013
	(.018)	(.158)	(.131)	(.046)	(.025)
After * Treated	-.258	-1.435	-1.155	-.367	-.055
	(.060)***	(.355)***	(.295)***	(.105)***	(.031)*
Obs.	1701	1701	1701	1700	1701
Adjusted R^2	.309	.400	.410	.584	.089
Panel B: Heterogeneity of treatment effect: matching on $(t-1)$ characteristics					
Owner works at firm in $(t-1)$					
After * Treated	-.105	-.616	-.523	-.205	.010
	(.102)	(.592)	(.456)	(.203)	(.062)
After * Treated * (Owner works at firm in $(t-1)$)	-.234	-1.282	-.988	-.246	-.099
	(.125)*	(.731)*	(.578)*	(.237)	(.072)
Obs.	1701	1701	1701	1700	1701
Adjusted R^2	.32	.409	.419	.584	.096

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. Sample restricted to +/− 5 years around the (imputed) year of founder death in regressions. See main text for details.

Table A.4
Descriptive statistics for CEO analysis

	Mean	StdDev	Min	Max
	(1)	(2)	(3)	(4)
Panel A: Death of CEO				
CEO death	.50	.50	.00	1.00
Age	49.30	11.73	19.00	74.00
Years of education	11.78	3.56	.00	21.00
Female	.17	.38	.00	1.00
Married	.61	.49	.00	1.00
Log equity at firm foundation	11.83	.67	10.75	15.16
Employees at firm foundation	3.82	3.99	1.00	31.00
Year of firm foundation	2002.22	1.90	1999.00	2006.00
Firm age	2.95	2.10	.00	8.00
Professional CEO	.45	.50	.00	1.00
Majority-founder CEO	.44	.50	.00	1.00
50%-founder CEO	.12	.32	.00	1.00
Panel B: Death of close family member of CEO				
Death of family member of CEO	.50	.50	.00	1.00
Age	48.31	10.01	19.00	75.00
Years of education	11.57	3.54	.00	19.00
Female	.24	.43	.00	1.00
Married	.77	.42	.00	1.00
Log equity at firm foundation	11.76	.66	10.75	15.85
Employees at firm foundation	3.82	4.21	1.00	33.00
Year of firm foundation	2001.85	1.97	1999.00	2007.00
Firm age	2.66	2.10	.00	8.00
Professional CEO	.38	.49	.00	1.00
Majority-founder CEO	.46	.50	.00	1.00
50%-founder CEO	.16	.37	.00	1.00

Note: The table depicts summary statistics of CEOs and the firms they work in, in the year before the (imputed) death event of the CEO (panel A) or of a close family member of the CEO (panel B). Firm employment and log(equity) are measured in year of firm foundation.

Table A.5
Effect of CEO death on firm performance

	Firm active	log(sales+10)	log(assets+10)	log(employees+1)	Return on assets
	(1)	(2)	(3)	(4)	(5)
After * Professional CEO	-.154	-.698	-.729	-.249	-.043
	(.083)*	(.496)	(.391)*	(.163)	(.050)
After * Majority-founder CEO	-.302	-1.764	-1.263	-.360	-.082
	(.083)***	(.486)***	(.424)***	(.156)**	(.039)**
After * 50%-founder CEO	-.155	-1.274	-1.128	-.446	.042
	(.179)	(.899)	(.751)	(.204)**	(.092)
Obs.	2112	2110	2112	2111	2112
Adjusted R2	.559	.643	.673	.673	.200

Note: Observations from year of founder death excluded from analysis because not clearly attributable to 'before' or 'after' the event. "After", i.e. dummy=1 for time after (imputed) year of founder death. Additional control variables: dummy variables for three CEO types, dummy variables for three CEO types interacted "After", log equity and its square in year of firm foundation, number of employees and its square in year of firm foundation and dummies for calendar year, firm age, calendar year interacted with one-digit sector. Sample restricted to +/− 5 years around the (imputed) year of CEO death. See main text for details.