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Sascha O. Becker
Hans K. Hvide

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of Labor

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Sascha O. Becker
CAGE, Warwick University,
CEPR, CESifo, Ifo and IZA

Hans K. Hvide
University of Bergen,
CEPR, University of Aberdeen and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Do Entrepreneurs Matter?*

In the large literature on firm performance, economists have given little attention to entrepreneurs. We use deaths of more than 500 entrepreneurs as a source of exogenous variation, and ask whether this variation can explain shifts in firm performance. Using longitudinal data, we find large and sustained effects of entrepreneurs at all levels of the performance distribution. Entrepreneurs strongly affect firm growth patterns of both very young firms and for firms that have begun to mature. We do not find significant differences between small and larger firms, family and non-family firms, nor between firms located in urban and rural areas, but we do find stronger effects for founders with high human capital. Overall, the results suggest that an often overlooked factor – individual entrepreneurs – plays a large role in affecting firm performance.

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

Keywords: entrepreneurship, firm performance, human capital

Corresponding author:

Sascha O. Becker
Department of Economics
University of Warwick
Coventry CV4 7AL
United Kingdom
E-mail: s.o.becker@warwick.ac.uk

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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 (e.g., Schumpeter, 1934), but geographical, institutional, and industry characteristics have been the focus of empirical work (see surveys by Bartelsman & Doms, 2000 and Syverson, 2011). One objective of the paper is to ask how much individual entrepreneurs contribute to firm performance.

It is hard to imagine the success of Apple Computers without Steve Jobs, or of Ford Motor Corporation without Henry Ford. Stories of smaller-scale entrepreneur successes are heard about almost every day. Little is known, however, whether entrepreneurs more generally 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 focus on the role of the entrepreneur, and whether the entrepreneur provides the 'glue' that keeps the firm together.

To examine the influence of entrepreneurs on firm performance, we could investigate whether the entrepreneur's engagement (captured by, for example, whether he is employed by the firm or not) is associated with firm performance. This strategy is problematic, of course, as engagement is likely to be affected by underlying economic conditions. To deal with this problem, 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 construct a large database that contains longitudinal accounting and employment information on the universe of incorporated, limited liability firms established in Norway between 1999 and 2007. The data contain initial ownership shares, which allows us to identify entrepreneurs; 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

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

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 ownership (341 death events). We track firm performance at a yearly level until the end of 2010, so that firms in our database are between zero and eleven years old. The data also include yearly panel information about each owner back to 1993, including family relation to the entrepreneur.

We ask whether entrepreneurs have a causal effect on firm performance. For each of the 341 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. We find robust evidence that firm performance drop after the entrepreneur's death. The magnitudes are large; for example, the mean effect on sales is about 60 percent, while the mean effect on employment is about 17 percent. Thus we find a large drop in firm performance after entrepreneur death, pointing to a large role played by entrepreneurs.

Firms experiencing entrepreneur death have about 20 percentage point lower survival rates two years after the death event. They also have significantly higher bankruptcy rates.[2] One could argue that entrepreneur death merely speeds up evolution by weeding out firms that likely would not flourish even if the entrepreneur stayed alive. Using quantile regressions, we find strong negative effects of entrepreneur death on sales and assets also among firms in the upper deciles of the firm performance distribution. We infer that entrepreneurs also have an important causal effect for the more productive firms.

One explanation for our results is that entrepreneur death creates turbulence. If so, we would expect a large immediate effect on firm performance, and a partial or full reversal over time. The point estimates indicate that the immediate effects of entrepreneur death are quite modest relative to the sustained effects that accumulate after some time. This finding suggests, again, that entrepreneurs have a causal effect on the growth path of

[2] 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.

the firm. Another explanation of our results is that they are somehow an artifact of our matching procedure. We therefore also analyze the effect of entrepreneur death using ordinary least squares, using all the firms in our database. The estimated coefficients are somewhat *larger* than in the analysis based on matching.

For how long in a firm's life does the entrepreneur matter? In order to examine this question, we analyze whether the drop in firm performance depends on firm age when the entrepreneur dies (recall that firm age ranges between zero and eleven years in the database). We find that the importance of the entrepreneur is largely independent of firm age; the entrepreneur effects do not appear to vanish even when the firm is close to ten years old. We think this is an important result; the majority of surviving firms reach a more 'mature' phase with moderate growth after about five or six years of existence. Thus the entrepreneur also has a large effect when the firms have reached this more mature phase.

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. We do not have access to dynamic ownership data but attempt to address this question in an indirect way: 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, and 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. Relatedly, we examine whether the death of a married entrepreneur has a different impact than the death of a non-married entrepeneur. Neither here do we find significant differences.[3]

Based on the endogenous growth literature (e.g., Glaeser et al., 1992), we ask whether the causal effect of individual entrepreneurs is smaller in urban areas, where the supply of entrepreneurs is likely to be denser and the entrepreneur possibly more substitutable. We find, however, no difference in the causal effect of entrepreneurs in rural versus urban

[3]In Section VI.A. we also discuss whether aspects of Norwegian inheritance law or capital gains tax could bias our results. We do not think this is the case.

areas. We find some evidence suggesting that high human capital entrepreneurs are more important, and that entrepreneurs are more important in firms in high human capital industries. We also apply other sector classifications (high/low growth sectors; high/low volatility sectors etc.), but do not find significant differences, a sign of the universality of the importance of entrepreneurs.

While the main analysis concentrates on individuals with more than 50 percent initial ownership, we also analyze the role of individuals with a 50 percent initial ownership (204 death events). Using difference-in-differences estimation, we find negative effects on firm performance that are about half the size of those in the main analysis. We also analyze the impact of the death of minority owners, i.e., individuals with less than 50 percent initial ownership share (495 death events). In unreported regressions, we find that the point estimates are very close to half of those of the 50 percent founders, i.e., a quarter of the effect for the founders with more than 50 percent ownership share.

A concern is that many firms in our sample are very small, and perhaps motivated by tax-reduction or providing private benefits rather than the desire for profits. To address the possibility that the effects differ for small and large firms, we interact the treatment effect with a dummy for firms below median size, in terms of initial equity. We find large effects for startups both below and above median size. We then interact the treatment effect with a dummy for firms with less than two employees in the first year. We find only minor differences in the effect of entrepreneur death for these types of firms.

An even simpler 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.[4]

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

[4]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 affect firm performance negatively.

by means of interaction effects. Section VII interprets the results and concludes.

II. Why should entrepreneurs matter?

Empirical studies of firm performance tend to take a neoclassical view of the firm in which entrepreneurs are homogenous 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 this type of theory is 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.[5]

A theoretical tradition that opens up a way for entrepreneurs to play a more important role in the production process 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, or ideas can be seen as the critical resource which the firm is initially organized around; for example, the entrepreneur can define a product line, hire people, or invest in physical assets that are complementary to himself (this is a sense in which the entrepreneur is an argument in the production function). Under this view, the entrepreneur can have two effects on firm performance. The first is the direct effect through providing the critical resource, and the second 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. These findings are consistent with spillovers from the entrepreneur to the productivity of the firm's employees.

[5] Other theories of entrepreneurship such as Hellmann (2007) and Hvide (2009) emphasize contractual frictions in established firms. These contractual frictions induce individuals with promising ideas to leave and start up their own firms. Although these theories can explain productivity differences between new firms, they cannot explain why individuals become non-substitutable in the new firms.

Although critical resource theory opens up for entrepreneurs being important in new firms, it says less about for how long in the firm's life. Do entrepreneurs enter the production function for more mature firms? This is a question we try to address in the empirical analysis. 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 contrast, are both owners and managers, which makes it difficult to separately identify the effects of losing the owner from the effects of losing the manager in the empirical analysis.[6] We are not convinced this is a major shortcoming as the distinction between operations and strategy is blurred in the context of very young firms.[7] By focusing on entrepreneur death, we quantify the joint effect of owners and managers for these young firms.

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 is negative for the sample overall, but they find a positive effect for CEOs that were also founders of the company, a finding verified with more recent data by Pérez-González (2006). As argued by Slovin and Sushka (1993), some of these results might be explained not by the ability of the entrepreneur but by an increase in the probability of a corporate control contest. Across the literature on productivity and firm performance, not very much is known about the importance of individuals.[8] Bertrand & Schoar (2003) documents differences in management styles between individuals, and find evidence consistent with

[6] In about 80 percent of the firms in our database that hire people, the entrepreneur is employed in the firm in the second year of operations. We cannot identify whether the entrepreneur is a manager, but find it very likely.

[7] For example, decisions that are relatively routine in mature firms, such as the pricing of individual products, can have major strategic importance when the firm is young. For firms where the owner is not an employee, we expect the owner to be involved also in decisions that are viewed as operational in more mature firms.

[8] 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 sourced 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."

CEOs of publicly listed companies affecting firm performance. Bloom & Van Reenen (2007) document that higher-quality management practices are correlated with several measures of productivity and firm performance in a sample of non-listed firms. Bloom & Van Reenen (2007) 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.

An old tradition from Coase (1937) debates what constitutes a firm and what keeps a firm together (physical or human assets). The property rights view of the firm (e.g., Grossman & Hart, 1986, Hart, 1995) emphasizes non-human assets, while Hart & Moore (1994) and Zingales (2000) emphasize human assets. Kaplan, Sensoy & Stromberg (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 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.

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.[9] 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.

[9] 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.[10] 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. 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. It is commonly known, however, that 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.[11]

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

[10] 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.

[11] 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.) The approximate median tax value of the firms in our sample is NOK 324.000, the 75 percentile is NOK 1.2 million, and the 90 percentile is NOK 4.5 million. In 2008, $1 was equal to about NOK 7.

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).

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 more than 50 percent of the initial shares.[12] The data include yearly accounting and employment measures for each firm until the end of 2010, so that the firms in our database are between zero and eleven years old. By comparison 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 slightly 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 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.

[12] For 1999, the data contain only a sample of the firms started. Diagnostic tests do not suggest any selection bias. The Norwegian dividend tax reform of 2006 implied that a large number of firms started up in NACE five-digit sector no. 65238 (portfolio investments) in 2006 were tax minimization vehicles (in fact, most of these firms were closed down in 2007). We therefore eliminated these firms from our database. 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.

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 a separate analysis below, we also look at owners with an exact 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]

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

[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.

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 37,011 firms, 341 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 real estate and transportation, and less likely to be in business services and financial intermediation. 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 2009 inclusive.[15] Another interesting descriptive is firm age at founder death. Founder death occurs at any firm age, from year 1 through year 10 (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

[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.

IV. Empirical strategy

IV.A. Estimation sample

It is natural *not* to compare the 341 firms with founder death to *all* 36,670 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 341 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 341 founders and for 36,567 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 observations.[19] We impose a caliper (i.e., radius) of 0.05, i.e., treated firms that have

[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 37,011 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.

[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.

no comparison unit and whose estimated propensity score is within 0.05 of their own estimated propensity score are discarded to avoid bad matches. Imposing this caliper, we only lose 2 of the 341 treated founders.[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 0.11 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 18.04. 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

[20] We also looked at results without imposing a caliper and the results are very similar, so imposing the caliper is not essential in our case, but follows common practice.

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 .02. Similarly, the variables entering the propensity score are no longer jointly significant, with a p-value of .957. The median absolute standardized bias drops from 18.04 before matching to 2.90 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 want to understand 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. In fact, when comparing firm performance measures in the year before 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 mea-

[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.

sures as indicators of differences between treated and control firms.[22] We find the median absolute standardized bias to be 2.06, so very small. Similarly, the pseudo-R^2 from a regression of the treatment dummy on these performance measures is 0.005, 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 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 for both treated firms and matched controls. Our estimation sample consists of the 339 treated firms and 339 matched controls.

[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.

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. 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 performance distribution. We turn to these issues below.

Startup performance can be measured by growth, survival, and profitability. We analyze how entrepreneur death affects all these aspects of firm performance. 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 firms that close down, we set the relevant variables equal to zero to measure the effect on sales, employment

[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.

and assets.[25] 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).[26] Firms that cease to exist have zero earnings and zero assets, and OROA is undefined. We impute OROA equal to zero for these observations.[27] Survival is assessed by whether a firm is active in given year or not.[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 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

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

[26] 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.

[27] 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.

[28] 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.

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

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).

For comparison, we also perform the analysis using OLS on all 37,011 firms in our database.[32] The estimated coefficients, reported in Table A.1, are somewhat larger than in the main matching analysis.[33] For reasons explained earlier we tend to put more weight on the matching results of Panel A than those OLS results.

It is possible that 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, as 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

[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] The 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.

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 exam-

[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.

ple, 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. Distributional effects

The above results indicate that, on average, entrepreneurs have a large and sustained causal effect on firm performance. One possibility, consistent with the strong negative effect on firm survival, is that entrepreneur death 'merely' speeds up evolution by weeding out weak firms. We are therefore interested in whether entrepreneur death has an effect across the firm performance distribution, not only for weak firms.

To address this 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 performance distribution. Our evidence 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, 75th and at the 90th 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. In fact, one way to view our results is that founder death shifts the distribution of firm outcomes to the left (towards weaker outcomes). For firms around median quality the consequence 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.

VI. Interaction effects

So far, we have for the most part concentrated on homogeneous treatment effects. One exception were the quantile regressions where we looked at differences in the effect of founder death over different parts of the outcome distribution. Now, we want to focus on whether certain types of entrepreneurs matter more or less, and in what type of 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.

As discussed previously, it is possible that unobserved factors in the years leading up to founder death could affect founder or firm characteristics. We should emphasize, therefore, that we interact treatment only with pre-determined controls, i.e., founder and firm characteristics from the year of foundation. Our results in the following section should therefore be interpreted as the effects of founder death broken down on different initial founder and firm characteristics, not on any intermediate characteristics that might have been affected by illness.

VI.A. Individual and firm characteristics

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.[35] Perez-Gonzales (2006) and Bennedsen et al. (2007) document negative effects on performance from family CEO appointments inside mature firms. To examine this question, we examine whether entrepreneur death affects family and non-family firms differently. 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. Although the estimated coefficients are negative, they are quite far from being significant. Both family firms and non-family firms seem to be equally affected by death of a majority owner.

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 when the firm is founded and those where not. In unreported regressions, we do not find any difference in treatment effects for founders with and without children.[36]

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.[37] 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

[35] 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.

[36] 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 to the performance of the firm.

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

as the alternative entrepreneurs' opportunity cost of time being higher. Another 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 at least upper-secondary education, i.e., having at least 12 years of education. While we find additional negative effects of founder death for highly educated founders for firm survival, employment, sales and assets, they are statistically significant only in the case of assets. In unreported regressions, 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.

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). 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 investigated 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 find only minor differences in results for startups below and above median size in terms of initial equity.[38] We then split firms up depending on whether they had two or more employees at the end of the first year or not, or whether the entrepreneur was the sole owner at the incorporation date or not. Again the differences are minor.

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, a sign of the universality of the importance of founders across

[38] The estimated results are weak for the largest 10-15 percent of the firms in our sample, but this subsample is not large enough to have reasonable standard errors. If we confine attention to the largest 25 percent of firms in our sample, the estimated effects are large and significant.

different types of firms.

VI.B. 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 third-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 a very strong negative effect of founder death on the survival of very young firms, and a smaller but still significant negative effect on survival for more mature firms. For firm growth, the time pattern is less clear – firm age at founder death seems to play less of a role. 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.[39]

VI.C. 50-percent owners

We have confined the sample to owners that have a majority share in the company. We now consider how the treatment effects vary with the entrepreneur being a majority owner or not. We would expect owners that have a substantial ownership fraction in the firm, but less than a majority, to have a smaller impact on the firm. To test this hypothesis, we performed a separate analysis of the importance of founders that have 50 percent share in the firm.

We have 204 firms where a 50 percent owner dies. Of those, 150 are firms with two equal partners, i.e., both holding 50 percent; 54 are firms where the 50 percent owner

[39] 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 covers slightly more than two of the four firm age brackets considered by Foster et al. (2008).

has more than one co-owner holding less than 50 percent each. We adopt the same methodology as in the majority-owner sample, but we add the number of 50-percent owners (i.e., one or two) as a matching variable in the propensity score matching and as a control variable in the regressions. The results, reported in Table A.3, suggest that these founders are important but not as important as the majority owners; the coefficients on the causal effect of the founder are about half the estimated effect of majority owners.

We can take this one step further and look at minority owners (495 death events). In unreported regressions, we find that the point estimates are very close to half of the effect that we get from the 50% founders, i.e., 1/4 of the effect for the founders with more than 50% ownership share; taking into account standard errors, a zero effect of death of minority owners cannot be rejected for the majority of outcome variables. This finding shows that death of those with a more limited role in the firm has a substantially smaller effect on firm performance than death of majority owners.

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.

We find large and sustained effects of entrepreneurs. Entrepreneurs strongly affect firm survival and growth patterns, and matter even for firms that have passed their infancy and begun to mature. One way to view our results is that founder death shifts firm quality to the left. For firms in the lower part of the quality distribution the consequence is a higher probability of closing down, while for firms higher up in the quality distribution, the effect will be a significant reduction in firm growth. We do not find significant differences between small and larger firms, family and non-family firms, nor between firms located in urban and rural areas, but do find stronger effects for founders with high human capital, and weaker effects for individuals that are part of an entrepreneurial team. Overall, the results suggest that an often overlooked factor – individual entrepreneurs – plays a large

role in affecting firm performance.

Surprisingly little is know about the importance of individuals for firm performance. One takeaway from the present paper is to provide causal evidence that individual entrepreneurs have a large quantitative impact on firm performance. Why do entrepreneurs matter so much? Although our findings are consistent with large direct effects, they are also suggestive of spillover effects; we find very large negative effects on sales after founder death but smaller negative effects on firm employment. Thus, reminiscent of the Azoulay et al. (2010) study on the death of research superstars, entrepreneurs could contribute to the productivity of the firm's employees. We also inform a separate and old literature on the nature of the firm, stemming from Coase (1937). Some theoretical views suggest that human factors are more important than physical assets. Using exogenously timed entrepreneur deaths, we find that one human factor - the entrepreneur - plays a large and persistent role.

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Table 1
Descriptive statistics in year of foundation

	Firms where founder dies (341 obs)				Firms where founder does not die (37,011 obs)			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Founder death	1	0	1	1	0	0	0	0
Age	51.48	9.88	20.00	67.00	43.17	10.16	16.00	67.00
Female	.13	.34	0	1	.15	.36	0	1
Single	.14	.35	0	1	.26	.44	0	1
Years of education	11.82	2.75	5.00	20.00	12.49	2.67	5.00	21.00
Dummy: years of education > 11 years	.53	.50	0	1	.69	.46	0	1
Log wealth in year before firm foundation	13.88	1.55	9.21	17.67	13.47	1.50	9.21	21.44
Log earnings in year before firm foundation	12.58	1.11	9.21	15.10	12.78	.88	9.21	18.36
Dummy: family firm	.13	.34	0	1	.08	.27	0	1
Log equity at firm foundation	11.97	.99	10.89	17.17	11.84	.80	4.09	19.31
Number of founders at firm foundation	1.47	.70	1	4.00	1.33	.62	1	5.00
Ownership share at firm foundation	.87	.18	.51	1	.91	.16	.50	1
Dummy: sole owner at firm foundation	.62	.49	0	1	.73	.45	0	1
Year of firm foundation	2002.39	2.23	1999	2007	2004.00	2.48	1999	2008
Agriculture and Fishery	.03	.17	0	1	.02	.14	0	1
Mining	0	0	0	0	.003	.05	0	1
Manufacturing	.05	.22	0	1	.04	.20	0	1
Utilities	.003	.05	0	1	.003	.05	0	1
Construction	.08	.27	0	1	.10	.30	0	1
Commerce	.19	.39	0	1	.19	.39	0	1
Financial Intermediation	.05	.22	0	1	.07	.25	0	1
Business Services	.18	.39	0	1	.20	.40	0	1
Other Services	.06	.25	0	1	.11	.32	0	1
Real estate activities	.26	.44	0	1	.21	.41	0	1
Transport, storage and communication	.06	.25	0	1	.04	.19	0	1

Standard errors in parentheses: * significance at ten, ** five, *** one percent.
Note: The table depicts summary statistics of founders and the firms they start up, broken down on 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	19	5.57						
2000	70	20.53	2000	1	0.29	1	33	9.68
2001	57	16.72	2001	7	2.05	2	55	16.13
2002	40	11.73	2002	7	2.05	3	49	14.37
2003	45	13.2	2003	16	4.69	4	50	14.66
2004	43	12.61	2004	24	7.04	5	42	12.32
2005	26	7.62	2005	27	7.92	6	43	12.61
2006	26	7.62	2006	46	13.49	7	26	7.62
2007	15	4.4	2007	47	13.78	8	19	5.57
			2008	71	20.82	9	20	5.87
Total	341	100	2009	95	27.86	10	4	1.17
			Total	341	100	Total	341	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	.008	.071	.089	.023	-.019	-6.844
	(.028)	(.057)	(.173)	(.138)	(.014)	(29.096)
After * Treated	-.195	-.185	-.914	-.942	-.026	-65.328
	(.036)***	(.057)***	(.201)***	(.195)***	(.017)	(38.662)*
Obs.	4430	4395	4427	4430	4430	4430
Adjusted R^2	.273	.225	.243	.289	.05	.113
Panel B: Effect of founder death on firm performance over time						
Treated * (3,4,5) years before founder death	.026	.050	.197	.091	-.016	2.657
	(.033)	(.065)	(.200)	(.152)	(.019)	(28.260)
Treated * (1,2) years before founder death	-.010	.091	-.015	-.038	-.020	-15.278
	(.031)	(.057)	(.184)	(.153)	(.015)	(38.689)
Treated * year of founder death	-.068	-.026	-.310	-.340	.002	28.155
	(.035)*	(.059)	(.193)	(.175)*	(.016)	(45.457)
Treated * (1,2) years after founder death	-.159	-.091	-.681	-.754	-.037	-67.594
	(.033)***	(.059)	(.191)***	(.182)***	(.012)***	(40.514)*
Treated * (3,4,5) years after founder death	-.232	-.148	-1.042	-1.167	-.055	-77.323
	(.048)***	(.083)*	(.275)***	(.274)***	(.016)***	(42.997)*
Obs.	5108	5072	5104	5108	5108	5108
Adjusted R^2	.262	.223	.24	.292	.052	.118

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	0	0	.038	-.0008	-.910
	(–)	(–)	(.142)	(.005)	(5.935)
After * Treated	0	0	-.663	.0008	.884
	(–)	(–)	(.215)***	(.007)	(8.972)
Obs.	4395	4427	4430	4430	4430
$Pseudo - R^2$	.000	.002	.236	.007	.015
50% percentile					
Treated	0	.032	.126	-.011	1.826
	(–)	(.198)	(.136)	(.008)	(8.663)
After * Treated	0	-.819	-2.175	-.006	-11.322
	(–)	(.300)***	(.205)***	(.012)	(13.097)
Obs.	4395	4427	4430	4430	4430
$Pseudo - R^2$	.089	.197	.199	.013	.008
75% percentile					
Treated	.057	.079	.166	-.019	-14.890
	(.054)	(.135)	(.085)*	(.015)	(28.885)
After * Treated	-.113	-.661	-.587	-.052	-16.846
	(.081)	(.204)***	(.129)***	(.022)**	(43.667)
Obs.	4395	4427	4430	4430	4430
$Pseudo - R^2$	.238	.220	.168	.094	.101
90% percentile					
Treated	.008	.023	.159	-.020	-40.583
	(.084)	(.152)	(.086)*	(.019)	(66.338)
After * Treated	-.158	-.489	-.383	-.035	-145.272
	(.127)	(.230)**	(.130)***	(.028)	(100.286)
Obs.	4395	4427	4430	4430	4430
$Pseudo - R^2$	.240	.177	.206	.135	.199

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)
Firm assets in year of foundation above/below median						
After * Treated	-.212	-.258	-1.165	-1.243	-.065	-66.705
	(.044)***	(.066)***	(.253)***	(.244)***	(.019)***	(38.221)*
After * Treated * (Firm assets above median)	.035	.109	.357	.445	.056	-17.194
	(.068)	(.107)	(.382)	(.374)	(.030)*	(78.122)
Obs.	4430	4395	4427	4430	4430	4430
Adjusted R^2	.274	.225	.244	.289	.051	.114
Firm with/without family members as co-owners						
After * Treated	-.199	-.203	-.974	-1.036	-.032	-70.251
	(.034)***	(.055)***	(.197)***	(.196)***	(.015)**	(33.777)**
After * Treated * (Family firm)	-.027	-.015	-.172	-.170	-.033	-41.684
	(.107)	(.170)	(.601)	(.543)	(.047)	(112.020)
Obs.	4430	4395	4427	4430	4430	4430
Adjusted R^2	.274	.229	.244	.289	.050	.113
Firm in sector with above/below median founder education						
After * Treated	-.162	-.186	-.815	-.880	-.028	-66.526
	(.037)***	(.061)***	(.219)***	(.212)***	(.015)*	(37.602)*
After * Treated * (Sector with founder education above median)	-.121	-.063	-.629	-.875	-.018	-86.120
	(.077)	(.112)	(.413)	(.381)**	(.041)	(86.711)
Obs.	4430	4395	4427	4430	4430	4430
Adjusted R^2	.276	.226	.244	.29	.052	.113

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.

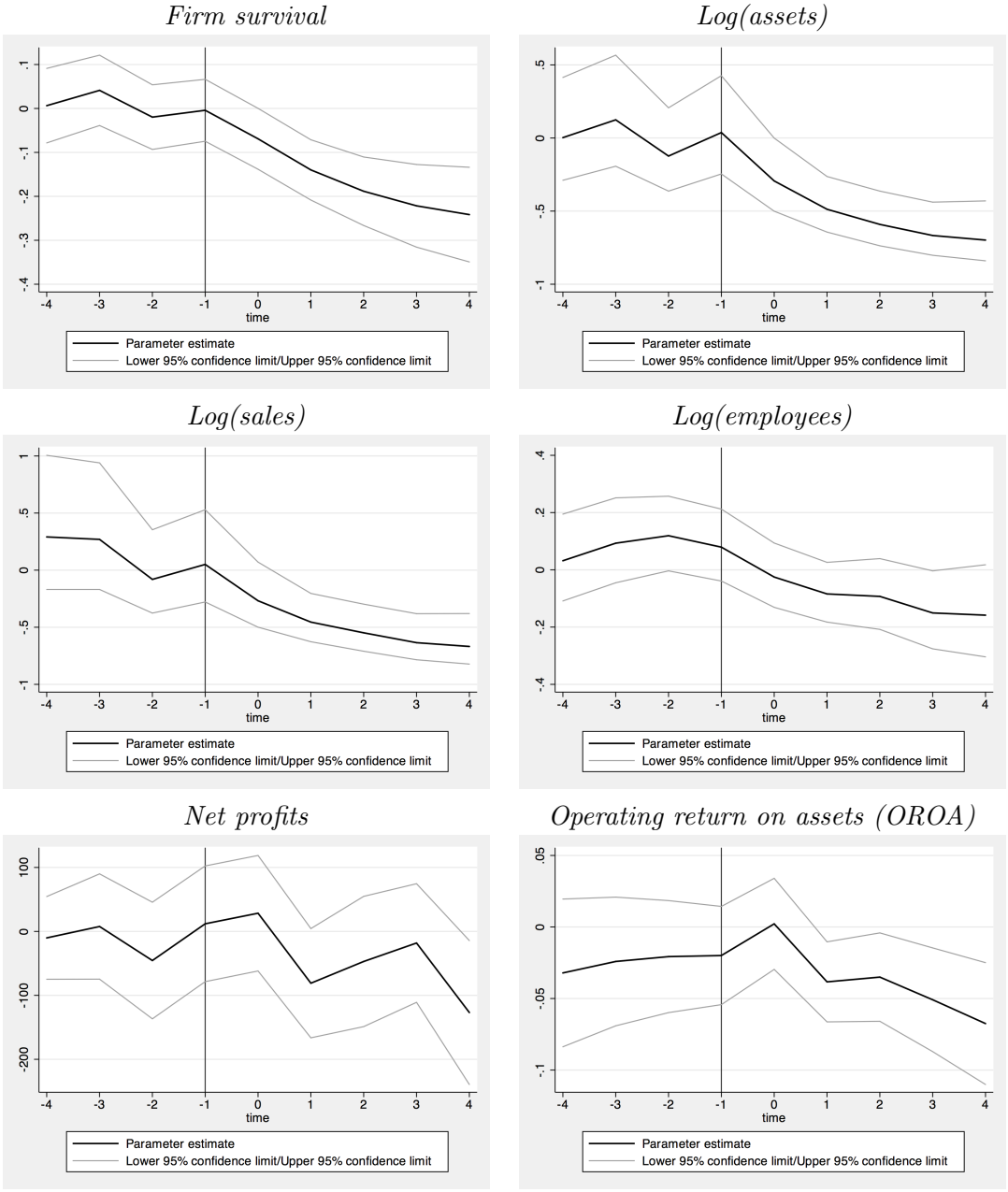
Table 6
Heterogeneity of the effect of founder death on firm performance (cont'd)

	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	-.167	-.182	-.823	-.892	-.025	-63.674
	(.037)***	(.063)***	(.225)***	(.215)***	(.016)	(40.287)
After * Treated * (Sector sales growth above median)	-.083	-.097	-.479	-.583	-.028	-94.557
	(.072)	(.106)	(.391)	(.363)	(.035)	(78.669)
Obs.	4352	4317	4349	4352	4352	4352
Adjusted R^2	.274	.226	.245	.297	.052	.101
Firm in sector with above/below median R&D						
After * Treated	-.177	-.224	-.900	-.935	-.031	-75.202
	(.037)***	(.060)***	(.221)***	(.211)***	(.016)*	(39.134)*
After * Treated * (Sector R&D above median)	-.061	.039	-.231	-.484	-.024	-64.871
	(.072)	(.107)	(.395)	(.356)	(.035)	(82.483)
Obs.	4352	4317	4349	4352	4352	4352
Adjusted R^2	.274	.223	.244	.296	.046	.102
Firm in sector with above/below median wages						
After * Treated	-.205	-.376	-1.424	-1.084	-.051	-53.059
	(.053)***	(.097)***	(.322)***	(.284)***	(.023)**	(47.805)
After * Treated * (Sector wages above median)	.004	.279	.608	.098	.016	-52.631
	(.067)	(.112)**	(.391)	(.367)	(.030)	(70.376)
Obs.	4422	4387	4419	4422	4422	4422
Adjusted R^2	.282	.246	.262	.291	.052	.099
Firm in sector with above/below median sales volatility						
After * Treated	-.260	-.297	-1.478	-1.334	-.048	-61.041
	(.041)***	(.070)***	(.243)***	(.208)***	(.019)**	(34.499)*
After * Treated * (Sector sales volatility above median)	.136	.186	1.002	.989	.018	-25.756
	(.065)**	(.095)*	(.350)***	(.400)**	(.028)	(76.822)
Obs.	4352	4317	4349	4352	4352	4352
Adjusted R^2	.276	.231	.254	.300	.044	.101

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

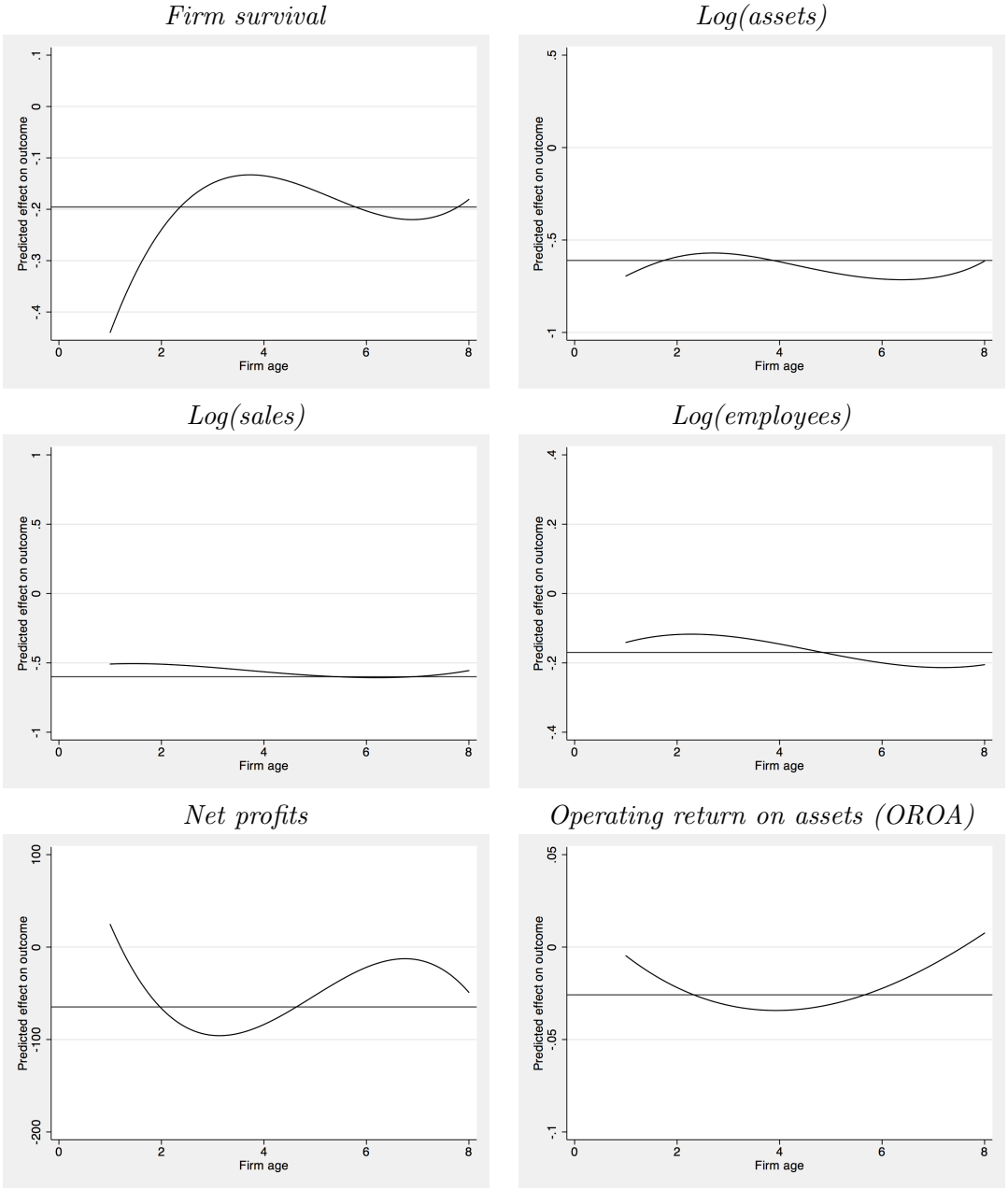
Treatment effects before and after founder death



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

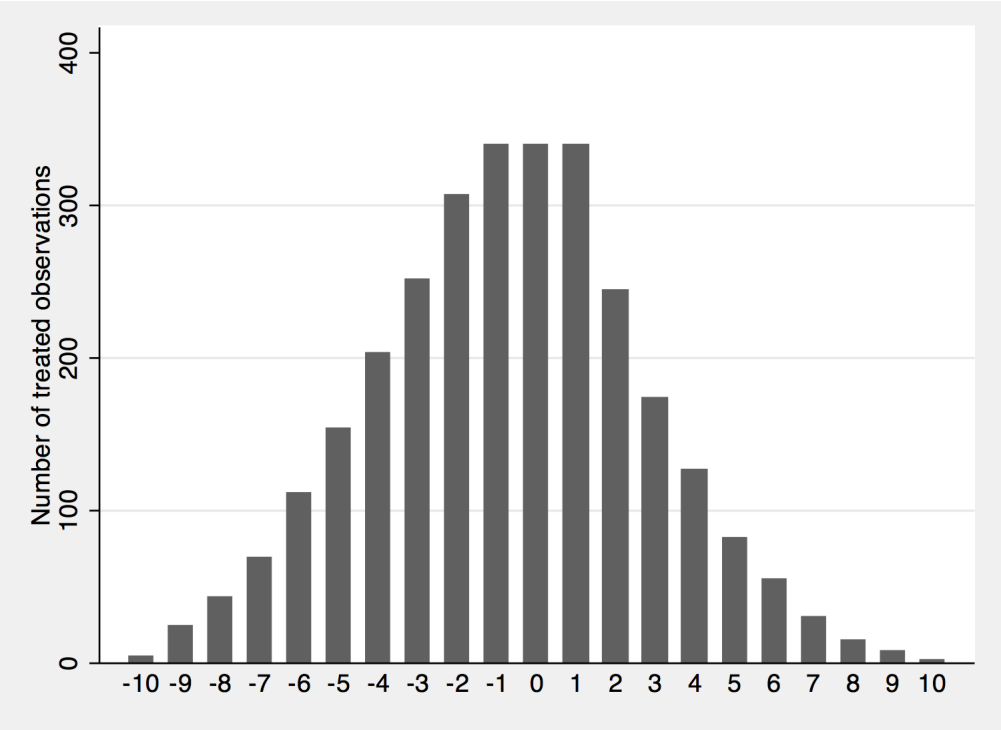
Figure 2

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)
Treated	.037	.055	.113	.009	-.002	-16.882
	(.017)**	(.038)	(.114)	(.096)	(.009)	(18.600)
After * Treated	-.352	-.275	-1.306	-1.559	-.033	-55.991
	(.025)***	(.042)***	(.150)***	(.152)***	(.011)***	(23.774)**
Obs.	259774	254408	259640	259757	259762	259774
Number of firms	37011	37011	37011	37011	37011	37011
Adjusted R^2	.127	.186	.223	.167	.038	.082

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	-.0001 (.0003)
Age squared	6.19e-06 (3.50e-06)
Single	.002 (.001)
Female	-.0003 (.0009)
Dummy: Age 60 or above	-.002 (.001)
Dummy: Urban area	.0000359 (.0006)
Dummy: years of education > 11 years	-.001 (.001)
Years of education	-.0002 (.0002)
Log wealth in year before firm foundation	.003 (.003)
Log wealth in year before firm foundation squared	-.0000375 (.0000777)
Log earnings in year before firm foundation	-.002 (.004)
Log earnings in year before firm foundation squared	.0001 (.0002)
Interaction between log wealth and log earnings	-.0001 (.0002)
Dummy: family firm	.0002 (.001)
Log equity at firm foundation	-.005 (.005)
Log equity at firm foundation squared	.0002 (.0002)
Dummy: Equity at firm foundation above median	.0004 (.001)
Firm started in 1999	.91 (.50)
Firm started in 2000	.89 (.56)
Firm started in 2001	.88 (.62)
Firm started in 2002	.85 (.71)
Firm started in 2003	.86 (.70)
Firm started in 2004	.85 (.73)
Firm started in 2005	.72 (1.01)
Firm started in 2006	.60 (1.07)
Firm started in 2007	.55 (1.11)

Continued on next page

Table A.2: continued from previous page

Agriculture and Fishery	.002 (.004)
Manufacturing	.002 (.003)
Utilities	.002 (.008)
Construction	.002 (.003)
Commerce	.002 (.003)
Financial Intermediation	.01 (.006)
Business Services	.003 (.003)
Other Services	-.0000155 (.002)
Real estate activities	.004 (.003)
Transport, storage and communication	.006 (.005)
Number of founders at firm foundation	-.0007 (.0009)
Ownership share at firm foundation	-.004 (.005)
Dummy: sole owner at firm foundation	-.0000376 (.002)
Obs.	36908
Pseudo-R^2	.12

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

Table A.3
Overall effect of founder death on firm performance for 50% owners

	Firm active	log(employees+1)	log(sales+10)	log(assets+10)	Return on assets	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	.003	.025	-.096	-.015	-.011	-29.159
	(.034)	(.065)	(.210)	(.182)	(.018)	(21.294)
After * Treated	-.119	-.151	-.546	-.559	-.002	35.000
	(.045)***	(.075)**	(.253)**	(.235)**	(.019)	(27.547)
Obs.	2698	2686	2697	2698	2698	2698
Adjusted R^2	.302	.224	.24	.221	.034	.047

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.