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Women in Top Corporate Positions and the Labor Share: Evidence from European Firms

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Abstract

The paper studies the representation of women in top corporate positions and the workers' share of firms' income. Using a large panel of European firms and identification from a shift–share instrument, the author estimates the effect of gender board diversity on the labor share. In the preferred specification, a 10 percentage point increase in gender board diversity is associated with a 0.75 percentage point rise in the labor share. The effect is stronger in services, in smaller firms, and among firms with persistently low productivity. A counterfactual analysis demonstrates a high semi-elasticity of employment as the driving mechanism behind these findings.

Keywords:
gender board diversity, labor share, corporate governance, female leadership

JEL Classification:
D33, G34, J16, J39

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Introduction

Women's participation in top management has been steadily increasing over the past few decades. Although women remain underrepresented relative to men, their presence in leadership roles is far higher than one or two decades ago, and the trend is likely to continue. This change raises important questions about how firms are managed. Prior research links leadership to a range of outcomes related to labor, including narrower gender pay gaps, greater use of flexible work arrangements, lower layoff rates, and other inclusive workplace policies. These findings point to systematic differences in how women and men in top governing positions approach labor decisions. However, the existing evidence typically singles out one outcome at a time like wages or employment, making overall effects harder to capture.

I contribute a comprehensive study of how increased women share in top corporate positions the labor share at the firm level. The labor share provides a comprehensive measure of how much of a firm's income is allocated to its workforce, encompassing changes in wages, hiring and compensation structures. Labor share is not merely byproduct of firms technology or business cycle but it reflects firm-level decisions, potentially including those associated with management.

To this end, I compile a multi-country firm-level panel dataset for Europe. The data link gender board composition to financial variables at the firm–year level. Coverage spans 1995–2020 and includes over 16 million firm–year observations on about 3 million firms, drawn from multiple European countries. This wide coverage is an asset. Europe's different governance systems and labor market rules (such as collective bargaining and employment protection) let me examine the same question in many institutional settings and reduce the risk that results are driven by one country's features. In addition, this setting lets me test whether the patterns hold across sectors and firm type.

Identifying a causal relationship between gender diversity and firm outcomes is challenging. Endogeneity can arise because board appointments are not random, creating selection bias in which unobserved firm characteristics are correlated with both board gender composition and the firm's labor share. It can also arise from reverse causality, where board composition and the labor share are jointly determined and may influence each other over time. Quota-based reforms offer useful quasi-experiments, but the limited number of affected firms and the specificity of those settings raise external validity concerns. To tackle these issues, I use a shift–share IV. The instrument projects a firm's board gender diversity from its pre-period exposure to country–industry trends in women's representation interacted with the firm's initial exposure, yielding variation plausibly orthogonal to contemporaneous firm shocks. With firm and year fixed effects, identification comes from how these external trends differentially shift otherwise similar firms over time. The exclusion restriction is that, conditional on fixed effects and controls, country–industry trends affect labor outcomes only via board composition.

I assess how increases in women's representation in top positions affect labor share of value added paid to workers, which captures both wages and employment rather than singling out one outcome. I do this in two steps. First, I map where the association is stronger or weaker, across sectors, firm size, and along the distributions of productivity and firms' typical labor share. This shows in which settings effect of increased women representation in corporate boards is strongest. Second, I examine the two channels affecting labor share by constructing counterfactual measures that discern changes in employment from changes in wage per worker. This allows me to see whether the labor share is affected mainly through employment policies or through wages. Taken together, I provide a causal estimate of the impact of gender board diversity on the labor share, an assessment of its heterogeneity, and the likely mechanisms.

Women in Top Positions and Firm Labor Outcomes

The labor share is largely overlooked in the empirical literature on women's leadership and firm-level outcomes. I synthesize evidence on wages, employment, leadership style, and other labor outcomes to assess how women's leadership affects the share of income paid to workers.

Women's Leadership, Wages, and Wage Gaps

Cardoso and Winter-Ebmer (2010) explore the impact of gender of the manager on gender wage policies within firms. The authors analyze whether firms led by women exhibit different wage structures and gender pay gaps compared to firms led by men, using Portuguese matched employer–employee data. Their findings indicate that women tend to earn more in women-led firms than in male-led firms, and conversely for men. They also find that, within firms, the gender pay gap is lower under woemn management. Hensvik (2014) shows that women managers tend to match with high-ability women, thereby reducing the gender wage gap via sorting. However, the study finds no evidence of within-firm preferential treatment for women, suggesting that selection rather than differences in wages drives the observed effects.

In contrast to the selection mechanism, other research highlights managerial impact on wage structures. Flabbi et al. (2019) examine how women managers impact the within-firm wage distribution and firm performance, using a sample of Italian employee–employer linked data from manufacturing. They find an impact at the tails of the wage distribution: women managers increase wages of women at the top and lower them at the bottom. They also find a substantial effect of women managers on firm performance when a significant part of the workforce are women. Their interpretation highlights the role of reduced statistical discrimination, by which female executives are better able to assess woman worker productivity, leading to more efficient talent allocation. Evidence from other country contexts confirms a similar pattern. Magda and Cukrowska-Torzewska (2019) use matched employer–employee data from Poland and find that female managers are associated with narrower gender wage gaps, especially in public sector organizations; in the private sector the link is not robust after controls. Their results suggest women leaders may promote equity, though effects are modest and context-dependent.

A complementary perspective is provided by research on internal labor markets. Tate and Yang (2015) investigate wage patterns after employment workers in new firms that emerge following plant closures. The findings indicate that while both men and women employees experience wage declines when transitioning to new firms, the wage penalty is more pronounced for women. However, women led firms tend to offer relatively smaller wage gaps between employed men and women compared to men-led firms. This suggests that women leaders plays a role in reducing gender wage disparities in new labor market opportunities, reinforcing the idea that women presence in leadership positions can contribute to fairer wage structures. Although the study does not focus primarily on leadership, their findings imply that women-led firms may offer more equitable initial wage during reorganization.

Recent research links leader gender of a manger to wages and gender gaps. Maida and Weber (2022) analyze the Italian gender quota reform and find only weak spillover effects on gender gaps in top roles or wages beyond the board level, suggesting that board gender composition at the top may be insufficient to drive broader equity without deeper organizational change. More direct links to firm-level wage setting are found in studies focused on managerial discretion. Theodoropoulos, Forth, and Bryson (2022) use British data to show that gender wage gaps are significantly smaller in workplaces where women constitute a large share of managers, particularly when those managers have discretion over wages. In firms with over 60% women managers, the gender wage gap is nearly eliminated. Using Italian linked employer–employee data Casarico and Lattanzio (2024) quantify the firm-side contribution to gender pay gaps. They show that firm pay premia account for 30% of the average gender earnings gap. Exploiting exogenous board gender shifts from Italy's quota law, they find evidence that a more gender-balanced board raises the bargaining power of newly hired women, showing how the leadership environment can affect pay setting.

Studies also show that women in top positions often face different wage conditions than men. Albanesi, Olivetti, and Prados (2015) find that women executives receive less performance-based pay and face more financial risk if firm performance drops. Gayle, Golan, and Miller (2012) show that women in executive jobs experience greater income uncertainty and are more likely to leave their roles sooner than men, even if they earn more on average after adjusting for their position. These challenges might shape how women on boards view decisions about jobs and wages. Because of their own experiences with risk and instability, they may support more stable employment and fairer pay practices. This, in turn, could lead firms with more women in leadership to give a larger share of their income to labor.

Employment Policies and Labor Cost Allocation

Beyond wages, a related strand of research focuses on how women leaders affects employment decisions and labor cost allocation. Matsa and Miller (2014) using firm-level data, found that private firms with women leaders exhibited lower probability of large-scale workforce reductions compared to male-owned firms. They also note that women-led firms were more labor intensive after the recession and more inclined to labor hoarding. Authors suggest that findings align with theories suggesting women emphasize employee well-being. However they may also perceive differently the costs of employment reduction compared to men. A similar pattern is documented by Matsa and Miller (2013), who study the impact of Norway's board gender quota and find that the presence of more women on boards led to fewer layoffs, higher relative labor costs, and reduced short-term profits. They interpret these outcomes as evidence of a gender-specific leadership style emphasizing workforce stability and long-term value over short-term financial performance. Importantly, this shift in employment policy was not driven by board inexperience or firm characteristics, but rather reflected a systematic difference in how women leaders approach labor decisions. Devicienti et al. (2019) further contribute to this evidence by showing that Italian firms with more women executives are significantly more likely to offer part-time work, especially to women. This leadership-driven increase in flexible work arrangements is associated with higher participation of women and improved work-life balance, without reducing wages or productivity. These results highlight how women leaders can reshape not only wage structures but also the organization of working time.

However, not all studies find meaningful differences. Gagliarducci and Paserman (2015) look into how gender composition of top management affects firm and employee outcomes from German panel of employee-employer linked data. Their identification strategy relies on the use of rich set of controls and fixed effects. They find no substantial link between presence of women and firm outcomes, including wage bill and employment. They also investigate hiring, promotion, and separation rates and find no systematic differences related to leader gender, suggesting limited behavioral divergence in labor-related decisions.

Finally, some work examines perceptions of fairness and workplace experience. Bertrand et al. (2019) analyzed if women board quota introduced in Norway changed situation of women in corporate positions and their wages in firms subject to regulation. They found no evidence of such impact. Moreover they do not find any improvement in promotion of women to top corporate positions in business. While the quota reduced wage gaps within boards, it had no observable spillover effects on outcomes for women, such as general pay levels or access to senior roles outside the boardroom. Lucifora and Vigani (2016) provide cross country evidence from survey data which implies presence of woman boss correlates with reduced lower gender discrimination and perceived gender bias, especially among women. They also find that women report higher job satisfaction and better work-life balance when supervised by a woman, suggesting that gender alignment in management influences subjective well-being at work. While these studies document outcomes, others explore the mechanisms behind them—particularly whether women lead differently.

Women Leadership Style

Research on leadership styles shows that women and men often lead in different ways, which may affect how firms treat their workers. Eagly, Johannesen-Schmidt, and van Engen (2003) find that women leaders are more likely to use transformational leadership, which includes mentoring, supporting employees, and focusing on long-term goals. Women are also more likely to use reward-based leadership, which is linked to positive outcomes. Paustian-Underdahl et al. (2024) show that over the last 50 years, women have been rated more highly than men in many leadership behaviors, such as ethical conduct, communication, and consideration for others. These styles suggest that women leaders may place more value on fairness, employee development, and workplace stability.

Further evidence supports the idea that women in leadership hold different values and preferences compared to their male counterparts. Adams and Funk (2012) show that women board members are more risk-averse, more socially oriented, and place greater emphasis on employee well-being and corporate social responsibility. These preferences appear to be intrinsic rather than driven by observable characteristics. In addition, Adams and Ferreira (2009) find that women directors increase board monitoring activity and governance quality. Women board members are more likely to attend meetings regularly, sit on monitoring committees, and are associated with greater CEO accountability. Although these governance effects do not/ always lead to higher firm performance, they indicate that women directors may foster a more disciplined, stakeholder-sensitive leadership environment. Taken together, these findings strengthen the argument that gender differences in leadership behavior and values can shape how firms approach employment, compensation, and internal distribution of income.

Contribution to the Literature

Overall, wage effects are mostly positive but small. Women leaders tend to narrow within-firm gaps, and the wage distribution shifts up mainly in its upper tail. Results vary by setting, and introduction of board quotas rarely affects wages below the board level. Employment effects indicate stability, with fewer layoffs, more labor hoarding, and more flexible hours. Other outcomes are mixed. Monitoring appears stronger and perceived fairness is higher, but promotions into top roles change little. Taken together, these firm-level effects suggest a small positive push on the labor share.

Most of this literature relies on linked employer–employee panels with firm and worker fixed effects. These designs produce correlations rather than causal effects. Some work uses board-quota reforms in quasi-experimental difference-in-differences or event-study settings. A few papers address endogeneity with instrumental variables for board gender composition.

This paper complements existing literature in two ways. First, it centers the labor share as a summary measure of how much firm income goes to workers. Unlike most of the literature, which singles out wages or employment, the labor share reflects with both wage and employment, so it captures their joint effect and links rising women's presence to income distribution within firm. Second, it proposes a novel shift–share instrumental variable to address endogeneity. This approach aims to identify causal effects in a setting where selection bias and reverse causality are persistent concerns.

Empirical framework

Model and Estimation Framework

To identify the effects of gender board diversity on firm labor share, I estimate the following equation:

$$\text{LS}_{i,t} = \alpha + \beta \, \text{GBD}_{i,t} + \theta_i + \tau_t + \varepsilon_{i,t}, \tag{1}$$

where i indexes firms and t indexes years. $\text{LS}_{i,t}$ is the labor share, measured as the ratio of total employee costs to value added. $\text{GBD}_{i,t}$ denotes the share of women in all board members for firm i in year t. Firm fixed effects θ_i absorb all time-invariant firm characteristics, such as sector, size, or establishment year. Time fixed effects τ_t account for common aggregate shocks or macro trends, including the general decline in labor share documented across advanced economies.

However, estimating this model via OLS may yield biased estimates of β if the appointment of women to boards is endogenous. Endogeneity can arise because board appointments are not random, which creates selection bias in which unobserved firm characteristics are correlated with both gender board composition and the firm's labor share. It can also arise from reverse causality, where gender board composition and the labor share are jointly determined and may influence each other over time. For example, a firm's profits rise so that value added grows faster than its wage bill, reducing the labor share, while at the same time the firm appoints women to the board. Such mechanisms make simple correlations biased. To address these concerns, I use a shift share instrumental variables approach.

Design of Instrumental Variable

The shift-share instrument is defined as:

$$z_{i,t} = \text{share}_{i,t_0} \cdot g_{k,c,t} = \left(\frac{\#\text{women}_{i,t_0}}{\text{board size}_{i,t_0}}\right) \cdot \left(\frac{\#\text{women}_{k,c,t}}{\text{board size}_{k,c,t}} \Big/ \frac{\#\text{women}_{k,c,t_0}}{\text{board size}_{k,c,t_0}}\right) \tag{2}$$

where t_0 is the firm-specific initial year of observation, k indexes industry, and c indexes country. Specifically, t_0 corresponds to the first year in which firm i is observed in the panel dataset. The instrument combines a firm's baseline exposure to gender diversity (its initial share of women on the board) with an exogenous shift given by the growth rate of the average women board share in the same industry and country. The shift component is plausibly exogenous, capturing national and sectoral changes in norms, policy, or supply of qualified women, while the firm-specific share captures differential exposure to these aggregate shifts.

This part of the empirical strategy is inspired by seminal work of Bartik (1991) and further supported by recent work that has applied shift-share designs to study gender and management outcomes. Notably, Flabbi et al. (2019) and Sieweke, Bostandzic, and Smolinski (2023) use similar instruments to examine the effect of women leaders on wage distribution and firm performance, respectively. Their findings support the relevance of this instrument and motivate its use in firm-level corporate studies.

The validity of the exclusion restriction requires that the instrument affects labor share only through its impact on board gender diversity. This implies that, conditional on fixed effects, industry-country-level trends in women representation in boards should not directly influence firm-level labor share via other unobserved channels. While this assumption is not directly testable, it is plausible in this context, particularly given the inclusion of firm and year fixed effects, which absorb time-invariant heterogeneity and macroeconomic shocks. Moreover, because the instrument is constructed from predetermined firm-level exposure ($\text{share}_{i,t0}$) and aggregate industry-country shifts ($g_{k,c,t}$), it mitigates concerns about reverse causality. This strategy follows the quasi-experimental logic presented in Borusyak, Hull, and Jaravel (2022), where identification is achieved through exogenous group-level trends interacted with predetermined exposure. In this framework, the credibility of the exclusion restriction relies on the assumption that, after conditioning on fixed effects, industry-country variation in gender diversity trends is uncorrelated with firm-specific shocks to labor share.

Formally, I implement the following two stage least squares system with fixed effects. The first stage projects $\text{GBD}_{i,t}$ on the instrument and fixed effects. The second stage relates the labor share to the fitted values of $\text{GBD}_{i,t}$. Firm and year fixed effects enter both stages, and standard errors are clustered at the firm level to account for serial correlation in within-firm variation over time.

$$\text{GBD}_{i,t} = \gamma_0 + \gamma_1 z_{i,t} + \theta_i + \tau_t + u_{i,t} \quad \text{(first stage)} \tag{3}$$

$$\text{LS}_{i,t} = \alpha + \beta \widehat{\text{GBD}}_{i,t} + \theta_i + \tau_t + \varepsilon_{i,t} \quad \text{(second stage)} \tag{4}$$

The described approach allows for a meaningful interpretation of the second stage coefficient, β. The instrument assigns higher predicted increases in gender board diversity to firms with higher initial women board representation. Thus second stage coefficient captures the average difference in labor share between firms that followed the industry trend more closely and those that did not, conditional on their initial women share. This, in turn, allows the coefficient to be interpreted as the causal effect of increasing gender diversity for firms that experienced larger predicted shifts due to their initial exposure.

Data

I analyze a panel of European companies between 1995 and 2020 based on data from the Orbis database, which provides detailed annual information on firm characteristics[1]. Crucially, it offers both information on the composition of company boards and financial

[1] For documentation and analysis of Orbis coverage and characteristics, see Kalemli-Özcan et al. (2024).

accounts; it also covers both publicly listed and privately held firms. This broad coverage makes it one of the most comprehensive cross-country sources available for analyzing gender diversity at the firm level.

Processing Orbis into an Estimation Sample

Since Orbis data are not readily usable in raw form, I apply a series of steps to clean, harmonize, and structure the data into a panel suitable for analysis. I process board composition data following the approach developed by Drazkowski, Tyrowicz, and Zalas (2024), which provides further details on coverage and methodology.

Harmonizing Board Data. Orbis records list names of individuals affiliated with each firm, along with their job titles. For each firm-year, I retain only those individuals identified as members of the company's top decision-making body, such as the management board (executive directors) or supervisory board (non-executive directors). This is determined by using job descriptions provided in Orbis, ensuring that board-level data are comparable across firms and over time.

Board member gender information is derived from a combination of Orbis data and name-gender dictionaries. While Orbis includes gender for some individuals, this field is often missing. To address this, I follow the Drazkowski, Tyrowicz, and Zalas (2024) by using a name-based algorithm that infers gender using country-specific name and gender dictionaries. When gender cannot be confidently assigned, the individual is marked as unknown. Such individuals are excluded from gender-related calculations, yet the corresponding firm-year observations are retained if the board data remain non-missing.

To extend coverage and ensure comparability across years, industry classifications are harmonized to the NACE Rev. 2 standard. This involves converting older coding systems, such as NACE Rev. 1 and Rev. 1.1, into the newer format. This harmonization is essential for constructing industry-level aggregates and tracking trends in board gender diversity across sectors in a consistent way.[2]

[2] NACE Rev. 1 and Rev. 1.1 industry classifications were were in use until 2007.

Orbis does not intrinsically provide a panel of board members. Orbis provides information on appointment and departure dates but these dates are often missing or inconsistently reported. Thus I construct a time-varying board composition by linking the same individuals across successive Orbis snapshots and recovering entry and exit when dates are unavailable, following the approach described in Drazkowski, Tyrowicz, and Zalas (2024).[3]

Financial Data. Orbis contains firm-level accounting information, enabling measurement of key economic variables. Firms with information on board composition are matched to their annual financial statements using unique firm identifiers. The matched dataset includes core financial variables drawn from balance sheets and profit and loss statements, such as total employee costs, value added, number of employees, revenues, and total assets. All monetary variables are expressed in thousands of euro. Nominal variables are deflated to constant prices using country-specific consumer price indices.

The Final Dataset. To conduct the empirical analysis, I merge board composition data with separately harmonized financial data from the Orbis database. The resulting final dataset is a harmonized firm-year panel that combines information on board gender composition with standardized financial variables. Each observation includes the total number of board members, the number of women on the board, and the corresponding women share, along with financial indicators such as employee costs and value added. The final dataset allows for estimation of the relationship between gender diversity and labor share. Note however, that merging financial data with management records reduces the number of usable observations, as often firms are missing financial information.

[3]Spells for each individual are reconstructed by tracking presence or absence across successive Orbis releases. When appointment and departure dates are reported, those dates are used directly. When dates are missing or unreliable, the entry year is taken as the first year the individual appears on a given board, and the exit year as the year after the last appearance across the snapshots.

Measurement of labor share. I define firm-level labor share as the ratio of total employee costs to value added. Although this measure is standard at the aggregate level, its interpretation at the firm level requires care. Recent work documents wide dispersion across firms (Autor et al. 2020, Kehrig and Vincent 2021). High productivity firms often exhibit low labor shares, which can reflect greater efficiency or capital intensity rather than weak bargaining. Some firms report labor shares above one when value added is small or negative. These patterns imply that heterogeneity in labor shares is informative. Following this insight, I examine whether the effects of gender diversity differ across the labor share distribution, comparing more labor intensive firms with firms that combine low labor shares and high productivity, to identify where gender board diversity matters most.

Construction of shocks

To construct the shift component $g_{k,t}$ of the instrument in equation (2), I compute yearly trends in gender board diversity by country, industry, and year using the full board composition dataset. This data includes all firms with board information, even when financial statements are not available. Using all boards maximizes coverage and avoids tying the instrument to the outcome sample. The calculation is based on sector-country-year cells with at least ten firms.

For each country c, industry k (using both two-digit and three-digit NACE classifications), and year t, I compute the average share of women on boards, defined as the ratio of women board members to total board size. The gender diversity trend $g_{k,t}$ is then calculated as the ratio of the average women board share in year t to its value in a baseline year t_0:

$$g_{k,c,t} = \frac{\sum_{i\in(c,k,t)} \#\text{women}_{i,t} / \sum_{i\in(c,k,t)} \text{board size}_{i,t}}{\sum_{i\in(c,k,t_0)} \#\text{women}_{i,t_0} / \sum_{i\in(c,k,t_0)} \text{board size}_{i,t_0}} \tag{5}$$

I then merge these country, industry, and year trends with the final estimation sample, which consists only of firms with financial statements, and interact them with each firm's baseline exposure share$_{i,t_0}$, where t_0 is the first year the firm is observed. In the appendix Table A2 I include country-level summary statistics of data used for calculation IV shift component.

Estimation sample

The final dataset used in the estimation is built from a harmonized and merged panel of firm-level observations that combine board composition data from the GBDD with harmonized financial variables from Orbis. The process of preparing the estimation sample involves several key filtering and construction steps.

First, a firm-year observation is included in the sample only if value added and labor costs are both available, since these are needed to calculate the labor share. Not all firms always report this information, so this step reduces the number of observations. I also keep only firms with valid NACE industry codes (two or three digits), which are necessary for linking with the industry-level trends used to construct the instrument. Finally, I restrict the sample to observations where the labor share is a ratio strictly between 0 and 1, ensuring meaningful interpretation.

Second, I focus on private sector firms, active in the business economy. Therefore I exclude certain NACE industry sections from the analysis. Specifically, the sample is restricted to firms in manufacturing, utilities, construction, trade, transportation, information and communication, real estate, and business services.[4] This filtering ensures that labor share reflects actual business decisions rather than being shaped by regulated wages or non-market objectives. It also improves comparability by limiting the sample to firms that operate under similar incentive structures, where profitability and workforce composition are driven by competitive pressures and managerial choices.

Finally, I compute the instrumental variable $z_{i,t}$ as defined in equation (2). This is done by multiplying the firm-specific baseline exposure share$_{i,t_0}$, calculated as the share of women on the board in the initial observation year t_0, with the industry-country-year-

[4]The following sections are excluded: A (agriculture), K (financial activities), and O to U (covering public administration, education, health, arts, other services, and household or extraterritorial activities).

specific gender trend $g_{k,t}$. The trend component is constructed at both two-digit and three-digit NACE levels. After constructing the instrument, I drop all firm-year observations where $t = t_0$, since these are used in the calculation of the base share and do not provide independent identifying variation.

I construct additional variables to capture firm characteristics. Year fixed effects are included to account for time-specific shocks, and broad industry categories are identified using NACE codes, distinguishing between manufacturing (codes 10–43) and services (codes 45–99). To classify firm size, I use the maximum number of employees ever recorded for each firm throughout the panel. Based on this measure, firms are categorized as small (fewer than 50 employees) or large (50 employees or more). This approach ensures that firm size remains constant over time. Once a firm is classified as large or small, that status does not change across years.

Summary statistics. Table 1 presents descriptive statistics for the final estimation sample. Panel A reports firm-level characteristics based on 16,634,414 firm-year observations, covering approximately 3,043,166 unique firms. The average labor share equals 0.68. The median is slightly higher at 0.74, which suggests a sample with a strong labor component. The mean women share in board is 0.20. The median is zero, indicating that many firm-years have no women on the board. Board size remains small across the sample. The average is about two members, and the median is just one. Many observations relate to small private firms with streamlined governance.

Firm size varies substantially. The median firm employs six people, while the average exceeds sixty. The difference reflects the influence of a small number of very large firms. Financial variables show similar skewness. I express value added and total assets in million euros. Mean values reach €4,830.60 million and €17,912.21 million, while medians are much lower at €277.78 million and €639.78 million. The data capture both small and large firms, consistent with the Orbis database and the structure of European firms.

Panel B of Table 1 groups the data by sector and firm size. 64 percent of the observations fall within services sectors. The rest come from manufacturing. Labor shares

Table 1. Summary Statistics

Panel A: Firm-Level Statistics (Full Sample)	Mean	Std. Dev.	Median
Labor share	0.68	0.23	0.74
GBD	0.20	0.33	0.00
Board size	1.96	2.44	1.00
Number of employees	63.16	2,039.51	6.00
Value added (mil. EUR)	4,830.60	1,388,438.21	277.78
Total assets (mil. EUR)	17,912.21	816,606.05	639.78
Number of observations		16,634,414	
Number of unique firms		3,043,166	
Panel B: Sample Composition by Group	Share of Obs.	Mean Labor Share	Mean GBD
Manufacturing (NACE 10–43)	36.00%	0.71	0.16
Services (NACE 45–99)	64.00%	0.67	0.23
Small firms (<50 employees)	84.97%	0.68	0.21
Large firms (≥50 employees)	15.03%	0.69	0.15

Notes: Panel A reports summary statistics for key variables based on firm-year observations. Monetary variables are expressed in real terms, deflated to 2015 euros. Panel B shows the distribution of observations by broad sector and firm size, along with their average labor share and women board share. Industry codes are based on NACE Rev. 2.

are slightly higher in manufacturing, reaching 0.71 compared to 0.67 in services. GBD is greater in services, where the average women share is 0.23. In manufacturing, the average is 0.16. The patterns correspond with known differences in employment structure and leadership across sectors. Smaller firms account for 84.97 percent of the sample. Labor shares show little variation across size groups. GBD is higher in smaller firms, while larger firms report lower values.

Country coverage and population weights. To ensure data quality and sufficient coverage, I exclude countries with fewer than 1000 firm-year observations from the estimation sample[5]. The country-level summary statistics in Appendix Table A1 reveal considerable differences in the number of observations across countries. However, the country shares in the estimation sample do not align with each country's share in the European economy. For example, France, the second largest economy in Europe, accounts for nearly four million firm-year observations. At the same time Germany, despite being the largest

[5]The following countries were excluded from the estimation sample due to insufficient coverage (fewer than 1000 firm-year observations): Croatia, Cyprus, Greece, Iceland, Liechtenstein, Lithuania, Malta, Montenegro, Russia, and Turkey.

economy in Europe, contributes substantially fewer observations. To correct for this imbalance, I merge annual country-level population weights into the dataset. Weights are defined as each country's share of the total population across all countries included in the estimation sample. These weights serve as a proxy for economic size and are used in estimation to prevent overrepresented countries from exerting disproportionate influence on the results.

Results

In this section, I present estimates of the impact of GBD on the labor share. I begin with the full-sample specification and then turn to analyzing heterogeneous effects across groups of firms.

The estimates from the full sample

My main results for the full sample are reported in Table 2. My preferred specification uses country weights. In column (2), where gender board diversity is instrumented with the shift–share, the coefficient is positive and statistically significant. A 10 percentage point increase in gender board diversity is associated with a 0.75 percentage point increase in the labor share.

Table 2. The impact of GBD on labor share

	With country weights		*Without country weights*	
	(1) OLS	(2) IV	(3) OLS	(4) IV
$GBD_{i,t}$	-0.004*** (0.001)	0.075*** (0.008)	-0.002*** (0.000)	0.017*** (0.004)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
First stage F stat.		1986.4460		2397.0772
C test p-value		0.0000		0.0000
No. of firms	2,502,659	2,502,659	2,504,251	2,502,659
No. of observations	16,634,414	16,634,414	16,648,734	16,634,414

Notes: Table reports estimates of equation (1). Columns (1) and (3) show fixed-effects OLS; columns (2) and (4) show 2SLS with GBD instrumented by the shift–share IV. All specifications include firm and year fixed effects; standard errors are clustered at the firm level. Columns (1)–(2) apply country population weights; columns (3)–(4) are unweighted. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

The first-stage F statistic exceeds conventional thresholds, and the reported C test rejects the null that gender board diversity is exogenous in the structural equation. Column (1) reports the weighted OLS estimate. It is negative, statistically different from zero, and small in magnitude. This contrast is consistent with the endogeneity concerns addressed by the IV.

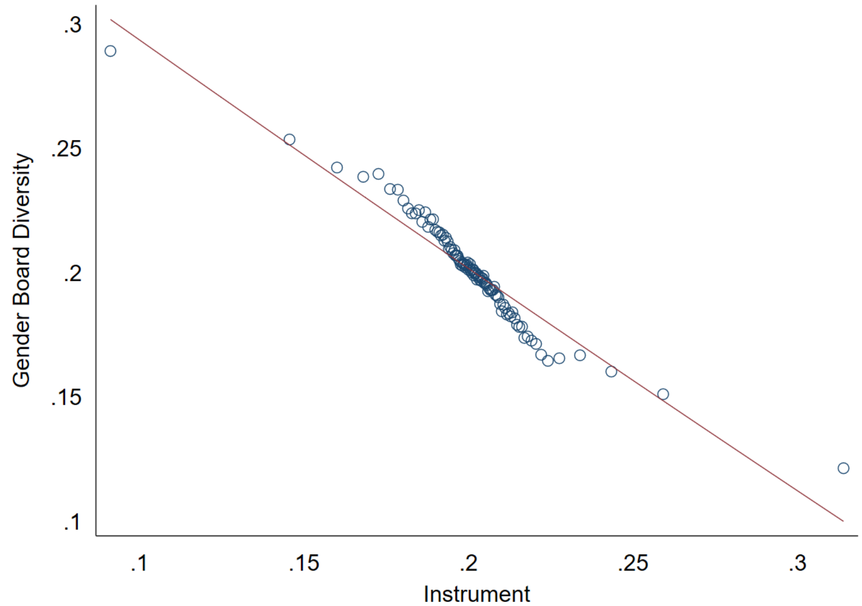
Columns (3) and (4) show the unweighted results. The unweighted OLS estimate remains small and negative. The unweighted IV estimate is positive and statistically significant. The direction of the effects aligns with the weighted results. Because the weighted specification reflects the underlying population of firms, I take column (2) as the main result. The pattern across all four columns indicates that once gender board diversity is instrumented, the estimated effect on the labor share is positive and robust

First stage results. Figure 1 is a visual representation of the first stage. It uses a binscatter plot. Observations are ordered by the instrument values, split into equal–size groups, and for each group plot mean of GBD (after removing firm and year fixed effects) against the group's mean instrument value. The fitted line is the first–stage regression.

Most points lie close to the line, with limited dispersion, indicating a good fit so the instrument predicts GBD closely. At the same time, the points are concentrated in the middle of the instrument's range and become sparse toward the tails, so the slope is identified mainly around the center. A few points sit farther from the line, but they are few and do not dominate the overall fit. Overall, the figure suggests a strong and stable first stage.

Appendix Table A3 reports the first stage regressions (and the corresponding reduced-form estimates) on the same samples and with the same fixed effects as in the Table 2. In the first stage, the instrument is a strong predictor of GBD since first stage F statistics strongly exceed conventional thresholds in both specifications, with and without applying country population weights. This is evidence of a tight fit that is consistent with Figure 1.

Figure 1. The binscatter of first stage estimates



Notes: Figure presents the first stage regression using binscatter. Observations are sorted by the values of instrument and grouped into equal-frequency bins; each point is the country-weighted mean of the instrument (x-axis) and GBD (y-axis) after partialling out firm and year fixed effects. The line is the fitted first stage regression from the preferred specification (Table 2, col. 4). Weights are country population weights; the sample matches the preferred specification.

Mechanism: wage vs. employment.

I previous specifications firm's labor share (LS_{it}) was defined as payroll over value added:

$$LS_{it} = \frac{WB_{it}}{VA_{it}} = \frac{\bar{w}_{it} L_{it}}{VA_{it}}, \qquad (6)$$

where $\bar{w}_{it}$ is the wage (payroll per worker) and L_{it} is employment. I use labor share formula to compare the roles of the employment and wage. Specifically, I build two counterfactual versions of the labor share for each firm. First, I take each firm's average wage over time and treat it as time-invariant. I then recompute the labor share using that fixed wage together with the firm's actual employment and value added. I refer to this variable as employment-varying labor share (LS^L) which indicates how changes in employment move the labor share. Second, I take each firm's average employment over time and recompute the labor share using that fixed employment together with the firm's actual wage and value added. I denote this variable as wage-varying labor share ($LS^{\bar{w}}$). This variable indicates how changes in wages move the labor share. I then re-estimate the preferred specification with same instrument, firm and year fixed effects and country weights using both counterfactual labor shares as the dependent variable. I show result in Table 3.

Table 3. GBD Effects by Margin: Wage-Varying vs. Employment-Varying Labor Shares

	Labor share		*Wage-varying labor share*		*Employment-varying labor share*	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
$\text{GBD}_{i,t}$	-0.006***	0.089***	0.000	0.029**	-0.003***	0.227***
	(0.001)	(0.011)	(0.001)	(0.012)	(0.001)	(0.012)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F stat.		1073.483		1073.483		1073.483
C test p-value		0.000		0.014		0.000
No. of firms	1,927,504	1,927,504	1,927,504	1,927,504	1,927,504	1,927,504
No. of observations	9,520,949	9,520,949	9,520,949	9,520,949	9,520,949	9,520,949

Notes: The estimation sample is restricted to firm-years for which both counterfactual labor shares lie between 0 and 1; columns (1)–(2) re-estimate the baseline on this restricted sample for comparison. All specifications include firm and year fixed effects, apply country population weights, and instrument GBD with the shift–share IV; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

I restrict the sample to firm–years in which both counterfactual measures lie between 0 and 1. The IV estimate for the standard labor share is positive, precise, and close to the main result reported earlier. The IV coefficient for the *wage-varying* labor share is positive but lower that main estimate. Then the IV coefficient for the *employment-varying* labor share is large, positive, and statistically significant. Thus the employment margin is the primary channel through which GBD raises the labor share on this sample, with the wage margin contributing a smaller, same-sign effect. This pattern points to the employment margin as the main channel through which GBD raises the labor share. By construction, the labor share increases when the wage bill grows faster than value added. In my counterfactual measures, most of the effect shows up when employment is allowed to vary with wages fixed, while the wage-varying series delivers a smaller coefficient but with the sign. This suggests that increasing GBD can affect hiring, retention or separations more compared to wages. Wage policies are often constrained by for instance collective agreements.

Heterogeneous effects of GBD

Average effect can mask meaningful variation across technologies and organizational settings. I therefore examine how the impact of GBD on labor share varies by sector, firm size, labor share and productivity distributions. In all cases I keep the specification, fixed effects, weights, and instrument as in the main specification.

Role of sector and size of firm. Sectors differ in production technology (e.g., services are more labor-intensive than manufacturing), and firm size may proxy organizational complexity. If GBD influences wage-setting through internal discretion, I expect larger effects where labor is a larger in terms of cost share and decision chains are shorter (smaller firms), and muted effects where pay structures and bargaining rules are more rigid (manufacturing, larger firms). I therefore split the main IV specification by sector and size, keeping the same fixed effects and weights, and report first-stage strength in each subgroup to ensure instrument relevance.

Table 4. The impact of GBD on labor share: heterogeneous effects by sector and size.

	Sector				*Size*			
	Manufacturing		*Services*		*$<$ 50 employees*		*$\geq$ 50 employees*	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
$GBD_{i,t}$	-0.004***	0.062***	-0.005***	0.089***	-0.004***	0.081***	-0.004**	-0.005
	(0.001)	(0.016)	(0.001)	(0.008)	(0.001)	(0.008)	(0.002)	(0.020)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F stat.		505.0		5432.3		2068.6		190.3
C test p-value		0.000		0.000		0.000		0.935
No. of firms	894,663	894,663	1,649,737	1,649,737	2,179,745	2,179,745	322,914	322,914
No. of observations	5,988,869	5,988,869	10,645,545	10,645,545	14,275,434	14,275,434	2,358,980	2,358,980

Notes: Table reports estimates of equation (1) with GBD instrumented by the shift–share IV. Sector splits follow the NACE classification (manufacturing vs. services). Size splits are based on firm-level average employment over the sample: $<$ 50 employees and $\geq$ 50 employees. All specifications include firm and year fixed effects and apply country population weights; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

In Table 4 I present results by sector (manufacturing vs. services) and by firm size

(<50 vs. ≥50 employees), keeping firm and year fixed effects and the country weights. The IV columns show that the positive effect of GBD on the labor share is present in both sectors but stronger in services than in manufacturing. This is consistent with GBD having more impact where production is more labor-intensive.

Turning to firm size, the IV estimate is strongly positive among smaller firms but not different from zero among larger firms. This pattern is consistent with the idea that board member may implement their priorities easier. In such firms, relations are more informal and decision chains are shorter, so decisions are faster. As a result, changes in GBD can pass through more directly to wage-setting and employment. Across all subgroups, the first stage is strong (high first stage F, and endogeneity tests indicate that IV is needed in services and among smaller firms but not in larger firms, where OLS and IV align and the effect is economically negligible.

GBD impact over Labor Share distribution. Labor share may not be a sufficient statistic for a firm's distributive attitude toward labor. Since value added enters the denominator of labor share, this ratio can be mechanically high, when profits and thus value added are low. Conversely, in capital intensive firms, the labor share may be low even in the absence of restrictive compensation policies. To separate these accounting effects from firms response, I sort firms by their median labor share, computed as the firm-level median over time. Then I define low, middle, and high labor share groups using 25th and 75th percentile cutoffs. The percentiles are country-specific so that comparisons are not driven by cross-country composition.

The results are presented in Table 5. The IV coefficients are positive and statistically significant throughout. The effect is largest among low labor share firms, remains sizable in the middle of the distribution, and is positive but smaller among high labor share firms. First-stage F statistics are high in each subsample, and endogeneity tests reject exogeneity of GBD across groups.

The main result is primarily driven by low and middle labor share firms. Because I sort on each firm's time-invariant median labor share, these groups capture persistent

Table 5. The impact of GBD and Labor Share across its distribution

	Low Labor Share		*Medium Labor Share*		*High Labor Share*	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
$GBD_{i,t}$	-0.007***	0.100***	-0.004***	0.087***	-0.003***	0.028***
	(0.002)	(0.017)	(0.001)	(0.012)	(0.001)	(0.011)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F stat.		1849.054		658.022		1266.621
C test p-value		0.000		0.000		0.004
No. of firms	645,988	645,988	1,186,175	1,186,175	670,496	670,496
No. of observations	4,158,576	4,158,576	8,317,187	8,317,187	4,158,651	4,158,651

Notes: Table reports estimates of equation (1) with GBD instrumented by the shift–share IV by labor share groups. Firms are assigned to time-invariant Low/Medium/High labor-share groups using each firm's median labor share over time and the country–industry–specific 25th and 75th percentiles of the firm-median distribution. All specifications include firm and year fixed effects and apply country population weights; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

types rather than temporary shocks. In the low labor share group, firms typically rely more on capital or enjoy a stronger product-market position, leaving more surplus to reallocate toward workers. Therefore higher GBD translates into a larger rise in the labor share. In the middle of the distribution the effect remains sizable. However for high labor share firms, the estimate is positive but smaller. This means that firms with already labor-intensive technology or tighter margins leave less possibility for GBD to affect the labor share.

GBD effects over the TFP Distribution. Highly productive firms tend to be capital-intensive and keeping labor costs relatively low. Their labor share can therefore be low even when compensation policies are generous. Low productivity firms use more labor relative to capital and scale of operation. They generate less surplus and their value added can be low, which pushes the labor share up. For these reasons the labor share is not a clean measure of distributive choices across firms with different productivity. I therefore study heterogeneity by TFP distribution to see whether the effect of GBD depends on technology rather than on governance.

I estimate TFP with a Cobb–Douglas production function that uses only capital and labor. I run this estimation separately for each country, two-digit NACE industry, and

Table 6. The impact of GBD and Labor Share across TFP distribution

	TFP sample		*Low TFP*		*Medium TFP*		*High TFP*	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
$GBD_{i,t}$	-0.004***	0.078***	-0.005***	0.141***	-0.005***	0.066***	-0.003**	0.045***
	(0.001)	(0.008)	(0.001)	(0.023)	(0.001)	(0.009)	(0.002)	(0.014)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F stat.		1930.594		183.744		1836.503		1606.231
C test p-value		0.000		0.000		0.000		0.001
No. of firms	2,338,675	2,338,675	585,847	585,847	1,091,141	1,091,141	837,576	837,576
No. of observations	15,862,555	15,862,555	3,959,839	3,959,839	7,942,041	7,942,041	4,732,534	4,732,534

Notes: Table reports estimates of equation (1) with gender board diversity (GBD) instrumented by the shift–share IV. TFP is estimated from Cobb–Douglas production functions (capital and labor) within country–2-digit NACE–year cells. Firms are assigned to time-invariant Low/Medium/High TFP groups using each firm's median TFP and the country–industry–specific 25th and 75th percentiles. All (TFP sample) restricts the estimation to firms for which TFP can be computed (those with the employment and capital data), so it is a subset of the full sample. All specifications include firm and year fixed effects and apply country population weights; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

year. For each firm I then take the median of its TFP across years. Within each country–industry I compute the 25th and the 75th percentiles of these firm-level medians. I assign firms to three time-invariant groups: low TFP if the firm median is below the 25th percentile, medium TFP if it lies between the 25th and the 75th percentiles, and high TFP if it is above the 75th percentile. This classification captures the firm's long-run position in the productivity distribution. The sample used in the TFP table is smaller than the full sample because TFP can be estimated only for firms that report employment.

In Table 6 I reports the results. The IV coefficients are positive and precisely estimated in all groups. In the sample with estimated TFP, the estimate is positive and similar in magnitude to the main result in Table 2. Across TFP groups, the estimates follow a monotonic pattern. The effect is largest in the low TFP group, remains sizable in the middle group, and is smaller though still positive in the high TFP group. First–stage F–statistics are high in every column, and tests of exogeneity reject that GBD is exogenous within each group. Overall, firms in the low TFP subsample drives main IV estimate up, since its coefficient is the largest.

Robustness results

Sensitivity to sample restrictions. The estimation sample contains many small firms. According to Table 1, average board size is below two members, and the typical firm is observed for about five years. These features matter for model with firm fixed effects. Short and uneven firm series make the panel unbalanced and limit within-firm variation in GBD. Due to very small boards, GBD can take very often specific values (like 0, 1/2, 1), which may amplify measurement error and can attenuate estimates. In addition, some firms do not change board members and at all, with GBD equal to 0 or 1 in every observed year, contributing no identifying variation. To test my result, To assess robustness, I re-estimate the model under restrictions designed to mitigate these concerns.

I report results in Table A4. Keeping only firms observed for at least five years leaves the IV estimate positive and statistically significant. Requiring average board size above three also preserves a positive and significant effect, though it is smaller and less precise, which is consistent with reduced within-firm movement in the GBD. Excluding firms with GBD that is always zero or always one does not change the result. Altogether, the benchmark result is robust to these sample and coverage choices. The estimates size vary with the restrictions, but the sign and statistical significance persist.

Sensitivity to instrument granularity and board definition. I conduct two more robustness exercises which address how the construction of instrument and measurement of gender diversity. In the first exercise, I replace the baseline 2-digit industry shift–share with a 3-digit NACE version. A more granular instrument reduces aggregation bias and uses richer within-industry variation. If level of aggregation drove the result, the IV estimate would weaken. I present the results in first part of Table A5. The 3-digit IV estimate remains positive and statistically significant and is close to the main result presented in Table 2.

In the next step, I narrow the treatment by measuring GBD on the management board only and instrument it with the corresponding shift–share. This focuses on members closer to day-to-day operations and reduces variation relative to the broader executive measure. I show this results in second part of Table A5. The IV coefficient remains positive but is smaller and less precise than the main result, which is natural given the and reduced variation. Overall these exercises show that increasing instrument granularity does not substantially change the result, whereas narrowing the GBD measure lowers the estimated magnitude.

Conclusion

This paper asked a simple question: when the share of women on corporate boards increases, does a larger share of the firm's income go to workers? Prior work usually studied wages and gender wage gaps, promotions into top roles, layoffs and separations, and the use of flexible hours. The labor share helps to link these pieces. It equals wage per worker times employment, divided by value added. Changes in wages and changes in employment both move this ratio. In practice, many of the outcomes studied in the literature operate through wages or through employment (for example, flexible hours and layoffs affect employment; pay policies affect wages). The labor share therefore summarizes their combined effect in a single firm-level measure.

To study this question, I built a large panel of European firms, which combines data on gender composition of boards with annual financial accounts. I set up a linear model with firm and year fixed effects estimated by two stage least squares. Gender board diversity is instrumented with a shift–share instrument that combines country and industry trends in women on boards with firm exposure to that shock. The preferred specification uses country weights so that estimates reflect the population of firms rather than the composition of the sample.

The main result is positive and economically meaningful. In the preferred estimate, a ten percentage point increase in women's board representation is linked to about a 0.75 percentage point rise in the labor share. I then examine which component of labor share if affected more. I recompute the labor share in two ways. First, I hold wages fixed at each firm's average and let employment vary. Second, I hold employment fixed and let wages vary. When wages are fixed and employment varies, the effect is large and precise. When employment is fixed and wages vary, the effect is positive but smaller. I also study how the main result differs across groups. It is stronger in services than in manufacturing. It is more pronounced among smaller firms. It is larger for firms with persistently low labor shares and for firms with low productivity. It is weaker for large firms and for highly productive firms. These patterns reveal that women effect is more pronounced in less complex environments where decision chains are shorter and implementation is faster.

These results add a distributional perspective to earlier work that has mostly treated wages, promotions, separations, or flexible work in isolation. By using the labor share, the paper puts those outcomes on a common scale and shows that more women on boards coincides with a higher share of income going to workers, mainly through employment. This dimension has been largely overlooked, so the evidence helps fill a gap and provides a benchmark. Overall, the estimated effect is modest in size but consistent across specifications and in line with earlier studies that find small wage gains and more stable employment. My results point in the same direction that higher women's presence in boardrooms shifts decisions toward workers, but the change is gradual rather than dramatic.

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Appendix A. Descriptive statistics

Table A1. Number of observations by country in estimation sample

country	# observations	# firms	# firms per year	# firms with only women	# firms with only men
Austria	51,530	8,813	352.52	100	5,060
Belgium	136,333	18,878	755.12	177	8,182
Bulgaria	348,236	72,064	3,002.67	11,008	44,506
Czech	333,413	71,740	3,260.91	4,168	45,390
Denmark	49,455	15,248	609.92	393	7,642
Estonia	30,248	10,461	871.75	628	4,600
Finland	558,326	93,625	3,745.00	2,296	26,472
France	3,633,113	600,226	24,009.04	56,693	366,903
Germany	539,284	129,866	5,194.64	3,236	84,379
Hungary	44,253	13,839	629.05	850	6,713
Ireland	13,966	3,601	144.04	5	1,019
Italy	1,736,895	401,026	16,709.42	23,111	206,941
Latvia	3,045	1,164	77.60	169	597
Luxembourg	7,689	1,946	88.45	40	1,017
Norway	392,451	87,988	4,630.95	1,296	33,157
Poland	265,469	58,668	2,793.71	4,980	36,417
Portugal	1,250,074	226,562	10,298.27	18,609	92,889
Romania	4,370	1,436	71.80	225	766
Serbia	14,791	8,127	812.70	1,541	6,124
Slovakia	208,033	48,868	2,221.27	4,222	31,143
Slovenia	5,662	1,322	73.44	85	688
Spain	5,384,133	887,440	35,497.60	97,816	552,674
Sweden	1,428,528	235,019	9,792.46	7,624	91,426
Switzerland	3,689	653	32.65	0	165
UK	181,150	41,438	1,657.52	562	19,960
Ukraine	10,278	3,148	165.68	197	2,501
Total	16,634,414	3,043,166	5,424.54	240,031	1,677,331

Notes: This table reports the number of observations and firms in the estimation sample by country. An observation refers to a firm-year. The column *# firms per year* shows the average number of unique firms observed annually. The columns *# firms with only women* and *# firms with only men* indicate the number of firms whose boards consisted exclusively of women or men, respectively, in all observed years. Firms that ever reported mixed-gender boards are not included in these counts. Low counts for some countries reflect limited of financial information.

Table A2. Number of observations by country in IV sample

country	# observations	# firms	# firms per year	# firms with only women	# firms with only men
Austria	1,777,559	290,802	8,812.18	27,885	202,952
Belgium	3,620,352	507,394	15,856.06	54,758	284,401
Bulgaria	2,235,409	495,498	17,696.36	126,823	301,668
Denmark	2,048,138	276,099	10,225.89	19,062	189,015
Estonia	1,262,107	185,785	7,741.04	29,918	113,217
Finland	2,446,696	304,991	10,516.93	14,286	107,091
France	11,807,228	1,938,509	64,616.97	295,780	1,332,697
Germany	13,172,913	1,955,250	54,312.50	166,019	1,385,835
Hungary	1,647,178	467,325	18,693.00	93,499	284,205
Ireland	1,868,377	216,558	6,562.36	5,669	61,469
Latvia	1,032,070	167,328	6,435.69	36,133	95,343
Luxembourg	188,947	38,897	1,341.28	2,416	22,403
Norway	1,958,236	357,670	13,247.04	14,094	185,184
Poland	1,322,917	380,944	14,109.04	51,511	266,083
Portugal	2,720,297	403,875	16,828.13	43,855	184,899
Romania	39,150	22,257	856.04	4,954	12,672
Serbia	255,835	96,465	4,593.57	21,227	71,646
Slovakia	1,227,294	264,943	11,519.26	35,276	176,729
Slovenia	25,016	7,693	274.75	906	4,764
Spain	10,973,161	1,508,997	44,382.26	193,814	975,539
Sweden	4,794,089	600,733	21,454.75	27,185	240,551
Switzerland	4,319,293	518,893	19,957.42	36,157	306,026
UK	8,642,177	2,882,992	80,083.11	377,577	1,875,066
Ukraine	164,104	47,837	2,079.87	11,643	32,125
Total	79,548,543	13,937,735	20,648.50	1,690,447	8,711,580

Notes: This table reports the number of observations and firms in the IV sample by country. An observation refers to a firm-year. The column *# firms per year* shows the average number of unique firms observed annually. The columns *# firms with only women* and *# firms with only men* indicate the number of firms whose boards consisted exclusively of women or men, respectively, in all observed years. Firms that ever reported mixed-gender boards are not included in these counts. Low counts for some countries reflect limited scope of financial information.

Appendix B. Additional results

Table A3. The First Stage and Reduced Form results.

	First stage: $\text{GBD}_{i,t}$		*Reduced Form:* Labor $\text{Share}_{i,t}$	
	(1) Unweighted	(2) Weighted	(3) Unweighted	(4) Weighted
$\text{IV}_{i,t}$	-0.390***	-0.313***	-0.006***	-0.024***
	(0.008)	(0.007)	(0.001)	(0.002)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
F statistic	2397.077	1986.446	20.112	107.464
No. of firms	2,502,659	2,502,659	2,502,659	2,502,659
No. of observations	16,634,414	16,634,414	16,634,414	16,634,414

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. Standard errors are clustered at the firm level.

Table A4. Sensitivity to Sample Restrictions

	At least 5 obs. per firm		*Board size >3*		*Exclude GBD always 0 or 1*	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
$\text{GBD}_{i,t}$	-0.005***	0.080***	-0.006***	0.062**	-0.003***	0.048***
	(0.001)	(0.008)	(0.002)	(0.025)	(0.001)	(0.006)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F stat.		1753.406		562.496		1544.241
C test p-value		0.000		0.007		0.000
No. of firms	1,437,070	1,437,070	272,756	272,756	969,481	969,481
No. of observations	13,094,232	13,094,232	2,086,958	2,086,958	7,167,101	7,167,101

Notes: Table reports estimates of equation (1) with GBD instrumented by the shift–share IV. Columns apply sample restrictions. At least 5 obs. keeps firms with at least five observed years; Board size >3 keeps firms whose average board size over the sample exceeds 3; Exclude constant GBD firms drops firms with constant GBD (always 0 or always 1). All specifications include firm and year fixed effects and apply country population weights; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.

Table A5. Sensitivity to Instrument Granularity and Board Definition

	3 digit IV		*Management Board*	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
$\text{GBD}_{i,t}$	-0.004***	0.084***	-0.001	0.013*
	(0.001)	(0.008)	(0.001)	(0.007)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
First Stage F stat.		2244.709		1950.464
C test p-value		0.000		0.054
No. of firms	2,502,659	2,497,601	1,445,485	1,445,485
No. of observations	16,634,414	16,557,552	9,005,630	9,005,630

Notes: Columns (1)–(2) use a shift–share instrument for GBD with shocks defined at 3-digit NACE level. Columns (3)–(4) redefine GBD as the share of women in the management board and use the corresponding shift–share instrument. All specifications include firm and year fixed effects and apply country population weights; standard errors are clustered at the firm level. First stage F stat. is the conventional F statistic; C-test p-value tests the null that GBD is exogenous. * p<0.1, ** p<0.05, *** p<0.01.