



Understanding the relationship between founder–CEOs and firm performance [☆]

Renée Adams ^a, Heitor Almeida ^{b,*}, Daniel Ferreira ^c

^a University of Queensland, Australia and ECGI, Belgium

^b University of Illinois, United States and NBER, United States

^c London School of Economics, United Kingdom, CEPR, United Kingdom and ECGI, Belgium

ARTICLE INFO

Article history:

Received 8 August 2007

Received in revised form 22 May 2008

Accepted 22 May 2008

Available online 10 July 2008

JEL classification:

G34

Keywords:

Founder–CEOs

CEO turnover

Instrumental variables

Family firms

Endogeneity

ABSTRACT

We use instrumental variables methods to disentangle the effect of founder–CEOs on performance from the effect of performance on founder–CEO status. Our instruments for founder–CEO status are the proportion of the firm's founders that are dead and the number of people who founded the company. We find strong evidence that founder–CEO status is endogenous in performance regressions and that good performance makes it less likely that the founder retains the CEO title. After factoring out the effect of performance on founder–CEO status, we identify a positive causal effect of founder–CEOs on firm performance that is quantitatively larger than the effect estimated through standard OLS regressions. We also find that founder–CEOs are more likely to relinquish the CEO post after periods of either unusually low or unusually high operating performances. All in all, the results in this paper are consistent with a largely positive view of founder control in large US corporations.

© 2008 Elsevier B.V. All rights reserved.

1. Introduction

In this paper we try to understand the nature of the relationship between founder–CEOs and firm performance. Unlike most of the previous literature, we take the endogenous nature of the founder's status as CEO seriously. We propose an instrumental variables approach to disentangle the effect of founder–CEOs on performance from the effect of performance on founder–CEO status. Our analysis suggests that founder–CEOs improve market valuations and operating performances of their firms and that the status of the founder as CEO is endogenous in performance regressions. Furthermore, our evidence shows that not taking the endogeneity of founder–CEO status into account leads one to underestimate its effect on performance. A likely explanation for this finding is that the average effect of performance on founder–CEO status is negative. In order to investigate this possibility further, we estimate the effect of past operating and market performance on changes in founder–CEO status. We find that founder–CEOs are more likely to relinquish the CEO post after periods of unusually high operating performances. In addition, unusually low operating performance also makes it more likely that the founder steps out, suggesting that founders are not inexorably entrenched.

Earlier research on the effects of founder–CEOs on operating performances and market valuations has produced mixed findings. Johnson et al. (1985) find a positive stock price reaction following the sudden death of a corporate founder. Morck et al. (1988) find a negative effect of founding family control on market valuations, but only for older firms. For the younger firms in their sample, the market value effect of having a member of the founding family as one of the top two executives is positive. Morck et al. (1998) find a negative correlation between heir control in Canadian firms and firm performance. Anderson and Reeb (2003) provide evidence consistent with family firms having higher market valuations and better accounting performances than non-family firms. Recently,

[☆] We wish to thank an anonymous referee, Yakov Amihud, David Sraer, Daniel Wolfenzon, and seminar participants at the CEPR/ECGI/INSEAD/NBER/University of Alberta joint conference on "The Evolution of Corporate Governance and Family Firms" at INSEAD, the 2004 European Meeting of the Econometric Society, and the 2004 Istanbul Conference on "Family Firms and Corporate Governance" for comments and suggestions. We also thank Victoria Ivashina, Kabir Kocnar, Michael Rodriguez and Adrienne Rumble for excellent research assistance. All remaining errors are our own.

* Corresponding author. 1206 south sixth street, Champaign IL61820, United States.

E-mail addresses: r.adams@business.uq.edu.au (R. Adams), halmeida@uiuc.edu (H. Almeida), d.ferreira@lse.ac.uk (D. Ferreira).

a new wave of research on the topic has arisen, with a focus on refining the evidence from the previous studies. One strand of this new generation of papers focuses on inherited control. The evidence from the US (Pérez-González, 2006) and Denmark (Bennedsen et al., 2007) is consistent with the original findings by Morck et al. (1998): inherited control by a family member is associated with a decline in firm performance. In contrast, Sraer and Thesmar (2007) find not only that family control is positively related to performance, but also that even heir-controlled family firms have better performances in France.

Another strand of this recent literature focuses on the effects of founder control on performance. In research contemporaneous to this paper, Fahlenbrach (in press), Palia et al. (in press), and Villalonga and Amit (2006) all find a positive relation between founder-CEOs and firm performance. While a natural process of mutual influence has produced some ideas and findings that are shared by all papers on this topic, our paper differs from these mainly in our identification strategy. Thus, our main contribution to this literature is our focus on the importance of endogeneity. We believe that this contribution has already had a positive effect on these contemporaneous papers. We also present some unique findings, especially with respect to the effect of firm performance on founder-CEO turnover.

In regressions of market valuations and return on assets on founder-CEO status and other controls, we propose two instruments for the founder-CEO status variable. The first is the proportion of the firm's founders who are dead. The second is the number of people who founded the company. We argue that these instruments plausibly satisfy the exclusion restriction for valid instruments, that is, they are unlikely to be related to performance other than through channels that we can control for in our empirical analysis. We estimate an endogenous dummy variable model of performance that takes into account the fact that the founder-CEO variable is binary. In this framework, we also provide evidence that our chosen instruments are significantly correlated with founder-CEO status.

Our primary sample consists of data on Fortune 500 firms over the 1992–1999 period, for which we could gather data on the proposed instruments. We find strong evidence that founder-CEO status is endogenous in performance regressions, which implies that the effect of founder-CEOs cannot be correctly estimated using ordinary least squares methods. After instrumenting for founder-CEO status, we find evidence consistent with a positive causal effect of founder-CEOs on firm performance. In addition, the endogenous dummy variable model allows us to provide evidence on the most likely direction of the effect of performance on founder-CEO status. Our evidence suggests that good performance *reduces* the likelihood that a founder retains the CEO title. This direction of reverse causality is compatible with the true effect of founder-CEOs on performance being larger than that estimated through OLS procedures.

Our finding that good performance makes it more likely that the founder is not in control is somewhat surprising in light of previous arguments concerning the endogeneity of founder-CEO status. Because the correlation between founder-CEOs and performance is positive in OLS regressions, the previous literature has emphasized endogeneity stories that could explain away this correlation. For example, Anderson and Reeb (2003) suggest that founder-CEOs could have superior inside information about the prospects of their firms. This could enable them to plan their departure from the firm when performance is likely to fall. This hypothesis is inconsistent with our findings.

In the last part of the paper we discuss alternative stories that can explain the negative effect of performance on founder-CEO status. The effect of good performance on founder-CEO departures might be due to a “controlled-succession” effect (Morck et al., 1989), whereby founders who wish to transfer control to their heirs can accomplish this more easily following good performance, or more simply to the fact that founders leave their companies only when they are in good shape (Wasserman's (2003) “paradox of entrepreneurial success”). Both these stories predict that founder-CEOs will step out after some period of consistently good performance. In addition, such a relationship might be linked to wealth effects: if CEOs want to retire when rich, they should be more willing to retire following good performance. Finally, it is possible that the effect of performance on founder-CEO status is driven by firms that perform badly, if for some reason founders are *more* likely to retain the CEO title in such firms. Such a relationship can be generated by bad governance, if firms with bad governance both perform poorly and are more likely to have a founder who is entrenched as the CEO.

To help differentiate among these stories, we examine the effect of past extreme performances on the likelihood that founders retain the CEO title. We find that unusually good past performance does increase the probability that founders step down. In addition, we find that unusually bad past performance also generates founder-CEO turnover. This finding helps reject the bad governance hypothesis. We find no support for the importance of wealth effects. These findings imply that the hypotheses that are most consistent with our evidence are the “controlled-succession” and the “paradox of entrepreneurial success” hypotheses.

We start in Section 2 by defining our problem formally. In Section 3 we describe our sample, which we use in Section 4 to examine OLS regressions of performance on founder-CEO status. In Section 5, we address the endogeneity of founder-CEO status. Section 6 provides further evidence on the relationship from performance to founder-CEO status, and Section 7 concludes the paper.

2. The endogenous dummy variable model

We start by formally defining our question. Suppose that we have a linear model in the population:

$$y = \alpha + \gamma f + \beta \mathbf{x} + u, \quad (1)$$

where the random variable y is a measure of firm performance, f is a binary random variable that takes the value of 1 if the CEO is one of the founders and zero otherwise, \mathbf{x} is a k -dimensional random vector of covariates, and α , γ and β are population

parameters. Under the assumption that $E[u|f, \mathbf{x}] = 0$, all parameters above can be consistently estimated by OLS applied to a random sample (y_i, f_i, \mathbf{x}_i) . The parameter of interest, γ , can be interpreted as the effect of founder–CEOs on performance for a randomly selected firm in the population.

Suppose now that we assume that $\text{Cov}(x_j, u) = 0$ for all $j = 1, \dots, k$ but $\text{Cov}(f, u) \neq 0$. In this case, we have an *endogenous dummy variable model* (Heckman, 1978). In such a model, OLS consistently estimates the slopes of the linear projection of y on $(1, f, \mathbf{x})$. Denote the projection slope on f by γ^s . Because now founder–CEO status is endogenous, in general we have that $\gamma^s \neq \gamma$. In other words, OLS will provide inconsistent estimates for the parameter of interest γ .

In order to get an intuitive grasp of the OLS inconsistency, assume for simplicity that all projection slopes but γ^s are zero.¹ Then,

$$\gamma^s = \gamma + \frac{\text{Cov}(f, u)}{\text{Var}(f)}. \quad (2)$$

The direction of the OLS inconsistency is determined by the sign of $\text{Cov}(f, u)$. For example, if founders are more likely to remain as CEOs when performance is good, we have that $\text{Cov}(f, u) > 0$ and the OLS estimator γ^{OLS} will over-estimate the true γ (asymptotically). Intuitively, if good performance causes founders to stay, the OLS estimate is misleadingly high, because the effect of performance on founder–CEO status is confounded with the effect of founder–CEOs on performance.

Previous research (e.g. Anderson and Reeb, 2003) has found a positive $\hat{\gamma}^{\text{OLS}}$ in regressions of firm performance on founder–CEO dummies and other controls. If the true value of γ is zero or even negative, a positive correlation between u and f could explain the positive founder effect one encounters in OLS regressions. To correctly interpret such performance regressions, it is thus crucial to investigate the sign and magnitude of $\text{Cov}(f, u)$, which is our goal in this paper.

3. Data description

Our primary sample consists of data from 1992–1999 on all publicly traded firms in the 1998 Fortune 500, excluding regulated financial firms and utilities, for which data are available on Standard and Poor's ExecuComp (2000). From ExecuComp we obtain the names of the sample firms' CEOs, CEO ownership and tenure as CEO, as well as some financial information. We gather the remaining financial information from Compustat, and the date of the firm's incorporation from Moody's Industrial Manuals (1999), proxy statements and annual reports for fiscal 1998. Our final sample consists of 2128 complete firm-years of data for 321 firms during the 1992–1999 time period.²

Since ExecuComp does not contain information on whether the CEO is also a founder, for all firm-years we checked whether the current CEO was one of the firm's founders in a variety of sources consisting of proxy statements, annual reports and the internet.³ We set *founderCEO* in a given year equal to 1 if any source explicitly named the current CEO as a founder or the main executive at the time the company began (including when it was spun-off).⁴

We use both a market-based measure of performance for our sample firms, a proxy for Tobin's Q , as well as an accounting measure, ROA. We define Tobin's Q to be the ratio of the firm's market value to its book value. The firm's market value is calculated as the book value of assets minus the book value of equity plus the market value of equity. We define ROA to be the ratio of net income before extraordinary items and discontinued operations to the book value of assets. Alternatively, we also use EBITDA instead of net income as the numerator for ROA.

We use our proxy for Tobin's Q as a measure of market valuation (scaled by the book value of assets), and not as a proxy for investment opportunities. *Holding the book value of assets constant*, maximizing Q is equivalent to maximizing the market value of the firm, which is considered the proper objective of the firm by most financial economists. Thus, in order for a Q regression to be properly interpreted as a valuation regression, we always include the book value of assets as a right-hand side variable.

We collected the data necessary to construct the instruments from a variety of sources including Lexis–Nexis, the International Directory of Company Histories (various volumes) and company histories on company websites, when available. In order to determine who the founders of the firms in our sample are, we first had to establish what the founding event of the firm in the form in which it appears in 1998 was (since our firms are taken from the 1998 Fortune 500 list). We consider the following four types of events to be founding events: a simple business start-up (e.g. a shop opening), a merger of equals, a spin-off of a division that was not previously a separate company that had been acquired, and a major change in ownership, e.g. an LBO, MBO or other acquisition,

¹ If not all projection slopes but γ^s are zero, we have that $\gamma^s - \gamma = \frac{b}{a} \text{Cov}(f, u)$, where b is the determinant of the covariance matrix of (f, \mathbf{x}) and a is the determinant of the covariance matrix of \mathbf{x} . Because these values are positive, the sign of the OLS inconsistency is still governed by the sign of $\text{Cov}(f, u)$. If some of the control variables in \mathbf{x} are also endogenous, however, then it is not possible to sign the OLS inconsistency in general.

² We lose 440 firm-years, mostly due to missing data in either ExecuComp, CRSP, or one of the sources that we use to collect data on founders. Because of our sampling procedure, there are no bankruptcies or M&A in our sample from 1992 to 1998 (firms could not have disappeared from the sample before 1998). We lose 33 firms in 1999. 11 of those were delisted in CRSP due to acquisitions in 1999 or 2000, and one firm was dropped from CRSP. The remaining 21 observations are missing because of missing data. There were no bankruptcies in 1999.

³ When we could find the name of the firm's original founders this procedure was straightforward. However, very few proxies, annual reports or company websites disclosed the names of the original founders. We were most successful conducting searches using the Google search engine.

⁴ We assigned a value of zero to the founderCEO dummy if the firm was incorporated at least 64 years prior to the current year. The longest period of time that a CEO has been working for his firm in the unrestricted Fortune 500 sample (before dropping firm-years with missing data) is 59 years. We set our cutoff of 64 (=59+5) years to account for possible missing data on CEO firm tenure. Since most firms are founded several years prior to the date of incorporation, our procedure ensures that we check more CEOs than are likely to be founders.

Table 1
Summary statistics

Variable	Mean	S.D.	Min.	Max.	<i>t</i> -stat diff.
<i>Entire sample (321 firms)</i>					
Q	2.04	1.15	0.92	9.99	
Log Q	0.58	0.40	-0.08	2.27	
ROA (net income/assets)	5.52	4.48	-15.94	22.44	
Founder CEO (dummy)	0.13	0.32	0.00	1.00	
CEO ownership (% of total shares)	0.02	0.05	0.00	0.40	
CEO tenure (number of years)	7.19	5.71	0.00	33.86	
CEO equity-based pay (fraction of total pay)	0.47	0.19	0.00	0.97	
Volatility (yearly volatility)	0.30	0.11	0.13	0.79	
Firm assets (log of assets in million dollars)	8.64	1.02	6.44	12.49	
Firm age (log years)	3.69	1.00	0.35	4.96	
Fraction dead founders	0.70	0.43	0.00	1.00	
Number founders	1.81	1.13	1.00	8.00	
<i>Founder-CEO sample (50 firms)</i>					
Q	2.58	1.62	1.02	7.68	
log Q	0.75	0.50	0.02	2.01	3.21
ROA (net income/assets)	6.66	6.05	-8.82	22.44	1.96
Fraction years with founder CEO (fraction of total years)	0.82	0.27	0.25	1.00	NA
CEO ownership (% of total shares)	0.05	0.09	0.00	0.40	6.36
CEO tenure (number of years)	12.01	8.68	1.00	33.96	6.96
CEO equity-based pay (fraction of total pay)	0.46	0.24	0.00	0.83	0.39
Volatility (yearly volatility)	0.39	0.12	0.21	0.72	7.11
Firm assets (log of assets in million dollars)	8.20	0.78	6.64	10.28	3.37
Firm age (log years)	2.64	0.92	0.35	4.32	9.10
Fraction dead founders	0.11	0.23	0.00	1.00	12.91
Number founders	2.28	1.37	1.00	7.00	3.28
<i>Non-founder-CEO sample (271 firms)</i>					
Q	1.94	1.02	0.92	9.99	
log Q	0.55	0.37	-0.08	2.27	
ROA (net income/assets)	5.31	4.10	-15.94	22.36	
CEO ownership (% total shares)	0.01	0.03	0.00	0.23	
CEO tenure (number of years)	6.30	4.46	0.00	24.50	
CEO equity-based pay (fraction of total pay)	0.48	0.18	0.00	0.97	
Volatility (yearly volatility)	0.28	0.09	0.13	0.79	
Firm assets (log of assets in million dollars)	8.72	1.04	6.44	12.49	
Firm age (log years)	3.88	0.80	0.60	4.96	
Fraction dead founders	0.81	0.37	0.00	1.00	
Number founders	1.72	1.06	1.00	8.00	

Sample consists of 321 publicly traded, non-regulated firms from the 1998 Fortune 500 that were available on ExecuComp (2000) during the years 1992–1999. Most financial and CEO data are from ExecuComp (2000). Firm age is collected from Moody's Manuals (1999), proxy statements and 10-Ks for fiscal 1998. Founder data are from a variety of sources consisting of proxy statements, annual reports and the internet. Tobin's Q is defined as (book value of assets-book value of equity + market value of equity) / book value of assets. ROA = net income before extraordinary items and discontinued operations / book value of assets. Founder CEO is equal to one if the CEO is a founder of the company. CEO ownership is defined as the ratio of the number of shares owned by the CEO after adjusting for stock splits to total shares outstanding. CEO equity-based pay is the value of annual option pay divided by the sum of salary, bonus and annual option pay. Volatility is the Black-Scholes volatility as reported in ExecuComp. Firm age is the number of years since the firm's first date of incorporation. Fraction of dead founders is the fraction of the firm's founders who are dead prior to the start of the sample period. Number of founders is the number of people founding the firm. All statistics are firm-level averages. The top panel reports summary statistics for the entire sample of which 260 firms were founded by start-ups, 32 by merger of equals, 16 through major ownership changes and 13 through spin-offs. The middle (bottom) panel reports statistics for firms that were (not) run by founders at some point during the sample period. The fourth column of the table reports *t*-statistics for a test that the sample averages are the same across the founder-CEO and non-founder-CEO samples.

that leads to a major change in the development of the company. In the case of a merger of equals, we consider the founders of the new company to be the founders of both firms that are merging. In the case of a spin-off we consider the founders to be the founders of the original company, as well as the CEO at the time of the spin-off if his name is explicitly mentioned in our data source. This indicates that the CEO is more likely to be important for the development of the company. If a company was acquired and spun-off again, we consider the founders to be the founders of the company pre spin-off. We also generally consider any person to be a founder of the company who is identified as such in any of our data sources. In some cases our sources also identify important investors in the company or the first CEO who was hired by a founder as founders.

We used company descriptions in the International Directory of Company Histories and company histories in Hoover's Company Profile Database, as well as information on the founders of the 1992 Fortune 200 firms in the [National Commission on Entrepreneurship's \(2001\)](#) study on entrepreneurs, as a starting point for identifying the founding event, and if possible, the names of the founders. This procedure worked better for firms that were founded recently than for older firms that had undergone several mergers or restructurings. Generally, older firms tended to have company histories on their websites that we could use to identify what the firm considers to be its main founding event. Once we identified the founding events, we searched archived stories from

Table 2

OLS regressions of firm performance on founder–CEO status

Independent variables	Dependent variable			
	log Q		ROA	
	(I)	(II)	(III)	(IV)
FounderCEO	0.242*** (3.31)	0.185*** (2.58)	2.770*** (2.91)	1.751* (1.91)
ln(assets)	-0.025 (-1.12)	-0.026 (-1.18)	-0.519** (-2.19)	-0.470** (-2.03)
ln(age)	-0.037 (-1.29)	-0.038 (-1.33)	-0.471 (-1.59)	-0.479* (-1.66)
Volatility	-0.858*** (-4.62)	-0.932*** (-5.13)	-16.217*** (-6.96)	-16.988*** (-7.50)
CEO ownership	.	1.327*** (2.81)	.	20.085*** (3.24)
CEO tenure	.	-0.002 (-0.68)	.	-0.012 (-0.45)
CEO equity pay	.	0.159*** (3.01)	.	0.955 (1.57)
Observations	2128	2128	2128	2128
Adj- R^2	0.30	0.32	0.19	0.20

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

This table reports results of regressing firm performance (measured alternatively by logQ and ROA) on founder–CEO status. Columns I and II report results using log Q as the performance measure. Columns III and IV report results using ROA. For each performance measure, we estimate the empirical model in Eqs. (3) and (4) in the text using OLS. All data are described in Table 1. The estimation period is 1992–1999. All regressions include year effects and 2-digit SIC industry dummies. Robust *t*-statistics (clustered by firm) are in parentheses.

the sources Forbes, Fortune and U.S. News on Lexis–Nexis for further information on the founders of the company and information on whether or not the founders died prior to 1992 and the year the founders died. We consider a founder to be alive after 1992 when we could either verify that he was alive after 1992 or we could not find an obituary for the founder and the founder is mentioned in news articles as playing an important role in the company after 1975.⁵ When we were unable to find the necessary information on Lexis–Nexis, we searched for the founders using Forbes' Peopletracker and the internet. Our final data set consists of 580 observations on founders for 321 firms in our sample. Our instruments are a straightforward per-firm average of the dummy indicating whether the founder died prior to 1992 and the per-firm sum of all founders.⁶

In Table 1 we present summary statistics concerning select financial variables and CEO characteristics. During our sample period a founder was the CEO at some point for 50 of our sample firms (15.6% of firms). On the whole a founder was the CEO during 11.1% of firm-years. Most firms (260) in our sample were founded by simple business start-ups. 32 firms were founded through mergers of equals, 13 were founded as the result of a spin-off and 16 were founded as the result of major ownership changes.⁷ The average number of founders in our sample is 1.8 with a standard deviation of 1.1 and a maximum of 8 founders. 50% of the firms were founded prior to 1961. This is reflected in the fact that the average proportion of founders who died prior to 1992 is 70.3% with a standard deviation of 43.5%.

Table 1 also presents summary statistics separately for firms in which the founder was the CEO at some point (founder–CEO sample), and those that were never run by their founders during our sample period (non-founder–CEO sample). In the fourth column of Table 1 we present *t*-statistics for a test that the averages are equal across the founder–CEO and non-founder–CEO samples. The founder–CEO sample contains firms that are on average younger, smaller, more profitable, more volatile, and which have higher valuations than firms in the non-founder–CEO sample. Founder–CEOs also have higher ownership and longer tenures than non-founder–CEOs. Finally, the instruments appear to be correlated with the founder–CEO variables in the expected direction. The fraction of dead founders is smaller, and the number of founders is larger in the founder–CEO sample. Column 4 shows that all these differences are statistically significant at a 1% level, with the exception of the difference in ROA, which is significant at the 10% level.

4. Founder CEOs and firm performance: OLS estimates

As a first step in understanding the relationship between founder–CEO status and firm performance, we check whether the retention of the CEO title by the founder is correlated with firm performance in our sample. That is, we try to replicate some of the findings of the related literature.

⁵ We chose the year 1975 because we believe that it allows for enough time (27 years) for information to be released about the founder if he was still alive. The actual year itself (1975) is ad hoc.

⁶ We have also experimented with variations in the construction of the instruments, in particular using a dummy for whether at least one founder is alive and using a dummy for multiple versus sole founders instead of these per-firm averages and sums. These different ways of constructing the instruments had no significant effect on the results.

⁷ Our results are qualitatively similar if we restrict the sample only to firms that were founded as start-ups. The subsamples of merger of equals, spin-off and change in ownership firms contain too few founder–CEO firms to consider these founding events in isolation. The number of firms with a founder–CEO that were founded as the result of a start-up, merger of equals, spin-off or change in ownership are 38, 3, 4 and 5, respectively.

Our measures of firm performance are Tobin's Q and ROA. We use two benchmark models for performance throughout this paper. The first one postulates that the variable founderCEO (f) might affect performance along with other firm-level characteristics, which are the log of total assets (a proxy for firm size), the log of firm age, a measure of stock return volatility and 2-digit industry dummies (we omit time and firm subscripts):

$$y = \alpha + \gamma f + \beta_1 \ln(\text{assets}) + \beta_2 \ln(\text{firm age}) + \beta_3 \text{volatility} + \text{industry dummies} + \text{time dummies} + u, \quad (3)$$

where y is the performance variable and f is the founder–CEO dummy variable. We do not use firm fixed-effects in our specification because our main explanatory variable (founderCEO) varies little over time for a given firm.⁸ To calculate all t -statistics, we use heteroskedasticity-corrected standard errors. In addition, to account for over-time correlation within the same firm, we cluster all standard errors at the firm level.

In Table 2, we report the results of regression (3) for the two performance measures. Column I reports the results using log Q as the performance measure,⁹ and column III reports the results using ROA as the performance measure. FounderCEO is positively correlated with both log Q and ROA, in both cases displaying high t -statistics. The other variables display the expected correlations with performance, although these results are not always statistically strong. In particular, larger, older, and more volatile firms tend to have lower Q and ROA.

Because it is plausible that founderCEO is correlated with CEO characteristics, these results could reflect a spurious correlation between founderCEO and performance that is due to omitted variables. In our second specification, we therefore include several CEO characteristics in an attempt to correct for this problem. We identify three obvious candidates for which founderCEO might be considered a good proxy variable. The first one is CEO ownership: it is likely that founders hold a disproportionately large fraction of the firm's equity. It is also reasonable to expect that founders would have long tenures in the firm before leaving the CEO position. Finally, the fraction of the CEO's compensation that is based on equity may be correlated with founderCEO because of differing pay-for-performance incentives for founders. Because all three of these variables might also have direct effects on performance, we add them to our original benchmark model to obtain:¹⁰

$$y = \alpha + \gamma f + \beta_1 \ln(\text{assets}) + \beta_2 \ln(\text{firm age}) + \beta_3 \text{volatility} + \beta_4 \text{CEO ownership} + \beta_5 \text{CEO tenure} + \beta_6 \text{CEO equity pay} + \text{industry dummies} + \text{time dummies} + u. \quad (4)$$

In columns II and IV of Table 2, we report the results of regression (4) for the two performance measures. Consistent with omitted variable concerns, we find in both specifications that the coefficients on founderCEO are smaller than those in the previous specifications. However, founderCEO is still positively correlated with log Q (a statistically strong relationship), and with ROA (a somewhat less precise effect). Both CEO ownership and equity pay are positively related to performance, although the relation for equity pay is only statistically strong in the log Q regression. CEO tenure has a statistically weak negative correlation with performance.¹¹

Our OLS estimates of the effects of founder–CEOs on the different measures of firm performance are comparable to the ones reported by Anderson and Reeb (2003). Using a different sample-selection procedure and different empirical models from the ones we use in this paper, they find that founder–CEOs have a marginal effect on Q of 0.47 units of Q . While our marginal effect for the average firm in our preferred specification is somewhat lower (0.37), our log-linear specification is not directly comparable to theirs, because our estimated marginal effects are not constant. When we re-estimate our preferred model using Q instead of log Q as the dependent variable, we obtain an estimated marginal effect of 0.52 ($t=2.30$; $p\text{-value}=0.022$), which is not statistically different from 0.47 at any reasonable significance level. Although the log-linear specification appears to produce more conservative estimates than the linear specification, we continue to use our log-linear specification for Q because it is a more reasonable approach given the bounded nature of Q , and also because the differences are small.

The similarities between our results and the ones found in Anderson and Reeb (2003) also extend to accounting measures of performance. They find that founder–CEOs have a marginal effect on ROA of 3.14 (when ROA is measured using net income, as in this paper), an effect that is somewhat larger than the one we report in column IV of Table 2 (1.75), but fairly close to the one we report in column III (2.77). They also use a different proxy for the return on assets based on EBITDA (earnings before interest, tax, depreciation, and amortization) in their regressions. For comparison, we re-estimated our two benchmark models using EBITDA instead of net income as the numerator for ROA. Our estimates for the coefficient on founderCEO are 0.031 ($t=2.35$; $p\text{-value}=0.01$) and 0.026 ($t=1.85$; $p\text{-value}=0.064$) for the first and second model, respectively, which are quite similar to the estimate of 0.035 in Anderson and Reeb (2003).¹²

⁸ If the explanatory variable changes slowly over time (as founderCEO does), firm fixed-effect regressions may fail to detect relationships in the data even when they exist. Using a larger sample, Fahlenbrach (2006) shows evidence of a positive correlation between founder CEOs and performance even with firm fixed effects.

⁹ We chose a log-linear specification for Q due to the fact that Q can never be negative. Using Q instead of log Q as the dependent variable might therefore generate fitted values that are outside of the natural range of Q .

¹⁰ Following Anderson and Reeb (2003), we use the value of CEOs' annual option pay divided by the sum of salary, bonus and option pay to measure a CEO's equity-based pay.

¹¹ Of course, it is possible that these control variables are endogenous in the performance regressions. For the purpose of comparability, we follow the previous literature here and interpret the results under the assumption that all regressors are uncorrelated with u .

¹² Although our results are similar when we use EBITDA instead of net income to construct ROA, it is easier to detect evidence consistent with the existence of an endogeneity problem using EBITDA. Thus our choice of net income instead of EBITDA is more conservative.

Overall, it appears that the magnitude of the estimated coefficient on the founder–CEO dummy in a linear performance regression is not very sensitive to the choice of the set of control variables. Furthermore, survivorship biases do not appear to be a major concern in such regressions. Anderson and Reeb's (2003) procedure of choosing firms in the S&P 500 in 1992 and then following them until 1999 introduces a different type of selection bias than our approach of choosing the firms in the Fortune 500 in 1998 and following them back in time. Anderson and Reeb's sample-selection procedure overweighs those firms that have survived as public companies throughout their sample period. Our selection procedure overweighs those firms that have grown larger (or remained in the Fortune 500) during our sample period. Nevertheless, the fact that our estimates are virtually identical to theirs suggests that these different types of survivorship biases are not creating a discrepancy between the two sets of findings. Furthermore, our findings are in broad agreement with those of Fahlenbrach (in press), Palia et al. (in press), and Villalonga and Amit (2006). Because each of these papers uses different samples and different control variables, we conclude that the positive relationship between the retention of the CEO title by one of the founders and both market and accounting measures of firm performance appears to be fairly robust.

The question to which we turn next is how should one interpret this relationship? Should one conclude that the retention of the CEO title by one of the company's founders leads to superior performance? Or is it the other way around, that is, is superior performance a reason for a founder to remain as CEO? A natural hypothesis is the following (see e.g. Anderson and Reeb, 2003). Founder–CEOs have superior inside information about the prospects of their firms, which they use to stay in power if they expect performance to be good and plan their departure if they expect performance to go down. This hypothesis is consistent with a positive correlation between founder–CEOs and performance, but in this case the causality goes from performance to the likelihood that the founder retains the CEO title. In other words, this is a situation in which $\text{Cov}(f, u) > 0$, which would (asymptotically) bias the OLS estimates of γ upwards.

To gain further insight into the nature of the relationship between founder–CEOs and firm performance, in the next section we use instrumental variables to isolate the effects of founder–CEOs on performance from other sources of variation.

5. Founder CEOs and firm performance: IV estimates

We deal with the possible endogeneity of founder–CEOs by means of instrumental variables methods. Assuming that we have a set of valid instruments \mathbf{z} for founder–CEO status (not including the elements in \mathbf{x}), we can consistently estimate γ by the following procedure: (i) estimate a binary response model (e.g. probit) of f on \mathbf{z} and some other controls, (ii) compute the fitted probabilities \hat{f} , and (iii) estimate γ by instrumental variables using \hat{f} as instruments for founder–CEO status.

This is a three-stage procedure. In the first stage, we estimate a probit of the determinants of founder–CEO status. In the second stage, we regress f on \hat{f} and \mathbf{x} . In the third stage, we regress y on \mathbf{x} and the fitted values of the second stage. This procedure is different from the “pseudo-IV” procedure of running an OLS regression of y on \hat{f} and \mathbf{x} . In the latter case, consistency is not guaranteed unless the first stage is correctly specified, and the standard errors need to be adjusted.

There are many advantages to this approach. First, it takes the binary nature of the endogenous variable into account. Although the two-stage least squares consistency of the second stage does not hinge on getting the functional form right in the first stage (see Angrist and Krueger, 2001), 2SLS leads to biased estimates in finite samples and it is not known how misspecification in the first stage may affect this bias. Second, unlike some of the alternative procedures, it does not require the binary response model of the first stage to be correctly specified. Third, although some regressors are generated in the first stage, the standard IV standard errors are still asymptotically valid (see e.g. Wooldridge, 2002, p. 623, procedure 18.1).

5.1. Discussion of the instruments

Here we discuss the economic arguments supporting the validity of the two different variables that we use as instruments for founderCEO, which are *dead founders* and the *number of founders*.

5.1.1. Dead founders

The first variable we use as an instrument is a dummy variable that takes the value of 1 if the founder died before the start of our sample period and zero otherwise (if there are multiple founders, we take the average of this variable among all founders). The motivation for this instrument is simple: dead founders cannot be CEOs. This instrument is similar to the standard “eligibility” instruments commonly used in the program evaluation literature, because being alive is a minimum eligibility criterion for a founder–CEO. However, to be a good instrument dead founders must also be uncorrelated with performance except through explanatory variables contained in the second stage regression. We find it unlikely that founders' deaths are caused by performance. The death of a founder should also be a fairly exogenous event without any direct effect on performance, except when the founder happens to be in control.

A possible caveat is that dead founders may be correlated with firm age, which could have direct effects on firm performance. Because we control for firm age in all of our regressions, we believe that this is not an important concern. It is conceivable that the “dead founder” variable may be correlated with the firm life cycle in a manner that is not captured by the firm age variable, but we have no strong reasons to believe that this should be the case. As is the case with any identification strategy, the results are only as good as the identification assumptions. We believe that ours are not too strong.

5.1.2. Number of founders

The second variable we use as an instrument is the number of founders of each firm. We believe that this variable also satisfies the conditions necessary for a valid instrument. First, the probability that the current CEO is one of the founders is increasing in the

Table 3

The determinants of founder–CEO status: probit model

Independent Variables	Dependent variable: founderCEO	
	(I)	(II)
Dead founders	–2.246*** (–7.04)	–2.707*** (–6.06)
Number of founders	0.198** (2.48)	0.343*** (2.78)
ln(assets)	–0.072 (–0.77)	0.113 (0.96)
ln(age)	–0.465*** (–3.89)	–1.079*** (–5.94)
Volatility	0.107 (0.12)	1.626 (1.63)
CEO ownership	.	3.300 (1.53)
CEO tenure	.	0.127*** (6.79)
CEO equity pay	.	–0.456 (–1.50)
Observations	2128	2128

*** ** * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

This table reports the results from the Probit model of whether the CEO is a founder of the company. Dead founders is the average of an indicator variable that takes the value of 1 if a given founder is dead as of 1992 and zero otherwise. Number of founders is the total number of founders for each firm. Details on the construction of the instruments are provided in the text. All other data are described in Table 1. The estimation period is 1992–1999. All regressions include year effects and 2-digit SIC industry dummies. Robust (clustered by firm) *t*-statistics are in parentheses.

number of founders, although since one founder often plays a more dominant role than the others we expect this correlation to be weaker than in the case of our other instrument.¹³ Second, it should be fairly exogenous in our setup. In particular, the number of founders is unlikely to have any direct effect on firm performance years after the founding event.

A possible caveat is that different industries might require different numbers of founders. However, we found no systematic variation in the number of founders across industry classifications. Furthermore, all our regressions include industry dummies.

5.1.3. Founder–CEOs or founder control?

A further concern has to do with the quality of the founderCEO variable. Arguably, this variable is only an imperfect proxy for a latent “founder control” variable. For example, Villalonga and Amit (2006) report that not only founder–CEOs, but also founder chairmen, have positive effects on performance. In principle, one should not attach too much significance to the titles; what is important is whether the founder has any influence on managerial decisions. Thus, we view the founderCEO variable simply as a proxy for founder control.

There is a potential problem if the measurement error associated with founderCEO is correlated with the proposed instruments, since the latter may be correlated with the latent “true variable.” Indeed, dead founders and the number of founders are likely to affect “true” founder control, and not only the type of control captured by the founderCEO dummy.

Although such a problem may undermine the interpretation of the estimate of γ as the “pure” effect of founder–CEOs on firm performance, it does not affect the qualitative interpretation of the results: the sign of the estimate of γ should reflect the sign of the effect of founder control on performance.

5.1.4. Local or global effects?

The use of instrumental variables methods implies that we can only hope to estimate local rather than global effects. In other words, our IV estimator γ^{IV} consistently estimates the average impact of founder–CEOs on the performance of those firms that are affected in their CEO choices by the value of the instruments. There is thus a question of whether our instruments define an interesting sub-population over which this effect is averaged.

Consider, for example, the dead founders variable. All firms that are run by founder–CEOs were affected in their choices by the instrument, because they can only have a founder–CEO if at least one of the founders is alive. Some but not all the firms without a founder–CEO were certainly affected by the instruments, too. Thus, the sub-population of firms affected by the instruments includes all firms in the “treatment group” (firms run by founders) plus some others in the “control group.” Consequently, we expect our IV approach to estimate an effect that is somewhere in between the “treatment on the treated effect” and the “average treatment effect.”

¹³ The case of Arrow Electronics illustrates how the number of founders may influence whether or not the current CEO is a founder (see Hoovers 2002, Fortune, January 12, 1981, p. 19 and The New York Times, December 6, 1980, p. 26). In 1968 three friends led a group of investors in acquiring a then obscure company called Arrow Electronics Corporation. After merging it with another company, they used it to found what is now one of the largest distributors of electronic components in the country. One of the partners, Duke Glenn, Jr., was the Chairman and CEO. The other two were Executive Vice-Presidents. In 1980 a hotel fire killed 13 members of Arrow's senior management including the founder-CEO and another founder. The remaining founder, John Waddell, was immediately named acting CEO and remained CEO with only brief interruptions until 1986. Although Waddell's primary responsibilities were in corporate administration and communications before the fire, the crisis led the board to choose him as acting CEO because he was one of the original founders.

Table 4

Firm performance and founder–CEO status: results from endogenous dummy variable model

Independent variables	Dependent variable			
	log Q		ROA	
	(I)	(II)	(III)	(IV)
FounderCEO	0.991*** (4.54)	0.432*** (3.58)	8.377*** (3.80)	3.627*** (2.84)
ln(assets)	-0.011 (-0.49)	-0.026 (-1.20)	-0.421* (-1.78)	-0.474*** (-2.05)
ln(age)	0.041 (1.36)	-0.014 (-0.48)	0.112 (0.36)	-0.299 (-0.97)
Volatility	-1.155*** (-4.71)	-1.030*** (-5.38)	-18.441*** (-6.90)	-17.725*** (-7.43)
CEO ownership	.	0.970* (1.86)	.	17.380*** (2.73)
CEO tenure	.	-0.005** (-2.11)	.	-0.038 (-1.37)
CEO equity pay	.	0.148*** (2.78)	.	0.870 (1.42)
Observations	2128	2128	2128	2128
Diff IV – OLS	0.750*** (10.42)	0.247*** (5.37)	5.607*** (6.30)	1.877*** (3.06)

*** ** * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

This table reports results of regressing firm performance (measured alternatively by log Q and ROA) on founder–CEO status. FounderCEO is instrumented with the predicted probability that the CEO is a founder, using the Probit models of Table 3. The estimation method takes the discrete nature of founderCEO explicitly into account. Columns I and II report results using log Q as the performance measure. Columns III and IV report results using ROA. All data are described in Table 1 or in the text. The estimation period is 1992–1999. All regressions include year effects and 2-digit SIC industry dummies. The bottom row of this table reports an estimate of the difference between the IV and the OLS coefficients on the founderCEO variable, computed using a Hausman (1978) specification test. Robust (clustered by firm) *t*-statistics are in parentheses.

5.2. Determinants of founder CEO status

As the first stage of our IV estimation of the endogenous dummy variable model, we estimate the model

$$Pr(f = 1|\mathbf{x}, \mathbf{z}) = \Phi(\theta_0 + \theta_1 \text{ dead founders} + \theta_2 \text{ number of founders} + \mathbf{x}\boldsymbol{\delta}), \quad (5)$$

where $\Phi(\cdot)$ is the cumulative distribution function for a standardized normal random variable, \mathbf{z} is the vector of instruments, and \mathbf{x} is a vector of control variables. It is important to highlight that our IV approach does not require this specification to be correct. It only requires the instruments to be correlated with the probability that the CEO is one of the founders.

From Table 3 we see that both proposed instruments are correlated with founderCEO. Consistent with intuition, dead founders is negatively related to founder–CEOs, with robust (clustered by firm) *z*-statistics of either -7.04 or -6.06 (depending on the set of controls), while the number of founders is positively associated with founder–CEOs with robust (clustered by firm) *z*-statistics of 2.48 and 2.78, depending on the specification. Thus, we conclude that our instruments not only move the founderCEO variable in the predicted direction, but also that our specifications do not appear to suffer from problems associated with “weak instruments.”

5.3. IV performance regressions

Table 4 reports the main results of this paper. The direct effect of founderCEO on performance is positive and significant at all conventional significance levels in all four specifications. Unlike in Table 2, these results can be interpreted as evidence of a causal effect of founder control on firm value and performance. In addition, a direct comparison of Tables 2 and 4 shows that all estimated coefficients on founderCEO are larger than their OLS counterparts. For example, the estimates from column II suggest that a firm with average *Q* ($Q=2.05$) will experience a drop of 0.87 units in its *Q* when its CEO is not one of its founders. This effect is about twice as large as the one estimated by OLS. The fact that the effect of founder–CEOs on performance in the endogenous dummy variable model is larger than that estimated by OLS suggests that performance has an average negative effect on whether the founder retains the CEO title. Formally, under the assumption that only founderCEO is endogenous, we have that

$$\text{sign}\left\{p \lim\left(\hat{\gamma}^{\text{OLS}} - \hat{\gamma}^{\text{IV}}\right)\right\} = \text{sign}\{\text{Cov}(f, u)\}. \quad (6)$$

Assuming that the asymptotic approximation holds, the fact that the IV estimate is larger than the OLS estimate suggests that $\text{Cov}(f, u) < 0$. Ignoring the endogeneity of the founder–CEO variable thus results in underestimation of the effect of founder control on performance.

At the bottom of Table 4, we report the differences between the endogenous dummy variable model estimates (IV) and OLS estimates of the effect of founder–CEOs on performance, along with their *t*-statistics, which are computed using the method in Hausman's (1978) specification tests. We find that all differences are statistically significant.

We summarize the results in this section as follows. First, we find clear evidence that founder–CEOs have a positive and large effect on firm performance and valuation, an effect that is larger than that estimated by OLS.¹⁴ We also find strong evidence that founder–CEO status is endogenous in performance regressions, and that once one factors out the direct effect of founder–CEOs on performance, the remaining correlation between firm performance and the likelihood that a founder retains the CEO title is negative. This negative effect of performance on founder–CEO status is consistent with different interpretations, which we discuss in more detail in the next section. However, it is inconsistent with the standard notion that good performance makes it more likely that founders retain the CEO title.

6. Past performance and founder–CEO departures

The previous section provides evidence that good performance has a negative effect on the retention of the CEO title by founders. This finding is somewhat surprising, since the idea that good performance might make founder–CEOs less likely to relinquish their titles has some intuitive appeal. Thus, the purpose of this section is to uncover some additional empirical relationships that might help us better understand this finding. We first advance some possible explanations for this result and then we provide empirical evidence on which explanations are consistent with our data.

6.1. Hypotheses development

There are four possible explanations for the positive effect of performance on founder–CEO departures, which we discuss here.

6.1.1. Bad governance

There is a large amount of evidence showing that poor performance increases the likelihood of CEO turnover (e.g., Warner et al., 1988; Weisbach, 1988; see also Goldman et al., 2003, for a comprehensive list of papers documenting this relationship). However, in firms with entrenched CEOs this link between poor performance and turnover could be weak. Morck et al. (1989) provide some evidence of entrenchment of founder-executives. In a sample of Fortune 500 firms, they find that firms whose top management teams contain members of the founding family are less likely to experience a complete turnover of top executives and to be targets of hostile takeovers than other firms. However, this type of entrenchment is not sufficient to explain our findings, because it only suggests that poor performance might have a small positive or no effect on the likelihood of a founder–CEO being replaced. In fact, we need a stronger notion of bad governance to explain our findings: poor performance should *decrease* the likelihood of founder–CEO turnover. This could happen if bad governance also has a direct negative effect on performance. In this case, the negative correlation between f and u could be due to an omitted variable measuring the overall quality of governance: firms with bad governance are more likely to perform badly and to have a founder who is entrenched as the CEO. Thus, the bad governance hypothesis suggests that poor performance and the likelihood of replacing a founder–CEO should be negatively related.

6.1.2. Controlled succession

Suppose that after good performance, CEOs are more likely to be able to choose their successors (Morck et al., 1989). In fact, Morck et al. (1989) claim that ordinary CEO successions are more likely to occur after periods of abnormally good performance. Founders, in particular, may value the ability to control succession, for it allows them to transfer control to their heirs. Thus, the controlled-succession hypothesis predicts that founder–CEOs will step out after some period of consistently good performance, while it has no prediction for how founder–CEO turnover is affected by bad performance.

6.1.3. Founder benevolence and the paradox of entrepreneurial success

Suppose that founders believe they have superior managerial capabilities (whether this is actually true is not important for the argument). Suppose also that founders are *benevolent*, i.e. they care more about the future of the company than a CEO who is not one of the original founders. Founders may therefore want to leave their companies only when they are in good shape. For example, Wasserman (2003) argues that a founder–CEO's success in achieving critical milestones, such as a successful completion of product development, makes it more likely that he will step down. He calls this phenomenon “the paradox of entrepreneurial success.”

6.1.4. Wealth effects

Founder CEOs who have much of their wealth invested in the firm benefit greatly from good firm performance. If they want to retire when rich, they should be more willing to retire early following good performance. Thus there should be a positive relationship between past good performance and the likelihood of subsequent founder–CEO departures. A related but somewhat darker story is that founder–CEOs might be better informed than other shareholders and may choose to leave the firm and sell their shares when performance is unusually high. Thus, founder–CEOs may leave their firms to “cash in” before the market valuation of their shares deteriorates. In both cases “wealth effects” are important, i.e. founder–CEOs want to leave their firms exactly when their firm-related wealth is high.

¹⁴ While our results are robust to restricting our sample to start-ups, we also examined whether the founding event is a potential omitted variable in these regressions. If the founding event is associated both with the likelihood the founder stays as CEO and with performance, our results could be biased. We did not find that dummies for the type of founding event were correlated with the likelihood of founder–CEO status in the first stage of our IV regressions. Moreover, the results are qualitatively the same after including these dummies in our performance regressions.

Table 5
Probit estimates of founder–CEO succession

Independent variables	Dependent variable: stepout					
	(I)	(II)	(III)	(IV)	(V)	(VI)
HighQ	0.019 (0.67)	.	0.099 (1.52)	.	0.033 (0.94)	.
LowQ	0.058 (1.23)	.	0.084 (1.58)	.	0.051 (0.93)	.
HighROA	.	0.077** (2.44)	.	0.153** (2.39)	.	0.075** (1.97)
LowROA	.	0.156*** (3.42)	.	0.152*** (3.24)	.	0.136** (2.53)
LaggedQ*HighQ	.	.	-0.028 (-1.37)	.	.	.
LaggedQ*LowQ	.	.	-0.299 (-1.13)	.	.	.
LaggedROA*HighROA	.	.	.	-0.002 (-1.27)	.	.
LaggedROA*LowROA	.	.	.	-0.0003 (-0.19)	.	.
Observations	459	466	459	466	358	363

*** ** * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

In this table we examine the effect of lagged performance on the likelihood that a founder retains the CEO title. We restrict our sample to firms that were run by one of their founders in any year in our sample. The dependent variable (stepout) is a dummy which takes the value of 1 in the firm-years in which a founder–CEO steps out and is 0 otherwise. For each firm that has experienced a change of command in this restricted sample, we leave out all observations in the years after the one in which the founder has relinquished the CEO title. Columns I and III report results using *Q* as the performance measure, and columns II and IV report results using ROA. The variable highQ is a dummy which is equal to one if the average of the first two lags of *Q* is at the top quartile of the *Q* distribution for that year, and is zero otherwise. The variable lowQ is a dummy which is equal to one if the average of the first two lags of *Q* is at the bottom quartile of the *Q* distribution for that year, and is zero otherwise. The variables highROA and lowROA are constructed in the same way. LaggedQ and laggedROA are the averages of the first two lags of each performance variable. All data are described in Table 1. The estimation period is 1992–1999. All regressions include year effects. Specifications in (V) and (VI) also include (the log of) firm age and one-year lagged CEO ownership, equity-based pay, CEO tenure, stock return volatility, and firm size (log of assets) as controls. The reported coefficients are marginal effects evaluated at the means of the data. Robust z-statistics for the underlying probit coefficients are in parentheses.

6.2. Empirical strategy and data description

In order to explore the empirical relevance of these hypotheses, we assess how well past performances, both good and bad, help predict future changes in command in which a founder–CEO steps out. Thus, we use the timing of events as an identification strategy. There are two main limitations of this procedure. First, predictive power does not imply causation, especially when variables reflect the behavior of forward-looking agents. Thus, we expect market measures of performance to be more plagued by endogeneity problems in predictive regressions than accounting measures, because the latter tend to be less influenced by the expectation of future events. Second, and perhaps most importantly, our tests in the previous sections detected a *contemporaneous* effect of performance on founder–CEO status. To the extent that performance exhibits some persistence, our approach in this section should be able to shed some light on the causes of this effect. However, one cannot fully capture this effect without accounting for its strictly contemporaneous component, for which identification by means of timing is not possible.

Our empirical strategy is as follows. When a CEO who is not a founder is replaced, this typically (though not necessarily) implies that the new CEO is also a non-founder. Therefore, turnover data in firms that are not initially run by founders are not useful for our purposes. Accordingly, we restrict our sample to firms that were run by one of their founders in any year in our sample. We then generate an indicator variable called *stepout* that takes the value of 1 in the firm-years in which a founder–CEO steps out and 0 otherwise. For each firm that has experienced a change of command in this restricted sample, we leave out all observations in the years after the one in which the founder has relinquished the CEO title.

The arguments we have outlined have predictions for how founder–CEO departures should be differentially affected by either good or bad past performance. Thus, in principle the relationship between performance and the likelihood of founder–CEO departures is potentially non-monotonic. In order to test some of these hypotheses jointly, we have to impose some empirical specifications that allow for the possibility of a non-monotonic relationship between the dependent and independent variables.

A simple and intuitive procedure is as follows. We create a dummy variable called *highQ* that equals 1 for very high values of lagged *Q* and is zero otherwise. We consider *Q* to be high if it is in the top quartile of the full sample *Q* distribution for that given year. Similarly, we create a dummy variable called *lowQ* that equals 1 whenever *Q* is in the bottom quartile of the full sample *Q* distribution for that given year, and is zero otherwise. The variables *highROA* and *lowROA* are defined in an analogous way. Because we want to see the effects of *persistent* past performance on CEO turnover, we use the averages of one- and two-year lagged *Q* and ROA to construct our measures of extreme performance.¹⁵

¹⁵ We chose two years because using three or more years would severely restrict our sample size.

An additional limitation of this procedure is that it severely reduces the number of usable observations. In the data that we use in Section 5 there are only 23 instances in which a founder relinquishes the CEO title. We therefore expand our sample to increase the number of changes in command for founder–CEOs. To do this we use Forbes executive compensation surveys (Forbes, 1992–1999), which identify whether or not the CEOs of the Forbes 800 are founders. We first match ExecuComp to the Forbes 800 firms to identify further instances of firms whose founders were CEOs. We then track these additional firms in the Forbes compensation surveys until 2001 to identify when the founder no longer held the CEO title. By this procedure we are able to expand our sample of founder–CEO departures from 23 to 50. We also identify 535 firm-years in which stepout is equal to zero. We obtain performance measures and other controls, such as total assets, volatility, CEO ownership and CEO tenure, from ExecuComp for the additional firm-years. We obtain firm age data from Jovanovic and Rousseau (2001).

6.3. Comparison of the Forbes 800 sample with Fortune 500 sample

Since we do not have data on the instruments (number of founders and dead founders) for this extended sample (Forbes 800), we cannot conduct the analysis of Section 5 in this sample. However, there are several reasons why we believe that the results of Section 5 would also hold in the extended Forbes 800 sample.

First, we can compare the OLS results reported in Table 2 with those that we obtain using the extended sample. To economize on space we report the OLS regressions for this extended sample in the text. We report the results for the specification with all the controls, which correspond to columns (II) and (IV) of Table 2. As in Table 2, these regressions include year and industry dummies. *T*-statistics, computed as in Table 2 using standard errors clustered by firm are in brackets. Both regressions contain 4,828 firm-years¹⁶:

$$\begin{aligned} \ln Q = & 0.127 \text{founder-CEO} - 0.053 \ln(\text{assets}) - 0.028 \ln(\text{firm age}) - 0.386 \text{volatility} + 0.432 \text{CEO ownership} \\ & (2.41) \quad (4.25) \quad (2.10) \quad (3.67) \quad (1.63) \\ & + 0.001 \text{CEO tenure} + 0.158 \text{CEO equity pay} \\ & (0.24) \quad (5.05) \end{aligned} \quad (7)$$

$$\begin{aligned} \text{ROA} = & 1.031 \text{founder-CEO} - 0.835 \ln(\text{assets}) - 0.218 \ln(\text{firm age}) - 10.165 \text{volatility} + 3.213 \text{CEO ownership} \\ & (1.64) \quad (6.07) \quad (1.67) \quad (8.15) \quad (1.06) \\ & + 0.016 \text{CEO tenure} + 0.529 \text{CEO equity pay} \\ & (0.91) \quad (1.52) \end{aligned} \quad (8)$$

These results are very similar to those reported in Table 2. For example, the coefficient on founder–CEO in the *Q* regression in column II of Table 2 is 0.185, and the *t*-statistic is 2.58. Similarly to Table 2, the correlation between ROA and founder–CEO is weaker than the correlation with *Q*, but it is still positive (and significant at a 10% level as in Table 2). All the coefficients on the control variables have the exact same signs. The main difference between the results in the two samples is that the coefficients on founder–CEOs are somewhat higher in the Fortune 500 sample.

Second, we note that despite the smaller sample we obtain strongly significant results in Table 4 (the IV performance regressions). In contrast, the larger size of the extended sample is important for the empirical analysis of founder–CEO departures that we conduct below. When we use only the original Fortune 500 sample the results are qualitatively identical but generally not significant, possibly due to the smaller sample size.

Third, as we also point out in Section 4 for our base sample, the OLS results reported above are very similar to those of Anderson and Reeb (2003), Fahlenbrach (in press), Palia et al. (in press), and Villalonga and Amit (2006). Because each of these papers uses different samples and different control variables, we conclude that the positive relationship between the retention of the CEO title by one of the founders and both market and accounting measures of firm performance appears to be fairly robust, and does not depend on the particular sample that is used.

6.4. Empirical results

Table 5 presents the results of probits estimating the likelihood of a founder–CEO stepping out as a function of high*Q* and low*Q* and year dummies, and also as a function of highROA and lowROA and year dummies. In order to facilitate the interpretation, we present the marginal effects evaluated at the means of the data. As one can see from column I, we find that the estimated coefficients on high*Q* and low*Q* are both positive, economically significant but statistically weak.¹⁷ In column II, we find that both highROA and lowROA help predict future changes in which a founder–CEO steps out. These effects are both economically and statistically strong. In particular, we find that an average founder-run firm in the top quartile of ROA is 8 percentage points more likely to replace its CEO in the near future than a firm with mediocre performance (in the second or third quartiles).

¹⁶ The number of observations is determined primarily by the match between Execucomp and the Forbes data and the availability of firm age data. During 1992–2001 only 6 of the Forbes firms went bankrupt. 302 firms were acquired and 13 firms delisted from CRSP for other reasons.

¹⁷ The lack of predictive power of *Q* is not surprising. In fact, Hermalin and Weisbach (1998) claim that this is exactly what theory predicts: accounting measures of performance reflect the characteristics of current managers, while stock market based measures of performance should also reflect the expectation of future management changes.

Table 6
Probit estimates of founder–CEO succession with ownership controls

Independent variables	Dependent variable: stepout		
	(I)	(II)	(III)
HighROA	0.082*** (2.61)	0.089** (2.55)	0.079* (1.93)
LowROA	0.136*** (3.05)	0.109** (2.23)	0.128** (2.17)
Lagged ownership	−0.334* (−1.81)	−0.334 (−1.34)	−0.290 (−1.10)
HighOWNERSHIP * highROA	.	−0.018 (−0.38)	−0.012 (−0.21)
HighOWNERSHIP * lowROA	.	0.064 (0.93)	0.020 (0.26)
Observations	461	461	363

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

In this table we examine the effect of ownership controls on the likelihood that a founder retains the CEO title. We add lagged ownership and two interaction terms between ownership and performance to the specification in column II of Table 6. We define an indicator variable called highOWNERSHIP which takes the value of 1 if lagged ownership is above 0.05. The estimation period is 1992–1999. All regressions include year effects. The specification in (III) also includes (the log of) firm age and one-year lagged equity-based pay, CEO tenure, stock return volatility, and firm size (log of assets) as controls. The reported coefficients are marginal effects evaluated at the means of the data. Robust z-statistics for the underlying probit coefficients are in parentheses.

The finding that past poor performance as measured by ROA increases the likelihood that a founder–CEO will leave the firm is at odds with the hypothesis that founders are inexorably entrenched. This result is not surprising, given the well-documented evidence on the disciplining role of CEO dismissals (Warner et al., 1988; Weisbach, 1988). What our evidence adds to this literature is the confirmation that founders are not immune to this disciplining device. On the other hand, the finding that past good performance as measured by ROA increases the likelihood that a founder–CEO will leave the firm in the near future is consistent with the other three hypotheses: controlled succession, founder benevolence and wealth effects.

The finding that founder–CEOs departures are more likely after periods of good accounting performance is unique to founder departures. For example, if we replicate regression II in Table 5 using all CEO departures rather than founder–CEO departures, we find that although poor ROA performance predicts CEO departures (the probit coefficient is 0.275, with a *p*-value of 7%), the coefficient on highROA is negative and not statistically significant. We have replicated all regressions we report in this session using either total or non-founder–CEO departures, and we found no statistically reliable evidence of good past performance increasing the likelihood of CEO departures (we do not report the detailed results to economize on space).

We also consider some robustness checks. First, our choice of cutoff (25%) to define both high and low performance is not important. Choosing any cutoff in the range of 10% to 35% always leads to estimated parameters that are significant at least at the 10% level in the ROA specification. As expected, as the cutoff approaches 50% the effects become much weaker and eventually not significant. For cutoffs lower than 10%, the standard errors tend to go up, which is consistent with the intuition that the precision of our estimates should decrease as the number of firms that are considered to be performing extremely well or extremely badly decreases.

As an additional robustness check, we also tried a more flexible specification that is capable of capturing richer non-monotonic relationships between performance and the probability of CEO departures. For each performance measure, we created two new variables: we interact both the high and the low performance dummies with the average of one- and two-year lagged performance. We estimate Probits using both the dummies and the interaction terms. This specification allows us to capture the additional effect that for example, performance has on founder–CEO departures *conditional on performance being high*. It also allow us to better explore the continuous nature of the underlying performance variables. We report the results of these extended specifications in columns III and IV of Table 5. We see that the statistical significance of the effects of both the low and high performance dummies is not affected by the inclusion of the interaction terms. Furthermore, these interaction terms appear to have no additional predictive power. This suggests that our original simpler specification is capturing most of the effects of performance on the likelihood of founder–CEO departures.

Finally, we also experimented with including other controls in our specifications. In columns V and VI we report the results when using all previous control variables: firm size, firm age, volatility, CEO ownership, equity-based pay, and tenure. All these variables (except for firm age) are lagged one year. Due to missing data, the number of observations is considerably reduced. However, the results are largely unchanged: both high and low past ROA predict founder departures, while *Q* remains an unreliable predictor of founder departures. We do not report the estimated coefficients on the controls for the sake of brevity. In the *Q* regression, only the estimated coefficient on CEO equity-based pay is significant at 10% (it enters with a negative sign). In the ROA regression, none of the firm and CEO controls display a statistically significant effect on the probability of founder departures. Thus, the firm-level controls have small effects on the probability of a founder–CEO departure.

To further discriminate among our hypotheses, we examine the importance of wealth effects in Table 6.¹⁸ If wealth effects are important determinants of founder–CEO departures, one should expect that founder–CEOs with more firm-related wealth leave more often. In column I, we see that the opposite holds: founder–CEOs with higher ownership stakes are less likely to leave the

¹⁸ We only report results using ROA. As before, the regressions using *Q* suggest similar results, but they are never statistically significant.

firm. Of course, ownership may affect the likelihood of departure for reasons that are not related to CEO wealth. For example, CEOs with more ownership might be more entrenched and thus less likely to be forced to leave. Strictly speaking, wealth effects should play a role only when CEOs are departing after good performance. To test this hypothesis more directly, we create a dummy variable that takes the value of 1 if founder–CEO ownership is “high” and 0 otherwise. We define high ownership as an above-average equity stake (the average ownership of founder–CEOs in our sample is approximately 5%).

We then interact the high ownership dummy with both highROA and lowROA. If founder–CEOs are departing after periods of good performance due to wealth effects, then we should expect that the sensitivity of departure to good performance should be higher when they have more ownership. Thus, we expect the interaction between highROA and *highOWNERSHIP* to have a positive effect on the probability of CEO departure. From column II, we see that this interaction term actually enters with a negative sign and is not statistically reliable. In column III we replicate column II with a full set of controls (none of the control variables enter significantly in the regression). Overall, we find no support for the hypothesis that wealth effects are responsible for the positive correlation between high performance and founder–CEO departures in our sample.

We can summarize our results as follows. The finding that firms with relatively good performances are less likely to be run by founder–CEOs appear to be due more to founders choosing to relinquish control after periods of good performance than to an omitted variable correlated with bad governance. In addition, our evidence suggests that wealth effects are not driving our results. This implies that the hypotheses that are most consistent with our evidence are the controlled-succession and the founder-benevolence hypotheses.¹⁹

7. Conclusions

In this paper, we provide strong evidence that founder–CEO status is influenced by firm performance. Using methods based on instrumental variables, we find that firm performance has a negative effect on the likelihood that a firm is run by one of its founders. After factoring out the negative effect of performance on founder–CEO status, we obtain a positive causal effect of founder–CEOs on firm performance, an effect that is quantitatively larger than the one estimated through standard OLS regressions. These results are robust to changes in model specification, and they hold for alternative measures of performance.

In order to further understand the negative effect of performance on founder–CEO status, we estimate the probability of founder–CEO departures as a (potentially non-monotonic) function of past performance. We find that both good and bad past accounting performance increases the likelihood that founder–CEOs will step out. The evidence is not consistent with the hypothesis that an omitted variable correlated with bad governance can explain the negative relationship between firm performance and founder–CEO status, nor with the hypothesis that founder–CEOs leave once they are wealthy enough. This leaves us with two potential arguments that might explain our finding: (1) founder–CEOs may value control over their succession more than non-founders, and (2) founder–CEOs may want to leave their companies “in good shape.”

All in all, the results in this paper suggest a largely positive view of founder control in large US corporations. On average, founders increase the performance of their firms and they only step out if their firms are doing well. The message that founder control is beneficial is consistent with most of the recent literature (e.g., Anderson and Reeb, 2003, Villalonga and Amit, 2006; Fahlenbrach, in press).

Our paper has implications for the growing literature on family firms. In particular, the positive effect of founders on performance suggests that the higher performance of family firms that is found in many studies could be driven primarily by firms in which the current CEO is a founder. In addition, the finding that founders are more likely to leave when their firms are doing well suggests that founder–CEO succession can be very different than the succession of professional CEOs. This is a potentially interesting area for future research.

References

- Anderson, R., Reeb, D., 2003. Founding-family ownership and firm performance: evidence from the S&P 500. *Journal of Finance* 58, 1301–1328.
- Angrist, J., Krueger, A., 2001. Instrumental variables and the search for identification: from supply and demand to natural experiments. *Journal of Economic Perspectives* 15, 69–87.
- Bennedsen, M., Kasper, Nielsen, Pérez-González, Francisco, Wolfenzon, Daniel, 2007. Inside the family firm: the role of families in succession decisions and performance. *Quarterly Journal of Economics* 122, 647–691.
- Fahlenbrach, R., in press, “Founder–CEOs, Investment Decisions, and Stock Market Performance,” forthcoming, *Journal of Financial and Quantitative Analysis*.
- Goldman, E., Hazarika, S., Shivdasani, A., 2003. What Determines CEO Turnover?. Working Paper. Kenan-Flagler Business School, University of North Carolina, Chapel Hill.
- Hausman, J.A., 1978. Specification tests in econometrics. *Econometrica* 46, 1251–1271.
- Heckman, J.J., 1978. Dummy endogenous variables in a simultaneous equations system. *Econometrica* 46, 931–960.
- Hermalin, B.E., Weisbach, M.S., 1998. Endogenously chosen boards or directors and their monitoring of the CEO. *American Economic Review* 88, 96–118.
- Johnson, B., Magee, R., Nagarajan, N., Newman, H., 1985. An analysis of the stock price reaction to sudden executive deaths. *Journal of Accounting and Economics* 7, 151–174.
- Jovanovic, B., Rousseau, P., 2001. Why wait? A century of life before IPO. *American Economic Review* 91, 336–341.
- Morck, R., Shleifer, A., Vishny, R., 1988. Management ownership and market valuation: an empirical analysis. *Journal of Financial Economics* 20, 293–315.
- Morck, R., Shleifer, A., Vishny, R., 1989. Alternative mechanisms for corporate control. *American Economic Review* 79, 842–852.
- Morck, R., Strangeland, D., Yeung, B., 1998. Inherited wealth, corporate control and economic growth: a Canadian disease. NBER Working paper.

¹⁹ To shed more light on CEO departures following good and bad performance, we conducted a simple event study around CEO departures. We used the ExecuComp item “leftofc”, which provides dates CEOs left the CEO position, to identify departure dates for our CEOs. We then conducted a market model event study using an estimation window of (–301, –46) and event windows of (–30, –1), (–1, +1) and (–1, +7) around these event dates. Because of missing departure dates and stock prices, we end with a sample of 74 observations on CEO departures, of which 9 are due to founder–CEO departures. The average stock price reaction is not significant. There is some indication that the reaction to founder–CEO departures is negative when past performance is low. However, we hesitate to infer too much from these results given the small sample size. We believe this could be an interesting area for future research.

- National Commission on Entrepreneurship, 2001. From the Garage to the Boardroom: The Entrepreneurial Roots of America's Largest Corporations," report.
- Palia, D., Ravid, S. Abraham, and Wang Chia-Jane , in press, "Founder versus Non-Founder in Large Companies: Financial Incentives and the Call for Regulation," forthcoming, *Journal of Regulatory Economics*.
- Pérez-González, F., 2006. Inherited control and firm performance. *American Economic Review* 96, 1559–1588.
- Sraer, D., Thesmar, David, 2007. Performance and behavior of family firms: evidence from the French stock market. *Journal of The European Economic Association* 5, 709–751.
- Villalonga, B., Amit, R., 2006. How do family ownership, management, and control affect firm value? *Journal of Financial Economics* 80, 385–417.
- Warner, Jerold B., Watts, Ross L., Wruck, Karen H., 1988. Stock prices and top management changes. *Journal of Financial Economics* 20, 461–492.
- Wasserman, N., 2003. Founder–CEO succession and the paradox of entrepreneurial success. *Organization Science* 14, 149–172.
- Weisbach, M.S., 1988. Outside directors and CEO turnover. *Journal of Financial Economics* 20, 431–460.
- Wooldridge, J.M., 2002. *Econometric Analysis of Cross Section and Panel Data*. MIT Press, Cambridge, MA.