Do entrepreneurs matter?*

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Abstract

Entrepreneurs strongly affect firm performance. 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 for more mature firms, which suggests that liquidity constraints due to 'founder specificity' may last well into a firm's life. The negative effects of founder death are predominantly driven by entrepreneurs that work full time in their firms. Overall, our results suggest that entrepreneurs play a large role in affecting financial constraints and firm performance.

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, 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 have been the focus of empirical work. One objective of the paper is to ask how much individual entrepreneurs contribute to firm performance.

Little is known whether typical entrepreneurs have much of an effect. In addition to informing our understanding of firm performance, this question relates to an old debate stemming from Coase (1937) over what constitutes a firm and keeps it together. We study whether the entrepreneur provides the 'glue', and for how long. Governments try to boost competitiveness through a vast array of policies. Whether entrepreneurs matter also 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).

To examine the influence of entrepreneurs on firm performance, 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 employ a unique database that contains longitudinal accounting and employment information on the universe of incorporated firms established in Norway between 1999 and 2007. The data contain initial ownership shares; we define an entrepreneur as an individual with a substantial ownership share in the firm when it is established. The database contains about 65,000 firms where an individual owns at least 50 percent of the shares initially, and more than 500 firms where one of these individuals die before the end of 2009. For most of the analysis we focus on individuals with more than 50 percent

[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. (2011) on CEOs, and Holtz-Eakin et al. (1994) and Andersen and Nielsen (2012) on the effect of windfall gains through inheritance on entrepreneurial activity.

ownership (283 death events). We track firm performance at a yearly level until the end of 2011, so that firms in the database are between zero and twelve years old.

We ask whether entrepreneurs have a causal effect on firm performance. For each of the firms where the entrepreneur dies ('treated' firms) we use propensity score matching to identify a similar firm ('matched control' firm), which we restrict to be started up in the same calendar year. The matched controls 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.

The difference-in-differences regressions provide robust evidence that firm performance drops after the entrepreneur's death. For example, entrepreneur death, on average, leads to a 45 percent reduction in sales, compared to the control group, and a 15 percent reduction in employment. Ordinary least squares regressions, including all firms in the database, yields even stronger results. We expected the group of businesses that experienced the death of the entrepreneur to have some kind of a dip in performance immediately after the death owing to the upheaval, but we anticipated there would be a bounce back. However, even four years after the death, this group shows no sign of recovering and the negative effect on performance appears to continue even further beyond that. Our results are partly driven by firms closing down. But quantile regressions suggests effects of entrepreneur death also for firms that do not go out of business.

A simple explanation for our findings could be reverse causality: poor firm performance leads to entrepreneurs having a higher probability of dying. To deal with this possibility, we look at whether there are pre-treatment differences between treated and matched controls. We do not find evidence of pre-treatment effects, which suggests that reverse causality is not a major concern.[2]

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

[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.

company, a finding verified with more recent data by Pérez-González (2006). We analyze whether the magnitude of 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. This suggests that lack of external financing due to 'founder specificity' may last well into a firm's life.

The results are consistent with a simple mechanism: entrepreneurs personally embed a major part of the value of the firm, and lesser entrepreneurial engagement harms firm performance. In Section II we discuss theoretical underpinnings of this mechanism. To explore the role of entrepreneurial engagement further, we study the differential effects of entrepreneurs that are more or less engaged in the daily operations, through being employed by the firm or not. We find that the negative effects of founder death are largely 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. Imagine a small restaurant. After the major initial decisions have been made, such as location, menu, and hiring of employees, one could think that the entrepreneur would not play a large role anymore. Our results suggest that this is not true; the entrepreneur's presence, for example in grooming the customer base and developing the menu, is of key importance.

Our results may not be evidence to a special role of the entrepreneur but the fragility of young firms. We therefore analyze the effects of the death of key workers, defined as *employees* with a minority ownership in the firm. For this set of individuals the effect of death is negative and statistically significant but of far lesser magnitude than the death of the entrepreneur (about a third). The effect of the death of minority *owners* (in all 479 death events) are even smaller.

Firms that experience entrepreneur death have about 13 percentage point lower survival rates in the years after the death event.[3] Perhaps heirs voluntary close down firms that were largely motivated by providing private benefits for the dead entrepreneur, so that the social loss is small. To examine this question we, in addition to quantile regres-

[3]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.

sions, 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 very little to support the notion that voluntary close-downs by heirs is driving our results.

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) and Bennedsen et al. (2007) 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. We also analyze whether firms are affected differently if the entrepreneur has adult children. We do not find that family firms, or firms where the entrepreneur has adult children, are any more or less resilient to the loss of the entrepreneur. It does not appear, therefore, that inheritance mechanisms plays much of a role in driving our results.[4] We conclude that entrepreneurial engagement have a large and long-lasting effect on firm performance.

The paper connects to several strands of literature. First, economists have shown that large and persistent differences in productivity across firms exist even after taking into account geographical, industry and firm age differences (see surveys by Bartelsman & Doms, 2000 and Syverson, 2011). Not much is known about the importance of individuals.[5] We point out that a factor missing in this literature – individual entrepreneurs – can explain some of the heterogeneity for young firms.[6] Kaplan, Sensoy & Stromberg (2009)

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

[5]The following quote from Syverson (2011) illustrates the state of knowledge about the importance of individual managers. Much of the same could be said about individual entrepreneurs: "Researchers have long proposed that managers drive productivity differences. Whether rooted in the talents of the managers themselves or the quality of their practices, this is an appealing argument. Managers are conductors of an input orchestra. They coordinate the application of labor, capital, and intermediate inputs. Just as a poor conductor can lead to a cacophony rather than a symphony, one might expect poor management to lead to discordant production operations. Still, perhaps no potential driver of productivity differences has seen a higher ratio of speculation to actual empirical study."

[6]Bertrand & Schoar (2003) documents differences in management styles between individuals, and find evidence consistent with CEOs of publicly listed companies affecting firm performance. Bloom & Van Reenen (2007) document that higher-quality management practices are correlated with several measures

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 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, Sensoy, and Stromberg would suggest.

Second, most previous work 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). Not much is known about the impact of entrepreneurs. A recent paper by Glaeser et al. (2012) use mines as an instrument for entrepreneurship and find a persistent link between entrepreneurship and city employment growth. In contrast, we use random variation created by death and find evidence suggesting that entrepreneurs have a large and sustained impact on their firms.

Third, we complement the theoretical literature on financing 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 in turn leads to financing constraints and underinvestment in entrepreneurial firms (Hart and Moore, 1994). The extent to which entrepreneurs are non-substitutable is a largely unexplored question. We show that entrepreneurs personally do embed a major part of the value of the firm, and for surprisingly long. Our results suggest that lack of external financing due to 'founder specificity' may last well into a firm's life.

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

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. Bennedsen et al. (2011) investigates the effect of CEO death on the performance of privately held firms (the average firm age is 15 years), and find that the average operating returns on assets decreases by about 1 percentage point in a four-year window around CEO death. Hvide & Panos (2012) find evidence consistent with more risk tolerant individuals being more likely to become entrepreneurs but also start up firms of poorer quality.

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 & 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 entrepreneur.[7]

A theoretical tradition that justifies non-substitutability is critical resource theory (Wernerfelt, 1984, and Rajan & 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

[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.

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 the duration of non-substitutability may determine for how long new 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 it is of interest 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 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).

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

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

[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.

[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 (2013). 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.

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 accounting and employment measures for each firm until the end of 2011, so that the firms in the database are between zero and twelve years old. Covering the population of new firms means that the majority of firms in the database are small. The advantage of this approach is that it will not be subject to selection biases commonly encountered in the literature that uses "tip-of-the-iceberg" datasets (e.g., Hall and Woodward, 2010).[12] We can note that a substantial fraction of firms are not tiny, even in the first year: the 75th percentile for book value of assets and number of employees in the first year of operations is about $250,000 and three, respectively.

Comparing with recent work 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.

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 1999-2010. Note that the D&B data contain yearly

[11] For 1999, the data contain only a sample of the firms started. Diagnostic tests do not suggest any selection bias. To avoid counting wealth management vehicles as start-ups, we eliminate finance and real estate firms (NACE 65-70). We also eliminated firms where the founder died in 2010 because there is no post-death information for them. 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.

[12] Relative to datasets covering the self-employed, as in Hamilton (2000), the advantage is that we can measure firm performance at a much more detailed level.

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 2009 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 'Brønnøysundregisteret'.* 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.[13] To avoid counting wealth management vehicles as start-ups,

[13]In contrast to Hvide & Moen (2010), the current dataset contains the population of new firms. A large literature focuses on the self-employed (e.g., Hurst & 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.

we omit finance and real estate firms (NACE 65-70). The inclusion of these firms gives similar results.

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 24,023 firms, 283 experience founder death during our sampling period.[14] 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 2000 and 2010 inclusive.[15] 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.

[14] 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. On average, start-ups have 2 employees. The median number of employees is 1 and the 90th percentile is 5 and the 99th percentile is 19. As a rough estimate, we expect our analyses to cover a solid majority of the proper startups.

[15] Remember that we deliberately excluded observations where the founder dies in 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.

INSERT TABLE 2 HERE

IV. Empirical strategy

IV.A. Estimation sample

It is natural *not* to compare the 283 firms with founder death to *all* 23,740 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 283 firms where the founder dies.[16] Our further analysis then proceeds on this matched sample.[17]

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 283 founders and for 23,740 controls.[18] *Ex ante*, the treated make up less than 1 percent of our sample. Based on the estimated propensity score, we use nearest-neighbor matching (without replacement) to combine treated and control

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

[17] For comparison, we also perform the analysis using OLS on all 24,023 firms in our database. Those results are presented in Table A.1.

[18] 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.

observations.[19] 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 bad matches. Imposing this caliper, it turns out, we lose no treated founders whatsoever.[20] 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 0.12 and that the variables entering the propensity score 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 15.47. 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

[19] 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.

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

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. Similarly, the variables entering the propensity score are no longer jointly significant, with a p-value of 0.997. The median absolute standardized bias drops from 15.47 before matching to 3.19 after matching.[21] Matching thus appears to be very successful at 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

[21] 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.

founder death, we can again use the median absolute standardized bias and the pseudo-R^2 of a regression of the treatment dummy on firm performance measures as indicators of differences between treated and control firms.[22] We find the median absolute standardized bias to be 3.23 so very small. Similarly, the pseudo-R^2 from a regression of the treatment dummy on these performance measures is 0.016, an indication that treated firms and controls do not differ at all 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 find no significant differences in the year before founder death. All t-statistics are below 1. 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.[23] Based on this, we can define 'before' and 'after' founder death

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

[23] 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.

for both treated firms and matched controls. Our estimation sample consists of the 283 treated firms and 283 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.[24] However, β_1 is also of interest because it provides for a test of (a 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

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

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.[25] 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.[26] To assess profitability, we use two measures. The first is net profits. The second is 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 in a large accounting and financial economics literature (see Bennedsen et al. 2007 and references therein).[27] 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.[28]

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

[25] 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.

[26] One might be tempted to exclude firm-year observations after firm closure, but that would introduce a bias.

[27] 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.

[28] We impute profits to zero. In an alternative specification, we impute OROA 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 quantile regressions at the third quantile.

death (including all years slightly strengthens the results).[29] 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 60 percent, while the mean employment effects are about 17 percent.[30] The large effect on sales but lesser effect on employees suggest that entrepreneurs contribute to the productivity of the firm's employees through spillover effects.[31] The estimated effects on profitability are quite modest compared to growth, which is likely due to asset sales after founder death. The data do not allow us to determine whether the reduction in assets are 'forced', i.e., due to financial distress, or whether it is due to 'voluntary' reallocation of resources to more productive usage outside the firm. That financial distress plays some role is suggested by the bankruptcy rates: 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). 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.

For comparison, we also perform the analysis using OLS on all 24,023 firms in the database.[32] The estimated coefficients, reported in Table A.1, are larger than in the main matching analysis.[33] For reasons outlined earlier we tend to put more weight on the matching results of Panel A than those OLS results.

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

[30] Remember that with log dependent variables, coefficients on dummy variables need to be transformed as exp(coefficient) − 1 to yield percentage effects.

[31] 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.

[32] 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.

[33] A likely reason for the larger OLS estimates is that they are not based on a fully saturated model, i.e. they do not capture the heterogeneity between treated and controls to the same extent that matching does.

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 deteriorates over time; our point estimates suggest that the immediate effects of entrepreneur death are quite modest relative to the effects that accumulate over time. For example, the effect on survival is about 16 percentage points 1-2 years after founder death, and 23 percentage points 3-5 years after founder death. Thus entrepreneur death leads to large and sustained negative effects on firm performance.

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

INSERT FIGURE 1 HERE

The graphs illustrate that over time, the difference between the control and treated group is accentuated.

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.[34]

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.

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 13 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

[34]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.

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, 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 at various quantiles of the conditional performance distribution. The evidence reported in Table 4 suggests that, at the lower quartile, there are no differences between treated and control firms, except for negative treatment effect on assets. The results at the lower end of the distribution are 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, however, effects at the 50th and 75th percentiles. There are negative effects of founder death on log(assets) and log(sales) and – at the 75th percentile – on return on assets. At both 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 across the firm performance distribution; for firms around median quality the consequence of entrepreneur death will be a much higher probability of closing down, while for firms higher up in the performance distribution, the effect will be a significant reduction in firm growth.

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. 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 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 & 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 entrepreneur. 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.

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 - and alternative mechanisms - in further detail.

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.

VI.B. Other deaths

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 479 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.

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 negative and statistically significant but of far lesser magnitude than the death of the entrepreneur (about a third, depending on the metric one applies). 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.[35]

VI.C. 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.[36] Perez-Gonzales (2006) and Bennedsen et al. (2007) document negative effects on performance from family CEO appointments inside mature firms. We approach these questions by introducing 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 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. The estimated coefficients are suggesting that family firms are more resilient to the loss of the entrepreneurs, but not significant at conventional levels. 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.[37] Thus we do

[35] 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 (200 death events).

[36] 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.

[37] To examine this question in more detail, we modified the matching function to match exactly on

not find evidence that the large effects of founder death is due to incompetence of family members.

VI.D. 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. Overall, we find few significant differences, which speaks to the universality of the importance of the entrepreneur as a critical resource, and difficulties that most type of young firms will have with obtaining external financing.

One dimension of interest is education of the founder. Ideally we would like to have a measure of entrepreneurial ability. Some measure of IQ, although not necessarily capturing *entrepreneurial* ability, might be of interest, but is only available for a very small subsample. But education as measured by schooling attainment is of interest in itself. We define highly educated as having completed upper-secondary education, i.e., having at least 12 years of education. We find additional negative effects of founder death for highly educated founders for firm survival, employment, sales and assets, statistically significant in the case of assets (not reported). In Table 5, we find similar results for average education level in the sector the firm is active in (the coefficients on individual human capital and on sector human capital change only marginally if we include both as explanatory variables). These results suggest that founders are especially important in human capital intensive firms and sectors.

To investigate industry effects further, we classified industries according to growth, R&D and volatility, in a manner similar to Bennedsen et al. (2007); the results did not reveal a clear pattern.

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. We find however, no difference in causal effect of entrepreneurs in rural and urban areas.[38] This might indicate that, even if there is a larger supply of

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 to the performance of the firm.

[38] An interesting question is whether the causal effect of entrepreneurs is smaller in urban areas with

(potential) entrepreneurs in a city, there could be mitigating demand-side effects, such as the alternative entrepreneurs' opportunity cost of time being higher.

We then split firms up depending on whether the entrepreneur was the sole owner at the incorporation date or not. Again 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).

VII. 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 exogenously timed entrepreneur deaths as a natural experiment to identify the causal effect of entrepreneurs on firm performance. In addition to informing our understanding of firm performance, this question also 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 attract outside investors, which leads to lack of financing and underinvestment in entrepreneurial firms.

The results suggest that individual entrepreneurs play a large role in affecting firm performance. Entrepreneurs strongly affect firm survival, profitability, and growth patterns, and matter even for firms that have passed their infancy and begun to mature. 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). 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. Our evidence provides support to the Schumpeter (1934) idea of entrepreneurs as

a higher entrepreneurship rate. This question is difficult to answer because Norway has only a few cities, so we cannot exploit much variation in entrepreneurship rates across cities.

a causative force. It also suggests that financing constraints may bind far into a firm's life.

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Table 1
Descriptive statistics in year of foundation

	Firms where founder dies (283 obs)				Firms where founder does not die (23,740 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	51.09	9.47	21	67	42.55	10.06	18	67
Female	.12	.32	.00	1.00	.16	.37	.00	1.00
Single	.16	.37	.00	1.00	.27	.44	.00	1.00
Years of education	11.76	2.88	5.00	20.00	12.38	2.64	5.00	21.00
Dummy: years of education $>$ 11 years	.51	.50	.00	1.00	.67	.47	.00	1.00
Log wealth in year before firm foundation	13.50	1.40	9.21	16.89	13.20	1.40	9.21	21.44
Log earnings in year before firm foundation	12.68	.84	9.21	14.15	12.73	.81	9.21	17.78
Number of employees	2.81	4.24	.00	33.00	2.51	5.07	.00	170.00
Dummy: family firm	.13	.33	.00	1.00	.07	.26	.00	1.00
Log equity at firm foundation	11.72	.62	10.89	16.03	11.75	.64	10.73	19.22
Number of founders at firm foundation	1.60	.74	1.00	4.00	1.40	.66	1.00	5.00
Ownership share at firm foundation	.83	.19	.51	1.00	.88	.18	.50	1.00
Dummy: sole owner at firm foundation	.52	.50	.00	1.00	.67	.47	.00	1.00
Year of firm foundation	2002.05	2.05	1999	2007	2003.28	2.33	1999	20070
Agriculture and Fishery	.04	.19	.00	1.00	.03	.16	.00	1.00
Mining	.004	.06	.00	1.00	.004	.06	.00	1.00
Manufacturing	.07	.26	.00	1.00	.06	.23	.00	1.00
Utilities	.004	.06	.00	1.00	.004	.06	.00	1.00
Construction	.12	.32	.00	1.00	.13	.34	.00	1.00
Commerce	.27	.44	.00	1.00	.27	.44	.00	1.00
Business Services	.26	.44	.00	1.00	.27	.44	.00	1.00
Other Services	.11	.31	.00	1.00	.16	.36	.00	1.00
Transport, storage and communication	.09	.29	.00	1.00	.05	.22	.00	1.00

Note: The table depicts summary statistics of 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	21	7.42						
2000	61	21.55	2000			1	18	6.36
2001	46	16.25	2001	6	2.12	2	34	12.01
2002	42	14.84	2002	5	1.77	3	28	9.89
2003	44	15.55	2003	16	5.65	4	52	18.37
2004	34	12.01	2004	21	7.42	5	27	9.54
2005	8	2.83	2005	18	6.36	6	40	14.13
2006	25	8.83	2006	34	12.01	7	27	9.54
2007	2	0.71	2007	31	10.95	8	18	6.36
			2008	40	14.13	9	21	7.42
			2009	57	20.14	10	12	4.24
			2010	55	19.43	11	6	2.12
Total	283	100	Total	283	100	Total	283	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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall effect of founder death on firm performance						
Treated	-.011	.072	.095	.041	-.005	26.343
	(.032)	(.066)	(.199)	(.159)	(.016)	(26.580)
After * Treated	-.121	-.155	-.600	-.881	-.036	-108.011
	(.041)***	(.067)**	(.229)***	(.205)***	(.018)**	(34.984)***
Obs.	4092	4092	4092	4092	4092	4092
Adjusted R^2	.200	.127	.151	.216	.042	.104
Panel B: Effect of founder death on firm performance over time						
Treated * (3,4,5) years before founder death	-.010	.052	.085	.009	.017	28.248
	(.035)	(.074)	(.217)	(.168)	(.021)	(28.301)
Treated * (1,2) years before founder death	-.012	.094	.106	.076	-.030	22.420
	(.036)	(.067)	(.215)	(.175)	(.018)	(32.631)
Treated * year of founder death	-.085	.001	-.271	-.328	-.005	-12.921
	(.041)**	(.070)	(.228)	(.197)*	(.020)	(39.223)
Treated * (1,2) years after founder death	-.133	-.068	-.513	-.729	-.031	-52.394
	(.039)***	(.068)	(.223)**	(.200)***	(.015)**	(32.158)
Treated * (3,4,5) years after founder death	-.132	-.102	-.497	-.981	-.056	-119.449
	(.050)***	(.089)	(.295)*	(.274)***	(.016)***	(46.997)**
Obs.	4658	4658	4658	4658	4658	4658
Adjusted R^2	.187	.122	.146	.210	.043	.110

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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)
25% percentile					
Treated	.000	.000	-.223	.001	2.507
	(.022)	(.067)	(.176)	(.006)	(8.214)
After * Treated	.000	.000	.091	-.002	-2.509
	(.032)	(.100)	(.262)	(.008)	(12.237)
Obs.	4092	4092	4092	4092	4092
R^2	.003	.024	.126	.010	.023
50% percentile					
Treated	.131	.191	.068	-.001	.000
	(.083)	(.206)	(.147)	(.008)	(2.266)
After * Treated	-.213	-.580	-2.027	-.001	1.01e-15
	(.124)*	(.308)*	(.219)***	(.011)	(3.375)
Obs.	4092	4092	4092	4092	4092
R^2	.129	.175	.182	.009	.002
75% percentile					
Treated	.151	.254	.188	-.038	-13.300
	(.064)**	(.150)*	(.123)	(.017)**	(22.734)
After * Treated	-.349	-.769	-.877	-.052	-38.184
	(.096)***	(.224)***	(.183)***	(.026)**	(33.867)
Obs.	4092	4092	4092	4092	4092
R^2	.122	.095	.122	.089	.083

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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Owner works at firm in $(t-1)$						
After * Treated	-.022	-.005	-.020	-.478	-.020	-101.691
	(.046)	(.073)	(.252)	(.244)**	(.022)	(40.229)**
After * Treated * (Owner works at firm in $(t-1)$	-.244	-.358	-1.408	-.994	-.028	-11.128
	(.084)***	(.142)**	(.470)***	(.420)**	(.038)	(76.694)
Obs.	4092	4092	4092	4092	4092	4092
Adjusted R^2	.294	.277	.275	.284	.077	.123
Firm assets in year of foundation above/below median						
After * Treated	-.160	-.221	-.812	-1.081	-.051	-118.179
	(.053)***	(.084)***	(.291)***	(.261)***	(.023)**	(38.065)***
After * Treated * (Firm assets above median)	.085	.148	.460	.465	.034	25.272
	(.083)	(.136)	(.466)	(.418)	(.038)	(75.605)
Obs.	4092	4092	4092	4092	4092	4092
Adjusted R^2	.202	.128	.154	.218	.045	.105
Firm with/without family members as co-owners						
After * Treated	-.145	-.164	-.726	-.975	-.029	-108.971
	(.043)***	(.072)**	(.241)***	(.218)***	(.020)	(36.535)***
After * Treated * (Family firm)	.172	.084	.968	.740	-.056	3.018
	(.129)	(.213)	(.750)	(.635)	(.052)	(118.250)
Obs.	4092	4092	4092	4092	4092	4092
Adjusted R^2	.201	.127	.152	.217	.043	.104
Firm in sector with above/below median founder education						
After * Treated	-.071	-.108	-.413	-.729	-.022	-104.147
	(.048)	(.082)	(.277)	(.249)***	(.020)	(44.532)**
After * Treated * (Sector with founder education above median)	-.187	-.185	-.699	-.555	-.050	-11.257
	(.092)**	(.134)	(.485)	(.425)	(.046)	(64.239)
Obs.	4092	4092	4092	4092	4092	4092
Adjusted R^2	.202	.127	.152	.218	.045	.104

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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Firm in sector with above/below median sales growth						
After * Treated	-.070	-.071	-.343	-.548	-.029	-77.310
	(.057)	(.104)	(.351)	(.299)*	(.026)	(49.150)
After * Treated * (Sector sales growth above median)	-.066	-.130	-.339	-.498	-.016	-50.207
	(.079)	(.138)	(.458)	(.400)	(.037)	(67.822)
Obs.	3892	3892	3892	3892	3892	3892
Adjusted R^2	.254	.148	.194	.279	.056	.113
Firm in sector with above/below median R&D						
After * Treated	-.073	-.106	-.432	-.660	-.027	-110.726
	(.050)	(.090)	(.294)	(.260)**	(.022)	(48.117)**
After * Treated * (Sector R&D above median)	-.084	-.106	-.246	-.402	-.025	24.572
	(.082)	(.130)	(.450)	(.397)	(.041)	(67.137)
Obs.	3892	3892	3892	3892	3892	3892
Adjusted R^2	.256	.149	.198	.272	.048	.107
Firm in sector with above/below median wages						
After * Treated	-.093	-.135	-.577	-.805	-.021	-50.021
	(.053)*	(.101)	(.310)*	(.274)***	(.023)	(44.227)
After * Treated * (Sector wages above median)	-.038	-.048	-.018	-.108	-.032	-120.488
	(.078)	(.125)	(.439)	(.398)	(.038)	(69.119)*
Obs.	3953	3953	3953	3953	3953	3953
Adjusted R^2	.258	.172	.208	.261	.048	.104
Firm in sector with above/below median sales volatility						
After * Treated	-.129	-.140	-.613	-.887	-.038	-69.473
	(.043)***	(.071)**	(.243)**	(.209)***	(.021)*	(34.306)**
After * Treated * (Sector sales volatility above median)	.167	-.043	.631	.465	-.002	-234.047
	(.114)	(.205)	(.646)	(.622)	(.053)	(132.179)*
Obs.	3892	3892	3892	3892	3892	3892
Adjusted R^2	.253	.146	.194	.272	.047	.11

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.

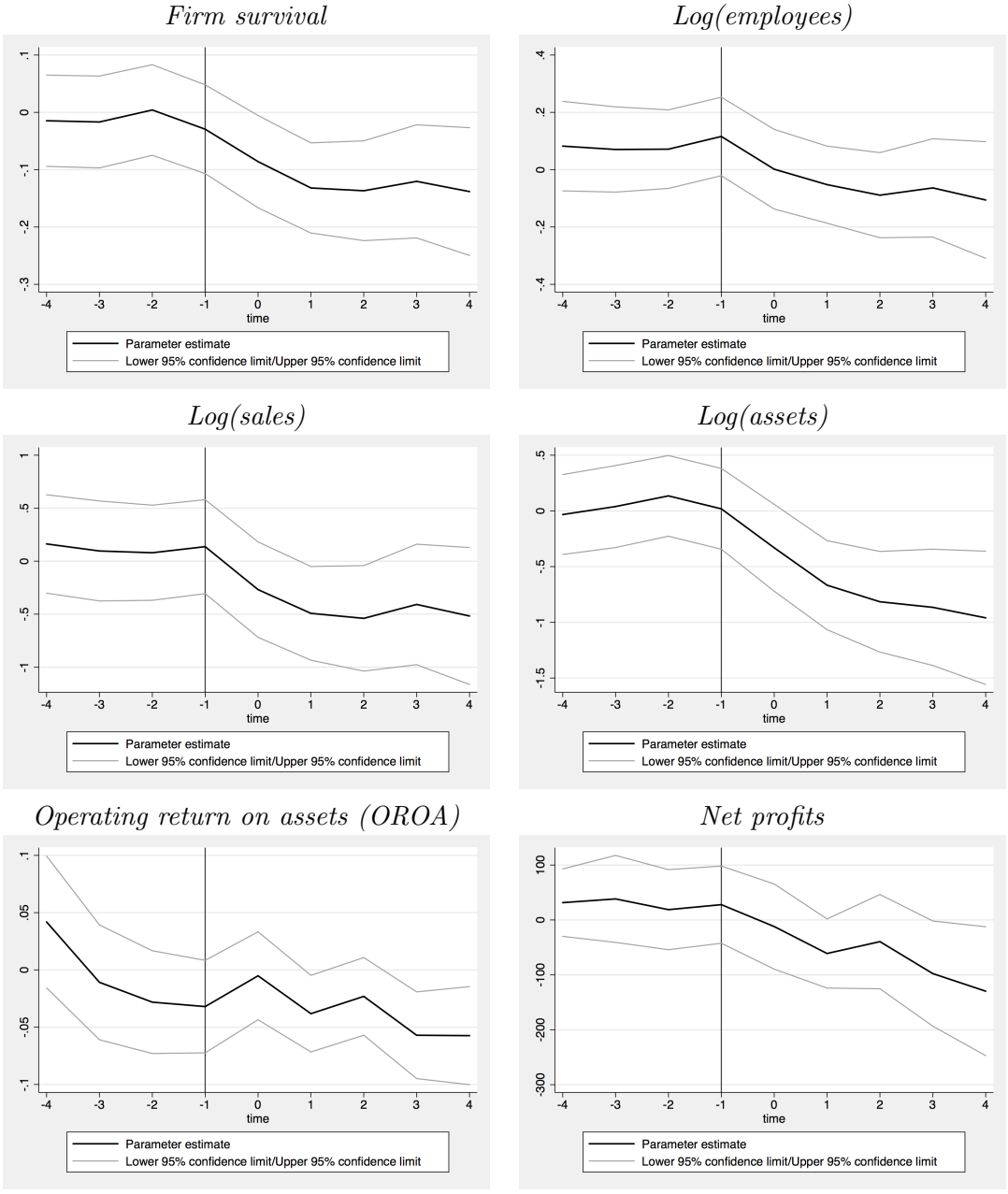
Table 6
Effect of founder death on firm performance for minority owners and 50% owners

	Firm active	log(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of founder death for minority owners						
Treated	-.021	.025	-.068	-.111	-.026	-31.027
	(.024)	(.059)	(.165)	(.131)	(.014)*	(26.803)
After * Treated	.007	-.104	.015	-.078	.025	-20.357
	(.031)	(.063)*	(.185)	(.160)	(.014)*	(30.635)
Obs.	7010	7010	7010	7010	7010	7010
Adjusted R^2	.126	.106	.084	.097	.028	.067
Effect of founder death for minority owners, interacted with dummy for whether owner works at firm in $(t-1)$						
After * Treated	.015	-.089	.064	-.064	.020	-18.115
	(.032)	(.066)	(.191)	(.169)	(.015)	(31.130)
After * Treated * (Owner works at firm in $(t-1)$)	-.079	-.047	-.258	-.045	.080	47.291
	(.083)	(.187)	(.561)	(.449)	(.046)*	(92.821)
Obs.	7010	7010	7010	7010	7010	7010
Adjusted R^2	.139	.144	.109	.114	.043	.079
Effect of founder death for 50-percent owners						
Treated	-.039	-.002	-.165	-.094	-.015	2.342
	(.040)	(.080)	(.248)	(.194)	(.022)	(25.545)
After * Treated	-.097	-.157	-.513	-.517	.007	5.498
	(.051)*	(.091)*	(.297)*	(.257)**	(.025)	(29.186)
Obs.	2450	2450	2450	2450	2450	2450
Adjusted R^2	.199	.140	.168	.212	.036	.053
Effect of founder death for 50-percent owners, interacted with dummy for whether owner works at firm in $(t-1)$						
After * Treated	-.070	-.094	-.324	-.471	.018	8.804
	(.055)	(.101)	(.323)	(.283)*	(.027)	(31.591)
After * Treated * (Owner works at firm in $(t-1)$)	-.110	-.290	-.808	-.134	-.030	7.729
	(.124)	(.218)	(.700)	(.604)	(.059)	(71.812)
Obs.	2450	2450	2450	2450	2450	2450
Adjusted R^2	.213	.163	.19	.222	.043	.059

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.

Figure 1

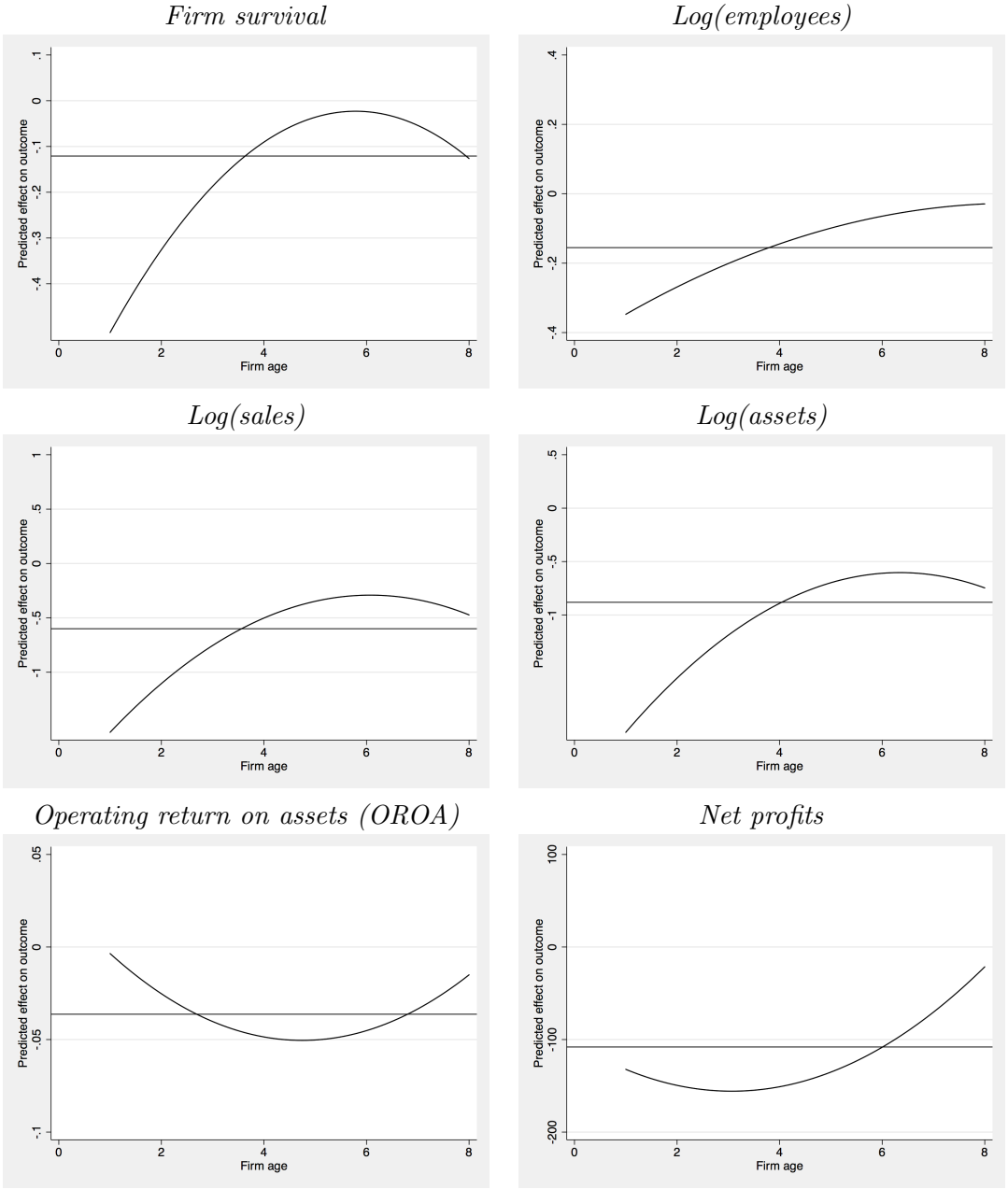
Treatment effects before and after founder death



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

Figure 2

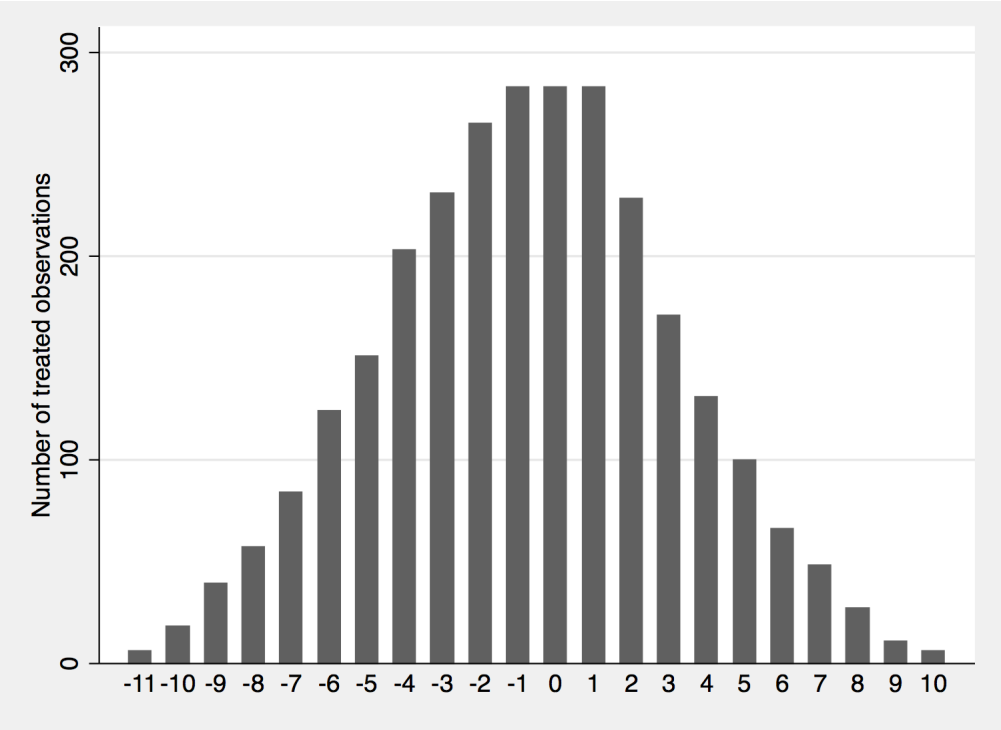
Treatment effect by firm age at founder death



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 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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Founder death	-.053	.026	-.204	-.212	-.007	5.861
	(.021)**	(.045)	(.137)	(.109)*	(.010)	(19.577)
After * Treated	-.159	-.247	-.837	-.999	-.033	-82.991
	(.030)***	(.050)***	(.176)***	(.163)***	(.012)***	(23.595)***
Obs.	209715	209577	209615	209699	209704	209715
Number of firms	24023	24023	24023	24023	24023	24023
Adjusted R^2	.159	.102	.115	.157	.040	.087

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	.0003
	(.0005)
Age squared	4.77e-06
	(5.38e-06)
Single	.003
	(.002)
Female	-.002
	(.001)
Dummy: Age 60 or above	-.003
	(.001)
Dummy: Urban area	.001
	(.001)
Dummy: years of education $>$ 11 years	-.002
	(.002)
Years of education	-.0002
	(.0003)
Log wealth in year before firm foundation	-.0004
	(.004)
Log wealth in year before firm foundation squared	-.0000114
	(.0001)
Log earnings in year before firm foundation	.009
	(.008)
Log earnings in year before firm foundation squared	-.0004
	(.0003)
Interaction between log wealth and log earnings	.0000349
	(.0003)
Dummy: family firm	.001
	(.002)
Log equity at firm foundation	-.003
	(.01)
Log equity at firm foundation squared	.0000974
	(.0005)
Dummy: Equity at firm foundation above median	-.0006
	(.002)
Firm started in 2000	.001
	(.002)
Firm started in 2001	-.0007
	(.002)
Firm started in 2002	-.002
	(.002)
Firm started in 2003	-.002
	(.002)
Firm started in 2004	-.003
	(.002)
Firm started in 2005	-.007
	(.0009)
Firm started in 2006	-.006
	(.001)
Firm started in 2007	-.007
	(.0007)

Continued on next page

Table A.2: continued from previous page

Agriculture and Fishery	.0004 (.004)
Mining	-.001 (.006)
Manufacturing	.001 (.003)
Utilities	.0001 (.007)
Construction	.0007 (.003)
Commerce	.0008 (.003)
Business Services	.001 (.003)
Other Services	-.0008 (.003)
Transport, storage and communication	.005 (.005)
Number of founders at firm foundation	-.0005 (.001)
Ownership share at firm foundation	-.003 (.006)
Dummy: sole owner at firm foundation	-.002 (.003)
Obs.	24023
Pseudo-R^2	.12

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(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall effect of founder death on firm performance: matching on $(t-1)$ characteristics						
Treated	-.016	.059	-.150	-.141	-.010	2.637
	(.017)	(.090)	(.174)	(.138)	(.022)	(41.625)
After * Treated	-.282	-.383	-1.383	-1.532	-.043	-148.529
	(.050)***	(.099)***	(.293)***	(.245)***	(.026)*	(58.411)**
Obs.	2333	2332	2333	2333	2333	2333
Adjusted R^2	.297	.216	.295	.332	.067	.110
Panel B: Heterogeneity of treatment effect: matching on $(t-1)$ characteristics						
Owner works at firm in $(t-1)$						
After * Treated	-.178	-.185	-.639	-1.041	.016	-68.480
	(.075)**	(.157)	(.443)	(.372)***	(.040)	(79.509)
afterint-work	-.200	-.381	-1.436	-.934	-.114	-147.838
	(.100)**	(.199)*	(.589)**	(.492)*	(.051)**	(115.598)
Obs.	2333	2332	2333	2333	2333	2333
Adjusted R^2	.303	.254	.314	.338	.072	.122

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.