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Nepotism and Labor-Market Spillovers Within the Firm

Alexandre Jenni*

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Abstract

An established body of literature finds that family firm control negatively affects firm performance. In this article, I study the effects of father-child CEO successions on the labor market trajectories of incumbent workers. Because occupations are unobserved in my data, I use the change in the identity of a firm top-earner as a proxy for CEO succession. Implementing my approach with matched employer-employee data from Switzerland, I find that about 3% of all CEO successions among middle-sized and large firms are father-child successions. I then use a matched difference-in-difference design to identify the effects of being exposed to a father-child CEO succession relative to a generic CEO succession on incumbent workers' turnover, earnings, and overall employment. Workers exposed to father-child CEO successions are 5 percentage points more likely to be continuously employed at a firm 5 years after the succession. The effect on firm tenure is strongest for workers at the bottom of the firm's income distribution. I find mild evidence for a decrease in incumbent workers' earnings after a father-child CEO succession, but no effect on overall employment probability. The absence of significant negative effects of family firm management on worker outcomes contrasts with the negative effects on firm outcomes estimated in the literature. It suggests that family CEOs may extract a smaller share of the firm surplus.

Keywords: nepotism, firm management, managerial succession, family firms, firm tenure

JEL: J53, M52, G30

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1 Introduction

When handing over managerial power, firm founders face a choice between appointing a family member or an unrelated person as the new chief executive officer (CEO). Research in corporate finance has shown that inherited managerial control lowers the profitability and overall performance of firms (Pérez-González, 2006; Bennedsen et al., 2007). Yet, a majority of firms in the world are controlled by their founders or their founders' descendants (La Porta, Lopez-de-Silanes, and Shleifer, 1999). What is more, countries often incentivize the intergenerational transmission of firm ownership, for example by exempting inherited firms from bequest taxation.

Can encouraging the intergenerational transmission of firm control be rationalized if the welfare of the firm's workers is taken into account? So far, there is no compelling evidence about the effects of inherited managerial control on the labor market trajectories of the firm's workers. In theory, the adverse effects of inherited managerial control on firm performance are ex-ante consistent with completely opposite effects on workers' labor market outcomes. On the one hand, family CEOs may be generally less competent (Bennedsen et al., 2007). Their appointment would then lead to a decrease in productivity at the firm, pushing down the salaries of the incumbent workers or encouraging them to leave the firm. On the other hand, family CEOs may show a higher concern for the firm's reputation or may have personal ties with the employees. They would then be more reluctant to extract a higher share of the firm surplus from the workers. As such, family-run firms might be characterized by lower turnover and higher salaries. It is plausible that the exact incidence of family CEOs on their employees' labor market trajectories varies by these employees' initial positions within the firm.

In this article, I propose a new empirical strategy to isolate the causal effect of family CEOs on the labor market performance of the firm's employees. Because family-run firms might differ from firms with external CEOs along various observable and unobservable characteristics, my identification strategy relies on the timing of CEO promotions. However, I abstain from making simple before-after comparisons. Indeed, the promotion of any new CEO is likely to affect employees' labor market trajectories through various channels. For example, new CEOs might decide to reorient the firm activities, thereby leading to systematically higher turnover in those firms. To abstract from the trends affecting all workers exposed to turnover in the CEO position, I compare workers exposed to a father-child CEO succession with those exposed to a non-familial succession.

To implement my strategy, I construct a matched employer-employee panel dataset from the Swiss social security records spanning the years 1981 to 2019. By linking the social security data to the Swiss Population and Household Statistics (STATPOP), I can identify parent-child relationships among workers. As with many other administrative datasets reporting workers' earnings, my dataset suffers from the limitation that it does not contain any information on the hierarchy of workers within the firm.

To overcome this obstacle, I develop a strategy to infer CEO turnover based on the earnings distribution within the firm. In short, I work under the assumption that the firm's CEO is the employee with the highest annual earnings in the firm. I then equate a year-to-year change in the identity of the firm's highest earner to the promotion of a new CEO. When implementing this procedure on firms with at least 5 employees, I find that, for the period covered by my data, about 12% of firms go through a CEO succession in any year. Strikingly, about 3% of all CEO successions are direct father-child successions.

The remaining challenge, however, is to identify a set of workers exposed to a non-familial CEO succession whose labor market trajectory provides the right counterfactual trajectories for the workers exposed to a father-child CEO succession if they *had been exposed to a non-familial succession instead*. To address this challenge, I first restrict the set of CEO successions to those in which the succession could have potentially happened between a father and a child. After implementing these restrictions, I am left with 6,592 CEO successions affecting approximately 300,000 workers. Among these successions, about 20% are father-child successions.

I then follow the job displacement literature and match each worker exposed to a father-child CEO succession to a worker exposed to an external CEO succession on pre-treatment characteristics¹. Having selected my control group, I estimate the relative effect of father-child CEO succession on incumbent

¹See e.g. Schmieder, Von Wachter, and Heining (2023) for the application of a similar design to study the effect of mass layoff and Arnold et al. (2023) in the context of mergers and acquisition.

workers' turnover, earnings, and overall employment using a matched difference-in-difference design. Before presenting my estimates, I am careful to check that my samples of treated and matched workers follow similar pre-trends in outcomes.

I organize my findings along three sets of results. First, I find strong evidence that family CEOs increase workers' tenure at the firm. I estimate that workers are approximately 5 percentage points more likely to be continuously employed at a firm 5 years after the CEO succession in the case of a father-child succession. This effect on tenure at the firm is strongest for workers at the bottom of the firm's income distribution. Second, I find mild evidence supporting the hypothesis that family CEOs decrease workers' earnings. At a 5-year horizon, the mean reduction in earnings among workers exposed to father-child CEO successions is 1%. This effect on earnings is only marginally significant. The earnings of the workers from the upper-middle part of the pre-succession income distribution are most negatively affected, although my estimates are too imprecise to make precise statements about the relative size of the effects across workers from different parts of the firm's hierarchy. Third, I reject any effect of family CEOs on workers' overall employment probability. This last finding comes with the caveat that my estimates might suffer from attenuation bias, as I only infer CEO successions from changes in the within-firm income ranks.

My results are relevant to the literature studying the effect of family control on firm performance. There is an entire strand of literature studying family firms and the implications of family control for the governance, financing, and performance of these firms (see Bertrand and Schoar (2006) for a review). Because family-run firms differ in systematic ways from other firms, the most credible estimates of the effect of familial control on firm performance leverage either the timing of CEO appointment (Pérez-González, 2006) or random fluctuations in the managers' propensity to transmit the firm to their children generated by the gender of the manager's first child (Bennedsen et al., 2007). The consensus from this literature is that family CEOs significantly underperform their peers when evaluated on different measures of firm performance and profitability. By contrast to these negative effects on firm performance, I only find mild evidence for the existence of adversarial effects of family CEOs on workers' outcomes, even at a relatively long time horizon. This suggests that family-run firms may be less profitable because their managers extract a smaller share of the firm surplus from the workers.

More generally, this paper contributes to the literature on CEO practices and firm performance. Papers in this literature seek to measure and explain differences in managers' practices (see e.g. Bloom and Van Reenen (2007)), to estimate the causal effect of specific managers' characteristics on firm performance (Acemoglu, He, and Le Maire, 2022) or to identify the value of CEOs' firm-specific human capital (Jäger and Heining, 2022). I find that, even though they have been shown to lower overall firm performance, family CEOs increase workers' tenure at the firm and do not significantly reduce their earnings. One possible interpretation of my results is that family CEOs differ systematically from other CEOs in their propensity to lay off workers when facing negative economic shocks. However, a test of this hypothesis would require having access simultaneously to firms' balance sheets and workers' labor market trajectories.

Finally, this paper connects with studies examining the role of the intergenerational transmission of employers for the labor market outcomes of the next generation. Corak and Piraino (2011) has pioneered this literature by providing systematic evidence that co-employment of parents and children is a common phenomenon, especially at the top of the parental income distribution. More recent literature attempts to quantify the effects of having a parental connection at a firm, either on the employment probability (San, 2021) or on the children's short- and long-term earnings (Engzell and Wilmers, 2021; Staiger, 2021). In this paper, I analyze the intergenerational transmission of employers from another angle by looking at the labor market trajectories of the coworkers of the worker who benefited from familial connections in the firm. I document that the intergenerational transmission of employers – if occurring at the level of CEO positions – has a significant impact on workers' tenure at the firm, especially for workers at the lower end of the income hierarchy within the firm.

This paper proceeds as follows. Section 2 presents our data, describes how we link kinship members, and introduces our approach to computing individual child penalties. Section 3 presents and discusses the empirical correlations in child penalty among kinship members. It also discusses how heterogeneous income profiles affect the interpretation of the child penalty correlations. Section 4 shows that both models can fit the empirical correlations in the data. It then discusses the model estimation results and explores whether the main results are driven by the geographical clustering of kinship members. Section

5 concludes.

2 Data

This project uses a merge between two Swiss administrative datasets: the Swiss population register and the Swiss social security records. I use the Swiss population register to identify father-child relationships and to obtain individual demographic variables. I rely on the Swiss social security records to construct a matched employer-employee panel and to obtain individual labor market outcomes. In addition, I also use the Swiss earnings survey to assign industry codes to the firms in my sample. In this section, I further describe the data sources, define the key variables, and outline the construction of the analysis sample.

2.1 Data Sources

I construct individual demographic variables using the years 2010 to 2020 of the Swiss Population and Household Statistics (STATPOP). The population register lists basic demographic information such as gender, year of birth, place of birth, and place of residence for all Swiss residents. On top of these individual characteristics, the dataset contains identifiers for the parents of each individual in the data. Parent-child relationships are only identified if both individuals lived at some point between 2010 and 2020 in Switzerland. It may be that some firm managers died before 2010 and cannot be matched to their children. However, because I restrict my analysis to successions occurring in 1991 or later, the number of unidentified father-child successions in the data should be limited.

Labor market variables come from the Swiss social security records spanning the years 1981 to 2019. This monthly panel contains the universe of formal employment, self-employment, unemployment spells, maternity leave benefits, and military service compensations.² For each individual and year, I sum income across these categories to derive a measure of total individual annual labor income. I deflate income to the 2018 price level using the Swiss Consumer Price Index. I also keep track of whether an individual is employed in a given year, which I define as having total yearly employment and self-employment earnings above CHF 10,000. In the years after 2010, I know an individual's residency status from the population register and can correctly impute either an income of 0 or a missing value if an individual does not have any entry in the social security records. For the years before 2010, information on the residency status information is unavailable. I work under the assumption that individuals without a social security record in a given year have no income that year³. Unfortunately, the social security records do not contain any information on occupations or on the hierarchical rank of employees within a firm.

I also use the social security records to construct a yearly matched employer-employee dataset. The raw data contains a firm identifier for each employment spell. Because these firm identifiers sometimes change over time for administrative reasons, I implement a worker-flow algorithm to create a unique and consistent firm identifier for each firm throughout the sample period⁴. This allows me to track where workers are employed and to compute their mean monthly income at each firm in a given year. As explained below, I use this mean monthly income variable to identify a firm's manager. While the social security records allow me to identify the worker composition of each firm, they do not contain any information about firm characteristics.

I supplement my matched employer-employee panel with information on industry codes from the Swiss Earnings Structure Survey (ESS). I use the four waves of the ESS containing workers' social security identifiers (2012, 2014, 2016 and 2018). The ESS is a firm-level survey carried out every second year on a random sample of firms and public enterprises. Because large firms are systematically surveyed, every wave of the data covers about 2 million workers, i.e. more than a third of all workers in Switzerland. The goal of the ESS is to characterize the earnings distribution in Switzerland. As such, most variables in the

²In Switzerland, and contrary to other countries' regulations, employees and self-employed individuals have to pay social security contributions on all their income.

³This method sometimes mistakenly attributes zero income to individuals who are temporarily away from Switzerland. However, data from 2010–2019 show that the assumption of zero income is mostly correct, in particular for women, who often tend to have no income in a given year.

⁴In particular, I assign the same identifier to two firms in two consecutive years if either (i) they have the same firm identifier and share at least a common employee (ii) each firm employs 3 or more workers and has at least 70% of their workers who are employed by the other firm in the corresponding year.

data are worker-level variables such as weekly hours worked, bonuses or monthly earnings⁵. However, the dataset also contains information on the industry in which sampled firms are active. I use the 2-digit 2008 General Classification of Economic Activities (NOGA) code to classify firms into different industries.

I match firms between my matched employer-employee panel and the ESS through the workers' social security identifiers. Indeed, the ESS does not contain the same firm identifiers as the social security database. This procedure is fuzzy because workers can be employed by multiple firms in each dataset in a given year. In practice, for each firm in the social security data, I identify all workers who appear in one of the ESS waves in a year in which they are employed by the firm. I then record the NOGA industry codes from each of the corresponding firms matched in the ESS data. These codes may be different for each match. I then aggregate over all four ESS waves to compute the share of a firm's workers who are matched to each NOGA code. Finally, I assign a NOGA code to a firm in the social security data if more than 70% of their employees found in the ESS data are matched to firms in the ESS with this specific NOGA code.

2.2 Identifying Managerial Successions

Because my data sources do not contain information on workers' occupations, I develop a strategy to infer managerial successions based on the within-firm wage distribution. In short, I work with the assumption that a firm's manager is the employee with the highest mean monthly earnings at that firm in a given year. I then define managerial succession as a year-to-year change in the identity of the firm's top earner. If the former manager is the father of the new manager, the succession is identified as a father-child succession. Although I would ideally like to identify a firm's manager directly from occupational records, my approach has the advantage that it can be reproduced in most administrative datasets, even though these datasets typically do not contain occupational information or contain occupational information that is not updated regularly.

More precisely, I use the following definitions to identify managerial successions and specific father-child successions in the data.

Top earner: A firm j 's top earner in year t is firm j 's employee with the highest mean monthly income at firm j , averaged over years $t-1$, t and $t+1$. I smooth monthly income over a three-year bandwidth to make the identification of managers more robust to bonuses or other extraordinary yearly payments. My procedure will inevitably generate "false positives", i.e. identify changes in firm management when there are not any. By using a stricter definition of a top earner, I reduce the number of such false positives without missing out on firms that go through substantial managerial changes.

Succession: A (managerial or top-earner) succession occurs in year t when there is a change in the identity of the firm's top earner from year $t-1$ to year t and the firm's top earner was the same in years $t-3$, $t-2$, and $t-1$. I, therefore, only define a top-earner change as a managerial succession if the former top-earner remained identical for at least three years. I impose this restriction to identify only managerial successions that may represent a deviation from a previous steady state. I will use an event-study design to identify the relative effect of non-familial and father-child successions. As such, firms that are exposed to continuous managerial changes are ill-suited to my analysis.

Father-child succession: A managerial succession in year t is a father-child succession if the father of the top-earner at time t was the firm's top earner in any of the years $\{t-5, t-4, \dots, t-1\}$. Hence, father-child successions are a subset of all managerial successions. I do not require the father to transmit the firm directly to his child to allow for situations in which the familial succession was unplanned and occurred over a few years. This distinction does not matter much for my results, as most father-child successions happen between two consecutive years.

Figure 1 plots the annual share of firms with managerial and father-child successions among all firms with at least 5 employees in the data. The blue series tracks the share of managerial successions over time, to be read on the left y -axis. The share of firms going through any managerial succession remains relatively stable and is slightly above 12% throughout the sample period, with an uptick in 1996. According to my definition, about 1 in every 10 middle-sized to large firms in Switzerland goes through

⁵Sampled firms have to report worker characteristics for a subsample of their employees only. Hence, even if a firm is sampled in a given year, I would not be able to construct a measure of hourly earnings for all of their employees in that year.

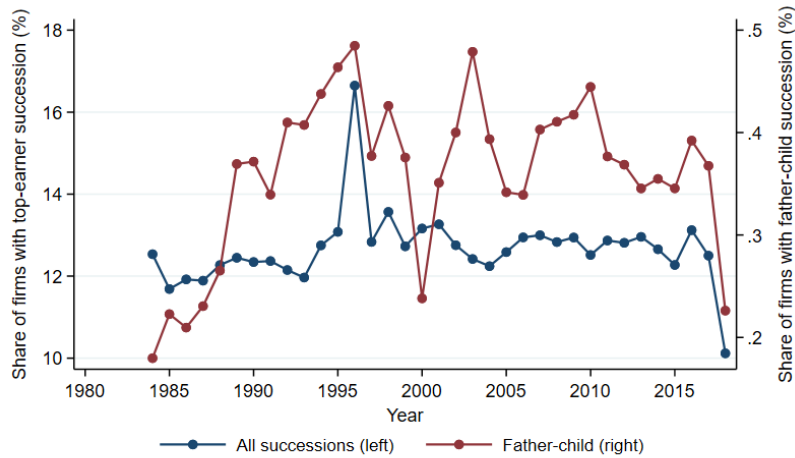


Figure 1: Share of firms with successions and direct father-child successions, by year

Notes: This figure plots the annual share of firms in year t with any managerial succession or a father-child succession among all firms with at least 5 employees in year $t - 1$ in the baseline data. The series start in 1984 and end in 2017 because 3 years of leading and lagging data are required to identify successions. The share of firms with any managerial succession is shown in blue. The corresponding values can be read on the left y -axis. The share of firms with a father-child succession, a subset of all managerial successions, is shown in red. The corresponding value can be read on the right y -axis.

a managerial succession in a given year. The red series shows the share of father-child successions, to be read on the right y -axis. The share of middle-sized to large firms going through a father-child succession is roughly 0.35% every year. There is a regular increase in the share of father-child successions until the early 1990s. This increase is likely artificial and driven by the data limitations. I might miss some father-child successions because workers cannot be matched to their fathers early on in the data⁶. Due to these limitations, my analysis sample will focus on successions taking place in 1991 or after. Over the sample frame, about 3 in every 100 managerial successions in the data are father-child successions. Although most successions do not occur directly between a father and a child, father-child successions are not uncommon and can be identified from the within-firm wage distribution.

2.3 Sample Construction

I now describe the steps conducted to construct my analysis sample and to allocate workers to the treatment or the control group. I first restrict the set of successions considered in my analysis. I consider only the successions occurring in the years 1991 to 2014. I ignore the years before 1990 because of the difficulty of matching workers to their fathers early in the data. I discard successions occurring after 2015 to have enough years of data to construct workers' outcomes up to 5 years after the succession. I impose that the firm's NOGA industry code can be inferred from the match with the ESS data. Finally, because the worker-flow algorithm used to construct the firm identifiers cannot identify small firms over time as accurately as larger ones, I restrict my attention to the 203,668 successions in which the firm has at least 5 employees in the year previous to the succession.

Because I am interested in the effect of a father-child managerial succession relative to a generic managerial succession on workers' labor market outcomes, I restrict the set of successions in the analysis to those in which the succession could have happened between a father and a child. First, I impose that the former top earner is a man aged between 56 and 70 at the time of the successions. Second, this former top earner must have a child, and his eldest child must be aged between 30 and 45 at the time of the succession. Third, I require that the top-earner succession is unique, in the sense that I do not

⁶Recall that individuals can be linked to their fathers if and only if they both reside in Switzerland in the years 2010-2020.

	(1)	(2)	(3)
	Treated	Matched controls	All treated and potential controls
Male	0.75	0.75	0.64
Age	40.6	40.2	41.4
Tenure (years)	3.74	3.75	3.76
Monthly income (CHF)	6,143	6,256	7,150
Log monthly income	8.65	8.66	8.75
Firm size (# employees)	97.3	84.9	12225
Age of prev. top earner	63.3	62.9	61.5
# workers	14,497	14,497	291,265
# successions	1,305	3,053	6,592

Table 1: Sample statistics

Notes: This table presents summary statistics for the main analysis sample of worker-succession spells. It reports the mean of each variable. The first column includes only the treated spells, i.e. workers who are exposed to a father-child succession. The second column refers to the control spells, i.e. workers who are exposed to a generic non-father-child managerial succession and were selected to be in the control group. The last column displays statistics for all treated and potential control spells, i.e. workers who are exposed to a managerial succession. All variables are computed the year preceding the succession.

observe another succession at the same firm between the same pair of top earners in the data. I include this requirement to identify successions that may represent a significant change in managerial practices, and because I am worried that firms with repeated successions between the same pairs of workers may be continuously led by one or both of them. After implementing all these restrictions, I am left with 6,592 restricted successions, of which about 20% are father-child successions.

Having identified the set of successions used in my analysis, I can now define my analysis sample of workers. I start by selecting the workers who were employed in year $t - 1$ at a firm j in which one of the analysis successions occurs in year t . I then assign the workers who were employed at time $t - 1$ at a firm with a father-child succession at time t to the treatment group. The remaining workers, who are exposed to a generic managerial succession, constitute the potential control group. I assign workers to either the treatment or the control group based on their lagged employment because employment at a firm at the time of the succession might be endogenous to the type of succession occurring. Due to endogeneity concerns, I exclude from my sample the workers who were themselves the firm's top earners at time $t - 1$ or t . Workers can enter the analysis sample multiple times if they are exposed to several successions. For consistency, I exclude from the final sample the workers who are assigned to both the treatment and the control group.⁷ However, workers may repeatedly appear in the control or in the treatment group. Hence, each observation in my analysis sample is not actually a worker, but more precisely a worker-succession observation.

I want to focus the analysis on workers with strong attachment to the labor force. I, therefore, impose the following restrictions, which emulate the typical restrictions used in the job-displacement literature to identify the effect of a mass layoff on workers' labor market outcomes (see e.g. Bertheau et al. (2023)). I only retain workers who are aged 30 to 55 at the time of the succession, whose monthly earnings are greater than CHF 1667 in each of the years $t - 3$, $t - 2$ and $t - 1$ leading to the succession⁸, and who are observed continuously in the data from year $t - 5$ to $t + 5$. After imposing these restrictions, I am left with 291,265 worker-succession spells, of which about 5% are in the treatment group.

Table 1 provides some summary statistics for the worker-succession spells in the analysis sample. Column 3 shows statistics for all worker-succession spells in the sample. The sample contains 64% male workers. In the year preceding a succession, the average worker has been employed for slightly less than 4 years at the firm and earns slightly more than CHF 7,000 per month. Successions tend to occur in large firms. The mean number of employees at the firms in which these workers are employed is above

⁷This procedure follows what is often done in the job-displacement literature (see e.g. Bertheau et al. (2023)).

⁸Workers can be employed at different firms in each year. They do not need to stay continuously employed in the firm in which the succession occurs.

10,000. The mean age of the previous manager is 61.5 the year before the succession, with a substantial share of the successions happening when the former manager is aged 60 to 65.

Column 1 of Table 1 displays statistics for the treated worker-succession spells, i.e. workers exposed to a father-child succession. The treated spells differ significantly from the average spells. The workers in treated spells earn less and work in smaller firms than workers in the potential control group. To account for these differences, and identify credible control spells, I will follow the job-displacement literature and implement a matched difference-in-difference design.

3 Empirical Design

I am interested in the effect of a father-child managerial succession relative to a generic managerial succession on a set of workers' labor market outcomes: continuity of employment at the firm, log monthly earnings, total annual earnings, and employment status. To estimate these effects, I first identify a comparison group among the set of potential control spells using nearest neighbor matching. I then run an event-study design to check for the common-trend assumption and show that the selected control spells appear to constitute a suitable control group for the treated worker-succession spells. Finally, I estimate a matched difference-in-difference specification to obtain a clean before-after comparison of the effect of father-child successions relative to generic managerial successions.

3.1 Selecting the Control Spells with Nearest Neighbor Matching

The labor market prospects of workers exposed to father-child successions may differ from those exposed to generic managerial successions due to these workers' observable and non-observable characteristics. While differences in average outcomes can be accounted for using worker fixed effects, the potential differential trends in labor market outcomes between the treated and the non-treated workers are more problematic and would bias the results of a difference-in-difference design. I follow the job displacement literature and use a matching strategy to identify a valid control group among the set of potential control worker-succession spells (Schmieder, Von Wachter, and Heining, 2023). For every worker in the treated group, I assign a control worker based on the following algorithm. I require that the control worker-succession spell perfectly matches the treated worker-succession spell on the year of the succession s , the firm's 2-digit industry code and the worker's gender. Thanks to the perfect matching of industry and succession year, workers in the treated and control groups should be roughly exposed to the same macroeconomic fluctuations. I then implement a nearest neighbor matching algorithm on age, firm tenure, log monthly earnings in year $s - 2$ and $s - 3$, firm size in year $s - 1$, and the age of the previous top earner at the firm. All variables are standardized to have a unit variance before running the nearest-neighbor algorithm. Because I am worried that the matches might be poor in less populated industry-code-by-gender-by-succession-year cells, I discard the 1% of the treated workers that are matched to the most distant control workers.

This strategy yields a group of workers exposed to generic successions comparable to the workers exposed to father-child successions. This is similar in spirit to the semi-parametric difference-in-difference estimator suggested by Abadie (2005). Because I create a specific control group in each year-by-gender-by-industry cell, and then stack these cells to create the estimation sample, my strategy is immune to the complications arising in difference-in-difference designs with events occurring in several periods identified by Goodman-Bacon (2021).

Column 2 in Table 1 shows summary statistics for the matched control workers. The mean variable values of the matched controls follow closely those of the treated workers, whose statistics are shown in column 1. This contrasts with the means of the entire pool of potential control workers, whose statistics are shown in column 3. If anything, matched control workers tend to work in slightly smaller firms than treated workers. Overall, and by design of the nearest-neighbor algorithm, matched controls are very similar to the treated workers on observable characteristics. Therefore, we might hope that their labor market outcomes constitute a valid counterfactual for the treated workers' outcomes after the father-child succession if that succession had been a generic non-paternal succession.

3.2 Event-Study Specification

I first assess the effect of father-child succession on workers' outcomes using an event-study specification. Let y_{its} be the outcome of interest for individual i at time t who is exposed to succession at time s . Define by D_i a dummy variable that takes value 1 if worker i is exposed to a father-child succession and 0 if worker i belongs to the control group of workers exposed to a non-paternal succession⁹. I estimate the following regression model

$$y_{its} = \sum_{k \in \{-5, -4, \dots, -3, -1, 0, \dots, 5\}} (\alpha_k + \beta_k D_i) I(t = s + k) + \pi_t + \psi_i + u_{its}. \quad (1)$$

The parameters of interest are the β_k coefficients, which measure the change in the outcomes of workers exposed to a father-child succession relative to the change in these outcomes among the workers exposed to a generic managerial succession with respect to the baseline year $s - 2$. I chose two years before the succession as the reference year to make the specification immune to contamination from possible anticipation effects of the succession in year $s - 1$. The specification controls for "year relative to the baseline" fixed effects (α_k). This is important because I impose that workers in the treated and the control groups have earnings above a certain threshold in the years leading to the succession, which tends to give a specific shape to their earnings profiles (Schmieder, Von Wachter, and Heining, 2023). In addition, I control for worker effects ψ_i and time effects π_t . Because of the matching procedure used to select the control workers, the outcomes of control workers are similar to those of treated workers. As such, the inclusion of worker fixed effects in equation (1) does not significantly affect the results. When estimating equation (1), I cluster the standard errors at the firm-succession level, which is the level of treatment assignment.

Estimation of equation (1) provides unbiased estimates of the dynamic treatment effects of father-child succession exposure relative to generic succession exposure if the control workers can generate valid counterfactuals for the treated workers had the treated workers been exposed to non-paternal succession. By construction, my method nets out any observed and unobserved factors that may lead to managerial succession in a firm. Hence, the remaining concerns about the validity of my empirical strategy should be factors influencing differentially workers exposed to paternal successions and workers exposed to non-paternal successions.

There are two potential threats to the parallel trend assumption required for the validity of the design. First, the decision to transmit the firm to a child may depend on the firm's performance. If the firm's prospects are generally poor, the manager might discourage his child from taking over the firm management. Because I match control workers exactly on the industry code and the year of the succession, systematic differences in the macroeconomic environment faced by the treated and the control workers are already accounted for. In addition, unless the change in firm performance is sudden, I should be able to observe these differential trends in performance in the pre-succession coefficients of equation (1). The absence of pre-trends would increase the trust in my estimates. Second, the exact timing of the succession might be endogenous to the firm's expected performance. Managers might wait for their firms to reach a stable situation before handing over the management to the next generation, while they might be less cautious in the case of an external succession. Here again, unless the change in firm performance is sudden, I should be able to observe these differential trends in performance in the pre-succession coefficients of equation (1). Nevertheless, this is a valid concern. An extension of this paper could look at successions whose exact timing is caused by exogenous events, such as managers reaching retirement age or suddenly dying.

Beyond the potential endogeneity of father-child successions, there is also a concern about a potential attenuation bias affecting my estimates. There are two reasons to think that the estimates coming from equation (1) are biased towards 0. First, my treatment variable – a change in the firm's top earner – is a proxy for the actual treatment – a change in managerial power. I attempt to reduce this bias by considering successions with established former top earners who have been the highest earners in the firm for at least 3 years, and by smoothing monthly earnings over a period of 3 years when identifying

⁹Recall that workers are assigned to the treated or the control group based on their employment at time $s - 1$. I look at incumbent workers' outcomes, irrespective of their firms and employment status after the succession.

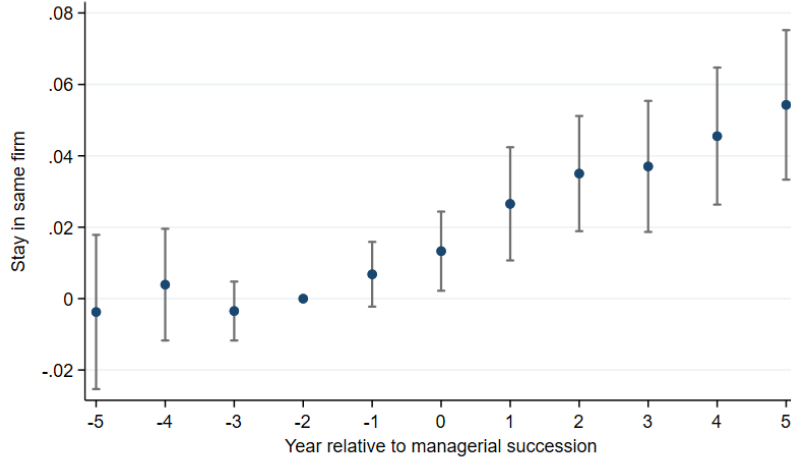


Figure 2: Event-study plot: dynamic effect of father-child succession on employment continuity
Notes: This figure shows the dynamic effect of being exposed to a father-child managerial succession instead of a non-paternal managerial succession on the probability that a worker is employed by the firm in which they were exposed to a succession. The blue series plots the coefficients of the event-study regression (1). The gray line shows 95% confidence intervals. The estimated effects are relative to the baseline year, two years before the firm succession occurs.

the top earners. Nevertheless, I cannot identify a change in managerial power with certainty. In many cases, the previous top earner remains on the firm’s payroll after the succession and might still exert some influence on managerial decisions. Of course, this informal influence may also be present in cases where there was truly a top-managerial succession in the firm. In any case, if I tend to infer a managerial change, even when there was not any, my estimates would be attenuated towards zero¹⁰. Second, and this is particularly relevant for the estimation of the event-study specification (1), I might not be able to identify the exact timing of the succession. It may well be that some managerial successions happen a year before or after what I infer from the data. If this were true, the estimates of the β_k coefficients in equation (1) might be attenuated towards zero in the years surrounding the imputed succession.

3.3 Matched Difference-in-Difference Design

In addition to the event-study specification, I estimate a matched difference-in-difference model. This design is a simpler version of the event-study specification in which I only compare the change in workers’ outcomes after a father-child succession to the corresponding change in workers’ outcomes after a non-paternal managerial succession. By pooling the workers’ outcomes over multiple periods before and after the succession, this strategy allows me to obtain more precise estimates of the effects of paternal succession. However, in contrast to the event-study design, the difference-in-difference design does not let me assess the presence of pre-trends in the data. For each outcome y of a worker i exposed to a succession in year s , I define the worker’s pre-succession outcome \bar{y}_{is}^0 as the mean of the worker’s outcome 3, 4 and 5 years before the succession:

$$\bar{y}_{is}^0 = \sum_{k=3}^5 y_{is-k} / 3. \quad (2)$$

¹⁰Even if the fuzziness with which I identify firm managerial successions affects father-child and non-paternal successions equally, my estimates might still be attenuated. If two-thirds of the identified managerial successions are true successions in either case, I am still left comparing a third of “treated” workers to “control” workers who went through a non-event, such that my estimates would be attenuated by a third compared to the true effects.

	(1)	(2)	(3)	(4)
	Employment continuity	Log earnings	Yearly earnings	Employment
After succ.	0.047 (0.010)	-0.011 (0.005)	-584 (625)	0.002 (0.005)
Observations	57,996	55,132	57,996	57,996

Table 2: Effect of father-child succession, difference-in-difference estimates

Notes: This table reports regressions results from the matched difference-in-difference specification from equation (1). Each column reports the difference-in-difference coefficient β for one of the outcome variables, along with the standard errors in parentheses and the number of worker-year observations. Standard errors are clustered at the succession level. Pre-succession outcomes are workers' mean outcomes in the years $s - 5$ to $s - 3$, while post-succession outcomes are workers' mean outcomes in the years $s + 3$ to $s + 5$. Employment continuity is defined as being employed at the firm in which a worker was exposed to the succession. Employment is a binary variable taking value 1 if and only if annual income is greater than CHF 10,000. Regressions are conducted on a balanced panel. Workers with missing pre- or post-succession earnings, along with the workers matched by the nearest neighbor algorithm, are excluded from the regression when the outcome is log monthly earnings. Income is in 2018 prices.

Symmetrically, I compute the post-succession outcomes \bar{y}_{is}^1 by averaging the worker-level outcomes 3, 4 and 5 years after the succession:

$$\bar{y}_{is}^1 = \sum_{k=3}^5 y_{is+ks} / 3. \quad (3)$$

I avoid using the years too close to the succession year because of the concern that I might not be able to identify the exact timing of the succession.¹¹

I then estimate the following difference-in-difference equation on my matched sample of treated and control workers

$$\bar{y}_{is}^k = (\alpha + \beta D_i) I(k = 1) + \psi_i + u_{iks}. \quad (4)$$

The β coefficient is the parameter of interest. It measures the before-after change in the outcomes of the workers exposed to a father-child succession relative to the change of outcomes of the workers exposed to a non-paternal succession. The model controls for changes in outcomes that affect all workers (α) and for worker effects ψ_i . Because of the algorithm used to match control workers, the inclusion of worker fixed effects does not significantly affect the results. When estimating equation (4), I cluster the standard errors at the firm-succession level, which is the level of treatment assignment.

4 Results and Discussion

4.1 Employment Continuity at the Firm

I begin by presenting the effect of father-child succession on the probability that a worker is employed at the same firm. I find strong evidence that workers are more likely to remain employed at firms undergoing a parent-child succession than at firms where a non-paternal succession occurs. Figure 5 plots the coefficients of the event-study specification (1) when the worker's outcome y_{its} is a dummy variable equal to 1 if worker i is employed at time t in the same firm as the one in which they are exposed to managerial succession at time s and zero if worker i is unemployed or works at a different firm. Coefficients are normalized relative to the reference year, two years before the succession. In this

¹¹This comes with the drawback that the assumptions required for the parallel trend assumption to be valid become stronger the farther we go away from the succession year.

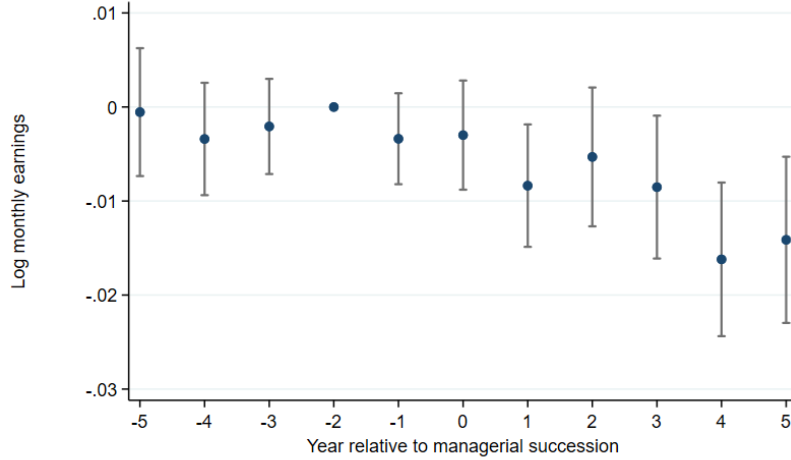


Figure 3: Event-study plot: effect of father-child succession on log monthly earnings

Notes: This figure shows the dynamic effect of being exposed to a father-child managerial succession instead of a non-paternal managerial succession on workers' log monthly earnings. The blue series plots the coefficients of the event-study regression (1). The gray line shows 95% confidence intervals. The estimated effects are relative to the baseline year, two years before the firm succession occurs. Income is in 2018 prices.

plot and all following plots using the same structure, the gray line shows the 95% confidence intervals for the coefficients. The point estimates before the reference year are close to 0. Not seeing any pre-trend in these coefficients is reassuring for the validity of the empirical design. After the succession occurs at $s = 0$, workers exposed to a father-child succession become more likely to work for the same firm than workers exposed to other managerial successions. The difference in probability of being employed at the same firm increases over time and reaches almost six percentage points 5 years after the succession. Column 1 in Table 2 shows the regression results of the difference-in-difference specification (4). Workers exposed to a father-child succession are about five percentage points more likely to stay in the same firms three to five years after the succession than workers exposed to non-paternal succession. This difference is precisely estimated. Recall that control workers are matched on age and tenure such that pre-existing differences in labor market trajectories or tendencies to move out of the labor force cannot explain this difference.

Which workers are likelier to stay within a firm if management is transmitted within the family? Are low-income workers less likely to be pushed out of the firm? Is the effect even negative for high-earning workers who are barred from accessing the highest managerial positions by nepotism? To look into possible heterogeneous effects, I first assign workers in each firm to five different groups based on their position in the firm's wage distribution one year before managerial succession. I then assign each pair of treated and control workers to any of five different estimation samples based on the within-firm income quantile of the treated worker¹² Finally, I estimate equation (4) again separately on each of the five estimating samples constructed.

The first row of Table 3 shows the estimation coefficients for each estimating sample, from the workers in the lowest quintile of the firm's income distribution to those in the highest quintile. Column (1) reports the coefficients and standard errors for the workers from the first quintile of the within-firm income distribution. Column (2) reports them for the workers from the second quintile, and so on¹³. Workers

¹²Hence, the within-firm relative position of matched workers does not matter for the assignment of pairs to the different samples.

¹³The number of observations is unbalanced across columns because the assignment of workers to different quintiles of the firm's income distribution occurs before the application of the selection criteria for the analysis sample. A substantial share of the workers from the lowest income quintile does not meet the minimum earnings requirement in years $s - 3$, $s - 2$, or $s - 1$, where s is the succession year. On the other hand, many of the workers in the top income quintile are top earners

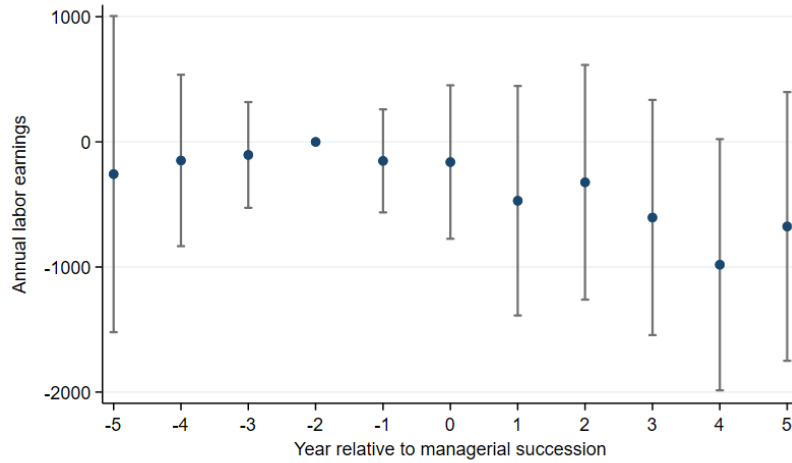


Figure 4: Event-study plot: effect of father-child succession on yearly earnings

Notes: This figure shows the dynamic effect of being exposed to a father-child managerial succession instead of a non-paternal managerial succession on workers' total annual earnings. The blue series plots the coefficients of the event-study regression (1). The gray line shows 95% confidence intervals. The coefficients are relative to the year of reference, two years before the firm succession occurs. Income is in 2018 prices.

from the lower half of the within-firm income distribution are most affected by the type of managerial succession. By contrast, I do not find any significant effects for the sample of workers from the top income quintile. Therefore, the empirical evidence is consistent with family managers being generally less likely to generate employee turnover, potentially because they have personal ties to the firm's workforce or are reluctant to initiate substantial managerial changes. Employees from the lower end of the within-firm income distribution are the most affected, perhaps because some higher-earning employees decide to leave the firm anyway to avoid being blocked from climbing the job ladder within the firm due to nepotism.

4.2 Earnings and Employment

I now turn to discussing the effects of father-child managerial succession on workers' income and employment. In general, the evidence for effects on earnings and employment is more mixed. I estimate a small negative effect of father-child succession on log monthly income and no significant effect on total income and employment. Figure 4 plots the coefficients of the event-study specification (1) when the outcome is workers' log monthly earnings. This regression is estimated on a balanced panel. Hence, individuals who do not have any earnings in any of the 11-year bandwidths are excluded from the regression. I also exclude their corresponding matches in the control or the treatment group. There is no significant pre-trend. After the managerial succession occurs, a small difference between the workers exposed to a father-child succession and the control group starts to emerge. Three to five years after the succession, workers exposed to a father-child succession earn about 1% less than workers in the control group. This effect is summarized in the regression coefficient in column 2 from Table 2. While this reduction of 1% of the income of the employees who stay in the labor force is significant at the 5%-level, the size of the effect remains moderate.

Although I estimate a small negative effect of father-child succession on log monthly earnings, I do not detect any significant effect on workers' total yearly earnings. Figure 4 shows the coefficients of the event-study specification with workers' total yearly earnings as the dependent variable. For this outcome as well, there is no significant pre-trend. Annual earnings of the workers exposed to a father-child succession seem to decrease slightly compared to the earnings of the workers in the control group, but the difference is not statistically significant at the 5% level. When pooling observations from the pre-succession and

themselves and therefore excluded from the analysis sample.

	(1)	(2)	(3)	(4)	(5)
Worker's within-firm income quintile	Q1	Q2	Q3	Q4	Q5
Employment continuity	0.085 (0.022)	0.090 (0.019)	0.052 (0.019)	0.050 (0.018)	0.009 (0.025)
Log earnings	-0.016 (0.013)	-0.016 (0.010)	-0.029 (0.008)	-0.025 (0.008)	0.026 (0.011)
Yearly earnings	-1,673 (1,012)	-2,201 (997)	-5,174 (1,024)	-1,837 (1,109)	11,700 (2,690)
Employment	0.012 (0.015)	0.027 (0.012)	-0.014 (0.011)	0.004 (0.010)	0.018 (0.017)
Observations	9,276	11,144	13,492	14,128	9,956

Table 3: Effect of father-child succession, by worker's income quintile

Notes: This table reports regressions results from the matched difference-in-difference specification from equation (1) separately for workers in each income quintile of their firm's wage distribution. Workers exposed to a father-child succession are first assigned to one of five samples based on their income rank within the firm, from the lowest quintile Q1 to the highest quintile Q5. I then estimate the matched difference-in-difference equation for each sample and outcome. Column 1 reports the difference-in-difference coefficients β for each outcome when estimated on the sample of workers in the lowest quintile, along with the standard errors in parentheses and the number of worker-year observations. Each subsequent column reports results for the next quintile. Standard errors are clustered at the succession level. See Table 2 for a definition of the pre- and post-periods and of the outcome variables.

the post-succession periods, I estimate a mean difference in total yearly income of 584 (Column 3, Table 2). Although this difference is far from being economically meaningful, it is not statistically significant. Therefore, there is only weak evidence that being exposed to a father-child succession reduces workers' mean total earnings. This result contrasts with findings from the corporate finance literature that the profitability of firms whose ownership is transmitted within the family is significantly lower than that of firms with non-familial successions (Bennedsen et al., 2007; Pérez-González, 2006). Either family-run firms in my sample are not significantly less profitable or family-run firms are willing to share a larger share of the surplus generated by their activities with their employees.

Estimates from the pooled sample of workers suggest a moderate and negative effect of father-child succession on workers' earnings, although only marginally statistically significant at best. Are workers from different parts of the firm's hierarchy affected differently? The second and third rows of Table 3 show the results from the estimation of equation (4) separately for the workers of each quintile of their firm's income distribution. Workers from the upper-middle part of the income distribution (third and fourth quintiles) suffer the largest relative decline in earnings. Their earnings are about 2.5% smaller than those of the workers in the control group. By contrast, workers from the top of the income distribution actually benefit from father-child successions. They earn more than their counterparts who are exposed to non-paternal successions.

I do not find any effect of the type of managerial succession on employment. I define being employed as having total yearly earnings above CHF 10,000. Figure 5 shows the coefficients of the event-study specification with employment as outcome. The type of succession does not seem to have any effect on the employment probability. Most point estimates are close to zero and none of them are statistically significant. When pooling observations from the pre-succession and the post-succession periods, I estimate a precise zero (Column 4, Table 2). When estimating the effect separately by a worker's within-firm income quintile, most of the estimates are insignificant (Bottom row, Table 3). If anything, workers from the bottom of the income distribution seem to be slightly more likely to be employed after a father-child succession. I conclude that there is no evidence of a strong effect of father-child successions on workers'

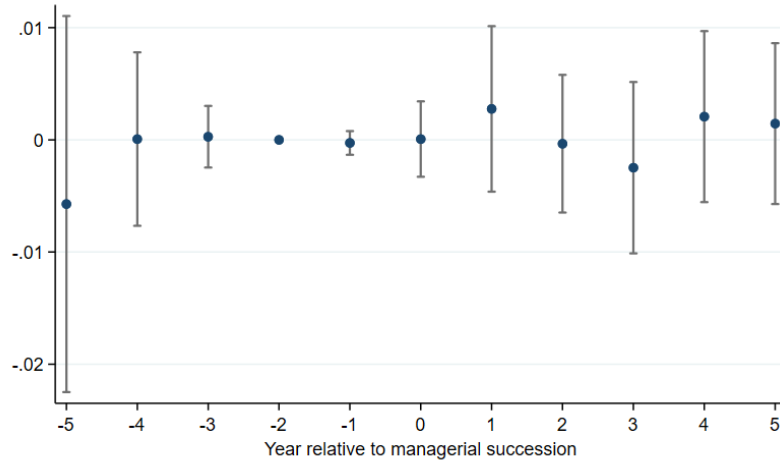


Figure 5: Event-study plot: effect of father-child succession on employment

Notes: This figure shows the dynamic effect of being exposed to a father-child managerial succession instead of a generic managerial succession on the probability that a worker is employed. Employment is defined as having yearly labor earnings over CHF 10,000 in 2018 prices. The blue series plots the coefficients of the event-study regression (1). The gray line shows 95% confidence intervals for the corresponding estimates. The coefficients are relative to the year of reference, which is two years before the firm succession takes place.

employment probability.

To sum up, while workers exposed to father-child successions are 5 p.p. more likely to remain employed at the firm, they endure a small relative decline in income and no change in their overall probability of employment. Workers from the lowest part of the firm income distribution are the most affected in terms of their probability of being continuously employed at the firm, with an estimated increase of up to 9 p.p. of their probability to stay at the firm. The estimated effects on earnings are small and borderline significant. They are slightly more negative for the workers in the middle of the firm’s hierarchy. These effects are consistent with family-run firms being generally less inclined to replace their workforce, perhaps because the managers have built personal connections with the employees. Why do workers stay at these firms if their earnings decrease relatively to workers exposed to non-paternal successions? One hypothesis is that non-financial amenities are higher in family-run firms. A simpler explanation, however, is that workers may need time to adjust because of labor market frictions as with any other labor market shock (Yagan, 2019; Jarosch, 2023).

5 Conclusion

In this article, I study the effects of intergenerational transmission of firm management on the labor market outcomes of incumbent workers. Using almost 40 years of comprehensive matched employer-employee data from Switzerland, I identify year-to-year changes in the identity of the highest-paid workers at middle-sized and large firms. Because recurrent information on workers’ occupations is unavailable, I use a succession at the top of the within-firm wage distribution as a proxy for a change in the identity of a firm’s CEO. In the baseline sample of all middle-sized to large firms, about 3% of all CEO successions are father-child successions. I then use a matched difference-in-difference design to identify the effects of being exposed to a father-child CEO succession relative to a generic CEO succession on incumbent workers’ labor market outcomes.

My analysis yields three main findings. First, I find that workers are 5 percentage points more likely to be continuously employed at a firm 5 years after the CEO succession in the case of a father-child succession. This relative decrease in the probability of leaving the firm is strongest for workers at the

bottom of the firm's income distribution. Second, I estimate a slight reduction in earnings for the workers exposed to father-child CEO successions. This differential effect on earnings is only marginally significant and amounts to a loss of about 1% of workers' baseline earnings. Workers from the upper-middle part of the pre-succession income distribution are the most significantly impacted by this reduction in earnings. Third, I estimate a precise null effect of father-child CEO succession on the overall employment probability of workers. This last finding comes with the caveat that my estimates might suffer from attenuation bias, as I only infer CEO successions from the firm's wage distribution.

My empirical findings can be put in perspective with the established result that firm profitability is negatively impacted by the intergenerational transmission of firm management (Pérez-González, 2006; Bennedsen et al., 2007). Overall, I only find mild evidence for the existence of adversarial effects of father-child CEO succession on workers' outcomes. The estimated increase in the probability of continuous employment at the firm might even be seen as a positive development for incumbent workers. One possible interpretation of the results is that family CEOs are more reluctant to lay off workers when facing negative economic shocks. As a result, firm profitability goes down. Workers might then decide to remain employed at family firms, even if their relative salaries decrease slightly, because they enjoy higher non-financial amenities in these firms. Further research on the topic would optimally have access simultaneously to firms' balance sheets and workers' labor market trajectories to provide additional evidence on the mechanisms through which the intergenerational transmission of firm management affects workers.

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