

Debiasing Second Moments of Estimated CEO Effects: A Placebo-Controlled Approach*

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Abstract

Understanding the link between estimated manager effects and firm outcomes is central to research on CEOs and firm performance. While fixed-effect estimators of mean CEO effects are unbiased under random mobility, most applied analyses use second moments of these estimates—variances, covariances, correlations, and event-study dynamics—that are systematically biased by small-sample noise. We develop a placebo-controlled event-study design that identifies and removes these biases without modeling the full error process. The method constructs placebo transitions that replicate the spell-length design of actual transitions while excluding periods around real changes. Moments computed on placebo spells recover the bias components that inflate variances and covariances; subtracting them yields debiased moments and regression slopes. A Monte Carlo study with six scenarios—baseline, long panel, persistent errors, unbalanced panels, excess variance, and all complications—illustrates when the correction matters most. We outline an application to the universe of Hungarian firms (1992–2022) that examines pre-trends and dynamic effects after CEO arrival, a common use case in the literature.

Keywords: CEO value, private firms, productivity

JEL Classification: D24, G34

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1 Introduction

The impact of individual CEOs on business practices and performance is a widely studied question in economics, corporate finance, and management. In absence of direct measures of CEO characteristics that are relevant for firm outcomes, studies often include CEO fixed effects to capture variations in latent CEO skills. [[cite Bertrand Schoar, more econ papers, also Strategic Management journals]] Under the standard assumption of “random mobility,” that is, that CEO moves are unrelated to past and future performance shocks of the firm, the mean of estimated CEO fixed effects is unbiased ().

Most applied work, however, is concerned with the second moments of the estimated fixed effects. We highlight three common use cases. First, variance decompositions reveal how much CEO effects contribute to the variance of certain firm outcomes, such as productivity or profitability (). Second, the covariance of the CEO effect with other variables helps to understand the mechanisms through which CEOs affect firm outcomes; for example, whether firms led by better CEOs export more (). [[FIXME: pick outcome from corporate finance or management literature about CEO policies, like investment]] Third, dynamic covariances are used in event studies to assess how outcomes evolve before and after CEO transitions, as a way to test for pre-trends and to understand post-transition dynamics ().

All these second moments are biased by small-sample noise in the estimated CEO effects, even under credible identification assumptions (Andrews et al., 2008; Gaure, 2014; Bonhomme et al., 2023). This is not a nuisance, but has important implications for the interpretation of empirical results. Perhaps most intuitively, small-sample bias overstates the role of CEOs in explaining firm performance. [[FIXME: use a good outcome variable to explain the second bias]] And, as we show below, the bias introduces spurious pre-trends and distorts post-transition dynamics in event studies.

Correcting for small-sample bias is challenging. Existing methods are either ad hoc, such as leave-one-out adjustments with the goal of removing own-observation noise (), or highly structural: estimate the bias from the variance-covariance of shocks and the design matrix and then apply Empirical Bayes shrinkage (Andrews et al., 2008; Bonhomme et al., 2023; Kline, 2024). While powerful, these require estimating many components and re-deriving formulas for each target second moment.

Our approach is different: we create a placebo-controlled experiment. For each firm that truly changes CEOs, we find a group of comparable non-changing firms, and use this placebo control group to estimate the mechanical noise in the second moments.¹ Under weak assumptions, the placebo group can be used to measure the exact bias in variance, covariance, and dynamic covariance. The difference in second moments between the treated and the placebo groups yields debiased estimates, without having to model and estimate the full variance-covariance structure of errors.

We illustrate our method in Monte Carlo experiments. [[EXPLAIN]]

We then use our method to measure the quantitative contribution of CEOs to firm growth in Hungarian data, 1992-2022. [[EXPLAIN]]

¹This step in the spirit of Jarosiewicz and Ross (2023), who compare the replicated results of Bertrand and Schoar (2003) with placebo regressions.

Our method relies on two key assumptions about the nature of shocks. First, we assume strict exogeneity, that is, that error terms are mean independent of the CEO path for all time periods. This ensures that the estimated CEO effects are mean unbiased. Second, we assume that the autocovariance of errors is the same between treated and control firms, up to a scalar multiplier. In other words, we allow for treated firms to be more volatile than control firms, but we assume that the temporal correlation structure of shocks is the same. We call this assumption “parallel second moments,” in reference to the weaker parallel trends assumption in difference-in-differences designs. Other than these two assumptions, we make no further restrictions on the error process, the number and length of CEO spells, or the heterogeneity of CEO effects.

To understand how these assumptions help debias the variance-covariance of estimated CEO effects, note that the bias terms are weighted sums of the autocovariances of errors, with weights depending on the research design, including the spell length of CEOs at each firm. The placebo design ensures that treated firms and control firms have the same spell-length distributions, and therefore the same weights. The parallel second moments assumption then implies that the bias terms are the same up to a scalar. And because covariance is a bilinear operator and variance is additive under strict exogeneity, the bias terms can be “precomputed” in the placebo group and simply subtracted from the treated moments.² In summary, the placebo group, by construction, has no CEO effects, so the variance and covariance of estimated placebo effects identify the bias terms. Subtracting these from the treated moments yields debiased estimates.

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The main idea of this correction is the creation of pseudo CEO transitions in the data³ We take firms with CEO transitions, record their event window. The event window is the period encompassing the years under the CEO prior to the CEO change and the period under the new CEO post the change. We match these firms to firms born in the same year, belonging to the same sector, that however do not experience a CEO transition during the years of the event window. Then, we assign to the control firms a placebo CEO transition happening in the same year as the treated firm experiences the true CEO transition.⁴ On the matched sample we implement two-way fixed effects regressions.⁵ 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.

Papers use three approaches when analyzing the effect of CEOs. One group studied the effects of a distinct, measurable variable, such as managerial practices used in the firm (Bloom et al., 2012) or a CEO attribute (age, gender, tenure, education or country of origin (Anderson et al., 2018; Henderson et al., 2006; Koren and Telegdy, 2023)). These studies are useful to show the effects of the studied variables, but they arguably capture little of the whole effect of CEOs

²This is not true for nonlinear transformations of variances and covariances, such as correlations and regression slopes. In these cases, we first debias all underlying variances and covariances and only then form the ratio.

³This step is in the spirit of Jarosiewicz and Ross (2023), who compare the replicated results of Bertrand and Schoar (2003) with placebo regressions.

⁴In addition to addressing the limited mobility bias, our approach has the advantage of creating a control group that is more similar to the treated firms than the universe of firms

⁵The estimation is similar to the two way fixed effects regression developed by Callaway and Sant’Anna (2021), but we exploit the fact that the control group also has a time of pseudo CEO change. We compare firms with a true CEO change with firms with a fake CEO change along the same event-time years.

over the firms they manage. Another group uses an external event to assess the effect of CEO turnover (e.g., Bennedsen et al. (2020)). While this approach brought about credible causal estimates, the heavy data needs render this approach impossible to use in most situations.

The third group employed firm and CEO fixed effects to separate the quality of firm from that of the CEOs (Bertrand and Schoar, 2003; Crossland and Hambrick, 2011; Quigley and Hambrick, 2015). While these studies employ a framework that can be used in many settings, the generalizability of their results is challenged by the data used because almost all study public companies only.⁶ Our data has information on the universe of Hungarian businesses and all their CEOs for the period between 1992 and 2022, tracking over 1 million firms and a similar number of CEOs.⁷ The overwhelming majority of the firms in our sample are small and medium-sized enterprises (SMEs). This adds a new dimension to the analysis that was largely missing from the literature and allows drawing conclusions about CEOs' effects on the whole economy. Our analysis is also distinctive in that earlier studies focused on developed countries, while our data come from Hungary. Indeed, the institutional context matters: Crossland and Hambrick (2011) show that informal and formal institutions affect CEO discretion.

To quantify the effect of CEOs on firm performance in our data, we perform a variance decomposition, which computes what proportion of the total dispersion of productivity change is explained by CEO transitions. As demonstrated by countless papers in labor economics, the variance is also contaminated by the limited mobility bias and we make a correction of it with the placebo method. With our carefully chosen control group we also take into account the mechanical effect of firm age on productivity dispersion: as time passes, the firm gathers shocks and so the dispersion will increase.

We find that CEO transitions explain about 30% of the productivity change in our data. The placebo correction is also important here: without it, this share would be about twice as large.

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; McKenzie and Woodruff, 2021), 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., 2007; Sraer and Thesmar, 2007). 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

⁶Quigley et al. (2022) is a notable exception studying CEO effects in private businesses, but the private sample comprises of large firms.

⁷Mandatory registration of all company directors ensures complete coverage of CEOs in the whole corporate sector.

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 The Econometric Problem

Let firm i in year t have outcome

$$y_{it} = z_{m(i,t)} + e_{it}, \quad \mathbb{E}[e_{it}|z_{is}] = 0, \quad (1)$$

where $z_{m(i,t)}$ collects the CEO effect at time t and e_{it} is a shock. We assume that $z_{m(i,t)}$ is piecewise constant, changing only when the CEO changes and that e_{it} is mean independent of the CEO path for all s (“strict exogeneity” or “random mobility”).

Such fixed effect models are often estimated in applied work. For example, Bertrand and Schoar (2003) ETC ETC

Under the strict exogeneity assumption, the ordinary least squares (OLS) estimator of z_m is unbiased. Equation (1) can be rewritten as a dummy-variable regression

$$y_{it} = \sum_n z_n D_{m(i,t)=n} + e_{it},$$

and the first-order condition for the OLS estimator \hat{z}_n is

$$0 = \sum_{i,t:m(i,t)=n} (y_{it} - \hat{z}_n),$$

or

$$\hat{z}_n = \frac{1}{T_n} \sum_{i,t:m(i,t)=n} y_{it} = z_n + \frac{1}{T_n} \sum_{i,t:m(i,t)=n} e_{it},$$

where T_n is the number of observations with CEO n . The estimator is unbiased because $\mathbb{E}[\hat{z}_n|z_n] = z_n$. The estimator, however, is only consistent as $T_n \rightarrow \infty$, that is, as the number of observations per CEO grows large. In practice, many CEOs have short tenures (REFs), so \hat{z}_n contains substantial noise.

Econometric applications often require the second moments of \hat{z}_n , for example, to estimate the variance of CEO effects or to run regressions of an outcome y_{it} on \hat{z}_n . The former could inform us about the importance of CEO heterogeneity (REFs), while the latter could inform us about the effect of CEO quality on firm performance and firm policies (REFs).

The small-sample error, however, contaminates *all second moments* of \hat{z}_n . The variance of \hat{z}_n is inflated relative to the variance of true CEO effects z_n (REFs), and the covariance of \hat{z}_n with other variables that are affected by e_{it} is also contaminated. A well-known example of this in the labor literature is “limited mobility bias” (Andrews et al., 2008), which states that in a two-way fixed effects model of worker and firm effects, the variance of both effects is biased upwards, and

their correlation is biased downwards.

Existing methods of bias correction for second moments take two broad forms. One is ad hoc, such as leave-one-out adjustments that hope to remove own-observation noise. These can be impractical in CEO settings with a single manager per firm-year and lack formal guarantees. The other is structural: estimate the bias from the variance–covariance of shocks and the design matrix and then apply Empirical Bayes–type shrinkage (e.g., Andrews et al. 2008, Bonhomme et al. 2023). While powerful, this requires estimating many components and re-deriving formulas for each target second moment. Our approach is different: we construct placebo transitions that replicate the treated design matrix and use the placebo moments to read off the bias term directly, then subtract it. This delivers debiased variances, covariances, and slopes without modeling the full shock process.

Consider the following example to fix ideas. Our method will be more general, but this example captures the key elements.

Example. Event window $t \in \{-2, -1, 0, +1, +2\}$ with baseline $t = -1$. Pre-spell length $T_1 = 2$ (years $-2, -1$ under CEO A). Post-spell length $T_2 = 3$ (years $0, +1, +2$ under CEO B). The contrast of interest is the change from the baseline year $t = -1$ to three years after the transition $t = +3$:

$$y_{i,+3} - y_{i,-1}.$$

Stack outcomes as $\mathbf{y}_i = [y_{i,-2}, y_{i,-1}, y_{i,0}, y_{i,+1}, y_{i,+2}]'$. Define the piecewise-constant CEO path \mathbf{z}_i so that $z_{i,-2} = z_{i,-1}$ (CEO A), $z_{i,0} = z_{i,+1} = z_{i,+2}$ (CEO B). The linear contrast for the example is implemented by the weight vector

$$\mathbf{w} = [0, -1, 0, 0, +1]'$$

so that

$$y_{i,+3} - y_{i,-1} = \mathbf{w}'\mathbf{y}_i.$$

Model setup. We are now ready to present the general setup in matrix notation. Let firm i be observed for T periods with outcome path $\mathbf{y}_i \in \mathbb{R}^T$ that decomposes into a (piecewise-constant) manager-effect path \mathbf{z}_i and shocks \mathbf{e}_i :

$$\mathbf{y}_i = \mathbf{z}_i + \mathbf{e}_i, \quad \mathbb{E}[\mathbf{z}_i \mathbf{z}_i'] = ??, \quad \mathbb{E}[\mathbf{z}_i \mathbf{e}_i'] = 0, \quad \mathbb{E}[\mathbf{e}_i \mathbf{e}_i'] = \Sigma. \quad (2)$$

The first assumption about the error terms is the standard strict exogeneity assumption, often stated as “random mobility.” Every period’s shock is mean independent of the entire CEO path. The second assumption allows for arbitrary autocorrelation in shocks over time (e.g., persistent shocks).

For any linear contrast with weights $\mathbf{w} \in \mathbb{R}^T$, define $y_i = \mathbf{w}'\mathbf{y}_i$, $z_i = \mathbf{w}'\mathbf{z}_i$, and $\varepsilon_i = \mathbf{w}'\mathbf{e}_i$, resulting in

$$y_i = z_i + \varepsilon_i, \quad \mathbb{E}[z_i \varepsilon_i] = 0, \quad \mathbb{E}[z_i^2] = \lambda. \quad (3)$$

The last assumption defines the variance of the change in the CEO fixed effect $z_i = z_{i,+3} - z_{i,-1}$ and is typically the object of interest (REFs).

Estimation. Introduce a design matrix \mathbf{D} to map rows to CEO spells as

$$\mathbf{D} = \begin{bmatrix} 1 & 0 \\ 1 & 0 \\ 0 & 1 \\ 0 & 1 \\ 0 & 1 \end{bmatrix},$$

and the diagonal matrix of spell lengths \mathbf{T} as

$$\mathbf{T} = \text{diag}(T_1, T_2) = \text{diag}(2, 3).$$

We introduce the notation

$$\mathbf{P} = \mathbf{D} \mathbf{T}^{-1} \mathbf{D}'$$

for the projection matrix which converts outcomes to within-spell means. In the example,

$$\mathbf{P} = \begin{bmatrix} 1/2 & 1/2 & 0 & 0 & 0 \\ 1/2 & 1/2 & 0 & 0 & 0 \\ 0 & 0 & 1/3 & 1/3 & 1/3 \\ 0 & 0 & 1/3 & 1/3 & 1/3 \\ 0 & 0 & 1/3 & 1/3 & 1/3 \end{bmatrix} \Rightarrow \mathbf{P} \mathbf{y}_i = \begin{pmatrix} \frac{1}{2} \sum_{t=-2}^{-1} y_{it} \\ \frac{1}{2} \sum_{t=-2}^{-1} y_{it} \\ \frac{1}{3} \sum_{t=0}^{+2} y_{it} \\ \frac{1}{3} \sum_{t=0}^{+2} y_{it} \\ \frac{1}{3} \sum_{t=0}^{+2} y_{it} \end{pmatrix}.$$

Concretely for the example, \hat{z}_i is a weighted combination of the two spell means that implements $y_{i,+3} - y_{i,-1}$.

Note that \mathbf{P} is idempotent ($\mathbf{P}^2 = \mathbf{P}$) and symmetric ($\mathbf{P}' = \mathbf{P}$). This means that computing the within-spell mean twice is the same as doing it once.

Naive OLS using estimated effects. Define the estimated effect for the contrast by

$$\hat{z}_i = \mathbf{w}' \hat{\mathbf{z}}_i = \mathbf{w}' \mathbf{P} \mathbf{y}_i = z_i + \eta_i, \quad \eta_i := \mathbf{w}' \mathbf{P} \mathbf{e}_i.$$

Also define the contrasted outcome

$$y_i = \mathbf{w}' \mathbf{y}_i = z_i + \varepsilon_i, \quad \varepsilon_i := \mathbf{w}' \mathbf{e}_i.$$

Consider the (conceptual) population regression of y_i on z_i , $y_i = \beta z_i + u_i$ with $u_i = \varepsilon_i$ and $\mathbb{E}[z_i u_i] = 0$. In practice we do not observe z_i and instead regress y_i on \hat{z}_i . The OLS slope is

$$\tilde{\beta} = \frac{\text{Cov}(y_i, \hat{z}_i)}{\text{Var}(\hat{z}_i)} = \frac{\beta \text{Var}(z_i) + \text{Cov}(\varepsilon_i, \eta_i)}{\text{Var}(z_i) + \text{Var}(\eta_i)}.$$

Covariance term. $\text{Cov}(\varepsilon_i, \eta_i)$ is typically positive because both ε_i and η_i are formed from the same underlying shocks averaged over overlapping windows (short spells and persistent shocks make this more pronounced). This inflates the numerator relative to $\beta \text{Var}(z_i)$.

Variance term (classical measurement error). $\text{Var}(\eta_i) > 0$ inflates the denominator relative to $\text{Var}(z_i)$, the standard attenuation channel.

Together, the slope $\tilde{\beta}$ can be biased up or down depending on the balance between the inflated covariance (numerator) and the inflated variance (denominator). With i.i.d. shocks and long, balanced spells, $\text{Cov}(\varepsilon_i, \eta_i) \approx 0$ and $\text{Var}(\eta_i)$ is small, so the attenuation channel dominates but vanishes as sample size grows.

Small-sample nature. Both components are small-sample phenomena driven by short T_1 and T_2 . As $T_1, T_2 \rightarrow \infty$, spell means average out shocks so $\text{Var}(\eta_i) \rightarrow 0$ and $\text{Cov}(\varepsilon_i, \eta_i) \rightarrow 0$, implying $\tilde{\beta} \rightarrow \beta$.

Placebo identification. We construct placebo spells among firms that do not change CEOs but replicate the observed spell-length patterns and event windows of treated firms while excluding any windows around actual transitions. For placebo spells, $\text{Var}(z) = 0$, so

$$\widehat{\text{Cov}}^{\text{pl}}(y, \hat{z}) = \hat{A}_g, \quad \widehat{\text{Var}}^{\text{pl}}(\hat{z}) = \hat{B}_g. \quad (4)$$

These identify the bias components nonparametrically. Debiased treated moments and slopes are then

$$\widehat{\text{Cov}}^{\text{db}}(y, \hat{z}) = \widehat{\text{Cov}}^{\text{tr}}(y, \hat{z}) - \hat{A}_g, \quad (5)$$

$$\widehat{\text{Var}}^{\text{db}}(\hat{z}) = \widehat{\text{Var}}^{\text{tr}}(\hat{z}) - \hat{B}_g, \quad (6)$$

$$\hat{\beta}_g^{\text{db}} = \frac{\widehat{\text{Cov}}^{\text{tr}}(y, \hat{z}) - \hat{A}_g}{\widehat{\text{Var}}^{\text{tr}}(\hat{z}) - \hat{B}_g}, \quad (7)$$

pooled across groups with weights if needed. Inference follows from a delta method for ratios or a firm-level bootstrap that resamples within design groups, preserving spell design.

3 Estimation

Our estimation proceeds in three steps: estimate total factor productivity, recover manager fixed effects through two-way fixed effects regression, and implement placebo-controlled event studies to separate true CEO effects from noise. Each step builds toward separating true CEO effects from the noise that contaminates raw estimates.

Having estimated the placebo-corrected CEO effect, we decompose the variance of TFP into a general component and one depending on the CEO. The share of the CEO dependent component in the total variance provides a quantification of the CEO effect on firm productivity.

Step 1: Estimating Total Factor Productivity. We estimate total factor productivity (TFP) following standard production function approaches. We estimate a Cobb-Douglas production function with capital, labor, and materials as inputs, controlling for sector-year fixed effects. The residual from this estimation represents log TFP, which we decompose into firm effects (λ_i), manager effects (z_m), and residual productivity shocks (ϵ_{it}):

$$\omega_{it} = \lambda_i + z_m + \epsilon_{it} \quad (8)$$

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.

Step 2: Recovering Manager Fixed Effects. We estimate a two-way fixed effects model with firm and manager fixed effects on log TFP. Our identification relies on the strict exogeneity assumption where the conditional expectation of the error term ϵ_{it} is zero for a given firm-manager pair, for every t : $E[\epsilon_{it}|\lambda_i, z_m] = 0$ for $t = 1...T$. This assumption allows for various forms of endogenous mobility but rules out systematic patterns where managers consistently join improving or declining firms. The estimated CEO fixed effects are unbiased, but inconsistent due to the fact that with the sample size N increasing, the length of tenure of CEO-s at a given firm does not increase.

The event study provides a diagnostic test for this identification assumption. Pre-trends in productivity before CEO transitions would suggest that the strict exogeneity assumption is violated. If productivity systematically rises before good CEOs arrive, we worry that the positive trend continues post-transition, violating $E[\epsilon_{it}|z_m, \lambda_i] = 0$ for $\forall t$. 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.⁸

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 are connected through a chain of shared CEO-firm 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. In TWFE models researchers are typically limited to analysis within the largest connected component of their network. We are not limited by the largest connected component, only by the set of firms which experience a CEO transition. As ours is a within firm analysis, the two CEO-s we compare at the event of a CEO transition always belong to the same connected component, and hence their estimated fixed effects are comparable.

Step 3: 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.⁹ A consequence of this small sample bias is that naively estimating the effect of CEO transitions on TFP will likely overestimate the true effect.

⁸The 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.

⁹Worker-firm fixed effect studies face similar challenges called “limited mobility bias” (Andrews et al., 2008). This literature has developed bias-correction methods for the variance estimates of fixed effects (Bonhomme et al., 2023; Gaure, 2014).

To understand the sources of small-sample bias and how we address it, we remove the firm fixed effect from TFP by subtracting the firm average:

$$\Delta\omega_{imt} = \Delta z_{mit} + \Delta\epsilon_{it}, \quad (9)$$

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:

$$\Delta\omega_{imt} = \Delta\epsilon_{it}. \quad (10)$$

By applying the estimation procedure to measure the effect of CEO transitions on productivity not only to real CEO transitions but also to placebo CEO transitions, we can measure and filter out the noise component.

We construct placebo CEO transitions to use as control transitions to actual CEO transitions in three steps. First, we identify the set of clean CEO transitions by looking at firms with one CEO per year, where firms have at least two consecutive CEO spells. From around 3.5 Million firm-year observations, we identify 59,954 CEO changes, involving 42,902 firms. On this sample of clean CEO changes we estimate the time-varying hazard of actual CEO changes. Second, we identify all firms with long CEO tenures during which no actual CEO change occurs. Third, we find possible control firms for every CEO transition experienced by a firm by matching on the firm’s birth cohort, sector, year of CEO transition, and we also require that the control firms have no CEO change during the $[-4, 3]$ interval around the actual CEO transition. Since there is a very large number of possible controls, we take a random sample of all possible controls for any given actual transition. Then, we assign the calendar year of the actual CEO transition to the set of control firms for any given actual CEO transition. The assigned timing of the fake CEO change follows the empirical hazard, ensuring the mechanical noise properties—averaging residual productivity over varying tenure lengths—match between actual and placebo groups. This is evidenced in Panel B of Table 2, which shows that the spell-length distribution in the placebo transitions mirrors the distribution of spell-lengths among the actual CEO changes. We have ultimately 676,370 placebo changes of CEO-s which serve as controls for the 59,954 actual changes of CEOs.

As an example, consider a firm born in 1995 which experienced a CEO change in 2000, the CEO stayed until 2005. The event window for the transition is $[1995, 2005]$. To find the set of placebo CEO changes, we identify the set of all firms which were born in 1995, belong to the same sector as our firm of interest, and had no CEO change between 1995 and 2005. Among all the possible control firms we take a random sample and assign to this random sample a placebo CEO transition for the year 2000, creating two artificial CEO-spells, one from 1995 to 2000 and one from 2001 to 2005.

To obtain the dynamics of TFP within firms that experience a CEO transition, we implement a modified differences-in-differences framework. In particular, we estimate the event study coef-

ficients for every event time g on the set of actual CEO transitions, and estimate analogous event study parameters on the set of placebo CEO transitions. We subtract the estimated coefficients of the placebo from the coefficients on the real transition event time by event time, and plot this difference. Our modified differences-in-differences estimator adapts the Callaway and Sant’Anna (2021) estimator for two treatment types. We implement it 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. Formally, we implement the event study around CEO transitions at time g , comparing actual changes (treatment) to placebo changes (control):

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

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

Variance Decomposition. To quantify the contribution of CEO transitions to long-term performance differences, we carry out a variance decomposition. The limited mobility bias will affect the firm component of the variance of ω_{it} (Kline, 2024), and we correct our estimate with the placebo procedure discussed above. In addition to this problem, we face another difficulty: due to cumulative shocks, the variance of productivity mechanically increases as the firm ages. Our method of estimating an unbiased variance is the following.

We take the birth year b of each firm,¹⁰ and compute the change in productivity, $\Delta_0 \omega_{it}$ between year t and b – note that with this method we remove firm fixed effects from the dependent variable. For our control firms with a pseudo CEO change this will be equal to the following:

$$\Delta_0 \omega_{it} = \sum_{a=1}^{t-b} \Delta \omega_{i,b+a} = \sum_{a=1}^{t-b} \Delta \epsilon_{i,b+a}, \quad (12)$$

while for treated firms (those with an actual CEO change):

$$\Delta_0 \omega_{it} = \Delta_0 z_{mi} + \sum_{a=1}^{t-b} \Delta \omega_{i,b+a} = \Delta_0 z_{mi} + \sum_{a=1}^{t-b} \Delta \epsilon_{i,b+a}, \quad (13)$$

The variance of productivity change for the control and treated groups is

$$\text{Var}(\Delta_0 \omega_{it} \mid D_{it} = 0) = (t - b) \sigma_0^2 \quad (14)$$

$$\text{Var}(\Delta_0 \omega_{it} \mid D_{it} = 1) = \text{Var}(\Delta_0 z) + (t - b) \sigma_1^2 \quad (15)$$

For the latter formula, we assumed that $\Delta_0 z$ is orthogonal to $\Delta_0 \epsilon$.

The key assumption for an unbiased estimator of $\text{Var}(\Delta_0 z)$ is that the variance of error terms depends only on firm age and treatment status, but not on the timing of treatment:

$$\text{Var}(\Delta_0 \epsilon_{it} \mid D_{it} = 1, t = b_i + a) = \text{Var}(\Delta_0 \epsilon_{it} \mid D_{it} = 0, t = b_i + a) \quad (16)$$

¹⁰In the estimation, we do not take the birth year but a latter year to get a full year of existence as baseline.

The estimation of the unbiased variance is as follows. We first compute the variance (relative to year b) as $(\Delta_0\omega - \overline{\Delta_0\omega})^2$, where the latter term is the mean of productivity within firm. We regress the variance on firm age - treatment status interactions and compute the age-adjusted variance by subtracting the estimated age - treatment effects from the actual variance. Finally, we run a simple difference-in-differences regression on the age-adjusted variance, which provides an unbiased estimate of the difference of $\text{Var}(\Delta z)$.

4 Monte Carlo Design and Interpretation

We use a simple Monte Carlo to show why second moments are biased and how the placebo correction behaves under empirically relevant complications. The guiding intuition from our design applies: (i) when shocks are i.i.d. and spells are balanced, naive and debiased profiles line up; (ii) with persistent shocks or short/unbalanced spells, spurious pre-trends may appear even if the causal pre-trend is zero; (iii) allowing for a scalar volatility difference across groups preserves identification via placebo moments.

Our starting point is a window with two spells per firm. Placebo firms never actually change CEOs; we split long no-change stretches into the same pre/post spell lengths as in the treated sample and align them in event time. We compute the same statistics in both samples with the same weights and then subtract the placebo quantity. Because covariance is bilinear and variance is additive under strict exogeneity, the subtraction removes the bias term without modeling the full autocovariance structure. If treated firms are more volatile, we estimate a groupwise scalar and multiply the placebo term before subtraction.

Before turning to specific parameter choices, we motivate the six scenarios from econometric first principles. Bias in second moments depends on two pillars: (i) the shock process (the autocovariance matrix Σ) and (ii) the spell design encoded by the event-time design matrix. Persistent shocks (AR(1) with $\rho > 0$) can create spurious pre-trends and distort dynamic slopes because estimated effects embed noise that is serially correlated. Short or uneven (unbalanced) spells amplify small-sample noise because within-spell means are computed over few observations; this inflates $\text{Var}(\hat{z})$ and, with persistence, also inflates the covariance component. Our scenarios therefore vary only along these two theoretically salient dimensions (persistence and spell length/design), plus a simple scale shift (excess variance in treated groups). We intentionally keep the rest of the setup simple so that each complication can be read as a clean stress test of the placebo debiasing.

The scenarios and figure panels follow the code in `src/montecarlo` and `src/figuremc.do`:

Interpreting Table 1 and Figure 1. Table 1 reports six Monte Carlo calibrations with different parameters. The baseline is very simple: balanced panels and i.i.d. (uncorrelated) error terms. Across scenarios we focus on three statistics that are widely used in applications: (i) the standard deviation of the change in the manager fixed effect when the firm moves from the first to the second CEO, a key input to variance decompositions; (ii) an event-study slope two full years after the CEO change (the third post year), which captures dynamic adjustment; and (iii) a pre-trend coefficient two years before the change, which should be zero in the data-generating

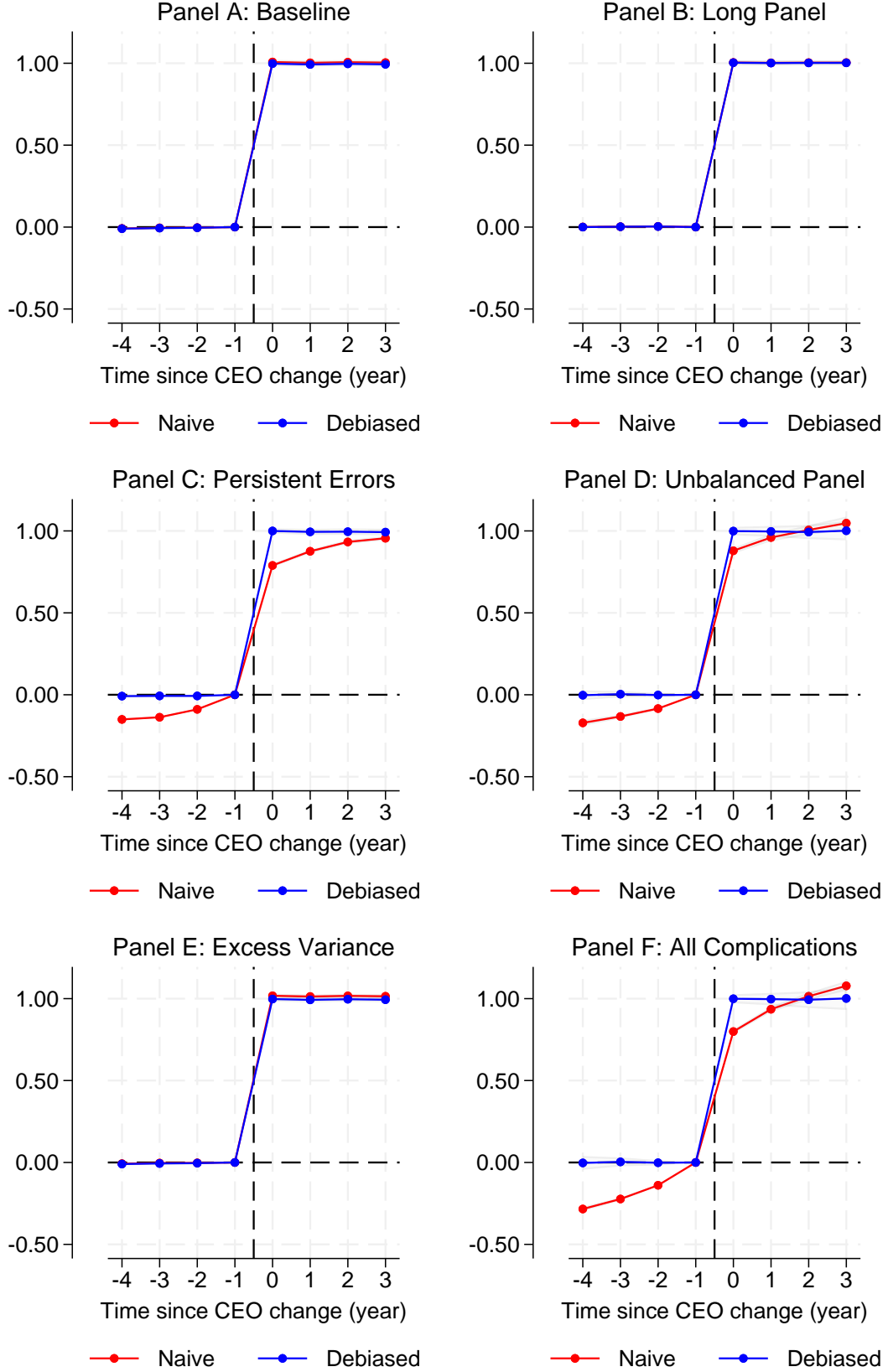


Figure 1: Monte Carlo event studies under six scenarios. Panels: Baseline, Long Panel, Persistent Errors, Unbalanced Panel, Excess Variance, All Complications. Bands are truncated to $[-0.75, 1.75]$ for readability. [[comment: add precise definitions for β_0 , β_1 , $\delta\beta$ from the estimator]]

process once the baseline year $t = -1$ is normalized to zero. The debiasing is designed to remove finite-sample noise in all three statistics.

In the baseline, the naive variance is slightly upward biased, while the debiased standard deviation of the manager-effect change recovers exactly 0.10, which is the true value by construction. The post-arrival event-study slope exhibits some sampling noise but nothing systematic. The long-panel scenario (spells of length 20) shows that all biases essentially disappear; this is a small-sample phenomenon in short panels.

With persistent errors ($\rho = 0.9$), the naive standard deviation is severely upward biased (about 23% in our calibration), and spurious pre-trends emerge: before the manager arrives, the naive estimate suggests a negative effect. Because this is a Monte Carlo, we know the true pre-trend is zero; the bias is entirely mechanical. The naive post-arrival slope at +2 is below one (about 0.93), consistent with a gradual, biased buildup. Debiasing removes the pre-trend and delivers a post-arrival slope indistinguishable from the true value of 1.00.

The unbalanced-panel scenario (with a 20% annual hazard of CEO change) compounds persistence and varying spell lengths. We again see a severe upward bias in the variance and evidence of pre-trends in the naive estimates. Debiasing removes these artifacts. Post-arrival dynamics look less gradual than under persistence alone, plausibly because typical spells are shorter in unbalanced panels, leaving less time for any gradual buildup to manifest.

In the excess-variance scenario, the treated group has higher shock variance than controls. Relative to the baseline, this produces more dispersion and a modest upward bias in the naive post-arrival slope; the debiased estimates correct both. Finally, the “all complications” scenario combines short and unbalanced spells, persistent errors, and excess variance. Here all problems are most severe: very large variance bias, pronounced pre-trends before the CEO arrives, and distorted dynamics. Yet the placebo correction handles them jointly: the debiased standard deviation matches 0.10, the post-arrival slope is 1.00, and the pre-trend is 0.00 across all six scenarios. [[note: if you want exact standard errors or scenario-specific percent deviations tabulated, we can add them once the csv outputs are finalized.]]

Figure 1 displays event-study slopes for the same six scenarios. The motivation for plotting these is that, with persistent errors and/or unbalanced panels—both common in applications—noise can introduce apparent pre-trends. Researchers routinely check pre-trends, either formally or informally, and may be misled if naive profiles are biased.

Panel A (baseline) shows little bias: differences between naive and debiased lines are barely visible. Panel B (long panel) confirms that bias vanishes with long spells. Panel C (persistent errors) is the key case: the naive profile displays pre-trends and a somewhat sluggish post-arrival adjustment. The intuition is that the naive estimator uses estimated fixed effects that embed noise. With persistence, firms that happen to draw positive shocks over the post period are more likely to be classified as receiving a “good” CEO; by the same logic, their recent past is mechanically below average, generating a spurious pre-trend. The placebo correction strips out precisely this noise component.

Panels D–F illustrate that the problems intensify with unbalanced spells and excess variance, and are most severe when all complications are present. The true event study is the debiased (placebo-corrected) line: it jumps from 0 at $t = 0^-$ to 1 at $t = 0^+$ and stays flat thereafter.

By contrast, the naive profiles can show strong pre-trends and distorted post dynamics. [[note: confirm which color/style corresponds to naive vs debiased in the final figure legend.]]

We expect to see exactly zero before and exactly one after the arrival of the CEO in TFP units when the true effect is normalized so that a one-unit increase in the fixed effect raises TFP by one. Other outcomes may have different scales, but the logic is the same. With persistence, short spells, or excess variance in treated firms, the naive profiles will typically display pre-trends and inflated post effects; subtracting the placebo series removes these artifacts.

5 Application: CEO Arrivals and Revenue in Hungarian Firms

We apply the placebo-controlled debiasing to the universe of Hungarian firms, focusing on CEO arrivals and firm revenue outcomes. The Hungarian data provide an ideal setting because we observe the full population of businesses over three decades, not only large corporations. This scope yields many CEO mobilities across firms, allowing us to study the arrival of new CEOs and the associated within-firm dynamics. We document the data construction, sample restrictions, and additional statistics in our companion paper [[note: add citation to companion paper]]. Here we summarize the minimal features needed for this methodological application and outline the design we will estimate.

We consider all CEO changes between 1992 and 2022 (31 years). We restrict to firms that have ever reached at least 5 employees in their lifetime, to exclude non-employer businesses that rarely change CEOs and would inflate the sample numerically without adding identifying transitions. After this restriction, there are about 60,000 CEO changes [[note: update exact count]]. Many transitions are from the first to the second CEO and from founders to non-founders; these categories are similar but not identical because founders can return later and some firms begin with outsider CEOs. We will include a table with summary statistics for these quantities [[note: add exhibit reference]].

Identification relies on random mobility (strict exogeneity): past and future shocks to outcomes are orthogonal to CEO mobility timing. Because applied work commonly examines pre-trends to assess plausibility, we present event-study profiles grounded in this assumption.

We use firm revenue as the outcome variable for clarity of exposition. We remove industry-year means so that the outcome is measured as a deviation from the sectoral environment.

Estimating CEO Spell Effects

For each transition from CEO A to CEO B, let the firm have two consecutive spells covering the entire tenures of A and B (not just the event window). We estimate a spell-level CEO effect as the within-spell mean of demeaned revenue. Concretely, let r_{it} denote firm revenue and let $\tilde{r}_{it} = r_{it} - \bar{r}_{st}$ be revenue demeaned by industry-year (s, t) cells. Let $\mathbb{1}\{\text{spell} = s\}$ indicate the A or B spell. The spell effect is

$$\hat{z}_{is} = \frac{1}{T_{is}} \sum_{t \in s} \tilde{r}_{it}, \quad s \in \{A, B\}. \quad (17)$$

Define the change in the CEO effect for transition i by

$$\Delta\hat{z}_i = \hat{z}_{iB} - \hat{z}_{iA}. \quad (18)$$

This statistic is the core object in variance decompositions; by theory and by Monte Carlo, its variance is upward biased in short panels and under persistence, which motivates our placebo debiasing.

[[note: if a firm has more than two consecutive spells, we treat adjacent spell pairs as separate transitions; multiple transitions per firm are allowed but clustered at the firm level.]]

Event-Study Specification

We then study revenue dynamics around CEO arrivals in event time. For each transition with event date g , define dummies for event-time leads/lags relative to g over the window $[-4, +3]$. We estimate

$$\tilde{r}_{it} = \alpha_i + \sum_{\ell \in \{-4, -3, -2, 0, 1, 2, 3\}} \beta_\ell \mathbb{1}\{t - g = \ell\} + \varepsilon_{it}, \quad (19)$$

with the baseline period normalized to zero at $t = -1$. The coefficients β_ℓ trace out the revenue response before and after CEO arrival. Under random mobility, we expect no pre-trends: $\beta_{-4}, \beta_{-3}, \beta_{-2} \approx 0$. Post-arrival dynamics are a question for the data: effects may be immediate or build gradually during the new CEO’s tenure. We will report both naive and placebo-corrected profiles; the latter subtract the corresponding placebo moments computed on matched non-changers with replicated spell designs.

Placebo Construction and Debiasing

Placebo transitions assign a fake CEO change at the same calendar year as an actual transition to control firms that remain under the same CEO and have long no-change runs covering the event window. We split these control runs into the same pre/post spell lengths as the treated transition and align them in event time. Because the design matrices and weights are the same by construction, and because we assume the error autocovariance (Σ) is the same up to a scalar between treated and placebo groups, the covariance and variance bias components can be “precomputed” in the placebo sample and subtracted from the treated estimates. This delivers debiased variance of $\Delta\hat{z}$ and debiased event-study coefficients β_ℓ .

[[note: add the companion paper citation for full data construction and additional outcomes (e.g., investment, materials, labor).]]

Hungary provides an ideal setting for studying CEO effects in the universe of private firms. The country offers complete administrative data coverage for all incorporated businesses, spanning 30 years from the transition economy of the 1990s through EU accession in 2004 to the present. CEO registration is mandatory for every registered firm. The 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 with double-entry bookkeeping. The data has information on financial variables and some additional information, such as sector of activity, employment, and ownership (state, domestic, foreign). The two datasets cover 1,063,172 firms over 31 years, yielding 9,627,484 firm-year observations before sample restrictions.

Constructing CEO time series across firms 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 begins) and 2010 (when birthdates reporting starts), though the 1990s data achieves reasonable coverage through name and address matching.

Finding CEOs among all individuals from the data 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 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; Shleifer and Vishny, 1994).

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.

Firms with multiple CEO changes are handled by splitting their history into adjacent transition pairs. After collapsing firm-CEO spells, intermediate spells are duplicated so that a firm with

spells 1/2/3 contributes two transitions, $1 \rightarrow 2$ and $2 \rightarrow 3$. Each transition defines a pseudo-firm, indexed by a constructed identifier that groups the true firm with the relevant spell window. Fixed effects are estimated separately for these pseudo-firms, so outcomes around different transitions are allowed to have distinct firm intercepts. These pseudo-firms are not treated as independent statistical units. We retain the original firm identifier and cluster standard errors by the true firm.

Appendix Table ?? presents the number of firms in our sample relative to the total number of firms in the data for several year along the time series. The number of Hungarian firms increases abruptly during the nineties and also in the first 15 years of 2000s, albeit at a smaller pace. The result of dropping the small firms results in using about one quarter of all firms. In the first year of study (1992) we have 26 thousand firms which increases to 115 thousand towards the end of the period. Overall, we have over one million firms and almost 10 million firm-years in the sample. The table also shows the number of CEOs each year, which presents a similar pattern; in 1992 we observe 32 thousand CEOs and in 2022 122 thousand. The total number of distinct CEOs is 340 thousand. Appendix Table ?? shows the industrial distribution of firms and CEOs. The largest sectors are Wholesale-Retail, Telecom and Business Services and Nontradable Services.

Table ?? shows the proportion of firms (and firm-years) by the number of CEOs observed in the firm during the period of observation. Not surprisingly, a majority of firms have only one CEO: 63% of firms and 80% of firm-years belong to this category. One quarter of firms had two CEOs while 13% at least three CEOs. This latter category comprises only 3% of firm-years. CEO spell lengths follow an exponential distribution with a 20 percent annual hazard rate, implying typical tenures of 3-7 years.

6 Results

CEO Transitions and Productivity Change. Table ?? shows the average effect of CEO transition on productivity as estimated by the naive OLS regression, the placebo effect and the corrected regression estimate.¹¹ To start with the effect on the whole sample, TFP increases by 0.5% around the CEO change. The placebo regression produces precisely measured zeros.¹² That is, even without a CEO change, TFP changes somewhat. The placebo controlled estimate equals 0.3%.

As most of our firms are family-owned, we look into an interesting CEO transition: when founders relinquish control. For comparison, we also run a separate regression on other CEO changes. When founders are the departing CEO, the estimated effect is 1%. For the other CEO changes we do not find any change in TFP. This is also a test of our method: when similar CEOs replace each other, the effect is zero, at least on average. On the contrary, when we measure the TFP change between two distinctly different CEOs, we do find an economically and statistically significant effect.¹³

¹¹The naive and the pseudo outcome regression are estimated on the treated and placebo samples, respectively. The controls are the not-yet-treated firm years. The corrected regressions are estimated on the merged samples and we use the two-way treatment method discussed in Section 3.

¹²This is reasonable because here we do not have a reason to believe the estimated effect by simple OLS regression is biased as we do not select on unobservables correlated with productivity.

¹³The reason for a positive effect of founder replacement may be the timing of the change. Founders usually

Finally, we analyze perhaps the most interesting question: how the productivity reacts to the arrival of a better or worse CEO than the incumbent? For this, we split the sample into two types of CEO transitions: when the incoming CEO has a higher or lower quality (as measured by the associated fixed effect) than the incumbent CEO. We also split the placebo sample into firms which experience an increase/decline of their TFP around the pseudo CEO change. The estimated coefficients are presented in the last two columns of Table ?? and demonstrate the importance of CEOs and also show how the placebo correction works. Better CEOs than the incumbent increase TFP by 6% while worse CEOs decrease it by 5%. This large effect, however, is upward biased, as the placebo regressions demonstrate. The pseudo CEO change also produces a sizable effect of $\pm 2.5\%$. Thus, the corrected effect is much smaller: better CEOs increase TFP by 2.7% while worse CEOs decrease it by 1.8%. Thus, the difference between a good and a bad CEO is 4.5%.

Figure ?? visualizes the evolution of TFP around the CEO change for four samples: all changes, when the incumbent is the founder of the firm, all other incumbents, and for the period of mature market economy (2004 – 2022).¹⁴ On each figure we present the event time regression estimates for the whole sample (black line) and for better and worse CEO replacements (blue and red lines). The estimations leave little pre-trend and we find large swings in TFP. If the incoming CEO is better than the incumbent, TFP increases by 3 – 4% while worse CEOs decrease it by about 2%. New CEOs have an effect immediately on the firm and this proves to be quite stable, as the estimated coefficients do not change much.

Differential Effects on Inputs. Figure ?? presents event studies examining how CEO transitions affect different firm inputs.

Good CEOs have immediate and substantial effects on operational inputs. Material costs increase by 30% and the wage bill by 13 percent (all effects highly significant). Firms under bad managers experience the opposite: a decline in material and employment costs by about 20%. These effects appear immediately in the year of CEO transition and persist throughout the post-period, consistent with new CEOs quickly adjusting operational scale.

In contrast, strategic variables show different patterns. Fixed assets exhibit a gradual change under good CEOs and little or no change under bad CEOs. The proportion of firms with intangible assets does not change at all around the CEO transition.

Contribution of CEOs to Firm Productivity. To assess how important CEOs are for firm performance, we conduct the variance decomposition. As we describe at the end of Section 3, we face two challenges. The variance is biased and it also depends on firm age. The outcome of our method is presented in Figure ?? for TFP in the top row and log revenue in the bottom row. The blue line shows the within-firm change in variance relative to the second year after firm foundation. The blue line on panels A and C show the evolution of this variance around the CEO transition. Before the CEO transition, the variance grows continuously. The transition

give up their central role in the firm when they find they no longer have enough energy to run the firm.

¹⁴The latter sample is used to make our results more comparable to mature market economies. In the 1990s the economy was changing rapidly as it underwent rapid economic liberalization, transition to market economy, foreign direct investment inflows and large scale privatization. We conduct this robustness check by restricting the sample to post-2004 data following Hungary’s EU accession.

increases the variance abruptly; in later years it stays relatively stable for TFP and continues growing for revenue. Part of this growth, however, is due to limited mobility bias, and part arises mechanically: as time passes, the chance of receiving shocks increases. To control for these biases, we estimate the same variance on the pseudo transition sample. As the red line shows, this also increases in time. Panels B and D of the figure show the evolution of the estimated variance in the two samples by firm age.

Our estimate of the contribution of CEO change to the total variance of TFP is the difference between the two estimates, and we quantify it in Appendix Table ???. The table presents the total variance, the unadjusted contribution of CEO change and the adjusted contribution. We analyze only one CEO change, and the contribution depends on the length of the period measured (the longer the period, the more the variance increases for reasons unrelated to CEO change). In the first 10 years of existence, the uncorrelated share of CEO change in the variance is 62%. Our correction decreases this number to 29%.

7 Conclusion

This paper estimates the contribution of CEOs to firm productivity by exploiting a unique administrative dataset covering the entire population of Hungarian private firms and their CEOs from 1992 to 2022. The novelty of the data lies in its unprecedented scope and completeness, allowing the study of CEO effects not only in large firms but crucially in small and medium-sized enterprises that dominate every economy. The combination of the dataset with a theoretically grounded model that distinguishes owner-controlled capital decisions from CEO-controlled operational inputs allows for a more precise attribution of productivity effects.

The paper develops a new placebo-controlled event study design that overcomes limited mobility bias, which contaminates studies using two fixed effects. By creating matched placebo CEO transitions in firms without actual leadership changes, the method effectively separates true CEO skill effects on productivity from mechanical noise. Empirically, the findings reveal that the true causal effect of CEO quality on firm productivity is economically meaningful but notably smaller than raw correlations suggest, explaining about 7–8 percent of productivity variation.

An alternative way to deal with the noise problem is to use observable manager characteristics as measures of skill. Observable characteristics such as education and work experience (De Pirro et al., 2025), foreign name as a proxy for international exposure (Koren and Telegdy, 2023), and the 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.

Table 1: Monte Carlo parameters by scenario

Scenario	Baseline	Long Panel	Persistent Errors	Unbalanced Panel	Excess Variance	All Complications
Parameters						
N_{treated}				50,000		
N_{control}				50,000		
$\sigma(\Delta z)$				0.10		
$\sigma(\epsilon_{\text{control}})$				0.05		
T_{max}	5	20	5	5	5	5
ρ	0.00	0.00	0.90	0.90	0.00	0.90
$\sigma(\epsilon_{\text{treated}})$	0.05	0.05	0.05	0.05	0.075	0.075
CEO change hazard	—	—	—	0.20	—	0.20
Estimates						
$\sigma(\Delta \hat{z})$ (OLS)	0.104	0.101	0.123	0.118	0.109	0.137
$\sigma(\Delta \hat{z})$ (debiased)	0.100	0.100	0.100	0.100	0.100	0.100
R^2 (OLS)	0.739	0.686	0.787	0.866	0.587	0.798
R^2 (debiased)	0.664	0.668	0.596	0.608	0.468	0.408
$\hat{\beta}_2$ (OLS)	1.007	1.004	0.933***	1.006	1.017***	1.015
$\hat{\beta}_2$ (debiased)	0.997	1.002	0.996	0.994	0.997	0.994
$\hat{\beta}_{-2}$ (OLS)	−0.003	0.004	−0.089***	−0.084***	−0.002	−0.139***
$\hat{\beta}_{-2}$ (debiased)	−0.004	0.004	−0.007**	−0.002	−0.004	−0.002

Notes: Every Monte Carlo simulation assumes 50,000 treated firms and 50,000 control firms. The standard deviation of true CEO effects is 0.10 and the standard deviation of errors in the control group is 0.05 in all scenarios. The table lists scenario-specific parameters: maximum spell length T_{max} , error autocorrelation ρ , error standard deviation in the treated group $\sigma(\epsilon_{\text{treated}})$, and the annual hazard of CEO change in the unbalanced panel scenarios. The *baseline* calibration assumes short balanced spells and i.i.d. errors. Columns (2) to (5) introduce one complication at a time. The *long panel* scenario extends the length of spells. The *persistent errors* scenario adds strong autocorrelation. The *unbalanced panel* scenario introduces firm-specific spell lengths drawn from a geometric distribution with a constant hazard of CEO change. The *excess variance* scenario adds a 50% increase in error volatility in the treated group relative to the control group. The *all complications* scenario combines unbalanced spells, persistent errors, and excess variance. Parameter values are chosen to represent realistic moments from our application where CEO tenures are ofte short, TFP shocks are persistent, and treated firms are more volatile than control firms.

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A Online Appendix: Additional Tables and Figures