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

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April, 2026

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 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 (insignificantly) increases total compensation when accounting for profit-based labor dividends. Within-firm wage inequality decreases markedly, and firms become 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.

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1 Introduction

How does majority employee ownership of a firm affect its wage distribution and overall performance? In labor-managed firms (LMFs), employees own the firm and 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 labor management has long attracted the attention of economists, political philosophers, and policy-makers as a means to increase worker participation in firms' decision making and ultimately contain inequality.[1]

Yet the theoretical implications of labor management for efficiency and equality remain 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 mutual monitoring (Kandel and Lazear, 1992). On the other hand, concerns about free-riding have prompted some to suggest that labor management would weaken individual incentives and lead to both lower productivity and wages (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) and sluggish growth in many developed economies, labor management raises a core economic question: should firms be owned and governed by their workers, much as political democracies are governed by citizens? And, relatedly, does labor management entail a tradeoff between efficiency and equality?

I study the reduced-form effects of labor management on firms' wage levels, wage inequality, hierarchical structure, employment and productivity, as well as the mechanisms driving such effects. Providing causal estimates of labor management is challenging because a firm's legal form is an endogenous choice. Firms that operate under labor management may differ systematically from conventional firms in ways that are directly related to wages, inequality,

[1] Labor-management had early proponents not only among nineteenth-century socialist writers (Owen, 1813; Proudhon, 1868), but also 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, 1989) 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.

or performance. Hence, a simple comparison of existing LMFs and conventional firms is unlikely to provide 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 for the 1980-2021 period. In the main specification, I use detailed data on the universe of private-sector firms to assemble a matched sample of conventional firms that *also* undergo a restructuring but remain conventionally owned. I then compare each WBO firm to a number of suitable comparison firms that restructure in the same year. 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 years 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. Conditional on observable characteristics, selection into a WBO may be driven by exogenous access to information. In some cases, workers learned about the possibility of a WBO from bankruptcy curators or external accountants.

My results show that labor management leads to substantial wage compression and hierarchical reorganization without harming efficiency. In a first step, I compare average employee earnings in the two sets of firms. 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 overall effect on total employee compensation is 2.4% and not significant. This estimate should be interpreted as an upper bound on the compensation gains from labor management. Thus, labor management shifts the composition of pay by reducing fixed wages and increasing performance-contingent income, with statistically insignificant (positive) changes in total compensation.

Second, I move on to study the implications for within-firm wage distributions. I find that labor management leads to a clear decrease in within-firm wage inequality. In the average firm and relative to the control group, the 90th percentile of wages 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 this wage compression. In particular, I show that the same result holds among workers who remain continuously employed before and after the restructuring, demonstrating that changes in wage policies drive the compression, not selective retention. In terms of aggregate trends, the implied reduction in the P90/P50 ratio (7.8%) offsets roughly one-third of the increase in wage inequality in the Italian private sector between 1985 and 2020 (Briskar et al., 2023).

In the next step, I move on to ask whether wage compression is mirrored by an adjustment in organizational structure, in ways consistent with increased worker control. The results show that 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.

Next, I study the employment effects of labor management. I find that employment initially drops by about 36.2% in the year following the restructuring, but the effect is statistically indistinguishable from zero from two years after the transition as WBO firms engage in intensive hiring, leaving no detectable long-run effect. Although the data do not allow me to accurately observe the reason for separation, 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 the newly formed firm should in principle retain all willing previous employees.[3]

To close the analysis, I study the implications of labor management for firm performance. To do this, I assemble detailed financial information for a subset of firms for the 1996-2021 period. After showing that this subset of firms is comparable to the main sample, I analyze effects on value added per worker using the same matched difference-in-difference specification. Despite changes in wage policies, organizational structure, and worker turnover, I find that labor management does not harm firm performance. The effect on value added per worker is positive (14%) but not significant. This could be due to the combined result of two opposing forces at work. On the one hand, ownership by workers may increase mutual

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

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

monitoring and tie employee compensation to firm performance thanks to labor dividends. On the other hand, since labor dividends ultimately depend on team effort, common ownership may decrease individual effort because of free-riding. Finally, the detailed financial information allows me to analyze 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 statistically significant.

Overall, the empirical evidence I present indicates that labor management can increase wage 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. Given that worker buyouts are rare, these findings suggest that facilitating such transitions may be a promising avenue for addressing wage inequality without large efficiency costs.

This paper relates to three strands of literature. First, it directly speaks 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; Dean, 2024; Blanchard, Burdin and Dean, 2025; Burdin and Garcia-Louzao, 2025) and other institutions with cooperative property rights like *kibbutzim* (Abramitzky, 2008) and agricultural cooperatives (Montero, 2022). Overall, this literature provides mixed evidence on wage levels, suggests reductions in wage inequality based largely on between-firm comparisons, and finds generally positive or null effects on productivity. More recently, Montero (2022) exploits plausibly exogenous variation in property rights to compare cooperative and conventionally owned farms, showing that relative productivity depends on the type of crop produced. Closer to this paper, Dean (2024) finds that workers employed by 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.[4] I make two contributions to this literature. First, I provide a novel identification strategy that accounts for selection of firms into labor management, therefore providing credible causal evidence of the effect of labor management in a modern developed economy. Second, I am able to precisely study the efficiency-equality tradeoff in labor management, thanks to detailed data on employee earnings, mobility and firms' financial information.

Second, this paper complements the growing literature on worker representation in firms.

[4]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.

Recent papers have provided causal evidence on the effect of codetermination, which confers minority control rights to workers (Jäger, Schoefer and Heining, 2021; Mina, Moschella and Tiedtke, 2025), and worker voice arrangements, which give workers formal consultation mechanisms but no voting rights (Harju, Jäger and Schoefer, 2025).[5] These papers found small positive (or null) effects on wages and labor productivity, and small reductions in within-firm wage inequality. The reductions of the P90/P50 ratio they find are at most 1.4%, substantially smaller than the effects in this paper. 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 complement 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 compensation.

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 both firms' wage policies and workers' sorting in explaining patterns in wage inequality. This paper stresses 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.

[5]For example, the German codetermination model studied in Jäger, Schoefer and Heining (2021) establishes a mandate for firms with 500-2,000 employees to assign one third of seats in the supervisory board to worker representatives. The Finnish model studied in Harju, Jäger and Schoefer (2025) 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.

2 Institutional Context

In this section, I outline the institutional features necessary to interpret my empirical findings. I first describe how corporate governance in worker cooperatives differs from conventional firms, emphasizing the key distinction in the allocation of voting rights. I then discuss taxation and access to credit, clarifying why these do not mechanically drive the results. Next, I explain the framework governing worker buyouts in Italy and why it provides a clean setting to study transitions to labor management. Finally, I review Italian wage-setting institutions to show that firms retain meaningful discretion over pay policies.

2.1 Comparative corporate governance: conventional firms vs. labor-managed firms

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. In Italy, worker cooperatives closely align with the labor-management model. Cooperatives (*Società a capitale variabile con scopo mutualistico*) generally distribute voting rights to members according to the 'one-head, one-vote' principle.[6] Up to the distribution of voting rights, both types of firms are governed as standard limited liability companies: 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.[7] Worker cooperatives are a specific type of cooperative in which at least two-thirds of voting rights have to be distributed to employee-members. Non-employee shareholders - financing partners - can have at most one third of voting rights.[8]

Figure 1 illustrates the main differences in corporate governance between the two firm types.

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

[7] If they specify so in their charter, CO firms can opt into different corporate structures, which however do not alter the distribution of voting rights among shareholders. 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.

[8] 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.

2.2 Tax regime

Taxation of employees' labor earnings. Employees face the same taxation of labor earnings regardless of whether their employer is a worker cooperative or a conventional firm. In both firm types, base wages are subject to income tax and social security contributions. Employee-members in worker cooperatives may receive additional year-end compensation in the form of labor dividends (*ristorni*). These dividends are not subject to social security contributions and, during the study period, were taxed at a flat 26%. This rate is comparable to the one applied to year-end bonuses in conventional firms.[9]

Taxation of firms' profits. Cooperatives may benefit from profit-tax exemptions on up to 70% of profits. In particular, profits allocated to indivisible reserves and labor dividends are exempt up to that threshold. This allocation is somewhat constrained: cooperatives must allocate at least 30% (and up to 70%) of profits to non-distributable reserves. These reserves capitalize the cooperative, cannot be appropriated by members, and are tax-exempt. The remaining share may be distributed to employee-members as labor dividends, and is tax-exempt up to the 70% threshold.[10]

Unlike conventional firms, cooperatives face limits on dividends to share capital. The dividend to shareholders cannot exceed 2.5% plus the yield on Postal Savings Bonds (*Buoni Fruttiferi Postali*) applied to the value of shares.[11] Given these limits, dividends to share capital are unlikely in worker cooperatives; in practice, worker cooperatives redistribute profits through labor dividends.

2.3 Worker buyouts

Worker buyouts (WBOs) are transitions to labor management in which a firm's assets are acquired by a worker cooperative formed by its former employees. Workers invest personal resources (severance pay, unemployment benefits, and sometimes savings). Under the Italian *Marcora* framework, the state-backed agency *Cooperazione Finanza Impresa* (CFI) can

[9] 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.

[10] 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.

[11] 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.

match their equity contributions up to a 1:1 ratio and extend additional subsidized loans linked to workers' capital, up to twice the workers' collective investment (CFI, 2022).

Employees of distressed firms sometimes learn about this option through personal networks or cooperative associations. In some cases, bankruptcy curators or external accountants suggest a WBO as a restructuring solution. Appendix A documents a collection of such cases from national and local news sources. These cases are illustrative rather than representative, but they are consistent with a mechanism in which, conditional on firm distress, information access may be exogenous to firm characteristics.

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). In the 1985–2021 period, CFI 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), halting new WBO financing until 2010. From 2011, CFI resumed financing and technical support for WBOs, with financing shifting partially from equity participation to matching loans. Using CFI administrative data, I identified 310 Marcora-supported WBO operations between 1986 and 2021.[12]

Because WBOs happen when a conventional firm is in financial distress and the owners want to liquidate it, my 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 allows meaningful firm-level wage setting, so labor management can plausibly affect firm wage policies. Sectoral collective bargaining is a central wage-setting institution in Italy and could constrain firm-level wage setting. However, unions and employer associations negotiate wage floors and non-wage items (for example holidays and paid sick leave), while firms remain free to pay above those minima.

Consistent with this institutional setup, within-sector wage dispersion is substantial: the variance of log weekly wages is 0.52 (77% of total variance), with both within-firm (0.36) and between-firm (0.16) components (Briskar et al., 2023).[13] This pattern persists even after

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

[13]This is calculated using 4-digit sectors, but results are similar with 2-digit sectors. For example, within

controlling for worker characteristics, both observed (age, tenure, gender) and time-invariant unobserved ones: the variance of firm pay premia from a two-way fixed-effects model à la Abowd, Kramarz and Margolis (1999) is 0.190 for 1997–2003 (the most recent available) (Devicienti, Fanfani and Maida, 2019). This is very close to Germany's 0.194 (Card, Heining and Kline, 2013). In short, similar workers are paid differently across firms. Wages also respond to locality-specific shocks, including negative labor supply shocks, further indicating that firms set wages above sectoral agreements (Dicarlo, 2022). Hence, changes in corporate governance could affect wage policies.

Potential differences in wages between conventional and labor-managed firms are not mechanically generated by different collective bargaining agreements. In fact, thanks to the INPS data, I observe that cooperative and conventional associations are typically co-signatories to the same agreements. Among the top 10 most frequent CBAs (covering 78.5% of employee-year observations) only the metalworking sector had separate agreements.[14] However, in 2023 the minimum wage floors were identical at €1.488,89 per month.

3 Data and Empirical Approach

3.1 Data and variable definitions

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

2-digit sector variance is 0.54.

[14] This CBA covers 2% of employee-year observations for worker cooperative employees. Information on signatory parties, wage floors, and other items is available from *Consiglio Nazionale Economia e Lavoro* (CNEL): https://www.cnel.it/Archivio-Contratti/Minimi-retributivi.

part-time and full-time workers comparable. I deflate wages by the 2015 CPI.[15] Employment at a given firm in a calendar year is obtained by counting the number of worker-year spells in that firm and year. INPS 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 (1996–2018) and Orbis (2019–2021).[16] 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. I set to missing all negative values of strictly non-negative outcomes such as 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. In addition, worker cooperatives can allocate at most 70% of profits to labor dividends.[17] Following these requirements, I define the weekly labor dividend for each employee to be 70% of weekly firm profits (if they are positive) times that employee's share of the weekly wage bill. For each worker i in labor-managed firm j in year t, the weekly dividend d_{it} is:

$$d_{it} = 0.7\pi_{jt}\frac{w_{it}}{wb_{jt}}$$

where π_{jt} are firm j's weekly profits, wb_{jt} is firm j's weekly wage bill, and w_{it} is worker i's

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

[16] Both Cerved and Orbis are private data providers and collect data from business registries.

[17] Recall from Section 2 that labor-managed firms must allocate at least 30% of profits to reserves.

weekly wage in year t. Employees' weekly total compensation is their wage plus the labor dividend, capped at most at 1.3 times their weekly wage.[18] 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 ones are worker-members.[19]

Although I cannot observe labor dividends in the INPS data, I use complementary firm-level information from Legacoop on dividend payments. These data form a biannual, unbalanced panel of 527 active worker cooperatives affiliated with Legacoop over 2015–2021, of which 43 are WBO firms. Appendix Table B3 reports summary statistics for this sample. About 46.6% of firms report distributing some labor dividends in the year. The share is similar for WBO firms (42.7%) and for firms aged five years or younger (43.8%). In the main analysis sample, 59.7% of firm-year observations for WBO firms record positive profits. Combining these figures implies that, conditional on making positive profits, a WBO firm distributes dividends with probability 0.715. This suggests that the adjustment to construct total compensation reflects a plausible dividend distribution scenario.

3.2 Sample definition

Starting from the universe of employee-employer spells in the private sector, I restrict the sample to workers aged 18-65 and with positive earnings. I retain one spell per worker-year, namely the spell with most full-time equivalent weeks.[20] I then collapse the data to a yearly unbalanced firm panel covering 1980–2021.

I construct a matched comparison group following Jäger, Heining and Lazarus (2026). The goal is to compare WBO transitions to conventional restructuring events that face similar pre-transition conditions, so that estimated differences isolate the effects of labor management

[18] For example, suppose employee i works full-time for worker-cooperative j and earns 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) * (0.7*50,000/52) = 84 euros. Her total weekly compensation will be 584 euros.

[19] As explained in more detail in Section 2, employment in worker cooperative does not automatically entail membership in the cooperative. Worker cooperatives are allowed to employ non-members, as long as the share of wages they pay to non-members 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-members in conventional firms (where by definition they cannot be worker-members), I do not use it for the main definition of total compensation. In this data, over 80% of employees of WBO firms are members.

[20] 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 worker cooperatives and conventional firms.

rather than generic restructuring dynamics.

Time notation. Let t denote calendar year, c event year, and $k = t - c$ event time. For firm j, the event year c_j is the founding year of the newborn worker cooperative in treated events and the founding year of the newborn conventional firm in comparison events. Treated and comparison events are therefore indexed by common cohorts c, with each cohort aligned on treatment timing.

Treated group. I identify WBO cooperatives by merging INPS-Cerved with data from Cooperazione Finanza Impresa (CFI) and Legacoop. After hand-collecting firm tax IDs, I flag 266 WBO cooperatives founded in 1986–2021 in social security records. I then link each cooperative to its predecessor firm. For each WBO, the predecessor is defined as the firm employing the largest share of post-transition WBO workers. On average, 80% of WBO workers come from a single predecessor. To reduce false links, I require that the predecessor disappears from INPS records within one year of WBO establishment.[21] Following manual checks, I drop cases without a clear predecessor. I also impose a minimum pre-transition scale of five employees (on average). This restriction keeps very small firms, where informal ties may dominate, from driving the results and ensures that measures of inequality are informative. I define cooperative founding year as event year c and assign predecessor and successor a common firm identifier j. The resulting treated sample contains 140 WBO events with at least three pre- and one post-event observation.

Pool for comparison group. To construct a pool of conventional restructuring events, I sample firms with the following characteristics: (i) conventionally owned, (ii) founded in years represented in WBO cohorts, (iii) founded with at least 60% of workers coming from the same prior employer, and (iv) the prior employer is a conventional firm that disappears from INPS records within one year. Condition (iii) is necessary because conventional restructurings are not directly labeled in the data; the 60% threshold is chosen to achieve the 80% average of incumbent workers in the treated group. I link each flagged successor to its predecessor and treat them as one firm over time. This procedure identifies 17,847 restructuring firms in 1986–2020.[22] Operationally, the new firm ID is generated by closure of the predecessor and opening of a successor, potentially with the same or different owners. By requiring the successor to be founded in the transition year, the pool intentionally excludes mergers. Appendix Table B1 reports pre-matching summary statistics for treated events and the donor pool.

[21] A predecessor may still appear in year t if it employed workers in early t.

[22] For 91% of controls, predecessor and successor are in the same province.

Matched sampling to select comparison group. I implement one-to-many coarsened exact matching by cohort, with replacement. For each WBO predecessor, I select control firms that match exactly in $k = -3$ on: average wage quintile, employment quintile, hiring rate quintile, and manufacturing dummy.[23] Matching in period to $k = -3$ aligns firms before the onset of acute financial distress. The chosen variables align firms on wage level, scale, labor-demand dynamics, and sector. Section 4.5 shows robustness to alternative matching variables, one-to-one matching, and synthetic-control-based approaches.

I obtain at least one match for 93 of 140 treated events. The final matched sample contains 326 comparison firms, i.e., 3.5 controls per treated firm on average (median 3). I denote each matched set by g. In the 1996–2021 period, Cerved-Orbis financial data are available for 34 treated and 190 comparison firms. These matched samples are the basis for all baseline event-study estimates in the next sections.

3.3 Summary statistics

Table 1 reports summary statistics for the full INPS 1980-2021 matched sample, and Table 2 presents statistics for the 1996-2021 subsample with financial data from Cerved-Orbis. Overall, matching balances the two groups on key dimensions. Even where some level differences remain, the difference-in-differences design is valid as long as the common trends assumption holds.

Wages, employment and worker demographics. WBO predecessors pay lower weekly wages: the average is about 12% below the comparison group (control mean in logs 6.288, or 539 euros). The 10th, 50th and 90th within-firm wage percentiles are also lower. WBO predecessors are somewhat smaller (78 versus 97 workers), but this difference is not statistically significant and both groups lie well above the key 15-employee threshold in the Italian labor market.[24] The hiring rate is 7.6 percentage points higher in WBO predecessors. Average age, tenure and the shares of part-time and fixed-term contracts are balanced. Comparison firms employ almost twice as many managers but the probability of having no managerial layer is similar (0.6 versus 0.5).[25]

[23] Related work often uses finer bins (e.g. deciles (Jäger, Heining and Lazarus, 2026)), but quintiles are preferable here due to the limited treated sample size.

[24] In Italy, the key firm-size threshold is 15 employees: above it, firing costs are higher, firms must hire at least one worker with a disability, and workers can establish a works council. The short-time work scheme *Cassa Integrazione Guadagni Straordinaria* applies above the 15-employee threshold in some sectors, including manufacturing (see Giupponi and Landais (2023)).

[25] Managers include both middle managers (*quadri*) and executives (*dirigenti*).

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). Because matching is one-to-many, the manufacturing share need not be identical in the two groups.[26]

Income statement and balance sheet variables. Table 2 shows that the 1996–2021 subsample is comparable to the main sample, although firms are somewhat smaller. Within this subsample, treated and comparison firms are similar in financial variables: value added per worker is 15% lower in WBO firms, EBITDA per worker is 3.4% higher, but neither difference is statistically significant. Both groups report similar debt-to-EBITDA ratios and negative profits on average, consistent with subsequent distress.

3.4 Estimating equation and identification strategy

I study the effects of labor management by estimating an event-study difference-in-differences specification:

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

where Y_{jt} denotes firm-level outcomes (for example average wage or value added per worker) for firm j at time t, α_{gt} indicate matched-set-by-year fixed effects, η_j are firm fixed effects, $\mathbb{1}\{\cdot\}$ is the indicator function, WBO_j equals 1 for treated firms, and c_j is the event year (the founding year of the successor firm). The error term is ε_{jt}, clustered at the firm level. Coefficients β_{6^-} and β_{6^+} absorb very early and very late 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 $k = -3$ to align to the period used for matching, and because it provides a pre-period window to evaluate potential anticipation effects. Year effects are estimated separately within each matched set, so identification relies on within-set variation over elapsed time. This is more restrictive than standard cohort-based event-study designs (e.g. Jäger, Heining and Lazarus (2026); Schmieder, Von Wachter and Heining (2023)) and avoids comparing firms that belong to the same cohort but have different pre-

[26] Treated manufacturing firms had more eligible controls firms and therefore contributed more to the control group.

treatment characteristics.[27] Results are similar when using cohort-by-year fixed effects.

I also report pooled post-treatment summaries: β_{Post} pools $k = 0, \ldots, 5$; β_{SR} pools $k = 0, 1, 2$; and β_{LR} pools $k = 3, 4, 5$. These pooled coefficients are useful when individual event-time estimates are noisy and provide compact summaries of average short- and medium-run effects.

Identification assumptions and potential threats to identification. The key identifying assumption is the common trends assumption (CTA): absent the treatment, outcomes in treated and comparison firms would have followed similar trends. I can evaluate this assumption with pre-event coefficients ($k < 0$) for the main outcome event studies. Results in Section 4 support the validity of the design.

However, there are channels – other than the main outcomes shown in Section 4 – that may cause concern. I discuss six such channels and summarize the evidence from balance tests and pre-trend coefficients.

One concern is that WBO predecessors may be systematically more distressed before transition, or on worse trends, than comparison firms. If so, workers might choose WBOs only when outside buyers are unavailable, violating common trends. In levels, pre-transition financials are balanced (Table 2): EBITDA per worker is almost identical across groups; profits are negative in both groups; and debt-to-EBITDA is, if anything, lower in WBO firms. Event-study evidence in financial outcomes further supports the validity of my research design. The $k = -1$ coefficient is not informative because reporting attrition in Cerved-Orbis is substantial in that period due to firms restructuring (14/34 treated firms and 107/190 controls; Figure 2, panel b). In period $k = -2$, coverage is much higher (79% treated, 87% controls), and coefficients for EBITDA, net profits, and debt-to-EBITDA are near zero (Figure 6, panels d–f). For these variables, coverage is also high in periods $k = -4, -5$ and coefficients are close to zero. Taken together, these patterns do not support differential pre-trends in financial conditions.

Second, workers in WBO predecessors may have systematically higher interpersonal trust, which would explain their ability to coordinate on collective action. I proxy interpersonal trust with (i) foreign-worker share and (ii) concentration of workers' birth municipality. The first proxy follows evidence that natives (always the majority in sample firms) tend to trust foreigners less and that trust decreases with local ethnic diversity (Cettolin and

[27] Similarly to my approach, 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.

Suetens, 2019; Dinesen, Schaeffer and Sønderskov, 2020). The second proxy is relevant because workers from the same town are more likely to share dense social networks and have higher levels of mutual trust.[28] Table 1, Panel B, shows that the foreign share is similar across groups. Birth municipality is more concentrated in WBO firms, but the difference is small in magnitude.[29]

Third, a related concern is that workers in WBO predecessors may have more homogeneous economic preferences, which could reduce conflict in collective decisions and increase expected WBO value. I proxy preference homogeneity with the within-firm standard deviation of age, relevant for the horizon problem in cooperatives: older employee-members may value long-horizon investments differently than younger employee-members. This proxy is balanced across groups, suggesting no sizable differential sorting on this dimension.

Fourth, political connections may simultaneously increase the probability of undertaking a WBO and ease regulatory constraints. However, political connections are uncommon in Italian firms below 100 employees – between 3.5% and 10% (Akcigit, Baslandze and Lotti, 2023) chance of employing at least one politician, and increasing in firm size. Both groups in my sample average below 100 employees, and control firms are if anything larger, making differential political connections an unlikely driver.

Fifth, workers may have stronger incentives to carry out a WBO when they hold more firm-specific human capital. I proxy this with in-sample tenure (that is starting from 1980).[30] Pre-transition tenure is very similar across groups (3.67 vs. 3.75 years).

Sixth, WBO firms can receive subsidized Marcora loans (maturities between 3 to 10 years and low/zero rates), raising the concern that treatment combines a transition to labor management and access to cheap credit. However, comparison firms also have access to broad SME support schemes that improve access to credit and lower financing costs: the Guarantee Fund (*Fondo di Garanzia*) and the Capital Goods (*Beni Strumentali*) schemes.[31] Micro-level take-up data are unavailable, but back-of-the-envelope calculation based on aggregate disbursements suggests high take-up among the comparison: 75% for the Guarantee Fund and 35% for the Capital Goods. Accordingly, my estimates should be interpreted as the reduced-form effects of entering the WBO pathway relative to continued access to standard SME subsidized credit.

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

[29] Under equal municipality shares, average HHIs imply roughly 4 and 5 origin municipalities in treated and control firms, respectively.

[30] This measure of firm tenure left-censors employment spells that began before 1980.

[31] The Guarantee Fund covers part of the default risk, improving access to credit, and the Capital Goods scheme subsidizes borrowing at 2.75% of loan value for five years, effectively lowering financing costs.

From anecdotal evidence, selection conditional on observable characteristics may be due to access to information about buyouts. As documented in Appendix A, in one case employees of a distressed firm learned about the possibility of doing a buyout through the bankruptcy curator assigned to their case. In another case, an external accountant alerted the group of employees that a WBO would be an option.

Overall, pre-trend evidence and balance checks indicate that the matched comparison group provides a credible counterfactual for treated firms. I now turn to the outcome estimates.

4 Empirical Results

4.1 Firm survival

Figure 2 shows survival and attrition rates for the two groups of firms. Panel (a) plots the ratio of firms that are still active at each elapsed time k after the transition. For each period k, the numerator is the number of firms active in that period, and the denominator is the standard risk set: firms that were active at $k = -3$ that have not been right-censored because their cohort is no longer observed at k.[32] This measure is equivalent to the discrete-time Kaplan-Meier estimate. Survival rates are virtually identical in the two groups after one year, but they are 10 percentage points (12%) higher in WBO firms after three years, and 13 p.p. (19%) higher after five years. This is consistent with the idea that worker-members 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; Alves et al., 2022). 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 mechanism in the following section. Survival rates turn out to be quite similar to the raw attrition rates plotted in Appendix Figure B1.

4.2 Wages

Average wage. Panel (a) of Figure 3 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

[32] 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 $k < 5$.

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 assumption. All pre-treatment coefficients are statistically indistinguishable from 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.

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.[33] Panel (b) of Figure 3 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 although not significant. The effect on the average total compensation is positive (2.4%) over the post-transition period and positive (3%) in the long run. The aggregated effect on the median profit-adjusted wage is also positive (3%). Hence, WBO firms are able to lower base wages and make total compensation contingent on good performance, something that conventional firms may be less capable of committing to, particularly after a restructuring. This may also explain the 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 3 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

[33] In Table 2 and in Appendix Table B2, I showed that they have similar average statistics and estimated effects, respectively.

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 wage distribution would mechanically become more 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 3, results are identical when estimated on the subsample of stayers: workers employed continuously by the same firm from three years before to two years after the restructuring. I restrict the time window to six consecutive years to ensure a sufficiently high number of workers in this subsample. This exercise fixes worker composition and allows me 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 almost half of the total increase (17.6%) of this measure of inequality over the 1985-2020 period in the Italian private sector (Briskar et al., 2023).[34]

In the 1996-2021 subsample of firms with available financial information, percentile effects are less precisely estimated (columns (1)–(4), Table 4) and differences are not statistically significant. After profit adjustment, differences are again statistically indistinguishable from each other. However, it is important to note that this does not arise from the profit distribution rule: by construction, the imputed dividend is proportional to wages and therefore inequality-preserving. Rather, it reflects weaker differential effects across percentiles in base wages within this subsample and lower statistical power.

4.3 Employment, worker composition and hierarchy

Employment. Panel (a) of Figure 4 shows event study estimates for log full-time-equivalent employment. Table 3 reports the aggregated coefficients. Following a buyout, employment falls sharply. In the short-run, employment falls by 27.4 log points, approximately a 24.0% decrease in the number of employees. However, this contraction is temporary. From two years after the transition onward, coefficients are close to zero, and the long-run aggregate effect is zero. Panel (b) decomposes the employment dynamics into hiring and separation

[34]To be precise, Briskar et al. (2023) use annual not weekly earnings.

rates. In the year following the restructuring, both rates increase in WBO firms, but the separation increase dominates, generating the employment drop. After one year, separations converge while hiring remains higher in WBO firms, so the employment gap closes after two years.

All but one pre-period coefficients are statistically zero. The coefficient β_{-1} for employment is negative and significant. Combined with the patterns in hires and separations, this points to elevated worker exits just before the restructuring. This pattern can be evidence of either reverse causality of the buyout to employment losses or anticipation effects. The first option is equivalent to saying that the buyout happens because many workers have left the firm; the second one instead means that many workers leave the firms because they know that the buyout is happening. Anticipation effects are more plausible in this setting. Workers' prior knowledge of the buyout is highly likely because of direct involvement. Additionally, some workers may have incentives to opt out of the buyout. First, it is financially risky: workers must 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 employee-members of a worker cooperative. On the contrary, reverse causality is unlikely. WBOs typically involve prolonged negotiations among workers, former owners, unions, cooperative federations, lenders, CFI, and in insolvency cases court-appointed administrators, making such rapid implementation unlikely.

The administrative structure of the data does not allow a clean decomposition of post-transition exits into quits versus layoffs, because successor firms are new legal entities and incumbents are mechanically recorded as newly hired if retained. Still, institutional features suggest a substantial voluntary component: some incumbent workers may choose not to participate, while WBO coalitions face explicit pressure to preserve employment and include as many predecessor workers as feasible.

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, and if anything WBO firms seem to use fixed-term contracts more than comparison firms after the transition. Lastly, 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 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 it 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 5 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

To assess efficiency outcomes, I use Cerved-Orbis balance-sheet and income-statement data linked to Social Security records for 1996–2021. Table 2 and Appendix Table B2 show that this sample is broadly comparable to the main sample in terms of pre-restructuring outcomes and effects on wages and employment. A key limitation is missing financial reporting in the year immediately before transition. Panel (b) of Figure 2 shows that at $k = -1$ only 41 percent of WBO firms (14/34) and 53 percent of controls (107/190) report financial data. I therefore display $k = -1$ coefficients with dashed lines and interpret them cautiously. Data is consistently available for all other periods.

Following the literature in labor economics, I measure firm productivity as log of value added per worker, where value added is 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 6 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

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

zero, which supports the common trends assumption. Post-transition coefficients are also close to zero, and 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 contrasts sharply with a naive cross-sectional comparison that regresses log value added per worker on a cooperative indicator with province-year and sector-year fixed effects in the full Cerved-Social Security panel (estimated coefficient: -29.6 percent). The results in the matched DiD design are consistent with previous literature, which found either no effect of being a worker cooperative on productivity, or a negative effect for the production of some type of goods, but positive effects for others (Fakhfakh, Pérotin and Gago, 2012; Montero, 2022).

For other firm outcomes, effects are similarly muted. Log revenue per worker is lower (-12 %) but imprecisely estimated; combined with value-added results, this is consistent with lower gross output alongside lower intermediate-input costs. Per-capita assets are lower (-20.5 percent) but not precisely estimated, providing no clear evidence of systematic underinvestment (Alchian and Demsetz, 1972). Profitability estimates are also noisy: EBITDA per capita is negative and insignificant, net profits per capita are positive and insignificant, and indebtedness (debt/EBITDA) is higher but imprecisely estimated.

4.5 Robustness checks

A potential concern is that my results reflect 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 select the comparison group using synthetic difference-in-differences (Arkhangelsky et al., 2021). In both cases, results are very similar to the main specification. I now discuss both approaches more in detail.

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.[36]

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.

The significance and sign of the main estimates does not change. Figures B2 through B14 reproduce the main figures for event study estimates on wages, employment, hierarchy and productivity. Overall, results are very similar when using these alternative matching specifications.

Synthetic differences-in-differences. Another potential concern is that results may be driven by the use of matching itself. 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 with differences-in-differences, by selecting a set of weights for units in the donor pool and time periods 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 500 bootstrap replications of the procedure, where

[36] 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.

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 B14 through B16 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

Labor-management appears to reduce within-firm wage inequality and organizational hierarchy, without reducing firm productivity. This paper studied worker buyouts (WBOs) in Italy as sharp transitions from conventional investor ownership to labor management and compared WBO firms to similar restructuring firms that remain conventionally owned. Using matched employer-employee administrative data covering 1980–2021, I estimated the effects of labor management on wages, wage inequality, organizational structure, employment, and productivity.

The main result is that first, base wages decline on average, but profit-based labor dividends offset this decline, leaving total compensation broadly unchanged. However, there is large and significant wage compression within the firm. Importantly, this pattern is driven by changes in wage policies rather than worker composition. Secondly, labor-managed firms also become less hierarchical, with fewer managerial layers, suggesting a reorganization of authority consistent with greater worker control.

Despite these changes in pay structure and organizational design, I find no evidence that labor management reduces firm performance. Measures of productivity, profitability, and investment remain similar to those in conventionally owned restructuring firms. While employment falls temporarily after the transition, this effect disappears within two years. Taken together, the results suggest that labor-managed firms can sustain levels of efficiency comparable to conventional firms while generating substantially more egalitarian wage structures. The results in this paper stand in contrasts to results found by studies of minority worker representation in firms (Jäger, Schoefer and Heining, 2021; Harju, Jäger and Schoefer, 2025; Mina, Moschella and Tiedtke, 2025), which find limited effects on wages and the wage distribution.

In sum, transitions to labor management seem to increase equity without harming efficiency. Policymakers interested in reducing wage inequality could consider promoting more transitions to labor management, or the 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 test whether market failures are preventing more conversions 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-restructuring 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 three years before the restructuring ($k = -3$). P-values are for t-test of the difference in means, with standard errors clustered at the firm level.

Table 2: Pre-restructuring 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 value added 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 three years before the restructuring ($k = -3$). 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	5,797	5,797	5,797	5,797	5,797	5,377	5,377	5,797	5,797	5,797	5,797

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^{**}	0.021	0.029	0.032	-0.010
	(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^{**}	0.030	0.031	-0.001	0.010
	(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^{**}	0.024	0.030	0.019	-0.003
	(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	0.881	0.858	0.666	0.852
N	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108

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 ($k = -3$). 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	6.230	6.202	5.999	6.511
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year × Group FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.870	0.844	0.690	0.849	0.870	0.863	0.723	0.926
N	5,797	5,797	5,797	5,797	1,998	1,998	1,998	1,998

Note: 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.

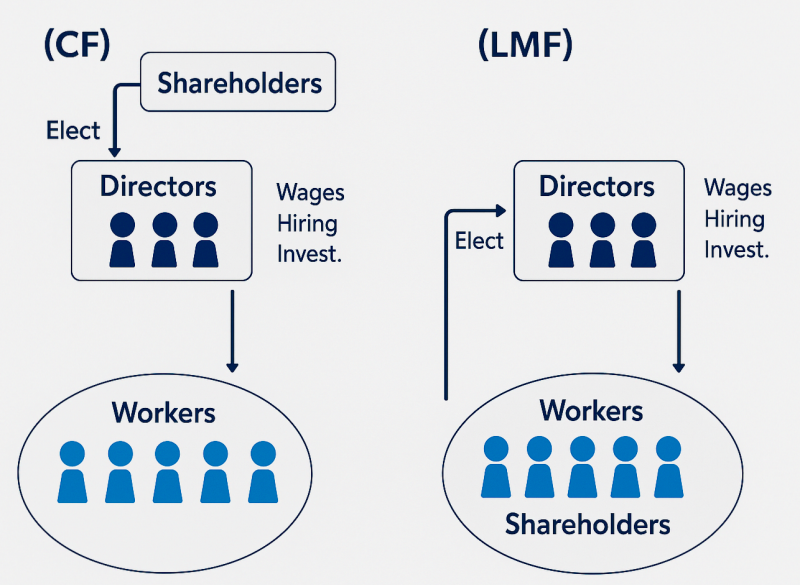
Table 6: Income statement and balance sheet outcomes: 1996-2021.

	(1)	(2)	(3)	(4)	(5)	(6)
	Per Employee					
	LogVA	LogSales	LogAssets	EBITDA	Profit	Indebtd.
β_{SR}	0.150	−0.111	−0.251	−3092.677	941.756	0.643
	(0.104)	(0.095)	(0.212)	(5583.268)	(3502.543)	(7.033)
β_{LR}	0.098	−0.156	−0.196	3.016	5625.994	4.090
	(0.111)	(0.101)	(0.227)	(5958.689)	(3738.055)	(7.523)
β_{Post}	0.131	−0.128	−0.230	−1917.514	2719.951	1.948
	(0.100)	(0.091)	(0.203)	(5345.435)	(3355.220)	(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	3,108	3,108	3,096	3,108	3,108	3,073

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

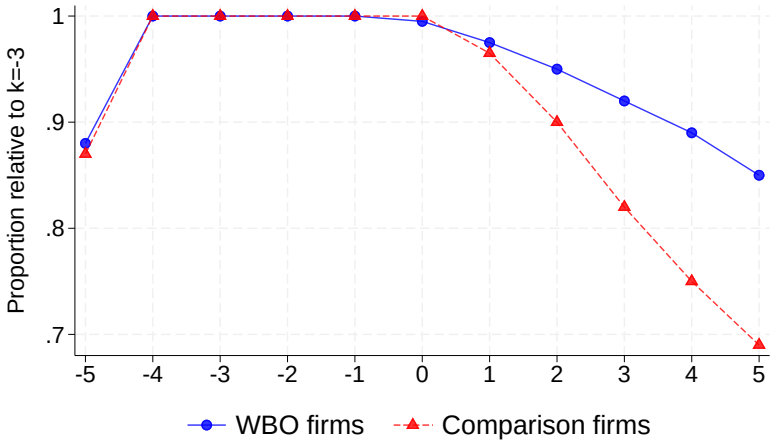
Figure 1: Corporate governance in conventional firms and labor-managed firms.



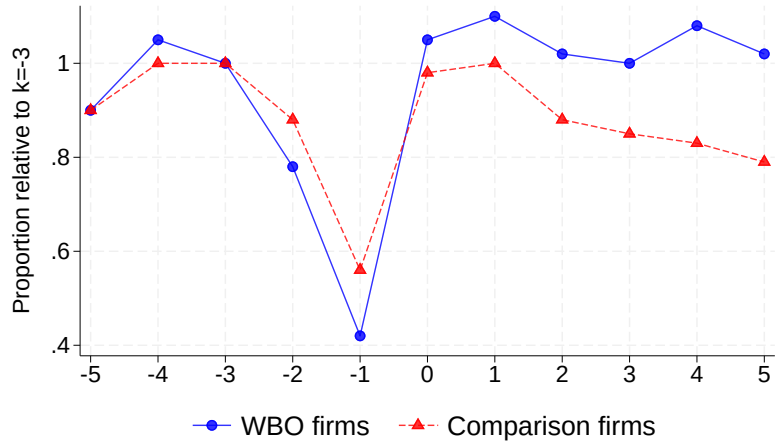
Note: Conventional firms distribute voting rights to shareholders according to capital ownership, whereas labor-managed firms distribute voting rights to members according to the 'one-head, one-vote' principle. Moreover, in Italy, employee-members must have at least two-thirds of votes and be paid at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

Figure 2: Survival proportions.

(a) 1980-2021 INPS panel



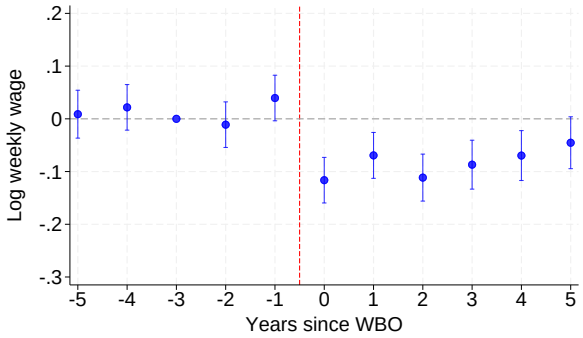
(b) 1996-2021 INPS-Cerved-Orbis panel



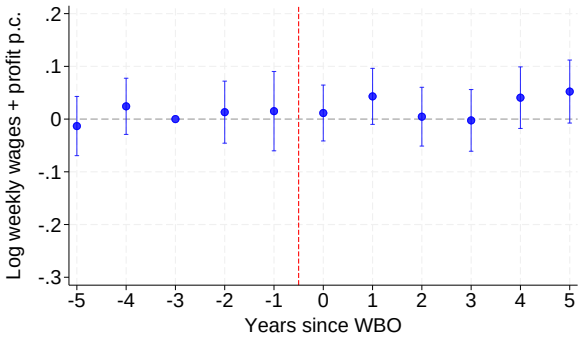
Note: WBO firms (blue solid line) vs. comparison firms (red dotted line). The survival rate is equivalent to the discrete time Kaplan-Meier estimate. For each elapsed time k, the numerator is the number of firms active at period k, and the denominator is the standard risk set, that is the number of firms that were active at the baseline period $k = -3$ and 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 3: Effects on wages

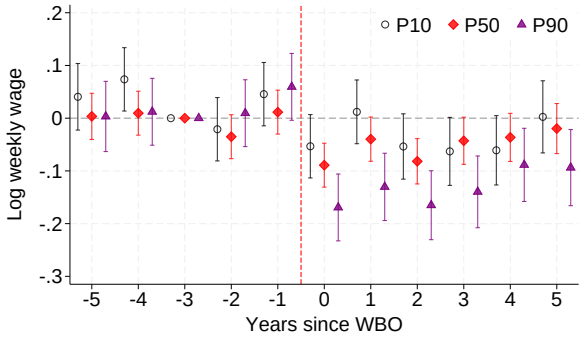
(a) Log of average wage



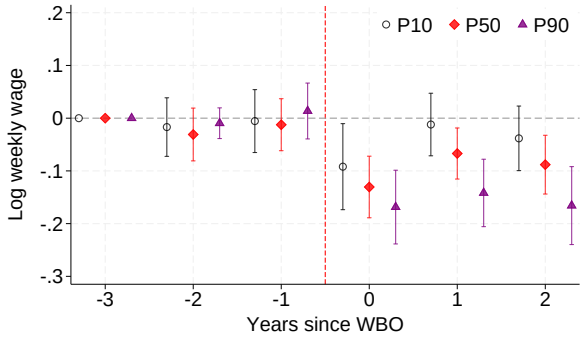
(b) 1996-2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



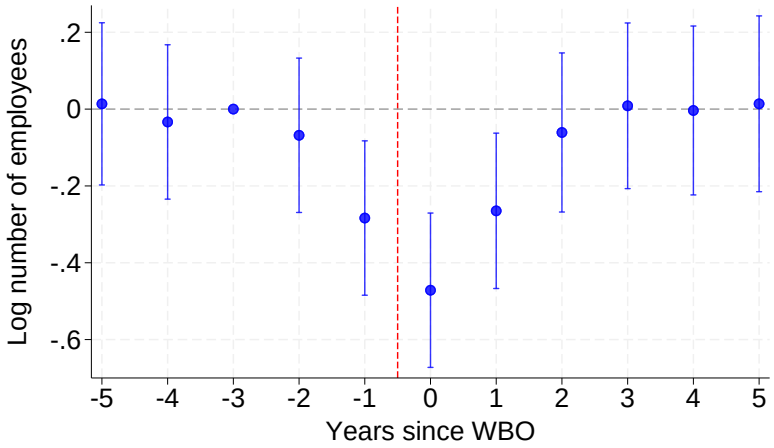
(d) Log of wage percentiles for stayers



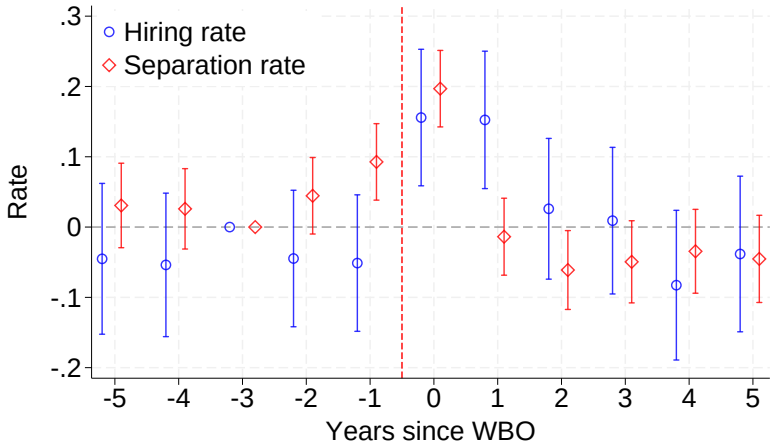
Note: Panel (a) presents the point estimates of the event study specification on the log of the average wage, with 95% confidence intervals. Panel (b) presents the point estimates for total compensation estimated using the subsample of firms in the period 1996-2021 for which financial statements are 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. 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 4: Effects on employment

(a) Log number of full-time employees



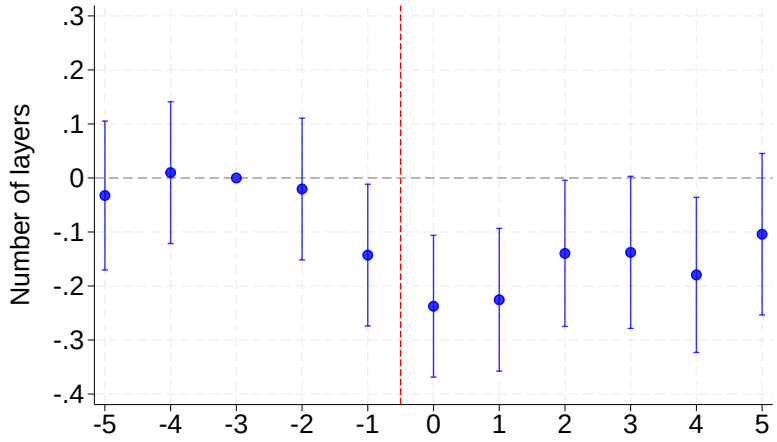
(b) Hiring and separation rates



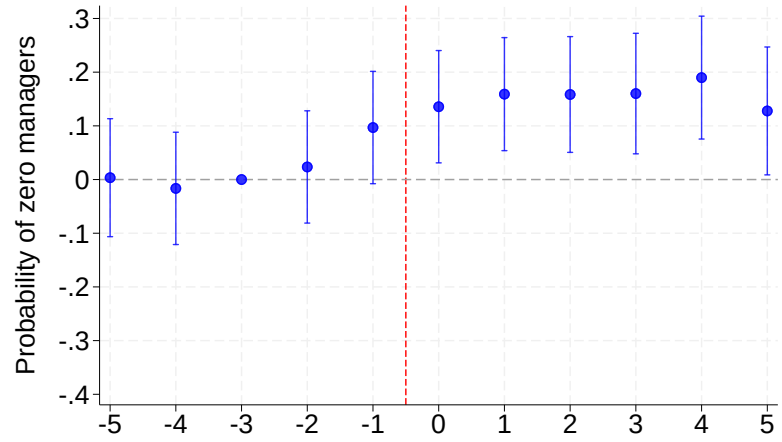
Note: Panel (a) presents the point estimates of the event study specification on the log of full-time equivalent employees, with 95% confidence intervals. Panel (b) presents results on the hiring and separation rates, indicated by blue hollow circles and red hollow diamonds, respectively. The hiring 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 5: Effects on firm hierarchy

(a) Number of layers



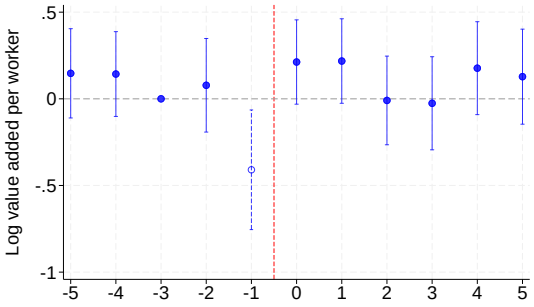
(b) Probability of no managerial layer



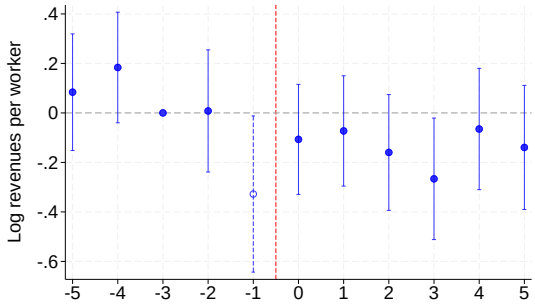
Note: Panel (a) presents the point estimates of the event study specification on the number of layers in the firm, with 95% confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well.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

Figure 6: (1996-2021) Effects on financial outcomes

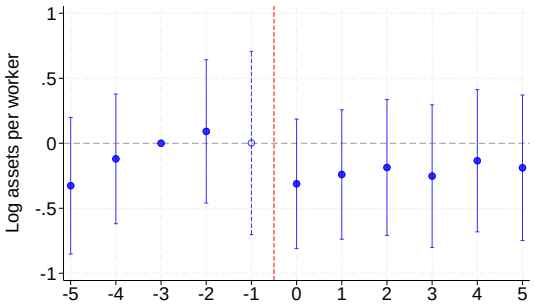
(a) Log of value added per worker



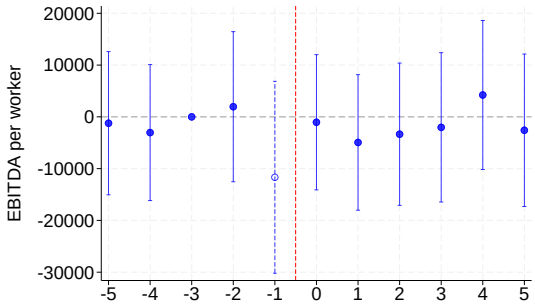
(b) Log of sales per worker



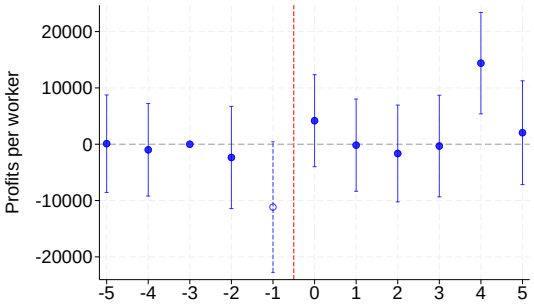
(c) Log total assets per worker



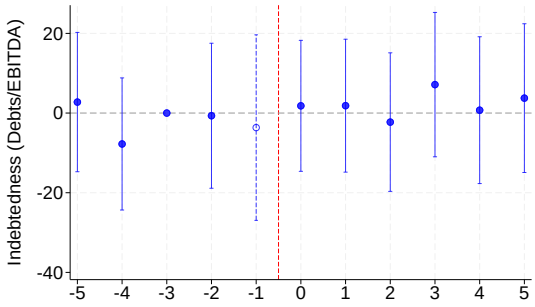
(d) EBITDA per worker



(e) Profits per worker



(f) Indebtedness (Debts/EBITDA)



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 107 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.

Appendix A Anecdotal Evidence on Selection

This appendix summarizes five case studies documented in national or local media, or online. For each case, I describe (i) how workers learned about the worker buyout option (information access), and (ii) the subsequent steps that led to the restructuring.

Cartiera Pirinoli. In this case, information about the WBO option appears to have arrived through insolvency institutions: the workers were informed by the *curatore fallimentare* (or liquidator) during bankruptcy proceedings. This is an example of an external information channel tied to judicial procedure rather than to workers' pre-existing plans. The president of the cooperative later told this story to a local news outlet (Luciano, 2023):

> "The administrative director and I had prepared the business plan for a consortium of Lombard entrepreneurs that seemed intent on taking over the company, which the previous owners had decided to let go bankrupt, laying off its 154 employees. Then came the cold shower: they pulled out. The bankruptcy trustee suggested that we take over the company ourselves by setting up a cooperative."

After this trigger, the pathway followed the standard WBO sequence: workers organized collectively, sought support from cooperative organizations, assembled financing, and then restructured the firm under cooperative ownership (Il Sole 24 Ore, 2025).

Greslab. Here the information shock arrived through a professional intermediary: workers report that the WBO route was suggested by an accounting office (Maurizio Labanti). This quote (Greslab, 2025, p. 137) tells the scene:

> 'In the first days of February, some [...] workers, following the advice they had been given, went to the office where Maurizio Labanti received them. The accountant listened carefully to them and, after brief reflection, suggested they try to form a cooperative in an attempt to revive the business. The future members of Greslab had never heard of "revived" companies; none of them had ever had any contact with the cooperative world; in the ceramics sector there was no experience of this kind; and the idea had never even come up in their conversations. They were disoriented."

Once informed, workers engaged with the cooperative ecosystem (associations and financial institutions), developed a structured business plan, and implemented the buyout (Bonani, 2017; Greslab, 2025).

Ceramiche Noi. In this case, the initial information channel is personal and informal (a friend). The key point for identification is that knowledge of the WBO mechanism appears to have originated not from a systematic firm-level characteristic, but from a contingent social contact. After this trigger, workers moved into the same institutional sequence observed elsewhere: contact with cooperative support actors, cooperative formation, financial assembly, and restart of operations (Ricci, 2019).

Gazzotti 18. In this case, information emerged through interactions with both Legacoop and the bankruptcy curator. This is a hybrid pathway (cooperative-association plus judicial channel), hence only partially external to the cooperative ecosystem. Workers then proceeded through the usual WBO implementation steps: organization of the worker group, legal setup of the cooperative vehicle, negotiation in bankruptcy, and gradual operational relaunch (Cicconi, 2019).

WBO Italcables. For Italcables, information access appears to have occurred through meetings with a financial institution (Banca Etica), the regional branch of a cooperative association (Legacoop Campania), and subsequently the institutional investor (CFI). The subsequent steps were institutionalized: exploration meetings, cooperative formation, financial package construction, and execution of the buyout (Fontana, 2026).

Across the five cases, the qualitative pattern is consistent: information about the WBO option often arrives through idiosyncratic intermediaries (curators, accountants, personal contacts, or institutional meetings). These cases are illustrative rather than representative, but they are consistent with a mechanism in which information access is exogenous conditional on observables. The anecdotal evidence is suggestive and complements, rather than establishes, the identification strategy.

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Appendix B Appendix Tables and Figures

Table B1: Pre-restructuring 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	17,847	140	

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

Table B2: 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	3,108	3,108	3,108	3,108	3,108	2,892	2,892	3,108	3,108	3,108	3,108

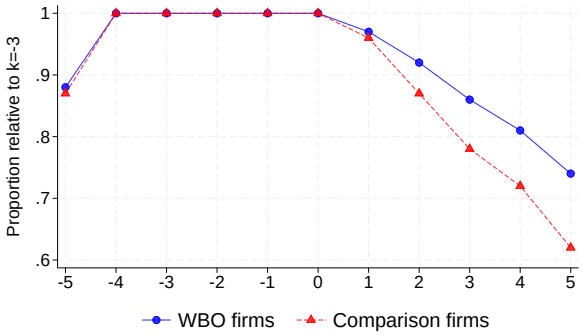
Note: Results on aggregated coefficients for the short run β_{SR} ($k = 0, 1, 2$), long run β_{LR} ($k = 3, 4, 5$) and over the whole post-transition period β_{Post} ($k = 0, ..., 5$). Standard errors are clustered at the firm level. Estimation is done over the subset of firms active in the 1996-2021 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.

Table B3: Summary statistics on labor dividends from the Legacoop sample: 2015–2021

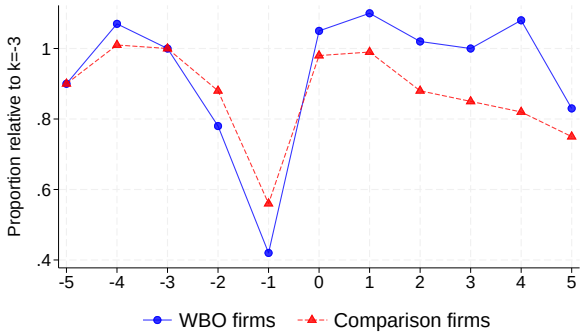
	Mean	SD	N
WBO firm	0.093	0.291	1,327
Firm age	30.650	24.053	1,327
Firm age if WBO=1	9.726	11.201	124
Distributes labor dividends	0.466	0.499	1,327
Distributes labor dividends if WBO=1	0.427	0.497	124
Distributes labor dividends if firm age < 6	0.438	0.497	208
Labor dividends (EUR)	164,188	701,655	1,327
Labor dividends (EUR) if WBO=1	90,468	180,057	124
Labor dividends (EUR) if firm age < 6	26,241	68,832	208
Number of firms	527		
Number of WBO firms	43		

Figure B1: Attrition rates.

(a) 1980-2021 INPS panel



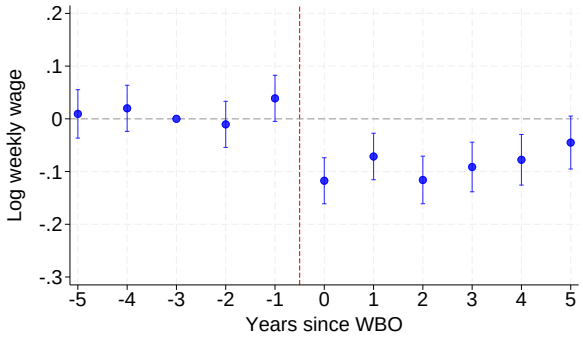
(b) 1996-2021 INPS-Cerved-Orbis panel



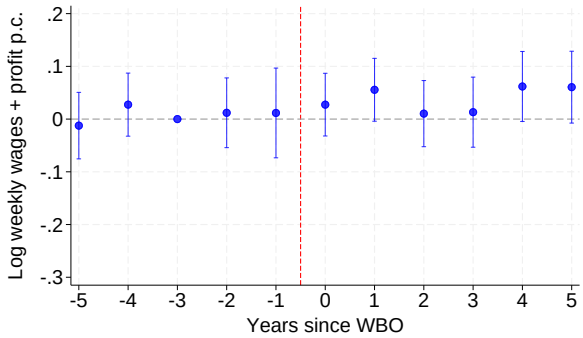
Note: WBO firms (blue solid line) vs. comparison firms (red dotted line). For each elapsed time k, the numerator is the number of firms active at period k, and the denominator is the number of firms that were active at the baseline period $k = -3$. For the Cerved-Orbis sample in Panel (b), survival rates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

Figure B2: Effects on wages. Matching specification 2

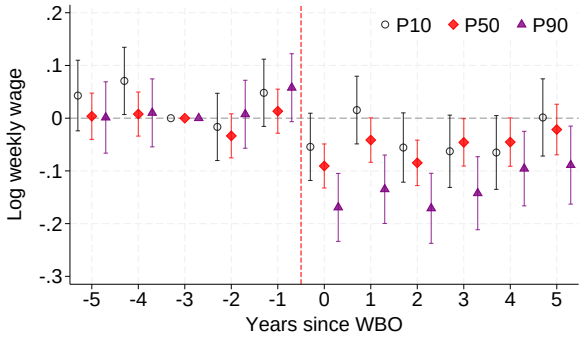
(a) Log of average wage



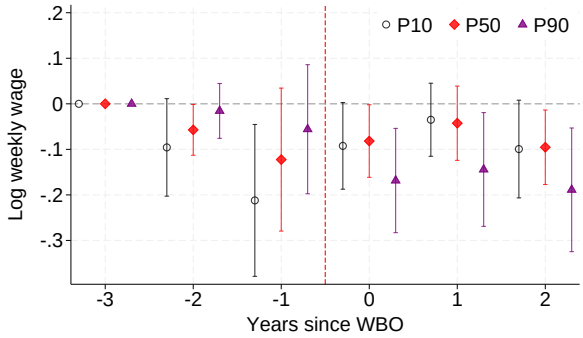
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



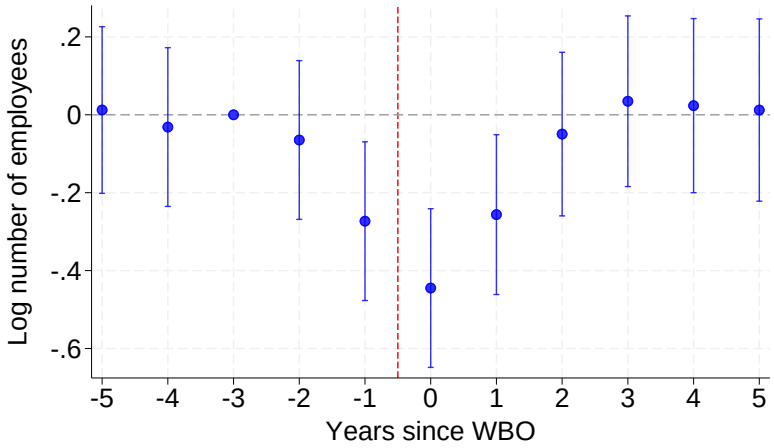
(d) Log of wage percentiles for stayers



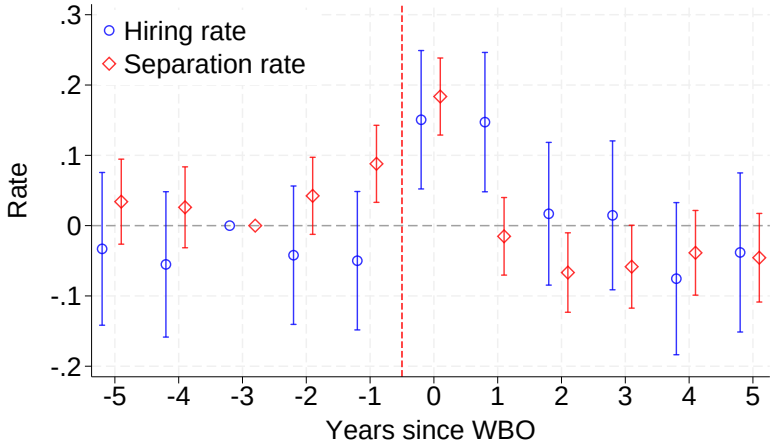
Note: Panel (a) presents the point estimates of the event study specification on the log of the average wage, with 95% confidence intervals. Panel (b) presents the point estimates for total compensation estimated using the subsample of firms in the period 1996–2021 for which financial statements are available. Total compensation is computed as weekly wages plus a wage-weigthed share of profits. 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 3, but with matching specification 2.

Figure B3: Effects on employment. Matching specification 2

(a) Log number of full-time employees



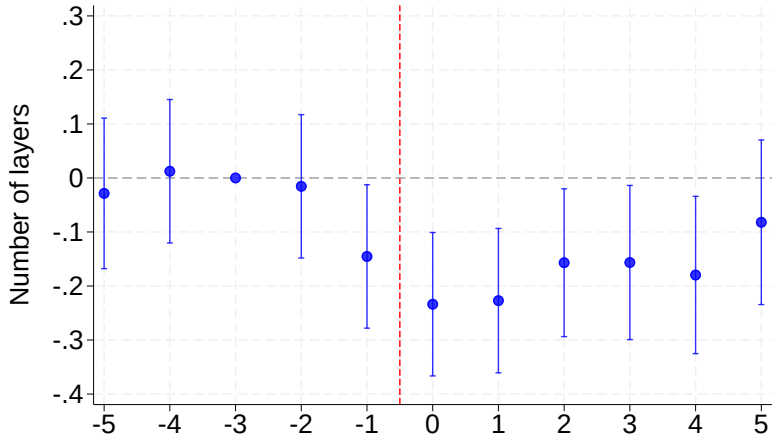
(b) Hiring and separation rates



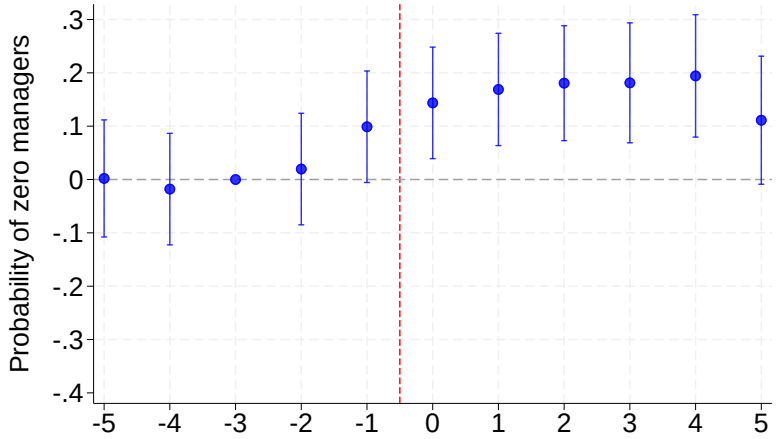
Note: Panel (a) presents the point estimates of the event study specification on the log of full-time equivalent employees, with 95% confidence intervals. Panel (b) presents results on the hiring and separation rates, indicated by blue hollow circles and red hollow diamonds, respectively. The hiring 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. This figure is the same as Figure 4, but with matching specification 2.

Figure B4: Effects on firm hierarchy. Matching specification 2

(a) Number of layers

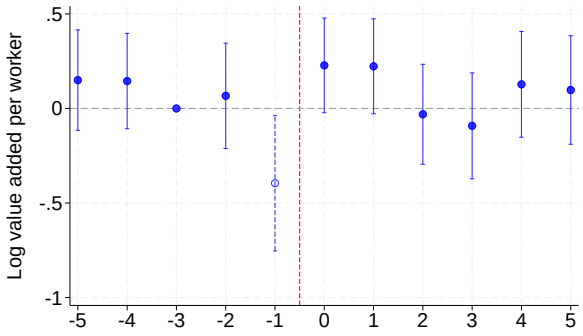


(b) Probability of no managerial layer

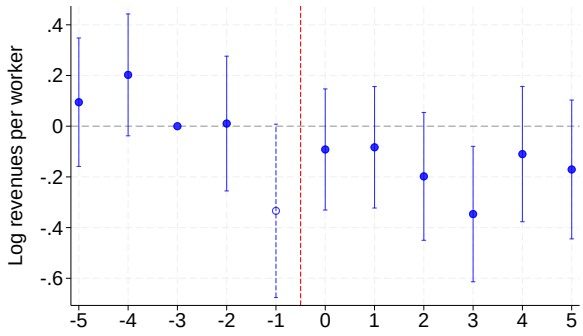


Note: Panel (a) presents the point estimates of the event study specification on the number of layers in the firm, with 95% confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well. Panel (b) presents the point estimates for the probability of having no managerial layer. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 5, but with matching specification 2.

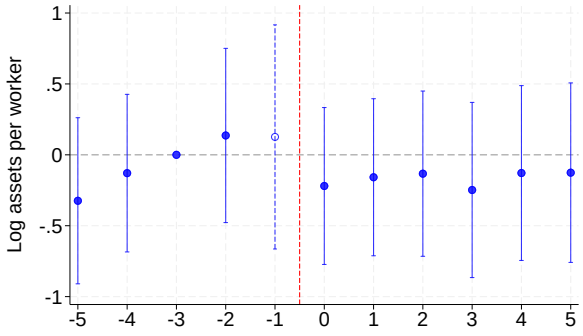
Figure B5: (1996-2021) Effects on income statement and balance sheet outcomes. Matching specification 2.



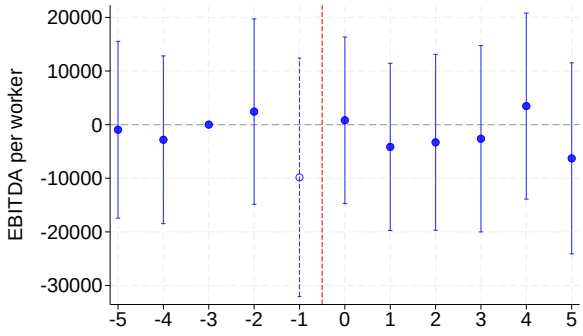
(a) Log of value added per worker



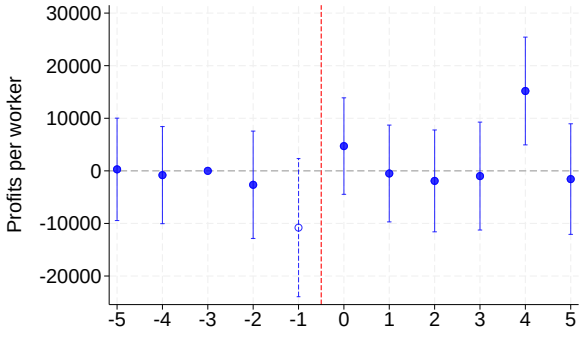
(b) Log of sales per worker



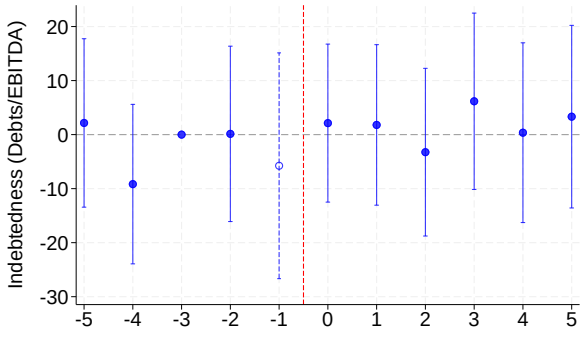
(c) Log total assets per worker



(d) EBITDA per worker



(e) Profits per worker

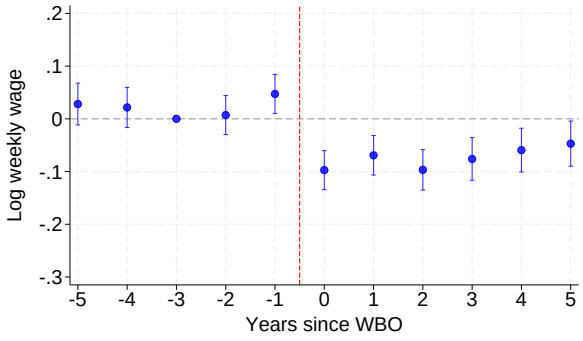


(f) Indebtedness (EBITDA/Debts)

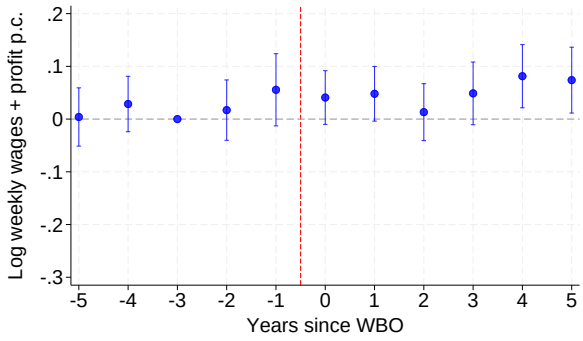
Note: Panel (a) presents the point estimates of the event study specification on the log of value added per worker, with 95% confidence intervals. Panel (b) presents the point estimates for the log of sales (revenues) per worker, and Panel (c) presents results for the log of assets per worker. Panel (d) presents results on 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 6, but with matching specification 2.

Figure B6: Effects on wages. Matching specification 3

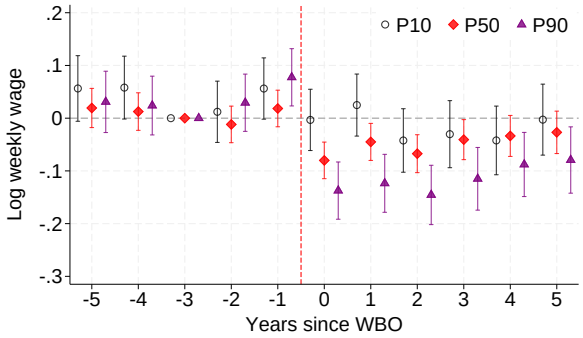
(a) Log of average wage



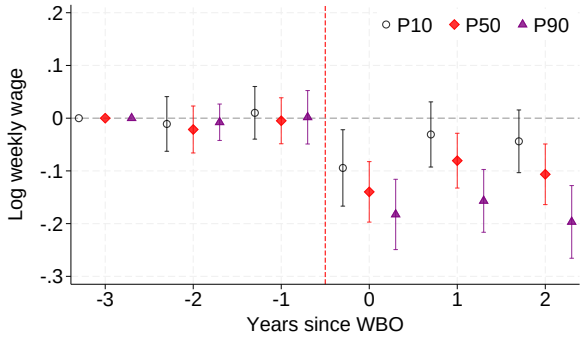
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



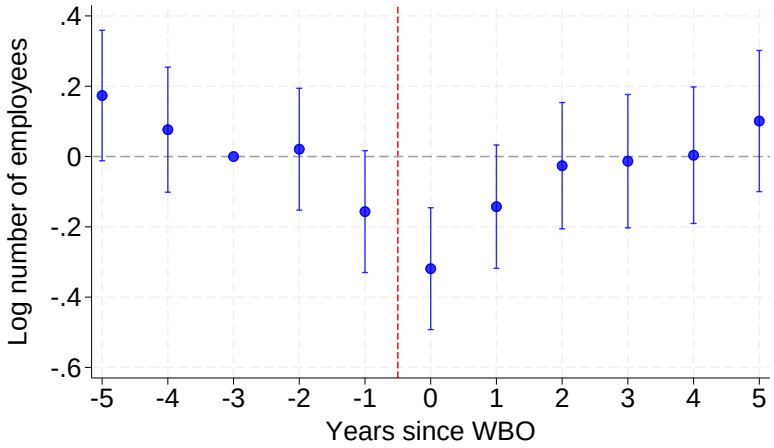
(d) Log of wage percentiles for stayers



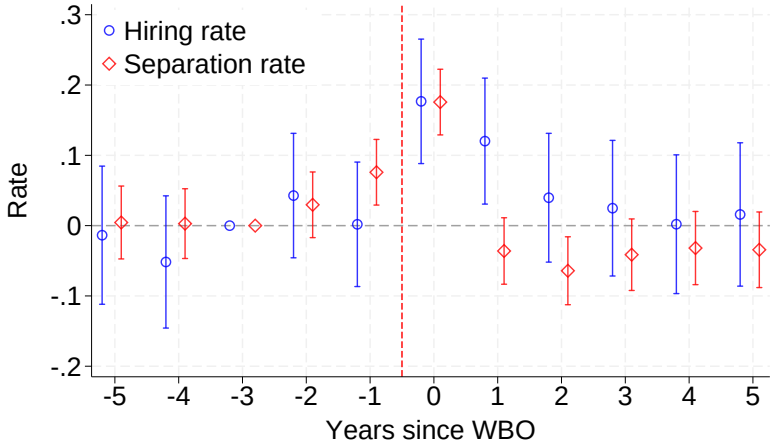
Note: Panel (a) presents the point estimates of the event study specification on the log of the average wage, with 95% confidence intervals. Panel (b) presents the point estimates for total compensation estimated using the subsample of firms in the period 1996–2021 for which financial statements are 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 3, but with matching specification 3.

Figure B7: Effects on employment. Matching specification 3

(a) Log number of full-time employees



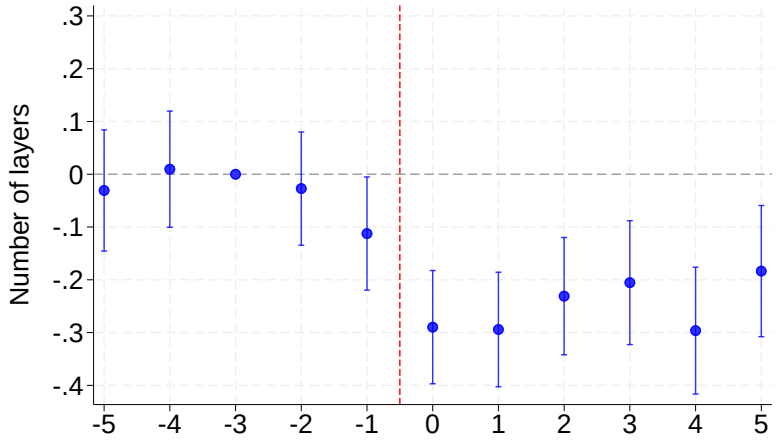
(b) Hiring and separation rates



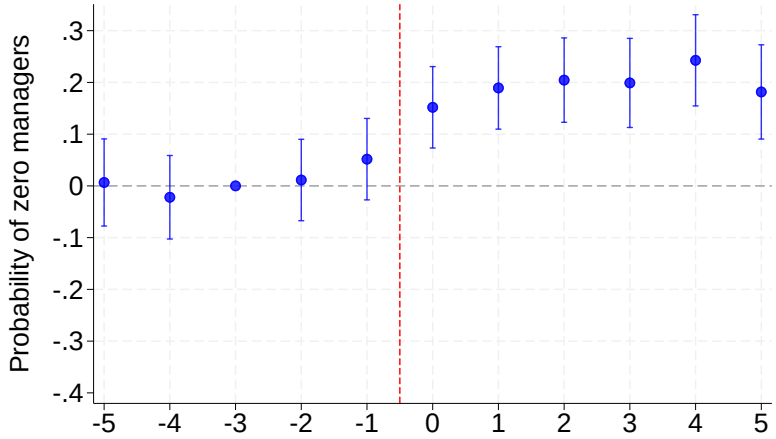
Note: Panel (a) presents the point estimates of the event study specification on the log of full-time equivalent employees, with 95% confidence intervals. Panel (b) presents results on the hiring and separation rates, indicated by blue hollow circles and red hollow diamonds, respectively. The hiring 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. This figure is the same as Figure 4, but with matching specification 3.

Figure B8: Effects on firm hierarchy. Matching specification 3

(a) Number of layers



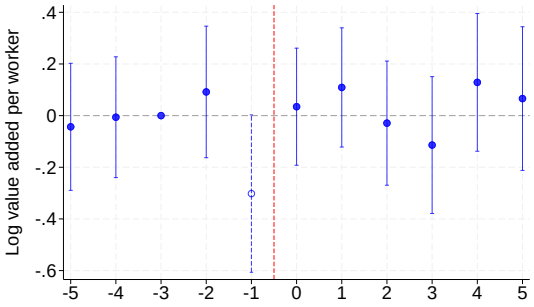
(b) Probability of no managerial layer



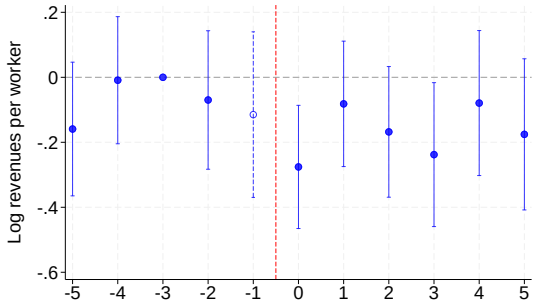
Note: Panel (a) presents the point estimates of the event study specification on the number of layers in the firm, with 95% confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well. Panel (b) presents the point estimates for the probability of having no managerial layer. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 5, but with matching specification 3.

Figure B9: (1996-2021) Effects on income statement and balance sheet outcomes. Matching specification 3.

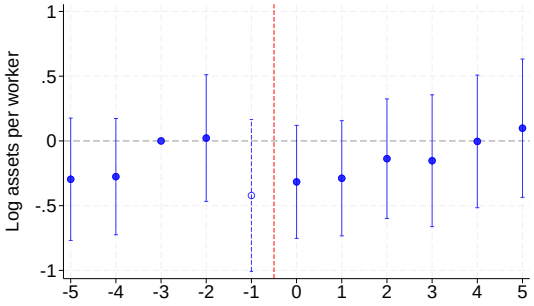
(a) Log of value added per worker



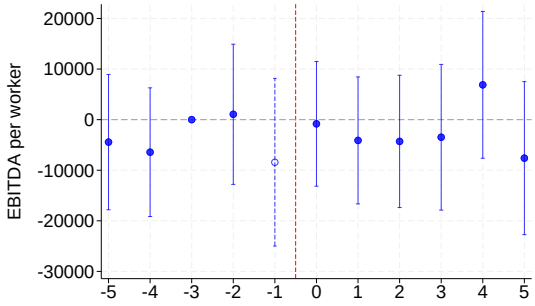
(b) Log of sales per worker



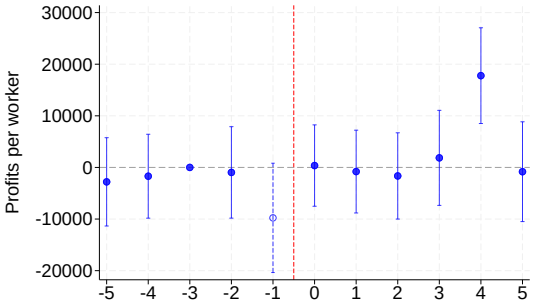
(c) Log total assets per worker



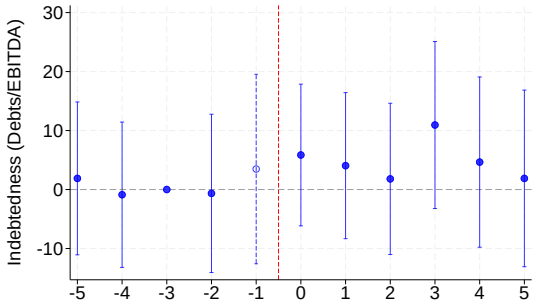
(d) EBITDA per worker



(e) Profits per worker



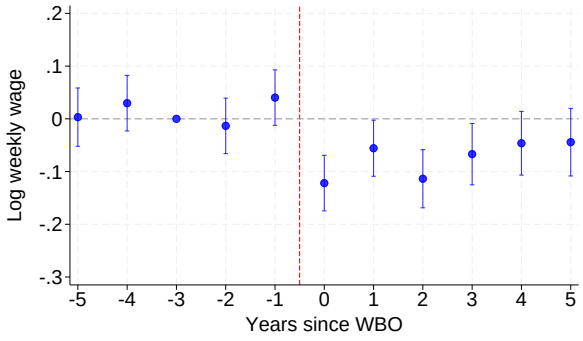
(f) Indebtedness (EBITDA/Debts)



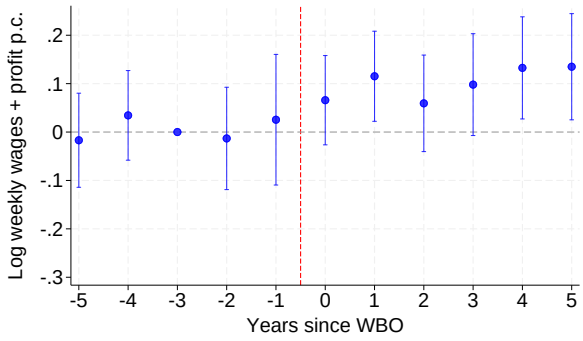
Note: Panel (a) presents the point estimates of the event study specification on the log of value added per worker, with 95% confidence intervals. Panel (b) presents the point estimates for the log of sales (revenues) per worker, and Panel (c) presents results for the log of assets per worker. Panel (d) presents results on 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 6, but with matching specification 3.

Figure B10: Effects on wages. Matching specification 4

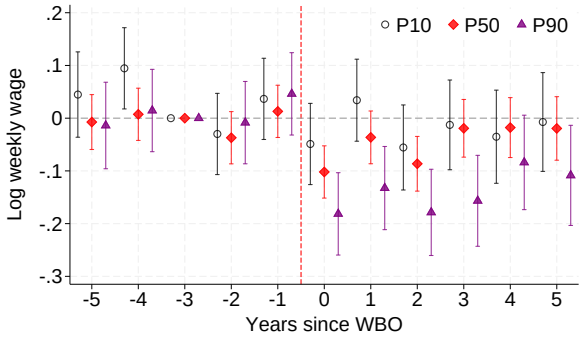
(a) Log of average wage



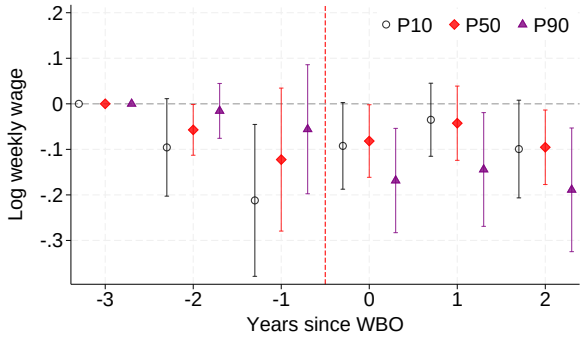
(b) 1996–2021: Log of average wages adjusted for profits



(c) Log of wage percentiles



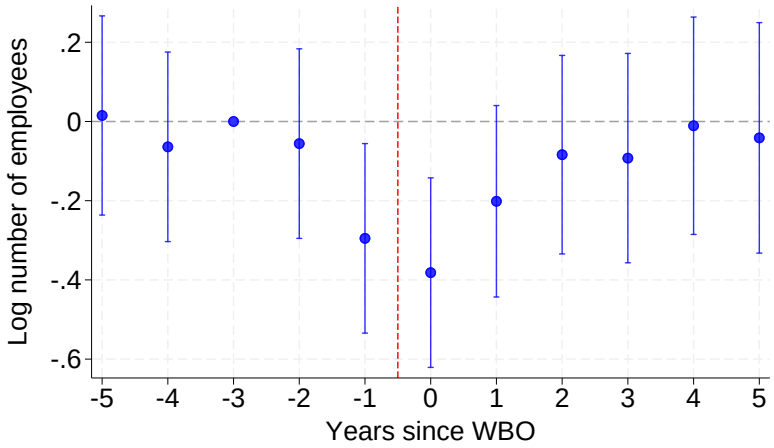
(d) Log of wage percentiles for stayers



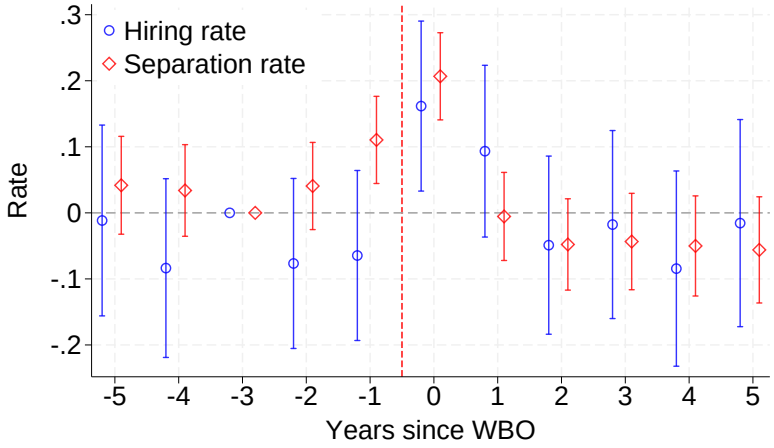
Note: Panel (a) presents the point estimates of the event study specification on the log of the average wage, with 95% confidence intervals. Panel (b) presents the point estimates for total compensation estimated using the subsample of firms in the period 1996–2021 for which financial statements are 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 3, but with matching specification 4.

Figure B11: Effects on employment. Matching specification 4

(a) Log number of full-time employees



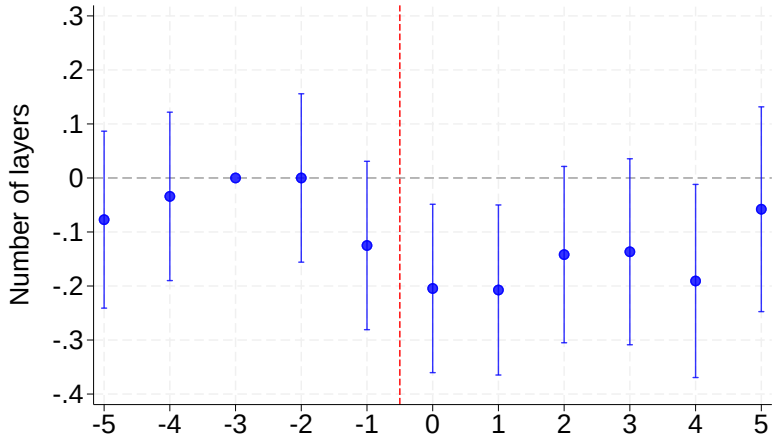
(b) Hiring and separation rates



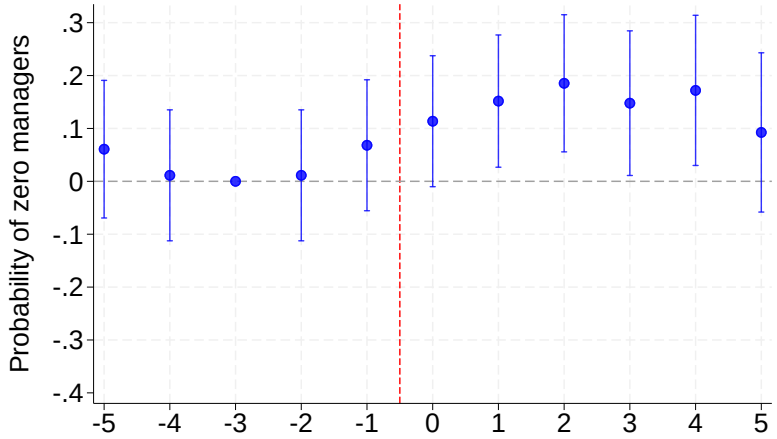
Note: Panel (a) presents the point estimates of the event study specification on the log of full-time equivalent employees, with 95% confidence intervals. Panel (b) presents results on the hiring and separation rates, indicated by blue hollow circles and red hollow diamonds, respectively. The hiring 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. This figure is the same as Figure 4, but with matching specification 4.

Figure B12: Effects on firm hierarchy. Matching specification 4

(a) Number of layers



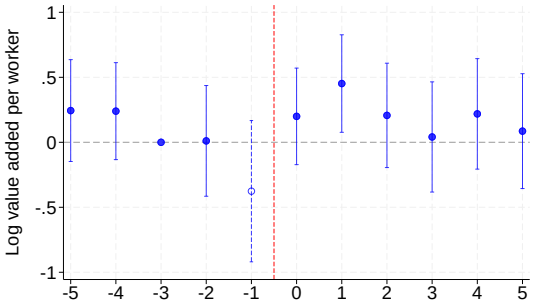
(b) Probability of no managerial layer



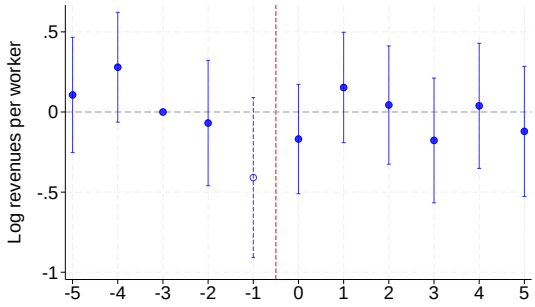
Note: Panel (a) presents the point estimates of the event study specification on the number of layers in the firm, with 95% confidence intervals. A firm has one layer if it only employs blue-collar or white-collar workers, two layers if it employs both, and three layers if it employs managers as well. Panel (b) presents the point estimates for the probability of having no managerial layer. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 5, but with matching specification 4.

Figure B13: (1996-2021) Effects on income statement and balance sheet outcomes. Matching specification 4.

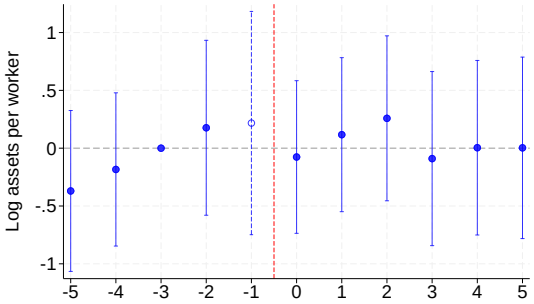
(a) Log of value added per worker



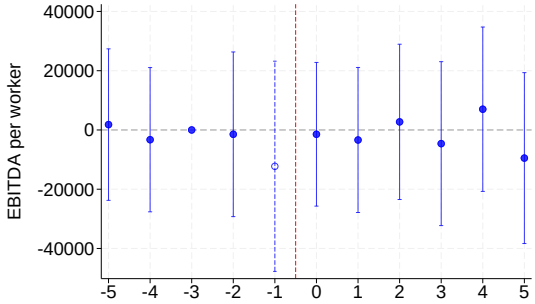
(b) Log of sales per worker



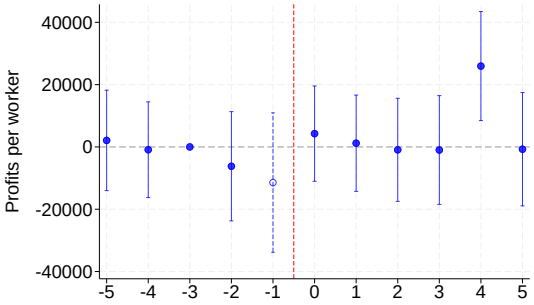
(c) Log total assets per worker



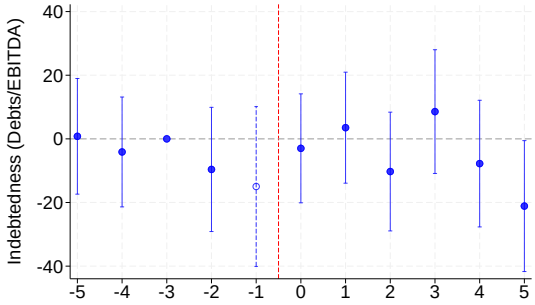
(d) EBITDA per worker



(e) Profits per worker

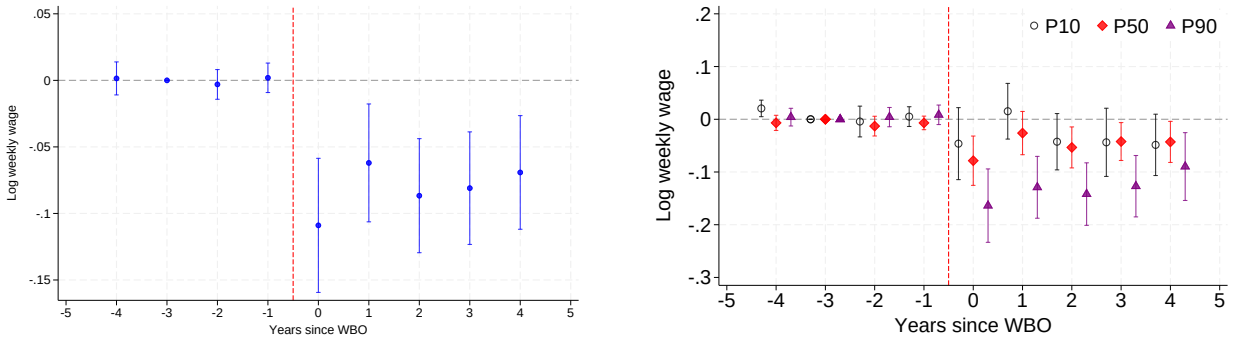


(f) Indebtedness (EBITDA/Debts)



Note: 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, and Panel (c) presents results for the log of assets per worker. Panel (d) presents results on 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. The event study specification includes year and firm-by-matched-group fixed effects. Standard errors are clustered at the firm level. This figure is the same as Figure 6, but with matching specification 4.

Figure B14: Effects on wages. Synthetic difference-in-differences.

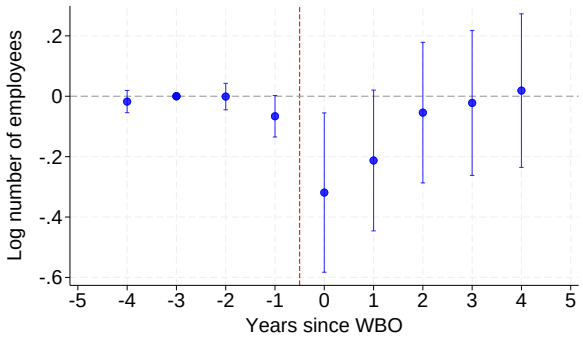


(a) Log of average wage

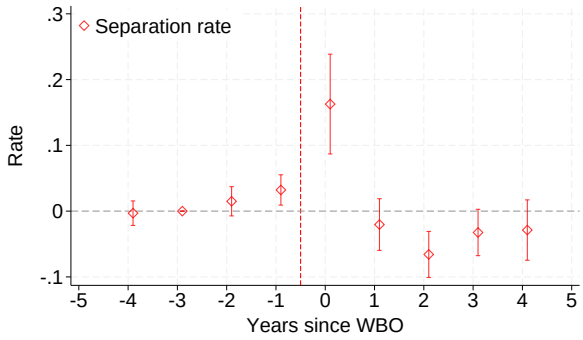
(b) Log of wage percentiles

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 500 replications and resampling is at the firm-level. Back to Section 4.5.

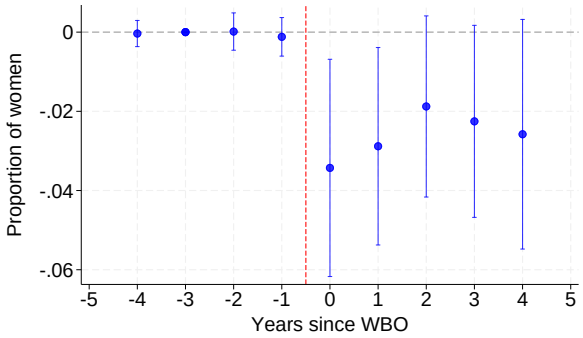
Figure B15: Effects on employment. Synthetic difference-in-differences.



(a) Log of full-time equivalent employees



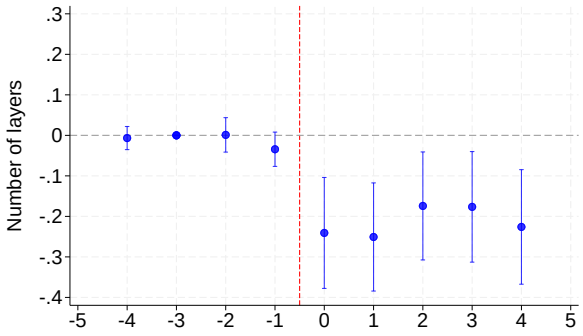
(b) Separation rate



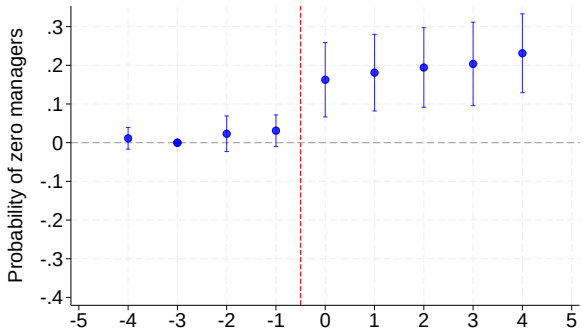
(c) Proportion of female employees

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 female employees. Bootstrap standard errors are computed using 500 replications and resampling is at the firm-level. Back to Section 4.5.

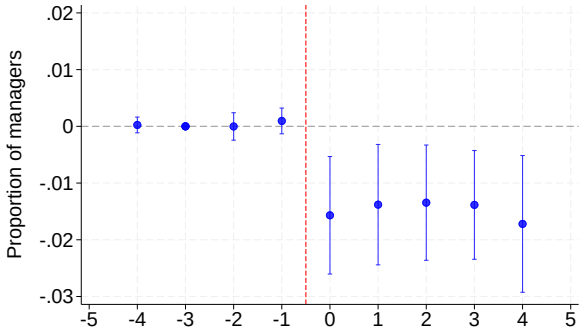
Figure B16: Effects on firm hierarchy. Synthetic difference-in-differences.



(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 the 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 500 replications and resampling is at the firm-level. Back to Section 4.5.

Appendix C Prevalence of labor-managed firms

A labor-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 labor-managed firms, but not the only one.

Italy. 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, including worker cooperatives but also other types of cooperatives, had 1,049,409 employees (7% of the total).[37]

Spain. 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. [38]

France. 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.[39,40]

Argentina. 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.[41]

[37] 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

[38] 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.

[39] 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

[40] 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.

[41] 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.