Buying Out the Means of Production: Wages and Productivity in Labor-Managed Firms*

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Abstract

This paper studies the effect of labor management – majority employee ownership of a firm – on firm-level wage distributions and performance. Using matched employer-employee administrative data from Italy, I exploit worker buyouts (WBOs) as sharp transitions from conventional ownership to labor management. I compare WBO firms to observationally similar restructuring firms that remain conventionally owned. Labor management reduces base wages by 9 percent, but increases total compensation once profit-based labor dividends are accounted for. Within-firm wage inequality decreases markedly, and the firm becomes significantly less hierarchical. I find no evidence of lower productivity or reduced investment. Overall, labor management generates substantial within-firm wage compression without reduced operational efficiency.

JEL Classification: G34, J31, J54, M54, P13

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1 Introduction

How does majority ownership of a firm by its employees affect its wage distribution and overall performance? Labor-managed firms (LMFs) are owned by their employees, who exercise control by electing directors under the 'one head, one vote' principle. This stands in contrast to conventional firms (CFs), in which investors own the firm and exercise control proportional to their equity stakes. LMFs have existed for over two centuries, and economists, political philosophers, and policy-makers have repeatedly advocated for labor-management as a way to increase worker participation in firms' decision making and ultimately contain inequality.[1] At a fundamental level, labor-managed firms raise a core question of economic organization: should firms be owned and governed by their workers, much as political democracies are governed by citizens?

Theoretically, the efficiency and equality implications of worker control are ambiguous. On the one hand, some authors have argued that LMFs should be more productive and pay higher wages than conventional firms thanks to improved motivation and co-monitoring (Kandel and Lazear, 1992). On the other hand, concerns about free-riding have prompted others to doubt its efficacy at providing individual incentives (Alchian and Demsetz, 1972; Holmström, 1982). Also, it is unclear whether labor management decreases *overall* wage inequality by redistributing wages from high- to low-wage workers, or only *within-firm* wage inequality by inducing exit of high-earners (Abramitzky, 2008). Given rising wage inequality (Song et al., 2019), decreasing worker bargaining power (Stansbury and Summers, 2020) but also sluggish growth in many developed economies, one key question is whether increasing worker representation in firms implies a tradeoff between equality and efficiency.

I study the reduced form effects of labor management on firms' wage levels, wage inequality, employment and productivity, as well as the mechanisms driving these results. Providing causal estimates of labor management is challenging because a firm's legal form is an endogenous choice. Firms that operate as labor-managed enterprises may differ systematically from conventional firms in ways that are directly related to wages, inequality, or performance. Hence, a simple comparison of existing LMFs and conventional firms is unlikely to provide

[1] Labor-management had early proponents among nineteenth-century socialist writers (Owen, 1813; Proudhon, 1868), as well as among classical liberal political economists: J. S. Mill predicted that labor-managed firms would become the dominant form of production (Mill, 1848). In the twentieth century, labor-management was advocated for by Nobel Laureate James Meade (Meade, 1993) and political theorist Robert Dahl (Dahl, 1990). Today, several European countries – including France, Spain, and Italy – maintain legal frameworks that promote labor-managed firms. In the United States, proposed legislation such as the National Worker Cooperative Development and Support Act would introduce federal support for the expansion of labor-managed firms.

credible causal estimates. To overcome this issue, I study worker buy-outs (WBOs). WBOs are sharp transitions from conventional ownership to labor management: employees of a conventional firm acquire its assets and transform it into a labor-managed firm. Because WBOs represent a discrete shift from investor control to worker control within the same productive unit, they provide an opportunity to study the effects of labor management using within-firm variation.

I implement this approach using matched employer-employee data from Italian social security records. In the main specification, WBO firms are compared to a matched sample of conventional firms that *also* undergo a restructuring in the same years but remain conventionally owned. This comparison nets out effects due to financial distress and cohort or age effects. Identification relies on parallel trends assumptions. To support the validity of such assumptions, I document that trends are parallel for all main outcomes in the periods before the restructuring. Additionally, I discuss the main sources of selection into a WBO: the level of financial distress, worker homogeneity and social capital, political connections of the firm and the specificity of human capital. I show that these characteristics are balanced across the two groups of firms.

The results show that labor management leads to substantial wage compression and hierarchical reorganization without harming efficiency. First, base weekly wages fall by about 9% and remain lower in the years after the buyout. However, employee-members in labor-managed firms also receive profit-based labor dividends. When accounting for such labor dividends, using the financial information available for a representative subsample of firms, the effect on total employee compensation is positive (3.4%). Thus, labor management shifts the composition of pay by reducing fixed wages while increasing performance-contingent income.

Second, labor management leads to a clear decrease in within-firm wage inequality. The 90th percentile of within-firm wage decreases by 12.7%, the median decreases by 5.5% and the 10th percentile by 3.5%. These differences are statistically and economically significant. Importantly, I am able to unpack the mechanisms driving wage compression. In particular, I show that the same result holds among workers who remain continuously employed before and after the restructuring, showing that changes in wage policies drive the compression, not selective retention. The implied reduction in the P90/P50 ratio offsets roughly one-third of the increase in this ratio observed in the Italian private sector between 1985 and 2020.

Third, wage compression is mirrored by an adjustment in organizational structure, in ways consistent with increased worker control. WBO firms significantly flatten their hierarchies:

they are 30.5% more likely to have no managerial layer at all and consequently operate with 0.178 fewer layers on average.[2] This suggests that labor-managed firms reallocate managerial functions and compensation to nonmanagerial workers.

Fourth, employment drops by about 23.7% in the short run after the restructuring, but the effect is a precisely estimated zero after two years due to intensive hiring. The short-run decline in employment is likely driven by voluntary quits. Workers may be reluctant to join a WBO because of financial risks, including the requirement to contribute their unemployment benefits, and additional uncertainty surrounding labor management. These separations are unlikely to be layoffs because WBOs are designed to protect jobs, and worker-members may face pressures from policy-makers to retain previous employees.[3]

Finally, despite changes in wage policies, organizational structure and worker turnover, labor management does not harm firm performance. For the subset of firms for which I have detailed financial information, the effect on value added per worker is positive (14%) but not significant. This could be due to the combined result of two opposite forces at work. On the one hand, ownership by workers may increase mutual co-monitoring – and reduce wage costs by cutting the managerial layer. On the other hand, common ownership may decrease individual effort because of free-riding. The detailed financial information also allows me to analyse effects on investment. I do not find that labor-managed firms significantly underinvest in the long-run: the estimated effect on the value of assets per worker is -21%, but not significant. This goes in contrast with prominent theoretical predictions (Alchian and Demsetz, 1972).

Overall, the empirical evidence I presented indicates that labor management increases equality without harming efficiency. It induces significant wage compression and hierarchical flattening, consistent with democratic worker control, but does not reduce efficiency-related outcomes such as average worker compensation or value added per worker.

This paper contributes to three strands of literature. First, it directly contributes to the empirical literature on labor-managed firms, worker cooperatives (Craig and Pencavel, 1992; Pencavel, Pistaferri and Schivardi, 2006; Burdin and Dean, 2009; Fakhfakh, Pérotin and Gago, 2012; Burdin, 2016; Blanchard, Burdin and Dean, 2025; Burdin and Garcia-Louzao, 2025) and other institutions with cooperative property rights like kibbutzim (Abramitzky,

[2] Here, I define a firm to have one layer if it only employs blue-collar workers, two layers if it employs blue- and nonmanagerial white-collar workers, three layers if it employs blue-collar, nonmanagerial white-collar and managerial workers.

[3] According to representatives of one large cooperative association in Italy, layoffs of pre-WBO employees are extremely rare.

2008) and agricultural cooperatives (Montero, 2022). Overall, this literature has found mixed evidence with respect to wage levels, suggests reductions in wage inequality, however not based on within-firm comparisons, and finds generally positive or null effects on productivity. I make two contributions to this literature. First, I provide a novel identification strategy that accounts for selection of firms into labor management. Therefore, I provide credible causal evidence of the effect of labor management in a modern developed economy. Second, thanks to the granular information about earnings for individual workers and detailed financial information I can muster for a representative subset of firms financial statements, I can precisely study the efficiency-equality tradeoff in labor management. Closer to this paper, Dean (2024) finds that workers employed by 30 Uruguayan recovered firms – firms that transitioned to labor-management after a bankruptcy – were earning less than employees of both LMFs formed from scratch and conventional firms. Recovered firms (*empresas recuperadas*) in Argentina and Uruguay represent transitions to labor-management and are therefore comparable to WBOs. However, these transitions are typically conflictual – resulting in strikes, occupation of the factory, legal disputes and periods of inactivity (14.2 months on average in Uruguay) (Martí, Thul and Cancela, 2014). For this reason Italian WBOs are arguably a cleaner setting to study the effects of labor management.

Second, this paper contributes to the growing literature on worker representation in firms. Recent papers have provided causal evidence on the effect of co-determination, which confers minority controls rights to workers (Jäger, Schoefer and Heining, 2021), and worker voice arrangements, which give workers formal consultation mechanisms but no voting rights (Harju, Jäger and Schoefer, 2021).[4] These papers found moderate or null effects on wages, moderate or null effects on within-firm wage inequality and moderate or null effects on labor productivity. I contribute to this literature by studying a more radical model of worker participation, showing that majority employee control produces larger effects on within-firm wage inequality and significantly alters firms' organizational structure. This suggests that worker participation can be seen as a continuum, but that there is an important qualitative shift above the 50 percent threshold in employees' voting rights. These papers and mine contribute to the larger literature on industrial relations, which has mainly focused on unions (Freeman and Medoff, 1984; Card, Devicienti and Maida, 2014; Farber et al., 2021; Frandsen, 2021). Consistent with the literature on unions, I find that labor management decreases within-firm wage inequality and has moderately positive effects on total compensa-

[4]For example, the German co-determination model studied in Jäger, Schoefer and Heining (2021) establishes a mandate for firms with 500-2,000 employees to assign 1/3 of seats in the supervisory board to worker representatives. The Finnish model studied in Harju, Jäger and Schoefer (2021) provided the right to representation in firms with more than 150 workers, which in practice resulted in the establishment of advisory boards with no formal decision-making authority.

tion. Internal participation mechanisms (like labor management and co-determination) and external ones (like unions) are clearly related, and their interaction and relative strengths merit further study.

Third, this paper contributes to the literature on the drivers of within-firm wage inequality and wage inequality in general (Lemieux, MacLeod and Parent, 2009; Song et al., 2019; Casarico and Lattanzio, 2024). This literature has established the importance of firms and workers' sorting into different types of firms in explaining patterns in wage inequality. I contribute to this literature by stressing the importance of firms' ownership structure in explaining within-firm inequality: a more egalitarian property right distribution leads to more egalitarian wage distributions. Importantly, I show that this happens through changes in wage policies and not because of changes in worker sorting into firms.

The rest of the paper is organized as follows: Section 2 explains the institutional context, Section 3 describes the data, sample construction and presents my empirical approach, Section 4 shows the empirical results. Finally, Section 5 concludes.

2 Institutional Context

In this section, I explain that governance in worker cooperatives differs to that of conventional firms in the way votes are allocated in the shareholder assembly – giving equal voting rights to all employee-members – and not on other dimensions. Taxation differs in some aspects but not in ways that would threaten the interpretation of the main effects. I then explain the relevant institutional framework for WBOs in Italy and why this is a good setting to study the effect of labor management. Finally I review Italian wage setting mechanisms to show that labor management can have an impact on wages.

2.1 Comparative corporate governance: conventional firms vs. worker cooperatives

For the purposes of my analysis, I define conventional firms (*Società per azioni*, *S.p.a*, or *Società a responsabilità limitata*, *S.r.l.*) to be for-profit firms that distribute voting rights according to capital ownership. Cooperatives (*Società a capitale variabile con scopo mutualistico*), on the other hand, generally distribute voting rights to partners according to

the 'one-head, one-vote' principle.[5] Apart from the way they confer voting rights in the shareholder assembly, conventional firms and cooperatives share many aspects of corporate governance. In both firm types, the shareholder assembly appoints a board of directors, which is responsible for the overall management of the firm and a supervisory board, which monitors the board of directors.[6] Worker cooperatives are a specific type of cooperative in which at least two-thirds of voting rights have to be distributed to worker-partners. Non-working partners - financing partners - can have at most one third of voting rights.[7] While there is a floor to worker-partner to partner ratio, there is no explicit minimum ratio of worker-partners to total employees. However, a worker cooperative must pay at least half of its wages to worker-partners and this indirectly regulates the number of non-partner employees it can hire.

Figure 2 illustrates the main differences in corporate governance between the two firm types.

2.2 Tax regime.

Taxation of employees' labor earnings. Employees are subject to the same taxation of labor earnings regardless of whether their employer is a worker cooperative or a conventional firm. Base wages are subject to income tax and social security contributions in both types of firms. Employee-members in worker cooperatives may receive additional compensation at the end of the year in the form of labor dividends (*ristorni*). Labor dividends are not subject to social security contributions and, during the study period, were taxed at a flat rate of 26%. This tax rate is comparable to the one applied to end-year bonuses in conventional firms.[8]

Taxation of firms' profits. Cooperatives may enjoy exemptions from taxes to profits for up to 70% of their profits. In particular, indivisible reserves and labor dividends are exempt

[5]Here I deliberately use the word 'partner' and not 'worker-partner' because cooperatives can be of different types: consumer, credit, producer and worker.

[6]If they specifically specify so in their charter, CO firms can opt into different corporate structures, which however do not alter the distribution of voting rights among partners. These alternatives to the default option are: the two-tier board system, like in Germany, and the single-board system, where control and supervision are both carried out by the board of directors.

[7]Financing partners can be natural or juridical persons. Each individual worker cooperative can decide to relax the 'one head - one vote' for financing partners, instead rewarding them according to their capital contributions. Again, this is allowed as long as financing partners do not, collectively, have more than one third of voting rights.

[8]Before 2021, the year-end bonus in conventional firms was taxed at 10% for the first 3,000 euros and at the employee's marginal income tax rate after that. Provided that the size of the bonus is small enough, employees in conventional firms may enjoy a lower average tax rate.

from profit taxes up to a combined total of 70%. The allocation of profits is not completely discretionary. Cooperatives must allocate a minimum of 30% (and up to 70%) of their profits into non-distributable reserves. These funds are meant to capitalize the cooperative, can never be appropriated by members and are tax-free. The remainder share of profits may be distributed as labor dividends to employee-members, and will be free from profit taxes subject to the 70% tax-free threshold.[9]

Unlike conventional firms, cooperatives face certain restrictions on dividends to share capital. The dividend to shareholders cannot exceed 2.5% plus the yield on Postal Savings Bonds (*Buoni Fruttiferi Postali*) of the value of shares.[10] Given these constraints, paying out dividends as a return to share capital is highly unlikely in worker cooperatives. Instead, worker cooperatives redistribute profits in the form of labor dividends.

2.3 Worker buyouts.

Worker buyouts (WBOs) are transitions to labor management that happen when the owners of a conventional firm sell its assets to a worker cooperative formed by previous employees of the firm. In Italy, the institutional framework for WBOs was established by the 1985 Marcora Law (L. 49/1985) and subsequently modified by later legislation (notably L. 57/2001 and the '*Nuova Marcora*' measures). Since 1986, the process has generally been overseen and financed by a state-backed entity *Cooperazione Finanza Impresa* (CFI). The institutional details, however, differ across subperiods: 1986–2001 (Marcora I regime), 2001–2010, 2011–today (Marcora II). The original version of the law (1985–2001) established CFI as a publicly funded agency that provided equity stakes and long-term subsidized financing for worker cooperatives arising from buyouts. This regime generated a substantial wave of WBOs in the late 1980s and 1990s. The Marcora Law was reformed in 2001 (L. 57/2001). During this transition, CFI temporarily stopped financing new WBOs, resulting in a near-total pause in new state-supported buyouts from 2002 to 2010. From 2011, CFI resumed financing and technical support for WBOs. Financing shifted partially from equity participation to matching loans: workers invest personal resources (severance pay, unemployment benefits, savings), and CFI can lend up to twice the workers' collective investment, with repayment horizons up to 10 years (CFI, 2022). Using CFI administrative data, I identified 310 Marcora-

[9]For example, if a worker cooperative allocates 30% of profits to reserves and 40% to labor dividends, both these will be exempt from profit tax and 30% of profits will be taxable. If it allocates 30% to reserves and 50% to dividends, it is still the case that 30% of profits will be taxable.

[10]The Postal Savings Bonds are government-backed savings certificates issued by *Poste Italiane*; their interest rate is set by the issuing authority and accrues over time according to the specific bond's schedule. For example, in 2025 the 20-year bond yields 2.50% on average until maturity.

supported WBO operations between 1986 and 2021.[11] From these, I identified 140 WBO worker cooperatives that (i) have been formed between 1986 and 2020, (ii) have a predecessor that can be identified with some confidence in the Social Security data, (iii) the predecessor had at least an average of 5 employees during the pre-period. I will explain the linking methodology in detail in Section 3.

Because WBOs happen when a conventional firm is in financial distress and the owners want to liquidate it, the empirical strategy will directly account for this negative productivity shock by selecting control firms that also eventually undergo a restructuring.

2.4 Wage setting in Italy.

Italy is a country with scope for firm-level wage setting, so labor management could have an effect on firm wage policies. Sectoral collective bargaining is a very important feature of the Italian labor market and it could in principle limit the scope for firm-level wage setting. However, unions and employer associations only negotiate wage floors and other non-wage items (holidays, paid sick leave). Therefore, employers can always pay above the minimum. Consistent with scope for firm-level wage setting, Italy has considerable within-sector raw wage dispersion: the variance of log weekly wages is 0.52 (77% of total variance), both within (0.36) and between-firm (0.16) (Briskar et al., 2022).[12] This is true even adjusting for workforce observable characteristics (age, tenure, education, gender): Italy and Germany also have comparable firm pay premia estimated using two-way fixed effects model that control for unobservable but fixed worker characteristics, on top of observable ones (Abowd, Kramarz and Margolis, 1999). Over the period 1997-2003 (the latest period for which these estimates are available), the dispersion in AKM firm pay premia was 0.190 in Italy and 0.194 in Germany (over the period 1996-2002) (Card, Heining and Kline, 2013; Devicienti, Fanfani and Maida, 2019). However, to the best of my knowledge, there is no decomposition of firm pay premia into within- and between-sector. Additionally, there is evidence that locality-specific shocks, like negative labor supply shocks, affect wages, which further suggests that there is scope for firms to set wages beyond sectoral agreements (Dicarlo, 2022). Hence changes in corporate governance could in principle affect wage policies.

Because cooperatives and conventional firms belong to different employer associations, they

[11] Some WBOs in the 1986-2001 period were backed by state agency called *Soficoop*. The CFI data consolidates buyouts overseen by both agencies.

[12] This is calculated using 4-digit sectors, but results are similar when using 2-digit sectors. For example, within 2-digit sector variance is 0.543.

may sign different collective bargaining agreements (CBAs). If the wage floors are different, this could explain why cooperatives and conventional firms have different wage distributions. Data from INPS allow me to observe what CBA regulates the employment relationship. In the majority of cases cooperative association and conventional employers associations are co-signatories to the same CBAs. The five most common CBAs for worker cooperative employees, which cover 66.84% of employee-years, were signed also by both major conventional employer associations (Confindustria and Confcommercio).[13] Among the top 10 CBAs, which cover 78.5% of total employee-year observations, only the metalworking sector had two different CBAs, one for cooperatives and one for conventional firms.[14] However, the minimum wage floors in 2023 were identical at 1.488,89 euros per month. In conclusion, it does not seem like different CBAs can explain potential differences in the wage distribution in the two types of firms.

3 Data and Empirical Approach

3.1 Data and variable definition

The main analysis is based on administrative matched employee-employer data from the Italian Social Security Administration (INPS) merged with firm-level balance sheet and income statement data from two private providers (Cerved and Orbis).

Matched employee-employer data: INPS panel. The matched employee-employer data are based on social security records and cover the universe of private sector employees and their employers from 1980 to 2021. It is a panel of yearly employee-employer relationships with information about gross earnings (both fixed and variable), occupation, contract characteristics (temporary vs. permanent, part-time vs. full-time), worker demographics (age, gender, place of birth, nationality) and some firm characteristics (sector, location, date of establishment). The main definition of earnings is weekly wages, obtained by dividing gross annual earnings (coming from the main employment relationship) by full-time equivalent weeks worked (again, in the main employment relationship); this makes the wages of part-time and full-time workers comparable. I deflate wages by the 2015 CPI.[15] Employment

[13] The information on signatory parties, wage floors and other items is available online from *Consiglio Nazionale Economia e Lavoro* (CNEL): https://www.cnel.it/Archivio-Contratti/Minimi-retributivi.

[14] This CBA covers 2% of employee-year observations for worker cooperative employees.

[15] This is useful for summary statistics but is irrelevant for the main event study estimates because of year effects.

at a given firm in a calendar year is obtained by counting the number of worker-year spells in that firm and year. The concept of founding year refers to the year in which the firm started its relationship with the social security administration. The Italian Social Security Administration assigns a different establishment ID (not firm ID) to establishments within the same firm if they operate in different sectors. For example, if a firm operates a gas station and a coffee shop next to the gas station, these will have two distinct establishment IDs. Since my analysis is at the firm level, I define a firm's sector as the sector of the establishment with most employees in the year before the transition.

Firm income statement and balance sheet data: Cerved-Orbis panel. Income statements and balance sheet data are available from two sources, Cerved and Orbis. Together, they cover the period 1996-2021 and contain yearly information about value added, sales, operating costs, profits, assets and debts. I deflate balance sheet and income statement variables by the 2015 CPI.[16] I set to missing all spuriously negative values of value added, revenues and assets. I winsorize all continuous variables at the 1% level on each tail. I will use this sample to estimate effects on firm performance and to adjust wages in worker cooperatives for potential dividends.

Total compensation in worker cooperatives. Employee-members in worker cooperatives may receive labor dividends (*ristorni*) at the end of the year. Unfortunately, I cannot observe dividends directly because they are not subject to social security contributions. However, I can observe firm profits whenever financial statements are available. Hence, I can impute the potential dividend that each worker is eligible to receive at the end of the year for a subsample of firms in the 1996-2021 period. This is intended as an upper bound on total labor earnings. The Civil Code (Article 2545-sexies) requires labor dividends (*ristorni*) in worker cooperatives to be "*proportional to the quality and quantity*" of the work done for the cooperative and at most 30% of their total labor earnings. Following this requirement, I define the weekly labor dividend for each employee to be weekly firm profits times that employees's share of the weekly wage bill. I cap the labor dividend to be at most 30% of the labor earnings. Employees' weekly total compensation is their wage plus the labor dividend.[17] It is important to note that this definition of total compensation is inequality preserving: it does not change percentile ratios computed over base wages. Finally, I opt to assign a labor dividend to all employees of the firm because I cannot reliably observe which

[16] Again, this is useful for descriptive statistics but irrelevant for the main estimates due to the year fixed effects.

[17] For example, suppose employee i is working full-time for worker-cooperative j and earning 500 euros per week. Worker-cooperative j has 10 full-time employees, a weekly wage bill of 4000 euros and yearly profits of 50,000 euros. Then i's weekly labor dividend will be (500/4000) * (50,000/52) = 120 euros. Her total weekly compensation will be 620 euros.

ones are worker-partners.[18]

3.2 Sample definition

Starting from the universe of yearly employee-employer spell in the private sector, I only keep employment relationships involving workers aged 18-65 and with positive earnings. I also keep one spell per worker, namely the one with most full-time equivalent weeks worked.[19] Then, I collapse the dataset to a yearly (unbalanced) panel of firms spanning 1980 to 2021. I use a matched sampling approach as in Jäger and Heining (2022) to select an appropriate comparison group to WBO firms. I identify a comparison group of firms that are in distress, are undergoing a restructuring, and have, three years prior to the restructuring, characteristics similar to the ones of treatment group of WBO predecessors.

Time notation. Let t denote a calendar year and c denote the event year, so $k = t - c$ denotes elapsed-time since the event. For each firm j, the event year c_j is defined as the establishment year of the newborn worker cooperative for treated firms, and the establishment year of the newborn conventional firm for comparison firms. So both treated and comparison firms may belong to a cohort c.

Treated group. I identify worker cooperatives formed through a worker buyout by matching INPS-Cerved data with data from Cooperazione Finanza Impresa (CFI), the government agency that oversees WBOs in Italy, and Legacoop, one of the major cooperative associations in Italy. While the dataset from Legacoop includes tax IDs, the one from CFI does not. I therefore hand collected tax IDs from the internet. Eventually, the CFI-Legacoop dataset has tax IDs for 266 worker cooperatives created through a WBO from 1986 to 2021. The first challenge is to link these worker cooperatives to their predecessor firms, which are legally separate firms, and have a different firm ID in social security records. To do this, I exploit detailed information about the working histories of employees in WBO firms. For each worker cooperative created via WBO, I define the predecessor as the firm who employed the

[18] As explained more in detail in Section 2, employment in worker cooperative does not automatically entail partnership in the firm. Worker cooperatives are allowed to employ non-partners, as long as the share of wages they pay to non-partners does not exceed 50%. There is a flag that potentially identifies employee-members which comes from a separate dataset (*Comunicazioni Obbligatorie*), available only from 2010. Due to the large number of missing values and worker spuriously flagged as worker-partners in conventional firms (where by definition they cannot be worker-partners), I do not use it for the main definition of total compensation. In this data, over 80% of employees of WBO firms are members.

[19] If there is a tie, I choose one spell randomly. This is preferable to breaking ties in favor of the spell with higher earnings to avoid selectively dropping spells, as there might be systematic differences in earnings between WCs and CFs.

largest number of the post-transition WBO workers. On average, 80% of WBO workers were employed by a common predecessor firm. To ensure that the match is accurate, I require the predecessor to disappear from the social security records at most one year after the establishment of the WBO firm.[20] I define the founding year of the worker cooperative as the event year c and assign both predecessor and successor firm a common identifier j. WBO firms with no clear predecessor are dropped following a manual check. I restrict my analysis to firms that, on average over the pre-restructuring period, have at least 5 employees. This allows me to focus on firms where formal firm organization is likely to play a role beyond informal social ties, and to be able to make meaningful statements about wage inequality. This leaves me with 140 WBOs for which I find a good predecessor match and have at least one observation before and after the WBO.

Pool for comparison group. I sample firms that undergo a restructuring, similarly to WBOs, but remain conventionally owned. I define conventional restructuring firms as follows: the firm is (i) conventionally owned, (ii) established in the same years as WBO firms, (iii) at least 60% of its employees in the year of establishment come from the same previous employer and (iv) this previous employer is conventionally-owned and ceased to exist in INPS records at most one year after the establishment of the new firm. Contrary to WBO events, I do not have direct information on conventional restructurings. Hence, a threshold as in (iii) is necessary. The choice of 60% is appropriate because it yields the treated group average share (80%) of workers employed in the predecessor. I link flagged firms that satisfy the requirements to their predecessor (the previous employer) and consider them the same firm for the remainder of the analysis. Using this definition, I find 17,846 restructuring firms in the years 1986-2020.[21] The event that leads to a firm getting a new ID is the closure of the predecessor firm and the consequent opening of a new firm. The closure may be due to bankruptcy or sale of the predecessor and the newly created firm may have the same owners or different ones. Notice that I am excluding mergers by specifying that the new firm must be founded in the year of the transition. Appendix Table A1 provides summary statistics for the treatment group of WBO firms and the donor pool for comparison firms.

Matched sampling to select comparison group. I implement a 1-to-many coarsened matching procedure separately for each cohort c, with replacement. For each WBO predecessor, I select firms that have exactly the same following characteristics in $k = -3$: quintile of average wage, quintile of employment, quintile of the hiring rate and a dummy variable for

[20] A firm may not immediately disappear from social security records. For example a firm would be in the records for 2005 if it employed one worker in January 2005.

[21] For 91% of control firms, the predecessor and the successor are in the same province.

manufacturing.[22] I use these variables to ensure that firms are comparable in terms of wage levels, size and sector. I set the matching period to $k = -3$ to make firms comparable before the onset of financial distress. Results are robust to using different matching variables, to keeping only 1 matched comparison firm, and to using synthetic control methods, as shown in Section 4.5.

I find a match for 93/140 WBO firms. The comparison group is composed of 326 firms in total, so that on average each treated firm has 3.5 comparison firms for each treated firm (median 3). I denote each matched group with g. Among these firms, I have financial information from the Cerved-Orbis sample for 34 WBO firms and 190 comparison firms over the 1996-2021 period.

3.3 Summary statistics

Table 1 contains summary statistics for the full INPS 1980-2021 matched sample, and Table 2 presents statistics for the 1996-2021 subsample for which financial information is available from Cerved-Orbis. It should be noted that, even in the presence of level differences, the difference-in-differences design is valid because it relies on the common trends assumption.

Wages, employment and worker demographics. WBO predecessors pay lower weekly wages: the average is about 12% lower than the comparison group (comparison-group mean in logs 6.288, or 539 euros). The 10th, 50th and 90th percentiles of the within-firm wage distribution are also lower in WBO predecessors. WBO predecessors are smaller, employing 78 workers on average, compared to 97 in the control group. This difference in levels is not statistically significant and should not be a source of concern, as the most important threshold in the Italian labor market is 15 employees.[23] The hiring rate is 7.6 percentage points higher in WBO predecessors. Average age, average tenure and the use of part-time and fixed-term contracts are balanced. Comparison firms have, in proportion, almost twice

[22] Other papers using coarse exact matching bin continuous variables by deciles (Jäger and Heining, 2022) or pentadeciles Arnold et al. (2023). Because I have few units in the treated group, I prefer quintiles in order not to lose too many observations.

[23] The most important threshold in firm size in the Italian labor market is at 15 employees, beyond which firms pay higher firing costs, are required to hire at least one worker with a disability and workers have a right to establish a works council. One of the two main short time work schemes (*Cassa Integrazione Guadagni Straordinaria*) is available only for firms above 15 employees in some sectors, most notably manufacturing, which is by far the most common sector in my sample. For other sectors, like wholesale and retail trade, it becomes available for firms above 50 employees. The other main short time work scheme (*Cassa Integrazione Guadagni Ordinaria*) is available for all manufacturing firms irrespective of size. For more info about Italian short time work schemes see the review in Giupponi and Landais (2023).

as many managers.[24] However, the probability of having no managerial layer at all is similar (0.6 and 0.5, respectively).

Firm sector. The two groups are roughly balanced in broad sector: manufacturing accounts for 52.7% of WBO firms and 61.3% of comparison firms, and the difference is not significant (p=0.14). Notice that since matching is one-to-many, the manufacturing share need not be identical in the two groups. Indeed, treated manufacturing firms had more eligible controls firms and therefore contributed more to the control group.

Income statement and balance sheet variables. Table 2 reports summary statistics for firms in the 1996-2021 for which financial information is available. First, note that this subsample is comparable to the main sample, even if firms are somewhat smaller. Second, within the subsample, the two groups of firms are similar in terms of financials. Although these differences are not significant, value added per worker is 15% lower in WBO firms, but EBITDA per worker is 3.4% higher. Importantly, both groups make negative profits on average, which resonates with the fact that they will eventually experience distress. Also, WBO predecessors do not have higher ratios of debts over EBITDA.

Overall, matching is successful in balancing the two groups on the most important dimensions.

3.4 Estimating equation and identification strategy

I estimate the effect of labor management by comparing outcomes for WBO firms to outcomes of comparison firms after the transition using an event study specification. I estimate the following specification via OLS:

$$Y_{jt} = \alpha_{gt} + \eta_j + \sum_{\substack{k=-5 \\ k\neq -3}}^{k=5} \beta_k \big(\mathbb{1}\{k = t - c_j\} \times WBO_j\big) + \big(\beta_{6^+}\mathbb{1}\{k \geq 6\} + \beta_{6^-}\mathbb{1}\{k \leq -6\}\big) \times WBO_j + \varepsilon_{jt} \tag{1}$$

where α_{gt} indicate calendar year times matched group fixed effects, η_j are firm fixed effects, $\mathbb{1}\{\cdot\}$ is the indicator function, WBO_j is a dummy variable equal to 1 if the firm is in the treatment group, c_j indicates cohort, that is the year of founding of the new firm, ε_{jt} is the error term, clustered at the firm level. Coefficients β_{6^-} and β_{6^+} absorb effects in very early

[24] As a reminder, I defined as managers both middle managers (*quadri*) and executives (*dirigenti*).

and very late elapsed time periods, respectively.

The coefficients of interest are β_k and capture the effect of labor management k years after the transition. I normalize the coefficients to be zero at $k = -3$, because that is the matching period, and to be able to evaluate potential anticipation effects. By estimating year FE separately for each matched group, I only exploit the variation within each matched group and elapsed time. This is more restrictive than within-cohort comparisons common in the labor economics literature (e.g. Jäger and Heining (2022); Schmieder, Von Wachter and Heining (2023)) and similar to the approach in Greenstone, Hornbeck and Moretti (2010). I do this to avoid comparing firms that are in the same cohort, but potentially have very different characteristics.[25]

Additionally, I estimate the average post-treatment coefficient β_{Post} by pooling all coefficients β_k when $k = 0, ..., 5$; a short-run coefficient β_{SR} by pooling β_k when $k = 0, 1, 2$; and a long-run coefficient β_{LR} by pooling β_k when $k = 3, 4, 5$.

Identification assumptions and potential threats to identification. The main assumption for identification is the common trends assumption (CTA), which requires that outcomes would have evolved similarly in the treatment and comparison groups absent the treatment. Because of the availability of pre-treatment data, and because I have matched only on levels, not trends, I will evaluate this assumption by looking at event study coefficients on pre-treatment periods $k < 0$. Coefficients should be close to zero if the two groups follow parallel trends before the treatment. Any shock that simultaneously affects outcomes and selection into a WBO is a threat to identification. As I will show in the Results Section, event studies support the common trends assumption.

Below I examine six potential sources of selection that could matter in levels: financial health, trust levels among workers, worker homogeneity, political connections, firm-specific human capital and access to subsidies. First, WBO predecessors may be in a worse financial situation in the baseline period $k = -3$ and/or may follow worse trajectories than comparison firms. This would suggest that investors might have been unwilling to buy WBO predecessors and that workers resorted to the buyout for lack of alternatives. Such a scenario would violate the CTA. Financial characteristics three years before the transition are balanced in levels, as shown in Table 2. EBITDA per worker is virtually the same in the two groups, profits per worker are negative for both groups, more negative for WBO firms but not significantly so, and debt to EBITDA ratios are lower for WBO firms, again not significantly. The financial

[25] Greenstone, Hornbeck and Moretti (2010) estimate the effect of opening a large plant on county-level total factor productivity but only exploit within-case variation, i.e. comparing counties that won the assignment case to counties that lost it.

data from Cerved-Orbis do not permit a powerful test of the pre-trend assumption in period $k = -1$, because they are available for only 41% (14/34) of WBO firms and 56% (107/190) of comparison firms, as shown in Panel (b) of Figure 3. For this reason, evaluating coefficients in $k = -1$ is not informative. Two years before the transition ($k = -2$) attrition is not as problematic: information is available for 79% of WBO firms and 87% of comparison firms. Coefficients are very close to zero and support common trends in the two groups in EBITDA, net profits and debts to EBITDA ratios. These can be seen in Panels (d), (e) and (f) of Figure 7. The same is true for coefficients in periods $k = -4, -5$, which show that WBO firms are not on worse long-term trends compared to comparison firms. In the same elapsed time periods, trends are common for value added per worker, sales per worker and profits.

Second, workers in WBO predecessors may have higher levels of trust. Trust refers to the ability to coordinate on socially optimal equilibria and higher levels of trust would explain the ability to overcome the collective action problem that underlies a WBO. This would be a threat to identification if trust affects outcomes, for example labor productivity, both when the firm is conventionally managed and when it is a worker cooperative. I proxy trust using two measures: the share of foreign workers to total employees and the concentration of employees' birth municipality. I use the share of foreign workers as a proxy for trust because research in political economy and political science has shown that natives (who are always the majority in the sample firms) tend to trust foreigners less and that trust is lower with higher local ethnic diversity (Cettolin and Suetens, 2019; Dinesen, Schaeffer and Sønderskov, 2020). Concentration of birth municipalities, which I measure via the Herfindahl-Hirschman index (HHI), is relevant because workers from the same town are likely to share denser social networks and have higher levels of mutual trust.[26] As shown in Panel B in Table 1, the foreign share in WBO predecessors is about the same as in conventional predecessors. Moreover, although birth municipality is significantly more concentrated in WBO firms, the size of the difference is not quantitatively large.[27]

Third, a related issue, workers in WBO predecessors may be more homogeneous in terms of economic preferences. Homogeneity of preferences in boards may make decision more timely and less conflictual. Therefore, more homogeneity could increase the expected value of a WBO for workers. I measure preference homogeneity using the standard deviation of age, which is relevant because of the horizon problem: older worker-partners have fewer incentives to invest in the firm compared to younger ones, and this may lead to conflict in decision-making. I find substantial balance in this dimension.

[26] This is consistent with evidence that Italian social capital and personal connections are highly local.

[27] For reference, if employees come from municipalities in equal shares, the resulting HHIs imply roughly 4 and 5 birth municipalities for WBO firms and comparison firms, respectively.

Fourth, WBO workers may have better access to information about the possibility of carrying out a WBO. If they acquired this information randomly, as for example through the assignment to a particular bankruptcy administrator, this would not be problematic.[28] On the contrary, it could be problematic if it is related to better connections to politicians and bureaucrats because political connections may smooth regulatory and bureaucratic frictions for connected firms (Akcigit, Baslandze and Lotti, 2023). Firms' political connections, measured as employing at least one politician in the firm, are not common: Akcigit, Baslandze and Lotti (2023) find that this probability is 3.5%, which however increases to 45% among firms with more than 100 employees. In my sample, both groups of firms have fewer than 100 employees on average and, if anything, comparison firms are larger. Therefore, it is unlikely that political connections explain the selection into doing a worker buyout.

Fifth, workers may have stronger incentives to carry out a WBO if they have acquired more firm-specific human capital. To proxy for this, I measure in-sample tenure (that is starting from 1980).[29] Workers in WBO predecessors have roughly the same tenure as workers in conventional firms, 3.67 vs. 3.75 years.

Sixth, WBO firms have access to subsidized loans. Loans to WBO firms under the *Marcora* framework have maturities between 3 to 10 years and are often at zero interest. A potential worry is that the WBO is effectively a bundle of two interventions, the transition to labor management and access to credit. At the same time, comparison firms also have access to guaranteed credit and subsidized loans through two large schemes: the Guarantee Fund (*Fondo di Garanzia*) and the Capital Goods (*Beni Strumentali*) schemes. The Guarantee Fund is a general credit-guarantee scheme for SMEs. It does not set loan rates directly but covers part of the default risk, improving access and sometimes marginally pricing. The Capital Goods scheme provides a subsidy equal to 2.75% of the loan for 5 years. It effectively lowers the interest rate faced by firms, but financing remains at a positive rate. Although microdata on take-up of these is not available, a back-of-the-envelope calculation using data on total subsidies paid suggests that take-up among the comparison group should be around 75% for the Guarantee Fund and 35% for the Capital Goods. Hence, my baseline estimates should be interpreted as the reduced-form effect of entering the WBO pathway relative to a counterfactual where firms can still access standard subsidized SME credit.

Having supported the validity of the research design, I proceed to present the empirical results.

[28] This was the case for the WBO of Cartiera Pirinoli, a firm in the paper sector in the Piedmont region.
[29] This measure of firm tenure is left censored for spells beginning before 1980.

4 Empirical Results

In this section, I first examine effects on firm survival. Second, I present results from event studies on average wages, wage percentiles and hierarchy. Third, I show results on employment, workforce composition and hierarchy. Lastly, I present results on productivity and other firm-level outcomes from financial statements.

4.1 Firm survival

WBO firms have a higher survival rate than conventional firms. This can be seen from Panel (a) of Figure 3, which shows the ratio of firms that are still active at each elapsed time k after the transition. The survival rate is equivalent to the Kaplan-Meier estimate. For each elapsed time k, the numerator is the number of firms active at k, and the denominator is the standard risk set, that is firms that were alive in $k = -3$ and have not yet been right censored because their cohort is not observed at k.[30] Survival rates are virtually identical in the two groups in $k = 1$, but they are 10 percentage points (12%) higher in WBO firms after 3 years, and 13 p.p. (19%) higher after 5 years. This is consistent with the idea that worker-partners prefer to take on less risk than conventional firm owners because they concentrate both their human and financial capital in the same firm (Bonin, Jones and Putterman, 1993).[31] Additionally, the structure of compensation in worker cooperatives may help firms survive periods of low productivity by making compensation conditional on good performance. I will provide evidence of this potential mechanism in the following section. Survival rates turn out to be quite similar to the raw attrition rates plotted in Appendix Figure A1.

4.2 Wages

Average wage. Panel (a) of Figure 4 shows the event study estimates of the effect of labor management on the average weekly wage. Column (1) of Table 3 shows the aggregated coefficients β_{SR}, β_{LR} and β_{Post} for the same outcome. The results show that labor management decreases average weekly wages by about 10% in the short run and by 7% in the long run. The coefficients on the periods before treatment can be used to assess the parallel trends

[30] For example, the risk set at $k = 5$ does not include firms in the 2017 cohort or after because the sample period ends in 2021, when all of them $k < 5$.

[31] Further supporting this idea, one experiment found that managers of LMFs are more risk averse than managers of conventional firms (Alves et al., 2022).

assumption. All pre-treatment coefficients are statistically zero. However, the coefficient β_{-1} is positive and somewhat large. This is likely due to workers' exit in anticipation of the WBO, which I will argue for when discussing employment results in two paragraphs.

Total worker compensation. The result that base wages decrease after the firm becomes labor managed may be surprising, given that the firm is becoming worker-owned. However, base wages are a lower bound for total worker compensation in WBO firms. Unlike conventional employees, employee-members of worker cooperatives are entitled to receive a share of the firms' profits at the end of the year. Hence it makes sense to analyze effects on workers' total compensation. As explained in Section 3, this exercise is constrained by two data limitations. First, since I cannot observe dividend distribution, I have to use information about firm profits to impute a potential dividend share for each employee. This measure can be seen as an upper bound for worker compensation. Second, I can only do this for a subset of firms for the 1996-2021 period, which however is comparable to the full sample.[32] Panel (b) of Figure 4 shows the results of the event study specification for this measure of total compensation. Columns (5)-(8) of Table 4 show aggregated coefficients on total compensation for the 1996-2021 panel. All point estimates on total compensation are positive. The aggregated effect on the median profit-adjusted wage is positive (4.5%) and significant. The effect on the average wage is positive (3.9%) over the post-transition period, though only marginally significant, and positive (5%) and significant in the long run. Hence, WBO firms are able to lower base wages and make total compensation contingent on good performance, something that conventional firms cannot credibly commit to. This may also explain higher survival rates of WBO firms.

Wage distribution. What about the distributional effects of labor management, does the firm become more egalitarian in its wage distribution? To answer this question, I estimate an event study specification with different percentiles of the within-firm wage distribution as outcomes. Panel (c) of Figure 4 shows the event study for percentiles 10, 50 and 90. Within-firm inequality decreases significantly after a WBO. The decrease in wage is significantly larger at the 90th percentile than at the median or 10th percentile. Over the post transition period, the effect at the 90th percentile is -12.7% (95% C.I.: -18.6%,-8.2%), whereas the effect at the median is -5.4% (95% C.I.: -8.9%, -2.1%) and the effect at the 10th percentile is -3.5% (95% C.I.: -8.1%,1.2%).

These results could be due to compositional effects. If high-earners leave the firm because they anticipate lower earnings, then the firm wage distribution will be mechanically more

[32] In Table 2 and in Appendix Table A2, I showed that they have similar average statistics and estimated effects, respectively.

egalitarian. Such a mechanism would question the effectiveness of labor management at reducing inequality. However, this is not the case following a worker buyout. As shown in Panel (d) of Figure 4, results are identical when estimated on the subsample of stayers. I define stayers to be workers employed continuously by the same firm from period $k = -3$ to period $k = 2$. I restrict the time window to ensure a sufficiently high number of workers in this subsample. This exercise fixes worker composition and allows to examine changes due to wage policies. Both one and two years after the transition, the estimated effects at the 90th percentile are significantly more negative than the effects at the median and 10th percentile, and very similar to estimates from the full sample. Hence, wage inequality decreases in worker owned firms. Unpacking this mechanism is important because it shows that WBOs have the potential to reduce both overall inequality and within-firm inequality. The implied change in the P90/P50 ratio (8%) roughly corresponds to a third of the total increase (17.6%) over the 1985-2020 period in the Italian private sector (Briskar et al., 2022).[33]

For the 1996-2021 subsample of firms with financial information, the point estimates for P10, P50 and P90 are not significantly different, as can be seen from Columns (1)-(4) of Table 4. By construction, when adjusting for profits, the differences in point estimates are also not significant. However, it is important to note that this is not due to how profits are redistributed but to the effects on base wages for this subsample. The profit adjustment I imputed is inequality-preserving because it is a bonus proportional to the wage. As described in the paragraphs above, this was chosen following legal requirements on worker-cooperative labor dividends.

4.3 Employment, worker composition and hierarchy

Employment. Panel (a) of Figure 5 shows event study estimates on the logarithm of full-time equivalent employees. Table 3 shows aggregated coefficients. Following a buyout, there is a large and negative decrease in employment. In the short-run, employment falls by 27.1 log points, approximately a 23.7% decrease in the number of employees. This decline is temporary. Starting from two years after the transition, the effect on employment is zero. The long-run coefficient is also zero. Panel (b) of Figure 5 shows coefficients for hiring and separation rates. The drop in employment in year 0 of the buyout is driven by higher separation rates. In year 0, both hiring and separation rates are higher in WBO firms. In year 1, separation rates are equal, but hiring rates are much higher in WBO firms. This determines the closure of the (relative) employment gap by year 2.

[33] To be precise, Briskar et al. (2022) use annual earnings and not weekly earnings.

All but one pre-period coefficients are statistically zero. The coefficient β_{-1} is negative and significant, which indicates that workers are leaving WBO firms at a higher rate than they are leaving comparison firms one year before the buyout takes place. Looking at hiring and separation rates in Panel (b) of Figure 5 confirms this interpretation. This pattern can be evidence of either (a.) endogeneity of the buyout to employment losses or (b.) anticipation effects. Option (a.) is equivalent to saying that the buyout happens because many workers have left the firm; (b.) instead means that many workers leave the firms because they know that the buyout is happening. There are reasons to prefer the interpretation of (.b) anticipation effects over (a.) endogeneity. Anticipation effects are plausible because workers' prior knowledge of the buyout is highly likely, after all either most of the workers are involved or their co-workers are. Additionally, there are reasons why workers may pass on the opportunity to participate in the buyout. First, it is financially risky: workers are asked to invest their private resources, such as unemployment benefits, severance pay and savings. Second, joining a WBO involves considerable uncertainty about firm performance and most workers are not familiar with being employed at a worker cooperative, let alone being members of one. On the contrary, endogeneity of the decision to carry out a WBO to employment losses is unlikely. Executing a buyout in less than a year is difficult because it involves multiple layers of negotiation among several stakeholders: employees, previous owners, trade unions, the cooperative employer associations, potential lenders and CFI (the government agency) and, if the predecessor is insolvent, the insolvency administrator.

With this data, I cannot separate quits from layoffs because firms in the post periods are new entities for Social Security and therefore stayers are considered new hires, whereas leavers are simply non-hired workers. However, there are good reasons to think that non-hires in WBO firms are due to voluntary quits rather than layoffs. As mentioned in the previous paragraph, employees of predecessor firms may not want to participate in the buyout. Additionally, WBO firms may be limited in their ability to exclude employees from the buyout because one of the stated aims of WBO is to protect jobs, so any coalition of worker-partners will face pressures from the government agency to include as many previous employees as possible.[34]

Job quality and workforce composition. In Columns (8) and (9) of Table 3, I show estimates of effects on the proportion of temporary employees, i.e. those on a fixed-term contract. In a dual labor market like Italy's, the proportion of temporary employees is a good proxy of average job quality in a firm because they do not guarantee a steady source of future income. I do not find any significant changes in this measure of average job quality,

[34]Checking whether leavers go to some other job or unemployment could give a hint on whether the separations are voluntary or not. However, this is not a perfect solution because searching for a job while receiving unemployment benefits is a valid strategy.

and if anything WBO firms seem to use fixed-term contracts more than comparison firms after the transition. I also do not find strong effects on the gender composition of the workforce. In the short run, WBO firms employ fewer women, but this effect dissipates in the long run.

Hierarchy. I find that firms become less hierarchical once they are labor managed. This is consistent with the idea that managerial functions in labor-managed firms are shared among co-workers. I proxy hierarchy using two inter-related measures: the number of layers in the firm and the probability of having no managerial layer. A firm has one layer if it employs either only blue-collar workers or only white-collar workers, it has two layers if employs both blue- and white-collar workers but no managers, and three layers if it employs workers in all three categories. I define managers as both middle-managers (*quadri*) and executives (*dirigenti*). Figure 6 shows results of event studies on the number of layers in the firm and the probability of having no managerial layer. Columns (10) and (11) in Table 3 present aggregated coefficients. Over the post-transition period, labor management reduces the number of layers by 0.178. Compared to the pre-treatment control mean of 2.405, this represents a 7.4% reduction. The probability of having no managerial layer at all increases by 15.48 percentage points (a 30.52% increase). The negative coefficient β_{-1} on the number of layers suggests that managers are exiting the firm in anticipation of the buyout, and that they are not being replaced by new hires.

4.4 Productivity, investment and profits

In order to understand effects on productivity and other measures of firm performance, I use income statement and balance sheet data from Cerved-Orbis (available in the Social Security databases) for the period 1996-2021. This sample is composed of firms active in the 1996-2021 period for which financial information is available. It is quite representative of the main sample. Table 2 shows summary statistics for firms in the 1996-2021 period, and Appendix Table A2 shows the main results on wages and employment reproduced for this subsample. An important limitation of this data is that most firms do not report income statements and balance sheets in the year before the transition. Panel (b) of Figure 3 shows sample attrition for the Cerved-Orbis sample. Both WBO firms and comparison firms have a high proportion of missing observations. For WBO firms, only 41% (14 firms out of 34) report data in $k = -1$, while for comparison firms this proportion is 53% (107/190). To highlight this limitation, I plot coefficients in period $k = -1$ in dashed lines. Data is consistently available for all other periods.

I measure firm productivity using the natural logarithm of value added per worker, following the literature in labor economics. Value added is computed as sales minus cost of goods and services. I then divide value added by the number of full-time equivalent workers and take its natural logarithm. Overall, I find that worker cooperatives are as productive as conventional firms. Panel (a) in Figure 7 shows event study estimates for this measure of productivity. Column (1) of Table 6 reports aggregated coefficients.[35] Excluding the year preceding the transition (for which data for only 14 WBO firms is available), all pre-transition coefficients are close to zero, which is evidence that the common trends assumption holds. Post-transition coefficients are also close to zero, if anything they are positive. When aggregated over the post-transition period, the effect on value added per worker is positive (14%) but not significant. This represents a large difference compared to the coefficient (-29.6%) estimated through a 'naive' regression of log value added per worker on a worker cooperative indicator variable and both sector-year and province-year fixed effects, estimated on the full sample of firm-year observation in Cerved-Social Security registry.[36] These results are consistent with previous literature, which found either no effect of being a worker cooperative on productivity, or negative effect for the production of some type of goods, but positive effects for others (Fakhfakh, Pérotin and Gago, 2012; Montero, 2022).

Log revenues per worker are lower (-12%) in worker cooperatives, but estimates are not significant. Together with the results on value added per worker, these findings suggest that WBO firms produce less but at lower costs. This could come from lower expenditure on materials or lower expenditure on external services. Worker cooperatives may shift some production and/or professional services in-house. Again excluding coefficients for $k = -1$, pre-transition coefficients are close to zero.

Contrary to predictions from prominent theory papers (Alchian and Demsetz, 1972), I do not find strong evidence that worker cooperatives underinvest. Per capita assets are lower in worker cooperatives (-20.5%), but estimates are very imprecisely estimated.

I find null effects on profitability. The effect on EBITDA per capita is negative (and not significant), while the effect on net profits per capita is positive (also insignificant). I also estimate effects on indebtedness, measured as debts over EBITDA. The estimated coefficient is positive, indicating more indebtedness, but very noisily estimated.

[35] As mentioned earlier, I plotted $k = -1$ coefficients in dashed lines to highlight the lack of data for this period.

[36] This sample contains 5,567,969 firm-year observations.

4.5 Robustness checks

A potential concern is that the results presented in the sections above are driven by the particular choice of matching parameters. To alleviate this concern, I run two sets of robustness checks. First, I perturb the matching specification by changing target variables and the number of matched units. Second, I build select the comparison group using synthetic difference-in-differences (Arkhangelsky et al., 2021). In both cases, results are very similar to the main specification.

Alternative matching specifications. Matching requires an arbitrary selection of parameters. To show that this is not driving the main results, I show estimates under three different matching specifications. As a reminder, the main matching specification (M1, for short) selects m_1 units from the donor pool via coarsened exact matching with the following variables: quintile of average wage, quintile of number of employees, quintile of hiring rate, and a manufacturing dummy. The number of matches m_1 is unconstrained. The alternative matching specifications are as follows:

M2: quintile of average wage, quintile of number of employees, quintile of hiring rate, and a manufacturing dummy. This specification is the same as M1 but it restricts the number of matches to $m_2 = 5$, using a propensity score to select comparison firms whenever more than 5 units are eligible controls for the same treated firm.[37]

M3: quintile of average wage, quintile number of employees, and a manufacturing dummy. The number of matches is limited to $m_3 = 5$ per treated firm. This specification is the same as M2 without the hiring rate in $k = -3$. Similarly, I use a propensity score to break ties.

M4: quintile of average wage, quintile of number of employees, quintile of hiring rate, and a manufacturing dummy. This specification is the same as M1 but it restricts the number of matches to $m_4 = 1$. I use a propensity score estimated with the model detailed above to break ties.

Overall, results are very similar when using different matching specifications. The significance and sign of the main estimates does not change. Figures B1 through B13 reproduce the

[37] I compute the propensity score by estimating a linear probability model for each cohort c that includes a degree-2 polynomial of the log of average wage, average worker age and gender proportion; linearly the standard deviation of age, the Herfindahl–Hirschman index (HHI) of the birth municipality, the proportions of foreign workers, managers, hiring rate; indicators for number of employees, firm age in years and two-digit sector.

main figures for event study estimates on wages, employment, hierarchy and productivity.

Synthetic differences-in-differences. Another potential concern is that results may be driven by the use of matching itself. There are reasonable alternatives to matching, like synthetic control methods. To show that my results do not depend on using matching, I estimate effects using synthetic differences-in-differences (SDID) (Arkhangelsky et al., 2021). SDID combines synthetic control methods to differences-in-differences, by selecting a set of weights for units in the donor pool to create a synthetic control unit that fits outcomes trends for treated units. The econometric literature on synthetic control methods, recommends limiting the size of the donor pool to avoid overfitting (Abadie and Vives-i Bastida, 2022). Hence, rather than using the full donor pool (17,846 firms) I define the SDID donor pool to be the matched sample using M3. This is the least demanding matching specification and hence yields the largest matched sample, with 125 treated firms and 498 comparison firms. This is helpful because SDID imposes a balanced-panel restriction on the estimation sample. I estimate SDID event study coefficients on the balanced subset of firms (94 WBO and 270 control) observed continuously from $k = -4$ to $k = 4$. I compute standard errors via 100 bootstrap replications of the procedure, where unit of sampling is the set of panel observations for each firm. It should also be noted that SDID selects a different set of weights for each outcome variable. Hence, the control group is effectively different in each estimation. Figures B13 through B15 show the results for wages, employment and hierarchy using SDID. Results are qualitatively and quantitatively very similar to the ones obtained using matching.

5 Conclusion

Understanding the implications of labor-management is important because labor-managed firms are present all over the world, and other forms of worker representation and cooperative property rights are widespread. Despite a rich theoretical literature (Alchian and Demsetz, 1972; Holmström, 1982; Kandel and Lazear, 1992), the empirical evidence is scant.

This paper studies the effect of labor management on a series of firm-level outcomes. I find that average wages decrease by about 9% and that this decrease is stable over time, but that the effect on total compensation is positive. Wage cuts are larger at the top of the distribution and smaller at the median and lower percentiles. This implies a reduction in within-firm inequality. I find that changes in wage policies are important to explain my results, and that therefore the reduction in within-firm inequality can potentially translate into a reduction in overall inequality. These results are consistent with theories about worker cooperatives and with previous empirical results. Moreover, I show that there is a strong disemployment effect in the short run, but not in the long run. I do not find evidence that worker cooperatives are less productive or profitable than conventional firms, nor that they invest less.

In sum, transitions to labor management seem to increase equity without harming efficiency. Policy makers interested in reducing wage inequality could consider promoting more transitions to labor-management, or formation of new labor-managed firms from scratch. However, it is not clear whether the current level of LMF formation is inefficient. Future research could use test predictions of theoretical papers to empirically establish if there are market failures that are preventing more conversion to labor management, or the formation of LMFs from scratch. Also, given the prominence of LMFs in some local labor markets, like the Basque Country in Spain or the Emilia-Romagna region in Italy, there is an interesting avenue for future research to establish the macro implications of labor management on monopsony power and the labor share.

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Main Tables

Table 1: Pre-transition summary statistics: 1980-2021

	Comparison firms	WBO firms	P-value Diff.
Panel A: Wages, employment and worker characteristics			
Log wage, avg.	6.288	6.157	0.000***
Employment, F.T.E.	96.979	78.161	0.156
Log wage, p10	5.943	5.861	0.002***
Log wage, p50	6.191	6.088	0.000***
Log wage, p90	6.586	6.403	0.000***
Hiring rate	0.101	0.177	0.049**
Separation rate	0.112	0.105	0.583
Age, avg.	39.851	39.445	0.467
Woman	0.303	0.229	0.007***
Manager	0.025	0.014	0.010**
N. Layers	2.405	2.344	0.359
Zero manager	0.512	0.602	0.122
Tenure, avg.	3.748	3.676	0.810
Fixed-term	0.056	0.040	0.297
Part-time	0.054	0.040	0.173
Panel B: Trust and preference homogeneity proxies			
HHI municipality	0.192	0.238	0.009***
Age, s.d.	9.435	9.675	0.306
Foreign share	0.068	0.049	0.108
Panel C: Firm characteristics and outcomes			
Firm age	17.807	16.355	0.327
Manufacturing	0.613	0.527	0.140
N, firms	326	93	

Note: Summary statistics for the estimation sample computed using observations at $k = -3$. P-values are for t-test of the difference in means, with standard errors clustered at the firm level.

Table 2: Pre-transition summary statistics: 1996-2021 Cerved-Orbis sample.

	Comparison firms	WBO firms	P-value Diff.
Panel A: Wages, employment and worker characteristics			
Log wage, avg.	6.359	6.240	0.003***
Employment, F.T.E.	73.668	48.912	0.021**
Log wage, p10	5.987	5.919	0.049**
Log wage, p50	6.253	6.172	0.030**
Log wage, p90	6.695	6.520	0.002***
Hiring rate	0.091	0.236	0.070*
Separation rate	0.112	0.129	0.511
Age, avg.	41.414	41.284	0.843
Woman	0.286	0.259	0.437
Manager	0.026	0.021	0.480
N. Layers	2.374	2.324	0.614
Zero manager	0.553	0.647	0.295
Tenure, avg.	4.282	3.962	0.493
Fixed-term	0.087	0.090	0.930
Part-time	0.077	0.090	0.508
Panel B: Trust and preference homogeneity proxies			
HHI municipality	0.198	0.248	0.041**
Age, s.d.	9.072	8.920	0.611
Foreign share	0.105	0.089	0.448
Panel C: Firm characteristics and outcomes			
Firm age	20.511	17.324	0.219
Manufacturing	0.789	0.735	0.507
Log VA per worker	10.746	10.602	0.257
Log sales per worker	12.077	12.048	0.829
Log assets per worker	10.900	10.980	0.736
EBITDA per worker	11348.516	11737.919	0.928
Profit per worker	−642.510	−2333.727	0.446
Indebtedness	2.686	1.274	0.633
N, firms	190	34	

Note: Summary statistics for the estimation sample computed using observations at $k = -3$. P-values are for t-test of the difference in means, with standard errors clustered at the firm level.

Table 3: Wages, employment and hierarchy.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Log wages				Employment					Hierarchy	
	Mean	Median	P10	P90	LogEmp	HiRate	SepRate	TempPr	Women	NoManagers	Layers
β_{SR}	-0.099^{***}	-0.070^{***}	-0.031	-0.155^{***}	-0.274^{***}	0.114^{***}	0.045^{**}	0.017	-0.024^{**}	0.151^{***}	-0.203^{***}
	(0.018)	(0.017)	(0.025)	(0.027)	(0.084)	(0.041)	(0.023)	(0.014)	(0.012)	(0.044)	(0.055)
β_{LR}	-0.069^{***}	-0.034^{*}	-0.043	-0.109^{***}	0.004	-0.035	-0.042^{*}	0.024	-0.005	0.161^{***}	-0.143^{**}
	(0.019)	(0.018)	(0.027)	(0.028)	(0.089)	(0.043)	(0.024)	(0.015)	(0.012)	(0.046)	(0.058)
β_{Post}	-0.086^{***}	-0.055^{***}	-0.036	-0.136^{***}	-0.159^{**}	0.053	0.010	0.020	-0.016	0.155^{***}	-0.178^{***}
	(0.017)	(0.017)	(0.024)	(0.025)	(0.080)	(0.039)	(0.022)	(0.014)	(0.011)	(0.041)	(0.052)
Mean DV	6.288	6.191	5.943	6.586	4.574	0.101	0.112	0.056	0.303	0.512	2.405
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.870	0.844	0.690	0.849	0.810	0.433	0.524	0.845	0.933	0.741	0.741
N	5797	5797	5797	5797	5797	5377	5377	5797	5797	5797	5797

Note: Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$), long run β_{LR} ($k = 3, 4, 5$) and over the whole post-transition period β_{Post} ($k = 0, ..., 5$). Standard errors are clustered at the firm level. The mean of the dependent variable (Mean DV) is for the comparison group three years before the transition ($k = -3$). Note that each column represents two separate regressions: one estimates β_{SR}, β_{LR}, and the other estimates β_{Post}. Within each column, the sample size N is exactly the same. For compactness, the table reports R^2 for the first regression only, but differences are very small.

Table 4: Wages and profit-adjusted wages: 1996-2021.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log Wages				Log Total Compensation			
	Mean	Median	P10	P90	Mean	Median	P10	P90
β_{SR}	-0.047^{**}	-0.039^{*}	-0.036	-0.078^{**}	0.033	0.041^{*}	0.044	0.003
	(0.021)	(0.021)	(0.033)	(0.034)	(0.023)	(0.022)	(0.034)	(0.035)
β_{LR}	-0.060^{***}	-0.059^{***}	-0.091^{***}	-0.080^{**}	0.049^{**}	0.051^{**}	0.019	0.030
	(0.023)	(0.022)	(0.035)	(0.037)	(0.024)	(0.024)	(0.036)	(0.038)
β_{Post}	-0.052^{**}	-0.047^{**}	-0.057^{*}	-0.079^{**}	0.039^{*}	0.045^{**}	0.035	0.013
	(0.021)	(0.020)	(0.031)	(0.033)	(0.022)	(0.021)	(0.032)	(0.034)
Mean DV	6.359	6.253	5.987	6.695	6.359	6.253	5.987	6.695
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.892	0.870	0.676	0.858	0.880	0.857	0.666	0.851
N	3108	3108	3108	3108	3108	3108	3108	3108

Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$), long run β_{LR} ($k = 3, 4, 5$) and over the whole post-transition period β_{Post} ($k = 0, ..., 5$). Standard errors are clustered at the firm level. The mean of the dependent variable (Mean DV) is for the comparison group three years before the transition ($k = -3$). Total compensation for each employee in a post-WBO firm is computed as weekly wage plus a wage-weighted share of profit if they are positive, or zero otherwise. Total compensation is capped at 1.3 of weekly wage. This follows legal requirements and standard practice in worker cooperatives. Estimation is done over the subset of firms active in the 1996-2021 period, for which financial information is available from the Cerved-Orbis data. Note that each column represents two separate regressions: one estimates β_{SR}, β_{LR}, and the other estimates β_{Post}. Within each column, the sample size N is exactly the same. For compactness, the table reports R^2 for the first regression only, but differences are very small.

Table 5: Wages and stayers' wages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Workers				Stayers			
	Mean	Median	P10	P90	Mean	Median	P10	P90
β_{SR}	-0.099^{***}	-0.070^{***}	-0.031	-0.155^{***}	-0.096^{***}	-0.095^{***}	-0.047	-0.159^{***}
	(0.018)	(0.017)	(0.025)	(0.027)	(0.022)	(0.022)	(0.030)	(0.023)
Mean DV	6.288	6.191	5.943	6.586	.	.	.	.
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.870	0.844	0.690	0.849	0.870	0.863	0.723	0.926
N	5797	5797	5797	5797	1998	1998	1998	1998

Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$). Standard errors are clustered at the firm level. The mean of the dependent variable (Mean DV) is for the comparison group three years before the transition ($k = -3$). Stayers are defined as workers employed continuously by a firm in the sample from three years before the transition to two years afterwards. Outcomes under the header are computed by taking a firm-level average among stayers.

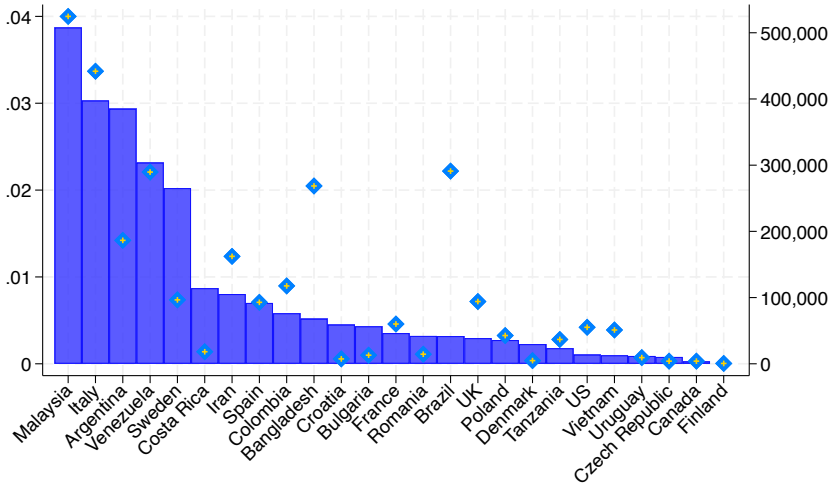
Table 6: Income statement and balance sheet outcomes: 1996-2021.

	(1)	(2)	(3)	(4)	(5)	(6)
	Per Employee					
	LogVA	LogSales	LogAssets	EBITDA	Profit	Indebtd.
β_{SR}	0.150	−0.111	−0.251	−3093.118	941.224	0.641
	(0.104)	(0.095)	(0.212)	(5583.242)	(3502.524)	(7.033)
β_{LR}	0.098	−0.156	−0.196	2.984	5625.956	4.090
	(0.111)	(0.101)	(0.226)	(5958.661)	(3738.035)	(7.523)
β_{Post}	0.131	−0.128	−0.230	−1917.799	2719.606	1.947
	(0.100)	(0.091)	(0.203)	(5345.410)	(3355.203)	(6.726)
Mean DV	10.746	12.077	10.900	1.1e+04	-642.510	2.686
Firm FE	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓
R^2	0.580	0.779	0.711	0.481	0.491	0.229
N	3108	3108	3096	3108	3108	3073

Note: Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$), long run β_{LR} ($k = 3, 4, 5$) and over the whole post-transition period β_{Post} ($k = 0, ..., 5$). Standard errors are clustered at the firm level. The mean of the dependent variable (Mean DV) is for the comparison group three years before the transition ($k = -3$). Estimation is done over the subset of firms active in the 1996-2021 period, for which financial information is available from the Cerved-Orbis data. Note that each column represents two separate regressions: one estimates β_{SR}, β_{LR}, and the other estimates β_{Post}. Within each column, the sample size N is exactly the same. For compactness, the table reports R^2 for the first regression only, but differences are very small.

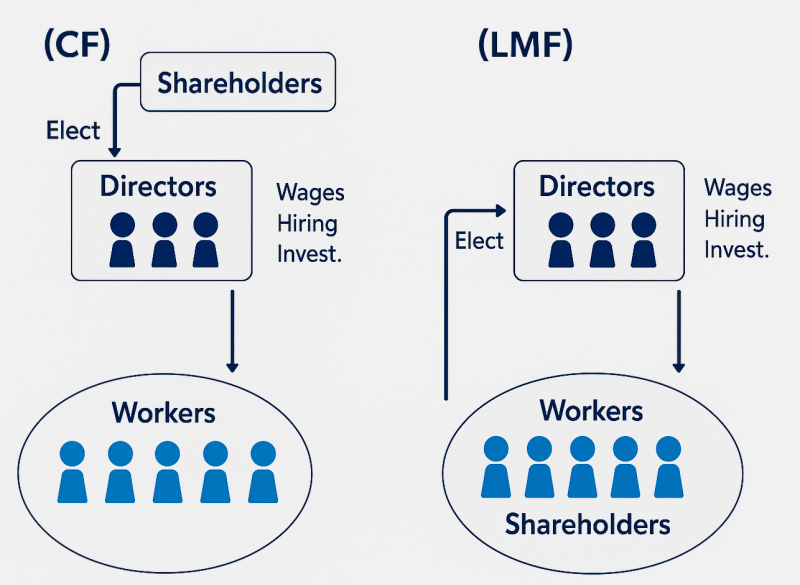
Main Figures

Figure 1: Prevalence of worker cooperatives in selected countries.



Note: The bars show the percentage of total private sector employment taken up by worker cooperatives (left axis). The diamonds show the absolute number of workers. Return to Section 2.

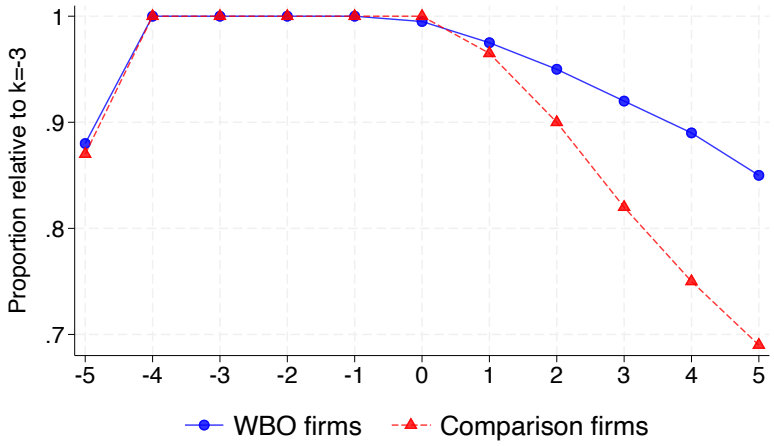
Figure 2: Corporate governance in conventional firms and labor-managed firms.



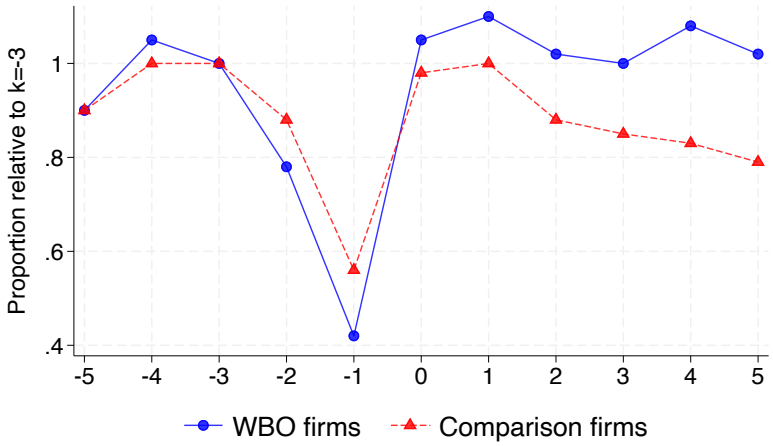
Note: conventional firms distribute voting rights to partners according to capital ownership, whereas labor-managed firms distribute voting rights to partners according to the 'one-head, one-vote' principle. Moreover, at worker-partners must have at least two-thirds of votes and be paid at least 50% of the wage bill. Return to Section 2.

Figure 3: Survival proportions.

(a) 1980-2021 INPS panel



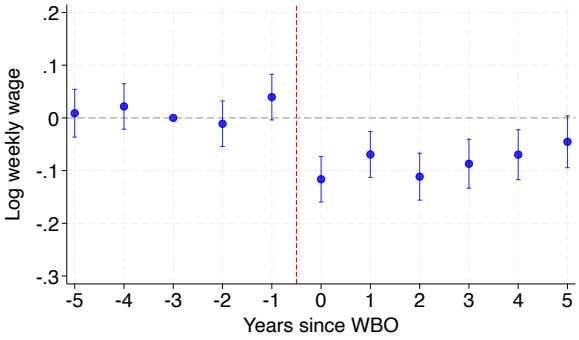
(b) 1996-2021 INPS-Cerved-Orbis panel



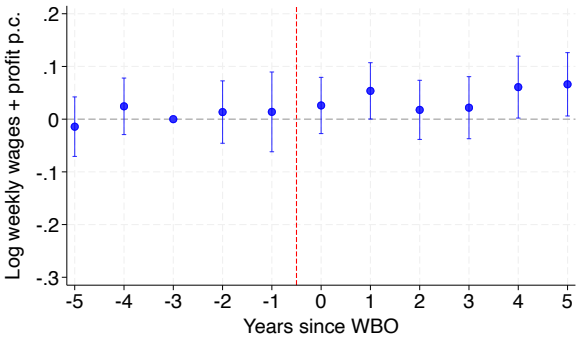
Note: WBO firms (blue solid line) vs. comparison firms (red dotted line). The survival rate is equivalent to the discrete time Kaplan-Meier estimate. For each elapsed time k, the numerator is the number of firms active at period k, and the denominator is the standard risk set, that is the number of firms that were active at the baseline period $k = -3$ and are have not been right censored. For example, the 2018 cohort (for which $k = 0$ in 2018), cannot be in the sample at $k = 4$. Notice that survival rates are 1 in the window $k \in [-4, 0]$ because the panel is balanced in these elapsed time periods. For the Cerved-Orbis sample in Panel (b), survival rates lower than 1 in pre-transition periods are due to firms not reporting income statements. Return to Section 4.1.

Figure 4: Effects on Wages

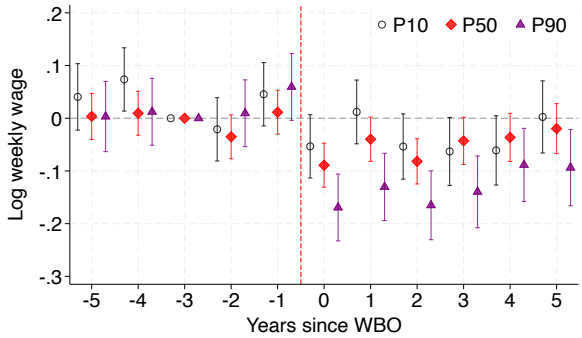
(a) Log of average wage



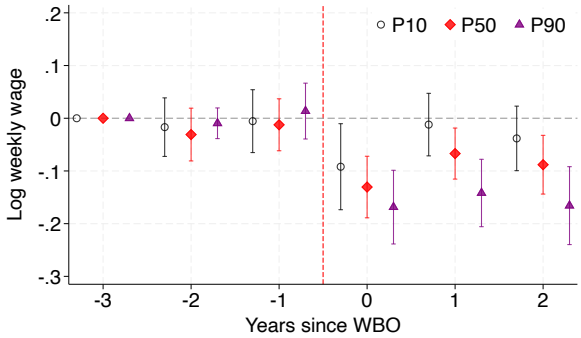
(b) 1996-2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



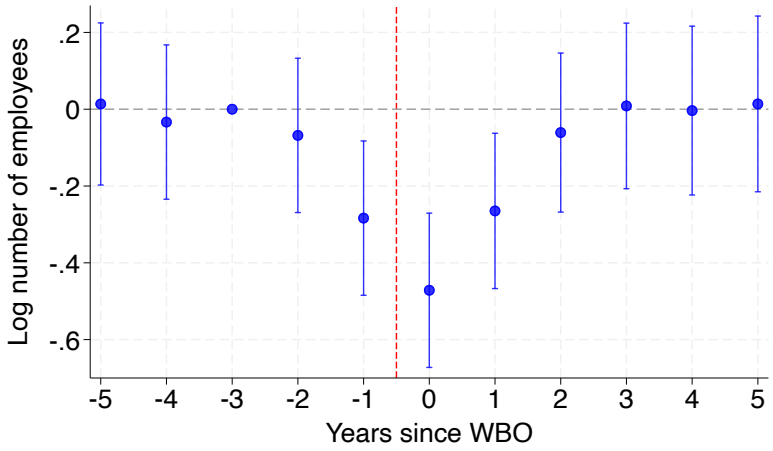
(d) Log of wage percentiles for stayers



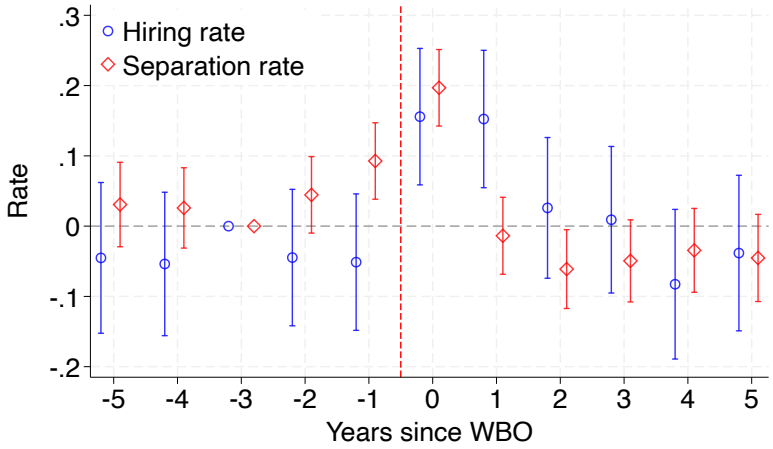
Note: Panel (a) presents the point estimates of the event study specification on the log of the average wage, with 95% confidence intervals. Panel (b) presents the point estimates for total compensation estimated using the subsample of firms in the period 1996-2021 for which financial statements are available. Total compensation is computed as weekly wages plus a per-capita share of profits proportional to the base wage. This adjustment follows rules for dividend distribution in worker cooperatives. For more details on profit adjustment, see Section 3. Panel (c) presents results on the log of the within-firm 10th, 50th and 90th percentiles, indicated by black hollow dots, red diamonds and purple triangles, respectively. Panel (d) presents results on wage percentiles for stayers only. Stayers are defined as workers employed continuously by a firm in the sample from three years before the transition to two years afterwards. Panels (e) and (f) present results on a restricted sample in the period 1996-2021 for which Cerved-Orbis data on firms' income statements are available. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. Back to Section 4.2.

Figure 5: Effects on Employment

(a) Log number of full-time employees



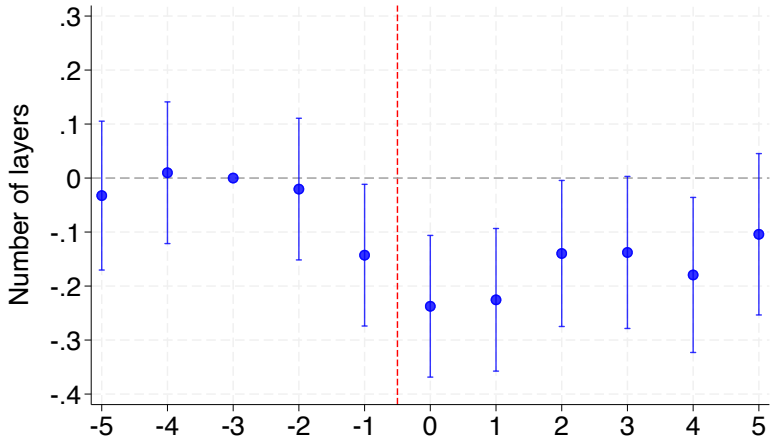
(b) Hiring and separation rates



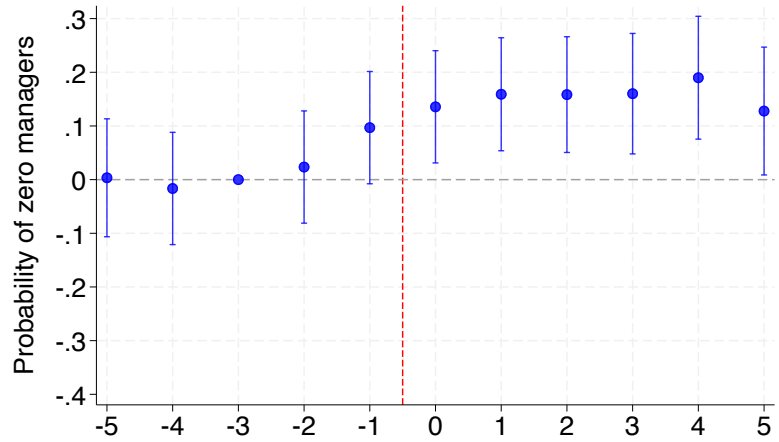
Note: Panel (a) presents the point estimates of the event study specification on the log of full-time equivalent employees, with 95% confidence intervals. Panel (b) presents results on the hiring and separation rates, indicated by blue holllow circles and red hollow diamonds, respectively. The hiring rate is calculated as the number of new hires over the total number of employees in a given year. The separation rate in year t is calculated as the number of employees that have left the firm by year $t+1$ over the total number of employees in year t. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. Back to Section 4.2.

Figure 6: Effects on Firm Hierarchy

(a) Number of layers

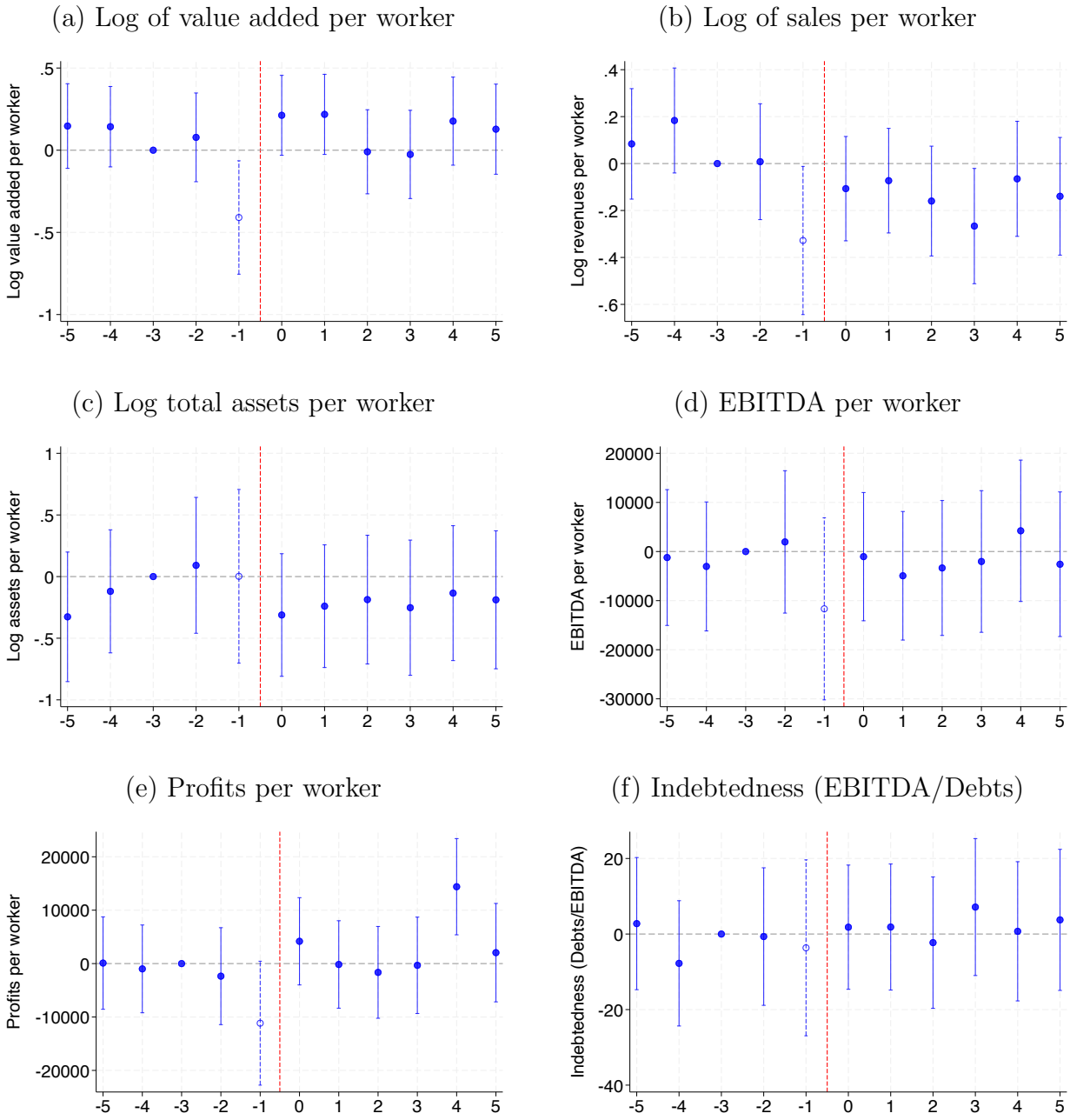


(b) Probability of no managerial layer



Note: Panel (a) presents the point estimates of the event study specification on the number of layers in the firm, with 95% confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well. Panel (b) presents the point estimates for the probability of having no managerial layer. Back to Section 4.2.

Figure 7: (1996-2021) Effects on Income Statement and Balance Sheet Outcomes



Note: Panel (a) presents the point estimates of the event study specification on the log of value added per worker, with 95% confidence intervals. Panel (b) presents the point estimates for the log of sales (revenues) per worker, Panel (c) presents results for the log assets (tangible and intangible) per worker. Panel (d) presents results on Earnings Before Interest, Taxes, Depreciation and Amortization (EBITDA) per worker and Panel (e) presents results for net profits per worker. Panel (f) presents results on a measure of indebtedness constructed as the ratio of debts over EBITDA. Coefficients are estimated using a subsample over the period 1996-2021 for which Cerved-Orbis data on firms' income statements are available. Estimates for period $k = -1$ are in light-shaded, dotted lines because financial information is available for only 54% of the sample (14 treated firms and 90 comparison firms). The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. Back to Section 4.4.

A Appendix Tables and Figures

Table A1: Pre-transition summary statistics: 1980-2021

	Comparison firms	WBO firms	P-value Diff.
Panel A: Wages, employment and worker characteristics			
Log wage, avg.	6.077	6.109	0.144
Employment, F.T.E.	41.647	71.286	0.000***
Log wage, p10	5.760	5.811	0.037**
Log wage, p50	6.034	6.053	0.327
Log wage, p90	6.325	6.346	0.446
Hiring rate	0.236	0.181	0.107
Separation rate	0.140	0.122	0.173
Age, avg.	37.917	38.579	0.129
Woman	0.370	0.256	0.000***
Manager	0.012	0.011	0.600
Tenure, avg.	3.077	3.489	0.073*
Fixed-term	0.104	0.039	0.000***
Part-time	0.126	0.031	0.000***
Panel B: Trust and preference homogeneity proxies			
HHI municipality	0.307	0.271	0.017**
Age, s.d.	9.183	9.478	0.172
Foreign share	0.110	0.040	0.000***
Panel C: Firm characteristics and outcomes			
Firm age	14.808	15.471	0.554
Manufacturing	0.417	0.529	0.008***
N, firms	17847	140	

Note: Summary statistics for the pre-matching sample computed using observations at $k = -3$. P-values are for t-test of the difference in means, with standard errors clustered at the firm level.

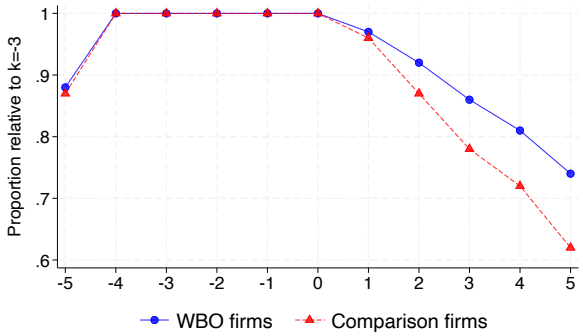
Table A2: 1996-2021 Cerved-Orbis sample: wages, employment and hierarchy.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Log wages				Employment					Hierarchy	
	Mean	Median	P10	P90	LogEmp	HiRate	SepRate	TempPr	Women	NoManagers	Layers
β_{SR}	-0.047^{**}	-0.039^{*}	-0.036	-0.078^{**}	-0.227^{**}	0.171^{***}	0.070^{**}	0.033	-0.004	0.145^{**}	-0.200^{**}
	(0.021)	(0.021)	(0.033)	(0.034)	(0.105)	(0.066)	(0.031)	(0.027)	(0.016)	(0.066)	(0.080)
β_{LR}	-0.060^{***}	-0.059^{***}	-0.091^{***}	-0.080^{**}	0.111	-0.053	-0.040	0.048^{*}	0.039^{**}	0.157^{**}	-0.152^{*}
	(0.023)	(0.022)	(0.035)	(0.037)	(0.112)	(0.071)	(0.033)	(0.029)	(0.017)	(0.070)	(0.085)
β_{Post}	-0.052^{**}	-0.047^{**}	-0.057^{*}	-0.079^{**}	-0.098	0.087	0.029	0.039	0.012	0.149^{**}	-0.182^{**}
	(0.021)	(0.020)	(0.031)	(0.033)	(0.101)	(0.064)	(0.030)	(0.026)	(0.015)	(0.063)	(0.077)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.892	0.870	0.676	0.858	0.851	0.420	0.529	0.822	0.936	0.749	0.742
N	3108	3108	3108	3108	3108	2892	2892	3108	3108	3108	3108

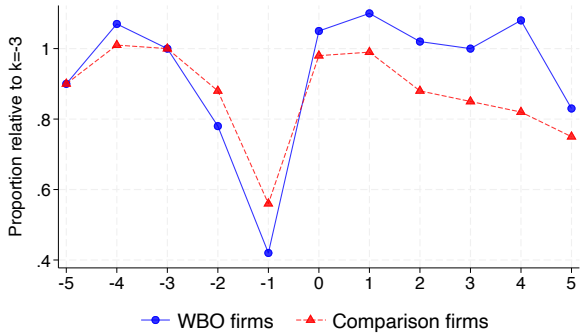
Note: Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$), long run β_{LR} ($k = 3, 4, 5$) and over the whole post-transition period β_{Post} ($k = 0, ..., 5$). Standard errors are clustered at the firm level. Estimation is done over the subset of firms active in the 1996-2021 period, for which financial information is available from the Cerved-Orbis data. Note that each column represents two separate regressions: one estimates β_{SR}, β_{LR}, and the other estimates β_{Post}. Within each column, the sample size N is exactly the same. For compactness, the table reports R^2 for the first regression only, but differences are very small.

Figure A1: Attrition rates.

(a) 1980-2021 INPS panel



(b) 1996-2021 INPS-Cerved-Orbis panel

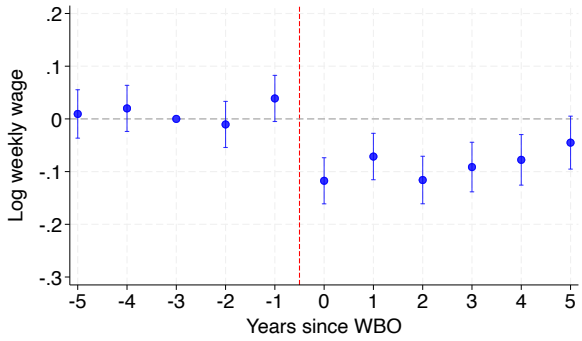


Note: WBO firms (blue solid line) vs. comparison firms (red dotted line). For each elapsed time k, the numerator is the number of firms active at period k, and the denominator is the number of firms that were active at the baseline period $k = -3$. For the Cerved-Orbis sample in Panel (b), survival rates lower than 1 in pre-transition periods are due to firms not reporting income statements. Return to Section 4.1.

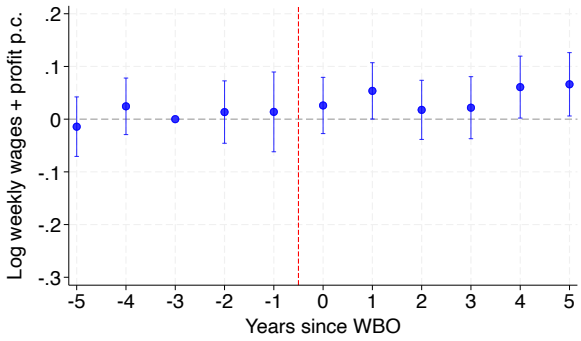
B Robustness

Figure B1: Effects on Wages. Matching specification 2

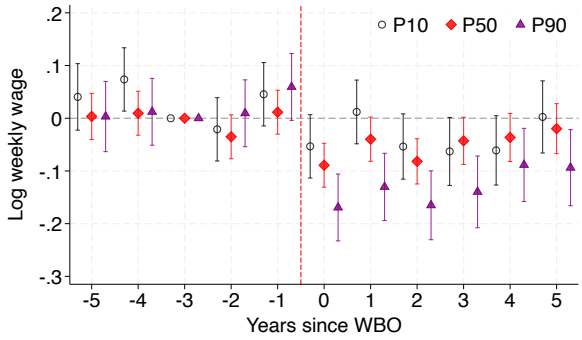
(a) Log of average wage



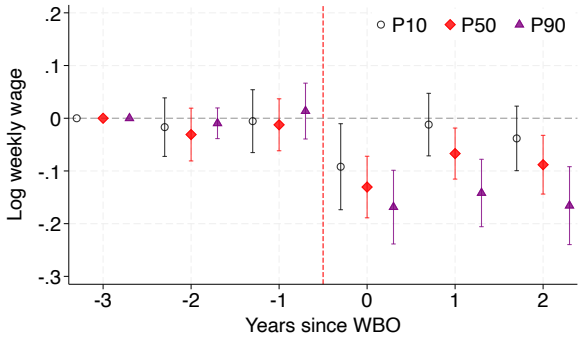
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



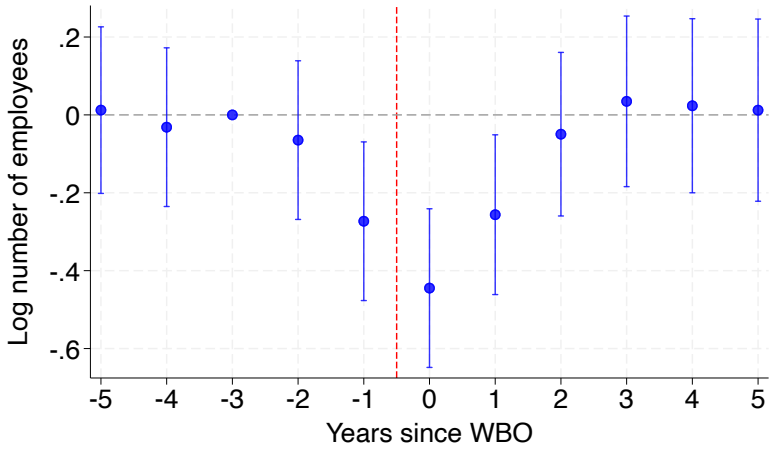
(d) Log of wage percentiles for stayers



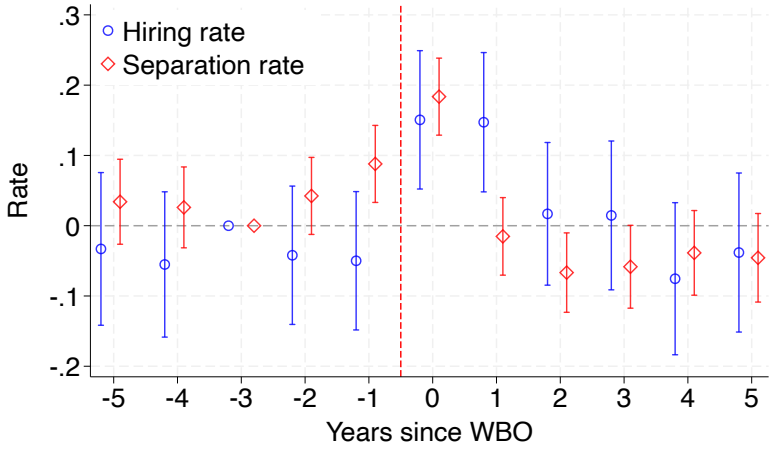
Note: Same as Figure 4, with matching specification 2.

Figure B2: Effects on Employment. Matching specification 2

(a) Log number of full-time employees



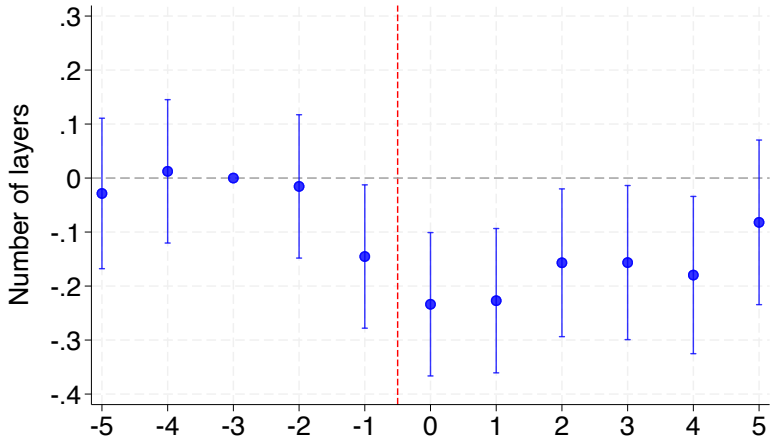
(b) Hiring and separation rates



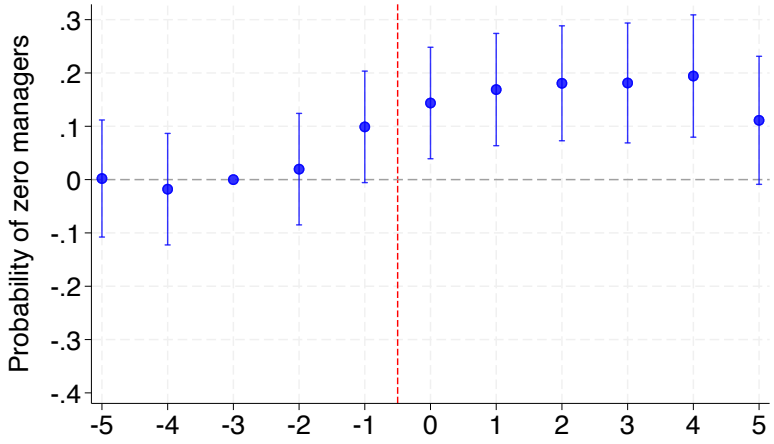
Note: Same as Figure 5, with matching specification 2.

Figure B3: Effects on Firm Hierarchy. Matching specification 2

(a) Number of layers

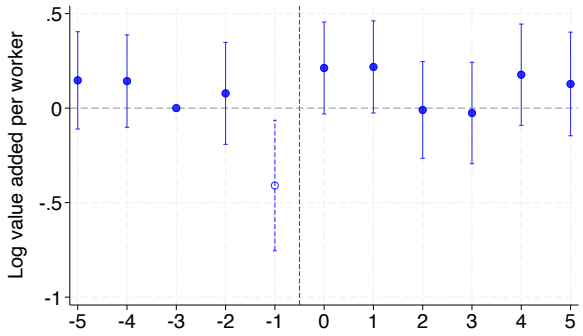


(b) Probability of no managerial layer

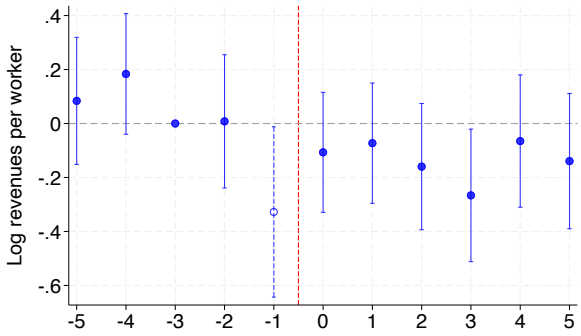


Note: Same as Figure 6, with matching specification 2.

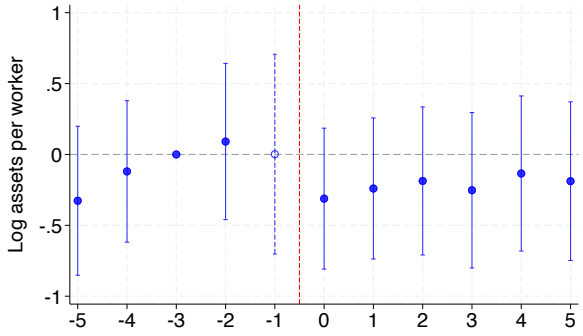
Figure B4: (1996-2021) Effects on Income Statement and Balance Sheet Outcomes. Matching specification 2.



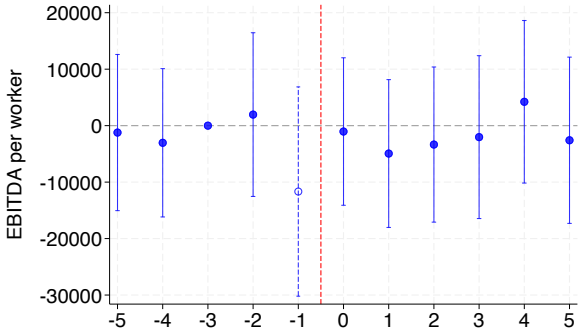
(a) Log of value added per worker



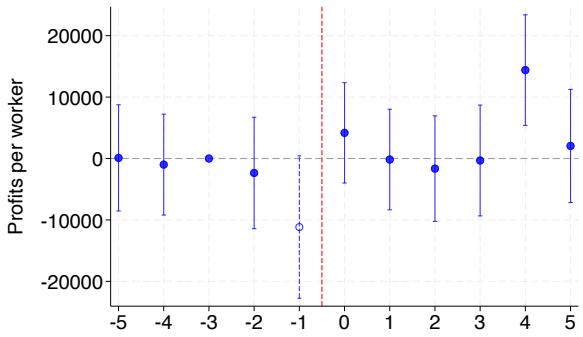
(b) Log of sales per worker



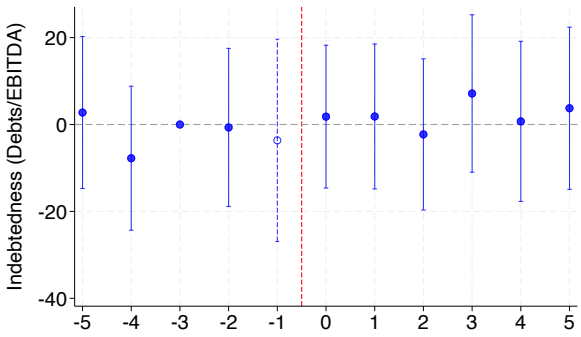
(c) Log total assets per worker



(d) EBITDA per worker



(e) Profits per worker

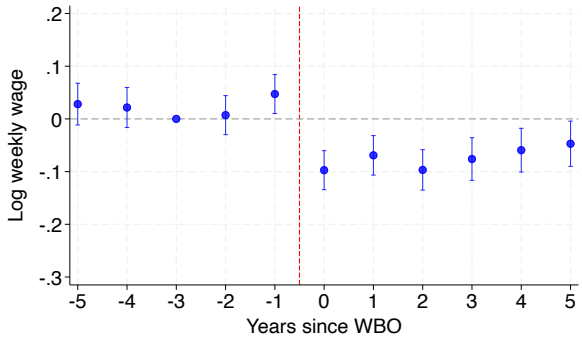


(f) Indebtedness (EBITDA/Debts)

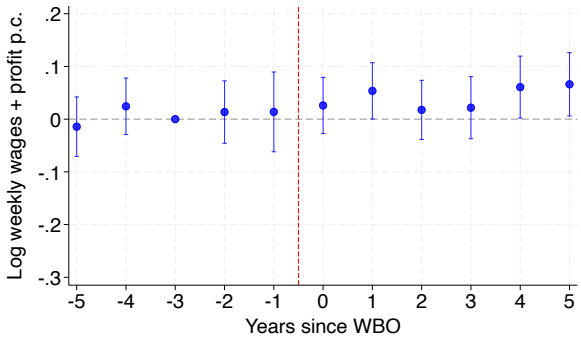
Note: Same as Figure 7, with matching specification 2

Figure B5: Effects on Wages. Matching specification 3

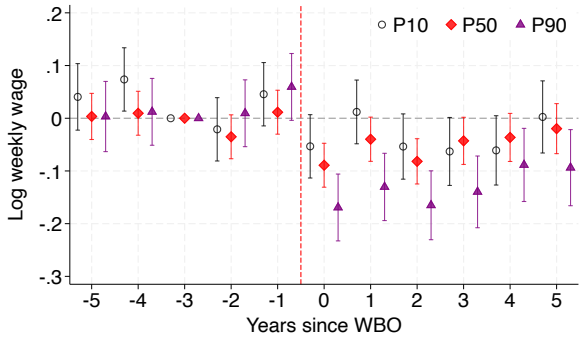
(a) Log of average wage



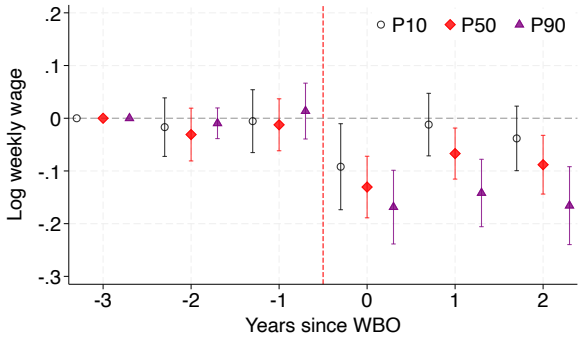
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



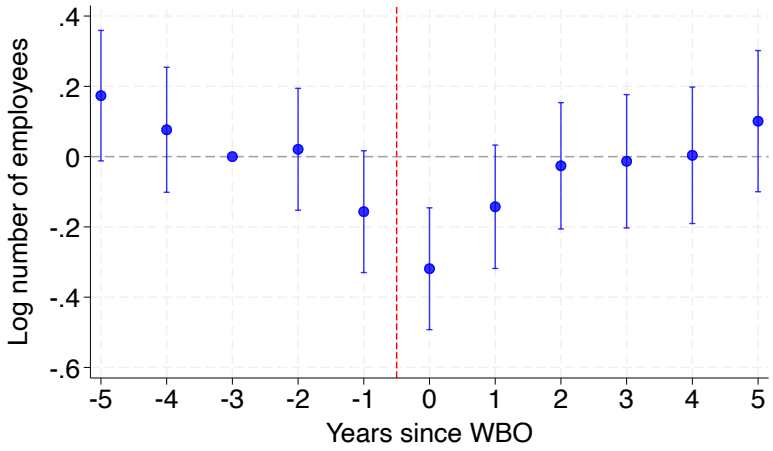
(d) Log of wage percentiles for stayers



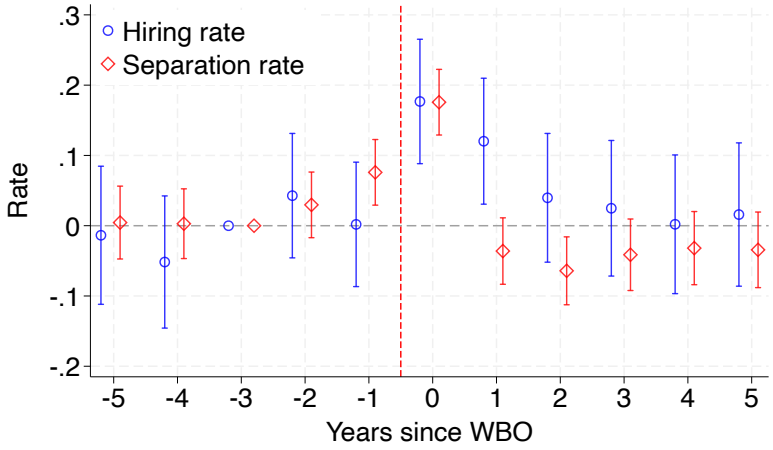
Note: Same as Figure 4, with matching specification 3.

Figure B6: Effects on Employment. Matching specification 3

(a) Log number of full-time employees



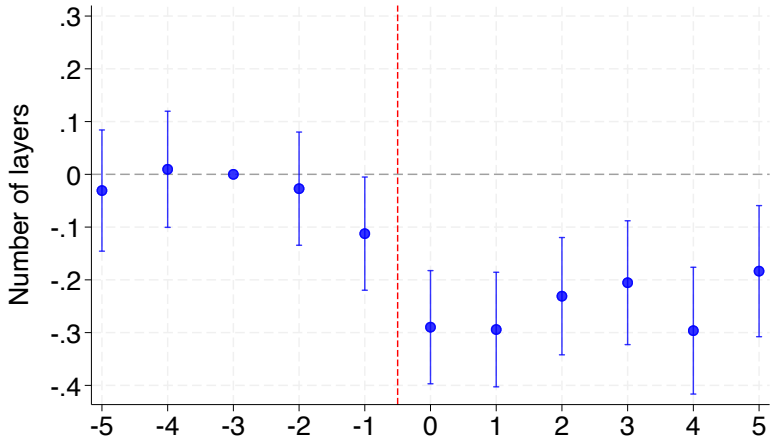
(b) Hiring and separation rates



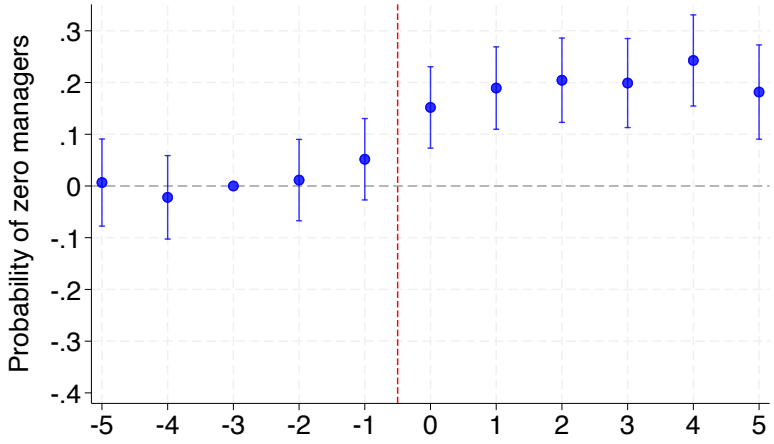
Note: Same as Figure 5, with matching specification 3.

Figure B7: Effects on Firm Hierarchy. Matching specification 3

(a) Number of layers



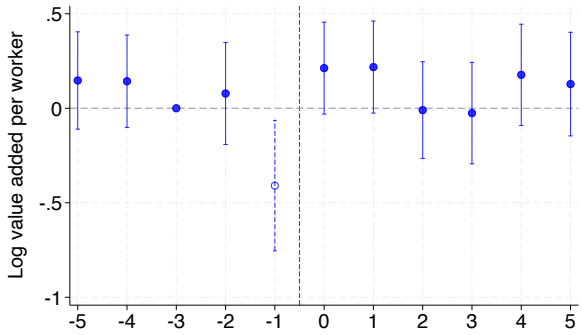
(b) Probability of no managerial layer



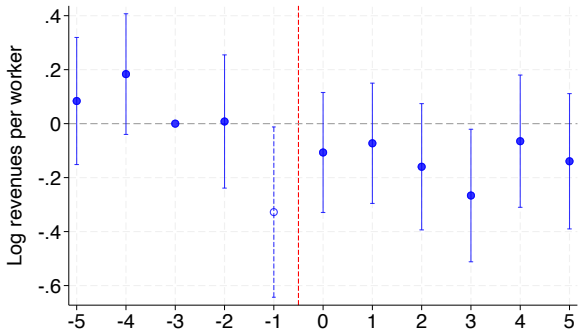
Note: Same as Figure 6, with matching specification 3.

Figure B8: (1996-2021) Effects on Income Statement and Balance Sheet Outcomes. Matching specification 3.

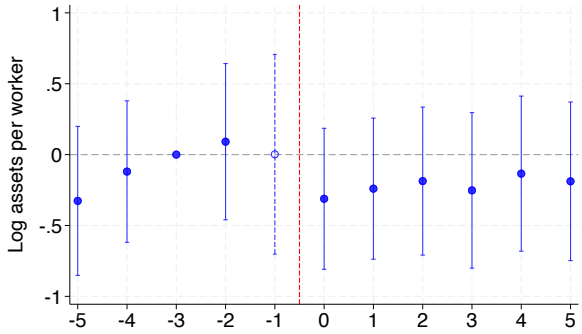
(a) Log of value added per worker



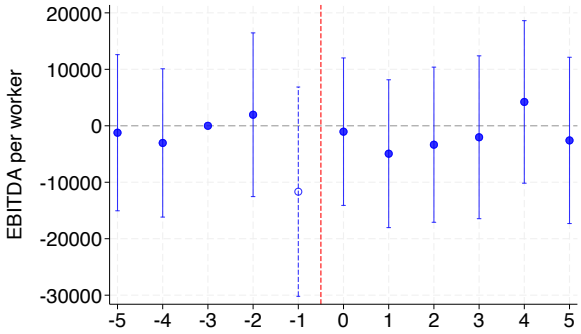
(b) Log of sales per worker



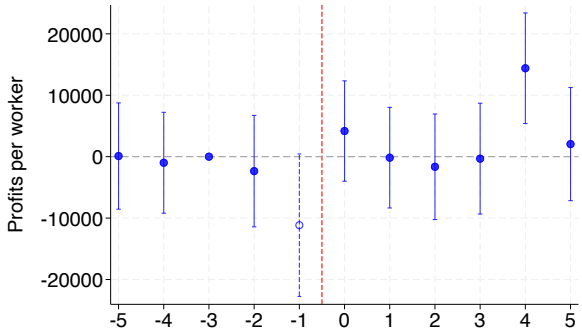
(c) Log total assets per worker



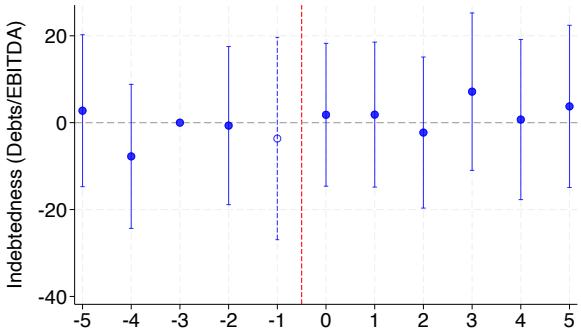
(d) EBITDA per worker



(e) Profits per worker



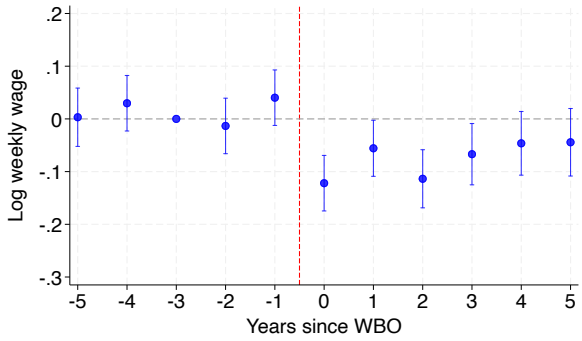
(f) Indebtedness (EBITDA/Debts)



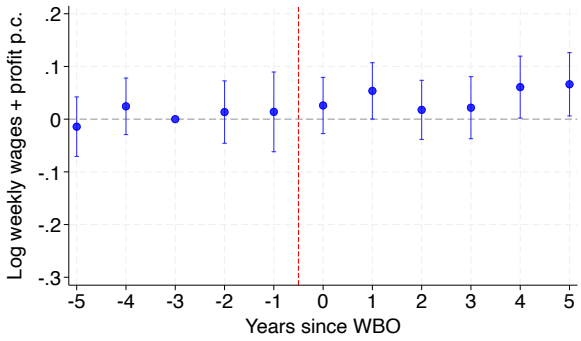
Note: Same as Figure 7, with matching specification 3.

Figure B9: Effects on Wages. Matching specification 4

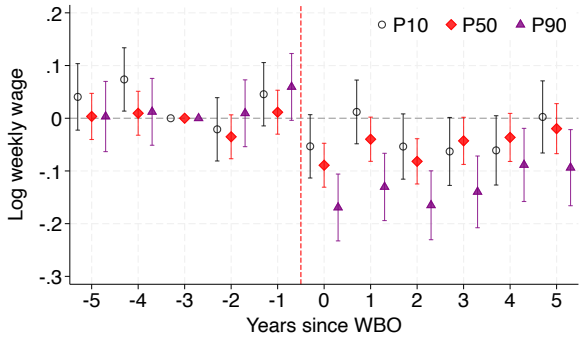
(a) Log of average wage



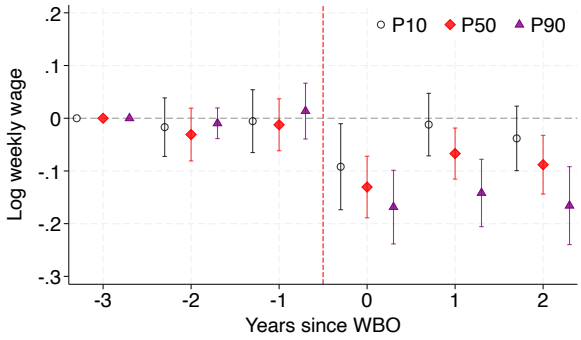
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



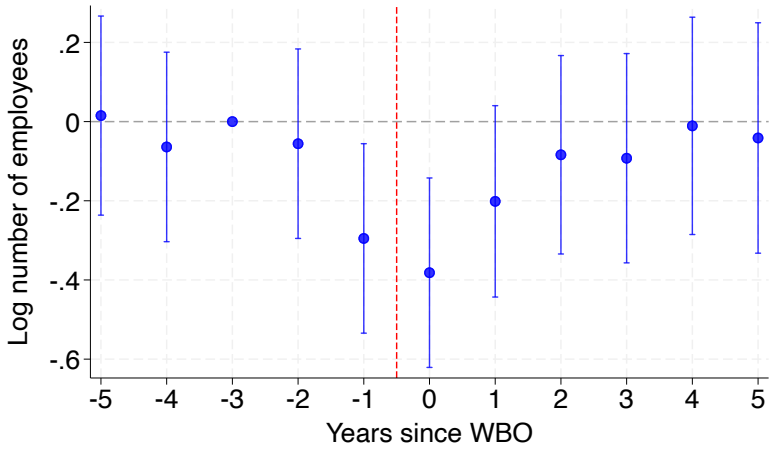
(d) Log of wage percentiles for stayers



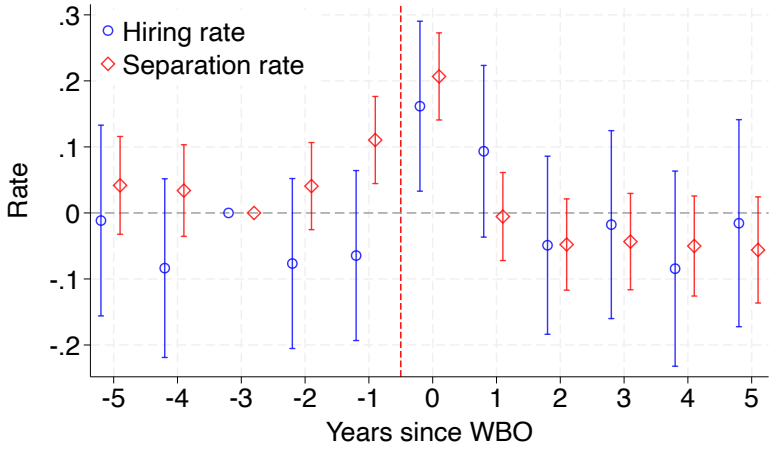
Note: Same as Figure 4, with matching specification 4.

Figure B10: Effects on Employment. Matching specification 4

(a) Log number of full-time employees



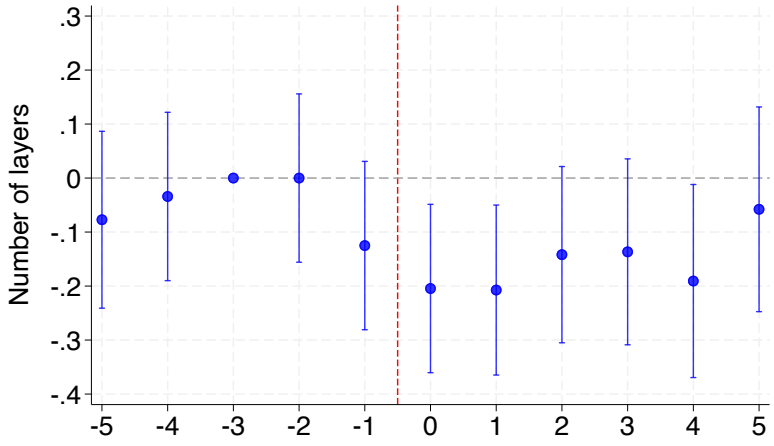
(b) Hiring and separation rates



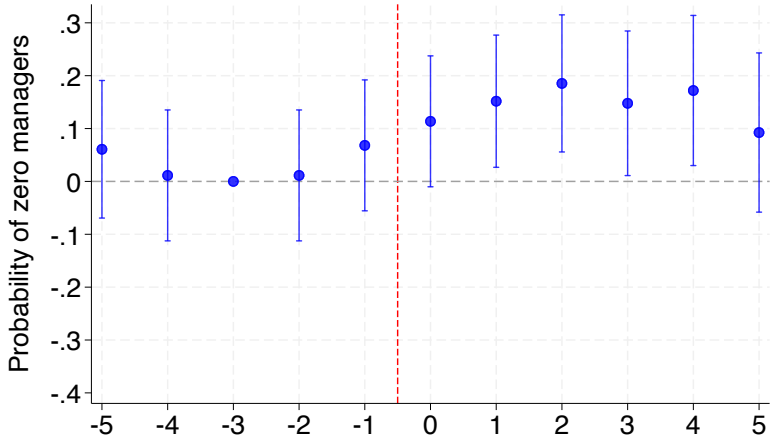
Note: Same as Figure 5, with matching specification 4.

Figure B11: Effects on Firm Hierarchy. Matching specification 4

(a) Number of layers



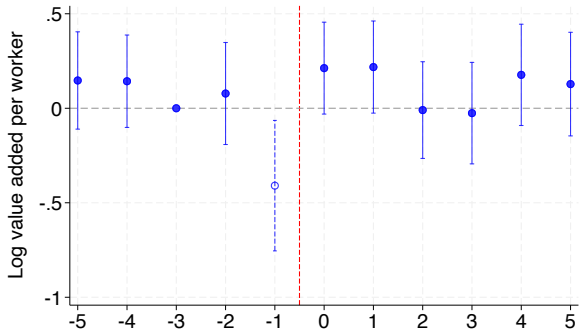
(b) Probability of no managerial layer



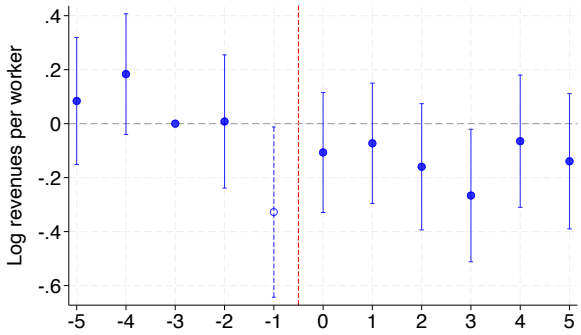
Note: Same as Figure 6, with matching specification 4.

Figure B12: (1996-2021) Effects on Income Statement and Balance Sheet Outcomes. Matching specification 4.

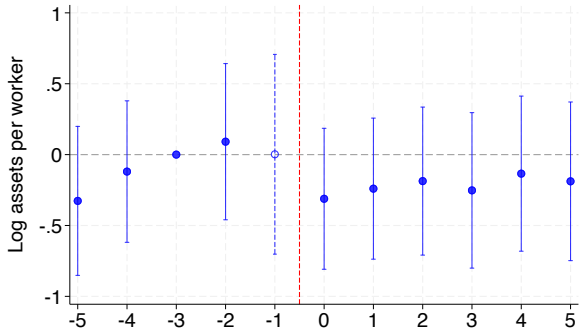
(a) Log of value added per worker



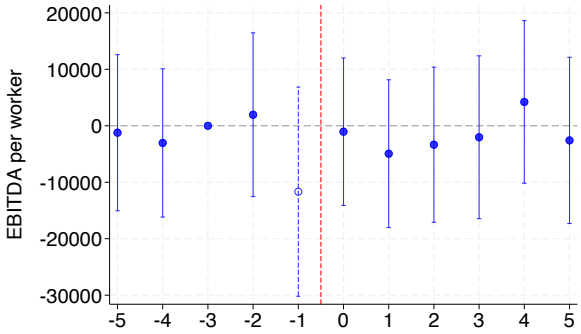
(b) Log of sales per worker



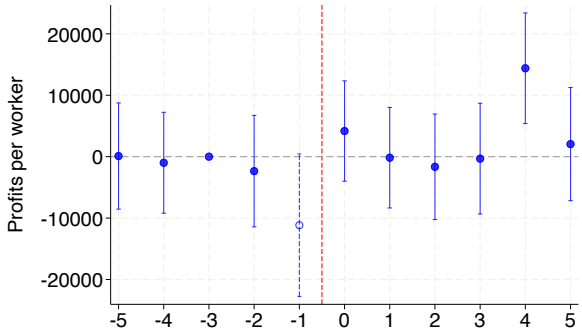
(c) Log total assets per worker



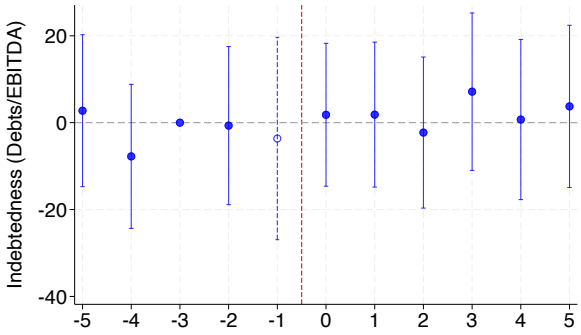
(d) EBITDA per worker



(e) Profits per worker

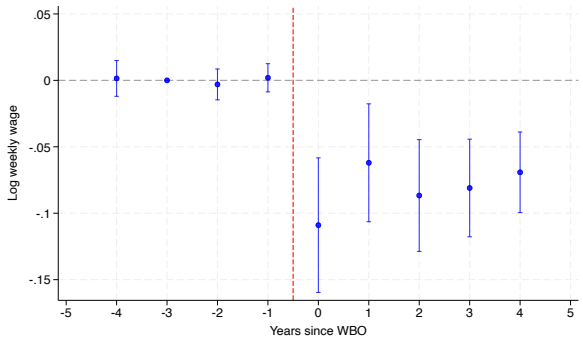


(f) Indebtedness (EBITDA/Debts)

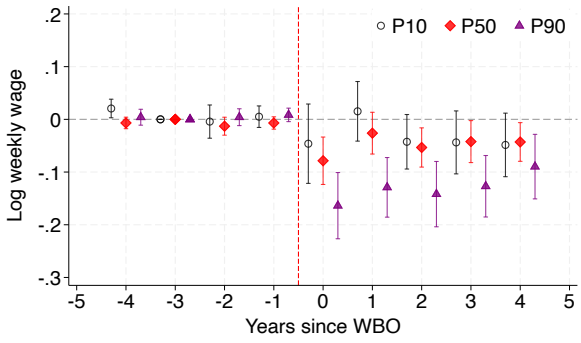


Note: Same as Figure 7, with matching specification 4.

Figure B13: Effects on Wages. Synthetic Difference-in-differences.



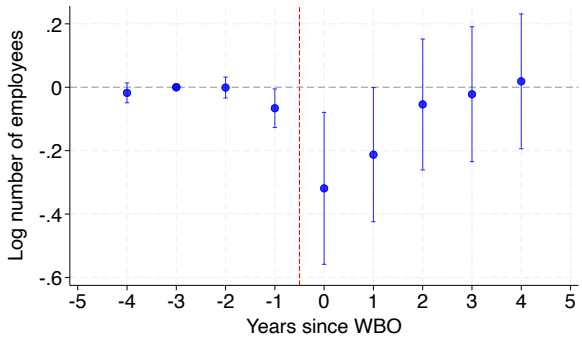
(a) Log of average wage



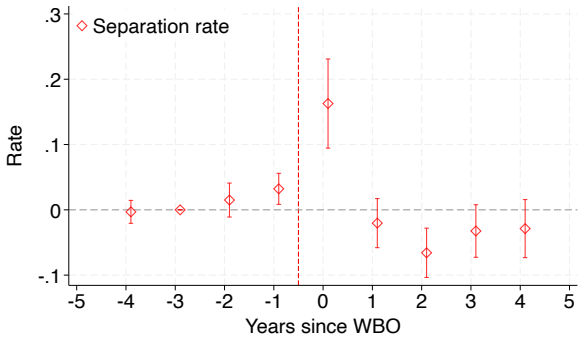
(b) Log of wage percentiles

Note: Panel (a) presents the point estimates of synthetic difference-in-differences for average wage, with 95% bootstrap confidence intervals. Panel (b) presents the point estimates for the log of the within-firm 10th, 50th and 90th percentiles, indicated by black hollow dots, red diamonds and purple triangles, respectively. Bootstrap standard errors are computed using 100 replications and resampling is at the firm-level. Back to Section 4.5.

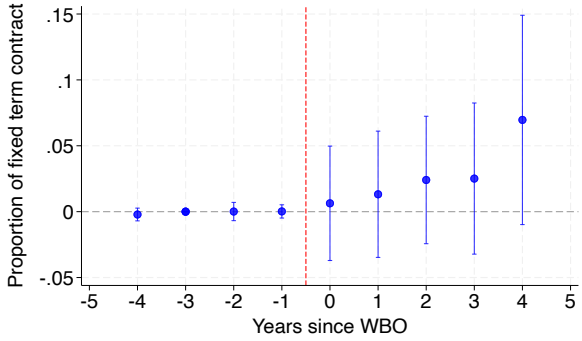
Figure B14: Effects on Employment. Synthetic Difference-in-differences.



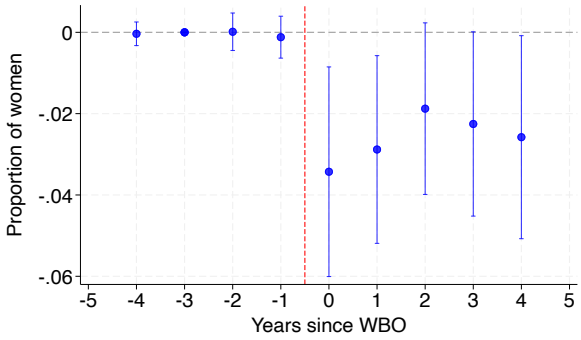
(a) Log of full-time equivalent employees



(b) Separation rate



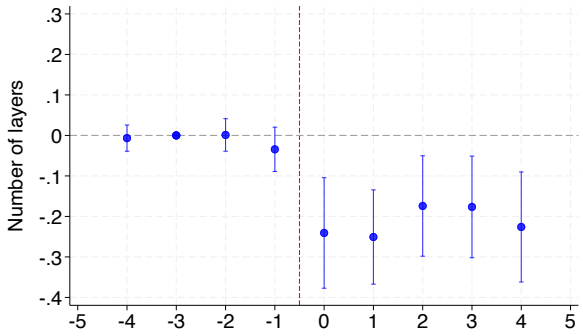
(c) Proportion of fixed-term employees



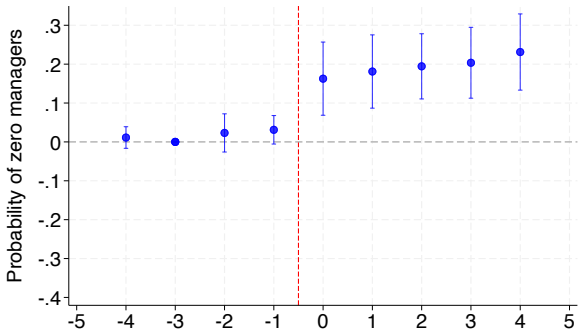
(d) Proportion of female employees

Note: Panel (a) presents the point estimates of synthetic difference-in-differences for the log of full-time equivalent employees, with 95% bootstrap confidence intervals. Panel (b) presents results on the separation rate. The separation rate in year t is calculated as the number of employees that have left the firm by year $t+1$ over the total number of employees in year t. Panel (c) presents results on the proportion of employees on fixed-term contract over the total number of employees, and Panel (d) presents results on the proportion of female employees. Bootstrap standard errors are computed using 100 replications and resampling is at the firm-level. Back to Section 4.5.

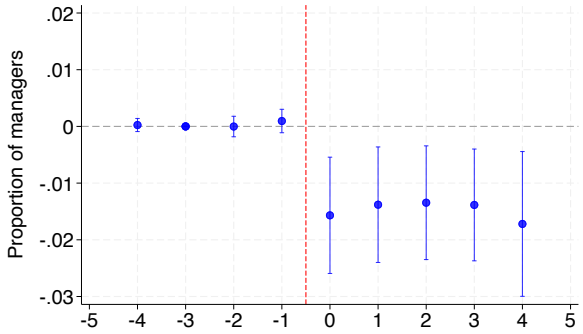
Figure B15: Effects on Firm Hierarchy. Synthetic Difference-in-differences.



(a) Number of layers



(b) Probability of no managerial layer



(c) Proportion of managers

Note: Panel (a) presents the point estimates of synthetic difference-in-differences for log of number of layers in the firm, with 95% bootstrap confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well. Panel (b) presents the estimates for the probability of having no managerial layer. Panel (c) presents results on the proportion of managers over the total number of employees. Bootstrap standard errors are computed using 100 replications and resampling is at the firm-level. Back to Section 4.5.

C Prevalence of worker cooperatives and labor-managed firms, other types of employee-ownership or employee-control, and other types of cooperatives.

A worker managed firm is characterized by being owned primarily by its workers and by being controlled democratically by them according to the one-head, one-vote principle. Worker cooperatives are the most common form taken by worker managed firms, but not the only one. In Italy in 2021, worker cooperatives *cooperative di produzione e lavoro* took up 3% of private sector employment, employing 441,897 workers out of 14,579,764 total employees in private firms. Cooperatives had 1,049,409 employees (7% of the total). ([38] In Spain in 2018, worker cooperatives (*cooperativas de trabajo asociado*) took 0.7% of private sector employment: there were 92,849 workers employed in worker cooperatives, out of 13,242,600 private sector employees. By including the 63,626 workers employed in *sociedades laborales*, the percentage becomes 1.2%. Cooperatives, including types other than worker-cooperatives, employed 241,923 employees. [39] In France, worker coops (SCOP, or *Société coopérative et participative*) employed 60,056 workers, which accounts for 0.35% of the 16,982,000 private sector employees.[40,41] In Argentina in 2021, there were 8140 worker cooperatives, employing 186,460 workers, accounting for 2.9% of the total 6,350,000 private sector workers.[42]

[38] The number of worker cooperative employees comes from INPS data; the number of total private sector employees comes from ISTAT and refers to employees of private firms: https://www.istat.it/en/enterprises?data-and-indicators

[39] Source for worker coop employees and total coop employees: https://www.mites.gob.es/es/sec_trabajo/autonomos/economia-social/estadisticas/index.htm. The source for total number of employees is INE: https://www.ine.es/dynt3/inebase/es/index.htm?padre=10905&capsel=10908.

[40] Data on coop employees comes from https://www.les-scop.coop/chiffres-cles-2023. Data on the total number of private sector employees comes from https://www.insee.fr/en/statistiques/7763770

[41] Interestingly, 51% of jobs in SCOPs and SCIFs (another type of cooperative) come from business conversions. The French coop association classifies conversions into: conversion of associations, rescue of a distressed firm and handover of a healthy firm.

[42] Data on the number of worker cooperatives and their employees comes from (Vuotto, 2022), who elaborated data from the *Instituto Nacional de Asociativismo y Economía Social* (INAES) in 2021. Data on the total amount of private sector employees comes from the government website https://www.argentina.gob.ar/sites/default/files/trabajoregistrado_2302_informe.pdf.