Does Tax-Benefit Linkage Matter for the Incidence of Payroll Taxes?*

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Abstract

We analyze earnings responses to six large payroll tax and income tax reforms in France. Our findings indicate full pass-through to workers when there is a strong and transparent link between contributions and expected benefits. In contrast, employer payroll taxes with no tax-benefit linkage exhibit limited pass-through to workers, while income tax nominally borne by employees show nearly full pass-through. Together with a meta-analysis of the literature, we interpret these results as empirical support for the long-standing hypothesis that tax-benefit linkage matters for the incidence of payroll taxes. In the absence of such linkage, our findings suggest that the individual-level incidence of payroll taxes aligns with their statutory incidence.

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Introduction

According to the OECD definition, payroll taxes are "compulsory payments paid to general government that confer *entitlement* to receive a future social benefit."[1] These tax represent an important part of total tax revenues in OECD countries (on average 26% of total tax revenues in 2021, or 9% of GDP), and an even larger share in countries with extended social insurance systems (33% of total tax revenues in France, or 15% of GDP).

Tax-benefit linkage, i.e., the extent to which contributions are tied to future benefits, is the key element that distinguishes payroll taxes from alternative forms of labor income taxation. In its purest form, a payroll tax can be described as a *quid pro quo* tax providing actuarially fair benefits.[2] There are, however, many different institutional settings that can lead to various degrees of tax-benefit linkage, from the U.K. National insurance contributions, which have very little linkage to individual benefits, to the Swedish Notional Defined Contributions system, which offers actuarially fair pensions.

A longstanding question in public economics revolves around determining the incidence of payroll taxes, i.e., who ultimately bears the burden, given that their statutory incidence is often divided between employers and employees. This is crucial for evaluating the redistributive consequences of various policies affecting the payroll tax schedule, such as payroll tax cuts for specific workers or industries, or increases to fund additional pension benefits. Traditionally, economists have relied on the standard textbook model to argue that statutory incidence does not matter, and that the ultimate tax burden can be shifted to either side depending on the elasticities of supply and demand. According to this conventional view, payroll taxes are expected to be mostly shifted to workers, as labor supply elasticities are generally considered to be dwarfed by demand elasticities. Empirical evidence on this matter is, however, mixed: while some studies find complete shifting of employer payroll taxes to employees (Gruber and Krueger, 1991; Gruber, 1994, 1997; Anderson and Meyer, 1997, 2000; Kim et al., 2022), others report only limited shifting (Bennmarker et al., 2009; Kugler and Kugler, 2009; Saez et al., 2012; Lehmann et al., 2013; Egebark and Kaunitz, 2018; Adam et al., 2019; Saez et al., 2019; Benzarti and Harju, 2021; Rubolino, 2022). An unanswered, yet important, question is how the specific design of payroll taxes, i.e., the degree of linkage to individual benefits, and its interaction with statutory incidence, matters for whether these taxes are shifted to wages and whether it plays a role in explaining the heterogeneous results in the literature.

[1] In this paper, we adopt the term "payroll taxes" to refer to Social Security contributions (SSCs), aligning with the U.S. convention. In several OECD countries, such as France and Sweden, specific payroll taxes exist alongside SSCs but with a distinct design, as they contribute to general government expenditures and do not provide entitlement to social insurance.

[2] See for instance the definition provided by R. A. Musgrave : "'Social Security' in *our* sense means *mandatory* provision for economic contingencies, financed out of *contributions* on a *quid pro quo* basis. Thus, all those subject to certain contingencies must contribute and the actuarial value of each person's benefits must match the cost of his contribution." (Musgrave, 1968, p. 24).

This paper provides evidence on the role played by tax-benefit linkage in determining tax incidence, whether the nominal burden of payroll taxes falls on employers or workers. Based on a long panel of administrative data, we estimate the individual-level pass-through of payroll taxes to wages using six large tax reforms in France over a period of forty years. Two of these reforms concern both employer and employee payroll taxes with a strong link between contributions paid and expected benefits, while two others concern employer payroll taxes with no tax-benefit linkage. The remaining two reforms are related to income taxation. These reforms closely resemble the two payroll tax reforms without linkage, as they exclusively impact labor earnings and distinctly lack any tax-benefit linkage. The first is a corporate income tax credit based on firms' payroll expenses, akin to an employer payroll tax cut affecting only workers below a certain earnings threshold. The second is an increase in the flat-rate income tax, similar to an employee payroll tax increase.

To estimate incidence, we implement difference-in-differences analyses for all these reforms. For the payroll tax reforms and the employer tax credit on payroll taxes, we compare wage earners above and below the relevant earnings thresholds, before and after the changes. For the flat-rate income tax reform, we compare workers with earnings subject to the income tax increase—those receiving profit-sharing bonuses—to other workers whose earnings were not affected. These approaches allow us to compare, for each reform, the changes in labor costs to the corresponding changes in posted earnings, to assess how much of the variation in payroll tax (or income tax) has been shifted to treated workers' earnings. By conducting separate estimations for each year post-reform, we can identify effects up to nine years after the initial implementation, providing insights into incidence in the medium to long term.

The central finding of our study is that payroll taxes with clear tax-benefit linkage are fully shifted to workers, a claim long made by the literature (Musgrave, 1959; Summers, 1989) but not backed by empirical evidence to date. In contrast, in the absence of tax-benefit linkage, our results indicate that economic incidence coincides with statutory incidence at the individual level, meaning that employee payroll taxes and income taxes are borne by workers, while employer payroll taxes are borne by employers.

Specifically, for the two payroll tax reforms with strong tax-benefit linkage, we find that increases in both employer and employee taxes were fully passed through to affected workers within five to six years. By contrast, for reforms without tax-benefit linkage, incidence at the individual level was aligned with statutory incidence. In the case of the income tax reform nominally incident on employees, the tax increase was fully passed through to workers in the form of lower earnings. Conversely, increases in employer payroll taxes without tax-benefit linkage and the corporate income tax credit (akin to an employer payroll tax cut) had no impact on workers' earnings. While our results, which are robust to a wide range of sensitivity checks, do not necessarily invalidate standard incidence analysis at a broader level of aggregation—such as firm-level or market-level

incidence—they challenge the conventional view that payroll taxes are entirely passed through to workers at the individual level.

To strengthen the external validity of our findings beyond the French context, we conduct a meta-analysis of existing studies on the incidence of payroll and income taxes. This analysis takes into account both the presence of tax-benefit linkage and the statutory incidence of these taxes. In our examination of twenty-six estimates of pass-through rates of employer payroll taxes to workers, we find an average pass-through of 103% for the nine payroll tax reforms with strong tax-benefit linkage. In contrast, the average pass-through is 16% for the payroll tax reforms with no tax-benefit linkage. This difference in pass-through is statistically significant and robust to controlling for potential confounders such as macroeconomic and labor market conditions. We then examine eight estimates of the pass-through of employee payroll taxes or income taxes. Here, we find an average pass-through to workers of 98%, with no statistically significant difference based on tax-benefit linkage. Hence, tax-benefit linkage appears as an empirically relevant factor in reconciling the highly heterogeneous and seemingly conflicting estimates of payroll tax incidence across existing studies. When these taxes are strongly tied to future benefits, they are fully shifted to workers, whereas in the absence of tax-benefit linkage, the tax burden at the individual level seems more aligned with statutory incidence.

Our main finding that, in the presence of strong tax-benefit linkage, payroll taxes are fully passed through to workers at the individual level, is consistent with a broad array of models of the labor market. Indeed, provided that workers perceive the link between taxes paid and future benefits entitlements, models featuring rational agents predict that such taxes will be borne by workers in the form of lower wages, regardless of other parameter values such as labor supply and demand elasticities. The underlying intuition is straightforward: when workers fully understand the expected benefits associated with payroll taxes, they do not perceive them as taxes. In line with these theoretical predictions, we find no evidence of differential exit rates from employment between treated and control groups in the case of payroll tax reforms with linkage. These results support the efficiency argument in favor of mandated benefits or strong tax-benefit linkage (Summers, 1989; Feldstein and Liebman, 2002).

In the absence of linkage, our finding that the pass-through of payroll taxes at the individual level aligns with their statutory incidence proves challenging to justify with standard models of competitive labor markets. Indeed, in such models, our pass-through estimates would imply implausibly low elasticities of substitution between treated and control workers. Instead, the modest individual-level pass-through observed in the case of employer payroll tax reforms lacking tax-benefit linkage appears more consistent with collective wage bargaining models, where intra-firm fairness norms are influenced by statutory incidence (Saez et al., 2019). Alternative explanations for these results, such as the flypaper effect (Hines and Thaler, 1995), could be considered to rationalize these

results, as could differences in the perception of employee and employer payroll taxes (Iturbe-Ormaetxe, 2015). Importantly, however, standard models do not exclude the possibility of payroll tax shifting to workers at the firm level, a scenario beyond the scope of our data but supported by recent evidence from Saez et al. (2019) in the Swedish context and Benzarti and Harju (2021) in Finland. Consistent with this interpretation, we find no evidence that payroll tax reforms without linkage affected the relative employment probabilities and job-to-job transitions of treated and control workers.

Related literature. This paper is related to four strands of the literature. First, it provides a rationale for the apparently conflicting results from previous work on payroll tax incidence. To the best of our knowledge, the role of tax-benefit linkage has not been empirically investigated in this body of research, despite being the distinctive feature of payroll taxation. Although several papers have found less than full pass-through of employer payroll taxes to workers (see references above), the proposed explanation has generally been that, depending on labor supply and demand elasticities, one could expect a wide range of pass-through estimates. We show that our empirical results are difficult to rationalize using this line of argument, whereas accounting for tax-benefit linkage can explain the disparities in pass-through estimates not only in France, but also in other countries. Second, our paper is directly related to the small literature examining the role of tax-benefit linkage in taxation design. While this literature has primarily been theoretical (Feldstein and Liebman, 2002; Iturbe-Ormaetxe, 2015), empirical evidence has only begun to emerge in recent years. Closely related to our work, French et al. (2022) examine the impact of tax-benefit linkage on labor supply by analyzing a pension reform in Poland that substantially strengthened the connection between pension contributions and pension benefits. Their findings offer compelling evidence that the introduction of a stronger contribution-benefit link positively affects labor supply. Instead, our paper focuses on evaluating the effects of tax-benefit linkage on earnings. Third, our paper complements the literature on statutory incidence (e.g., Slemrod, 2008; Saez et al., 2012; Kopczuk et al., 2016; Hargaden and Roantree, 2019; Fox et al., 2022; Jiménez et al., 2024) by providing another example in which statutory incidence is likely to matter, contrary to the tax-collection invariance prediction of the standard competitive framework. Fourth, we contribute to the literature on the distributional effects of taxation. Many policy analyses of tax reforms assume full shifting of payroll taxes to workers without considering tax-benefit linkage (Carloni, 2021). Our results challenge this assumption, indicating that when contributions are not tied to future benefits, conclusions about the redistributive effects of payroll taxes may be erroneous (Bozio et al., 2023; Saez and Zucman, 2023).

Organization. The remainder of the paper is organized as follows. Section 1 introduces the standard conceptual framework for analyzing the incidence of payroll taxes with and without tax-benefit linkage. Section 2 presents the institutional design of earnings taxation in France and provides an overview of the specific reforms under examination. Section 3

describes the administrative data we exploit and the computation of taxes from these data. Section 4 presents the empirical framework, and the results are reported in Section 5. Section 6 discusses the findings and their interpretation. Section 7 concludes.

1 Conceptual Framework

To formalize our analysis and clarify the role of tax-benefit linkage in payroll taxation, we use the standard competitive labor market model as a benchmark. While, as discussed in Section 6.2, our incidence results in the absence of linkage cannot be easily rationalized within this framework, it provides a useful starting point. In this section, we define tax-benefit linkage and examine its implications for the pass-through of payroll taxes to workers and their effect on employment.

General setup and earnings concepts. Our benchmark framework is the standard equilibrium model of tax incidence in competitive markets (Feldstein, 1974). We consider a one-sector model, in which a representative firms' output is produced using two labor inputs: treated workers (L_T), who are subject to changes in payroll taxes due to the reform under investigation, and control workers (L_C), who are unaffected by the reform. The firm's production function $F(L_T, L_C)$ is assumed to be homogeneous of degree one and to exhibit positive and diminishing marginal products. A key parameter of this general production function is the elasticity of substitution between the two types of workers, which we denote by σ.

For simplicity, we assume that all payroll taxes are nominally paid by the firm, and we denote by w_k the posted, or gross wage, of each worker of type k, where $k \in \{T, C\}$.[3] The corresponding labor cost to the firm is denoted by $z_k \equiv w_k(1 + \tau_k)$, where τ_k is the rate of payroll taxes nominally paid by the firm for a worker with posted earnings w_k.[4]

Labor supply with tax-benefit linkage. On the supply side, we denote by $\tilde{w}_k$ the *perceived* wage of workers of type $k \in \{T, C\}$, with $\tilde{w}_k \equiv w_k(1 + q\tau_k)$, i.e., their posted wage augmented by the perceived benefits from payroll taxes. Following Gruber (1997), tax-benefit linkage is modeled as a reduced-form parameter q whose value lies in the interval $[0, 1]$. This parameter, assumed to be constant across worker types, measures the extent to which workers perceive and value the benefits accruing from payroll taxes relative to cash income. It captures the actuarial fairness of payroll taxes, which determines the extent of linkage between these taxes and future benefit entitlements at the individual

[3]The term *gross* is somewhat misleading as it does not include payroll taxes nominally paid by employers. However, it is the commonly used term in Europe to describe posted earnings, e.g., *gross earnings* in the U.K., *salaire brut* in France, and *Bruttoverdienst* in Germany.

[4]The concept of labor cost is close to total compensation, which includes various fringe benefits provided by employers (e.g., health insurance, forms of leave, pension plans) but differs in that non-legally binding compensation is generally not included (in the U.S. context, see, e.g., Pierce, 2001).

level. The case with $q = 1$ corresponds to full linkage, i.e., a situation akin to the *quid pro quo* tax described in the classic public finance literature (Musgrave, 1968).

We model the extensive margin of labor supply by assuming that workers of type k have an indirect utility function $V(\tilde{w}_k, R_k) = V(w_k(1 + q\tau_k), R_k)$, which is increasing in their perceived wage, $\tilde{w}_k$, and non-labor income, R_k. The labor supply of a worker of type k can be expressed as

$$L_k^S = L_k^S(\tilde{w}_k, R_k) = L_k^S(w_k(1 + q\tau_k), R_k), \qquad k = C, T.$$

We further denote by $\eta_k^S \equiv \frac{\mathrm{d} \ln L_k^S}{\mathrm{d} \ln w_k}$ the labor supply elasticity of workers of type k with respect to the posted wage.

Pass-through of payroll taxes to wages. The pass-through rate of payroll taxes to the wage of treated workers relative to control workers, i.e., the proportional change in the relative wage of treated workers resulting from a one percent change in the rate of payroll taxes levied on these workers, is denoted by ρ. This measure can be understood as pass-through at the individual level in the sense that treated workers are those directly affected by the change in payroll taxes. Within the above framework, one can derive a general closed-form formula for pass-through (see Appendix A). Assuming that treated and control workers have similar labor supply elasticities (i.e., $\eta_T^S \approx \eta_C^S \equiv \eta^S$), the formula simplifies to

$$\rho = -\frac{\mathrm{d} \ln \left(\frac{w_T}{w_C}\right)}{\mathrm{d} \ln (1 + \tau_T)} \approx \frac{\sigma + \eta^S \cdot q}{\sigma + \eta^S}. \tag{1}$$

Equation (1) clarifies the theoretical interpretation of our empirical estimates when a competitive labor market equilibrium is assumed. This expression generalizes the well-known partial equilibrium incidence formula (Kotlikoff and Summers, 1987; Gruber, 1997) to a general equilibrium with two types of workers.

Three polar cases. In this model, the pass-through of payroll taxes depends on the elasticity of labor supply, η^S, the elasticity of substitution between both types of workers, σ, and the (perceived) tax-benefit linkage, q. Three polar cases can be highlighted:

(i) If workers value the benefits as much as the payroll tax paid ($q = 1$), then regardless of labor supply and substitution elasticities, payroll taxes are entirely shifted to treated workers ($\rho \approx 1$). In that case, payroll taxes are not perceived as a tax, as they fund benefits that are fully valued by workers. As a result, labor supply and labor demand remain unchanged, implying that employment (or hours worked) for both types of workers is unaffected by the tax.

(ii) If there is no perceived tax-benefit linkage ($q = 0$) and the elasticity of substitution

between worker types is much larger than the labor supply elasticity ($\sigma \gg \eta^S$), then employer payroll taxes are fully shifted to workers ($\rho \approx 1$). This is the standard assumption made in the labor supply and taxation literature, where σ is typically considered large, while η^S is assumed to be small. In this scenario, the employment level of treated workers declines relative to that of control workers, with the magnitude of the decline proportional to the labor supply elasticity.

(iii) If there is no perceived tax-benefit linkage ($q = 0$) and the labor supply elasticity is much larger than the elasticity of substitution ($\eta^S \gg \sigma$), then there is no pass-through to relative wages ($\rho \approx 0$). In this case, the relative labor cost of both worker types increases in direct proportion to the additional payroll tax levied on treated workers. Consequently, the employment level of treated workers declines relative to that of control workers, with the magnitude of the decline proportional to the elasticity of substitution.

Note that the first polar case (i) remains applicable across a wide range of labor market models. The fundamental insight is that the relationship between tax-benefit linkage and pass-through to wages, as outlined in the above framework, holds in virtually all models where workers fully value the future benefits associated with higher payroll taxes. Indeed, setting $q = 1$ essentially implies disregarding taxes and their potential effects in the considered model.

The dynamics of wage adjustments to changes in payroll taxes are not described in this simple framework, which implicitly assumes complete wage flexibility. In the very short term (the day after the reform), however, one expects the economic incidence of an increase in payroll taxes to closely align with the nominal incidence, meaning that the labor cost should increase by the amount of additional payroll taxes. Depending on the extent of labor market rigidities, wages might take time to adjust, possibly through changes in nominal wage growth or turnover. Hence, the primary empirical measure of interest is the medium- to long-term pass-through of payroll tax changes to workers.

2 Tax Reforms

Table 1 provides an overview of the key features of the six reforms analyzed in this paper, along with the corresponding theoretical predictions on incidence. The reforms are classified based on tax-benefit linkage, statutory incidence, and the type of tax reform (i.e., whether they pertain to payroll taxes or income taxes). Below, we describe each category of reforms separately: (i) payroll tax reforms with tax-benefit linkage, where both payroll tax rates and associated benefits were modified; (ii) payroll tax reforms without tax-benefit linkage, where tax rates changed but benefits remained unaffected; and (iii) income tax reforms that resemble payroll tax reforms in design—specifically, those that tax

labor earnings at the individual level—and clearly lack any tax-benefit linkage. Figure 1 presents, for each reform, the marginal tax rates for treated and control workers (i.e., those above and below the relevant threshold), highlighting the magnitude of the tax changes induced by each reform. Before discussing each reform in detail, we provide an overview of earnings taxation in France to contextualize these policy changes.

2.1 Earnings Taxation in France

Payroll taxes are a major component of taxation in France, serving as the main funding source of the welfare system. These taxes finance essential programs, including health care, pensions, unemployment benefits, and child benefits. Although all payroll taxes are earmarked for social spending, they differ in their effective tax-benefit linkage. Some are unrelated to the amount of benefits they finance (e.g., child care, health care). Others are indirectly linked to earnings-related benefits (e.g., the main pension scheme, unemployment insurance). Finally, some exhibit a very strong linkage, where contributions are directly tied to the benefits received (e.g., complementary pensions).

Although French payroll taxes exhibit considerable variation in the benefits they support, their structure adheres to a uniform schedule. The tax base is posted earnings w (referred to as gross earnings, or *salaire brut* in French). The payroll tax schedule comprises distinct thresholds expressed as multiples of the Social Security threshold (SST). For non-executive workers, different marginal tax rates apply to each of four wage brackets: (i) below the SST; (ii) between the SST and 3 times the SST; (iii) between 3 and 4 times the SST; and (iv) above 4 times the SST. Formally, employer payroll taxes for non-executive workers are computed as follows:

$$\begin{aligned}\text{Employer payroll taxes} = \tau_1^r \cdot \min(w, \overline{w}) + \mathbb{1}_{w>\overline{w}} \cdot \tau_2^r \cdot [\min(w, 3\overline{w}) - \overline{w}] \\ + \mathbb{1}_{w>3\overline{w}} \cdot \tau_3^r \cdot [\min(w, 4\overline{w}) - 3\overline{w}] + \mathbb{1}_{w>4\overline{w}} \cdot \tau_4^r \cdot [w - 4\overline{w}],\end{aligned}$$

where τ_k^r represents the marginal employer payroll tax rate that applies to wage bracket k and $\mathbb{1}(\cdot)$ denotes the indicator function. For executives, the formula remains analogous, with the only difference being the addition of an extra threshold at 8 times the SST.[5] Similarly, employee payroll taxes are computed using the same bracket structure but with a different set of marginal tax rates applied to each wage bracket.

In 1991, a dedicated flat-rate income tax known as the *Contribution social généralisée* (CSG) was implemented to address the growing expenses associated with health care spending. While this contribution was earmarked to finance Social Security benefits, it differs from a conventional payroll tax. Instead, it operates as a flat-rate tax on all earnings with no upper threshold. The CSG tax rate, initially set at 0.4%, underwent gradual

[5]The term "executives" stands for *cadres* in French. Workers in this category are in occupations that typically require five years or more of tertiary education, such as senior managers and engineers.

increments, reaching 7.5% in 1998. This increase was partially offset by a reduction in employee payroll taxes.

2.2 Payroll Tax Reforms with Tax-Benefit Linkage

Reform L1—Complementary pensions for non-executives. The first reform with tax-benefit linkage (Reform L1) examined in this paper is an increase in pension payroll taxes that was implemented in the early 2000s to support the ARRCO complementary pension scheme. This scheme, designed for non-executive workers in the private sector, is overseen by employer and employee unions. It operates as an unfunded defined contribution point-based system. Participants contribute to this scheme through both employer and employee payroll taxes. The contributions are converted from euros to points using a shadow price p_t^b, representing the value in euros to acquire a point. This conversion is indexed by the year (t) in which the contribution was made. Points accumulate throughout an individual's career, starting in t_0, and are later transformed into annuity pensions at retirement, using a separate shadow price p_R^s indexed by the retirement year R. Hence, the pension amount at retirement (denoted by B_R) can be expressed as a function of past payroll tax contributions, $\tau_t \cdot w_t$:

$$B_R = \sum_{t=t_0}^{R-1} \frac{\tau_t \cdot w_t}{p_t^b} \times p_R^s. \tag{2}$$

In 1996, a major reform was decided by the employer and employee unions overseeing the scheme. This reform aimed to address the ARRCO's financial sustainability due to rising life expectancy. Additionally, it included a steep increase in pension contribution rates for earnings above the SST, rising from 4.5% in 1999 to 12% in 2005 for employer payroll taxes, and from 3% to 8% for employee payroll taxes (see Figure 1, Panels A and B). This adjustment in pension contribution rates above the threshold resulted in an increase in the anticipated pension level for the affected workers, which was directly proportional to the reform-induced change in contribution rates (denoted by $\Delta\tau$):

$$\Delta B_R = \left(\sum_{t=t_0}^{R-1} \frac{w_t}{p_t^b} \times p_R^s \right) \Delta\tau. \tag{3}$$

Equation (3) highlights the strong tax-benefit linkage inherent to Reform L1. Workers directly impacted by the rise in pension payroll taxes could anticipate a proportional increase in their *individual* pension benefits.

Reform L2—Complementary pensions for executives. The complementary pension scheme for executives (AGIRC) covers earnings exceeding the SST. Until 1991, executives' earnings were subject to pension contributions up to 4 times the SST. In 1991, the cap

on payroll taxes was removed for earnings between 4 and 8 times the SST, leading to a 7.02 percentage point (pp) increase in employer payroll taxes for earnings above 4 times the SST, along with a 2.34 pp increase in employee payroll taxes (see Figure 1, Panels C and D). The pension rules of AGIRC closely resemble those of ARCCO, ensuring strong linkage from contributions to pension benefits in Reform L2, akin to Reform L1. The primary distinction is that the change in payroll taxes rates occurred at a much higher level of the earnings distribution (P99 vs. P70).

Importantly, Reforms L1 and L2 did not have any retroactive effects on pension accruals: the increase in pension benefits was proportional only to the additional contributions paid after the reform. However, these reforms increased social security wealth, which could, in turn, have encouraged earlier retirement at a later stage.

Perception of the tax-benefit linkage. Although we lack direct evidence on individuals' perceptions of the reforms, our collection of union and media archives strongly suggests that these reforms were widely understood as initiatives aimed at improving pension rights for the affected workers. For instance, an article in the daily newspaper *Le Monde* regarding Reform L1 states that "the agreement also includes the provision that wage earners whose wages exceeds the Social Security threshold would be able to secure a better pension for themselves".[6] Archival records from French trade unions further emphasize the prominent role of tax-benefit linkage in the unions' claims during negotiations (see Appendix B for details). For instance, the C.F.D.T. union argued to its members that "This demand from C.F.D.T. will allow non executives to improve their future pensions and will translate into increased pay compensations for employers."[7] For Reform L2, the main debate was within employer unions, as the insurance industry complained that increasing payroll taxes for highly paid executives would reduce savings through pension funds.

2.3 Payroll Tax Reforms without Tax-Benefit Linkage

Reform NL1—Uncapping of payroll tax for family benefits. The first reform without tax-benefit linkage that we examine involves the uncapping of the payroll tax for family benefits in the late 1980s. This specific payroll tax contributes to universal child benefits, which are unrelated to individual contributions or employment status. The absence of tax-benefit linkage is evident to individuals, particularly since the statutory family payroll tax is nominally paid by employers. Over a two-year period spanning 1989 to 1990, this payroll tax was uncapped, transitioning from being applied only to the fraction of earnings below the SST to being applicable to full earnings. The marginal rate above the SST increased from 0% to 7%, while the rate below the SST decreased from 9% to 7% (see Figure 1, Panel A).

[6] "La baisse des retraites complémentaires est programmée," *Le Monde*, April 27, 1996.
[7] *Le retraité militant C.F.D.T.*, No. 96.06, June 1996, pp. 6–8.

Reform NL2—Uncapping of payroll tax for health care. The second non-linkage reform was implemented in the early 1980s and involves the uncapping of the health care payroll tax. This payroll tax contributes to funding the French health care system, supporting health insurance that reimburses individuals for expenses from both public and private health care providers. It can be characterized as non-contributory in the sense that the level of insurance coverage is not tied to the amount of contributions paid. Initially, the health care payroll tax took the form of a large employer tax for earnings up to the SST, coupled with a considerably smaller employee tax. In the early 1980s, the employer part of this payroll tax was uncapped in two stages (see Figure 1, Panel A). In November 1981, the marginal employer health care payroll tax rate on the fraction of earnings above the SST increased from 4.5% to 8%, and further to 12.6% in January 1984 (+8.1 pp in total). Reform NL2 was part of a broader set of health care reforms aimed at balancing the budget of the public insurance scheme.[8]

2.4 Income Tax Reforms

In addition to the payroll tax reforms described above, we extend the analysis to two income tax reforms. Despite their designation, these reforms are comparable to the two previously discussed payroll tax reforms without linkage (NL1 and NL2) in that they exclusively impact labor earnings and lack any tax-benefit linkage. The two reforms differ in their statutory incidence (on employers vs. employees) and the nature of the change (increase vs. decrease). The first is a corporate income tax credit based on firms' payroll expenses, akin to an employer payroll tax cut. The second is an increase in the flat-rate income tax, similar to an employee payroll tax increase.

Reform NL3—Corporate income tax credit for payroll expenses. In 2013, a tax credit on the corporate income tax (CIT) was introduced. It amounted to 6% of the wage bill accruing to all workers paid below 2.5 times the national minimum wage (see Figure 1, Panel E). Since no tax credit was granted to firms for employees paid above this threshold (which was close to P80 of the earnings distribution, slightly above the SST), the reform impacted the average rather than the marginal employer tax rate at the threshold. Despite being framed as a CIT reduction, this reform essentially operated as a large employer payroll tax cut. Its primary objective was to stimulate employment by reducing labor costs (Carbonnier et al., 2022). The main difference with a payroll tax cut is that the tax credit was remitted to firms one year later than a standard payroll tax cut (i.e., the first tax credit was repaid to firms in 2014 based on their payroll expenses in 2013). The resemblance of this CIT reduction to a payroll tax cut is further evidenced by

[8]The uncapping of the health care payroll tax was partly motivated by employment concerns for low-wage earners. In the French daily newspaper *Le Monde*, dated November 12, 1981, the French Minister of Health N. Questiaux is quoted as stating: "The decision to increase payroll taxes only above the threshold has been motivated by our desire to spare firms with a large number of employees."

a subsequent reform in 2019, which transformed the tax credit into a standard payroll tax cut with minimal changes, except for the fiscal implications of advancing the tax cut.

Reform NL4—Increases in flat-rate personal income tax. In 1991, a flat-rate income tax (CSG) was introduced to replace a fraction of employee payroll taxes. Between 1996 and 1998, the CSG tax rate for labor earnings increased from 2.9% to 8.0%, while the employee payroll tax rate for healthcare was reduced from 6.8% to 0.75% over the same period. For most wage earners, this resulted in a slight decrease in the taxation of their earnings. However, due to the broader tax base of the CSG compared to the payroll tax it replaced, some workers faced an increase in their tax rate (see Figure 1, Panel F). Specifically, earnings from profit-sharing bonuses (*participation*), incentive plans (*intéressement*), and company saving plans (*plan d'épargne entreprise*) were initially exempted from employee payroll tax but subjected to the CSG increase.[9] The 1996–1998 CSG tax rate hike was therefore similar to an employee-side tax increase for workers receiving a substantial portion of their earnings through profit-sharing. By design, this tax reform did not involve any changes in social benefits, meaning that tax-benefit linkage was entirely absent.

3 Data

Administrative Social Security data. The primary data source for this study is the matched employer-employee DADS (*Déclaration Annuelle de Données Sociales*) Panel. This dataset is constructed by the French Statistical Office, the *Institut National de la Statistique et des Études Économiques* (INSEE), from the mandatory declarations submitted by employers for each of their employees. The main purpose of these declarations is to provide the different Social Security schemes with the earnings information necessary to determine workers' eligibility to benefits and to compute their levels, notably for pension schemes. INSEE processes the raw DADS data and provides researchers with user files under restricted access. The panel version of the DADS is a 1/25th sample of private sector employees, born in October of even-numbered years, starting from 1976. In 2002, the sample size was doubled to represent 1/12th of all private sector workers. The dataset includes around 1.1 million workers annually from 1976 to 2001 and 2.2 million workers from 2002 onwards. The data provide information on each firm (identifier, sector, size) and individual job spells (start and end date, earnings, occupation, part-time or full-time status). Starting in 1993, additional variables such as the number of hours of work, the

[9]Profit-sharing plans result from agreements between employer and employee representatives but become mandatory once a firm reaches 50 employees. In contrast, incentive plans are established on a voluntary basis, although they also tend to be more prevalent above the 50 employees threshold. Company saving plans become mandatory as soon as a firm establishes a profit-sharing agreement. An important characteristic of profit-sharing and incentive plans is that they benefit all employees of a firm proportionally to their posted earnings.

CSG tax base, and earnings net of payroll taxes and CSG are available in the panel (see Appendix C.1 for further details). Unfortunately, some years of the original data sources are unavailable (1981, 1983, and 1990), resulting in gaps in the panel data for those specific years.

Other data sources. The DADS data for earnings are provided in the form of "net taxable earnings," which excludes complementary earnings such as profit-sharing bonuses, incentive plans, and company saving plans. To overcome this limitation, an external source, the ACEMO-PIPA database, is used for the analysis of Reform NL4. This dataset is a survey conducted by the French Ministry of Labor to gather information on firms' ownership structure, profit-sharing agreements, incentive plans, and savings plans. Covering all firms with 10 or more employees, the ACEMO-PIPA survey can be matched with the DADS data at the firm level.

Computation of payroll taxes. Microsimulation techniques are required to compute individual-level labor costs based on the information available in the DADS data. This study employs the TAXIPP model, developed by the Institut des Politiques Publiques (IPP), specifically leveraging the payroll tax module. The model takes as input the payroll tax schedule, as collected in the IPP Tax and Benefit Tables (Institut des Politiques Publiques, 2023), and calculates employer and employee payroll taxes, reductions in employer payroll taxes, flat-rate income tax (CSG and CRDS), and various other payroll taxes. The main challenge in computing payroll taxes from the DADS Panel arises from the missing information in the raw data. Two data limitations must be noted. First, because net taxable earnings are the only earnings measure available throughout the period under study, we need to compute posted earnings and labor cost using the microsimulation model. Second, since hours worked are not recorded in the DADS data before 1993, the payroll taxes for part-time workers cannot be accurately computed. We therefore exclude these workers from our empirical analysis of payroll tax incidence.

To assess the accuracy of our imputed payroll taxes, we conducted two series of tests. First, we validated our imputations of posted earnings using precise tax base information for employee payroll taxes available since 1993. Second, we used firm-level data recording total employer payroll taxes to verify the accuracy of our labor cost measurement. Both evaluations indicate that imputation errors are of negligible magnitude and are unlikely to substantially affect our results (see Appendix C.2 for details).

4 Empirical Approach

We leverage the six tax reforms described in Section 2 to identify the earnings responses to changes in tax rates. For most reforms (L1, L2, NL1, NL2 and NL3), our empirical strategy compares the earnings of workers on either side of the relevant threshold before

and after each tax change. While the threshold determines a shift in the marginal tax rate, our first-stage analysis exploits a change in the average tax rate for workers above vs. below the threshold—except for NL3, where the reform directly altered the average tax rate. The validity of this difference-in-differences (DiD) approach hinges on the assumption that, in the absence of the reform, the average earnings of workers in the treatment and control groups would have followed parallel trends. For the income tax reform on the employee side (NL4), we adopt a different definition of the treatment and control groups, distinguishing workers based on whether their firms distributed profit-sharing bonuses before the reform.

4.1 Baseline Specification

To evaluate the incidence of employer payroll taxes on posted earnings (reforms L1, L2, NL1, NL2, and NL3), we rely on a model that is estimated using two-stage least squares (2SLS). For each reform, we estimate the first-stage and reduced-form effects of the payroll tax change using the following panel DiD specification:

$$\log(1+\tau_{i,t}) = \alpha + \theta_i + \eta_t + \sum_{k=-m}^{r} \beta_k \cdot (T_i \times \mathbb{1}\{t = t_0 + k\}) + \varepsilon_{i,t}, \tag{4}$$

$$\log w_{i,t} = \mu + \nu_i + \pi_t + \sum_{k=-m}^{r} \gamma_k \cdot (T_i \times \mathbb{1}\{t = t_0 + k\}) + \upsilon_{i,t}. \tag{5}$$

The first-stage equation (4) expresses $\log(1+\tau_{i,t})$, where $\tau_{i,t} \equiv (z_{i,t}/w_{i,t}) - 1$ is the average employer payroll tax rate that applies to the posted earnings $w_{i,t}$ of worker i in year t, as a function of worker fixed effects (θ_i), year fixed effects (η_t), and the full set of interactions between the treatment group indicator T_i and the year fixed effects, which include m pre-reform years and r post-reform years. The treatment group indicator T_i takes the value 1 if the earnings of worker i imply exposure to the payroll tax reform under consideration (i.e., they are above the SST for reforms L1, NL1 and NL2, above 4 times the SST for reform NL2, and below 2.5 times the minimum wage for NL3), and 0 otherwise. The coefficients β_k on the interaction terms are normalized to zero in the reference year (t_0), which corresponds to the last available pre-reform year. Each coefficient measures the post-reform log difference in average employer payroll tax rates between treated and control workers in year k, relative to the reference year.

The reduced-form equation (5) expresses the log of posted earnings, $\log(w_{i,t})$, as a function of the same variables. Under the assumption that the labor cost trends would have remained parallel for all years $k \geq 1$ in the absence of reform, the coefficients γ_k for $k \geq 1$ capture the reform's impact on posted earnings after k years.[10]

[10]Note that our DiD estimates measure the relative incidence of payroll tax changes between treated

Consistent with Equation (1), the estimated pass-through of employer payroll taxes to workers k years after the reform, denoted by ρ_k, is given by the opposite of the ratio between the reform's reduced-form effects on earnings and the employer payroll tax rate, i.e., $\hat{\rho}_k = -\hat{\gamma}_k/\hat{\beta}_k$. This parameter $\hat{\rho}_k$ can be recovered by estimating the following equation separately for each post-reform year $k \geq 1$ using 2SLS:

$$\log w_{i,t} = \lambda + \phi_i + \omega_t - \rho_k \cdot \log(1 + \tau_{i,t}) + \sum_{\substack{l=-m \\ l \neq k}}^{r} \delta_l \cdot (T_i \times \mathbb{1}\{t = t_0 + l\}) + \zeta_{i,t}, \qquad (6)$$

where ϕ_i and ω_t denote worker and year fixed effects, and the interaction term $T_i \times \mathbb{1}\{t = t_0 + k\}$ is used as an instrument for $\log(1 + \tau_{i,t})$. This procedure ensures that the earnings response in any post-reform year is appropriately scaled by the size of the first-stage effect specific to that year. Indeed, the first-stage and reduced-form equations of this 2SLS model directly correspond to equations (4) and (5), meaning that $\hat{\rho}_k$ is recovered as a Wald ratio using 2SLS. To account for serial correlation in individual earnings, standard errors are clustered at the worker level in all specifications.[11]

For the two reforms that also affected employee payroll tax rates (L1 and L2), we complement the above specification by estimating the pass-through of the overall payroll tax change to workers. In the first-stage equation (4), the average payroll tax rate $\tau_{i,t}$ is modified to incorporate both employee and employers payroll taxes expressed as a fraction of earnings net of these taxes, which we denote by $n_{i,t}$. In the reduced-form equation (5), $\log w_{i,t}$ (posted earnings) is replaced by $\log n_{i,t}$ (net earnings). This alternative specification is also used to evaluate the effects of the income tax reform that increased the taxation of earnings from profit-sharing bonuses (Reform NL4). For this reform, the treatment group indicator T_i takes the value 1 if worker i was employed by a firm that distributed profit-sharing before the reform, and 0 otherwise.

Earnings vs. hourly wage. Since hours worked are not observed before 1993, we can only consistently measure total earnings responses to payroll tax changes. As a result, we use the log of posted earnings as the dependent variable for all reforms, meaning that our estimates capture both changes in hours worked and wage rates. Two arguments lead us to interpret our estimates as incidence effects. First, our empirical analysis includes only full-time wage earners employed throughout the entire year. This mitigates potential behavioral responses, such as switching from full-time to part-time work, that might otherwise distort the results. Second, an increase in payroll taxes would typically be

and control workers and therefore do not directly capture aggregate incidence (i.e., the overall impact on both groups). This issue is further discussed in Section 6.2.

[11] Since $w_{i,t} = z_{i,t}(1 - \tau^z_{i,t})$ where $\tau^z_{i,t} \equiv 1 - (w_{i,t}/z_{i,t})$ (i.e., the average employer payroll tax expressed as a fraction of the labor cost rather than as a fraction of the posted earnings), the pass-through parameter ρ_k can be equivalently recovered by replacing $\log(1 + \tau_{i,t})$ by $\log(1 - \tau^z_{i,t})$ in the first-stage equation (4) and by replacing $\log w_{i,t}$ (posted earnings) by $\log z_{i,t}$ (labor cost) in the reduced-form equation (5). The pass-through parameter is then given by $\hat{\rho}_k = 1 + \hat{\gamma}_k/\hat{\beta}_k$ using this alternative specification.

expected to reduce hours worked (if substitution effects dominate income effects), leading to a decline in total earnings. If this were the case, our estimates would be biased towards finding more shifting to workers, as the reduction in hours would be confounded with incidence effects, resulting in an overestimation of the pass-through parameter ρ. However, as we will see, our earnings-based estimates for Reforms NL1 and NL2 suggest almost no shifting of the payroll tax increases to wages, while our estimates for Reform L2 are actually slightly larger than one, consistent with full pass-through.

4.2 Payroll Tax Reforms

Definition of the treatment and control groups. For reforms L1, L2, NL1 and NL2, the identifying variation stems from changes in the marginal payroll tax rate around either 1 or 4 SST. When defining the treatment group, a key consideration is the trade-off between expanding the range of earnings above the threshold in the reference year (which mechanically inflates the reform-induced variation in average payroll tax rates) and minimizing the risk of dissimilar earnings trends between the treatment and control workers. For reforms L1, NL1 and NL2 (occurring at 1 SST), we define the treatment group as workers whose posted earnings in the reference year were between the SST and 1.4 times the SST that year, i.e., between P67 and P88 of the earnings distribution (see Table 2). The control group comprises workers in a narrower range of posted earnings in the base year, between 0.9 and 1 times the SST (i.e., between P58 and P72 of the earnings distribution). This range is broad enough to construct a sizeable control group while avoiding the inclusion of workers whose earnings may have been affected by diffusion effects resulting from increases in the national minimum wage.

We adopt a similar approach for Reform L2, with the exception that the relevant threshold for this reform is at 4 times the SST. The treatment group is defined as workers with earnings between 4 and 5.5 times the SST in the reference period (top 1% of the earnings distribution), whereas the control group consists of workers with earnings between 2.5 and 4 times the SST in the reference year (between P97 and P100 of the earnings distribution). In Section 5.5, we evaluate the sensitivity of our pass-through estimates to using alternative definitions of the treatment and control groups.

Sample construction. We construct distinct panels of workers for each reform. Each sample comprises all workers observed in employment in the reference year, and we follow these workers throughout the relevant study period, spanning 3 to 4 years before the reference year and 6 to 9 years after. These time windows were chosen to be as wide as possible while avoiding contamination of the estimated earnings responses to each reform with the effects of other reforms. Across all reforms, we consider only workers who are employed throughout the year and were working full-time in the reference year. These restrictions are necessary because we lack information on hours worked before

1993, preventing accurate computation of payroll taxes for part-time workers. With the exception of Reform L2, which concerns only highly paid executive workers, we focus on non-executives. This is because Reform L1 specifically targeted non-executives, and we aim to ensure that our results for payroll tax reforms with and without tax-benefit linkage are established on comparable samples.[12]

4.3 Income Tax Reforms

Definition of the treatment and control groups. Reform NL3 differs slightly from the other reforms in that it induced a change in the average, rather than the marginal, implicit payroll tax rate at the reform's earnings threshold (2.5 times the minimum wage). Thus, unlike the other reforms, the variation in the average payroll tax rate induced by NL3 does not depend on the range of earnings considered to define treated workers. For this reform, we adopt a symmetric window, with the treatment group consisting of workers between 2.3 and 2.5 times the minimum wage in the reference year (between P76 and P80 of the earnings distribution), while the control group comprises those between 2.5 and 2.7 times the minimum wage that year (between P80 and P84 of the earnings distribution).

The definition of the treatment group is different in the case of Reform NL4, as exposure to this reform was determined by the structure of earnings, specifically whether or not employees received complementary earnings through their firm's profit-sharing agreement. Such earnings, which were previously exempt from payroll taxes, became subject to the new flat-rate withholding tax following the reform. In the data, the amount of shared profits at the firm level is observed from 1993 onward (i.e., three years before NL4). We assign workers to the treatment and control groups based on whether their firm distributed profit-sharing bonuses at least once in the pre-reform period. Since profits are subject to substantial volatility, the receipt of profit-sharing is susceptible to mean reversion. To address this issue, we define treated workers as those who received profit-sharing distributions at least once during the pre-reform years (1993–1995) rather than only in the reference year. These workers (and their firms) may differ from those who did not receive profit-sharing before the reform. As with other reforms, our identification strategy relies on the parallel trends assumption, which we indirectly assess by verifying that the earnings trajectories of treated and control workers were parallel during the pre-reform period.

Given that the observed profit-sharing is distributed to workers in proportion to posted

[12]In addition to these general principles, the samples employed to estimate the effects of Reforms L1 and L2 are adjusted to accommodate specific features of these reforms. When examining Reform L1, the sample is restricted to individuals who, in the reference year, were employed in firms established before 1997, given the distinct timing of the increase in payroll tax rates for firms created after this date. For Reform L2, we use a balanced panel to mitigate mean reversion—affected workers exhibit very high earnings—and we define the treatment and control groups based on whether the workers' earnings were above or below the reform's threshold in either the reference year (1989) or the year before (1988), as the year 1990 is missing in the data.

wages, we are able to accurately calculate the individual amount of profit-sharing.[13] This allows us to precisely add its gross value to the yearly labor cost and its net value, accounting for income tax, to workers' net wage earnings. To estimate the other two components of complementary earnings (*intéressement des salariés* and *plan d'épargne entreprise*), we use data from the ACEMO-PIPA 2001 survey to calibrate their average amount as a share of observed profit-sharing and add it to the latter.[14]

Sample construction. The construction of worker panels for reforms NL3 and NL4 follows a similar approach to that used for the other reforms. The sample for Reform NL4 differs only slightly from the others. At the worker level, we adopt the same restriction as for other reforms and focus on full-time and full-year employees. To be able to allocate profit-sharing, we focus on firms observed each year in the balance-sheet data. To make earnings trajectories comparable in the treatment and control groups, we further restrict the sample to workers in firms making no losses in the pre-treatment period and having at least 10 employees before the reform (the distribution of profit-sharing is almost nonexistent below this level).

Summary statistics. Table 2 presents summary statistics of the samples used in the analysis of each reform. As expected, workers in the treatment groups have higher earnings than those those in the control group (except for NL3 where the treated workers have earnings below the threshold). Additionally, they tend to be slightly older and are more likely to be male. Notably, workers affected by the different reforms are represented across the entire earnings distribution.

5 Results

Below, we present the main results derived from the empirical approach outlined in the previous section. We start with the payroll tax reforms that involve tax-benefit linkage (L1 and L2), then examine the payroll tax reforms without linkage (NL1 and NL2), and finally, we analyze the two income tax reforms that also lack linkage (NL3 and NL4).

[13] Specifically, the share of a firm's profits that must be distributed to its employees is calculated as $0.5\times(P - 0.05 \times EQ)*(WB / VA)$, where P denotes the firm's fiscal profits, EQ its equity, VA its value-added, and WB its wage bill. The amount is then distributed proportionally based on the share of the wage bill accruing to each worker. In the French context, Nimier-David et al. (2023) present evidence that profit-sharing plans do not impact firms' behavior in terms of investment or productivity.

[14] We find that these amounts are null when no *participation* is provided by the firm and otherwise closely track the level of profit-sharing. Accounting for *intéressement* and *plans d'épargne entreprise* results in an overall profit-sharing amount that is 44% higher relative to wages. We use this estimate to proxy the overall complementary earnings associated with observed profit-sharing in the data.

5.1 Payroll Tax Reforms with Tax-Benefit Linkage

Earnings responses. The labor cost and posted earnings responses to the payroll tax reforms with linkage (L1 and L2) are visually represented in Figure 2. For each reform, this figure compares the evolution of real average posted earnings (left panel) and real average labor cost (right panel) between the treatment and control groups around the reform years. All earnings measures are normalized to 100 in the reference year, indicated by a vertical red dotted line. The gray shaded area indicates the reform's implementation period.

The Figure provides reassuring evidence that the pre-reform trends are well-aligned for both reforms. In the post-reform period, we observe slower earnings growth for workers directly affected by the increases in payroll taxes compared to the non-treated workers (see Panels A and C). Conversely, in both cases, the labor cost of treated workers rapidly converged to that of the control group (see Panels B and D). Together, these findings provide a strong indication that the tax hikes were passed through to workers' earnings.

Panels A and C of Figure 3 present the differential growth in posted earnings and labor costs between treated and control workers with respect to the reference year (1999 for L1 and 1989 for L2), for both pre- and post-reform years. The vertical difference between the solid red line (labor cost) and the solid blue line (posted earnings) corresponds to the first-stage effect, i.e., the log difference in the average employer payroll tax rates between treated and control workers, relative to the reference year.

Pass-through estimates. The 2SLS estimates of the pass-through of the payroll tax increase to workers, based on Equation (6), are reported in Panels B and D. When computing these estimates for each post-reform year, the panel DiD specification accounts for the fact that workers' earnings, and hence their effective payroll tax rates, vary over time. Since Reform L1 (an increase in pension payroll taxes for non-executives) was rolled out over several years, we need to observe the estimates at $t_0 + 6$ to fully capture its impact.

The results reveal a complete shift of the employer payroll tax increase to treated workers following Reform L1. Post-reform, these workers' posted earnings gradually decreased relative to the control group. As a result, after an initial rise, the relative labor cost of the two groups of workers declined and returned to its pre-reform level. After the reform was fully implemented, the earnings of treated and control workers exhibited similar growth rates. Pass-through estimates, both in the final year ($t_0 + 9$) and on average over the last three years, are reported in column 1 of Table 3. These estimates confirm that the pension payroll tax increase was almost entirely shifted to workers within three years after completion of the reform, with an estimated pass-through rate very close to and not significantly different from 1. Depending on the time frame considered, the estimates imply that between 88% and 101% of the increase in the labor cost was shifted to workers.

Column 2 shows the pass-through estimates when both employer and employee payroll taxes are taken into account. With values between 93% and 100%, these estimates are consistent with both employer and employee payroll taxes being shifted to workers.[15]

For Reform L2, which involved an increase in the pension payroll tax for executives, Panels C and D of Figure 3 show a similar pattern: the rise in labor cost resulting from higher payroll taxes was fully shifted to workers through reduced earnings. Given the limited sample size around the 4 SST threshold, statistical tests cannot conclusively reject equality between the effects of the reform on labor cost and its effects on posted earnings at the end of the period. Nonetheless, the point estimates indicate a sustained decrease in posted earnings for treated workers relative to the control group. The associated pass-through estimates, ranging from 74% to 126%, are not significantly different from 1 (see columns 3 and 4 of Table 3), providing suggestive evidence that the increase in pension payroll tax for executives was effectively transferred to workers.

5.2 Payroll Tax Reforms without Linkage

Earnings responses. We find very different earnings responses when we consider the two payroll tax reforms that have no tax-benefit linkage (NL1 and NL2), compared to the two previous reforms with linkage. (L1 and L2). Figure 4 shows that average posted earnings moved in parallel for the treatment and control groups before and after both reforms (see Panels A and C), whereas their labor costs diverged markedly after each reform (see Panels B and D).

Pass-through estimates. The results for Reform NL1, which involved the uncapping of the payroll tax for family benefits in 1989 and 1990, are shown in Panels A and B of Figure 5, with the corresponding pass-through estimates reported in column 1 of Table 4. Panel A does not reveal systematic differences in pre-trends between the treatment and control groups, although pre-reform differences in posted earnings and labor costs are not perfectly aligned. The observed effects differ sharply from those observed for Reforms L1 and L2: the increase in the family payroll tax led to a rise in labor costs and only a slight decrease in posted earnings. Six years after the reform, the effect on the labor cost of treated workers relative to the control group remains positive and significantly different from zero. We thus find limited evidence of a shift to workers, as net earnings appear to have experienced only a modest decline post-reform. The pass-through estimates are not significantly different from zero, both when measured in the last year (25%) and on average over the last three years (−2%).

[15] Appendix Figure E1 shows the evolution of log-differences in net earnings and labor costs for Reforms L1 and L2, along with the corresponding pass-through estimates. In general, the pass-through of employer and employee payroll taxes cannot be separately identified when they are modified at the same time, as was the case for Reforms L1 and L2. In our setting, however, the fact that these reforms had no long-term effects on labor costs implies that both employer and employee payroll taxes were fully shifted to workers.

Turning to Reform NL2, which involved the uncapping of the health care payroll tax in 1981 and 1983, the results are presented in Panels C and D of Figure 5, and the corresponding pass-through estimates are reported in column 2 of Table 4. Panel C of Figure 5 shows some evidence of non-parallel pre-reform trends in labor costs, suggesting that the results for this reform, being the earliest among those considered in this study, should be interpreted with caution. The findings are qualitatively similar to those obtained for Reform NL1, showing a sharp increase in labor costs for treated workers compared to the control group, that persisted for at least five years. The pass-through estimates are 44% when measured in the last year of the post-reform period and 41% when averaged over the last three years, both significant at the 5% level. These estimates, however, could be contaminated by differential pre-trends in earnings. In Section 5.5, we address this concern by controlling for worker-specific trends. The resulting estimates, reported in column 4 of Appendix Table E1, yield smaller pass-through rates of 23% in the last post-reform year (not significantly different from zero) and 24% on average over the last three years (marginally significant at the 10% level), in line with the findings for Reform NL1.

5.3 Income Tax Reforms

Results from the two income tax reforms (NL3 and NL4) provide additional evidence on reforms without tax-benefit linkage, with NL3 affecting the employer side and NL4 affecting the employee side.

Earnings responses. Panels A and B of Figure 6 offer visual insights into the effects of Reform NL3—a payroll tax reduction for earnings below 2.5 times the minimum wage implemented in 2013—on labor costs and posted earnings. Consistent with a limited pass-through to workers, the average labor cost of workers in the treatment group (those earning between 2.3 and 2.5 times the minimum wage in the reference year) notably decreased compared to the control group (workers with pre-reform earnings between 2.5 and 2.7 times the minimum wage), whereas the relative posted earnings of the two groups were unaffected by the reform.

For the flat-rate income tax reform (NL4), Panels C and D of Figure 6 show the evolution of net earnings (including profit-sharing) and labor costs around the reform for the treatment and control groups, i.e., employees of firms distributing profit-sharing bonuses in the pre-treatment period, and employees of firms that did not, respectively. Following the reform, the average net earnings of the treatment group grew at a slower pace compared to the control group, while their relative labor cost remained constant. This pattern suggests that the tax increase in the treatment group was fully incident on workers.

Pass-through estimates. The results for Reform NL3 are shown in Panels A and B of Figure 7, with the corresponding pass-through estimates reported in column 3 of Table 4. The DiD coefficients show no evidence of differential pre-trends, while the labor cost for the treated workers experienced a notable decrease compared to the control group following the reform. The pass-through estimates are slightly negative (-0.06 in the last year and -0.05 on average over the last three years) and not significantly different from zero.

The estimates for Reform NL4 are presented in Panels C and D of Figure 7 and in column 4 of Table 4. Unlike Reform NL3, the DiD estimates indicate a decline in net earnings for treated workers relative to the control group, with an estimated pass-through to workers of 0.89 after four years and 0.75 on average over the last two years (both significant at the 1% level).

Together, these results indicate that the pass-through of the two income tax reforms coincided with their statutory incidence: on employers for reform NL3 and on workers for Reform NL4.

5.4 Tests for Differences in Pass-Through

Table 5 presents a series of formal statistical tests for our main research hypotheses. These tests are based on the pass-through estimates reported in Tables 3 and 4 for the last year in each reform window.

Reforms with tax-benefit linkage. In Panel A, we start by showing that, for the two payroll tax reforms with linkage (L1 and L2), we cannot reject that their incidence was on workers while we can reject that it was on employers. Conducting chi-squared tests for the hypotheses that either both reforms were borne by employees or both were borne by employers, the test statistics strongly reject an incidence on employers (p-value ≤ 0.01), while an incidence on employees cannot be rejected (p-value > 0.99). This holds true whether we consider only the pass-through of changes in the employer part of these two reforms or the pass-through of changes in both employer and employee payroll taxes.

Reforms without tax-benefit linkage. In Panel B of Table 5, we test the hypothesis that the economic incidence of tax reforms without linkage is aligned with their statutory incidence four to six years after the reforms have been fully implemented. This involves jointly testing whether payroll tax reforms NL1 and NL2 were passed on to employers, whether the corporate income tax reform NL3 was also passed on to employers, and whether the income tax reform NL4 was passed on to employees. A chi-squared test indicates that this set of hypotheses cannot be rejected (p-value $= 0.15$), while the hypothesis that these reforms had the opposite incidence is strongly rejected (p-value < 0.001).

Difference in pass-through by tax-benefit linkage. In Panel C, we test whether the average incidence of tax reforms with and without linkage is identical. We compare the

mean estimated pass-through of reforms with linkage to the mean estimated pass-through of reforms without linkage, with means weighted by the inverse variance of each estimate to account for precision, as is customary in meta-analyses. When considering reforms statutorily incident on employers (the employer part of L1 and L2, and reforms NL1, NL2 and NL3), we reject at the 5% level that their pass-through is the same (p-value = 0.032). This finding is consistent with the hypothesis that among reforms statutorily incident on employers, only those with linkage are passed on to workers. Finally, when examining reforms statutorily incident on workers, we cannot reject that those with linkage (the "full" reforms L1 and L2, including the employee part) have, on average, the same incidence as the reform without linkage (NL4). This supports the hypothesis that both types of reforms are passed on to workers.

The formal statistical tests presented in Table 5 do not reject our primary hypotheses concerning the incidence of payroll tax reforms with and without linkage, while they reject the alternative hypotheses.

5.5 Robustness Checks

We performed a series of sensitivity analyses to assess the robustness of our findings (see Appendix E).

Definition of the treatment and control groups. In our first sensitivity analysis, we tested the robustness of our results to using alternative definitions of the treatment and control groups.[16] This involves systematically varying the upper and lower earnings limits that determine the assignment of workers to either group in the reference year. The estimates for different choices of the treatment group's upper earnings limit and the control group's lower earnings limit are presented in Appendix Figures E2 and E3. The results generally appear to be robust across different choices of the earnings limits, although narrowing the earnings range inevitably entails a loss in statistical precision.

Controlling for pre-reform trends. The key identifying assumption underlying our analysis is that absent payroll tax reforms, the average earnings of treated and control workers would have followed parallel trends. To account for potential deviations from the common trend assumption, we conducted a series of robustness checks using an augmented version of Equation (6) that includes worker-specific linear time trends. The pass-through of payroll taxes to workers k years after a reform, denoted as ρ_k, is estimated using the following equation, which is estimated separately for each post-reform year using 2SLS:

$$\log w_{i,t} = \lambda + \phi_i + \omega_t + \psi_i \cdot t - \rho_k \cdot \log(1 + \tau_{i,t}) + \sum_{\substack{l=1 \\ l \neq k}}^{r} \delta_l \cdot (T_i \times \mathbb{1}\{t = t_0 + l\}) + \zeta_{i,t}, \qquad (7)$$

[16] This test was not performed for Reform NL4 since the treatment and control groups for this reform are not defined in relation to an earnings threshold.

where $\psi_i \cdot t$ denotes worker-specific linear time trends and the interaction term $T_i \times \mathbb{1}\{t = t_0 + k\}$ is used as an instrument for $\log(1 + \tau_{i,t})$.

The results based on this augmented specification are presented in Panel A of Appendix Table E1. As noted earlier in Section 5.2, controlling for worker-specific trends reduces the pass-through estimates for Reform NL2 (from 0.44 to 0.23), reinforcing the finding of close to zero incidence on workers from this employer payroll tax increase without linkage. For the other reforms, our main conclusions remain qualitatively unchanged.

Removing workers close to the earnings thresholds. In Panel B of Appendix Table E1, we test the robustness of our pass-through estimates to removing workers initially close to the threshold ("donut hole"), as these workers may have a higher probability of switching groups in the years following the reform. The estimates are generally robust to this change in sample definition, with the exception of Reform L2, for which the donut hole estimates appear more fragile due to the limited sample size and the potential mean reversion induced by the bottom truncation of earnings in the treatment group, which is composed of workers with very high earnings. Additionally, for Reform NL2, although differential pre-trends are more pronounced when we remove workers close to the threshold, once we account for worker-specific trends, the pass-through estimates remain small and not significantly different from zero.

Non-parametric evidence. To complement the evidence presented above, we provide non-parametric graphical representations of the variation underlying our pass-through estimates. Appendix Figure E4 shows the (demeaned) percentage change in workers' posted earnings and labor cost between the last available year and the reference year (y-axis) plotted against the ratio between their pre-reform posted earnings and the relevant earnings threshold (x-axis), which determines treatment status. The graphs exhibit a clear pattern where changes in labor costs and gross earnings align well for workers on the non-treated side of the earnings distribution (i.e., on the left-hand side of the graphs except for reform NL3), whereas the gap between labor costs and posted earnings widens as one moves away from the reform's earnings threshold for workers on the treated side. This pattern confirms that, in line with our identification strategy, our pass-through estimates are driven by the differential variation in the average tax rates faced by workers located on either side of the relevant threshold.

Placebo reform. We conducted a placebo test to check whether underlying trends in earnings inequality during the period of study could challenge the common trend assumption. For this test, we focused on the only sufficiently long period when no payroll tax reform occurred, i.e., 1992 to 1999. Setting the placebo reform in 1996, we defined our treatment and control groups in the placebo reference year 1995, using the same earnings thresholds as for Reforms L1, NL1, and NL2. The results, reported in Appendix Figure E5,

show no strong evidence of differential earnings trends between the treatment and control groups for this placebo reform.

6 Interpretation and Discussion

This section discusses the interpretation of our results with regard to two key questions. First, can our findings on the empirical relevance of tax-benefit linkage for payroll tax incidence be generalized beyond the specific French reforms analyzed in this study? Second, to what extent do our estimates for reforms without tax-benefit linkage challenge the conventional wisdom about payroll tax incidence?

6.1 Meta-Analysis of the Literature

We believe that our results can contribute to reconciling the seemingly contradictory findings in the empirical literature on the incidence of payroll taxes. Indeed, existing evidence supporting full pass-through of employer payroll taxes to workers, e.g., Gruber (1997)'s analysis of the Chilean pension reform or Kim et al. (2022)'s study of the Singaporean pension payroll tax, often arises from contexts where tax-benefit linkage is strong. Conversely, studies showing no shifting typically involve payroll tax reforms lacking tax-benefit linkage, e.g., the Greek reform examined by Saez et al. (2012), the French reform studied by Lehmann et al. (2013), and the Swedish reform analyzed by Saez et al. (2019).

Pass-through of employer payroll taxes. Our meta-analysis of the literature on payroll tax incidence confirms that tax-benefit linkage is a key factor to reconcile the conflicting results across studies. Panel A of Figure 8 plots the estimated pass-through rate of employer payroll taxes to workers from 19 published papers that employ robust identification strategies (see Appendix Table D2 for details), to which we add our own estimates.[17] We ascertain the degree of linkage and its likely perception based on the authors' descriptions of the reforms. Out of 26 estimates of pass-through rates for payroll tax reforms, 9 relate to reforms with clear tax-benefit linkage, 14 involve payroll taxes with no linkage, while the degree of linkage is uncertain for the remaining three. A clear pattern emerges, indicating that results showing limited pass-through to workers stem from natural experiments concerning payroll taxes with no tax-benefit linkage. Conversely, studies finding full shifting to workers predominantly originate from settings with strong linkage. Based on these estimates, we find an average pass-through rate of 103% for employer payroll taxes with strong tax-benefit linkage, in contrast to an average pass-through of 16% for employer payroll taxes without linkage.

[17]The selection of papers included in this meta-analysis is based on the survey by Melguizo and González-Páramo (2013) and the literature reviews in Saez et al. (2019) and Kim et al. (2022).

Alternative groupings of the estimates in the literature do not reveal other characteristics that might better explain the diverging results. For instance, there is no discernable pattern based on the year of the reform or the level of aggregation at which the empirical analysis is conducted (the average pass-through is 48% when estimated at the individual level compared to 52% when estimated at the firm level).

In Panel A of Appendix Table D1, we present a more formal meta-analysis of the literature using a regression framework that controls for a variety of potential confounders. A basic regression of study-specific pass-trough estimates on indicators for the degree of linkage (none, strong, uncertain) yields a statistically significant coefficient of 0.85 on the strong-linkage indicator relative to the no-linkage baseline (column 2). When controlling for time-period fixed effects (in decades) and broadly defined regions (U.S., Europe, Rest of the world), the coefficient on the strong-linkage indicator slightly decreases to 0.74 but remains statistically significant (column 3). In column 4, we introduce controls for the unit of observation of the analysis (worker, firm, market level), the country's macroeconomic conditions (GDP growth and inflation rate) and its labor market conditions (unemployment rate, OECD indicator of strictness of employment protection legislation, union density, rate of collective bargaining coverage, and whether wage negotiations take place at the firm level). None of these controls achieves statistical significance, while the coefficient on the strong-linkage indicator remains significant at the 1% level with an estimated difference of 1.00 relative to the no-linkage baseline.

Pass-through of employee-side taxes. Panel B of Figure 8 plots the estimated pass-through rate on workers of taxes statutorily imposed on employees, including both employee payroll taxes and income taxes. The available literature (including this study) provides only eight estimates of employee-side pass-through, drawn from four income tax reforms and four employee payroll tax reforms, highlighting the limited empirical evidence in this area. On average, we find an average pass-through to workers of 98%, in line with the statutory incidence of the corresponding taxes. In Panel B of Appendix Table D1, we conduct a meta-analysis of these estimates and find that tax-benefit linkage has no statistically significant impact on the incidence of employee-side taxes when introducing time and geographical controls.

6.2 Discussion

Our study's central finding is that payroll taxes with clear tax-benefit linkage are fully shifted to workers, regardless of their statutory incidence, whereas the economic incidence of taxes without linkage coincides with their statutory incidence at the individual level. How can these results be reconciled with existing models of the labor market?

Pass-through, employment effects and efficiency with tax-benefit linkage. Our finding that payroll taxes with strong tax-benefit linkage are fully passed through to workers at the individual level is consistent with a wide array of labor market models. The underlying intuition is straightforward: when payroll taxes are closely tied to future benefits, they are not perceived as a tax, but rather as a deferred wage contribution. In this case, the effective tax component influencing net wages is reduced, as forward-looking workers internalize the value of future benefits when making labor supply decisions. As a result, these workers are not deterred by wage reductions caused by higher payroll taxes, which facilitates full pass-through. If this interpretation holds, payroll tax increases without linkage should lead to no labor supply response (see Section 1). In Appendix F, we test this prediction by examining the labor market transitions of treated vs. control workers following the different payroll tax reforms. Specifically, we analyze the probability that workers remain employed in the same firm as in the reference year and the probability that they are observed with positive earnings in the data, indicating their continued employment in the private sector. Our results show no significant effects of Reforms L1 and L2 on either of these employment margins.[18] These findings support the idea that payroll taxes fully shifted to workers do not negatively impact employment. More generally, they provide empirical support for the efficiency argument that a strong tax-benefit linkage mitigates the deadweight loss induced by payroll taxation.

Statutory incidence without tax-benefit linkage. Few studies have empirically tested the relevance of statutory incidence in the context of payroll taxation (see, e.g., Saez et al., 2012; Hargaden and Roantree, 2019), largely due to the limited availability of reforms affecting employee-side payroll taxes. However, as highlighted in Section 6.1, the results of most studies investigating the incidence of employer payroll taxes are consistent with the hypothesis that, in the absence of tax-benefit linkage, the individual-level pass-through of payroll taxes aligns with their statutory incidence. Our findings confirm this hypothesis using both payroll and income taxes.

These results are difficult to reconcile with the standard competitive model, as they would imply implausibly low elasticities of substitution between treated and control workers (see the discussion in Appendix A) and, more generally, contradict the "theorem of invariance of tax incidence" (Dalton, 1954). Several studies have shown that this theorem does not hold in various settings, including differences in remittance (Slemrod, 2008) or tax evasion (Kopczuk et al., 2016). However, in the context of payroll taxation, remittance and evasion are unlikely to be valid explanations, as the split between the employer part and the employee part of payroll taxes is a pure convention (both are remitted by firms).

A possible explanation for the observed statutory incidence might be attributed to differing perceptions of each side of the payroll tax (Iturbe-Ormaetxe, 2015) or to behavioral

[18]While for L2 the evidence is only suggestive due to the lack of statistical power, the estimates for L1 are precise enough to reject a differential exit rate of 1.3% between the treated and control workers.

factors such as those giving rise to flypaper effects in the context of intergovernmental transfers (Hines and Thaler, 1995). In the absence of tax-benefit linkage, individuals typically negotiate on posted earnings and could consider employer payroll tax as "firm's taxes," which they are not required to support individually. An alternative explanation, proposed and formalized by Saez et al. (2019), relates to the existence of fairness norms within firms, whereby total compensation (wage and fringe benefits) is expected to adhere to internal equity norms. In this situation, employer payroll taxes are less likely to be shifted to wages at the individual level, as this would alter relative compensation within the firm. By contrast, when there is tax-benefit linkage, it is clear that wage earners receiving higher benefits through increased payroll taxes are not placed at a disadvantage. Facing such fairness norms, firms might attempt to shift the payroll taxes nominally incident on employers to workers by imposing lower wage growth on *all* employees, irrespective of whether their *individual* payroll tax rates are subject to these increases. In this scenario, the relative wages of treated and control workers are unaffected by changes in payroll taxes, although both groups ultimately bear the full burden at the firm level. Such firm-level shifting of employer payroll taxes has been evidenced by Saez et al. (2019) in the case of Sweden, and by Benzarti and Harju (2021) in the case of Finland. In the French context, employer-side taxes without tax-benefit linkage could also have been shifted to workers at the firm level. Hence, while our results indicate that these taxes do not appear to be borne by the affected workers at the individual level, the aggregate burden could ultimately fall on workers. This explanation is consistent with our finding that reforms NL1, NL2 and NL3 did not have sizeable effects on the relative employment levels of treated and control workers (see columns 3, 4 and 5 of Appendix Table F1), with the point estimates being small and either statistically insignificant or wrong-signed.[19,]

In sum, the empirical evidence for reforms without tax-benefit linkage appears to challenge the standard competitive model and to be more consistent with models that incorporate fairness norms regarding total compensation within firms. In such a framework, both the statutory incidence and the existence of tax-benefit linkage might affect fairness perceptions, suggesting that some taxes could be shifted easily at the individual level, while others would require shifting at a more aggregated level. However, the lack of direct evidence on pass-through and employment effects at the firm level in our specific context necessitates caution when interpreting the exact implications of our findings for taxes without linkage and their potential welfare consequences.

[19]These results align with those of Carbonnier et al. (2022), who also examine the employment effects of Reform NL3. Unfortunately, we are unable to test for tax shifting and employment effects at the firm level, as we only have access a 1/25th or a 1/12th sample of employees. As a result, we cannot test whether the taxes that were not shifted at the individual level were instead shifted to all workers (both treated and control) within firms.

7 Conclusion

We study six major tax reforms in France over the past thirty years—including two payroll tax reforms with strong tax-benefit linkage, two payroll tax reforms without linkage, and two income tax reforms, also without linkage. While these reforms differ in design, they all occur within the same French labor market context and provide clear variations in tax-benefit linkage, allowing for a systematic analysis of their effects. Using a panel difference-in-differences framework that exploits between-worker variation in tax rates over time, we find that payroll tax increases are fully passed through to workers when a strong relationship exists between contributions and expected benefits, with pass-through estimates close to 100%, regardless of statutory incidence. In contrast, when tax-benefit linkage is absent, we find limited individual-level pass-through of employer payroll taxes to workers within five to six years, with estimates not significantly different from zero. In the case of an employee income tax reform, we find full shifting to workers, suggesting that in the absence of a contribution-benefit link, economic incidence at the individual level aligns with statutory incidence. A meta-analysis of the literature confirm that these marked differences in pass-through extend beyond the six French reforms examined, emphasizing the importance of accounting for tax-benefit linkage to reconcile the conflicting estimates of payroll tax incidence across existing studies.

From a policy perspective, our results provide empirical support for the argument that a clear tax-benefit linkage enhances efficiency when using payroll taxes to fund social insurance programs. However, our analysis has some limitations. First, we conduct limited tests on potential quantity responses. While do not find employment effects in cases of strong tax-benefit linkage, as expected, our ability to detect quantity effects in the absence of linkage is constrained by data limitations. Second, aggregate incidence remains uncertain for non-contributory payroll taxes: while we can firmly establish that contributory payroll taxes are fully borne by workers, the zero pass-through result observed at the individual level for non-contributory employer payroll taxes may still imply some degree of shifting to workers at the firm or market level. Further research is needed to better understand these dynamics and their broader implications for the optimal design of payroll taxes.

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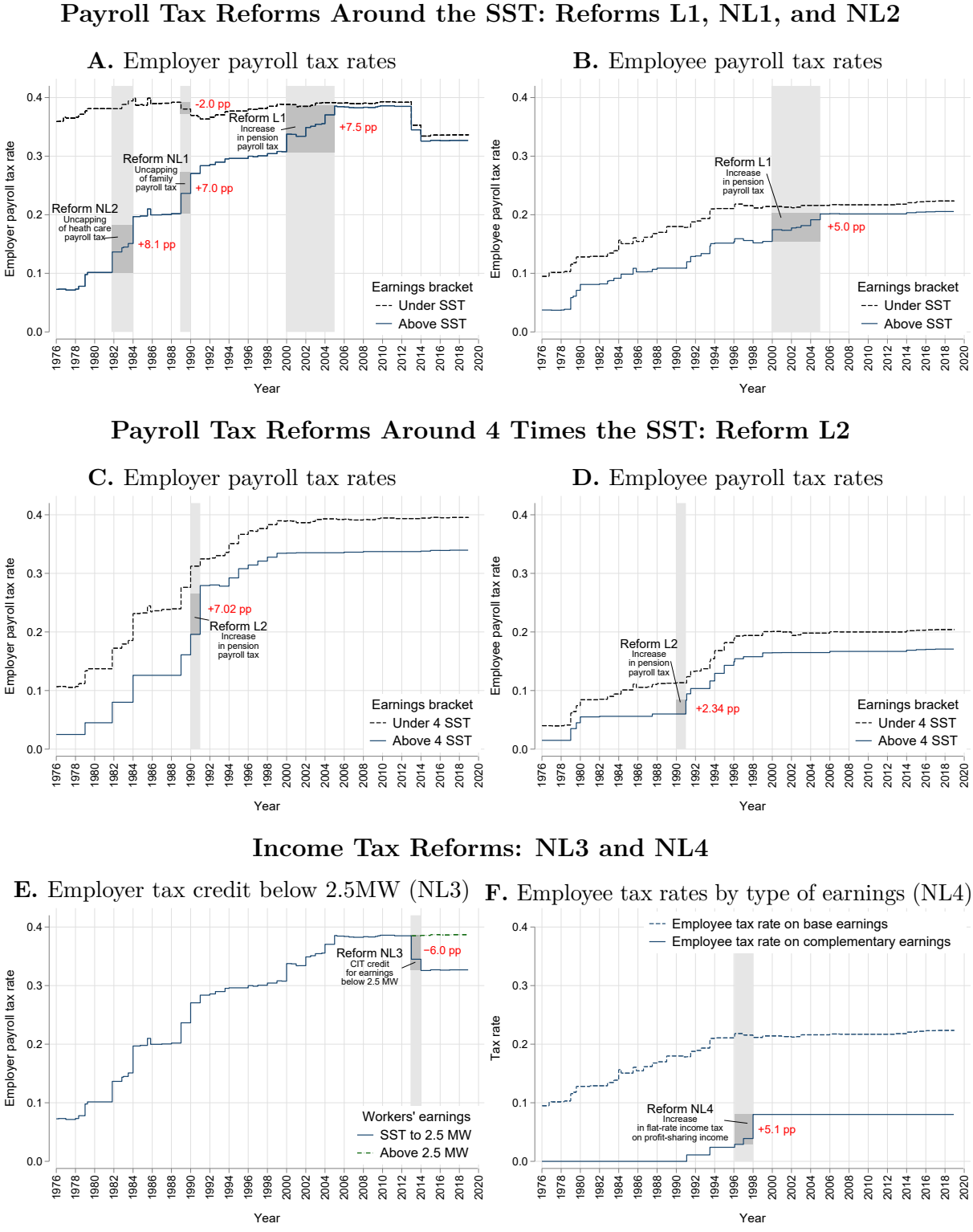
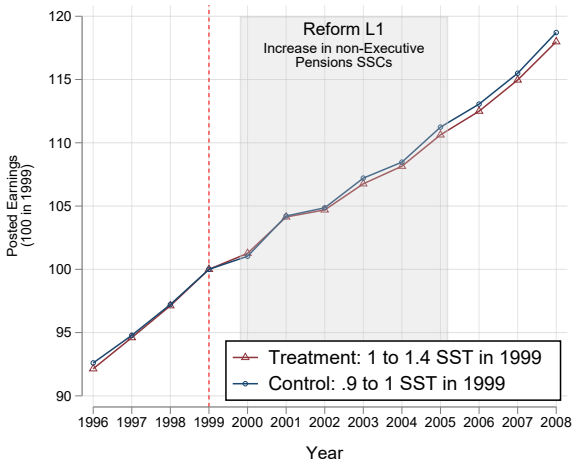


Figure 1 – Payroll Tax and Income Tax Reforms: Marginal Tax Rates Above and Below Earnings Thresholds, 1976–2019

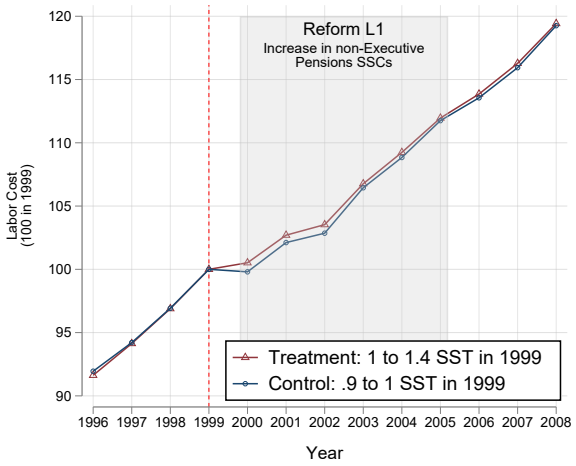
Notes: Marginal payroll tax rates are expressed as a fraction of posted earnings. These rates are applied to different earnings brackets, defined with respect to the Social Security threshold (SST) or the Minimum Wage (MW). In Panels A and B (Reforms L1, NL1, and NL1), the rates apply to non-executive workers, affiliated with the ARRCO complementary pension scheme, while in Panels C and D (Reform L2), they apply to executive workers, affiliated with the AGIRC complementary pension scheme. In Panel E, Reform NL3 induced a cut in average tax rate, rather than a change in the marginal tax rate. In Panel F (Reform NL4), profit-sharing income refers to earnings received by employees under mandatory profit-sharing schemes (*intéressement et participation*). Reform NL4 led to an increase in the CSG rate on earnings (inclusive of profit-sharing income), which was offset by a reduction in employee payroll taxes, which previously did not apply to profit-sharing income.
Sources: Institut des Politiques Publiques (2023); TAXIPP 0.4.

Reform L1: Increase in Pension Payroll Tax for Non-Executives (2000–2005)

A. L1: Posted earnings (w)

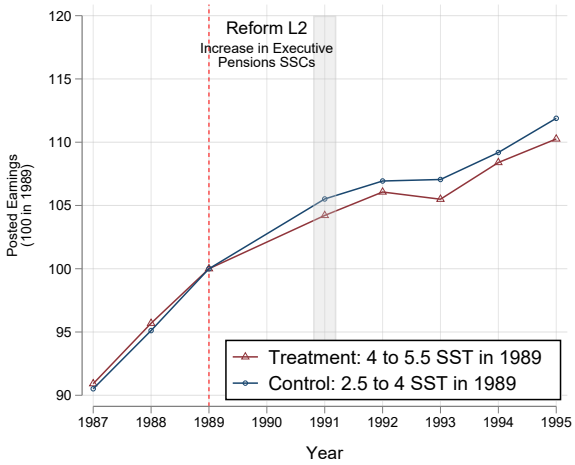


B. L1: Labor cost (z)



Reform L2: Increase in Pension Payroll Tax for Executives (1991)

C. L2: Posted earnings (w)



D. L2: Labor cost (z)

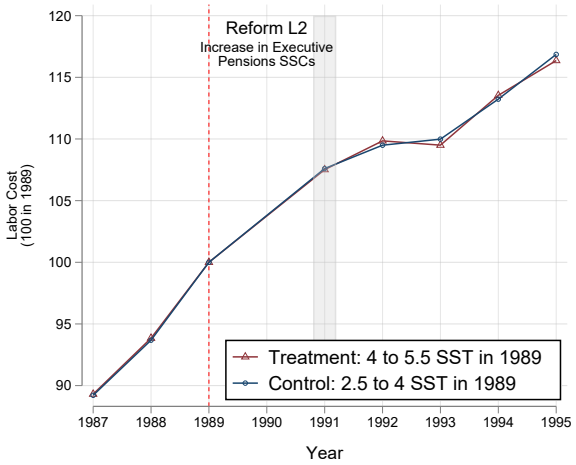
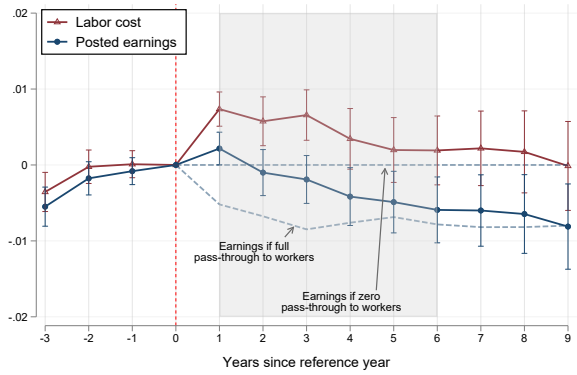


Figure 2 – Earnings Responses to Payroll Tax Reforms with Linkage

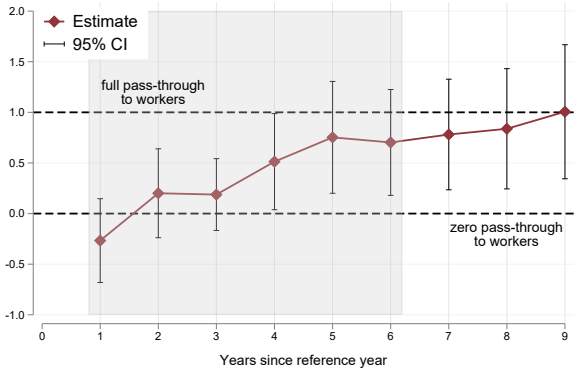
Notes: The figure shows the evolution of average real posted earnings (Panels A and C) and average real labor cost (Panels B and D) for reforms pertaining to payroll taxes with tax-benefit linkage (L1 and L2), separately for workers in the treatment and control groups. For Reform L1 (Panels A and B), the treatment (control) group consists of workers whose posted earnings in the reference year (1999) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform L2 (Panels C and D), the treatment (control) group consists of workers whose posted earnings in the reference year (1989) were between 4 and 5.5 (2.5 and 4) times the SST that year, using a balanced panel of workers. Earnings levels are normalized to 100 for each group in the reference year. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year.
Sources: DADS Panel; TAXIPP 0.4.

Reform L1: Increase in Pension Payroll Tax for Non-Executives (2000–2005)

A. L1: DiD estimates

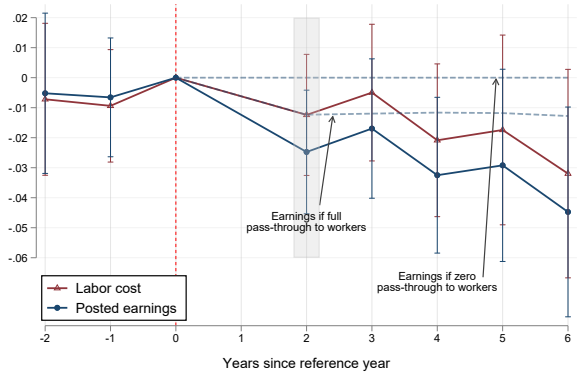


B. L1: Pass-through to workers (2SLS)



Reform L2: Increase in Pension Payroll Tax for Executives (1991)

C. L2: DiD estimates



D. L2: Pass-through to workers (2SLS)

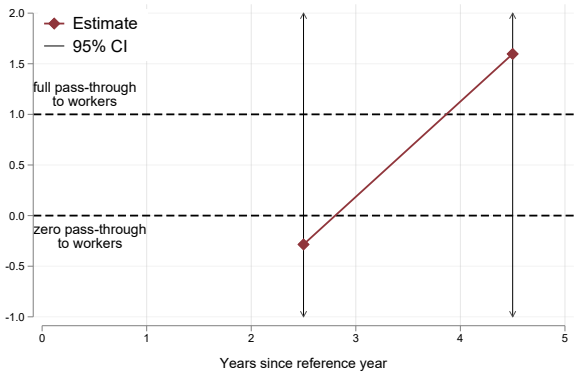
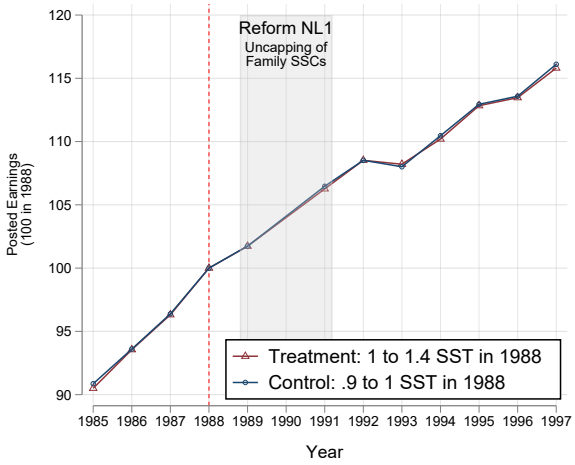


Figure 3 – Estimated Pass-Through to Workers for Payroll Tax Reforms with Linkage

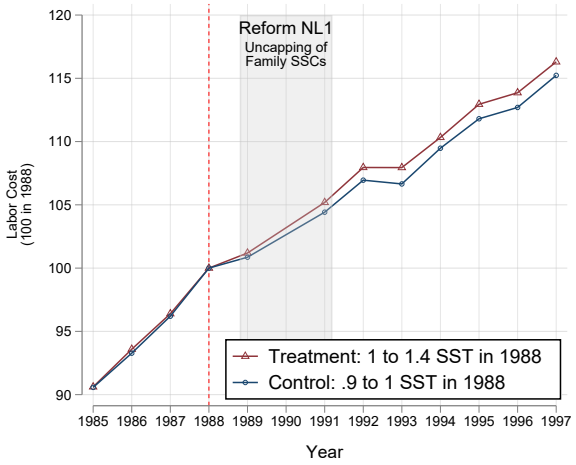
Notes: Panels A and C show the differential growth in the log of real posted earnings (circle markers) and average real labor costs (triangle markers) between two groups of workers that were affected differently by reforms L1 and L2, while Panels B and D show the estimated pass-through of the payroll tax increases to workers. For Reform L1 (Panels A and B), the treatment (control) group consists of workers whose posted earnings in the reference year (1999) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform L2 (Panels C and D), the treatment (control) group consists of workers whose posted earnings in the reference year (1989) were between 4 and 5.5 (2.5 and 4) times the SST that year, using a balanced panel of workers. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year. In Panels A and C, the markers represent the differential growth in posted earnings and labor costs between treated and control workers for both pre-reform and post-reform years, relative to the reference year. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level. The blue dashed lines represent the predicted evolution of posted earnings under the counterfactual scenarios of zero pass-through and full pass-through of the payroll tax change to workers. Panels B and D present estimates of pass-through to workers, obtained from a 2SLS estimation of Equation (6) in the main text, where $\log(1+\tau)$ is instrumented by the interaction between the treatment group and year dummies. Diamond markers show the estimates while the vertical T-bars show the 95% confidence intervals (arrows indicate confidence intervals that extend beyond the y-axis range displayed). In the case of Reform L2, given the limited sample size, we aggregate years 2 and 3, as well as years 4 and 5.
Sources: DADS Panel; TAXIPP 0.4.

Reform NL1: Uncapping of Family Payroll Tax (1989 and 1990)

A. NL1: Posted earnings (w)

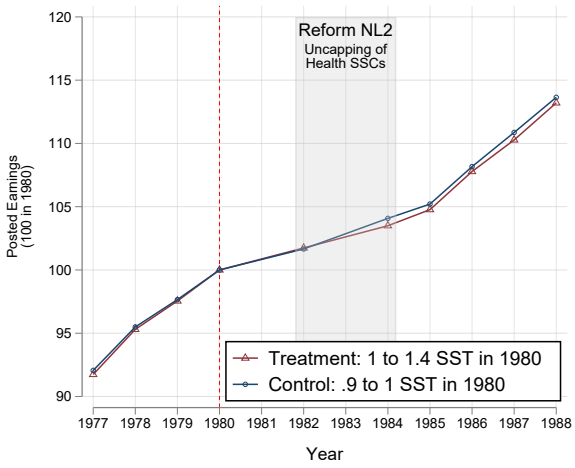


B. NL1: Labor cost (z)



Reform NL2: Uncapping of Health Care Payroll Tax (1981 and 1983)

C. NL2: Posted earnings (w)



D. NL2: Labor cost (z)

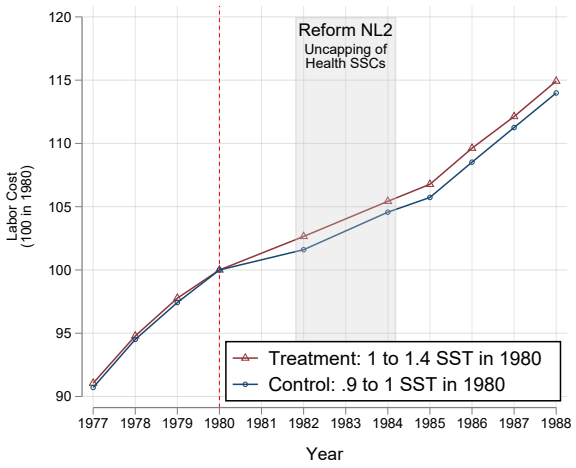


Figure 4 – Earnings Responses to Payroll Tax Reforms without Linkage

Notes: The figure shows the evolution of average real posted earnings (Panels A and C) and average real labor cost (Panels B and D) for reforms pertaining to payroll taxes without tax-benefit linkage (NL1 and NL2), separately for workers in the treatment and control groups. The results for Reform NL1 (uncapping of family payroll tax) are shown in Panels A and B, while the results for Reform NL2 (uncapping of health care payroll tax) are shown in Panels C and D. For both reforms, the treatment (control) group consists of workers whose posted earnings in the reference year (1988 for NL1 and 1980 for NL2) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. Earnings levels are normalized to 100 for each group in the reference year. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year.
Sources: DADS Panel; TAXIPP 0.4.

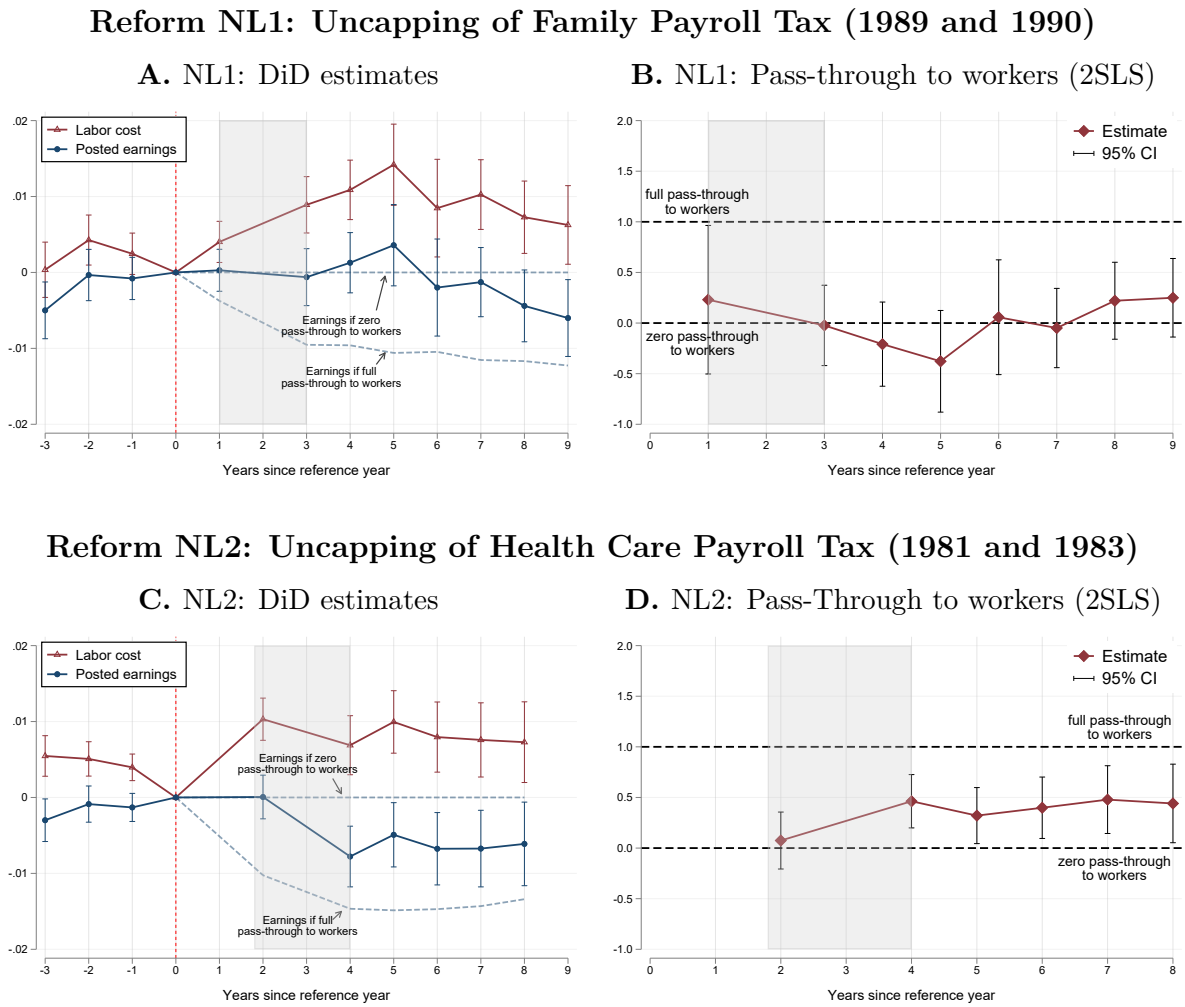
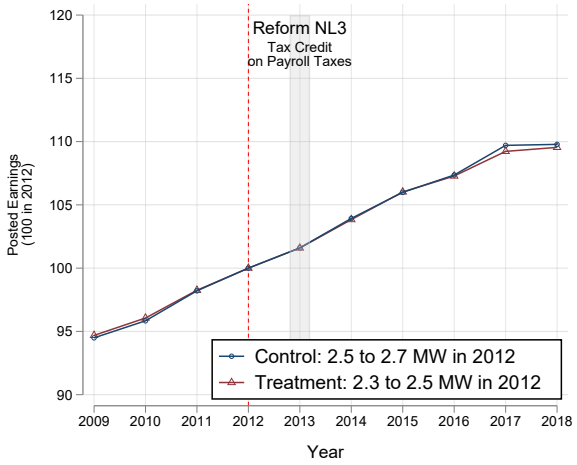


Figure 5 – Estimated Pass-Through to Workers for Payroll Tax Reforms without Linkage

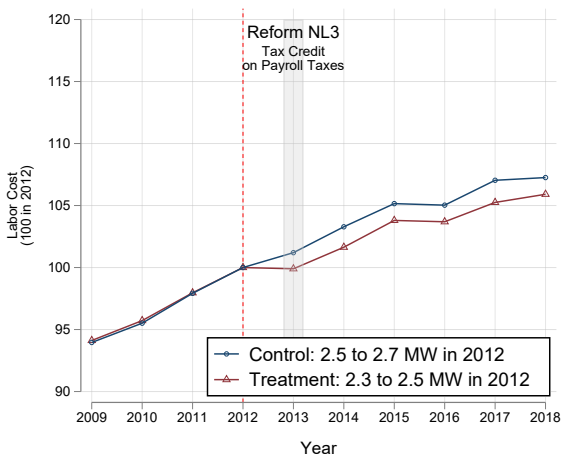
Notes: Panels A and C show the estimated log differences in average real posted earnings (circle markers) and average real labor costs (triangle markers) between two groups of workers that were affected differently by reforms NL1 and NL2, while Panels B and D show the estimated pass-through of the payroll tax increases to workers. The results for Reform NL1 (uncapping of family payroll tax) are displayed in Panels A and B, while the results for Reform L2 (uncapping of health care payroll tax) are displayed in Panels C and D. For both reforms, the treatment (control) group consists of workers whose posted earnings in the reference year (1988 for NL1 and 1980 for NL2) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year. In Panels A and C, the markers represent the differential growth in posted earnings and labor costs between treated and control workers for both pre-reform and post-reform years, relative to the reference year. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level. The blue dashed lines represent the predicted evolution of posted earnings under the counterfactual scenarios of zero pass-through and full pass-through of the payroll tax change to workers. Panels C and D present estimates of pass-through to workers, obtained from a 2SLS estimation of Equation (6) in the main text, where $\log(1+\tau)$ is instrumented by the interaction between the treatment group and year dummies. Diamond markers show the estimates while the vertical T-bars show the 95% confidence intervals.
Sources: DADS Panel; TAXIPP 0.4.

Reform NL3: Employer Tax Credit on Payroll Taxes

A. NL3: Posted earnings (w)

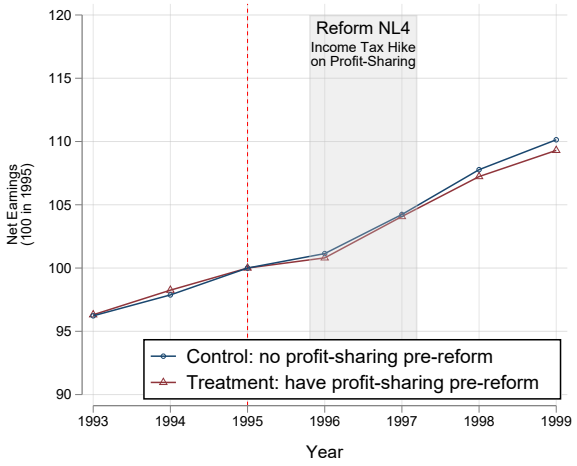


B. NL3: Labor cost (z)



Reform NL4: Employee Income Tax Hike on Profit-Sharing

C. NL4: Net earnings (n)



D. NL4: Labor cost (z)

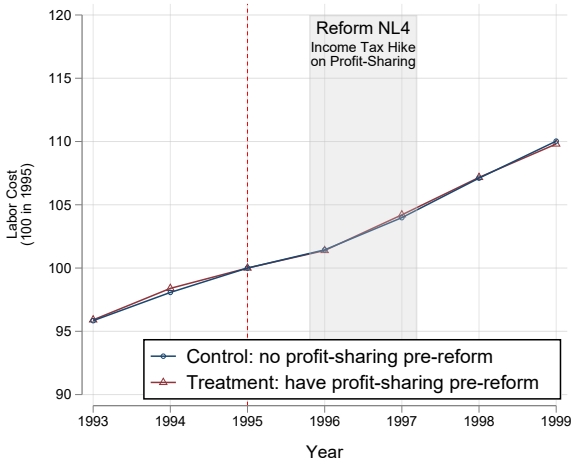
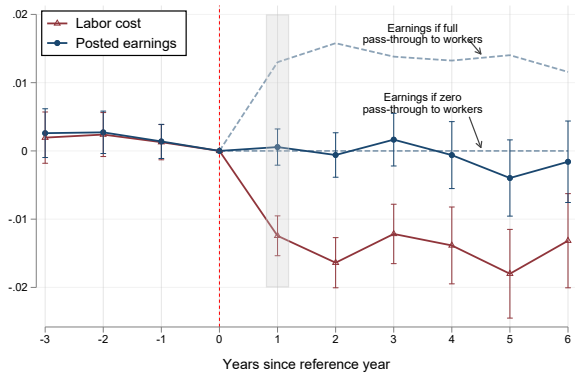


Figure 6 – Earnings Responses to Income Tax Reforms Based on Payroll

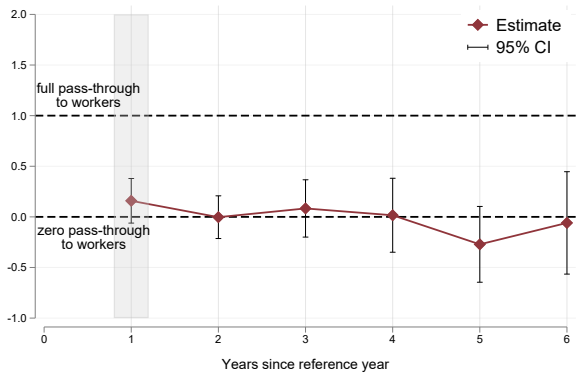
Notes: The figure shows the evolution of average real posted earnings (Panel A), real net earnings (Panel C), and average real labor cost (Panels B and D) for income tax reforms without tax-benefit linkage (NL3 and NL4), separately for workers in the treatment and control groups. The results for Reform NL3 (an employer tax credit on payroll taxes for earnings below 2.5 times the minimum wage) are shown in Panels A and B, while the results for Reform NL4 (an increase in employees' flat-rate withholding tax on earnings from profit-sharing) are shown in Panels C and D. For Reform NL3, the treatment (control) group consists of workers whose posted earnings in the reference year (2012) were between 2.3 and 2.5 (2.5 and 2.7) times the minimum wage (MW) that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform NL4, the sample is restricted to workers employed in firms with non-negative profits during the pre-reform period (1993–1995) and having at least 10 employees. The treatment group consists of workers in the firms that distributed profit-sharing bonuses at least once in the pre-reform, while the control group consists of workers in the firms that did not distribute such bonuses. Earnings levels are normalized to 100 for each group in the reference year. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year.
Sources: DADS Panel; TAXIPP 0.4.

Reform NL3: Employer Tax Credit on Payroll Taxes

A. NL3: DiD estimates

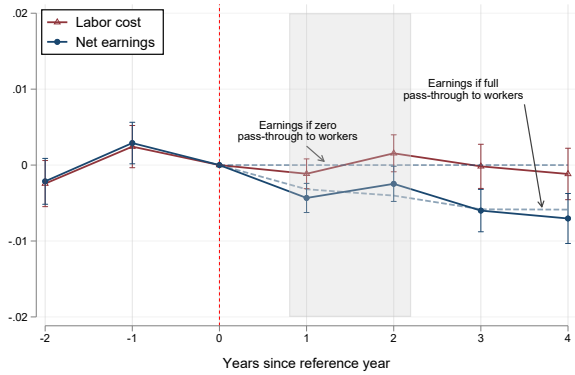


B. NL3: Pass-through to workers (2SLS)



Reform NL4: Employee Income Tax Hike on Profit-Sharing

C. NL4: DiD estimates



D. NL4: Pass-through to workers (2SLS)

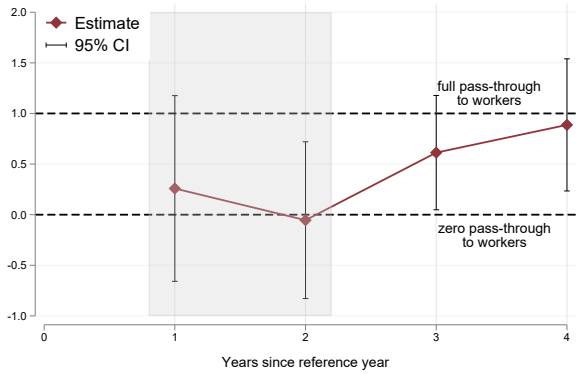


Figure 7 – Estimated Pass-Through to Workers for Income Tax Reforms Based on Payroll

Notes: Panels A and C show the estimated log differences in the average real posted earnings (Panel A, circle markers), the average real net earnings (Panel C, circle markers), and the average real labor cost (triangle markers) between two groups of workers that were affected differently by reforms NL3 and NL4, while Panels B and D show the estimated pass-through of the tax increases to workers. The results for Reform NL3 (an employer tax credit on payroll taxes for earnings below 2.5 times the minimum wage) are displayed in Panels A and B, while the results for Reform NL4 (an increase in employees' flat-rate withholding tax on earnings from profit-sharing) are displayed in Panels C and D. For Reform NL3, the treatment (control) group consists of workers whose posted earnings in the reference year (2012) were between 2.3 and 2.5 (2.5 and 2.7) times the minimum wage (MW) that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform NL4, the sample is restricted to workers employed in firms with non-negative profits during the pre-reform period (1993–1995) and having at least 10 employees. The treatment group consists of workers in the firms that distributed profit-sharing bonuses at least once in the pre-reform, while the control group consists of workers in the firms that did not distribute such bonuses. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year. In Panels A and C, the markers represent the differential growth in posted earnings (Panel A) or net earnings (Panel B) and labor costs between treated and control workers for both pre-reform and post-reform years, relative to the reference year. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level. The blue dashed lines represent the predicted evolution of posted earnings (Panel A) and net earnings (Panel C) under the counterfactual scenarios of zero pass-through and full pass-through of the payroll tax change to workers. Panels B and D present estimates of pass-through to workers, obtained from a 2SLS estimation of Equation (6) in the main text, where $\log(1+\tau)$ is instrumented by the interaction between the treatment group and year dummies. Diamond markers show the estimates while the vertical T-bars show the 95% confidence intervals.
Sources: DADS Panel; TAXIPP 0.4.

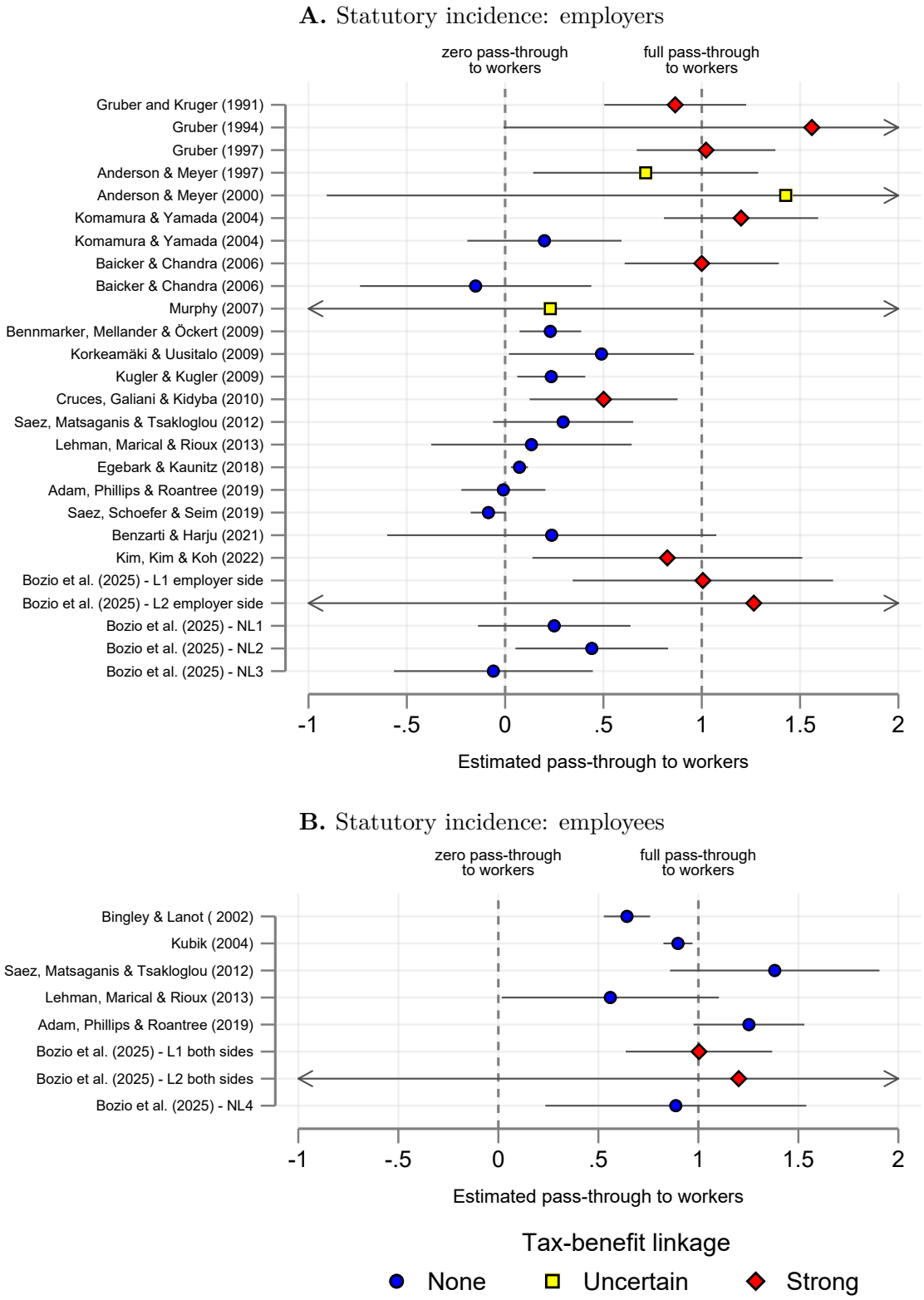


Figure 8 – Meta-Analysis of Payroll Tax Incidence

Notes: The sources used to construct this figure can be found in Appendix Table D2. The papers are ordered by the corresponding year/period of the reform(s) being studied, from the oldest (top) to the most recent (bottom). The markers show the study-specific estimated pass-through rate of payroll taxes to workers. The estimates in Panel A are for payroll taxes statutorily incident on employers, while those in Panel B are for payroll taxes or income taxes statutorily incident on employees. The horizontal T-bars denote the 95% confidence intervals around the point estimates. Arrows indicate confidence intervals that extend beyond the x-axis range displayed.

Table 1 – Overview of Tax Reforms and Theoretical Predictions on Incidence

Tax-Benefit linkage	Statutory incidence	Type of tax reform	Reform(s)	Predicted Incidence	
				Wages	Quantities
Yes	Employers	Payroll tax	L1, L2	A broad class of models predict full pass-through to workers at the individual level	No effects on quantities if there is full pass-through to workers
	Employees	Payroll tax	L1, L2		
No	Employers	Payroll tax	NL1, NL2	Predicted wages and quantity responses depend on model and elasticities	
		Income tax (payroll-based corp. tax credit)	NL3		
	Employees	Payroll tax	[None]		
		Income tax	NL4		

Notes: This table displays all possible combinations of tax-benefit linkage (2) × statutory incidence (2) × type of tax reform (2), alongside the reforms analyzed in this paper and the corresponding theoretical predictions about their incidence and employment effects (see Appendix A). By definition, income tax with linkage does not occur.

Table 2 – Summary Statistics for the Treatment and Control Groups

Sample:	Control Group		Treatment Group	
	Mean	S.D.	Mean	S.D.
Panel A. Reform L1: Increase in pension payroll tax for non-executives (2000–2005)				
Reference year (t_0): 1999				
Posted earnings:	0.9 to 1 SST		1 to 1.4 SST	
Percentile rank in the earnings distribution	[P64–P72]		[P72–P88]	
Age	41.4	(8.9)	43.1	(8.4)
Male	0.72	(0.45)	0.75	(0.43)
Annual posted earnings	30,078	(911)	36,457	(3,415)
Annual labor cost	43,366	(1,673)	52,276	(4,838)
Number of individuals	19,626		33,605	
Number of observations	183,022		314,511	
Panel B. Reform L2: Increase in pension payroll tax for executives (1991)				
Reference year (t_0): 1989				
Posted earnings:	2.5 to 4 SST		4 to 5.5 SST	
Percentile rank in the earnings distribution	[P97-P100]		[P100]	
Age	44.6	(6.1)	46.9	(6.4)
Male	0.94	(0.23)	0.94	(0.24)
Annual posted earnings	84,092	(11,012)	125,608	(13,750)
Annual labor cost	114,270	(14,941)	167,338	(18,155)
Number of individuals	1,321		334	
Number of observations	10,568		2,672	
Panel C. Reform NL1: Uncapping of family payroll tax (1989 and 1990)				
Reference year (t_0): 1988				
Posted earnings:	0.9 to 1 SST		1 to 1.4 SST	
Percentile rank in the earnings distribution	[P61–P69]		[P69–P86]	
Age	39	(9.3)	40.6	(8.8)
Male	0.74	(0.44)	0.77	(0.42)
Annual posted earnings	26,074	(789)	31,805	(3,025)
Annual labor cost	37,498	(1,363)	45,091	(3,953)
Number of individuals	24,390		46,579	
Number of observations	203,900		393,571	
Panel D. Reform NL2: Uncapping of health care payroll tax (1981 and 1983)				
Reference year (t_0): 1980				
Posted earnings:	0.9 to 1 SST		1 to 1.4 SST	
Percentile rank in the earnings distribution	[P58–P67]		[P67–P86]	
Age	38	(10.7)	39.4	(10.1)
Male	0.76	(0.43)	0.79	(0.41)
Annual posted earnings	22,409	(683)	27,432	(2,627)
Annual labor cost	31,850	(1,041)	38,065	(3,080)
Number of individuals	30,988		63,134	
Number of observations	209,487		438,438	
Panel E. Reform NL3: Employer tax credit on payroll taxes (2013)				
Reference year (t_0): 2012				
Posted earnings:	2.5 to 2.7 MW		2.3 to 2.5 MW	
Percentile rank in the earnings distribution	[P80-P84]		[P76-P80]	
Age	45.4	(9.1)	44.9	(9.3)
Male	0.75	(0.43)	0.74	(0.44)
Annual posted earnings	42,185	(939)	38,922	(934)
Annual labor cost	61,486	(2,061)	56,765	(1,992)
Number of individuals	13,374		20,448	
Number of observations	108,679		166,955	
Panel F. Reform NL4: Employee income tax hike on profit-sharing (1996 and 1997)				
Reference year (t_0): 1995				
Group:	No profit-sharing		Has profit-sharing	
Percentile rank in the earnings distribution	[P1-P100]		[P1-P100]	
Age	39.0	(9.9)	38.9	(9.6)
Male	0.70	(0.46)	0.67	(0.47)
Annual posted earnings	30,578	(23,460)	32,368	(22,213)
Annual labor cost	43,322	(32,952)	45,506	(31,100)
Number of individuals	29,326		72,538	
Number of observations	153,976		410,144	

Notes: Each panel provides summary statistics for the sample used in the analysis of each reform. The statistics are presented for the reference year and for the baseline sample of full-time and full-year workers, separately for the treatment and control groups. Gross earnings and labor costs are expressed in 2010 euros. Standard deviations are shown in parentheses. Mean differences between the treatment and control groups are significant at the 5% level for all variables.
Sources: DADS Panel; TAXIPP 0.4.

Table 3 – Pass-Through of Payroll Tax Reforms with Tax-Benefit Linkage

	Payroll tax reforms			
Reform:	L1 Increase in pension payroll tax for non-executives		L2 Increase in pension payroll tax for executives	
Reference year (t_0):	1999	1999	1989	1989
Last year ($t_{\max}$):	2008	2008	1995	1995
Side:	Employer	Employer + Employee	Employer	Employer + Employee
	(1)	(2)	(3)	(4)
Pass-through to workers:				
Last year	1.01***	1.00***	1.26	1.20
	(0.34)	(0.19)	(2.01)	(1.52)
Final years	0.88***	0.93***	0.74	0.78
	(0.28)	(0.15)	(1.46)	(1.17)
N	496,039	496,039	13,240	13,240

Notes: This table reports difference-in-differences estimates of the pass-through to workers of payroll tax reforms with tax-benefit linkage. For Reform L1 (columns 1 and 2), the treatment (control) group consists of workers whose posted earnings in the reference year (1999) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform L2 (columns 3 and 4), the treatment (control) group consists of workers whose posted earnings in the reference year (1989) were between 4 and 5.5 (2.5 and 4) times the SST that year, using a balanced panel of workers. The pass-through estimates are obtained from a 2SLS regression of log(posted earnings) (columns 1 and 3) or log(net earnings) (columns 2 and 4) on $\log(1+\tau)$ (see Equation (6) in the main text), where τ denotes the average employer payroll tax rate (columns 1 and 3) or the sum of the average employer and employee payroll tax rates (columns 2 and 4), and $\log(1+\tau)$ is instrumented by the interaction between the treatment group and year dummies. "Last year" is the pass-through estimate in the last year of the post-reform period, while "Final years" is the average over the last three years. Standard errors clustered at the individual level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Sources: DADS Panel; TAXIPP 0.4.

Table 4 – Pass-Through of Payroll and Income Tax Reforms without Tax-Benefit Linkage

	Payroll tax reforms		Income tax reforms	
Reform:	NL1 Uncapping of family payroll tax	NL2 Uncapping of health care payroll tax	NL3 Tax credit on employer payroll tax	NL4 Employee income tax on profit-sharing
Reference year (t_0):	1988	1980	2012	1995
Last year ($t_{\max}$):	1997	1988	2018	1999
Side:	Employer	Employer	Employer	Employee
	(1)	(2)	(3)	(4)
Pass-through to workers:				
Last year	0.25	0.44**	−0.06	0.89***
	(0.20)	(0.20)	(0.26)	(0.33)
Final years	−0.02	0.41***	−0.05	0.75***
	(0.18)	(0.13)	(0.14)	(0.80)
N	594,632	642,289	275,307	555,033

Notes: See notes of Table 3. For Reforms NL1 and NL2 (columns 1 and 2), the treatment (control) group consists of workers whose posted earnings in the reference year (1988 for NL1 and 1980 for NL2) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform NL3 (column 3), the treatment (control) group consists of workers whose posted earnings in the reference year (2012) were between 2.3 and 2.5 (2.5 and 2.7) times the minimum wage (MW) that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform NL4 (column 4), the sample is restricted to workers employed in firms with non-negative profits during the pre-reform period (1993–1995) and having at least 10 employees. The treatment group consists of workers in the firms that distributed profit-sharing bonuses at least once in the pre-reform, while the control group consists of workers in the firms that did not distribute such bonuses. For Reform NL4, "Final years" is the average of the pass-trough estimates over the last two years post-reform. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Sources: DADS Panel; TAXIPP 0.4.

Table 5 – Tests for Differences in Pass-Through

	Test-statistic (1)	p-value (2)	Rejected at $p < 0.05$? (3)
Panel A. Incidence of payroll tax reforms with linkage			
a) Test: incidence is on employers			
Employer payroll tax: H0: $\rho(\text{L1}) = \rho(\text{L2}) = 0$	$\chi^2(2) = 9.26$	0.010	Yes
Employer + employee payroll tax: H0: $\rho(\tilde{\text{L1}}) = \rho(\tilde{\text{L2}}) = 0$	$\chi^2(2) = 29.48$	< 0.001	Yes
b) Test: incidence is on employees			
Employer payroll tax: H0: $\rho(\text{L1}) = \rho(\text{L2}) = 1$	$\chi^2(2) = 0.018$	0.991	No
Employer + employee payroll tax: H0: $\rho(\tilde{\text{L1}}) = \rho(\tilde{\text{L2}}) = 1$	$\chi^2(2) = 0.018$	0.991	No
Panel B. Incidence of payroll and income tax reforms without linkage			
a) Test: incidence is statutory			
H0: $\rho(\text{NL1}) = \rho(\text{NL2}) = \rho(\text{NL3}) = 0, \rho(\text{NL4}) = 1$	$\chi^2(4) = 6.72$	0.152	No
b) Test: incidence is not statutory			
H0: $\rho(\text{NL1}) = \rho(\text{NL2}) = \rho(\text{NL3}) = 1, \rho(\text{NL4}) = 0$	$\chi^2(4) = 46.30$	< 0.001	Yes
Panel C. Difference in incidence between tax reforms with and without linkage			
a) Test: same incidence for reforms statutorily incident on employers			
H0: $\text{mean}(\rho(\text{L1}), \rho(\text{L2})) = \text{mean}(\rho(\text{NL1}), \rho(\text{NL2}), \rho(\text{NL3}))$	z-score $= 2.14$	0.032	Yes
b) Test: same incidence for reforms statutorily incident on employees			
H0: $\text{mean}(\rho(\tilde{\text{L1}}), \rho(\tilde{\text{L2}})) = \rho(\text{NL4})$	z-score $= 0.31$	0.755	No

Notes: ρ(R) denotes the pass-through parameter for reform R in the last year of the estimation period. For reforms L1 and L2, ρ(L1) and ρ(L2) correspond to the estimated pass-through to workers of the employer payroll tax reforms, while $\rho(\tilde{\text{L1}})$ and $\rho(\tilde{\text{L2}})$ correspond to the estimated pass-through to workers of the whole reform (employer plus employee payroll taxes). For tests presented in Panels A and B, we rely on the fact that the sum of k squared independent reduced centered normal distributions follows a $\chi^2(k)$ distribution. For tests presented in Panel C, each point-estimate entering a mean is weighted by the inverse of its estimated variance. We then rely on the fact that a linear combination of independent normal distributions follows a normal distribution.
Sources: Estimates used for the formal tests are those provided in Tables 3 and 4 for the last year of the estimation period.

(For Online Publication)

Appendix to

Does Tax-Benefit Linkage Matter for the Incidence of Payroll Taxes?

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This Appendix provides complements to the theoretical framework presented in Section 2 (Appendix A), perceptions of the reforms from press and labor union archives (Appendix B), information on the data sources (Appendix C), a meta-analysis of payroll tax incidence (Appendix D), robustness checks (Appendix E), and estimates of the effects of the reforms on employment outcomes (Appendix F).

A Theory Appendix

This appendix derives general formulas for the pass-through to workers of employer payroll taxes with tax-benefit linkage. Section A.1 presents the general setup and specifies our definition of the tax-benefit linkage. Section A.2 derives the partial equilibrium pass-through formula in the standard competitive labor market model. Section A.3 extends the analysis to a a general equilibrium framework with two types of workers.

A.1 Setup and General Overview

Production function. We consider a representative firm that uses $n+1$ factors of production: capital (denoted by K) and n types of labor inputs, which we denote by L_1 to L_n. In particular, the labor inputs include those of the treated and control workers that

we consider in the empirical analysis, which are denoted by L_T and L_C, respectively. The firm's production function $F(K, L_1, ..., L_n)$ is assumed to be homogeneous of degree one and to exhibit positive and diminishing marginal products.

A potential caveat when linking the theory to our research design is that we empirically define treated and control workers based on their wage levels rather than on an exogenous measure of skills such as educational attainment. The models discussed in this appendix are used to study the market response to changes in payroll taxes, and the treatment and control groups are empirically defined as a function of wages *prior* to the reforms, which can be considered as exogenous to the payroll tax increases. Our approach, therefore, consists in defining workers with different pre-reform wage levels as different inputs in production, and to evaluate how their wages respond to changes in payroll taxes under alternative models.

For simplicity, we assume that all payroll taxes are nominally paid by the firm and we denote by w_k the posted wage of type-k workers. The corresponding labor cost to the firm is denoted by $z_k \equiv w_k(1 + \tau_k)$, where τ_k is the rate of payroll taxes that are nominally paid by the firm for a worker with posted wage w_k.

The firm's after-tax profit is given by

$$\Pi = pF(K, L_1, ..., L_n) - rK - \sum_k z_k L_k,$$

where p is the output price and r is the cost of capital. Corporate taxes are ignored for simplicity. When computing the pass-through formulas under the general competitive equilibrium, we will assume that there are only two types of inputs, which are the theoretical counterparts of the control (L_C) and treated (L_T) workers in our empirical analysis. We further normalize the output price p to be 1 without loss of generality.

Labor supply with tax-benefit linkage. On the supply side, we denote by $\tilde{w}_k \equiv w_k(1 + q\tau_k)$ the perceived wage of type-k workers, i.e., their posted wage augmented by the perceived benefits from employer payroll taxes. The tax-benefit linkage is modeled as a reduced-form parameter q whose value lies in the interval $[0, 1]$. This parameter, which is assumed to be constant across worker types, measures the extent to which workers value employer payroll taxes relative to cash income. It subsumes two dimensions of tax-benefit linkage that we do not model separately: (i) the actuarial fairness of payroll taxes, which determines the degree of linkage between these taxes and future benefit entitlements at the individual level, and (ii) the salience of this linkage to workers.

If one assumes perfect information (strong salience), the degree of tax-benefit linkage can be measured as the ratio between the expected flow of benefits accrued from additional payroll taxes paid, and the value of this flow if it had been instead saved at the market rate of return r. This purely actuarial approach neglects the fact that payroll taxes may

force workers to over-save in comparison to their counterfactual savings in the absence of social insurance. It also ignores that forcing agents to save may be beneficial if they are too myopic to fully internalize the future benefits of their savings. Measuring the salience of tax-benefit linkage (the extent to which agents are informed of and understand the linkage) and its exact consequences on workers' utility is a challenging task. For these reasons, we simply model the linkage through a single reduced-form parameter q that enters the indirect utility function and represents the present value of the payroll taxes paid in monetary terms. This indirect utility may be seen as the result from the maximization of a utility function $U(C_k, L_k)$, which depends on consumption C_k and employment L_k, under the constraint $C_k \leq w_k(1 + q\tau_k)L_k + R_k$, where R_k denotes non-labor income. The case with $q = 1$ represents fully salient linkage, i.e., a situation akin to the *quid pro quo* tax described in the classic public finance literature (Musgrave, 1968). The opposite polar case with $q = 0$ means that there is no linkage or that there is linkage but that future benefits are not perceived by workers because of lack of salience.

We model the extensive margin of labor supply by assuming that type-k workers have an indirect utility function $V(\tilde{w}_k, R_k) = V(w_k(1 + q\tau_k), R_k)$, which is increasing in their perceived wage $\tilde{w}_k$ and non-labor income R_k. The labor supply of a type-k worker can then be expressed as

$$L_k^S = L_k^S(\tilde{w}_k, R_k) = L_k^S(w_k(1 + q\tau_k), R_k), \qquad k = 1, \ldots, n. \tag{A.1}$$

Denoting η_k^S the labor supply elasticity of workers of type k, we have

$$\eta_k^S \equiv \frac{\mathrm{d} \ln L_k^S}{\mathrm{d} \ln w_k} = (1 + q\tau_k) w_k \frac{l_k^S}{L_k^S}, \qquad k = 1, \ldots, n, \tag{A.2}$$

where we use $l_k^S \equiv \partial L_k^S / \partial \tilde{w}_k$ to denote the partial derivative of the labor supply of type-k workers with respect to the perceived wage $\tilde{w}_k$. Since non-labor income R_k is assumed to be independent of payroll taxes, it will be omitted in future notations.

Definition of payroll tax pass-through: individual vs. firm level. In our setup, we consider two groups of workers: treated workers (L_T), for whom employer payroll taxes change as the result of the reform being studied, and control workers (L_C), whose employer payroll taxes are unchanged.

We define the pass-through of employer payroll taxes to workers at the individual level as the pass-through of payroll taxes to the wages of treated workers relative to control workers. This corresponds to the main empirical specification in the paper. Pass-through is understood at the individual level since the individual wages of the treated workers are directly affected by the change in payroll taxes.

We define the pass-through of employer payroll taxes to workers at the firm level as the pass-through of employer payroll taxes to control workers, i.e., to workers in a firm affected

by the change in employer payroll taxes but whose labor cost is nominally unchanged by the reform under study. This tax shifting can be understood as firm-level pass-through in the sense that individual-level pass-through can be zero (no change in the relative wages of treated and control workers) while employer payroll taxes are shifted to all workers in the firm, whether treated or control.

A.2 Partial Equilibrium with Market Clearing and Linkage

We start by deriving the simple partial equilibrium formula for the pass-through of payroll taxes with tax-benefit linkage (Kotlikoff and Summers, 1987; Gruber, 1997).

The labor supply of workers of type k is given by Equation (A.1). On the demand side, the partial equilibrium approach ignores possible substitutions between inputs and assumes that the labor demand for type-k workers depends only on their labor cost, i.e.,

$$L_k^D = L_k^D(z_k) = L_k^D\Big(w_k(1+\tau_k)\Big), \qquad k = 1, \ldots, n. \tag{A.3}$$

Denoting η_k^D the labor demand elasticity of type-k workers, we have

$$\eta_k^D \equiv \frac{\mathrm{d}\ln L_k^D}{\mathrm{d}\ln w_k} = (1+\tau_k) w_k \frac{l_k^D}{L_k^D}, \qquad k = 1, \ldots, n,$$

where $l_k^D \equiv \partial L_k^D / \partial z_k$ denotes the partial derivative of the labor demand of type-k workers with respect to the labor cost z_k.

Totally differentiating the labor supply and demand conditions (A.1) and (A.3) yields

$$\mathrm{d}\ln(L_k^S) = \eta_k^S\Big(\mathrm{d}\ln w_k + \mathrm{d}\ln(1+q\tau_k)\Big), \qquad k = 1, \ldots, n,$$

and

$$\mathrm{d}\ln(L_i^D) = \eta_k^D\Big(\mathrm{d}\ln w_k + \mathrm{d}\ln(1+\tau_k)\Big), \qquad k = 1, \ldots, n,$$

where η_k^S and η_k^D are the labor supply and labor demand elasticities for type-k workers.

Equating variations in supply and demand and rearranging terms, we obtain the following formula for the pass-through rate of payroll taxes to workers:

$$\frac{\mathrm{d}\ln w_k}{\mathrm{d}\ln(1+\tau_k)} = -\frac{\eta_k^D - q\frac{1+\tau_k}{1+q\tau_k}\eta_k^S}{\eta_k^D - \eta_k^S}, \qquad k = 1, \ldots, n. \tag{A.4}$$

In the absence of tax-benefit linkage ($q = 0$), this expression simplifies to the standard incidence formula in which the pass-through rate of payroll taxes to workers depends solely on the relative magnitude of the labor supply and labor demand elasticities: if $|\eta_k^D| \gg |\eta_k^S|$, payroll taxes are fully passed through to workers; if $|\eta_k^D| \ll |\eta_k^S|$, the pass-through to

workers is zero. With full linkage ($q = 1$), payroll taxes are fully passed through to workers irrespective of the labor demand and supply elasticities.

A.3 General Equilibrium in Competitive Labor Markets

In this section, we adapt Feldstein (1974)'s general equilibrium model of tax incidence to incorporate tax-benefit linkage in the analysis of the pass-through of payroll taxes to workers.

We assume a one sector model in which the representative firms' output is produced using two labor inputs (L_T and L_C) with constant returns to scale:

$$Y = F(L_T, L_C). \tag{A.5}$$

The assumption of perfectly competitive markets implies that both types of workers are paid their marginal products. Denoting F_T (resp. F_C) the partial derivative of the production function F with respect to L_T (resp. L_C), we have

$$F_T = w_T(1 + \tau_T), \tag{A.6}$$
$$F_C = w_C(1 + \tau_C). \tag{A.7}$$

The system is completed by the two labor supply equations:

$$L_T^S = L_T^S(\tilde{w}_T) = L_T^S\Big(w_T(1 + q\tau_T)\Big), \tag{A.8}$$
$$L_C^S = L_C^S(\tilde{w}_C) = L_C^S\Big(w_C(1 + q\tau_T)\Big). \tag{A.9}$$

We consider a small change in the rate of payroll taxes that applies to treated workers, $\mathrm{d}\tau_T$. The effects of this tax change on the equilibrium wages of treated and control workers can be analyzed by totally differentiating equations (A.5) through (A.9):

$$F_{TT} \cdot \mathrm{d}L_T + F_{CT} \cdot \mathrm{d}L_C = \mathrm{d}w_T(1 + \tau_T) + \mathrm{d}\tau_T \cdot w_T, \tag{A.10}$$
$$F_{CT} \cdot \mathrm{d}L_T + F_{CC} \cdot \mathrm{d}L_C = \mathrm{d}w_C(1 + \tau_C), \tag{A.11}$$
$$\mathrm{d}L_T = l_T^S \cdot \left[\mathrm{d}w_T(1 + q\tau_T) + q\mathrm{d}\tau_T \cdot w_T\right], \tag{A.12}$$
$$\mathrm{d}L_C = l_C^S \cdot \left[\mathrm{d}w_C(1 + q\tau_C)\right]. \tag{A.13}$$

Note that because $F(\cdot, \cdot)$ is homogeneous of degree 1, the marginal products F_T and F_C are homogeneous of degree 0. By Euler's theorem, this implies

$$F_{CT} = \frac{F_{TT} \cdot F_{CC}}{F_{CT}}. \tag{A.14}$$

Pass-through to treated workers. Using Equation (A.14) to substitute for F_{CT} in the system formed by equations (A.10) to (A.13), one obtains the following expression for the change in the wage rate of treated workers resulting from a one percent change in the rate of payroll taxes that are levied on their wage:

$$\frac{\mathrm{d}\ln w_T}{\mathrm{d}\ln(1+\tau_T)} = \frac{1+\tau_T}{w_T}\cdot\frac{\mathrm{d}w_T}{\mathrm{d}\tau_T} = -\frac{\left[F_{CC}\cdot l_C^S\left(\frac{1+q\tau_C}{1+\tau_C}\right)-1\right]+qF_{TT}\cdot l_C^S}{\left[F_{CC}\cdot l_C^S\left(\frac{1+q\tau_C}{1+\tau_C}\right)-1\right]+\left(\frac{1+q\tau_T}{1+\tau_T}\right)F_{TT}\cdot l_T^S}. \tag{A.15}$$

This expression can be rewritten as a function of economically meaningful parameters. Let $\alpha_T \equiv (1+\tau_T)w_T L_T/Y$ denote the labor share of treated workers. Using Euler's equations, the local elasticity of substitution between treated and control workers, which we denote by σ, can be shown to satisfy the following equations:[1]

$$\sigma = -\frac{(1+\tau_T)w_T(1-\alpha_T)}{F_{TT}\cdot L_T} \tag{A.16}$$

and

$$\sigma = -\frac{(1+\tau_C)w_C\alpha_T}{F_{CC}\cdot L_C}. \tag{A.17}$$

Using Equation (A.2) to replace the labor supply elasticities of treated and control workers in the elasticity of substitution formulas (A.16) and (A.17) yields

$$F_{TT}\cdot l_T^S\left(\frac{1+q\tau_T}{1+\tau_T}\right) = -(1-\alpha_T)(\eta_T^S/\sigma) \tag{A.18}$$

and

$$F_{CC}\cdot l_C^S\left(\frac{1+q\tau_C}{1+\tau_C}\right) = -\alpha_T(\eta_C^S/\sigma). \tag{A.19}$$

Plugging equations (A.18) and (A.19) into (A.15) and rearranging terms yields the following formula for the pass-through rate of payroll taxes to treated workers:

$$\frac{\mathrm{d}\ln w_T}{\mathrm{d}\ln(1+\tau_T)} = -\frac{1+\alpha_T(\eta_C^S/\sigma)+q\left(\frac{1+\tau_T}{1+q\tau_T}\right)(1-\alpha_T)(\eta_T^S/\sigma)}{1+\alpha_T(\eta_C^S/\sigma)+(1-\alpha_T)(\eta_T^S/\sigma)}. \tag{A.20}$$

Full tax-benefit linkage ($q=1$) implies that payroll taxes are entirely shifted to treated workers. In the absence of linkage ($q=0$), the pass-through rate depends on the relative magnitudes of the labor supply elasticities of treated and control workers, η_T^S and η_C^S, and the elasticity of substitution between worker types, σ. If σ is very large relative to η_T^S

[1] Note that the derivations do not require a constant labor share α_T (as would be the case for a Cobb-Douglas production function) nor a constant elasticity of substitution (as for a CES production function); these parameters are only used to describe a local property of the production function in the neighborhood where the economy is operating when the tax change is introduced.

and η_C^S, changes in payroll taxes are full passed through to treated workers. If, instead, $\eta_T^S \approx \eta_C^S \gg \sigma$, payroll taxes are passed through to treated workers in proportion of their share α_T in total output.

Pass-through to control workers. Using similar calculations as for the treated workers, we obtain the following formula for the pass-through of treated workers' payroll taxes to control workers:

$$\frac{\mathrm{d}\ln w_C}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\alpha_T(\eta_T^S/\sigma)\left(\frac{1-q}{1+q\tau_T}\right)}{1+\alpha_T(\eta_C^S/\sigma)+(1-\alpha_T)(\eta_T^S/\sigma)}. \tag{A.21}$$

Full tax-benefit linkage ($q = 1$) implies that control workers' wages are unaffected by changes in treated workers' payroll taxes. In the absence of linkage ($q = 0$), the pass-through to control workers depends on the relative magnitude of the labor supply elasticities of treated and control workers, η_T^S and η_C^S, and the elasticity of substitution between both types of workers, σ. If σ is very large relative to η_T^S, the wages of control workers are unaffected by changes in treated workers' payroll taxes. In the case where $\eta_T^S \approx \eta_C^S$, the pass-through to control workers simplifies to $\alpha_T/(1+\sigma/\eta_T^S)$. In this case, when labor supply elasticities are large relatively to σ, treated workers' payroll taxes are shifted to control workers in proportion of the labor share of treated workers α_T in total output.

Individual-level pass-through (relative wages). Combining equations (A.20) and (A.21) yields the following formula for the pass-through of an increase in the payroll taxes of treated workers to the relative wages of treated and control workers, which is the parameter that we estimate in our empirical analysis:

$$\frac{\mathrm{d}\ln\left(\frac{w_T}{w_C}\right)}{\mathrm{d}\ln(1+\tau_T)} = -\frac{1+\alpha_T(\eta_C^S/\sigma)+\left(q\left(\frac{1+\tau_T}{1+q\tau_T}\right)-\alpha_T\right)(\eta_T^S/\sigma)}{1+\alpha_T(\eta_C^S/\sigma)+(1-\alpha_T)(\eta_T^S/\sigma)}. \tag{A.22}$$

Full tax-benefit linkage ($q = 1$) implies a 100% pass-through of treated workers' payroll taxes to the wage ratio between treated and control workers. In the absence of linkage ($q = 0$), the pass-through rate depends on the relative magnitudes of the labor supply elasticities of treated and control workers, η_T^S and η_C^S, and the elasticity of substitution between worker types, σ. If σ is very large, changes in treated workers' payroll taxes are fully passed through to the relative wages of treated workers. If $\eta_T^S \approx \eta_C^S \gg \sigma$, the relative wages of treated and control workers are unaffected by the tax change.

In the special case where the labor supply elasticities of treated and control workers are equal (i.e., $\eta_T^S = \eta_C^S = \eta^S$), the above pass-through formulas simplify to

$$\frac{\mathrm{d}\ln w_T}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\sigma+\eta^S\left[\alpha_T+q\left(\frac{1+\tau_T}{1+q\tau_T}\right)(1-\alpha_T)\right]}{\sigma+\eta^S} \approx -\frac{\sigma+\eta^S\left[\alpha_T+q(1-\alpha_T)\right]}{\sigma+\eta^S}, \tag{A.23}$$

$$\frac{\mathrm{d}\ln w_C}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\eta^S \cdot \alpha_T \left(\frac{1-q}{1+q\tau_T}\right)}{\sigma+\eta^S} \approx -\frac{\eta^S \cdot \alpha_T(1-q)}{\sigma+\eta^S}, \tag{A.24}$$

$$\frac{\mathrm{d}\ln\left(\frac{w_T}{w_C}\right)}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\sigma+\eta^S \cdot q\left(\frac{1+\tau_T}{1+q\tau_T}\right)}{\sigma+\eta^S} \approx -\frac{\sigma+\eta^S \cdot q}{\sigma+\eta^S}, \tag{A.25}$$

where the approximations hold when the payroll tax rate τ_T is small. Equation (A.25) corresponds to Equation (1) in the main text and clarifies the theoretical interpretation of our empirical estimates when a competitive labor market equilibrium is assumed (ignoring capital as well as interactions with the product market). Note that this relatively simple expression for the pass-through of employer payroll taxes to relative wages does not require a specific functional form for the production function as it holds for any function with constant returns to scale.

Quantity responses. The effects of a change in the payroll tax rate of treated workers on employment levels for both worker types can be derived from Equations (A.12) and (A.13) using the above pass-through formulas. In the special case where labor supply elasticity is equal across worker types ($\eta^S_T = \eta^S_C = \eta^S$), the employment effects are given by:

$$\frac{\mathrm{d}\ln L_T}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\eta^S\left(\frac{1-q}{1+q\tau_T}\right)(\sigma+\eta^S\alpha_T)}{\sigma+\eta^S} \approx -\frac{\eta^S(\sigma+\eta^S\alpha_T)(1-q)}{\sigma+\eta^S}, \tag{A.26}$$

$$\frac{\mathrm{d}\ln L_C}{\mathrm{d}\ln(1+\tau_T)} = -\frac{(\eta^S)^2\alpha_T\left(\frac{1-q}{1+q\tau_T}\right)}{\sigma+\eta^S} \approx -\frac{(\eta^S)^2\alpha_T(1-q)}{\sigma+\eta^S}, \tag{A.27}$$

$$\frac{\mathrm{d}\ln\left(\frac{L_T}{L_C}\right)}{\mathrm{d}\ln(1+\tau_T)} = -\frac{\sigma\cdot\eta^S\left(\frac{1-q}{1+q\tau_T}\right)}{\sigma+\eta^S} \approx -\frac{\sigma\cdot\eta^S(1-q)}{\sigma+\eta^S}. \tag{A.28}$$

Under full tax-benefit linkage ($q = 1$), changes in the payroll tax rate of treated workers have no impact on the employment levels of treated and control workers. In the absence of linkage ($q = 0$), employment effects depend on the relative magnitude of the labor supply elasticity, η^S, and the elasticity of substitution between worker types, σ. If $\sigma \gg \eta^S$, a 1% increase in the payroll tax rate or treated workers reduces the employment of treated workers by η^S%, while the employment level of control workers remains unchanged. Conversely, if $\eta^S \gg \sigma$, the same 1% increase in the payroll tax rate of treated workers reduces their employment by $(\sigma+\eta^S\alpha_T)$% and that of control workers by $\eta^S\alpha_T$%, implying a relative employment decline of σ% for treated workers relative to control workers.

Firm-level pass-through (impact on control workers). Firm-level shifting of payroll taxes with limited effects on the relative wages of treated and control workers cannot be easily rationalized within the standard competitive labor market model. To simplify the discussion, we consider the special case where the elasticity of labor supply is constant across worker types, i.e., $\eta^S_T = \eta^S_C = \eta^S$. It follows immediately from Equation (A.24) that

in the case of full tax-benefit linkage ($q = 1$), the wages of control workers are not affected by a change in the tax levied on treated workers.

Let us now assume no linkage ($q = 0$). Equation (A.24) shows that the extent to which a tax change for treated workers is passed through to control workers depends on the labor share of treated workers, α_T, and the relative magnitude of the elasticity of substitution, σ, and of the labor supply elasticity, η^S. The wage of control workers will respond only if the labor share of treated workers, α_T, is large enough and if σ is not too large relative to η^S. Assuming a labor supply elasticity of 0.5,[2] our pass-through estimates for Reforms NL1 (0.25), NL2 (0.44) and NL3 (-0.06) would imply implausibly low values of the elasticity of substitution between treated and control workers, between -0.03 and 0.39. As a matter of comparison, elasticities of substitution between skilled and unskilled workers (college and high-school equivalents) in the U.S. have been estimated to be around 1.5 (e.g., Autor et al., 2008) and to be even larger in France, with estimates ranging from 2 to almost 4 (Charnoz et al., 2011; Verdugo, 2014). Our treated and control workers are likely to be better substitutes than college and high-school equivalents in empirical studies of the skill premium. Indeed, the pre-reform gap between the average wages of these workers is around 25%, whereas the wage gap between college and high-school equivalents in France is around 50%. Based on Equation (A.25), this observation leads us to conclude that the limited pass-through of employer payroll taxes to the relative wages of treated and control workers found in the cases of Reforms NL1, NL2, and NL3 is difficult to reconcile with the standard competitive model of the labor market—unless implausible assumptions are made regarding the elasticity of substitution between both types of workers. Similarly, our formula for the pass-through to control workers (Equation A.24) makes a large pass-through to control workers hardly plausible in the context of the standard model.

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[2]Labor supply elasticity estimates are numerous and vary widely (Blundell and MaCurdy, 1999). The estimate of 0.5 is taken from the meta-analysis by Chetty (2012). For France, available estimates are lower, between 0.1 and 0.3 (Bourguignon and Magnac, 1990).

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B Perceptions of the Payroll Tax Reforms

In this appendix, we provide a comprehensive account of the information extracted from archives belonging to press outlets and unions concerning the reforms under investigation.

B.1 L1: Pension Payroll Tax Increase for Non-Executives (1996)

Press articles. Reform L1 was decided in an agreement from April 25, 1996. It affected payroll taxes for the pension scheme of non-executives (ARRCO). The majority of press articles discussing this reform highlight the fact that it secured approval from a majority of labor unions, with the notable exception of the *Confédération Générale du Travail* (C.G.T.), historically affiliated with the French Communist Party. The agreement saw the endorsement of *Force Ouvrière* (F.O.), the *Confédération Française Démocratique du Travail* (C.F.D.T.), the *Confédération Générale des Cadres* (C.G.C.), the *Confédération Française des Travailleurs Chrétiens* (C.F.T.C.), and the employer federation *Conseil National du Patronat Français* (C.N.P.F.).[3] A prevailing theme in the press is that the agreement resulted in a reduction in to the rate of return for these schemes. For instance, an article from *Les Échos* stresses that "100 Francs of contribution are offering today (...) 8.94 Francs of pension, and the reform will lead to a reduction to 7.20 Francs of pension."[4]

Most newspaper articles suggest that the positive counterpart of the reduction in the returns to pension contribution was the planned increase in contributions for individuals with earnings above the Social Security threshold. For instance, an article from *Le Monde* states that "the agreement also includes the provision that wage earners whose wage exceeds the Social Security threshold would be able to secure a better pension for themselves: The contribution rate is set to increase to 16% by 2005 for employees of existing firms, and as early as 2000 for firms established after January 1, 1997."[5]

[3] "Tous d'accord sauf la CGT sur les retraites complémentaires," *Libération*, April 27, 1996.

[4] "Retraites complémentaires: négociations pour un accord à l'arraché," *Les Échos*, April 26, 1996.

[5] "La baisse des retraites complémentaires est programmée," *Le Monde*, April 27, 1996.

Archives from the C.F.D.T. union. We were granted access to the archives of the C.F.D.T. union, where we had the opportunity to review all internal documents as well as materials such as documents, leaflets, or magazines directed towards C.F.D.T. members.

In a document specifically addressing pension benefits for union members,[6] the agreement on the complementary schemes AGIRC-ARRCO, dated April 25, 1996, is outlined under the title "Improving Pension Rights". The document further details the increase in payroll tax contributions as follows: "*Commencing from January 1, 2000, for existing firms and January 1, 1997, for new firms, the mandatory social security contribution rate will gradually rise to 16%, as opposed to the previous rate of 6%, applicable to the portion of the wage exceeding the Social Security threshold.* ***This measure will enable non-executives to increase their pension benefits above the threshold by a factor of 2.66.*** *Firms have the option to implement this measure immediately.*"

In one of the union's magazines, the agreement is advocated as a commendable compromise: "*The agreement is aimed at ensuring the financial balance of the schemes until the year 2005. It does not involve any reduction in pension rights already acquired or pension benefits already claimed. The reduction pertains to the volume of pension rights to be allocated in the future. The C.F.D.T. obtained a reduction of these cuts in pension rights and a more gradual implementation.*" The text proceeds to highlight the positive aspects of this compromise, particularly the increase in payroll tax rates above the threshold: "*The agreement also incorporates numerous positive elements that are significant in terms of additional pension rights for future retirees. For non-executives, this involves the implementation of a rate of 16% for wages above the Social Security threshold.*" When detailing the measures, the article underscores the enhancement in future pensions attributed to the advocacy of the C.F.D.T. union: "*From January 1, 1997, for new companies and from January 1, 2000, for existing companies, non-executive employees and their companies will gradually contribute 16% (instead of 6%) of the salary above the Social Security threshold.* ***This demand from the C.F.D.T. will allow non-executives to enhance their future pensions and will result in increased pay compensations for employers.***"[7]

B.2 L2: Pension Payroll Tax Increase for Executives (1988)

Reform L2, established in an agreement from March 24, 1988, raised the upper threshold for payroll taxation from 4 times the SST to 8 times the SST. As a result, previous voluntary complementary pensions schemes for those earning levels, specifically IRICASE, IRCASUP, and the *Caisse des cadres supérieurs du bâtiment et des travaux publics* (CCSBTP), were incorporated into the AGIRC scheme. Importantly, the capital funds from these previous schemes were transferred to the AGIRC fund.

[6] *Bulletin du Retraité CFDT*, No. 140, July August-September 1996.
[7] *Le retraité militant CFDT*, No. 96.06, June 1996, pp. 6–8.

There are few press articles covering this reform, which impacted a very small group of highly paid wage earners. The daily newspaper *Le Monde* portrayed the debate surrounding the reform primarily as a conflict within employer unions. The insurance industry, opposed to the reform, argued against it, while the rest of the employers favored providing additional pensions to their top-paid executives. The insurance industry contended that mandating pension payroll taxes for high earners would diminish the incentives for saving through private pension funds, thereby reducing capital accumulation. Consequently, the head of the employer unions, who was also the CEO of a major insurance firm, resigned from his leadership role in the employer union.[8]

B.3 NL1: Increase in Family Payroll Tax (1989)

The announcement made by the French Prime Minister Michel Rocard in September 1988 to remove the cap on the employer payroll tax for family benefits was part of a broader policy initiative known as the "Plan Emploi," aimed at combating unemployment. The public debate pitted employer organizations and executives' unions, critical of the reform, against other labor unions that supported the idea of higher contributions for firms employing higher-paid workers.[9] We have come across press articles addressing the potential incidence or impact on the wages of workers earning above the Social Security threshold. However, the debate between employee unions (executives vs. non-executives) suggests a concern that the reform might affect the wages of those in higher-paid positions.

B.4 NL2: Increase in Health Care Payroll Tax (1981)

On November 12, 1981, the newspaper *Le Monde* covered the political debate sparked by the French government's announcement to raise the health care payroll tax.[10] Alain Bocquet, a French MP from the Communist Party, expressed disapproval of the 1% increase in employee payroll taxes across all earnings. However, he endorsed the increase in employer payroll taxes, stating, "*Il faut faire payer les patrons!*" (You have to make the bosses pay!). In contrast, the center-right opposition criticized the payroll tax hikes targeting firms, with one MP from the U.D.F. (center-right party) remarking, "*On ne peut faire mieux en matière de politique anti-emploi*" (You can't do better in terms of anti-job policy). In another article, *Le Monde* reports on the negative reactions from most labor unions.[11] The majority of unions criticized the government's plans, particularly

[8] "L'accord sur les retraites des cadres a été signé. Le président de la Fédération des assurances abandonne ses fonctions au CNPF", *Le Monde*, 3.26.1988.

[9] "Incertitudes autour de l'effet sur l'emploi du déplafonnement des cotisations familiales," *Le Monde*, 8.9.1988.

[10] "Solidarité nationale : M. Bocquet (P.C.) proteste contre l'augmentation des cotisations de la Sécurité sociale," *Le Monde*, 12.11.1981.

[11] "Réactions hostiles du patronat et des syndicats. Les retombées amères de la solidarité," *Le Monde*, 12.11.1981.

the increase in employee payroll taxes across all earnings. Employer organizations also strongly opposed the rise in employer payroll taxes. The article further highlights the inconsistency of uncapping employer payroll taxes during a period of high unemployment, pointing out the anticipated adverse effects on investment and employment decisions.

Following the announcement of the payroll tax increases, a contentious debate unfolded between the government and employer organizations. In an article from *Le Monde*, the minister in charge, Nicole Questiaux, is quoted as stating, "*The financial measures were chosen with the goal of combating unemployment. The decision to increase employers' payroll taxes exclusively above the Social Security threshold is justified by our intention to protect what are referred to as labor-intensive companies.*"[12]

B.5 NL3: Employer Tax Credit on Payroll Taxes (2013)

Reform NL3 was introduced following the release of a report commissioned by the French government from Louis Gallois, a former CEO of major industrial firms. The report, titled *Pacte pour la compétitivité de l'industrie française* (Pact for the Competitiveness of French Industry), was published in November 2012. It proposed various measures, including a recommendation to reduce payroll taxes by a total of 30 billion euros for wages up to 3.5 times the minimum wage. It also suggested shifting these contributions to other tax bases (such as VAT or the flat-rate income tax CSG) while simultaneously reducing public spending. Shortly after the report's publication, the government announced its intention to implement most of the recommended measures. This included the introduction of a tax credit calculated on the payroll of wage earners up to 2.5 times the minimum wage.

The debate surrounding the reform in the press focused less on employee and employer unions and more on political parties, as the National Assembly was responsible for discussing and voting on the reform. Within the left-leaning majority, many MPs expressed concerned about giving a "blank check" to firms without any counterparts.[13] On the other hand, right-leaning MPs, who opposed the government, were divided over whether to support this employer payroll tax cut.[14] Meanwhile, the main employer union emphasized the necessity of maintaining the tax credit up to 2.5 times the minimum wage without any reduction.[15] The fact that the payroll tax cut was legislated as a tax credit strengthened the perception that it would not impact future benefits.

B.6 NL4: Increase in Flat-Rate Income Tax CSG (1997)

The introduction of Reform NL4 in 1997 was motivated by the need to increase tax revenues across a broad income base. As a result, the increase of the flat-rate income tax

[12] "La polémique se poursuit sur les charges des entreprises," *Le Monde*, 11.21.1981
[13] "Compétitivité: bronca dans les rangs socialistes sur le crédit d'impôt," *Le Monde*, 11.28.2012.
[14] "A l'Assemblée, la droite souligne les faiblesses du crédit d'impôt," *Le Monde*, 12.5.2012.
[15] "Le gouvernement peaufine son crédit d'impôt pour la compétitivité," *Les Echos*, 11.27.2012.

CSG impacted all forms of income, including labor and capital income, as well as most income types previously exempt from taxation (such as pensions, tax-free savings schemes, profit-sharing schemes). The combination of a reduction in employee payroll taxes and an increase in the CSG essentially resulted in a net increase in taxation, lowering the taxation of earnings subject to payroll taxes while raising it for all other income that was formerly taxed at a lower rate. Press reports from that period emphasized that this change would impact all individuals with either capital income or tax-free savings. In *Le Monde*, the reform's philosophy was described as "taxing less labor income and more capital income" and was noted for enjoying broad political support.[16]

C Data Sources

C.1 Earnings Data

DADS panel data. Our primary data source is the matched employer-employee DADS (*Déclaration Annuelle de Données Sociales*) Panel, developed by the French Institut National de la Statistique et des Études Économiques (INSEE). This panel is constructed from the mandatory annual declarations submitted by all employers for each of their employees.

The DADS Panel comprises a 1/25th sample of private sector employees born in October of even-numbered years, starting from 1976. In 2002, the sample size was doubled to represent 1/12th of all private sector workers. The dataset include approximately 1.1 million workers annually between 1976 and 2001, and 2.2 million workers from 2002 onwards. Some original data sources are currently unavailable for certain years (1981, 1983, and 1990), leading to missing data in the panel.

Earnings definition. The DADS Panel provides only one measure of annual earnings that remains consistent over the entire period: net taxable earnings, as reported to tax authorities by employers (variable `SN`). Net taxable earnings represent earnings after deduction of Social Security contributions but before deduction of the non-income-tax-deductible component of flat-rate contributions, namely the *Contribution sociale généralisée* (CSG) and the *Contribution au remboursement de la dette sociale* (CRDS).

From 1993 onwards, the panel includes additional earnings variables: the CSG tax base for gross earnings (variable `SB`) and net earnings (variable `NETNET`). Before 1993, INSEE provides an estimate of gross earnings based on the reported net taxable earnings, but gross earnings for payroll tax purposes are not available in the data released by INSEE. Net earnings correspond to the amount effectively paid by firms to employees after deduction of specific employee contributions to restaurant vouchers or public transport passes, but before payment of the income tax.

[16] "La contribution sociale généralisée bénéficie d'un consensus politique," *Le Monde*, 6.9.1997.

As a result, we use the only raw information available throughout the entire period, i.e., net taxable earnings, and employ our microsimulation model to compute gross (posted) earnings, as well as labor costs inclusive of employer payroll taxes.

Multiple employers. The DADS data offers details on all job spells for each worker in the sample. For individuals with multiple employers, we calculate the total earnings by summing up earnings across all employers, as the payroll tax rate is determined based on the total annual earnings.

C.2 Micro-Simulation of Payroll Taxes

TAXIPP model. Microsimulation techniques are required to compute the individual-level labor cost based on the information available in the DADS Panel data. Our study relies on the TAXIPP model developed at the Institut des Politiques Publiques (IPP), specifically leveraging the payroll tax module. This model takes as input the payroll tax schedule as input, source from the IPP Tax and Benefit Tables,[17] and computes employee and employer payroll taxes, reductions in employer payroll taxes, flat-rate income tax (CSG and CRDS), and other payroll taxes. The TAXIPP model simulates the complexity of French payroll taxes in great detail, including local Social Security schemes such as the one in place in the Alsace-Moselle region.

Identification of private sector firms. We identify private sector firms based on the variable `ST` (i.e., status), which categorizes firms according to their public or private ownership. However, a challenge arises from the waves of privatization and nationalizations experienced by several large firms in the 1980s. For instance, in 1986, the center-right government executed a large-scale privatization of major state-owned companies, which was partly reversed in 1988, before another wave of privatization occurred in 1993. Consequently, some firms end up classified in the private sector while they were originally in the public sector (and vice versa). To address this issue, we restrict our sample to firms consistently identified as private sector entities throughout the entire period. This restriction ensures the exclusion of large utilities and certain publicly-owned firms that underwent privatization during the specified period.

Identification of the applicable Social Security threshold. The Social Security threshold (SST) is defined for the pay period and the number of hours worked as specified in the employment contract. However, because we lack information on the number of hours worked before 1993, determining the SST for individuals working part-time during that period is not feasible. To address this, we identify individuals working full-year based on the duration of the job spell (variable `DP` or *durée de paie*). Additionally, we identify

[17] `http://www.ipp.eu/en/ipp-tax-and-benefit-tables/social-security-contributions/`.

full-time workers using the variable `CE` (or *condition d'emploi*), available throughout the entire period. From 1993 onwards, we leverage the effective number of hours worked (variable `NBHEUR`) to precisely determine the SST applicable to each worker, including those working part-time or less than a full year.

For individuals with multiple jobs, we aggregate earnings and job spells, as payroll taxes are calculated based on the total remuneration across all jobs.

Firms created after 1997. For the implementation of Reform L1, the ARRCO agreement of April 1996 specified that firms created from 1997 onwards would experience a faster increase in the payroll tax rate compared to existing firms. Specifically, firms established after 1996 were subject to the maximum employer payroll tax rate of 12% as early as 2000, whereas pre-existing firms reached the maximum rate in 2005.

The phasing-in of Reform L1 means that we need to identify the date of firm creation to accurately apply the payroll tax schedule. We take advantage of another version of the DADS data, the DADS Postes, which is available since 1993 and covers the universe of all private sector employees and the firms in which they work. We construct a panel of firms using data from 1996 to 2008, which enables us to identify firms created from 1997 onwards. We then match this information with the DADS Panel data using each firm's unique identifier.

Identification of executives (*cadres*). The DADS data do not explicitly indicate the complementary pension scheme to which the wage earner is affiliated (either ARRCO for non-executives or AGIRC for executives). We proxy each worker's status by relying on the information provided by the employer on the employee's occupation. In most instances, identification is straightforward, as the occupation label explicitly mentions the title "executive" (*cadre*). However, classification becomes more uncertain for certain occupation categories that could be interpreted either within or outside the executive group. To mitigate classification errors, we exclude from the sample workers with such ambiguous occupations. Additionally, apprentices and interns, who are not subject to regular payroll taxes, are also excluded from the sample.

The 35-hour week reform. The DADS data lack specific information on the precise date when firms transitioned to the 35-hour week in the early 2000s, a factor influencing eligibility for certain payroll taxes reductions. To estimate this date, we rely on the reported working hours for each firm. By calculating the share of the firm's employees declared as working 35 or 39 hours, we determine the highest share and assign each firm to the corresponding weekly working hours regime.

Validation of the labor cost simulation. To validate our labor cost simulation, we compared our simulated gross earnings (*zh*) with the exact variable used as the tax base for employee payroll taxes. This variable, introduced in the data in 1996, is available for

later years but does not cover all our reforms. For this comparison, we used the 2017 data from the DADS panel, analyzing the true gross earnings measure[18] against the gross earnings simulated with TAXIPP. The difference between the logarithm of the simulated earnings and the logarithm of true earnings is centered close to zero, with most deviations falling within a 2% margin of error.

Validating the accuracy of our simulation of employer payroll taxes (and, therefore, labor cost) is somewhat more intricate, as the linked employer-employee data never include them. However, we can assess the alignment of firm-level aggregates with what firms declare in their corporate income tax (CIT) returns (BIC-IS data source, DGFiP). To perform this comparison, we applied the TAXIPP micro-simulation model to the exhaustive linked employer-employee data (DADS Postes) for the year 2017. From the linked employer-employee data, we retain private sector employers and derive firm-level aggregates for gross earnings (exact) and labor costs (simulated), with employer payroll taxes calculated as the difference between these aggregates. In the CIT data, the exact wage bill and employer payroll taxes of each firm are recorded.

We select firms in the CIT data that are subject to the normal CIT regime, and whose fiscal exercise runs between the 1st of January and the 31st of December. While there are potential reasons for differences in total gross wages between the CIT data and the DADS data, such as the treatment of profit-sharing and amounts distributed in the context of incentive plans, we can still compare employer payroll taxes when total gross earnings align between both sources. This allows us to evaluate the accuracy of our simulation.

To ensure a meaningful comparison, we narrow our focus to firms with total gross earnings differing by less than 5 percentage points in the DADS and CIT data. The correlation between data sources for employer payroll taxes is 98%, with a rank-correlation also at 98%. To account for potential differences due to temporary workers from staffing agencies (not present in the DADS data), we further refine the sample to include only main positions (*emplois non annexes*) and repeat the comparison. Tn this refined sample the correlation increases to 99.4%, while the rank-correlation remains at 98%.

[18]The corresponding variable is `BASCSG` in the DADS data.

D Meta-Analysis of Payroll Tax Incidence

Table D1 – Meta-Analysis: Regression Estimates

Estimates:	Dep. var.: estimated pass-through of payroll tax to workers			
	This paper	This paper + literature review		
	(1)	(2)	(3)	(4)
Panel A. Payroll taxes statutorily incident on employers				
Constant	0.253	0.070**	0.076**	−0.085
	(0.131)	(0.028)	(0.032)	(0.095)
Tax-benefit linkage				
None	ref.	ref.	ref.	ref.
Strong	0.760**	0.851***	0.738***	1.002***
	(0.132)	(0.100)	(0.228)	(0.101)
Uncertain		0.647***	0.514**	0.240
		(0.062)	(0.233)	(0.155)
Controls				
Time period FEs			Yes	Yes
Region FEs			Yes	Yes
Unit of observation				Yes
Macroeconomic conditions				Yes
Labor market conditions				Yes
N	5	26	26	26
Panel B. Payroll taxes and income taxes statutorily incident on employees				
Constant	0.887	0.850***	0.858***	
	(.)	(0.076)	(0.055)	
Tax-benefit linkage				
None	ref.	ref.	ref.	
Strong	0.119	0.156*	−0.164	
	(.)	(0.076)	(0.269)	
Controls				
Time period FEs			Yes	
Region FEs			Yes	
N	3	8	8	

Notes: This table reports the results of meta-regression analyses of the literature on payroll tax incidence. Column 1 uses only the pass-through estimates reported in this study, while columns 2–4 additionally include estimates found in the literature and listed in Appendix Table D2. The models are linear regressions where the dependent variable is the study-specific pass-through estimate, and the observations are weighted by the inverse of the estimate's variance. The control variables include time period fixed effects, referring to the decade in which the analyzed period begins (1970s, 1980s, 1990s, 2000s, 2010s), region fixed effects (U.S., Europe, Rest of the world), the unit of observation of the analysis (worker, firm, market level), the country's macroeconomic conditions (GDP growth and inflation rate) and its labor market conditions (unemployment rate, OECD indicator of strictness of employment protection legislation, union density, rate of collective bargaining coverage, and whether wage negotiations take place at the firm level). The control variables are computed as means over the post-reform years considered in each study, and all control variables are mean-centered. Robust standard errors are shown in parentheses. In column 4, an arbitrary value is assigned to missing values for the OECD indicators of strictness of employment protection legislation and union density, and dummy variables are included to control for missing values. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Sources: The studies used in the meta-analysis are described in Appendix Table D2. The control variables are sourced from publicly available data. GDP growth and inflation rates are obtained from the World Bank database, while inflation rates for Argentina come from the Argentinean Central Bank. Data on the strictness of employment protection legislation, collective bargaining rates, firm-level wage negotiation, and union density are drawn from the OECD database. Unemployment rates are obtained from the OECD for Japan, Sweden, the U.K., and the U.S.; the World Bank for Argentina, Colombia, Finland, Greece, and Singapore; and INSEE for France.

Table D2 – Summary of Empirical Studies of Payroll Tax Incidence using Micro Data

Study	Country	Payroll Tax/Reform	Tax-benefit linkage?	Method	Unit of observation	Estimated rate of pass-through to workers
Panel A. Payroll taxes statutorily incident on employers						
Gruber and Krueger (1991)	U.S.	Expansion of workers' compensation insurance program between 1979 and 1988	Strong	DD: variation in workers' compensation rates across states and over time	Worker	0.865 (0.184) (Table 5, column 7)
Gruber (1994)	U.S.	State and federal mandated maternity benefits between 1975 and 1979	Strong	DDD: variation in cost of mandate between different groups of workers across states and over time	Worker	1.56 (0.80) (Table 5, column (iii))
Gruber (1997)	Chile	Reduction in employer payroll taxes after privatization of Social Security system in 1981	Strong	DDD: variation in payroll tax rates between white-collar and blue-collar workers across firms over time	Firm	1.022 (0.180) (Table 3, column 1)
Anderson and Meyer (1997)	U.S.	Unemployment insurance between 1978 and 1984	Uncertain	DD: variation in unemployment insurance tax rates across firms over time	Firm	0.715 (0.292) (Table 3, column 1)
Anderson and Meyer (2000)	U.S.	Adoption of experience-rated unemployment insurance by Washington State in 1985	Uncertain	DD: variation in unemployment insurance tax rates across firms over time	Worker	1.427 (1.191) (Table 3, column 1)
Komamura and Yamada (2004)	Japan	Introduction of mandatory long-term care insurance in 2000	Yes (health insurance) No (long-term care)	DD: variation in employers' contribution rates to health insurance and long-term care insurance over time	Firm	Health insurance: 1.20 (0.2) Long-term care insurance: 0.2 (0.2) (Tables 1 and 2, FE model)
Baicker and Chandra (2006)	U.S.	Employer-provided health insurance between 1996 and 2002	Strong	IV: malpractice payments used as instrumental variable for health premiums	Worker	Covered workers: 1.00 (0.20) Non-covered workers: −0.15 (0.30) (based on Table 4, columns 1 and 3)[a]
Murphy (2007)	U.S.	Unemployment insurance between 1992 and 2002	Uncertain	DD: variation in unemployment insurance tax rates across firms over time	Worker	0.23 (1.01) (Table 6, Panel A, column 3)
Bennmarker, Mellander and Öckert (2009)	Sweden	Payroll tax reduction in Northern Sweden in 2002	None	DD: reform-induced variation in payroll taxes between target and control regions and over time	Firm	0.23 (0.08) (Table 4, column 3)
Korkeamäki and Uusitalo (2009)	Finland	Payroll tax reduction in Northern Finland between 2003 and 2005	None	DD: reform-induced variation in payroll taxes between target and control regions and over time	Firm	0.49 (0.24) (based on Table 7, column 2)[b]
Kugler and Kugler (2009)	Columbia	Sharp increase in payroll taxes for pensions and health following the 1993 Social Security reform	None	DD: variation in payroll tax rates across firms over time	Firm	0.2346 (0.0883) (Table 3, column 1)
Cruces, Galiani and Kidyba (2010)	Argentina	Reform mandating new fully funded pension system in 1993	Strong	DD: variation in payroll taxes across geographical areas and over time	Area-level aggregates	0.501 (0.192) (Table 4, column 2)
Saez, Matsaganis and Tsakloglou (2012)	Greece	Cohort-based payroll tax increase in 1992	None	RDD: based on date of entry in the labor force	Worker	0.295 (0.182) (based on Table 5, column 1)[c]
Lehmann, Marical and Rioux (2013)	France	Reduction of payroll tax around the minimum wage between 2003 and 2006	None	IV: regression using predicted change in tax rates	Worker	0.134 (0.260) (based on Table 2, column 3)[d]

(Continued on next page)

Table D2 – Summary of Empirical Studies of Payroll Tax Incidence using Micro Data (Continued)

Study	Country	Payroll Tax/Reform	Tax-benefit linkage?	Method	Unit of observation	Estimated rate of pass-through to workers
Panel A. Payroll taxes statutorily incident on employers (continued)						
Egebark and Kaunitz (2018)	Sweden	Payroll tax reduction for young workers (aged 26 or less) in 2009	None	DD: variation in payroll tax rates between age groups and over time	Worker	0.073 (0.022) (based on Table 6, column 3)[e]
Adam, Phillips and Roantree (2019)	U.K.	Reforms of National Insurance Contributions between 1982 and 2015	None	IV regression using predicted change in tax rates	Worker	−0.009 (0.109) (based on Table 3, column 6)[f]
Saez, Schoefer and Seim (2019)	Sweden	Payroll tax reduction for young workers (aged 26 or less) between 2007 and 2009	None	DD: variation in payroll tax rates between age groups and over time	Worker	Medium-run: −0.085 (0.046) (based on Table 1, Panel A)[g]
Benzarti and Harju (2021)	Finland	Different payroll tax rates across firms before 2010	None	RDD based on capital depreciation threshold	Firm	0.237 (0.427) (based on Table 2, column 1)[h]
Kim, Kim and Koh (2022)	Singapore	Payroll tax reduction when workers turn 60	Strong	RDD: variation in payroll tax rates at age 60	Worker	Medium-run: 0.825 (0.35) (based on p. 11 and Table 4)[i]
Panel B. Payroll and income taxes statutorily incident on employees						
Bingley and Lanot (2002)	Denmark	Variation in income tax rates between municipalities during the period 1980–1991	None	IV: variation in local payroll taxes across workers in the same firm	Worker	0.643 (0.0592) (based on Table 2, column 5)[j]
Kubik (2004)	U.S.	Tax Reform Act of 1986	None	DD: compare wages of workers before and after 1986 in occupations affected vs. not affected	Occupation-year	0.898 (0.037) (based on Table 4, column 3)[k]
Saez, Matsaganis and Tsakloglou (2012)	Greece	Cohort-based payroll tax increase in 1992	None	RDD based on date of entry in the labor force	Worker	1.382 (0.267) (based on Table 5, column 1)[l]
Lehmann, Marical and Rioux (2013)	France	Reduction of payroll tax around the minimum wage between 2003 and 2006	None	IV: regression using predicted change in tax rates	Worker	0.560 (0.277) (based on Table 2, column 3)[m]
Adam, Phillips and Roantree (2019)	U.K.	Reforms of National Insurance Contributions between 1982 and 2015	None	IV regression using predicted change in tax rates	Worker	1.253 (0.1414) (based on Table 3, column 6)[n]

Notes: DD: differences-in-differences. DDD: triple differences. IV: instrumental variables. RDD: regression discontinuity design. Standard errors of point estimates are shown in parentheses. [a] The pass-through estimate in Baicker and Chandra (2006) is computed using the fact that health insurance premiums are about 20% of wage and salary income at the mean. [b] The pass-through estimate in Korkeamäki and Uusitalo (2009) is computed using the fact that the Finnish payroll tax experiment reduced payroll taxes by 4.1% on average. [c] The pass-through estimate for employer payroll taxes in Saez et al. (2012) is computed as (minus) the ratio between the estimated discontinuity in the log of posted earnings w and the estimated discontinuity in $\log(1+\tau_R)$, where τ_R denotes the employer marginal payroll tax rate, and the standard error of the estimated pass-through rate is computed using the delta method. [d] The pass-through estimate for employer payroll taxes in Lehmann et al. (2013) is computed as one minus the estimated pass-through rates to employers (absolute value of coefficient β_ρ^P). [e] The pass-through estimate in Egebark and Kaunitz (2018) is computed using the fact that employer payroll tax rate was reduced from 32.42% to 15.52%. [f] The pass-through estimate in Adam et al. (2019) is computed as one minus the estimated pass-through rates to employers (absolute value of coefficient $\beta_{X,\rho}^{R,0}$). [g] The pass-through estimate in Saez et al. (2019) is computed as one minus the estimated pass-through rate to employers. [h] The pass-through estimate in Benzarti and Harju (2021) is computed using the fact that the employer payroll tax rate increases from approximately 22% to 24.6% at the capital depreciation threshold. [i] The pass-through estimate in Kim et al. (2022) is computed assuming the first stage is perfectly predictive. [j] The pass-through estimate in Bingley and Lanot (2002) is computed as one minus the estimated pass-through rates to employers (0.3570). [k] The pass-through estimate in Kubik (2004) is computed using the fact that the median marginal tax rate was 20.92% (Table 1, column 1). [l] The pass-through estimate for employee payroll taxes in Saez et al. (2012) is computed as the ratio between the estimated discontinuity in the log of net earnings c and the estimated discontinuity in $\log(1-\tau_R)$, where τ_E denotes the employee marginal payroll tax rate, and the standard error of the estimated pass-through rate is computed using the delta method. [m] The pass-through estimate for income taxes in Lehmann et al. (2013) is computed as one minus the estimated pass-through rates to employers (absolute value of coefficient β_ρ^I). [n] The pass-through estimate in Adam et al. (2019) is computed as one minus the estimated pass-through rates to employers (minus the value of coefficient $\beta_{X,\rho}^{E,0}$).

E Robustness Checks

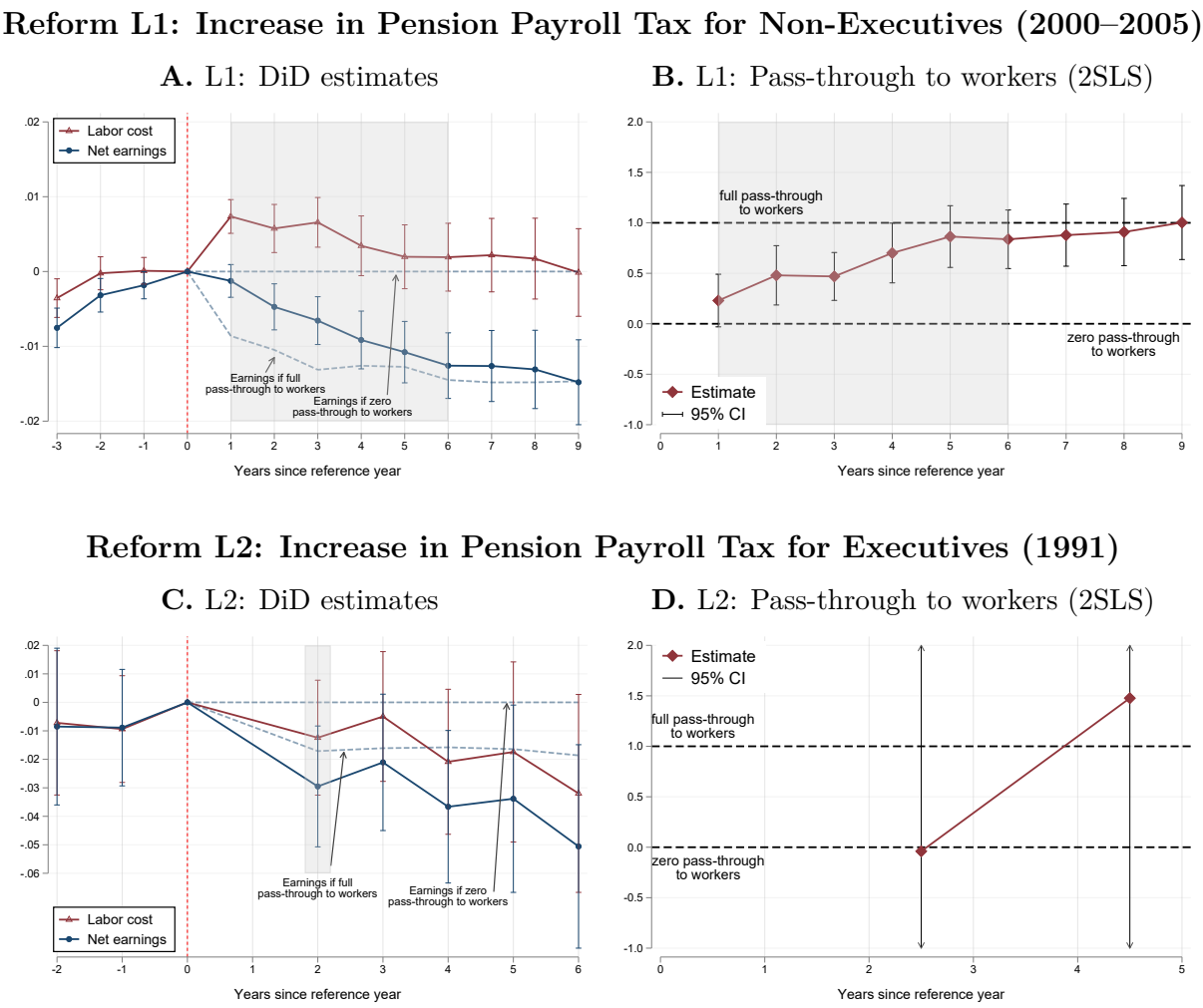
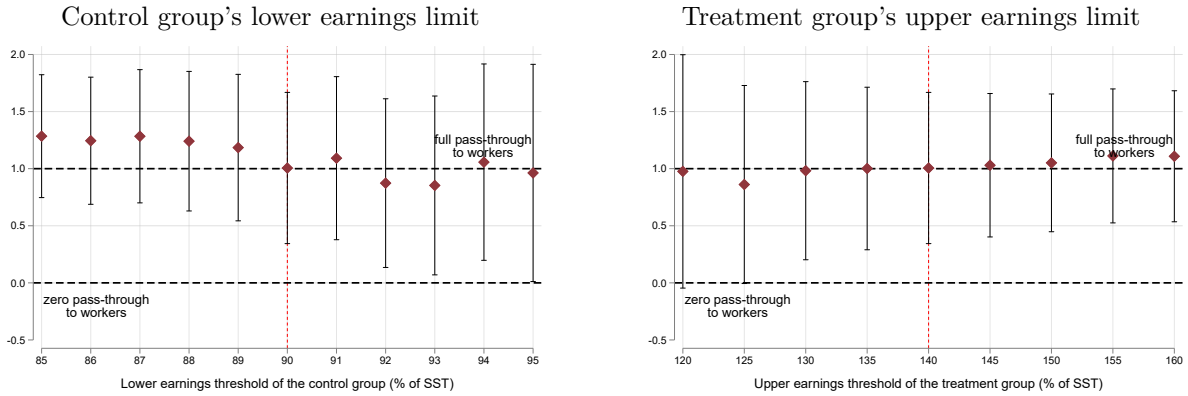


Figure E1 – Estimated Pass-Through to Workers for Payroll Tax Reforms with Linkage (Net Earnings)

Notes: Panels A and C show the estimated log differences in average real net earnings (circle markers) and average real labor costs (triangle markers) between two groups of workers that were affected differently by reforms L1 and L2, while Panels B and D show the estimated pass-through of the payroll tax increases to workers. For Reform L1 (Panels A and B), the treatment (control) group consists of workers whose posted earnings in the reference year (1999) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform L2 (Panels C and D), the treatment (control) group consists of workers whose posted earnings in the reference year (1989) were between 4 and 5.5 (2.5 and 4) times the SST that year, using a balanced panel of workers. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year. IIn Panels A and C, the markers represent the differential growth in net earnings and labor costs between treated and control workers for both pre-reform and post-reform years, relative to the reference year. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level. The blue dashed lines represent the predicted evolution of net earnings under the counterfactual scenarios of zero pass-through and full pass-through of the payroll tax change to workers. Panels B and D present estimates of pass-through to workers, obtained from a 2SLS estimation of Equation (6) in the main text, where $\log(1+\tau)$ (τ: sum of employer and employee payroll taxes rates measured relative to net earnings) is instrumented by the interaction between the treatment group and year dummies. Diamond markers show the estimates while the vertical T-bars show the 95% confidence intervals (arrows indicate confidence intervals that extend beyond the y-axis range displayed). In the case of Reform L2, given the limited sample size, we aggregate years 2 and 3, as well as years 4 and 5.
Sources: DADS Panel; TAXIPP 0.4.

A. Reform L1: Increase in Pension Payroll Taxes for Non-Executives



B. Reform L2: Increase in Pension Payroll Taxes for Executives

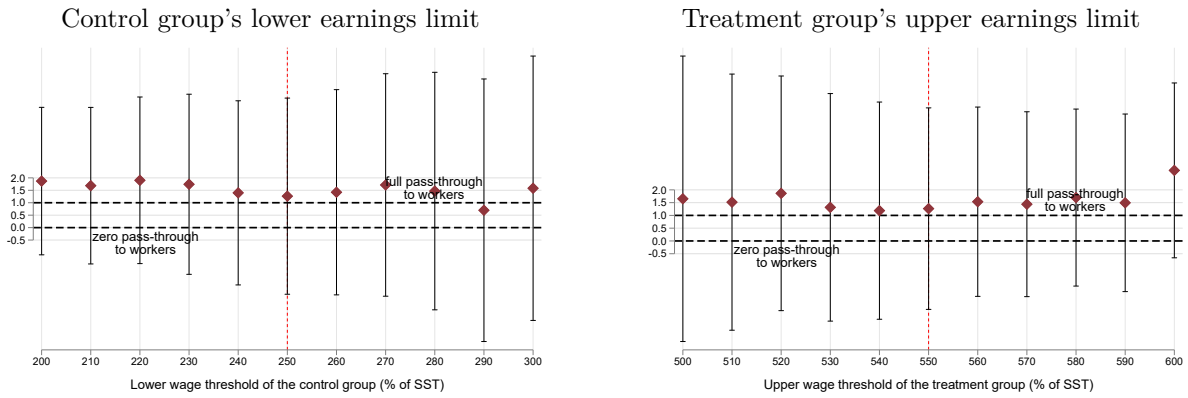
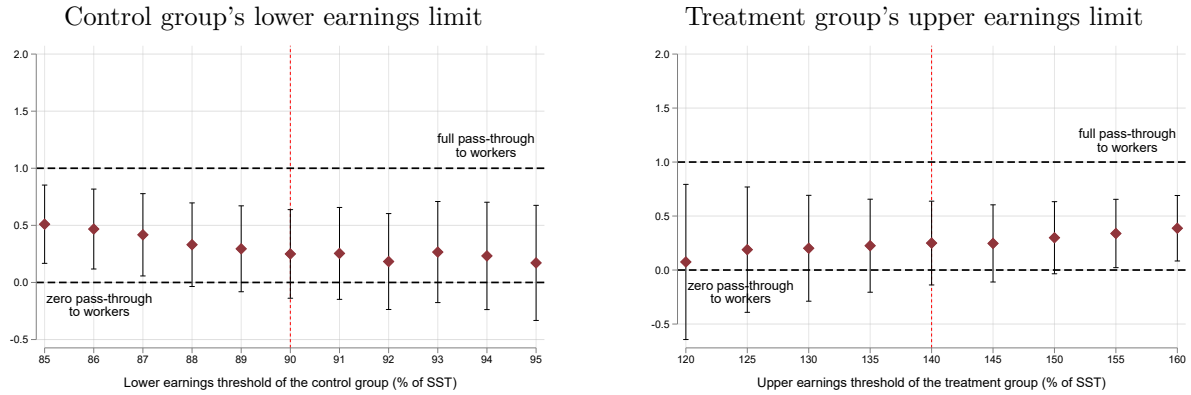


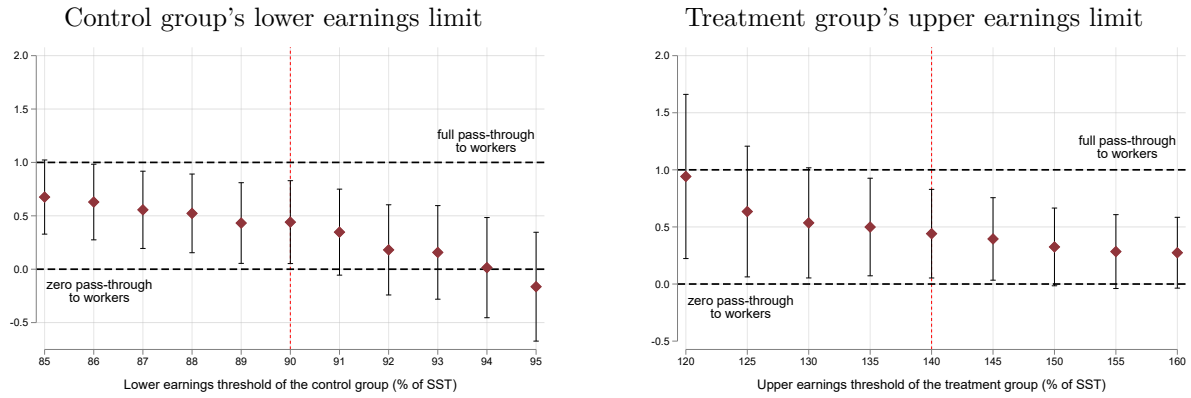
Figure E2 – Estimated Pass-Through to Workers for Reforms with Linkage: Sensitivity to Lower and Upper Earnings Limits of the Treatment and Control Groups

Notes: The pass-through estimates shown in this figure are evaluated in the last year of the estimation sample for each reform, and are based on a specification that does not include worker-specific linear time trends. The left panels show the estimates obtained for various choices of the control group's lower earnings limit (expressed in percentage of the Social Security threshold or SST) while keeping fixed the upper limit of the treatment group. The right panels show the estimates when varying the treatment group's upper earnings limit (expressed in percentage of the Social Security threshold) while keeping fixed the lower limit of the control group. The red vertical dotted lines show the limit that are used in the baseline specification. The markers represent the estimated pass-through of employer payroll taxes to workers. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level.
Sources: DADS Panel; TAXIPP 0.4.

A. Reform NL1: Uncapping of Family Payroll Tax



B. Reform NL2: Uncapping of Health Care Payroll Tax



C. Reform NL3: Employer Tax Credit on Payroll Taxes

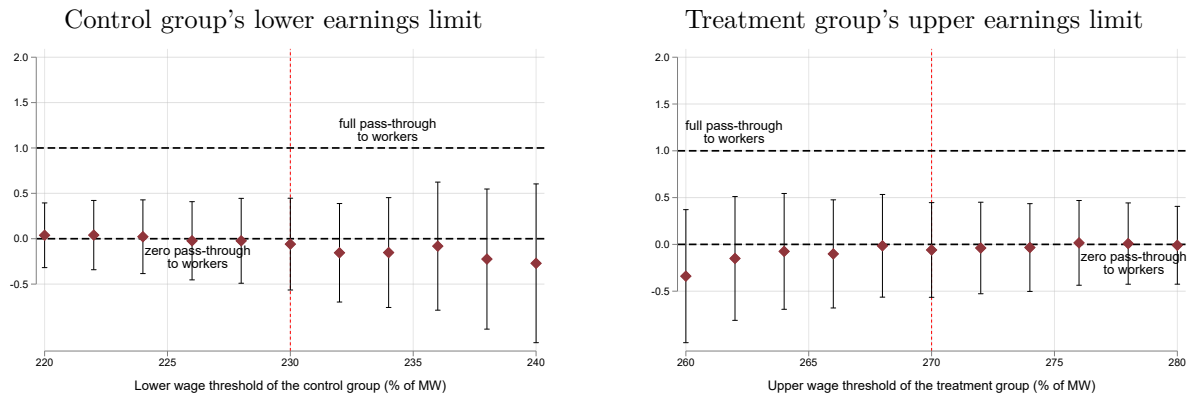
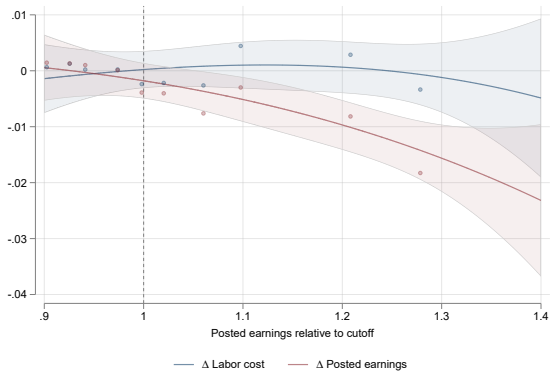


Figure E3 – Estimated Pass-Through to Workers for Reforms without Linkage: Sensitivity to the Lower and Upper Earnings Limits of the Treatment and Control Groups

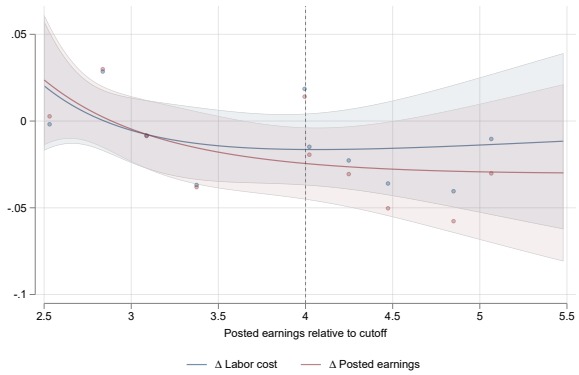
Notes: The pass-through estimates shown in this figure are evaluated in the last year of the estimation sample for each reform, and are based on a specification that does not include worker-specific linear time trends. The left panels show the estimates obtained for various choices of the control group's lower earnings limit (expressed in percentage of the Social Security threshold, or the minimum wage in the case of NL3) while keeping fixed the upper limit of the treatment group. The right panels show the estimates when varying the treatment group's upper earnings limit (expressed in percentage of the Social Security threshold, or the minimum wage in the case of NL1) while keeping fixed the lower limit of the control group. The red vertical dotted lines show the limit that are used in the baseline specification. The markers represent the estimated pass-through of employer payroll taxes to workers. The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level.
Sources: DADS Panel; TAXIPP 0.4.

Payroll Tax Reforms with Linkage

A. Reform L1

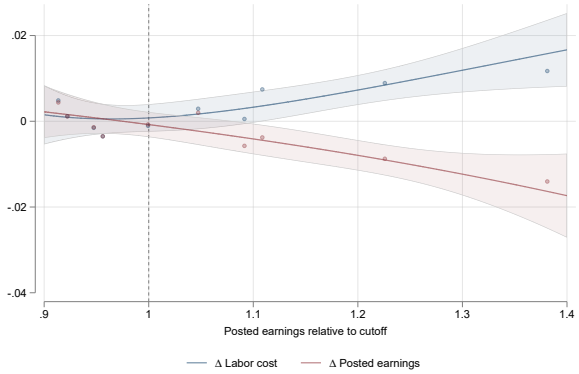


B. Reform L2

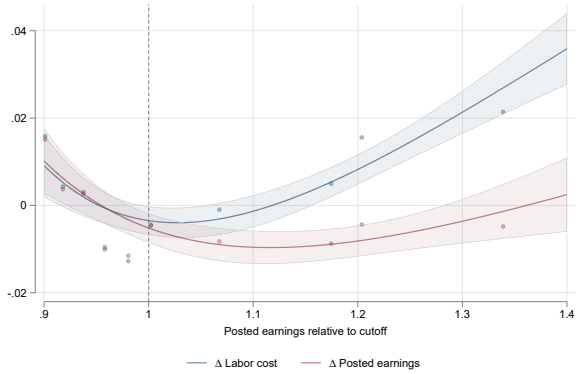


Payroll Tax Reforms without Linkage

C. Reform NL1



D. Reform NL2



Tax Reform without Linkage

E. Reform NL3

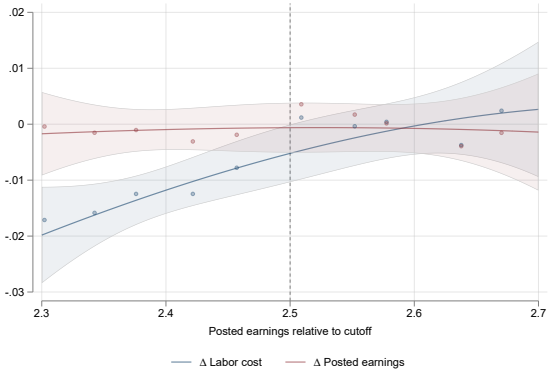


Figure E4 – Non-Parametric Visualization of Treatment Effects

Notes: Each figure compares the growth rate (demeaned) of posted earnings (red) and labor costs (blue) depending on workers' posted earnings in the reference year. The y-axis shows the log-differences in posted earnings and labor cost between the last year in the estimation window and the reference year, averaged by bins of posted earnings (relative to the reform's earnings threshold) in the reference year (x-axis). The vertical dotted gray bars indicate the earnings threshold separating the treatment group from the control group in the reference year. Circles represent binned averages, with a total of 5 bins on each side of the threshold. Panels A and B show the results for reforms with tax-benefit linkage (L1 and L2, respectively), while Panels C to E show the results for reforms without linkage (NL1, NL2, and NL3, respectively). Lines are polynomial fits, shaded areas represent 95% confidence intervals.
Sources: DADS Panel; TAXIPP 0.4.

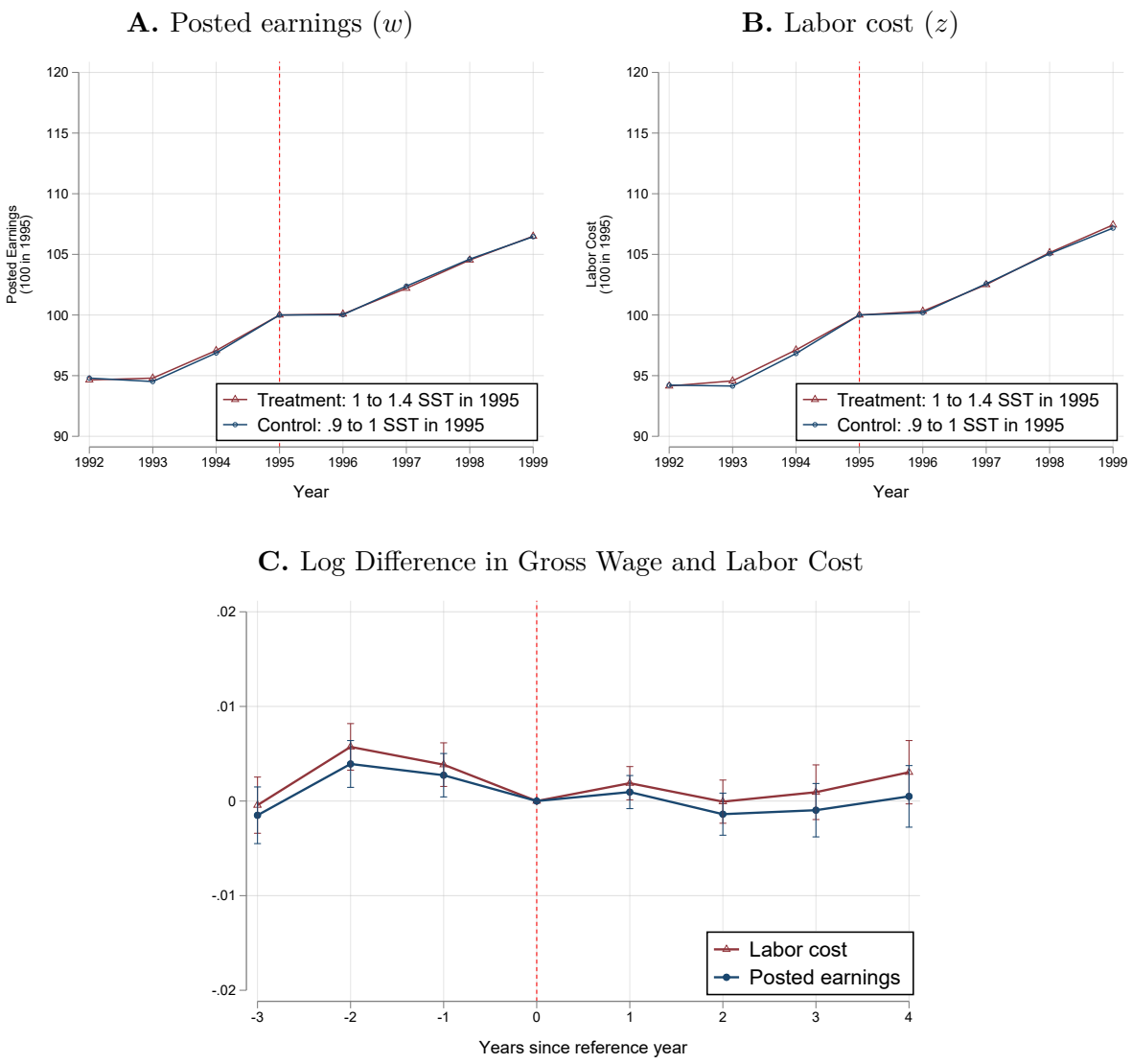


Figure E5 – Placebo Reform (1996): Graphical Evidence on Earnings Responses

Notes: The top panels of the figure show the evolution of average real gross earnings (Panel A) and of average real labor cost (Panel B) between 1992 and 1999 for two groups of workers around a placebo reform which is set to take place in 1996 at the posted earnings cutoff of 1 SST. The difference-in-differences estimation is performed on an unbalanced panel of workers who are observed in the last pre-reform year (denoted by a red vertical dotted line) and at least another year. Earnings levels are normalized to 100 for both groups in the placebo reference year (1995). The treatment group includes workers whose gross earnings in 1995 were 1 to 1.4 times the SST that year. The control group includes individuals whose gross earnings in 1995 were 0.9 to 1 times the SST that year. Panel C shows the estimated log differences between the average real gross earnings (circle markers) and the average real labor cost (triangle markers) of the two groups of workers. The markers represent the parameter estimates on the interaction between the treatment group and year dummies, which is normalized to zero in the placebo reference year (1995). The vertical T-bars show the 95% confidence intervals around the point estimates, with standard errors clustered at the individual level.
Sources: DADS Panel; TAXIPP 0.4.

Table E1 – Incidence of Payroll and Income Tax Reforms: Alternative Specifications

	Reforms with linkage		Reforms without linkage		
	Payroll tax reforms				Income tax reform
Reform:	L1 Increase in pension payroll tax for non-executives	L2 Increase in pension payroll tax for executives	NL1 Uncapping of family payroll tax	NL2 Uncapping of health care payroll tax	NL3 Employer tax credit on payroll taxes
Reference year (t_0):	1999	1989	1988	1980	2012
Final year ($t_{\max}$):	2008	1995	1997	1988	2018
	(1)	(2)	(3)	(4)	(5)
Panel A. Baseline specification					
Without worker-specific trends					
Last year	1.01***	1.26	0.25	0.44**	−0.06
	(0.34)	(2.01)	(0.20)	(0.20)	(0.26)
Final years	0.88***	0.74	−0.02	0.41***	−0.05
	(0.28)	(1.46)	(0.18)	(0.13)	(0.14)
With worker-specific trends					
Last year	1.27***		0.07	0.23	0.06
	(0.34)		(0.25)	(0.15)	(0.44)
Final years	1.21***		0.00	0.24*	0.02
	(0.31)		(0.24)	(0.13)	(0.26)
N	496,039	13,240	594,632	642,289	275,307
Panel B. Removing workers close to threshold ("donut hole")					
Without worker-specific trends					
Last year	0.97***	−0.48	0.34*	0.82***	0.04
	(0.33)	(1.96)	(0.21)	(0.21)	(0.25)
Final years	0.83***	0.12	−0.05	0.62***	0.05
	(0.27)	(1.44)	(0.19)	(0.14)	(0.14)
With worker-specific trends					
Last year	1.47***		0.22	0.26	0.30
	(0.35)		(0.27)	(0.17)	(0.43)
Final years	1.35***		0.11	0.26*	0.20
	(0.32)		(0.25)	(0.15)	(0.26)
N	342,423	11,944	419,719	458,947	208,839

Notes: See notes of Table 3 and Table 4 in the main text. The different columns correspond to different payroll tax or income tax reforms. The pass-through estimates reported in Panel A are obtained from the baseline specification (Equation 6) and the augmented specification that controls for worker-specific trends (Equation 7) using the same samples as in Tables 3 and 4. The estimates reported in Panel B are obtained using the same specifications as in Panel A but removing workers with earnings close to the earnings threshold in the reference year. We exclude workers with posted earnings between 0.95 and 1.05 times the SST in the reference year for reforms L1, NL2, NL3, workers between 2.25 and 2.75 times the minimum wage MW for Reform NL1, and workers between 3.75 and 4.25 times the SST for Reform L2. This restriction is not applicable to NL4 as treatment is not defined based on an earnings threshold for this reform. Standard errors clustered at the individual level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Sources: DADS Panel; TAXIPP 0.4.

F Effects on Employment Outcomes

One way in which adjustments might have occurred in response to employer payroll tax changes that were not passed through to workers is through a reduction in the employment prospects of treated workers relative to control ones.

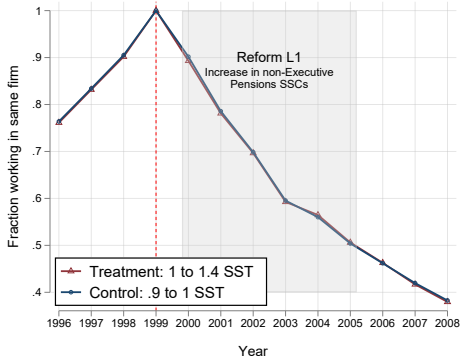
Our identification strategy, which defines groups based on posted earnings in the reference year, implies that both treated an control workers are all employed in the reference year. Differences in the baseline employment probabilities between the two groups of workers will, therefore, induce differential pre-trends when analyzing the evolution of their employment probabilities before and after the reforms. To address this issue, we adopt an approach similar to Saez et al. (2019) by using propensity score reweighting to correct for differential trends in employment probabilities between the treatment and control groups. To compute these propensity weights, we use information on age, gender, *département* of residence, 2-digit occupation, the number of years the worker has been employed pre-reform, 2-digit industry code (which varies across reforms), and firm size. We then use these propensity scores to reweight workers in the graphical and regression analyses.

We focus on two employment outcomes. Our main outcome is the probability of remaining employed in the same firm as in the reference year. If workers in the treatment group, whose labor cost has increased relative to the control group, have experienced a reduction in their employment probabilities, it should be be reflected in the differential retention rates between the two groups of workers. Second, we examine the probability of having a non-zero wage in our data, which corresponds to the probability of being employed in the private sector in a given year.

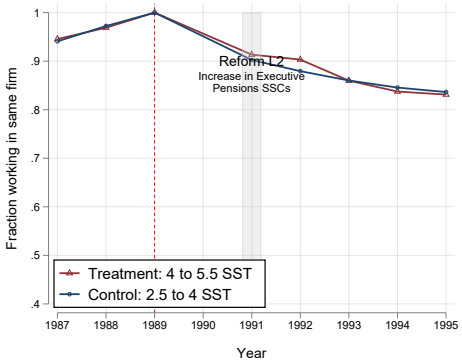
The evolution of the employment probabilities for both treated and control workers around the reform years are shown in Figure F1 (probability of being employed in the same firm as in the reference year) and Figure F2 (probability of being employed in the private sector). The corresponding difference-in-difference estimates are reported in Table F1. The estimates in Panel A are obtained from a baseline specification similar to Equation (5) in the main text, while the estimates in Panel B additionally control for worker-specific trends.

Payroll Tax Reforms with Linkage

A. L1: Increase in pension payroll tax for non-executives

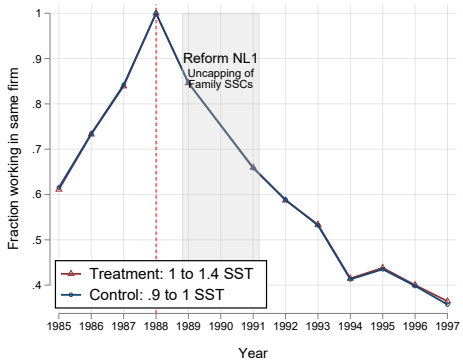


B. L2: Increase in pension payroll tax for executives

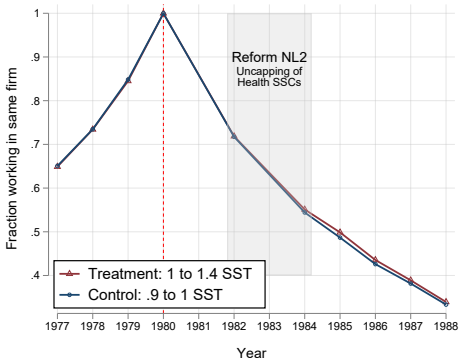


Payroll Tax Reforms without Linkage

C. NL1: Increase in family payroll tax



D. NL2: Increase in health care payroll tax



Income Tax Reform without Linkage

E. NL3: Employer tax credit on payroll taxes

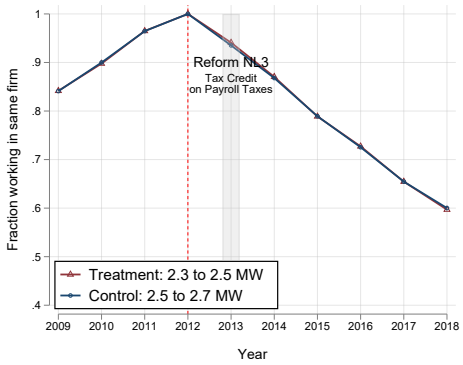
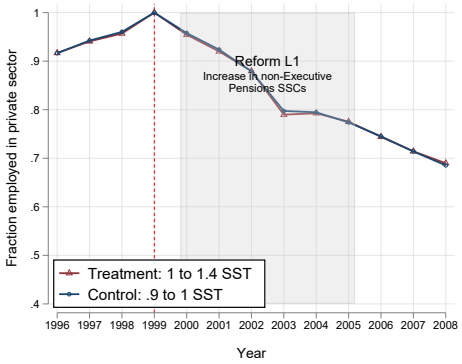


Figure F1 – Probability of Being Employed in Same Firm as in the Reference Year

Notes: For each reform, the figure shows the evolution of the probability to be employed in the same firm as in the reference year for the treated and control workers separately. For the definitions of the treatment and control groups, see notes of Tables 3 and 4. In each panel, the gray shaded area indicates the reform's implementation period, while the red vertical dotted line marks the reference year. Observations are reweighted using inverse propensity score weights based on worker characteristics (age, gender, *département* of residence, two-digit occupation, and number of years employed pre-reform) and firm characteristics (two-digit industry code and firm size) in the reference year (see methodology described in Appendix F).
Sources: DADS Panel; TAXIPP 0.4.

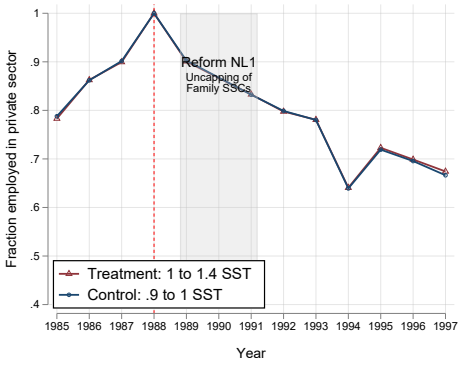
Payroll Tax Reform with Linkage

A. L1: Increase in pension payroll tax for non-executives

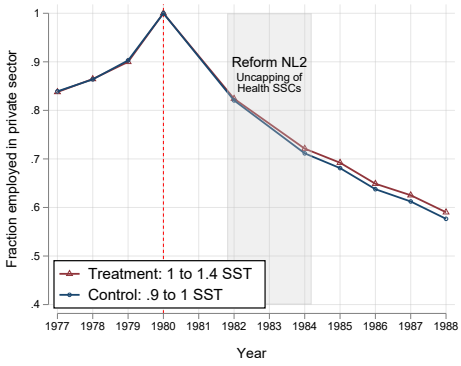


Payroll Tax Reforms without Linkage

B. NL1: Increase in family payroll tax



C. NL2: Increase in health care payroll tax



Income Tax Reform without Linkage

D. NL3: Employer tax credit on payroll taxes

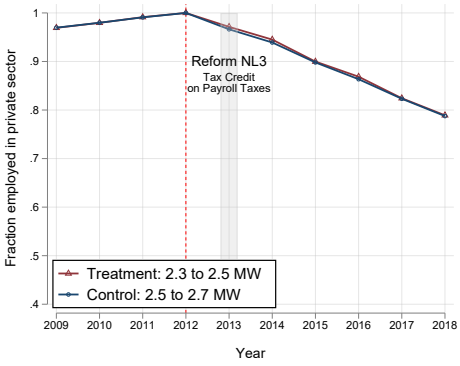


Figure F2 – Probability of Being Employed in the Private Sector

Notes: For each reform, the figure shows the evolution of the probability of being employed in the private sector for the treated and control workers separately. For the definitions of the treatment and control groups, see notes of Tables 3 and 4. Reform L2 is not included since the sample used to analyze this reform is restricted to workers employed every year. The gray shaded area indicates the reform's implementation period, while the red vertical dotted lines mark the reference years. Observations are reweighted using inverse propensity score weights based on worker characteristics (age, gender, *département* of residence, 2-digit occupation, and number of years employed pre-reform) and firm characteristics (2-digit industry code and firm size) in the reference year (see methododology described in Appendix F). Estimates for Reform L2 are not provided because the sample utilized for analyzing this reform is restricted to workers observed every year in the data.

Table F1 – Employment Outcomes: Difference-in-Differences Estimates

	Reforms with linkage		Reforms without linkage		
	Payroll tax reforms				Income tax reform
Reform:	L1 Increase in pension payroll tax for non-executives	L2 Increase in pension payroll tax for executives	NL1 Uncapping of family payroll tax	NL2 Uncapping of health care payroll tax	NL3 Employer tax credit on payroll taxes
Reference year (t_0):	1999	1989	1988	1980	2012
Final year ($t_{\max}$):	2008	1995	1997	1988	2018
	(1)	(2)	(3)	(4)	(5)
Panel A. Baseline specification					
Dep. var.: Probability of remaining employed in the same firm					
Last year	−0.0031	−0.0056	0.0073*	0.0059*	−0.0032
	(0.0047)	(0.0147)	(0.0041)	(0.0034)	(0.0055)
Dep. var.: Probability of remaining employed in the private sector					
Last year	0.0044		0.0079**	0.0136***	0.0015
	(0.0044)		(0.0040)	(0.0035)	(0.0046)
Panel B. Controlling for worker-specific trends					
Dep. var.: Probability of remaining employed in the same firm					
Last year	−0.0085	−0.0034	−0.0027	0.0050	−0.0035
	(0.0146)	(0.0091)	(0.0145)	(0.0111)	(0.0114)
Dep. var.: Probability of remaining employed in the private sector					
Last year	0.0085		−0.0029	0.0142	0.0003
	(0.0101)		(0.0122)	(0.0089)	(0.0066)
N	691,366	13,088	846,048	930,940	338,120

Notes: The different columns correspond to different payroll tax or income tax reforms. The coefficients reported in each column are difference-in-differences estimates of the effects of the reform on two employment outcomes: (i) the probability of remaining employed in the same firm as in the reference year and (ii) the probability of remaining employed in the private sector. For Reform L1 (column 1), the treatment (control) group consists of workers whose posted earnings in the reference year (1999) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform L2 (column 2), the treatment (control) group consists of workers whose posted earnings in the reference year (1989) were between 4 and 5.5 (2.5 and 4) times the SST that year, using a balanced panel of workers. For Reforms NL1 and NL2 (columns 3 and 4), the treatment (control) group consists of workers whose posted earnings in the reference year (1988 for NL1 and 1980 for NL2) were between 1 and 1.4 (0.9 and 1) times the SST that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. For Reform NL3 (column 5), the treatment (control) group consists of workers whose posted earnings in the reference year (2012) were between 2.3 and 2.5 (2.5 and 2.7) times the minimum wage (MW) that year, using an unbalanced panel of workers who are observed in the reference year and at least another year. The estimates in Panel A are obtained from a baseline specification similar to Equation (5) in the main text. The estimates in Panel B additionally control for worker-specific trends. Observations are reweighted using inverse propensity score weights based on worker characteristics (age, gender, *département* of residence, 2-digit occupation, and number of years employed pre-reform) and firm characteristics (2-digit industry code and firm size) in the reference year (see methodology described in Appendix F). Estimates for the probability of being employed in the private sector are not reported for Reform L2 because the sample being used to analyze this reform is restricted to workers observed every year in the data. Standard errors clustered at the individual level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Sources: DADS Panel; TAXIPP 0.4.