Estimating the Value of CEOs in Privately Held Businesses*

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Abstract

We estimate CEO value in private firms using Hungarian administrative data covering over one million firms and CEOs from 1992-2022. Our model separates owner-controlled inputs (capital, location) from CEO-controlled inputs (labor, materials). We introduce a placebo-controlled event study comparing actual CEO transitions to randomly assigned fake transitions in stable firms. Raw estimates suggest 22.1 percent performance differences between good and bad CEOs, but 14.8 percent is mechanical noise. The true causal effect is 7.4 percent—only 33 percent of the raw correlation. Three-quarters of apparent CEO variation is spurious, explaining weak results in studies using manager fixed effects as explanatory variables.

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JEL Classification: [To be added]

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1 Introduction

To what extent do CEOs contribute to firm performance? Good management can improve firm productivity, as suggested by interventions that improve management practices (Bloom et al., 2013), training programs that build capabilities (McKenzie and Woodruff, 2021), or leadership changes that reshape organizations (Bertrand and Schoar, 2003; Bennedsen et al., 2020; Metcalfe et al., 2023). Quantifying individual CEO contribution is complicated because modern businesses rely on a variety of inputs: tangible and intangible capital (plants, production lines, technical and organizational know-how, brand value), labor, intermediate inputs, and raw materials. Among these, only managerial capital is under CEO control, and its effects are difficult to isolate from other inputs.

Starting from Bertrand and Schoar (2003), event studies around CEO changes attribute all changes in observable business policies to leadership changes. In public companies with dispersed ownership, CEOs and boards of directors have broad power in running the business. In private firms, ownership concentration gives owners direct control over strategic choices, creating a division of decision rights that blurs attribution of outcomes to CEOs versus owners (Fama and Jensen, 1983; Jensen and Meckling, 1976; Burkart et al., 2003). Owners may have primary decision rights on large investment projects, branding, and acquisitions. This distinction is empirically important: Quigley et al. (2022) find CEO effects are nearly twice as large in Swedish private firms compared to public ones, though our results suggest much of this apparent effect is noise. Accurately measuring CEO contribution in private businesses is important for studying the manager market and executive pay practices. Recent work has compared CEO pay to CEO value in public firms (Tervio, 2008; Gabaix and Landier, 2008), but similar analysis for private firms remains limited.

We make three contributions. First, we develop a model that accounts for the division of control between owners who set fixed inputs (tangible and intangible capital) and CEOs who optimize variable inputs (labor, materials). This framework clarifies what CEOs can and cannot affect, enabling cleaner identification of their contribution. Second, we assemble comprehensive administrative data covering the universe of Hungarian firms and their CEO networks over three decades (1992-2022), tracking over 1,063,172 firms and 339,993 CEOs. Third, we introduce a placebo-controlled event study design that separates true CEO effects from the mechanical noise that contaminates fixed effects estimates when managers have short tenures.

Our modeling approach treats CEOs in private firms as plant managers responsible for operational decisions. This simplification underestimates their strategic importance but provides a better approximation than models of superstar CEOs in public companies. This characterization aligns with evidence from family firms where professional managers face constraints on strategic decisions while maintaining operational autonomy (Zellweger, 2017), and with studies of multi-plant firms where headquarters retains capital allocation authority while delegating production decisions to plant managers (Bloom et al., 2012a).

Comprehensive administrative data from Hungary offers several advantages. Mandatory registration of all company directors, including CEOs, ensures complete coverage without selection bias. The transition economy context likely features greater variation in managerial quality than mature markets, enhancing statistical power. The large sample size and long time span enable construction of CEO mobility networks essential for identification. The institutional context matters: Crossland and Hambrick (2011) show that national institutions affect CEO discretion, with civil-law countries like Hungary typically granting less managerial autonomy than common-law systems, making our owner-CEO division particularly relevant.

Because CEO tenures in our data are short, estimated CEO effects contain substantial noise from averaging residual productivity shocks over few observations. This small-sample bias, known in the labor economics literature as "limited mobility bias" (Andrews et al., 2008), complicates interpretation of fixed effects estimates. To address this, we introduce a placebo-controlled event study design.

To illustrate our approach, consider a firm with the same CEO from 2000-2010. We randomly assign a fake transition in 2005, creating two pseudo-CEOs. If estimated 'effects' for these pseudo-CEOs diverge substantially, this reveals the noise problem in fixed effects estimation. By comparing actual CEO transitions to these placebo transitions, we can correct the fixed effects estimates for noise, isolating the true CEO contribution.

Our headline result challenges conventional wisdom about CEO importance. The naive comparison suggests firms hiring good CEOs outperform those hiring bad CEOs by 22.1 percent—a large effect consistent with the view that leadership quality is paramount. However, our placebo control reveals 14.8 percent of this difference arises from noise in the estimation process, not skill differences. The true causal effect of CEO quality is 7.4 percent: economically meaningful but only 33 percent of what raw correlations suggest. Three-quarters of apparent variation in CEO quality is spurious.

Our work connects to the broader literature on management practices and firm productivity. Randomized controlled trials demonstrate that management training and consulting improve firm performance (Bloom et al., 2013), but these interventions change practices rather than people. Whether replacing managers generates similar gains remains contentious. Evidence from public sector organizations suggests modest manager effects (Fenizia, 2022; Janke et al., 2024), while studies of family firms find larger impacts when professional managers replace family members (Bennedsen et al., 2007b). Our results for private firms fall between these extremes: CEOs matter, but less than raw correlations suggest.

Methodologically, our paper builds on the two-way fixed effects literature in labor economics that decomposes wages into worker and firm components (Abowd et al., 1999; Card et al., 2018). These studies face similar challenges from limited mobility creating small-sample bias (Andrews et al., 2008) and have developed bias-correction methods (Bonhomme et al., 2023; Gaure, 2014). We adapt this framework to the CEO-firm setting but add placebo controls to separate signal from noise. This approach is valuable when studying managers who, unlike workers, have few observations per individual, making traditional bias-correction methods less effective. Recent work has documented apparently increasing CEO effects over time (Quigley and Hambrick, 2015), but these studies do not account for the mechanical noise we identify. Lippi and Schivardi (2014) find that concentrated ownership in Italian firms distorts executive selection and reduces productivity by 10%, providing motivation for our framework separating owner and CEO decisions.

2 A Model of Production with Owner- and Manager-Controlled Inputs

Our model highlights a key institutional feature of private firms: the separation of strategic decisions (owner-controlled) from operational decisions (manager-controlled).[1] This division of decision rights, common in private businesses, affects how we identify manager effects: we cannot control for variable inputs when estimating manager productivity because variable input levels are an outcome of manager decisions and are correlated with productivity.[2]

Firms produce output using a Cobb-Douglas production function with both fixed and variable inputs. The production function for firm i with manager m at time t is:

$$Q_{imt} = A_i Z_m \varepsilon_{it} K_{it}^{\alpha} L_{imt}^{\beta} M_{imt}^{\gamma} \tag{1}$$

where A_i represents time-invariant organizational capital (location, brand value, customer relationships), Z_m captures manager skill, ε_{it} is residual productivity, K_{it} is physical capital, L_{imt} is labor input, and M_{imt} is intermediate input usage.

Standard production function estimation combines the first three components into a single measure: $\Omega_{it} = A_i Z_m \varepsilon_{it}$, called total factor productivity (TFP). Our framework decomposes TFP into firm-specific advantages (A_i), manager-specific skill (Z_m), and residual productivity shocks (ε_{it}) to identify the manager contribution.

We assume physical capital investment (K_{it}) and organizational assets (A_i), including location choices, brand development, and CEO hiring decisions are predetermined. Managers control labor hiring (L_{imt}) and input purchases (M_{imt}).

The production function exhibits decreasing returns to scale in variable inputs ($\beta + \gamma < 1$), which pins down optimal firm size even under perfect competition. Fixed inputs A_i and Z_m create firm-specific and manager-specific advantages that generate economic rents.

Managers maximize profit by choosing variable inputs optimally given the predetermined choices. Under sector-specific output price P_{st}, wage rate W_{st}, and material price ϱ_{st}, the first-order conditions yield closed-form solutions for optimal input demands. Substituting these back into the revenue function gives:

$$R_{imst} = (P_{st} A_i Z_m \varepsilon_{it})^{1/\chi} K_{it}^{\alpha/\chi} W_{st}^{-\beta/\chi} \varrho_{st}^{-\gamma/\chi} (1-\chi)^{(1-\chi)/\chi} \tag{2}$$

Revenue increases in manager skill Z_m, organizational capital A_i, and physical capital K_{it}, while decreasing in input prices. The elasticity of revenue with respect to manager skill is $1/\chi > 1$, with $\chi := 1 - \beta - \gamma$, the share of surplus in revenue.

The surplus accruing to fixed factors (factors other than labor and materials), which owners

[1] Both theoretical results (Fama and Jensen, 1983; Jensen and Meckling, 1976; Burkart et al., 2003; Schulze and Zellweger, 2021) and empirical evidence (Durand and Vargas, 2003; Gao and Li, 2015; **?**; **?**; Nakazato et al., 2011; Gompers et al., 2023; Bloom et al., 2012b,0; Buffington et al., 2017) confirm that in privately held firms and firms with concentrated ownership, managerial discretion on strategic decisions is limited and owner control dominates.

[2] A similar point was made by Gandhi et al. (2020), who emphasize that freely adjustable inputs are "bad controls" because they are determined by productivity.

and managers jointly maximize, equals revenue minus payments to variable inputs:

$$S_{imst} = R_{imst} - W_{st}L_{imt} - \varrho_{st}M_{imt} = \chi R_{imst} \tag{3}$$

Under Cobb-Douglas technology, surplus is a constant fraction χ of revenue. This proportionality allows us to work directly with revenue, simplifying estimation while preserving economic insights. Taking logarithms of the revenue equation:

$$r_{imst} = C + \frac{\alpha}{\chi}k_{it} + \frac{1}{\chi}z_m + \frac{1}{\chi}a_i + \frac{1}{\chi}p_{st} + \frac{1}{\chi}\epsilon_{it} - \frac{\beta}{\chi}w_{st} - \frac{\gamma}{\chi}\rho_{st} \tag{4}$$

where lowercase letters denote logarithms (e.g., $\epsilon_{it} = \log \varepsilon_{it}$) and C is a constant.

The value of replacing manager m with manager m' at the same firm is the extent to which surplus changes as the manager changes:

$$r_{im'st} - r_{imst} = \frac{1}{\chi}(z_{m'} - z_m) \tag{5}$$

Manager value equals the skill difference scaled by $1/\chi$. This scaling reflects a leverage effect: a 1% better manager hires more variable inputs, thereby increasing revenue and surplus by more than 1%.

Our model abstracts from corporate governance frictions that could lead managers to make suboptimal decisions. These frictions are less problematic in concentrated ownership settings where owners retain control over strategic choices. While agency problems may affect long-term vision, risk-taking, and entrepreneurship, the incentive to increase revenue and reduce operating costs remains aligned between owners and managers in most private firms.

The model excludes dynamic considerations such as adjustment costs or forward-looking behavior. Strategic decisions are forward-looking due to their persistence, but we treat them as predetermined from the manager's perspective. Even though the optimization framework is static, our empirical application allows for arbitrary time-series correlation within and across variables. The data validate this choice: strategic variables like capital evolve slowly with little correlation to manager changes, while operational variables adjust immediately following CEO transitions.

3 Corporate Data from Hungary

Hungary provides an ideal setting for studying CEO effects in private firms. The country offers complete administrative data coverage for all incorporated businesses with mandatory CEO registration, spanning 30 years from the transition economy of the 1990s through EU accession in 2004 to the present. This coverage enables us to track CEO careers across firms and construct mobility networks necessary for identification.

Our analysis combines two administrative datasets. The firm registry, maintained by Hungarian corporate courts, contains records on all company representatives—individuals authorized to act on behalf of firms. These records include CEOs and other executives with signatory rights, tracked through a temporal database where each entry reflects representation status over specific time intervals. Updates occur when positions change, personal identifiers are modified, or reporting standards evolve. The registry provides names, addresses, dates of birth (from 2010), and mother's names (from 1999), though numerical identifiers exist only from 2013 onward.

The balance sheet dataset contains annual financial reports for all Hungarian firms required to file statements. This includes sales revenue, export revenue, employment counts, tangible and intangible assets, raw material and intermediate input costs, personnel expenses, and indicators for state and foreign ownership. The two datasets cover 1,063,172 firms over 31 years, yielding 9,627,484 firm-year observations before sample restrictions.

Identifying individual CEOs poses some challenges. Before 2013, no numerical identifiers existed, requiring entity resolution based on names, addresses, mother's names, and birthdates. We link records across these dimensions to create unique person identifiers, enabling tracking across firms and over time. Matching quality improves after 1999 (when mother's names reporting commences) and 2010 (when birthdates reporting starts), though the 1990s data achieves reasonable coverage through name and address matching.

CEO identification within firms requires heuristics since job titles are inconsistently recorded. When explicit "managing director" titles exist, we use them directly. For remaining cases, we assume all representatives are CEOs if three or fewer exist at the firm. When more than three representatives are present, we assign CEO status based on continuity with previously identified CEOs. Time spans between appointments are often unclosed or non-contiguous, requiring imputation based on sequential information, assuming representatives remain active if their tenure includes June 21 of each year.

We apply several restrictions to create a sample suitable for productivity analysis. First, we exclude mining and finance sectors due to specialized accounting frameworks and regulatory environments. Second, we drop firms ever having more than 2 simultaneous CEOs (removing observations for firms with complex governance) to avoid complex governance structures that complicate identification. Third, we exclude firms with more than 10 CEO changes over the sample period (removing observations for unstable firms) to reduce noise from misclassified transitions. Fourth, we remove all state-owned enterprises, as their objectives and constraints differ from private businesses (Orbán, 2019).

We restrict attention to firms that employ at least 5 workers at some point in the firm lifecycle. This filter removes a substantial portion of observations but eliminates shell companies, tax optimization vehicles, and self-employment arrangements masquerading as corporations. The remaining firms represent genuine businesses with meaningful economic activity where management quality affects performance.

We exclude public firms and joint-stock companies from our analysis. The few companies traded on the Budapest Stock Exchange operate under different governance structures, compensation schemes, and disclosure requirements than private businesses. We also exclude cooperatives and other non-standard corporate forms where multiple managing directors share executive authority, as these organizational structures complicate identification of individual CEO effects.

Given the 1990s' rapid economic liberalization, transition to market economy, and foreign direct investment inflows, we conducted robustness checks restricting the sample to post-2004 data following Hungary's EU accession. The results remain unchanged, confirming our findings

Table 1: Sample Over Time

Year	Total firms	Sample firms	CEOs	Connected component	
				Firms	CEOs
1992	98,780	25,833	31,746	1,423	1,713
1995	171,759	45,828	53,704	2,659	3,028
2000	280,386	73,837	83,862	4,783	5,100
2005	326,905	92,242	104,380	6,283	6,474
2010	384,570	103,892	116,680	7,405	7,084
2015	433,371	116,543	124,960	8,332	7,488
2020	424,501	115,755	123,504	7,789	6,841
2022	454,106	113,387	121,730	7,419	6,509
Total	1,063,172	217,737	339,993	14,416	22,001

Notes: This table presents the evolution of the sample from 1992 to 2022. Column (1) shows the total number of distinct firms with balance sheet data. Column (2) shows the number of distinct firms after applying data quality filters. Column (3) shows the number of distinct CEOs. Columns (4) and (5) show the subset of distinct firms and CEOs that belong to the largest connected component of the manager network, where managers are connected if they have worked at the same firm. The table shows every fifth year plus the first year (1992), last year (2022), and totals of distinct counts.

are not driven by the transition period's institutional environment (see Figure 1 below).

CEO mobility creates the manager-firm network essential for fixed effects identification. The largest connected component of the bipartite firm-CEO network contains 22,001 managers and a comparable number of firms, 6.5 percent of all managers but representing a substantial share of economic activity.

CEO tenure varies across firms. While 63 percent of firms retain the same CEO throughout their observed lifetime, the remaining firms experience leadership transitions that enable identification. Among firm-years with CEO information, 80 percent have single CEOs, 17 percent have two, and 3 percent have more (before our sample restrictions). CEO spell lengths follow an exponential distribution with a 20 percent annual hazard rate, implying typical tenures of 3-7 years.

Table 2: CEO Patterns and Spell Length Analysis

Panel A: Number of CEOs per Firm

CEOs	Firm-Year	Firm
1	80%	63%
2	17%	24%
3	2%	8%
4+	1%	5%
Total	9,627,484	1,012,113

Panel B: Spell Length Distribution

Length (Years)	Actual Spells	Placebo Spells
1	22%	27%
2	15%	18%
3	12%	14%
4+	52%	41%
Total	99,328	13,860

Notes: Panel A shows the distribution of CEO counts per firm across firm-years and firms. Panel B compares the spell length distribution between actual CEO transitions and synthetically generated placebo transitions. The similar distributions validate our placebo methodology.

4 Estimation

Our estimation proceeds in four steps: measuring the surplus share (the share of revenue accruing to fixed factors), estimating the revenue function, recovering manager fixed effects, and validating causality through event studies. Each step builds toward separating true CEO effects from the noise that contaminates raw estimates.

Step 1: Measuring the Surplus Share. The parameter χ—the share of surplus in revenue—determines how manager skill translates into firm performance. Under Cobb-Douglas technology, this share equals one minus the combined revenue shares of labor and materials. Following Gandhi et al. (2020), we measure χ from the data as:

$$\hat{\chi}_s = 1 - \frac{\sum_{i \in s}(W_{st} L_{it} + \varrho_{st} M_{it})}{\sum_{i \in s} R_{it}} \tag{6}$$

where the summation runs over firms in sector s. This approach yields sector-specific estimates of χ. The $1/\chi$ scaling in our framework creates a leverage effect where manager skill is amplified in its impact on firm performance.

Step 2: Estimating the Revenue Function. With $\hat{\chi}$ measured, we estimate the revenue function to recover the capital elasticity and control for observable factors. Using lowercase letters for logarithms, the estimating equation is the empirical counterpart of (4):

$$r_{imst} = \frac{\alpha}{\chi} k_{it} + \frac{1}{\chi} z_m + \lambda_i + \mu_{st} + \tilde{\epsilon}_{it} \tag{7}$$

where $r_{imst} = \log R_{imst}$ is log revenue, $k_{it} = \log K_{it}$ is log capital, λ_i captures firm fixed effects, μ_{st} are sector-year fixed effects, and $\tilde{\epsilon}_{it} = \epsilon_{it}/\chi$ is rescaled residual productivity. We include controls for firm age, intangible asset presence, and foreign ownership, though these minimally affect the capital coefficient.

The key assumptions are: (1) all firms within a sector face the same prices, and (2) for each manager, residual productivity $\tilde{\epsilon}_{it}$ has zero mean when averaged across all their firms and time periods: $E[\bar{\epsilon}_m] = 0$ where $\bar{\epsilon}_m = \frac{1}{N_m} \sum_{i,t \in m} \tilde{\epsilon}_{it}$ and the sum runs over all firm-year observations under manager m.

We do not require random manager mobility or that residual productivity has zero mean at the point of CEO transition. Manager assignment can be endogenous: good managers may systematically move to firms experiencing positive shocks or be hired when firms anticipate improvements. We only require that these shocks average to zero over a manager's entire career. This is a weaker assumption than random assignment but still substantive: it rules out managers who systematically arrive at permanently improving (or declining) firms.

We estimate using OLS with high-dimensional fixed effects via `reghdfe` (Correia, 2023). The coefficient on log capital is $\hat{\alpha}/\hat{\chi}$, which we multiply by $\hat{\chi}$ to recover $\hat{\alpha}$.

Step 3: Recovering Manager Fixed Effects. After estimating the revenue function, we compute log total factor productivity by removing the contribution of capital and sectoral prices from revenue:

$$\omega_{imst} = \hat{\chi} r_{imst} - \hat{\alpha} k_{it} - \hat{\mu}_{st} = z_m + \lambda_i + \epsilon_{it} \tag{8}$$

This measure of log TFP contains manager skill, firm effects, and residual productivity. In standard production function estimation, this entire term would be treated as a single TFP measure. Our decomposition separates the manager contribution from other sources of productivity.

We estimate a two-way fixed effects model with firm and manager fixed effects. Our identification relies on the zero-mean condition described above: residual productivity must average to zero for each manager across their career. This allows for various forms of endogenous mobility but rules out systematic patterns where managers consistently join permanently improving or declining firms.

The event study provides a diagnostic test for this identification assumption. Pre-trends in productivity before CEO transitions would suggest (though not prove) that the zero-mean assumption is violated. If productivity systematically rises before good CEOs arrive, we worry that the positive trend continues post-transition, violating $E[\bar{\epsilon}_m] = 0$. Conversely, the absence of pre-trends makes it harder to construct plausible endogeneity stories. While we cannot rule out contemporaneous shocks that coincide exactly with CEO changes (e.g., owners simultaneously firing the CEO and adopting new technology), such precise timing is less plausible than gradual changes that would manifest as pre-trends. Our event studies show no significant pre-trends, supporting but not proving our identification assumption.[3]

The system of fixed effects is identified only within connected components: groups of firms and managers linked through mobility. Two managers can be compared if they worked at the same firm or connect through a chain of shared employment relationships. We can estimate $\hat{z}_m$ for every manager, but only up to a constant term that may vary across connected components. Our largest connected component contains 22,001 managers, enabling comparisons within this network. We normalize the manager effect to be mean zero in the largest connected component.

The connected component represents a non-random subset of the economy, with member firms larger than average. This selection is not problematic for our identification strategy since we allow manager effects to correlate arbitrarily with observable characteristics. We cannot extend the analysis to singleton firms or disconnected components where manager fixed effects lack a common reference point for interpretation.

Step 4: Placebo-Controlled Event Studies. Even when ϵ is orthogonal to z, estimated fixed effects contain substantial small-sample noise when manager transitions are infrequent and manager tenures are short.[4]

To understand the sources of small-sample bias and how we address it, we remove the firm

[3]The absence of strong pre-trends in our data contrasts with evidence from Cornelli et al. (2013) showing boards actively monitor and replace CEOs when performance deteriorates in public firms, suggesting our private firm transitions may be less performance-driven. While Jenter and Lewellen (2021) find 38-55% of turnovers are performance-induced in U.S. public firms, our private firm setting likely features more random CEO transitions given the absence of market pressures and board oversight.

[4]Worker-firm fixed effect studies face similar challenges called "limited mobility bias" (Andrews et al., 2008) and have developed bias-correction methods (Bonhomme et al., 2023; Gaure, 2014).

fixed effect from TFP by subtracting the firm average:

$$\Delta\omega_{imt} = \Delta z_{m_{it}} + \Delta\epsilon_{it}, \tag{9}$$

where $\Delta x_{it} := x_{it} - \frac{1}{N_i}\sum_\tau x_{i\tau}$ denotes the deviation of a variable from its within-firm mean. When a firm changes CEOs, the change in log TFP captures both the true skill difference and accumulated noise. The noise component—the average of residual productivity shocks during each manager's tenure—dominates the signal when tenures are short.

Our solution leverages a simple insight: when CEOs do not change, we still observe variation in log TFP driven purely by noise:

$$\Delta\omega_{imt} = \Delta\epsilon_{it}. \tag{10}$$

By applying the same estimation procedure to "non-changes," we can measure and filter out the noise component.

We construct placebo CEO transitions in three steps. First, we estimate the time-varying hazard of actual CEO changes. Second, we identify firms with long CEO tenures (7+ years) where no actual change occurs. Third, we randomly assign placebo transitions to these stable firms using the estimated hazard function.

For instance, a firm with the same CEO from 1992-2000 might receive a placebo transition in 1996, artificially splitting the tenure into two pseudo-CEOs. The timing follows the empirical hazard, ensuring the mechanical noise properties—averaging residual productivity over varying tenure lengths—match between actual and placebo groups.

To validate our placebo methodology, we verify that synthetically generated CEO transitions match actual patterns. Taking firms with stable leadership (same CEO for 7+ years), we randomly assign placebo transitions using the empirical hazard function. The resulting spell length distribution mirrors actual CEO changes (Panel B of Table 2).

We implement the event study around CEO transitions at time g, comparing actual changes (treatment) to placebo changes (control):

$$\omega_{imt} = a_i + \gamma_{t-g} + \epsilon_{it} \tag{11}$$

The coefficients γ_{t-g} capture the evolution of log TFP in event time, where $t - g \in [-4, 3]$ and we normalize $\gamma_{-1} = 0$.

Because both groups receive a "treatment" (actual or placebo), standard difference-in-differences cannot be used. We adapt the Callaway and Sant'Anna (2021) estimator for two treatment types using the `xt2treatments` package (Koren, 2024). The key innovation is precisely aligning transitions in event time—both actual and placebo changes occur at $t = 0$, enabling a clean comparison of dynamics between treated and control firms.

5 Results

5.1 Production Function Estimates

Table ?? in the Appendix reports our estimates of the surplus share χ by industry. The estimates range from 0.06 to 0.19 across included sectors, with wholesale, retail, and transport showing the lowest values and professional services the highest. These estimates imply that a 1% increase in TFP increases revenue by 5 to 16 percent through the leverage effect of scaling variable inputs.

Table ?? presents the revenue function estimates. We include controls for log capital, firm age, presence of intangible assets, and foreign ownership, along with firm-CEO and sector-year fixed effects. The capital coefficient is precisely estimated at 0.333 (standard error 0.001), consistent with capital's limited but significant role in private businesses. The intangible asset dummy shows a positive coefficient of 0.277, while foreign ownership contributes 0.027 to log revenue. Firm age exhibits diminishing returns with a coefficient of -0.094 on log age. These controls explain 76% of the variation in log revenue. Robustness checks varying the control set and sample restrictions, reported in the Appendix, yield similar estimates.

Founder-managed firms exhibit lower revenue conditional on owner inputs, implying lower total factor productivity—consistent with evidence that family firms sacrifice growth for control (Bennedsen et al., 2007b). The performance penalty from family succession is well-documented, with heir CEOs reducing ROA by 6 percentage points in Danish firms (Bennedsen et al., 2007a).

Table 3: Surplus Function Estimation Results

	(1) Revenue	(2) EBITDA	(3) Wagebill	(4) Materials	(5) Revenue	(6) Revenue
Fixed assets (log)	0.333***	0.340***	0.300***	0.384***	0.324***	0.326***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.006)
Has intangible assets	0.277***	0.171***	0.279***	0.324***	0.268***	0.380***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.015)
Foreign owned	0.027**	0.017	0.074***	0.012	0.029**	0.050
	(0.013)	(0.013)	(0.013)	(0.015)	(0.013)	(0.034)
Founding owner	-0.094***	-0.063***	-0.030***	-0.114***	-0.083***	-0.138***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.017)
Non-founding owner	-0.005	-0.006	-0.005	-0.010	0.001	0.009
	(0.007)	(0.006)	(0.007)	(0.009)	(0.007)	(0.023)
Observations	2877766	2248678	2823989	2930821	2877766	195221

Standard errors in parentheses
All models include firm-CEO-spell fixed effects and industry-year fixed effects. Outcome variables are log-transformed. Models (5) and (6) include quadratic controls for firm age and CEO tenure.
Model (6) restricts to largest connected component.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The stability of our estimates across samples validates the connected component analysis. Revenue function coefficients are nearly identical when estimated on the connected component versus the full sample (Table 3, Model 6), suggesting production technology and manager effects operate similarly across both groups. While selection into the connected component is non-random—these firms are larger and hire external managers more often—the economic relationships appear invariant to this selection, supporting our use of the connected component to identify manager fixed effects.

5.2 The Placebo Test

Figure 1 presents a four-panel analysis. Panel (a) compares TFP of firms receiving a better CEO (improving Z_m) to those receiving a worse CEO (declining Z_m). The two lines correspond to actually treated firms and placebo-treated firms, allowing us to isolate true CEO effects from mechanical noise. There are substantial pretrends in the raw data, but these are mirrored in the placebo group, indicating they arise from noise rather than true skill differences.

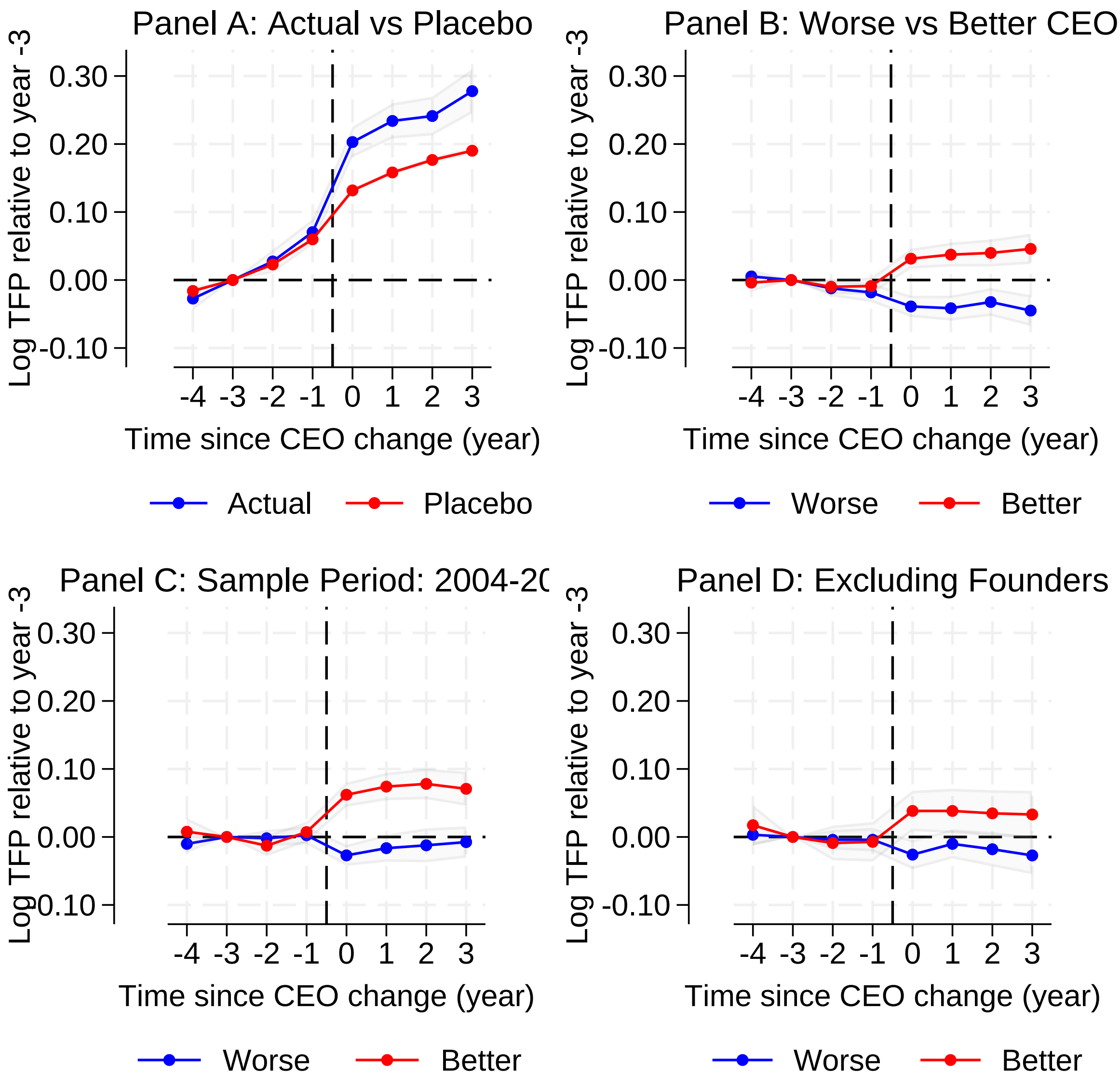

Figure 1: Placebo-Controlled Event Studies of CEO Transitions

Using the placebo-treated firms as control (Panel b), we can isolate the true CEO effect. The difference between the two lines in Panel (b) shows that better CEOs increase TFP by 4.4

percent, whereas worse CEOs decrease it by 3.3 percent, yielding a difference of 7.7 percent. This is about a third of the raw estimate. The absence of pretrends suggest that TFP changes are driven by CEO changes rather than the other way around.

Panel (c) confirms robustness in the post-2004 period, excluding the first decade of transition. If anything, the CEO effects are slightly larger in this more stable institutional environment. Panel (d) shows the effects of outsider-to-outsider transitions, excluding founder-managed firms. This setting provides the cleanest identification of CEO effects separate from ownership changes, albeit in a smaller sample, and, hence, larger standard errors.

Appendix Figure A2 presents complementary evidence using the variance of outcomes, which increases sharply at CEO transitions. This is consistent with real heterogeneity in CEO quality, which implies greater outcome dispersion when CEOs change.

5.3 Validation: Differential Effects on Manager vs Owner Variables

Our model predicts that CEOs should primarily affect outcomes they control (labor, materials) rather than those controlled by owners (capital, organizational structure). Figure 5.3 presents event studies for owner-controlled variables (fixed assets, intangibles, foreign ownership) and manager-controlled variables (employment, materials, revenue).

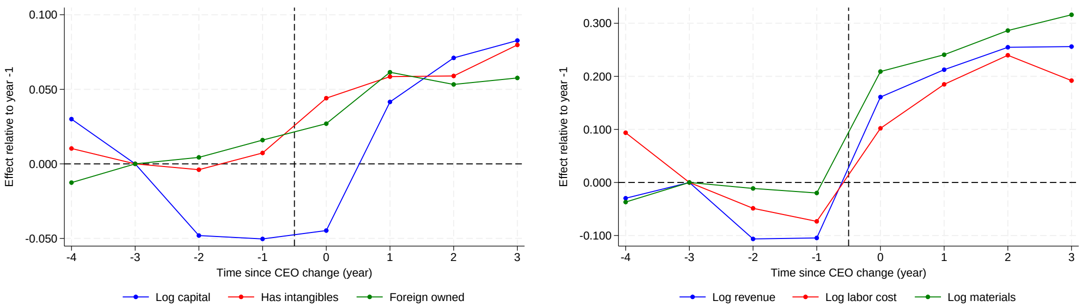

Figure 2: Evolution of Owner- and Manager-Controlled Variables Under New CEOs

Good CEOs have immediate and substantial effects on manager-controlled variables. Log employment increases by 18.7 percent, log materials by 28.0 percent, and log revenue by 28.1 percent, all effects highly significant. (See Appendix A5 for estimated treatment effects and standard errors.) These effects appear immediately in year 0 and persist throughout the post-period, consistent with new CEOs quickly adjusting operational scale.

In contrast, owner-controlled variables show different patterns. Fixed assets exhibit no significant change (5.5 percent, not statistically different from zero). Foreign ownership probability and intangible asset use increase gradually over time, rather than jumping immediately. While the average effect is significant for these inputs, the slow buildup is consistent with our assumption that managers cannot quickly change these inputs.

6 Conclusion

Our main finding: three-quarters of apparent CEO effect is spurious. The naive comparison of firms hiring good versus bad CEOs suggests a 22.1 percent performance difference. Our placebo-controlled event study reveals 14.8 percent of this gap arises from mechanical noise in fixed effects estimation. The true causal effect is 7.4 percent—a quarter of what raw correlations suggest.

CEOs matter modestly, with their primary impact through scaling variable inputs. Better managers expand operations by immediately increasing employment and material purchases, generating higher revenues within the constraints of owner-controlled capital. The 7.4 percent productivity advantage translates into 28.1 percent higher revenues through this scaling mechanism. The immediate response of manager-controlled variables contrasted with unchanged capital stocks validates our model's division of decision rights.

These findings have implications for corporate governance and productivity research. The widespread practice of using manager fixed effects as explanatory variables is flawed—with 67 percent noise contamination, such estimates suffer from severe attenuation bias. This explains the weak and inconsistent relationships found in studies relating manager "quality" to firm policies or outcomes. Researchers should not trust raw CEO fixed effects as measures of managerial ability.

Future research should use observable manager characteristics. Education and work experience (De Pirro et al., 2025), foreign name as a proxy for international exposure (Koren and Telegdy, 2023), and selectiveness of entry cohorts (Koren and Orbán, 2024) offer more reliable, though narrower, measures of specific dimensions of managerial quality. These observables capture only partial aspects of CEO ability but avoid the mechanical noise that contaminates fixed effects estimates.

Our placebo-controlled approach offers a general solution for short-panel settings where traditional methods fail. This extends beyond CEOs to teachers, doctors, judges, or any setting where individual effects are estimated from limited observations. The method requires only the ability to construct credible placebo treatments—randomly splitting stable spells in our case—making it widely applicable. Rather than attempting complex analytical corrections that become unreliable with few observations, the placebo approach directly measures and removes the noise component.

Our findings offer both methodological and substantive contributions. The placebo control technique provides a practical tool for future research on manager effects, applicable beyond our specific context. For policy, the modest but real CEO effects suggest that while management quality matters, other factors—organizational capital, market position, ownership structure—play larger roles in firm performance. Understanding this balance helps calibrate expectations about what management improvements can realistically achieve in private businesses.

Our findings reconcile conflicting evidence about management importance in the literature. While management consulting interventions show large effects, studies using manager fixed effects find inconsistent results. Our decomposition reveals why: most apparent manager variation is noise, not signal. This has broader implications for understanding productivity differences across firms. Management quality matters, but less than commonly believed. Other factors—organizational capital, market position, institutional environment—likely play larger roles in explaining firm performance differences. For economic development policy, this suggests that improving institutional frameworks and reducing market frictions may be more important than focusing solely on management training programs.

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Table A1: Plant Manager Autonomy in Family-Controlled Firms

	(1) Investment	(2) Investment	(3) Marketing	(4) Product	(5) Hiring
Family ownership	-0.369**	-0.200**	-0.344**	-0.299**	0.086
	(0.161)	(0.100)	(0.153)	(0.151)	(0.068)
Observations	2,915	2,379	3,133	3,114	3,138
Country FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Data source: Bloom, Sadun, and Van Reenen (2012). Sample restricted to private (non-publicly traded) firms.

Investment autonomy measured as maximum capital investment plant manager can approve (USD).

Other autonomy dimensions are binary indicators for full autonomy (score = 5 on 1-5 scale).

PPML = Poisson Pseudo-Maximum Likelihood. Standard errors clustered at firm level.

All specifications include country and 2-digit SIC industry fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Industry Breakdown

Industry (NACE)	Obs.	Firms	CEOs	Surplus share (%)
Agriculture, Forestry, Fishing (A)	320,485	25,951	55,535	7.9
Manufacturing (C)	1,022,252	90,035	179,205	13.7
Wholesale, Retail, Transportation (G,H)	2,887,229	296,237	550,110	6.4
Telecom, Business Services (J,M)	1,968,969	186,013	345,304	18.8
Construction (F)	962,938	112,633	183,144	11.4
Nontradable Services (Other)	2,775,169	277,749	527,661	13.5
Mining, Quarrying (B)*	13,428	1,151	2,922	23.8
Finance, Insurance, Real Estate (K,L)*	201,527	22,344	48,153	48.0

Notes: This table presents industry-level summary statistics using the TEAOR08 classification system. Column (1) shows the industry name and corresponding NACE sector codes. Column (2) shows the total number of firm-year observations in the balance sheet data (1992-2022). Column (3) shows the number of distinct firms with balance sheet data. Column (4) shows the number of distinct managers (CEOs) from the firm registry data. Column (5) shows the average EBITDA as a percentage of revenue. Mining (sector B) and Finance/Insurance/Real Estate (sectors K,L) are excluded from the main analysis due to different production function characteristics. The NACE classification follows the Hungarian adaptation of the NACE Rev. 2 system.

Table A3: The revenue function by sector

	(1) Agriculture	(2) Manufacturing	(3) Wholesale, Retail, Transportation	Tele
Tangible and intangible assets (log)	0.320***	0.296***	0.257***	
	(0.006)	(0.003)	(0.002)	
Intangible assets share	0.071	0.011	-0.006	
	(0.059)	(0.025)	(0.014)	
Foreign owned	-0.070*	0.046*	0.008	
	(0.042)	(0.024)	(0.015)	
Observations	208269	748880	1893882	

Controls: firm-CEO-spell fixed effects; industry-year fixed effects.

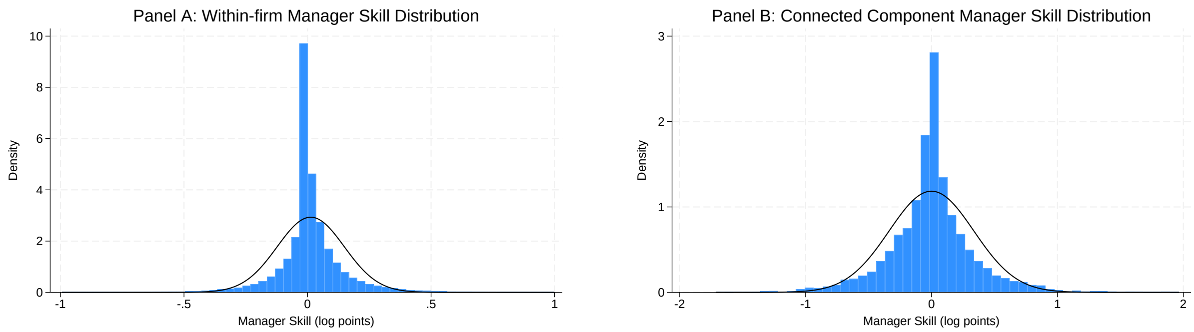

Figure A1: Manager Skill Distributions

Notes: Panel A shows the distribution of within-firm manager skill variation for firms with multiple CEOs. Panel B shows the distribution of manager skills in the largest connected component of managers. Both distributions show manager skills in log points after normalization and scaling.

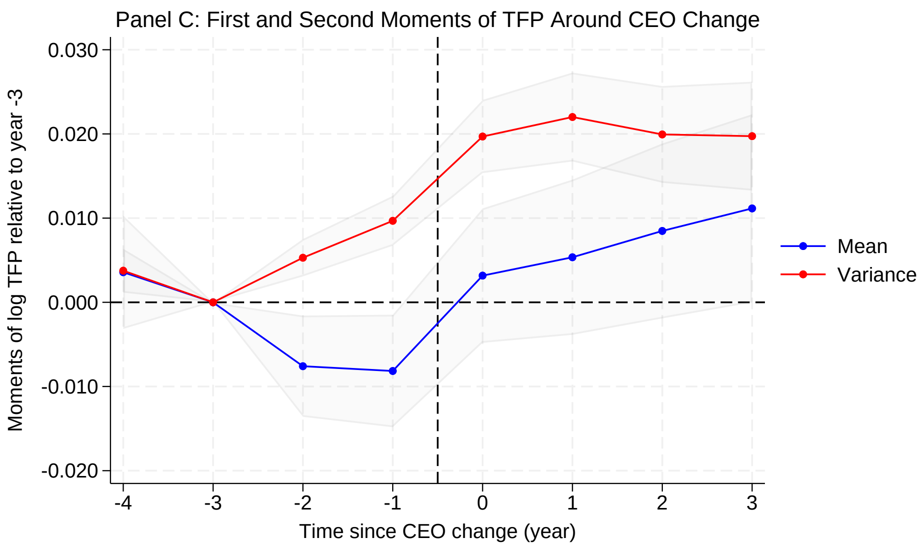

Figure A2: TFP Moments Around CEO Transitions

A Online Appendix: Additional Tables and Figures

A.1 Production Manager Autonomy on Family Firms

A.2 Industry Breakdown

A.3 Manager Skill Distributions

A.4 Variance-Based Evidence for CEO Heterogeneity

Figure **??** presents complementary evidence using the variance of log TFP around CEO transitions. Under our framework, if CEO changes introduce real heterogeneity in managerial quality, the cross-sectional variance of outcomes should increase at the transition point—some firms get better CEOs, others get worse ones. In contrast, pure noise or firm-specific trends would not systematically alter variance.

The variance analysis shows that actual CEO transitions are associated with increased dispersion in outcomes, while placebo transitions show no such effect. This provides model-free evidence that CEO transitions introduce real heterogeneity in firm performance, supporting our identification strategy.

A.5 Treatment Effects on Owner vs Manager Variables

	(1)	(2)	(3)
	Fixed assets (log)	Has intangible assets	Foreign owned
Better CEO	0.055	0.057***	0.048***
	(0.076)	(0.019)	(0.012)
Observations	81181	81181	81181

	(1)	(2)	(3)
	Sales (log)	Wagebill (log)	Materials (log)
Better CEO	0.281***	0.187***	0.280***
	(0.067)	(0.064)	(0.069)
Observations	81181	81181	81181