THE EFFECTS OF MANDATORY PROFIT-SHARING ON WORKERS AND FIRMS: EVIDENCE FROM FRANCE*

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Since 1967, all French firms with more than 100 employees have been required to share a fraction of their excess profits with their employees. Through this scheme, firms with excess profits distribute, on average, 10.5% of their pretax income to workers. In 1990, the eligibility threshold was reduced to 50 employees. We exploit this regulatory change to identify the effects of mandated profit-sharing on firms and their employees. The cost of mandated profit-sharing for firms is evident in the significant bunching at the 100-employee threshold observed prior to the reform, which completely disappears post-reform. Using a difference-in-differences strategy, we find that at the firm level, mandated profit-sharing (i) increases the labor share by 1.8 percentage points, (ii) reduces the profit share by 1.4 percentage points, and (iii) has small to nonexistent effects on investment and productivity. At the employee level, mandated profit-sharing increases lower-skilled workers' total compensation and leaves high-skilled workers' total compensation unchanged. Overall, mandated profit-sharing redistributes excess profits to lower-skilled workers in the firm without generating significant distortions or productivity effects. *JEL codes:* J31, J33, M52.

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I. Introduction

Public policies favoring profit-sharing between firm owners and employees are pervasive around the world.[1] These schemes raise two important questions. The first one is an incidence question: do they really benefit workers, or are employers simply substituting fixed wages for profit-sharing? The second one relates to their economic effects: do they foster firm productivity by aligning the incentives of owners and employees? Do they reduce corporate investment by decreasing the returns to capital?

Answering these questions is challenging. First, profit-sharing adoption is an endogenous decision, which complicates causal inference in observational data. For instance, firms expecting large gains may be more likely to introduce profit-sharing, and workers anticipating future productivity gains may lobby for its adoption. Both channels would create an upward bias in the observed relationship between productivity, wages, and profit-sharing. So far, the literature mostly relies on observational and cross-sectional evidence (e.g., Blasi et al. 2008). Some studies use firm fixed effects to absorb unobserved heterogeneity (e.g., Wadhwani and Wall 1990; Kruse 1992; Bell and Neumark 1993), and others model adoption explicitly using Tobit selection models with firm characteristics as instruments (e.g., FitzRoy and Kraft 1987; Cahuc and Dormont 1997). A notable exception is Knez and Simester (2001), which studies Continental Airlines' introduction of performance-based bonuses in 1995.

The second challenge is data availability: information about profit-sharing has to be combined with firm-level accounting data. As a result, most studies have relied on small samples, limiting the quality of inference (e.g., FitzRoy and Kraft 1987 focuses on 65 medium-sized metalworking firms in West Germany).

This article contributes to our understanding of profit-sharing by combining a credible identification strategy with large-scale administrative data. In 1967, France introduced a requirement for firms with more than 100 employees to redistribute part of their "excess profits"—profits above 5% of book equity—to employees. The goal was to align the interests of workers and shareholders, thereby raising productivity and reducing labor conflict. The law introduced a formula, still used today, that determines employees' profit share based on observable accounting data and effectively

1. For a review of profit-sharing schemes in OECD countries, see OECD (1995).

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acts as a substantial tax on profits. Absent behavioral responses, a firm with a labor share of two-thirds and a return on equity of 10% has to transfer 10.5% of pretax profits from shareholders to workers. In 1990, the threshold was lowered from 100 to 50 employees. We exploit this reform using two administrative sources: the universe of corporate tax returns, which records annual profit-sharing payments, and a matched employer-employee panel covering about 4% of private-sector workers in France.

Our empirical analysis proceeds in three steps, corresponding to the three main questions of the article. First, we ask whether mandated profit-sharing is costly for firms and who ultimately bears that cost. Whether firms perceive the regulation as a net cost or benefit is a priori unclear. Profit-sharing may boost productivity, and the scheme also carries tax advantages for both firms and workers. If firms can offset mandated profit-sharing by lowering wages while keeping workers' net-of-tax compensation unchanged, mandated profit-sharing would benefit firms at the expense of the tax authority.[2] If wages adjust only partially, however, mandatory profit-sharing raises total compensation and may impose a net cost on firms, especially if productivity gains are limited.

We study firms' willingness to pay to avoid mandated profit-sharing using a bunching analysis. Importantly, profit-sharing is the only regulation that starts at 100 employees.[3] Before 1991, we find substantial bunching just below 100 employees. This bunching disappears quickly after the threshold is lowered to 50 employees. Using the post-reform distribution as a counterfactual, we estimate an excess density of 24.8% between 95 and 99 employees. We therefore reject the hypothesis that firms perceive profit-sharing as a net benefit, despite its tax advantages. This interpretation holds whether bunching reflects real employment reductions (Garicano, Lelarge, and Van Reenen 2016; Aghion, Bergeaud, and Van Reenen 2021) or underreporting (Askenazy, Breda, and Pecheu 2022), as long as underreporting is costly. As we discuss in Section IV.B, the evidence suggests that most bunching in our

2. While French law prevents employers from unilaterally cutting nominal wages (Article 1134 of the Civil Code), profit-sharing may slow the growth of real wages, especially in a period of positive inflation such as 1991–1997, when inflation averaged above 2%.

3. Many French regulations bind at 50 employees, such as the requirement to establish a workers' council. No comparable regulation starts at 100 employees (Garicano, Lelarge, and Van Reenen 2016).

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setting reflects actual employment reductions rather than misreporting. This finding suggests that firms have limited ability to shift the cost of profit-sharing to workers through lower wages and that any productivity gains are smaller than the induced cost.

We confirm this interpretation with a firm-level intent-to-treat difference-in-differences design. The treatment group consists of firms with 55–85 employees in 1989–1990, which would become subject to profit-sharing in 1991 absent employment adjustments. We exclude firms with 50–55 employees, which can easily avoid the mandate after the reform, and firms with 85–100 employees, many of which are pre-reform bunchers and may differ systematically from other firms. We use two control groups: firms with 120–300 employees, likely covered before and after the reform, and firms with 35–45 employees, likely never covered.[4]

Our identifying assumption is that without the reform, outcomes such as labor shares and productivity would have evolved similarly in the treatment and two control groups. Consistent with this assumption, we show parallel pre-trends for all outcomes. Using both smaller and larger firms as controls is a key feature of our design, as it mitigates concerns that our estimates capture differential sensitivity to aggregate shocks by firm size.

We find that workers capture the full gain from mandated profit-sharing and that the cost is borne by firm owners and the tax authority. Among firms with positive excess profits, the mandate raises the total compensation share (i.e., wage bill plus profit-sharing divided by value added) by 1.8 percentage points. We estimate that 77% of this increase comes at the expense of shareholders, as the profit share falls by 1.38 percentage points. The remainder is borne by the tax authority through lower corporate income tax receipts, as profit-sharing reduces taxable profits. By contrast, the wage share is unchanged, suggesting that profit-sharing does not reduce wages. We return to this result later using worker-level data.

Second, we study whether mandated profit-sharing affects firm behavior. We begin with firm-level productivity. Across a wide range of standard productivity measures, we consistently estimate effects close to zero. For most measures, our intent-to-treat estimates rule out productivity effects larger than 1% in absolute value. We also find no effect on "softer" proxies for effort and pro-

4. We exclude firms in the 45–50 employee range because they also bunch around the threshold.

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ductivity, such as sick leave and the probability of working extra hours. These results contrast with the existing empirical literature, which typically finds productivity gains of 3%–5% from the endogenous adoption of profit-sharing.[5] One possible interpretation is that profit-sharing payments are too small to meaningfully affect worker incentives. This explanation appears unlikely in our setting. The French scheme is sizable: in our data, mandated profit-sharing represents a transfer to employees of about 10.5% of firms' pretax profits. In addition, treated firms are relatively small—55 to 85 employees—which should limit standard free-riding concerns.

If profit-sharing does not raise productivity, it may still discourage investment by lowering the return to capital. We find only small and mostly insignificant negative effects on investment and the capital-labor ratio. For example, our baseline estimates rule out an investment rate decline greater than 0.0057. As a simple model makes clear, this finding is not surprising. The formula applies to excess profits—earnings above 5% of book equity—rather than total profits. As long as a firm's return on equity (ROE) is not too far from 5%, the implied wedge on the return to capital remains small, limiting distortions to investment. Overall, our results suggest that mandated profit-sharing, as implemented in France, creates few distortions for firms.

Third, we examine how the effects of mandated profit-sharing are distributed across workers in the firm, using linked employer-employee data covering 4% of private-sector workers in France. We first replicate our core finding: workers in firms subject to the mandate do not experience lower base wages, while their total compensation rises by about 3.5%. The absence of wage incidence could reflect either workers' aversion to risky pay or downward wage rigidity. We find little support for the risk channel, as profit-sharing only slightly increases the volatility of workers' earnings. Instead, the evidence is more consistent with wage rigidity. Given the binding minimum wage and widespread collective bargaining in France, wages are likely more rigid for low- and middle-skill workers (e.g., Gautier, Roux, and Castillo 2022). Consistent with this view, the increase in total compensation is concentrated among workers at the lower end of the skill distribution—blue-collar workers, clerks, supervisors, and skilled technicians.

5. See FitzRoy and Kraft 1987; Wadhwani and Wall 1990; Kruse 1992; Cahuc and Dormont 1997; Prendergast 1999; Doucouliagos et al. 2020.

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By contrast, among managers, engineers, and executives, profit-sharing reduces base wages enough to leave total compensation unchanged. These findings point to wage rigidity as an important mechanism behind our results and show that mandated profit-sharing is progressive in firms, primarily benefiting lower-skill workers.

Our analysis of a large mandatory profit-sharing scheme is especially relevant in the current context of rising concern about distribution. The labor share has fallen in most industrialized countries (Karabarbounis and Neiman 2014), the share of pre-tax real income accruing to the bottom 50% in the United States has stagnated since 1980 (Piketty, Saez, and Zucman 2018), and recent work points to growing monopsony power in local labor markets (Stansbury and Summers 2020; Berger, Herkenhoff, and Mongey 2022). Against this backdrop, our findings suggest that mandated profit-sharing, as implemented in France, can serve as a redistributive tool with limited efficiency costs, shifting rents from shareholders to lower-skill workers. This redistribution is not costless, however: each dollar transferred to workers reduces corporate tax revenue by about 20 cents.[6] It is also uneven, since it benefits workers in profitable firms, and excess profits are highly persistent in the data.

Our quasi-experimental approach contributes to the profit-sharing literature by identifying the causal effects of mandated profit-sharing on worker compensation and firm outcomes. It also relates to the literature on employee stock ownership plans, a common but voluntary form of profit-sharing in U.S. firms (see Matsa 2018 for a review). In particular, Kim and Ouimet (2014) find that employee stock ownership plan adoption raises productivity in small firms. The difference between their findings and ours may reflect either selection into voluntary adoption or differences between voluntary and mandated profit-sharing schemes. Our article is also related to Tolentino (2022), who studies mandated profit-sharing in Peru by exploiting a 20-employee eligibility threshold and finds larger effects on investment and productivity than we do. One possible explanation is that the Peruvian scheme applies to all posttax profits, whereas the French scheme applies only to profits above a 5% return on book equity. In addition, Tolentino (2022) documents bunching at the 20-employee

6. A full fiscal evaluation would also require analyzing the implications for personal income tax, which we leave for future work.

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threshold even before the reform, suggesting that other regulations may contribute to the estimated distortions.

This article also relates to the literature on rent-sharing and labor-market institutions. Kline et al. (2019) show that firm-level rent shocks are partially passed through to workers, consistent with imperfect competition in labor markets. DiNardo and Lee (2004) use a regression discontinuity design to study unionization and find small, insignificant effects on wages and productivity.[7] Jäger, Schoefer, and Heining (2021) study German codetermination and find no effect on wages, even in firms with more flexible pay setting. Relative to these institutions, the mandated profit-sharing scheme we study generates a sizable increase in workers' total compensation and in labor's share of value added.

The rest of the article is organized as follows. Section II provides institutional details and a conceptual framework. Section III describes the data. Section IV analyzes bunching below the 100-employee threshold before the 1990 reform. Section V presents the firm-level difference-in-differences analysis. Section VI studies heterogeneity by skill using employer-employee data. Section VII concludes.

II. Institutional Setting and Economic Framework

We first discuss the institutional details of profit-sharing in France, as well as the 1990 reform that we use for identification. We develop a simple model to analyze the economic implications of profit-sharing.

II.A. Institutional Details

On August 17, 1967, Charles de Gaulle signed an executive order mandating all firms with more than 100 employees to distribute a share of their profits to their employees. Redistributive concerns were at the heart of this decision. Anticipating the opening of the common market and the ensuing increase in foreign competition, firms were engaging in massive investment to modernize their capital stock (Lasserre 1968). Without external financing, firms had to generate large internal funds to finance these investments, which they partly achieved by limiting workers' wage growth. In this context, mandated profit-sharing was

7. See also Lee and Mas (2012), who exploit close union elections and find negligible effects of union representation on shareholder value.

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viewed as a way to allow workers to benefit from these investments without reducing the internal funds available to companies.

In practice, the law requires firms to set aside, every year, a positive amount \$RSP to be distributed to employees.[8] This amount is determined according to the following formula:

$$(1) \quad \$\text{RSP} = \frac{1}{2} \times \frac{\text{Wage bill}}{\text{Value added}} \times \left(\underbrace{\text{Net income} - 5\% \text{ Book equity}}_{\text{excess-profits}} \right)^{+}.$$

The formula has a simple intuition. Five percent was the lawmakers' perception of fair compensation to shareholders. Every additional dollar of profit above this compensation (what we call excess profits) should be split between shareholders and their employees. The sharing rule is scaled by the firm's labor share to reflect the contribution of labor in production.

Note that this scheme redistributes a quantitatively large fraction of profits. This can be seen from a simple calibration of equation (1). The ratio of RSP to pretax income is given by:

$$\frac{RSP}{\text{Pre-Tax Income}} = \frac{1}{2} \times \frac{\text{Wage bill}}{\text{Value added}} \times (1 - \tau) \times \left(1 - \frac{5\%}{ROE} \right),$$

where $ROE = \frac{\text{Net Income}}{\text{Book Equity}}$ is the firm's return on equity and τ is the corporate income tax rate. For the median firm in our sample period, with a ROE of 12%, a labor share of 0.56, and given a corporate income tax rate τ of 37% in 1990, profit-sharing would amount to approximately 10.2% of pretax income. In the data, firms with positive excess profits and subject to mandatory profit-sharing distribute, on average, 10.5% of pretax income after 1991.[9]

The distribution of \$RSP to each individual employee is typically proportional to their current wage (DARES 2008). There is a cap on how much an employee can receive in a given year. In 1990, this cap was equivalent to 63% of the average net wage in the private sector.[10] The scheme also has several tax implications. On the employee side, employees have the option to defer receiving their

8. RSP stands for Réserve Spéciale de Participation, or special profit-sharing fund.

9. This number is higher than our calibration in part because the corporate income tax was reduced to 33% after 1993.

10. Note, however, that when the cap is binding, the firm still has to set aside \$RSP, but employees only receive their share in later years when the cap is no longer binding.

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profit-sharing income for five years, in which case it is exempt from personal income tax. On the employer side, profit-sharing is exempt from payroll tax, in contrast to regular wages. As a result, employers have a fiscal incentive to prefer profit-sharing over regular wages. This incentive is a priori quite attractive as payroll taxes in France are among the highest in the world. Note also that like wages, profit-sharing reduces firms' corporate income tax: For fiscal purposes, profit-sharing is treated as a cost in firms' taxable income.

A firm is required to implement profit-sharing if, during the past year, there were six months or more during which the end-of-month head count was greater than 100. Firms under this threshold could still voluntarily establish a profit-sharing fund and benefit from the same fiscal advantages. In practice, however, very few firms below the 100 threshold did so.[11] On October 17, 1990, the left-leaning French parliament voted to extend the coverage of mandatory profit-sharing by lowering the threshold to 50 employees. The law was officially amended on November 9, 1990.[12] Importantly for our identification strategy, while many firm-level regulations kick in at 50 employees (most notably, the requirement to establish a council of worker representatives) (Garicano, Lelarge, and Van Reenen 2016), profit-sharing was the only regulation based on the 100-employee threshold before 1990.

The law still applies today. There have been only a few changes to the scheme, and all these changes happened outside of our sample period (1985–1997). Most notably, the cap limiting how much employees can receive from profit-sharing was increased by 25% in 2007. Since 2009, firms have been required to pay payroll taxes on the income paid to workers through profit-sharing. The rate was set at 2% in 2009 and increased to 20% in 2012. Interestingly, the formula used to compute \$RSP (equation (1)) is still the original formula from the 1967 law. In 2019, 5.3 million workers (about 36% of the workforce) received income from this profit-sharing scheme, for an average annual amount of €1,499 or about 3.8% of recipients' wages (Briand 2021).

11. In 1989, among firms in the 35–85 employees range, 6.8% of firms paid some profit-sharing to their workers.

12. Beyond the change in the eligibility threshold, the only other modification made by the new law was to allow firms to use a floor wage when calculating the share of profits distributed to employees from the RSP (Journal Officiel 1990).

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II.B. Economic Framework

We develop a simple augmented user cost model to analyze how mandated profit-sharing affects firm behavior. The objective is to study how firms' input choices and outcomes are affected by profit-sharing, in particular as a function of the incidence of profit-sharing on workers' wages. We take this incidence parameter as exogenous. The model has two periods and is in partial equilibrium. In the first period, the company purchases capital k by borrowing debt d and bringing equity $e = k - d$. In the second period, the firm hires l workers and combines them with capital k to generate revenues $y = F(k, l)$. In what follows, we assume that F is Cobb-Douglas with possibly decreasing returns to scale $F(k, l) = \left(k^{\alpha} l^{1-\alpha}\right)^{\theta}$, although our results hold for more generic production functions.

Workers receive $wl + RSP$, where RSP corresponds to the total amount of profit-sharing mandated by law and w is their wage.[13] The firm also pays back $(1 + r_d)d$ to debt holders, where r_d is the interest on debt. After production, the capital stock depreciates and the firm sells it for $(1 - \delta)k$. We assume the price of capital is the price of the produced good, which we normalize to one. Finally, the firm faces a tax rate τ on its accounting profits, so that it pays a corporate income tax of $\mathcal{T} = \tau \left(y - wl - r_d d - \delta k - RSP\right)$.[14]

Shareholders select the equity they bring, e, and the capital stock to purchase, k, to maximize the present value of their cash flow, which they discount at the rate r_e. Finding the optimal amount of debt and equity would require a theory of capital structure. Although interesting, the interplay between capital structure and profit-sharing is beyond the scope of this exercise. We thus assume that the firm's capital structure is fixed and denote the constant equity-to-assets ratio as $\phi = \frac{e}{k}$. This assumption of a fixed capital structure allows us to assume that r_e and r_d are independent of k. We define $r = (1 - \tau) r_d \frac{d}{k} + r_e \frac{e}{k}$, the firm's weighted

13. Note that to simplify exposition, we do not model payroll taxes. Because RSP is exempt from payroll taxes, mandated profit-sharing could reduce the marginal cost of labor and thus increase employment and investment. The model neglects this effect and can be seen as an upper bound on the negative effect of mandated profit-sharing on employment and investment.

14. In France, like in the United States, firms benefit from a depreciation tax shield and a debt tax shield. Also note that RSP can be deducted from taxable income.

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average cost of capital (WACC), which is then also independent of k.

To analyze the effect of profit-sharing, we make three more assumptions. First, we assume that RSP, the amount of shared profits, is determined by a slightly simplified version of equation (1):

$$RSP = \gamma\left((1-\tau)(F(k,l) - wl - \delta k - r_d d) - \rho \times e\right), \tag{2}$$

where ρ is the cost of equity in the regulatory formula (5% in the French system). The simplification is that γ is assumed constant, while in the actual regulation, it is half the firm's labor share: $\gamma = \frac{1}{2} \times \frac{wl}{y}$. We ignore such dependence for clarity.[15] The second assumption is on the incidence of profit-sharing on the wages paid to workers. We assume that $w = w^* - \lambda \frac{RSP}{l}$, where w^* is the wage without profit-sharing.[16] When $\lambda > 0$, the firm can lower workers' wages to account for the revenue they receive from profit-sharing.[17] $\lambda = 1$ corresponds to full incidence—profit-sharing does not increase workers' total compensation. The third assumption is that profit-sharing does not increase firm productivity (i.e., F does not depend on profit-sharing). This assumption departs from traditional analyses of profit-sharing (e.g., Weitzman 1986a, 1986b), which typically focus on the positive incentives generated by profit-sharing. However, it is consistent with our empirical findings below (see Section V.D) and clarifies the core message of the model.

We can now derive predictions regarding the effects of profit-sharing on firm outcomes, input choices, and avoidance. All derivations can be found in Online Appendix B.

PREDICTION 1 (TOTAL COMPENSATION, PROFIT, AND TAX SHARE). As long as $\lambda < 1$, mandated profit-sharing leads to an increase in the share of total compensation over value added ($\frac{wl+RSP}{F(k,l)}$), a decrease in the profit share ($\frac{\text{Net Income}}{F(k,l)}$), and a de-

15. Extending the model to allow γ to be half the labor share yields similar findings.

16. The model assumes that workers are homogeneous, so they all receive the same share of profit-sharing, $\frac{RSP}{l}$.

17. In our static setting, we assume that owners might be able to reduce nominal wages to pass through some of the profit-sharing cost to workers. While the French law prevents employers from decreasing nominal wages unilaterally, they can reduce the growth of real wages dynamically, which is what our model captures.

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crease in the tax share ($\frac{\mathcal{T}}{F(k,l)}$). If $\lambda = 1$, mandated profit-sharing does not affect these outcomes.

These predictions are intuitive. Mandated profit-sharing strictly benefits workers except when firms can pass through its cost in the form of lower wages (i.e., $\lambda = 1$). As soon as $\lambda < 1$, workers' total compensation (wages plus profit-sharing) increases with mandated profit-sharing. This increase in the total compensation share comes at the expense of shareholders (through a decrease in the profit share) and the government (through a decrease in the tax share).

The model delivers two simple first-order conditions (FOCs) for labor and capital:

$$F_l(k,l) = w^* \quad \text{and}$$

$$F_k(k,l) = \underbrace{\delta + \frac{r}{1-\tau}}_{\text{standard user cost}} + \underbrace{\phi\Delta\frac{\gamma(1-\lambda)}{1-\gamma(1-\tau)}}_{\text{distortion}}, \tag{3}$$

where $\Delta = r_e - \rho$ is the difference between the firm's cost of equity and the cost of equity in the regulatory formula equation (1). Our second prediction on the distortive effect of mandated profit-sharing directly derives from these FOCs.

PREDICTION 2 (DISTORTIONS INDUCED BY MANDATED PROFIT-SHARING).

i. For a given capital stock k, mandated profit-sharing does not distort labor demand l.
ii. When $r_e \approx 5\%$ or $\lambda \approx 1$, mandated profit-sharing does not distort investment nor hiring. When $r_e > 5\%$ and $\lambda < 1$, mandated profit-sharing increases the firm's cost of capital, reduces the firm's capital stock, and thus employment.

For a reasonable calibration of the model, this distortion to the cost of capital is small, at about 0.44 percentage points, or less than 2% of the standard user cost of capital.

The absence of distortion on labor demand, conditional on k, is intuitive: labor expenses are fully deductible from the profit-sharing base (i.e., excess profits); mandated profit-sharing thus acts as a linear tax on earnings, preserving marginal incentives. In contrast, equation (3) clearly highlights the distortion on capital induced by mandated profit-sharing and how it differs from

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the corporate income tax. Like corporate taxes, profit-sharing increases the cost of capital, but only when the actual cost of equity is greater than the cost of equity in the regulatory formula (i.e., when $r_e > \rho$). When $r_e = \rho$, regulatory excess profits coincide with shareholder profits, so that, as in the new view of dividend taxation, investment is undistorted. When there is a distortion ($r_e > \rho$), it is exacerbated by a limited wage incidence, as mandated profit-sharing then represents a larger loss of profits for shareholders.

Quantitatively, we can predict how mandated profit-sharing should affect the cost of capital for treated firms (those with 55–85 employees in both 1989 and 1990) in the post-reform period. To avoid capturing behavioral responses, we measure treated firms' characteristics (ϕ, γ) immediately before the reform. We then calibrate the remaining elements of the formula using the post-reform economic environment (1991–1997). In the data, over the two years before the reform, treated firms have a median equity share ϕ of 16.6% and a median labor share of 56% so that $\gamma = \frac{1}{2}\frac{wl}{VA} \approx 0.28$.[18] During the posttreatment period, the average annual interest rate on Treasury bills in France was 7.3%. Assuming a risk premium of 5.5%, a firm with a β of one would have a cost of equity $r_e = 12.8\%$. In the post-reform data, the median interest rate paid on treated firms' debt across all firms is 10.7% and the average corporate income tax rate is 33.1%. As a result, the calibrated WACC r is 8.1%. We follow Chodorow-Reich et al. (2024) and set the depreciation rate δ to 10%. Finally, we assume that there is no wage incidence of profit-sharing, that is, $\lambda = 0$, which is consistent with our empirical findings below.

With these assumptions, we find that the pretax user cost of capital is 22.1% and the distortion term is 0.44 percentage points, so that mandated profit-sharing leads to an increase in the cost of capital of about 2%. This effect on the cost of capital should lead to a small effect on the investment of firms that become subject to mandated profit-sharing. For instance, Chodorow-Reich (2025) estimates a slope of the investment ratio to the log cost of capital of −0.424 in the United States (see his Table 2, last row). Using this elasticity in our French setting, mandated profit-sharing should lead to a decrease in the investment ratio of treated firms of about 0.85 percentage points right after the reform.

18. We calculate the equity share as the ratio of book equity to net total assets, our measure of capital.

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PREDICTION 3 (BUNCHING). Mandated profit-sharing implemented above a size threshold will generate bunching below the threshold whenever $\lambda < 1$.

When $\lambda < 1$, shareholders' profits are lower with mandated profit-sharing. Thus, if profit-sharing is mandated above a size threshold $\bar{l}$, it creates incentives for some firms to remain below the threshold. The loss from operating below the optimal size is then outweighed by the gain from avoiding profit-sharing. This prediction is driven partly by two assumptions we made in the model. First, we do not model payroll tax. Because profit-sharing is exempt from payroll tax, firms might strictly benefit from profit-sharing if wage incidence is high enough (i.e., if λ is close to one). Second, our model assumes away the possibility that mandated profit-sharing increases productivity (which is consistent with our empirical analysis). If either payroll tax exemptions or productivity gains were strong enough, firms would not bunch but would instead implement voluntary profit-sharing schemes. However, the substantial bunching below the threshold we uncover in our empirical analysis suggests that these positive effects are dominated by the direct cost of profit-sharing for shareholders.

II.C. Comparison of Profit-Sharing and Corporate Income Tax

This section compares the French mandated profit-sharing system with the corporate income tax, the other major redistributive tool transferring resources from shareholders to workers. While the effects of corporate taxation on wages and firm activity are well documented, much less is known about the causal effect of profit-sharing. We show that the two systems differ along several key dimensions, with important implications for both workers and firms.

1. Policy Bases and Distortions. A key difference between the two policies lies in their respective tax bases. Mandated profit-sharing targets excess profits, defined as net income in excess of 5% of book equity. In contrast, the corporate income tax applies to corporate earnings. As a result, these policies generate distinct distortions on capital investment (see equation (3)). The corporate income tax creates a uniform distortion across firms (fixing firms' WACC), while the distortion induced by mandated profit-sharing depends on two firm-specific factors: (i) the gap between the firm's cost of equity and the regulatory benchmark ($\Delta = r_e - 5\%$); and

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(ii) the wage incidence of profit-sharing λ. Again, when the actual cost of equity corresponds to the regulatory cost ($\Delta = 0$), or when there is full incidence ($\lambda = 1$), profit-sharing does not distort investment. Beyond the tax base, the rates also differ: the corporate income tax is linear in earnings, while the share of excess profits to be distributed to workers under profit-sharing is proportional to the firm's labor share.

2. Wage Incidence and Avoidance. Corporate taxes are collected by the government and spent through various public policies. Mandated profit-sharing is instead directly redistributed to workers inside the firm. This is an important distinction to consider when evaluating the wage incidence of these policies. The corporate income tax affects wages through general equilibrium effects (see Fuest, Peichl, and Siegloch 2018). Instead, mandated profit-sharing can directly affect workers' compensation by granting them a share of their firm's excess profits.

This difference is also likely to matter in terms of avoidance. Because they do not directly receive proceeds from the tax paid by their firms, workers have little incentive to monitor corporate tax compliance. By contrast, with mandated profit-sharing, each euro of excess profit diverted by the firm reduces workers' collective income by roughly 10–15 cents, creating a stronger incentive to scrutinize firm reporting. Anecdotally, workers' councils do hire accountants to verify fiscal statements and, in particular, the profit-sharing formula. This idea is consistent with existing evidence of corporate income tax avoidance in France (e.g., Bach 2017) and the evidence below that firms do not avoid mandated profit-sharing on the intensive margin.

3. Possible Productivity Effects. Unlike the corporate income tax, which primarily serves a redistributive function, profit-sharing has traditionally been viewed by economists as a mechanism to enhance firm productivity and improve labor relations (see Kruse, Blasi, and Park 2008). These productivity and well-being considerations were a key motivation for the adoption of mandated profit-sharing in France. We investigate the effects of profit-sharing on total factor productivity and sick days (our proxy for well-being and effort) in Section V.

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III. Data

To quantify the impact of profit-sharing on workers and firms, we combine firms' balance sheet data with linked employer-employee data over the period 1985–1997.

III.A. Financial Statements

On the firm side, we exploit accounting information from tax files in the Bénéfices Réels Normaux (BRN) files from 1985 to 1997, the Fichier des Comptes Antérieurs à SUSE (FICAS) data. These data are made available by the French Statistical Office through their secured remote server. They contain income statements and balance sheets collected by the Treasury for the universe of firms in the economy that file under the so-called normal tax regime. These data are used to determine tax liabilities. They are audited by the tax authority, with significant penalties applied for misreporting. The files contain approximately 600,000 firms per year. Importantly, they provide, as an accounting item, the total amount of profit-sharing paid by firms to their employees.

III.B. Wage Data

Our data on French workers' employment histories come from the matched employer-employee DADS (Déclarations Annuelles de Données Sociales) panel. These data are an extract from the DADS Fichier Postes, an exhaustive administrative data set that contains the Social Security records of all salaried employees in private sector firms. The DADS Panel tracks all workers in the data set who were born in October of an even year, which amounts to an overall coverage of slightly more than 4% of the French population working in the private sector. The data set provides information on a worker's employment spell (duration, start and end date during the year, total gross and net wages, tenure within the firm, and two-digit occupation). It also contains information about the worker: age, gender, years of labor market experience, and region of residence. These data cover 1985–1997 with the exclusion of 1990, for which the data are not available.

III.C. Analysis Sample

We restrict the sample to corporations with a nonmissing identifying number that operate in the for-profit sector with headquarters in mainland France. We use this entire sample to analyze

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bunching at the 100-employee threshold over time. Further details on the sample construction procedure are described in Online Appendix A.

For our difference-in-differences analysis, we apply the same restrictions and focus on firms without a reporting gap. We define the treatment status using firm-level employment in 1989 and 1990. Treated firms have 55–85 employees in both 1989 and 1990. Control firms have either 35–45 employees (small control group, likely never treated firms) or 120–300 employees (large control group, likely always treated firms) in both 1989 and 1990. This definition leads us to drop firms that (i) have missing employment data in 1989 or 1990, (ii) are close to the 50- or 100-employee thresholds in 1989 and 1990, or (iii) see their employment counts switch across these groups in these two years. The main analysis is run on an unbalanced sample of firms observed at least in 1989 and 1990. We show in Section V.C the robustness of our main findings when estimated on a balanced sample of firms. Because the data do not include hours, we restrict the worker-level analysis to employees working full time and focus on their daily wage. Interestingly, for our analysis, the measure of wage provided in the data captures various aspects of compensation, including base wage, bonuses, and in-kind benefits that are subject to taxes (e.g., company car, housing, or meal tickets). As a result, our study allows for many forms of substitution between profit-sharing and other forms of compensation. For the rest of the study, we call this measure of compensation (excluding profit-sharing) the base wage.

Our final analysis sample contains 11,374 unique firms in the firm-level panel and 128,824 unique workers in the matched employer-employee data. Tables I and II provide summary statistics for our firm-level and individual-level data sets over 1985–1997. Online Appendix Tables A.1 and A.2 provide statistics separately for the treated and control groups, as well as the distribution of firms across detailed industries.

IV. Bunching Analysis

IV.A. Quantifying Pre-Reform Bunching Around the Threshold

We start our empirical analysis by focusing on the pre-reform period. Before 1991, only firms with more than 100 employees

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TABLE I

SUMMARY STATISTICS: FIRM LEVEL

	Mean (1)	Std. dev. (2)	P5 (3)	P10 (4)	P25 (5)	P50 (6)	P75 (7)	P90 (8)	P95 (9)
Employment	88	63	30	35	40	63	128	191	225
Sales	8,495.1	8,785.1	1,237.2	1,584.1	2,570.2	5,296.7	11,061	19,749	27,725.8
Value added	2,346.1	1,974.3	535.5	672.5	964.2	1,574.7	3,136.6	5,265.1	6,709
Formula	24.1	41.9	0	0	0	4.4	29.2	76.5	118.5
Profit-sharing	13.7	34.7	0	0	0	0	0	52	93.8
Wage bill	1,301.1	999.9	367.6	428.7	575.8	901	1,769.8	2,858.8	3,479.9
Total compensation	1,320.6	1,026.6	367.9	429.4	576.8	907.8	1,799.6	2,915.9	3,562
$\frac{\text{Profit-sharing}}{\text{Value added}}$	0.003	0.007	0	0	0	0	0	0.015	0.022
$\frac{\text{Wages}}{\text{Value added}}$	0.577	0.113	0.387	0.431	0.504	0.576	0.647	0.71	0.763
$\frac{\text{Total compensation}}{\text{Value added}}$	0.581	0.111	0.398	0.441	0.511	0.579	0.649	0.712	0.764
$\frac{\text{Profit}}{\text{Value added}}$	0.047	0.109	−0.134	−0.048	0.006	0.04	0.099	0.173	0.225
$\frac{\text{Taxes}}{\text{Value added}}$	0.034	0.041	0	0	0.002	0.018	0.054	0.097	0.125
Investment rate	0.13	0.22	−0.22	−0.09	0.01	0.1	0.25	0.42	0.52
$\frac{\text{Value added}}{\text{Employment}}$	26.9	10.2	14.4	16.3	20	24.7	31.2	40.5	47.9
$\frac{\text{Sales}}{\text{Employment}}$	99.9	75.7	28.2	34.1	47	71	130.9	211.4	254.5
Firms with excess profits	0.65								
Firms paying some profit-sharing	0.26								
Number of unique firms	11,374								

		Manu-facturing	Con-struction	Retail	Services
	Share	0.45	0.12	0.18	0.24

Notes. This table provides summary statistics of the main firm-level characteristics computed using firms' financial statements over the period 1985–1997. Sales, value added, formula, profit-sharing, wages, and total compensation are expressed in thousands of euros (in 1984 value). Total compensation is defined as the sum of the wage bill plus profit-sharing. Taxes stands for corporate income taxes. Formula corresponds to the minimal amount of profit-sharing that should be paid by firms subject to mandatory profit-sharing according to the rule defined by the law. Investment rate is defined as capital expenditures in tangible assets normalized by the net value of tangible assets. All ratios are trimmed at the 2.5% and 97.5% percentiles.

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TABLE II

SUMMARY STATISTICS: WORKER LEVEL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Distribution									
	Mean	Std. dev.	P5	P10	P25	P50	P75	P90	P95
Gross wage (daily)	39.5	17.6	22.9	24.4	27.8	33.9	44.6	62.3	77.8
Total compensation (daily)	40.1	18	23.2	24.7	28.2	34.5	45.3	63.6	79.2
Years of experience	16.2	11.4	1.4	3	6.8	14	24	33.6	38
Tenure	7	9.1	0	0	0.5	3.5	9.5	19.5	28
Age	36.4	10.7	21	23	27	35	44	52	55
Unique workers	128,824								
Observations	449,936								
Panel B: Employment shares									
		Male	Blue collar	Clerks	Supervisors	Managers	Executives		
	Share	0.68	0.55	0.19	0.18	0.08	0.01		

Notes. This table provides summary statistics of the main worker-level characteristics computed using the linked employer-employee data over the period 1985–1997. Gross wage and total compensation are expressed in euros (in 1984 value). Total compensation is defined as the sum of base wage plus profit-sharing. Profit-sharing is imputed proportionally to the wage using firms' financial statements (see Section VI for more details). Daily wage and daily total compensation are trimmed at the 2.5% and 97.5% percentiles.

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were covered by the profit-sharing regulation. After 1991, the eligibility threshold was decreased to 50 employees. While the policy offers substantial tax advantages to income paid through the profit-sharing scheme (no payroll tax and no personal income tax for workers), it is likely a net cost for firm owners. In the data, only a small share of firms below the eligibility threshold voluntarily adopt profit-sharing through the scheme (see Section II). This fact suggests that the tax advantages and potential productivity gains from profit-sharing may not be sufficient to compensate firms for the increase in total labor costs it generates. The distribution of employment around the eligibility threshold offers a revealed-preference approach to investigate the magnitude of this disincentive. Note that this interpretation holds whether bunching is real (i.e., firms effectively reduce employment to stay below the threshold) or results from underreporting (Askenazy, Breda, and Pecheu 2022, as long as such underreporting is costly—a standard assumption in public finance (e.g., Bachas and Soto 2021).

A caveat to the bunching analysis is that our employment measure does not perfectly match the regulatory definition. The tax data report average end-of-quarter head counts over the fiscal year, whereas the law applies when total head count exceeds 100 for at least six months (see Section II.A). As a result, bunching in our measure is expected to be fuzzy.

Figure I reports the distribution of firm employment pre-reform (1985–1989) and post-reform (1992–1997).[19] We bin the data in buckets of 5 employees starting at 60 employees until 150. We use bins of five employees because employment tends to bunch at multiples of five, likely a reporting error due to rounding. This binning makes the plot smoother, but it is not responsible for the main bunching result, as can be seen from the raw data in Online Appendix Figure D.1. Figure I shows significant excess mass in the 95–99 bin, with 24.8% more density at this bin than in the counterfactual. There is also marginally significant and quantitatively smaller excess mass in the 90–94 and 85–89 bins. Overall, if we assume that this bunching is real and not due to underreporting (an assumption we discuss below), it represents a loss of about 1.67%

19. Since the law was signed in the last quarter of 1990, we exclude 1990 and 1991 from this bunching analysis.

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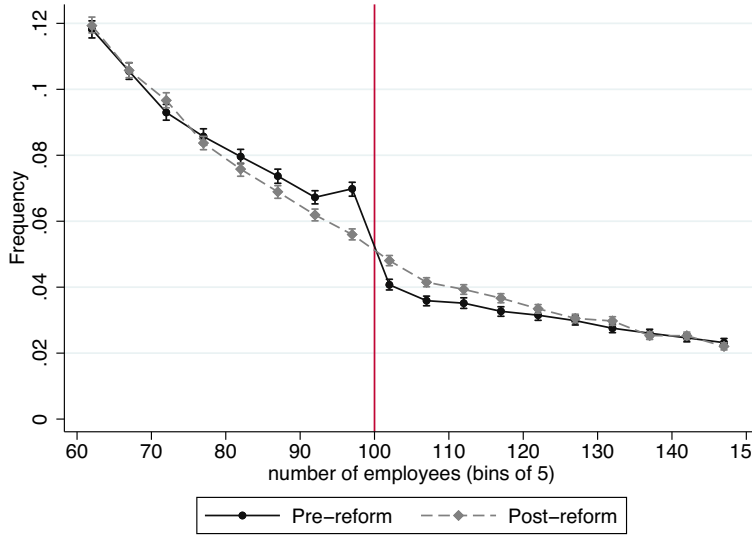


FIGURE I

Excess Mass at the 100-Employee Threshold

This figure shows the share of firms by employment bins for firms with more than 60 employees and strictly less than 150 employees. We use bins of 5 employees starting at 60 employees. The solid line corresponds to the pre-reform distribution (1985–1989); the dashed line corresponds to the post-reform distribution (1992–1997). The vertical bars are 95% confidence intervals. They are computed using a bootstrap. Firms' employment count comes from tax files.

of the employment in affected firms.[20] Using parametric counterfactuals (e.g., Pareto or polynomials) leads to similar results.[21]

Figure I leaves open the possibility that firms bunch only temporarily before crossing the 100-employee threshold, implying short-lived distortions from profit-sharing. To test for such dynamic bunching, we adapt the approach of Garbinti et al. (2023).

20. To calculate the employment loss due to bunching, we consider firms in the 85–120 employee range (i.e., firms for which there is significant bunching—see Figure I) and calculate the change in total employment for these firms if the size distribution for this range was the distribution post-reform (our counterfactual). This calculation assumes no extensive-margin response to the policy removal.

21. Online Appendix Figure D.2 plots the firm-size distribution compared to a Pareto distribution. Again, we see the excess mass and the missing mass in the pre-period, which disappears in the post-period. Interestingly, the Pareto coefficient is the same both in the pre- and post-periods, indicating that the firm-size distribution is stable over time.

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We classify firms into four groups based on their year-t employment: (1) 95–99, (2) 90–94, (3) 85–89, and (4) 80–84 employees. For each group, we defined the normalized change in head counts, which corresponds to the three-year change relative to the increase needed to reach 100 employees:

$$\begin{aligned}\text{normalized change}_{t+3} = & \underbrace{(L_{t+3} - L_t)}_{\text{head count change}} \\ & - \underbrace{(100 - L_t)}_{\text{change needed to cross threshold}} = L_{t+3} - 100.\end{aligned} \tag{4}$$

For each group of firms, we then build a placebo distribution using firms well above 100 employees, for which employment growth should be unconstrained. For example, for the 95–99 group, one placebo set is firms with 125–129 employees in t, assuming a placebo threshold at 130. Their placebo normalized change is thus defined as $L_{t+3} - 130$. More generally, for treated firms in the [95–j; 99–j] range (for j= {0, 5, 10, 15}), we construct a placebo distribution using:

$$\begin{aligned}&\text{normalized change}^{\text{placebo}}_{t+3} = L_{t+3} - (130 + k + j) \\ &\text{if } L_t \in [125 + k, 129 + k] \text{ with } k \in \{0, 5, .., 25\}.\end{aligned} \tag{5}$$

Online Appendix Figure D.3 reports the distribution of normalized change before and after the reform and for each group relative to their respective placebo. The figure shows that in the pre-reform period, the 100-employee threshold significantly constrained three-year employment growth for firms in the 85–99 range, especially so for firms in the 95–99 range, and, to a lesser extent, those in the 90–94 range. These results suggest that the size-based nature of the French profit-sharing scheme creates persistent, rather than temporary, distortions in firm growth.

IV.B. Bunching or Misreporting?

The evidence in Figure I shows that firms perceive mandated profit-sharing as a significant cost despite its tax benefits and the possible productivity gains it may generate. This conclusion holds irrespective of whether bunching is real or due to underreporting, as long as underreporting is costly. The nature of the observed bunching, however, is crucial to assess whether the regulatory threshold generates employment losses. In the context of the many regulations that kick in at 50 employees, Askenazy, Breda,

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and Pecheu (2022) argue that bunching is mainly driven by underreporting.

In our institutional context, underreporting is costly to firms, and thus, we expect it to be infrequent. Firms around the 100-employee threshold have workers' councils who actively monitor the firm's true employment count because misreporting may significantly affect their income. Consulting firms offer legal assistance to workers' councils to help them verify eligibility and calculate the RSP formula.[22] There are also examples of employees or unions suing firms for trying to misreport employment counts to avoid sharing profits.[23]

We conduct three tests to determine whether the observed bunching is real or the result of misreporting. The first test compares how firms report employment and wages in the tax files. The head count reported in the tax file is not an accounting item, unlike the wage bill, which—for firms with more than 50 employees—must be certified by an external auditor (*Commissaire aux comptes*) and is also reported to Social Security for payroll tax purposes. Reporting is therefore likely to be more accurate for the wage bill than for employment counts. If firms understate employment to avoid profit-sharing while reporting wages truthfully, the reported wage bill per employee should spike just below the 100-employee threshold before the reform. Figure II groups firms into 5-employee bins starting at 60 employees and reports, for each bin, the log of average labor costs (wages plus payroll taxes) per employee, with 95% confidence intervals. Panel A covers the pre-reform period (1985–1989) and Panel B the post-reform period (1992–1997). In both periods, there is no visible discontinuity in labor cost per employee around the 100-employee threshold.

Our second test uses an employment measure that is harder to manipulate. Specifically, we use the DADS Panel, which records employment from payroll tax returns and is therefore more likely to be accurately reported (Askenazy, Breda, and Pecheu 2022).[24]

22. A list of such firms can be found on https://www.cse-guide.fr/fiches-pratiques/assistance-juridique/ (in French).

23. See for instance the case of Bea Systems at https://www.legifrance.gouv.fr/juri/id/JURITEXT000023223011/ (in French).

24. One of the tests in Askenazy, Breda, and Pecheu (2022) compares employment counts from tax files with reconstructed figures from the exhaustive matched employer-employee data set (DADS). We cannot replicate this test because comprehensive data are only available after 1994. This is why we use the panel version of the DADS files instead.

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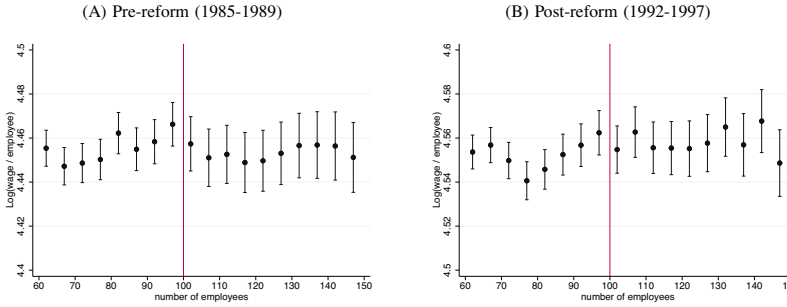


FIGURE II

Wage Bill per Employee and Number of Employees

For each firm in the sample, we compute the log ratio of firms' total wage bill to their number of employees. The figure reports the average value of this log ratio by employment bins, where we use bins of 5 employees starting at 60 employees. The sample period is 1985–1989 (Panel A) and 1992–1997 (Panel B). Firm's employment count and wage data come from tax files. The vertical bars correspond to 95% confidence intervals.

Online Appendix Figure D.4 plots, by five-employee bins of our main employment variable, the average difference between that variable and the DADS employment measure. If firms underreported employment in the accounting data relative to DADS, we would expect a negative gap just below the 100-employee threshold—that is, lower employment in the tax files than in DADS. In practice, the gap is on average positive, though small (about one to two employees), even just below the threshold: firms tend to report slightly higher employment in tax files than in DADS. We observe only a small, statistically insignificant drop of roughly 0.5 employees to the left of the threshold, too small to shift a firm from the 100–104 bin to the 95–99 bin. Moreover, the gap is smaller for firms in the 100–104 bin than for those in the 85–99 bin, despite the absence of incentives to underreport above the threshold. These results, in line with our first test, suggest that firms do not systematically underreport employment at the 100-employee threshold.

Our third test examines whether bunching firms differ systematically in profitability and productivity from firms just above the threshold.[25] Online Appendix Figure D.5 plots the average

25. The pretax profit measure is also pre–profit-sharing, that is, taxable income before paying profit-sharing and taxes.

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value added per employee and profit per employee around the 100-employee threshold, separately for pre-reform years (when the eligibility threshold was 100) and post-reform years (after the threshold was decreased to 50 employees). Profit per employee—and to a lesser extent, value added per employee—is significantly higher just below the 100-employee threshold. This pattern is consistent with more profitable firms having stronger incentives to bunch below 100 employees to avoid profit-sharing requirements.

In sum, this bunching analysis reveals that (i) despite its tax advantage and potential productivity gains, mandatory profit-sharing creates a significant net cost for firms; and (ii) the patterns observed are consistent with real employment bunching, indicating that the eligibility threshold creates inefficiencies—up to a 1.67% employment loss among affected firms.

V. Firm-Level Evidence

V.A. Empirical Strategy

We exploit the decrease in the regulatory threshold from 100 to 50 employees in 1991 to estimate how mandatory profit-sharing affects firms. We use a simple difference-in-differences strategy with two separate control groups. As described in Section III, we assign a firm to the treatment group if its employment count in 1989 and 1990 is 55–85 employees. We do not use firms closer to the 50- and 100-employee thresholds to limit the influence of bunching firms. This approach corresponds to an intent-to-treat design: firms in the treatment group have a high chance of being subject to mandatory profit-sharing in the years after the reform since they meet the new eligibility criteria in the two years before the reform. However, as shown in the previous section, actual treatment status is not certain. Firms may adjust employment in the post-reform years and fall below the 50-employee eligibility threshold, in which case they would not be mandated to share profits. We then turn to the Wald estimator that rescales our reduced-form estimates by the actual probability of paying profit-sharing.

The empirical design provides two natural control groups: (i) small control firms are firms whose employment count in 1989 and 1990 is 35–45 employees, (ii) large control firms are firms with 120–300 employees in both 1989 and 1990. Small control firms are firms with a low likelihood of being subject to mandatory profit-sharing in both the pre- and post-reform periods. Large control

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firms are firms with a high likelihood of being subject to mandatory profit-sharing in both periods.

Having two different control groups is an important aspect of our empirical strategy. France entered a recession in 1992, which lasted until the end of 1993. If large firms respond differently to the business cycle, comparing treated firms' outcomes with only one of the control groups could lead to spurious inference: what would be identified as the causal effect of the reform could be driven by the differential response of firms of different sizes to macroeconomic shocks. Our identification thus crucially relies on comparing treated firms—firms that become subject to mandatory profit-sharing after 1991—to control firms that are either smaller or larger. In what follows, we present treatment effects relative to each of these two control groups separately, as well as relative to the two groups combined. In the Online Appendix, we show the robustness of our main findings to different definitions of the treatment and control status.

Figure III shows the actual share of firms formally subject to the regulation over time as a function of their assignment to the three different groups: treatment, small control, and large control. We confirm that actual treatment is highly (although imperfectly) correlated with treatment assignment. In 1997, 26.8% of firms in our treatment group end up having fewer than 50 employees; 14.4% of the firms in our small control group have more than 50 employees in 1997 and are thus required to share profits. Finally, about 13.6% of the firms in our large control group start in 1985 with fewer than 100 employees and are thus initially not subject to mandatory profit-sharing. Overall, our intent-to-treat variable strongly predicts actual treatment status.

Our regression analysis uses the following specification, where i is a firm, c is a county,[26] s is an industry, and t is a year:

$$Y_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}_l\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}, \quad (6)$$

where α_i corresponds to firm fixed effects and absorbs any fixed-in-time differences across firms. δ_{ct} and μ_{st} corresponds to county-by-year and industry-by-year fixed effects. These controls ensure that our results are not driven by industry or county-level shocks

26. A county corresponds to a *département*. There are 94 *départements* in mainland France, each with an average population of about 700,000 inhabitants.

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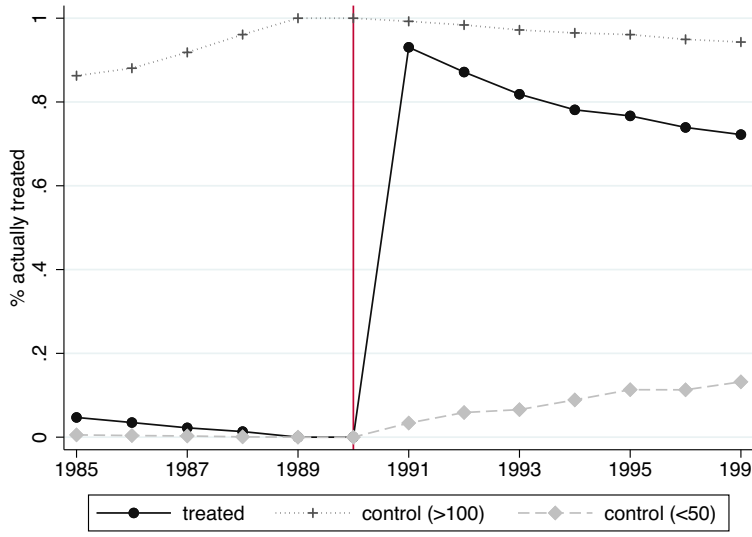


FIGURE III

Treatment Status and Actual Treatment

This figure plots the share of firms that are actually mandated to have a profit-sharing scheme by treatment status. Treated firms have between 55 and 85 employees in 1989 and 1990 (dark solid dots). Control (>100) are firms with 120 to 300 employees in 1989 and 1990 (dotted dark gray crosses). Control (<50) are firms with 35 to 45 employees in 1989 and 1990 (dashed light gray diamonds). The vertical solid red line corresponds to 1990, the year the reform is voted and implemented.

that would similarly affect treated and control groups.[27] $\mathbb{1}_{\{i \in \text{Treated}\}}$ is a dummy variable equal to one if a firm has 55–85 employees in 1989 and 1990. $\mathbb{1}_{\{t \geqslant 1991\}}$ is a dummy variable equal to one after 1990, the introduction of the reform. $\mathbb{1}_{\{i \in \text{Control}_l\}}$ is a dummy variable equal to one when firm i is in the control group l, with $l \in \{\text{small}, \text{large}\}$. For each regression, we separately report three estimated treatment effects corresponding to the estimated β^T when using small firms, large firms, or both as a control group.

27. Time-varying county and industry fixed effects might absorb some variations between firms that could be leveraged to estimate the effects of profit-sharing. As a robustness check, we reproduced the main results of the article with only time and firm fixed effects. The conclusion remained unchanged. These estimations are available on request.

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We also trim all variables defined as ratios at the 2.5% and 97.5% levels.[28]

The identifying assumption in our intent-to-treat design is a standard parallel-trend assumption: in the absence of treatment, the outcome of interest (e.g., labor share, profit share, investment, or productivity) for firms in the 35–45 employee group and in the 120–300 employee group would have evolved similarly to those of firms in the 55–85 employee group. In the following analysis, we systematically confirm that prior to 1991, firms in the treatment and the two control groups follow parallel trends, indicating that firms below the 50-employee threshold (resp., above the 100 threshold) provide suitable counterfactuals for firms mandated to share profits.

We also report local average treatment effect (LATE) estimators of profit-sharing on various corporate outcomes, using 2SLS. As discussed already, treated firms' employment can cross the 50-employee threshold in the post-period. Despite this noncompliance, we can obtain a LATE of mandatory profit-sharing by using $\mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}}$ as an instrument for the probability of sharing profits (or the amount of profits shared with employees). More precisely, we use a 2SLS approach where we first estimate:

$$(\mathbb{1}_{\{\text{profit-sharing}\}})_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \gamma^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \gamma^C \mathbb{1}_{\{i \in \text{Control}_l\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \eta_{icst}. \tag{7}$$

We use the predicted value from equation (7) and estimate:

$$Y_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \psi(\widehat{\mathbb{1}_{\{\text{profit-sharing}\}}})_{icst} + \nu_{icst}, \tag{8}$$

where ψ is the causal effect of profit-sharing on outcome Y for compliers, that is, for firms in the treatment group who end up being required to share profits with their employee through the scheme. We also run specifications where the main endogenous variable is the ratio of profit-sharing to value added instead of the dummy for profit-sharing used in equations (7) and (8).

In what follows, we present regression results for various firm-level outcomes Y, using both reduced-form treatment effects (equation (6)) and 2SLS estimates of the effect of mandatory

28. We use a conservative trimming rule given the skewness observed in some of the dependent variables used in our analyses. We confirm the robustness of our findings to alternative cleaning methods. Online Appendix Table D.1 reproduces the main results when the variables are winsorized at the 2.5% and 97.5% instead of trimmed.

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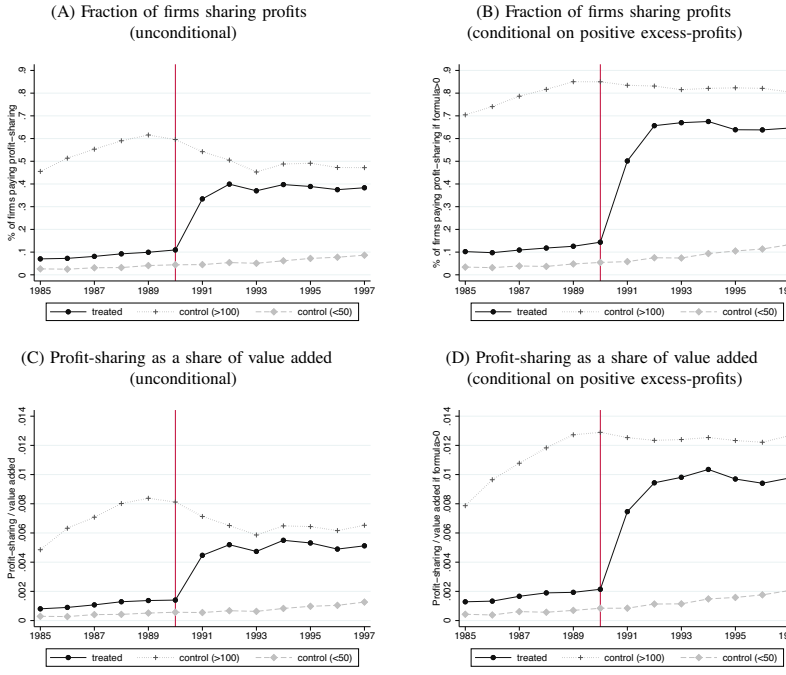


FIGURE IV

Firms Sharing Profits over Time, by Treatment Status

Panel A plots the share of firms in each group that reports a strictly positive amount of profit-sharing on their income statement. Panel B conditions on firms with positive excess profits. Panel C plots the average ratio of profit-sharing to value-added in each group over time. Panel D conditions on firms with positive excess profits. Excess profits are defined as accounting net income minus 5% of book equity. Treated firms have 55–85 employees in 1989 and 1990 (dark solid dots). Control (>100) are firms with 120–300 employees in 1989 and 1990 (dotted dark gray crosses). Control (<50) are firms with 35–45 employees in 1989 and 1990 (dashed light gray diamonds).

profit-sharing (equations (7) and (8)). We also report graphical versions of the reduced-form regressions (as well as evidence from raw data in the Online Appendix).

V.B. Profit-Sharing Response

To document the validity of our approach, we first examine the evolution of profit-sharing in the three groups. Figure IV, Panel A shows the share of firms reporting a strictly positive amount of profit-sharing in their income statement, by treatment status and

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over time. In the treated group, the share rises sharply in 1991—from about 10% before 1990 to roughly 40% thereafter. In contrast, the share in both control groups shows no change around 1990. For small control firms—those unlikely ever to be subject to the regulation—the share stays below 10% throughout, with only a slight upward trend, likely reflecting favorable late 1980s economic conditions that encouraged voluntary profit-sharing, or the intent-to-treat design (some small control firms cross the 50-employee threshold after 1990, as shown in Figure III). Large control firms also exhibit a gradual increase in the pre-period. This is intuitive since 14% of these firms had fewer than 100 employees in 1985 but grew past the 100-employee threshold before 1989, mechanically raising their probability of sharing profits in the pre-period.

Panel B reproduces the same analysis but focuses on firms with positive "excess profits," defined according to the regulatory formula in equation (1). Firms with negative excess profits are formally subject to the regulation but will not report profit-sharing in their income statement, as they have no profits to distribute. A caveat with this approach is that our measure of excess profits does not exactly match the regulatory definition, since some required items are missing from our data set. In particular, the regulation is based on fiscal net income, while our data report accounting net income. These two measures can differ because of nondeductible items (e.g., certain types of compensation) and tax loss carryforwards.[29]

With this caveat in mind, Panel B shows that for treated firms with positive excess profits, the probability of paying profit-sharing rises from about 10% before 1990 to about 65% after 1990, once they become subject to the regulation. The fact that this share remains below 100% may reflect the intent-to-treat design, measurement error in excess profits, or imperfect compliance.[30] In con-

29. Online Appendix Figure D.6 shows that our reconstructed formula is a good, though imperfect, proxy for the actual amount of profit-sharing paid. The sample includes firms with 50–300 employees in 1992–1997 (post-reform, when profit-sharing is mandatory). We divide the reconstructed formula, normalized by value added, into 30 bins and plot on the y-axis the actual average ratio of profit-sharing to value added. While the R^2 is high, the slope is 0.67, significantly below 1.

30. Online Appendix Table D.2 indicates high compliance: among firms above the size threshold with a positive reconstructed formula, only 3% report no profit-sharing.

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trast, neither control group experiences a sharp change in the probability of paying profit-sharing around 1990. These results confirm that the intent-to-treat design generates large changes in the propensity of treated firms to share profits with employees.

Online Appendix Figure D.7 further confirms the validity of our design by showing how the adoption of profit-sharing evolves over time across the size distribution. For each five-employee bin, we plot the average share of firms reporting profit-sharing for years before (1985–1987, 1988–1989) and after the reform (1992–1994, 1995–1997). Firms with 55–95 employees see a sharp rise—from about 10% to 40%—consistent with the time-series evidence in Figure IV. Firms below 45 or above 120 employees show no significant change.

Figure IV, Panels C and D examine the share of profit-sharing in value added rather than the fraction of firms sharing profits. Panel C shows the evolution of the average ratio by treatment status, and Panel D restricts the sample to firms with positive excess profits. Among treated firms with positive excess profits, profit-sharing accounts for less than 0.2% of value added before 1990, rising to about 1% immediately after they become subject to mandated profit-sharing. No comparable change is observed in either control group. In the large control group, the ratio rises gradually in the late 1980s, mirroring the pattern in Panel A of Figure IV for the same reason—treatment status is based on 1989–1990 employment counts.

Table III estimates equation (6) to quantify the effect of the reform on the propensity to share profits with employees. In column (1), the dependent variable is a dummy equal to one when the firm reports a strictly positive amount of profit-sharing on its income statement. In column (2), the dependent variable is the ratio of profit-sharing to value added for firms with positive value added. Panel A provides the estimate of β^T relative to firms in the large control group; Panel B uses firms in the small control group as reference; Panel C uses both simultaneously. Firms in the treatment group experience a significant increase in the unconditional probability of sharing profits of 25–36 percentage points, consistent with the graphical evidence in Figure IV.

Finally, we provide a simple sanity check of our empirical design. Treated firms with low or negative excess profits should not report any profit-sharing, even after the reform; thus, in treated firms, the ex post increase in mandated profit-sharing should be concentrated among firms with a high regulatory formula. We con-

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TABLE III

EFFECTS OF PROFIT-SHARING ON WORKERS' COMPENSATION AND SHAREHOLDERS' PROFITS

	$\mathbb{1}_{\{\text{Profit-sharing}>0\}}$	$\frac{\text{Profit-sharing}}{\text{Value added}}$	$\frac{\text{Wage}}{\text{Value added}}$	$\frac{\text{Tot. compensation}}{\text{Value added}}$	$\frac{\text{Profits}}{\text{Value added}}$	$\frac{\text{Taxes}}{\text{Value added}}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Relative to large control						
Treatment × post	0.3642***	0.0047***	0.0011	0.0054***	−0.0054***	−0.0013*
	(0.0096)	(0.0002)	(0.0019)	(0.0018)	(0.0020)	(0.0007)
Panel B: Relative to small control						
Treatment × post	0.2542***	0.0035***	0.0020	0.0059***	−0.0036**	−0.0012*
	(0.0073)	(0.0001)	(0.0018)	(0.0017)	(0.0018)	(0.0007)
Panel C: Relative to both groups						
Treatment × post	0.3046***	0.0040***	0.0016	0.0057***	−0.0044***	−0.0013**
	(0.0075)	(0.0001)	(0.0016)	(0.0015)	(0.0017)	(0.0006)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.53	0.50	0.57	0.56	0.35	0.55
Observations	132,588	127,666	123,555	123,537	124,373	124,403

Notes. This table reports intent-to-treat estimates of the effect of mandatory profit-sharing at the firm level. $\mathbb{1}_{\{\text{Profit-sharing}>0\}}$ is a dummy variable equal to one if a firm reports a strictly positive amount of profit-sharing on its income statement. $\frac{\text{Tot. compensation}}{\text{Value added}}$ is the ratio of firms' wage bill plus profit-sharing to its value added. $\frac{\text{Profits}}{\text{Value added}}$ is the ratio of firms' net income to its value added. $\frac{\text{Taxes}}{\text{Value added}}$ is the ratio of firms' corporate income tax to its value added. All the ratios are defined only for firms with positive value added. Treatment is a dummy variable equal to one if a firm has 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. We estimate the following equation using OLS:

$$Y_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}.$$

α_i corresponds to firm fixed effects, δ_{ct} to county-by-year fixed effects, and μ_{st} to industry-by-year fixed effects. Panel A reports β^T when the large control is the reference group. Panel B reports β^T when the small control is the reference group. Panel C reports β^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees in 1989 and 1990. The small control group consists of firms with 35–45 employees in 1989 and 1990. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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firm this by estimating the following triple-difference model:

$$\begin{aligned} Y_{icst} = {} & \gamma^T \pi_{it} . \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\ & + \theta_1^T \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \theta_2^T \pi_{it} . \mathbb{1}_{\{i \in \text{Treated}\}} \\ & + \gamma^C \pi_{it} . \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \theta_1^C \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\ & + \theta_2^C \pi_{it} . \mathbb{1}_{\{i \in \text{Control}_l\}} + \mu_1 \pi_{it} . \mathbb{1}_{\{t \geqslant 1991\}} \\ & + \mu_2 \pi_{it} + \alpha_i + \delta_{ct} + \mu_{st} + \epsilon_{icst}, \end{aligned} \tag{9}$$

where π_{it} is our (noisy) reconstruction of the profit-sharing formula, normalized by value added. γ^T captures the effect of becoming eligible for mandated profit-sharing in years when excess profits are large enough to trigger payments. Online Appendix Table D.3, column (1) estimates equation (9) using the probability of reporting positive profit-sharing as the dependent variable; column (2) uses the ratio of profit-sharing to value added. Panel A uses large firms as the control group; Panel B uses small firms. In all cases, the increase in profit-sharing among treated firms is significantly larger for those with higher ex post excess profits. Columns (7) and (8) repeat the analysis after splitting π_{it} into terciles instead and find similar results.

This exercise provides a useful validity check for our empirical design. If we observed an increase in profit-sharing even among firms with low or negative current excess profits, the design would be flawed. However, this triple-difference cannot be used directly to refine our identification of the causal effect of mandated profit-sharing on corporate behavior, since it relies on ex post realization of excess profits. In Section V.E, we address this limitation by splitting the sample based on firms' average pre-period excess profits, thereby creating an ex ante measure of "exposure to the reform."

V.C. Incidence on Workers and Shareholders

We now look at the incidence of the reform on workers' compensation and firm owners' profits. Table III, columns (3) and (4) estimate equation (6) using the wage share and the total compensation share as dependent variables. We find a precisely estimated zero effect of mandatory profit-sharing on the wage share, implying that owners do not reduce the cost of profit-sharing by lowering wages. In contrast, we find a significant increase in the total compensation share of treated firms: 0.5 percentage points relative to

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large control firms and 0.6 percentage points relative to small control firms. The results are robust to various definitions of the control groups. Online Appendix Figure D.8 reproduces the analysis using alternative control groups, including (i) more narrowly defined groups around the thresholds, (ii) groups with smaller lower bounds and larger upper bounds, and (iii) a larger doughnut hole around the 50 and 100 thresholds. In all cases, we observe a clear nonnegative effect on wages and a positive and significant effect on total compensation.

We visually confirm the robustness of these findings—and, in particular, the absence of pre-trends—in Figure V, where we report event-study graphs showing year-by-year treatment effects relative to each control group separately (relative to large controls on the left, relative to small controls on the right). Panel A focuses on wages, and Panel B on total compensation. The results mirror those reported in Table III. In addition, the figure shows small and insignificant coefficients in the pre-reform period, consistent with the parallel-trend assumption. We further assess the validity of our regression results using raw data in Online Appendix Figure D.9. Panel A plots the evolution of the wage share for firms in our sample (normalized in levels to zero in 1990). This evolution is strikingly similar for firms in the treatment and control groups.[31] Panel B shows that as a result, the total compensation share—the fraction of value added that goes to workers in the form of wages and profit-sharing—increases sharply for treated firms right after 1990. This increase in the total compensation share matches approximately the increase in the profit-sharing share observed in Figure IV, Panel C.

Who pays for this increase in the total compensation share? Table III, column (5) shows that the profit share of treated firms falls by about 0.5 percentage points (0.4 percentage points) relative to large control firms (small control firms). The difference between the effect on the labor share and the profit share is entirely explained by a reduction in the tax share, that is, the ratio of the corporate income tax to value added (Table III, column (6)). Since profit-sharing is deducted from the firm's taxable income, part of the increase in workers' compensation comes at the expense of the government. These findings are confirmed by the

31. As described in Section III, our measure of wages includes not only base wages but also bonuses and in-kind benefits, allowing us to take into account various substitution patterns with profit-sharing.

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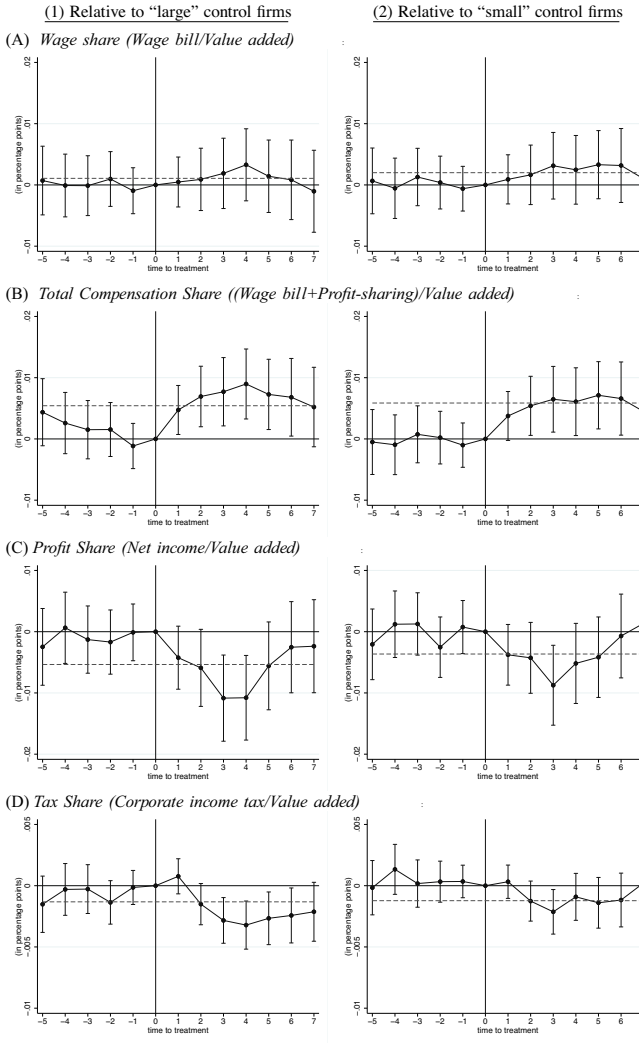


FIGURE V

Event Study: Wage Share, Total Compensation Share, Profit Share, and Tax Share

This figure reports event-study plots where we replace the post dummy in equation (6) by time-to-treatment dummies. We report the coefficients on the time-to-treatment dummies interacted with the treatment dummy, together with their 95% confidence intervals. The left panels correspond to the treatment effect relative to large control firms (firms with 120–300 employees in 1989 and 1990); the right panel relative to the small control firms (firms with 35–45 employees in 1989 and 1990). The dashed gray line reports the difference-in-differences coefficient computed over the whole period.

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event-study plots in Figure V. Panel C focuses on the share of net income in value added, and Panel D analyzes the share of corporate tax in value added. These figures shows no evidence of differential trends before and after the reform.

Finally, for completeness, we also report raw data on the evolution of profit and tax shares relative to value added for the three groups in Online Appendix Figure D.10. Overall, the patterns align well, both qualitatively and quantitatively, with the regression results. We note, however, a sharp increase in the profit share from 1985 to 1990, observed across both treated and control firms. This rise likely reflects the relatively strong growth of the French economy in the late 1980s, coupled with the large reduction in the corporate income tax rate during this period.[32] After 1991, by contrast, we observe a sharp reduction in the profit share for firms in the treatment group relative to the two controls. As with employee compensation and wages, we reproduce this analysis using alternative control group definitions in Online Appendix Figure D.8. Again, our key takeaways remain robust to these changes.

We run several robustness checks. First, we check that endogenous attrition is not driving our results. Online Appendix Table D.4 restricts the sample to firms that are present every year in our sample. We focus here on columns (1)–(6), where the outcome variables are profit-sharing, wage and compensation shares, profit, and tax shares. The findings on this balanced sample are similar to our baseline results. Firms in the treatment group do not experience a significant change in their wage share. Their total compensation share, however, increases by a significant 0.65 percentage points, and their profit share decreases by 0.36 percentage points (only significant at the 10% level) and their tax share by 0.11 percentage points (only significant at the 10% level relative to the large control group). The balanced and unbalanced samples produce similar results in part because exit rates do not differ between our three groups (Online Appendix Figure D.11). In yet another robustness check, we redefine our control and treatment groups to compare firms of more similar size. Online Appendix Table D.5 divides the treatment group into two subgroups and compares small control firms to small treated firms (55–69 employees) and large control

32. The statutory corporate income tax rate is 50% in 1985, 45% in 1986 and 1987, 42% in 1988, 39% in 1989, 37% in 1990, 34% in 1991 and 1992, and 33% after 1993. This reduction applies uniformly to the firms in our three groups.

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firms to large treated firms (70–85 employees). Again, the results remain consistent with our main findings.

The results presented so far correspond to intent-to-treat effects. They do not measure the actual treatment effect on the treated. To better quantify the incidence of profit-sharing on base wage, Table IV presents IV estimates from equations (7) and (8). In column (1), we present OLS estimates of a regression of the wage share on a profit-sharing dummy, controlling for firm, industry-year, and county-year fixed effects. Such an OLS regression is in the spirit of the empirical literature on profit-sharing adoption reviewed in the introduction. We find that profit-sharing is associated with a large and significant (5 percentage points) reduction in the wage share, suggesting a large incidence of profit-sharing on wages. IV results do, however, show that these OLS estimates are strongly biased. In column (2), we instrument the probability to share profits using our intent-to-treat design (i.e., using the interaction of a treatment dummy equal to one if a firm has 55–85 employees in 1989 and 1990 and a post dummy equal to one after 1990). Consistent with the reduced-form findings, the IV regression estimates a precise zero effect of profit-sharing on the wage share—a result in sharp contrast with the OLS estimate. Column (4) shows that sharing profits leads to an increase in the total compensation share of 1.78 percentage points. Columns (5)–(8) repeat this analysis but use the ratio of profit-sharing to value added as the main independent variable. We find that a 1 percentage point increase in profit-sharing relative to value added leads to an insignificant increase in the wage share and a highly significant 1.5 percentage point increase in the total compensation share. Note that we cannot reject a point estimate of one (i.e., a full pass-through of profit-sharing to employees). One plausible explanation for the discrepancy between OLS and IV estimates is that firms voluntarily signing profit-sharing agreements might be experiencing productivity shocks that temporarily reduce the firm's labor share (e.g., because of imperfect rent-sharing).

Online Appendix Table D.6 analyzes profit and tax shares and reports results consistent with analyses of wages and total compensation. Whereas OLS results find that profit-sharing is strongly correlated with higher profit and tax shares, IV estimates show that these estimates are also biased. Using the reform as an instrument, we find that for actually treated firms, the profit share significantly decreases by 1.38 percentage points (column (2)) and the tax share by 0.41 percentage points (column (4)). As a result,

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TABLE IV

EFFECTS OF PROFIT-SHARING ON WAGES AND TOTAL COMPENSATION AT THE FIRM LEVEL, IV ESTIMATES

	$\frac{\text{Wage}}{\text{Value added}}$		$\frac{\text{Tot. compensation}}{\text{Value added}}$		$\frac{\text{Wage}}{\text{Value added}}$		$\frac{\text{Tot. compensation}}{\text{Value added}}$	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Panel A: Profit-sharing dummy								
$\mathbb{1}_{\{\text{Profit-sharing}>0\}}$	−0.0534***	0.0049	−0.0402***	0.0178***				
	(0.0009)	(0.0050)	(0.0009)	(0.0049)				
Panel B: Profit-sharing to value added ratio								
$\frac{\text{Profit-sharing}}{\text{Value added}}$					−3.3884***	0.6159	−2.4518***	1.5055***
					(0.0529)	(0.3856)	(0.0524)	(0.3815)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cragg Donald F-stat.		7,289		7,330		3,974		3,998
Kleibergen Paap F-stat.		1,686		1,689		976		977
Observations	123,536	123,536	123,537	123,537	120,560	120,560	120,457	120,457

Notes. This table provides the impact of profit-sharing on the wage share (respectively, the total compensation share) at the firm level. $\mathbb{1}_{\{\text{Profit-sharing}>0\}}$ is a dummy variable equal to one if a firm reports a strictly positive amount of profit-sharing on its income statement. $\frac{\text{Profit-sharing}}{\text{Value added}}$ is the ratio between the amount of profit-sharing paid by a firm and its value added. Columns (1), (3), (5), and (7) use OLS estimations. Columns (2), (4), (6), and (8) instrument profit-sharing using the interaction between a Treatment dummy, equal to one if the firm has 55–85 employees in 1989 and 1990, and a Post dummy equal to one after 1990. The parameters are estimated using a 2SLS procedure. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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shareholders bear about 77% of the cost and the tax authority pays the remaining 23% through lower corporate taxes.

V.D. Real Effects

1. Productivity. Does mandated profit-sharing raise firm productivity? As emphasized in the literature, profit-sharing may raise productivity by encouraging employees to work harder (e.g., leading to more effort, reduced sick days, or fewer strikes). This was also a motivation for the French reform: the "alignment" of shareholders' and workers' interests should result in increased productivity and possibly profits. Panel A of Figure VI and Online Appendix Figure D.12 show that this is not the case. There is no evidence that treated firms' total factor productivity (TFP) (measured using Ackerberg, Caves, and Frazer 2015) or their Return on Assets (ROA, measured as the ratio of a firm's EBITDA to net asset value) increased after 1991 relative to firms in either control group. Table V confirms the absence of an effect on firms' productivity using various estimation methods to measure productivity (e.g., Olley and Pakes 1996; Levinsohn and Petrin 2003; Wooldridge 2009; Ackerberg, Caves, and Frazer 2015). We find a precisely estimated zero effect on TFP. For instance, column (1) indicates that we can reject with 95% confidence that mandated profit-sharing increased productivity by more than 0.33%. Note that of the outcomes we study, productivity is perhaps the one for which the external validity of our results is most limited. Voluntary profit-sharing may have more effect on productivity (workers want to reciprocate) than firm compliance with regulation (workers just take it as given and do not feel compelled to reciprocate).

Finally, we consider "softer" measures of worker effort, such as sick leave days and the probability of working extra hours, as reported in the Labor Force Survey. Online Appendix Table D.7 shows that mandatory profit-sharing does not lead to a reduction in sick days or an increase in extra-time work. These findings suggest that mandatory profit-sharing does not increase workers' efforts.

2. Investment. Panel B of Figure VI and Online Appendix Figure D.13 plot, respectively, the event-study estimates and the raw data on the evolution of investment by treatment group (normalized in levels to zero in 1990). Investment is defined as the ratio of capital expenditures in tangible

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TABLE V

EFFECTS ON FIRMS' TFP

	Levinsohn-Petrin (1)	Ackerberg-Caves-Frazer (2)	Olley-Pakes (3)	Olley-Pakes (ACF correction) (4)	Wooldridge (2009) (5)	Levinsohn-Petrin (wage) (6)	Levinsohn-Petrin (ACF correction) (7)	Ackerberg-Caves-Frazer (wage) (8)
Panel A: Relative to large control								
Treatment × post	−0.0074	−0.0064	−0.0057	−0.0089**	−0.0081	−0.0049	0.0031	−0.0028
	(0.0051)	(0.0048)	(0.0046)	(0.0043)	(0.0051)	(0.0047)	(0.0106)	(0.0066)
Panel B: Relative to small control								
Treatment × post	−0.0029	−0.0021	−0.0026	−0.0026	−0.0042	−0.0020	−0.0023	0.0010
	(0.0046)	(0.0043)	(0.0043)	(0.0040)	(0.0046)	(0.0043)	(0.0091)	(0.0063)
Panel C: Relative to both groups								
Treatment × post	−0.0049	−0.0040	−0.0040	−0.0054	−0.0060	−0.0033	0.0001	−0.0006
	(0.0042)	(0.0039)	(0.0038)	(0.0036)	(0.0042)	(0.0039)	(0.0084)	(0.0056)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjisted R^2	0.94	0.87	0.91	0.86	0.95	0.97	0.73	0.98
Observations	122,145	122,179	122,195	122,190	122,161	122,193	106,704	122,193

Notes. This table reports intent-to-treat estimates of the effect of mandatory profit-sharing on various definitions of total factor productivity (TFP) at the firm level. Firms' TFP is computed separately for each two-digit industry using the NAF classification measured in 1990. It is estimated using: Levinsohn and Petrin (2003) methodology (column (1)), Ackerberg, Caves, and Frazer (2015) (ACF) methodology (column (2)), Olley and Pakes (1996) methodology (column (3)), Olley and Pakes (1996) methodology with Ackerberg, Caves, and Frazer (2015) correction (column (4)), Wooldridge (2009) methodology (column (5)), Levinsohn and Petrin (2003) methodology using the wage bill as a free input instead of employment (column (6)), Levinsohn and Petrin (2003) methodology with Ackerberg, Caves, and Frazer (2015) correction (column (7)), and Ackerberg, Caves, and Frazer (2015) methodology using the wage bill as a free input instead of employment (column (8)). Treatment is a dummy variable equal to one if firms have 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. We estimate the following equation using OLS:

$$Y_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}.$$

α_i corresponds to firm fixed effects, δ_{ct} to county-by-year fixed effects, and μ_{st} to industry-by-year fixed effects. Panel A reports β^T when the large control is the reference group. Panel B reports β^T when the small control is the reference group. Panel C reports β^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees in 1989 and 1990. The small control group consists of firms with 35–45 employees in 1989 and 1990. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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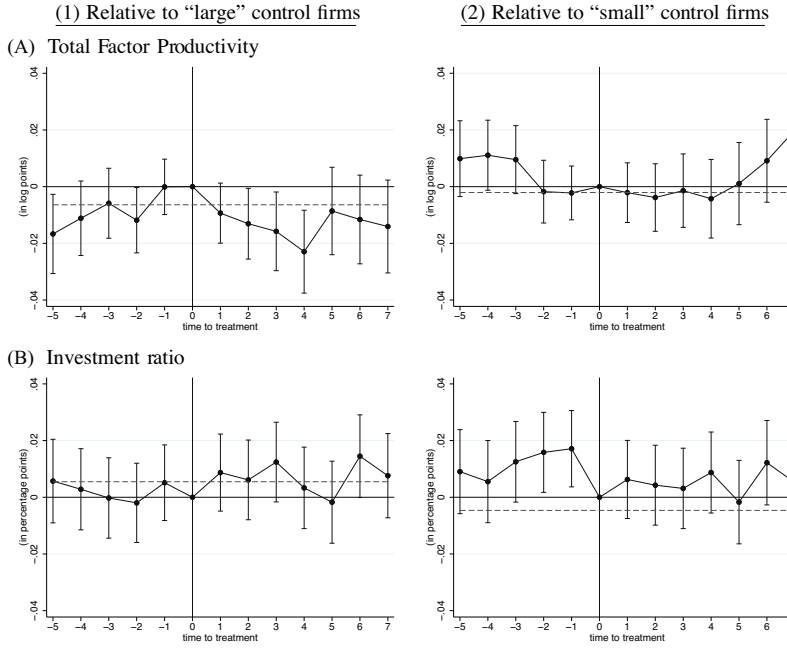


FIGURE VI

Event Study: TFP and Investment Ratio

This figure reports event-study plots where we replace the Post dummy in equation (6) by time-to-treatment dummies. We report the coefficients on the time-to-treatment dummies interacted with the treatment dummy, together with their 95% confidence intervals. The left panels correspond to the treatment effect relative to large control firms (firms with 120–300 employees in 1989 and 1990); the right panel relative to the small control firms (firms with 35–45 employees in 1989 and 1990). Total factor productivity is computed using Ackerberg, Caves, and Frazer's (2015) methodology. It is computed separately for each two-digit industry using the NAF classification measured in 1990. Investment ratio is defined as capital expenditures in tangible assets normalized by the net value of tangible assets. The dashed gray line reports the difference-in-differences coefficient computed over the whole period.

assets to the net value of tangible assets. Both figures reveal no discernible reduction in the investment rate after the introduction of mandated profit-sharing. In the raw data, we observe a sharp decline in investment during the 1991–1993 recession, but this downturn affected all firms simultaneously. Table VI, column (1) confirms that the reform had no effect on firms' investment ratio. Column (2) shows a small negative but insignificant effect

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TABLE VI
EFFECTS ON FIRMS' INVESTMENT

	Investment rate	$\frac{\text{Investment}}{\text{Capital}_{pre}}$	Capital (log)	Tangible capital (log)	$\log(\frac{\text{Capital}}{\text{Labor}})$
	(1)	(2)	(3)	(4)	(5)
Panel A: Relative to large control					
Treatment × post	0.0055	−0.0114**	0.0030	−0.0268*	−0.0133*
	(0.0036)	(0.0046)	(0.0106)	(0.0159)	(0.0080)
Panel B: Relative to small control					
Treatment × post	−0.0046	−0.0012	−0.0248**	−0.0065	−0.0057
	(0.0034)	(0.0044)	(0.0096)	(0.0151)	(0.0075)
Panel C: Relative to both groups					
Treatment × post	0.0000	−0.0059	−0.0124	−0.0155	−0.0091
	(0.0030)	(0.0039)	(0.0087)	(0.0133)	(0.0067)
Firm FEs	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.16	0.15	0.91	0.85	0.88
Observations	123,811	125,653	125,934	125,929	124,483

Notes. This table reports intent-to-treat estimates of the effect of mandatory profit-sharing on investment at the firm level. The investment rate is defined as capital expenditures in tangible assets normalized by the net value of tangible assets. Column (2) normalizes capital expenditures by the average of the net value of tangible assets computed over the pre-policy period. Columns (3) and (4) provides the results for the log of the net value of total assets and of tangible assets respectively. Finally, the capital-labor ratio is the log of net total assets divided by employment. Treatment is a dummy variable equal to one if a firm has 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. We estimate the following equation using OLS:

$$Y_{icst} = \alpha_i + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}.$$

α_i corresponds to firm fixed effects, δ_{ct} to county-by-year fixed effects, and μ_{st} to industry-by-year fixed effects. Panel A reports β^T when the large control is the reference group. Panel B reports β^T when the small control is the reference group. Panel C reports β^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees in 1989 and 1990. The small control group consists of firms with 35–45 employees in 1989 and 1990. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

on investment when normalized by average pre-reform net tangible assets. Columns (3) and (4) similarly find a small, negative insignificant effect on capital growth, using either the log of net assets (column (3)) or the log of net tangible assets (column (4)) as the dependent variable. Finally, column (5) finds again a small, negative, insignificant effect of mandated profit-sharing on firms' capital-labor ratio, which can also be seen graphically in Online Appendix Figure D.14.

This finding of a small negative investment response is consistent with the simple calibration of the model described in Section II.B. As shown in this section, the cost of capital of a firm

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subject to profit-sharing is given by:

$$R = \underbrace{\delta + \frac{r}{1-\tau}}_{\text{standard user cost}} + \underbrace{\phi(r_e - \rho)\frac{\gamma(1-\lambda)}{1-\gamma(1-\tau)}}_{\text{profit-sharing distortion}}, \tag{10}$$

and a simple calibration of the profit-sharing distortion term shows that it represents a small 0.44 percentage point wedge in the cost of capital, or less than 2% of the calibrated standard user cost. Given the typical semi-elasticity of investment rate to the cost of capital in the literature (Chodorow-Reich 2025), this increase in the cost of capital should lead to a decrease in investment of 0.85 percentage points or less, which is in line with the finding in Table VI of a small, negative, and insignificant effect on investment.

3. Robustness. In Online Appendix Table D.4, columns (7) and (8), we confirm the robustness of our findings on investment and productivity on the balanced sample of firms. In addition, Online Appendix Figure D.8 and Online Appendix Table D.5 show that our results are robust to various definitions of the control and treatment groups. Consistent with the small effect on productivity and investment, we find no systematic effects on firm size, measured by value added. Online Appendix Figure D.15 shows that treated and control firms followed the same trend in terms of size before and after the 1990 reform.

V.E. Triple-Difference Analysis

After the 1991 reform, firms with more than 50 employees are required to share profits with their employees. Whether this requirement is binding in expectation depends on whether the firm typically generates excess profits (i.e., fiscal net income above 5% of book equity). In the data, excess profits are highly persistent: Online Appendix Figure D.16 shows that across all firms and years, there is an 80% chance that a firm generates positive excess profit in year t conditional on generating excess profits in year $t-1$. This persistence implies that the requirement to share profits bears more on certain firms (the ones that typically generate higher excess profits) than others. We can use this finding to further refine our identification strategy.

We define $\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}$ as the firm-level average ratio of the reconstructed regulatory formula for profit-sharing to value added,

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computed over the pre-reform period. Firms with a higher $\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}$ are firms that generated larger excess profits before the reform. Since excess profits are persistent, we expect firms with a higher $\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}$ in the treatment group to be more "exposed" to the reform. For these firms, mandated profit-sharing is more likely to lead to actual profit-sharing. We estimate the following triple-difference model:[33]

$$
\begin{aligned}
Y_{icst} = {} & \phi^T \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}} \right)_i . \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\
& + \phi^C \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}} \right)_i . \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\
& + \xi^T \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \xi^C \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\
& + \nu \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}} \right)_i . \mathbb{1}_{\{t \geqslant 1991\}} \\
& + \alpha_i + \delta_{ct} + \mu_{st} + \omega_{icst}. \qquad (11)
\end{aligned}
$$

Equation (11) relies on a weaker identifying assumption than our main specification (equation (6)) since it compares, within each control and treatment group, firms with high versus low average pre-reform excess profits. This within-size bucket comparison further alleviates concerns that our main estimates may capture some size effects. Table VII reports the estimates for ϕ^T and ξ^T. These results are also presented graphically in event-study format in Online Appendix Figure D.17. The discussion below focuses on the table, but the graphical evidence conveys the same pattern. Column (1) shows that for firms with negative average excess profits pre-reform, being in the treatment group increases the probability of profit-sharing by a significant 17 percentage points, which represents about half of the overall treatment effect (30 percentage points in Table III, Panel C, column (1)). Column (1) also shows that the treatment effect is significantly larger for firms with higher average pre-reform excess profits. For a firm with an average pre-reform formula of about 1.3% of its value added,

33. Note that the specification in equation (11) does not include the interaction of $\left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}} \right)_i \times \mathbb{1}_{\{i \in \text{Treated}\}}$ and $\left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}} \right)_i \times \mathbb{1}_{\{i \in \text{Control}_l\}}$ because these terms are absorbed by the firm fixed effects α_i.

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TABLE VII

HETEROGENEOUS EFFECTS ON EXPOSED FIRMS

	$\mathbb{1}_{\{\text{Profit-sharing}>0\}}$	$\frac{\text{Profit-sharing}}{\text{Value added}}$	$\frac{\text{Wage}}{\text{Value added}}$	$\frac{\text{Tot. compensation}}{\text{Value added}}$	$\frac{\text{Profits}}{\text{Value added}}$	$\frac{\text{Taxes}}{\text{Value added}}$	Investment rate	TFP Ackerberg-Caves-Frazer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Relative to large control								
Treatment × post	0.1926***	−0.0001	0.0009	0.0005	0.0012	0.0008	0.0013	−0.0029
	(0.0127)	(0.0002)	(0.0028)	(0.0027)	(0.0031)	(0.0008)	(0.0053)	(0.0071)
Treatment × post × $\frac{\text{Formula}_{pre}}{\text{Value added}}$	17.0758***	0.4999***	0.0074	0.4857**	−0.6432***	−0.2260***	0.4025	−0.3808
	(0.9620)	(0.0216)	(0.2057)	(0.1936)	(0.2208)	(0.0778)	(0.3595)	(0.5101)
Panel B: Relative to small control								
Treatment × post	0.1593***	0.0014***	0.0023	0.0035	0.0001	−0.0005	−0.0080	−0.0003
	(0.0094)	(0.0001)	(0.0026)	(0.0026)	(0.0028)	(0.0008)	(0.0053)	(0.0064)
Treatment × post × $\frac{\text{Formula}_{pre}}{\text{Value added}}$	9.3580***	0.2170***	0.0348	0.2941	−0.4123**	−0.1587**	0.2892	−0.3655
	(0.7736)	(0.0162)	(0.2024)	(0.1961)	(0.2089)	(0.0771)	(0.3553)	(0.4771)
Panel C: Relative to both groups								
Treatment × post	0.1746***	0.0007***	0.0016	0.0021	0.0006	0.0001	−0.0036	−0.0015
	(0.0098)	(0.0002)	(0.0023)	(0.0023)	(0.0025)	(0.0007)	(0.0046)	(0.0057)
Treatment × post × $\frac{\text{Formula}_{pre}}{\text{Value added}}$	12.7851***	0.3329***	0.0247	0.3843**	−0.5180***	−0.1911***	0.3296	−0.3696
	(0.7864)	(0.0170)	(0.1772)	(0.1689)	(0.1870)	(0.0666)	(0.3117)	(0.4200)

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TABLE VII
CONTINUED

	$\mathbb{1}_{\{\text{Profit-sharing}>0\}}$	$\frac{\text{Profit-sharing}}{\text{Value added}}$	$\frac{\text{Wage}}{\text{Value added}}$	$\frac{\text{Tot. compensation}}{\text{Value added}}$	$\frac{\text{Profits}}{\text{Value added}}$	$\frac{\text{Taxes}}{\text{Value added}}$	Investment rate	TFP Ackerberg-Caves-Frazer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.53	0.50	0.57	0.57	0.35	0.58	0.16	0.87
Observations	132,366	127,600	123,480	123,464	124,304	124,346	123,614	122,086

Notes. This table reports intent-to-treat estimates of the impact of mandatory profit-sharing at the firm level, for different levels of ex ante exposure to the reform. $\mathbb{1}_{\{\text{Profit-sharing}>0\}}$ is a dummy variable equal to one if a firm reports a strictly positive amount of profit-sharing on its income statement. $\frac{\text{Tot. compensation}}{\text{Value added}}$ is the ratio of firms' wage bill plus profit-sharing to its value added. $\frac{\text{Profits}}{\text{Value added}}$ is the ratio of firms' net income to its value added. $\frac{\text{Taxes}}{\text{Value added}}$ is the ratio of firms' corporate income tax to its value added. All ratios are defined only for firms with positive value added. The investment rate is defined as capital expenditures in tangible assets normalized by the net value of tangible assets. TFP stands for total factor productivity; it is computed using the method designed by Ackerberg, Caves, and Frazer (2015) separately for each two-digit industry using the NAF classification measured in 1990. Treatment is a dummy variable equal to one if a firm has 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. $\overline{\frac{\text{Formula}_{pre}}{\text{Value added}}}$ corresponds to the average pre-reform ratio of the firm-level profit-sharing formula normalized by its value added. We estimate the following equation using OLS:

$$\begin{aligned} Y_{icst} \;=\; & \phi^T \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}\right)_i . \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \phi^C \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}\right)_i . \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} \\ & + \xi^T \mathbb{1}_{\{i \in \text{Treated}\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \xi^C \mathbb{1}_{\{i \in \text{Control}_l\}} . \mathbb{1}_{\{t \geqslant 1991\}} + \nu \left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}\right)_i . \mathbb{1}_{\{t \geqslant 1991\}} \\ & + \alpha_i + \delta_{ct} + \mu_{st} + \omega_{icst} . \end{aligned}$$

α_i corresponds to firm fixed effects, δ_{ct} to county-by-year fixed effects, and μ_{st} to industry-by-year fixed effects. Panel A reports ξ^T and ϕ^T when the large control is the reference group. Panel B reports ξ^T and ϕ^T when the small control is the reference group. Panel C reports ξ^T and ϕ^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees in 1989 and 1990. The small control group consists of firms with 35–45 employees in 1989 and 1990. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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the probability of sharing profits post-reform almost doubles relative to firms with negative pre-reform excess profits. Column (2) shows even stronger effects on the amount of profit-sharing paid by firms. Columns (1) and (2) thus confirm the premise of this triple-difference analysis: treated firms with larger pre-reform excess profits are more exposed to the policy.

Columns (3)–(6) reinforce our main findings on incidence. They show that relative to control firms, treated firms with larger average pre-reform excess profits experience a significant increase in their total compensation share and a significant decrease in their profit and tax shares. By contrast, columns (3)–(6) do not show any significant effect for treated firms with negative average pre-reform excess profits. Columns (7) and (8) further support the findings from Section V.D and reveal no significant effects on investment or TFP, even for treated firms more heavily exposed to the reform. Online Appendix Table D.8 estimates a similar triple-difference equation but replaces the continuous variable $\left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}\right)$ with a dummy variable equal to one if $\left(\overline{\frac{\text{Formula}_{\text{pre}}}{\text{Value added}}}\right)$ is above its in-sample median value. The results are qualitatively similar.

V.F. Avoidance

Because profit-sharing entails a net cost for firm owners, we might expect firms to attempt to avoid it. Avoidance can occur along two main dimensions: the extensive and the intensive margins.

First, consistent with our economic framework, we find evidence of avoidance at the extensive margin. All things equal, firms prefer not to share profits. As discussed in Section IV, firms reduced their employment below the regulatory threshold (100 employees) before 1991 so that they were no longer mandated to share profits. Figure VII shows that this is also the case around the 50-employee threshold, after the reform. This figure plots the probability that the firm's employment in $t + 1$ is strictly below the 50-employee cutoff as a function of employment in year t. Panel A focuses on the period around the reform. The gray diamonds correspond to $t = 1989$, so $t + 1$ is the last year before the reform. The dark circles correspond to $t = 1990$, so firms with more than 50 employees in $t + 1$ now have to share profits. Firms around the 50-employee threshold (i.e., between 46 and 57 employees) be-

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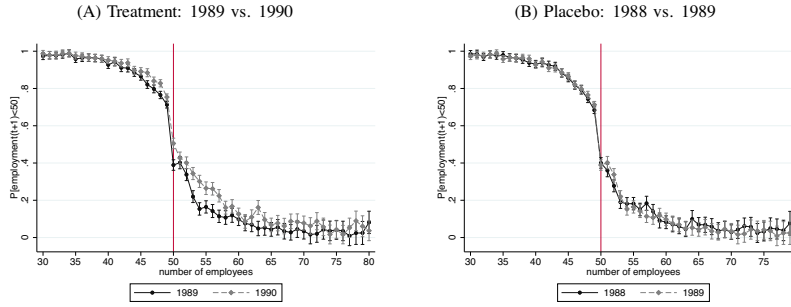


FIGURE VII

Crossing the 50 Employee Threshold

This figure shows the average probability that a firm's employment in year $t+1$ is strictly lower than 50, by employment counts in t. The sample period is 1989 and 1990 (Panel A) and 1988 and 1989 (Panel B). The vertical bars are 95% confidence intervals.

come significantly more likely to either go below or remain below the threshold once the regulation starts to kick in at 50 employees. There is thus evidence of significant avoidance of the profit-sharing regulation at the extensive margin. It remains, however, a "local" phenomenon: there is no significant increase in the probability of going below/remaining below the threshold for firms with more than 57 employees or fewer than 45 employees. Panel B reproduces this exercise on the pretreatment period and, as expected, finds no effect of this alternative "placebo" treatment. The absence of an effect in 1988 and 1989 also suggests little anticipation by firms prior to the reform.[34]

Second, we find no evidence of avoidance at the intensive margin, that is, firms intentionally reducing excess profits. Although this empirical result may appear surprising, it is consistent with our economic framework, which predicts limited distortions conditional on being subject to profit-sharing (provided the cost of equity is not far from 5%). It may also reflect that workers have strong incentives to monitor profit reporting, since any diversion of cash flows in this context directly reduces their total compensa-

34. It is worth mentioning that there was already significant bunching around the 50-employee threshold before the 1990 reform due to other regulations (see Garicano, Lelarge, and Van Reenen 2016; Aghion, Bergeaud, and Van Reenen 2021). This implies a large drop in the probability of having fewer than 50 employees in $t+1$ for firms of size 50 versus 49, both before and after the implementation of mandatory profit-sharing.

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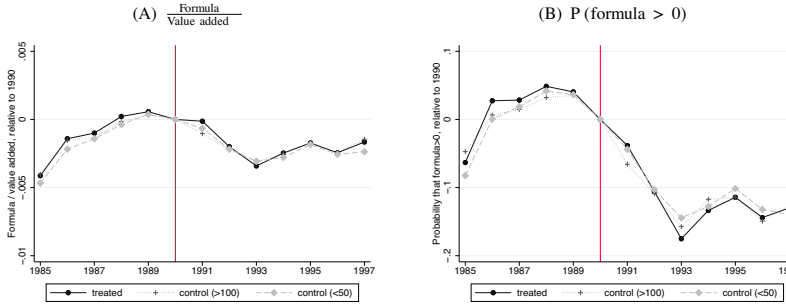


FIGURE VIII

Profit-Sharing Formula, by Treatment Status

Panel A provides the average formula to value added ratio for firms over time by treatment status. Panel B shows the probability that the formula is positive (i.e., firms have excess profits) by year and treatment status. "Formula" corresponds to the regulatory formula used to compute the total dollar amount to be shared with employees (equation (1)). Treated firms have 55–85 employees in 1989 and 1990 (dark solid dots). Control (>100) are firms with 120–300 employees in 1989 and 1990 (dotted dark gray crosses). Control (<50) are firms with 35–45 employees in 1989 and 1990 (dashed light gray diamonds).

tion (see the discussion in Section II.C). Figure VIII, Panel A plots the average realized (reconstructed) regulatory formula as a share of value added for firms in each group. If firms engage in avoidance, we would expect a significant decline in the regulatory formula for treated firms after 1991. Instead, the evolution is similar across groups over time. Panel B shows the evolution (normalized in levels to zero in 1990) of the probability that the regulatory formula is positive (and thus that firms are required to share some of their profits). Consistent with Panel A, we do not see any evidence of changes suggestive of avoidance. Online Appendix Table D.9 confirms these results using our regression framework. Overall, our analysis indicates that entrepreneurs did not evade the regulation by reducing or underreporting excess profits. Instead, avoidance appears confined to the extensive margin, with firms remaining just below the regulatory thresholds.

VI. EMPLOYEE-LEVEL EVIDENCE

VI.A. Effects of Profit-Sharing on Employee-Level Wages

In the last part of the article, we exploit worker-level data to investigate how mandated profit-sharing affects workers of differ-

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ent skills. We start by reproducing our estimates of the effect of the regulation on employees' compensation using worker-level data.

The matched employer-employee data do not report the amount of profit-sharing received by individual workers, so we impute it. We merge employee-level data with accounting data (which report aggregate profit-sharing). We assume that individual profit-sharing is paid proportionally to the employee's wage up to a cap determined by law. Formally, we use the following formula:

$$\text{Profit-sharing to employee } i \text{ in firm } j = \min\left(\min\left(\frac{\text{wage}_i}{\text{wage bill}_j}, \frac{\text{cap}_1}{\text{wage bill}_j}\right) \times \text{RSP}_j, \text{cap}_2\right).$$

The firm's wage bill and employee's wage are reported in the DADS; the total amount of profit-sharing paid to employees (RSP_j) is reported in the tax file. The formula above accounts for the facts that (i) the share of profit-sharing that accrues to any given employee is capped by the law (cap_1), and (ii) the total euro amount of profit-sharing an employee can earn in a given year is also capped (cap_2).

Our imputation method is justified by the practice of profit-sharing, which, most of the time, is paid in proportion to wages (DARES 2008).[35] We statistically verify that our imputation method is sensible using employee-level data on profit-sharing payouts for 2005. The ECMOSS survey provides employee-level information on wages and profit-sharing payouts for 94,948 employees in 8,820 firms. In Online Appendix Figure D.18, we show the regression result of the individual profit-sharing-to-wage ratio on the firm-level total profit-sharing payouts-to-total wages ratio (what we use for imputation). We restrict ourselves to firms with more than 50 employees with at least 12 employees observed in the survey. We find a slope coefficient of one and an R^2 of 85%, vindicating our imputation method.

Using this imputation, Table VIII replicates the firm-level incidence analysis of Section V with employee-level data, allowing us to control for worker characteristics. We begin with the firm

35. In 2008, 37% of employees received profit-sharing proportionally to their wage, 5% proportionally to their tenure in the firm, and 3% uniformly; finally 54% of employees received profit-sharing based on a formula including mostly a proportional component.

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TABLE VIII

EFFECTS ON WAGE AND TOTAL COMPENSATION AT THE EMPLOYEE LEVEL

	Log(wage)				Log(total compensation)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Relative to large control								
Treatment × post	0.0029	0.0038	0.0043	0.0044	0.0127***	0.0141***	0.0153***	0.0155***
	(0.0037)	(0.0028)	(0.0031)	(0.0029)	(0.0037)	(0.0029)	(0.0033)	(0.0031)
Panel B: Relative to small control								
Treatment × post	−0.0005	−0.0016	−0.0013	−0.0021	0.0082*	0.0076**	0.0080*	0.0072*
	(0.0048)	(0.0036)	(0.0041)	(0.0038)	(0.0048)	(0.0037)	(0.0042)	(0.0039)
Panel C: Relative to both groups								
Treatment × post	0.0022	0.0027	0.0031	0.0030	0.0118***	0.0127***	0.0138***	0.0137***
	(0.0035)	(0.0027)	(0.0030)	(0.0028)	(0.0035)	(0.0028)	(0.0031)	(0.0029)
Employee controls	No	Yes	No	Yes	No	Yes	No	Yes
Employee FEs	No	No	Yes	Yes	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.32	0.63	0.84	0.85	0.32	0.63	0.84	0.84
Observations	436,970	436,335	383,306	382,696	436,820	436,186	383,158	382,549

Notes. This table reports intent-to-treat estimates of the effect of mandatory profit-sharing on workers' compensation at the employee level. Wage is the employee's daily gross wage. Total compensation is the sum of the employee's daily gross wage and the daily amount of profit-sharing received during the year. Treatment is a dummy variable equal to one if the firm the employee works for has 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. We estimate the following equation using OLS:

$$Y_{jicst} = \alpha_i + \phi_j + \zeta X_{jt} + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}.$$

α_i corresponds to firm fixed effects, ϕ_j to employee fixed effects, δ_{ct} to county-by-year fixed effects, and μ_{st} to industry-by-year fixed effects. The employee-level controls X_{jt} include gender, age, age^2, tenure, tenure^2, experience, experience^2, and the employee's two-digit occupation. Panel A reports β^T when the large control is the reference group. Panel B reports β^T when the small control is the reference group. Panel C reports β^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees. The small control group consists of firms with 35–45 employees. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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sample from Section V and use the same treatment assignment: treated if firm employment was 55–85 in both 1989 and 1990; small control group if 35–45; and large control group if 120–300. We merge this sample with the employer-employee data set, which provides wages and characteristics for a 4% random sample of each firm's employees. Finally, we estimate the following intent to treat equation using OLS:

$$Y_{jicst} = \alpha_i + \phi_j + \zeta X_{jt} + \delta_{ct} + \mu_{st} + \beta^T \mathbb{1}_{\{i \in \text{Treated}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \beta^C \mathbb{1}_{\{i \in \text{Control}\}} \times \mathbb{1}_{\{t \geqslant 1991\}} + \epsilon_{icst}. \quad (12)$$

j indexes employees, i the firm they work for, c the county where they are employed, s the industry of their establishment and t the year of observation. In columns (1) and (5), we include α_i (firm fixed effects), δ_{ct} (county-by-year fixed effects), and μ_{st} (industry-by-year fixed effects). In columns (2) and (6), we add employee-level controls X_{jt}: gender, age, age^2, tenure, tenure^2, experience, experience^2, and the employee's two-digit occupation code. In columns (3) and (7), we include employee fixed effects ϕ_j but do not include employee-level controls. Columns (4) and (8) include all controls. Columns (1)–(4) use log daily gross wage as the dependent variable; columns (5)–(8) use log daily total compensation (the sum of gross wage and profit-sharing).

Consistent with the firm-level analysis, we find a null effect on daily gross wages and a positive, statistically significant effect on total compensation in all four specifications. Relative to the large control group, employees working for firms in the treatment group experience an increase in their total compensation of about 1.3% to 1.5%, depending on the control variables included. The effect is smaller and less precisely estimated when estimated against the small control group (about 0.7%–0.8%). We therefore confirm with employee-level data that the wage incidence of mandated profit-sharing is, on average, zero.

Interestingly, estimates with and without individual-level controls tend to be very similar. Online Appendix Table D.10 displays the evolution of workers' characteristics, such as age, experience, and occupation, for the three groups of firms. Overall, we do not find evidence of a change in workers' observable characteristics in firms sharing their profits after the reform. We conclude that the increase in workers' total compensation found in Section V is not the result of a change in workforce composi-

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tion. This confirms that profit-sharing had little incidence on base wages.

VI.B. Profit-Sharing and Employee Risk Exposure

One potential explanation for our finding that mandatory profit-sharing does not affect wages is that profit-sharing might have low value for risk-averse workers. However, the data suggest this explanation is unlikely: Profit-sharing introduces little additional risk to workers' total compensation. To assess this more formally, we proceed in two steps.

First, we examine the extent to which profit-sharing contributes to labor income volatility over time. We construct a panel of workers employed in large firms (more than 100 employees) in 1989, who were thus exposed to mandatory profit-sharing. We estimate that the standard deviation of the growth of total compensation is, on average, 3.2% higher than for wages. This modest increase in volatility reflects two factors. First, profit-sharing represents a small fraction of total compensation—around 4% on average—so even if profits are significantly more volatile than wages, it has limited impact on overall income risk. Second, profit-sharing is persistent over time: as shown in Online Appendix Figure D.16, 80% of firms with excess profits in year $t-1$ also report excess profits in year t.

Second, we assess how much workers actually value profit-sharing. Because our regressions use log compensation as the dependent variable and find a positive effect of mandatory profit-sharing, the results imply a utility gain for workers under a standard constant relative risk aversion (CRRA) utility function with relative risk aversion (RRA) of one. In this case, log compensation directly maps to utility, so the increase in expected income outweighs the added risk. To quantify this, we estimate the average certainty equivalent of mandated profit-sharing in our sample, assuming a CRRA utility function. Online Appendix C describes the methodology, and Online Appendix Table D.11 reports the ratio of the estimated certainty equivalent to the actual average amount of profit-sharing received by workers, across different values of relative risk aversion. In a simple log specification (RRA $= 1$), 1 euro of profit-sharing is worth 89 cents. Said differently, workers would be ready to pay 11 cents per euro of profit-sharing to eliminate risk. Even under a high RRA of five, the value remains substantial: 1 euro of profit-sharing is still worth 62 cents.

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Overall, these results suggest that the additional risk introduced by profit-sharing is modest and does not materially reduce its value to workers. This reinforces our conclusion that risk considerations cannot explain the lack of wage incidence from mandatory profit-sharing.

VI.C. Heterogeneity Analysis

Another interpretation for the low incidence of mandated profit-sharing on wages is that firms might have a hard time reducing wages (or slowing down their increase). This is especially relevant in France, where the minimum wage is high and binding and about 98% of workers are covered by collective agreements (OECD and AIAS 2021). These institutions considerably limit the ability of firms to reduce wages or wage growth for low- and middle-skill workers (Avouyi-Dovi, Fougère, and Gautier 2013; Fougère, Gautier, and Roux 2018; Gautier, Roux, and Castillo 2022). At the same time, there is more flexibility for high-skill white collars (the "cadres").[36] To test this hypothesis, we compare how higher- and lower-skill workers are affected by mandated profit-sharing.[37]

We split employees into three categories based on their occupation, available in the employer-employee data: (i) lower skill (clerks and blue-collar workers), our reference group; (ii) intermediate skill (supervisor or skilled technicians); and (iii) high skill (managers, engineers, and executives). We augment equation (12) by interacting the Treatment and Post dummies with the High Skill and Intermediate Skill dummies. Table IX reports the baseline treatment effect (Treatment $\times$ Post) and the triple interactions. Columns (1) and (4) reproduce the average results for the entire workforce (and are thus identical to Table VIII, columns (1) and (5)). In columns (2) and (4), we estimate the effect by skill group using only firm, industry-year, and county-year fixed effects. In columns (3) and (6), we add time-varying employee-

36. Contrary to other workers, the "cadres" are not paid by the hour. They typically have more autonomy and responsibility in their work. As a result, there is more flexibility in their compensation.

37. We use occupations, rather than alternative measures based on wages, for two main reasons. First, occupations in France are hierarchical and persistent over time, providing a good proxy for wages without relying directly on the outcome of interest. Second, collective agreements rely heavily on occupations, rather than wages, to determine wage floors.

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TABLE IX

EFFECTS ON EMPLOYEE WAGE AND TOTAL COMPENSATION: LOW VERSUS HIGH SKILL

	Log(wage)			Log(total compensation)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Relative to large control						
Treatment × post	0.0029	0.0050	0.0066**	0.0127***	0.0153***	0.0170***
	(0.0037)	(0.0032)	(0.0029)	(0.0037)	(0.0033)	(0.0030)
Treatment × post × intermediate skill		−0.0019	−0.0041		−0.0020	−0.0042
		(0.0085)	(0.0077)		(0.0086)	(0.0078)
Treatment × post × high skill		−0.0261*	−0.0276**		−0.0303**	−0.0310**
		(0.0152)	(0.0138)		(0.0153)	(0.0139)
Panel B: Relative to small control						
Treatment × post	−0.0005	−0.0052	−0.0021	0.0082*	0.0046	0.0076*
	(0.0048)	(0.0044)	(0.0039)	(0.0048)	(0.0045)	(0.0040)
Treatment × post × intermediate skill		0.0087	0.0060		0.0081	0.0055
		(0.0124)	(0.0109)		(0.0125)	(0.0110)
Treatment × post × high skill		−0.0206	−0.0194		−0.0275	−0.0261
		(0.0229)	(0.0207)		(0.0230)	(0.0207)

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TABLE IX
CONTINUED

	Log(wage)			Log(total compensation)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Relative to both groups						
Treatment × post	0.0022	0.0029	0.0048*	0.0118***	0.0131***	0.0151***
	(0.0035)	(0.0031)	(0.0028)	(0.0035)	(0.0031)	(0.0029)
Treatment × post × intermediate skill		0.0001	−0.0021		−0.0001	−0.0023
		(0.0083)	(0.0075)		(0.0084)	(0.0076)
Treatment × post × high skill		−0.0250*	−0.0262*		−0.0296**	−0.0301**
		(0.0149)	(0.0135)		(0.0150)	(0.0136)
Employee controls	No	No	Yes	No	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.32	0.56	0.63	0.32	0.56	0.63
Observations	436,970	436,970	436,335	436,820	436,820	436,186

Notes. This table reports intent-to-treat estimates of the effect of mandatory profit-sharing on workers' compensation at the individual level, by skill. Wage is the employee's daily gross wage. Total compensation is the sum of the employee's daily gross wage and the daily amount of profit-sharing received during the year. Treatment is a dummy variable equal to one if the firm the employee works for has 55–85 employees in 1989 and 1990. Post is a dummy variable equal to one after 1990, the introduction of the reform. Intermediate skill is a dummy equal to one if the employee's job description is supervisor or skilled technician. High skill is a dummy equal to one for managers, engineers, and executives. The baseline skill includes clerks and blue-collar workers. We augment the regression model in Table VIII by interacting the Treatment and Post dummies with the High skill and Intermediate skill dummies. We only report the baseline treatment effect (Treatment × post) and the triple interactions. Columns (1) and (3) report the baseline estimates. Columns (2) and (4) report estimates separately by skills, controlling for firm, industry-year, and county-year fixed effects. Columns (3) and (6) also include employee-level controls X_{jt}: gender, age, age^2, tenure, tenure^2, experience, and experience^2. Panel A reports the treatment effect relative to the large control group and Panel B relative to the small control group. Panel C reports β^T when both small and large firms are used as controls. The large control group consists of firms with 120–300 employees in 1989 and 1990. The small control group consists of firms with 35–45 employees in 1989 and 1990. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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level controls (gender, age, age^2, tenure, tenure^2, experience, experience^2).[38]

Table IX shows that mandated profit-sharing does not increase the total compensation of high-skill workers. Relative to lower-skill workers in large control firms, the base wage of high-skill workers in treated firms decreases significantly by 2.5% once their employers become subject to profit-sharing requirements (Panel C, columns (2) and (3)). Estimates using small control firms are less precise but yield effects of similar magnitude. Overall, both control groups yield estimates of similar magnitude.[39]

As a result of this adjustment to their base wage, the total compensation of high-skill workers in treated firms does not increase following the introduction of mandatory profit-sharing (columns (5) and (6)). In contrast, we find that the increase in total compensation induced by mandated profit-sharing is concentrated among low- and intermediate-skill workers.

The estimates in Table IX correspond to intent-to-treat effects. We report 2SLS estimates (i.e., estimates of the effect of profit-sharing instrumented by our policy shock) in Table X. To benchmark our results, we report in columns (1) and (4) OLS estimates of profit-sharing on average wages and total compensation. This specification corresponds to standard cross-sectional regressions implemented in the literature on profit-sharing. Columns (2) and (5) present LATE estimates for the whole population (irrespective of skills). Profit-sharing does not affect wages, but increases total compensation by a highly significant 3.5%.

We finally explore the LATE estimates by skill level in columns (3) and (6). We find that the response of wages to profit-

38. We control for a rich set of covariates to account for individual heterogeneity instead of using a worker fixed effect. Given the somewhat limited size of the sample, there are relatively few moves across firms, making it difficult to estimate a standard two-way fixed effects model. Keeping this caveat in mind, including a worker fixed effect yields qualitatively similar results, albeit estimated with less precision.

39. Interestingly, Fuest, Peichl, and Siegloch (2018) find that wages respond significantly to the corporate income tax in Germany, whereas our empirical analysis finds no incidence of profit-sharing on wages. Although these results may appear contradictory, they likely reflect the differences in labor market institutions between the countries. Wage rigidity is pervasive in the French labor market (see Gautier, Roux, and Castillo 2022 or Fougère, Gautier, and Roux 2018), which is also characterized by a high binding minimum wage. In France, the share of workers covered by collective agreements exceeds 95%, whereas it is approximately 50% in Germany (OECD economic indicators).

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TABLE X

EFFECTS ON EMPLOYEE WAGE AND TOTAL COMPENSATION: LOW VERSUS HIGH SKILL, IV ESTIMATES

	Log(wage)			Log(total compensation)		
	OLS (1)	IV (2)	IV (3)	OLS (4)	IV (5)	IV (6)
$\mathbb{1}_{\{\text{profit-sharing}>0\}}$	0.0019	0.0073	0.0137*	0.0281***	0.0352***	0.0423***
	(0.0015)	(0.0075)	(0.0079)	(0.0016)	(0.0076)	(0.0081)
$\mathbb{1}_{\{\text{profit-sharing}>0\}} \times \mathbb{1}_{\{\text{Intermediate Skill}\}}$			−0.0057			−0.0071
			(0.0209)			(0.0211)
$\mathbb{1}_{\{\text{profit-sharing}>0\}} \times \mathbb{1}_{\{\text{High-Skill}\}}$			−0.0822*			−0.0939**
			(0.0428)			(0.0437)
Employee controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
K-P F-stat.		1,173	393		1,173	393
K-P F-stat. (intermediate)			195			195
K-P F-stat. (high skill)			67			67
Null effect on high-skill (p-value)			.103			.228
Observations	436,215	436,215	436,215	436,186	436,186	436,186

Notes. This table provides the sensitivity of log wage (respectively log total compensation) to profit-sharing at the individual level, by skill. Wage is the employee's daily gross wage. Total compensation is the sum of the employee's daily gross wage and the daily amount of profit-sharing received during the year. $\mathbb{1}_{\{\text{Profit-sharing}>0\}}$ is a dummy variable equal to one if the firm the employee works for reports a strictly positive amount of profit-sharing on its income statement. Intermediate skill is a dummy equal to one if the employee's job description is supervisor or skilled technician. High skill is a dummy equal to one for managers, engineers, and executives. The baseline skill includes clerks and blue-collar workers. We augment the regression model in Table VIII by interacting profit-sharing with the High skill and Intermediate skill dummies. Columns (1) and (4) are OLS estimates. In columns (2), (3), (5), and (6), estimates instrument profit-sharing variables using the interactions between a Treatment dummy, equal to one if the firm the employee works for has 55–85 employees in 1989 and 1990; a Post dummy, equal to one after 1990; and the skill dummies. All regressions include the following employee-level controls: gender, age, age^2, tenure, tenure^2, experience, experience^2, and the employee's two-digit occupation. All regressions also include firm fixed effects, industry-year fixed effects, and county-year fixed effects. The parameters are estimated using a 2SLS procedure. K-P F-stat. stands for Kleibergen-Paap F-statistics. Standard errors (in parentheses) are clustered by firm. * $p < .10$, ** $p < .05$, *** $p < .01$.

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sharing is negative for high-skill workers but zero for lower-skill ones. As a result, the effect on total compensation is strongly positive for low- and medium-skill workers but not statistically different from zero for the average high-skill worker. In Online Appendix Table D.12, we explore robustness of this finding using a continuous transformation of profit-sharing instead of a dummy (we use a square root transformation to accommodate zeros). This alternative specification provides qualitatively similar results.

Overall, the results in this section depict a consistent picture: (i) on average, workers' base wages remain unchanged when their firm becomes subject to mandatory profit-sharing; (ii) total compensation increases; (iii) the resulting increase in earnings volatility is small; and (iv) these effects are concentrated among low- and medium-skill workers, while higher-skill workers benefit significantly less from mandated profit-sharing. One possible interpretation is that wage rigidity at the lower end of the skill distribution limits firms' ability to substitute between wages and profit-sharing, making the French mandatory profit-sharing scheme more progressive within firms.

VII. Conclusion

This article evaluates the economic effects of a large mandated profit-sharing scheme in France on workers and firms. This scheme requires all firms above a certain size threshold to pay about one-fourth of their excess profits (net income in excess of 5% of book equity) to their employees, proportionally to their wages. We identify the effects of the scheme by exploiting a 1990 reform that reduced the requirement threshold from 100 employees to 50 employees. Our empirical analysis uncovers several important findings.

At the firm level, we find that the total compensation share (wage bill plus profit-sharing over value added) increases by about 1.8 percentage points and that more than three fourths of this increase in labor cost is borne by firm owners through reduced profits. This increase in total compensation at the firm level conceals some heterogeneity. Using worker-level data, we show that high-skill workers (managers, engineers, and executives) do experience a reduction in fixed wages that matches the average profit-sharing they receive through the scheme. In contrast, lower-skill workers fully benefit from firms' requirement to share profits with no incidence on their base wage. A possible interpretation is that lower-

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skill workers' wages exhibit significant rigidity, which is especially plausible in an economy with extended collective agreements and a binding minimum wage. Overall, mandated profit-sharing, despite its tax advantages, represents a net cost for firm owners, a finding we confirm by showing significant bunching below the 100-employee threshold before 1991.

We also exploit this setting to analyze the real effects of profit-sharing. We find that, despite its large magnitude for firm owners—about 10.5% of pretax income for firms with positive excess profits—mandated profit-sharing has a very limited effect on firms' productivity and investment. Overall, our analysis suggests that mandated profit-sharing is essentially a redistributive tool, which acts as a non-distortive tax on owners directly paid to employees, particularly lower-skilled ones, in the same firms.

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The data and code underlying this article is available in Nimier-David, Sraer, and Thesmar (2026), in the Harvard Dataverse, https://doi.org/10.7910/DVN/7C8MVB.

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