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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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Eclipse of Rent-Sharing: The Effects of Managers' Business Education on Wages and the Labor Share in the US and Denmark<sup>^</sup>

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**ABSTRACT**

This paper provides evidence from the US and Denmark that managers with a business degree (“business managers”) reduce their employees' wages. 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 diffusion of the practice of appointing business managers within industry, region and size quartile cells, we provide additional evidence that these are causal effects. We establish that the proximate cause of these (relative) wage effects are changes in rent-sharing practices following the appointment of business managers. Exploiting exogenous export demand shocks, we show that non-business managers share profits with their workers, whereas business managers do not. But consistent with our first set of results, these business managers show no greater ability to increase sales or profits in response to exporting opportunities. Finally, we use the influence of role models on college major choice to instrument for the decision to enroll in a business degree in Denmark and show that our estimates correspond to causal effects of practices and values acquired in business education - rather than the differential selection into business education of individuals unlikely to share rents with workers.

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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 used to hover around 65% between the 1950s and 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 significant 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 (Hall and Soskice, 2001; 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 have been a major contributing factor to the decline in the labor share and slowdown of wage growth. We provide evidence from the US and Denmark suggesting that CEOs with business-school degrees (business undergraduates or MBAs), who have been managing a growing share of firms over time, have significantly reduced wage growth and the share of labor. Combining evidence from the US and Denmark is useful for our agenda for two reasons. First, it highlights that the mechanism we focus on is not just a US phenomenon and operates under diverse institutional conditions. Second, the Danish data enable us to explore some of the mechanisms via which these effects are realized.

We present three main findings. The *first* is our headline result: in both the US and Denmark, when a chief executive officer with a business degree (“business manager” for short) takes over from a non-business manager, there is a significant decline in wages 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 wages in the five years following the appointment of a business manager in the US. The same approach in Denmark yields similar and only slightly smaller results: 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 much of an employment, output, investment, or productivity response, which suggests that business managers are not more productive than their non-business peers.

Taken at face value, our estimates suggest that the effects of business managers are sizable but not implausibly large. In the US, for example, where the fraction of workers employed by business managers has increased from 26% to 43% between 1980 and 2020, our estimates indicate that business managers can explain about 20% of the decline in the labor share. They also account 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 11% in 1995 to 19% in 2011), our mechanism accounts for 6% of the decline in the labor share. The reduction in wages following the accession of a business manager leads to an increase in profits: the return on assets (ROA) increases by 3 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 values of companies that appoint business managers. We further show that all else equal, managers with business degrees earn more than non-business managers.<sup>1</sup>

In addition to the lack of pre-trends in various firm outcomes, we augment our event-study and difference-in-differences estimates by showing that a switch from one non-business manager to another non-business manager has zero impact on all of the outcomes we focus on. We also find no impact on these outcomes due to a switch from a non-college manager to one with a college degree. Hence, our estimates are not capturing mechanical “Hawthorne” effects associated with the arrival of a new manager or the reign of a more educated leader.

These checks notwithstanding, 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 need to cut labor costs. To bolster the argument that our results capture causal effects of business managers on wages and the labor share, we use two strategies. First, we obtain very similar estimates when we focus on manager retirements and deaths, which are arguably less endogenous than other switches from non-business to business managers. Second, we develop an instrumental variable (IV) strategy. We document a clear pattern of “diffusion” of the appointment of business managers within peer groups of firms in both the US and Denmark. We define peer groups as region-industry-size class cells and show that after a first business manager is appointed within a cell, its peers become significantly more likely to appoint business managers as well.<sup>2</sup> We control for differential trends by industry, region, and size class to ensure that this strategy exploits within-group variation. We also confirm that the IV effects are robust to controlling for correlation in output and wages across peers. Although this IV strategy is not a panacea against all endogeneity concerns and may be confounded with cross-cell trends, it is conceptually attractive in part because it exploits a completely different source of variation than

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<sup>1</sup>There are a few other differences between managers with and without a business degree. For example, in Denmark those with a business degree use more debt financing and are a little more likely to adopt robots. We confirm that these are not major channels for our effects.

<sup>2</sup>This is similar to the strategy used in Acemoglu et al. (2019) to isolate potentially exogenous variation in country-level democracy based on the idea that there are regional democratization waves.

our non-IV estimates. Using this approach, we estimate similar—about 50% larger than our baseline results—effects of business managers in both Denmark and the US.<sup>3</sup>

We additionally show that the negative wage effects apply throughout the wage and skill distribution. In Denmark, we show this using both education and wage percentiles of workers, whereas in the US, where we do not have accurate information on worker education, we establish this pattern using variation by worker wage percentile. In both the US and Denmark, the effects are larger for low-skilled workers than for high-skilled ones.

The (relative) wage cuts following the start of the term of a business manager are not without costs to the company. In both the US and Denmark, we find that worker quits increase, especially for more skilled workers (though these estimates are less precise than our main results). 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* major result identifies changes in *rent-sharing* as the major mechanism for our results. To identify how rent-sharing differs in companies run by business managers, we focus on plausibly exogenous variation in firm profits driven by firm-specific export demand shocks in Denmark. In particular, we adopt a strategy first proposed by Hummels et al. (2014) and isolate changes in a firm’s export demand driven by differential changes in the imports of six-digit products by their main exporting destinations. Put differently, this strategy focuses on firms that export to different foreign markets and exploits the differential evolution of overall (non-Danish) imports in these markets. Using this strategy, we explore how business-manager and non-business-manager firms respond to export shocks. 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. Consistent with our previous results, non-business managers share the resulting rents with their workers. Our rent-sharing elasticity estimates suggest that a 10% increase in value added per worker is associated with a 1.9% increase in wages. Alternately, a 10% increase in profit per worker is associated with a 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 find a precise zero impact on wages following such an increase in exports. This differential rent-sharing response explains the entirety of our event-study estimates. 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

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<sup>3</sup>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 IV strategy may be estimating more systemic effects of practices associated with business education. This reasoning also suggests that the quantitative effects reported above should be considered as lower bounds.

because cutting wages is challenging 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.<sup>4</sup>

*Third*, in the Danish data we explore whether business manager effects are due to the selection of individuals who are more prone to take a hard line against labor into business degrees, or whether instruction and socialization in business degree programs produce managers who do not share rents with their employees. To answer this question, we exploit the presence of “role model” effects in the choice of college majors—whereby individuals are more likely to follow the choices of their role models (e.g., see Lockwood and Kunda, 1997; Lockwood, 2006 for general discussion, and Arcidiacono and Nicholson, 2005; Rask and Bailey, 2002; Poldin et al., 2015; Wiswall and Zafar, 2015). We collect the high school records of Danish managers and define the role models for each individual as students in the same high school and with similar academic performance, but one year ahead. We confirm that role models matter for college choice, with a 10% increase in the fraction of role models pursuing a business degree associated with a 1%–3% increase in a student’s likelihood of enrolling in a business program. Exploiting this source of variation, we estimate that most of the wage and labor share results we present can be explained as the treatment effect of business education rather than a selection effect.

Stepping back, we interpret these findings as capturing the impact of management practices and values imparted by business schools and business degrees. Although we do not have direct evidence on these channels, 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 started arguing 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 started being taught in business schools (Bennis and O’Toole, 2005; Ghoshal, 2005; Maren, 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 started viewing 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

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<sup>4</sup>We also adapt the strategy of Hummels et al. (2014) to intermediate input imports, exploiting exports of six-digit products of countries from which a firm sources its intermediates. For our purposes, such intermediate import shocks are less attractive than export demand shocks, because they may not be Hicks-neutral and may affect (some types of) labor directly. Nevertheless, using this source of variation, we find analogous results—positive intermediate import shocks lead to significant rent-sharing for non-business managers and zero rent-sharing for business managers.

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.

To the extent that business schools were the vanguard of these ideas that became more widely held among managers and were further propagated by management consultants, 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 significantly even more to the decline in the labor share and the slowdown in wages than our range of estimates (see also footnote 3).

Our paper is related to several distinct literatures. First, we directly contribute to the literature on the decline of the labor share (and thus the slower growth of wages than productivity) in industrialized nations. (The first paragraph of this paper cites several of the leading publications in this stream of literature.) Second, this paper 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.

In economics, our paper is most closely linked to the literature on rent-sharing in corporations. Several 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. More broadly, several recent papers emphasize the (growing) importance of firm effects in wages and wage inequality (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.

Another important literature on which we build is that of 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). Systematic evidence on this was provided by Bertrand and Schoar’s (2003) pioneering study, and several papers before and since then 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 notable 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 either the role of business school education or general managerial approaches in corporate wage policies. Nor are we aware of studies in this literature that attempt to isolate exogenous variation, which is a key aspect of our empirical strategy. Similar to Bertrand and Schoar (2003), we find that business managers lead to better accounting performance, and our analysis suggests that this positive effect is largely driven by lower labor costs.

Last but not least, 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 find no effects on output or productivity, but a major impact on rent-sharing. Interestingly, in this context, 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.

The rest of the paper is organized as follows. Section 2 describes our data. Section 3 outlines our empirical strategy. In Section 4, we analyze the causal 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 in Denmark to explore the role of rent-sharing in our wage and labor share results. Section 6 uses another IV strategy to address the question of selection into business education. Section 7 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 traded US companies from the BoardEx database of Management Diagnostics Ltd. For each CEO, we observe the name of the school and the degree earned for all post-secondary schools attended. We classify MBAs and any degrees from



a business school as business degrees.<sup>5</sup> We then match the CEOs in BoardEx to Compustat firms between 1980 and 2020. Our final sample contains 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.<sup>6</sup>

Figure 1 plots the share of Compustat firms with business-degree CEOs from 1980. In 1980, only 26% of the Compustat firms had CEOs with business degrees, but by 2020, this figure had grown to 43%. Almost all of the increase comes from the share of CEOs with MBAs, which rose from 24% in 1980 to 37% in 2020. Harvard Business School contributed 19% of the business degrees of CEOs, followed by Wharton (8%) and Stanford (5%).

Among the US public 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 a substantial degree of freedom in operations, and their vision and styles are important determinants of firm policies (Bertrand and Schoar, 2003). Past research has shown that CEOs are not homogeneous and differ in terms of overconfidence (Malmendier and Tate, 2005, 2008), risk aversion (Malmendier et al., 2011; Benmelech and Frydman, 2015; Schoar and Zuo, 2017), time preference (Graham et al., 2015), resoluteness (Kaplan et al., 2012), and gender bias (Duchin et al., 2021).

## 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 (CEOs with business degrees) on firm outcomes and worker wages. We match Compustat firms to the Longitudinal Business Database (LBD), which covers all non-farm establishments with paid employees in the US from 1987 to 2018. It 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 those involved in mergers and acquisitions or buyouts.

We merge the firm-level data with individual worker-level information, including employment, wage, 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. Wages include 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

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<sup>5</sup>Business schools are 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). Business degrees include bachelors, masters and executive programs of business schools.

<sup>6</sup>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.

worker-level data from 22 states and the District of Columbia, which covers about half of the US population.<sup>7</sup> The LEHD wage data are currently available from 1992 through 2014. We also merge the LBD firm-level data with the Business Register (SSEL) data on annual firm sales.

### 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 many of the firm’s activities, such as the number of full-time employees, value added, and industry affiliation. We also use other firm registers to obtain financial data from balance sheets and income statements.<sup>8</sup> For all of our analyses, we exclude firms with 5 or fewer employees. As in the US, our data exclude firms involved in 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 income, education, and occupation. We measure the hourly wage rate as annual labor income 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, and each establishment has a unique identifier. 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. While 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

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<sup>7</sup>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.

<sup>8</sup>The survey from which we draw the accounting data (e.g., profits, investments) has a rolling panel structure where firms are selected based on their employment as of November in the previous year. Firms with 50 or more employees are always sampled. Information on sales and employment is available for all firms.

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 and Chief Executives”) and the highest job hierarchy category.<sup>9</sup> We are also able to link managers over time. Using this link, 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 manager as the one with the highest wage income. As in the US data, we have one manager per firm-year in the Danish data. We also obtain data on managers’ education histories. A manager has a business degree if he or she has business major at any level of education, including short education, professional BA, BA, MA, and PhD.<sup>10</sup>

Figure 2 plots the fraction of managers in Denmark with business degrees over time. Panel (a) shows that the share of firms with business-degree managers has increased by two thirds 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

We are interested in the causal effects on firm and worker outcomes after a firm appoints a manager (CEO) with a business degree (“business manager” for short). We also explore the mechanisms via which managers influence wages and the labor share (and other outcomes). Additionally, we are interested in identifying whether the effects of managers with business degrees are driven by the fact that individuals with certain characteristics and approaches to management select into business schools, or whether these effects are related to the values and ideas students learn in business schools.

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<sup>9</sup>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. In practice, over 80% of our managers are identified from observations that have both managerial occupation codes and the highest job hierarchy category.

<sup>10</sup>About 75% of managers with business MAs also have a business BA or a business professional BA. In Denmark, professional BA programs consist of both theoretical and practical teaching. The practical teaching takes place as an internship at a workplace.

The key relationship we are interested in estimating can be summarized as:

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

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,  $\lambda_i$  summarizes the firm fixed effects,  $\delta_t$  corresponds to time effects, and  $\varepsilon_{it}$  is an error term. The coefficient of interest is  $\gamma$ , which is the effect of business managers on firm and worker outcomes. In our event studies, we allow the effects to vary by event time, and in some of the specifications we allow these effects to vary by worker skill or wage percentile.

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.

Throughout, we follow Borusyak et al. (2021) and use an “imputation” estimator to compute the event-study estimates. This estimator ensures consistency in the presence of two-way fixed effects and avoids issues of spurious identification and negative weights on some observations (de Chaisemartin and D’Haultfœuille, 2020). In practice, this estimator is constructed in three steps. First, the unit fixed effects, time fixed effects, and the coefficients of other control variables are estimated from regressions using untreated observations only. Second, the treatment effect for each treated observation is computed from the first-step regression as the difference between the actual outcome and the potential untreated outcome. Finally, using the second-step estimated effects, we compute the average treatment effect on the treated.

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 have only one event 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, wages and other outcomes would have followed parallel trends in treated and control firms. In all specifications, we control for industry  $\times$  year fixed effects, firm size quintile  $\times$  year fixed effects, and region(state)  $\times$  year fixed effects.

In Danish worker-level regressions, the control group comprises workers employed by the aforementioned control group of firms, and we additionally include time-varying worker characteristics (experience, experience squared, marital status, union membership) and worker  $\times$  firm fixed effects to track the same worker over time and filter out changes in worker composition. Standard errors are clustered at the firm level.

In the US worker-level regressions, because the total number of workers is very large (over 100 million), we adopt a matching procedure for implementing worker-level regressions. Following Smith et al. (2019), we match each treated firm that switches from a non-business manager to a business manager to control firms that are in the same industry, state, employment decile, and wage decile and have never had a business manager before the event. The effect of business manager on wages is estimated from the differential evolution of wages of *staying* workers between treated firms and matched control firms.

Although we verify that there are no pre-trends among firms appointing 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 managers (and potentially causing the replacement of the existing manager). We deal with the first problem by verifying that there are no other major changes at the time of managerial transitions (we discuss a few minor changes and why they are unlikely to account for our results in the next section).

To confront the second problem, we adopt two complementary strategies. 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).

We additionally develop an instrumental variable (IV) strategy 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  $\times$  industry  $\times$  initial size quartile 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 a two-stage least squares (2SLS) strategy. The first-stage equation is:

$$B_{it} = \sum_{k=1}^3 \theta_k Z_{i,t-k} + X'_{it} \beta^F + \lambda_i^F + \delta_t^F + \epsilon_{it}, \quad (2)$$

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 firms in the same region  $\times$  industry  $\times$  size peer group that did not have a business manager at the beginning of the sample.<sup>11</sup> 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

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<sup>11</sup>In the 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.

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.

To explore the role of changes in rent sharing in mediating the effects of business managers on labor outcomes, we focus on the sample of exporting firms in Denmark. In this sample, we follow Hummels et al. (2014) and develop a source of exogenous variation in firm sales and profits driven by changes in firm-specific demand for exports. Specifically, we use differences in exporting destination by six-digit product for each firm and exploit the fact that the demand for exports from Danish firms is changing differentially across these destinations (proxied by their total non-Danish 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 Danish firms exporting that product to the German market, and we proxy for German demand by using variation in the overall German imports for the product in question (except from Denmark).

More concretely, the predicted firm-specific exports are defined as

$$WID_{jt} = \sum_{c,k} s_{jck}^e WID_{ckt}, \quad (3)$$

where  $s_{jct}^e$  is the pre-sample share of exports to country  $c$  and six-digit product  $k$  of firm  $j$ , and  $WID_{ckt}$  is country  $c$ 's total purchases of product  $k$  from other countries at time  $t$ . Imports from Denmark 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,  $\sum_k s_{jk}^e WID_{kt}$ , where  $s_{jk}^e$  is firm  $j$  total exports of six-digit product  $k$ ) to focus solely on the comparison of firms exporting similar products to different destinations. Because our main regressions are in levels with worker fixed effects, as in (1), we explore the effects of export-driven rents by including the level of predicted firm-specific exports,  $WID_{jt}$ , on the right-hand side of our wage regressions with worker  $\times$  firm fixed effects. 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). In the Appendix we also use the same strategies and similarly constructed variables to exploit exogenous variation in intermediate imports.

Finally, to study whether the effects of business managers are due to selection of who studies business or driven by the causal impact of business programs, we develop another IV strategy. We leverage the idea that choices of college majors are often influenced by the decisions of peers and role models. We focus on role model effects, from the previous cohort, in order to avoid the most severe form of common shocks affecting students in the same class. We thus construct the role model group for each student based on the preceding cohort from the same high school in the same GPA quartile (we also experiment with using the previous three cohorts rather than just the preceding one). The restriction to the same GPA quartile is motivated by the prior literature in this area, which finds that students with similar achievements have greater effects on each other (e.g., Poldin et al., 2015). Furthermore, since admission to different study programs is based on high-school GPA, students with similar GPAs have roughly the same programs to select from (Daly et al., 2022).

Specifically, the first stage for this IV strategy is

$$BM_i = \beta BM_{s_i, c_i - 1, q_i}^{Peer} + \alpha_{s_i c_i} + \omega_{q_i} + \epsilon_i, \quad (4)$$

where the dependent variable is a dummy for whether person  $i$  from high school  $s_i$ , graduating in cohort  $c_i$  and in GPA quartile  $q_i$ , chooses to study business. Our key right-hand side variable,  $BM_{s_i, c_i - 1, q_i}^{Peer}$ , is the share of students in high school  $s_i$ , cohort  $c_i - 1$ , and GPA quartile  $q_i$  who enroll in a business program. The presence of cohort  $\times$  school fixed effects  $\alpha_{s_i c_i}$  and GPA quartile fixed effects  $\omega_{q_i}$  ensures that this equation exploits only within-cohort, within-high school, and within-GPA quartile variation.

Our second-stage equation in this case is at the level of individual managers and can be written as:

$$\tilde{y}_i = \tilde{\gamma} BM_i + \alpha_{s_i c_i} + \omega_{q_i} + \varepsilon_i. \quad (5)$$

Here  $\tilde{y}_i$  is the average firm outcome of the firms managed by person  $i$ . Because our key outcomes are at the firm or individual worker level (such as the labor share or wages), while this regression is at the level of individual managers, we now first residualize the dependent variables using the same covariates as in our main models (industry  $\times$  year fixed effects, firm size quintile  $\times$  year fixed effects, and state/region  $\times$  year fixed effects) and firm fixed effects.

Our exclusion restriction in this last IV strategy is that, conditional on high school, cohort, and GPA quartile, the outcomes at the firm where a student is later employed as a manager are not affected

by the college major choice of the manager’s high school model group. This exclusion restriction could be violated if there are correlated shocks that affect the skills or other characteristics of several adjacent cohorts in a given high school that may then impact their future firms. To build confidence in this exclusion restriction, we perform a number of placebo tests and show that students in previous cohorts in the same high school but in different GPA quartiles have no effect on major choice or future firm outcomes. Nor do students in the same GPA quartile but at different high schools, and nor do cohorts farther apart than three years.

## 4 The Effects of Business Managers on Wages and the Labor Share

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

### 4.1 Event-Study Evidence from the US

Figure 3 presents our main event-study figures for the US, 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 log annual wage. These negative effects are significant, and five years after the switch, the annual income of affected workers has declined by 6.7% (standard error = 2.5%). Notice that this specification includes worker  $\times$  firm fixed effects, and thus the wage impacts are not driven by compositional changes.

The second panel depicts a similarly large and negative impact on the labor share. Since we do not observe value added in the US Census data, the labor share is defined as wage bill divided by sales. Five years after the switch to a business manager, the share of labor in *sales* is 1.8 percentage points lower, which is a 9% decline relative to the average sales-based labor share of 20%, or, if we convert this to the standard labor share in *value added*, it is equivalent to a 5 percentage point decline. Notably, before the switch, there is no differential pre-trend in either wages or the labor share.

The bottom two panels show that the switch from a non-business to a business manager is not associated with an increase in sales, employment, or investment. Some of the estimates are less precise than our wage results, but in all cases we find no evidence of either differential trends before or any divergence after the switch to a business manager. Similar changes in employment and output also imply 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 other negative consequences of their wage policies (such as less cooperation with workers or greater quits by some high-skilled workers, which we document later). In any case, the absence of any firm growth or productivity effects make the interpretation of our wage results more straightforward.



Some of the individual estimates in Figure 3, especially for the labor share, are imprecise because we allow the effect of business managers to be completely unrestricted over time. In Panel B of Table 1 below, we impose a constant coefficient for all post-treatment periods and estimate it by OLS in the same sample of firms experiencing a single transition from non-business manager to business manager. We now see precisely estimated effects both for average wage and the labor share:  $-0.063$  (standard error =  $0.011$ ) for the former and  $-0.023$  (standard error =  $0.003$ ) for the latter in our baseline specification.<sup>12</sup>

These results are robust across a range of specifications (that are not disclosed). In the Appendix, we show that they are unchanged although less significant when we look at changes in firm average wage (Figure A1).

## 4.2 Event-Study Evidence from Denmark

Figure 4 presents analogous results for Denmark. Panel (a) depicts precise and sizable effects on the hourly wage. In Denmark, we also have data on workers' annual income, showing very similar patterns. In particular, five years after the switch to a business manager, the annual income of affected workers is 3.6% lower (standard error = 0.6%). Likewise, hourly wages are lower by 2.8% (standard error = 0.5%). The second panel shows that the labor share also declines, though the estimates are less precise in this case. Our point estimate after five years indicates a 2.9 percentage point impact on the labor share (equivalent to a 4% decline starting from a base of 68%).

The bottom two panels again confirm that business managers appear to have no impact on productivity or other economic outcomes—we see no differential trends in value added, sales, employment, and investment. In addition, in all cases, there are no differential pre-trends before the switch to a business manager.

Panel B of Table 2 shows even more precisely estimated and strongly significant results when we impose constant effects:  $-0.029$  (standard error =  $0.006$ ) for average wage and  $-0.024$  (standard error =  $0.007$ ) for the labor share. The results are robust to using firm average wage (Figure A1 in the Appendix) and varying the fixed effects we control for (Table A1).

## 4.3 Quantitative Magnitudes

How large are these effects? To answer this question, we take the point estimates for the wage and labor share effects five years after the switch to a business manager and, in line with the evidence in the previous subsection, 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

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<sup>12</sup>The interpretation of the quantitative magnitudes in Table 1 is slightly different, since it summarizes 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 wages and the labor share grow over time.

business managers.

This calculation implies that in the US the change towards business managers is responsible for a 1 percentage point decline in the labor share (in value added) between 1980 and 2017,<sup>13</sup> explaining 20% of the total decline in the labor share between these dates. Likewise, the same switch explains 15% of the slowdown in wage growth in the US. In particular, 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.<sup>14</sup>

The numbers for Denmark, where the fraction of the workforce working under business managers has grown by less, are smaller, but still meaningful. Specifically, we find that the increase in the fraction of business managers has led to a 0.2 percentage point decline in the labor share from 1995 to 2011, which accounts for 6% of the overall 4 percentage point decline in labor share over this period.

We view these numbers as lower bounds, 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 Reversals

We also looked at the effects of a switch from a business manager to a non-business manager in Denmark (which is much rarer, as suggested in Figure 2 above).<sup>15</sup> Figure A2 in the Appendix confirms that these switches have no impact on wages or the labor share. These results suggest that the effects of business managers on management-labor relations may be permanent and are not reversed even after a non-business manager takes the reins later.

#### 4.5 Placebos

In addition to the pre-trend checks and our further exploration of endogeneity concerns, we also conducted two placebo exercises. Figure 5 presents the results of a switch from one non-business manager to another non-business manager in the US (Panel A) and the same exercise in Denmark (Panel B). Reassuringly, in both cases we estimate precise zero effects. This confirms that our results are not driven by mechanical Hawthorne effects, whereby the arrival of a new manager shakes things up and changes wages, regardless of whether he or she has a business degree.

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<sup>13</sup>The share of business managers increased by 17% from 1980 to 2020 (Figure 1), and its contribution to the decline of the labor share is computed as  $63\% \times 9\% \times 17\% \approx 1$  percentage point.

<sup>14</sup>Our estimates suggest that the effects of business managers on wages and the labor share continue to grow after five years. If we instead use estimates after eight years (which are less precise but larger), the implied quantitative magnitudes would be 26% of the total decline in labor share in the US and 9% in Denmark. This provides an additional reason why our quantitative magnitudes should be viewed as lower bounds.

<sup>15</sup>Such transitions are even rarer in the US, and hence we estimate this specification only on Danish data.

We also explore wage and labor share changes following a switch from a manager without a college degree of any sort to one with any college degree (Panel C),<sup>16</sup> or a switch from a manager without a master’s degree to a manager with a master’s degree (Panel D). We find no evidence of a negative impact on wages or the labor share from these transitions. If anything, some specifications show a small positive (albeit insignificant) impact. This evidence suggests that the estimates reported so far are tightly linked to managers having a business degree and are not driven just by managers having college or higher education in general.

In Appendix Figure A3, we also show that economics degrees are not associated with similar negative effects on wages and the labor share, so our estimated wage 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.

#### 4.6 Endogeneity Concerns I: Confounding Factors

A first endogeneity concern centers on whether other, concurrent changes might be responsible for the patterns we document. To examine firm-level confounds, we look at the differences between business and non-business managers across a wide range of firm-level outcomes in Denmark in Table A2. The only notable differences between business and non-business managers concern robot purchases and leverage (liability). As Figure A4 in the Appendix shows, these changes occur after the manager changes, and thus we interpret them not as confounding factors, but as potential outcomes of new business managers’ overall strategies.<sup>17</sup>

In addition, the changes are not large enough to account for the wage and labor share declines we observe. For example, in the case of robots, Acemoglu et al. (2020) estimate a 4 percentage point fall in the labor share following the adoption of robots in French manufacturing (see also Koch et al., 2021, for an almost identical estimate in Spanish manufacturing). Using Danish data, Humlum (2019) estimates a 6% decline in the wages of production workers. Using these numbers, the increase in robot purchases associated with business managers would lead to much smaller effects than those we estimate. For example, they can account for at most 4% of the reduced labor share that we estimate and at most 5% of the reduction in wages that we estimate.

The empirical relationship between leverage and wages, on the other hand, is ambiguous. Berk et al. (2010) show that an increase in leverage should lead to an increase in wages because firms need to pay workers more to compensate them for the higher unemployment risk associated with leverage.

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<sup>16</sup>We cannot do this exercise in the US, since BoardEx data do not distinguish workers with just a college degree from those without a college degree.

<sup>17</sup>The reason why, despite greater robot adoption, investment is not higher among firms managed by business graduates may be that robots are still a small part of the overall capital stock of many companies or because robots may substitute some other worker-complementary investments (see Acemoglu and Restrepo, 2021).

On the other hand, Michaels et al. (2019) find a negative empirical relationship between leverage and wages: a 10 percentage point increase in leverage is associated with average wages that are lower by 1.4%. According to their estimates, the increase in leverage can account for at most 6% of the wage decline we find.

The hiring of business managers may also coincide with changes in firms' occupational or organizational structure. Business managers may also adopt different organizational forms or outsource certain tasks and change workers' wages as a result. In Appendix Figure A5, we plot the average hierarchy level and occupational wage, as well as the share of workers in low-wage occupations around non-business to business manager transitions in Denmark. We find no significant change in any of these outcomes, suggesting that organizational changes are unlikely to be a major confound or mechanism.

The appointment of business managers may be correlated with other management changes. First, business degrees are more prevalent among younger cohorts, raising the concern that the effects may be driven by differences in manager ages. However, Appendix Figure A6 shows that 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. Second, business managers who take over family firms may renege on previous implicit contracts between family CEOs and workers. Appendix Figure A7 shows 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).<sup>18</sup>

#### 4.7 Endogeneity Concerns II: Evidence from Manager Retirements and Deaths

The second threat to our interpretation is that firms that appoint business managers are fundamentally different from those with non-business managers. Although our event-study graphs are reassuring and show no pre-trends before manager transitions, one might still be concerned about endogenous switches to business managers and time-varying omitted factors. In this subsection, we adopt our first strategy to deal with this concern.

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.<sup>19</sup> Specifically, we define retirement as the cessation of employment of a previous manager who was 62 or older upon the arrival of a new manager. We also directly observe the deaths of managers (we drop deaths after the age of 65 for this exercise).

Figure 6 is analogous Figure 4, but focuses only on retirements and deaths. The results in the

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<sup>18</sup>Another concern is that business managers are hired due to activist campaigns. We find that 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, and that our results are robust to excluding those firms.

<sup>19</sup>Because our matched employer-employee data in the US is a subsample, we are not able to do this exercise in the US case.

top and bottom panels are very similar, both qualitatively and quantitatively. For example, after five years, a switch to a business manager following the retirement of a non-business manager is associated with a 3.2% decline in workers' annual income and a 3.4 percentage point decline in the labor share.

#### 4.8 Endogeneity Concerns III: Evidence from the Diffusion of Business Managers

Our second strategy for dealing with time-varying omitted factors is to estimate IV models as explained in Section 3. The first-stage relationships for the US and Denmark, which follow equation (2), are presented in Appendix Tables A3 and A4, while the two panels of Figure 7 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 quartile. Our identification interprets this diffusion as resulting from a “fad” or from learning from peers. What we 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.

Tables 1 and 2 present IV estimates exploiting this source of variation 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 average firm wage 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 the IV estimates 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 20% in our baseline case). In Denmark, this number

would be 9% (as opposed to 6% in our baseline).

The main concern with our IV estimates 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 initial firm size, we conduct several robustness tests to further address this concern. In columns 1, 4, and 7 in Table A5, we control for a full set of interactions between wage 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 wages of other firms in the same cell to allow value added and wages 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 wages and labor share remain negative, significant, and close to our baseline estimates. These robustness checks increase confidence that our estimates correspond to the causal effects of business managers on wages and the labor share.

#### 4.9 Heterogeneous Effects

In Appendix Figures A8 and A9, we look at whether the effects are heterogeneous across worker types. In the US, we can do this only by worker wage percentile (because we do not have precise information on education). In this case, we estimate negative impacts on both workers above or below the median wage, but the effects on lower-wage workers are about twice as large as the ones for higher-wage workers. In Denmark, we find that the effects are again larger for low-skilled workers, regardless of whether we classify them by wage percentiles or education. Appendix Figure A10 shows that workers at the top tail of the wage distribution experience smaller yet still significant wage losses.

In Appendix Figure A11, we also look at heterogeneous effects by the level of managers' business degrees in Denmark. We find negative and significant wage effects for managers with business BAs and professional BAs and for those with business MAs, with the effects being slightly larger for business MAs. In additional (unreported) results, we also find slightly larger, but not significantly different, effects for larger firms (which are much more likely to employ business managers).

We suggested in the Introduction that our business manager effects may reflect the impact of ideas related to shareholder value and reengineering the corporation. If so, we should expect such effects to be concentrated among managers who received their business education after 1980, when these ideas were more popular and widespread in business schools. Consistent with this expectation, Appendix Figure A12 shows that the effects are almost twice larger for Danish managers who received their business degrees after 1980: wages decline by 4.9% and the labor share declines by 3.4 percentage

point within five years after transitions to business managers in the post-1980 cohorts, compared to a 2.7% decline in wages and a 1.9 percentage point decline in labor share for business managers in the pre-1980 cohorts (the results for the US are similar, but not reported).

Finally, Appendix Figure A13 shows very similar wage effects for union and non-union workers, confirming that the impact of business managers is not confined to unionized workers, while Figure A14 depicts a small and statistically insignificant negative effect on unionization rates.

#### 4.10 Who Benefits?

The results presented so far indicate that business managers reduce wages and the labor share, and these effects are felt by workers in different parts of the skill distribution. An important question remains: Who benefits from having a business manager? 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 wages paid to workers should imply higher profits. Figure 8 shows that following a switch to business manager, ROA increases by about 3 percentage points in the US, and by about 1.5 percentage point in Denmark.

Higher profits also translate into higher stock market prices. We match our US firms to Compustat and explore the effects of appointing a business manager on stock market valuations.<sup>20</sup> Panel C of Figure 8 shows a significant increase in shareholder returns following the appointment of a business manager. Hence, one clear group of beneficiaries from the practices brought about by business managers are shareholders.<sup>21</sup>

Table A6 and A7 in the Appendix look 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 significant premium for business managers, who earn at least 5%–8% more than their non-business-school peers. In Panel B of Table A6, we also look at the composition of compensation for business CEOs. Business CEOs receive a higher percentage of their earnings from options, and a lower percentage of their earnings from salary and bonus, but the differences are small and insignificant. This suggests that the differences between business and non-business managers cannot be explained by differences in incentive pay. There is also no difference in the percentage of earnings from stocks, suggesting that the higher compensation of business CEOs is

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<sup>20</sup>In principle, we can do the same in Denmark, but this would leave us with a tiny sample, consisting of fewer than 200 firms, and hence for this exercise, we focus on the US sample.

<sup>21</sup>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.

not mechanically driven by the higher value of the company's stocks they own.<sup>22</sup>

Overall, although other reasons may explain why business managers earn more than their peers without a business education, our evidence implies that their ability to reduce wages may be an important component of their "success" (especially since they do not seem to have greater productivity or achieve higher sales).

#### 4.11 Costs of Business Managers

Management and wage policies associated with business managers may have some costs for the firm as well. Figure 9 and Appendix Table A8 explore the effects of business managers on worker quits. Consistent with the notion that workers are unhappy with policies and practices that lead to lower wages and labor share, we find that quits increase following a switch to a business manager in both the US and Denmark. For example, our estimate for Denmark in Table A8 is a 3.7 percentage point rise in worker quits once a business manager takes over. This does not merely reflect a Hawthorne effect: Appendix Figure A16 shows that the arrival of a non-business manager is not associated with higher quit rates.

The results also show that quit effects are slightly larger for higher-skilled workers, despite the fact that they suffer smaller (relative) wage reductions than low-skilled workers. This differential effect presumably reflects these workers' access to better outside options.

In summary, our quit findings indicate that the switch to business managers has noteworthy costs associated with increased quits, especially among the more skilled workers.

Overall, the evidence in this section documents that business managers pay lower wages to their employees and reduce the labor share of their firm. Several exercises suggest that these are the causal effects of appointing a manager with a business degree. There is no evidence that business managers are more productive or increase sales relative to non-business managers. Rather, it is their differential wage policies that translate into higher profits and higher returns for shareholders, and they themselves appear to be paid more as well. In the next section, we provide evidence that their effects on wages and the labor share are a consequence of their different rent-sharing practices.

## 5 Business Managers and Rent-Sharing

In this section, we provide evidence that the negative impact of business managers on wages and the labor share is driven by their unwillingness to share rents with workers. This section focuses on results from Denmark, where we can construct firm-level export demand shocks.

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<sup>22</sup>We also find no significant change in the stock option payments received by workers, shown in Appendix Figure A15.



## 5.1 Exports, Rents, and Wages

Our main estimates, which follow the methodology outlined in Section 3, are presented in Table 3. The table shows that positive export (demand) shocks lead to higher hourly wages and higher annual income 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. We compute rent-sharing elasticities for non-business managers as follows: in Appendix Table A9, we show that export shocks increase profits per worker by 16%–17% and raise value added per worker by 9%, and thus the point estimates in column 1 of Table 3 imply that a 10% increase in profits (value added) per worker is associated with a 1% (1.9%) increase in hourly wages in firms run by non-business managers. This elasticity is in the ballpark of the estimates in the literature.<sup>23</sup> The analogous elasticities for business managers are zero. These results indicate that business managers significantly change rent-sharing patterns, moving away from sharing some of the increases in profits and sales with workers.

Quantitatively, these estimates suggest that most of the business manager effects we have documented so far are due to their lower propensity of sharing rents. In particular, the value added per worker increases by 13% on average in the five years following the accession of business managers in the event study sample. If these managers continued to share rents in the same way as non-business managers, we would have seen 2.3% faster wage growth and a 2.5 percentage point higher labor share during the same period. These effects thus explain the bulk of business manager effects in the Danish sample (3% slower wage growth and a 3 percentage point lower labor share within five years).

Table 4 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: neither business managers nor non-business managers appear to cut wages in response to negative export shocks. This likely reflects the fact that wage cuts are difficult to impose on workers.<sup>24</sup> However, there is a notable difference between the behaviors of business and non-business managers after a positive export shock: business managers do not increase wages following an (exogenous) increase in exports, whereas non-business managers share the resulting rents. This asymmetry of positive export shocks explains the patterns shown in Table 3 and adds nuance to them, suggesting that the differences between business and non-business managers are more pronounced during good times, when there are more rents to be shared.

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<sup>23</sup>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.

<sup>24</sup>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.

Panel A of Table 5 adds to these results by focusing on switches from non-business to business managers. It separately estimates the effects of export shocks on value added per worker, the hourly wage, log annual income, and the labor share before and after such manager transitions. It demonstrates that there is rent-sharing before the switch, which ceases after a business manager takes office. Panel B 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 results in Panel A, we now see very similar elasticities, which is reassuring for our interpretation.

Appendix Table A9 and Figure A17 provide a number of additional results that bolster our confidence in these estimates. First, we show that there are 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, both firms with business managers and firms with 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.

Table 6 further supports our interpretation by estimating a version of the models in Table 3, but now instrumenting for whether firms have business managers (based on the first stage from equation (2) using the peer firms' hiring of business managers). The instrumented business manager effect is once again associated with a reduced rent-sharing elasticity, further building the case for a causal relationship between business managers and reduced rent sharing.

The rent-sharing results are robust as well. In Table A10, we obtain very similar results when we control for overall variation in product level exports and thus focus only on variation across firms exporting similar products to different destinations. In Table A11, we present similar estimates for a balanced panel of firms. In Table A12, we bolster our interpretation by confirming that our results are driven by managers with a business degree, and there are no analogous effects for managers with non-business college degrees, who continue to share rents just as intensively as managers without a college degree. In Table A13, we again confirm that the difference between business and non-business managers is present for both union and non-union workers, though the rent-sharing estimates for firms with non-business managers is less precise in the much smaller non-union sample.

Consistent with the rent-sharing mechanism, we find that the effects of business managers are larger in industries with higher concentration where there are, Presumably, more rents to be shared. Figure A18 repeats our event-study specification from Section 4 for industries with high vs. low concentration (as measured by the Herfindahl-Hirschman index). We find that declines in wages and the labor share following non-business to business manager transitions are about twice as large in highly concentrated

industries than in low-concentration ones, which is consistent with the notion that business managers reduce wages primarily through sharing less rents with the workers.

## 5.2 Intermediate Imports and Wages

Export shocks are particularly attractive because they generate a source of variation in the demand for a firm’s products without directly impacting its production technology or costs. A complementary source of variation comes from the supply of internationally sourced intermediate inputs.<sup>25</sup> These could be interpreted as a favorable cost shock for the firm (as greater availability or cheaper supply of imported intermediates likely reduce costs). Compared to the export shocks, the drawback is that some of these intermediates may be associated with automation technologies, such as numerically controlled machinery or robotic peripherals, thus directly competing against some types of workers (see Acemoglu and Restrepo, 2021). For this reason, these results should be interpreted with caution.

Nevertheless, consistent with our export results, Table A16 shows that rent-sharing responses are very different between these two types of firms (Table A15 in the Appendix confirms that these shocks are associated with similar increases in value added for firms run by business and non-business managers). Non-business managers share the resulting increase in sales and profits with their workers, with an elasticity of about 0.12 (computed in the same way as in the previous subsection). In contrast, there is again no evidence of rent-sharing for business managers.

Tables A17, A18, and A19 carry out the same additional specifications and checks as in the previous subsection, confirming broadly similar results for intermediate imports as well.

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

## 6 Selection vs. Causal Effect of Business Education

The results presented so far paint a picture in which managers with a business education reduce wages and the labor share, and this effect seems to go hand-in-hand with a significantly reduced proclivity to share rents with workers. Our various placebo checks and results from CEO retirements and IV models suggest that these are the causal effects of having a manager with a business education.

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<sup>25</sup>We also looked at the implications of competition from Chinese imports at the product level, as in Autor et al. (2013). Table A14 in the Appendix, however, shows differential exit effects for firms impacted by the Chinese shock, regardless of whether they are led by a business or non-business manager. These exit effects complicate the interpretation of Chinese import shocks on wages and the labor share, and we do not find consistent rent-sharing differences between the two types of managers in response to Chinese import shocks. Table A14 also shows that, reassuringly, there are no exit effects from export or intermediate input shocks.

These results still leave open the possibility that the causal effects on wages and labor share we estimate are associated with the selection of individuals with certain characteristics into business programs and do not reflect the impact of acquiring a business education per se. These two types of causal effects lead us, of course, to very different interpretations. If the former is true, our results would suggest that some managers have different styles, and they just happen to also go to business programs. If the latter is true, our estimates would imply that the consequences we detect originate from business education itself—either because of the curricula of these programs or some other values individuals acquire in the process. In this section, we attempt to distinguish this selection channel from a true causal effect of business education.

We build on the prior literature on college major choice, which suggests a powerful influence of peers and role models (see the references in the Introduction). Specifically, we use the Danish data to construct a role model group for our sample of managers as students who attended the same high school, graduated one year (or in some specifications two or three years) prior, and were in the same GPA quartile. This role model group helps us focus on the influence of older individuals (thus avoiding the most direct common shocks that influence students in the same class/cohort). It leverages the notion that individuals are more heavily influenced by others in their school and those ahead of them, whom they may know or can directly observe. It also factors in the idea that high-performing students are more likely to be influenced by the choices of other high-performing students (Poldin et al., 2015). As noted in Section 3, our exclusion restriction is that conditional on a manager’s high school, cohort, and GPA quartile, the outcomes at firms that he or she will later run are not affected by the college choice of his or her role model group.

Table 7 Panel A presents our first-stage relationship. Columns 1 and 2 are for the full sample of individuals we observe in high school and both control for a full set of school fixed effects, cohort fixed effects, and GPA quartile fixed effects. Column 2 additionally controls for school  $\times$  cohort fixed effects. Both columns show a strong association between the share of the role model group studying business and the share of the current cohort enrolling in a business degree. The highly significant coefficients and the F-statistics, which hover above 100, confirm that the hypothesized role model effects are quite strong. Columns 3 and 4 restrict the sample to individuals who later become company managers. In this sample, too, we find strong role model effects, but F-statistics are now lower, reflecting the much smaller sample, though still above 10.

Panel B of the same table shows that the effects are stronger for role models one or two years older than for those three years older. This is plausible, since high school students are less likely to know and interact with students three cohorts ahead of them than those in less distant cohorts.

Our main results, from estimates of equation (5), are presented in Table 8. Column 1 reports the

impact of business school education on the likelihood of ever becoming a manager (again for the full sample, with the first stage given by column 2 of Table 7). We see that the role-model-induced business education increases the likelihood of an individual becoming a manager by about 9 percentage points. Columns 2–4 report the causal effects of role-model-induced business major choice among managers on worker and firm outcomes. In these columns, we focus on the sample of individuals who later become managers, and the first-stage relationship is now given in column 4 of Table 7. In these specifications, we additionally control for the average probability of becoming a manager in each school-cohort-GPA quartile, since students in different schools, cohorts, or GPA quartiles have different likelihoods of becoming managers, as shown in column 1.<sup>26</sup> In each case, the estimated effects of business managers are very similar to our event-study results. For example, in these IV regressions, a manager with a business degree reduces average annual wages by 3.9% and the labor share by 2.8%. On the other hand, Table A21 confirms that role-model-induced business education does not lead to significant differences in other firm outcomes including employment, sales, value added, and investment. Therefore, most of our baseline effect of business managers on wages and the labor share is due to the effect of having a business education itself rather than selection into business degrees.

Table A22 in the Appendix confirms the robustness of these results by varying the set of fixed effects we control for. We find very similar, though somewhat less precisely estimated, effects when we include separate high school and cohort fixed effects, rather than a full set of school  $\times$  cohort effects.

Table A23 reports a number of Falsification exercises to support our exclusion restriction. First, we look at the effects of placebo role models, constructed as students who are in the same high school and in same cohort but in different GPA quartiles (Panel A), or in the same GPA quartile and the same cohort but in a different high school (Panel B). These placebo role models' major choices do not predict the choice of business major significantly conditional on our controls, confirming that the first stage of the IV is indeed driven by the influence of a close group of role models—one or two years ahead and in the same GPA quartile. Perhaps more important, we show that this placebo source of variation does not predict the wages or the labor share of firms later led by these managers, further bolstering our argument.

In Panel C of Table A23, we report similar results using another group of placebo role models — students in the same high school and same GPA quartile, but more than three years ahead of the current cohort. Once again, the estimates support our exclusion restriction and interpretation.

Overall, these estimates suggest that our results capture the causal effect of obtaining a business education, and are not driven by the fact that individuals less likely to share rents with workers select into business education.

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<sup>26</sup>The results are essentially identical when this variable is not included; see Table A20 in the Appendix.

## 7 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 contributing factor has been changes in wage policies of firms associated with business education of their managers/CEOs. We explore the effect of business managers on wages and the labor share using matched employer-employee datasets from the US and Denmark. In both countries, business managers reduce the wages of their employees. For example, five years after the appointment of a business manager, wages 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 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. Using manager retirements and deaths and an IV strategy based on the diffusion of the practice of appointing business managers within industry, region, and size quartile cells, we provide additional evidence that our results correspond to the causal effects of managers with a business degree. Our IV strategy is appealing not just because our instrument is orthogonal to changes in our core outcomes before the appointment of business managers or in other variables before or after managerial transitions; it is also attractive because it captures the broader spread of practices associated with business education within groups of firms with similar characteristics. Arguably reflecting this broader spread, our IV estimates are about 50% larger than our event-study or OLS estimates. Both sets of estimates imply that the contribution of managers with business education to the decline in the labor share in Denmark and in the US is sizable, but still no more than 20%-30% of the overall change.

We establish that the proximate cause of these (relative) wage effects are changes in rent-sharing practices following the appointment of business managers. In this part of the analysis, we use exogenous export demand shocks and establish that they lead to similar-sized output effects for firms run by business and non-business managers. However, while non-business managers share greater sales and profits with their workers (in fact with fairly high elasticities), business managers do not.

Finally, we use the influence of role models on college major choice to instrument the decision to enroll in a business degree in Denmark and demonstrate that our estimates correspond to causal effects of practices and values acquired in business education—rather than the selection of individuals averse to rent-sharing into business education.

In line with this last set of estimates, we interpret our results as reflecting the business-school-led shift towards emphasizing shareholder values (following Milton Friedman, 1970) and attempts to

reengineer corporations by making them leaner (Hammer and Champy, 1993). Unfortunately, in the current paper, this is no more than an interpretation, since we do not have any direct evidence on the micro channels through which business education changes managers' overall approach and wage policies.

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, especially in 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, more research is needed to bolster the case that estimates such as ours correspond to causal effects of business schools, rather than the selection of different types of individuals into business education. Our strategy based on role models on business major choice provided evidence that most of our estimates are due to the causal effect of business education. Nonetheless, it is important to supplement this finding with alternative approaches.

Fourth, we conjectured that our results may be related to the spread via business schools of shareholder values and ideas about corporation reengineering. Business schools are, of course, not the only institutions pushing firms in this direction. The other major nexus where the same effect has emerged over the last four decades has been management consulting, and it would be interesting to explore the effects of management consulting advice on firms' wage policies.

Finally, and relatedly, our study does not address what types of values and practices managers acquire in business education. It would be very interesting to match employee and manager information to the curriculum and networking practices of specific business schools to shed more light on this question.

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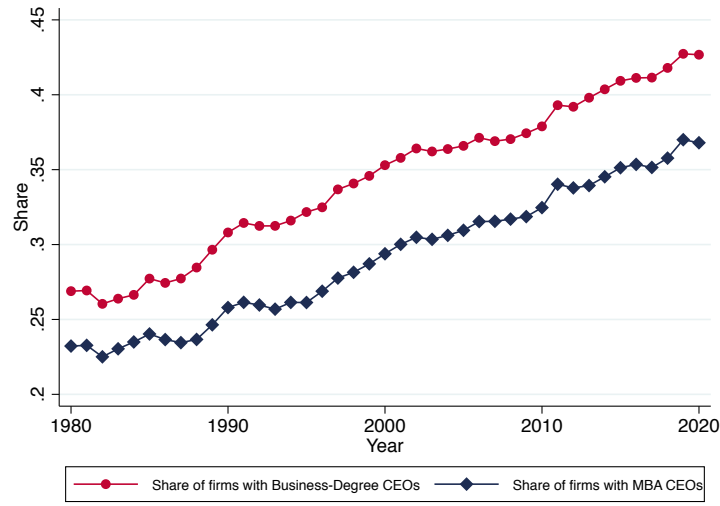
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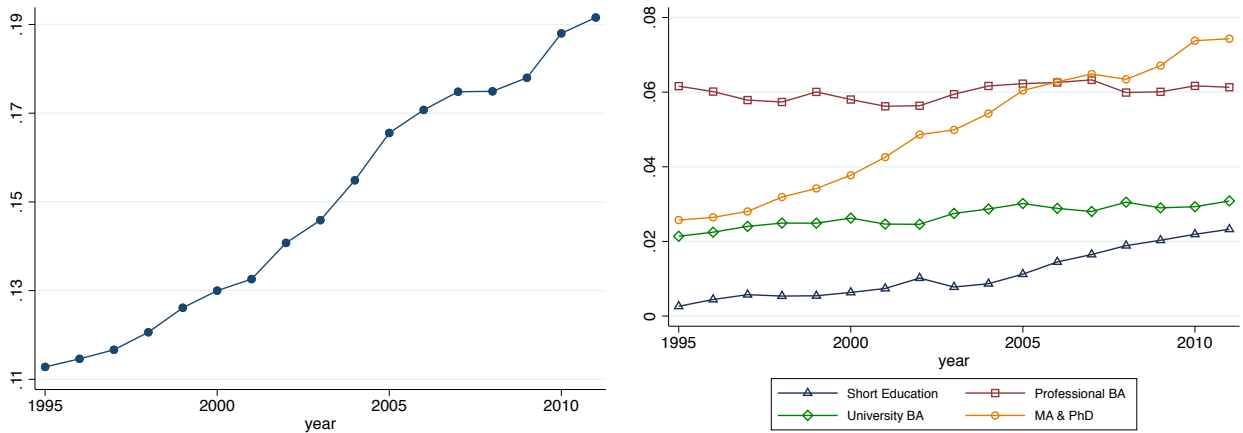
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Figure 1: Share of Compustat Firms with Business CEOs in the US



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 1980 to 2020. The education information of CEOs is from the BoardEx dataset.

Figure 2: Share of Business Managers in Denmark

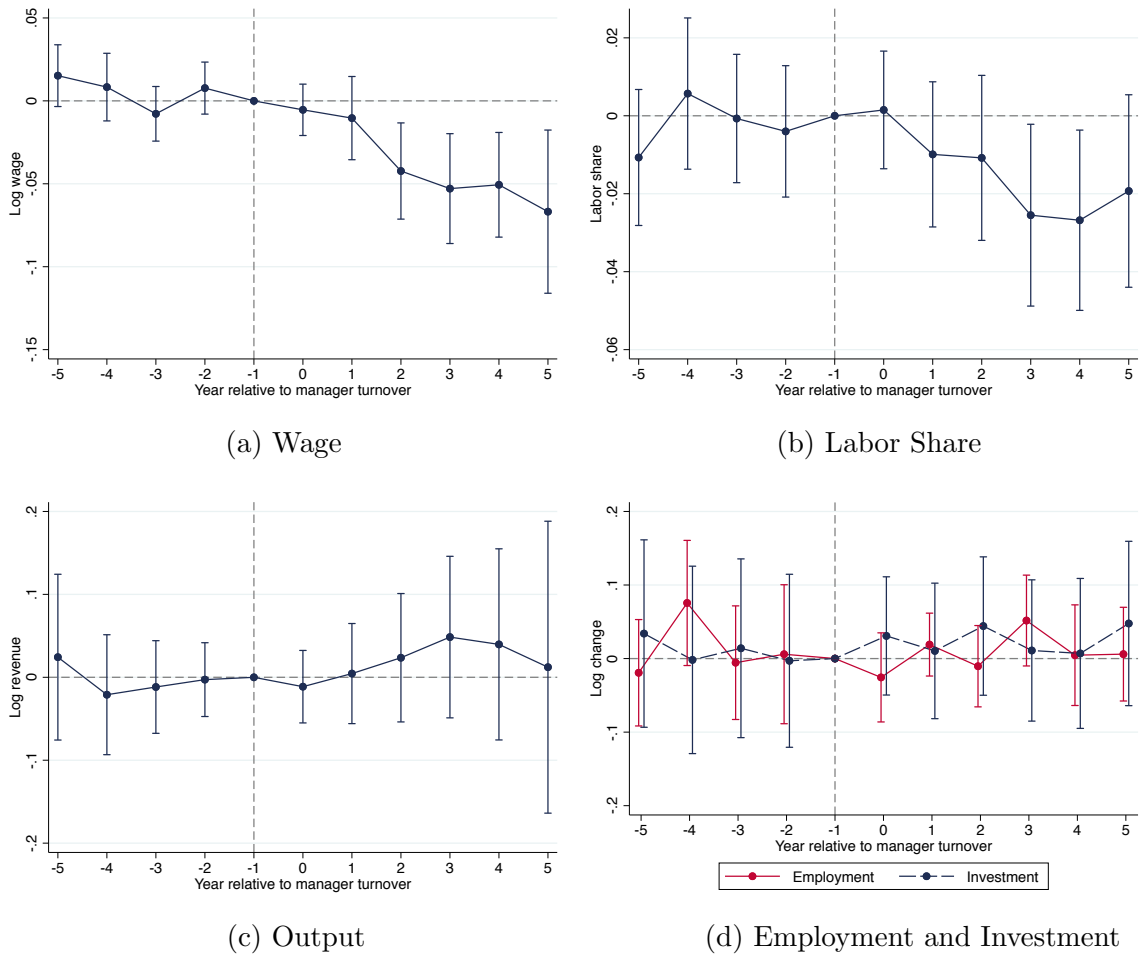


(a) Share of Business Managers

(b) by Degree Level

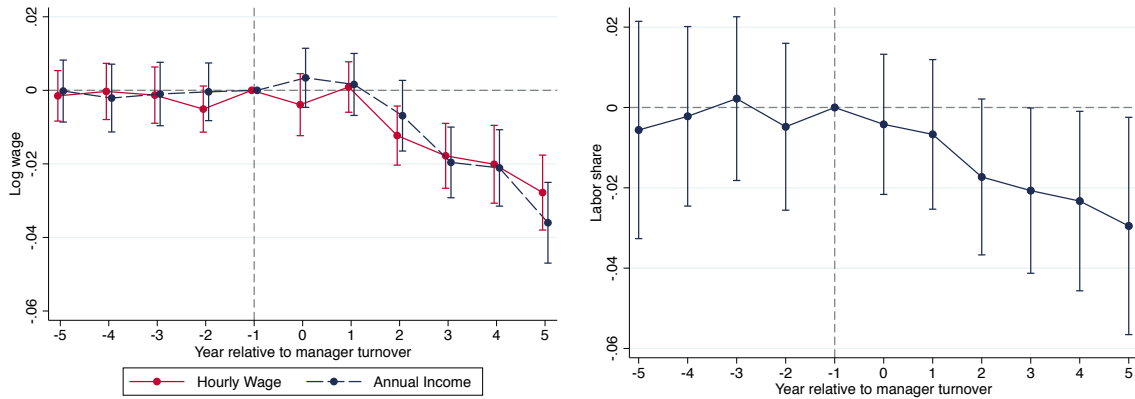
This figure plots the share of business managers in Denmark from 1995 to 2011. Panel (a) plots the share of all firms with over 5 employees in the private sector whose manager has a business degree, while Panel (b) plots the share of firms whose manager has a short-education business degree, a professional 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



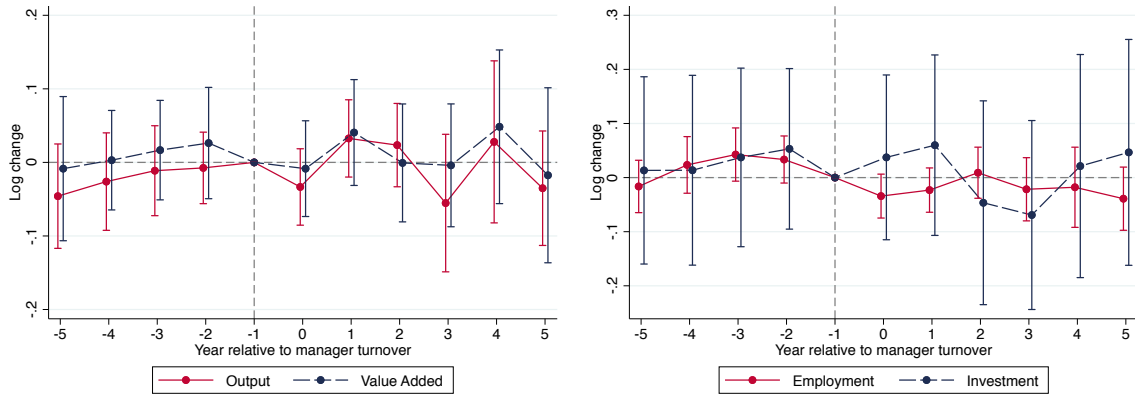
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in the US. The sample includes firms that have non-business managers in all years, and firms that have one non-business to business manager transition event during the sample period. Panel (a) uses worker-level data and the matching estimator is described in Section 3. Panels (b), (c), and (d) use firm-level data and include firm fixed effects, industry $\times$ year fixed effects, state $\times$ year fixed effects, and initial size quintile by year fixed effects. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a), the labor share in Panel (b), log sales in Panel (c), and log employment and log capital expenditure in Panel (d). The labor share is defined as total wage bill divided by sales. Investment rate is calculated from the Compustat data. 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) Wage

(b) Labor Share

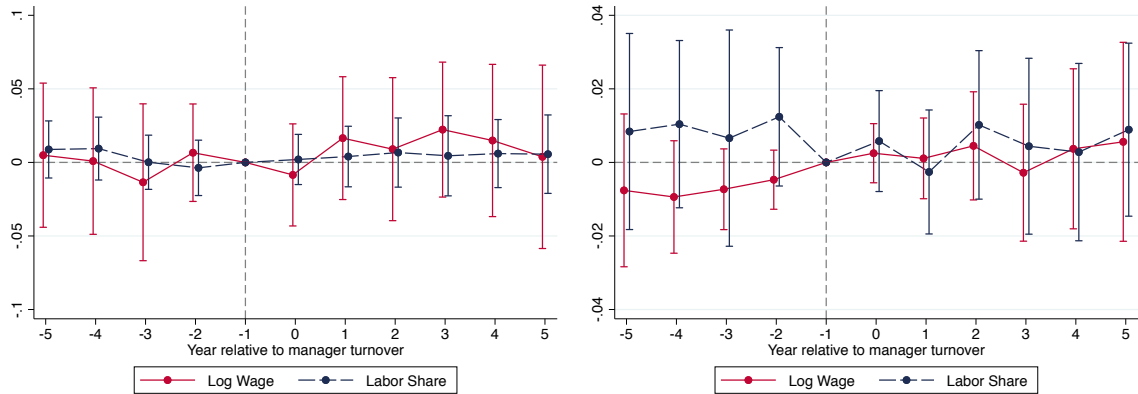


(c) Output and Value Added

(d) Employment and Investment

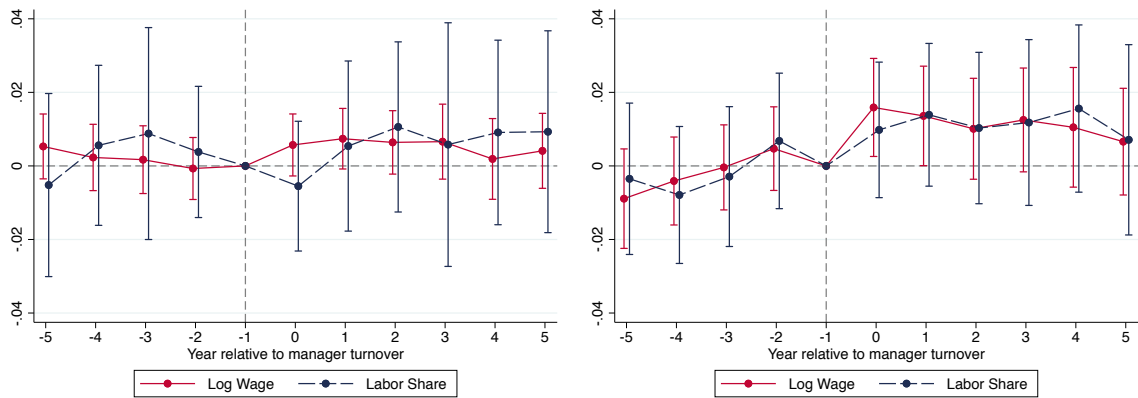
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have one non-business to business manager transition event during the sample period. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a), the labor share in Panel (b), log output and log value added in Panel (c), and log employment and log investment in Panel (d). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

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



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

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



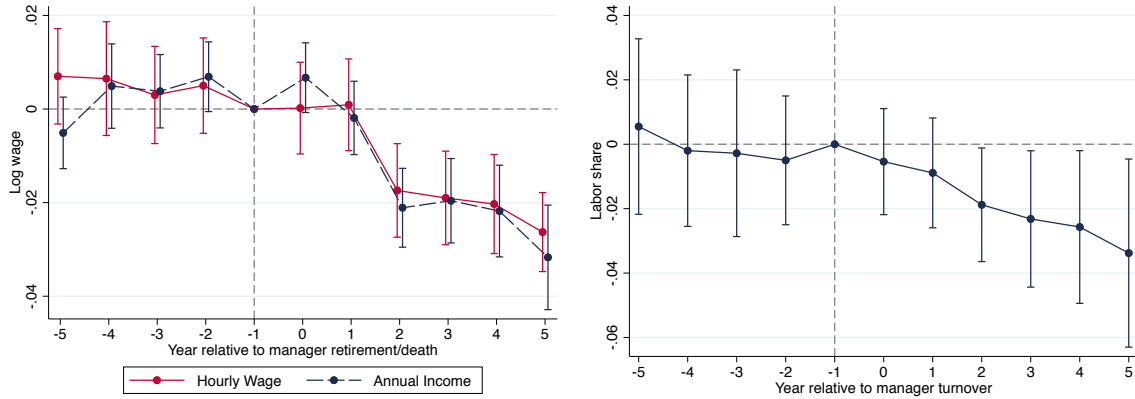
(c) Non-college to college, Denmark

(d) Non-master to master, Denmark

This figure plots event-study estimates and 95% confidence intervals based on placebo manager transitions. In Panel (a), events are manager transitions from a non-business manager to another non-business manager in the US. In Panel (b), events are manager transitions from a non-business manager to another non-business manager in the Denmark. In Panel (c), events are manager transitions from a non-college-educated manager to a college-educated manager in Denmark. In Panel (d), events are manager transitions from a manager without a master’s degree or PhD to a manager with a master’s degree or PhD in Denmark. The dependent variables are log annual wage at the worker level and the labor share at the firm level. Standard errors are clustered at the firm level.

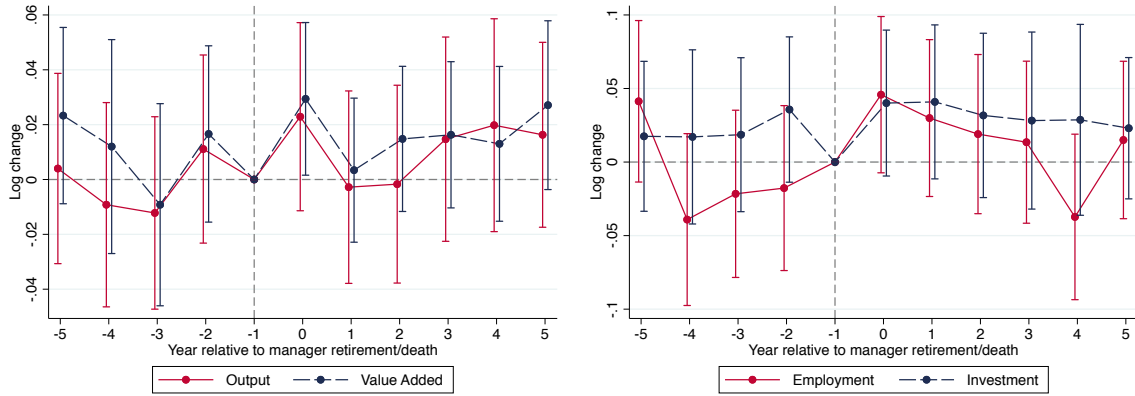


Figure 6: Changes in Firm and Worker Outcomes around Transitions to Business Managers due to Manager Retirements and Deaths in Denmark



(a) Wage

(b) Labor Share

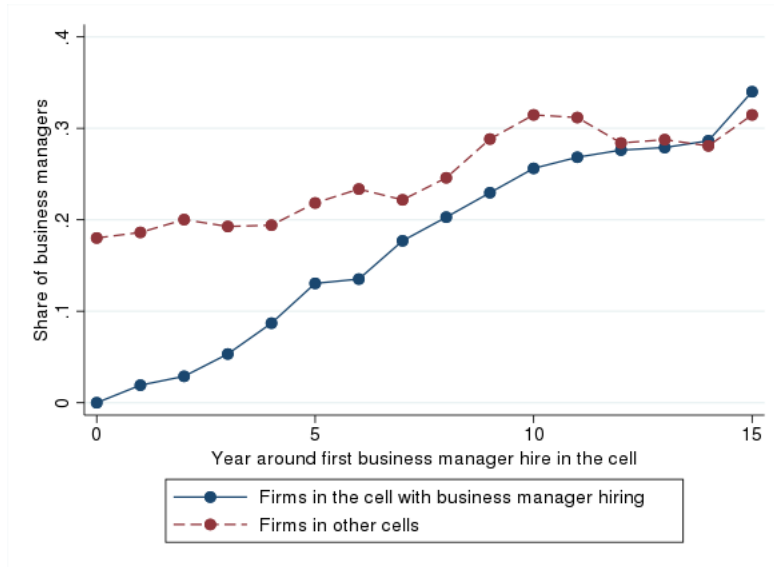


(c) Output and Value Added

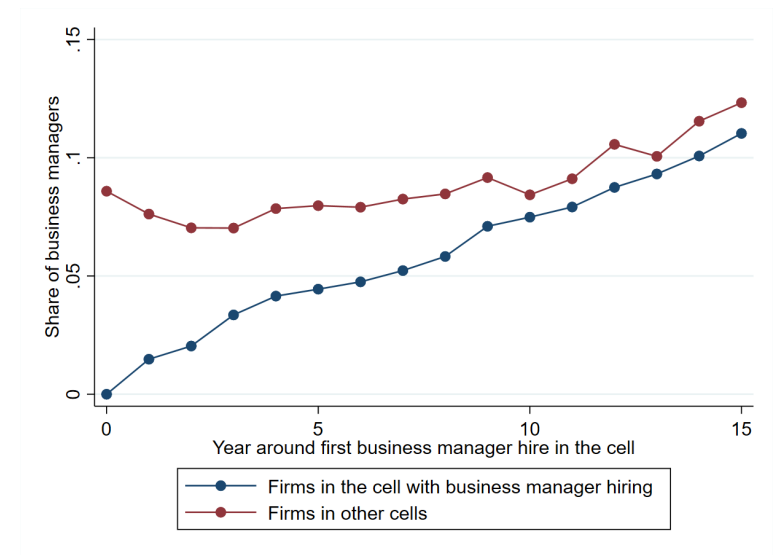
(d) Employment and Investment

This figure plots event-study estimates and 95% confidence intervals, where events are retirements (as defined in the text) or sudden deaths of non-business managers with the successor being a business manager in Denmark. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a), the labor share in Panel (b), log output and log value added in Panel (c), and log employment and log investment in Panel (d). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Figure 7: Visual Representation of the First Stage for Business Manager Transitions



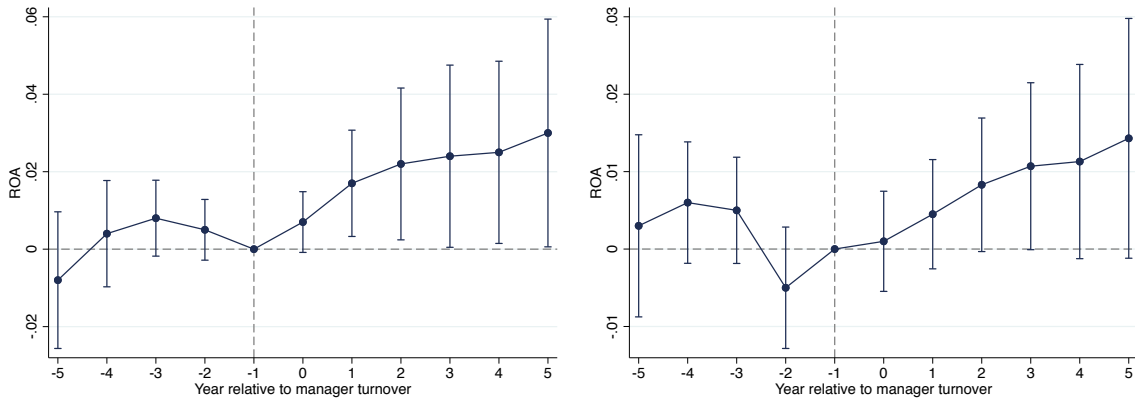
(a) US



(b) Denmark

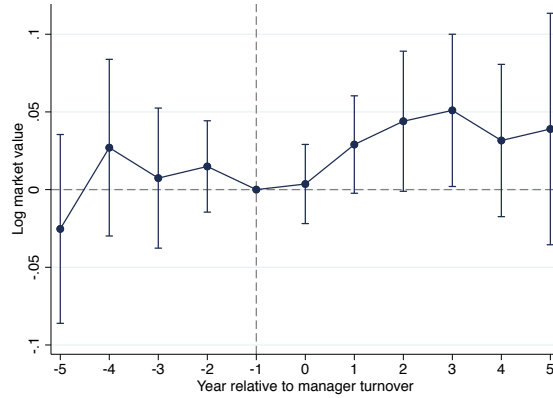
This figure plots average fraction of firms in a region-industry-size cell with no past business manager (blue), where zero is the year of first business manager hiring in the cell. For comparison, we also plot average fraction of firms in other cells (red) in the same year. Panel (a) is for the US and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

Figure 8: Changes in Return on Assets and Market Value around Non-Business to Business Manager Transitions



(a) ROA, US firms

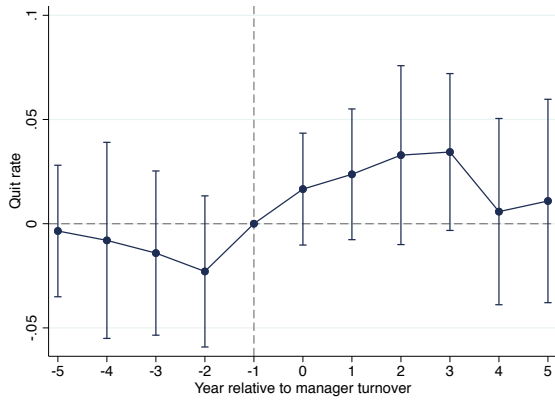
(b) ROA, Danish firms



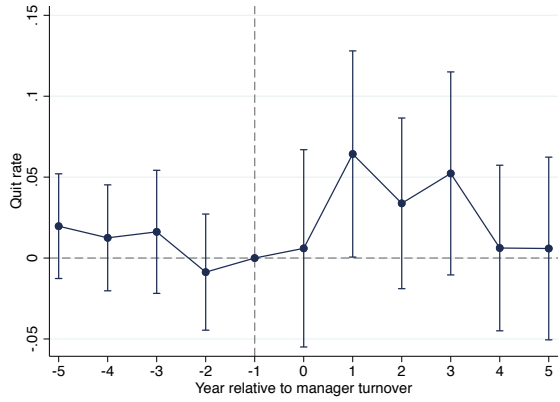
(c) Market value, US firms

This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. Panels (a) and (c) use US Compustat data, and Panel (b) uses data from Denmark. In Panels (a) and (b), the dependent variable is return on assets (ROA), defined as earnings before interests and taxes (or profits in Denmark) divided by total assets. In Panel (c), the dependent variable is log market value, which is total assets minus the common equity book value and adding back the market value of the common equity. All specifications include firm fixed effects, industry $\times$ year fixed effects, state(region) $\times$ year fixed effects, and initial size quintile by year fixed effects. Standard errors are clustered at the firm level.

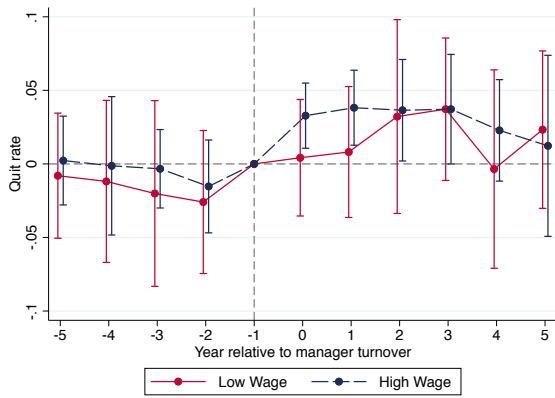
Figure 9: Changes in Quit Rates around Non-Business to Business Manager Transitions



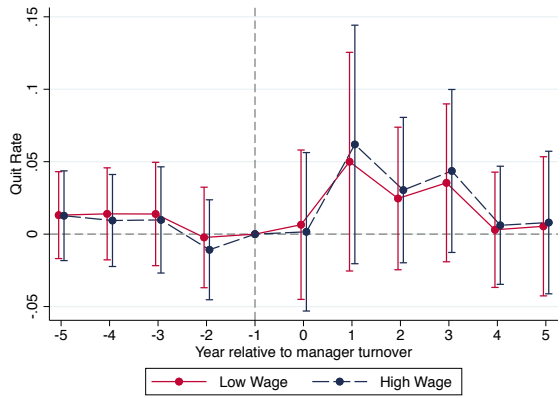
(a) Quit rates of all workers, US



(b) Quit rates of all workers, Denmark



(c) Quit rates by wage level, US



(d) Quit 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 the share of workers that quit the firm to join another firm or become unemployed in the following year. In Panels (c) and (d), the solid line is quit rate for workers below the median wage, and the dashed line is quit rate for workers above the median wage. All regressions include firm fixed effects, industry $\times$ year fixed effects, state(region) $\times$ year fixed effects, and initial size quintile by year fixed effects, and observations are weighted by employment. Standard errors are clustered at the firm level.

Table 1: 2SLS and OLS Estimates of Business Managers on Firm Outcomes in the US

**Panel A: IV**

	Log Sales			Log Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	-0.027 (0.052)	-0.051 (0.046)	-0.032 (0.043)	-0.094 (0.055)	-0.075 (0.044)	-0.105 (0.041)	-0.035 (0.019)	-0.023 (0.015)	-0.036 (0.016)
Log Average Wage t-1	x	x	x	x	x	x	x	x	x
Log Average Wage t-2		x	x		x	x		x	x
Log Average Wage 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

**Panel B: OLS**

	Log Sales			Log Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	-0.006 (0.008)	-0.004 (0.008)	-0.012 (0.007)	-0.059 (0.013)	-0.047 (0.013)	-0.063 (0.011)	-0.021 (0.003)	-0.012 (0.003)	-0.023 (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

This table reports 2SLS (Panel A) and OLS (Panel B) estimates of business managers on log sales, log average wage 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 (2) and is based on the average lagged business manager of peer firms (those in the same industry, region and size quartile). Table A3 in the Appendix presents the first-stage estimates and Figure 7 shows them visually. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Columns 1, 4 and 7 control for one-year lags of firm (log) sales and (log) average wage, columns 2, 5 and 8 control for one and two-year lags of firm sales and average wage, and columns 3, 6 and 9 control for one, two and three-year lags of firm sales and average wage. The dependent variables are log sales in columns 1–3, log average annual wage 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 2: 2SLS and OLS Estimates of Business Managers on Firm Outcomes in Denmark

**Panel A: IV**

	Log Value Added			Log Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.105 (0.068)	0.091 (0.068)	0.078 (0.067)	-0.048 (0.024)	-0.038 (0.024)	-0.044 (0.024)	-0.037 (0.027)	-0.046 (0.027)	-0.052 (0.027)
Log Average Wage t-1	-0.179 (0.010)	-0.155 (0.010)	-0.157 (0.010)	0.129 (0.005)	0.128 (0.005)	0.125 (0.005)	0.002 (0.004)	0.010 (0.004)	0.011 (0.004)
Log Average Wage t-2		-0.028 (0.011)	-0.018 (0.011)		-0.011 (0.005)	-0.010 (0.005)		-0.018 (0.005)	-0.009 (0.005)
Log Average Wage t-3			-0.066 (0.010)			-0.038 (0.005)			-0.020 (0.004)
Log Value Added t-1	0.479 (0.004)	0.441 (0.005)	0.440 (0.005)	0.006 (0.002)	0.006 (0.002)	0.006 (0.002)	-0.011 (0.002)	-0.019 (0.002)	-0.021 (0.002)
Log Value Added t-2		0.069 (0.005)	0.060 (0.005)		-0.001 (0.002)	0.002 (0.003)		0.018 (0.002)	0.005 (0.002)
Log Value Added t-3			0.016 (0.004)			-0.007 (0.002)			0.026 (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 Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.007 (0.015)	0.008 (0.015)	0.008 (0.015)	-0.028 (0.006)	-0.028 (0.006)	-0.029 (0.006)	-0.025 (0.007)	-0.024 (0.007)	-0.024 (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 average wage and the labor share in Denmark. For the 2SLS models, the first stage is described in equation (2) and is based on the average lagged business manager of peer firms (those in the same industry, region and size quartile). Table A4 in the Appendix presents the first-stage estimates and Figure 7 shows them visually. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Columns 1, 4 and 7 control for one-year lags of firm (log) value added and (log) average wage, columns 2, 5 and 8 control for one and two-year lags of firm value added and average wage, and columns 3, 6 and 9 control for one, two and three-year lags of firm value added and average wage. The dependent variables are log value added in columns 1–3, log average annual wage 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 3: Business Managers and Wage Response to Export Shocks in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.017 (0.003)	0.022 (0.004)	-0.013 (0.009)	0.013 (0.003)	0.015 (0.004)	-0.001 (0.008)
Export Shock*Business Manager	0.002 (0.007)	0.010 (0.007)	-0.027 (0.012)	-0.003 (0.007)	-0.001 (0.006)	-0.018 (0.009)
Log Output				0.013 (0.003)	0.013 (0.005)	-0.163 (0.011)
Log Employment				0.014 (0.004)	0.045 (0.005)	0.164 (0.012)
Log Capital-labor Ratio				0.003 (0.001)	0.000 (0.001)	-0.010 (0.004)
Share of High-skilled Workers				0.088 (0.020)	0.076 (0.025)	0.224 (0.056)
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE			Y			Y
Obs	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

This table reports the coefficients from regressions of wages and the labor share on export shocks and their interactions with a firm-level indicator 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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ 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 income 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 4: Response to Positive and Negative Export Shocks in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Pos Export Shock*Non-Business Manager	0.022 (0.005)	0.017 (0.004)	-0.006 (0.012)	0.020 (0.005)	0.022 (0.004)	-0.003 (0.011)
Pos Export Shock*Business Manager	0.006 (0.011)	0.008 (0.007)	-0.033 (0.013)	0.005 (0.011)	0.008 (0.007)	-0.026 (0.013)
Neg Export Shock*Non-Business Manager	0.004 (0.004)	0.001 (0.003)	-0.011 (0.008)	-0.006 (0.004)	0.005 (0.004)	-0.008 (0.008)
Neg Export Shock*Business Manager	0.007 (0.010)	0.006 (0.007)	-0.016 (0.014)	0.003 (0.010)	0.010 (0.007)	-0.004 (0.013)
Log Output				0.012 (0.003)	0.013 (0.005)	-0.164 (0.011)
Log Employment				0.013 (0.004)	0.044 (0.005)	0.165 (0.012)
Log Capital-labor Ratio				0.003 (0.001)	0.000 (0.001)	-0.010 (0.004)
Share of High-skilled Workers				0.079 (0.020)	0.072 (0.028)	0.228 (0.056)
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE			Y			Y
Obs	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

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This table reports the coefficients from regressions of wages and the labor share on positive and negative export shocks and their interactions with a firm-level indicator for having a business manager. Positive and negative export shocks are defined in section 3. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ 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 income 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 5: Response to Export Shocks Before and After Manager Transitions in Denmark

**Panel A: Non-Business to Business Manager Transitions**

	Value Added per Worker		Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Export Shock*Pre	0.084 (0.042)	0.074 (0.033)	0.031 (0.007)	0.017 (0.003)	0.030 (0.009)	0.021 (0.006)	0.006 (0.009)	-0.013 (0.007)
Export Shock*Post	0.103 (0.044)	0.082 (0.039)	0.012 (0.008)	0.002 (0.008)	0.013 (0.009)	-0.012 (0.010)	-0.021 (0.011)	-0.028 (0.010)
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	1,582	6,296	544,119	1,303,209	544,117	1,303,050	1,402	5,504

**Panel B: Placebo Non-Business to Non-Business Manager Transitions**

	Value Added per Worker		Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Export Shock*Pre	0.083 (0.032)	0.079 (0.021)	0.017 (0.003)	0.018 (0.003)	0.019 (0.004)	0.020 (0.004)	-0.012 (0.009)	-0.014 (0.008)
Export Shock*Post	0.082 (0.031)	0.081 (0.021)	0.018 (0.003)	0.019 (0.003)	0.018 (0.004)	0.020 (0.004)	-0.010 (0.009)	-0.014 (0.009)
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	1,881	5,950	597,871	1,244,708	597,808	1,244,543	1,770	5,211

This table reports the coefficients from the regression of wages, value added per worker and the labor share on export shocks before and after manager transitions. *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. In Panel A, columns 1, 3, 5 include firms with one manager transition from a non-business manager to a business manager, and columns 2, 4, 6 also include firms that always had non-business managers (for which *Pre* equals 1 for all years). In Panel B, columns 1, 3, 5 include firms with one manager transition from a non-business manager to a non-business manager, and columns 2, 4, 6 also include firms that always 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, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. 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 income 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, 3, 7, 8 are weighted by firm employment. Standard errors are clustered at the firm level.

Table 6: IV Estimates of Wage Response to Export Shocks in Denmark

	Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*(1-Predicted Business Manager)	0.019 (0.002)	0.013 (0.001)	0.021 (0.004)	0.017 (0.003)	-0.002 (0.012)	-0.002 (0.010)
Export Shock*Predicted Business Manager	0.001 (0.004)	-0.006 (0.003)	0.000 (0.007)	-0.001 (0.004)	-0.028 (0.013)	-0.019 (0.011)
Log Output		-0.004 (0.003)		0.011 (0.005)		-0.174 (0.016)
Log Employment		0.092 (0.006)		0.099 (0.010)		0.172 (0.017)
Log Capital-labor Ratio		0.002 (0.001)		-0.006 (0.001)		-0.011 (0.005)
Share of High-skilled Workers		0.395 (0.033)		0.277 (0.058)		0.322 (0.075)
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	737,000	737,000	737,000	737,000	2,917	2,917

This table reports the IV estimates of the effect of business manager on the relationship between export shocks and wages and the labor share. The predicted business manager in each column is the predicted value from regressing whether a firm has a business manager on the lags of the share of its peer firms in the same industry $\times$ region $\times$ size cell that have a business manager, controlling for the same variables and fixed effects as that column. 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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 2, 4 and 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 2, log annual income of workers in columns 3 and 4, and the labor share of firms (wage bill divided by value added) in columns 5 and 6. Standard errors are clustered at the firm level.

Table 7: First Stage for Role Model Effects in Business Major Choice in Denmark

**Panel A:**

	Business Degree			
	(1)	(2)	(3)	(4)
Share of Role Models with Business Degree	0.101 (0.007)	0.101 (0.008)	0.204 (0.062)	0.291 (0.083)
School FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
School-cohort FE	N	Y	N	Y
GPA Quartile FE	Y	Y	Y	Y
F statistic	187.2	141.3	10.7	12.4
Obs	505,971	505,970	13,736	13,076

**Panel B:**

	Business Degree			
	(1)	(2)	(3)	(4)
Share of Role Models with Business Degree (one year before)	0.087 (0.008)	0.093 (0.009)	0.246 (0.069)	0.280 (0.092)
Share of Role Models with Business Degree (two years before)	0.083 (0.008)	0.083 (0.009)	0.115 (0.068)	0.133 (0.091)
Share of Role Models with Business Degree (three years before)	0.056 (0.008)	0.055 (0.009)	0.074 (0.069)	0.092 (0.091)
School FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
School-cohort FE	N	Y	N	Y
GPA Quartile FE	Y	Y	Y	Y
Obs	469,527	469,526	11,570	10,931

This table reports the first stage for our role model effect regressions. The dependent variable is a dummy for whether the person has a business degree. The key right hand side variable (instrument) is the share of role models with a business degree, where role models are defined as students in the previous cohort in the same high school and same GPA quartile. In Panel B, we also look at role model groups defined by students in the same high school and same GPA quartile but two and three cohorts before. Columns 1 and 2 are for the sample of all students in our data set, while columns 3 and 4 are for the sample of all students who later become a manager in at least one firm in our data set. All columns control for high school fixed effects, cohort fixed effects and GPA quartile fixed effects. Columns 2 and 4 also control for high school×cohort fixed effects. Standard errors are clustered at the high-school level.

Table 8: 2SLS Estimates of Business Major Choice on Firm and Worker Outcomes in Denmark

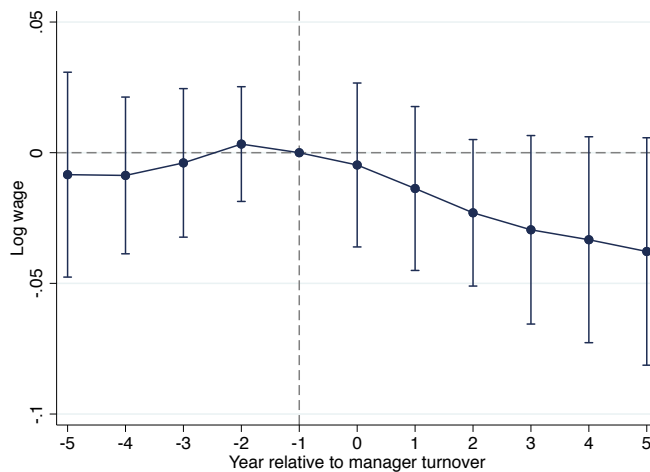
	Becoming a manager	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)	(4)
Business Degree	0.087 (0.043)	-0.039 (0.017)	-0.045 (0.022)	-0.028 (0.015)
School-cohort FE	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y
F statistic	141.3	12.4	12.4	13.0
Obs	505,971	13,076	13,076	9,191

This table reports 2SLS estimates of the effect of business major choice of managers on workers annual and hourly wages and the firm’s labor share. The first stage is provided in Table 7. Column 1 looks at the effect of becoming a manager for the full sample of students (first stage corresponding to column 2 of Table 7), while columns 2–4 are for the sample of managers (first stage corresponding to column 4 of Table 7). In column 2 the dependent variable is residualized log average annual wage of workers employed under the manager. In column 3 the dependent variable is residualized log average hourly wage of workers employed under the manager. In column 4, the dependent variable is residualized labor share of the firm operated by the manager. These outcomes are residualized by regressing them on industry×year fixed effects, firm fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. All columns control for high school×cohort fixed effects and GPA quartile fixed effects. Columns 2–4 additionally control for the average propensity to become a manager within the school-cohort-GPA quartile group. Standard errors are clustered at the high-school level. First-stage F-statistics for the excluded instruments are reported at the bottom.

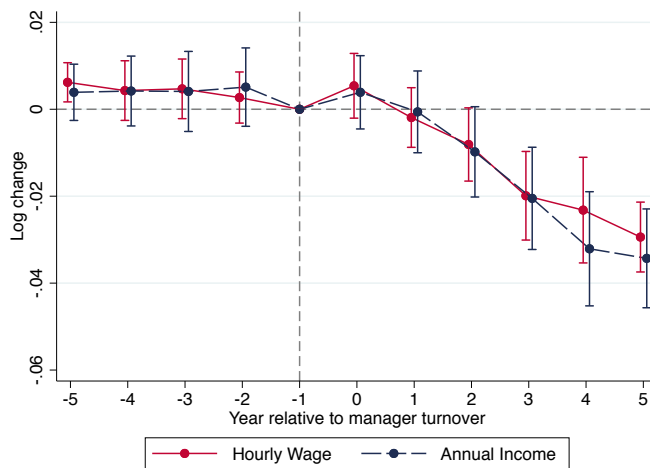
# A Appendix (For Online Publication)

## A.1 Additional Figures and Tables

Figure A1: Changes in Firm Average Wage around Non-Business to Business Manager Transitions



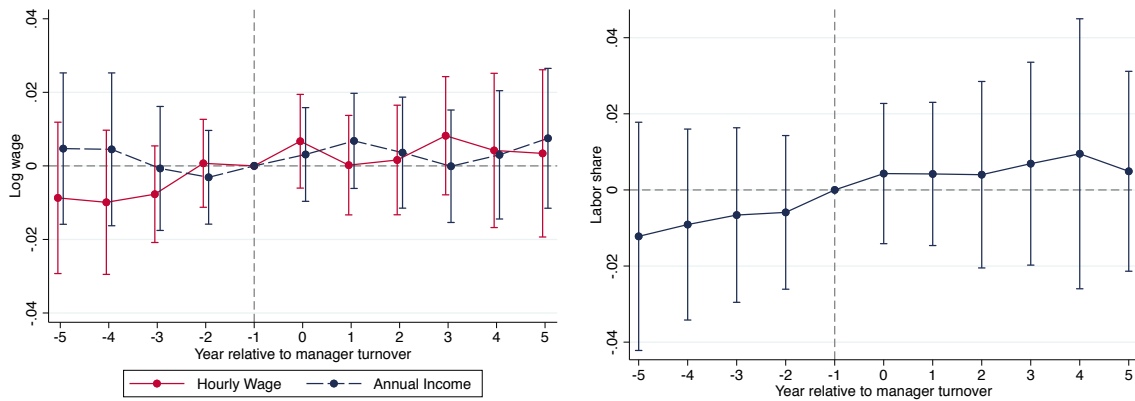
(a) US



(b) Denmark

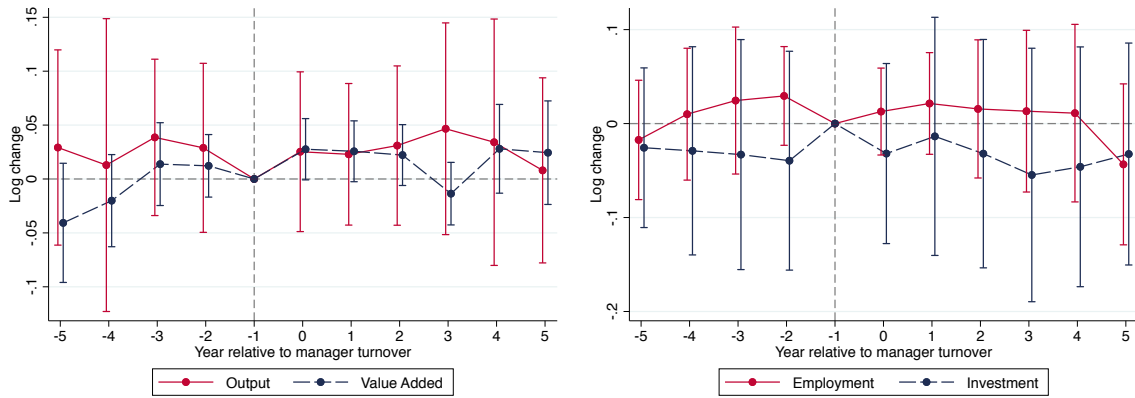
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager. Panel (a) is based on US data and the dependent variable is log average annual income. Panel (b) is based on Danish data and the dependent variables are log average hourly wage and log average annual income. All regressions include firm fixed effects, industry $\times$ year fixed effects, region(state) $\times$ year fixed effects, and initial size quintile by year fixed effects. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Figure A2: Changes in Firm and Worker Outcomes around Business to Non-Business Manager Transitions in Denmark



(a) Wage

(b) Labor Share

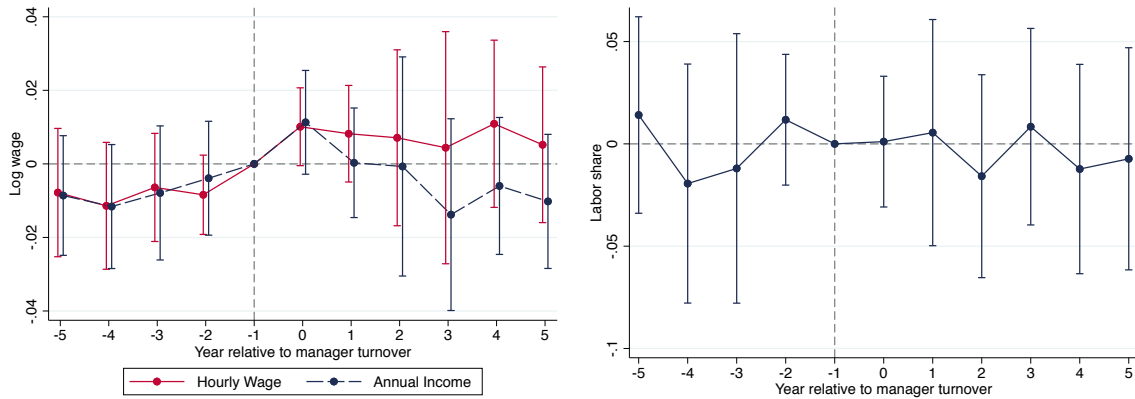


(c) Output and Value Added

(d) Employment and Investment

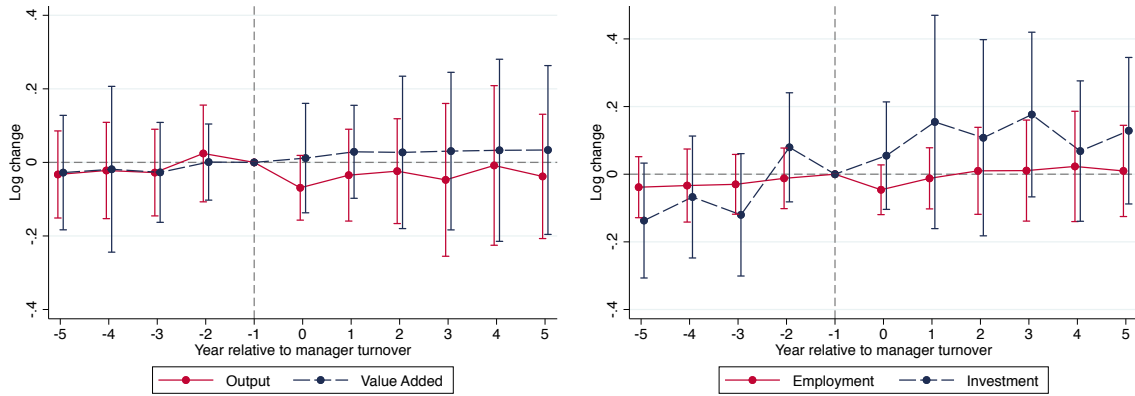
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a business manager to a non-business manager in Denmark. The sample includes firms that have business managers in all years, and firms that have one business to non-business manager transition event during the sample period. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a), the labor share in Panel (b), log output and log value added in Panel (c), and log employment and log investment in Panel (d). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Figure A3: Changes in Firm and Worker Outcomes around Non-Economics to Economics Manager Transitions in Denmark



(a) Wage

(b) Labor Share

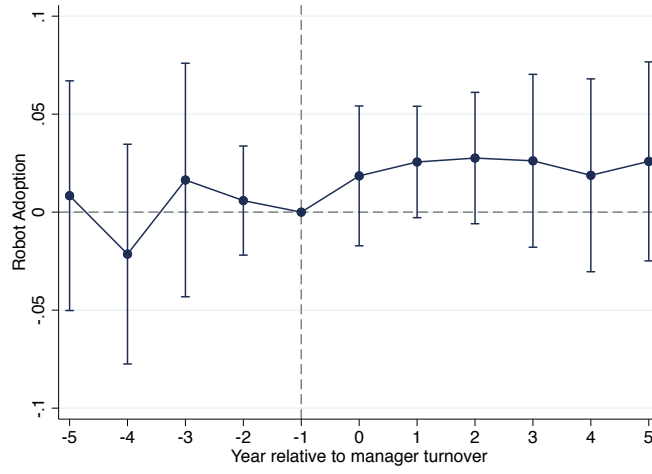


(c) Output and Value Added

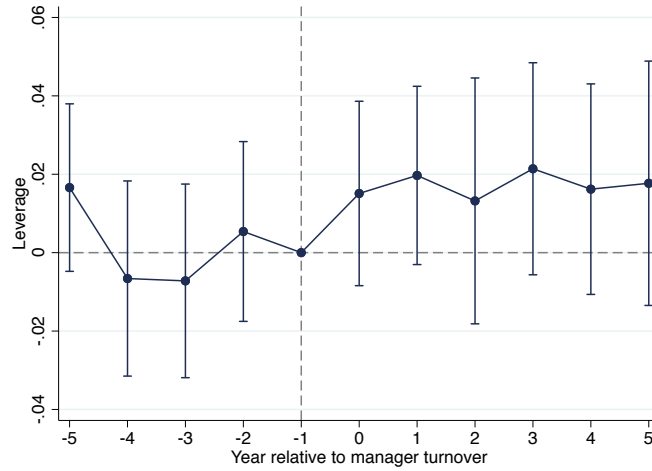
(d) Employment and Investment

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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a), the labor share in Panel (b), log output and log value added in Panel (c), and log employment and log investment in Panel (d). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Figure A4: Changes in Robot Purchases and Leverage around Non-Business to Business Manager Transitions in Denmark



(a) Robot Adoption

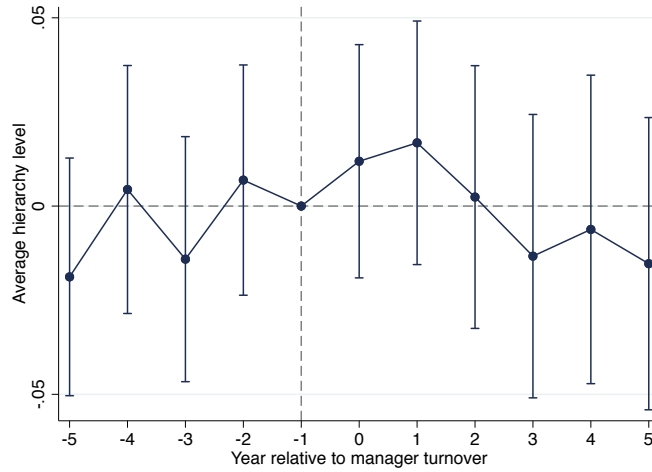


(b) Leverage

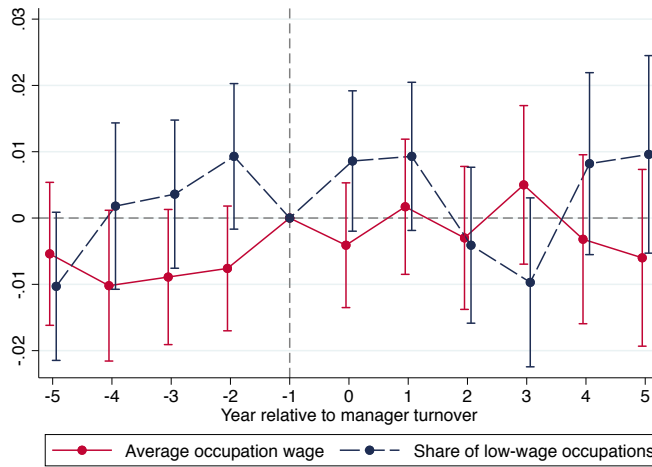
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. The dependent variables are a dummy variable for robot adoption in Panel (a), and leverage (total debt divided by total assets) in Panel (b). Robot adoption is proxied by positive imports of robots. All regressions include firm fixed effects, industry×year fixed effects, region×year fixed effects, and initial size quintile by year fixed effects, and observations are weighted by employment. Standard errors are clustered at the firm level.



Figure A5: Changes in Organization and Occupation Structure around Non-Business to Business Manager Transitions in Denmark



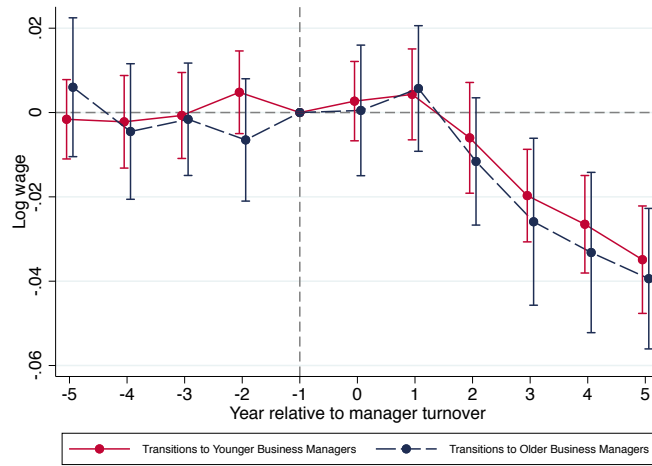
(a) Average Hierarchy Level



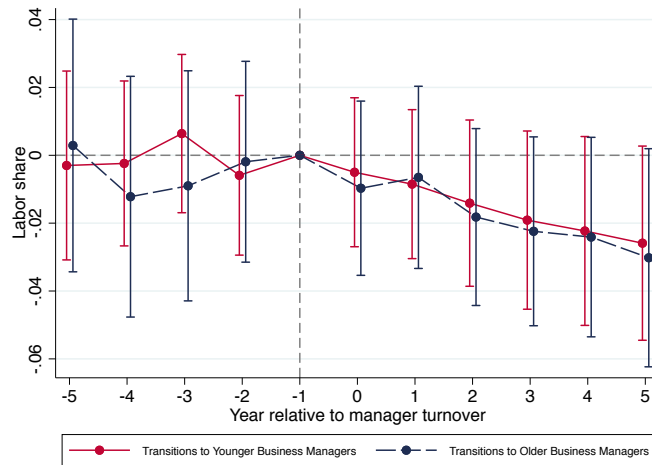
(b) Occupation Average Wage and Share of Low-skilled Occupations

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 variable is the average hierarchy level of workers of the firm. 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 initial size quintile by year fixed effects, and observations are weighted by employment. Standard errors are clustered at the firm level.

Figure A6: Changes in Wages and the Labor Share around Non-Business to Younger or Older Business Manager Transitions in Denmark



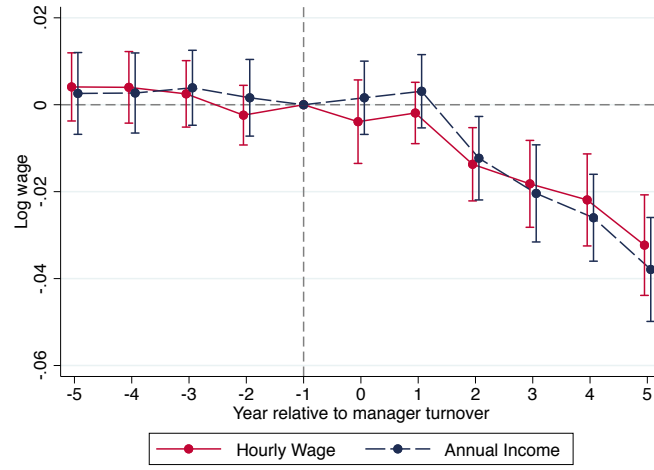
(a) Wage



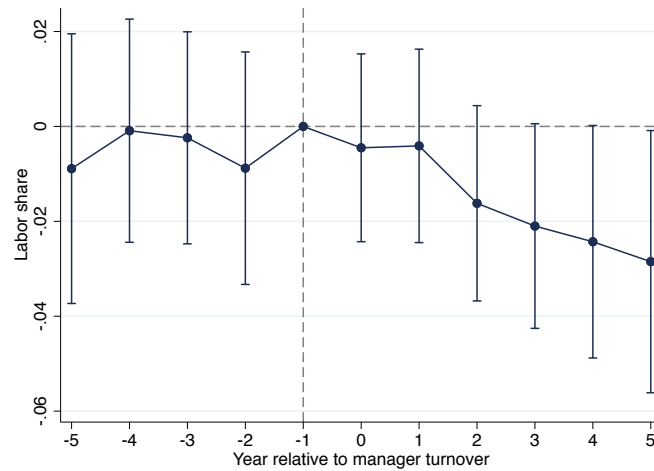
(b) Labor Share

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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log annual income in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Figure A7: Changes in Wages and the Labor Share around Non-Business to Business Manager Transitions Excluding Family CEOs in Denmark



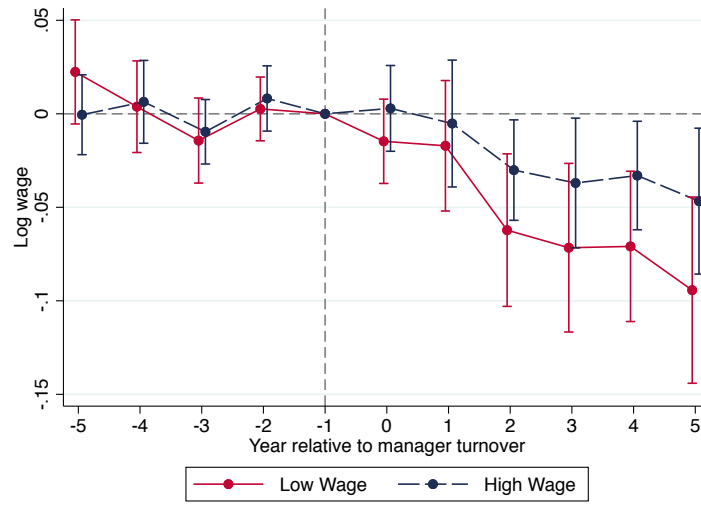
(a) Wage



(b) Labor Share

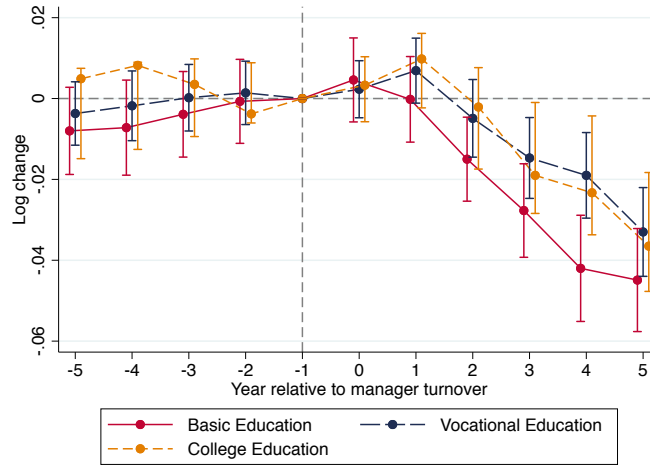
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. We exclude manager transitions involving family CEOs, who are related by blood or marriage to the previous or successor CEOs at their firms. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Figure A8: Heterogeneous Effects of Business Managers on Wages in the US



This figure plots event-study estimates and 95% confidence intervals separately by different worker groups, where events are manager transitions from a non-business manager to a business manager in the US. The blue line plots wage effects for workers with initial wage above the median (“high wage” workers), and the red line plots wage effects for workers with initial wage below the median (“low wage” workers). The dependent variable is log annual wage.

Figure A9: Heterogeneous Effects of Business Managers on Wages in Denmark



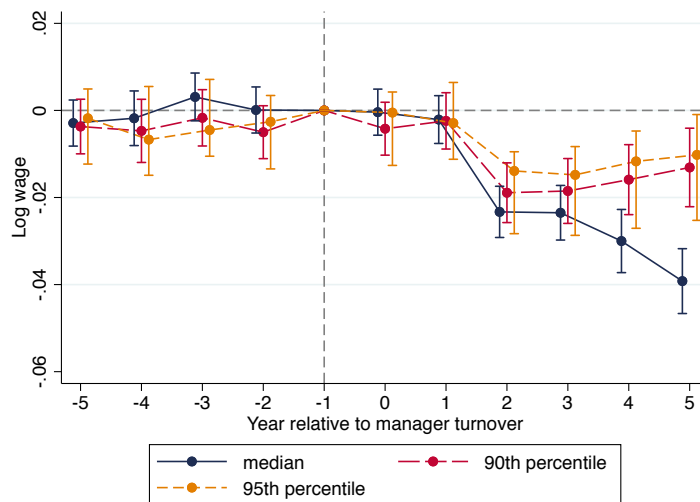
(a) By Education



(b) By Wage Level

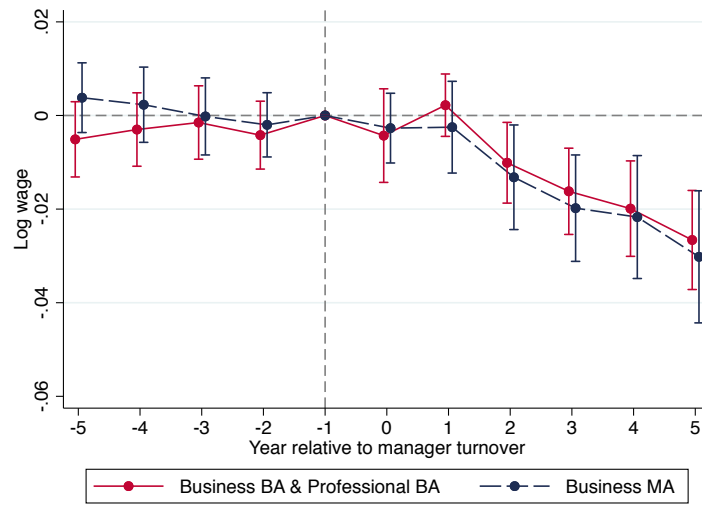
This figure plots event-study estimates and 95% confidence intervals separately by different worker groups, where events are manager transitions from a non-business manager to a business manager in Denmark. Panel (a) separately estimates the event-study regressions by workers' education level (basic education, vocational education, and college education). Panel (b) splits the workers into two groups based on their initial wage level ("high wage" workers are workers with initial wage above the median, and "low wage" workers are workers with initial wage below the median). The dependent variable is log annual wage.

Figure A10: Changes in Wage Percentiles around Non-Business to Business Manager Transitions in Denmark



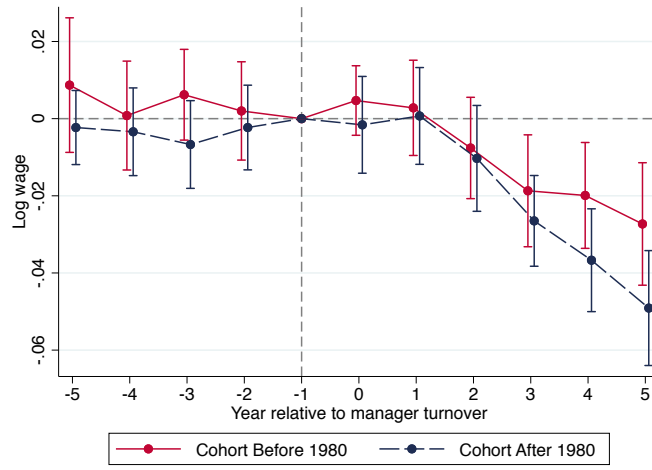
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. The dependent variables are percentiles of the wage distribution (median, 90th percentile, 95th percentile) within the firm in a given year. All regressions include firm fixed effects, industry×year fixed effects, state(region)×year fixed effects, and initial size quintile by year fixed effects, and observations are weighted by employment. Standard errors are clustered at the firm level.

Figure A11: Heterogeneous Effects on Wages by the Degree Level of Business Managers in Denmark

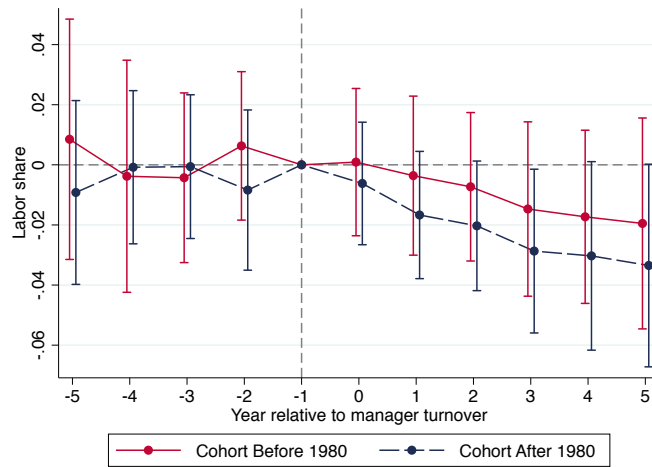


This figure plots event-study estimates and 95% confidence intervals from separate regressions for manager transitions from a non-business manager to a business manager who has a business BA or professional BA degree but doesn't have a business MA degree (red line) and manager transitions from a non-business manager to a business manager with a business MA degree (blue line) in Denmark. The dependent variable is log annual wage. Standard errors are clustered at the firm level.

Figure A12: Changes in Wages and the Labor Share around Non-Business to Business Manager Transitions for Business Managers in Different Cohorts in Denmark



(a) Wage

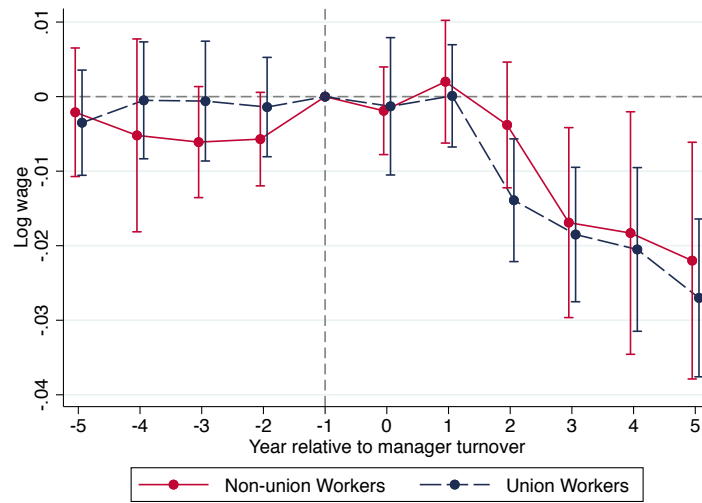


(b) Labor Share

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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (a) additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The dependent variables are log hourly wage and log annual income in Panel (a) and the labor share in Panel (b). The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

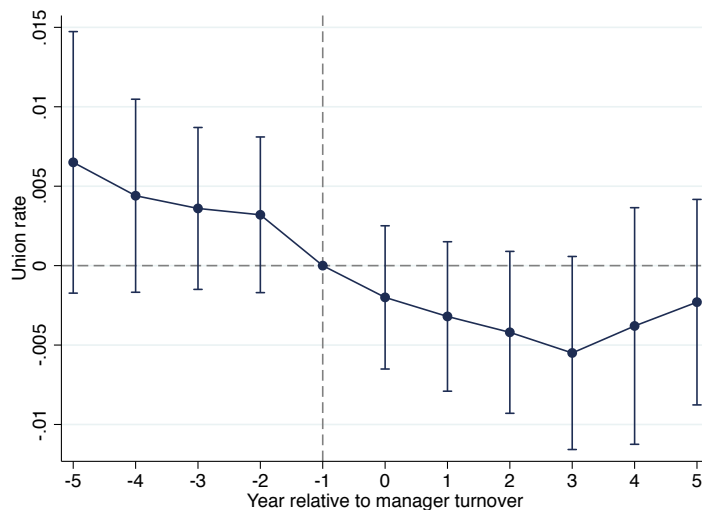


Figure A13: Changes in Wages around Non-Business to Business Manager Transitions for Union and Non-Union Workers in Denmark



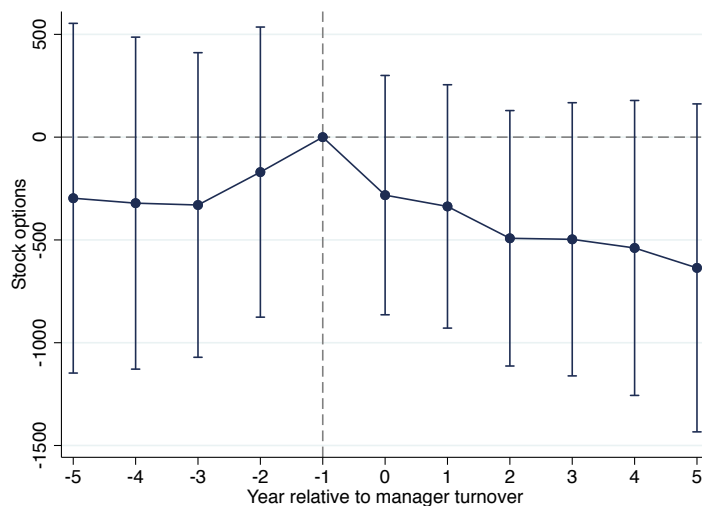
This figure plots event-study estimates and 95% confidence intervals separately for workers who are union members and workers who are not union members, where events are manager transitions from a non-business manager to a business manager in Denmark. The dependent variable is log annual wage. Standard errors are clustered at the firm level.

Figure A14: Changes in Unionization Rates around Non-Business to Business Manager Transitions in Denmark



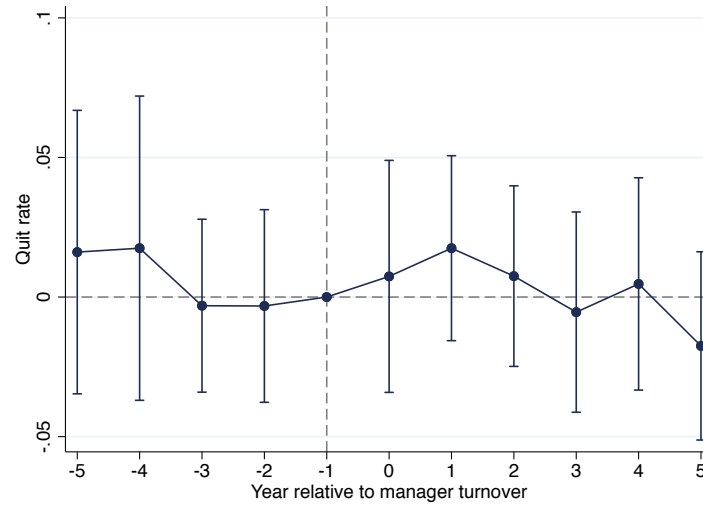
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. The regression controls for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ 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. Standard errors are clustered at the firm level.

Figure A15: Changes in Stock Options around Non-Business to Business Manager Transitions in Denmark



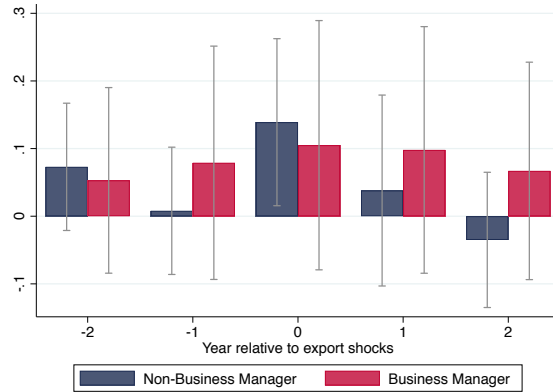
This figure plots event-study estimates and 95% confidence intervals, where events are manager transitions from a non-business manager to a business manager in Denmark. The sample includes firms that have non-business managers in all years, and firms that have one non-business to business manager transition event during the sample period. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects, industry $\times$ year fixed effects, firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. The dependent variable is the value of stock option payments. Standard errors are clustered at the firm level.

Figure A16: Changes in Quit Rates around Placebo Manager Transitions in Denmark

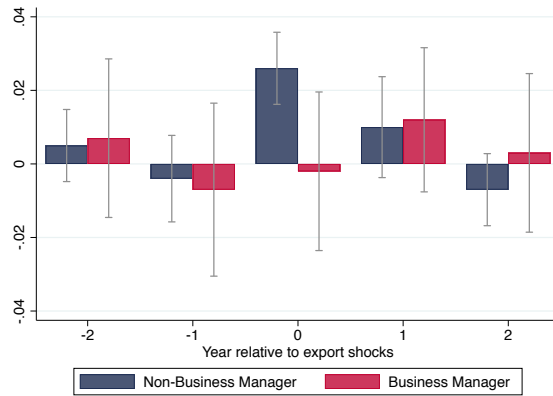


This figure plots event-study estimates and 95% confidence intervals, where events are placebo manager transitions from a non-business manager to a non-business manager in Denmark. The dependent variable is the share of workers that quit the firm to join another firm or become unemployed in the following year. All regressions include firm fixed effects, industry $\times$ year fixed effects, state(region) $\times$ year fixed effects, and initial size quintile by year fixed effects, and observations are weighted by employment. Standard errors are clustered at the firm level.

Figure A17: Output and Wage Response to Leads and Lags of Trade Shocks in Denmark



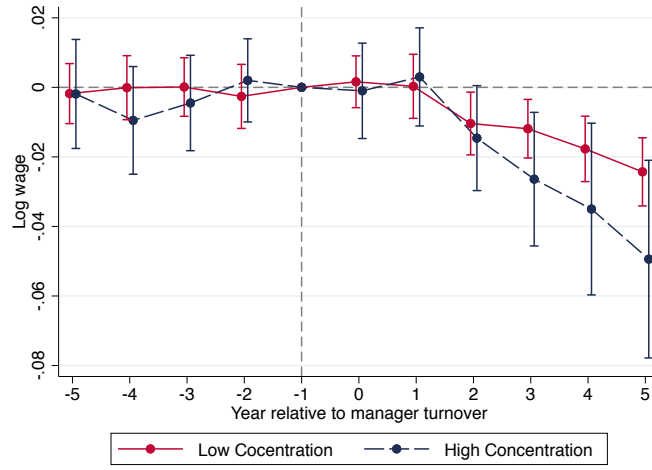
(a) Value Added



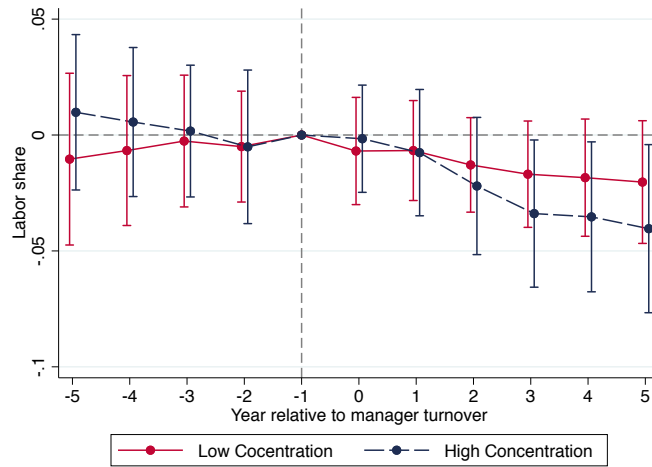
(b) Wage

This figure reports the coefficients and 95% confidence intervals from regressing log value added (in Panel (a)) and log annual income (in Panel (b)) on the leads and lags of export shocks 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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions in Panel (b) additionally control for firm $\times$ 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 A18: Changes in Wages and the Labor Share around Non-Business to Business Manager Transitions in High-Concentration and Low Concentration Industries in Denmark



(a) Wage



(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. The dependent variables are log annual income in Panel (a) and the labor share in Panel (b). 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, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. Worker-level regressions in Panel (a) additionally control for firm×worker fixed effects, quadratic in experience, and union and marital status dummies. Firm-level regressions are weighted by employment. The labor share is defined as total wage bill divided by value added. All standard errors are clustered at the firm level.

Table A1: OLS Estimates of Business Managers on Firm Outcomes with Varying Fixed Effects in Denmark

	Log Value Added			Log Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.007 (0.022)	0.009 (0.021)	0.013 (0.017)	-0.026 (0.005)	-0.029 (0.005)	-0.033 (0.005)	-0.023 (0.007)	-0.021 (0.007)	-0.019 (0.006)
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	N	Y	Y	N	Y	Y	N	Y	Y
Region-year FE	N	Y	Y	N	Y	Y	N	Y	Y
Size quintile-year FE	N	N	Y	N	N	Y	N	N	Y
Obs	44,731	44,731	44,731	44,731	44,731	44,731	44,962	44,962	44,962

This table reports the OLS estimates regressing firm-level value added, average wage, and the labor share on a dummy for business manager using the same sample as Table 2 in Denmark. All columns control for firm fixed effects and year fixed effects. Columns 2, 3, 5, 6, 7, and 9 additionally control for industry×year fixed effects and region×year fixed effects, and columns 3, 6, and 9 additionally control for initial size quintile by year fixed effects. The dependent variables are log sales in columns 1–3, log average annual wage in columns 4–6, and the 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.

Table A2: The Effect of Business Managers on Other Firm Outcomes in Denmark

	Payout	Log Cash	Log Rent	Log Liability	Leverage	Avg Routine Index	Purchase Robot
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Business Manager	0.009 (0.005)	-0.028 (0.016)	0.015 (0.009)	0.009 (0.002)	0.014 (0.003)	-0.005 (0.004)	0.024 (0.008)
Firm FE	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y
Region-year FE	Y	Y	Y	Y	Y	Y	Y
Size quintile-year FE	Y	Y	Y	Y	Y	Y	Y
Obs	267,472	267,472	267,472	267,472	267,472	267,472	267,472

This table reports the coefficients from regressions of firm outcomes on an indicator for having a business manager for the sample of all firms in Denmark from 1995 to 2011. All regressions control for firm fixed effects and industry  $\times$  year fixed effects, region  $\times$  year fixed effects, and initial size quintile by year fixed effects. Payout is a dummy variable that equals one if the divided payment is positive. Leverage is total debt divided by total assets. The routine index is defined as in Autor and Dorn (2013). Purchase of robots is measured using the imports of robot as in Humlum (2019). Observations are weighted by firm employment and standard errors are clustered at the firm level.



Table A3: 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 (0.039)	0.083 (0.059)	0.158 (0.039)	0.081 (0.059)	0.164 (0.047)	0.066 (0.065)	0.149 (0.047)	0.053 (0.065)
Peer Firm Business Manager t-2		0.270 (0.058)		0.274 (0.058)		0.285 (0.065)		0.291 (0.065)
Peer Firm Business Manager t-3		0.242 (0.052)		0.239 (0.052)		0.411 (0.059)		0.394 (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 wages							Y	Y
F statistic	16.7	30.2	16.2	29.8	12.3	43.6	9.9	41.9

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 quartile) in the US. The first stage equation is described in equation (2). 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 initial size quintile by year fixed effects in columns 3 and 4, industry $\times$ year fixed effects and region $\times$ year fixed effects in columns 5 and 6, and lags of firm value added and average wage 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 A4: 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 (0.035)	0.098 (0.040)	0.166 (0.034)	0.095 (0.040)	0.277 (0.038)	0.226 (0.042)	0.324 (0.049)	0.326 (0.052)
Peer Firm Business Manager t-2		0.258 (0.045)		0.255 (0.045)		0.331 (0.045)		0.141 (0.054)
Peer Firm Business Manager t-3		0.225 (0.047)		0.219 (0.047)		0.216 (0.048)		0.361 (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 wages							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 quartile) in Denmark. The first stage equation is described in equation (2). 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 initial 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 average wage 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 A5: Robustness of 2SLS Estimates of Business Managers on Firm Outcomes in Denmark

	Log Value Added			Log Average Wage			Labor Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Manager	0.064 (0.067)	0.029 (0.083)	0.046 (0.075)	-0.040 (0.024)	-0.063 (0.031)	-0.046 (0.027)	-0.053 (0.027)	-0.066 (0.032)	-0.056 (0.029)
Log Average Wage t-1	-0.152 (0.010)	-0.156 (0.011)	-0.157 (0.010)	0.122 (0.005)	0.139 (0.005)	0.125 (0.005)	0.008 (0.005)	0.003 (0.005)	0.012 (0.005)
Log Average Wage t-2	-0.020 (0.011)	0.007 (0.013)	-0.025 (0.011)	-0.012 (0.005)	-0.007 (0.006)	-0.008 (0.006)	-0.011 (0.005)	-0.021 (0.006)	-0.008 (0.005)
Log Average Wage t-3	-0.067 (0.010)	-0.050 (0.010)	-0.066 (0.010)	-0.040 (0.005)	-0.034 (0.005)	-0.035 (0.005)	-0.020 (0.004)	-0.033 (0.005)	-0.019 (0.005)
Log Value Added t-1	0.436 (0.005)	0.468 (0.005)	0.434 (0.005)	0.007 (0.002)	0.006 (0.003)	0.006 (0.002)	-0.020 (0.002)	-0.031 (0.002)	-0.020 (0.002)
Log Value Added t-2	0.062 (0.005)	0.068 (0.005)	0.064 (0.005)	0.003 (0.003)	0.003 (0.003)	0.002 (0.003)	0.004 (0.002)	-0.001 (0.002)	0.004 (0.002)
Log Value Added t-3	0.015 (0.004)	0.004 (0.005)	0.016 (0.004)	-0.009 (0.002)	-0.007 (0.002)	-0.007 (0.002)	0.024 (0.002)	0.034 (0.002)	0.026 (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 average wage and the labor share in Denmark. The first stage is described in equation (2) and is based on the average lagged business manager of peer firms (those in the same industry, region and size quartile). Table A4 in the Appendix presents the first-stage estimates and Figure 7 shows them visually. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects, and one, two and three-year lags of firm value added and average wage. Columns 1, 4, and 7 control for a full set of interactions between wage 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 wages of other firms in the same cell. The dependent variables are log value added in columns 1–3, log average annual wage 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 A6: Compensation of Business Managers in the US

**Panel A: Compensation Level**

	Log Wage of Managers			
	(1)	(2)	(3)	(4)
Business Degree	0.164 (0.012)	0.137 (0.012)	0.065 (0.010)	0.048 (0.013)
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	37,873	36,495	36,049	35,971

**Panel B: Composition of Compensation**

	Percent Salary	Percent Bonus	Percent Stock	Percent Options	Percent Incentive Plan	Percent Other
	(1)	(2)	(3)	(4)	(5)	(6)
Business Major	-0.004 (0.003)	-0.005 (0.003)	-0.001 (0.003)	0.006 (0.004)	0.002 (0.002)	0.002 (0.003)
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	35,971	35,971	35,971	35,971	35,971	35,971

This table reports the coefficients from regressions of compensation of CEOs from the Execucomp dataset on an indicator for having a business degree. The sample includes all CEOs from 1992 to 2019. In Panel A, the dependent variable is log total compensation (including salary, bonus, total value of restricted stock granted, total value of stock options granted using Black-Scholes, incentive plan payouts, and other compensation). 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 value added), and column 4 additionally controls for firm fixed effects. In Panel B, 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 value added). Standard errors are clustered at person level.

Table A7: Compensation of Business Managers in Denmark

	Log Wage of Managers			
	(1)	(2)	(3)	(4)
Business Degree	0.451 (0.005)	0.142 (0.005)	0.105 (0.004)	0.084 (0.005)
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	280,389	280,012	280,012	267,850

This table reports the coefficients from regressions of log annual wage of managers on an indicator for having a business degree. The sample includes all managers from 1995 to 2011. All columns include year fixed effects, column 2 additionally controls for manager characteristics (gender, education level, experience, age), column 3 additionally controls for firm characteristics (log employment and log value added), and column 4 additionally controls for firm fixed effects. Standard errors are clustered at person level.

Table A8: The Effect of Business Managers on Quit Rates in Denmark

	All workers	High-wage workers	Low-wage workers
	(1)	(2)	(3)
Business Manager	0.037 (0.021)	0.039 (0.022)	0.035 (0.021)
Firm FE	Y	Y	Y
Industry-year FE	Y	Y	Y
Region-year FE	Y	Y	Y
Size quintile-year FE	Y	Y	Y
Obs	44,731	44,731	44,731

This table reports the OLS estimates regressing firm-level quit rates on business manager using the same sample as Table 2 in Denmark. All columns control for firm fixed effects, industry×year fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. The dependent variable is the share of workers who leave the firm in the next year. Observations are weighted by firm employment and standard errors are clustered at the firm level.

Table A9: Business Managers and Firm Output 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	0.157 (0.086)	0.093 (0.031)	0.384 (0.084)	0.243 (0.065)	0.150 (0.030)	0.457 (0.101)
Export Shock*Business Manager	0.171 (0.093)	0.086 (0.040)	0.424 (0.122)	0.265 (0.077)	0.179 (0.049)	0.491 (0.142)
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Obs	6,686	8,285	8,285	8,285	8,285	8,173

This table reports the coefficients from regressions of firm-level outcomes on export shocks and their interactions with a firm-level indicator 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, initial size quintile by year fixed effects, region×year fixed effects, and industry×year fixed effects. 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.

Table A10: Wage Response to Export Shocks Controlling for Product Export Shocks in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.019 (0.004)	0.022 (0.005)	-0.005 (0.011)	0.016 (0.004)	0.016 (0.005)	-0.003 (0.011)
Export Shock*Business Manager	0.002 (0.009)	0.008 (0.008)	-0.025 (0.015)	-0.003 (0.009)	0.000 (0.008)	-0.015 (0.014)
Log Output				0.012 (0.004)	0.011 (0.005)	-0.182 (0.015)
Log Employment				0.017 (0.004)	0.051 (0.006)	0.163 (0.016)
Log Capital-labor Ratio				0.005 (0.002)	0.001 (0.002)	-0.013 (0.005)
Share of High-skilled Workers				0.082 (0.025)	0.058 (0.028)	0.156 (0.067)
Product Export Shocks	0.010 (0.005)	0.016 (0.005)	-0.016 (0.013)	0.012 (0.005)	0.012 (0.005)	-0.014 (0.010)
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE			Y			Y
Obs	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

This table reports the coefficients from regressions of wages and the labor share on export shocks and their interactions with a firm-level indicator 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, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ 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 income 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 A11: Wage Response to Export Shocks Using a Balanced Panel of Firms in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	0.020 (0.004)	0.033 (0.005)	-0.012 (0.012)	0.014 (0.004)	0.024 (0.004)	-0.008 (0.011)
Export Shock*Business Manager	0.006 (0.007)	0.013 (0.008)	-0.024 (0.013)	0.002 (0.007)	0.001 (0.007)	-0.019 (0.012)
Log Output				0.013 (0.004)	0.009 (0.004)	-0.148 (0.014)
Log Employment				0.019 (0.005)	0.047 (0.005)	0.168 (0.015)
Log Capital-labor Ratio				0.002 (0.001)	-0.001 (0.001)	-0.020 (0.005)
Share of High-skilled Workers				0.085 (0.024)	0.111 (0.029)	0.216 (0.072)
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	1,082,559	1,082,553	3,156	1,082,559	1,082,553	3,156

This table reports the coefficients from regressions of wages and the labor share on export shocks and their interactions with a firm-level indicator for having a business manager. The sample is a balanced sample of firms between 1995 and 2006 (i.e. firms that exit in every single year of that period). In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ 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 income 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 A12: College-Degree Managers and Wage Response to Trade Shocks in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-College Non-Business Manager	0.016 (0.005)	0.030 (0.005)	-0.016 (0.012)	0.014 (0.005)	0.025 (0.005)	-0.000 (0.011)
Export Shock*College Non-Business Manager	0.017 (0.003)	0.016 (0.005)	-0.011 (0.013)	0.014 (0.003)	0.010 (0.004)	-0.004 (0.012)
Export Shock*Business Manager	0.001 (0.007)	0.009 (0.007)	-0.025 (0.015)	-0.004 (0.007)	-0.001 (0.006)	-0.018 (0.013)
Log Output				0.012 (0.003)	0.013 (0.005)	-0.163 (0.012)
Log Employment				0.013 (0.004)	0.045 (0.005)	0.165 (0.012)
Log Capital-labor Ratio				0.003 (0.001)	0.000 (0.001)	-0.010 (0.004)
Share of High-skilled Workers				0.076 (0.021)	0.075 (0.026)	0.208 (0.057)
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	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

This table reports the coefficients from regressions of wages and the labor share on export shocks and their interactions 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, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. 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 income 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 A13: Wage Response to Export Shocks for Union and Non-Union Workers in Denmark

	Union		Non-Union	
	Log Hourly Wage	Log Income	Log Hourly Wage	Log Income
	(1)	(2)	(3)	(4)
Export Shock*Non-Business Manager	0.018 (0.003)	0.024 (0.005)	0.010 (0.004)	0.016 (0.007)
Export Shock*Business Manager	0.001 (0.007)	0.009 (0.007)	0.002 (0.008)	-0.001 (0.010)
Industry-year FE	Y	Y	Y	Y
Worker-firm FE	Y	Y	Y	Y
Obs	1,555,089	1,554,942	197,545	197,528

This table reports the coefficients from regressions of wages on export shocks and their interactions with a firm-level indicator 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, initial size quintile by year fixed effects, region×year fixed effects, industry×year fixed effects, firm×worker fixed effects, quadratic in experience, and union and marital status dummies. The dependent variables are log hourly wage in columns 1 and 3, and log annual income in columns 2 and 4. Standard errors are clustered at the firm level.

Table A14: Effects of Trade Shocks on Firm Exit in Denmark

	Exit - 1 year			Exit - 3 years		
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock*Non-Business Manager	-0.003 (0.021)			0.026 (0.023)		
Export Shock*Business Manager	-0.045 (0.028)			-0.028 (0.029)		
Intermediate Import Shock*Non-Business Manager		0.014 (0.012)			0.001 (0.017)	
Intermediate Import Shock*Business Manager		0.008 (0.020)			0.001 (0.024)	
Chinese Import Shock*Non-Business Manager			0.000 (0.011)			0.033 (0.016)
Chinese Import Shock*Business Manager			0.006 (0.010)			0.035 (0.020)
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Obs	8,286	8,286	6,380	8,286	8,286	6,380

This table reports the coefficients from regressions of firm exit on trade shocks (export shocks, intermediate import shocks, and Chinese import shocks) and their interactions with a firm-level indicator for having a business manager. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. The dependent variable is an indicator variable for firm exit in next year in columns 1–3, and an indicator variable for firm exit in the next 3 years in columns 4–6. Regressions are weighted by firm employment, and standard errors are clustered at the firm level.

Table A15: Business Managers and Firm Response to Intermediate Import Shocks in Denmark

	Log Profit Per Worker	Log Value Added Per Worker	Log Intermediate Imports	Log Value Added	Log Employment	Log Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Intermediate Import Shock*Non-Business Manager	0.101 (0.067)	0.043 (0.026)	0.309 (0.102)	0.136 (0.045)	0.093 (0.024)	0.152 (0.094)
Intermediate Import Shock*Business Manager	0.115 (0.070)	0.049 (0.033)	0.370 (0.146)	0.137 (0.064)	0.088 (0.042)	0.126 (0.142)
Industry-year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Obs	6,686	8,285	8,072	8,285	8,285	8,173

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This table reports the coefficients from regressions of firm-level outcomes on intermediate import shocks and their interactions with a firm-level indicator for having a business manager. Intermediate import shocks are shocks to intermediate imports supply from origin country-product combinations the firm imports from as defined in the text. All regressions control for firm fixed effects, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. 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.

Table A16: Business Managers and Wage Response to Intermediate Import Shocks in Denmark

	Log Hourly Wage	Log Income	Labor Share	Log Hourly Wage	Log Income	Labor Share
	(1)	(2)	(3)	(4)	(5)	(6)
Intermediate Import Shock*Non-Business Manager	0.012 (0.003)	0.011 (0.003)	-0.005 (0.009)	0.009 (0.003)	0.005 (0.003)	-0.000 (0.008)
Intermediate Import Shock*Business Manager	-0.000 (0.005)	0.007 (0.005)	-0.016 (0.014)	-0.004 (0.005)	0.001 (0.005)	-0.012 (0.014)
Log Output				0.013 (0.003)	0.013 (0.005)	-0.163 (0.011)
Log Employment				0.014 (0.004)	0.045 (0.005)	0.164 (0.012)
Log Capital-labor Ratio				0.003 (0.001)	0.000 (0.001)	-0.010 (0.004)
Share of High-skilled Workers				0.088 (0.020)	0.076 (0.025)	0.224 (0.056)
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE			Y			Y
Obs	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

This table reports the coefficients from regressions of wages and the labor share on intermediate import shocks and their interactions with a firm-level indicator for having a business manager. Intermediate import shocks are shocks to intermediate imports supply from origin country-product combinations the firm imports from as defined in the text. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. 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 income 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 A17: Response to Positive and Negative Intermediate Import Shocks in Denmark

	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>	<u>Log Hourly Wage</u>	<u>Log Income</u>	<u>Labor Share</u>
	(1)	(2)	(3)	(4)	(5)	(6)
Pos Intermediate Import Shock*Non-Business Manager	0.025 (0.005)	0.017 (0.004)	0.007 (0.013)	0.023 (0.005)	0.011 (0.005)	0.009 (0.012)
Pos Intermediate Import Shock*Business Manager	-0.002 (0.010)	0.009 (0.005)	-0.015 (0.014)	0.006 (0.010)	0.002 (0.006)	-0.014 (0.013)
Neg Intermediate Import Shock*Non-Business Manager	-0.004 (0.005)	0.006 (0.005)	-0.004 (0.012)	0.007 (0.005)	0.008 (0.005)	-0.010 (0.011)
Neg Intermediate Import Shock*Business Manager	0.002 (0.008)	0.010 (0.006)	-0.002 (0.012)	0.011 (0.009)	0.008 (0.006)	-0.011 (0.015)
Log Output				0.012 (0.003)	0.013 (0.005)	-0.164 (0.011)
Log Employment				0.013 (0.004)	0.044 (0.005)	0.165 (0.012)
Log Capital-labor Ratio				0.003 (0.001)	0.000 (0.001)	-0.010 (0.004)
Share of High-skilled Workers				0.079 (0.020)	0.072 (0.028)	0.228 (0.056)
Industry-year FE	Y	Y	Y	Y	Y	Y
Worker-firm FE	Y	Y		Y	Y	
Firm FE			Y			Y
Obs	1,783,859	1,783,694	5,157	1,783,859	1,783,694	5,157

This table reports the coefficients from regressions of wages and the labor share on positive and negative intermediate import shocks and their interactions with a firm-level indicator for having a business manager. Positive and negative intermediate import shocks are defined in the text. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ 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 income 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 A18: Response to Intermediate Import Shocks Before and After Non-Business to Business Manager Turnovers in Denmark

**Panel A: Non-Business to Business Manager Transitions**

	Value Added per Worker		Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intermediate Import Shock*Pre	0.034 (0.037)	0.021 (0.034)	0.018 (0.010)	0.016 (0.003)	0.026 (0.011)	0.023 (0.004)	-0.005 (0.010)	-0.006 (0.008)
Intermediate Import Shock*Post	0.061 (0.039)	0.026 (0.038)	-0.001 (0.005)	-0.004 (0.006)	-0.007 (0.005)	-0.016 (0.008)	-0.015 (0.012)	-0.017 (0.0011)
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	1,582	6,296	544,119	1,303,209	544,117	1,303,050	1,402	5,504

**Panel B: Placebo Non-Business to Non-Business Manager Transitions**

	Value Added per Worker		Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intermediate Import Shock*Pre	0.054 (0.041)	0.019 (0.022)	0.013 (0.004)	0.016 (0.003)	0.011 (0.004)	0.016 (0.004)	-0.006 (0.014)	-0.006 (0.009)
Intermediate Import Shock*Post	0.052 (0.041)	0.018 (0.022)	0.013 (0.004)	0.015 (0.003)	0.014 (0.004)	0.017 (0.003)	-0.004 (0.013)	-0.006 (0.009)
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	1,881	5,950	597,871	1,244,708	597,808	1,244,543	1,770	5,211

This table reports the coefficients from the regression of wages, value added per worker and the labor share on intermediate import shocks before and after manager transitions. *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. In Panel A, columns 1, 3, 5 include firms with one manager transition from a non-business manager to a business manager, and columns 2, 4, 6 also include firms that always had non-business managers (for which *Pre* equals 1 for all years). In Panel B, columns 1, 3, 5 include firms with one manager transition from a non-business manager to a non-business manager, and columns 2, 4, 6 also include firms that always 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, initial size quintile by year fixed effects, region×year fixed effects and industry×year fixed effects. 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 income 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, 3, 7, 8 are weighted by firm employment. Standard errors are clustered at the firm level.

Table A19: IV Estimates of Response to Intermediate Import Shocks in Denmark

	Log Hourly Wage		Log Income		Labor Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Intermediate Shock*(1-Predicted Business Manager)	0.010 (0.002)	0.011 (0.001)	0.007 (0.003)	0.008 (0.002)	-0.014 (0.011)	-0.007 (0.010)
Log Intermediate Shock*Predicted Business Manager	-0.001 (0.004)	0.002 (0.002)	-0.005 (0.007)	-0.003 (0.004)	-0.026 (0.011)	-0.019 (0.012)
Log Output		0.047 (0.003)		0.066 (0.005)		-0.176 (0.016)
Log Employment		-0.021 (0.005)		-0.022 (0.009)		0.173 (0.017)
Log Capital-labor Ratio		0.001 (0.001)		-0.007 (0.001)		-0.011 (0.005)
Share of High-skilled Workers		-0.251 (0.031)		-0.418 (0.054)		0.328 (0.075)
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	737,000	737,000	737,000	737,000	2,917	2,917

This table reports the IV estimates of the effect of business manager on the relationship between intermediate import shocks and wages and the labor share. The predicted business manager in each column is the predicted value from regressing whether a firm has a business manager on the lags of the share of its peer firms in the same industry $\times$ region $\times$ size cell that have a business manager, controlling for the same variables and fixed effects as that column. Intermediate import shocks are shocks to intermediate imports supply from origin country-product combinations the firm imports from as defined in the text. In all columns we control for firm fixed effects, initial size quintile by year fixed effects, region $\times$ year fixed effects and industry $\times$ year fixed effects. Worker-level regressions additionally control for firm $\times$ worker fixed effects, quadratic in experience, and union and marital status dummies. Columns 2, 4 and 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 2, log annual income of workers in columns 3 and 4, and the labor share of firms (wage bill divided by value added) in columns 5 and 6. Standard errors are clustered at the firm level.



Table A20: 2SLS Estimates of Business Major Choice on Firm Outcomes Without Controlling for Probability of Becoming a Manager in Denmark

	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)
Business Degree	-0.038 (0.017)	-0.046 (0.023)	-0.027 (0.015)
School-cohort FE	Y	Y	Y
GPA Quartile FE	Y	Y	Y
F statistic	12.1	12.1	12.8
Obs	13,076	13,076	9,191

This table reports 2SLS estimates of the effect of business major choice of managers on workers annual and hourly wages and the firm’s labor share. The first stage is provided in Table 7. Column 1 looks at the effect of becoming a manager for the full sample of students (first stage corresponding to column 2 of Table 7) , while columns 2–4 are for the sample of managers (first stage corresponding to column 4 of Table 7). In column 2 the dependent variable is residualized log average annual wage of workers employed under the manager. In column 3 the dependent variable is residualized log average hourly wage of workers employed under the manager. In column 4, the dependent variable is residualized labor share of the firm operated by the manager. These outcomes are residualized by regressing them on industry×year fixed effects, firm fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. All columns control for high school×cohort fixed effects and GPA quartile fixed effects. Standard errors are clustered at the high-school level. First-stage F-statistics for the excluded instruments are reported at the bottom.

Table A21: 2SLS Estimates of Business Major Choice on Firm Performance Measures in Denmark

	Residual log employment	Residual log sales	Residual log value added	Residual log investment
	(1)	(2)	(3)	(4)
Business Degree	0.061 (0.104)	-0.064 (0.161)	-0.029 (0.144)	-0.044 (0.182)
School-cohort FE	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y
F statistic	12.4	13.0	13.0	13.0
Obs	13,076	9,191	9,191	9,191

This table reports 2SLS estimates of the effect of business major choice of managers on workers annual and hourly wages and the firm's labor share. The first stage is provided in Table 7. The sample includes all students who later become a manager in at least one firm in our data set (first stage corresponding to column 4 of Table 7). In column 1 the dependent variable is residualized log employment of the firm(s) operated by the manager. In column 2 the dependent variable is residualized log sales of the firm(s) operated by the manager. In column 3 the dependent variable is residualized log value added of the firm(s) operated by the manager. In column 4 the dependent variable is residualized log investment of the firm(s) operated by the manager. These outcomes are residualized by regressing them on industry×year fixed effects, firm fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. All columns control for high school×cohort fixed effects, GPA quartile fixed effects, as well as the average propensity to become a manager within the school-cohort-GPA quartile group. Standard errors are clustered at the high-school level. First-stage F-statistics for the excluded instruments are reported at the bottom.

Table A22: 2SLS Estimates of Business Major Choice on Firm and Worker Outcomes with School and Cohort Fixed Effects in Denmark

	Becoming a manager	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)	(4)
Business Degree	0.064 (0.037)	-0.035 (0.021)	-0.039 (0.028)	-0.022 (0.017)
School FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y
F statistic	187.2	10.7	10.7	10.9
Obs	505,971	13,076	13,076	9,191

This table reports 2SLS estimates of the effect of business major choice of managers on workers annual and hourly wages and the firm’s labor share. The first stage is provided in Table 7. Column 1 looks at the effect of becoming a manager for the full sample of students (first stage corresponding to column 2 of Table 7) , while columns 2–4 are for the sample of managers (first stage corresponding to column 4 of Table 7). In column 2 the dependent variable is residualized log average annual wage of workers employed under the manager. In column 3 the dependent variable is residualized log average hourly wage of workers employed under the manager. In column 4, the dependent variable is residualized labor share of the firm operated by the manager. These outcomes are residualized by regressing them on industry×year fixed effects, firm fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. All columns control for high school fixed effects, cohort fixed effects, and GPA quartile fixed effects. Columns 2–4 additionally control for the average propensity to become a manager within the school-cohort-GPA quartile group. Standard errors are clustered at the high-school level. First-stage F-statistics for the excluded instruments are reported at the bottom.

Table A23: First Stage and Reduced Form Estimates of Business Major Choice Based on Placebo Role Models in Denmark

**Panel A: Placebo peers in same high school and different GPA quartile**

	Business degree		Becoming a manager	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Business Degrees in Same High School and Different GPA Quartiles	0.010 (0.012)	-0.052 (0.108)	-0.010 (0.006)	0.006 (0.039)	0.032 (0.044)	0.010 (0.044)
School FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y	Y	Y
Obs	505,963	13,076	505,963	13,076	13,076	9,191

**Panel B: Placebo peers in different high school and same GPA quartile**

	Business degree		Becoming a manager	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Business Degrees in Same GPA Quartile and Different High Schools	0.046 (0.036)	0.042 (0.332)	0.027 (0.019)	-0.029 (0.121)	-0.055 (0.136)	0.011 (0.137)
School FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y	Y	Y
Obs	505,970	13,076	505,970	13,076	13,076	9,191

**Panel C: Placebo peers in same high school, GPA quartile, but more than three cohorts ahead**

	Business degree		Becoming a manager	Residual log annual wage	Residual log hourly wage	Residual labor share
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Business Degrees in Same High School, GPA Quartile, and Three Cohorts Ahead	0.018 (0.008)	0.051 (0.059)	0.007 (0.005)	0.005 (0.021)	-0.014 (0.024)	0.019 (0.024)
School FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
GPA Quartile FE	Y	Y	Y	Y	Y	Y
Obs	504,138	13,076	504,138	13,076	13,076	9,191

This table reports the placebo first stage and reduced form of the role model IV. The dependent variable is whether a person has a business degree in columns 1 and 2, whether a person becomes a manager in column 3, residualized log average annual wage of workers employed under the manager in column 4, residualized log average hourly wage of workers employed under the manager in column 5, and residualized labor share of the firm operated by the manager in column 6. These outcomes are residualized by regressing them on industry×year fixed effects, firm fixed effects, region×year fixed effects, and initial size quintile by year fixed effects. Columns 3–6 include only individuals who end up becoming a manager in at least one firm during our sample period. In Panel A, the independent variable is the share of students in the same school, previous cohort, but different GPA quartile who have business degrees. In Panel B, the independent variable is the share of students in the previous cohort, same GPA quartile, but a different high school who have business degrees. In Panel C, the independent variable is the share of students in the same high school and same GPA quartile, but more than three cohorts ahead who have business degrees. All columns control for high school fixed effects, cohort fixed effects, and GPA quartile fixed effects. Columns 4–6 additionally control for the average propensity to become a manager within the school-cohort-GPA quartile group. Standard errors are clustered at the high-school level.