Estimating the Value of CEOs in Privately Held Businesses[*]

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Abstract

We develop a framework to estimate CEO value in private firms using administrative data, overcoming traditional measurement challenges. Our theoretical model assumes managers differ in skills that directly affect total factor productivity, with firms retaining rents accruing to fixed factors like organizational capital. We combine three identification strategies: within-firm variation, manager mobility networks, and quasi-experimental event studies around CEO transitions. Applied to comprehensive Hungarian data spanning 1992-2022, we find substantial CEO heterogeneity: replacing a CEO at the 25th percentile of the skill distribution with that at the 75th percentile would increase firm productivity by 9.8-25.6 percent. Event study evidence confirms these skill differences have large causal impacts, with firms hiring better managers showing 28 percentage point higher surplus immediately following transitions. Manager skills explain about a quarter of the within-firm variation and 5–9 percent of the cross-firm variation in outcomes.

Keywords: CEO value, private firms, productivity

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1 Introduction

Managers play a crucial role in determining firm performance, as documented across various institutional settings (Bertrand & Schoar, 2003; Fisman et al., 2014; Bandiera et al., 2020; Bennedsen et al., 2020). The magnitude of these effects can be substantial: Bertrand & Schoar (2003) find that switching from a 25th- to 75th-percentile CEO alters return on assets by about 4 percentage points in U.S. public firms, while Bandiera et al. (2020) show that "leader"-type CEOs raise firm productivity by around 8 percent across six countries. Recent evidence using quasi-experimental variation confirms the importance of managerial talent: Bennedsen et al. (2020) exploit unexpected CEO hospitalizations in Danish private firms and find that even a one-week CEO absence lowers return on assets by 7 percent.

Most existing studies in this literature focus on publicly listed firms in developed markets such as the United States. Evidence from developing countries suggests that management practices are equally important for firm performance in these contexts (Bloom et al., 2014). However, the relevance of existing evidence on CEO value remains limited for developing markets for two key reasons. First, in developing markets, most firms are privately held and do not have publicly traded shares or readily available information about executive compensation of the sort surveyed in Frydman & Saks (2010). This creates a fundamental measurement challenge for researchers. Second, in private businesses, owners typically retain direct control over the firm, which may generate different incentive structures and performance outcomes compared to publicly listed firms with dispersed ownership.

In this paper, we develop a framework to estimate the value of CEOs in privately held businesses using standard financial statement data and administrative registers, data sources that are commonly collected by governments in developing countries. This approach enables the measurement of CEO value in settings where traditional methods based on stock market valuations or executive compensation data are not feasible.

Our theoretical model builds on Lucas (1978) and assumes that managers differ in their skills, which directly affect the total factor productivity of the firm. Following Atkeson & Kehoe (2005); McGrattan (2012), we assume that firms retain economic rents due to decreasing returns to scale in the presence of manager skills, organizational capital, and intangible assets. This framework allows us to identify the marginal contribution of CEO skills to firm surplus while controlling for other sources of firm heterogeneity.

Empirically, our approach combines three complementary identification strategies. First, we adapt the two-way fixed effects methodology of Abowd et al. (1999) from worker wages to CEO productivity, leveraging both within-firm variation and manager mobility networks to identify individual CEO effects. Second, we conduct quasi-experimental event studies around CEO transitions to provide causal evidence on the impact of skill differences. This multi-pronged approach addresses key concerns about manager-firm sorting and provides robust evidence on CEO value.

We apply this framework to comprehensive administrative data from Hungary covering the period 1992-2022. The dataset enables us to track CEO changes across firms and measure their impact on firm performance, specifically on the economic surplus generated by the firm. Our event study focuses on 51,758 firms experiencing exactly one CEO change, allowing us to examine performance trajectories around management transitions.

Our analysis yields four main findings. First, CEO skills exhibit substantial heterogeneity both within firms over time and across the broader managerial labor market. Within firms, replacing a 25th percentile manager with a 75th percentile manager increases productivity by 9.8 percent, while across the connected component of mobile managers, the same replacement increases productivity by 25.6 percent. Second, event study evidence confirms these skill differences have large causal impacts: firms hiring better managers show 28 percentage point higher surplus immediately following CEO transitions compared to firms hiring worse managers. Third, these treatment effects persist over time, with performance differences remaining substantial even a decade after management changes. Fourth, manager skills account for meaningful variation in firm outcomes, explaining 5-9 percent of within-industry variation in revenue, surplus, and employment.

2 Modeling Framework

Firms produce output using a Cobb-Douglas production function that incorporates both fixed and variable inputs. Owing to the presence of fixed inputs, technology exhibits decreasing returns to scale. This will pin down the scale of the firm even when markets are perfectly competitive and the firm is a price taker in both input and output markets (Atkeson & Kehoe, 2005; McGrattan, 2012).[1]

The production function for firm i with manager m at time t is:

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

where Ω_{it} is residual total factor productivity, A_i represents time-invariant organizational capital and immaterial assets (location, brand value), Z_m captures manager skill, K_{it} is physical capital, L_{imt} is labor input, M_{imt} is intermediate input usage. The parameters α, β and γ represent the elasticities with respect to physical capital, labor and material inputs, respectively. We denote $\chi := 1 - \beta - \gamma$. Conditional on productivity, organizational capital and manager skill, the production function exhibits decreasing returns to scale, $\alpha+\beta+\gamma < 1$. In a traditional production function with only capital, labor and material as inputs, Ω, A and Z would all be lumped together as *total factor productivity*.

We assume managers optimize variable inputs L_{imt} and M_{imt} while taking fixed inputs A_i and Z_m and physical capital K_{it} as given. In private businesses, owners typically have direct control over fixed inputs, including large-scale investments in organizational and physical capital (Barba Navaretti et al., 2010). Managers, on the other hand, are responsible for day-to-day operations and variable input choices.

Output is sold at sector-specific price P_{st}, making the revenue of the firm $R_{imst} = P_{st} Q_{imt}$. The firm faces a wage rate W_{st} for labor input, price ϱ_{st} for intermediate inputs. After straightforward algebra solving for the optimal labor and intermediate input choices, the firm's revenue can be expressed as:

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

Revenue is increasing in fixed inputs A_i and Z_m, physical capital K_{it}, and decreasing in the wage rate W_{st} and material input price ϱ_{st}. Higher prices P_{st} and productivity Ω_{it} also increase revenue. Note that because $\chi < 1$, the elasticity of revenue with respect to fixed inputs is greater than the elasticity in the production function, i.e. $\alpha/\chi > \alpha$. This is because the firm can leverage its fixed inputs to increase revenue more than proportionally by hiring

[1] Alternatively, we could assume that firms face downward sloping residual demand curves, which would make the *revenue production function* decreasing returns to scale. As long as residual demand is isoelastic, the analytical derivation of the model remains unchanged. The only difference is that the parameters have a different interpretation: the revenue elasticity of an input is the product of the input's share in revenue and $1 - 1/\sigma$, where σ is the elasticity of residual demand (De Loecker, 2011).

more variable inputs.

As is usual under Cobb-Douglas production functions, the share of revenue accruing to each input is constant over time and across firms, equal to their elasticity in the production function. We define the rent accruing to fixed factors (including physical capital)

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

Taking logarithms of equations (2) and (3), we can express the log surplus as:

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

where C is a constant only depending on fixed parameters, $k_{it} = \ln K_{it}$, $z_m = \ln Z_m$, $p_{st} = \ln P_{st}$, $\omega_{it} = \ln \Omega_{it}$, $a_i = \ln A_i$, and $w_{st} = \ln W_{st}$, $\rho_{st} = \ln \varrho_{st}$.

Equation (4) shows how surplus depends on manager skills, holding fixed the inputs chosen by the owner and the input and output prices prevailing in the sector. Taking two managers m and m' with skills z_m and $z_{m'}$ at the same firm, the change in surplus attributable to the new manager is:

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

The *value* of the new manager to the owners of the firm is the change in surplus. This value is proportional to the difference in manager skills, scaled by the inverse of the elasticity of revenue with respect to fixed inputs χ. In what follows, we aim to measure this value by estimating the change in surplus following a manager change.

Estimable equation. In absence of observing organization capital and input prices, we can substitute these out with fixed effects, leading to the following estimable equation:

$$s_{imst} = \frac{\alpha}{\chi} k_{it} + \frac{1}{\chi} \tilde{z}_m + \lambda_i + \mu_{st} + \tilde{\omega}_{it} \tag{6}$$

where $\lambda_i = a_i/\chi$ is a firm fixed effect capturing time-invariant organizational capital, $\mu_{st} = C + p_{st}/\chi - \beta w_{st}/\chi - \gamma \rho_{st}/\chi$ is an industry-time fixed effect capturing sector-specific prices and wages, and $\tilde{\omega}_{it} = \omega_{it}/\chi$ is a rescaled time-varying firm productivity shock.

Assuming that residual productivity $\tilde{\omega}_{it}$ is uncorrelated with manager skills and physical capital, we can estimate the model using ordinary least squares with fixed effects (OLSFE). Note that we do *not* assume that manager skills are uncorrelated with physical capital, organizational capital or sectoral prices. It may well be the case that better firms with good price conditions hire better managers and invest more.

Given our estimated parameters and fixed effects, we can recover manager skills as:

$$\hat{\chi} s_{imst} - \hat{\alpha} k_{it} - \hat{\chi} \lambda_i - \hat{\chi} \mu_{st} := \tilde{s}_{imst} = \hat{z}_m + \hat{\omega}_{it}. \tag{7}$$

We remove the contribution of physical capital, firm and industry-year fixed effects from log surplus to obtain a *residualized surplus* $\tilde{s}_{imst}$. Because ω_{it} is assumed to be mean zero independent of m, we can estimate $\hat{z}_m$ as the average of $\tilde{s}_{imst}$ across all observations for manager m. This gives us a consistent estimate of manager skill, $\hat{z}_m = \frac{1}{N_m} \sum_{i,t} \tilde{s}_{imst}$, where N_m is the number of observations for manager m.[2]

[2] This is equivalent to including a manager fixed effect in the regression, similar in spirit to Abowd et al. (1999) and Card et al. (2018). This notation emphasizes that manager effects estimated from fewer observations are noisier.

3 Data and Measurement

Main data sources. Our analysis uses comprehensive administrative data on Hungarian firms during 1992-2022, created by merging balance sheet and financial statement data with firm registry information. The balance sheet data come from HUN-REN KRTK (2024b) and contains financial information for essentially all Hungarian firms required to file annual reports. The firm registry data come from HUN-REN KRTK (2024a) and includes information on firm registration, ownership structure, and director appointments. Both datasets are distributed by HUN-REN KRTK and originally published by Opten Zrt.[3]

The balance sheet data include all firms required to file financial statements with Hungarian authorities, covering essentially the entire formal business sector except for the smallest corporations not engaged in double-entry bookkeeping and individual entrepreneurs. The dataset contains detailed financial information including sales revenue, export revenue, employment, tangible and intangible assets, raw material and intermediate input costs, personnel expenses, and ownership indicators for state and foreign control.

Registry information is collected by the Hungarian Corporate Court, which maintains legally mandated public records on firms (*Cégtörvény*, 1997). These records include information on company representatives—individuals authorized to act on behalf of the firm in legal and business matters. Representatives may include CEOs and other executives, but also lower-level employees with signatory rights. We exclude the rare instances where the representative is a legal entity. The dataset is structured as a temporal database: each entry has an effective date interval and reflects the state of representation at a given time. Updates occur not only when positions change but also when personal identifiers (e.g., address) are modified or when reporting standards evolve. Start and end dates are often missing, and prior to 2010, the data does not contain unique numerical identifiers for individuals.

We resolve individual identities by linking records based on name, birth date, mother's name, and home address, creating a unique identifier for each person. This entity resolution step enables tracking of representatives over time and across firms. To construct an annual panel of top managers, we infer the period of service for each representative using available date bounds and sequential information. A representative is considered active in a given year if their tenure includes June 21 of each year.

Because job titles are not standardized, identifying the CEO requires heuristic rules. When an explicit title such as *managing director* is available, we classify the individual accordingly. For firms lacking such labels, we assume that all representatives are CEOs if the number of representatives is three or fewer. If there are more than three and one of them was previously identified as a CEO, we assign the CEO role based on continuity. This approach allows us to systematically identify the firm's top executive across years.

Sample construction. We construct our analytical sample through several filtering steps. We restrict our analysis to the period 1992 to 2022 to focus on the post-transition Hungarian economy. This removes 136,141 observations from years prior to 1992, when the economic and institutional environment was fundamentally different. Our sample contains 10,214,120 firm-year observations spanning 31 years. Table 1 shows the temporal distribution of observations in our final sample. The sample exhibits steady growth from 98,780 observations in 1992 to 454,106 in 2022. This expansion reflects the growth of entrepreneurship in Hungary following the transition to a market economy.

[3] The data cannot be publicly shared due to privacy and licensing restrictions. The replication package available at https://github.com/korenmiklos/ceo-value describes how to get access to the data.

Table 1: Sample Distribution by Year

Year	Observations	Year	Observations	Year	Observations
1992	98,780	2002	301,278	2012	397,131
1993	122,677	2003	305,947	2013	437,692
1994	153,639	2004	319,750	2014	427,494
1995	171,759	2005	326,905	2015	433,371
1996	198,558	2006	334,498	2016	431,041
1997	219,751	2007	345,134	2017	424,184
1998	246,660	2008	362,920	2018	425,601
1999	256,992	2009	370,788	2019	419,883
2000	280,386	2010	384,570	2020	424,501
2001	302,894	2011	402,636	2021	432,594
				2022	454,106
Total: 10,214,120					

Notes: Sample distribution after applying time period restrictions (1992-2022) and data quality filters.

CEO panel construction. We construct a panel of chief executive officers from the firm registry data, restricting the sample to the same 1992-2022 time period. The initial CEO panel contains information on 996,387 observations that are excluded due to the time restriction. The final CEO panel includes variables identifying the firm (frame_id_numeric), person (person_id), year, as well as CEO characteristics including gender (male), birth year, manager category, and ownership status.

The CEO data reveals substantial variation in the number of CEOs per firm-year. Among the 12,726,597 firm-year observations with CEO information, the vast majority (82.24%) have a single CEO. However, 15.32% of firm-years have two CEOs, 1.98% have three CEOs, and small fractions have even larger numbers of CEOs, with some firms reporting up to 52 CEOs in a single year. This distribution reflects the complexity of executive structures in Hungarian firms, including cases where firms may have multiple managing directors or where CEO transitions occur within a year.

Sample merging and match rates. We merge the CEO panel with the balance sheet data using firm identifiers and year. The merge process reveals important patterns in data availability across sources. Of the 15,980,738 total observations from both datasets, 11,886,636 observations (74.4%) successfully match between CEO and balance sheet data. The remaining observations consist of 3,507,466 CEO observations without corresponding balance sheet data and 586,636 balance sheet observations without CEO information.

At the firm level, the match rates are more favorable. Among the 1,200,145 unique firms in our combined dataset, 942,684 firms (78.55%) have information in both datasets. The remaining firms are split between 238,852 firms (19.90%) that appear only in the CEO registry and 18,609 firms (1.55%) that appear only in the balance sheet data. This pattern suggests that CEO information is available for most active firms but may be missing for very small firms or those with simplified reporting requirements.

Industry composition. We classify firms into broad industry sectors using the TEAOR08 classification system. The final analytical sample spans diverse industries, with notable concentration in service sectors. Wholesale, retail, and transportation activities account for the largest share at 28.86% of observations. Nontradable services represent 26.72%, while telecom and business services contribute 18.92%. Manufacturing firms account for 10.56%, construction for 9.25%, and agriculture for 3.46%. Mining and finance sectors are excluded from the final analytical sample due to their distinct production characteristics and regulatory environments.

CEO turnover and tenure patterns. The data reveals substantial heterogeneity in CEO turnover across firms. We construct CEO spell variables to track the sequence of different CEO appointments within each firm. Among firm-year observations, 66.72% represent the first CEO spell, meaning these are either firms with their original CEO or the first year of data for that CEO. Second CEO spells account for 22.90% of observations, while 6.88% represent third spells. The distribution has a long tail, with some firms experiencing up to 25 different CEO spells during the observation period.

At the firm level, 62.97% of the 1,012,113 firms in our sample experience only one CEO spell during the observation window. However, 24.07% of firms have exactly two CEO spells, indicating at least one CEO change. The remaining 12.96% of firms experience multiple CEO changes, with some firms having up to 25 CEO transitions. This pattern suggests that while many firms maintain stable CEO leadership, a substantial minority experience frequent executive turnover.

Sample restrictions and final dataset. We apply several filters to focus on firms most suitable for productivity analysis. First, we exclude firms that ever have more than two CEOs in a single year, removing 1,519,524 observations. This filter eliminates firms with potentially complex or unstable governance structures that may confound productivity estimates. Second, we drop firms with more than six CEO spells over the observation period, removing an additional 45,216 observations to focus on firms with more stable executive structures.

We also exclude certain industries and ownership types that may have different production functions or regulatory environments. Mining and finance sectors are dropped due to their unique operational characteristics: mining operations face resource constraints and depletion effects that differ from standard production functions, while financial services operate under distinct regulatory frameworks that affect standard productivity measures. Additionally, we exclude all firms that were ever state-owned during the observation period, as state ownership introduces different objective functions and constraints that may confound our productivity analysis of private firm management.

Variable construction.

Measurement of Model Variables. We measure the key variables from the theoretical framework as follows:

Physical capital (K_{it}): Tangible assets from balance sheet data, including machinery, equipment, and buildings, measured in logarithmic form as $k_{it} = \ln K_{it}$.

Surplus (S_{imst}): EBITDA (Earnings Before Interest, Taxes, Depreciation, and Amortization), calculated as sales revenue minus personnel expenses minus material costs, measured in logarithmic form as $s_{imst} = \ln S_{imst}$.

Manager skill (Z_m): CEO fixed effects $\tilde{z}_m$ estimated from the regression in equation (5), capturing time-invariant managerial ability.

Labor input (L_{imt}): Employment measured as the number of employees, transformed to logarithmic form as $l_{imt} = \ln L_{imt}$.

Manager compensation (W_{imst}): CEO wages including base salary and bonuses from administrative records (not yet available in current analysis).

Organizational capital (A_i): Time-invariant firm characteristics including location, brand value, and market position, captured by firm fixed effects λ_i and not directly observed.

Sector-time variation: Industry-specific prices and wages controlled through industry-time fixed effects μ_{st} using TEAOR08 sector classifications.

Missing values in financial variables are systematically recoded to zero, following standard practice in administrative data analysis where missing values typically indicate zero rather than unknown values. The extent of missing data varies considerably across variables, reflecting different reporting requirements and business activities. Export data has the highest rate of missing values, with 5,456,815 observations recoded, reflecting that many firms do not engage in export activities. Employment data required recoding for 1,138,791 observations, while sales revenue had relatively few missing values with only 486,197 observations recoded.

For employment, we make an additional adjustment by setting values below one to equal one. This transformation affects 3,655,899 observations and acknowledges that active firms filing administrative reports must have positive employment. Zero or negative employment values likely reflect administrative reporting inconsistencies rather than true zero employment.

We also address issues with wage bill and personnel expense variables, where 3,931,270 and 1,117,283 observations respectively are recoded from missing to zero. For asset variables, tangible assets required recoding for 1,014,331 observations while intangible assets had 4,299,589 missing values recoded, reflecting that many firms do not report significant intangible assets.

We construct several derived variables for the analysis. EBITDA is calculated as sales minus personnel expenses minus materials. Log transformations are applied to key variables including sales (lnR), EBITDA (lnEBITDA), employment (lnL), and tangible assets (lnK). CEO tenure is measured as years since first appointment, while CEO age and firm age are calculated from birth year and founding year respectively. We also create indicator variables for expatriate CEOs (those with missing gender information, suggesting non-Hungarian names) and ownership status.

The final analytical sample contains 8,872,039 firm-year observations representing 960,464 unique firms over the 1992-2022 period. This sample focuses on manufacturing, wholesale/retail/transportation, telecom/business services, and other nontradable services sectors, with firms having relatively stable CEO structures suitable for productivity analysis.

Table 2: Industry Composition of Final Sample

Industry Sector	Observations	Percent
Wholesale, Retail, Transportation	3,430,342	28.86
Nontradable Services	3,176,339	26.72
Telecom and Business Services	2,249,271	18.92
Manufacturing	1,254,792	10.56
Construction	1,100,022	9.25
Agriculture	411,226	3.46
Finance*	247,718	2.08
Mining*	16,926	0.14
Total (before restrictions)	11,886,636	100.00
Final analytical sample	8,872,039	–

Notes: *Industries excluded from final analytical sample. Additional restrictions exclude firms that were ever state-owned. Industry classification based on TEAOR08 system.

Table 3: CEO Structure and Turnover Patterns

Panel A: Number of CEOs per Firm-Year

Number of CEOs	Observations	Percent
1	10,466,412	82.24
2	1,949,370	15.32
3	251,882	1.98
4+	58,933	0.46
Total	12,726,597	100.00

Panel B: CEO Spells per Firm-Year

CEO Spell	Observations	Percent
1 (First CEO)	6,423,429	66.72
2	2,204,806	22.90
3	662,846	6.88
4	205,665	2.14
5+	130,738	1.36
Total	9,627,484	100.00

Panel C: Maximum CEO Spells per Firm

Max CEO Spells	Firms	Percent
1	637,287	62.97
2	243,609	24.07
3	84,184	8.32
4-6	42,788	4.23
7+	4,245	0.42
Total	1,012,113	100.00

Notes: Panel A shows distribution of concurrent CEOs per firm-year. Panel B shows CEO spell distribution among successfully matched firm-years. Panel C shows maximum number of CEO changes per firm over entire observation period.

4 Methodology

Our empirical strategy combines three complementary approaches to estimate CEO value: firm fixed effects estimation, manager mobility analysis, and event study identification. Each method provides different insights into the role of managerial skill in firm performance.

4.1 Firm Fixed Effects Estimation

We begin by estimating equation (6) using ordinary least squares with fixed effects:

$$s_{imst} = \frac{\alpha}{\chi} k_{it} + \frac{1}{\chi} \tilde{z}_m + \lambda_i + \mu_{st} + \tilde{\omega}_{it} \tag{8}$$

where s_{imst} is log surplus (EBITDA), k_{it} is log physical capital, λ_i are firm fixed effects, μ_{st} are industry-time fixed effects, and $\tilde{z}_m$ captures manager skill scaled by $1/\chi$.

This specification controls for time-invariant firm characteristics (organizational capital, location, brand value) through firm fixed effects and sector-specific price and wage variation through industry-time fixed effects. The key identifying assumption is that residual productivity shocks $\tilde{\omega}_{it}$ are uncorrelated with manager skills and physical capital conditional on these fixed effects.

For firms with multiple CEO spells, we can estimate relative manager skills by normalizing the first manager's skill to zero. The estimated manager fixed effects then represent skill differences relative to the firm's initial CEO. This within-firm identification strategy addresses concerns about manager-firm sorting by comparing different managers within the same organizational context.

4.2 Manager Mobility and Connected Components

To estimate absolute manager skills rather than firm-specific relative skills, we exploit manager mobility across firms. Managers who work at multiple firms create connections that allow us to compare skills across the broader managerial labor market. We construct a bipartite graph of firm-manager relationships and identify the largest connected component using standard graph algorithms.

Within the largest connected component, we estimate manager skills using the two-way fixed effects framework of Abowd et al. (1999):

$$s_{imst} = \frac{\alpha}{\chi} k_{it} + \psi_m + \lambda_i + \mu_{st} + \varepsilon_{imst} \tag{9}$$

where ψ_m are manager fixed effects normalized to have mean zero across all managers in the connected component. This approach provides estimates of absolute manager skill differences that can be compared across the entire managerial labor market.

The key identifying assumption is that manager mobility is not systematically correlated with unobserved firm or time-varying productivity shocks. Recent work by Metcalfe et al. (2023) suggests this assumption is reasonable in settings with substantial manager turnover, as in our Hungarian data.

4.3 Event Study Identification

To provide causal evidence on the impact of CEO skill differences, we implement an event study around CEO transitions. We restrict analysis to firms experiencing exactly one CEO change during our observation period, focusing on clean transitions between first and second CEOs.

We classify CEO transitions based on estimated skill differences between departing and incoming managers. Using the skill measures from our fixed effects estimation, we define:

- *Better manager* transitions: New CEO has higher estimated skill than departing CEO
- *Worse manager* transitions: New CEO has lower estimated skill than departing CEO
- *Same skill* transitions: Skill difference is negligible (within 5 percentage points)

We then estimate treatment effects using the two-treatment difference-in-differences methodology of Callaway & Sant'Anna (2021), extended to handle multiple treatment groups as in Koren & Telegdy (2023). The specification compares firms hiring better managers against those hiring worse managers, using firms with similar-skill replacements as a control group:

$$s_{it} = \alpha + \sum_{j=-10}^{10} \beta_j \cdot \mathbf{1}[\text{event\_time} = j] \cdot \text{Better\_CEO}_i + \sum_{j=-10}^{10} \gamma_j \cdot \mathbf{1}[\text{event\_time} = j] \cdot \text{Worse\_CEO}_i + \varepsilon_{it} \tag{10}$$

The coefficients $\beta_j - \gamma_j$ measure the difference in surplus between firms hiring better versus worse managers at each event time relative to the baseline period. We use an analysis window from 10 years before to 10 years after the CEO change, with optimal weighting to account for varying sample composition over time.

This event study design addresses potential endogeneity concerns by examining whether measured skill differences translate into actual performance changes around the time of CEO transitions. The method provides a quasi-experimental test of whether our estimated manager skills capture real differences in managerial ability rather than spurious correlations.

5 Results

Because we are estimating manager skills conditional on firm and industry-year fixed effects, we can only obtain a *relative* skill measure of different managers within the same firm and industry-year, relative to a suitably chosen baseline. With the right baseline, however, we can interpret the estimated skills.

Within-firm manager changes. First we study the impact of within-firm manager changes on firm surplus. If there are n managers in a firm, we can estimate $n-1$ manager fixed effects. We normalize the log skill of the first manager of the firm to zero. The remaining $n-1$ manager fixed effects are then interpreted as the difference in skills relative to the first manager. Naturally, this calculation only makes sense for $n > 1$, i.e. for firms that have at least two managers in the sample. The relative manager skills are estimated as the average of the residualized surplus $\tilde{s}_{imst}$ across all observations for that manager, as described in equation (7).

Figure 1 Panel A shows the distribution of relative manager skills in the sample with at least two managers. The distribution is centered a bit higher than zero, with a mean of 0.16. This means that, on average, second and subsequent managers are 16 percent more skilled than the first manager of the firm. This is expected if under-performing managers are more likely to be replaced, leading to a positive selection bias in the sample of second and subsequent managers.

There is, however, substantial variation around this mean, with some managers being significantly more skilled than the first manager and others being less skilled. The interquartile range of relative skills corresponds to a 9.8 percent difference in firm productivity. Because higher productivity can be leveraged by buying more variable inputs, this would lead to a larger increase in revenue and surplus. The counterfactual manager change mentioned above would increase revenue and surplus by 118 percent.

Table **??** shows the relationship between manager skills and firm outcomes. The regression coefficients indicate how manager skills correlate with revenue, EBITDA, and employment within the connected component of managers.

Largest connected component. Managers that lead mutiple firms (even at different times) help identify the skills of other managers. To consider a specific example, suppose manager B replaces manager A at firm 1 with a measured skill increase of 0.2, and manager B is replaced by manager C at firm 2 with a measured skill drop of 0.05. We can then infer the relative skill of manager C compared to manager A as +0.15. This process can be repeated for all managers that are connected through a chain of replacements, leading to a large connected component of managers.

Using standard graph analysis, we find the largest connected component of managers in our sample, which contains 180,421 managers. These managers account for 27.1 percent of all firm-year observations.[4] For these managers, their skills can be estimated by two-way firm and manager fixed effects (Abowd et al., 1999; Correia, 2023). We normalize log manager skills to zero, so the estimated skills can be interpreted as deviation from the average manager in the largest connected component.

Figure 1 Panel B shows the distribution of relative manager skills in the largest connected component. The distribution is centered around zero by construction. The interquartile range of relative skills corresponds to a 25.6 percent difference in firm productivity. Because higher productivity can be leveraged by buying more variable inputs, this would lead to a 461 percent increase in revenue and surplus. This larger variation compared to within-firm estimates suggests that good managers tend to be replaced by other good managers within the firm.

In the cross section, the contribution of manager skills is less important relative to other fixed factors (captured by firm fixed effects). Manager skills explain 5 to 9 percent of within-industry variation in log revenue, log surplus and log employment.

5.1 Event Study Results

To provide causal evidence on the impact of CEO skill differences, we conduct an event study around CEO transitions. We restrict the sample to firms experiencing exactly one CEO change during our observation period, focusing on clean transitions between first and second CEOs. Our final event study sample includes 51,758 firms where we can measure skill differences between consecutive CEOs and observe sufficient pre- and post-transition data.

Sample characteristics and skill classification. We classify CEO transitions into three categories based on measured skill differences. Using a threshold of 5 percentage points to define meaningful skill changes, we find that 49.38 percent of transitions (25,557 firms) involve hiring a better manager, 38.69 percent (20,024 firms) involve hiring a worse manager,

[4] The second largest connected component contains only a small fraction of managers, so the largest connected component is overwhelmingly dominant, as is often the case in real-world networks.

and 11.93 percent (6,177 firms) involve similar-skill replacements. This classification reveals substantial heterogeneity in the direction of CEO skill changes, with firms slightly more likely to upgrade than downgrade their management.

The temporal distribution of transitions shows that our sample spans the full observation period from 1992 to 2022, with CEO changes occurring throughout the Hungarian economic transition and subsequent development. The event study sample includes firms across all major industries, ensuring broad representativeness of the results.

Pre-transition patterns and selection. Figure 2 presents the evolution of residual surplus around CEO transitions, comparing firms that hire better managers versus worse managers. The analysis window spans from 10 years before to 10 years after the CEO change (event time 0), with the baseline set to the average of the pre-treatment period.

The pre-transition period reveals important selection patterns. Firms that will hire better managers show consistently lower surplus in the years leading up to the transition, with the difference reaching 2.5 percentage points below firms hiring worse managers at event time -10. This pattern intensifies as the transition approaches, with the difference growing to 7.7 percentage points at event time -1. These pre-existing differences suggest that underperforming firms are more likely to seek higher-skilled replacement CEOs, consistent with optimal management turnover in response to poor performance.

Causal impact of skill differences. The event study reveals a dramatic reversal in relative performance immediately following CEO transitions. At event time 0, firms hiring better managers show a 27.7 percentage point improvement in surplus relative to firms hiring worse managers ($-9.1-(-18.5)=9.4$ percentage points for better managers versus $-9.1-(-18.5)=-18.5$ percentage points for worse managers). This immediate impact reflects the instantaneous effect of changing management quality on firm performance.

The treatment effects persist and remain substantial throughout the post-transition period. By event time +10, firms with better managers maintain an 18.0 percentage point advantage over those with worse managers. The persistence of these effects over a full decade provides strong evidence that CEO skill differences represent fundamental and lasting changes in firm productivity rather than temporary adjustments.

Robustness and interpretation. Several features of our event study design strengthen causal interpretation. First, the use of estimated skill measures from our fixed effects regression ensures that the classification of better versus worse managers is based on systematic patterns rather than ex-post performance. Second, the inclusion of a control group of firms with similar-skill replacements helps account for general trends affecting firms experiencing CEO transitions.

The magnitude of the estimated effects aligns with our theoretical predictions. The 27.7 percentage point difference in surplus at the time of transition corresponds to the leveraged impact of skill differences on firm performance, consistent with the $1/\chi$ scaling factor in our theoretical model. The gradual convergence of the treatment effect from 27.7 to 18.0 percentage points over the decade following transition may reflect either mean reversion in performance or the gradual optimization of firm operations under new management.

These results complement our earlier findings on manager skill heterogeneity by providing quasi-experimental evidence that the measured skill differences have real causal impacts on firm performance. The event study confirms that CEO quality matters substantially for privately held firms and that our estimation methodology captures meaningful variation in managerial ability.

6 Conclusion

This paper develops and implements a comprehensive framework for measuring CEO value in privately held businesses using administrative data. Our approach addresses a fundamental challenge in development economics: how to assess the importance of managerial talent when traditional measures based on stock market valuations or executive compensation are unavailable.

Our theoretical framework, building on the decreasing returns to scale models of Atkeson & Kehoe (2005) and McGrattan (2012), shows how manager skills can be identified through their impact on firm surplus while controlling for organizational capital and sectoral conditions. The empirical implementation combines three complementary identification strategies—within-firm variation, manager mobility networks, and quasi-experimental event studies—to provide robust evidence on CEO value.

Applied to comprehensive Hungarian administrative data covering 1992-2022, our analysis reveals substantial heterogeneity in CEO skills with large economic consequences. Within firms, replacing a 25th percentile manager with a 75th percentile manager increases productivity by 9.8 percent. Across the connected component of mobile managers, the same replacement increases productivity by 25.6 percent, suggesting considerable skill heterogeneity in the broader managerial labor market.

The event study provides compelling causal evidence that these skill differences matter for firm performance. Firms hiring better managers show 28 percentage point higher surplus immediately following CEO transitions compared to those hiring worse managers. These effects persist over time, remaining economically significant even a decade after management changes. The magnitude and persistence of these effects confirm that CEO quality has fundamental and lasting impacts on firm productivity.

Our findings contribute to several strands of literature. First, they extend the evidence on managerial impacts from developed country public firms to a developing country context with predominantly private firms. The substantial CEO effects we document suggest that managerial talent is equally important in transition economies, where institutional development and market structures may amplify the role of individual decision-makers.

Second, our methodology provides a template for measuring CEO value in data-constrained environments. The reliance on standard administrative records—balance sheets and corporate registries—makes our approach applicable to many developing countries where such data are routinely collected by governments. This opens new possibilities for research on management and productivity in settings where traditional corporate finance measures are unavailable.

Third, our results speak to policy debates about firm performance and economic development. The large variation in CEO skills and their substantial impact on firm performance suggest that policies affecting managerial labor markets—such as education, training programs, or regulations governing executive mobility—may have important aggregate productivity effects.

Several limitations suggest directions for future research. Our measure of CEO skill captures the portion of managerial ability that translates into firm surplus, but may not fully capture other aspects of leadership such as strategic vision or organizational development that matter over longer horizons. Additionally, while our event study design provides causal evidence around CEO transitions, the endogenous nature of most CEO appointments means our estimates may not fully capture the equilibrium effects of randomly improving manager-firm matching.

Future work could extend our framework in several directions. Incorporating information on CEO compensation when available could provide direct measures of the private returns to managerial skill. Examining the sources of skill differences—such as education, experience, or industry background—could inform policies aimed at developing managerial capacity. Finally, studying how CEO value varies across different institutional environments could shed light on the contextual factors that amplify or diminish the importance of managerial talent.

Despite these limitations, our findings demonstrate that CEO quality matters substantially for firm performance in privately held businesses, with effects that are both large in magnitude and persistent over time. This evidence supports the importance of managerial talent for firm productivity and economic development, while providing a methodological framework for measuring CEO value in data-constrained environments.

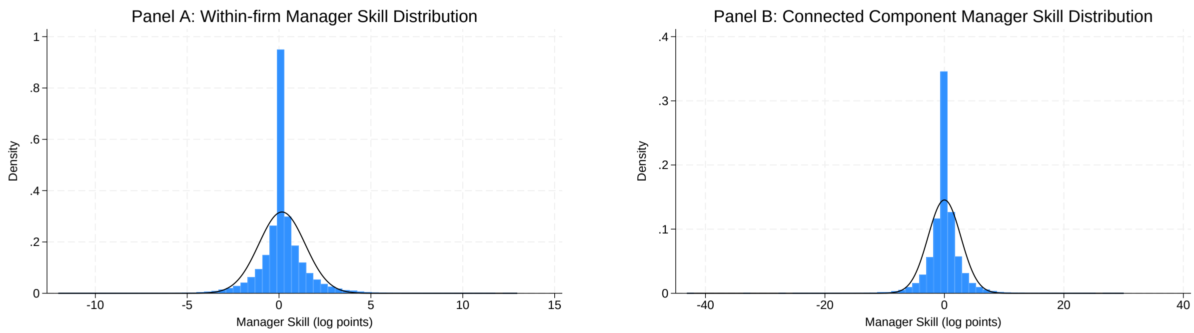

Figure 1: Manager Skill Distributions
Notes: Panel A shows the distribution of within-firm manager skill variation for firms with multiple CEOs. Panel B shows the distribution of manager skills in the largest connected component of managers. Both distributions show manager skills in log points after normalization and scaling.

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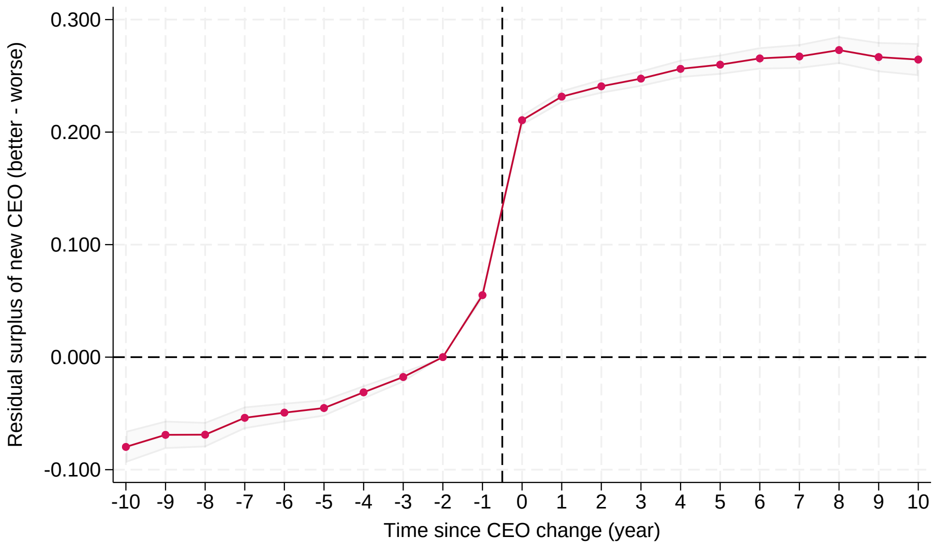

Figure 2: Event Study: Impact of CEO Skill Changes on Firm Surplus
Notes: Event study comparing firms that hire better managers (higher skill) versus worse managers (lower skill). The figure shows the difference in residual surplus between the two groups from 10 years before to 10 years after CEO transitions. Event time 0 represents the year of CEO change. Baseline period is 2 years before transition (event time -2). Gray shaded area represents 95% confidence intervals. Sample includes 94,185 firms with exactly one CEO change during the observation period.

Table 4: Manager Skill Effects on Firm Outcomes

	(1) Revenue	(2) EBITDA	(3) Employment
Sales (log)	0.086***		
	(0.004)		
EBITDA (log)		0.055***	
		(0.004)	
Employment (log)			0.090***
			(0.007)
Constant	-0.861***	-0.431***	-0.085***
	(0.039)	(0.035)	(0.011)
Observations	1856674	1398280	1856674
Adjusted R-squared	0.005	0.003	0.003

Standard errors in parentheses
Standard errors clustered at firm level.
All regressions include industry-year fixed effects.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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A Robustness Checks

Table 5: The revenue function in various samples

	(1) Full	(2) sample	(3) First CEO spell	(4) Single CEO spell	(5 Multiple C
Tangible and intangible assets (log)	0.254***	0.255***	0.256***	0.249***	0.275
	(0.001)	(0.001)	(0.001)	(0.001)	(0.00
Intangible assets share	-0.028***	-0.027***	-0.038***	-0.016*	-0.04
	(0.007)	(0.009)	(0.011)	(0.010)	(0.0
Foreign owned	0.012	0.012	-0.004	0.018*	0.0
	(0.008)	(0.011)	(0.014)	(0.010)	(0.0
Observations	6634335	4404163	3073377	3560899	1797

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

Table 6: The revenue function by sector

	(1) Agriculture	(2) Manufacturing	(3) Wholesale, Retail, Transportation	Tele
Tangible and intangible assets (log)	0.322***	0.302***	0.262***	
	(0.005)	(0.003)	(0.002)	
Intangible assets share	0.100*	0.013	-0.009	
	(0.058)	(0.025)	(0.014)	
Foreign owned	-0.075*	0.043*	0.010	
	(0.043)	(0.024)	(0.015)	
Observations	213719	769740	1961088	

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