Review of Finance, 2021, 1–23
doi: 10.1093/rof/rfab015
Advance Access Publication Date: 22 May 2021

Entrepreneur Death and Startup Performance*

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Abstract

How large is entrepreneurs' personal importance to startups? We use the death of nearly 1,500 entrepreneurs as a source of exogenous variation, and find large and sustained negative effects on growth and profitability. For small startups, the effects go mainly via firm survival, while for larger startups the effects are mainly on firm growth. For larger startups, the mean effect on sales is about 60%. The effects appear to be driven by entrepreneur specialness rather than leadership transition; the effects of death of entrepreneur managers are economically and statistically stronger than the death of managers that are not entrepreneurs.

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

Keywords: Credit constraints, Entrepreneurship, Firm performance

accepted May 21, 2021 by Editor • • •.

1. Introduction

While corporate finance theory takes the existence of firms as given, financial economists are increasingly interested in where firms come from and which factors affect their inception and growth (Rajan, 2012). In this paper, we provide evidence on the personal importance of the entrepreneur in the beginning of a firm's life, by analyzing the effects on the startup of entrepreneur death. We compare the growth and profitability of startups hit by this shock with startups that are not.

Ever since Coase (1937) economists have debated why firms exist and what keeps them together. Analyzing the role of the entrepreneur can inform us of what constitutes the "core" of a firm early on, and how it develops over time. Also, if entrepreneurs personally embed a major part of the value of the firm, it will be difficult to pledge the value of the

* For valuable input, thanks to many seminar audiences and to John Moore, Kasper Meisner Nielsen, and Steve Tadelis. This paper replaces "Do entrepreneurs matter?," which was based on a much smaller sample of startups.

firm to outside investors, which could lead to credit constraints and underinvestment in entrepreneurial firms (Hart and Moore, 1994). The extent to which entrepreneurs are non-substitutable is largely an unexplored question.[1]

We use novel and extraordinarily detailed data from Norway that cover the universe of firms newly incorporated between 1999 and 2015. The data identify all initial owners with at least 10% ownership for all these firms, and we define an entrepreneur broadly as an individual with at least 10% ownership share initially. We identify about 1,500 death events (among in total 73,500 startups). Using yearly accounting figures submitted to the Norwegian tax authorities allow us to track startup performance before and after the death event. Our empirical methodology is a difference-in-differences setup where we compare "treated" startups, that is, where the founder dies, with control startups where the founder does not die. We include calendar year- and firm-fixed effects.

The empirical analysis provides robust evidence that entrepreneur death strongly affects firm performance, both on firm growth, as measured by sales or employment, and on firm profitability. The effects are large; for example, the negative effect on sales of entrepreneur death is about 60%, and persistent. Young startups hit by founder death are particularly vulnerable, but even more mature startups (>80 years old) experience a major setback.

In order to analyze how founder death affects startups of different size, we split the startups into deciles of initial employment. For small startups, the effects of founder death go mainly via firm survival. For larger startups (five or more employees during the first 2 years), the effects are mainly on firm growth, and large: sales and assets are about 50% less than in the control group of firms not affected by the shock. As an alternative way to assess effects for larger startups, we analyze how founder death affects a startup's chance of being among the top deciles of sales or employment. Again the effects are large; for example, founder death reduces the chance of being in the top 10% by about 2.5 percentage points.

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

One obstacle to a causal interpretation of our findings is that poor firm performance could lead the entrepreneur to have a higher probability of dying. In other words, firms experiencing founder death might start to perform poorly between the startup date and the time of founder death. However, we find no pre-treatment effects, suggesting that even if

1 Several recent papers use death as an exogenous event to study the causal effects of individuals, for example, Azoulay, Zivin, and Wang (2010) on the spillover effects of research superstars; Bennedsen, Perez-Gonzalez, and Wolfenzon (2020) on the value of CEOs; 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 Andersen and Nielsen (2012) on the effect of windfall gains through inheritance on entrepreneurial activity.

founder death is expected in some cases, it does not affect firm performance until the founder dies.

The paper connects to the literature in several ways. First, the paper informs a large and vibrant empirical literature that explores factors (e.g., founder ability, access to finance, legislative framework) that affect startup performance.[2] We complement this literature by directly measuring the impact of entrepreneurs, and by demonstrating that the impact is large in a setting with plausibly exogenous variation in entrepreneur engagement. Kaplan, Sensoy, and Strömberg (2009) shows that for firms that undertake an Initial Public Offering, the entrepreneur plays a limited role once the firm is venture capital backed. Our findings suggest that the entrepreneur being relatively "unimportant" does not extend to a wider universe of start-ups. More closely related, Smith *et al.* (2019) study the effect of business owner death for US firms where the owner has yearly fiscal income of at least one million US dollars. They find large effects; for example, the drop in profits per worker 4 years after founder death are about 53%, which is somewhat larger than the 36% drop in profits in our main specification.[3] The relation between our results and that of Smith *et al.* (2019) is further discussed in Section 4.1.

Our findings also inform theory. In the theory of the firm (Hart, 1995), the entrepreneur controls the physical assets of the firm, much like a manager, and is important due to this role rather than his intrinsic qualities. The entrepreneur is to a large extent substitutable. In contrast, human capital oriented theories (Hart and Moore, 1994; Diamond and Rajan, 2005) posit that the entrepreneur has some unique set of characteristics or knowledge that are not easily replaced. Under the first type of theories, which emphasizes control aspects, we would expect modest or temporary effects of the entrepreneur's death, while under the second type of theory, we would expect large and permanent effects. Our results support the human capital oriented theories. Rajan (2012) models a synthesis of these views, in that the firm is less reliant on the entrepreneur as it ages, consistent with our findings. Appendix B contains a review of additional theoretical work.

2. Data and Institutional Background

2.1 Data Collection

We construct a database that consists of the universe of incorporated, limited liability, firms started up in Norway between 1999 and 2015. [4]The data include yearly detailed accounting and employment measures for each firm until the end of 2015, so that the firms in the

2 This literature is too large to list exhaustively. A few recent examples are Azoulay, Fons-Rosen, and Zivin (2019) on founder age; Gompers *et al.* (2010) on ability differences and performance persistence for serial entrepreneurs; Ewens and Marx (2018) and Hall and Woodward (2010) on venture capital funding; Hvide and Jones (2018) on legislative framework; Hvide and Oyer (2018) on within-family transfer of human capital; Kerr and Nanda (2009) on financial constraints; Kerr, Lerner, and Schoar (2014) on business angels; Lerner and Malmendier (2013) on peer learning; and Levine and Rubinstein (2017) on founder aptitude.

3 The number reported in Smith *et al.* (2019) is 81%. However, this is the effect after normalizing the ownership fraction to 100%.

4 For the year 1999, the data contain only a sample of the firms started. Diagnostic tests do not suggest any selection bias.

database are between 0 and 16 years old.[5] We confine attention to firms that have at least one employee (which may be the entrepreneur himself) in the first year of operations. To avoid counting wealth management vehicles as start-ups, we eliminate finance and real-estate firms (NACE sector codes 65-70). The inclusion of these firms gives similar results.

The data are compiled from three different registers:

1. *Accounting information* from Dun and Bradstreet's database of accounting figures based on the annual financial statements submitted to the tax authorities. This data include variables such as five-digit industry code, sales, assets, number of employees, and profits for the years 1999–2015. Note that the D&B data contain yearly information on *all* Norwegian incorporated limited liability companies, and not a sample as in the US equivalent. Incorporated companies are required to have an external auditor certifying the accounting statements in the annual reports.
2. *Founding documents submitted by new firms* to the government agency "Brønnoysundregisteret." This register includes start-up year, capitalization, and the personal identification number and ownership share of all initial owners with at least 10% ownership stake.
3. *Data on individuals* from 1993 to 2015 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 allow 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 (contain no names of individuals).

For each new firm identified in (1), we create a list of owners identified through (2) and compile their associated socio-demographic information from (3). For a small fraction of firms, the first year of financial reporting, defined through (1), is different than the year of incorporation defined by (2). For these firms, we define the first year as the first year of financial reporting.

We define an entrepreneur broadly, as a person with at least 10% ownership of the total shares in a newly established limited liability firm (we interchangeably refer to this person as an "entrepreneur" or a "founder").

2.2 Descriptives

Table I presents descriptive statistics. Founder characteristics 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 the first year of operations, except for employment, which is taken to be the maximum of the first two calendar years of operation to account for the fact that the first year of operations usually falls

5 We eliminated firms where the founder died after 2014 because we want to have at least 2 years of post-death information (including the year of death) for any firm in our analysis. We also drop firms where the founder was older than 67 years, that is, beyond retirement age, when founding the firm, and firms that are subsidiaries of listed companies during the first 2 years. Our main results do not change if we include these firms.

Table I. Descriptive statistics

The table depicts summary statistics in the first year of operations for the sample of all founders and the firms they start up, broken down by whether the founder dies (treated) or not (control). Founder characteristics refer to the first year of operations, with the exception of log wealth and log earnings which are taken as the log of 5-year averages prior to firm foundation. Firm characteristics refer to the first year of operations, except for employment, which is taken to be the maximum of the first two calendar years of operation to account for the fact that the first year of operations usually falls short of a full calendar year. Year of death and firm age at death are shown only for firms whose founder dies.

	(1) Treated mean	(2) SD	(3) Min	(4) Max	(5) Control mean	(6) SD	(7) Min	(8) Max
Age	50.88	10.35	20.00	67.00	40.84	10.13	7.00	67.00
Female	0.16	0.37	0.00	1.00	0.24	0.43	0.00	1.00
Single	0.17	0.38	0.00	1.00	0.36	0.48	0.00	1.00
Education (eight categories)	4.05	1.64	0.00	8.00	4.36	1.62	0.00	8.00
Log wealth	13.51	1.29	9.23	18.90	13.07	1.24	9.21	20.47
Log earnings	12.82	0.64	9.75	15.38	12.80	0.64	9.21	17.78
Year of firm foundation	2003.15	3.24	1999.00	2013.00	2007.39	4.62	1999.00	2014.00
Ownership share at firm foundation	0.52	0.29	0.10	1.00	0.70	0.31	0.10	1.00
Dummy: sole owner at firm foundation	0.21	0.41	0.00	1.00	0.47	0.50	0.00	1.00
Family firm	0.26	0.44	0.00	1.00	0.13	0.34	0.00	1.00
Log equity at firm foundation	4.86	0.70	3.22	8.59	4.48	0.85	−0.18	11.59
Employment	4.79	9.73	1.00	232.00	4.15	7.88	1.00	1,246.00
Other/unknown sector	0.05	0.22	0.00	1.00	0.18	0.38	0.00	1.00
Mining	0.01	0.09	0.00	1.00	0.00	0.05	0.00	1.00
Manufacturing	0.08	0.26	0.00	1.00	0.04	0.20	0.00	1.00
Utilities	0.00	0.05	0.00	1.00	0.00	0.03	0.00	1.00
Construction	0.14	0.35	0.00	1.00	0.11	0.32	0.00	1.00
Commerce	0.27	0.44	0.00	1.00	0.24	0.42	0.00	1.00
Business services	0.23	0.42	0.00	1.00	0.20	0.40	0.00	1.00
Other services	0.15	0.36	0.00	1.00	0.18	0.39	0.00	1.00
Transport, storage, and communication	0.08	0.27	0.00	1.00	0.05	0.21	0.00	1.00
Sudden death	0.17	0.38	0.00	1.00				
Year of death	2010.02	3.32	2000.00	2014.00				
Firm age at founder death	6.87	3.81	0.00	15.00				
N	1,459.00				72,089.00			

short of a full calendar year. The median book value of assets and number of employees in the first year of operations are about $100,000 and 3, respectively.

Table I 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). The founders who die are older and, likely as a consequence, wealthier and less educated. The sectoral composition is very similar. Founder death occurs at any firm age, from year 0 to year 15 (the maximum firm age possible given our sample). The median firm size is 3, that is, 50% of our firms have between zero and three employees. The average firm size is 5. The 75th percentile is five employees, the 90th percentile is ten, and the 95th percentile is fifteen. Using newly available data on cause of founder death, we follow the definition of Andersen and Nielsen (2011) to define sudden deaths.[6] A total of 1,459 founders die, and about 17% are sudden deaths.

2.3 Institutional Background

For general descriptives on Norway, see for example, Doskeland and Hvide (2011). 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 100,000 in the sample period (in 2008, $1 was equal to about 7 NOK). 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 was a flat 28% on net capital gains in the sample period. The tax value of a firm, which is included in its owners' wealth statements, is calculated as 60% 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 toward 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 the results.[7]

3. Empirical Strategy

We ask whether entrepreneurs affect firm performance and answer this question using the timing of entrepreneur deaths as a source of exogenous variation. Here, we describe the empirical strategy in more detail.

6 Unexpected deaths occur instantaneously or within a few hours of an abrupt change in the person's previous clinical state, for example, cardiac arrest. See Andersen and Nielsen (2011) for details.

7 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, that is, they pay NOK 24,000 in inheritance tax each. (NOK 24,000 is equivalent to about $3,000) This is unlikely to be a challenge for most Norwegian households. 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. After January 1, 2014 the inheritance tax was abolished.

Entrepreneurs that die within the sample period are older on average (and, as a consequence, wealthier) than entrepreneurs that do not die. For each firm where the entrepreneur dies ("treated" firms) we therefore use propensity score matching to identify a similar firm ("matched control" firm), restricted to be started up in the same calendar year. The matching procedure is described in detail in Appendix A, the essence is to for each treated firm find a matched control firm that is similar in the year of foundation. After matching, we use the year of founder death at treated firms to impute the counterfactual year of founder death for the matched controls ("nearest neighbor"), so that "after" founder death is defined for both groups. We use a standard difference-in-differences regression approach

$$\text{Performance}_{it} = \alpha_i + \beta_1 * \text{after}_{it} * \text{treated}_i + \beta_2 * \text{after}_{it} + \delta_t + \epsilon_{it}, \quad (1)$$

where β_1 is the coefficient of interest, measuring the difference between treated firms and matched control firms after founder death. We include firm-fixed effects α_i to accommodate unobservable differences between startups. Firm-fixed effects will capture all the (time-invariant) variables that entered the original matching procedure. Year and firm age-fixed effects, captured by δ_t, are also included. To assess the effects across the startup size distribution, we interact the treated dummy in (1) with deciles of initial firm size.

Turning to the performance measures, survival is assessed by whether a firm is active in given year or not.[8] To assess startup growth, we examine the effect of entrepreneur death on sales, on number of employees, and on the (book) value of assets and EBITDA. One might be tempted to exclude firm-year observations after firm closure, but that would introduce a bias.[9] Firms that cease to exist have zero employees, a zero wage sum, zero sales, and zero assets. Note that throughout, we use the inverse hyperbolic sine of the outcome variables.[10] To minimize the role played by imputations, we also use quantile regressions and non-parametric specifications. In the latter, we replace absolute measures of firm performance in Equation (1) with relative measures, such as a yearly dummy variable for a startup having sales in the top 10% of its age cohort. Non-surviving firms have a value of the dummy variable equal to zero.

To assess the robustness of our main results, we also estimate β_1 using all 73,500 firms in the database. The regression specification in this case equals

$$\text{Performance}_{it} = \alpha_i + \beta_1 * \text{after}_{it} * \text{treated}_i + \delta_t + \epsilon_{it}. \quad (2)$$

Note that the main parameter of interest, β_1, is also identified using this specification, while β_2 from Equation (1) is not because only treated firms have "after" = 1.

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

9 An example illustrates this point: if founder death has a devastating effect so that only one firm survives, the one with highest quality among these firms, our regression estimates for firm growth would be positive.

10 The inverse hyperbolic sine (see Burbidge, Magee, and Robb, 1988) is defined as $\text{IHS}(x) = \ln(x + ((x^2 + 1)^{1/2}))$ and is similar to the log transformation for small values but also defined for negative values and zeroes. See Azoulay, Fons-Rosen, and Zivin (2019) for a recent application. In terms of interpretation of marginal effects, except for very small values of x, the inverse hyperbolic sine is approximately equal to $\ln(2x)$ or $\ln(2) + \ln(x)$. Marginal effects can be interpreted in the same way as a standard logarithmic dependent variable.

As another way to assess robustness, we also estimate β_1 using treated firms only, in order to accommodate that there could be differences between treated and control firms not captured by matching or firm-fixed effects. The treated-only regression specification is

$$\text{Performance}_{it} = \alpha_i + \beta_1 * \text{after}_{it} + \delta_t + \epsilon_{it}, \tag{3}$$

where only treated firms are included in the regression.

4. Results

4.1 Basic Results

Table II, Panel A, presents the results from the estimation of Equation (1) on the matched sample. For details on the matching procedure, see Appendix A. We consider a window from 5 years before to 5 years after founder death; the results using all firm-years are very similar.

Entrepreneur death has significant negative effects on firm survival and growth. The effect on survival is about 10.3 percentage points, the effect on sales about 60%, and on assets about 58%.[11] The effect on employment is smaller (about 21%) and the effect on wage payments about 53%. The final column shows a negative effect of founder death on profitability of 36%; below we compare this to the findings of Smith *et al.* (2019).

The results reported in Table II are highly robust. Panel B shows that matching with replacement yields nearly identical results. This is not surprising as our matching with replacement barely ever uses matched controls more than once, so matching with and without replacement are nearly the same thing in our setting. In panel C, we enlarge the window around founder death to ±10 years. In panel D, we not only match firms exactly on year of foundation, but also on firm size in the year of foundation. In all cases, results are very similar.[12]

One obstacle to a causal interpretation of these findings is that poor firm performance could lead the entrepreneur to have a higher probability of dying. If so, we would expect performance differences between treated and matched control firms prior to founder death. The matching procedure ensures that there are no differences between the treated and matched control firms at the time of foundation. But there could still be an anticipation effect ("pre-trends") if founder death is foreseeable. We check for pre-trends by means of graphs. The coefficient plots in Online Appendix Figure A.1, based on regressions interacting the treatment effect with event time in the matched sample, show the absence of pre-treatment differences.[13] We take this as clear evidence that treated and control firms are

11 Remember from footnote 10, that marginal effects of IHS-transformed dependent variables are computed in the same way as log dependent variables; coefficients on dummy variables need to be transformed as exp(coefficient) $-$ 1 to yield percentage effects. For instance the coefficient of –0.857 in column 5 of panel A translates into a –57.56% effect: $\exp(-0.857) - 1 = -0.5756$.

12 As specified in Equations (2) and (3), in Online Appendix Table A.1, we also consider an unmatched sample where we use the full set of control firms (panel A), and a before-after analysis on the sample of treated firms only (panel B). While numerically different, results are qualitatively similar, attesting to strong effects of founder death on various firm outcomes.

13 One exception is IHS(EBITDA) which is generally a somewhat more volatile variable.

Table II. Matched sample, pooled across all ownership categories: effect of founder death on firm performance

Coefficient of interest is β_1 in Equation (1). Standard errors in parentheses: *significance at 10%, **5%, ***1% level. Yearly panel of treated and control firms. Sample restricted to ±5 years around the (imputed) year of founder death in panels A and C, and ±10 years in panel B. Fixed effects at the firm level. Observations from year of founder death excluded because not clearly attributable to "before" or "after" the event. [a]Control variables: year effects, firm age dummies, and the variable "After," that is, dummy = 1 for years after (imputed) year of founder death.

	(1) Firm active	(2) his (employees)	(3) IHS (wage sum)	(4) IHS (sales)	(5) IHS (assets)	(6) IHS (EBITDA)
Panel A. Matching without replacement (reference specification)						
After × Treated	−0.109***	−0.235***	−0.757***	−0.939***	−0.857***	−0.453***
	(0.015)	(0.035)	(0.114)	(0.129)	(0.115)	(0.140)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.21	4.24	4.99	5.01	1.92
R-squared	0.25	0.15	0.18	0.21	0.22	0.01
Observations	21,546	21,344	21,546	21,546	21,546	21,546
Panel B. Matching with replacement						
After × Treated	−0.104***	−0.234***	−0.701***	−0.892***	−0.813***	−0.451***
	(0.015)	(0.035)	(0.114)	(0.128)	(0.116)	(0.141)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.22	4.26	5.02	5.05	1.95
R-squared	0.25	0.14	0.18	0.21	0.22	0.01
Observations	22,194	21,982	22,194	22,194	22,194	22,194
Panel C. Original matching algorithm: ±10 years around founder death						
After × Treated	−0.100***	−0.218***	−0.696***	−0.843***	−0.799***	−0.408***
	(0.015)	(0.038)	(0.120)	(0.135)	(0.123)	(0.140)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.60	1.24	4.35	5.17	5.18	1.97
R-squared	0.31	0.16	0.21	0.25	0.26	0.01
Observations	30,922	30,589	30,922	30,922	30,922	30,922
Panel D. Alternative matching within year and size category (±5 years around founder death)						
After × Treated	−0.113***	−0.233***	−0.799***	−0.964***	−0.840***	−0.304**
	(0.015)	(0.036)	(0.117)	(0.132)	(0.118)	(0.141)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.19	4.21	4.95	5.02	1.90
R-squared	0.25	0.15	0.19	0.21	0.22	0.01
Observations	20,686	20,465	20,686	20,686	20,686	20,686

not only comparable at firm foundation, but that matched pairs of treated firms and controls also develop similarly until the year right before founder death.[14]

Potential concerns about deaths being foreseeable raise the issue of sudden versus non-sudden deaths. We would expect that, if founder death is anticipated, its effects on firm performance are milder because the founder and those around her (investors and/or family members) may put preparations in place that soften the impact of founder death.[15] In other words, we expect sudden deaths to have a stronger effect on firm performance. In Online Appendix Table A.1, we show that this is indeed the case; the effects of sudden death (panel C) are overall considerably larger than the effects of non-sudden death (panel D).

The fact that firms experiencing sudden death of their founder struggle more could be the result of the turmoil triggered by surprise or because firms with sudden deaths differ from other treated firms. To probe into this issue, we perform a second matching analysis *within* the set of treated firms where we try to identify the nearest neighbors for firms experiencing sudden deaths among those experiencing non-sudden deaths. In panel E, we use only those treated (and their original controls) which experienced sudden deaths and those non-sudden deaths that are most similar in terms of founder and firm characteristics to the sudden-deaths firms. The magnitudes of coefficient estimates in panel E are in between those in panels C and D, suggesting that sudden deaths are telling both about a surprise element resulting in some turmoil, but also that firms experiencing sudden deaths are somewhat different from firms experiencing other types of deaths. For instance, founders experiencing sudden death are more likely to be young and male.

The graphical evidence in Online Appendix Figure A.1 is also informative about whether the effects of founder death are only transient. These graphs show no bounce-back, as would be the case if the effects were transient due to, for example, turbulence created by entrepreneur death, and it appears that founder death has a permanent negative effect on firm growth.

The results reported in Table II are driven by two separate effects; firms closing down (and getting zero assets and sales) and firms surviving but decreasing their scale, relative to the control group. It is possible that entrepreneur death weeds out weak firms, but there being small effects on higher quality firms.[16] As a first take on this question (see also the non-parametric regressions in Section 4.2), in Online Appendix Table A.2, we look at quantile regressions for the same type of specification as in Table II, but where we compare the performance of treated and control firms above median of the conditional performance

14 The coefficient plots in Online Appendix Figure A.2 show no detectable pre-trends also for non-sudden deaths. To probe even further into whether there are pre-treatment differences, we can use the pseudo-R^2 of a regression of the treatment dummy on our firm performance measures in the year before founder death. The pseudo-R^2 from a regression of the treatment dummy on these performance measures is 0.0008, an indication that treated firms and controls do not differ in their performance in the year before founder death (not reported). 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 1.2 (not reported).

15 For example, Jenter, Matveyev, and Roth (2018) report that non-sudden deaths of CEOs are associated with a positive stock market return. Presumably, the reason is that a prolonged illness with a low-productive CEO is detrimental to the firm.

16 For example, if the startup is unprofitable but the entrepreneur enjoys private benefits, heirs could close down the startup after entrepreneur death. This mechanism would be consistent with large treatment effects in Table II without a drop in the underlying quality of the startup after the death of the entrepreneur.

distribution (below the median, there are smaller differences between treated and control firms, which is largely explained by the fact that both treated and control firms go out of business). Online Appendix Table A.2 shows that there are negative effects of founder death on assets and sales at all deciles between the median the 90th percentile. For example, at the 80th percentile, the effects on sales and on assets are about 18% for both variables. This suggests large effects of entrepreneurs across the startup performance distribution.

These findings provide to our knowledge the first evidence of the importance of founders based on credibly exogenous variation in founder engagement. Smith *et al.* (2019) study the effect of business owner death for US firms where the owner has yearly fiscal income of at least 1 million US dollars. Smith *et al.* (2019) do not report firm age characteristics, but given their considerable size these firms are unlikely to be startups. Smith *et al.* (2019) report a drop in profits of about 53% following business owner death. This is somewhat larger than the 36% drop in startup profits in our Table II, which is rather puzzling, as one would expect death to have a larger impact for young firms. Although the methodologies employed are similar, we include firms that have closed down prior to founder death in our sample (this choice is to be able to assess the potentially important pre-treatment effects), while Smith *et al.* (2019) include only firms that are active in the year before founder death. This means that our estimated coefficients will mechanically be lower—founder death cannot have effects on firms that have already closed down. If we exclude from our sample firms that are inactive in the year prior to founder death, as in Smith *et al.* (2019), we obtain a 48% drop in profitability (based on a coefficient estimate of −0.651 in Online Appendix Table A.1, Panel F), in other words similar to in Smith *et al.* (2019).[17]

Our findings provide evidence that the entrepreneur is important for the individual startup. For the broader significance of the findings, as noted by, for example, Decker *et al.* (2014) young firms are on aggregate an important driver behind job creation in the USA. Some simple calculations suggest that the founder specificity we document is important for aggregate job creation. The total private sector employment in Norway was about 1.5 million jobs in 2000, and grew by about 180,000 jobs between 2000 and 2007.[18] The startups we follow employed about 70,000 individuals after 2 years of operation, that is, almost 40% of net job creation. According to our estimates, the founder effect on startup employment is about 18%, so that the founder effect is about 7% (40% * 18%) of total net job creation. These calculations do not take into account the relatively high exit rate and thus job loss for startups. It turns out, however, that a high exit rate is close to being compensated by growth among surviving startups. After 5 years of operation, for example, the aggregate employment in the startups we follow is about 60,000 jobs. Thus, the founder effect appears to be quite important for net job creation, at least in Norway.

17 The average ownership in Smith *et al.* (2019) is 65.7%, while we focus on all initial owners, not only large ones. In Online Appendix Table A.3, we calculate effects of founder death for majority founders, where the average ownership share is similar to in Smith *et al.* (2019). Our reported coefficient is –0.793, which translates into a 55% drop in profitability, that is, larger than for our main sample. On the other hand, their analysis is of mature firms, and our results are weaker for more mature firms in our sample as shown in Table IV. Note also that the Smith *et al.* (2019) sample firms are considerably larger than us. As shown in Table III, however, it is unclear whether the profitability effects are stronger for larger firms in our sample.

18 See http://www.kommuneprofilen.no/Profil/Sysselsetting/DinRegion/syss_sektor.

4.2 Startup Size

Table II establishes large average effects, but does not say anything about how founder death affects different parts of the startup size distribution. It is possible that the results in Table II are explained by tiny startups closing down rather than by effects on larger startups, for example.

In Table III, panel A, we interact the treatment dummy with dummies for deciles of initial employment. As the first five deciles contain tiny startups (one to two employees), these are collapsed into one dummy. Panel B of Table III gives the mean firm size and the standard deviation in each of the six groups. For example, the ninth decile has from six to nine initial employees, and the tenth decile has from nine initial employees and upward.

For small startups, with less than four employees (i.e., up to and including the sixth decile), founder death strongly impacts firm survival; when the entrepreneur dies these firms have much larger probability of closing down than the control firms. For larger startups, there are small effects on survival but large results on firm growth. For example, in the tenth decile, the estimated effect on survival is about 3% and statistically insignificant, while the effect on sales is about 54% and on employment about 45%. The results on profitability are statistically significant for two out of six categories, and appear less monotonic as we move up the size distribution.

These results show that founder death has large effects on both small and larger startups. As (unreported) robustness checks, we have tried several different specifications, such as using employment level in the year before founder death rather than initially, and used assets instead of employment as startup size measure. The results are qualitatively very similar.

To delve more into how founder death affects higher quality startups, we let the outcome be whether a firm in a given year exceeds the 70th, 80th, and 90th percentile of the employment and sales distribution within its age cohort.[19] Non-surviving firms get dummies equal to zero. We include startup-fixed effects, so the interpretation of the coefficients is whether founder death changes the probability of a startup exceeding these thresholds. The regression results, reported in Table III, panel C, tell us that there are large effects of founder death. For example, for employment, the probability of reaching the 80th percentile drops by 5 percentage points for treated firms, and by 2.4 percentage points for the 90th percentile. Table III, panel D, repeats the analysis, but defining percentiles across all firms in the same age cohort and two-digit sector. The results are very similar.

5. Other Forms of Heterogeneity

In this section, we explore further cross-sectional variation in the effects of founder death.

5.1 Founder Engagement

We analyze whether the effects of death depend on whether the entrepreneur works for the firm or not in the year prior to death. We interact the treatment effect in Equation (1) with a dummy for whether the entrepreneur had the firm as his main employer 1 year prior to death.[20] The results are reported in Table IV, panel A, and show that the negative effects of founder death are predominantly driven by entrepreneurs that are employed by their firm

19 For sales, the 70th, 80th, and 90th percentiles are 1.3 million NOK (150,000 USD), 3 million, and 7.5 million NOK, respectively, in our estimation sample.

20 Apart from the triple interaction term After $\times$ Treated $\times$ Work($t-1$), regressions also include interactions of After $\times$ Work($t-1$).

Table III. Matched sample, pooled across all ownership categories: size regressions

Standard errors in parentheses: *significance at 10%, **5%, ***1% level. Yearly panel of treated and matched control firms. Sample restricted to ±5 years around the (imputed) year of founder death. Fixed effects at the firm level. Observations from year of founder death excluded because not clearly attributable to "before" or "after" the event. [a]Control variables: year effects, firm age dummies, and the variable "After," that is, dummy = 1 for years after (imputed) year of founder death. Panel A interacts the treatment dummy with decile of employment in startup year. Panel B shows the corresponding mean and standard deviation of employment by decile. Panel C uses as dependent variable a dummy for whether a firm is above the 70th, 80th, and 90th percentile of employment or sales in its own age cohort, that is, among firms with the same startup year. Panel D uses as dependent variable a dummy for whether a firm is above the 70th, 80th, and 90th percentile of employment or sales in its own age cohort, that is, among firms with the same startup year and two-digit sector.

	(1) Firm active	(2) IHS (employees)	(3) IHS (wage sum)	(4) IHS (sales)	(5) IHS (assets)	(6) IHS (EBITDA)
Panel A. Treatment effect heterogeneity						
After × Treated × Deciles 1–5	-0.161^{***}	-0.105^{***}	-0.799^{***}	-1.132^{***}	-1.174^{***}	-0.542^{***}
	(0.020)	(0.037)	(0.146)	(0.165)	(0.150)	(0.179)
After × Treated × Decile 6	-0.105^{***}	-0.205^{**}	-0.831^{***}	-0.955^{***}	-0.603^{**}	0.049
	(0.038)	(0.081)	(0.307)	(0.347)	(0.300)	(0.370)
After × Treated × Decile 7	−0.047	-0.194^{**}	−0.311	−0.514	−0.324	−0.231
	(0.039)	(0.095)	(0.311)	(0.349)	(0.307)	(0.360)
After × Treated × Decile 8	-0.058^{*}	-0.345^{***}	-0.719^{**}	-0.732^{**}	-0.567^{**}	-0.822^{**}
	(0.035)	(0.091)	(0.292)	(0.318)	(0.283)	(0.323)
After × Treated × Decile 9	−0.060	-0.441^{***}	-0.828^{***}	-0.815^{**}	-0.643^{**}	−0.593
	(0.040)	(0.108)	(0.320)	(0.359)	(0.311)	(0.384)
After × Treated × Decile 10	−0.026	-0.597^{***}	-0.914^{***}	-0.777^{**}	-0.659^{*}	−0.228
	(0.039)	(0.132)	(0.351)	(0.395)	(0.338)	(0.370)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.21	4.24	4.99	5.01	1.92
R-squared	0.72	0.78	0.74	0.75	0.77	0.45
Observations	21,546	21,344	21,546	21,546	21,546	21,546

	(1) Employment mean	(2) Standard deviation
Panel B. Distribution of startup sizes		
Deciles 1–5	1.509	0.566
Decile 6	3.054	0.022
Decile 7	3.976	0.282
Decile 8	5.321	0.510
Decile 9	7.679	1.050
Decile 10	21.45	23.210

	(1) Pr(Empl > F_{70})	(2) Pr(Empl > F_{80})	(3) Pr(Empl > F_{90})	(4) Pr(Sales > F_{70})	(5) Pr(Sales > F_{80})	(6) Pr(Sales > F_{90})
Panel C. Staying on top of the distribution? (by firm age cohort)						
After × Treated	-0.078^{***}	-0.050^{***}	-0.024^{**}	-0.082^{***}	-0.051^{***}	-0.026^{***}
	(0.015)	(0.014)	(0.011)	(0.015)	(0.014)	(0.010)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.40	0.28	0.15	0.41	0.29	0.15
R-squared	0.04	0.02	0.00	0.04	0.02	0.01
Observations	21,546	21,546	21,546	21,546	21,546	21,546
Panel D. Staying on top of the distribution? (by firm age cohort and two-digit sector)						
After × Treated	-0.056^{***}	-0.045^{***}	-0.017^{*}	-0.069^{***}	-0.043^{***}	-0.030^{***}
	(0.015)	(0.013)	(0.009)	(0.014)	(0.013)	(0.009)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.30	0.21	0.10	0.32	0.22	0.11
R-squared	0.01	0.01	0.00	0.01	0.01	0.01
Observations	21,546	21,546	21,546	21,546	21,546	21,546

prior to death. Even though a founder works for the firm or not is likely to be endogenous to his health, and by extension treatment, this result reinforces the idea that entrepreneur engagement is a critical factor to young firms.

An alternative way to explore the role of entrepreneurial engagement is via ownership shares. We defined an entrepreneur as a person with at least 10% ownership of the total shares in a newly established limited liability firm. If larger ownership shares reflect, on average, larger actual engagement in the firm, we would expect the death of an owner with a larger ownership share to be more detrimental to the firm than for someone who owns less. Online Appendix Table A.3 splits the results into three panels: majority owners, 50% owners, and minority owners, and shows that this is indeed the case. The results are strongest for majority owners, somewhat weaker for 50% owners, and weakest, although still significant for minority owners.[21]

Size of the ownership fraction is a proxy for founder engagement but can also reflect other characteristics. One possibility could have to do with access to capital: A larger ownership fraction could reflect higher initial wealth, and such a founder could be important as a source of liquidity. In Online Appendix Tables A.8–A.10, we summarize founder characteristics, broken down by whether the founder is a majority owner, a 50% owner, or a minority owner. We find surprisingly small differences on personal wealth (prior to starting up the firm) and other characteristics, which does not lend much support the liquidity explanation. One interesting possibility is that a higher ownership fraction is a proxy for that person being the "creative force" behind the firm. There is to our knowledge currently no easy way to test this conjecture as we do not have a suitable proxy for creativity. For example, we do not have access to data on patenting activity. To lack a good proxy for creativity is not just a shortcoming of the current paper, but the whole entrepreneurship literature.

5.2 Family Firms

Part of the explanation for the strong effects of founder death could be that post-death, the control of the firm is transferred to less competent family members. Perez-Gonzalez (2006), Bennedsen *et al.* (2007), and Bertrand *et al.* (2008) document negative effects on performance from family CEO appointments inside mature firms. In order to investigate how family firms fare facing founder death, we interact the treatment dummy in Equation (1) with a family firm dummy. The family firm dummy equals one if that founder has at least one other founder who is a family member: about 12% of the founders are in family firms by this definition.

In panel B of Table IV, we find evidence that family firms are somewhat more resilient to the death of the founder. Throughout, effects on firm performance are roughly half in size for family firms compared with non-family firms in terms of point estimates. Note that part of this finding is mechanic: we showed in Online Appendix Table A.3 that effects are stronger for larger ownership shares, and, by definition, 100% owners have no co-owners.

21 We should caution that from an optimal contracting viewpoint, ownership fractions are endogenous: with more frictions (moral hazard or asymmetric information), the entrepreneur will have more problems raising funding from other parties, and will be "stuck" with a higher ownership fraction. Thus, ownership fractions can be correlated with the amount of frictions, which would be an alternative explanation for these results. Empirically, endogenous ownership fractions are arguably less of a concern because the use of outside equity is not common at inception.

Table IV. Matched sample, pooled across all ownership categories: heterogeneity

Standard errors in parentheses: *significance at 10%, **5%, ***1% level. Yearly panel of treated and matched control firms. Sample restricted to ±5 years around the (imputed) year of founder death. Fixed effects at the firm level. Observations from year of founder death excluded because not clearly attributable to "before" or "after" the event. [a]Control variables: year effects, firm age dummies, and the variable "After," that is, dummy = 1 for years after (imputed) year of founder death, as well as the appropriate interaction term between "After" and the variable(s) of interest, for example, "After" × Work($t-1$) in Panel A. Panel A interacts founder death with a dummy for owner being employed at firm in year before founder death. Panel B interacts founder death with a dummy for family firms (i.e., family member as co-owner). Panel C interacts founder death with dummies for different categories of firm age at founder death.

	(1) Firm active	(2) IHS (employees)	(3) IHS (wage sum)	(4) IHS (sales)	(5) IHS (assets)	(6) IHS (EBITDA)
Panel A. Founder employed at firm in ($t-1$)						
After × Treated	−0.053***	−0.133***	−0.378***	−0.506***	−0.479***	−0.426**
	(0.019)	(0.043)	(0.141)	(0.158)	(0.143)	(0.166)
After × Treated × Owner works in firm in ($t-1$)	−0.161***	−0.302***	−1.175***	−1.300***	−1.093***	−0.228
	(0.035)	(0.082)	(0.273)	(0.310)	(0.273)	(0.339)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.21	4.24	4.99	5.01	1.92
R-squared	0.72	0.78	0.74	0.75	0.77	0.45
Observations	21,546	21,344	21,546	21,546	21,546	21,546
Panel B. Family firm						
After × Treated	−0.125***	−0.264***	−0.858***	−1.073***	−0.938***	−0.545***
	(0.018)	(0.043)	(0.142)	(0.160)	(0.142)	(0.173)
After × Treated × Family firm	0.068*	0.112	0.398	0.545*	0.299	0.455
	(0.038)	(0.085)	(0.282)	(0.317)	(0.282)	(0.341)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.21	4.24	4.99	5.01	1.92
R-squared	0.72	0.78	0.74	0.75	0.77	0.45
Observations	21,546	21,344	21,546	21,546	21,546	21,546
Panel C. Heterogeneity by firm age						
After × Treated × Firm Age 0 or 1	−0.194***	−0.366***	−1.206***	−1.308***	−1.374***	−0.813
	(0.052)	(0.107)	(0.412)	(0.461)	(0.388)	(0.560)
After × Treated × Firm Age 2 or 3	−0.142***	−0.285***	−0.879***	−1.267***	−1.042***	−0.781**
	(0.036)	(0.089)	(0.298)	(0.321)	(0.280)	(0.346)
After × Treated × Firm Age 4 or 5	−0.117***	−0.251***	−0.786***	−0.981***	−0.876***	−0.439
	(0.031)	(0.068)	(0.238)	(0.267)	(0.244)	(0.290)
After × Treated × Firm Age 6 or 7	−0.098***	−0.248***	−0.696***	−0.906***	−0.883***	−0.677**
	(0.032)	(0.071)	(0.241)	(0.270)	(0.243)	(0.287)
After × Treated × Firm Age 8 or higher	−0.092***	−0.196***	−0.700***	−0.803***	−0.737***	−0.223
	(0.020)	(0.048)	(0.156)	(0.178)	(0.157)	(0.186)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.58	1.21	4.24	4.99	5.01	1.92
R-squared	0.72	0.78	0.74	0.75	0.77	0.45
Observations	21,546	21,344	21,546	21,546	21,546	21,546

We also investigate ownership changes in the aftermath of entrepreneur death.[22] We classify ownership into two categories, the entrepreneur and family members, and outsiders. Outsiders could be either individuals that are non-family member, or corporate owners. In Online Appendix Figure A.3, we graph how ownership evolves for these two categories in event time. We see that even 5 years after founder death, the family still owns more than a third of the company on average, starting from just over half in the years before founder death. So there is no doubt that the family plays an important role for the surviving firms.

5.3 Firm Age

In the theoretical framework of Rajan (2012), a firm becomes less reliant on the entrepreneur as it ages. To investigate this hypothesis, we interact the founder death dummy in Equation (1) with dummies for firm age when the founder dies. Table IV, panel C, shows that the results are declining in firm age, but strong even for the more mature startups.[23] Thus, we find support for Rajan (2012). The result that founder death has a very large effect even when the firm has reached a more mature phase suggests that financing constraints and underinvestment of the type described by Hart and Moore (1994) may be present for a long time in a firm's life.

Overall, our estimates of the effects of founder death vary in predictable ways with firm characteristics, but are throughout negative. Bruce Johnson *et al.* (1985); Perez-Gonzalez (2006); and Jenter, Matveyev, and Roth (2018) report a positive stock market response of the death of some types of CEOs, particularly aging founders, an effect that is likely because old founders are "entrenched" and unwilling to relinquish control of the firm. Quite likely, the reason why we fail to find positive effect of founder death is that the firms in our sample have not reached an age where the founder is entrenched.

6. Leadership Transition

Entrepreneurs have two roles: they found the company and provide its blueprint, and typically manage the firm. It is possible that the results of Sections 4.1 and 5 are not due to entrepreneur specialness, but rather detrimental effects of abrupt leadership transition and the costs of adapting to a new leader. In order to address this question, we employ data that provide the end-of-year identity of the firm's CEO. The idea behind the analysis is simple: if entrepreneur specialness drives the results, we would expect the negative effects of entrepreneur-CEO death to be larger than for professional CEOs. Conversely, if professional CEO death yields equally strong results, this would suggest that our results are predominantly due to leadership transition.

In this section, we also include a brief descriptive analysis on who becomes CEO after an entrepreneur-CEO dies.

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

23 Note that here, we use an asymmetric interval of –5 to + 2 years around founder death, to make sure both younger and older firms have a comparable post-death period. Yet, results are quite similar when using our standard symmetric 5-year window around founder death.

6.1 Comparing Shocks

As in the main analysis, we use propensity score matching to identify a matched control group of firms where the CEOs do not decease. As CEO identity can change over time, we perform matching at the firm-year level and find comparable firms in the year before CEO death. We require a matched control to be started up in the same year and have the same firm age as the treated firm (this implies that treated and matched control will be measured in the same calendar year). We also require exact matching on CEO type, that is, we match founder-CEOs to founder-CEOs and non-founder CEOs to non-founder CEOs. Appendix A contains more details on the matching procedure. In the regressions, we include the same fixed effects as in the main analysis.

Online Appendix Table A.4 presents descriptive statistics. There are 372 CEOs in both the treated and the matched control groups. About half are entrepreneur-CEOs and half are professional CEOs. The treated and matched control groups are very similar in terms of mean CEO characteristics and mean startup characteristics.

Panel A of Table V presents the main results. Both the death of an entrepreneur-CEO and a professional CEO have a large negative effect on startup outcomes, but the death of an entrepreneur-CEO entails an about 50% larger effect than the death of a professional CEO.

Panel B performs the same regression, but on startups with at least five employees during the first 2 years. Here, the difference (in percentage terms) is even larger between entrepreneur-CEOs and professional CEOs.

Panel C repeats the regression from Panel A, but breaks the coefficient for entrepreneur-CEOs into three separate coefficients, for majority owners, 50% owners, and minority owners. Here, the coefficient for professional CEOs is similar to the coefficient for minority owners, but much lower than the coefficients for 50% and majority owners.

To conclude, we find that the effects of death of CEO-entrepreneurs are economically and statistically much stronger than for professional CEOs. The results from previous sections thus appear to be largely driven by the specialness of the entrepreneur, and not by leadership transition of young firms.

6.2 CEO Succession

The CEO data also allow us to analyze the succession decision, that is, who becomes CEO after the entrepreneur-CEO dies, conditional on firm survival. Though firm survival is, of course, endogenous, it is of interest to understand more about the differences in new versus old CEO characteristics such as origins and experience.

In Online Appendix Table A.7, panel A, we focus on the subset of treated entrepreneur-CEOs of Table V where the firm survives the CEO death event and gets a new CEO (this leaves us with about half of the original sample of entrepreneur-CEOs). On sociodemographic characteristics, the new CEOs are substantially younger than the previous CEOs, more than 6 years on average. They are also more likely to be female (29% for new CEOs versus about 11% for old CEOs), even among those that are "external" hires (23%). Interestingly, the new CEO earns substantially more than the old CEO, almost 30%, which is consistent with the CEO having higher ability than the old CEO. On the other hand, the lower wage paid to the old CEO could reflect a "lock-in" effect for that person (i.e., wage payments less than marginal productivity due to moving costs), or, in view of the fact that the old CEO has substantially larger ownership fraction than the new CEO (62% versus

Table V. Matched sample, CEO analysis: effect of CEO death on firm performance

Standard errors in parentheses: *significance at 10%, **5%, ***1% level. Yearly panel of treated and matched control firms. Sample restricted to ±5 years around the (imputed) year of founder death. Fixed effects at the firm level. Observations from year of founder death excluded because not clearly attributable to "before" or "after" the event. [a]Control variables: year effects, firm age dummies, and the variable "After," that is, dummy = 1 for years after (imputed) year of founder death.

	(1) Firm active	(2) IHS (employees)	(3) IHS (wage sum)	(4) IHS (sales)	(5) IHS (assets)	(6) IHS (EBITDA)
Panel A. Full CEO sample						
After × Treated	−0.254***	−0.453***	−1.788***	−2.233***	−1.825***	−1.144***
	(0.035)	(0.072)	(0.258)	(0.290)	(0.257)	(0.318)
After × Treated × Entrepreneur-CEO	−0.130**	−0.393***	−1.522***	−1.370***	−1.271***	−1.753***
	(0.056)	(0.129)	(0.432)	(0.483)	(0.415)	(0.641)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.81	1.53	5.84	6.96	6.74	3.22
R-squared	0.61	0.75	0.67	0.67	0.68	0.43
Observations	5,610	5,531	5,610	5,610	5,610	5,610
Panel B. CEO sample with more than five employees at foundation						
After × Treated	−0.086	−0.272*	−0.496	−0.810	−0.671	−1.832***
	(0.061)	(0.155)	(0.504)	(0.556)	(0.474)	(0.584)
After × Treated × Entrepreneur-CEO	−0.170*	−0.684**	−2.502***	−1.970**	−1.495*	−1.419
	(0.100)	(0.272)	(0.814)	(0.942)	(0.789)	(1.172)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.85	2.32	7.00	8.04	7.54	3.97
R-squared	0.57	0.69	0.63	0.63	0.66	0.46
Observations	1,723	1,700	1,723	1,723	1,723	1,723
Panel C. CEO sample: by ownership share						
After × Treated × Entrepreneur-majority CEO	−0.370***	−0.663***	−2.622***	−3.173***	−2.802***	−1.925***
	(0.043)	(0.092)	(0.317)	(0.360)	(0.334)	(0.390)
After × Treated × Entrepreneur-50% CEO	−0.270***	−0.538**	−2.318***	−2.756***	−1.790***	−1.275*
	(0.085)	(0.218)	(0.809)	(0.788)	(0.671)	(0.729)
After × Treated × Entrepreneur-minority CEO	−0.199*	−0.464***	−1.577**	−2.159**	−1.486**	0.004
	(0.103)	(0.174)	(0.701)	(0.879)	(0.751)	(0.929)
After × Treated × Professional CEO	−0.130***	−0.188**	−0.781***	−1.091***	−0.788***	−0.256
	(0.039)	(0.080)	(0.282)	(0.322)	(0.283)	(0.348)
Controls[a]	✓	✓	✓	✓	✓	✓
Mean Dep. Var.	0.81	1.54	5.87	7.00	6.77	3.21
R-squared	0.59	0.75	0.66	0.66	0.68	0.42
Observations	6,354	6,275	6,354	6,354	6,354	6,354

30%), it could reflect that the old CEO had a preference for reinvesting surplus in the firm rather paying out a higher salary.

Almost half of the new CEOs, 43%, have a close prior connection to the firm, in being either a co-founder, spouse, or family member (child, sibling, or parent) of the deceased

CEO, or an employee prior to the death event. Among these “internal” successions, a family link is most frequent: about 58% (25%/43%) are either spouse or a family member, and about 44% (19%/43%) are a co-founder. There do not appear to be large post-transition performance differences: the mean survival rate is 79% for internal transitions and 75% for external transitions (not reported in table).

In Online Appendix Table A.7, panel B, we focus on the subset of treated professional CEOs of Table V, where the firm survives the CEO death event and gets a new CEO. On most dimensions, the differences are small. One difference that stands out is personal wealth: the new CEO in professional-CEO firms have about NOK 4.0 million in wealth on average, while the average for entrepreneur-CEO firms is about NOK 2.5 M. Likely, this greater wealth is associated with easier access to capital. Another difference that stands out is the fraction of new CEOs that are co-founders: In the event of an entrepreneur-CEO dying, 19% of new CEOs are co-founders, and in the event of professional-CEO dying, 49% of the new CEOs are founders. The reason is likely quite prosaic: When a professional-CEO dies, a founder is more likely to be alive and take over management of the firm. This is also likely the reason why incoming CEOs in this case are wealthier, on average.

In terms of average tenure for the incoming CEO, there is not much difference between firms that were previously run by entrepreneur-CEOs (3.6 years) and firms previously run by professional CEOs (4.1 years). Likewise, previous experience, proxied by age, is minimal: the difference in age between new CEOs hired to replace entrepreneur-CEOs and founder-CEOs is just 1.7 years.

7. Conclusion

We have used deaths of nearly 1,500 entrepreneurs as a source of plausibly exogenous variation, and find large and sustained effects on firm performance across the firm size distribution. For small startups, the effects go mainly via firm survival, while for larger startups the effects are mainly on firm growth. We find larger effects of entrepreneur death for younger firms, but the effects are still very large for startups that are more than 8 years old. The effects appear to be driven by entrepreneur specialness rather than leadership transition: the effects of death of entrepreneur managers are economically and statistically stronger than the death of managers that are not entrepreneurs.

These results point to entrepreneurs playing a large and unique role not previously documented. Much of the existing evidence in favor of the importance of entrepreneurs is based on comparing environments with high versus low entrepreneurship rates (e.g., Acs *et al.*, 2009). However, these findings are open to several interpretations. A key contribution of our analysis is to directly measure the impact of entrepreneurs, and to show that it is large even compared with managers.

Supplementary Material

Supplementary data are available at *Review of Finance* online.

Appendix A: Matching

This appendix describes the procedure we use to match “treated” firms (firms where the founder dies) with similar “control” firms (where the founder does not die).

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, that is, the variables shown in Table I. Characteristics measured at a later point, for example, in the year before founder death, might already be subject to endogeneity bias because of the foreshadowing of (later) founder death. We use nearest neighbor matching to select those firms in the control group whose *ex ante* probability of experiencing founder death ("propensity score") is closest to that of the firms where the founder dies.

To estimate the propensity score, we run a probit model of founder death on the characteristics from Table I.[24]

Ex ante, the treated make up only around 2% of our sample. Based on the estimated propensity score, we use nearest-neighbor matching (without replacement) to combine treated and control observations. We impose a caliper (i.e., radius) of 0.05, that is, 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, we lose only a tiny fraction of treated (less than 1% among treated majority owners). 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. Furthermore, we match within ownership category, that is, we match within samples of majority owners, 50% owners, and minority owners.

In line with the differences detected in Table I between treatment and control group, the pre-treatment characteristics have substantial explanatory power in predicting founder death. Online Appendix Table A.5 shows that the pseudo-R^2 is 0.19. The variables entering the propensity score estimation are jointly significant at the 1% level.[25] 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. To formally test this, we re-run the same propensity score specification on the matched sample, that is, on the sample of treated and *matched* controls. After matching, the pseudo-R^2 drops to 0.01 (from the 0.19 reported in Online Appendix Table A.5). Similarly, the variables entering the propensity score are no longer jointly significant, with a *P*-value of 0.999.[26] Matching thus appears to be very successful

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

25 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. The median absolute standardized bias before matching is 7.6. Rosenbaum and Rubin (1985) suggest that a value of 20 is "large," that is, in line with the other two indicators above, treated and control groups do differ *ex ante*, even if not dramatically so.

26 The median absolute standardized bias drops from 7.6 before matching to 2.7 after matching.

at reducing (or even removing) differences in observable pre-treatment characteristics. In other words, our matched sample consists of firms where the founder dies and a set of "twin firms" who are *ex ante* observationally identical, but where the founder does not die. We consider the matched control group as a useful comparison group that approximates the counterfactual outcome of the treated firms.

Appendix B: Additional Theoretical Discussion

Neoclassical models view entrepreneurs as homogeneous inputs in the production process, and substitutable once a firm has been founded. For example, in Kihlstrom and Laffont (1979), the entrepreneur bears residual risk but does not contribute to firm performance. In sorting models (e.g., Lucas, 1978; Evans and Jovanovic, 1989; Lazear, 2005), individuals with high entrepreneurial ability become entrepreneurs, while individuals with low entrepreneurial ability become workers. Entrepreneurial ability may include the ability to spot previously unrecognized opportunities, as in Kirzner (1997). 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. The same applies in the theory of the firm, as in Hart (1995). 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. Or, as stated by Aldrich and Ruef (2006, p.107), "For young companies, personnel instability disrupts organizational knowledge if it still resides in individuals rather than in rules and routines. By contrast, if an organization has evolved to the stage where established routines and competencies codify selection criteria, turnover may have little effect on performance. Indeed, turnover may actually increase opportunities for learning in established organizations by disrupting existing patterns of communication and bringing in new knowledge."

One theoretical tradition that underpins the non-substitutability assumption of Hart and Moore (1994) is critical resource theory (Wernerfelt, 1984; Rajan and Zingales, 1998, 2001), where a firm is a set of specific investments built around a critical resource or resources. In the current context, the entrepreneur's human capital, personality, and ideas can be seen as the critical resource which the firm is initially organized around (this is a sense in which the entrepreneur shapes the production function of the firm). The entrepreneur then invests in physical and human assets that are complementary to himself, and may not be fully substitutable because other individuals lack his combination of traits. Under this view, the entrepreneur can have two effects on firm performance. The first is the direct effect through own productivity, and the second, which works via providing the critical asset, is positive spillover effects on the other assets of the firm. Critical resource theory says less about for how long the entrepreneur is essential. One reason to be concerned about this question is that the duration of non-substitutability influences how long firms are financially constrained and subject to underinvestment. Our methodology allows us to analyze this question.

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