Eclipse of Rent-Sharing: The Effects of Managers' Business Education on Wages and the Labor Share in the US and Denmark*

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Abstract

We provide evidence from the US and Denmark that CEOs with a business degree ("business managers") reduce wages and the labor share (relative to non-business managers). Within five years of the appointment of a business manager, wages decline by 6% and the labor share by 5 percentage points in the US, and by 3% and 3 percentage points in Denmark. Firms appointing business managers are not on differential trends and do not enjoy higher output, investment, or employment growth thereafter. Using manager retirements and deaths and an IV strategy based on the composition of the board of directors at the time of new CEO appointments, we present additional evidence that these are causal effects. Exploiting exogenous export demand shocks, we establish that a key mechanism for these wage effects is changes in rent-sharing practices following the appointment of business managers and provide evidence suggesting that these effects are driven by business managers prioritizing shareholder value.

Keywords: business education, labor share, management, rent-sharing, shareholder value, wages.
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1 Introduction

The labor share of national income has fallen in several industrialized nations over the last three decades (Karabarbounis and Neiman, 2014). In the US, the labor share in the private nonagricultural business sector was about 65% in the 1980s but now stands at below 60%. Concurrently, the annual growth rate of median (real) wages, which was typically above 2% between the 1950s and the 1970s, has been only 0.3% since 1980, despite considerable productivity growth. The bargaining power of labor has not receded as much in Nordic countries, which have a tradition of strong trade unions and various pro-labor institutions (Freeman, 2007). Nevertheless, the Danish corporate sector labor share has also fallen from 69% in 1999 to 65% in 2014 (Hémous and Olsen, 2020). Many factors have been proposed as potential drivers of these pervasive changes in the nature of labor markets, including capital accumulation (Blanchard, 1997; Piketty and Zucman, 2014), automation (Acemoglu and Restrepo, 2019; Acemoglu et al., 2020), the rise of superstar firms (Autor et al., 2020), growing concentration and market power (Gutiérrez and Philippon, 2017; De Loecker et al., 2020), and the declining power of unions (DiNardo et al., 1996; Farber et al., 2021).

In this paper, we argue that changing managerial attitudes and practices towards rent-sharing—sharing of a firm's profits with workers—have been a major contributing factor to the decline in the labor share and the slowdown in wage growth. We provide evidence from the US and Denmark suggesting that chief executive officers (CEOs) with business-school degrees, who have been managing a growing share of firms over time, have reduced wage growth and the share of labor.

We present two main findings. The *first* is our headline result: in both the US and Denmark, when a CEO with a business degree ("business manager" for short) takes over from a non-business manager, there is a significant decline in worker earnings and the labor share of the firm (relative to non-business manager firms). With our event-study design, we find a 5 percentage point decline in the labor share and a 6% decline in worker earnings in the five years following the appointment of a business manager in US publicly-listed ("public") firms. We find similar, though slightly smaller, results in Denmark focusing on both public and private firms—a 3 percentage point decline in the labor share and a 3% decline in wages. In neither country do we see any differential trends in the labor share, wages, employment, output, or investment before the term of the business manager begins. Nor do we detect an employment, output, investment, or productivity response, which suggests that business managers are not more productive than their non-business peers. We additionally show that the negative wage impacts apply throughout the wage distribution, though the effects are somewhat larger for lower-wage workers.

Our estimates suggest that the effects of business managers are sizable but not implausibly large. In the US, where the fraction of workers employed by business managers has increased from 27% to 45%

between 1981 and 2019, our estimates indicate that the growth in the share of business managers can explain about 16% of the decline in the labor share and accounts for approximately 15% of the slowdown of wage growth since 1980. In Denmark, where the increase in the fraction of workers employed by business managers is smaller (rising from 7% in 1995 to 14% in 2011), our mechanism accounts for 6% of the decline in the labor share. Predictably, the reduction in worker earnings following the appointment of a business manager raises profits: the return on assets (ROA) increases by 1.6 percentage points in the US and by 1.5 percentage points in Denmark. In the US, we also find an increase of about 5% in the stock market returns of companies that appoint business managers. We further show that all else equal, managers with business degrees earn more than non-business managers.

An obvious concern with our estimates is the endogeneity of the decision to appoint a business manager—perhaps firms turn to business managers when they are having problems or need to cut labor costs. To bolster our interpretation, we first show that the results are very similar when we focus on CEO retirements and deaths in Denmark and CEO retirements in the US, which are arguably less endogenous than other switches from non-business to business managers. Second, we develop several instrumental-variable (IV) strategies. In the US, we exploit the composition of a company's board of directors at the time of the appointment of a new CEO, due to the previous CEO retiring. We show that when there are more directors with an MBA, the appointment of a business manager becomes much more likely. We also document that pre-retirement, there are no differential trends by board composition (likely because most boards are not involved in the day-to-day running of companies). Exploiting this source of variation, we estimate similar effects of business managers on worker earnings and the labor share to our event-study results.[1]

We additionally document a clear pattern of "diffusion" of the appointment of business managers within peer groups of firms (defined by region-industry-size class cells) in both the US and Denmark. Exploiting this diffusion, we also estimate similar, but this time somewhat larger, effects of the appointment of business managers.

In both countries, we additionally find that worker separations increase, especially for higher-wage workers. This finding suggests that, although business managers are not reducing overall employment, some of the more valuable employees leave after their reign starts.

Our *second* main finding identifies *rent-sharing* practices as an important mechanism for our results. We focus on plausibly-exogenous variation in profits driven by firm-specific export demand shocks, building on Hummels et al. (2014). This strategy isolates changes in a firm's export demand driven by differential changes in the imports of six-digit products by their main exporting destinations and

[1]Another issue is whether these results could be due to "selection" of who attends business schools. The working paper version of this paper presented evidence suggesting that selection cannot explain our results, and we explore this issue more systematically in a companion paper.

enables us to explore how business and non-business managers respond to these plausibly-exogenous export shocks.

This strategy is useful for two distinct reasons. For one, it provides a distinct test of differences between the wage policies of business and non-business managers: while the event-study methodology focuses on changes in a company's CEO over time, this approach zeroes in on the response of firms presided by business and non-business managers to similar shocks. More importantly, it is informative about the mechanisms via which different wage and labor share outcomes emerge across firms run by business and non-business managers.

In Denmark, we find no major differences in terms of productivity, sales, employment, or investment responses to export demand shocks—thus no compelling evidence that business managers are more productive or adaptable in this context either. However, confirming our main results on different wage policies of business and non-business managers, we find that they respond very differently in terms of wages and the labor share. Non-business managers share the rents generated by export shocks with their workers: a 10% increase in value added per worker is associated with a 1.7% increase in wages. Alternately, a 10% increase in profit per worker leads to a 1.1% increase in wages. In contrast, business managers do not share these export-driven rents with their employees, and for the firms they run, we estimate a precise zero impact on wages following such an increase in exports. Further explorations reveal that this effect itself is almost completely driven by positive export shocks: following an exogenous decline in exports, business and non-business managers behave similarly, presumably because cutting nominal wages is difficult even for business managers who prioritize cost reductions. However, following a positive export shock, non-business managers share the resulting rents and raise wages, whereas business managers do not. In the US, exports are less important for our sample of firms, and we find similar but somewhat less-precisely estimated results.

Stepping back, we interpret these findings as capturing the impact of management practices and values imparted by business schools and business degrees. Two ideas commonly propagated in business schools may significantly affect the priorities and approaches adopted by managers with business degrees. The first is the emphasis on shareholder values, as advocated in 1970 by Milton Friedman, who stated that "The social responsibility of business is to increase profits." Following Friedman, other economists and business school scholars argued that managers were not sufficiently devoted to maximizing shareholder value (e.g., Jensen and Meckling, 1976; Jensen and Murphy, 1990). These ideas became popular and were taught in business schools (Bennis and O'Toole, 2005; Ghoshal, 2005; Marens, 2014), and classic textbooks in corporate finance, such as Brealey and Myers (1980) and Copeland and Weston (1979), espoused that the goal of managers should be to maximize shareholder value. Under their influence, (some) managers may have come to view workers not as stakeholders in the corporation

but rather as sources of costs to be reduced (Freeman and Reed, 1983; Rappaport, 1999; Freeman, 2010). The second idea is the emergence of a business school doctrine advocating reengineering and creating lean corporations (Hammer and Champy, 1993; Davenport, 1993; Womack and Jones, 2003). Although the emphasis was not on wage cuts per se, identifying and removing "unnecessary" costs started being viewed as an integral part of successful management (Marens, 2011; Holt, 2020; Hassard and Morris, 2021). The dual emphasis on shareholder value and corporate leanness may have made managers unwilling to share rents with their workers. Using text data from firms' annual 10-K filings, we show that business managers indeed put more emphasis on shareholder value and company leanness than their non-business predecessors.

To the extent that business schools were the vanguard of these ideas, which then became more widely held among managers, our estimates should be viewed as lower bounds on the effects of these management practices. If so, in both the US and Denmark management practices prioritizing shareholder value and cost cutting may have contributed even more to the decline in the labor share and the slowdown in wages than what our estimates indicate.

Our paper is related to several literatures. First, we directly contribute to a growing body of work on the decline of the labor share (and thus the slower growth of wages than productivity) in industrialized nations, including the papers cited in the first paragraph. Second, our work is connected to the literature on business ethics concerning what business schools teach and how it has changed over time, and several of the relevant papers in this literature were just cited as well.

We are also closely linked to the literature on rent-sharing in corporations. A few papers present evidence of rent-sharing, documenting, for example, that workers in industries with greater profits, rents, or capital investments receive higher wages (e.g., Blanchflower et al., 1996; Van Reenen, 1996; Hildreth and Oswald, 1997). The recent paper by Kline et al. (2019), for instance, exploits exogenous changes in profitability resulting from patent grants and shows significant wage gains for workers, though emphasizing that these gains are quite unequally distributed. Caldwell and Harmon (2019) provide evidence from Denmark on the importance of workers' outside options in rent sharing. A few recent papers emphasize the (growing) importance of firm effects in wages and wage inequality, which is also partly related to differences in rent-sharing practices (e.g., Card et al., 2013, 2016; Song et al., 2019; Garin and Silvério, 2019). To the best of our knowledge, this literature has not investigated the role of managers, and particularly their education and values, in rent-sharing.

We also build on the literature on management styles. The idea that managers and their approaches matter for corporations is commonplace in the management literature (e.g., Hambrick and Mason, 1984) and has been emphasized in theoretical discussions (e.g., Jensen and Meckling, 1979; Hermalin and Weisbach, 1998). Bertrand and Schoar's (2003) pioneering study, and several papers before and since

theirs, have documented the role of managerial characteristics in various corporate decisions (e.g., Chevalier and Ellison, 1999; Malmendier and Tate, 2005, 2008; Kaplan et al., 2012; Graham et al., 2013; Custódio and Metzger, 2014). Several papers have looked at CEOs' MBA education and find mixed evidence on the effect of business managers on firm performance (Bhagat et al., 2010; Miller and Xu, 2016, 2019). With the exception of He and le Maire (2020), who document significant manager effects in Danish wages, this literature does not focus on worker outcomes and, to the best of our knowledge, has not investigated the role of business school education. Nor are we aware of studies in this literature that attempt to isolate exogenous variation, which is a key aspect of our empirical strategy. As in Bertrand and Schoar (2003), we find that business managers lead to better accounting performance, but our analysis suggests that this positive effect is largely driven by lower labor costs.

Lastly, we build on and contribute to the literature on the effects of economics and business educations on ethical behavior (e.g., Frank et al., 1993, 1996; Frey and Meier, 2003; Bauman and Rose, 2011). Unlike much of this literature, which focuses on lab experiments and surveys, we investigate the implications of the appointment of a manager with a business degree on some of the most important corporate outcomes, and we detect no effects on output or productivity, but find a major impact on rent-sharing.[2]

The rest of the paper is organized as follows. Section 2 describes our various data sources. Section 3 outlines our empirical strategy. In Section 4, we analyze the effects of business managers on wages and the labor share using event studies and IV strategies. Section 5 exploits exogenous variation due to export demand shocks to explore the role of rent-sharing in our wage and labor share results. Section 6 concludes. The Online Appendix contains additional results.

2 Data and Context

In this section, we present the various data sets we use and provide details on sample construction. We also discuss the relevant institutional context in Denmark and the US.

2.1 US Managers with Business Degrees

We obtain biographical information for CEOs of publicly-listed US companies, including the name of school and degree (but not major) for all post-secondary education, from the BoardEx database of Management Diagnostics Ltd. We classify CEOs with any degree from a business school as "business managers",[3] and then match them to Compustat firms between 1980 and 2020. Our sample contains

[2]Wang and Murnighan (2011) and Wang et al. (2011) argue that business school curricula have adverse social consequences because they emphasize economic incentives. In contrast to this argument, we do not find similar effects from Danish managers with only economics degrees.

[3]We classify schools with "Business School", "School of Business", "College of Business", or "School of Management" in the school name (with a few exceptions such as Wharton and INSEAD) as business schools. Business degrees include bachelors, masters and executive programs of business schools.

around 9,900 US publicly-listed firms with complete information on CEOs, and throughout we have a single CEO/manager per firm in each year.[4]

Figure 1 plots the evolution of the share of Compustat firms run by CEOs with business degrees. In 1981, only 27% of the Compustat firms had CEOs with business degrees. By 2019, this figure had grown to 45%. Almost all of the increase comes from the share of CEOs with MBAs, which rose from 23% in 1981 to 39% in 2019. In our sample, Harvard Business School contributes 19% of the business degrees of CEOs, followed by Wharton (8%) and Stanford (5%).

In US publicly-listed firms, CEOs are appointed by the board of directors and are responsible for making major corporate decisions. While boards are expected to oversee and monitor managers on behalf of shareholders, CEOs have substantial freedom in operations, and their vision and styles are important determinants of firm policies (Bertrand and Schoar, 2003).

2.2 US Data on Firm and Worker Outcomes

We use firm- and worker-level data from the US Census Bureau to estimate the impact of business managers on firm outcomes and worker earnings. We match the publicly-listed firms in Compustat to the Longitudinal Business Database (LBD), which covers all non-farm establishments with paid employees in the US from 1987 to 2018. This dataset provides information on plant-level owner (firm), geographic location (state and county), industry (six-digit NAICS), employment, and payroll. We aggregate this information to the firm level. Our sample excludes firms that change their IDs, such as target firms of mergers and acquisitions or buyouts.

We merge the firm-level data with individual worker-level information, including employment, earnings, gender, race, and age, from the Longitudinal Employer Household Dynamics (LEHD) data. The LEHD data are constructed using administrative records from the state unemployment insurance (UI) system and the associated ES-202 program. Earnings include salary and wage earnings as well as bonuses, stock options, profit distributions, the cash value of meals and lodging, tips, and other gratuities in most states, and, in some states, employer contributions to certain deferred compensation plans such as 401(k) plans. We have access to LEHD worker-level data from 22 states and the District of Columbia, which covers about half of the US population.[5] The LEHD earnings data are currently available from the 1980s through 2014 (the start date varies across states and ranges from 1985 to 2002). We merge the LBD firm-level data with the Business Register (SSEL) data on annual firm revenue. When discussing our results at a high level or summarizing results that apply both to the

[4]The coverage of BoardEx expanded over time, and after 2000, the majority of publicly-listed firms can be found in BoardEx. As a result, our sample includes 2,500 firms in 1995, 5,300 firms in 2005, and 5,200 firms in 2015.

[5]The 22 states are: Arizona, Arkansas, California, Colorado, Delaware, Illinois, Indiana, Iowa, Kansas, Maryland, Missouri, Montana, Nebraska, Nevada, New Jersey, New Mexico, North Dakota, Ohio, Oklahoma, Pennsylvania, Tennessee, and Virginia.

US and Denmark, we sometimes refer to "wages", and it should always be borne in mind that our US data are for worker earnings—not hourly wages.

2.3 Danish Matched Employer-Employee Data

For Danish firms and workers, we use data from several administrative registers at Statistics Denmark. Our firm data come from the Firm Statistics Register (FirmStat), which covers the universe of private-sector Danish firms from 1995 to 2011. FirmStat assigns each firm a unique identifier and provides annual data on firm activities, including the number of full-time employees, value added, and industry affiliation. We additionally use other firm registers to obtain financial data from balance sheets and income statements.[6] For all of our analyses, we exclude firms with 5 employees or less. As in the US, we exclude target firms of mergers and acquisitions.

The worker data are extracted from the Integrated Database for Labor Market Research (IDA), which covers the entire Danish population between the ages of 15 and 74, including the unemployed and those who do not participate in the labor force. Each person has a unique identifier, and the IDA database provides annual data on many of the individual's socioeconomic characteristics, such as annual worker earnings, education, and occupation. We measure the hourly wage rate as annual labor earnings plus mandatory pension fund payments divided by annual hours. Each employed worker is matched to an establishment, which is a unique physical work location such as an office, store, or factory. To match firms to workers, we draw on the Firm-Integrated Database for Labor Market Research, or FIDA, which links every firm in FirmStat to its workers in IDA who are employed by that firm in the last week of November.

Denmark has a high union membership rate (70%–75%), and more than 80% of workers are covered by a collective agreement. Although wage bargaining has been historically centralized, it was decentralized during the period we study. In the beginning of our sample in 1995, less than 10% of workers are covered by the standard rate system (where wages are set by the industry collective agreement). The wages of the rest of the workers are mostly negotiated at the firm or individual level, with a wage floor set by the industry collective agreement, which is binding only for the least experienced workers (Dahl et al., 2013). In many cases, the bargaining at the firm or establishment level occurs between managers and shop stewards, and agreements cover wage increases but not employment levels (Ilsøe, 2012). In general, managers play an important role in wage bargaining and shaping firms' wage policy (He and le Maire, 2020).

To identify CEOs/top managers, we use a combination of occupation codes (ISCO) and job hierarchy (PSTILL). A firm's manager is defined as an employee with occupation code 1210 ("Directors

[6]The survey from which we draw the accounting data has a rolling panel structure with firms selected based on their employment as of November in the previous year. Firms with 50 or more employees are always sampled, and information on sales and employment is available for all firms.

and Chief Executives") and the highest job hierarchy category.[7] If a person is the manager at firm A in year t and year $t+2$, and there is no manager at firm A at year $t+1$, we assume the same person is the manager in year $t+1$. Over 85% of worker-year observations in our sample period have at least one manager in the firm. For firms with multiple employees that meet these criteria in a given year, we define the (top) manager as the one with the highest earnings. Consequently, as in the US data, we have one manager per firm-year. We use managers' education histories to classify them into business and non-business managers.[8]

Figure 2 plots the evolution of the fraction of managers in Denmark with business degrees. Panel (a) shows that the share of firms run by managers with business degrees has doubled from 1995 to 2011. Panel (b) shows that, similar to the US, this increase is mostly driven by an increase in business MA degrees. The top three Danish business schools—Copenhagen Business School, Aarhus University School of Business and Social Sciences, and University of Southern Denmark Business School—account for over 50% of all business degrees and over 70% of managers with business degrees.

3 Empirical Strategy

The key estimating equation in this paper can be written as

$$y_{it} = \gamma B_{it} + X'_{it}\beta_t + \lambda_i + \delta_t + \varepsilon_{it}, \tag{1}$$

where B_{it} is an indicator variable for whether the manager at firm i in year t has a business degree. In addition, X_{it} denotes a vector of covariates, λ_i summarizes the firm fixed effects, δ_t corresponds to time effects, and ε_{it} is an error term. Our coefficient of interest, γ, is the effect of business managers on firm and worker outcomes. In our event-study estimates, we allow the effects to vary by event time. In additional specifications, we also allow γ to vary by worker, firm, and industry characteristics.

We use a number of different strategies to estimate equation (1). Our first and most central strategy is a series of event studies focusing on firms that transition from being run by non-business managers to being run by business managers. These event studies enable us to confirm that firms switching to business managers are not on differential trends before the events and provide a transparent way of estimating and displaying our results. We follow Sun and Abraham (2021) and use an interaction-weighted estimator to compute the event-study estimates. This estimator ensures consistency in the

[7] If a firm has no worker with both the occupation code 1210 and the highest job hierarchy category, managers are defined as the workers with occupation code 1210. If there is no worker with the occupation code 1210, managers are defined as those with other managerial occupation codes (1221-1339) and the highest job hierarchy code. Finally, if a firm has no worker with managerial occupation codes, managers are defined as those with the highest job hierarchy code. Over 80% of our managers are identified from observations that have data for both managerial occupation codes and the highest job hierarchy category.

[8] Specifically, a manager is classified as having a business degree if he or she has business major at any level of education, including short education, professional BA, BA, MA, and PhD. About 75% of managers with business MAs also have a business BA or a business professional BA.

presence of two-way fixed effects and avoids issues of spurious identification and negative weights on some observations (de Chaisemartin and D'Haultfœuille, 2020).

Our main events are transitions from non-business managers to business managers. We restrict treated firms to those that have never hired a business manager before the event and hired a business manager for the first time during the sample period. The control group consists of firms that have non-business managers throughout our sample period. The identifying assumption is that, absent the event, worker earnings and other outcomes would have followed parallel trends in treated and control firms. In all specifications, we control for industry × year fixed effects, firm size quintile × year fixed effects, and region(state) × year fixed effects. Standard errors are clustered at the firm level.

In the worker-level regressions, we adopt a computationally-feasible matching procedure, as the number of workers is very large. Following Smith et al. (2019), we match each treated firm (switching from a non-business manager to a business manager) to a control firm that is in the same industry, state or region, and employment quintile, has never had a business manager before the event, and has the closest propensity score of hiring a business manager in the next year. The effect of business managers on worker earnings is estimated from the differential evolution of earnings of *staying* workers between treated firms and matched control firms.[9]

Although we confirm that there are no pre-trends among firms hiring business managers, there are two additional endogeneity concerns. First, there may be other organizational, economic, or financial changes implemented at the same time as new business managers are appointed, confounding the impact of business managers with the effects of these other changes. Second, there may be time-varying omitted factors changing at the same time as the CEO transition (and potentially causing the replacement of the previous CEO). We deal with the first problem by verifying that there are no other major changes at the time of managerial transitions.

To confront the second problem, we adopt three complementary strategies: (1) We confirm that the results are similar when we focus on the subsample in which previous non-business managers die or "retire" (where we define retirement as separations by CEOs over the age of 62).

(2) We develop an instrumental variable (IV) strategy based on board composition around manager retirements in the US. We document that the share of external board directors with business degrees strongly predicts the hiring of a business manager after the retirement of the previous CEO. We capture this relationship with the following first-stage equation:

$$BM_i \times Post_{it} = BoardShare_i \times Post_{it} + \alpha_i + \mu_t + \epsilon_{it}, \tag{2}$$

[9]In the US LEHD data, we only consider workers with positive earnings in all four quarters, and we define a *stayer* in year t as a worker who had positive earnings at the firm during the fourth quarter in year $t-1$ and the first quarter in year $t+1$. In Denmark, we define a *stayer* in year t as a worker who had positive earnings at the firm in both year $t-1$ and year $t+1$.

where BM_i is an indicator variable for the incoming manager having a business degree, $Post_{it}$ is a dummy for years after retirement, and $BoardShare_i$ is the share of external directors with a business degree (lagged one or five years). We control for firm fixed effects α_i and year fixed effects μ_t. We only include firms that have not previously had a business manager and where the current non-business manager retires. About a third of these firms hire a business manager as replacement. We use equation (2) as the first stage to estimate our second-stage equation

$$y_{it} = \beta BM_i \times Post_{it} + \alpha_i + \mu_t + \eta_{it}. \tag{3}$$

The exclusion restriction in this IV strategy is that the share of external board directors with business degrees only affects changes in firm outcomes around retirement through the hiring of business managers, and we later provide evidence that supports this exclusion restriction.[10]

(3) We also look at how wages respond to demand shocks in firms operated by business and non-business managers. This strategy provides a very different confirmation of the effects of business managers and is additionally informative about the mechanism via which these wage differences emerge, highlighting differences in rent-sharing between business and non-business managers. Specifically, we follow Hummels et al. (2014) and develop a source of exogenous variation in firm sales and profits driven by changes in firm-specific export demand. We use differences in exporting destination by six-digit product for each firm and exploit the fact that the demand for exports from these firms is changing differentially across these destinations (proxied by their total non-Danish or non-US imports of the focal six-digit product). The reasoning for this source of variation is that the relationship between an exporter and its customers is specific and is typically built over time. As a result, a change in demand for a product in, say, Germany will disproportionately impact firms exporting that product to the German market, and we proxy for German demand using variation in the overall German imports for the product in question (except from our focal country, either Denmark or the US). In this strategy, the predicted firm-specific exports are defined as

$$WID_{jt} = \sum_{c,k} s^e_{jck} WID_{ckt}, \tag{4}$$

where s^e_{jct} is the pre-sample share of exports to country c and six-digit product k of firm j, and WID_{ckt} ("world import demand") is country c's total purchases of product k from other countries at time t. Imports from Denmark or the US are left out in order to avoid any mechanical correlation between WID_{ckt} and the exports of the firm in question. In some specifications, we additionally control for product-level variation in overall exports (namely, $\sum_k s^e_{jk} WID_{kt}$, where s^e_{jk} is firm j total exports of six-digit product k) to focus solely on the comparison of firms exporting similar products to different

[10] We also use a second IV strategy, based on the diffusion of the practice of hiring business managers in an industry-region size quartile cell, and we describe this strategy briefly later.

destinations. We estimate the effects of export-driven rents on firms by regressing firm outcomes on the level of predicted firm-specific exports, WID_{jt}, with firm fixed effects. In worker-level wage regressions, we also include worker $\times$ firm fixed effects to focus on wage changes of staying workers. This is analogous to including the change in WID_{jt} on the right-hand side of regressions for change in (log) wages. With a slight abuse of terminology, we will refer to WID_{jt} as "export shock". To explore the asymmetric rent-sharing implications of positive and negative export shocks, we also create two additional variables: $WID^{+}_{jt} = WID^{+}_{jt-1} + \max\{WID_{jt} - WID_{jt-1}, 0\}$ and $WID^{-}_{jt} = WID^{-}_{jt-1} + \min\{WID_{jt} - WID_{jt-1}, 0\}$ (both initialized at 0 at the beginning of the sample). We then estimate their separate effects (in levels, this is analogous to including $\Delta WID^{+}_{jt} = \max\{WID_{jt} - WID_{jt-1}, 0\}$ and $\Delta WID^{-}_{jt} = \min\{WID_{jt} - WID_{jt-1}, 0\}$ in regressions for wage changes).

4 The Effects of Business Managers on Worker Earnings and the Labor Share

In this section, we provide evidence on the effects of business managers on worker earnings and the labor share in the US and Denmark.

4.1 Event-Study Evidence from the US

We start with event studies of firms hiring a business manager for the first time. The sample consists of firms that switch from a non-business manager to a business manager and firms that are always run by non-business managers. The summary statistics for the US sample are presented in Appendix Table A1. There are 1300 firms that hired business managers for the first time during our sample period.

Figure 3 presents our main event-study figures for publicly-listed US firms, using specifications that control for industry $\times$ year fixed effects, firm size quintile $\times$ year fixed effects, and state $\times$ year fixed effects. The first panel shows a negative impact of a switch from a non-business manager to a business manager on worker earnings. These negative effects are significant, and five years after the switch, earnings per worker have declined by 6.2% (standard error = 2.5%). Staying workers' earnings also decline by 5.7% (standard error = 2.0%) five years after the switch, suggesting that these earnings results are not driven by compositional changes.[11]

The second panel depicts a similarly large and negative impact on the labor share, although some of the event-study point estimates are not statistically significant. Since we do not observe value added in the US Census data, the labor share is defined as wage bill from the LBD divided by sales. Five years after the switch to a business manager, the share of labor in *sales* is 1.7 percentage points lower (which is a 7% decline relative to the average sales-based labor share of 26%). Or, if we convert this

[11] Appendix Figure A1 plots the nominal earnings in treated and control firms and shows that treated firms have slower wage growth but on average do not have nominal wage cuts. We return to the issue of nominal wages in the next section.

to the standard labor share in *value added*, it is equivalent to a 5 percentage point decline. Notably, before the switch, there are no differential pre-trends in either worker earnings or the labor share.

Some of the individual event-study estimates in Figure 3 are imprecise and insignificant because we allow the impact of business managers to be unrestricted over time. As an alternative strategy, we impose a constant coefficient for all post-treatment periods. These static treatment effects of transitioning from non-business manager to business are -0.0365 (standard error = 0.0141) for earnings per worker and -0.0129 (standard error = 0.0059) for the labor share in our baseline specification, as depicted in the figure.[12]

The bottom two panels demonstrate that the switch from a non-business to a business manager is not associated with an increase in revenue, employment, or investment. Some of the estimates are less precise than our results for worker earnings, but in all cases we find no evidence of either differential trends before or any divergence after the switch to a business manager. Since employment and output responses are quite similar, there are also no significant changes in labor productivity. This may be either because business managers implement no meaningful improvements, or because any positive effects they have are canceled by possible negative consequences of their wage policies (such as less cooperation with workers or loss of some high-skilled workers, which we document later).

Appendix Figures A2 and A3 look at other firm outcomes around these manager transitions. We see no change in the probability of acquiring another firm or R&D expenses. There is no significant change in terms of costs of goods sold or selling, general and administrative (SG&A) expenses, suggesting that business managers do not cut non-labor costs. We also find no impact of business managers on the probability of firm exit, market share, or the entry and exit rates of plants.

These results are robust across a range of specifications. In the Appendix, we confirm that they are unchanged when we include firm age by year fixed effects (Appendix Figure A4) and when we consider a balanced panel of firms (Appendix Figure A5).

4.2 Event-Study Evidence from Denmark

The summary statistics for the Danish sample are presented in Appendix Table A2. In Denmark, 2366 firms hired business managers for the first time during our sample period.

Figure 4 presents analogous results for Denmark. Panel (a) depicts precise and sizable effects on worker earnings. In Denmark, we additionally have data on workers' hourly wages, which behave very similarly to annual earnings, as shown in Panel (b). Five years after the switch to a business manager, the annual earnings of staying workers are 3.0% lower (standard error = 0.6%). Likewise, hourly wages of staying workers are lower by 4.0% (standard error = 1.1%). Panel (c) shows that the labor share

[12]The interpretation of these quantitative magnitudes is slightly different, since they summarize the average effect after a switch to a business manager, whereas the numbers we report from our event studies are for the impact after five years, and the figures show that the effects of business managers on worker earnings and the labor share grow over time.

declines as well, though the estimates are less precise in this case. Our point estimate after five years indicates a 3.4 percentage point impact on the labor share (equivalent to a 4% decline starting from a base of 76%).[13]

We find a similar impact on (annual) earnings per worker, which decrease by 2.6% five years after the transition. This again suggests that our wage effects are not explained by changes in worker composition. Appendix Figure A7 additionally shows that there is no significant change in average worker quality (measured by AKM worker fixed effects) around these events.

Panels (d) to (f) show no evidence that business managers improve productivity or other firm outcomes in Denmark as well—we see no differential trends in value added, sales, employment, or investment. In addition, in all cases, there are no differential pre-trends before the switch to a business manager.

Appendix Figures A8 and A9 look at other firm outcomes. As in the US, we see no change in the probability of firm exit, R&D expenses, material costs, or rental costs, and thus there is no evidence that business managers are cutting non-labor costs. Nor are there any significant changes in market share or entry and exit rates of plants. In Danish data, we can also look at outsourcing or robot purchase, and we do not find an impact of business managers on these quantities either.

In the Appendix, we show that we obtain similar results when including firm age by year fixed effects (Appendix Figure A10) and when we consider a balanced panel of firms (Appendix Figure A11). The results are also robust to varying the fixed effects we control for (Table A3).

4.3 Quantitative Magnitudes

To explore the implied quantitative magnitudes, we take the point estimates for earnings and the labor share effects five years after the switch to a business manager and, in line with the evidence in the previous subsection, we assume that these are permanent. We then compute the aggregate consequences of these impacts by measuring the increase in the fraction of employees in our sample operating under business managers.

This calculation implies that in the US the shift towards business managers is responsible for a 1 percentage point decline in the labor share (in value added) since 1980. We compute this number using the fact that the share of business managers increased by 18% from 1981 to 2019 (Figure 1) and thus its contribution to the decline of the labor share is $65\% \times 7\% \times 18\% = 0.82$ percentage

[13]The effects on wages and the labor share are smaller in Denmark than in the US. To examine whether this difference is driven by the smaller firm size in the Danish sample compared to publicly-listed US firms, we reweight the Danish sample to mimic our US sample in terms of size. In particular, for each year, we calculate 20 equal-sized bins based on the employment distribution of publicly-listed US firms, and apply bin-level weights equal to the fraction of Danish firms in a bin divided by the fraction of publicly-listed US firms in that bin. Appendix Figure A6 shows that the reweighting produces larger effects on wages and the labor share (3.8% decline in earnings per worker and 3.9 percentage point decline in the labor share after five years), but the effects are still smaller than those found in our US sample.

points. Equivalently, the increase in the prevalence of business managers explains about 16% of the total decline in the labor share between 1980 and 2017. Likewise, the same switch explains 15% of the slowdown in wage growth in the US: average real wage growth declined from 2% growth per annum before 1980 to 0.3% growth per annum after 1980, and our estimates imply that without the increase in the fraction of business managers, it would have declined only to 0.6% per annum.

The numbers for Denmark, where the fraction of the workforce working under business managers has grown by less, are smaller, but still meaningful. The increase in the fraction of business managers is estimated to have led to a 0.24 percentage point decline in the labor share from 1995 to 2011, accounting for 6% of the overall 4 percentage point decline in labor share over this period.

In the US, since we only observe managers' education for publicly-listed firms, these numbers assume that there is a similar increase in the share of business CEOs and a similar impact of business CEOs on wages and the labor share among private firms. If private firms appoint fewer business CEOs or business CEOs have a smaller impact for these firms, our estimates would overstate the aggregate impact of business managers in the US. On the other hand, these numbers may understate the overall impact, since business managers' attitudes and practices may have also become more common among non-business managers during this period, and business managers themselves may have started using these methods more intensively, as we discuss later.

4.4 Confounding Factors, Threats, and Additional Results

There are several concerns related to endogeneity and confounding factors, which we begin exploring in this subsection. We find no evidence that our results are significantly confounded by any one of the long list of variables we have access to. Nor do we find any evidence suggesting that our results are impacted by major omitted variable biases.

First, firms appointing business managers may have distinct characteristics, which then influence their subsequent evolution. Notice that constant effects of these differences are already absorbed by firm fixed effects in our estimates, and our event-study graphs show that these differences are not a source of differential trends. Nevertheless, it is useful to understand them and explore their implications more closely.

In Appendix Tables A4 and A5, we show that firms appointing business managers are larger and older than firms that never hire a business manager. They also tend to be in more highly-concentrated industries. Nevertheless, they do not differ in terms of industry growth or Chinese import shocks. Conditional on industry, region, and size, firms hiring business managers have higher worker earnings but not a higher labor share in both US and Denmark. In the US, firms hiring business managers also have higher SG&A expenses. In Denmark, firms hiring business managers have higher revenue per worker. However, in both countries, they are similar in terms of R&D, investment, and input

costs. Appendix Tables A6 and A7 confirm these differences by estimating the propensity to appoint a business manager based on firm characteristics. They show that firms that are larger and have higher worker earnings are more likely to hire business managers in the US. On the other hand, in Denmark it is only larger firms that are more likely to appoint business managers, and worker earnings do not predict hiring of business managers conditional on firm size.

In the Appendix, we further verify that none of these characteristics meaningfully affect our results. In Figures A12 and A13, we include only treated firms in the sample and use firms switching to business managers in the last year of the sample as the control group of firms. In Figures A14 and A15, we match each treated firm to a control firm with similar characteristics based on the estimated propensity scores. These estimates are somewhat less precise but the point estimates are similar to the baseline event-study estimates. Finally, we control for earnings per worker quintile $\times$ year fixed effects in Figures A16 and A17, which lead to very similar estimates, bolstering our assumption that, absent the treatment, the treated and control firms are on parallel trends.

Second, one may be concerned that there are other organizational shifts taking place at the same time as the appointment of a business manager and that these may be partially responsible for the patterns we document. Alternately, if there are such shifts, they may also be the mechanisms via which the effects of appointing a business manager are realized. In any case, we detect no major concurrent changes in acquisitions, R&D expenditures, non-labor costs, outsourcing, or robot purchases, as noted above. We also do not find any changes in firms' occupational or organizational structure associated with the accession of a business manager to the CEO position. Appendix Figure A18 plots the share of workers in each hierarchy level, as well as the average occupational wage and the share of workers in low-wage occupations around non-business to business manager transitions in Denmark; there are no significant changes in any of these outcomes.

A related issue is that our results may be driven by the mere fact that the identity of the CEO changes or they may be caused by other differences between business and non-business CEOs. We do not find any evidence supporting these concerns. Figure 5 confirms that a switch from one non-business manager to another non-business manager or from one business manager to another business manager has no similar effects (see Panels (a) and (b) of the figure for the US and Panels (c) and (d) for Denmark). This figure also shows that a switch from a manager without a college degree to one with a college degree has no similar effects (Panel (e)),[14] and nor does a switch from a manager without a master's degree to a manager with a master's degree (Panel (f)).[15]

[14]If anything, in some specifications there is a small positive (albeit insignificant) impact. We cannot do this exercise in the US, since almost all CEOs in our US sample have a college degree.

[15]In Appendix Figure A19, we also show that, in Denmark, economics degrees are not associated with similar negative effects on earnings and the labor share, so our estimated earnings and labor share effects appear to be driven by management practices or wage policies of managers who have received business education, and not by selection into or education in economics-related fields more broadly. We are not able to perform the same exercise in the US, since we

A few additional robustness checks are reassuring in the context of these concerns as well. Appendix Figure A21 shows that the results are similar for switches from younger non-business managers to older business managers and for switches from older non-business managers to younger business managers. Appendix Figure A22 documents that our results are robust to excluding firms with family CEOs in Denmark (we define "family CEOs" as managers who are related by blood or marriage to the managers who precede or succeed them at their firms). We also find similar effects for internally-promoted business managers and externally-hired business managers in Appendix Figure A23. A small fraction (less than 10%) of firms with non-business to business manager transitions are targeted by activists during the five years before the manager transitions in the US, but our results are robust to excluding those firms (not reported). Appendix Figure A24 documents that the much less common switches from a business manager to a non-business manager lead to an increase in the labor share, but do not raise worker earnings in the US, while Appendix Figure A25 shows that these switches have no impact on worker earnings or the labor share in Denmark. The more muted effects of reversals are likely due to the fact that firms reverting back to non-business managers appear to face deteriorating business opportunities (as can be seen in Panel (c) of Figure A24). It is also possible that some of the effects of business managers on management-labor relations are permanent and are not reversed even after a non-business manager takes the reins later.[16] Non-business CEOs who replace business managers may also have more similar priorities to business-managers than other non-business managers.

Additionally, in Appendix Figure A26, we look at whether the effects are heterogeneous across worker types. In the US, we estimate negative impacts on workers in all four earnings quartiles, with somewhat larger effects on lower-earning workers. In Denmark, the effects are also larger for lower-wage workers.

In Appendix Figure A27, we look at differences across types of business degrees using our Danish data. We find negative and significant effects on wages and the labor share for both managers with business and professional BAs and those with business MAs. In additional (unreported) results, we find slightly larger, but not significantly different, effects for larger firms (which are much more likely to employ business managers). Lastly, Appendix Figure A28 shows that union workers experience larger declines in earnings than non-union workers, suggesting that the impact of business managers concentrates on unionized workers. Figure A29 depicts a small and statistically insignificant negative effect on unionization rates.

Third, these checks notwithstanding, the appointment of a business manager may coincide with

do not observe college major. Appendix Figure A20 shows that managers with engineering degrees also have different effects than business managers in Denmark.

[16] After a firm switches from a business CEO to a non-business CEO, the firm may still have several managers with business degrees in important positions. For example, in Denmark, 20.7% of managers, excluding the CEO, have business degrees after business to non-business CEO transitions, compared to 9.9% for firms with non-business CEOs.

other changes in the firm's costs, profit opportunities, or objectives, leading to a classic omitted variable bias. We deal with this class of threats to the validity of our estimates in the next subsections, first by focusing on switches to business managers driven by the retirement or death of the previous CEO, and then discussing results from two IV strategies.

4.5 Evidence from Manager Retirements and Deaths

In the Danish data, we can determine whether the switch to a business manager is due to the retirement or death of a previous non-business manager, while in the US we can only focus on CEO retirements. Specifically, we define retirement as the cessation of employment of a previous manager who is 62 or older.

Figure 6 is analogous to Figure 4, but focuses only on retirements and deaths.[17] The results are very similar, both qualitatively and quantitatively. After five years, a switch to a business manager following the retirement of a non-business manager is associated with a 3% decline in worker earnings and a 3.8 percentage point decline in the labor share.

Figure 7 looks at manager retirements in the US. The results are noisier but quantitatively similar to the baseline event-study estimates that use all non-business to business manager transitions: the hiring of a business manager following retirement is estimated to be a 5.2% decline in worker earnings and a 2.8 percentage point decline in the labor share of sales after five years.

One aspect of endogeneity this strategy does not fully deal with is that, conditional on retirement, firms still choose whether to appoint a business manager. Our instrumental-variable (IV) strategy, presented next, zeroes in on a plausibly-excludable source of variation for whether these firms appoint a new CEO with a business degree after the previous CEO retires.

4.6 Instrumental-Variables Estimates

Our main IV strategy, described in Section 3, focuses on the composition of the board of directors at the time of the retirement of an existing (non-business) CEO. This exercise focuses on US data, since in Denmark we do not have information on the board of directors to be able to implement this strategy. The first-stage relationship in the US data is documented in column 1 of Table 1 and has an F-statistic over 100 in all specifications. This confirms that the (lagged) share of external board directors with business degrees strongly predicts whether a firm hires a business manager to replace the retiring manager.

Columns 2 to 5 of Table 1 present the IV estimates of the effect of business managers on workers' earnings, labor share, revenue, employment, and investment exploiting this source of variation.

[17] There are 14 transitions from non-business managers to business managers due to manager deaths and 276 transitions due to manager retirements in Denmark during our sample period.

Consistent with the OLS results, business managers reduce worker earnings and the labor share, without significant impacts on revenue, employment, or investment.

The main concern with this IV strategy is that companies that have a larger fraction of board directors with business degrees may have different wage policies, for example, because board members with business degrees put greater emphasis on cost-cutting. Table 2 explores this issue and documents that board composition does not predict differential evolution of worker earnings, the labor share, or revenue before a CEO retirement event. This is plausible, since boards are not involved in the day-to-day running of companies, but do play a critical role in the appointment of new CEOs.

We find similar results using alternative measures of board composition. In Panel B of Table 1 and Table 2, we use the share of external directors with business degrees five years before retirement, which yields very similar results. In Appendix Table A8, we use the share of all directors including executive directors. This specification has an even stronger first stage and produces very similar results. We also obtain similar estimates when we control for firm size quintile fixed effects in Appendix Tables A9 and A10 to account for potential correlation between board composition and firm size.

Overall, the IV strategy exploiting the composition of the board zeroes in on a very different source of variation than our event-study estimates, but generates very similar results, bolstering our confidence in the reliability of our baseline findings.[18]

Finally, in Appendix A.1, we report results from an alternative IV strategy both for the US and Denmark. We document that there is "diffusion" of the appointment of business managers within peer groups of firms (defined by region-industry-size class cells). Appendix Figure A30 shows that after a first business manager is appointed within a cell, its peers become significantly more likely to appoint business managers as well. We use this as the first stage of this second IV strategy and additionally control for differential trends by industry, region, and size class to ensure that this strategy exploits within-group variation. Appendix Tables A11 to A13 show that this IV strategy produces similar, albeit somewhat larger, effects from the appointment of business managers and that these estimates are quite robust to different strategies for controlling for differential trends across cells.[19]

[18] An alternative interpretation is that some companies' boards appoint business managers because they intend to reduce wage growth. However, this interpretation is difficult to reconcile with the fact that the same board composition has no predictive power before the retirement of the previous CEO. Moreover, even if this interpretation is correct, it is closely related to ours—it presumes that business managers are better at reducing wages.

[19] Our interpretation for this difference in magnitude is that practices associated with business managers diffuse more broadly within our cells, so that firms without business managers might also become more likely to adopt the same managerial practices and those with already-existing business managers might use them more intensively. Consequently, our second IV strategy may be estimating more systemic effects of practices associated with business education. This reasoning also suggests that the quantitative effects implied by our event-studies and main IV strategies may be lower bounds.

4.7 Who Benefits?

The results presented so far indicate that business managers reduce worker earnings and the labor share, and these effects are felt by workers in different parts of the wage distribution. We now explore who benefits from the appointment of business managers. We start by looking at changes in firm profitability, measured by return on assets (ROA). Since business managers do not change the growth or productivity of firms, lower earnings for workers should imply higher profits. Panel (a) and Panel (d) of Figure 8 confirm that following a switch to business manager, ROA increases by about 1.5 percentage points in both the US and Denmark.

The higher profits are shared with shareholders in the form of dividends and stock buybacks. Panel (b) shows that payout to shareholders (including stock buybacks) increases by 1.6% of assets 5 years after the switch, whereas there is no significant change in the amount of cash held by the firm in the US. Panel (e) documents that in Denmark dividends increase by about 1.5% of assets 3 years after the switch (although it decreases thereafter), and there is no change in cash holdings. Higher profits also translate into higher stock market prices in the US.[20] Panel (c) of Figure 8 shows an increase in stock returns in the two years following the appointment of a business manager. Hence, one clear group of beneficiaries from the practices brought about by business managers are shareholders.[21]

Table 3 looks at whether business managers earn more than non-business managers in the US and Denmark, respectively. In the US, we obtain CEOs' total compensation, including salary, bonus, stocks and stock options, and incentive payouts, from the Compustat Execucomp data. The results point to a statistically significant, but quantitatively modest, premium for business managers, who earn 2.5% more in the US and 9.7% more in Denmark than their non-business-school peers, after controlling for manager characteristics, firm characteristics, and firm and year fixed effects. In Appendix Table A14, we look at the composition of compensation for business CEOs in the US. Business CEOs receive a slightly higher percentage of their earnings from stock options and a lower percentage of their earnings from salary and bonus. Appendix Table A15 demonstrates that business managers also get a larger part of their compensation from stock options in Denmark, but the difference is quantitatively small (about 0.1%) due to the small number of publicly-listed firms in Denmark.[22]

While shareholders and CEOs with a business degree themselves benefit, this does not mean that there are no costs for the firm. Figure 9 explores the effects of business managers on worker separations. Consistent with the notion that workers are unhappy with policies and practices that reduce their wages

[20] The same analysis in Denmark would leave us with a tiny sample, consisting of fewer than 200 firms, and hence for this exercise, we perform this exercise only in the US.

[21] This result is consistent with recent work by Greenwald et al. (2019), who estimate that 44% of the increase in the US stock market value between 1989 and 2017 was due to reallocation from workers to shareholders, and with Stansbury and Summers (2020), who document an association between declining worker power, lower wages and higher profits.

[22] We also find a small increase in the stock option payments received by non-manager workers, shown in Appendix Figure A31.

and the labor share, we find that worker separations increase following a switch to a business manager in both the US and Denmark. For example, our estimate for the US indicates a 6.2 percentage points rise in worker separations once a business manager takes over. The results additionally show that in both US and Denmark, separation effects are slightly larger for higher-wage workers, despite the fact that they suffer smaller (relative) earnings reductions than low-wage workers. This differential effect presumably reflects these workers' access to better outside options. Firms compensate for part of the loss of high-skilled workers by hiring new employees. Figures A32 and A33 in the Appendix show that business managers increase hiring and the newly-hired workers have higher earnings and education levels compared to existing workers, although they have similar earnings and education levels compared to workers who leave the firm. Appendix Figure A34 confirms that the average worker age does not change around non-business to business manager transitions.

In summary, our findings indicate that the appointment of a business manager generates significant benefits for shareholders and CEOs with business degrees themselves also earn more, while workers experience slower wage growth and there is also evidence suggesting that employees, and especially higher-wage workers, are more likely to leave the firm.

4.8 Interpretation

Our interpretation of these results is based on the idea that business school education encourages (or provides good justifications for) practices that favor shareholders at the expense of workers. This is consistent with the emphasis in business schools on shareholder value (following Milton Friedman, 1970) and lean corporations and reengineering (Hammer and Champy, 1993; Womack and Jones, 2003), as discussed in the Introduction. While we are not able to fully resolve whether this is the main mechanism at work, three pieces of evidence suggests that it is an important channel. First, we document in the next section that business managers reduce the sharing of "rents", or profits, with workers. Second, Appendix Figure A35 shows that our baseline event-study effects are larger for Danish managers who received their business degrees after 1980: earnings per worker decline by 3.6% and the labor share declines by 5.5 percentage points within five years after transitions to business managers in the post-1980 cohorts, compared to a 1.1% decline in earnings per worker and a 0.8 percentage point decline in the labor share for business managers in the pre-1980 cohorts. The results for the US are similar (but are not reported due to disclosure restrictions). These findings are consistent with the role of business schools' emphasis on shareholder value, which became more widespread after 1980.

Third, and most importantly, in this subsection, we show that following the appointment of a business-educated CEO, the company's purported mission shifts further towards shareholder value and cost-cutting.

To measure corporate values, we use text analysis from the Management Discussion and Analysis

(MD&A) section of firms' annual Form 10-K filings in the US. (We do not have a sufficient number of publicly-listed Danish firms to perform this exercise in Denmark). The MD&A section provides a narrative explanation from the perspective of management about the company's financial condition, operations, and cash flows. It complements the quantitative information found in the financial statements with qualitative insights about the company's operations and strategic direction. Hassan et al. (2019) use the MD&A section of 10-K filings to measure firms' political risks.

We construct our measures using a machine learning approach that builds on Li et al. (2021). We focus on three corporate cultural values: *shareholder value*, *lean thinking*, and *employee*. Practically, we use the word embedding model (*word2vec*) to learn the meanings of all words and phrases in MD&A section of 10-Ks based on their respective contexts. We then construct a "value dictionary" of words and phrases that appear in close association with each corporate value. For example, the method automatically identifies words and phrases, such as *profitability*, *shareholder wealth*, and *earnings growth target*, as part of the value dictionary associated with *shareholder value*.[23] Finally, we calculate the *shareholder value* score of a firm based on a weighted-frequency count of the words and phrases in the firm's MD&A section that are in our shareholder value dictionary. The methodology is detailed in Appendix A.2. For interpretability, we standardize all scores to have mean zero and unit standard deviation.

Figure 10 presents event-study estimates of the impact of business managers on corporate values. In Panel (a), we calculate the score using only seed words of *shareholder value*, and in Panel (b), we use all words and phrases in the value dictionary associated with *shareholder value*. Both measures show an increase in emphasis on shareholder value after a switch from a non-business manager to a business manager. Perhaps predictably given significant measurement error in our classification of corporate values, the results are more precise when we use a broader set of words and phrases associated with shareholder value. In both panels, there is no pre-trend, and we see a stark increase in mentions of shareholder values in the MD&A section within a year of the appointment of a business manager, and these mentions remain at a higher level afterwards.

Panel (c) shows that business managers also put more emphasis on lean thinking and cost-cutting. In contrast, Panel (d) shows a small, negative and insignificant effect of business managers on values associated with the *employee* category.

Overall, the results in this subsection suggest that the emphasis on shareholder value maximization and cost reduction in business schools could be an important channel for the lower worker earnings

[23] We start with seed words that define each corporate value. For *shareholder value*, we use seed words *shareholder value*, *stockholder value*, *share owner value*, *stock owner value*, *stock returns*, and *shareholder returns*. For *lean thinking*, the seed words are of those with the highest frequency from Womack and Jones (2003) and Hammer and Champy (2006), such as *lean production*, *cost reduction*, and *reengineered processes*. For *employee*, the seed words are *stakeholder*, *people*, *team*, *talent*, *partner*, *employee*, *human capital*, *empower*, *teamwork*, *personnel*, and *qualified*.

and higher shareholder returns that follow the appointment of a CEO with a business degree.

5 Business Managers and Rent-Sharing

In this section, we study the responses of firms run by business and non-business managers to changes in export demand. This analysis is useful, first, as a different validation of our main results: rather than event-studies, now we look at how firms presided over by business and non-business managers change worker pay in response to similar shifts in (export) demand. Second and more importantly, this analysis provides direct evidence that the negative impact of business managers on wages and the labor share is driven by changes in rent-sharing practices.

5.1 Main Results

We start with Danish results, which are more precisely-estimated. We present the summary statistics for this sample in Appendix Table A16.[24] Exporters in our sample are larger than the average firm. Nevertheless, Appendix Figure A36 shows that non-business to business manager transitions are still associated with a decline in wages and the labor share, and there is no evidence that such a transition has a positive effect on output, employment, or investment in this sample.

Our main estimates follow the methodology outlined in Section 3. Table 4 shows that positive export (demand) shocks lead to higher hourly wages and higher annual worker earnings when a non-business manager is in charge, but not when the company is run by a business manager. Relatedly, positive export shocks under business managers are associated with declines in the labor share. The differences in the effects of export shocks on hourly wages and labor share between firms headed by business managers and firms headed by non-business managers are significant at the 5% level. These findings confirm our headline results that there is less wage growth under business managers than non-business managers. They also point to differential rent-sharing practices—how increases in revenues and profits are shared with employees—as a major mechanism for our results.

To further explore this issue, we compute rent-sharing elasticities for non-business managers: export shocks increase profits per worker by 16% and raise value added per worker by 7–10% (see Appendix Table A17). Hence, the point estimates in column 1 of Table 4 imply that a 10% increase in profits (value added) per worker is associated with a 1.1% (1.7%) increase in hourly wages in firms run by non-business managers. This elasticity is in the ballpark of the estimates in the literature.[25] The analogous elasticities for business managers are essentially zero. These results indicate that business

[24] We follow Hummels et al. (2014) and only keep firms that have both positive exports and imports and more than 50 employees.

[25] In particular, Jäger et al. (2020) provide an overview of the rent-sharing elasticities in the literature. Most estimates of the elasticity of wages to value added per worker lie between 0.05 and 0.2.

managers alter rent-sharing practices, moving away from sharing some of the increases in profits and sales with workers.

5.2 Additional Results and Robustness Checks

Table A18 in the Appendix explores this issue further by distinguishing between positive and negative export shocks, constructed as described in Section 3. The estimates from this exercise reveal another interesting pattern: business and non-business managers behave very differently after positive shocks, consistent with the results in Table 4. However, neither business managers nor non-business managers cut (nominal) wages in response to negative export shocks.[26]

Table 5 adds to these results by adopting a hybrid strategy between those of the previous section and this section. Namely, we look at how switches from non-business to business managers impact rent-sharing in the face of positive export shocks. It separately estimates the effects of export shocks on value added per worker, hourly wages, annual earnings, and the labor share before and after transitions to a business manager. It demonstrates that there is rent-sharing before the switch, which ceases after a business manager takes office. Appendix Table A19 repeats the specification in Table 4 for the sample of exporters with non-business to business manager transitions and finds similar results.

Appendix Table A20 performs a version of the same placebo exercises we reported in the previous section, focusing on switches from a non-business manager to another non-business manager. In comparison with the estimates in Table 5, we now see very similar elasticities, which is reassuring for our interpretation.

Appendix Table A17 and Figure A37 provide a number of additional results that bolster our confidence in these estimates. First, we show that there are also no major differential responses to export shocks in terms of increases in value added, employment, and investment between non-business and business managers. In particular, firms operated by both business and non-business managers experience a similar increase in exports, profits, value added, employment, and investment following these shocks. Second, we do not detect effects from leads and lags of export shocks on value added or wages, which is also reassuring for our identification strategy.

The rent-sharing results are robust as well. In Table A21, we obtain similar results when we control for overall variation in product level exports, focusing only on variation across firms exporting similar products to different destinations. In Table A22, we present similar estimates for a balanced panel of firms.[27] In Table A24, we bolster our interpretation by confirming that there are no analogous

[26] This likely reflects the fact that wage cuts are difficult to impose on workers. Downward wage rigidity is in line with the patterns documented in several works on the US and European labor markets. See, for example, Grigsby et al. (2021) and Hazell and Taska (2020) for the US, Nickell and Quintini (2003) for the UK, and Fehr and Goette (2005) for Switzerland.

[27] Table A23 shows that there are no exit effects from export shocks.

effects for managers with non-business college degrees, who continue to share rents just as intensively as managers without a college degree. In Table A25, we confirm that the difference between business and non-business managers is larger for union workers than non-union workers.

Consistent with the rent-sharing mechanism, we find that the effects of business managers are larger in less competitive industries where there are, presumably, more rents to be shared. In particular, Appendix Figure A38 shows larger wage and labor share declines following a switch to a business manager in high-concentration industries (defined as being above the median of the Herfindahl-Hirschman index). Appendix Figure A39, in turn, depicts larger estimates for high-growth firms, than low-growth firms.

Table 6 presents analogous rent-sharing results using export demand shocks for the US. The estimates are broadly similar to the Danish case, but are less precise. We suspect that this is because we have a small number of exporters in our US Compustat sample and exports make up a smaller share of these firms' revenues than in our Danish sample.[28]

Overall, the evidence in this section indicates that business managers are much less likely to share with their employees rents that result from exogenous demand shifts. In contrast, there is a stable pattern of rent-sharing among non-business managers.

6 Discussion and Conclusion

Wage growth has slowed down and the labor share in national income has declined in many advanced economies over the last three decades. We argue that a major contributing factor has been changes in wage policies of firms run by managers/CEOs with a business education. We explore the effect of business managers on worker earnings and the labor share using matched employer-employee datasets from the US and Denmark. In both countries, business managers reduce the earnings of their employees. For example, five years after the appointment of a business manager, worker earnings decline by 6% and the labor share by 5 percentage points in the US, and 3% and 3 percentage points in Denmark (relative to firms operated by non-business managers).

Our evidence, using a number of distinct strategies, suggests that business managers are not more productive: firms appointing business managers are not on differential trends and do not enjoy higher sales, productivity, investment, or employment growth following their accession. We also establish that a key mechanism for these (relative) wage effects is changes in rent-sharing practices following the appointment of business managers. We document that exogenous export demand shifts lead to similar-sized output and export responses from firms run by business and non-business managers. However,

[28] An earlier version of the paper also showed similar results from changes in the supply of internationally-sourced intermediate inputs in Denmark. These results are available upon request.

while non-business managers share greater profits with their workers, business managers do not.

We interpret our results as reflecting the business-school-led shift towards emphasizing shareholder value and attempts to reengineer corporations by making them leaner. According to this interpretation, business managers are either more inclined—or have a better set of justifications—to keep wage growth down and increase the share of firm revenues accruing to shareholders and capital, even in the face of positive shocks boosting profits.

We view our paper as a first step in understanding how different management practices and ideologies might affect the labor market, wages, and inequality. Within this agenda, there are many fruitful areas for future research.

First, our US and Danish results are remarkably similar. Although there are significant differences in labor market institutions between the two countries, perhaps the most important of those, industry-level wage bargaining, had already declined in Denmark and was fairly limited during our study window. It would be valuable to investigate the effects of business education on wages and labor market outcomes in other countries, including those where centralized union bargaining is still prevalent, in order to obtain a more holistic understanding of how management practices interact with labor market institutions.

Second, our methodology is silent on exactly what practices business managers are changing and how these impact wages. An interesting next step would be to combine our approach with those in studies such as Bloom and Van Reenen (2007), which measure management practices at a granular level. Such an exercise might shed more light on what aspects of management practices matter for wage policies and inequality.

Third, we conjectured that our results may be related to the spread of ideas about shareholder value and corporate reengineering fire business schools. Business schools are, of course, not the only institutions pushing firms in this direction. The other major nexus for these ideas has been management consulting (Bogdanich and Forsythe, 2023), and it would be interesting to explore the effects of management consulting advice on firms' wage policies. Private equity buyouts have also been shown to have similar negative effects on worker earnings despite raising productivity at target firms (Davis et al., 2014).

Finally, an important question is whether estimates such as ours correspond to causal effects of business school education, rather than the selection of different types of individuals into business degrees. We plan to explore this issue, and the broader question of exactly what values and practices business schools impart, in a companion paper using a variety of different strategies.

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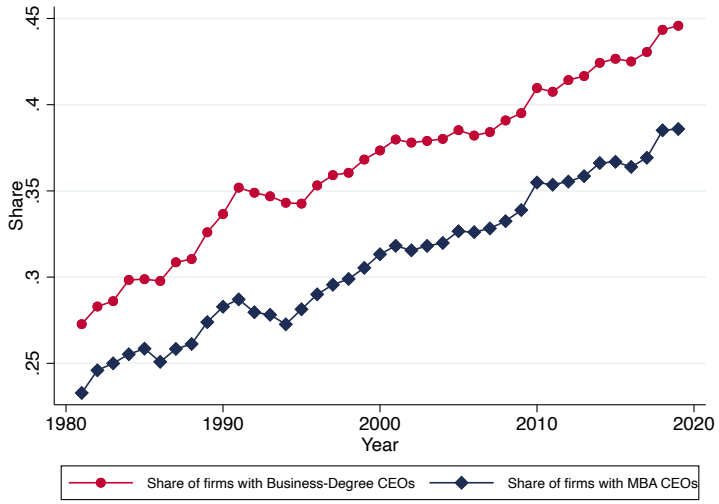
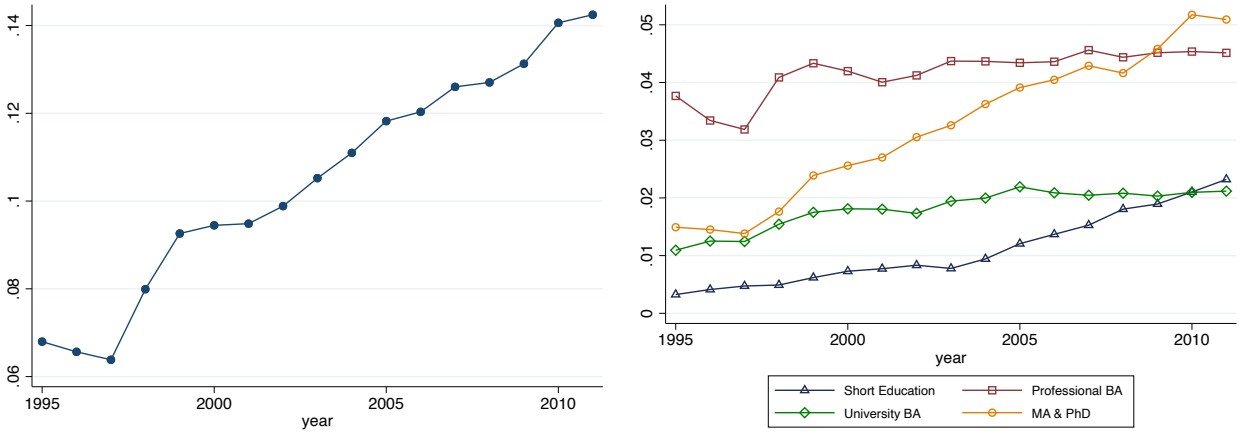
Figure 1: Share of Compustat Firms with Business CEOs in the US



This figure plots the share of Compustat firms that have CEOs with business degrees ("business managers") and the share of Compustat firms that have CEOs with MBA degrees from 1981 to 2019. The education information of CEOs is from the BoardEx dataset.

Figure 2: Share of Business Managers in Denmark

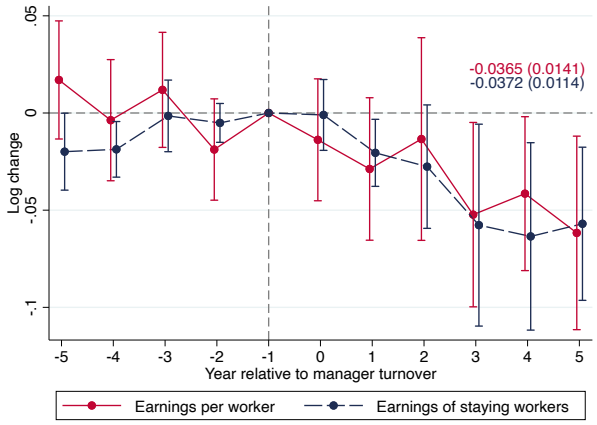


(a) Share of business managers

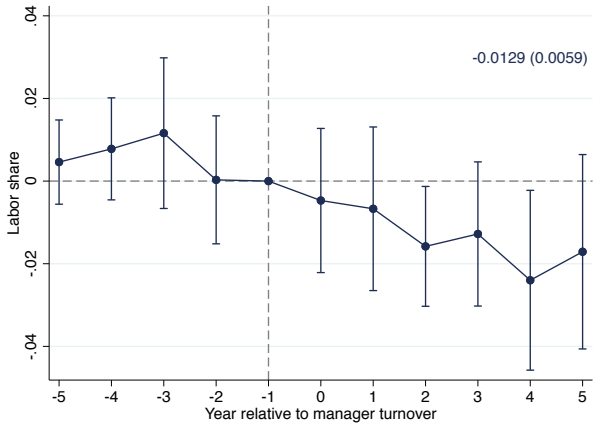
(b) by degree level

This figure plots the share of business managers in Denmark from 1995 to 2011. Panel (a) plots the share of all firms with over 5 employees whose manager has a business degree, while Panel (b) plots the share of firms whose manager has a short-education business degree, a professional BA in business, a university BA in business, and a MA or PhD degree in business (for each manager with a business degree, only the highest business degree is recorded).

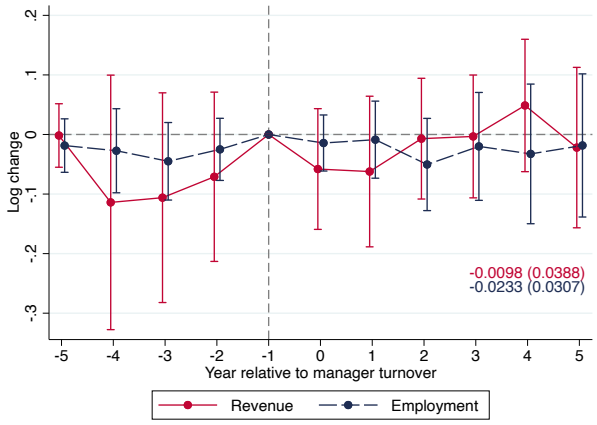
Figure 3: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions in the US



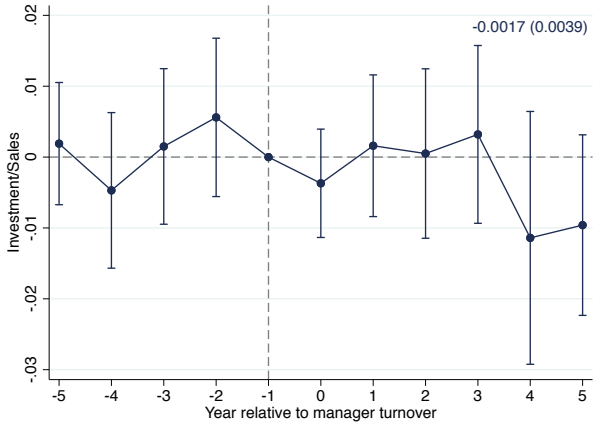
(a) Worker earnings



(b) Labor share



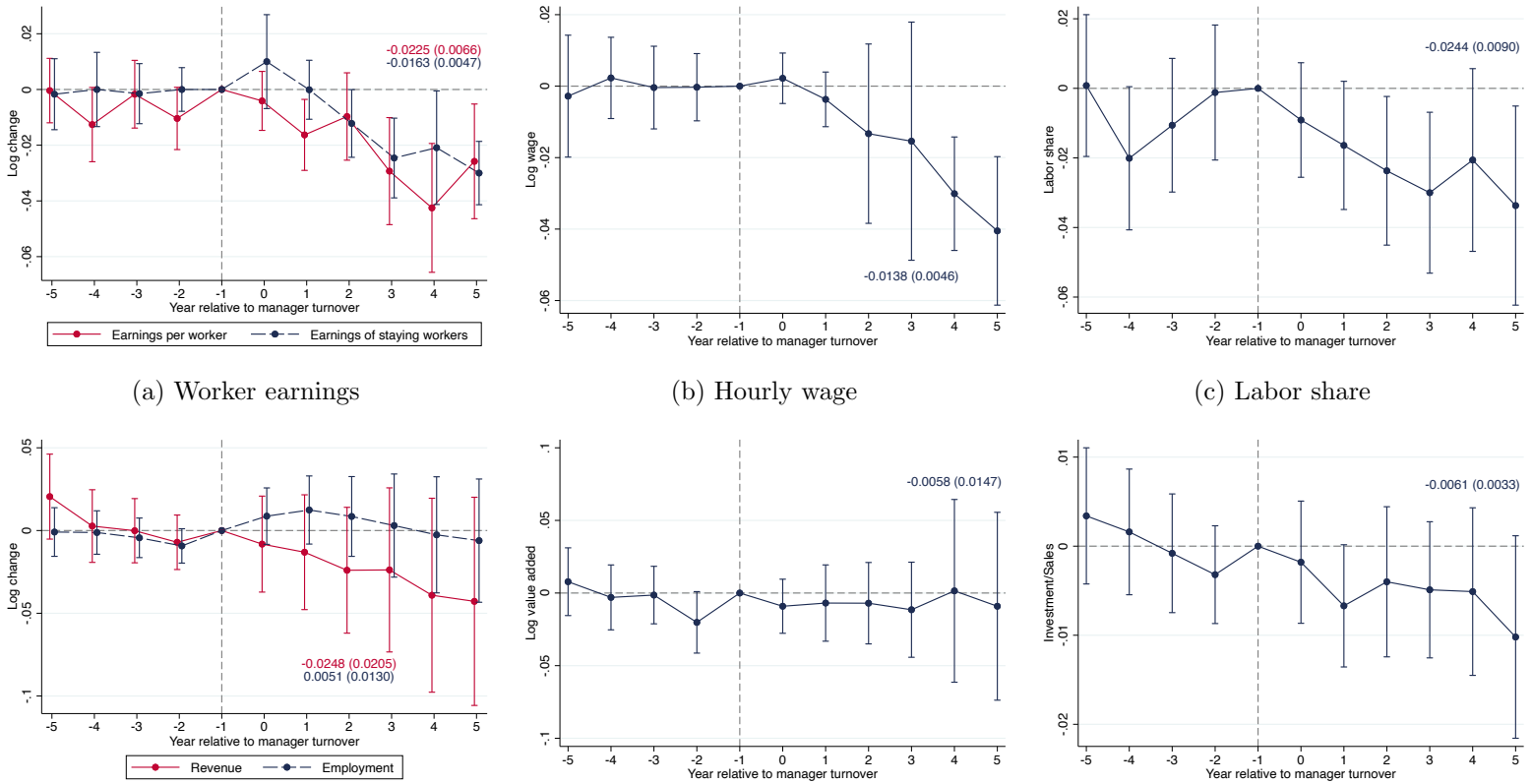
(c) Revenue and employment



(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker and log earnings of staying workers in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. For log earnings of staying workers in Panel (a), we use the matching estimator is described in Section 3. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure 4: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions in Denmark

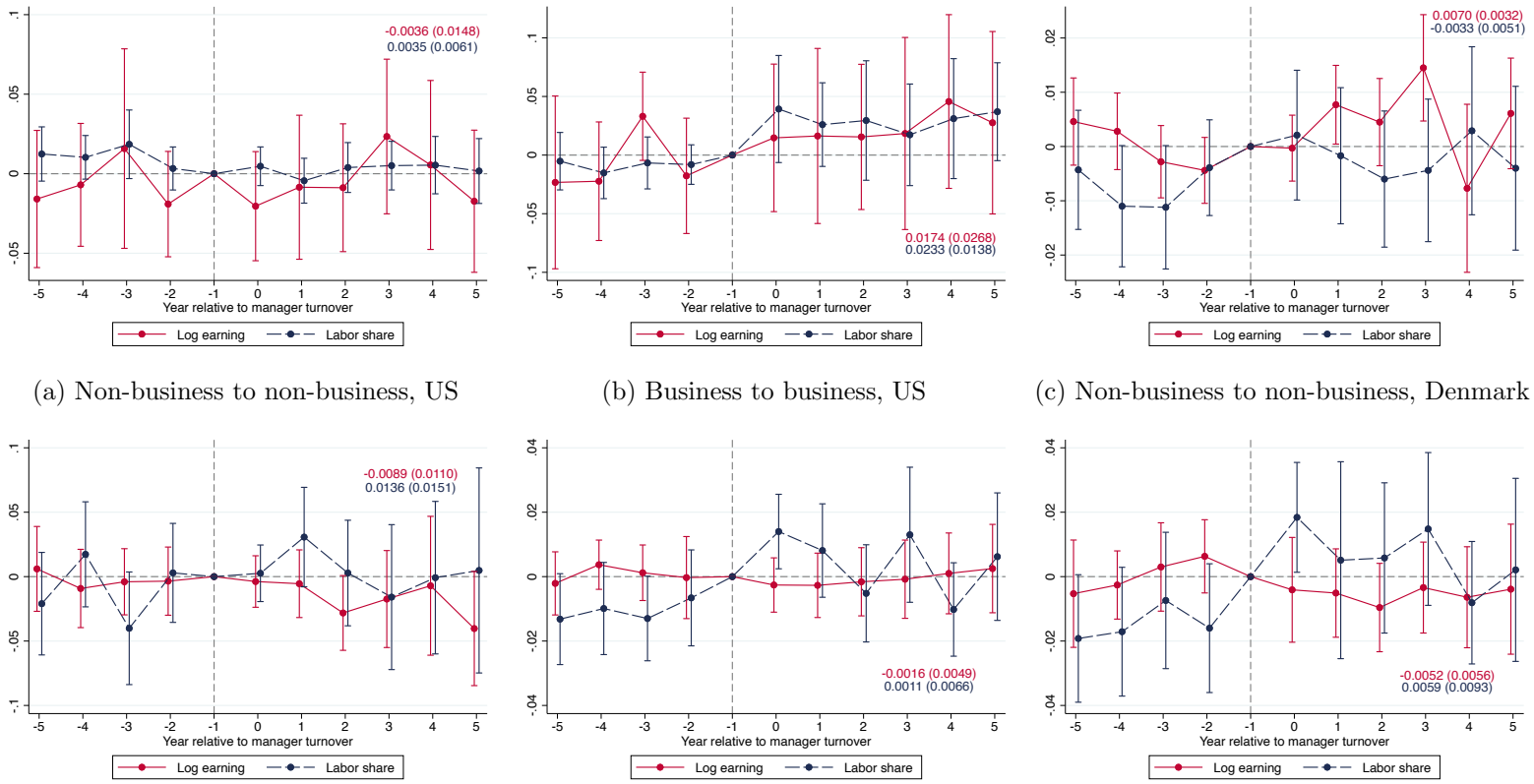


(a) Worker earnings (b) Hourly wage (c) Labor share

(d) Revenue and employment (e) Value added (f) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker and log earnings of staying workers in Panel (a), log hourly wage of staying workers in Panel (b), the labor share in Panel (c), log revenue and log employment in Panel (d), log value added in Panel (e), and investment divided by sales in Panel (f). The labor share is defined as total wage bill divided by value added. For log earnings of staying workers in Panels (a) and (b), we use the matching estimator is described in Section 3. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure 5: Changes in Worker Earnings and the Labor Share around Placebo Manager Transitions



(a) Non-business to non-business, US

(b) Business to business, US

(c) Non-business to non-business, Denmark

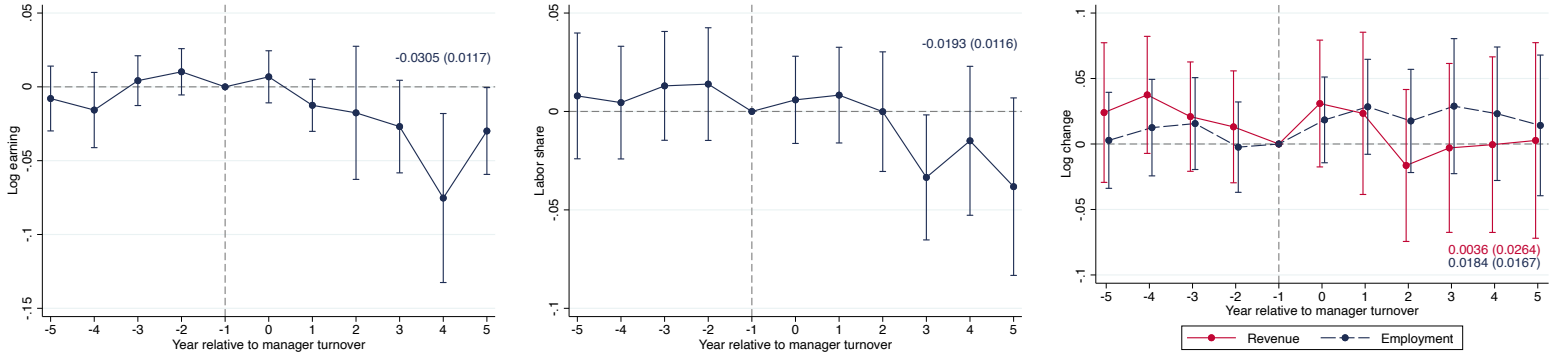
(d) Business to business, Denmark

(e) Non-college to college, Denmark

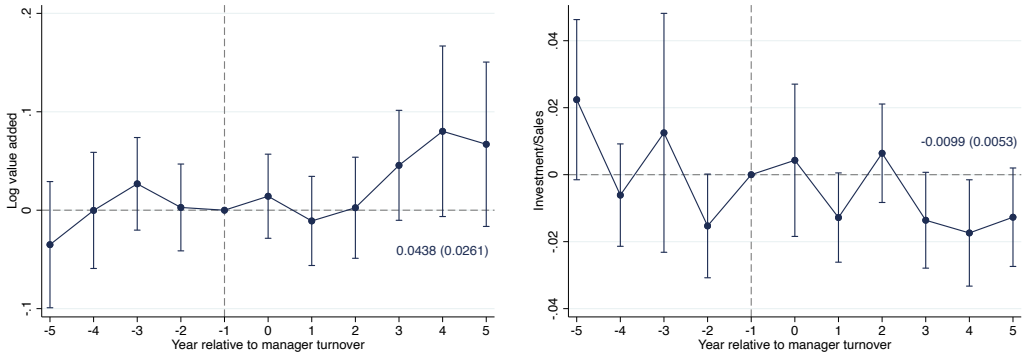
(f) Non-master to master, Denmark

This figure plots event-study estimates and 95% confidence intervals for log earnings per worker and labor share based on placebo manager transitions. In Panel (a), events are transitions from a non-business manager to another non-business manager in the US. In Panel (b), events are transitions from a business manager to another business manager in the US. In Panel (c), events are transitions from a non-business manager to another non-business manager in the Denmark. In Panel (d), events are transitions from a business manager to another business manager in Denmark. In Panel (e), events are transitions from a non-college-educated manager to a college-educated manager in Denmark. In Panel (f), events are transitions from a manager without a master's degree or PhD to a manager with a master's degree or PhD in Denmark. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure 6: Changes in Firm and Worker Outcomes around Transitions to Business Managers due to Manager Retirements and Deaths in Denmark



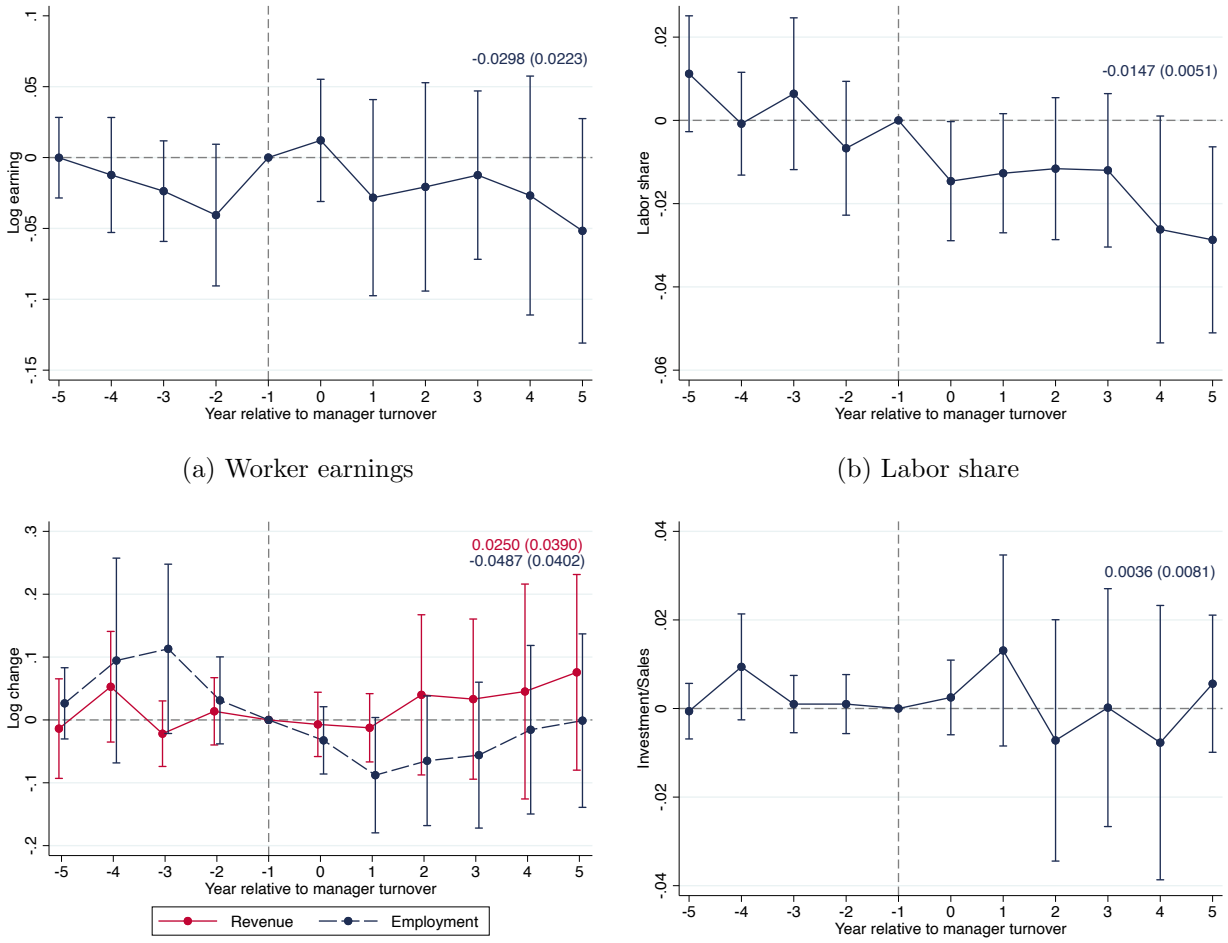
(a) Worker earnings (b) Labor share (c) Revenue and employment



(d) Value added (e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are retirements or sudden deaths (as defined in the text) of non-business managers with the successor being a business manager in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure 7: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions due to Manager Retirements in the US



(a) Worker earnings

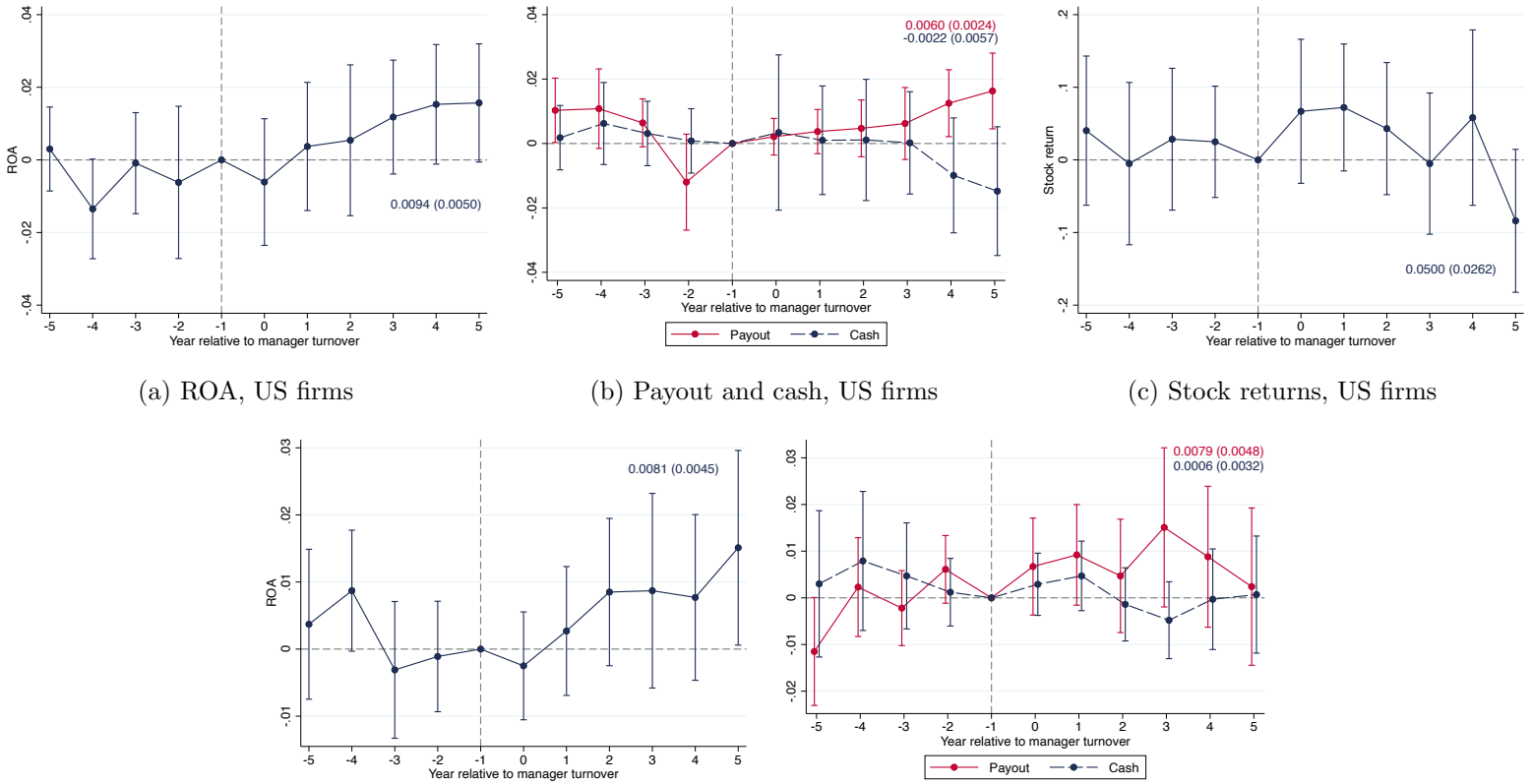
(b) Labor share

(c) Revenue and employment

(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are retirements (as defined in the text) of non-business managers with the successor being a business manager in the US. All regressions control for firm fixed effects, firm size quintile by year fixed effects, state×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure 8: Changes in Firms' Financial Performance around Non-Business to Business Manager Transitions



(a) ROA, US firms

(b) Payout and cash, US firms

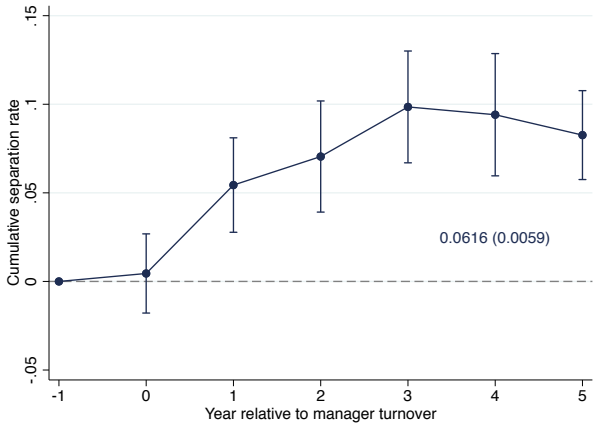
(c) Stock returns, US firms

(d) ROA, Danish firms

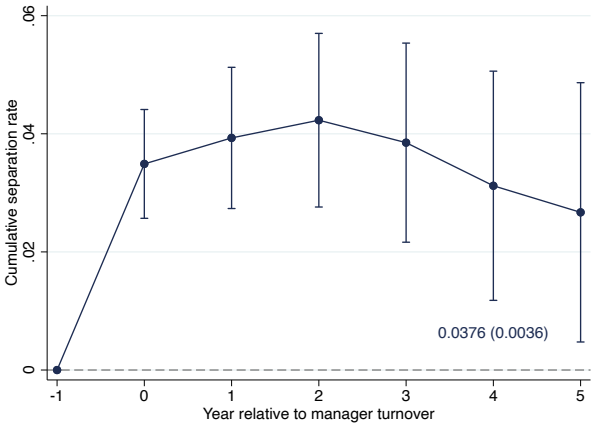
(e) Payout and cash, Danish firms

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. In Panels (a) and (d), the dependent variable is return on assets (ROA), defined as earnings before interests and taxes (or profits in Denmark) divided by total assets. In Panels (b) and (e), the dependent variables are payout (total dividends plus stock buybacks divided by total assets) and cash divided by total assets. In Panel (c), the dependent variable is annual stock return of US firms from CRSP. All specifications include firm fixed effects, industry×year fixed effects, state(region)×year fixed effects, and firm size quintile by year fixed effects. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

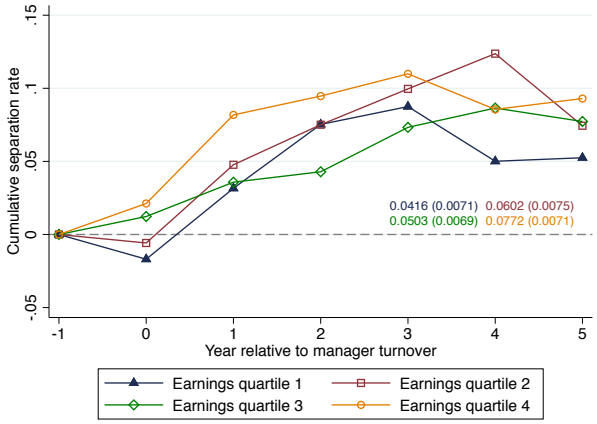
Figure 9: Changes in Separation Rates around Non-Business to Business Manager Transitions



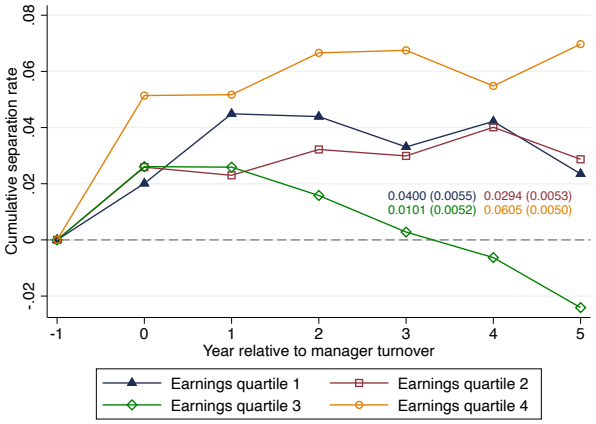
(a) Separation rates of all workers, US



(b) Separation rates of all workers, Denmark



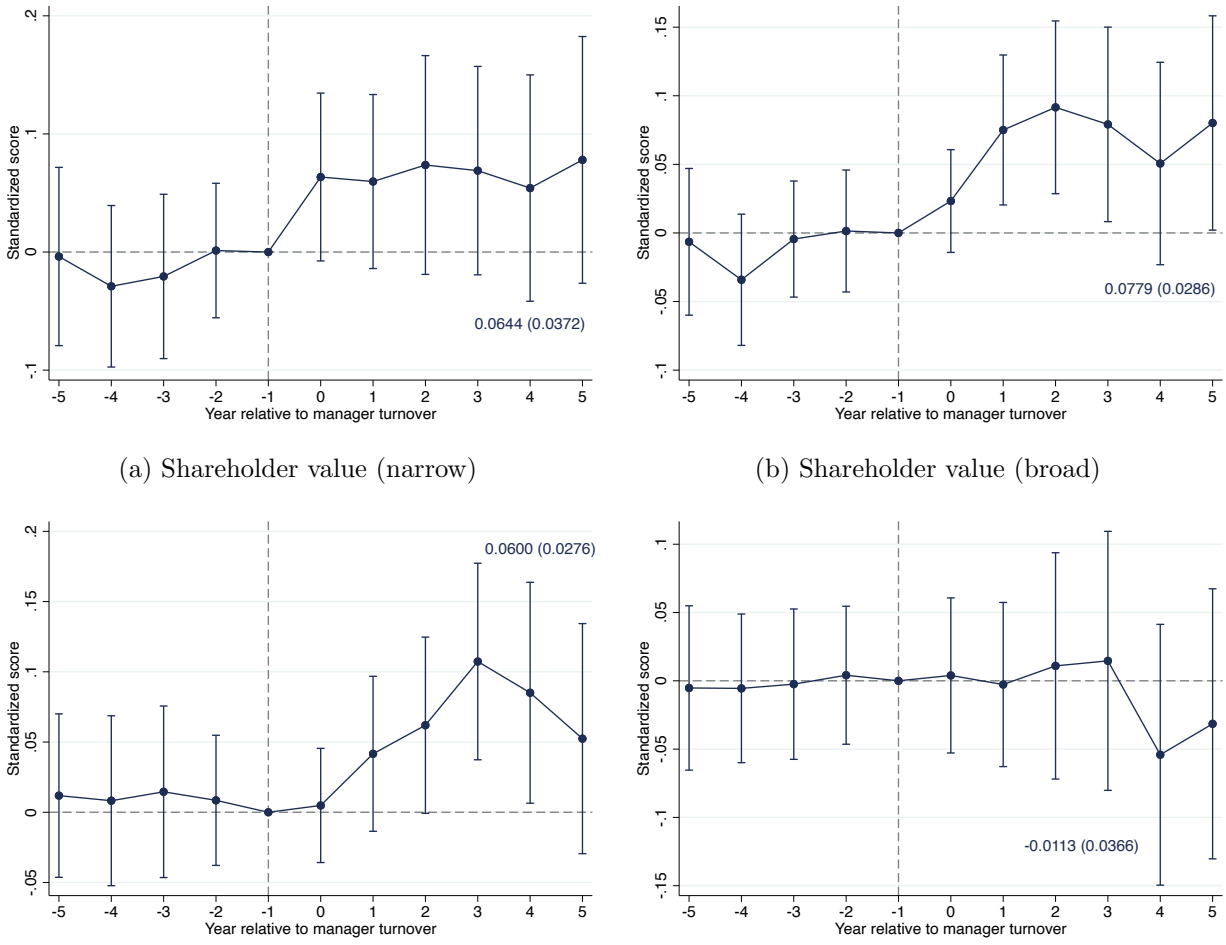
(c) Separation rates by earnings level, US



(d) Separation rates by wage level, Denmark

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. Panels (a) and (b) use US data and Panels (c) and (d) use data from Denmark. The dependent variable is cumulative separation rate, defined as the share of workers initially employed at the firm in year -1 who are no longer employed at the firm in a given year. In Panel (c), workers are divided into four groups based on their annualized earnings in year -1. In Panel (d), workers are divided into four groups based on their hourly wage in year -1. The estimates are based on the matching estimator described in Section 3. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure 10: Changes in Corporate Value around Non-Business to Business Manager Transitions in the US



(a) Shareholder value (narrow)

(b) Shareholder value (broad)

(c) Lean thinking

(d) Employee

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in the US. The dependent variables are the *narrow shareholder value* score in Panel (a), the *broad shareholder value* score in Panel (b), the *lean thinking* score in Panel (c), and the *employee* score in Panel (d). We derive these corporate values using the management discussion and analysis (MD&A) sections of firms' 10-Ks, and the methodology is described in detail in Section 4.8 and Appendix A.2. All scores are standardized to mean zero and standard deviation of one. All specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Table 1: 2SLS Estimates of Business Managers on Firm Outcomes Using Board Composition Around Manager Retirements in the US

Panel A: Share of External Directors with Business Degrees 1 Year Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.5739					
	(0.0116)					
Business Manager×Post Retirement		-0.0681	-0.0279	0.0487	0.0660	-0.0016
		(0.0192)	(0.0076)	(0.0595)	(0.0866)	(0.0053)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

Panel B: Share of External Directors with Business Degrees 5 Years Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.5807					
	(0.0113)					
Business Manager×Post Retirement		-0.0527	-0.0373	0.0102	-0.0117	-0.0012
		(0.0182)	(0.0074)	(0.0605)	(0.0451)	(0.0050)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

This table reports the first-stage and 2SLS estimates based on board composition around manager retirements in the US. The sample is the set of firms where a non-business manager retires. The instrument in two panels, respectively, are the share of external board directors with a business degree one year before the retirement and the share of external board directors with a business degree five years before the retirement. In all columns, we control for firm fixed effects and year fixed effects. Column 1 of each panel reports the first-stage estimates of the IV (described in equation 2). Columns 2–5 report 2SLS estimates of business managers on firm outcomes. The dependent variables are log earnings per worker in column 2, the firm's labor share (wage bill divided by sales) in column 3, log revenue in column 4, log employment in column 5, and investment divided by sales in column 6. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table 2: Board Composition and Pre-trends Before Retirement

	Δ Log Worker Earnings $_{t-2,t-1}$	Δ Log Worker Earnings $_{t-3,t-2}$	Δ Log Worker Earnings $_{t-4,t-3}$	Δ Log Worker Earnings $_{t-5,t-4}$
	(1)	(2)	(3)	(4)
Business Director Share$_{t-5}$	-0.0135	-0.0036	-0.0215	0.0178
	(0.0273)	(0.0244)	(0.0248)	(0.0224)
Year FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000
	Δ Labor Share $_{t-2,t-1}$	Δ Labor Share $_{t-3,t-2}$	Δ Labor Share $_{t-4,t-3}$	Δ Labor Share $_{t-5,t-4}$
	(5)	(6)	(7)	(8)
Business Director Share$_{t-5}$	-0.0043	0.0008	-0.0022	0.0043
	(0.0063)	(0.0054)	(0.0056)	(0.0049)
Year FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000
	Δ Log Revenue $_{t-2,t-1}$	Δ Log Revenue $_{t-3,t-2}$	Δ Log Revenue $_{t-4,t-3}$	Δ Log Revenue $_{t-5,t-4}$
	(9)	(10)	(11)	(12)
Business Director Share$_{t-5}$	-0.0147	0.0293	0.0080	-0.0202
	(0.0297)	(0.0343)	(0.0306)	(0.0325)
Year FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000

This table reports the correlations between the IV and pre-trends in firm outcomes before manager retirement. The independent variable is the share of external board directors with a business degree five years before manager retirement. The dependent variable are pre-trends in log earnings per worker in columns 1–4, pre-trends in the labor share in columns 5–8, and pre-trends in log revenue in columns 9–12. For example, column 1 is the change in log earnings per worker from 2 years before the retirement to 1 year before the retirement, column 2 is the change in log earnings per worker from 3 years before the retirement to 2 years before the retirement, etc. All columns control for year fixed effects. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table 3: Compensation of Business Managers

Panel A: US

	Log Compensation of Managers			
	(1)	(2)	(3)	(4)
Business Degree	0.1739	0.1617	0.0747	0.0251
	(0.0121)	(0.0121)	(0.0103)	(0.0123)
Year FE	Y	Y	Y	Y
Manager Characteristics	N	Y	Y	Y
Firm Characteristics	N	N	Y	Y
Firm FE	N	N	N	Y
Obs	36,607	36,607	36,607	36,607

Panel B: Denmark

	Log Compensation of Managers			
	(1)	(2)	(3)	(4)
Business Degree	0.4555	0.4487	0.2692	0.0967
	(0.0105)	(0.0105)	(0.0080)	(0.0079)
Year FE	Y	Y	Y	Y
Manager Characteristics	N	Y	Y	Y
Firm Characteristics	N	N	Y	Y
Firm FE	N	N	N	Y
Obs	267,403	267,403	267,403	267,403

This table reports the coefficients from regressions of compensation of CEOs on an indicator for having a business degree. In Panel A, the sample includes all CEOs from 1992 to 2019, and the dependent variable is log total compensation from the Execucomp dataset (including salary, bonus, total value of restricted stock granted, total value of stock options granted using Black-Scholes, incentive plan payouts, and other compensation). In Panel B, the sample includes all managers from 1995 to 2011, and the dependent variable is log total annual earnings of managers. In both panels, all columns include year fixed effects, column 2 additionally controls for manager characteristics (gender, experience, age), column 3 additionally controls for firm characteristics (log employment and log sales), and column 4 additionally controls for firm fixed effects. Standard errors are clustered at the person level.

Table 4: Business Managers and Wage Response to Export Shocks in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.0168	0.0221	-0.0030	0.0132	0.0149	0.0012
	(0.0029)	(0.0044)	(0.0130)	(0.0029)	(0.0037)	(0.0122)
Export Shock*Business Manager	0.0020	0.0097	-0.0122	-0.0037	-0.0010	-0.0087
	(0.0066)	(0.0067)	(0.0126)	(0.0066)	(0.0063)	(0.0116)
Log Output				0.0128	0.0131	-0.1084
				(0.0032)	(0.0051)	(0.0155)
Log Employment				0.0143	0.0454	0.0979
				(0.0038)	(0.0052)	(0.0202)
Log Capital-labor Ratio				0.0030	0.0000	-0.0112
				(0.0014)	(0.0013)	(0.0050)
Share of High-skilled Workers				0.0860	0.0774	0.2704
				(0.0202)	(0.0253)	(0.1013)
F-statistic	5.7	2.8	5.9	7.4	5.3	7.6
F-test p value	0.017	0.092	0.015	0.007	0.021	0.006
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE	Y	Y	Y	Y	Y	Y
Obs	1,776,520	1,776,520	5,313	1,776,520	1,776,520	5,313

This table reports the coefficients from regressions of wages and the labor share on export shocks interacted with a dummy for having a business manager. Export shocks are shocks to export demand from destination-product combinations the firm exports to as defined in the text. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table 5: Response to Export Shocks Before and After Manager Transitions in Denmark

	Value Added per Worker		Log Hourly Wage		Log Annual Earnings		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Export Shock*Pre	0.1031	0.1086	0.0335	0.0184	0.0503	0.0214	0.0269	-0.0275
	(0.0573)	(0.0326)	(0.0075)	(0.0034)	(0.0087)	(0.0054)	(0.0272)	(0.0113)
Export Shock*Post	0.1051	0.1117	0.0171	0.0058	0.0202	-0.0104	0.0038	-0.0396
	(0.0556)	(0.0328)	(0.0076)	(0.0065)	(0.0075)	(0.0082)	(0.0264)	(0.0118)
F-statistic	0.0	0.1	2.8	3.4	8.2	13.3	0.4	0.6
F-test p value	0.839	0.730	0.094	0.064	0.004	0.000	0.541	0.452
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Worker-firm FE			Y	Y	Y	Y		
Obs	1,008	3,905	511,754	1,273,209	511,754	1,273,209	1,008	3,905

This table reports the coefficients from the regression of wages, value added per worker and the labor share on export shocks before and after transitions from a non-business manager to a business manager. *Pre* is a dummy variable that equals 1 if the observation is before the manager transition, and *Post* is a dummy variable that equals 1 if the observation is after the manager transition. Columns 1, 3, 5, 7 include firms with a manager transition from a non-business manager to a business manager, and columns 2, 4, 6, 8 also include firms that always had non-business managers (for which *Pre* equals 1 for all years). In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the observation is after the manager transition. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Dependent variables are log value added per worker in columns 1 and 2, log hourly wage in columns 3 and 4, log annual earnings in columns 5 and 6, and the labor share (wage bill divided by value added) in columns in columns 7 and 8. Firm-level regressions in columns 1, 2, 7, 8 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Pre* and the coefficient of *Export Shock*Post* at the bottom of the table.

Table 6: Business Managers and Response to Export Shocks in the US

	Log Export	Log Revenue	Log Employment	Log Earnings per Worker		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Export Shock*Non-Business Manager	0.1584	0.0922	0.0366	0.0272	0.0259	0.0021	0.0047
	(0.0985)	(0.0283)	(0.0239)	(0.0154)	(0.0152)	(0.0064)	(0.0066)
Export Shock*Business Manager	0.1938	0.0717	0.0441	-0.0184	-0.0135	-0.0135	-0.0112
	(0.1309)	(0.026)	(0.0249)	(0.0169)	(0.0084)	(0.0084)	(0.0083)
Log Revenue					0.0817		-0.0374
					(0.0270)		(0.0102)
Log Employment					-0.1289		0.0222
					(0.0337)		(0.0094)
F-statistic	0.1	0.6	0.1	8.7	7.8	3.7	3.9
F-test p value	0.769	0.452	0.777	0.003	0.005	0.054	0.048
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size quintile-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	14,500	14,500	14,500	14,500	14,500	14,500	14,500

This table reports the coefficients from regressions of firm-level outcomes on export shocks interacted with a dummy for having a business manager. Export shocks are shocks to export demand from destination-product combinations the firm exports to as defined in the text. The dependent variables are log exports in column 1, log revenue in column 2, log employment in column 3, log earnings per worker in columns 4 and 5, and the labor share (wage bill divided by sales) in columns 6 and 7. All regressions control for firm fixed effects, firm size quintile by year fixed effects, state×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Columns 5 and 7 also control for log revenue and log employment. Regressions are weighted by firm employment, and standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

A Appendix (For Online Publication)

A.1 Evidence from the Diffusion of Business Managers

Our second instrumental variable (IV) strategy is based on the idea that hiring a manager with a business degree becomes popular among certain types of firms at different times. This is similar to the strategy of Acemoglu et al. (2019), who exploited regional democratization waves as an instrument for a country switching from nondemocracy to democracy. In our context, we create region × industry × firm size quintile cells, and document that after the appointment of the first business manager within a cell, the likelihood of other firms in the cell also appointing a business manager increases significantly. We then use this relationship as the first stage of this two-stage least squares (2SLS) strategy. The first-stage equation is:

$$B_{it} = \sum_{k=1}^{3} \theta_k Z_{i,t-k} + X'_{it}\beta^F + \lambda_i^F + \delta_t^F + \epsilon_{it}, \tag{5}$$

where $Z_{it} = \frac{1}{|I_i|} \sum_{j \in \{I_i : j \neq i, C_j = C_i, B_{jt_0} = 0\}} B_{jt}$ is the instrument, defined as the jackknifed average of business managers among publicly listed firms in the same region × industry × size peer group that did not have a business manager at the beginning of the sample.[29] In the first stage, we include lags of the instrument up to three years, because the influence of peer firms may be felt with a lag. In practice, we find that lags beyond three years do not predict business manager hiring significantly. In all specifications, we control for year and firm fixed effects. We also include region, size quintile, and industry fixed effects interacted with year fixed effects, ensuring that we exploit only within-cell variation in the diffusion of business managers.

Our second-stage equation is the same as (1). The exclusion restriction in this case is that, conditional on our covariates, the timing of the first switch to a business manager in a cell is orthogonal to future outcomes of other firms in that cell. We provide a number of placebo exercises to bolster confidence in this exclusion restriction.

The first-stage relationships for the US and Denmark, which follow equation (5), are presented in Appendix Tables A26 and A27, while the two panels of Appendix Figure A30 depict the first-stage results visually. In these figures, we can see that the blue line, corresponding to firms in a cell following the first hiring of business manager, starts out lower than the average of other cells, as shown by the red line (which is by definition, since firms in the former cell initially had no business managers). However, we also see fast convergence of the two lines, indicating that the practice of hiring managers with a business degree diffuses rapidly across companies in the same industry, region, and size quintile. Our identification interprets this diffusion as resulting from a "fad" or from learning from peers. What we

[29] In this second IV analysis, we exclude firms that already had business managers at the start of our sample so that we only estimate the impact of hiring a business manager, which our event-study estimates suggest is different from the effect of firing a business manager.

require for our exclusion restriction is that this diffusion driven by first adoption is orthogonal to other factors subsequently affecting wages and the labor share in the same cell.

Appendix Tables A11 and A12 present estimates from this second IV strategy for the US and Denmark, respectively. The first three columns in each table show the effects on sales/value added, which are insignificant, although not precisely zero as in our event studies. The next six columns in each table depict robust negative effects on earnings per worker and labor share in both countries.

The bottom panel of both tables presents the OLS estimates in the same sample, exploiting the switch from a non-business to a business manager, essentially replicating the event-study design. Broadly, the IV estimates in Panel A are about 50% larger than the OLS estimates. Our interpretation of this difference between OLS and IV is related to the preceding discussion on management practices and attitudes associated with the increasing popularity of business programs over time. For example, suppose that, in a given cell, business managers and thus business management methods become more popular. This will lead to the hiring of more business managers, but simultaneously there will also be some adoption of these methods by existing non-business managers and their more intensive use by business managers relative to other cells.

If this interpretation is correct, then estimates from this second IV strategy will capture some of the systemic impacts of business school doctrines on changes in management-labor relations. Reflecting this, if we use the IV estimates, the implied magnitudes are correspondingly larger. In the US, with the IV estimates, the switch towards management practices associated with business schools would explain close to 40% of the decline of the labor share (as opposed to 16% in our baseline case). In Denmark, this number would be 9% (as opposed to 6% in our baseline).

The main concern with this IV strategy is that economic shocks correlated within firm cells might simultaneously affect the hiring of business managers and firm wages. While the industry $\times$ year fixed effects, region $\times$ year fixed effects, and size quintile $\times$ year fixed effects already absorb industry-level and region-level shocks and shocks related to firm size, we conduct several robustness tests to further address this concern. In columns 1, 4, and 7 in Appendix Table A13, we control for a full set of interactions between earnings per worker quintile in 1995 and year dummies, which should take out common shocks related to the initial wage level. In columns 2, 5, and 8 of this table, we include cell-specific trends to deal with unobserved heterogeneity across industry-region-size cells. In columns 3, 6, and 9, we control for three lags of value added and worker earnings of other firms in the same cell to allow value added and worker earnings to be flexibly correlated within cells. In all columns, we estimate very similar first-stage F-statistics, and the IV estimates of the effect on worker earnings and labor share remain negative, significant, and close to our baseline estimates.

A.2 Text Analysis of the MD&A Section of 10-K Filings

To analyze the texts of the MD&A Section of 10-K Filings, we use the word embedding model following Li et al. (2021). The word embedding model is a computational approach for representing text data in a format that allows words with similar meaning to have a similar representation. By mapping words into a high-dimensional space, this model enables the capture of contextual nuances and semantic similarities between words.

A.2.1 Preprocessing of Text Data

1. **Tokenization**: The text is initially broken down into a list of words for each sentence, a process known as tokenization, which structures the unprocessed text.

2. **Lemmatization**: We recorded the lemmatized form of all words. This step ensures that different forms of the same word are recognized as identical in analysis.

3. **Phrase Identification**: The process identifies phrases that have a collective meaning and links their lemmatized form with a delimiter to maintain their contextual value.

4. **Named Entity Normalization**: Concurrently, any named entities within the text are replaced with placeholders that denote the entity type, thus standardizing the entities while retaining their categorical importance.

5. **Filtering and Normalization**: The tokens undergo a series of filtering steps where numerical digits, punctuation, and single-letter words (except for significant ones like 'I' or 'a') are removed. This helps in reducing noise and focusing on more meaningful content.

6. **Stopword Removal and Case Standardization**: Commonly used stopwords are removed to emphasize more informative words, and all remaining tokens are converted to lowercase to maintain consistency across the data set.

A.2.2 Training of the Embedding Model

We employ the Bigram model to link words that frequently appear together, creating compound words or phrases. The mathematical principle behind the Bigram model is to calculate the probability of a word given the presence of its preceding word, effectively treating the text as a Markov chain. Parameters that can be set for this model include the minimum frequency of occurrence for word pairs to be considered (to filter out infrequent pairs) and the strength of the association needed to form a bigram (which determines how likely two words are to form a phrase based on their co-occurrence). It is important to note that if two adjacent words have already been preprocess to a phrase, they can potentially be linked again with another word by the Bigram model, leading to trigram phrases.

Similarly, we can recursively identify trigram phrases by building upon the bigrams, linking a third word to an already established bigram if they frequently co-occur in the corpus.

Word embedding, as used in Word2Vec, involves mapping words to a continuous vector space where similar words are positioned closely together. The mathematical concept is based on the context in which a word appears, training the model either to predict a word given its context (Skip-Gram) or to predict the context given a word (CBOW). Because words are translated into vectors, the model captures semantic meaning and relates words to each other based on the similarity of their contexts.

A.2.3 Generating the Dictionary

We begin with a set of seed words for each corporate value. For *shareholder value*, we use seed words *shareholder value*, *stockholder value*, *share owner value*, *stock owner value*, *stock returns*, and *shareholder returns*. For *lean thinking*, the seed words are words with the highest frequency from Womack and Jones (2003) and Hammer and Champy (2006): *lean thinking*, *lean enterprise*, *lean techniques*, *lean production*, *lean principles*, *lean system*, *lean methods*, *reduce costs*, *cost reduction*, *production cost*, *target cost*, *low cost*, *lead time*, *reengineering*, *reengineered processes*, *business processes*, *lower costs*, *labor costs*, and *division labor*. For *employee*, the seed words are *stakeholder*, *people*, *team*, *talent*, *partner*, *employee*, *human capital*, *empower*, *teamwork*, *personnel*, and *qualified*. Words that reach a certain threshold of similarity to these seed words are added to the corresponding dimension in the dictionary.

A.2.4 Scoring Documents for Semantic Content

This process provides a score for each article against each dimension, representing the emphasis or relevance of the article to those specific areas.

The TF score is defined as the frequency of a word's occurrence in a document. The mathematical formula is:

$$TF(t) = \frac{\text{Number of times term } t \text{ appears in a document}}{\text{Total number of terms in the document}}$$

Using the compiled dictionary, we calculate the TF-IDF score for all terms across all documents, quantifying how often dictionary terms appear in each document.

The TF-IDF score is a statistical measure used to evaluate the importance of a word to a document in a collection or corpus. The formula for TF-IDF is:

$$TFIDF(t, d) = TF(t, d) \times IDF(t)$$

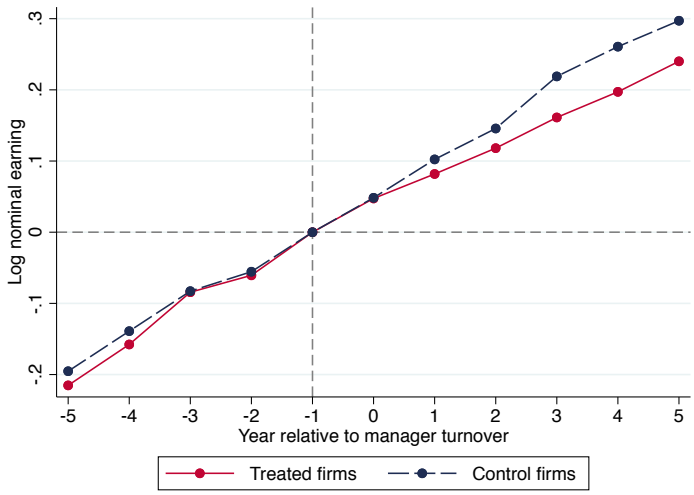
where:

$$IDF(t) = \log\left(\frac{\text{Total number of documents}}{\text{Number of documents with term } t}\right)$$

We generated a lexicon from all tokens, and then the IDF score is computed based on this lexicon and the previously calculated TF scores.

A.3 Additional Figures and Tables

Figure A1: Nominal Earnings Per Worker in Treated and Control Firms

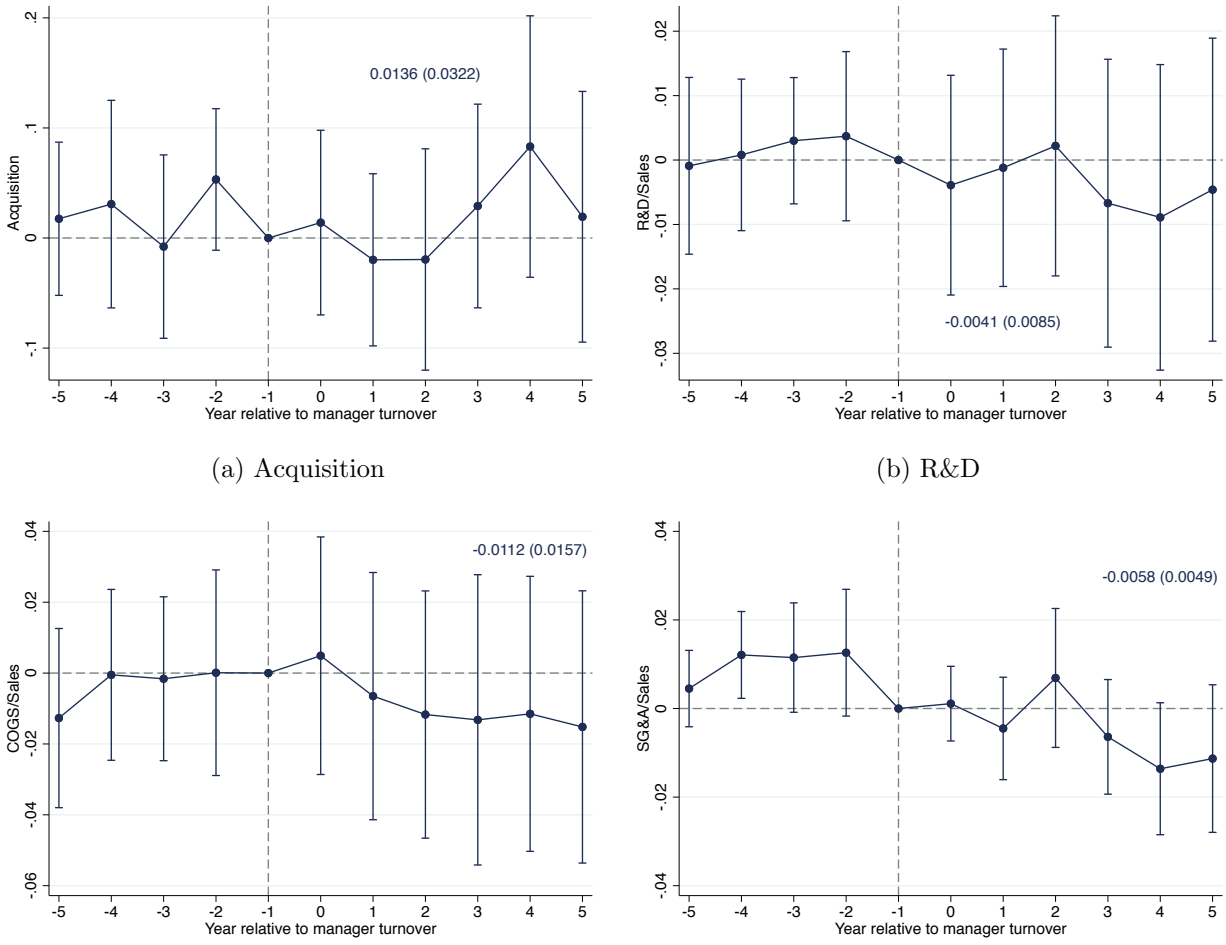


(a) US

(b) Denmark

This figure plots the nominal worker earnings trends in treated and matched control firms. Panel (a) is based on US data and Panel (b) is based on Danish data. In each panel, the dashed blue line is the log (nominal) earnings per worker of matched control firms, normalizing the earnings per worker in year -1 to zero. The solid red line in Panel (a) (Panel (b)) is the sum of the log (nominal) earnings per worker of matched control firms plus the estimated event study coefficients in Figure 3 (Figure 4) Panel (a).

Figure A2: Changes in Other Firm Outcomes around Non-Business to Business Manager Transitions in the US



(a) Acquisition

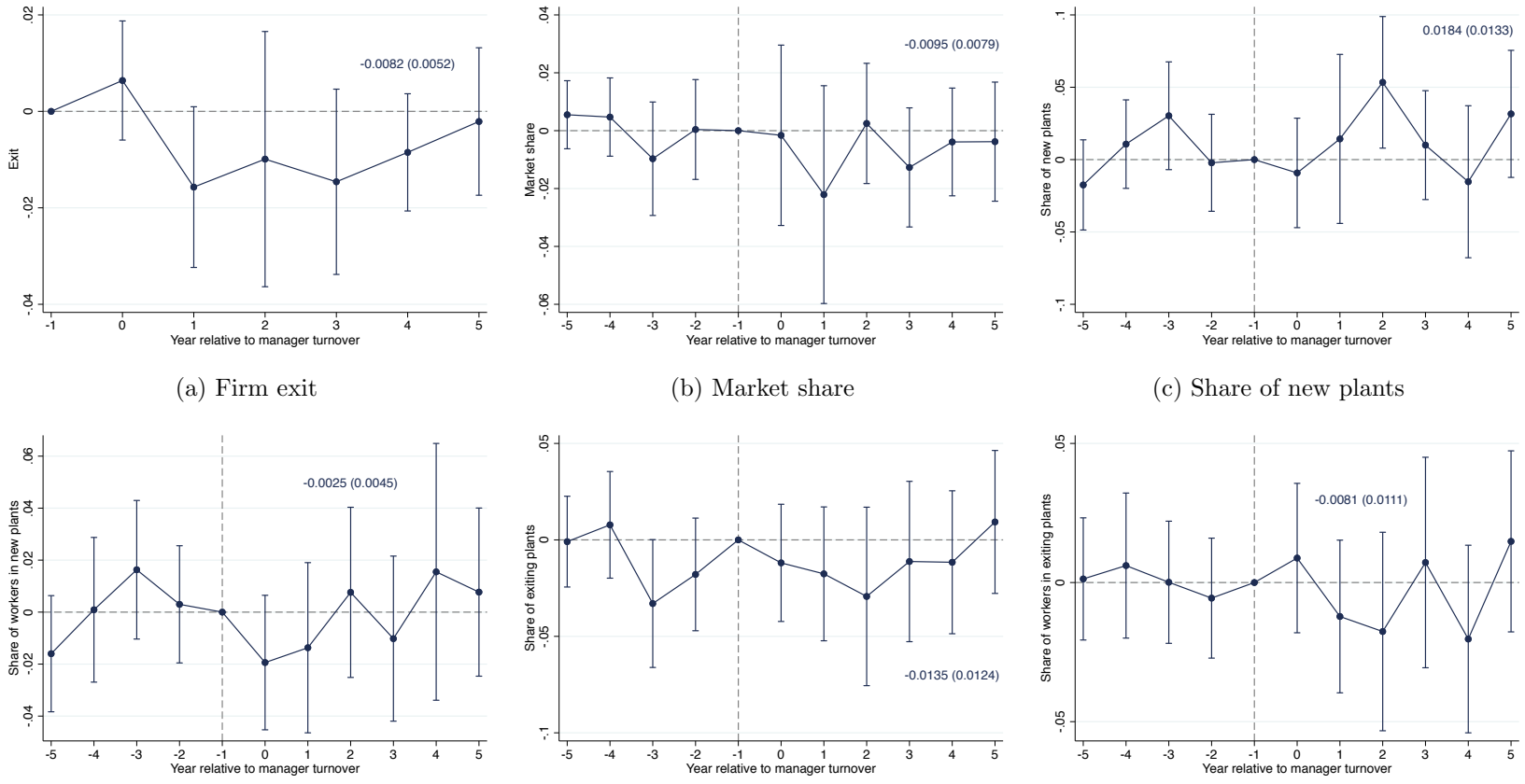
(b) R&D

(c) Costs of goods sold

(d) SG&A expenses

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. All specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are a dummy variable for acquisition in Panel (a), R&D expenses divided by sales in Panel (b), costs of goods sold divided by sales in Panel (c), and SG&A expenses divided by sales in Panel (d). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A3: Changes in Entry and Exit around Non-Business to Business Manager Transitions in the US

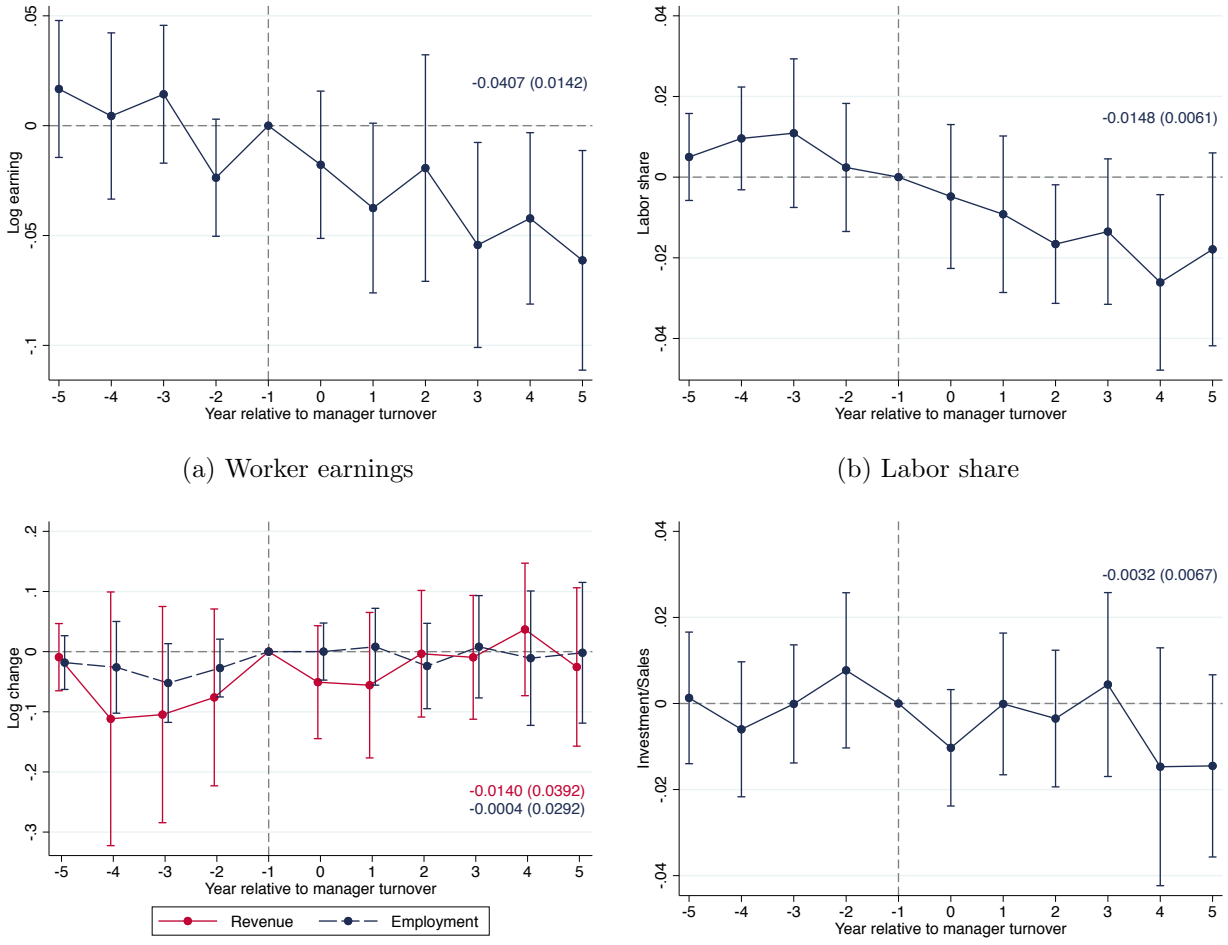


(a) Firm exit (b) Market share (c) Share of new plants

(d) Share of workers in new plants (e) Share of exiting plants (f) Share of workers in exiting plants

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. All specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are a dummy variable for firm exit in Panel (a), market share in the 4-digit NAICS industry in Panel (b), the share of new plants in Panel (c), the share of workers in new plants in Panel (d), the share of plants exiting next year in Panel (e), and the share of workers in plants exiting next year by sales in Panel (f). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A4: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions in the US Controlling for Firm Age×Year Fixed Effects



(a) Worker earnings

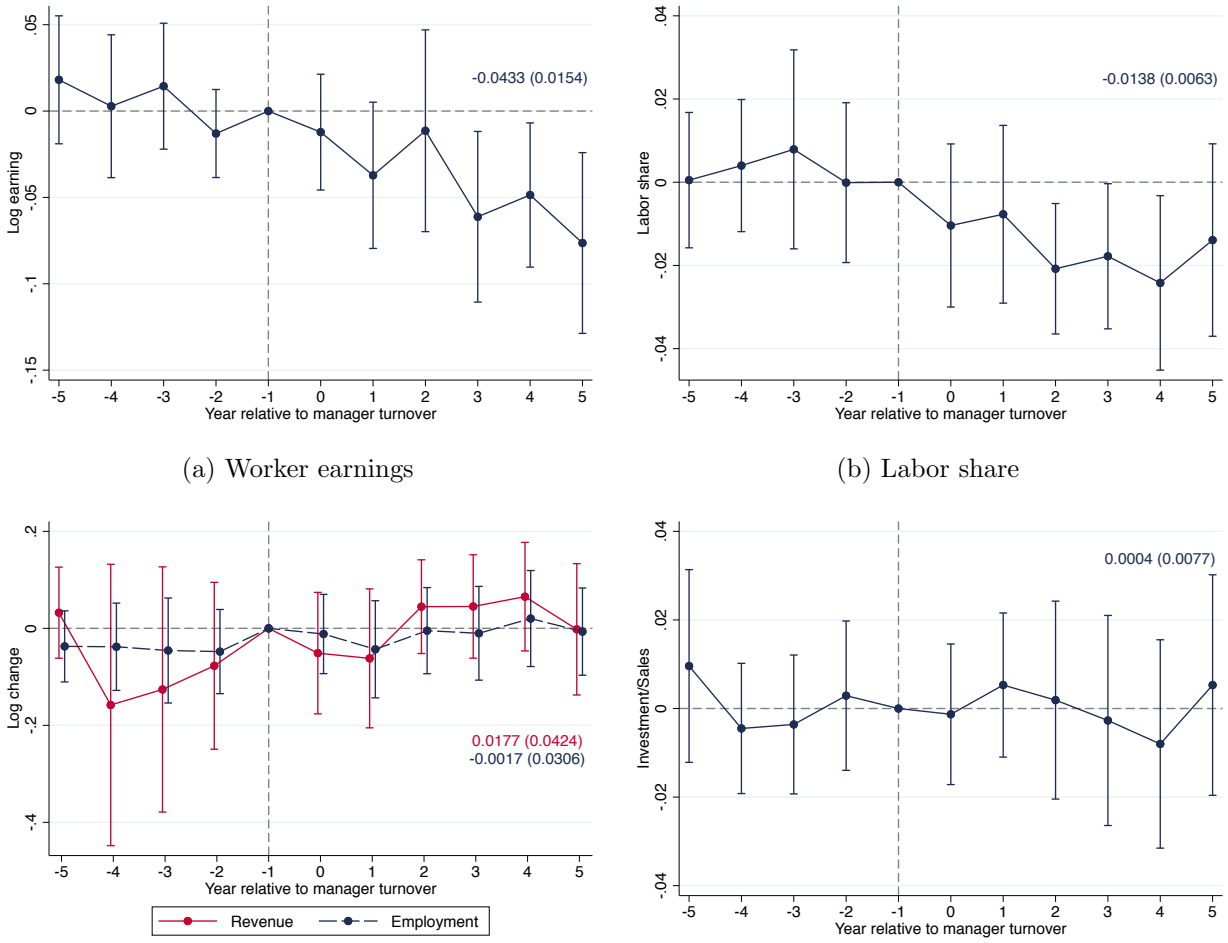
(b) Labor share

(c) Revenue and employment

(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All specifications include firm fixed effects, firm age×year fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A5: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions for a Balanced Panel of Firms in the US



(a) Worker earnings

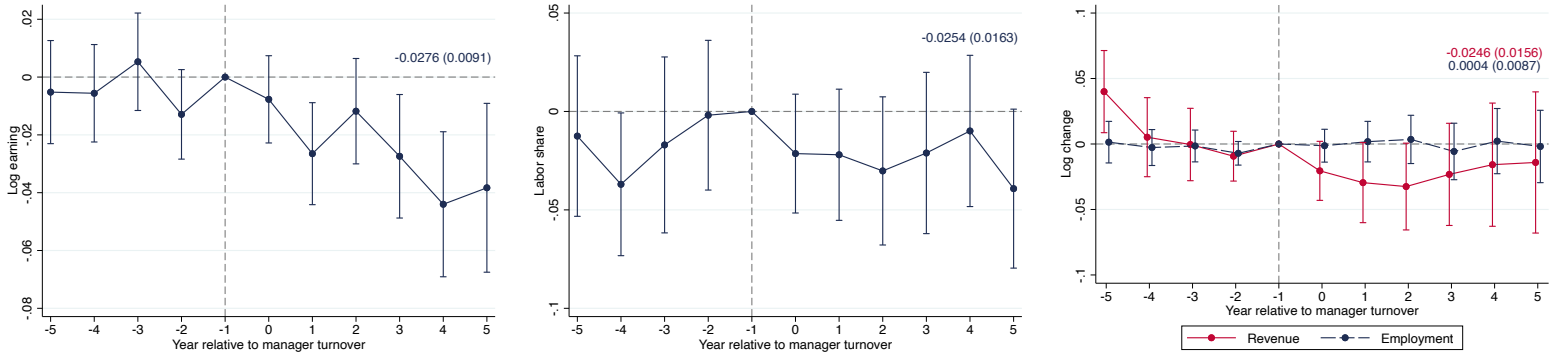
(b) Labor share

(c) Revenue and employment

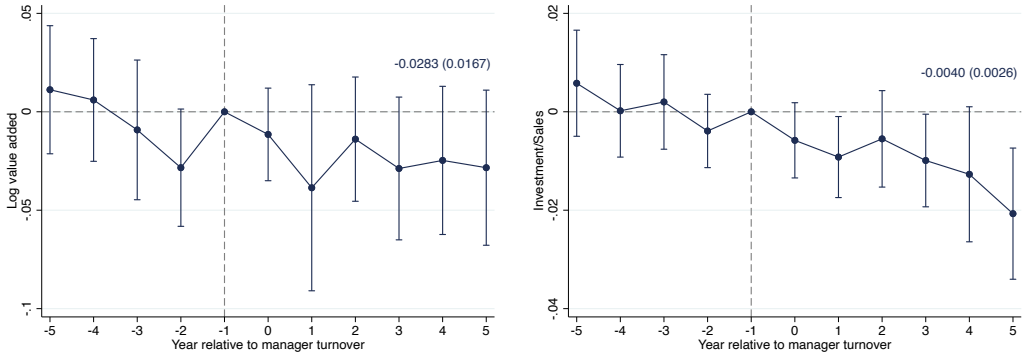
(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. We only keep firms with full observations in each year from 5 years before the manager transition to 5 years after the manager transition. All specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A6: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions in Denmark Reweighting by Firm Size



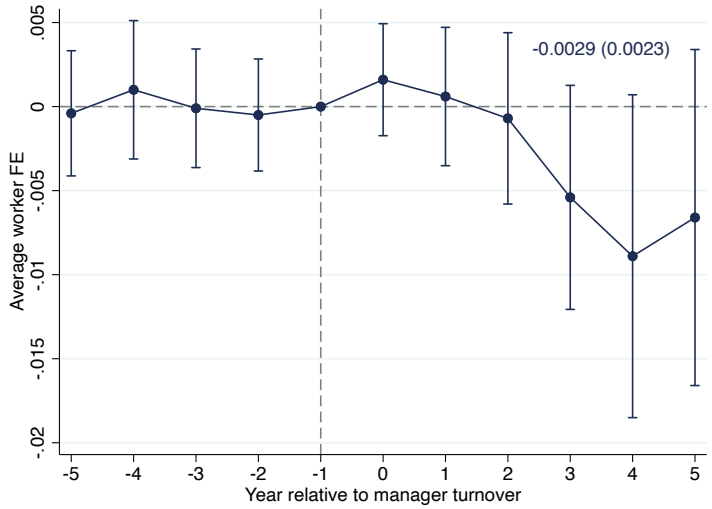
(a) Worker earnings (b) Labor share (c) Revenue and employment



(d) Value added (e) Investment

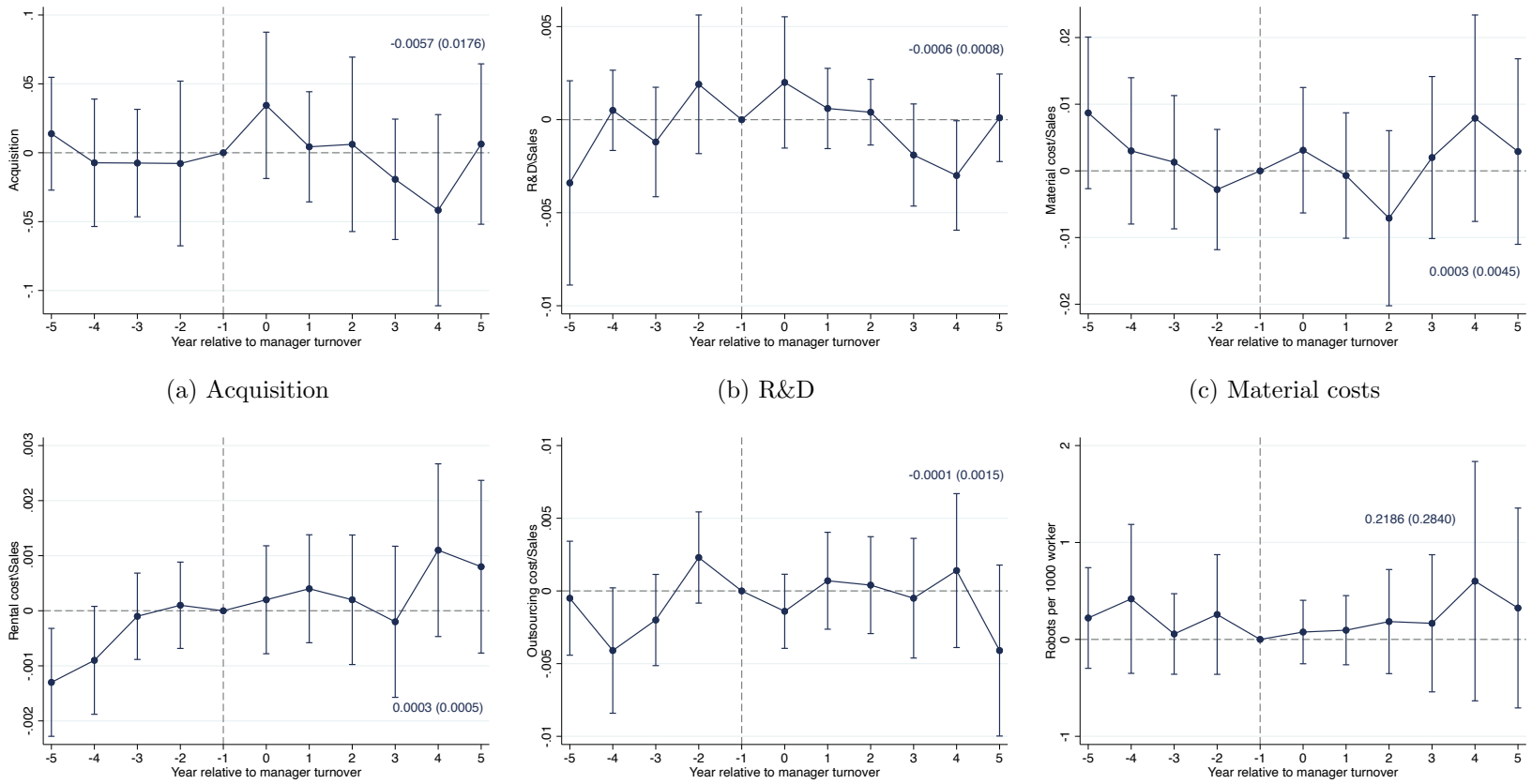
This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. All specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects. Observations are weighted by firm employment multiplied by firm-size-bin-level weights. For each year, we calculate 20 equal-sized bins based on the employment distribution of publicly-listed US firms, and apply bin-level weights equal to the fraction of Danish firms in a bin divided by the fraction of publicly-listed US firms in that bin. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A7: Changes in Worker Quality around Non-Business to Business Manager Transitions in Denmark



This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The dependent variable is the firm's average worker fixed effects, which are estimated from two-way-fixed-effect regressions with worker fixed effects and establishment fixed effects and log annual wage as the dependent variable. The regression controls for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by firm employment. we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure A8: Changes in Other Firm Outcomes around Non-Business to Business Manager Transitions in Denmark

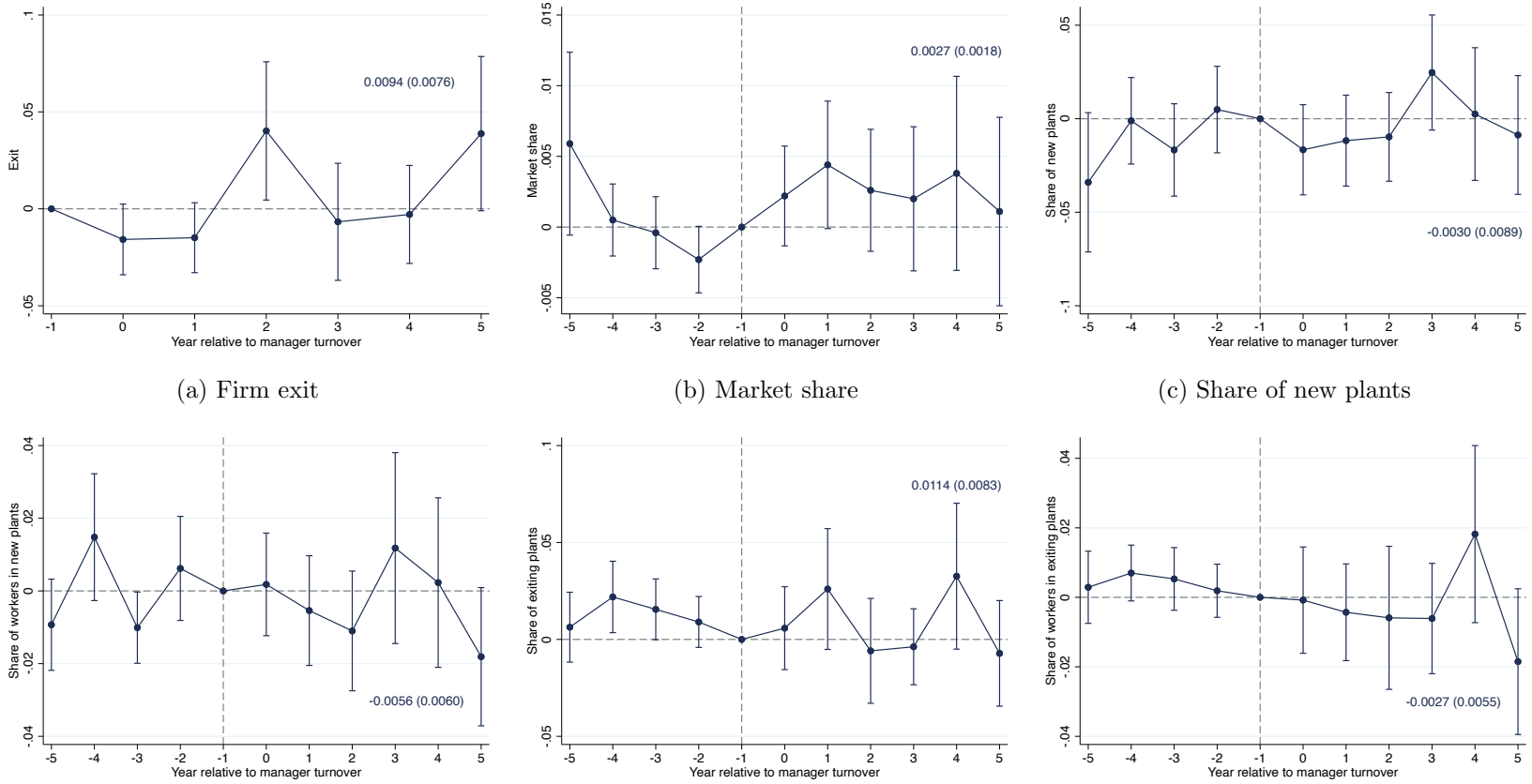


(a) Acquisition (b) R&D (c) Material costs

(d) Rental costs (e) Outsourcing costs (f) Robot purchase

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. All specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are a dummy variable for acquisition in Panel (a), R&D expenses divided by sales in Panel (b), material costs divided by sales in Panel (c), rental costs divided by sales in Panel (d), outsourcing costs divided by sales in Panel (e), and the number of robots per thousand workers in Panel (f). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A9: Changes in Entry and Exit around Non-Business to Business Manager Transitions in Denmark

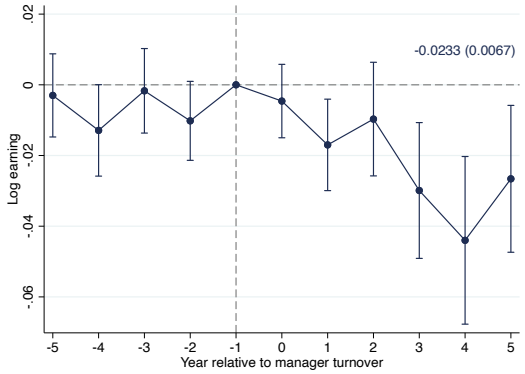


(a) Firm exit (b) Market share (c) Share of new plants

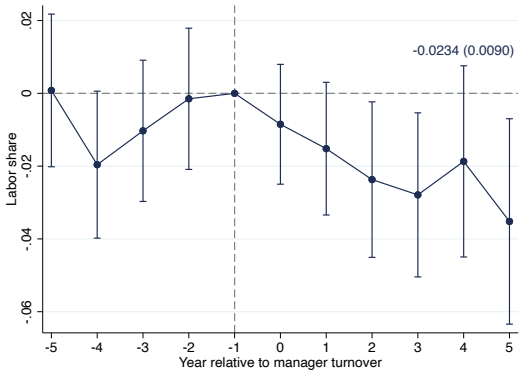
(d) Share of workers in new plants (e) Share of exiting plants (f) Share of workers in exiting plants

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. All specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are a dummy variable for firm exit in Panel (a), market share in the industry in Panel (b), the share of new plants in Panel (c), the share of workers in new plants in Panel (d), the share of plants exiting next year in Panel (e), and the share of workers in plants exiting next year by sales in Panel (f). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

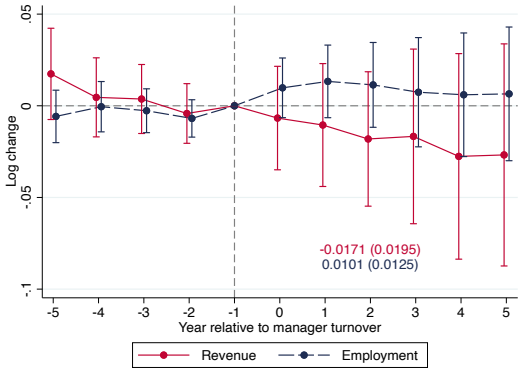
Figure A10: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions in Denmark Controlling for Firm Age×Year Fixed Effects



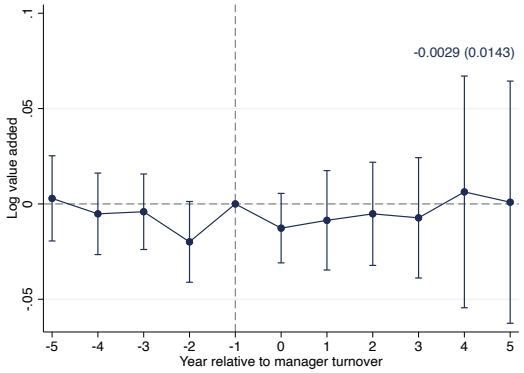
(a) Worker earnings



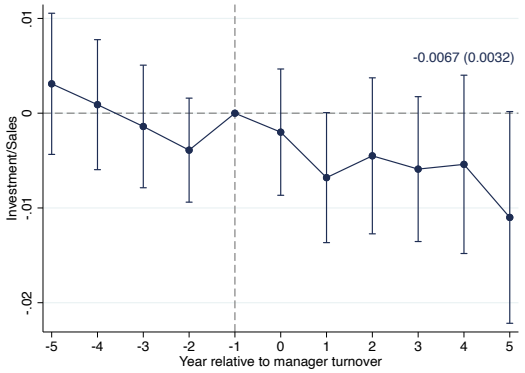
(b) Labor share



(c) Revenue and employment



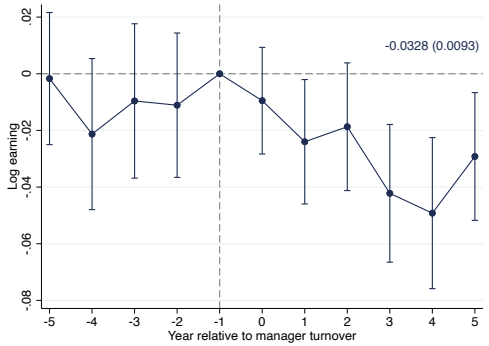
(d) Value added



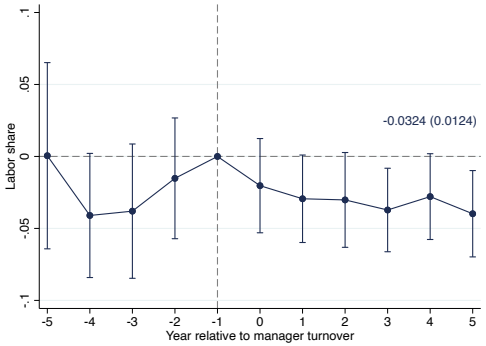
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All specifications include firm fixed effects, firm age×year fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

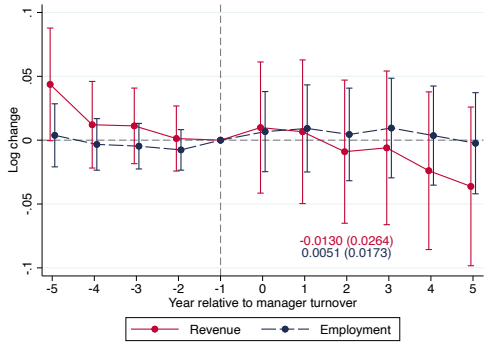
Figure A11: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions for a Balanced Panel of Firms in Denmark



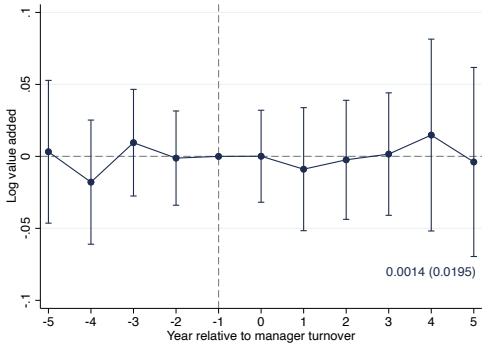
(a) Worker earnings



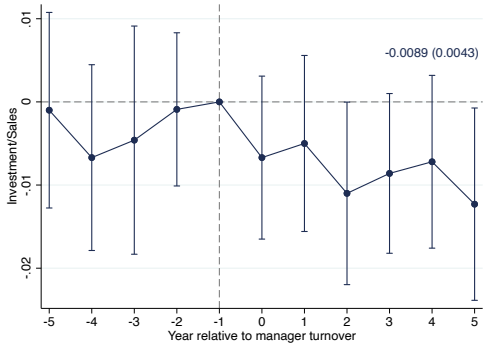
(b) Labor share



(c) Revenue and employment



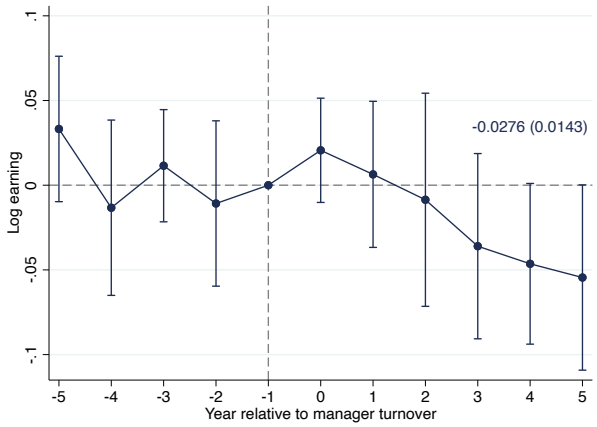
(d) Value added



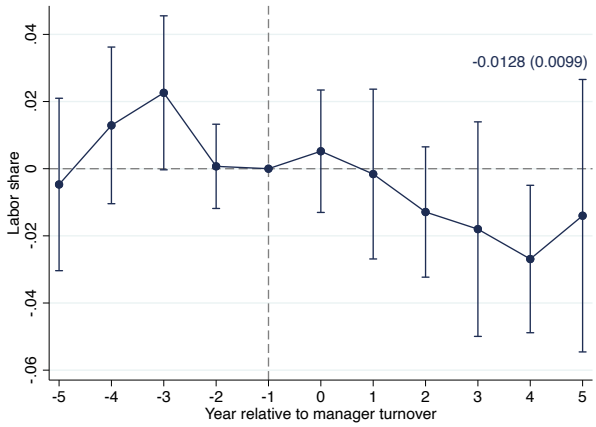
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. We only keep firms with full observations in each year from the manager transition to 5 years after the manager transition. All specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

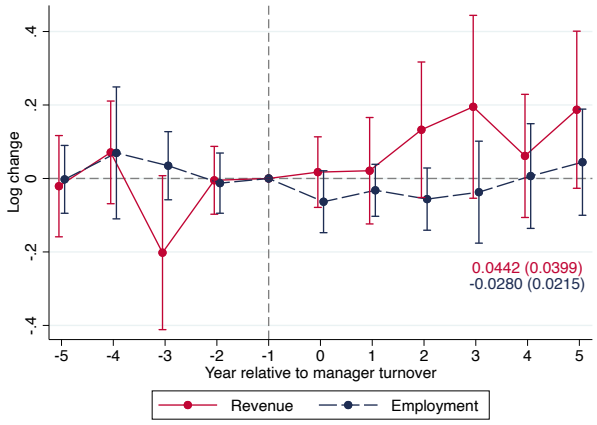
Figure A12: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Using the Last-Treated Firms as Control in the US



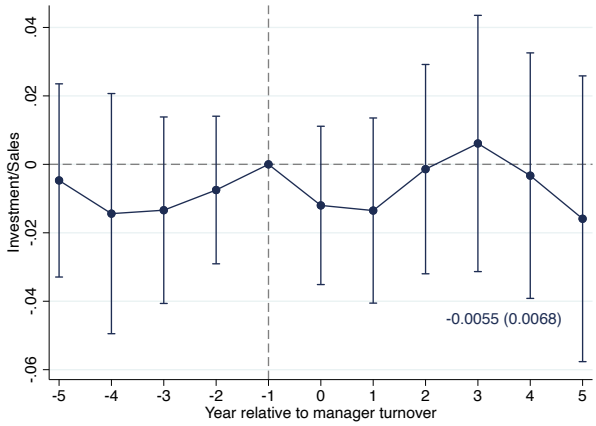
(a) Worker earnings



(b) Labor share



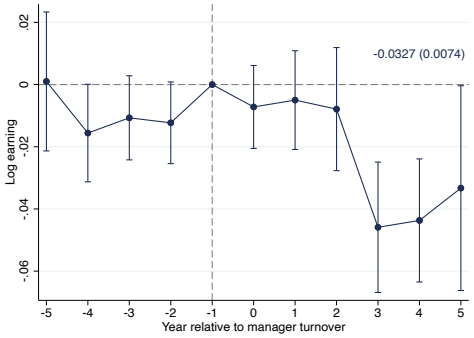
(c) Revenue and employment



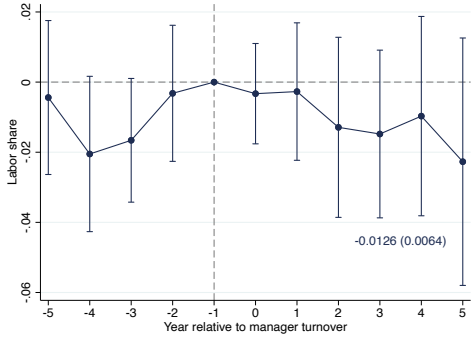
(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have a non-business to business manager transition event during the sample period, and firms that have the non-business to business manager transition event in the last year of the sample period are used as the control group. All specifications include firm fixed effects and year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

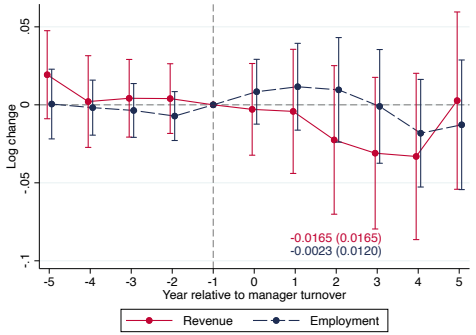
Figure A13: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Using the Last-Treated Firms as Control in Denmark



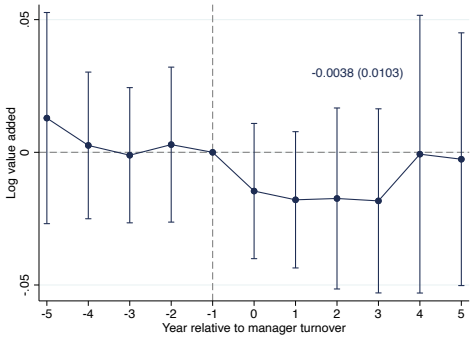
(a) Worker earnings



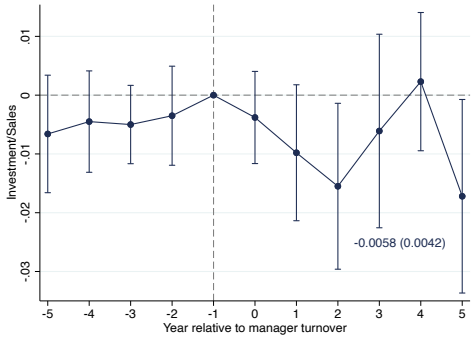
(b) Labor share



(c) Revenue and employment



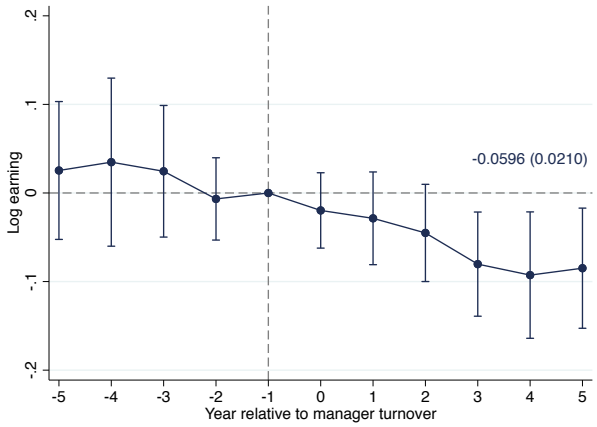
(d) Value added



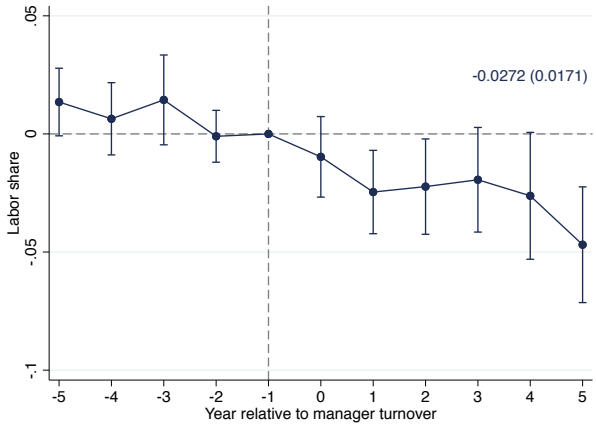
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have a non-business to business manager transition event during the sample period, and firms that have the non-business to business manager transition event in the last year of the sample period are used as the control group. All specifications include firm fixed effects and year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

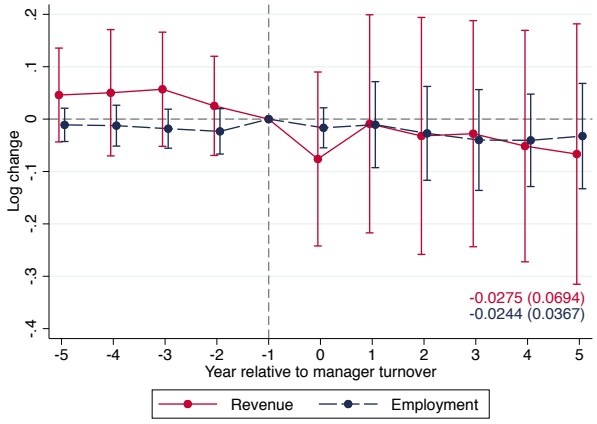
Figure A14: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Using Propensity Score Matching in the US



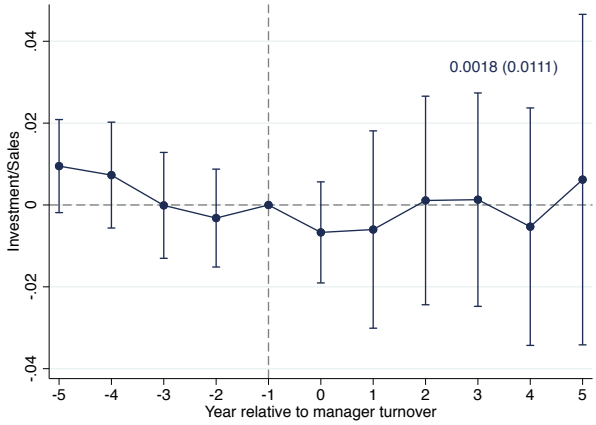
(a) Worker earnings



(b) Labor share



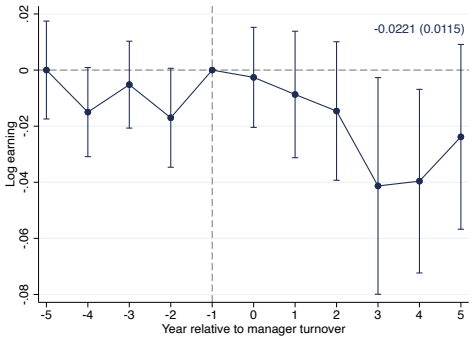
(c) Revenue and employment



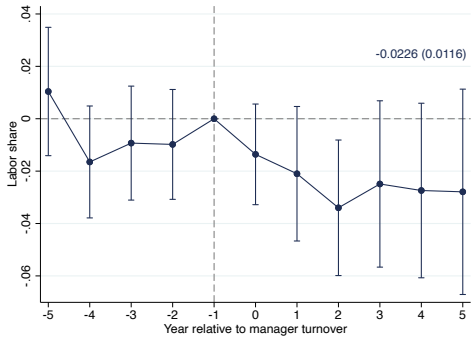
(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have a non-business to business manager transition event during the sample period and their matched control firms. Each treated firm is matched to a control firm in the same industry, state, and size quintile that has the closest propensity score. All specifications include firm fixed effects and year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

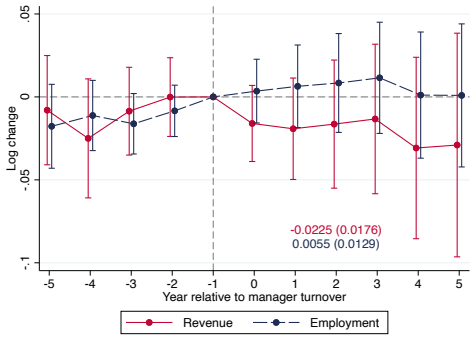
Figure A15: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Using Propensity Score Matching in Denmark



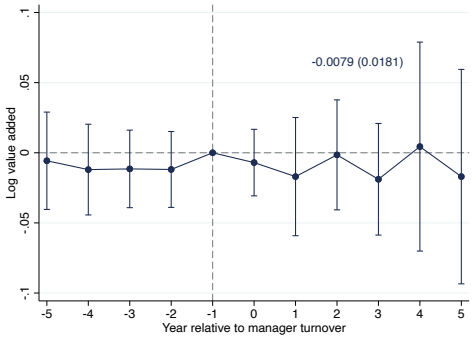
(a) Worker earnings



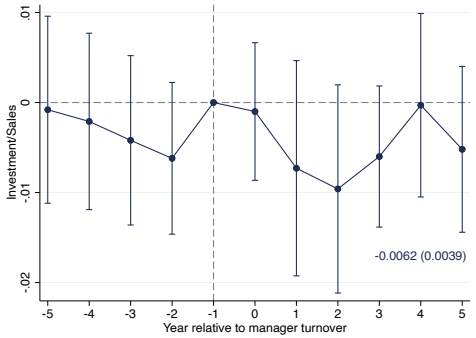
(b) Labor share



(c) Revenue and employment



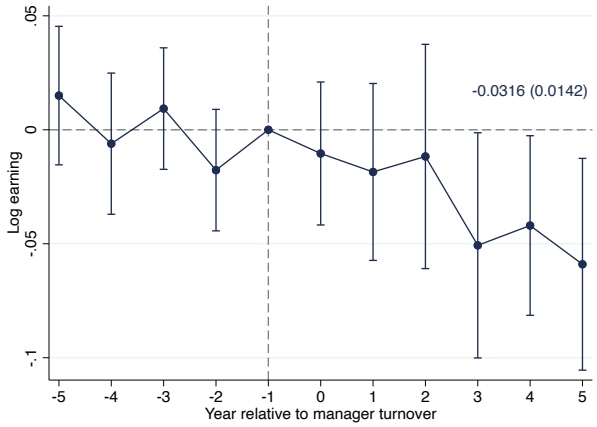
(d) Value added



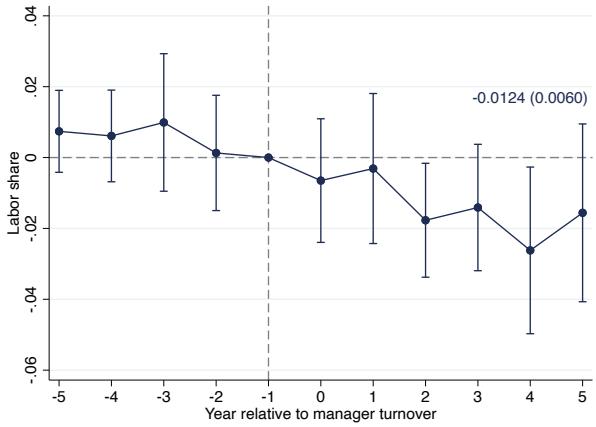
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have a non-business to business manager transition event during the sample period and their matched control firms. Each treated firm is matched to a control firm in the same industry, region, and size quintile that has the closest propensity score. All specifications include firm fixed effects and year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

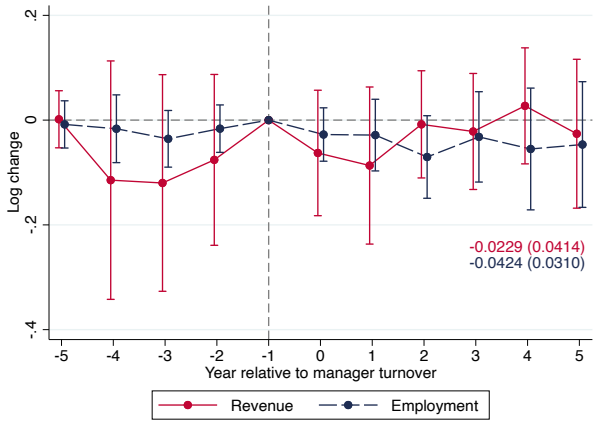
Figure A16: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Controlling for Earnings Per Worker Quintile Fixed Effects in the US



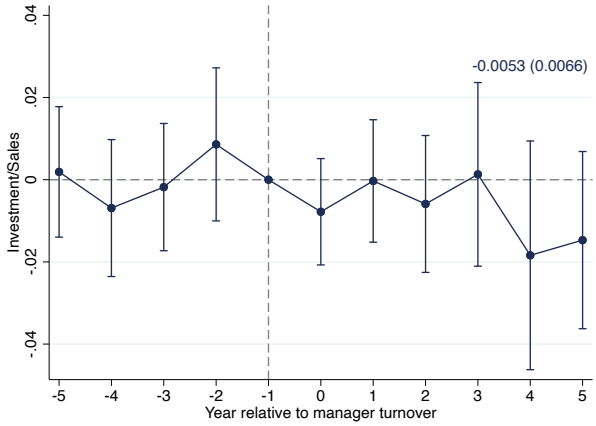
(a) Worker earnings



(b) Labor share



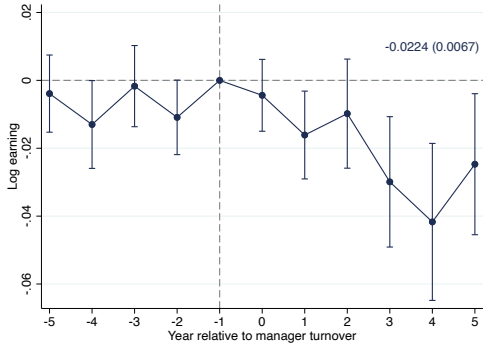
(c) Revenue and employment



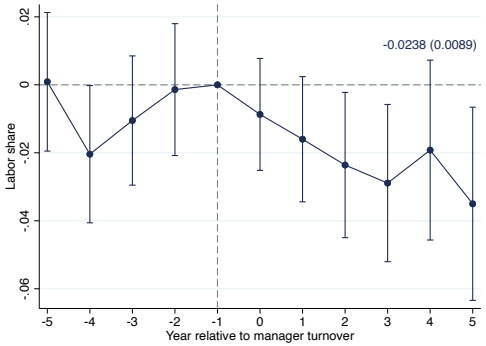
(d) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, firm size quintile by year fixed effects, and earnings per worker quintile by year fixed effects. Observations are weighted by employment. The dependent variables are log earnings per worker and log earnings of staying workers in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. For log earnings of staying workers in Panel (a), we use the matching estimator is described in Section 3. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

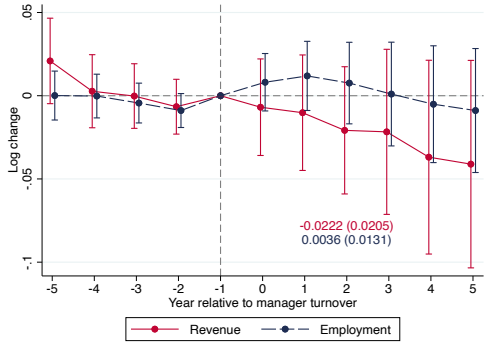
Figure A17: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions Controlling for Earnings Per Worker Quintile Fixed Effects in Denmark



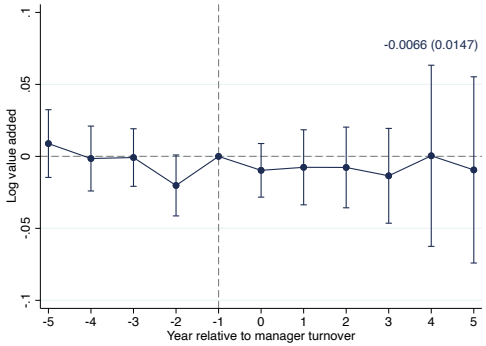
(a) Worker earnings



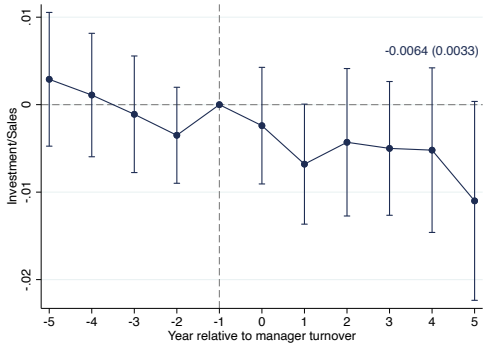
(b) Labor share



(c) Revenue and employment



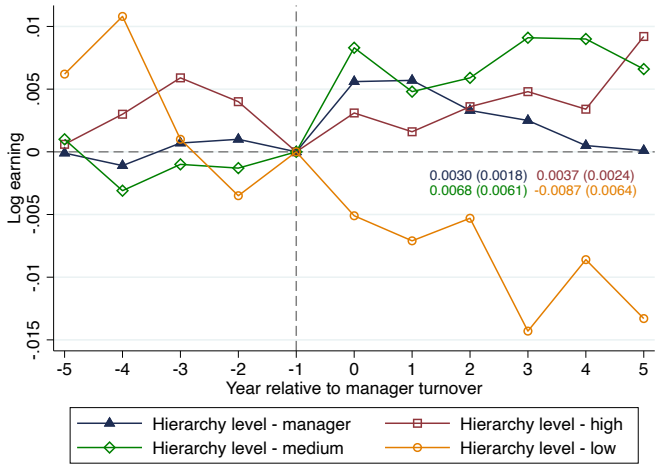
(d) Value added



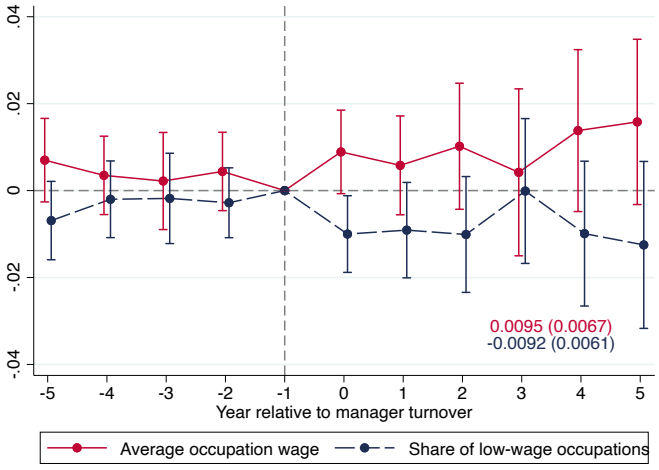
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, firm size quintile by year fixed effects, and earnings per worker quintile by year fixed effects. Observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. For log earnings of staying workers in Panel (a), we use the matching estimator is described in Section 3. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A18: Changes in Organization and Occupation Structure around Non-Business to Business Manager Transitions in Denmark



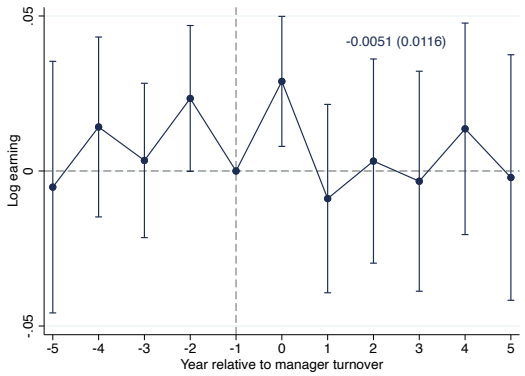
(a) Hierarchy levels



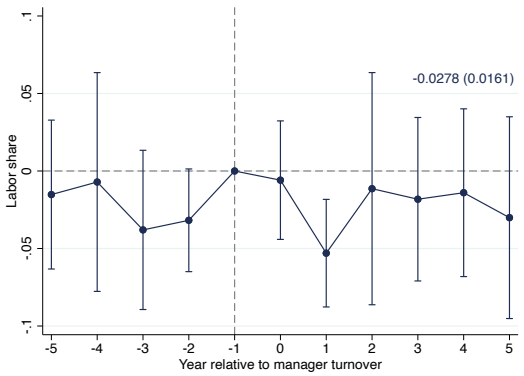
(b) Occupation average wage and share of low-skilled occupations

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. In Panel (a), the dependent variables are the share of workers in each hierarchy level (manager, high, medium, and low). In Panel (b), the dependent variables are mean occupation-level average wage and the share of workers in low-wage occupations (occupations in the lowest quartile of the wage distribution). All regressions include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

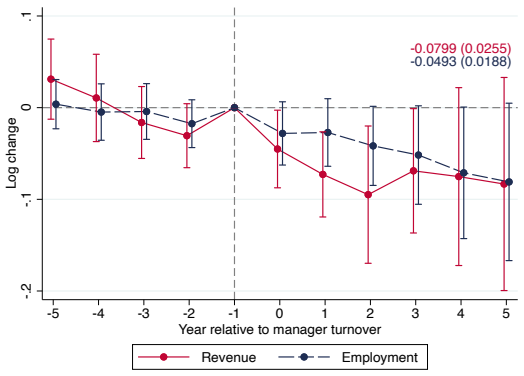
Figure A19: Changes in Firm and Worker Outcomes around Non-Economics to Economics Manager Transitions in Denmark



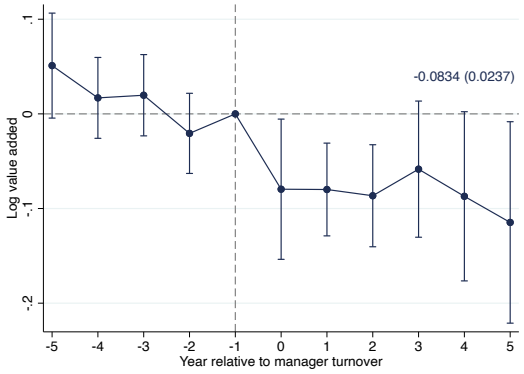
(a) Worker earnings



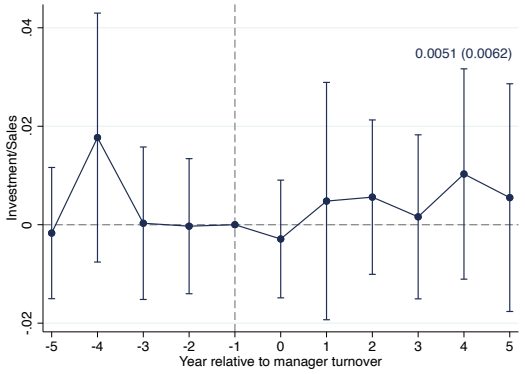
(b) Labor share



(c) Revenue and employment



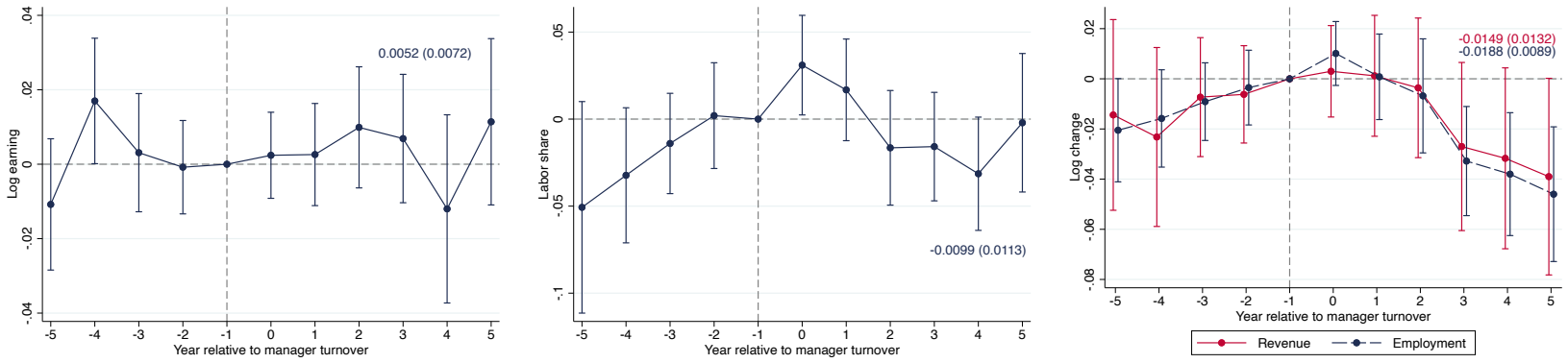
(d) Value added



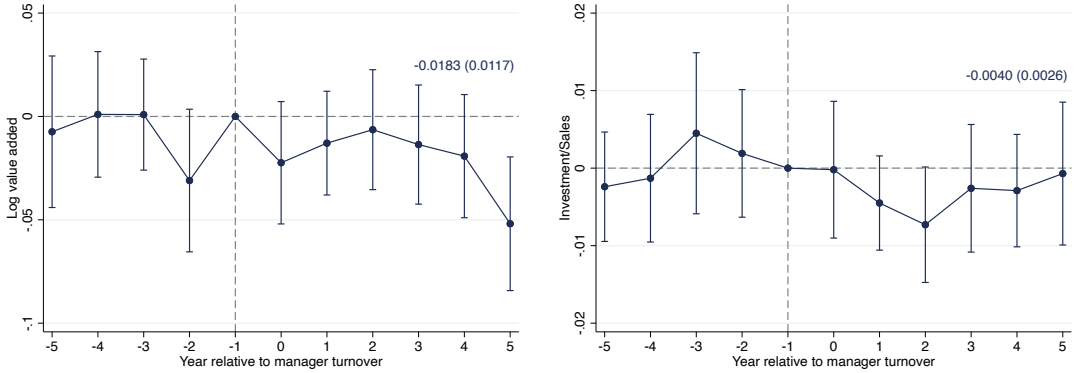
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a manager without an economics degree to a manager with an economics degree in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A20: Changes in Firm and Worker Outcomes around Non-Engineering to Engineering Manager Transitions in Denmark



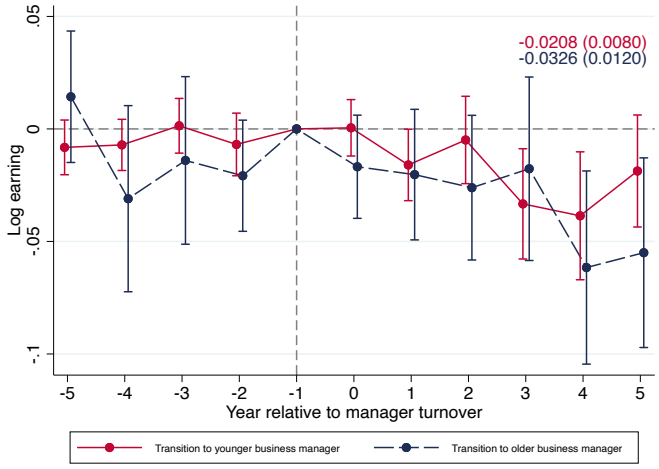
(a) Worker earnings (b) Labor share (c) Revenue and employment



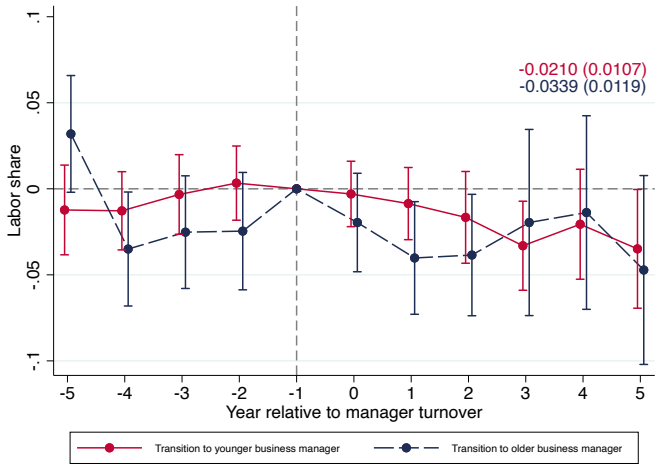
(d) Value added (e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a manager without an engineering degree to a manager with an engineering degree in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A21: Changes in Worker Earnings and the Labor Share around Non-Business to Younger or Older Business Manager Transitions in Denmark



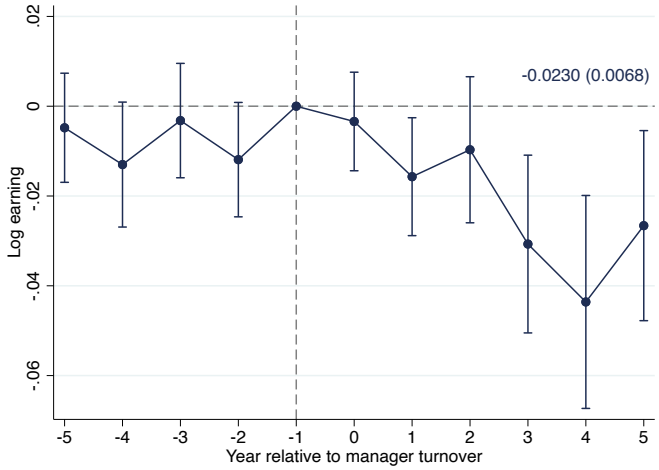
(a) Worker earnings



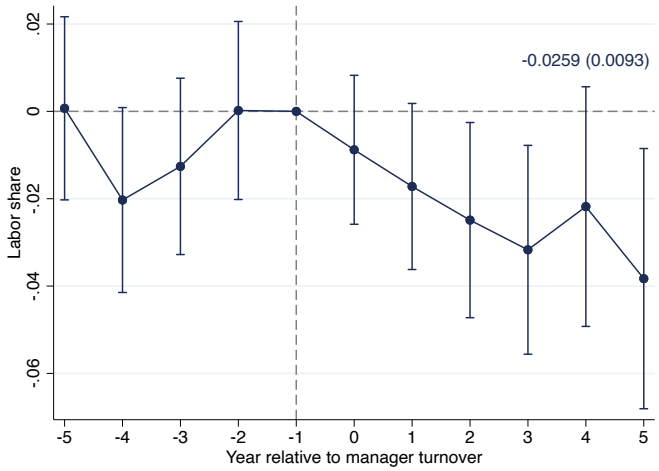
(b) Labor share

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a younger business manager (solid lines) or an older business manager (dashed lines) in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A22: Changes in Worker Earnings and the Labor Share around Non-Business to Business Manager Transitions Excluding Family CEOs in Denmark



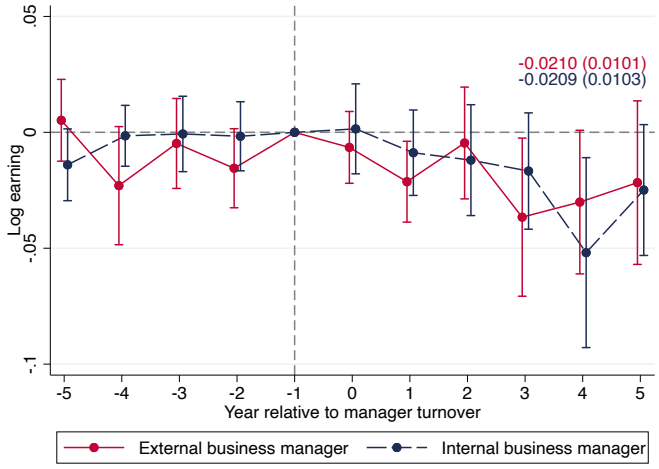
(a) Worker earnings



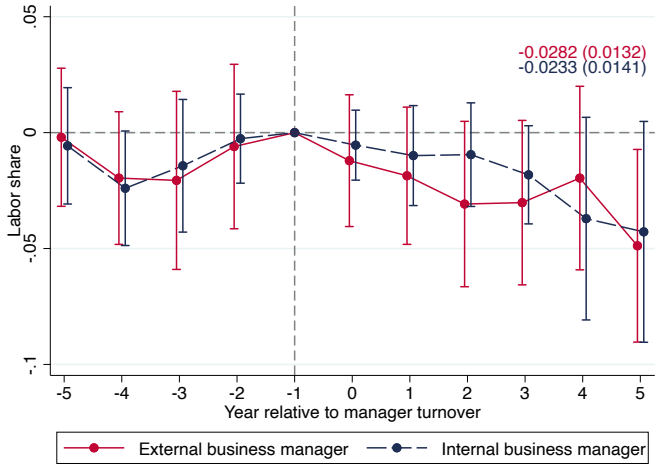
(b) Labor share

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. We exclude manager transitions involving family CEOs, who are related by blood or marriage to the previous or successor CEOs at their firms. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A23: Changes in Worker Earnings and the Labor Share around Non-Business to External or Internal Business Manager Transitions in Denmark



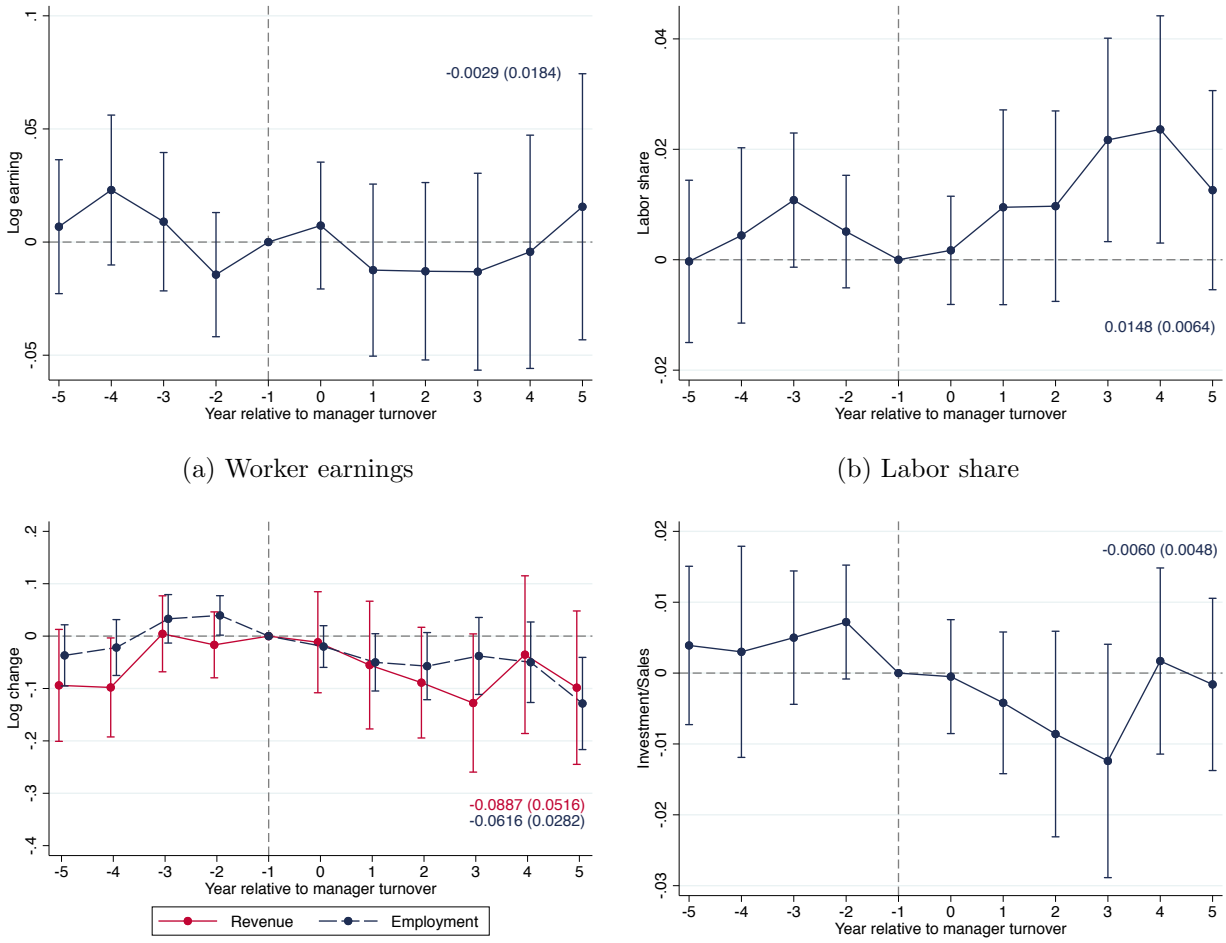
(a) Worker earnings



(b) Labor share

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to an externally-hired business manager (solid lines) or an internally-promoted business manager (dashed lines) in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A24: Changes in Firm and Worker Outcomes around Business to Non-Business Manager Transitions in the US



(a) Worker earnings

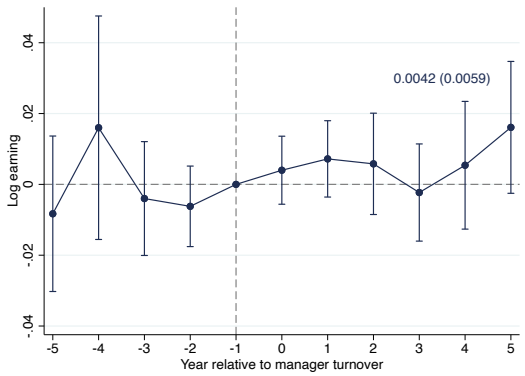
(b) Labor share

(c) Revenue

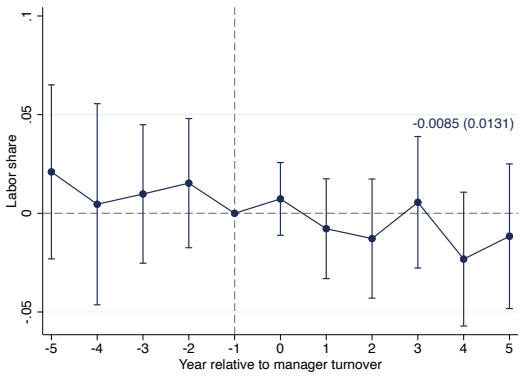
(d) Employment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a business manager to a non-business manager in the US. The sample includes firms that have business managers in all years, and firms that have a business to non-business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), and investment divided by sales in Panel (d). The labor share is defined as total wage bill divided by sales. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

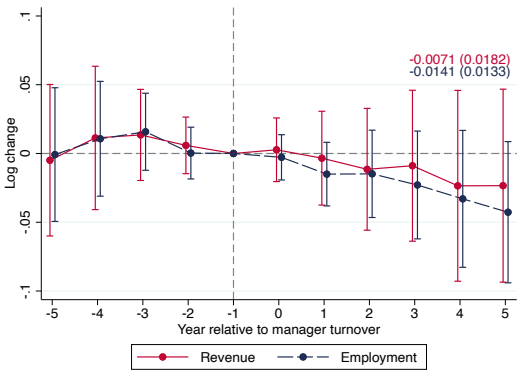
Figure A25: Changes in Firm and Worker Outcomes around Business to Non-Business Manager Transitions in Denmark



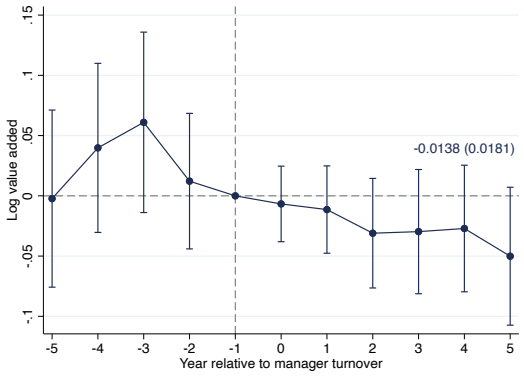
(a) Worker earnings



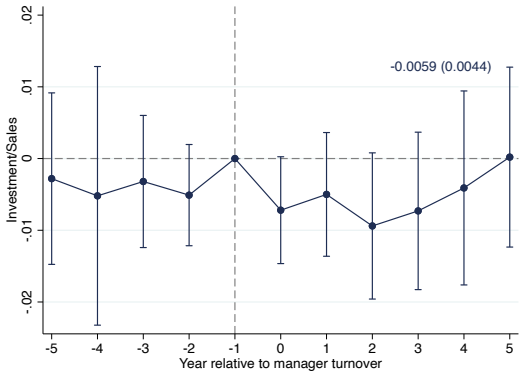
(b) Labor share



(c) Revenue and employment



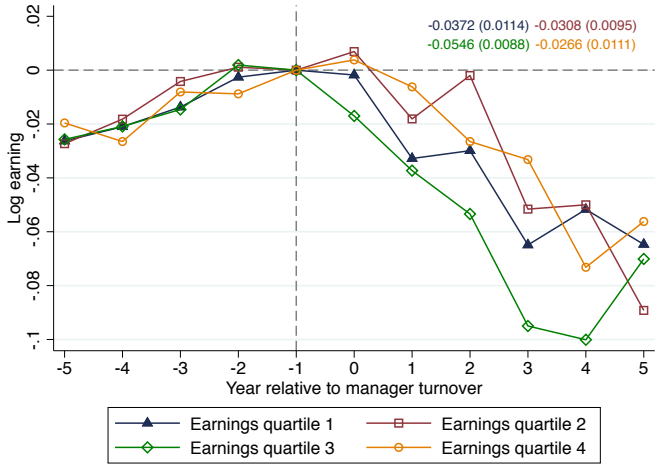
(d) Value added



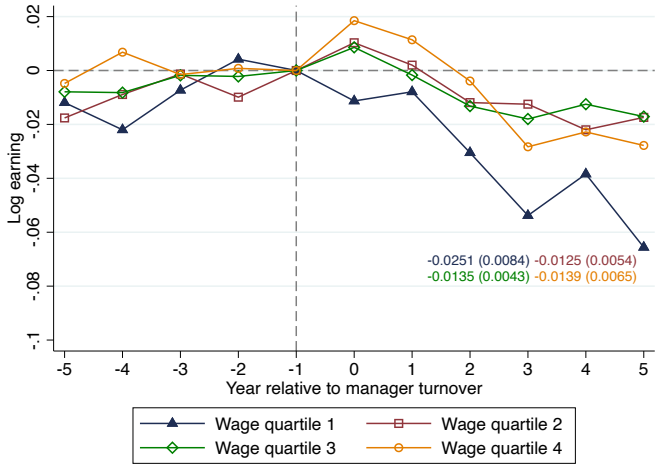
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a business manager to a non-business manager in Denmark. The sample includes firms that have business managers in all years, and firms that have a business to non-business manager transition event during the sample period. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A26: Heterogeneous Effects of Business Managers on Worker Earnings



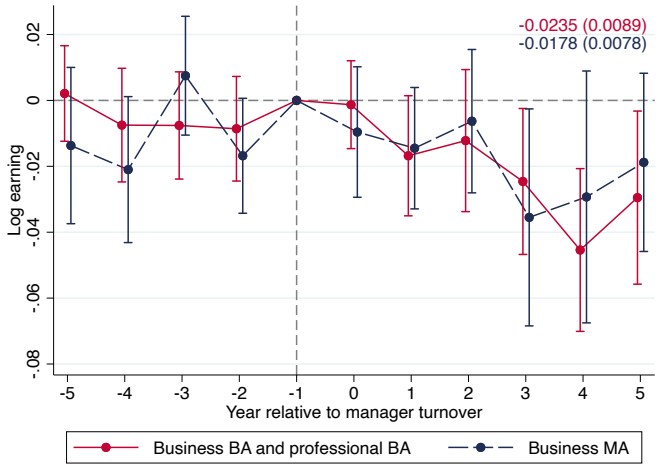
(a) US



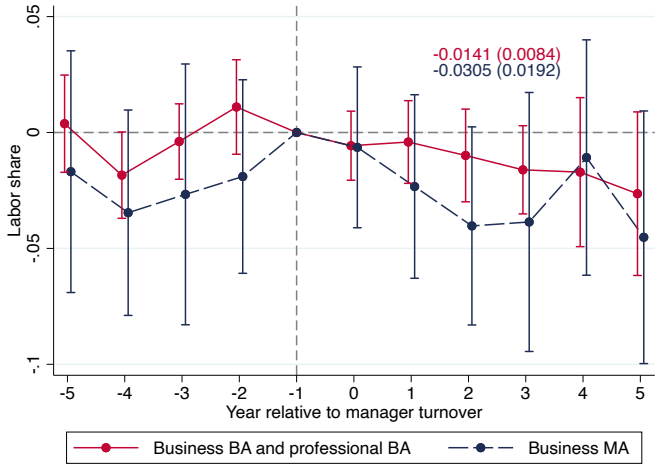
(b) Denmark

This figure plots event-study estimates and 95% confidence intervals separately by different worker groups, where events are transitions from a non-business manager to a business manager. The dependent variable is log annual earnings. The estimates are based on the matching estimator described in Section 3. In Panel (a), workers are divided into four groups based on their annualized earnings in year -1. In Panel (b), workers are divided into four groups based on their hourly wage in year -1. For each group, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal.

Figure A27: Heterogeneous Effects by the Degree Level of Business Managers in Denmark



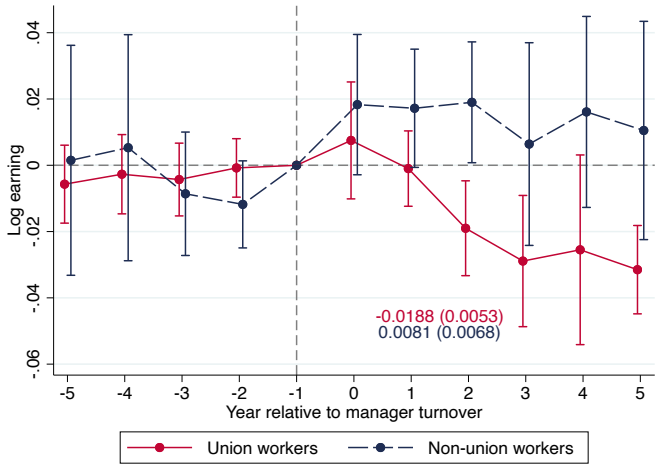
(a) Worker earnings



(b) Labor share

This figure plots event-study estimates and 95% confidence intervals from separate regressions for transitions from a non-business manager to a business manager who has a business BA or professional BA degree but doesn't have a business MA degree (solid line) and transitions from a non-business manager to a business manager with a business MA degree (dashed line) in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure A28: Changes in Wages around Non-Business to Business Manager Transitions for Union and Non-Union Workers in Denmark

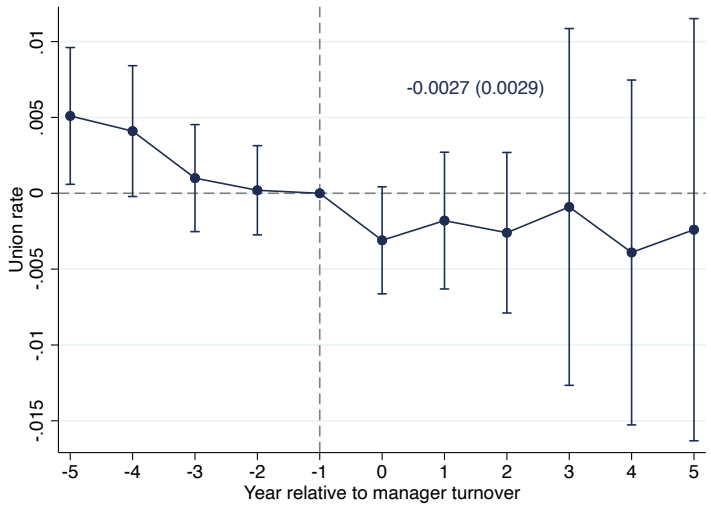


(a) Log annual earnings

(b) Log hourly wage

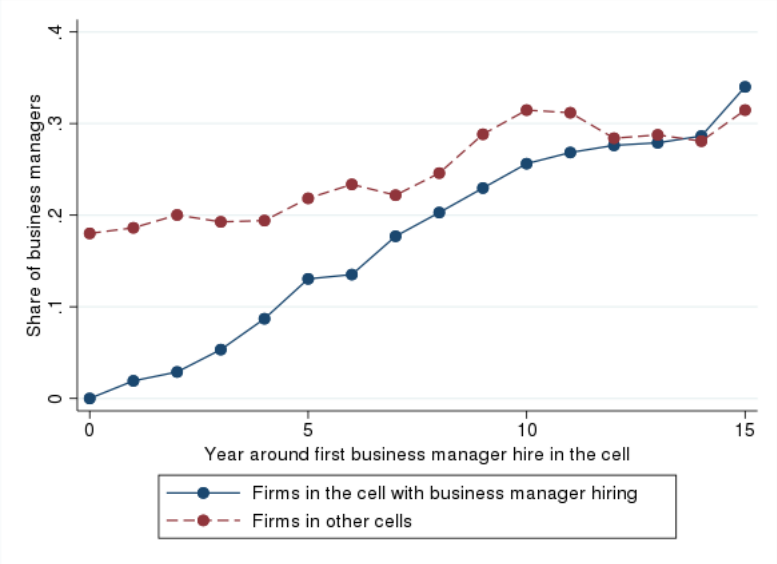
This figure plots event-study estimates and 95% confidence intervals separately for workers who are union members and workers who are not union members, where events are transitions from a non-business manager to a business manager in Denmark. The estimates are based on the matching estimator described in Section 3. In Panel (a), the dependent variable is log annual earnings. In Panel (b), the dependent variable is log hourly wage. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure A29: Changes in Unionization Rates around Non-Business to Business Manager Transitions in Denmark

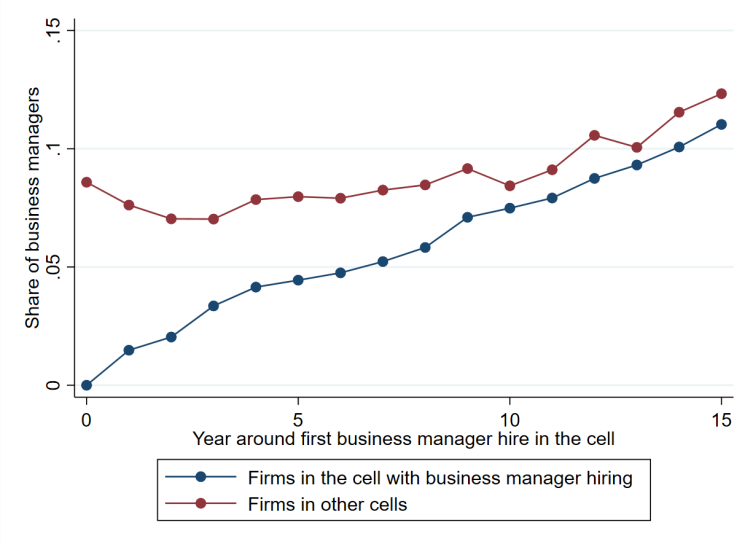


This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The regression controls for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and observations are weighted by firm employment. The dependent variable is the share of workers at the firm who are union members. we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

Figure A30: Visual Representation of the First Stage for Business Manager Transitions



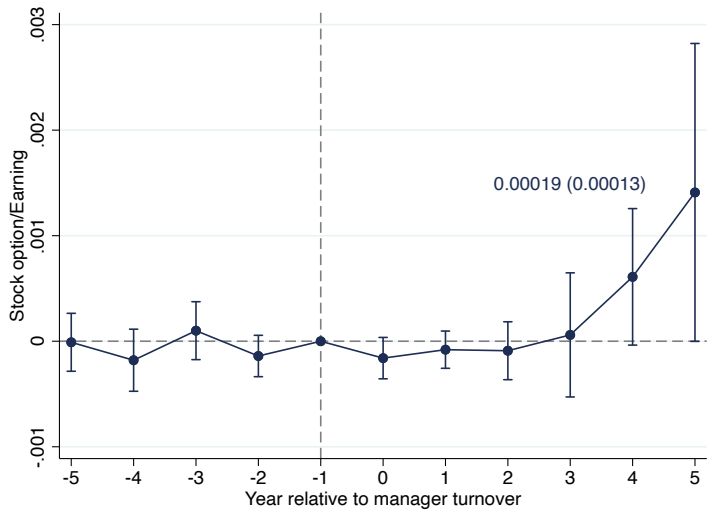
(a) US



(b) Denmark

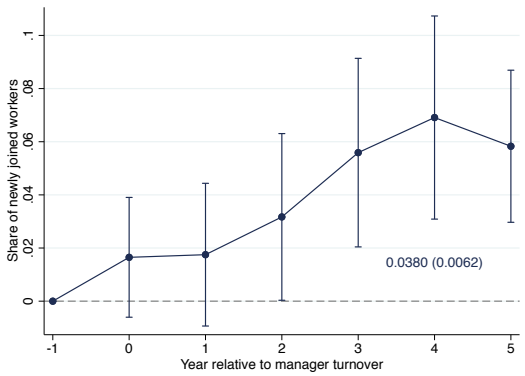
This figure plots the average fraction of firms in a region-industry-size cell with no past business manager (blue), where zero is the year of first business manager hiring in the cell. For comparison, we also plot average fraction of firms in other cells (red) in the same year. Panel (a) is for the US and Panel (b) for Denmark. See Appendix A.1 for further details.

Figure A31: Changes in Workers' Stock Options around Non-Business to Business Manager Transitions in Denmark

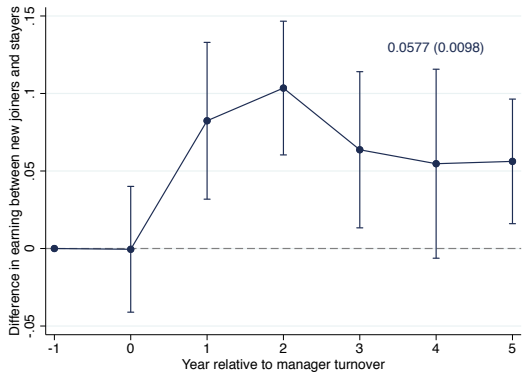


This figure plots event-study estimates and 95% confidence intervals of workers' stock options, where events are manager transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. The dependent variable is the value of stock option payments received by non-manager workers divided by the total earnings of non-manager workers. All regressions include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. Standard errors are clustered at the firm level.

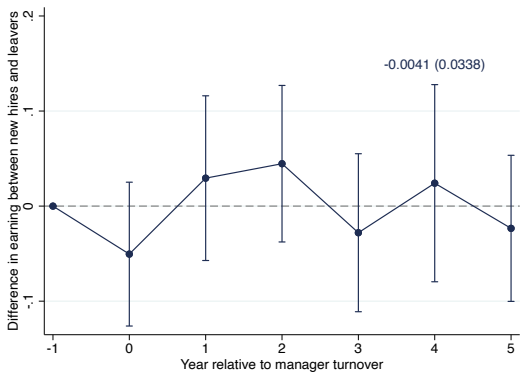
Figure A32: Changes in Worker Composition around Non-Business to Business Manager Transitions in the US



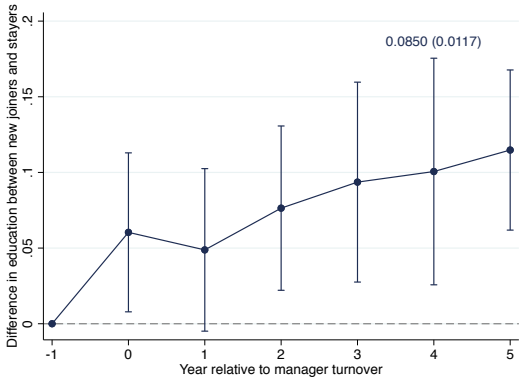
(a) Share of newly joined workers



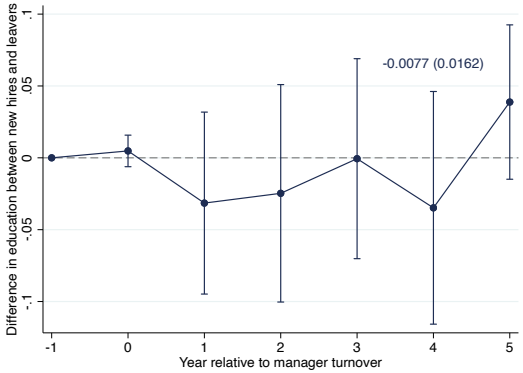
(b) Joiners' earnings - stayers' earnings



(c) New hires' earnings - leavers' earnings



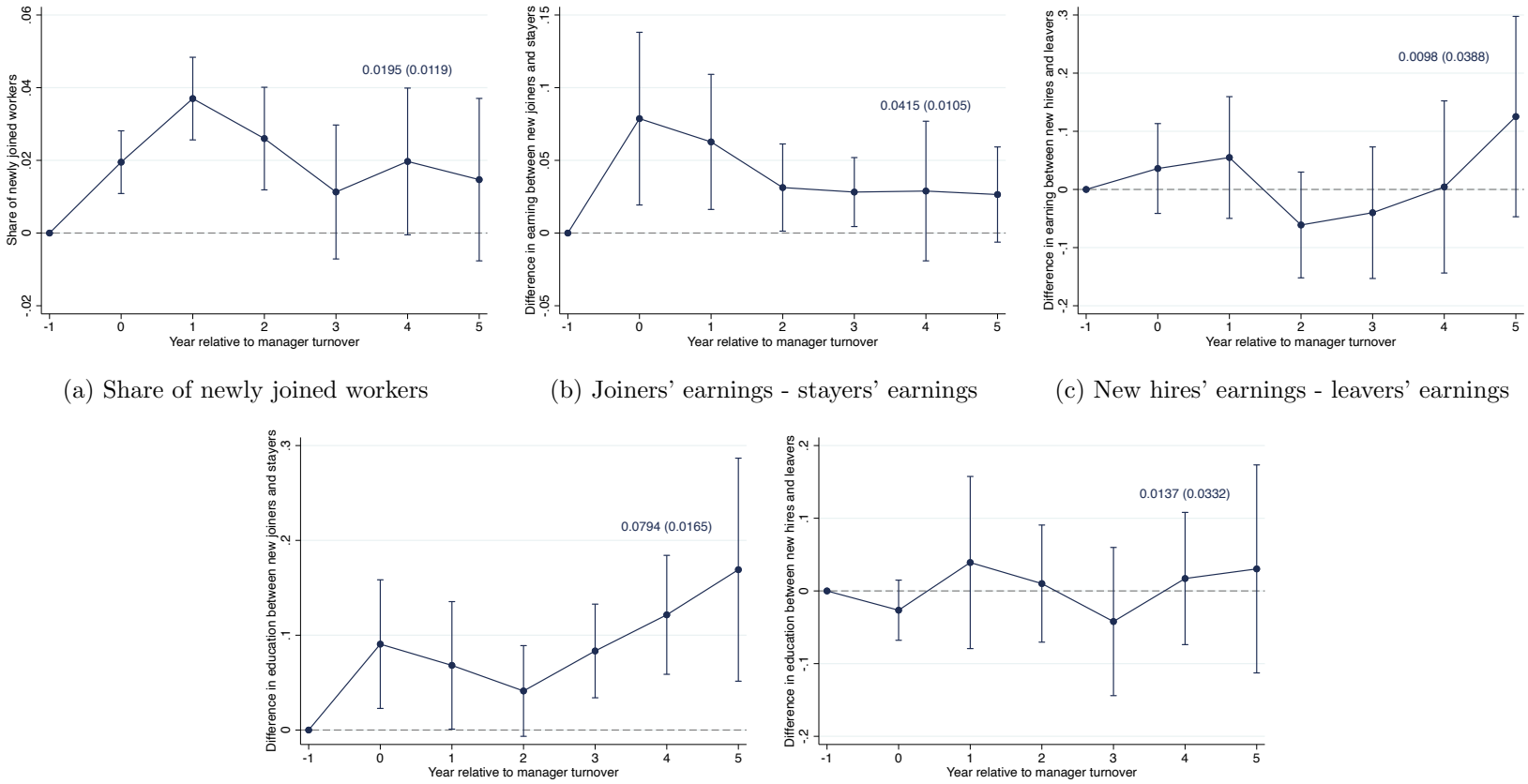
(d) Joiners' education - stayers' education



(e) New hires' education - leavers' education

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in the US. The estimates are based on the matching estimator described in Section 3. The dependent variables are the share of workers who join after year -1 in Panel (a), the log average earnings of workers who join after year -1 minus the log average earnings of stayers (workers who join on or before year -1 and remain in the firm until a given year) in Panel (b), the log average earnings of new hires minus the log average earnings of leavers (workers who separate in the following year) in Panel (c), the average education level of workers who join after year -1 minus the average education level of stayers in Panel (d), and the average education level of new hires minus the average education level of leavers in Panel (e). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A33: Changes in Worker Composition around Non-Business to Business Manager Transitions in Denmark



(a) Share of newly joined workers

(b) Joiners' earnings - stayers' earnings

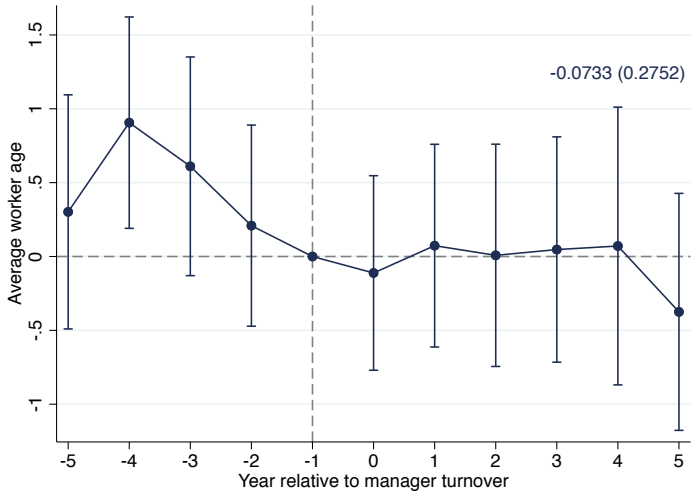
(c) New hires' earnings - leavers' earnings

(d) Joiners' education - stayers' education

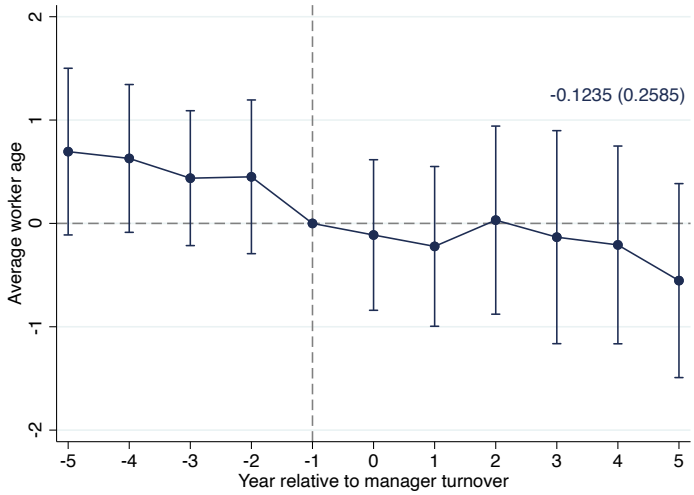
(e) New hires' education - leavers' education

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The estimates are based on the matching estimator described in Section 3. The dependent variables are the share of workers who join after year -1 in Panel (a), the log average earnings of workers who join after year -1 minus the log average earnings of stayers (workers who join on or before year -1 and remain in the firm until a given year) in Panel (b), the log average earnings of new hires minus the log average earnings of leavers (workers who separate in the following year) in Panel (c), the average education level of workers who join after year -1 minus the average education level of stayers in Panel (d), and the average education level of new hires minus the average education level of leavers in Panel (e). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A34: Changes in Worker Age around Non-Business to Business Manager Transitions



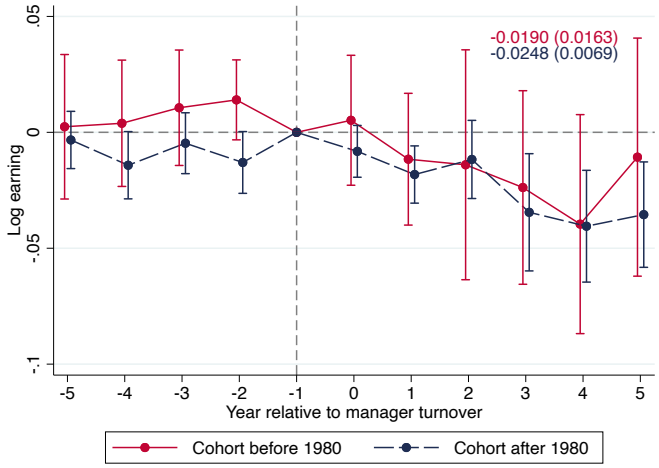
(a) US



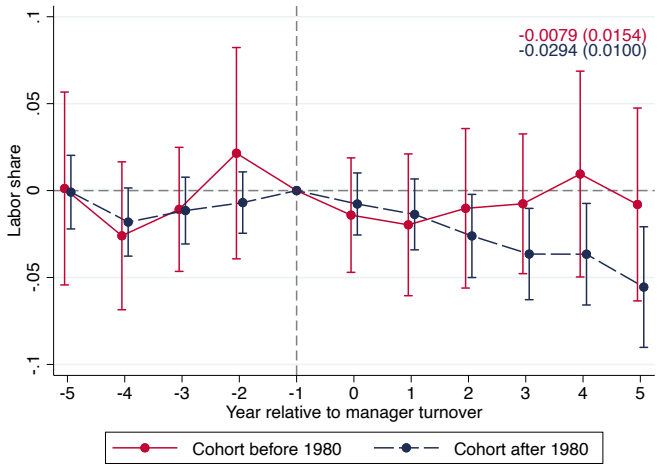
(b) Denmark

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager. Panel (a) is based on US data and Panel (b) is based on Danish data. The dependent variable is the average worker age. The sample includes firms that have non-business managers in all years, and firms that have a non-business to business manager transition event during the sample period. All firm-level specifications include firm fixed effects, industry×year fixed effects, state(region)×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A35: Changes in Worker Earnings and the Labor Share around Non-Business to Business Manager Transitions for Business Managers in Different Cohorts in Denmark



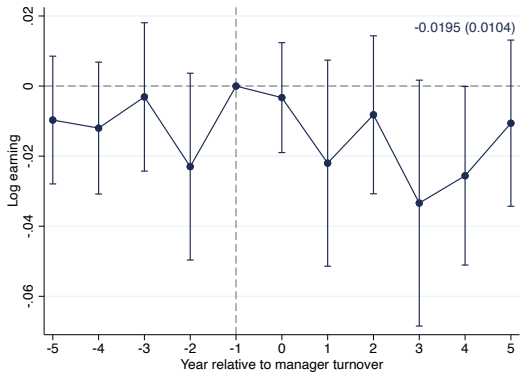
(a) Worker earnings



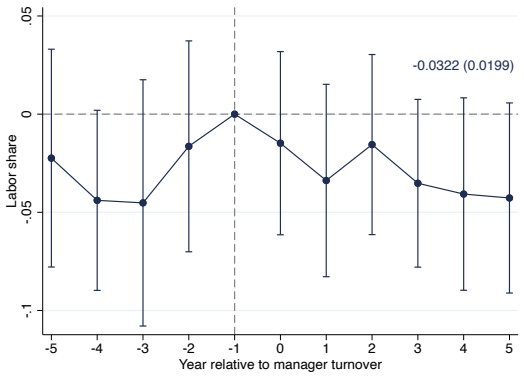
(b) Labor share

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager who gets the business degree before 1980 (solid line) or after 1980 (dashed line) in Denmark. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, and industry×year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

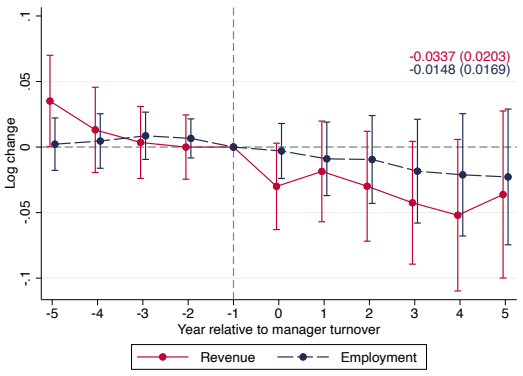
Figure A36: Changes in Firm and Worker Outcomes around Non-Business to Business Manager Transitions among Exporters in Denmark



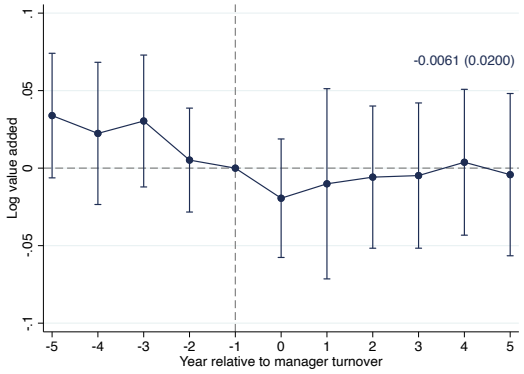
(a) Worker earnings



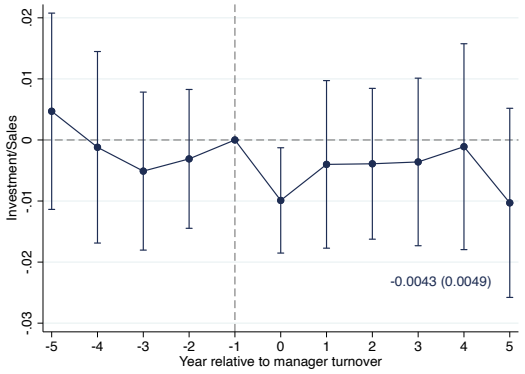
(b) Labor share



(c) Revenue and employment



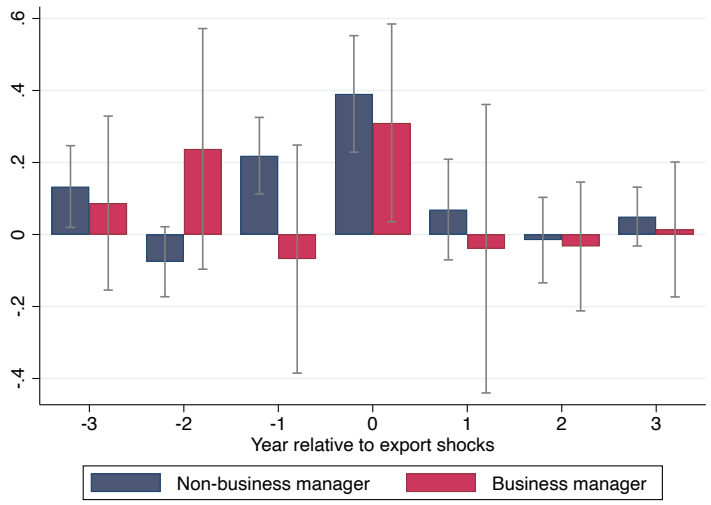
(d) Value added



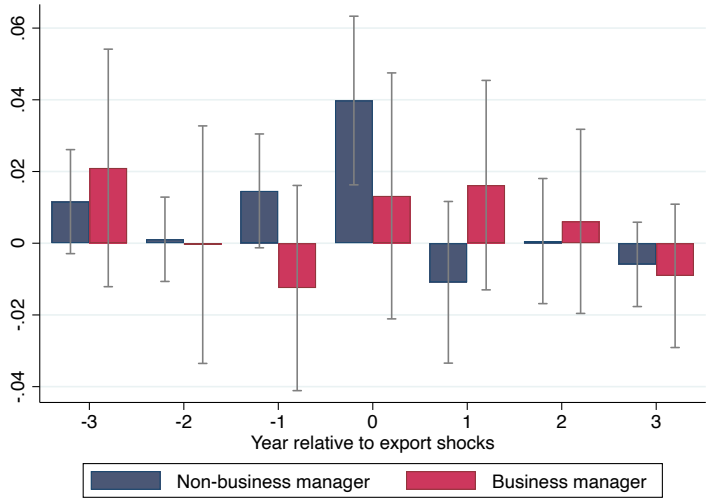
(e) Investment

This figure plots event-study estimates and 95% confidence intervals, where events are transitions from a non-business manager to a business manager in Denmark. The sample includes exporters (firms in the regression sample of Table 4) that have non-business managers in all years, and exporters that have a non-business to business manager transition event during the sample period. All specifications include firm fixed effects, industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects, and observations are weighted by employment. The dependent variables are log earnings per worker in Panel (a), the labor share in Panel (b), log revenue and log employment in Panel (c), log value added in Panel (d), and investment divided by sales in Panel (e). The labor share is defined as total wage bill divided by value added. In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A37: Output and Wage Response to Leads and Lags of Trade Shocks in Denmark



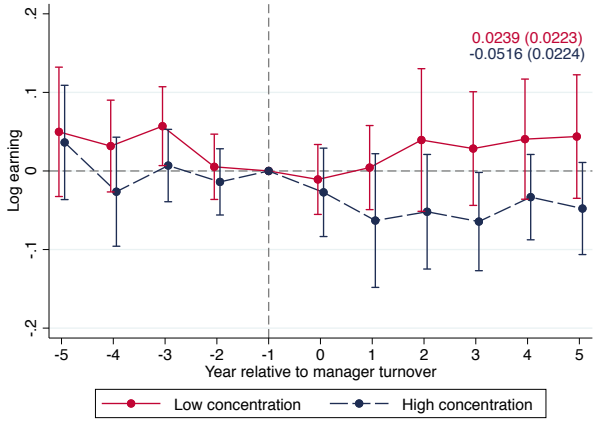
(a) Value added



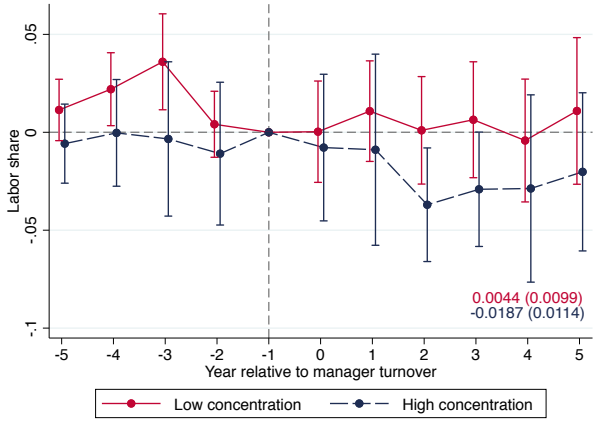
(b) Worker earnings

This figure reports the coefficients and 95% confidence intervals from regressing log value added (in Panel (a)) and log annual earnings (in Panel (b)) on the leads and lags of export shocks up to 3 years interacted with business manager. The blue bars are coefficients for leads and lags of export shocks interacted with business manager dummy, and red bars are coefficients for leads and lags of export shocks interacted with non-business manager dummy. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. Worker-level regressions in Panel (b) additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. In Panel (a), observations are weighted by firm employment. Standard errors are clustered at the firm level.

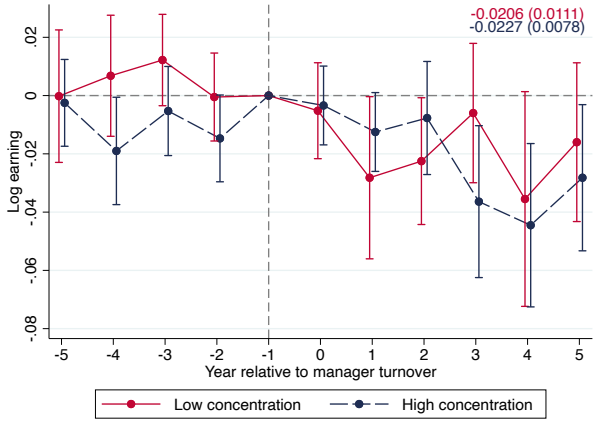
Figure A38: Changes in Worker Earnings and the Labor Share around Non-Business to Business Manager Transitions in High-Concentration and Low-Concentration Industries



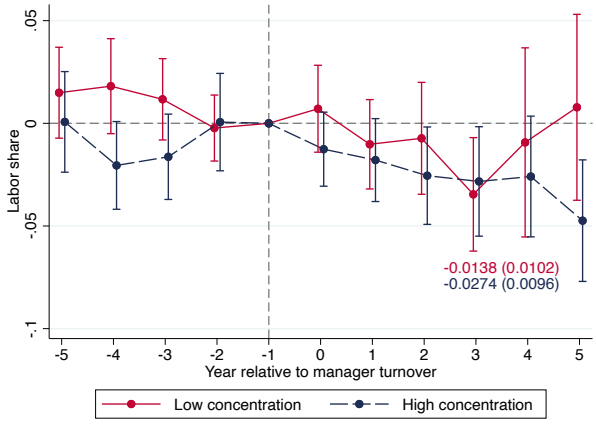
(a) Worker earnings, US



(b) Labor share, US



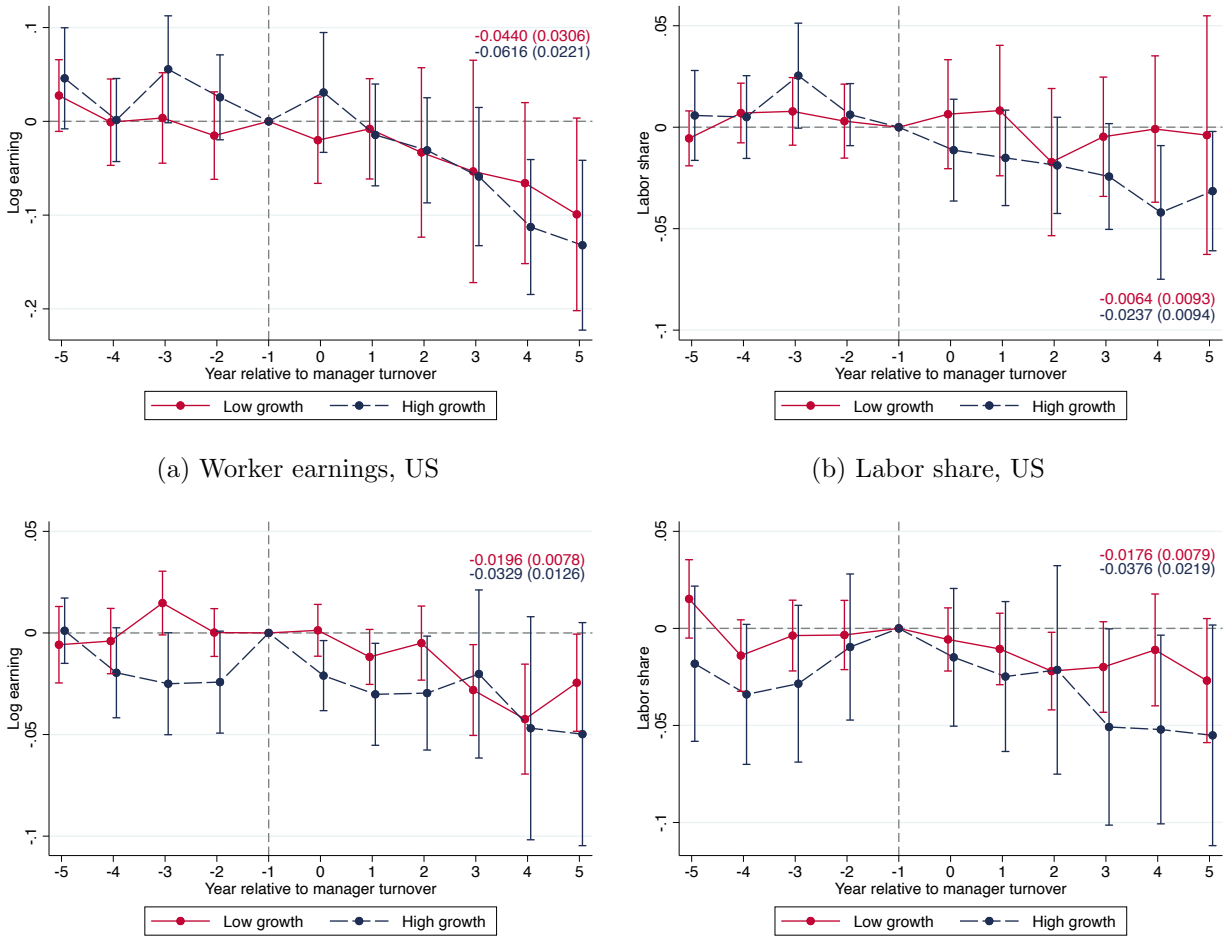
(c) Worker earnings, Denmark



(d) Labor share, Denmark

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. Panels (a) and (b) use US data, and Panels (c) and (d) use Danish data. The dependent variables are log earnings per worker in Panels (a) and (c) and the labor share in Panels (b) and (d). In each panel, the solid lines use the sample of firms in low-concentration industries, and the dashed lines use the sample of firms in high-concentration industries. High-concentration (low-concentration) industries are industries with above-median (or below-median) Herfindahl-Hirschman index. All regressions control for firm fixed effects, firm size quintile by year fixed effects, state(region)×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The labor share is defined as total wage bill divided by revenue in Panel (b) and total wage bill divided by value added in Panel (d). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Figure A39: Changes in Worker Earnings and the Labor Share around Non-Business to Business Manager Transitions in High-Growth and Low-Growth Firms



(a) Worker earnings, US

(b) Labor share, US

(c) Worker earnings, Denmark

(d) Labor share, Denmark

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. Panels (a) and (b) use US data, and Panels (c) and (d) use Danish data. The dependent variables are log earnings per worker in Panels (a) and (c) and the labor share in Panels (b) and (d). In each panel, the solid lines plots the treatment effects for firms with below-median sales growth from year -6 to year -1, and the dashed lines plots the treatment effects for firms with above-median sales growth from year -6 to year -1. All regressions control for firm fixed effects, firm size quintile by year fixed effects, state(region)×year fixed effects and industry×year fixed effects, and observations are weighted by employment. The labor share is defined as total wage bill divided by revenue in Panel (b) and total wage bill divided by value added in Panel (d). In each panel, we also report the coefficient (and standard error) of the static treatment effect, where all post-treatment indicators are assumed to be equal. All standard errors are clustered at the firm level.

Table A1: Summary Statistics for the US Sample

	Mean	Standard Deviation
Employment (US Census)	3356	22000
Employment (Compustat)	4446	33290
Payroll (Thousands)	152000	772000
Log Earnings per Worker	4.065	0.7101
Revenue (US Census, Millions)	1054	6285
Sales (Compustat, Millions)	1117	7766
Labor Share (Wage Bill/Sales)	0.2647	0.1726
Investment (Millions)	66.86	447.7
Investment/Sales	0.1148	0.3634
Cash/Assets	0.2078	0.2456
Assets (Millions)	2053	10500
Share of New Plants	0.1134	0.254
Share of Exiting Plants	0.1214	0.2799
Share of Employment in New Plants	0.0912	0.2457
Share of Employment in Exiting Plants	0.1042	0.2784
Number of Establishments	60.71	289.3
ROA	0.0973	0.1211
Acquisition	0.2352	0.4241
Age	14.54	12.49
Annualized Stock Return	0.1117	0.5648
Number of Observations	41500	
Number of Treated Firms	1300	
Number of Control Firms	2700	

This table reports the mean and standard deviation of firm-level variables for the sample of US firms used in Figure 3. We also reports the number of observations, the number of treated firms (firms that have a non-business to business manager transition event during the sample period) and the number of control firms (firms that have non-business managers in all years) at the bottom of the table.

Table A2: Summary Statistics for the Danish Sample

	Mean	Standard Deviation
Employment	31.54	153.6
Payroll (Thousands DKK)	10321	52155
Log Earnings per Worker	5.686	0.3179
Sales (Millions DKK)	54.23	482.6
Value Added (Millions DKK)	16.11	128.0
Labor Share (Wage Bill/Value Added)	0.7612	0.2657
Investment (Millions DKK)	2.728	55.05
Investment/Sales	0.0489	0.0886
Cash/Assets	0.0975	0.1072
Assets (Millions DKK)	46.15	886.3
Share of New Plants	0.0199	0.1143
Share of Exiting Plants	0.0175	0.1097
Share of Employment in New Plants	0.0135	0.0982
Share of Employment in Exiting Plants	0.0130	0.1003
Number of Establishments	1.463	5.797
ROA	0.0771	0.1951
Acquisition	0.0052	0.0716
Age	15.57	8.770
Number of Observations	237660	
Number of Treated Firms	2366	
Number of Control Firms	26964	

This table reports the mean and standard deviation of firm-level variables for the sample of Danish firms used in Figure 4. We also reports the number of observations, the number of treated firms (firms that have a non-business to business manager transition event during the sample period) and the number of control firms (firms that have non-business managers in all years) at the bottom of the table.

Table A3: Event-study Estimates of Business Managers on Firm Outcomes with Varying Fixed Effects in Denmark

	Log Value Added				Log Earnings per Worker				Labor Share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Business Manager	-0.0262	-0.0085	-0.0021	-0.0058	-0.0220	-0.0171	-0.0205	-0.0225	-0.0231	-0.0269	-0.0268	-0.0244
	(0.0147)	(0.0147)	(0.0144)	(0.0147)	(0.0101)	(0.0059)	(0.0066)	(0.0066)	(0.0106)	(0.0089)	(0.0089)	(0.0090)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Region-year FE	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Size quintile-year FE	N	N	N	Y	N	N	N	Y	N	N	N	Y
Obs	237,660	237,660	237,660	237,660	279,357	279,357	279,357	279,357	237,660	237,660	237,660	237,660

This table reports the coefficients (and standard errors) of the static treatment effect from the event-study analysis, where events are manager transitions from a non-business manager to a business manager in Denmark. All columns control for firm fixed effects and year fixed effects. Columns 2, 3, 4, 6, 7, 8, 10, 11, and 12 additionally control for industry×year fixed effects. Columns 3, 4, 7, 8, 11, and 12 control for region×year fixed effects. Columns 4, 8, and 12 additionally control for firm size quintile by year fixed effects. The dependent variables are log sales in columns 1–4, log earnings per worker in columns 5–8, and the labor share (wage bill divided by value added) in columns 9–12. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A4: Differences in Firm Characteristics Between Treated and Control Firms in the US

	Log Employment	Log Earnings per Worker	Labor Share	Age	ROA	Log Revenue per Worker	Investment /Sales	R&D /Sales	SG&A /Sales	COGS /Sales	Import Shock	1-Year Industry Growth	5-Year Industry Growth	Industry HHI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Hire Business Manager	0.6762	-0.0480	-0.0608	9.650	-0.0136	0.1635	-0.0128	-0.0111	0.0067	0.0106	-0.0235	0.0250	0.0571	0.0507
	(0.1481)	(0.0933)	(0.0367)	(1.923)	(0.0087)	(0.0934)	(0.0119)	(0.0168)	(0.0098)	(0.0129)	(0.0243)	(0.0371)	(0.0809)	(0.0231)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500

	Log Employment	Log Earnings per Worker	Labor Share	Age	ROA	Log Revenue per Worker	Investment /Sales	R&D /Sales	SG&A /Sales	COGS /Sales
	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Hire Business Manager	0.2516	0.0496	0.0155	5.723	-0.0123	0.0415	0.0034	0.0062	0.0393	-0.0230
	(0.0547)	(0.0245)	(0.0107)	(1.143)	(0.0087)	(0.0736)	(0.0139)	(0.0295)	(0.0094)	(0.0143)
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500	41,500

This table reports the differences in firm characteristics between treated and control firms in the US. The independent variable is a dummy variable for hiring a new business manager in the following year. The dependent variables are log employment in columns 1 and 15, log earnings per worker in columns 2 and 16, the labor share (wage bill divided by sales) in columns 3 and 17, firm age in columns 4 and 18, return on assets in columns 5 and 19, log revenue per worker in columns 6 and 20, investment divided by sales in columns 7 and 21, R&D expenses divided by sales in columns 8 and 22, SG&A expenses divided by sales in columns 9 and 23, costs of goods sold divided by sales in columns 10 and 24, Chinese import shock (growth in Chinese imports in the firm's industry from 1992 to 2007) in column 11, industry employment growth over the last year in column 12, industry employment growth over the last 5 years in column 13, and industry's Herfindahl-Hirschman index in column 14. Columns 1 to 14 include year fixed effects, and columns 15 to 24 include industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A5: Differences in Firm Characteristics Between Treated and Control Firms in Denmark

	Log Employment	Log Earnings per Worker	Labor Share	Age	ROA	Log Revenue per Worker	Log Value Added per Worker	Investment /Sales	R&D /Sales	Material Costs /Sales	Robots per Worker	1-Year Industry Growth	5-Year Industry Growth	Industry HHI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Hire Business Manager	10.361	0.0871	-0.0240	0.8831	-0.0006	0.0805	0.1253	0.0101	0.0065	-0.0378	0.1677	-0.0010	-0.0085	0.0287
	(0.0500)	(0.0274)	(0.0181)	(0.5242)	(0.0089)	(0.0701)	(0.0509)	(0.0069)	(0.0042)	(0.0159)	(0.2091)	(0.0115)	(0.0795)	(0.0109)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205

	Log Employment	Log Earnings per Worker	Labor Share	Age	ROA	Log Revenue per Worker	Log Value Added per Worker	Investment /Sales	R&D /Sales	Material Costs /Sales	Robots per Worker
	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)
Hire Business Manager	0.3316	0.0208	-0.0070	0.2080	0.0001	0.0685	0.0276	0.0059	0.0026	-0.0063	-0.0555
	(0.0261)	(0.0081)	(0.0112)	(0.4545)	(0.0084)	(0.0188)	(0.0147)	(0.0043)	(0.0034)	(0.0073)	(0.2689)
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205	190,205

This table reports the differences in firm characteristics between treated and control firms in Denmark. The independent variable is a dummy variable for hiring a new business manager in the following year. The dependent variables are log employment in columns 1 and 15, log earnings per worker in columns 2 and 16, the labor share (wage bill divided by sales) in columns 3 and 17, firm age in columns 4 and 18, return on assets in columns 5 and 19, log revenue per worker in columns 6 and 20, log value added per worker in columns 7 and 21, investment divided by sales in columns 8 and 22, R&D expenses divided by sales in columns 9 and 23, material costs divided by sales in columns 10 and 24, number of robots per thousand workers in columns 11 and 25, industry employment growth over the last year in column 12, industry employment growth over the last 5 years in column 13, and industry's Herfindahl-Hirschman index in column 14. Columns 1 to 14 include year fixed effects, and columns 15 to 25 include industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A6: Propensity to Hire a Business Manager in the US

	Hire Business Manager			
	(1)	(2)	(3)	(4)
Log Earnings per Worker	0.0042	0.0079	0.0054	0.0061
	(0.0010)	(0.0011)	(0.0012)	(0.0013)
Log Employment		0.0099		0.0057
		(0.0011)		(0.0022)
Age		0.0057		0.0041
		(0.0013)		(0.0014)
ROA		-0.0030		-0.0018
		(0.0010)		(0.0011)
Log Revenue per Worker		-0.0022		-0.0008
		(0.0012)		(0.0013)
Year FE	Y	Y	Y	Y
Industry-year FE			Y	Y
State-year FE			Y	Y
Size quintile-year FE			Y	Y
Obs	41,500	41,500	41,500	41,500

This table reports estimates for the propensity score to hire a business manager in the US. The dependent variable is a dummy variable hiring a new business manager in the following year. All independent variables are normalized to have mean zero and standard deviation of one. All columns include year fixed effects, and columns 3 and 4 include industry×year fixed effects, state×year fixed effects, and firm size quintile by year fixed effects. Standard errors are clustered at the firm level.

Table A7: Propensity to Hire a Business Manager in Denmark

	Hire Business Manager			
	(1)	(2)	(3)	(4)
Log Earnings per Worker	0.0127	0.0016	0.0045	0.0022
	(0.0040)	(0.0041)	(0.0018)	(0.0031)
Log Employment		0.0211		0.0256
		(0.0027)		(0.0029)
Age		-0.0018		-0.0001
		(0.0023)		(0.0021)
ROA		-0.0007		-0.0002
		(0.0031)		(0.0023)
Log Value Added per Worker		0.0058		0.0014
		(0.0060)		(0.0030)
Year FE	Y	Y	Y	Y
Industry-year FE			Y	Y
Region-year FE			Y	Y
Size quintile-year FE			Y	Y
Obs	190,205	190,205	190,205	190,205

This table reports estimates for the propensity score to hire a business manager in Denmark. The dependent variable is a dummy variable hiring a new business manager in the following year. All independent variables are normalized to have mean zero and standard deviation of one. All columns include year fixed effects, and columns 3 and 4 include industry×year fixed effects, region×year fixed effects, and firm size quintile by year fixed effects. Standard errors are clustered at the firm level.

Table A8: 2SLS Estimates of Business Managers on Firm Outcomes Using All Board Directors in the US

Panel A: Share of All Directors with Business Degrees 1 Year Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.7905					
	(0.0126)					
Business Manager×Post Retirement		-0.0513	-0.0236	-0.0273	0.0709	-0.0012
		(0.0157)	(0.0064)	(0.0500)	(0.0379)	(0.0043)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

Panel B: Share of All Directors with Business Degrees 5 Years Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.7734					
	(0.0132)					
Business Manager×Post Retirement		-0.0458	-0.0335	0.0598	0.0501	-0.0014
		(0.0163)	(0.0067)	(0.0540)	(0.0403)	(0.0044)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

This table reports the first-stage and 2SLS estimates based on board composition around manager retirements in the US. The sample is the set of firms where a non-business manager retires. The instrument in two panels, respectively, are the share of external board directors with a business degree one year before the retirement and the share of external board directors with a business degree five years before the retirement. In all columns, we control for firm fixed effects and year fixed effects. Column 1 of each panel reports the first-stage estimates of the IV (described in equation 2). Columns 2–5 report 2SLS estimates of business managers on firm outcomes. The dependent variables are log earnings per worker in column 2, the firm's labor share (wage bill divided by sales) in column 3, log revenue in column 4, log employment in column 5, and investment divided by sales in column 6. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A9: 2SLS Estimates of Business Managers on Firm Outcomes Using Board Composition Around Manager Retirements with Firm Size Quintile*Year Fixed Effects in the US

Panel A: Share of External Directors with Business Degrees 1 Year Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.5746					
	(0.0117)					
Business Manager×Post Retirement		-0.0493	-0.0255	0.0598	0.0677	-0.0019
		(0.0192)	(0.0076)	(0.0594)	(0.0871)	(0.0053)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Size quintile-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

Panel B: Share of External Directors with Business Degrees 5 Years Before Retirement

	Business Manager ×Post Retirement	Log Worker Earnings	Labor Share	Log Revenue	Log Employment	Investment /Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Business Director Share×Post Retirement	0.5819					
	(0.0114)					
Business Manager×Post Retirement		-0.0371	-0.0350	0.0272	0.0197	-0.0014
		(0.0182)	(0.0074)	(0.0603)	(0.0449)	(0.0050)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Size quintile-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18,000	18,000	18,000	18,000	18,000	18,000

This table reports the first-stage and 2SLS estimates based on board composition around manager retirements in the US. The sample is the set of firms where a non-business manager retires. The instrument in two panels, respectively, are the share of external board directors with a business degree one year before the retirement and the share of external board directors with a business degree five years before the retirement. In all columns, we control for firm fixed effects, year fixed effects, and firm size quintile by year fixed effects. Column 1 of each panel reports the first-stage estimates of the IV (described in equation 2). Columns 2–5 report 2SLS estimates of business managers on firm outcomes. The dependent variables are log earnings per worker in column 2, the firm's labor share (wage bill divided by sales) in column 3, log revenue in column 4, log employment in column 5, and investment divided by sales in column 6. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A10: Board Composition and Pre-trends Before Retirement Controlling for Size Quintile Fixed Effects

	Δ Log Worker Earnings $_{t-2,t-1}$	Δ Log Worker Earnings $_{t-3,t-2}$	Δ Log Worker Earnings $_{t-4,t-3}$	Δ Log Worker Earnings $_{t-5,t-4}$
	(1)	(2)	(3)	(4)
Business Director Share$_{t-5}$	-0.0080	-0.0064	-0.0248	0.0171
	(0.0277)	(0.0241)	(0.0256)	(0.0231)
Year FE	Yes	Yes	Yes	Yes
Size Quintile FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000
	Δ Labor Share $_{t-2,t-1}$	Δ Labor Share $_{t-3,t-2}$	Δ Labor Share $_{t-4,t-3}$	Δ Labor Share $_{t-5,t-4}$
	(5)	(6)	(7)	(8)
Business Director Share$_{t-5}$	-0.0033	0.0016	-0.0023	0.0034
	(0.0065)	(0.0055)	(0.0055)	(0.0048)
Year FE	Yes	Yes	Yes	Yes
Size Quintile FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000
	Δ Log Revenue $_{t-2,t-1}$	Δ Log Revenue $_{t-3,t-2}$	Δ Log Revenue $_{t-4,t-3}$	Δ Log Revenue $_{t-5,t-4}$
	(9)	(10)	(11)	(12)
Business Director Share$_{t-5}$	-0.0175	0.0180	0.0007	-0.0203
	(0.0295)	(0.0356)	(0.0313)	(0.0326)
Year FE	Yes	Yes	Yes	Yes
Size Quintile FE	Yes	Yes	Yes	Yes
Obs	1000	1000	1000	1000

This table reports the correlations between the IV and pre-trends in firm outcomes before manager retirement. The independent variable is the share of external board directors with a business degree five years before manager retirement. The dependent variable are pre-trends in log earnings per worker in columns 1–4, pre-trends in the labor share in columns 5–8, and pre-trends in log revenue in columns 9–12. For example, column 1 is the change in log earnings per worker from 2 years before the retirement to 1 year before the retirement, column 2 is the change in log earnings per worker from 3 years before the retirement to 2 years before the retirement, etc. All columns control for year fixed effects and firm size quintile fixed effects. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A11: 2SLS and OLS Estimates of Business Managers on Firm Outcomes in the US

Panel A: IV

	Log Revenue			Log Earnings per Worker			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	-0.027	-0.051	-0.032	-0.094	-0.075	-0.105	-0.035	-0.023	-0.036
	(0.052)	(0.046)	(0.043)	(0.055)	(0.044)	(0.041)	(0.019)	(0.015)	(0.016)
Log Earnings t-1	x	x	x	x	x	x	x	x	x
Log Earnings t-2		x	x		x	x		x	x
Log Earnings t-3			x			x			x
Log Sales t-1	x	x	x	x	x	x	x	x	x
Log Sales t-2		x	x		x	x		x	x
Log Sales t-3			x			x			x
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-statistic	32.6	44.8	41.9	32.6	44.8	41.9	32.6	44.8	41.9
Obs	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300

Panel B: OLS

	Log Revenue			Log Earnings per Worker			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	-0.006	-0.004	-0.012	-0.059	-0.047	-0.063	-0.021	-0.012	-0.023
	(0.008)	(0.008)	(0.007)	(0.013)	(0.013)	(0.011)	(0.003)	(0.003)	(0.003)
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300

This table reports 2SLS (Panel A) and OLS (Panel B) estimates of business managers on log sales, log earnings per worker and the labor share in the US. The sample in both panels is the set of firms with a single transition from non-business to business manager or firms with non-business managers as in our event-study regressions. For the 2SLS models, the first stage is described in equation (5) in Appendix A.1 and is based on the average lagged business manager of peer firms (those in the same industry, region and size quintile). Table A26 in the Appendix presents the first-stage estimates and Figure A30 shows them visually. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. Columns 1, 4 and 7 control for one-year lags of firm (log) sales and (log) earnings per worker, columns 2, 5 and 8 control for one and two-year lags of firm sales and earnings per worker, and columns 3, 6 and 9 control for one, two and three-year lags of firm sales and earnings per worker. The dependent variables are log revenue in columns 1–3, log earnings per worker in columns 4–6, and the firm's labor share (wage bill divided by sales) in columns 7–9. Observations are weighted by firm employment and standard errors are clustered at the firm level. First-stage F-statistics for the excluded instruments are reported at the bottom of Panel A.

Table A12: 2SLS and OLS Estimates of Business Managers on Firm Outcomes in Denmark

Panel A: IV

	Log Value Added			Log Earnings per Worker			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.105	0.091	0.078	-0.048	-0.038	-0.044	-0.037	-0.046	-0.052
	(0.068)	(0.068)	(0.067)	(0.024)	(0.024)	(0.024)	(0.027)	(0.027)	(0.027)
Log Earnings t-1	-0.179	-0.155	-0.157	0.129	0.128	0.125	0.002	0.010	0.011
	(0.010)	(0.010)	(0.010)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Log Earnings t-2		-0.028	-0.018		-0.011	-0.010		-0.018	-0.009
		(0.011)	(0.011)		(0.005)	(0.005)		(0.005)	(0.005)
Log Earnings t-3			-0.066			-0.038			-0.020
			(0.010)			(0.005)			(0.004)
Log Value Added t-1	0.479	0.441	0.440	0.006	0.006	0.006	-0.011	-0.019	-0.021
	(0.004)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log Value Added t-2		0.069	0.060		-0.001	0.002		0.018	0.005
		(0.005)	(0.005)		(0.002)	(0.003)		(0.002)	(0.002)
Log Value Added t-3			0.016			-0.007			0.026
			(0.004)			(0.002)			(0.002)
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-statistic	32.2	32.0	32.4	31.6	31.1	31.4	32.0	31.3	31.7
Obs	44,731	44,539	44,474	44,731	44,550	44,490	44,962	44,761	44,697

Panel B: OLS

	Log Value Added			Log Earnings per Worker			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.007	0.008	0.008	-0.028	-0.028	-0.029	-0.025	-0.024	-0.024
	(0.015)	(0.015)	(0.015)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	44,731	44,539	44,474	44,731	44,550	44,490	44,962	44,761	44,697

This table reports 2SLS (Panel A) and OLS (Panel B) estimates of business managers on log value added, log earnings per worker and the labor share in Denmark. For the 2SLS models, the first stage is described in equation (5) in Appendix A.1 and is based on the average lagged business manager of peer firms (those in the same industry, region and size quintile). Table A27 in the Appendix presents the first-stage estimates and Figure A30 shows them visually. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. Columns 1, 4 and 7 control for one-year lags of firm (log) value added and (log) earnings per worker, columns 2, 5 and 8 control for one and two-year lags of firm value added and earnings per worker, and columns 3, 6 and 9 control for one, two and three-year lags of firm value added and earnings per worker. The dependent variables are log value added in columns 1–3, log earnings per worker in columns 4–6, and the firm's labor share (wage bill divided by value added) in columns 7–9. Observations are weighted by firm employment and standard errors are clustered at the firm level. First-stage F-statistics for the excluded instruments are reported at the bottom of Panel A.

Table A13: Robustness of 2SLS Estimates of Business Managers on Firm Outcomes in Denmark

	Log Value Added			Log Earnings per Worker			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.064	0.029	0.046	-0.040	-0.063	-0.046	-0.053	-0.066	-0.056
	(0.067)	(0.083)	(0.075)	(0.024)	(0.031)	(0.027)	(0.027)	(0.032)	(0.029)
Log Earnings t-1	-0.152	-0.156	-0.157	0.122	0.139	0.125	0.008	0.003	0.012
	(0.010)	(0.011)	(0.010)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Log Earnings t-2	-0.020	0.007	-0.025	-0.012	-0.007	-0.008	-0.011	-0.021	-0.008
	(0.011)	(0.013)	(0.011)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)
Log Earnings t-3	-0.067	-0.050	-0.066	-0.040	-0.034	-0.035	-0.020	-0.033	-0.019
	(0.010)	(0.010)	(0.010)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Log Value Added t-1	0.436	0.468	0.434	0.007	0.006	0.006	-0.020	-0.031	-0.020
	(0.005)	(0.005)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Log Value Added t-2	0.062	0.068	0.064	0.003	0.003	0.002	0.004	-0.001	0.004
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Log Value Added t-3	0.015	0.004	0.016	-0.009	-0.007	-0.007	0.024	0.034	0.026
	(0.004)	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-statistic	32.3	28.1	25.5	31.2	27.8	24.6	31.6	28.2	24.8
Obs	44,474	44,474	43,631	44,490	44,490	43,650	44,697	44,697	43,853

This table reports 2SLS estimates of business managers on log value added, log earnings per worker and the labor share in Denmark. The first stage is described in equation (5) in Appendix A.1 and is based on the average lagged business manager of peer firms (those in the same industry, region and size quintile). Table A27 in the Appendix presents the first-stage estimates and Figure A30 shows them visually. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects, and one, two and three-year lags of firm value added and earnings per worker. Columns 1, 4, and 7 control for a full set of interactions between earnings quintile in 1995 and year dummies. Columns 2, 5, and 8 control for cell-specific trends (year interacted with dummies for each cell). Columns 3, 6, and 9 control for three lags of value added and earnings of other firms in the same cell. The dependent variables are log value added in columns 1–3, log earnings per worker in columns 4–6, and the firm's labor share (wage bill divided by value added) in columns 7–9. Observations are weighted by firm employment and standard errors are clustered at the firm level. First-stage F-statistics for the excluded instruments are reported at the bottom of Panel A.

Table A14: Types of Compensation of Business Managers in the US

	Percent Salary	Percent Bonus	Percent Stock	Percent Options	Percent Incentive Plan	Percent Other
	(1)	(2)	(3)	(4)	(5)	(6)
Business Major	-0.0038	-0.0051	-0.0001	0.0070	0.0012	0.0008
	(0.0028)	(0.0020)	(0.0015)	(0.0027)	(0.0021)	(0.0033)
Year FE	Y	Y	Y	Y	Y	Y
Manager Characteristics	Y	Y	Y	Y	Y	Y
Firm Characteristics	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Obs	36,607	36,607	36,607	36,607	36,607	36,607

This table reports the coefficients from regressions of different components of compensation from the Execucomp dataset on an indicator for having a business degree in the US. The dependent variable is each form of compensation as a percentage of total compensation, and all columns include year and firm fixed effects, manager characteristics (gender, experience, age), firm characteristics (log employment and log sales). Standard errors are clustered at person level.

Table A15: Types of Compensation of Business Managers in Denmark

	Log Hourly Wage	Percent Stock Option	Percent Fringe Benefits
	(1)	(2)	(3)
Business Major	0.0583	0.0011	0.0011
	(0.0075)	(0.0005)	(0.0011)
Year FE	Y	Y	Y
Manager Characteristics	Y	Y	Y
Firm Characteristics	Y	Y	Y
Firm FE	Y	Y	Y
Obs	244,072	266,660	266,660

This table reports the coefficients from regressions of manager compensation on an indicator for having a business degree in Denmark. The dependent variables are log hourly wage in column 1, the share of compensation in the form of stock options in column 2, and the share of compensation in the form of fringe benefits in column 3. All columns include year and firm fixed effects, manager characteristics (gender, experience, age), and firm characteristics (log employment and log sales). Standard errors are clustered at person level.

Table A16: Summary Statistics for the Sample of Danish Exporters

	Mean	Standard Deviation
Employment	226.4	599.4
Payroll (Thousands DKK)	75095	229476
Log Earnings per Worker	5.734	0.2607
Sales (Millions DKK)	382.9	1499
Value Added (Millions DKK)	138.8	533.0
Labor Share (Wage Bill/Value Added)	0.7264	0.2289
Investment (Millions DKK)	21.94	107.1
Investment/Sales	0.0559	0.0808
Cash/Assets	0.0555	0.0810
Assets (Millions DKK)	407.7	2187
Share of New Plants	0.0290	0.1200
Share of Exiting Plants	0.0281	0.1172
Share of Employment in New Plants	0.0130	0.0848
Share of Employment in Exiting Plants	0.0113	0.0829
Number of Establishments	2.102	4.084
ROA	0.0529	0.1525
Acquisition	0.0186	0.1352
Age	19.60	6.993
Number of Observations	11400	
Number of Firms	1051	

This table reports the mean and standard deviation of firm-level variables for the sample of Danish exporters used in Table 4. We also reports the number of observations and the number of firms at the bottom of the table.

Table A17: Business Managers and Firm Response to Export Shocks in Denmark

	Log Profit per Worker	Log Value Added per Worker	Log Export	Log Value Added	Log Employment	Log Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.1576	0.0972	0.3251	0.2381	0.1409	0.3620
	(0.0865)	(0.0446)	(0.0740)	(0.0547)	(0.0328)	(0.0990)
Export Shock*Business Manager	0.1614	0.0696	0.2995	0.2138	0.1442	0.2667
	(0.0875)	(0.0474)	(0.0770)	(0.0568)	(0.0342)	(0.1051)
F-statistic	0.0	2.0	1.2	1.5	0.1	5.7
F-test p value	0.905	0.153	0.273	0.224	0.725	0.017
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Obs	5,665	5,665	5,665	5,665	5,665	5,665

This table reports the coefficients from regressions of firm-level outcomes on export shocks interacted with a dummy for having a business manager. Export shocks are shocks to export demand from destination-product combinations the firm exports to as defined in the text. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. The dependent variables are log profits per worker in column 1, log value added per worker in column 2, log value of exports in column 3, log value added in column 4, log employment in column 5, and log investment in column 6. Regressions are weighted by firm employment, and standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A18: Response to Positive and Negative Export Shocks in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Pos Export Shock*Non-Business Manager	0.0223	0.0173	0.0065	0.0204	0.0223	0.0023
	(0.0048)	(0.0044)	(0.0156)	(0.0047)	(0.0044)	(0.0149)
Pos Export Shock*Business Manager	0.0056	0.0077	-0.0136	0.0045	0.0079	-0.0128
	(0.0111)	(0.0072)	(0.0372)	(0.0108)	(0.0075)	(0.0344)
Neg Export Shock*Non-Business Manager	-0.0038	0.0011	-0.0044	-0.0059	0.0051	-0.0034
	(0.0044)	(0.0028)	(0.0121)	(0.0044)	(0.0038)	(0.0118)
Neg Export Shock*Business Manager	0.0069	0.0064	-0.0156	0.0031	0.0100	-0.0089
	(0.0105)	(0.0074)	(0.0371)	(0.0102)	(0.0070)	(0.0338)
F-statistic (Positive Shocks)	2.0	1.4	0.3	1.9	2.8	0.2
F-test p value (Positive Shocks)	0.152	0.242	0.597	0.168	0.095	0.670
F-statistic (Negative Shocks)	0.9	0.4	0.1	0.7	0.4	0.0
F-test p value (Negative Shocks)	0.336	0.529	0.765	0.413	0.528	0.874
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE	Y	Y	Y	Y	Y	Y
Obs	1,776,520	1,776,520	5,313	1,776,520	1,776,520	5,313

This table reports the coefficients from regressions of wages and the labor share on positive and negative export shocks interacted with a dummy for having a business manager. Positive and negative export shocks are defined in section 3. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Pos Export Shock*Non-Business Manager* (or *Neg Export Shock*Non-Business Manager*) and the coefficient of *Pos Export Shock*Business Manager* (or *Neg Export Shock*Business Manager*) at the bottom of the table.

Table A19: Wage Response to Export Shocks at Firms with Non-Business to Business Manager Transitions in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.0326	0.0394	-0.0042	0.0281	0.0304	-0.0131
	(0.0065)	(0.0070)	(0.0254)	(0.0064)	(0.0067)	(0.0253)
Export Shock*Business Manager	0.0171	0.0275	-0.0098	0.0139	0.0135	-0.0193
	(0.0069)	(0.0076)	(0.0260)	(0.0069)	(0.0071)	(0.0254)
Log Output				0.0150	0.0204	-0.0739
				(0.0045)	(0.0061)	(0.0258)
Log Employment				0.0037	0.0181	0.0615
				(0.0062)	(0.0067)	(0.0278)
Log Capital-labor Ratio				0.0058	0.0052	-0.0299
				(0.0026)	(0.0026)	(0.0118)
Share of High-skilled Workers				0.0846	0.0660	0.3540
				(0.0281)	(0.0363)	(0.2052)
F-statistic	5.7	2.8	5.9	7.4	5.3	7.6
F-test p value	0.017	0.092	0.015	0.007	0.021	0.006
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE	Y	Y	Y	Y	Y	Y
Obs	486,754	486,754	1,062	486,754	486,754	1,062

This table reports the coefficients from regressions of wages and the labor share on export shocks interacted with a dummy for having a business manager for the sample of Danish exporters with a non-business to business manager transition used in Table 5. Export shocks are shocks to export demand from destination-product combinations the firm exports to as defined in the text. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A20: Response to Export Shocks Before and After Placebo Manager Transitions in Denmark

	Value Added per Worker		Log Hourly Wage		Log Annual Earnings		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Export Shock*Pre	0.0577	0.0929	0.0152	0.0179	0.0138	0.0226	-0.0028	-0.0107
	(0.0344)	(0.0323)	(0.0031)	(0.0031)	(0.0042)	(0.0039)	(0.0150)	(0.0102)
Export Shock*Post	0.0672	0.0947	0.0149	0.0173	0.0162	0.0228	-0.0075	-0.0118
	(0.0356)	(0.0326)	(0.0030)	(0.0031)	(0.0041)	(0.0040)	(0.0154)	(0.0105)
F-statistic	0.7	0.1	0.0	0.3	3.4	0.0	1.1	0.1
F-test p value	0.410	0.829	0.831	0.605	0.065	0.893	0.287	0.712
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Worker-firm FE			Y	Y	Y	Y		
Obs	1,782	5,469	599,701	1,950,716	599,701	1,950,716	1,782	5,469

This table reports the coefficients from the regression of wages, value added per worker and the labor share on export shocks before and after placebo transitions from a non-business manager to another non-business manager. *Pre* is a dummy variable that equals 1 if the observation is before the manager transition, and *Post* is a dummy variable that equals 1 if the observation is after the manager transition. Columns 1, 3, 5, 7 include firms with a manager transition from a non-business manager to another non-business manager, and columns 2, 4, 6, 8 also include firms that had non-business managers and no manager transitions (for which *Pre* equals 1 for all years). In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the observation is after the manager transition. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Dependent variables are log value added per worker in columns 1 and 2, log hourly wage in columns 3 and 4, log annual earnings in columns 5 and 6, and the labor share (wage bill divided by value added) in columns in columns 7 and 8. Firm-level regressions in columns 1, 2, 7, 8 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Pre* and the coefficient of *Export Shock*Post* at the bottom of the table.

Table A21: Wage Response to Export Shocks Controlling for Product Export Shocks in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.0191	0.0219	-0.0281	0.0155	0.0157	-0.0168
	(0.0040)	(0.0050)	(0.0134)	(0.0038)	(0.0046)	(0.0129)
Export Shock*Business Manager	0.0018	0.0080	-0.0344	-0.0032	0.0005	-0.0241
	(0.0089)	(0.0081)	(0.0139)	(0.0090)	(0.0080)	(0.0132)
Log Output				0.0120	0.0106	-0.1469
				(0.0033)	(0.0052)	(0.0146)
Log Employment				0.0174	0.0510	0.1382
				(0.0045)	(0.0057)	(0.0161)
Log Capital-labor Ratio				0.0050	0.0014	-0.0179
				(0.0018)	(0.0019)	(0.0060)
Share of High-skilled Workers				0.0820	0.0579	0.0819
				(0.0245)	(0.0281)	(0.0818)
Product Export Shocks	0.0086	0.0162	-0.0331	0.0119	0.0117	-0.0443
	(0.0052)	(0.0053)	(0.0199)	(0.0051)	(0.0049)	(0.0203)
F-statistic	4.0	2.5	2.2	4.7	2.9	3.4
F-test p value	0.045	0.113	0.142	0.031	0.088	0.065
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE	Y	Y	Y	Y	Y	Y
Obs	1,210,720	1,210,720	3,797	1,210,720	1,210,720	3,797

This table reports the coefficients from regressions of wages and the labor share on export shocks interacted with a dummy for having a business manager. All regressions control for product demand shocks at the firm level, constructed as the weighted average of total imports (excluding imports from Denmark) at six-digit product level, with the weights being the ex-ante shares of sales in each six-digit product. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A22: Wage Response to Export Shocks Using a Balanced Panel of Firms in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.0191	0.0245	0.0031	0.0153	0.0181	0.0050
	(0.0031)	(0.0051)	(0.0127)	(0.0029)	(0.0043)	(0.0123)
Export Shock*Business Manager	0.0084	0.0108	-0.0058	0.0024	0.0003	-0.0052
	(0.0058)	(0.0071)	(0.0124)	(0.0057)	(0.0066)	(0.0118)
Log Output				0.0152	0.0133	-0.1087
				(0.0036)	(0.0052)	(0.0159)
Log Employment				0.0153	0.0426	0.0982
				(0.0043)	(0.0054)	(0.0206)
Log Capital-labor Ratio				0.0026	-0.0002	-0.0117
				(0.0015)	(0.0014)	(0.0053)
Share of High-skilled Workers				0.0861	0.0909	0.2570
				(0.0214)	(0.0268)	(0.1034)
F-statistic	3.3	3.0	5.4	4.8	5.9	7.7
F-test p value	0.070	0.081	0.020	0.028	0.015	0.005
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Obs	1,473,178	1,473,178	4,461	1,473,178	1,473,178	4,461

This table reports the coefficients from regressions of wages and the labor share on export shocks interacted with a dummy for having a business manager. The sample is a balanced sample of firms between 1995 and 2006 (i.e. firms that exist in every single year of that period). In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A23: Effects of Export Shocks on Firm Exit in Denmark

	Exit - 1 year	Exit - 3 years
	(1)	(2)
Export Shock*Non-Business Manager	-0.0012	0.0173
	(0.0172)	(0.0200)
Export Shock*Business Manager	-0.0119	0.0067
	(0.0186)	(0.0200)
F-statistic	2.2	1.4
F-test p value	0.142	0.235
Industry-year FE	Y	Y
Firm FE	Y	Y
Obs	8,430	8,430

This table reports the coefficients from regressions of firm exit on export shocks interacted with a dummy for having a business manager. All regressions control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, and a dummy variable for whether the firm has a business manager. The dependent variable is an indicator variable for firm exit in next year in column 1 and an indicator variable for firm exit in the next 3 years in column 2. Regressions are weighted by firm employment, and standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A24: College-Degree Managers and Wage Response to Trade Shocks in Denmark

	Log Hourly Wage	Log Annual Earnings	Labor Share	Log Hourly Wage	Log Annual Earnings	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-College Non-Business Manager	0.0152	0.0301	-0.0036	0.0128	0.0253	0.0005
	(0.0049)	(0.0051)	(0.0130)	(0.0047)	(0.0047)	(0.0122)
Export Shock*College Non-Business Manager	0.0175	0.0163	-0.0113	0.0140	0.0093	-0.0076
	(0.0035)	(0.0052)	(0.0128)	(0.0034)	(0.0043)	(0.0119)
Export Shock*Business Manager	0.0003	0.0092	-0.0154	-0.0047	-0.0010	-0.0128
	(0.0066)	(0.0067)	(0.0126)	(0.0065)	(0.0062)	(0.0116)
Log Output				0.0125	0.0127	-0.1087
				(0.0035)	(0.0053)	(0.0155)
Log Employment				0.0131	0.0450	0.0982
				(0.0040)	(0.0054)	(0.0202)
Log Capital-labor Ratio				0.0028	0.0000	-0.0115
				(0.0014)	(0.0014)	(0.0050)
Share of High-skilled Workers				0.0743	0.0748	0.2787
				(0.0207)	(0.0264)	(0.0999)
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Obs	1,707,311	1,707,311	5,305	1,707,311	1,707,311	5,305

This table reports the coefficients from regressions of wages and the labor share on export shocks interacted with whether the manager has no college degree, has a college degree but no business degree, or has a business degree. Export shocks are shocks to export demand from destination-product combinations the firm exports to as defined in the text. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, a dummy variable for whether the firm has a business manager, and a dummy variable for whether the firm has a college-degree manager. Worker-level regressions additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 3–6 also control for time-varying firm characteristics (log output, log employment, log capital-labor ratio, share of high-skilled workers). The dependent variables are log hourly wage of workers in columns 1 and 4, log annual earnings of workers in columns 2 and 5, and the labor share of firms (wage bill divided by value added) in columns 3 and 6. Firm-level regressions in columns 3 and 6 are weighted by firm employment. Standard errors are clustered at the firm level.

Table A25: Wage Response to Export Shocks for Union and Non-Union Workers in Denmark

	Union Workers		Non-Union Workers	
	Log Hourly Wage	Log Annual Earnings	Log Hourly Wage	Log Annual Earnings
	(1)	(2)	(3)	(4)
Export Shock*Non-Business Manager	0.0176	0.0233	0.0021	0.0015
	(0.0031)	(0.0047)	(0.0038)	(0.0048)
Export Shock*Business Manager	0.0007	0.0091	0.0003	-0.0005
	(0.0069)	(0.0068)	(0.0075)	(0.0098)
F-statistic	6.7	3.4	0.1	0.0
F-test p value	0.009	0.064	0.812	0.843
Industry-year FE	Y	Y	Y	Y
Worker-firm FE	Y	Y	Y	Y
Obs	1,548,697	1,548,697	196,686	196,686

This table reports the coefficients from regressions of wages on export shocks interacted with a dummy for having a business manager. Columns 1 and 2 only include union workers, and columns 3 and 4 include only non-union workers. In all columns we control for firm fixed effects, firm size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, firm×worker fixed effects, a dummy variable for whether the firm has a business manager, quadratic in experience, and union and marital status dummies. The dependent variables are log hourly wage in columns 1 and 3, and log annual earnings in columns 2 and 4. Standard errors are clustered at the firm level. We report the F-statistic and corresponding p-value for the F-test testing the difference between the coefficient of *Export Shock*Non-Business Manager* and the coefficient of *Export Shock*Business Manager* at the bottom of the table.

Table A26: First Stage of Diffusion IV in the US

	Business Manager							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer Firm Business Manager t-1	0.160	0.083	0.158	0.081	0.164	0.066	0.149	0.053
	(0.039)	(0.059)	(0.039)	(0.059)	(0.047)	(0.065)	(0.047)	(0.065)
Peer Firm Business Manager t-2		0.270		0.274		0.285		0.291
		(0.058)		(0.058)		(0.065)		(0.065)
Peer Firm Business Manager t-3		0.242		0.239		0.411		0.394
		(0.052)		(0.052)		(0.059)		(0.059)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Size quintile-year FE			Y	Y	Y	Y	Y	Y
Industry*year FE					Y	Y	Y	Y
Lagged sales and earnings							Y	Y
F-statistic	16.7	30.2	16.2	29.8	12.3	43.6	9.9	41.9
Obs	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300

This table reports the first-stage estimates instrumenting business manager with up to three lags of average business manager of peer firms (those in the same industry, region and size quintile) in the US. The first stage equation is described in equation (5) in Appendix A.1. The dependent variable is a dummy for business manager. Odd columns regress on one-year lag of the average business manager of peer firms, and even columns include three years' lags of the average business manager of peer firms. All columns control for firm fixed effects and year fixed effects. We add firm size quintile by year fixed effects in columns 3 and 4, industry×year fixed effects and region×year fixed effects in columns 5 and 6, and lags of firm value added and earnings per worker as controls in columns 7 and 8. Observations are weighted by firm employment and standard errors are clustered at the firm level. First-stage F-statistics for the excluded instruments are reported at the bottom.

Table A27: First Stage of Diffusion IV in Denmark

	Business Manager							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer Firm Business Manager t-1	0.115	0.098	0.166	0.095	0.277	0.226	0.324	0.326
	(0.035)	(0.040)	(0.034)	(0.040)	(0.038)	(0.042)	(0.049)	(0.052)
Peer Firm Business Manager t-2		0.258		0.255		0.331		0.141
		(0.045)		(0.045)		(0.045)		(0.054)
Peer Firm Business Manager t-3		0.225		0.219		0.216		0.361
		(0.047)		(0.047)		(0.048)		(0.057)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Size quintile-year FE			Y	Y	Y	Y	Y	Y
Industry*year FE					Y	Y	Y	Y
Region*year FE					Y	Y	Y	Y
Lagged value added and earnings							Y	Y
F-statistic	11.0	30.6	23.5	28.9	52.8	44.4	43.8	32.9
Obs	82,805	76,516	82,805	76,516	82,805	76,516	51,246	48,649

This table reports the first-stage estimates instrumenting business manager with up to three lags of average business manager of peer firms (those in the same industry, region and size quintile) in Denmark. The first stage equation is described in equation (5) in Appendix A.1. The dependent variable is a dummy for business manager. Odd columns regress on one-year lag of the average business manager of peer firms, and even columns include three years' lags of the average business manager of peer firms. All columns control for firm fixed effects and year fixed effects. We add firm size quintile by year fixed effects in columns 3 and 4, industry×year fixed effects and region×year fixed effects in columns 5 and 6, and lags of firm sales and earnings per worker as controls in columns 7 and 8. Observations are weighted by firm employment and standard errors are clustered at the firm level. First-stage F-statistics for the excluded instruments are reported at the bottom.