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THE EFFECTS OF A SHORTER WORKWEEK ON EMPLOYMENT, OUTPUT, AND PRODUCTIVITY

Pauline Carry
Claire Montialoux
Elio Nimier-David
Alexandra Roulet
Nina Roussille

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ABSTRACT

Do workweek reductions create jobs and raise productivity, as intended by policymakers? In 2000, France cut the standard workweek from 39 to 35 hours at constant monthly pay and paired the reduction with employer payroll tax cuts. We combine matched employer–employee, balance-sheet, and survey data to study the effects on workers, firms, and local labor markets. We first exploit the staggered implementation by firm size to identify the impact of the reform package. We then use differential exposure to the hours cut and tax cut to separate their effects. The reform package left employment unchanged. This zero masks large offsetting effects: absent the tax cuts, the shorter workweek would have reduced employment by 4%. The effects are skill-biased, raising employment among managers and professionals while reducing it among other employees. Finally, shorter hours raised total factor productivity by 2%, limiting the output decline to 1% despite a 6% fall in total hours. Surveys point to work intensification and organizational changes as potential drivers of productivity gains.

Pauline Carry
Princeton University
pauline.carry@princeton.edu

Alexandra Roulet
INSEAD
alexandra.roulet@insead.edu

Claire Montialoux
Sciences Po
Department of Economics
and CNRS
claire.montialoux@sciencespo.fr

Nina Roussille
Massachusetts Institute of Technology
Department of Economics
and NBER
nroussil@mit.edu

Elio Nimier-David
Cornell University
elio.nimier.david@cornell.edu

1 Introduction

> *"We shall endeavour to spread the bread thin on the butter—to make what work there is still to be done to be as widely shared as possible. Three-hour shifts or a fifteen-hour week may put off the problem for a great while."*
>
> —John Maynard Keynes, *Economic Possibilities for our Grandchildren* (1930)

Proposals to shorten the workweek have resurfaced at every major change in the labor market. Reducing working hours was advocated as a response to the Great Depression in the United States and during the European unemployment crisis of the 1990s. More recently, the expansion of remote work has renewed interest in shorter workweeks, while advances in artificial intelligence have prompted new policy proposals.[1] These policies have important aggregate implications: they contribute to substantial differences in hours worked across countries (Gethin & Saez, 2025).

France's 35-hour workweek reform has become a reference point in international debates over shorter working hours.[2] In 2000, France reduced the standard workweek from 39 to 35 hours at constant monthly pay and compensated firms with payroll tax cuts. More than two decades later, the reform remains contested, and a marker of the left-right political divide.[3] The debate centers on the effects of shorter working hours on employment, output, and productivity. It persists for two reasons.

First, the economic effects of shorter hours are theoretically ambiguous. Whether shorter workweeks increase employment depends on firms' response to higher labor costs and potential productivity gains. Preserving monthly pay raises hourly wages, while hiring additional workers entails fixed costs. Firms may therefore reduce production rather than replace lost hours. Productivity gains from reduced fatigue or organizational changes may limit the decline in output, but also reduce the need for additional workers. These competing forces make employment effects ambiguous and raise questions about whether firms should be compensated. In Spain, a 2025 bill to shorten the workweek was blocked in parliament partly because it offered no employer compensation, while in Italy, a 2026 proposal offering subsidies failed amid objections to their cost.

Second, empirical evidence remains limited and yields conflicting conclusions. Existing studies of the 35-hour reform rely on variation that captures only part of the policy. Chemin and Wasmer (2009) use two public holidays in Alsace-Moselle as a source of variation, equivalent to about 9% of the reduction in working hours under the reform, while Crépon, Leclair, and Roux (2004) study the minority

[1] OpenAI's *Industrial Policy for the Intelligence Age* (2026) calls for trials of a four-day workweek without pay cuts. In September 2026, Senator Sanders and Representative Takano reintroduced the Thirty-Two Hour Workweek Act, motivated in part by the prospect of sharing AI-driven productivity gains.

[2] In Spain, both sides of the debate over the 2025 bill to cut the legal workweek invoked the French reform: supporters cited it as a successful precedent, while critics pointed to the political divisions it had left in France. In the United States, Senator Bernie Sanders' Thirty-Two Hour Workweek Act (2024) explicitly invoked France's 35-hour workweek (prepared remarks, Senate HELP Committee hearing, March 14, 2024).

[3] Most presidential campaigns since then have featured proposals either to lengthen the legal workweek or to shorten it further. In the 2016 primary of the right, Juppé proposed raising it to 39 hours, while Fillon proposed abolishing it altogether, in favor of firm-level bargaining. Ahead of the 2027 election, Retailleau proposed raising working time, while a Senate bill filed in August 2026 offers employers payroll tax cuts for moving to a 32-hour workweek.

of firms that adopted the 35-hour workweek voluntarily before the mandate took effect. The former find no employment effects, while the latter find positive effects.[4] Evidence from other contexts is also mixed. Pioneering this literature, Hunt (1999) finds no employment gains at the industry level following reductions in standard hours in 1980s Germany. Crépon and Kramarz (2002) document job losses among incumbent workers after France's 1982 reduction from 40 to 39 hours, while Fishback, Vickers, and Ziebarth (2024) find an increase in manufacturing employment following workweek reductions during the Great Depression in the US.[5]

We provide causal evidence on the effects of the French 35-hour reform on workers, firms, and local labor markets. We make three contributions. First, we develop a comprehensive analysis of how firms adjust their labor inputs, examining not only overall employment but also total hours, the distribution of working hours within firms, and workforce composition across skill groups. Second, we estimate the effects of shorter hours on labor productivity, total factor productivity, and output. Third, we disentangle the effects of shorter hours and accompanying subsidies to assess how fiscal policies can offset the costs of workweek reductions.

Enacted in 1998, the reform had two components. The first was a mandate to cut the standard workweek from 39 to 35 hours while preserving monthly pay. It was phased in by firm size: firms with more than 20 employees had to comply by January 2000, and smaller firms by January 2002. The second was a cut in employer payroll taxes for firms that adopted the 35-hour workweek. The cut targeted low-wage jobs: it was worth 11% of the wage for a job paying 1.3 times the minimum wage, but only 3% at 1.7 times. The 35-hour reform is informative for three reasons. The shock is large: a four-hour cut, or about 10% of the standard workweek. The staggered mandate by size generates variation for identification. Finally, although the bundling of the hours cut and the tax cut has long been seen as an obstacle to evaluation, we show that it allows us to study both the effects of cutting hours and the role of fiscal subsidies in offsetting their costs for firms.

We develop an econometric framework to disentangle the effects of the workweek reduction and the payroll tax cut. Our analysis proceeds in three steps. First, we exploit the staggered implementation by firm size in a difference-in-differences design, comparing firms above the 20-employee threshold (mandated firms) with those below it (non-mandated firms), based on pre-reform size. Because payroll tax cuts were available from 1998 to any firm adopting the 35-hour workweek, this identifies the effect of the mandate relative to fiscal incentives alone. Second, we instrument adoption of the 35-hour

[4] Estevão and Sá (2006) also find no employment effect using the Labor Force Survey, but their estimates of employment and worker flows rely largely on over-time comparisons with controls. Kramarz et al. (2008) extend the analysis of Crépon, Leclair, and Roux (2004) to non-adopters but acknowledge that their estimates are not causal. Finally, Goux, Maurin, and Petrongolo (2014) exploit this reform to study a different question: they use it as a shock to one partner's working hours to examine how individuals adjust their own labor supply in response. See Askenazy (2013) for a review.

[5] Using industry-by-country data on several European reforms, Batut, Garnero, and Tondini (2023) point to negative employment effects. Evidence on Portugal's 1996 reduction is mixed: Raposo and van Ours (2010) find lower job separation rates for full-time workers, while Asai, Lopes, and Tondini (2024) find employment losses among firms that had resisted earlier reductions. Skuterud (2007) studies a workweek reduction in Quebec that did not require pay preservation and finds no employment gains. Germain (2025) documents positive effects of the 1910 nine-hour day in Belgian coal mines.

workweek with the size-based mandate to estimate the effect of operating under the 35-hour regime, including access to payroll tax cuts, relative to remaining at 39 hours without subsidies. Third, we separate the effect of reducing hours from that of the tax cut by exploiting two additional measures of exposure: the pre-reform share of full-time jobs, which determines how strongly the hours reduction binds, and the pre-reform wage distribution, which determines the size of the subsidy. This allows us to decompose both the effects of the mandate and those of adopting the 35-hour workweek into the two reform components.

The paper combines administrative and survey data. Matched employer–employee registers provide information on hours, earnings, employment, and skill composition for the universe of private-sector jobs. We use balance-sheet data to measure value added and inputs and to estimate total factor productivity. We rely on administrative records to identify firms claiming the reform's payroll tax cuts. Labor force surveys allow us to compare employer- and worker-reported hours and document how firms implemented the reduction across occupations. Finally, we use additional surveys to examine firm reorganization and work intensification following the reform.

We first report the effects of the mandate on working time and wages. Relative to the fiscal incentive alone, the mandate had a large effect on hours worked: by 2001, the average workweek in mandated firms had fallen by two hours relative to non-mandated firms, averaging over all jobs in the firm including part-time ones. Hourly wages rose by 1.7%, less than the mechanical increase implied by a two-hour reduction at constant pay, for two reasons. First, the average includes part-time workers and new hires. For full-time incumbents, wages rose by 4%. Second, firms engaged in wage moderation, slowing nominal wage growth between the reform's announcement in 1998 and its enforcement in 2000. Finally, the payroll tax cuts absorbed 60% of the wage increase, such that hourly labor costs rose by 0.7% in 2001.

Turning to employment, the reform package did not generate work-sharing. Headcount in mandated and non-mandated firms followed virtually identical paths before and after the reform, with point estimates close to zero. Adapting the approach of Cengiz et al. (2019), we estimate employment effects by weekly-hours bin. Jobs above 37 hours fall sharply and, as expected, jobs at 35 to 37 hours increase. But this increase falls short of the decline, and the remainder shows up below 35 hours: part-time employment rises, so that the adjustment extends beyond the jobs directly targeted by the statutory reduction. Incumbents already working 35 hours or less saw some reduction in weekly hours and some increase in hourly pay. Firm-level workweek agreements suggest that fairness toward part-time workers contributed to this response, echoing spillovers documented using wage changes (Dube, Giuliano, & Leonard, 2019; Giupponi & Machin, 2022; Hazell et al., 2022). Combining these margins, we find that the mandate reduced total hours worked in the firm by 6%. Our finding of no work-sharing is consistent with evidence from other working-time regulations: firms do not reallocate hours in response to restrictions on part-time work in France (Carry, 2026), do not hire to avoid overtime costs in the US (Quach, 2024), and do not adjust employment following an expansion of paid vacation in Germany

(Koenig & Quach, 2026).

Our policy decomposition reveals that the zero employment effect of the reform package masks two large, offsetting forces. The workweek reduction mandate lowers employment by 4.1%, while the payroll tax cut has the opposite effect, fully offsetting employment losses. Nonparametric evidence corroborates this: among firms in the top 30% of exposure to the workweek reduction and the bottom 30% of exposure to the tax cut, the effect of the mandate on employment is −3.9%. The tax cut prevents employment losses by offsetting the rise in labor costs: we find no pass-through to wages.[6] The gross fiscal cost per job saved is 60% of average annual earnings, in line with other French payroll tax reforms (Cahuc, Carcillo, & Le Barbanchon, 2019; Kramarz & Philippon, 2001) and similar to the unemployment insurance replacement rate.[7] Conditional on the workweek reduction taking place, the subsidy is therefore close to fiscally neutral. The package as a whole is not: because it reduces total hours and output without raising employment, it entails a substantial fiscal cost.

Our decomposition helps reconcile the mixed employment estimates in earlier studies. Although these studies focus on the reform package, they exploit variation that places different weights on the workweek reduction and the payroll tax cuts. Chemin and Wasmer (2009) find no significant employment effect of the reform package, consistent with our finding that the two components offset each other. In contrast, Crépon, Leclair, and Roux (2004) estimate positive effects using tax-cut exposure to instrument adoption among early voluntary adopters. This is also consistent with our decomposition, as their instrument primarily captures the subsidy component. More broadly, our results suggest that differences in the institutions that govern labor costs may help explain the mixed findings across different workweek reduction reforms.

Beyond overall employment effects, we find that the impact of the reform is skill-biased. For managers and high-skill professionals, the reform operates largely as intended: average weekly hours per worker fall, but employment rises enough to leave total hours at the firm unchanged. For lower-skill workers, both employment and total hours decline. Among those who remain employed, earnings fall by 1.7% for lower-skill workers, while we detect no significant reduction for high-skill workers. These patterns reflect differences across workers rather than occupational relabeling and arise primarily within firms, through changes in skill composition, as opposed to differential effects across firms.[8]

We extend our analysis to the commuting-zone (CZ) level to capture employment in all firms, including entry and exit, as well as spillovers across firms. We compare CZs with different pre-reform exposure, measured by the share of employment in full-time jobs at firms with more than 20 employees. Employment falls by 5% in a CZ with average exposure relative to one with zero exposure. This

[6]This result is consistent with the tax cut's explicit purpose and with the tax change leaving worker benefits unaffected (Bozio et al., 2025). It also aligns with Benzarti and Harju (2021), who find that payroll tax cuts are most effective at preserving employment when firms face adverse shocks.

[7]Evidence from other countries shows significant employment effects of payroll taxes (Saez, Schoefer, & Seim, 2019; 2021; Guo, 2024; Lobel, 2026).

[8]Transitions across the two occupation groups are exceedingly rare, and the decline in lower-skill employment and the increase in high-skill employment affect different populations in terms of age and gender.

negative effect is consistent with our firm-level decomposition: relative to the firm-level mandate, the CZ design loads more heavily on the workweek reduction than on the tax cut. The CZ results also confirm the skill-biased employment effects.

The reform package reduced value added by 1%, compared with an estimated 1.6% decline absent the subsidy. This contraction is much smaller than the 6% decline in total hours, implying a 4.3% increase in value added per hour. To net out the response of other inputs, we estimate firm-level total factor productivity (TFP) using the control-function approach of Ackerberg, Caves, and Frazer (2015). A standard specification with total hours as the labor input yields a 3.5% increase in TFP by 2001. Because the reform changed firms' skill composition, we distinguish hours worked by high- and lower-skill workers, which reduces the estimate to 2%. We show that productivity gains are not explained by changes in within-skill worker composition, measured by pre-reform worker fixed effects. The gains persist with a flexible production function separating employment from hours per worker, ruling out decreasing returns to long hours as an explanation.

Evidence from four surveys points to changes in work organization as the driver of productivity gains. Before the reform, workers report substantial slack on the job and employers report scope to raise output without additional labor. After the reduction in working time, 42% of workers who remain employed report doing a similar amount of work in less time, and this intensification appears partly firm-led: workers report task reassignment and new work processes. This echoes evidence on remote work showing that changes in work organization can boost productivity (Barrero, Bloom, & Davis, 2023). High-skill workers are more likely to report such changes, consistent with evidence that organizational change is skill-biased (Caroli & Van Reenen, 2001). This greater complementarity with reorganization may explain why employment losses are concentrated among lower-skill workers.

Overall, 30% of the decline in output is borne by shareholders and 70% by workers through the wage bill, a loss that falls entirely on lower-skill workers. Welfare effects for these workers are likely heterogeneous: they depend on how they valued an hour of leisure relative to their wage before the reform. Workers who were on their labor supply curve or preferred to work more hours at the prevailing wage are likely to experience welfare losses on average. High-skill workers, by contrast, experience reduced hours, higher employment, and preserved earnings, suggesting welfare gains.

Finally, the effects of the reform up to 2003 are consistent with our baseline estimates. To estimate these longer-run effects, we exploit the fact that mandate status continues to predict firms' full-time workweeks even after all firms were required to comply. We also show that our results are not driven by mean reversion and are robust across alternative samples.

The paper proceeds as follows. Section 2 describes the reform, Section 3 presents the data and descriptive evidence, and Section 4 introduces our econometric framework and empirical strategies. Section 5 presents the effects of the reform package, Section 6 disentangles the effects of the workweek reduction from those of the tax cut, and Section 7 reports local-level results. Section 8 discusses longer-run effects and mechanisms, and Section 9 concludes.

2 Institutional Background

2.1 Outline of the Reform, Context and Timing

France's first attempt to implement the 35-hour workweek relied on incentives rather than a mandate. The *Loi Robien*, enacted in June 1996 and named after its sponsor, center-right MP Gilles de Robien, offered firms reductions in social security contributions if they cut working hours by roughly 10%, conditional on either hiring additional workers or avoiding layoffs. Take-up was very limited: only 2,953 agreements had been signed by June 1998 (Fiole & Roger, 2002). Fiscal incentives alone thus proved insufficient to induce a widespread reduction in working time.

The Jospin government, elected in June 1997 at the head of a left-wing coalition, turned to a mandatory reduction in hours. On October 10, 1997, the Prime Minister announced a reduction of the statutory workweek from 39 to 35 hours. Implementation was staggered by firm size: firms with more than 20 employees would become subject to the 35-hour statutory workweek in January 2000, smaller firms in January 2002.[9] Although shorter working hours had already been debated, the firm-size threshold and implementation deadlines had not previously been announced. The resulting "Aubry I" law, named after Labor Minister Martine Aubry, was adopted by the National Assembly in May 1998 and enacted on June 13, 1998. In addition to establishing mandatory deadlines, the law introduced financial incentives for firms to reduce working time before they became subject to the mandate. In France, social security is primarily financed through payroll taxes on labor income. These consist of employee contributions, withheld from workers' gross earnings, and employer contributions, paid by firms on top of gross wages.[10] Firms could receive reductions in employer social security contributions if they voluntarily implemented a pay-preserving reduction in working time and met employment-creation or employment-preservation conditions. The second law, "Aubry II," promulgated on January 19, 2000, extended these subsidies to all firms adopting the 35-hour workweek and relaxed the employment conditions.

The Aubry reforms rested on a dual rationale. First, the laws were advanced as economic policy tools for work-sharing, aimed at reducing the then-high unemployment rate (10.6% in 1997) by redistributing labor among more workers. The government projected that the shift to a 35-hour workweek would generate several hundred thousand jobs, as firms would hire additional workers to maintain output levels under reduced individual hours. The government further argued that the reform would largely "pay for itself": higher employment would reduce unemployment-benefit expenditures and raise social-contribution revenues, while productivity gains from firms reorganizing production would help offset the cost of the payroll tax reductions. Second, they embodied a normative argument of social progress, seeking to improve quality of life, facilitate work–life balance, and restore dignity to work. Martine

[9] We detail the legal firm size measure as described in the law as well as the measure we rely on for our analysis in Section 4.2.3.

[10] This split is statutory: it determines who remits the contribution, not who bears it. Employer contributions may be passed through to workers in the form of lower gross wages.

Aubry herself underscored this dimension with the maxim "*travailler moins, c'est vivre mieux*" ("work less to live better").[11]

Following the 2002 presidential and legislative elections, the newly elected right-wing government softened the Aubry reforms. In 2003, payroll tax reductions previously reserved for firms adopting 35-hour agreements were extended to all firms, including those that had retained the 39-hour workweek.

The Aubry reforms sparked a sharply polarized public debate. Trade unions largely supported the policy, while employer associations warned that statutory hours reductions would raise labor costs, weaken competitiveness, and constrain firms' ability to adjust production.[12] Even years after the reform, academics and policymakers remain divided on its effects. Reflecting on the reform's legacy, Askenazy (2021) notes that the Aubry laws remain "at the centre of [public] debate", and a "persistent and fundamental marker of the political divide between the left and right in France".

Finally, statutory working time has been the subject of policy debate well beyond France, with many countries introducing reforms to working-hour regulations. Table A.1 situates the French reform among workweek reductions implemented elsewhere. The requirement to reduce the full-time workweek while preserving pay is a common feature of these reforms. In contrast, the pairing with payroll tax cuts is largely specific to France, with Belgium as the one other case. We exploit this feature to assess the role of accompanying fiscal policies (Section 4).

2.2 Workweek Reduction

When a firm adopted the 35-hour workweek, either early and voluntarily or following the statutory change at the deadline, full-time employees moved from a 39-hour to a 35-hour contractual schedule. For incumbent workers, the reduction in hours was to occur without a corresponding reduction in monthly earnings: while the reduction in working time itself could be imposed, a reduction in contractual pay required the employee's consent. Maintaining monthly earnings therefore implied an approximately 11.4% increase in hourly compensation. Preservation of monthly earnings was commonly referred to as "35 heures payées 39" (35 hours paid as 39) by the government. Newly hired employees were also subject to the 35-hour statutory workweek and had to receive the same pay as incumbents performing equal or similar work. For minimum-wage workers, this principle was reinforced by the statutory *Garanties Mensuelles de Rémunération* (GMR), which prevented monthly pay falls.[13]

[11] This phrase was later parodied by right-wing President Nicolas Sarkozy during his 2007 presidential campaign. In contrast to the work-sharing rationale of the 35-hour workweek, Sarkozy promoted a pro-overtime agenda with the slogan "*travailler plus pour gagner plus*" ("work more to earn more").

[12] In February 1998, leaders of the principal French and Italian employers' federations signed a joint declaration condemning the proposal to legislate a 35-hour week. They stated: "The law, by imposing an additional cost on enterprises, destroys all possibility of creating new jobs and of fostering enterprises' competitiveness […] it also destroys all possibility of social dialogue and negotiation […] and limits the necessary variety of possible solutions which could be adopted".

[13] The guarantee took the form of a supplement topping up the hourly minimum wage so that monthly earnings matched the level prevailing before the reduction from 39 to 35 hours. It applied to full-time workers and, on a pro-rated basis, to part-time workers whose hours were reduced. Because the applicable GMR depended on the date at which the firm moved to 35 hours and was subsequently revalued, several regimes coexisted: GMR1 for firms reducing hours between June 1998 and June 1999, GMR2 between July 1999 and June 2000, GMR3 between July 2000 and June 2001, and GMR4 between

The 35-hour reform did not impose a hard cap on weekly working hours.[14] Instead, it defined a new threshold for overtime, lowering it from 39 to 35 hours. Workers could still work beyond 35 hours, but those hours were treated as overtime and paid at a premium. From January to December 2000, in firms with more than 20 employees, hours worked from the 36^{th} to the 39^{th} were subject to a 10% premium, hours between the 40^{th} and the 43^{rd} to a 25% premium, and hours beyond the 43^{rd} to a 50% premium. Starting in January 2001, the premium on hours worked from the 36^{th} to the 39^{th} increased to 25% while the rest of the schedule remained unchanged. The statutory annual overtime cap (i.e., the maximum number of hours a worker can legally work beyond the statutory workweek) also stayed constant at 130 hours per year.[15] For firms with 20 employees or fewer, the pre-reform overtime rules remained in effect until 2002.

The 35-hour reform was intended to reduce working time for all employees, including those whose work schedules were not organized around a fixed number of clocked hours each week. For most workers, firms could implement the reduction through shorter workdays (for instance, 45 minutes less each day), a shorter workweek (a half-day every week or a full day every other week), or days off accumulated over the year (*Réduction du Temps de Travail* days, henceforth RTT). For *cadres* (managers and high-skill professionals), whose working time was less often organized around a fixed weekly schedule, the reform instead operated mainly through additional paid time off (RTT days), thereby reducing working time on an annual basis. The reform gave workers and firms substantial flexibility in how to organize the reduction. It could take the form of shorter workdays, shorter workweeks, regular days off, or RTT days allocated over the year. Workers could also use *comptes épargne-temps* (time-saving accounts) to bank accrued time off for later use. The reform also facilitated the annualization of working time, and assessing compliance with working-time limits over a longer reference period. In Section 3.3, we document empirically the distribution of these different arrangements and show that, in practice, the use of annualization and time-saving accounts was relatively uncommon.

While implementing the new workweek did not itself require a collective agreement, firms were encouraged to negotiate with workers over the modalities of implementation. Access to the payroll tax cuts associated with adopting the 35-hour workweek required implementation under a collective agreement. As a result, the conditions of implementation were in practice negotiated in many firms.[16]

July 2001 and June 2002. As of July 2002, GMR1, GMR2, GMR3, and GMR4 stood at €1,100.67, €1,114.35, €1,133.49, and €1,147.52 per month, respectively.

[14]The maximum number of working hours allowed by French law was 48 per week, a limit already in place since 1982.

[15]To allow firms time to adjust to the new 35-hour definition, the Aubry laws introduced a transitional schedule that progressively lowered the weekly threshold at which overtime hours began to count toward this annual cap. From January to December 2000, only hours worked beyond an average of 37 hours per week counted toward the 130-hour annual limit. In other words, employees working 36 or 37 hours still received the mandated overtime premium, but these hours did not count toward the annual cap. Between January and December 2001, this threshold was lowered further, and the cap began applying to hours beyond 36 per week. These new rules applied only to large firms (with more than 20 employees).

[16]For smaller firms without established bargaining structures or worker representatives, the reform allowed a mandated employee (an ordinary employee authorized by a trade union) to negotiate and sign an agreement on behalf of workers.

Enforcement. Compliance was monitored by the labor inspectors, who could enter premises without notice, examine the timekeeping records, and verify on payslips that hours beyond the statutory workweek had been paid as overtime. Unions and employee representatives, parties to most agreements, provided a second line of monitoring. Non-compliance exposed firms to three costs. Unpaid or unrecorded overtime was a criminal offense carrying a fine per employee concerned, and deliberately understating hours on payslips constituted concealed employment (*travail dissimulé*), punishable by up to two years' imprisonment. Workers could claim up to five years of unpaid overtime in labor courts, and were owed damages equal to six months' pay where concealed employment was established. Finally, firms receiving the payroll tax cuts could have the subsidy suspended or withdrawn, and amounts already received recovered.

2.3 Payroll Tax Reduction

This Section describes the main elements needed to understand the payroll tax reductions. The details are described in Appendix A.4.

Aubry I. Under the Aubry I Law of June 13, 1998, firms that voluntarily adopted the 35-hour workweek and signed a collective agreement before the deadline became eligible, upon claiming, for a temporary subsidy. This subsidy was a flat, per-worker, per-year payroll tax deduction, granted for five years from the date the reduction took effect, covering existing and newly hired workers alike. To qualify, most firms were required to reduce the contractual workweek of full-time workers by at least 10% and commit to job creation (typically a 6% increase in employment).[17] Such a firm received 9,000 F per employee the first year (2,103 EUR in 2024 euros, or 5.4% of the average annual full-time wage in 1998), declining by 1,000 F annually thereafter (234 EUR, or 0.6% of the average wage).

Aubry II. Enacted on January 19, 2000, Aubry II granted a permanent subsidy to firms transitioning to the 35-hour workweek.[18] It consisted of two payroll tax deductions: a flat 4,000 francs per employee per year (914 EUR in 2024 euros, or 2.3% of the average annual full-time wage in 2000), and a degressive component targeted at low- and middle-wage workers, worth 17,500 francs per full-time minimum-wage worker (4,000 EUR, or 10.1% of the average wage) and tapering to zero at 1.73 times the minimum wage. Amounts were pro-rated for part-time workers. Firms had to commit to broadly creating or preserving employment but, unlike under Aubry I, no minimum threshold applied.

Figure 1 documents the incremental payroll tax cut introduced by the Aubry II reform. Before the reform, employers benefited from the Juppé subsidy for low-wage workers, which was independent of firm size and working hours. Aubry II replaced this schedule for firms adopting the 35-hour workweek

[17] A smaller subset of firms could argue that they resorted to working time reductions to avoid layoffs or social restructuring. These firms were also eligible for the temporary subsidy even without committing to job creation.

[18] This subsidy was called permanent to contrast with the temporary subsidy under Aubry I. As for the Aubry I subsidy, firms had to claim the subsidy and show proof of a collective agreement.

with a more generous payroll tax reduction. The figure plots the difference between the two schedules—that is, the additional subsidy associated with 35-hour adoption—while Appendix Figure A.1 reports the two schedules separately. Tax cuts are expressed as a share of the worker's full-time-equivalent wage, with wages normalized by the full-time minimum wage. The incremental subsidy peaks at about 11% of wages around 1.3 times the minimum wage, then declines sharply before flattening above 1.73 times the minimum wage. The 1997 wage distribution shows that a substantial share of employment was concentrated near the peak of the subsidy schedule.

3 Data and Descriptive Evidence

3.1 Data

To quantify the effects of the workweek reduction reform, we combine linked employer–employee records with firms' balance-sheet data, worker and employer surveys, and administrative records on workweek-reduction agreements over the period 1994–2003.

Matched Employer–Employee Data. The *Déclarations Annuelles de Données Sociales* (DADS) is a rich matched employer–employee dataset produced by the French national statistical agency (INSEE). It is based on mandatory employer reports used for payroll taxes and social security contributions. Every year, firms have to declare, for each worker-job match, the wage, number of hours worked, and length of the employment spell (in days). The data also include individual demographic characteristics such as age and gender, as well as four-digit industry and occupation codes.

The cross-section version of this dataset (*DADS Postes*) covers the universe of private-sector jobs and firms since 1994. Firms are consistently identified across years, but worker identifiers are reshuffled every two years in the version accessible to researchers. As a complement to the cross-sectional data, we leverage a worker-level panel version (*DADS Panel*). Since 1976, the panel tracks a 1-in-25 random sample composed of individuals born in October of even-numbered years.

Firm-Level Balance-Sheet and Income Statement Data. To study output and productivity, we link the DADS with the FICUS (*Fichier Complet Unifié de SUSE*) database using firms' unique identifiers. The FICUS files provide firm-year balance-sheet and income statement data drawn from mandatory corporate tax filings. This linked dataset allows us to observe detailed inputs, profits, and value added, from which we construct measures of total factor productivity (TFP).

Workweek Reduction Agreements. We use administrative records covering the universe of claims for payroll tax reductions by firms under the reform. The records report the collective agreement required to claim the subsidy (date of signature, date of implementation), and the applicable subsidy regime (Robien, Aubry I, or Aubry II). Figure A.2 shows the cumulative number of employees covered by workweek-reduction agreements between 1996 and 2003. The majority of agreements were

signed between 1999 and 2001, consistent with the phased deadlines and fiscal incentives of the Aubry laws. Merging these data with the matched employer–employee records allows us to compute job-level payroll tax reductions, as explained in Section 2.3 and Appendix A.4.

Labor Force Survey. To supplement the administrative data, we use the French Labor Force Survey (LFS), which draws on a nationally representative sample of approximately 75,000 households per year. The LFS records workers' actual working time, allowing us to verify patterns observed in the administrative data. The 2001 wave also includes a one-off supplement on working time changes for 14,000 workers. This module asks whether a collective agreement to reduce hours was signed in the worker's firm, whether the reform reduced the worker's own hours, and, if so, what form it took. We exploit these data to analyze how the reform was implemented across occupations.

Additional Surveys to Explore the Mechanisms. To provide descriptive evidence on how workers and firms adjusted work organization in response to the reform, we draw on three additional surveys. First, the *Enquête RTT et Modes de Vie*, conducted by the Labor Ministry (DARES) in 1999, was designed specifically to study the effects of working-time reductions and includes questions on changes in work organization and job satisfaction following the reform. The survey was administered to 1,618 full-time workers. Second, we use the 1998 wave of the DARES Working Conditions Survey (*Conditions de Travail*), which provides information on idle time and work intensity before the reform. Third, the INSEE *Enquête de Conjoncture dans l'Industrie* asks manufacturing firms whether they could increase production without increasing labor input. We use this measure to capture the scope for firms to raise output prior to the reform.

3.2 Sample and Variable Definitions

Sample. We restrict the sample to corporations with a valid firm identifier operating in the for-profit sector in mainland France. For the core analysis, we use a balanced panel of firms present between 1994 and 2001. We consider workers aged 18–60. We exclude apprentices, interns, as well as workers employed directly by households or in agriculture, a standard restriction in French data. We also exclude firms with extreme employment growth (i.e., top and bottom 1% over the period), separately for smaller ($\leq$ 20 employees) and larger firms.

Workweek. For each job spell, we compute average weekly hours as total hours worked over the year divided by the number of days employed, multiplied by seven. Hours recorded in the matched employer–employee data are hours that give rise to compensation and therefore include both regular and overtime hours. Because the data are reported annually, we observe average annual weekly hours. The measure also captures reductions implemented through additional days off (*RTT*): employees may retain the same schedule when at work, but hours worked above the reduced workweek are compensated through additional time off later in the year. These hours are therefore not recorded as additional paid

working hours, as the compensation is through time, not pay. As a result, even when implemented through additional days off rather than shorter workdays, the reform lowers the average weekly hours we measure.

Hourly Wage and Payroll Taxes. The hourly wage is measured as gross earnings divided by hours worked over the job spell. The linked employer–employee data report employee- but not employer-paid contributions at the job level. We therefore follow Bozio et al. (2025) and compute employer payroll taxes using *TAXIPP*, the tax simulation model developed by the *Institut des Politiques Publiques* (IPP). We adapt the model to incorporate the Aubry payroll tax reductions (see Appendix A.4), using the Workweek Reduction Agreements data to identify the date at which each firm becomes eligible for a subsidy and the applicable subsidy regime.[19]

We assess the accuracy of these computations in Appendix Figure A.4 by aggregating employer payroll taxes to the firm level and comparing them with the corresponding totals reported in firms' balance-sheet data. As a benchmark for the consistency of the two data sources, Panel (a) plots firm-level log total wage bills in FICUS against their counterparts in DADS for 1997 and 2001. Panel (b) similarly compares log employer payroll taxes reported in FICUS with those obtained from our computations. The close alignment between the two measures indicates that our imputed payroll taxes closely match the firm-level amounts reported in the balance-sheet data.

Skill Groups. We divide workers into two main categories based on their occupation code: (i) Managers and high-skill professionals (i.e., executives, managers, and engineers), and (ii) Other workers (i.e., clerks, blue-collar workers, supervisors, and technicians).

Firm-level Outcomes. For average outcomes, such as the firm-level workweek, we average across all job spells in a given year, weighting each spell by the number of days employed. Firm-level employment and total hours are computed by summing days employed and hours worked, respectively, across all job spells.

3.3 Aggregate Descriptive Evidence on the Adoption of the Reform

Figure 2 plots the distribution of weekly hours for all workers in the linked employer-employee data, separately for the pre-reform year (1997) and post-reform year (2003). In 1997, the distribution exhibits a pronounced mass point at 39 hours, the statutory workweek at the time. By 2003, the dominant mass point is instead at 35 hours, the new statutory workweek, while the concentration at 39 hours is substantially reduced.

Figure A.5 shows the same shift in self-reported hours from the Labor Force Survey: the share of

[19]The model takes as inputs the payroll tax schedule and DADS information on earnings and job spells. At the worker × year level, it computes employer and employee payroll taxes, reductions in employer payroll taxes, the CSG and CRDS, and other payroll contributions.

workers reporting 39 hours falls sharply, and the share reporting 35 hours rises. Studying workers' own reports, rather than firm declarations, confirms that the reduction in hours reflects an actual change in working time experienced by workers, not merely changes in employer reporting practices.

The DARES worker survey on changes induced by the reform supports broad compliance with the intended reduction in working time. 86% of respondents report a reduction in working time at least as large as intended: either their workweek was reduced as intended, or hours above 35 were compensated through overtime pay or additional days off.

Drawing on the 2001 supplement to the Labor Force Survey, Figure 3 documents how the reform was implemented across skill groups, and further confirms the hours shift. Panel (a) reports the share of workers whose employer signed a workweek reduction agreement and, conditional on such an agreement, the share whose own working time was reduced. In 2001, only large firms were required to comply. Managers and high-skill professionals are more likely to work in a firm covered by an agreement (69% versus 50%), consistent with their greater concentration in larger firms, which were already subject to the 35-hour mandate in 2001. Conditional on working in a firm with an agreement, however, the reform reduced the hours of 87% of managers and 93% of other employees.

Panel (b) reports, among workers whose hours were reduced, how the reduction was implemented. For most managers and high-skill professionals, the reduction took the form of additional days off (*RTT*). Among other employees, implementation was more varied, combining shorter workdays (35%), half-days off (23%), and additional full days off (28%). The prevalence of half-days and full days off also made the reduction harder to erode informally, reinforcing compliance. Most respondents report neither an annualization of their work schedule nor the use of time-saving accounts.

The next section provides additional evidence on take-up and relates it to our identification strategies.

4 Econometric Framework

The reform bundles two components: (i) a mandatory reduction in the statutory workweek, phased in earlier for larger firms, and (ii) payroll tax cuts available to any firm that signs a collective agreement to reduce its workweek, whether or not it is subject to the mandate. This section defines the parameters of interest, clarifies their interpretation, and develops the empirical strategies used to identify them. We consider three sets of objects: the intent-to-treat effect of the mandate, the local average treatment effect of operating under the 35-hour regime, and the separate effects of the reform's two components. We also present descriptive evidence illustrating the main sources of variation underlying each empirical strategy. Appendix B provides details and proofs.

4.1 Setup

A firm i is observed in multiple periods $t \in \{0, 1, \dots\}$, with the reform announced and implemented between periods 0 and 1. Two binary treatments describe a firm's reform status. First, $H35_{it} \in \{0, 1\}$

indicates whether firm i operates under the 35-hour regime in period t, either through voluntary adoption or because it is legally required to do so. Second, $TC_{it} \in \{0, 1\}$ indicates that the firm has claimed the payroll tax cut. The institutional context imposes two restrictions: the tax cut does not exist before the reform,

$$TC_{i0} = 0, \tag{1}$$

and it is available only to firms operating under the 35-hour workweek,

$$TC_{it} \leq H35_{it}. \tag{2}$$

A firm may adopt the 35-hour regime voluntarily at any time. The regime makes it eligible for the tax cut from period 1, but claiming remains an endogenous choice of the firm.

The legal mandate requires large firms to adopt the 35-hour regime from period 1 onward. Because actual mandate status depends on firm size measured after the announcement, we instead define a predicted mandate status, $S_i \in \{0, 1\}$, based on pre-announcement firm size. This measure is therefore predetermined and time-invariant. For simplicity, we refer to firms with $S_i = 1$ as mandated and those with $S_i = 0$ as non-mandated.

Notations. We index potential outcomes by the two components of the reform, the 35-hour regime and the payroll tax cut, so that $Y_{it}(h35, tc)$ denotes the potential outcome for firm i at time t under treatment status $(h35, tc)$. Treatment take-up is itself potentially affected by the mandate. We therefore write $H35_{it}(s)$ for potential adoption of the 35-hour regime under predicted mandate status s, and $TC_{it}(h35)$ for potential take-up of the payroll tax cut given 35-hour adoption status $h35$. Following Angrist, Imbens, and Rubin (1996), we use the compact mandate-level notation

$$Y_{it}(s) \equiv Y_{it}\left(H35_{it}(s), TC_{it}\left(H35_{it}(s)\right)\right). \tag{3}$$

4.2 Object 1: The Intent-to-Treat Effect of the Mandate

4.2.1 Definition and Interpretation

The first object of interest is the intent-to-treat (ITT) effect of the mandate on mandated firms:

$$\Delta_t^{ITT} = \mathbb{E}\Big[\underbrace{Y_{it}(1)}_{\substack{\text{Mandate} \\ \text{+ Tax incentive}}} - \underbrace{Y_{it}(0)}_{\substack{\text{Tax incentive only} \\ \text{(Adoption is voluntary)}}} \mid S_i = 1 \Big], \quad t \geq 1. \tag{4}$$

Δ_t^{ITT} captures the effect of legally requiring firms to adopt the 35-hour regime, as proxied by our predetermined status S, relative to offering them a fiscal incentive to do so voluntarily. $Y_{it}(1)$ reflects the firm's potential outcome under the mandate, which depends on 35-hour implementation and, conditional on implementation, whether the firm claims the associated payroll tax cut. The counterfactual

$Y_{it}(0)$ holds the rest of the reform fixed: the payroll tax incentive remains available, and the firm may voluntarily adopt the 35-hour regime and claim the subsidy. Thus, Δ_t^{ITT} answers the following policy question: What is the effect of mandating a reduction in the workweek rather than relying only on a fiscal incentive to induce it? Because some firms may implement the workweek reduction without being required to do so, the ITT captures the policy-relevant effect of imposing the legal requirement under actual take-up behavior.

4.2.2 Identification

For any outcome Y, define the difference-in-differences contrast DiD_t^Y as the period-t-versus-1997 change among mandated firms relative to the corresponding change among non-mandated firms:

$$\mathrm{DiD}_t^Y = \big(\mathbb{E}[Y_{it} \mid S_i = 1] - \mathbb{E}[Y_{i0} \mid S_i = 1]\big) - \big(\mathbb{E}[Y_{it} \mid S_i = 0] - \mathbb{E}[Y_{i0} \mid S_i = 0]\big). \tag{5}$$

DiD_t^Y identifies Δ_t^{ITT} under three assumptions (Appendix B, Proposition B.1): (A1) *no anticipation*, whereby pre-reform outcomes do not respond to the future mandate; (A2) *the Stable Unit Treatment Value Assumption (SUTVA)*, whereby a firm's outcomes depend only on its own mandate status; and (A3) *parallel trends*, whereby, absent the mandate, average outcomes of mandated and non-mandated firms would have evolved in parallel. Importantly, parallel trends is imposed on $Y_{it}(0)$, the potential outcome under no mandate but with the payroll tax incentive in place. Under this counterfactual regime, firms in both groups may voluntarily adopt the 35-hour regime and, upon adoption, claim the associated tax cut. Thus, the non-mandated firms trace out the evolution of outcomes under the fiscal-incentive-only regime that provides the counterfactual for mandated firms.

4.2.3 Empirical Implementation

We estimate the dynamic effects of the mandate using the following event-study specification:

$$y_{it} = \sum_{\tau=1994, \tau\neq 1997}^{2001} \alpha_\tau \times S_i \times \mathbb{1}[t = \tau] + \mu_i + \rho_{rt} + \phi_{ct} + u_{it}, \tag{6}$$

where y_{it} is an outcome for firm (or worker) i in year t, μ_i is a firm (or worker) fixed effect, and ρ_{rt} and ϕ_{ct} are industry-by-year and county-by-year fixed effects, where industry and county are measured in 1997 and held fixed thereafter. Counties correspond to the 96 *départements* of metropolitan France. The coefficients α_τ capture the differential evolution of outcomes for mandated firms relative to non-mandated firms, with 1997 as the omitted year. The pre-reform coefficients provide placebo tests of the parallel-trends assumption. Standard errors are clustered at the firm (or worker) level.

We estimate Equation (6) on a balanced panel over 1994–2001, ending the sample before the mandate takes effect for the non-mandated group in January 2002. Figure C.1 examines differential firm exit between mandated and non-mandated firms and finds no effect by 2001. In Section 8, we provide additional evidence up to 2003.

Mandate Status and Treatment Status. The legal mandate status depends on firm size over the three years preceding January 1, 2000. The law specifies precise criteria based on monthly employment over this period.[20] To construct a predetermined measure of mandate status, we define S_i using firm size in 1997, before firms could adjust employment in response to the reform announcement. Our baseline sample includes firms with 10–48 full-time equivalent (FTE) employees. We exclude firms close to the 20-employee threshold to limit misclassification, and firms with 50 or more employees, which face additional regulations (Garicano, Lelarge, & Van Reenen, 2016; Aghion, Bergeaud, & Van Reenen, 2023; Nimier-David, Sraer, & Thesmar, 2026). We classify firms with 22–48 FTE employees as mandated and those with 10–18 as non-mandated. We assess robustness to alternative size ranges.

We observe payroll tax cut claims (TC) directly in firm administrative records. To measure whether a firm operates under the 35-hour regime ($H35$), we proceed in three steps. First, we set $H35_{it} = 1$ if the modal weekly hours among jobs exceeding 30 hours are below 38, and zero otherwise. This exploits the concentration of working hours within firms to identify the 35-hour regime, including among firms that voluntarily adopt. Second, we use monthly employment over 1997–1999 to identify firms legally required to operate under the 35-hour regime. We set $H35_{it} = 1$ for these firms, hence capturing firms operating under the regime with paid overtime above 35 hours. Unlike S_i, this measure uses post-announcement employment and therefore captures actual legal status rather than predicted status. Third, we verify that firms claiming the payroll tax cut are classified as under the 35-hour regime. For very few cases where this is not the case (0.9% of firms in 2001), we set $H35_{it} = 1$, since claiming the tax cut requires operating under the 35-hour regime. Our ITT estimates and their decomposition into reform components are unaffected by these measurement choices, which matter only for the LATE via the first stage.

Figure C.2 supports our classification choices. Panel (a) shows that the 1997 firm-size distribution is smooth around the threshold, consistent with the absence of manipulation and in line with the reform being announced after this date. Panels (b) and (c) plot the share of firms under 35-hour regime against 1997 firm size in 1997 and 2001. Before the reform, it is flat across firm sizes, with no difference around the threshold. About 20% of firms are classified at 35 hours in 1997 reflecting firms relying on long part-time contracts or with short full-time schedules. By 2001, the share is substantially higher among larger firms. Because S_i predicts rather than exactly measures legal mandate status, adoption does not exhibit a sharp discontinuity at the threshold.

Descriptive Evidence and Take-Up. Table 1 reports 1997 summary statistics for mandated and non-mandated firms. The two groups are similar along most dimensions, including worker age, gender composition, average hours, full-time work, the share of low-wage workers, and occupational composition. The main difference is industry composition: non-mandated firms are slightly less concentrated in manufacturing and more concentrated in construction and services. Our specifications absorb industry-

[20] A firm is above the threshold if it employs more than 20 workers for at least 12 months, consecutive or not, during this period. If employment remains below 20 throughout 1999, the deadline is postponed to January 2002.

by-year fixed effects to account for these differences. The last column reports economy-wide statistics, showing that our sample is also broadly similar to the overall labor market along these dimensions.

Figure 4 illustrates the first source of variation by plotting the workweek distribution separately for mandated and non-mandated firms in each year. Before the reform announcement, the distributions are nearly identical, with a pronounced mass point at 39 hours. Between announcement and implementation, both groups shift modestly toward 35 hours, consistent with anticipatory adoption and the introduction of the tax incentive, while 39 hours remains the dominant mass point. Starting in 2000 and continuing in 2001, mandated firms exhibit a pronounced reallocation toward 35 hours and part-time work. The shift is much smaller among non-mandated firms, some of which nevertheless adopt early. By 2002, these smaller firms also shift toward 35 hours as their mandate takes effect.

To present the effects of the mandate, we report both raw outcome averages for mandated and non-mandated firms and estimates of α_τ. We begin with the two treatments. Figure 5 shows the evolution of the share of firms operating under the 35-hour regime, $H35$, and claiming the payroll tax cut, TC. Between 1994 and 1997, $H35$ is similar and roughly constant in both groups. Once the mandate takes effect, 98% of mandated firms are classified as operating under the 35-hour regime, reflecting the close correspondence between legal and predicted mandate status. Among non-mandated firms, it rises to 35% by 2001, due to voluntary adoption to claim the tax cut and misclassification of mandate status by our size proxy. This relatively limited take-up shows that the fiscal incentive alone was insufficient to induce most firms to adopt the 35-hour regime. Panel (d) shows that, by 2001, mandated firms are 30 percentage points more likely to claim the payroll tax cut. Yet many firms operating under the 35-hour regime do not claim the subsidy, which requires a formal agreement with workers on the terms of implementation, consistent with Kramarz et al. (2008).

Assessing the Identifying Assumptions. First, defining mandate status using 1997 firm size sharply limits the scope for anticipation, since the reform was only announced at the end of that year. Consistent with this, we find no evidence of bunching around 20 employees and little difference in working time before the reform across the two groups.

Second, we test the parallel-trends assumption using the pre-reform event-study coefficients, α_τ, for 1994–1996 relative to 1997. As Section 5 shows, these estimates are generally small and statistically indistinguishable from zero: our main outcomes evolved similarly in mandated and non-mandated firms before the reform. Those results confirm the absence of anticipation. The two groups were also similarly exposed to the new fiscal incentives. They had comparable 1997 full-time shares (Figure C.3, Panel (a)) and comparable predicted exposure to the payroll tax cut based on 1997 wages (Panel (b)). Thus, the two groups faced similar ex ante gains and costs from the reform's two components.[21] In addition, Figure A.3 shows that voluntary adoption under the earlier Robien fiscal incentive, when no mandate had been announced or implemented, was negligible in both groups.

[21] Table 1 further shows that the two groups had the same share of jobs below 1.73 times the minimum wage, where the payroll tax schedule features a kink.

Finally, we provide three pieces of evidence relevant for SUTVA. First, throughout Section 5, we report the raw evolution of outcomes for mandated and non-mandated firms alongside the difference-in-differences estimates, allowing us to verify that the reform is not associated with a discrete break in outcomes for the non-mandated group. Second, Section 7 estimates effects at the commuting-zone (CZ) level, thereby incorporating spillovers across firms within local labor markets; these estimates are consistent with the firm-level results. Third, Appendix D provides complementary worker-level evidence.

4.3 Object 2: The Effect of Operating under the 35-Hour Regime

4.3.1 Definition and Interpretation

Our second object of interest is the local average treatment effect (LATE) of operating under the 35-hour regime:

$$\theta_t^{LATE} = \mathbb{E}\Big[\underbrace{Y_{it}(1, TC_{it}(1))}_{\substack{\text{35h regime} \\ \text{+ Tax claim response}}} - \underbrace{Y_{it}(0,0)}_{\substack{\text{39h regime} \\ \text{+ No tax claim}}} \;\Big|\; \underbrace{S_i = 1}_{\text{Mandated}},\ \underbrace{H35_{it}(1) - H35_{it}(0) = 1}_{\text{Compliers, } C_{it}=1}\Big], \qquad t \geq 1. \tag{7}$$

θ_t^{LATE} captures the effect of the 35-hour regime, including the firm's associated decision to claim the payroll tax cut. It is defined for compliers,

$$C_{it} \equiv \mathbb{1}\{H35_{it}(1) = 1,\ H35_{it}(0) = 0\},$$

that is, firms for which the mandate proxy correctly predicts legal status and that would not have adopted the 35-hour regime under the fiscal incentive alone. For these firms, the counterfactual $Y_{it}(0,0)$ entails both remaining under the 39-hour regime and not claiming the tax cut, since the subsidy is only available under the 35-hour regime (Equation (2)).

For a complier firm, this treatment bundle can be decomposed into a workweek component and a tax-cut component:

$$Y_{it}\big(1, TC_{it}(1)\big) - Y_{it}(0,0) = \underbrace{Y_{it}(1,0) - Y_{it}(0,0)}_{\equiv\, \tau_{it}^{H35}} + TC_{it}(1)\ \underbrace{\big[Y_{it}(1,1) - Y_{it}(1,0)\big]}_{\equiv\, \tau_{it}^{TC}}, \tag{8}$$

and hence

$$\theta_t^{LATE} = \mathbb{E}\big[\tau_{it}^{H35} \mid S_i = 1, C_{it} = 1\big] + s_t^{TC}\, \mathbb{E}\big[\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1, TC_{it}(1) = 1\big], \tag{9}$$

where $s_t^{TC} = \mathbb{P}\big[TC_{it}(1) = 1 \mid S_i = 1, C_{it} = 1\big]$ is the tax-cut take-up rate among compliers. Thus, the LATE captures the effect of the reform package for firms induced to adopt by the mandate: the first term is the effect of the 35-hour regime absent the tax cut, while the second is the additional effect

of the tax cut multiplied by the take-up among compliers. Since $\Delta_t^{ITT} = \pi_t^C \theta_t^{LATE}$, where π_t^C is the complier share among mandated firms (see Appendix B, Lemma B.1(ii)–(iii)), the ITT likewise reflects the combined effect of the two components. Section 4.4 develops a strategy to separate them.

4.3.2 Identification

To identify the LATE, we impose four additional assumptions: (A4) *relevance*—the mandate affects adoption of the 35-hour regime; (A5) *exclusion*—the mandate affects outcomes only through the realized treatment bundle; (A6) *monotonicity*—no firm adopts the 35-hour workweek only when it is *not* mandated; and (A7) *mandate-invariance of tax-cut take-up*—among firms whose adoption status is unaffected by the mandate, the mandate does not alter the decision to claim the tax cut. The last assumption rules out, for example, always-takers claiming the tax cut only when mandated. Under these assumptions, the Wald-DiD ratio identifies the LATE (Appendix B, Proposition B.2):

$$\widehat{\theta}_t^{LATE} = \frac{\mathrm{DiD}_t^{Y}}{\mathrm{DiD}_t^{H35}} = \frac{\widehat{\Delta}_t^{ITT}}{\widehat{\pi}_t^{C}}. \tag{10}$$

Here, DiD_t^{H35} is the difference-in-differences contrast with 35-hour adoption as the outcome. Under no anticipation and parallel trends for adoption, it identifies the average effect of the mandate on adoption; monotonicity then implies that this effect equals the share of compliers, π_t^C.[22]

4.3.3 Empirical Implementation

We estimate the Wald-DiD ratio using a two-period version of specification (6), restricting the sample to 1997 and 2001. Specifically, we estimate the LATE by two-stage least squares, instrumenting operating under the 35-hour regime with the mandate interacted with the post-reform period:

$$\begin{aligned} H35_{it} &= \gamma_0 + \gamma_1\, S_i \times \mathbb{1}[t \geq 2000] + \alpha_i + \eta_{rt} + \psi_{ct} + \upsilon_{it}, \\ y_{it} &= \beta_0 + \beta_1\, \widehat{H35}_{it} + \mu_i + \rho_{rt} + \phi_{ct} + \epsilon_{it}. \end{aligned} \tag{11}$$

Here, γ_1 identifies π_{2001}^C and β_1 identifies θ_{2001}^{LATE}. Because the model is just identified, the 2SLS coefficient is numerically equal to the ratio of the reduced-form and first-stage DiD coefficients.

Assessing the Additional Identification Assumptions. First, Figure 5, Panels (a)–(b), establishes the relevance of the mandate: by 2001, mandated firms were 63 percentage points more likely than non-mandated firms to operate under the 35-hour regime. This translates into a substantial reduction in working time, with average weekly hours falling by two hours (Figure 6).

[22] Our design differs from the fuzzy difference-in-differences of de Chaisemartin and D'Haultfœuille (2018), even though adoption of the 35-hour workweek also rises among non-mandated firms. Here the mandate is a policy instrument: potential adoption is indexed by mandate status, and compliers are defined relative to the mandate rather than to time. Voluntary adoption by non-mandated firms, together with its effect on outcomes, is part of the no-mandate potential outcome $Y_{it}(0)$ and is differenced out under parallel trends. Our setting corresponds to the instrumented difference-in-differences design of Miyaji (2024).

Second, the exclusion restriction is motivated by our use of pre-announcement firm size and the absence of pre-reform differential trends provide complementary evidence that mandate status is not proxying for pre-existing differences.

Third, violations of monotonicity appear unlikely: such a violation would require a firm to adopt the 35-hour workweek when not mandated but not to adopt when legally required to do so and at risk of labor inspection and sanctions for non-compliance (see Section 2).

Finally, mandate-invariance of tax-cut take-up requires that the mandate not alter the tax-claim decision of firms whose adoption status it leaves unchanged. Never-takers satisfy it by construction, and for always-takers the mandate is unlikely to change the incentive to claim, since potential gains are similar across mandated and non-mandated firms (Figure C.3). Moreover, this assumption implies $0 \leq \text{DiD}_t^{TC} \leq \text{DiD}_t^{H35}$, or equivalently $\widehat{s}_t^{TC} \in [0, 1]$: a ratio above one would imply that the mandate induced additional tax claims beyond the 35-hour adoptions it generated (Appendix B, Proposition B.3).[23] In 2001, the DiD in tax-cut take-up is 0.30, compared with 0.63 for 35-hour regime, yielding $\widehat{s}_{2001}^{TC} = 0.47$ (Figure 5). Thus, nearly half of complier firms claim the tax cut upon adoption.

4.4 Object 3: Separating the Two Policy Components

4.4.1 Objectives and Definitions

The ITT and LATE defined above bundle the effects of the workweek reduction and the payroll tax cut. To separate them, we target the two components of Equation (9):

$$\begin{aligned} &\mathbb{E}\left[\tau_{it}^{H35} \mid S_i = 1, C_{it} = 1\right], \\ &\mathbb{E}\left[\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1, TC_{it}(1) = 1\right]. \end{aligned} \tag{12}$$

We use two predetermined firm-level measures of exposure. First, $F_i \in [0, 1]$ is the firm's 1997 share of full-time jobs, defined as jobs with strictly more than 35 hours. Because the statutory workweek reduction applies to full-time workers, firms with a larger full-time share are more exposed to this component of the reform. Second, P_i measures exposure to the payroll tax cut. Exploiting the wage-dependent tax-cut schedule (Figure 1), we compute the firm's predicted tax cut based on its 1997 wage distribution.

4.4.2 Identification

Conditional Objects. The logic of Sections 4.2 and 4.3 extends to firms with a given exposure level (F_i, P_i). For each $x = (f, p)$ in the support of (F_i, P_i), conditional versions of the ITT $\Delta_t^{ITT}(x)$, complier share $\pi_t^C(x)$, tax-cut take-up $s_t^{TC}(x)$, and LATE $\theta_t^{LATE}(x)$ are identified from conditional DiD contrasts (Appendix B, Proposition B.4). Variation in $\theta_t^{LATE}(x)$ along each exposure dimension provides suggestive evidence on the sign of the corresponding component effect. However, the conditional LATE is the sum of the conditional workweek effect and the conditional tax-cut effect weighted by take-

[23] That inequality is necessary, not sufficient.

up, $\theta_t^{LATE}(x) = \beta_t^{H35}(x) + s_t^{TC}(x)\,\beta_t^{TC}(x)$ (Lemma B.2), so at each x, the identified quantities yield only one equation in the two unknown component effects, leaving them partially identified without additional structure (Appendix B, Proposition B.5).

We first discretize the joint exposure distribution. Define $B_i^F = \mathbf{1}\{F_i \geq f^*\}$ and $B_i^P = \mathbf{1}\{P_i \geq p^*\}$, for prespecified cutoffs (f^*, p^*), and let $B_i = (B_i^F, B_i^P)$ with $b \in \{0,1\}^2$. The exposure-group ITT is

$$\Delta_{bt}^{ITT} = \mathbb{E}\big[Y_{i1}(1) - Y_{i1}(0) \mid S_i = 1, B_i = b\big], \qquad b \in \{0,1\}^2. \tag{13}$$

Identification requires two additional assumptions: (A8) *conditional parallel trends*—parallel trends holds within each exposure group; and (A9) *overlap*—each group contains both mandated and non-mandated firms. Under these assumptions, Δ_b^{ITT} is identified by a binned difference-in-differences specification that fully interacts mandate status, time, B_i^F, and B_i^P (Appendix B, Proposition B.6).

Linear Model. Point identification of the two component effects requires linking them across exposure levels (Appendix B, Proposition B.7). We impose (A10) *linear exposure effects*: for all (F_i, P_i) and $t \geq 1$,

$$\begin{aligned} \mathbb{E}\big[\tau_{it}^{H35} \mid F_i, P_i, S_i = 1, C_{it} = 1\big] &= \beta_t^{H35} F_i, \\ \mathbb{E}\big[\tau_{it}^{TC} \mid F_i, P_i, S_i = 1, C_{it} = 1,\, TC_{it}(1) = 1\big] &= \big(\beta_t^{TC} + \gamma_t F_i\big) P_i. \end{aligned} \tag{14}$$

This structure assumes that each component's effect vanishes at zero own exposure and that per-unit effects vary linearly with exposure. We allow the tax-cut effect to interact with workweek exposure through γ_t: economically, this captures the idea that the tax cut, designed to compensate firms for the workweek constraint, may be more valuable where that constraint binds more strongly.[24] Residual heterogeneity across firms is otherwise unrestricted.

The linear structure allows us to extrapolate across exposure levels and separately identify the two policy components, rather than only comparing firms that are more or less exposed to each. For the LATE (Appendix B, Corollary B.1),

$$\begin{aligned} \theta_t^{LATE} = \beta_t^{H35}\, \mathbb{E}\big[F_i \mid S_i = 1, C_{it} = 1\big] + \beta_t^{TC}\, \mathbb{E}\big[P_i TC_{it}(1) \mid S_i = 1, C_{it} = 1\big] \\ + \gamma_t\, \mathbb{E}\big[F_i P_i TC_{it}(1) \mid S_i = 1, C_{it} = 1\big]. \end{aligned} \tag{15}$$

Similarly, the ITT decomposes as

$$\Delta_t^{ITT} = \underbrace{\beta_t^{H35} \mathbb{E}\big[F_i\, \Delta H35_{it} \mid S_i = 1\big]}_{\text{Workweek reduction}} + \underbrace{\beta_t^{TC} \mathbb{E}\big[P_i\, \Delta TC_{it} \mid S_i = 1\big]}_{\text{Tax cut}} + \underbrace{\gamma_t \mathbb{E}\big[F_i P_i\, \Delta TC_{it} \mid S_i = 1\big]}_{\text{Interaction}}. \tag{16}$$

[24] Without this structure, 2SLS with multiple treatments generally does not recover positively weighted averages of each component's own effect. Because the mandate shifts both components for the same firms, each estimate also loads on the effect of the other component (Bhuller & Sigstad, 2024). The linear exposure structure absorbs these cross-loadings. We provide supporting nonparametric evidence in Section 6.1.

The three terms correspond, respectively, to the effect of the workweek mandate absent the fiscal incentive, the direct effect of the tax cut, and the interaction between the two components (Appendix B, Corollary B.2).

4.4.3 Empirical Implementation

We begin with two preliminary exercises. First, we estimate the conditional ITT and LATE over subsets of the (F, P) support, applying Equations (6) and (11) to the corresponding subsamples. Second, we estimate Δ_b^{ITT} for the four exposure groups b using a fully saturated binned specification that interacts mandate status, year indicators, B^F, and B^P. We then impose the linear exposure model to fully decompose the reform package. We start with the decomposition of the LATE and then use these estimates to decompose the ITT.

Step 1: Decomposition of the LATE. Under restriction (14), we estimate the component effects by two-stage least squares using

$$\begin{aligned} Y_{it} = \beta_t^{H35}\left(F_i H35_{it}\right) + \beta_t^{TC}\left(P_i TC_{it}\right) + \gamma_t\left(F_i P_i TC_{it}\right) \\ + \delta_t'\left(F_i, P_i, F_i P_i\right)' \mathbb{1}[t \geq 2000] + \mu_i + \rho_{rt} + \phi_{ct} + \varepsilon_{it}, \end{aligned} \tag{17}$$

where $\left(F_i H35_{it},\ P_i TC_{it},\ F_i P_i TC_{it}\right)$ are endogenous because they depend on take-up of the two treatments. We instrument these three regressors with

$$S_i \times \mathbb{1}[t \geq 2000] \times \left(1, F_i, P_i, F_i P_i\right)'.$$

The four instruments for three endogenous regressors make the model overidentified, allowing us to test the overidentifying restrictions.

$\widehat{\beta}_t^{H35}$ estimates the effect of operating under the 35-hour regime absent the tax cut, per unit of full-time exposure, while $\widehat{\beta}_t^{TC}$ and $\widehat{\gamma}_t$ characterize the tax-cut effect and its interaction with the workweek reduction. The exposure-by-period controls ($(F_i, P_i, F_i P_i)' \mathbb{1}[t \geq 2000]$) absorb any post-reform change associated with (F_i, P_i) that is common to both groups, so the component effects are identified from how the mandated versus non-mandated gap varies with exposure and rely only on the absence of mandate-specific exposure trends. As in Equation (6), we include industry-by-year and county-by-year fixed effects.[25]

Step 2: Decomposition of the ITT. Equation (16) expresses the ITT as the product of the component effects estimated in Step 1 and the mandate-induced changes in exposure-weighted treatment.

[25] Excluding the industry-by-year and county-by-year fixed effects leaves the estimates largely unchanged.

Estimating the latter by difference-in-differences yields

$$\widehat{\Delta}_t^{ITT} = \underbrace{\widehat{\beta}_t^{H35} \times \mathrm{DiD}_t^{F\times H35}}_{\text{Workweek reduction}} + \underbrace{\widehat{\beta}_t^{TC} \times \mathrm{DiD}_t^{P\times TC}}_{\text{Tax cut}} + \underbrace{\widehat{\gamma}_t \times \mathrm{DiD}_t^{F\times P\times TC}}_{\text{Interaction}}, \tag{18}$$

where $\mathrm{DiD}_t^{F\times H35}$, $\mathrm{DiD}_t^{P\times TC}$, and $\mathrm{DiD}_t^{F\times P\times TC}$ measure the doses of each component induced by the mandate. The first term is the ITT effect of mandating the 35-hour workweek absent the tax cut; the second isolates the direct contribution of the tax cut, and the third captures their interaction. Under the linear exposure model, the three terms must sum to the directly estimated ITT from Section 4.2. Comparing the two therefore provides an overidentification check on the linear exposure restriction (Appendix B, Corollary B.2).

Tests. Figure C.4 reports the joint distribution of (F, P) separately for mandated and non-mandated firms. Three features are reassuring for identification. First, there is substantial independent variation in the two exposure measures: firms can be highly exposed to either component, to both, or to neither. Second, the distributions and their supports are very similar across mandate groups, supporting the overlap assumption. Third, although we do not observe firms with exactly $F = 0$ or $P = 0$, both measures have wide empirical support. This variation allows us to estimate the relationship between outcomes and exposure over a broad range and limits the extrapolation required to evaluate each component at zero exposure.

We assess conditional parallel trends by estimating event studies within different regions of the (F, P) support. The corresponding pre-reform coefficients provide exposure-specific placebo tests.

For the LATE decomposition, we assess instrument strength using the conditional F-statistics of Sanderson and Windmeijer (2016) for each endogenous regressor.

Finally, we assess the linear exposure restriction in three ways. First, we compare the ITT decomposition implied by the linear model with the fully saturated binned estimates, which allow effects to vary flexibly across the four exposure groups. Second, with four instruments for three endogenous regressors, the model is overidentified: under the linear specification, different exactly identified subsets of instruments should imply the same decomposition, which we assess with a Hansen J test. Relatedly, the estimated components should sum to the directly estimated ITT in Equation (18). Since this is not mechanically imposed, finding that they do sum ex post provides support for the linear exposure assumption. Third, motivated by recent work showing that 2SLS with multiple instruments can be sensitive to functional-form restrictions under heterogeneous effects (Blandhol et al., 2022; Bhuller & Sigstad, 2024), we replace the continuous exposure instruments with binned counterparts and include the corresponding binned exposure-by-post controls. This specification relaxes the parametric first-stage restrictions in (F_i, P_i) and allows for nonparametric second-stage controls. Its overidentification test assesses whether a common set of component effects fits the exposure-group ITTs.

5 The Effects of the Reform Package

This section first presents our estimates of the intent-to-treat effects of the mandate relative to only providing the fiscal incentive. We then report the local average treatment effects of operating under the 35-hour regime in Section 5.5. We document effects on working time and compensation, then turn to employment, productivity, and distributional effects. Appendix D complements these results with a worker-level analysis of incumbent workers.

5.1 Weekly Hours, Wages and Labor Cost

Figure 6 reports the intent-to-treat effects of the mandate on hours, wages, and labor costs. The left panels show average outcomes by year from 1994 to 2001 for mandated and non-mandated firms. The right panels report estimates of α_τ from the event-study specification in Equation (6).

Weekly Hours. Before the reform, mandated and non-mandated firms followed parallel trends, with average weekly hours around 36 (Figure 6(a)).[26] Hours began declining in both groups in 1998, consistent with the introduction of fiscal incentives. The decline was larger among mandated firms, with the gap widening in 2000 and 2001. By 2001, average weekly hours reached 33 in mandated firms, compared with 35 in non-mandated firms. Panel (b) confirms this divergence: estimates of α_τ are close to zero before the announcement and reach -2 hours in 2001.

The mandate reduced average weekly hours by 2 hours, that is half the 4-hour statutory reduction in the full-time workweek. Three factors account for the gap. First, these averages include part-time jobs, which were not directly subject to the 35-hour limit. Second, hours are not capped at 35: firms can use overtime. Third, hours also declined in non-mandated firms, which we interpret as a response to the fiscal incentive, as well as some misclassifications using the size proxy in 1997. The 2-hour estimate therefore captures the effect of the mandate relative to the fiscal incentive alone.

Hourly Wage. The provisions to maintain pay at lower hours would, mechanically, imply a large increase in hourly wages. To examine how this adjustment materialized in practice, Figure 6 Panels (c) and (d) report results for average firm-level hourly wages. Prior to the reform, mandated and non-mandated firms exhibit similar wage trends. Starting in 1998, when the mandate had been announced but was not yet in force, wages stagnate in mandated firms while continuing to rise in non-mandated firms. Once the mandate takes effect, wage growth in mandated firms accelerates sharply. We estimate an increase of 1.7% in 2001 relative to 1997.

The 1.7% rise in firm-level hourly wages by 2001 is economically meaningful, but well below the 11.4% mechanical increase for a full-time incumbent worker moving from 39 to 35 hours at unchanged pay. Three factors help reconcile these magnitudes. First, average weekly hours fall by only 2 hours. Starting from 36 hours, unchanged pay would therefore imply an hourly-wage increase of approximately

[26] The average pools full- and part-time workers and therefore sits below the 39-hour full-time schedule.

5.9%. Second, the pay-maintenance requirement primarily affects incumbent full-time workers: it does not mechanically require comparable wage adjustments for part-time workers or new hires. Consistent with this distinction, our worker-level analysis finds a larger increase for full-time incumbents in mandated firms, reaching 4% in 2001 relative to incumbents in non-mandated firms (Figure D.1). Third, mandated firms engaged in wage moderation ahead of implementation, partly offsetting the mandated increase in hourly wages, as in Trejo (1991). Relative to 1999, the wage increase is 2.1% in 2001.

Hourly Labor Cost. The payroll tax reduction offsets a large share of the wage increase. While average firm-level hourly wages rise by 1.7% in 2001, hourly labor costs—the sum of wages and payroll taxes—increase by only 0.7% (Figure 6, Panels (e) and (f)). For full-time incumbent workers, hourly labor costs increase by 3% in 2001 (Figure D.1).

5.2 Employment and Total Hours

Employment. Figure 7 reports the effect of the mandate on employment, measured by the number of jobs in the firm. Panel (a) shows average employment growth relative to 1997 in each group, while Panel (b) reports estimates of α_τ in equation (6) using log employment as the outcome. Mandated and non-mandated firms follow identical trends before the reform and continue to do so after the mandate takes effect. We find no effect on employment despite the large reduction in weekly hours, providing no support for the policy's central work-sharing mechanism: that firms would replace the hours lost per worker by hiring additional workers. Our point estimates are close to zero: the 95% confidence interval rules out employment gains larger than 0.8% and losses larger than 0.9%. Our worker-level analysis, reported in Panel (a) of Figure D.2, similarly finds no effect on employment among incumbents.[27]

Employment Along the Hours Distribution. Although the reform had no effect on total firm employment, it could still change how jobs are distributed along the hours distribution, as weekly hours decreased. To study the causal effect on the entire hours distribution, we adapt the approach of Cengiz et al. (2019) to our context:

$$\frac{\#\text{ jobs bin } j_{it}}{\#\text{ jobs}_{i,1997}} = \sum_{k=25}^{45} \lambda_k \times S_i \times After_t \times \mathbb{1}[j = k] + \mu_{ij} + \rho_{jrt} + \phi_{jct} + u_{ijt} \tag{19}$$

where # jobs in bin j_{it} is the number of jobs in one-hour workweek bin j in firm i and year t, and # jobs$_{i,1997}$ is firm employment in 1997. The first and last bins group all jobs below 25 and above 45 hours, respectively. S_i indicates mandated firms, and $After_t$ equals one in 2001 and zero in 1997. The specification includes firm-by-hours-bin fixed effects μ_{ij}, hours-bin-by-industry-by-year fixed effects

[27] Panel (a) of Figure D.2 shows estimates for the probability that an incumbent worker remains employed, separately for a placebo pre-reform period and the post-reform period. We report estimates by pre-reform working time because workers initially working more than 35 hours were directly exposed to the reduction in the statutory workweek. The estimated effects on employment are close to zero throughout.

ρ_{jrt}, and hours-bin-by-county-by-year fixed effects ϕ_{jct}. We estimate it on the balanced panel of firms and cluster standard errors at the firm level.

Figure 8 reports estimates of λ_k and 95% confidence intervals. Each coefficient measures the effect of the mandate on employment in a given hours bin, expressed as a share of the firm's 1997 employment. Summing the coefficients across bins recovers the zero effect on total employment.

The reform generates a pronounced leftward shift in the hours distribution. Employment rises sharply among jobs around 35 hours and falls even more strongly among jobs at 39 hours and above. The decline in long-hours jobs, however, is only partly offset by growth around the new statutory workweek: the remainder is offset by significant employment gains among jobs with workweeks between 25 and 34 hours. The reform therefore induced a broader reallocation toward shorter-hours and part-time jobs, rather than simply moving workers from 39 to 35 hours.[28]

Our worker-level analysis (Panels (b) and (c) of Figure D.2) shows that the reform also affected incumbent workers who were already working 35 hours or less before the reform. Relative to workers in non-mandated firms, workers employed in mandated firms in 1997 experience both shorter weekly hours and higher hourly wages. Effects are largest for workers initially working 39 hours or more, but those initially working 30–35 hours still experience a 75-minute reduction in weekly hours and a 2.5% increase in hourly wages. The reduction thus extended beyond workers bound by the new statutory limit, consistent with fairness considerations documented in response to wage changes (Dube, Giuliano, & Leonard, 2019; Giupponi & Machin, 2022; Hazell et al., 2022). Supporting this interpretation, Appendix A.5 presents excerpts from firm-level workweek-reduction agreements that explicitly invoke equity between full-time and part-time workers. Part-time workers were also eligible for the payroll tax reduction, so higher wages could in principle reflect subsidy pass-through. However, Section 6.2 finds no evidence of such pass-through.

Total Hours. The mandate reduced weekly hours per job without changing the number of jobs. As a result, total firm hours—defined as the sum of annual hours across all employees—declined. Panels (c) and (d) of Figure 7 show that mandated and non-mandated firms track each other closely before the reform. However, by 2001 total hours are 5.9% lower in mandated firms. Panel (c) of Figure E.1 decomposes this decline into hours above 35—those directly targeted by the reform—and hours at or below 35. Both contribute substantially: by 2001, hours above 35 fall by 3.5% of pre-reform total hours, while hours at or below 35 fall by 2.4%. The former is expected, though not mechanical: the reform did not prohibit workweeks above 35 hours, but changed the incentives governing overtime. The decline in hours at or below 35 is more surprising and reflects the reallocation toward part-time employment documented above.

[28] Consistent with this reallocation, Panel (a) of Figure E.1 reports estimates of α_τ from our main event-study specification using the firm-level part-time employment share as the outcome. Panel (b) of Figure E.1 shows that mandated and non-mandated firms exhibited similar pre-reform trends in bin-specific employment. Figure E.2 shows that the shift occurs for both men and women. In net terms, employment decreases by 0.4% for men and increases by 0.5% for women.

5.3 Distributional Effects of the Reform

To study the reform's distributional consequences, we estimate firm- and worker-level effects separately for higher-skill workers and other employees. Figure 9 presents our results.

Weekly Hours. Panel (a) reports estimates of α_τ from Equation (6) for average weekly hours by skill group. Both groups experience significant declines in their workweek: by 2001, weekly hours fall by 1 hour and 20 minutes for higher-skill workers and by 2 hours for other workers. This pattern is consistent with the 2001 Labor Force Survey supplement, in which higher-skill workers also report reductions in working time, primarily through additional days off (Figure 3).

Total Hours and Employment. Panel (b) reveals a sharp divergence in total hours across skill groups. Total hours worked by lower-skill workers decline significantly, reaching -6% by 2001, whereas total hours for higher-skill workers remain essentially unchanged. The stability of total hours among higher-skill workers implies an offsetting increase in their employment. Panel (c) confirms this pattern: higher-skill employment rises by 1% of 1997 total employment.[29] Because higher-skill workers account for only 10% of employment before the reform, this represents a sizable increase in their headcount. In contrast, the decline in total hours among other workers reflects both shorter workweeks and lower employment: by 2001, lower-skill employment falls by 1% of pre-reform total employment. The effects across the two groups therefore offset each other, recovering the zero effect on overall firm employment. These heterogeneous effects reflect within-firm changes in workforce composition rather than differential growth of firms with initially different skill mixes. Panel (a) of Figure E.3 shows that the share of lower-skill workers declines in mandated relative to non-mandated firms. Panels (b) and (c) further decompose employment by full-time and part-time status within each skill group. Both full-time and part-time employment increase among managers and high-skill professionals. Among other workers, by contrast, full-time employment declines significantly, while part-time employment increases by a smaller amount.

Individual Earnings. Panel (d) of Figure 9 turns to the effects on annual earnings. Using the worker panel, we study incumbents based on their employer in 1997 (mandated or non-mandated) and focus on their highest-paying job. Estimates at the worker level are less precise as they rely on the 4% sample, but they suggest that managers preserved their total earnings, while other employees experienced a significant 1.7% drop. This indicates that the employment reduction and the earnings reduction conditional on continued employment are concentrated in the same skill group.

A natural question is whether the workweek reduction increased multiple-job holding, so that lower earnings per job need not translate into lower total earnings. To test this, we recompute worker earnings by summing across all jobs held in a given year, instead of only considering their highest-paying job.

[29] The outcome is $\frac{\text{Employment by skill in } t}{\text{Employment in 1997}}$, which allows us to include firms with no workers in a given skill group.

Figure D.3 shows that the estimated earnings effects by skill group are virtually unchanged. Lower-skill workers therefore do not offset their per-job earnings losses by taking additional jobs.

Discussion. These findings point to a skill-biased effect of workweek reduction reforms, leading to increased inequality in employment and earnings between managers and other employees. Because skill groups are defined by occupation, they could in principle change within worker over time. Two pieces of evidence indicate that our results reflect genuine heterogeneous effects across workers rather than occupational relabeling. First, transitions between the two groups are rare (Figure E.4), consistent with their distinct institutional status, including separate pension systems. Second, the employment gains and losses accrue to different workers. Figure E.5 shows that the decline in lower-skill employment is concentrated among men, whereas the increase in higher-skill employment also largely benefits women. Similarly, lower-skill employment falls among workers older than 55, with no corresponding increase in higher-skill employment, while higher-skill employment rises among workers younger than 30 with no offsetting decline in lower-skill employment. These patterns are inconsistent with a simple relabeling of the same workers and point to heterogeneous employment effects across individuals.

We investigate three mechanisms that could explain the opposite effects across skills. First, the payroll tax cut is unlikely to drive the divergence. If anything, the subsidy should disproportionately benefit lower-skill workers because it targets low-wage jobs. Consistent with this, hourly labor costs for incumbent workers rise more for higher-skill employees (4%) than for other employees (2.5%), as shown in Figure D.4. Second, heterogeneous pre-existing rents across occupations could explain those differences. In 1998, France had the highest minimum wage relative to the median wage in the OECD, at 60%. Firms may therefore have had less surplus to absorb higher labor costs for lower-skill workers. Third, these effects may reflect differences in the ability to reorganize work. Section 8.2 provides descriptive evidence of organizational changes that appear better suited to higher-skill workers. These results are consistent with Caroli and Van Reenen (2001), who show that organizational change is skill-biased, and with Lawson, Lelarge, and Spanos (2023), who find that French firms facing larger increases in the minimum wage reorganize production around more skilled workers.

5.4 Output and Productivity

Figure 10 reports the effects of the mandate on output (Panels (a) and (b)), labor productivity (Panels (c) and (d)), and total factor productivity (Panels (e) and (f)). The left panels show average outcomes by year × mandate status, while the right panels report estimates of α_τ from Equation (6).

Output. We measure output using value added from firms' balance-sheet data. Mandated and non-mandated firms follow similar trends over 1994–1997, before the reform. A divergence emerges in 1999 and widens through 2001, with slower growth among mandated firms. By 2001, we estimate a 1% reduction in value added due to the reform. This effect is small relative to the 5.9% decline in hours: a back-of-the-envelope calculation assuming a Cobb–Douglas production function in labor and capital

with a labor share of two-thirds would predict a 3.9% reduction in output, all else equal.

Labor Productivity. The small decline in value added relative to hours implies a large increase in labor productivity. By 2001, value added per hour increases by 4.3% in mandated relative to non-mandated firms. While value added per hour is a standard measure of productivity, part of this increase may mechanically reflect limited adjustment of capital in the short- and medium-run. In addition, employment results by skill group (Section 5.3) show a change in the composition of the workforce that is not accounted for by this measure. We therefore turn to total factor productivity, which accounts for capital and labor composition.

Total Factor Productivity (TFP). We estimate firm-level TFP using the control-function approach of Ackerberg, Caves, and Frazer (2015) for value added, separately by major industry.[30] We use materials as the proxy variable for unobserved productivity shocks and log tangible capital as the state variable. We consider several definitions of the labor input in the production function. First, we use total hours worked in the firm. Under this specification, the mandate raises TFP by 3.5% in 2001. Second, we allow hours worked by managers and high-skill professionals to enter separately from hours worked by other employees. The estimated TFP effect falls to 2.2%, implying that the initial estimate partly reflected the change in skill composition. Finally, we allow productivity to depend on how a given number of hours is allocated across workers. For each skill group, the labor input enters separately through the average workweek and the number of workers. This specification allows nonlinear returns to hours over the 35–39 range—for instance because of fatigue or task indivisibility (Feldstein, 1967; Pencavel, 2015; Collewet & Sauermann, 2017). The estimate barely changes, falling to 2.0% in 2001. We therefore obtain a residual TFP increase of about 2% after accounting for changes in skill composition and the allocation of hours across workers.

Beyond the reallocation across our two main skill groups, further changes in worker composition are unlikely to explain the productivity increase. Figure E.6 shows the evolution of average worker fixed effects in mandated and non-mandated firms, separately for managers and high-skill professionals and for other employees.[31] Because worker fixed effects are constant over time by construction, changes in these averages capture changes in worker composition. The relevant comparison is therefore between mandated and non-mandated firms within each skill group. Within each skill group, the two series evolve in parallel throughout the period, with no break at either the reform announcement or the mandate's implementation. We find no evidence that mandated firms upgraded the quality of their workforce within skill groups. To explain the residual TFP increase, Section 8.2 provides instead

[30] Estimating the production function using either the full 1994–2001 period or only the pre-reform period yields similar results.

[31] We estimate worker fixed effects from a two-way fixed effects regression of log hourly wages on worker, firm, and year fixed effects, using the largest connected set of workers and firms over 1994–1997, prior to the reform. Because this exercise requires the panel dimension of the data, we use the 4% sample. We then average the estimated worker effects across workers employed in each group of firms and year, weighting by days worked in the firm-year.

evidence on organizational changes using surveys.

Incidence Across Workers and Shareholders. Productivity gains cushion the decline in value added but do not eliminate it. Figure E.7 shows how this decline is distributed across workers and shareholders. Because lower-skill workers experience both employment and earnings losses, they bear most of the decline in labor compensation, while the higher-skill wage bill increases despite lower output. Profits also fall following the reform. Combining the estimated effects on value added, labor compensation, and profits, an accounting decomposition attributes 70% of the decline in value added to lower labor compensation, concentrated among non-managers, and the remaining 30% to lower profits.

5.5 The Effects of Operating under the 35-Hour Regime

The estimates reported so far are intent-to-treat effects of the mandate to cut hours, relative to the fiscal incentive alone. We now estimate the local average treatment effect (LATE): the effect of operating under the 35-hour workweek for firms induced to do so by the mandate. Section 4.3 describes the estimation and interpretation in detail. Importantly, operating under the 35-hour regime also changes firms' take-up of the accompanying payroll tax cut. The LATE therefore captures the effect of operating under the 35-hour workweek together with the associated tax-cut response, relative to remaining under 39-hour and hence not claiming the subsidy.

Table 2 reports the results. Column (1) gives the first stage, also shown in Panel (b) of Figure 5: the mandate raises the share of firms under the 35-hour regime by 63 percentage points, with Kleibergen–Paap F statistic of 28,917. Hence, the LATE estimates are approximately 1.59 times their ITT counterparts (see the Wald-DiD ratio in Equation (10)). This rescaling accounts for adoption among non-mandated firms and measurement error in mandate status induced by our firm-size measure in 1997. Column (2) reports the effect on tax-cut take-up, TC. It identifies tax-cut take-up among compliers, s^{TC}, which we estimate at 0.47.

Columns (3)–(11) report estimates of β_1 in Equation (11) for the remaining outcomes. Operating under the 35-hour regime reduces the average workweek by three hours. It raises hourly wages by 2.8% but hourly labor costs by only 1.1%, implying that the payroll tax cut offsets about 60% of the wage increase. Total hours fall by 9.9%, while the effect on the number of jobs remains close to zero. This aggregate null masks substantial workforce recomposition: employment of managers and high-skill professionals rises by 1.4% of 1997 employment, while employment of other workers falls by the same amount. Value added declines by 1.7%, while TFP rises by 2.8%.

Both the ITT and the LATE estimates capture the combined effects of the workweek reduction and the payroll tax cut. Section 6 separates these two components.

5.6 Robustness Checks

Beyond the diagnostics embedded in our design, including the pre-reform falsification tests, we assess mean reversion and sensitivity to the choice of firm-size bands.

We first test whether our results reflect mean reversion. Because treatment status is assigned on firm size in a single year, firms classified as small that year might subsequently revert toward their long-run size even absent the reform, mechanically generating divergent employment trends. This concern is limited by design: treatment is assigned using 1997 size among firms continuously observed since at least 1994, so firms just below the threshold are unlikely to be predominantly young, fast-growing firms. Moreover, we focus on firms close to the 20-size threshold on both sides. To test for mean reversion directly, we run a placebo exercise that reproduces our assignment rule in a period without any reform. In Figure C.5, we assign placebo treatment based on firm size in 1995 and estimate event-study coefficients over the subsequent four years, matching the horizon of our baseline analysis. If assignment on size alone generated spurious dynamics, these placebo estimates would diverge from zero. Instead, employment effects remain close to zero and statistically insignificant throughout the placebo period.

Second, recent work documents heterogeneous responses to payroll taxes by firm size (Lobel, 2026) and productivity (Bíró et al., 2022). This is unlikely to be a major concern in our baseline specification, since the mandated group is restricted to firms with 22–48 employees, keeping treated firms relatively close to the 20-employee cutoff. We nevertheless assess sensitivity to this choice by varying the upper bound for the mandated group to 40, 70, and 100 employees, while leaving the non-mandated group and empirical specification unchanged. Figure C.6 reports the resulting estimates across outcomes. The point estimates remain similar and the confidence intervals overlap substantially, indicating that our main results are not sensitive to the firm-size range used to define the mandated group.

6 The Reform Decomposition

This section presents our main results separating the effects of the workweek reduction from those of the tax cuts. The corresponding empirical strategies are detailed in Section 4.4. We begin with two preliminary exercises that illustrate the logic of our decomposition before imposing more parametric structure to decompose both the LATE and the ITT. We present these exercises for employment in the firm.

6.1 Preliminary Evidence

Conditional ITT and LATE. We estimate the ITT and LATE over different regions of the (F, P) support by applying Equations (6) and (11) to the corresponding subsamples. We classify firms as having high or low exposure to each reform component according to whether their pre-reform full-time share F_i and predicted tax cut P_i lie in the top or bottom 30% of their respective distributions. In particular, to isolate the effect of the workweek reduction, we focus on firms with high exposure to the 35-hour mandate but low exposure to the tax cut.

Figure 11 reports the ITT estimates, and Table F.1 reports the corresponding conditional ITT and LATE estimates. The pre-reform coefficients provide a test of parallel trends within each subsample

and are uniformly close to zero and statistically insignificant. While the full-sample ITT is also close to zero and insignificant, substantial heterogeneity emerges across the exposure groups after the reform. Employment falls among firms with high exposure to the workweek reduction, among firms with low exposure to the tax cuts, and most sharply among firms satisfying both criteria. In particular, firms that are in the top 30% of the full-time-share distribution and also in the bottom 30% of the tax-cut distribution, employment falls significantly by 3.9% by 2001. By contrast, among firms with low exposure to both reform components, the estimated effect is close to zero.

Table F.1 also shows that estimates of s^{TC}, the effect of operating under the 35-hour workweek on tax-cut take-up, vary as expected across subsamples. Firms with high exposure to the workweek reduction and low exposure to the tax cuts are the least likely to claim the subsidy. For these firms, reducing working time is relatively costly, while the associated subsidy provides only a limited offset.

These estimates bound the effect of the workweek reduction without additional functional-form assumptions. At exposure level $x = (f, p)$, $\theta^{LATE}(x) = \beta^{H35}(x) + s^{TC}(x)\beta^{TC}(x)$ (Lemma B.2). If the payroll tax cut does not reduce employment, $\beta^{TC}(x) \geq 0$, then $\beta^{H35}(x) \leq \theta^{LATE}(x)$. For firms with high exposure to the workweek reduction and low exposure to the tax cut (Table F.1, column 4), $\theta^{LATE} = -0.054$ (s.e. 0.016). Thus, among these compliers, operating under the 35-hour workweek without tax cuts would have lowered employment by at least 5.4%.

These results suggest that the workweek reduction reduces employment, whereas the tax cuts play a mitigating role. We next show this pattern more systematically using a fully saturated specification.

Saturated Binned Specification. Next, we estimate Δ_b^{ITT} for employment across the four exposure groups defined by the interaction of B^F and B^P, using a fully saturated specification that interacts mandate status, year indicators, B^F, and B^P.[32] This specification imposes no restrictions on how the effects vary across exposure groups and thus provides a benchmark for assessing the linear exposure model estimated below.

Figure F.1 reports the employment effects for each group. Panel (a) contrasts firms with high exposure to the workweek reduction and low exposure to the tax cuts with firms that have high exposure to the tax cuts and low exposure to the workweek reduction. Employment in the former group declines steadily after 1997, reaching −4.5% by 2001, whereas employment in the latter group increases. Panel (b) contrasts firms with high exposure to both components with firms with low exposure to both. The estimates for both groups remain close to zero, consistent with the two components offsetting one another when exposure to both is high and with limited effects when exposure to both is low.

These results indicate that the two reform components operate in opposite directions. They are not, however, sufficient to separately identify the two effects. Because F_i and P_i are correlated, restricting the support of one exposure measure also changes the distribution of the other. In practice, firms with low exposure to a given component still have positive exposure to it. We therefore turn to the

[32] As above, B^F and B^P are indicators equal to one for firms in the top 30% of the corresponding exposure distribution and zero for firms in the bottom 30%.

linear exposure model, which allows us to extrapolate to zero exposure and answer two counterfactual questions: What would have been the effect of the workweek reduction in the absence of the subsidy? And what was the additional effect of the tax cuts?

6.2 Decomposition of the Effect of Operating under the 35-hour Regime (LATE)

We now turn to the linear exposure model and estimate Equation (17) by two-stage least squares, instrumenting the three exposure-weighted endogenous regressors with $S_i \times \mathbb{1}[t \geq 2000] \times (1, F_i, P_i, F_i P_i)'$.

Table F.2 reports first-stage estimates and Sanderson–Windmeijer F Statistics. The joint Kleibergen–Paap statistic is 169. Table 3 reports estimates of β^{H35}, β^{TC}, and γ for our main outcomes. To ease interpretation, we normalize each exposure by its mean and center the interaction term. For each outcome, we report three specifications: (i) OLS, (ii) 2SLS using the continuous exposure instruments, and (iii) 2SLS using binned versions of the instruments.[33] The third specification retains the linear outcome model but replaces the continuous instruments with discrete shifts in exposure, providing a sensitivity test to the functional form.

The results separate sharply the effects of the two reform components. First, the increase in hourly wages is driven by the 35-hour regime. We estimate $\hat{\beta}^{H35} = 3.0\%$, while $\hat{\beta}^{TC}$ is small and statistically insignificant. Thus, we find no evidence that the payroll tax cuts were passed through to workers in the form of higher hourly wages. Second, the estimates for hourly labor costs provide a useful validation of the decomposition: operating under the 35-hour workweek raises hourly labor costs, whereas the payroll tax cuts reduce them. Third, the two components have opposite effects on employment. The 35-hour regime reduces the number of jobs by 6.3%, while the payroll tax cuts increase employment by 13.3% conditional on operating under the 35-hour workweek and claiming the subsidy.

Because we have four instruments for three endogenous regressors, the bottom of Table 3 reports p-values from Hansen J tests of the overidentifying restrictions. For all outcomes, we fail to reject these restrictions. This provides empirical support for the linear exposure specification: under the model, alternative exactly identified subsets of the instruments should imply the same decomposition of the reform into its workweek-reduction and tax-cut components. If the linear specification were misspecified, different sources of identifying variation could instead imply systematically different decompositions of the reform. The version with binned instruments supports the opposite effects of the two reform components.

Finally, as an external validation of our decomposition, we compare the implied elasticity of employment with respect to labor costs with existing estimates from the literature. Combining $\hat{\beta}^{TC}$ with the change in labor costs that the tax cut would generate for the average firm yields an elasticity of -2.7. While large in magnitude, this estimate is comparable to previous estimates for France: Cahuc, Carcillo, and Le Barbanchon (2019) find an elasticity of about -4 for hiring credits, while Kramarz and Philippon (2001) estimate an elasticity of about -2 for minimum-wage workers.

[33] For specification (iii), we restrict the sample to firms whose exposure to each component lies in either the top or bottom 30% of the corresponding distribution.

6.3 Decomposition of the Effects of the Mandate (ITT)

We now use the estimated model to decompose the reduced-form effect of the mandate itself. This is the central policy counterfactual: what would have been the effect of requiring firms to adopt the 35-hour workweek in the absence of the payroll tax cuts, and how much did the accompanying tax cuts change that effect?

Following Equation (18), we combine the estimates of β^{H35}, β^{TC}, and γ with the mandate-induced changes in each treatment take-up. Figure F.2 reports these mandate-induced dose responses. They translate the LATE coefficients into ITT effects by accounting for both firms' exposure to each component and the extent to which the mandate changed treatment take-up.

Figure 12 reports the resulting decomposition of the ITT into the workweek-reduction component, the tax-cut component, and their interaction. It also reports the sum of these three components and compares it with the direct ITT estimate from Section 5, which does not rely on the linear exposure restriction. The figure presents results for employment and average hourly labor costs; Figure F.3 reports the corresponding decomposition for our other outcomes.

Panel (a) of Figure 12 shows that the workweek-reduction component of the mandate lowers firm employment by 4.1%: $\hat{\beta}^{H35} \times \text{DiD}_t^{F\times H35} = -0.063 \times 0.661 = -0.041$. The payroll tax cuts have an almost exactly offsetting effect, increasing employment by 4.2%. Thus, absent the payroll tax cuts, the mandate would have produced a substantial decline in employment; the subsidy offsets this decline and yields the near-zero employment effect of the reform package documented in Section 5.

Importantly, nothing in the estimation mechanically imposes this offset or forces the decomposition to reproduce the direct ITT. The component effects are identified from how the gap between mandated and non-mandated firms varies with exposure to the workweek reduction and the tax cuts, whereas the direct ITT is identified from the average gap alone. Nevertheless, the sum of the estimated components closely matches the direct reduced-form estimate. This agreement provides an additional validation of the linear exposure restriction underlying the decomposition. The interaction component is small. Because exposure is centered in the interaction term, the mandate-induced dose response for this component is limited, and the corresponding estimates of γ are imprecise.

The implied counterfactual employment effect is economically substantial: without the payroll tax cuts, the 35-hour mandate would have reduced employment by 4.1%. This magnitude is also consistent with the conditional ITT estimates and those from the saturated binned specification, neither of which relies on the linearity assumption.[34] Because we separately estimate the per-unit effect of operating under the 35-hour, $\hat{\beta}^{H35}$, and the dose-response, $\text{DiD}_t^{F\times H35}$, those results can also be used to simulate a mandate inducing a different take-up by varying the latter.

Why did the payroll tax cuts offset such a large share of the employment decline? Panel (b) shows that they absorbed a substantial fraction of the increase in labor costs generated by the shorter work-

[34] In Section 6.1, we estimate the ITT among firms with high exposure to the workweek reduction but low exposure to the tax cuts and find a similarly sized employment decline. Although that exercise cannot fully eliminate exposure to the subsidy, it provides direct evidence for the counterfactual implied by the linear decomposition.

week. The workweek-reduction component raises average hourly labor costs by 2.1%, while the tax-cut component lowers them by 1.5%. Their sum therefore reproduces the approximately 0.7% increase in hourly labor costs estimated directly from the ITT. Put differently, the tax cuts absorbed roughly two-thirds of the increase in hourly labor costs generated by the workweek reduction. The decomposition of hourly wages helps explain this result. Panel (a) of Figure F.3 shows essentially no wage response to the tax cuts, indicating that the subsidy accrued primarily to firms rather than being passed through to workers in the form of higher wages. Two features of the setting make this finding particularly plausible. First, the payroll tax cuts were explicitly designed as part of the reform to offset the increase in labor costs associated with shorter working time, as reflected in their conditionality on workweek reduction. This interpretation is consistent with Benzarti and Harju (2021), who show that payroll tax cuts can be particularly effective at preserving employment when firms face adverse shocks. Second, our finding is consistent with Bozio et al. (2025), who show that the pass-through of payroll tax changes to wages is generally limited when the tax change does not alter workers' associated benefits, as in our setting.

Finally, Figure F.3 shows that value added would have fallen by 1.6% in the absence of the payroll tax cuts, compared with 1% under the full reform, although the estimate of the workweek component is relatively imprecise. The decomposition also confirms that the increase in TFP is driven by the workweek-reduction component of the reform.[35]

Fiscal Cost. Conditional on the workweek reduction, the payroll tax cut preserved 4.1% of employment. With an average firm size of 35.5 employees, this corresponds to 1.46 jobs saved per firm. In 2001, the mandate reduced employer payroll tax payments by 7.2%, or approximately €21,000 per firm (Figure E.8), implying a direct fiscal cost of about €14,400 per job saved, or 60% of average annual gross earnings in our sample.

The net fiscal cost of the tax cut is substantially lower. In 2001, unemployment benefits replaced approximately 60% of prior earnings.[36] The unemployment benefits avoided by the additional employment therefore offset the direct cost of the subsidy, assuming that those workers would have claimed benefits. The tax cut also preserves tax revenues through its positive effects on value added and worker earnings. Accounting for these margins implies a negative net fiscal cost of the tax cut.

By contrast, the net fiscal cost of the full reform package is positive. The workweek reduction and payroll tax cut jointly leave employment unchanged while reducing value added and worker earnings. Employer payroll tax revenues therefore fall without corresponding employment gains, while the contraction in other tax bases further reduces government revenue. In Section 8.3 we discuss the welfare effects of the reform.

[35] If anything, the tax-cut component lowers TFP. One interpretation is that, by preserving jobs that would otherwise have been destroyed following the increase in labor costs, the subsidy dampens the compositional productivity gains generated by the workweek reduction.

[36] The daily allowance was the larger of (i) a proportional component equal to 57% of the daily reference wage and (ii) a formula combining a 40% replacement rate with a flat component. At the earnings levels of workers in our sample, this implies a gross replacement rate close to 60%.

7 Market-Level Evidence

Most results in Sections 5 and 6 are firm-level estimates based on differences across firms over time. We now examine the reform at the local labor-market level for two reasons. First, local outcomes incorporate potential spillovers across firms, including worker reallocation. This concern is limited given the zero firm- and worker-level employment effects, and the absence of breaks in employment trends for either mandated or non-mandated firms. Still, a market-level analysis captures within-market spillovers directly. Second, it broadens the scope of the analysis to the entire local economy by incorporating firm entry, firm exit, as well as larger firms excluded from our baseline sample.

7.1 Empirical Strategy

Our strategy compares the evolution of local outcomes across markets with greater versus lower exposure to the reform. We define local labor markets as commuting zones (CZs), of which there are 304 in metropolitan France. For each CZ c, we measure exposure to the reform, $Exposure_c$, as the pre-reform employment share in full-time jobs at firms with more than 20 employees. This measure generates substantial variation across CZs. Figure 13, Panel (a), plots its distribution: exposure averages 0.36, with a standard deviation of 0.06 and a range from 0.2 to 0.6.

We estimate intent-to-treat effects of exposure using the following event-study specification:

$$y_{ct} = \sum_{\tau=1994, \tau \neq 1997}^{2001} \psi_\tau \times Exposure_c \times \mathbb{1}[t = \tau] + \mu_c + \phi_t + u_{ct}, \tag{20}$$

where μ_c are CZ fixed effects absorbing time-invariant differences across local labor markets and ϕ_t are year fixed effects absorbing aggregate shocks. The coefficients ψ_τ trace the differential evolution of outcomes in more versus less exposed CZs. Our CZ-by-year outcomes y_{ct} are average weekly hours per job, log total hours, log total employment, log employment by skill group, and log value added.[37] To facilitate interpretation, we normalize $Exposure_c$ by its mean, so that ψ_τ measures the effect for a CZ with average exposure relative to a CZ with no exposure.

7.2 Results

Figure 13, Panel (c), reports estimates of ψ_τ for average weekly hours. By 2001, weekly hours fall by 2 hours in a CZ with average exposure relative to a CZ with zero exposure. Panel (d) reports effects on employment and total hours. By 2001, a CZ with average exposure has 10% fewer total hours and 5% fewer jobs than a CZ with no exposure to the reform. As in the firm-level analysis, the decline in weekly hours is therefore not offset by higher employment. Panel (e) reports employment effects by skill group and yields the same pattern as the firm-level estimates: employment of managers and high-skill professionals rises by 9%, while employment of other workers falls by 7%. Because managers

[37] Because balance-sheet data are reported by firm rather than establishment, the value-added sample is restricted to single-establishment firms and multi-establishment firms whose activity is concentrated predominantly in one location.

and high-skill professionals account for only about 10% of employment, these effects aggregate to the negative employment effect in Panel (d). The similarity with the firm-level results indicates that the skill-specific employment responses are not driven by cross-firm reallocation, since they are also observed at a more aggregate level. Panel (f) reports the effect on local value added, computed as the sum of value added across firms in the CZ. By 2001, value added is 5% lower in a CZ with average exposure, although the estimate is not statistically significant.

Our firm-level decomposition shows that the workweek reduction and the payroll tax cut had opposite employment effects. To understand how the CZ-level estimates relate to these results, we examine how CZ exposure relates to each reform component. Figure 13, Panel (b), shows that normalized CZ exposure is strongly associated with the pre-reform full-time share, which determines exposure to the workweek reduction. The regression slope is 0.21 (standard error 0.018). In contrast, CZ exposure is unrelated to the predicted payroll tax cut based on 1997 wages, with a slope of -0.03 (standard error 0.02). The CZ design therefore captures primarily variation in exposure to the workweek reduction, rather than the tax cut. As a benchmark, Panel (b) also reports the corresponding firm-level regressions using mandate status, the source of variation underlying our firm-level ITT estimates. Both slopes are close to zero, consistent with similar distributions of exposure to each component across mandated and non-mandated firms (Figure C.3). These differences help reconcile the employment estimates: while the two reform components offset each other at the firm level, leaving employment unchanged, the CZ design places greater weight on the workweek reduction, yielding a negative employment effect consistent with our firm-level decomposition.

8 Discussion

Our main results capture the effects of the reform up to 2001. We now extend the analysis to 2003, noting that these longer-run estimates rely on stronger identification assumptions. We conclude our study with descriptive evidence on the mechanisms underlying the productivity gains and skill-biased effects, as well as on worker welfare.

8.1 Longer-Run Effects

The main analysis ends in 2001 because, from 2002, all firms were formally subject to the 35-hour workweek, eliminating the difference in mandate status underlying our baseline design. In practice, some firms scheduled to comply in 2002 never reduced hours (Figure 4, Panel (f)), so mandate status continued to predict weekly hours. We use this residual variation to estimate the effects of reducing the workweek up to four years later.

Context and Design. Following the 2002 presidential and legislative elections, the newly elected right-wing government softened the implementation of the 35-hour reform. In particular, the Fillon payroll tax reductions replaced the Aubry subsidies and applied to firms regardless of working time,

while enforcement of the 35-hour requirement was relaxed. As a result, some firms scheduled to adopt in 2002 remained at 39 hours. The mandate status determined by 1997 employment therefore continued to predict weekly hours beyond 2001. To show this, we measure adoption as whether the modal hours within each firm are around 35. Appendix Figure G.1 documents this persistence by extending our baseline event study through 2003, using this indicator as the outcome. The first-stage effect declines from 35 percentage points in 2001 to 18 percentage points in 2003, but remains economically and statistically significant.

A simple IV regression of modal hours at 35 on mandate status in these later years would not, however, identify an effect at a well-defined horizon: at any given date, adopters have reduced hours for different lengths of time. Appendix G therefore develops a horizon-specific design that distinguishes effects one to two years after adoption from those three to four years later. We instrument time since adoption using interactions between mandate status and calendar year. To link the resulting comparisons across cohorts, we assume that the effect at a given horizon is the same for firms initially scheduled to adopt early and late. This allows us to chain the estimates and recover effects at longer horizons.

Results. Table G.1 shows our estimates. Adopting the 35-hour workweek decreases employment by 3% (significant at the 10% level) three to four years later. The institutional changes after 2001 are important for interpreting this estimate. Effects three to four years after adoption are identified from outcomes in 2003, whereas the one-to-two-year estimates draw on several calendar years. By 2003, payroll tax reductions were no longer conditional on adopting the 35-hour workweek. The negative employment effect therefore does not reflect adjustment dynamics alone: it also captures the consequences of the shorter workweek once the adoption-contingent subsidies had been removed. Employment losses are therefore consistent with the decomposition in Section 6. Value added also declines by more at a longer horizon. Hourly wages remain higher after four years, suggesting that, although the initial increase was smaller than intended, it is persistent. Finally, the productivity gains documented after two years also persist: TFP remains significantly higher three to four years after adoption.

8.2 Descriptive Evidence on Organizational Changes

To investigate the mechanisms behind the productivity increase and workers' perceptions of the reform, we draw on four surveys (Labor Force Survey, Working Conditions Survey, *Enquête RTT et Modes de Vie* and *Enquête de Conjoncture dans l'Industrie*) described in Section 3.1. These results are descriptive complements to the causal evidence above. By construction, the worker surveys condition on employment after the workweek reduction and therefore exclude workers who subsequently left employment. They also capture relatively early adopters: the LFS supplement was conducted in 2001 and the Enquête RTT in 1999. Finally, because each survey is observed at a single point in time, they do not support difference-in-differences estimation, although some questions ask respondents to compare outcomes with the pre-reform period.

Figure H.1 reports the results. Panel (a) documents substantial pre-reform slack, suggesting scope

for firms to absorb shorter hours without proportionate reductions in output. In the 1998 Working Conditions Survey, 23% of workers report some idle time at work. Moreover, 28% of managers and high-skill professionals and 17% of other employees report sometimes lacking work because of slow business. Employers also appear aware of this slack: in the manufacturing survey, firms report that they could increase output by 10% on average without additional hiring.

Panel (b) documents work intensification and reorganization following shorter workweek implementation. Forty-two percent of workers report performing the same tasks in less time, while 26% report better organization of their work, compared with only 9% reporting worse organization. These adjustments were not costless: 32% report greater stress, compared with 11% reporting less stress.

Panel (c) shows that these changes were more pronounced among higher-skill workers: 52% of them report accomplishing the same tasks in less time and 43% report better work organization, versus 40% and 22%, respectively, for other employees. This greater adaptability to organizational change provides a potential mechanism for the skill-biased employment effects documented in Section 5.3.

Panel (d) describes the organizational changes workers attribute to the reform. Among affected workers, 48% report greater required versatility, 38% changes in work procedures, 38% a reallocation of tasks across workers, and 36% greater use of information technologies and new equipment or software. These responses suggest that the intensification of work reflected firm-level organizational changes rather than workers simply increasing their individual pace of work.

8.3 Worker Satisfaction

Figure H.2 uses the worker survey *Enquête RTT* in 1999 and reports worker satisfaction following the workweek reduction. The survey was conducted on employed workers. In Panel (a), 67% of managers and 58% of other employees report that the reform improved their daily life, compared with 7% and 14% reporting a deterioration. Assessments of working conditions are much more mixed: 32% of managers report an improvement and 26% a deterioration, while the corresponding shares among other employees are 25% and 28%. Panel (b) uses the 2001 Labor Force Survey supplement to compare workers in firms that had and had not implemented a workweek reduction. Satisfaction with working time is 3 percentage points higher in firms that had implemented the reform, for both skill groups. Taken together, these results are consistent with a trade-off: work intensification and greater stress may have increased the disutility of time spent at work for some workers, while shorter hours increased leisure and improved overall satisfaction conditional on continued employment.

The majority of managers reporting an improvement in overall life satisfaction is consistent with our findings: their employment probability increased with the reform, and their earnings remained constant despite fewer hours. For lower-skill workers, our results have a priori ambiguous welfare implications due to the joint reduction in hours, employment, and earnings. Appendix H.2 uses MaCurdy preferences and our empirical estimates to characterize which parameter values generate a positive versus a negative effect on welfare. It shows that the main driver is whether workers wanted to work more or fewer hours, at their hourly wage, prior to the reform. For instance, when the ratio of the

marginal rate of substitution between leisure and consumption to the wage is one half—workers would have liked to work more—welfare effects are negative for all values of risk aversion and of the Frisch elasticity we consider. When this ratio exceeds one—workers would have liked to work less—welfare gains dominate for moderate risk aversion. Jarosch, Pilossoph, and Swaminathan (2025) find that this ratio exceeds one for two-thirds of full-time workers in Germany. Although a comparable measure is unavailable for France during our period, Carry (2026) finds that, among workers at 35 hours in 2013, more than half would prefer to work longer at their current hourly wage, while the remainder are satisfied with their current hours.[38]

9 Conclusion

This paper provides causal evidence on the employment, output, and productivity effects of a large, pay-preserving reduction in statutory working time. France's 1998 Aubry laws reduced the full-time workweek from 39 to 35 hours while simultaneously cutting employer payroll taxes, particularly at low wages. We exploit the size-based rollout, combined with predetermined measures of firms' exposure to each component, to separate the effects of the workweek reduction from those of the tax cuts.

The reform did not generate work-sharing. Absent the payroll tax cuts, the workweek reduction would have lowered employment by 4.1%. The tax cuts offset this decline almost exactly, leaving the overall reform approximately employment-neutral. A commuting-zone design covering the entire economy corroborates this interpretation. The reform also substantially reshaped the distribution of hours, increasing the prevalence of part-time employment.

The reform's incidence was unequal across skill groups. For managers and high-skill professionals, the policy worked as intended: weekly hours fell, monthly earnings were preserved, and employment rose. For other employees, employment, total hours, and monthly earnings all declined. This heterogeneity is consistent with two mechanisms: a binding minimum wage that left firms less surplus to absorb higher costs for lower-skill jobs, and organizational changes that, according to survey evidence, favored higher-skill workers.

The case for shorter hours is stronger on productivity: after accounting for changes in workforce composition, total factor productivity increased by 2%. Surveys document both slack in pre-reform working time and substantial work intensification and reorganization after the reform. These productivity gains cushioned, but did not eliminate, the output loss, at 1%. This decline was borne by lower-skill workers, through lower labor compensation (about 70%), and by shareholders, through lower profits (about 30%).

These findings speak to the three outcomes that have structured the debate over shorter-workweek policies for decades: employment, output, and productivity. They also offer two broader lessons. First, the distributional consequences of labor market reforms can be difficult to infer from the instruments

[38] In the US, Lachowska et al. (2026) use a revealed preference approach and find that workers would, on average, prefer to work more hours at their current hourly wage.

they use: here, payroll tax relief was targeted toward low wages, yet the losses from the reform were concentrated among lower-skill workers. Second, evaluating a policy package only as a whole can obscure the effects of its individual components. Viewed solely through the aggregate reform, the workweek reduction would appear employment-neutral, while the payroll tax cuts would appear to have created few jobs. In fact, the workweek reduction alone would have reduced employment substantially, and the tax cuts preserved nearly all of the jobs it put at risk. Because major labor market reforms often come as bundles, designs that exploit differential exposure to each component, like the one developed here, can help recover offsetting effects that aggregate evaluations mask.

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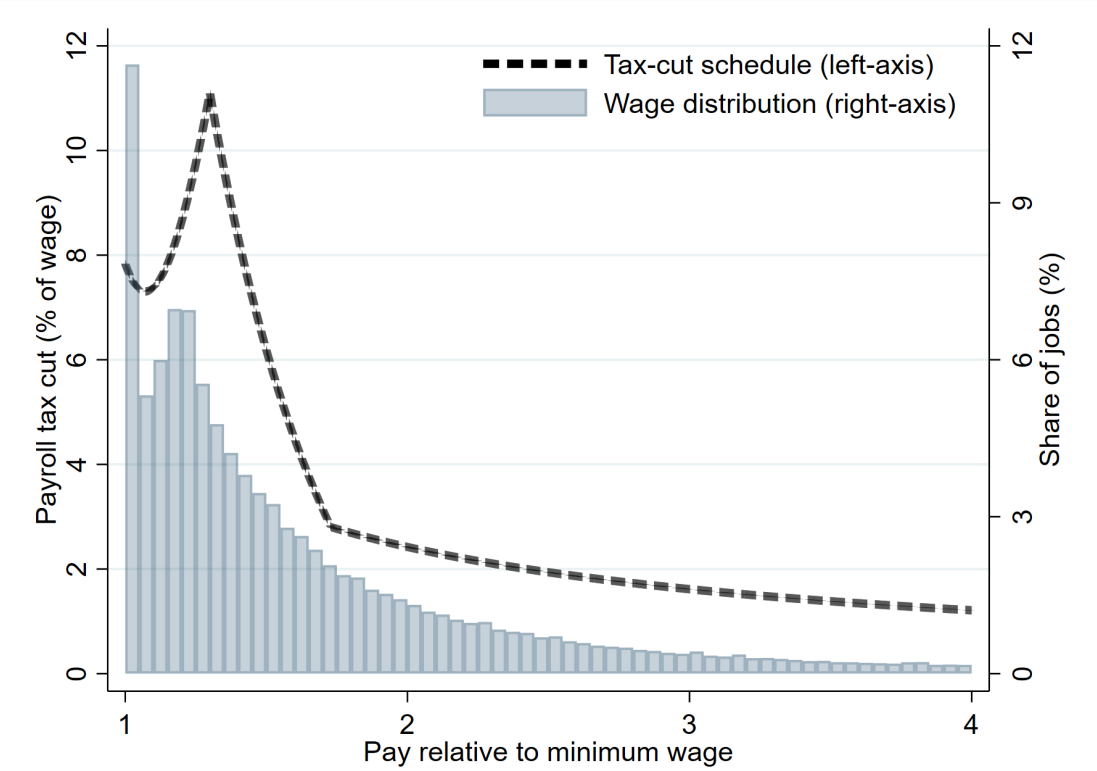
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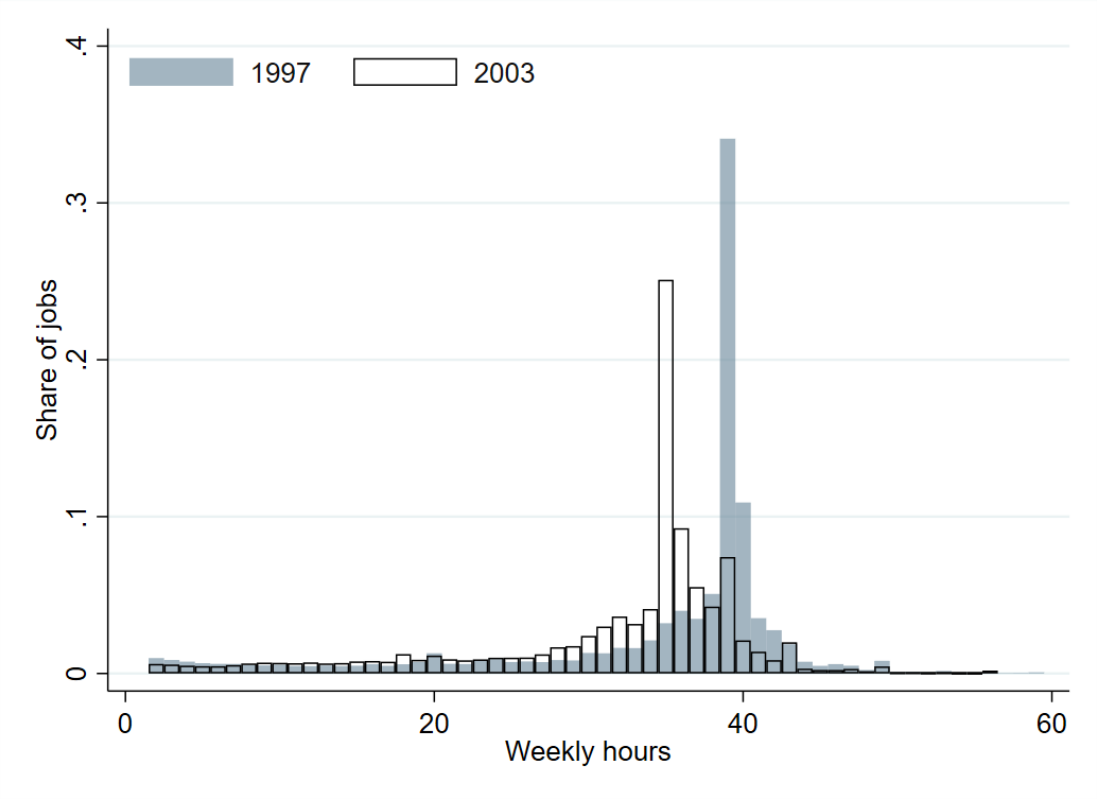
Main Figures

Figure 1: Payroll Tax Cut Schedule



Notes: This figure shows the employer payroll tax cut associated with adoption of the 35-hour workweek. The horizontal axis reports workers' full-time-equivalent wages, normalized by the full-time minimum wage. The black dashed line shows the additional tax cut conditional on adopting the 35-hour workweek, measured as the difference between the tax cut available to adopters and the pre-existing schedule, which was independent of working hours. The tax cut is expressed as a percentage of wages (left axis), alongside the 1997 wage distribution (right axis). Appendix Figure A.1 displays the pre-existing schedule and the schedule available to adopters separately.

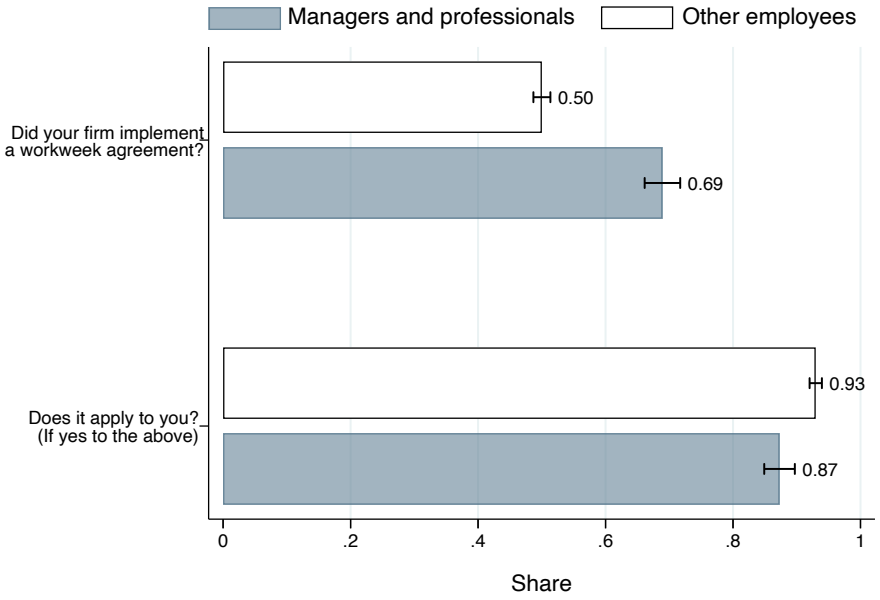
Figure 2: Distribution of Weekly Hours, Pre- and Post-Reform



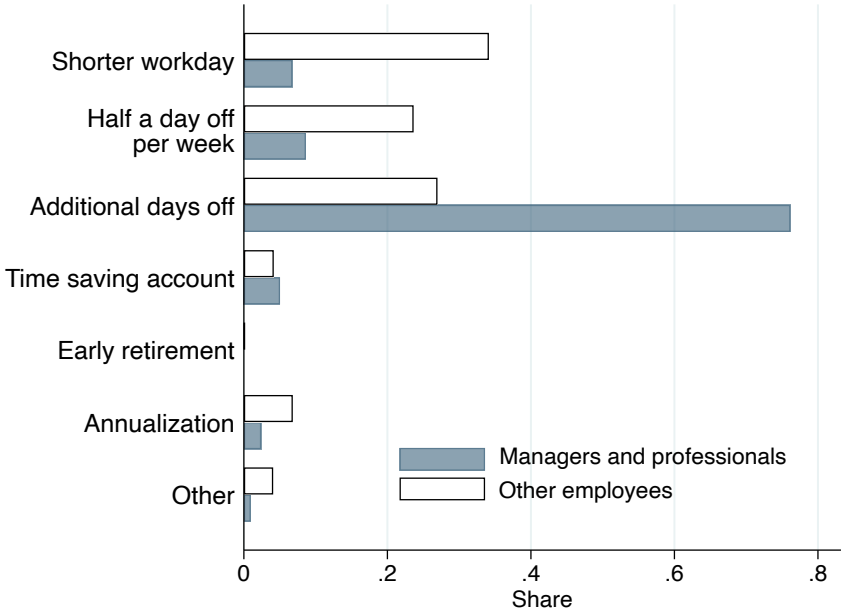
Notes: This figure shows the distribution of weekly hours worked in 1997 (shaded bars) and 2003 (outlined bars). Weekly hours are computed from annual hours reported in the matched employer–employee records, divided by employment spell length and scaled to a weekly measure. The y-axis reports the share of workers in each one-hour bin. The sample includes private-sector workers aged 18–60.

Figure 3: Modes of Workweek Reduction by Skill Group (2001)

(a) Worker-Reported Workweek Reduction Agreements



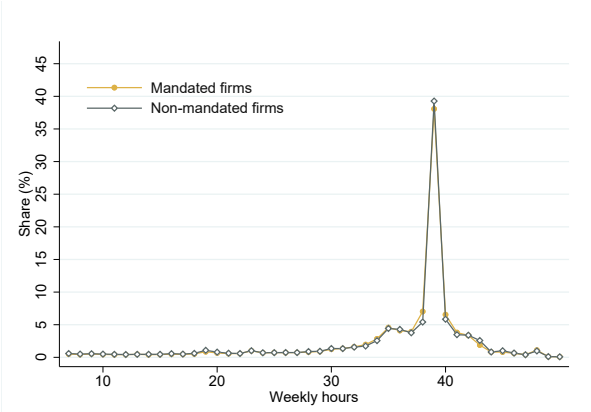
(b) Worker-Reported Modes of Working Time Reduction



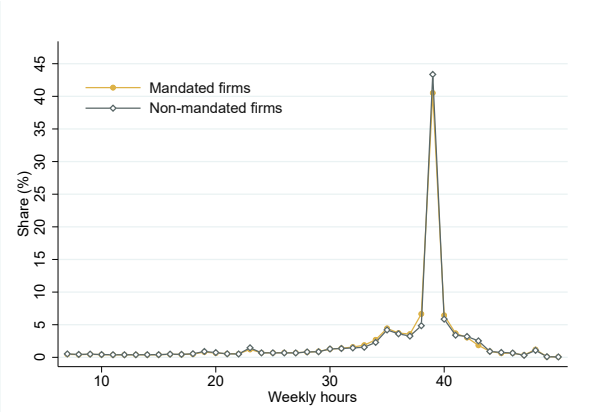
Notes: This figure shows how workweek reductions were implemented, using the 2001 supplement to the Labor Force Survey (LFS). Results are reported separately for managers and high-skill professionals and for other employees. Panel (a) reports the share of workers indicating that their employer implemented a workweek reduction agreement and, among them, the share whose own workweek was reduced. Whiskers denote 95% confidence intervals. Panel (b) shows, among workers covered by such an agreement, the reported modes of working-time reduction by skill group. Respondents could report more than one mode.

Figure 4: Distribution of Weekly Hours by Firm Size, 1997–2002

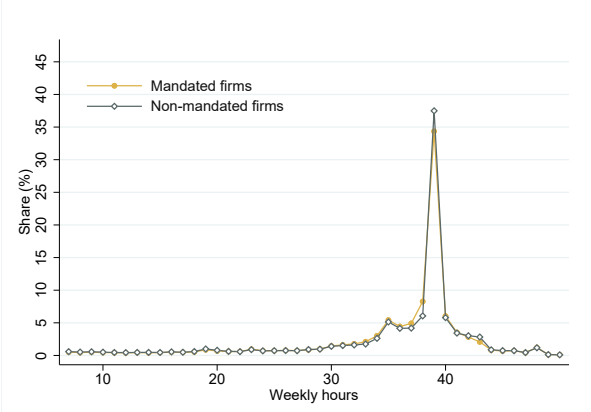
(a) 1997



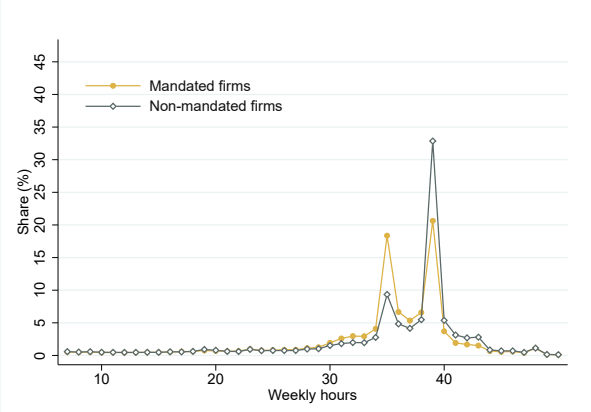
(b) 1998



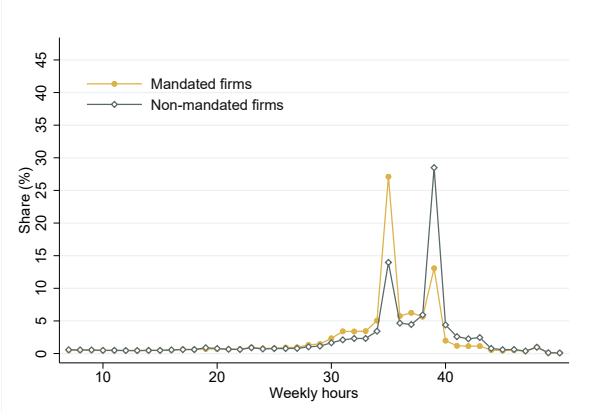
(c) 1999



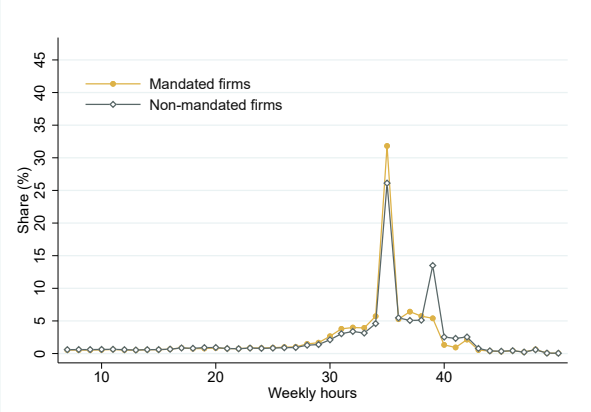
(d) 2000



(e) 2001



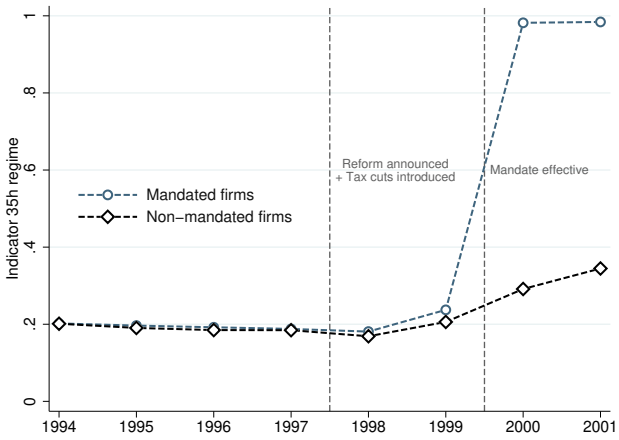
(f) 2002



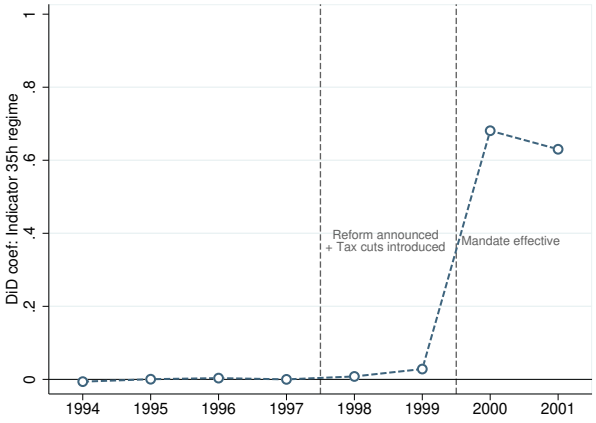
Notes: This figure reports the distribution of weekly hours in mandated and non-mandated firms from 1997 to 2002 at the job level. Each panel corresponds to a calendar year, with mandated firms shown by yellow solid circles and non-mandated firms by gray outlined diamonds. Weekly hours are computed from the matched employer–employee records as total annual hours divided by days employed and multiplied by 7. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 5: Firm-Level 35-Hour Regime and Tax Cut Take-Up

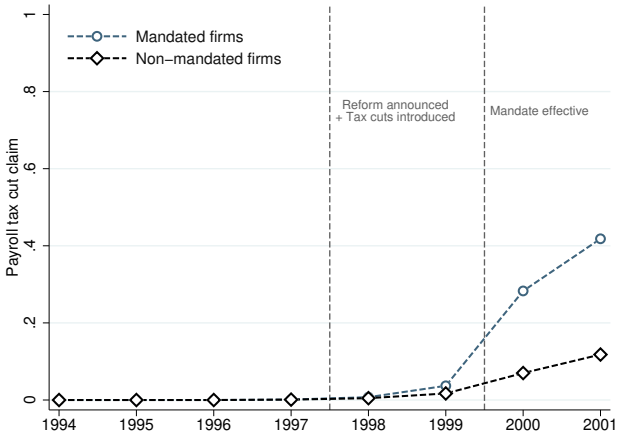
(a) Raw Avg.: 35h Regime ($H35$)



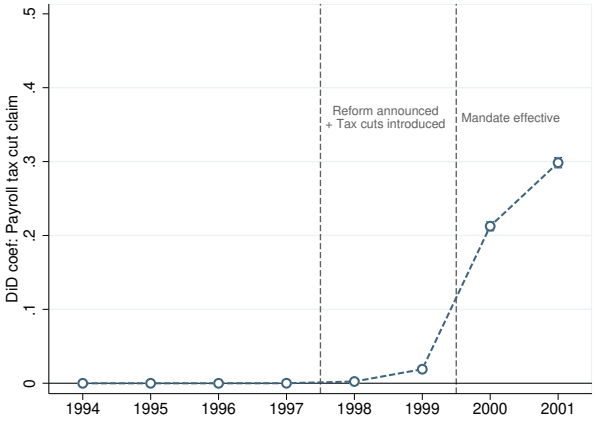
(b) DiD: 35h Regime ($H35$)



(c) Raw Avg.: Payroll Tax Cut Claimed (TC)



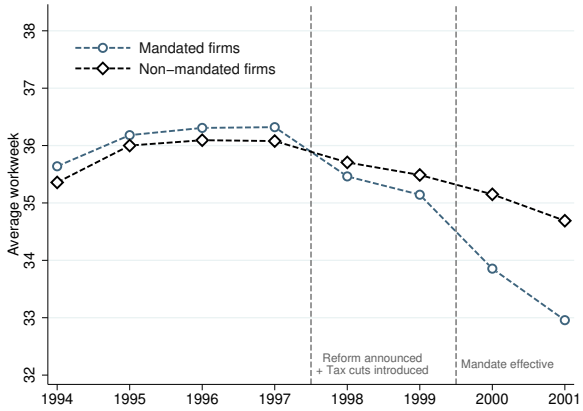
(d) DiD: Payroll Tax Cut Claimed (TC)



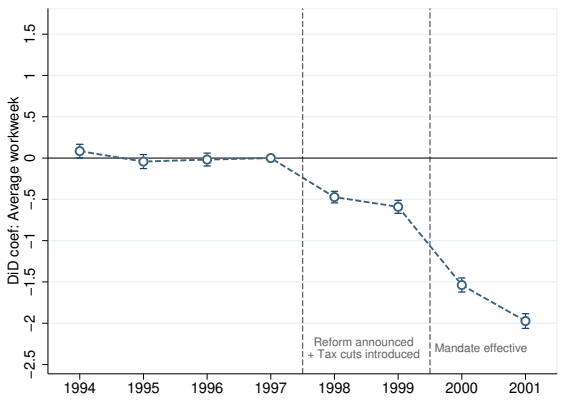
Notes: This figure reports the intent-to-treat effects of the legal mandate on adoption of the 35-hour regime (Panels (a)–(b)) and take-up of the payroll tax cut (Panels (c)–(d)). The left panels display raw averages by mandate status, while the right panels report the corresponding difference-in-differences estimates of α_τ from Equation (6). Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 6: Firm-Level Effects of the Mandate: Weekly Hours, Hourly Wage and Labor Cost

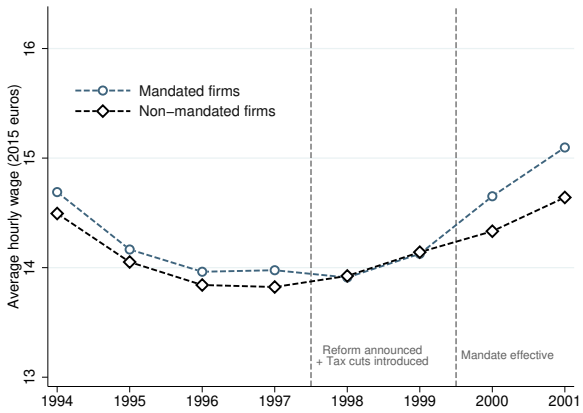
(a) Raw Avg.: Weekly Hours



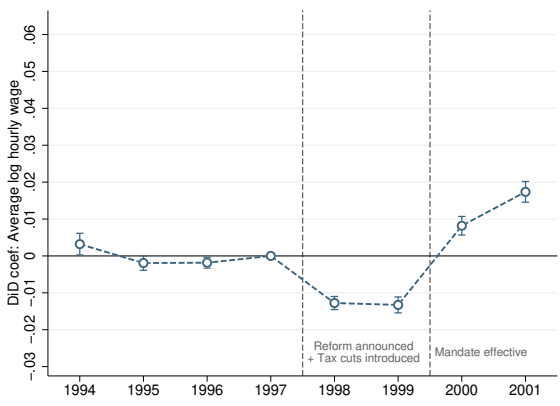
(b) DiD: Weekly Hours



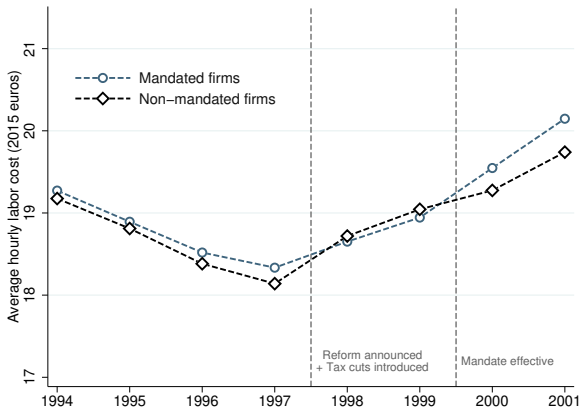
(c) Raw Avg.: Hourly Wage



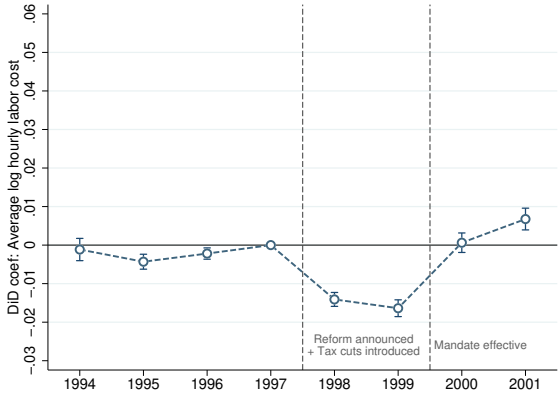
(d) DiD: Log Hourly Wage



(e) Raw Avg.: Hourly Labor Cost



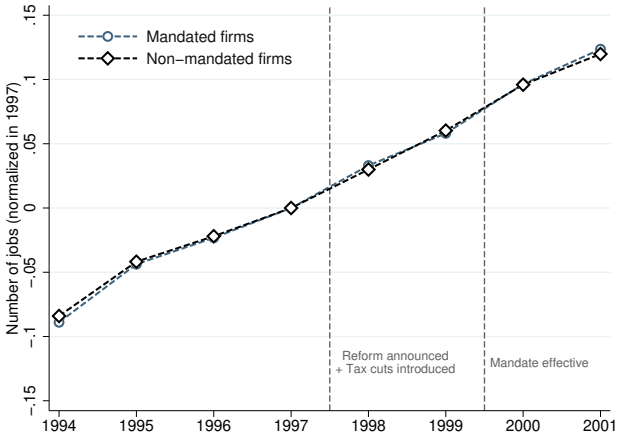
(f) DiD: Log Hourly Labor Cost



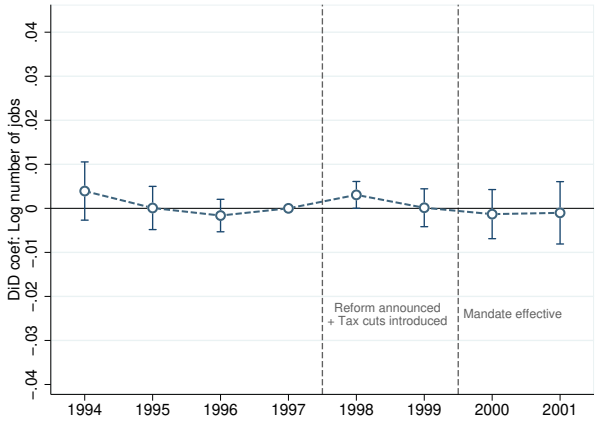
Notes: This figure reports the intent-to-treat effects of the legal mandate on average weekly hours (Panels (a)–(b)), average hourly wages (Panels (c)–(d)), and average hourly labor cost (Panels (e)–(f)). The left panels display raw averages by mandate status, while the right panels show the corresponding difference-in-differences estimates of α_τ from Equation (6). Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 7: Firm-Level Effects of the Mandate: Employment and Total Hours

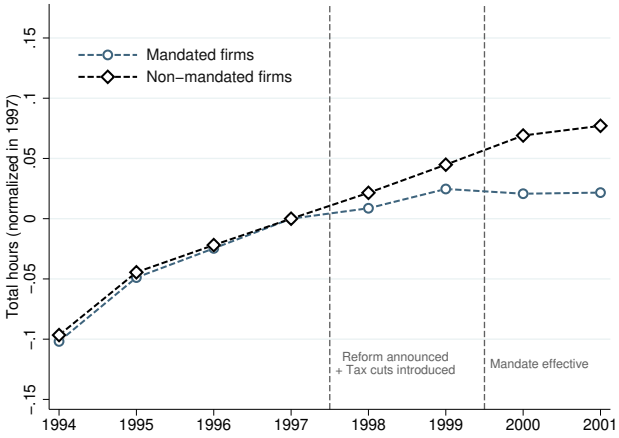
(a) Raw Avg.: Log Employment



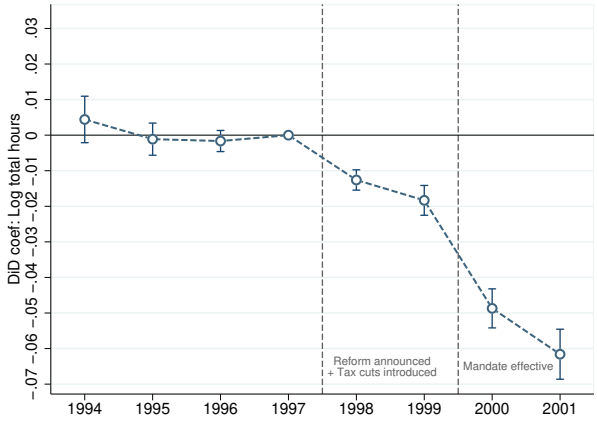
(b) DiD: Log Employment



(c) Raw Avg.: Log Total Hours

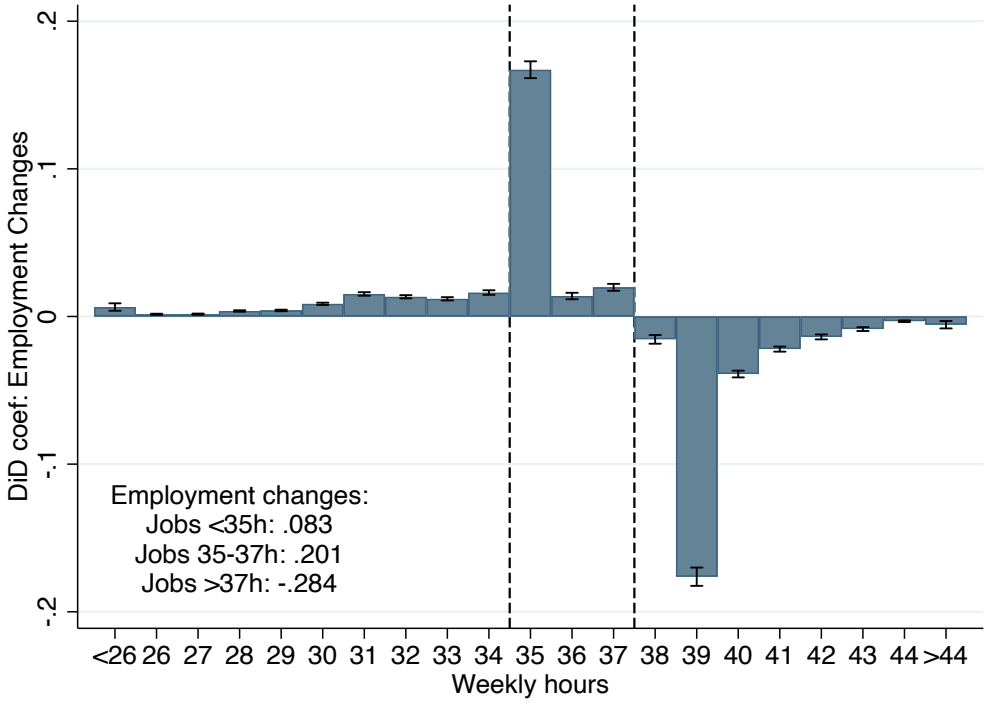


(d) DiD: Log Total Hours



Notes: This figure reports the intent-to-treat effects of the legal mandate on firm-level employment (Panels (a)–(b)) and total hours worked (Panels (c)–(d)). The left panels display raw averages by mandate status, normalized to their 1997 values. The right panels show the corresponding difference-in-differences estimates of α_τ from Equation (6), with outcomes expressed in logs. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

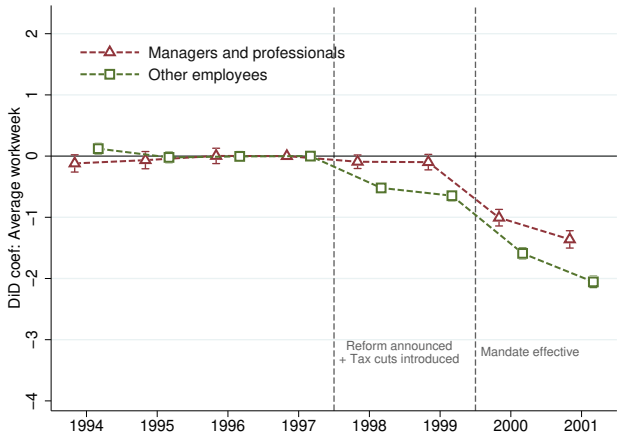
Figure 8: Firm-Level Employment Effects Across Weekly Hours Bins



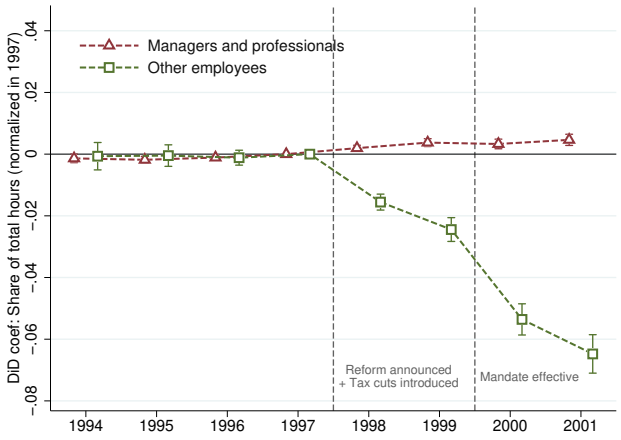
Notes: This figure reports firm-level difference-in-differences estimates of λ_k from Equation (19). Each bar shows the estimated change in employment in mandated relative to non-mandated firms within a given weekly-hours bin between 1997 and 2001, expressed as a share of total firm employment in 1997. The estimates are therefore on a common scale across bins and sum to the aggregate employment effect. Whiskers denote 95% confidence intervals. Dashed vertical lines mark 35 and 37 hours. The figure also reports aggregate changes for jobs below 35 hours, between 35 and 37 hours, and above 37 hours, obtained by summing the corresponding bin-level estimates. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 9: Effects of the Mandate Across Skill Groups

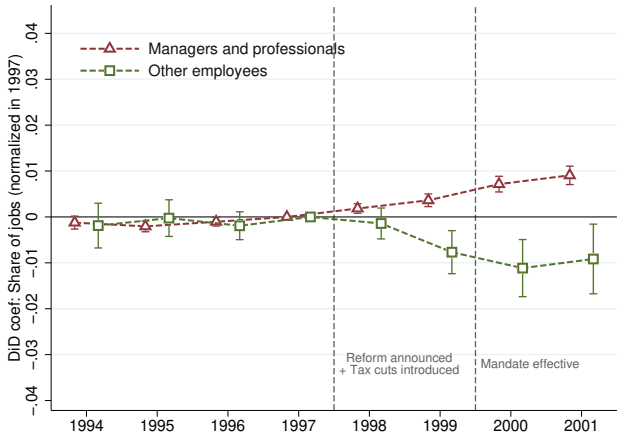
(a) Firm-Level: Weekly Hours



(b) Firm-Level: Hours Share of 1997 Total



(c) Firm-Level: Employment Share of 1997 Total



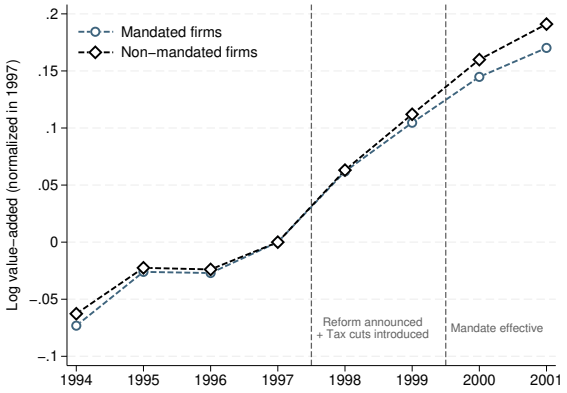
(d) Worker-level: Log Earnings



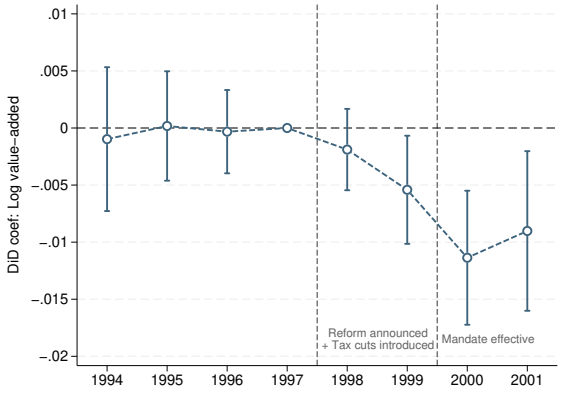
Notes: This figure reports the intent-to-treat effects of the legal mandate on outcomes by skill group. Red triangles correspond to executives, managers, and engineers; green squares to clerks, blue-collar workers, supervisors, and technicians. Panels (a)–(c) report firm-level difference-in-differences estimates of α_τ from Equation (6). Panel (a) reports average weekly hours. Panels (b) and (c) report total hours and employment in each skill group, expressed as a share of the firm's total hours and total employment in 1997, respectively. Panel (d) reports worker-level estimates for log annual earnings, obtained by estimating Equation (6) on the 4% worker panel, with S_i defined by the size of the worker's 1997 employer. Whiskers denote 95% confidence intervals. The firm-level sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 10: Firm-Level Effects of the Mandate: Output and Productivity

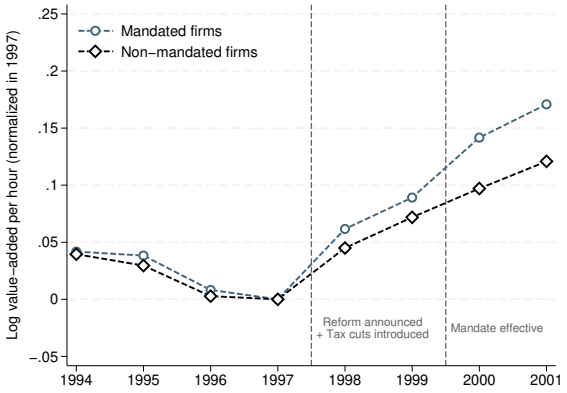
(a) Raw Avg.: Log Value Added



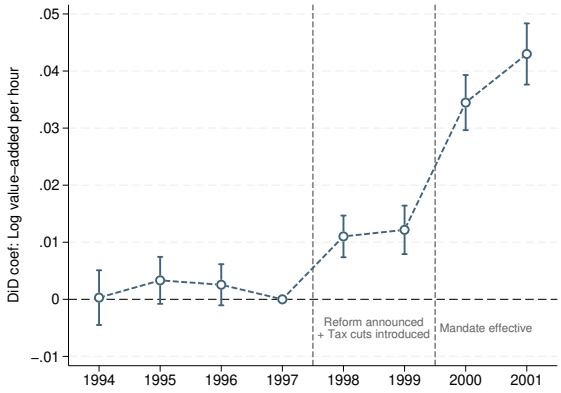
(b) DiD: Log Value Added



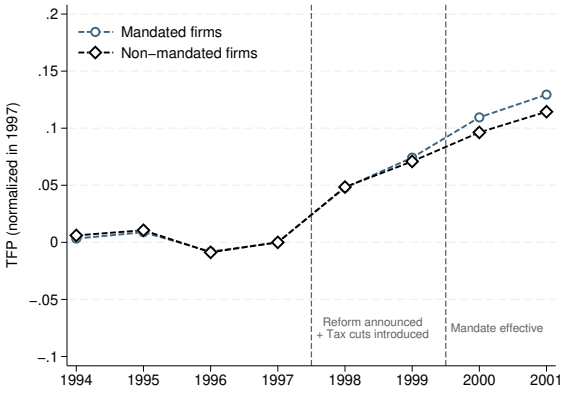
(c) Raw Avg.: Log Value Added per Hour



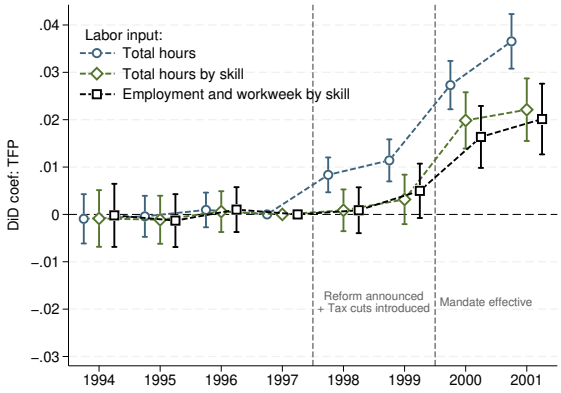
(d) DiD: Log Value Added per Hour



(e) Raw Avg.: TFP

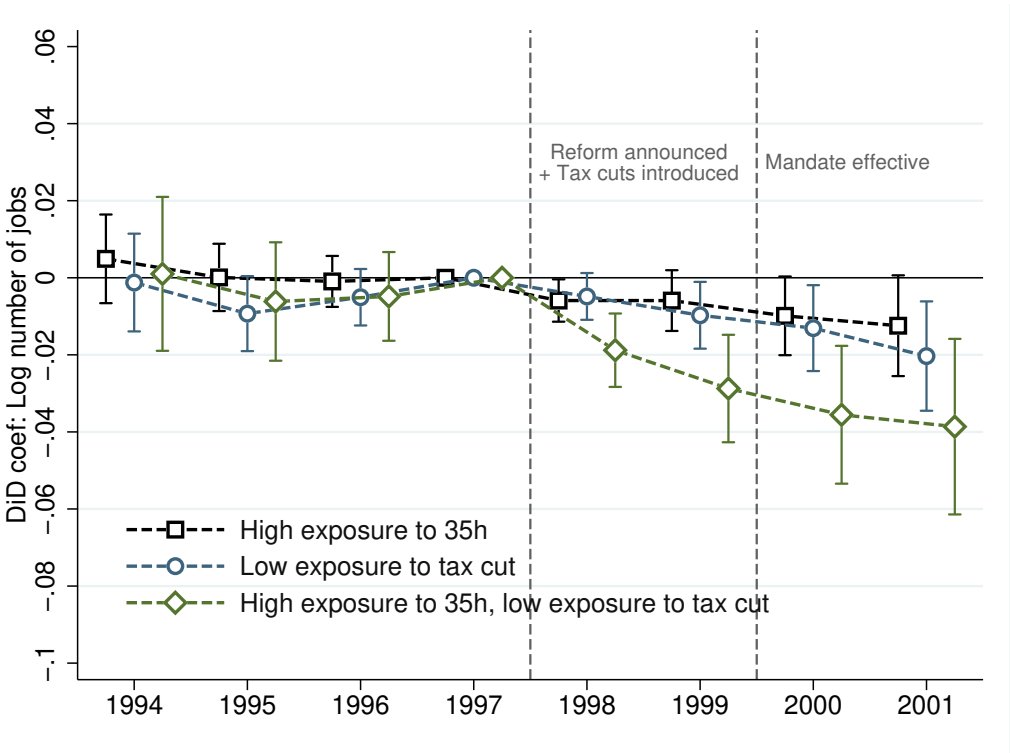


(f) DiD: TFP



Notes: This figure reports the intent-to-treat effects of the legal mandate on firm-level value added (Panels (a)–(b)), value added per hour (Panels (c)–(d)), and total factor productivity (Panels (e)–(f)). The left panels display raw averages by mandate status, normalized to their 1997 values. The right panels show the corresponding difference-in-differences estimates of α_τ from Equation (6). Outcomes are expressed in logs. Total Factor Productivity is estimated using three specifications: (i) using total hours as the free input, (ii) using total hours separately for higher- and lower-skill workers, and (iii) introducing separately average weekly hours and employment. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

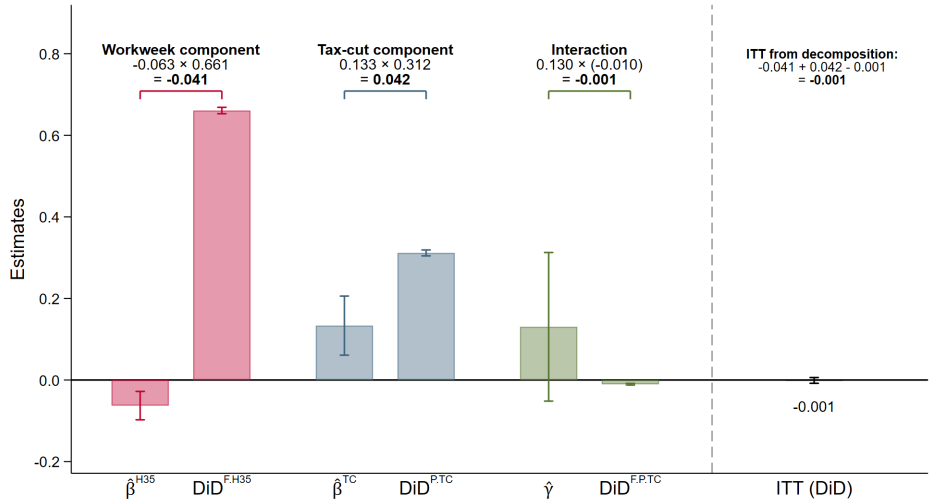
Figure 11: Firm-Level Employment Effects of the Mandate: High Exposure to the Workweek Reduction and Low Exposure to the Tax Cut



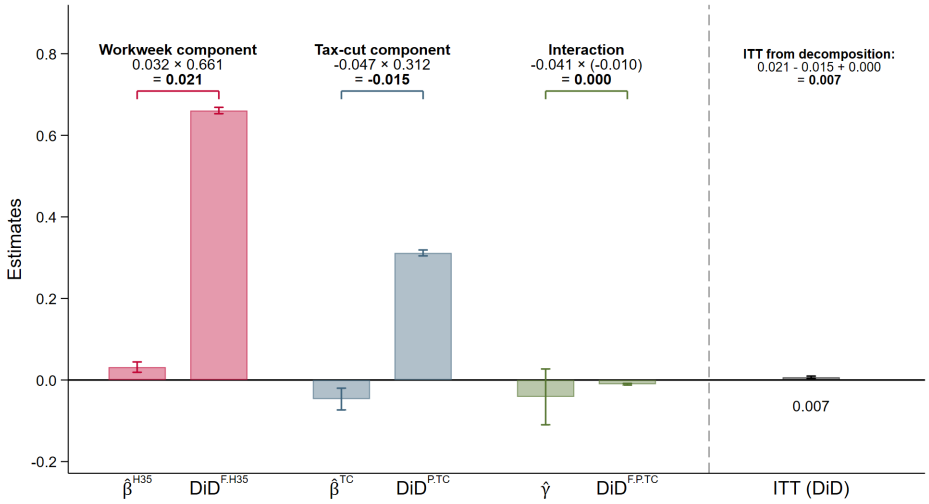
Notes: This figure reports *conditional* intent-to-treat effects of the legal mandate on firm-level employment. We estimate Equation (6) separately across three subsamples defined by pre-reform exposure to the two components of the reform. High exposure to the 35-hour workweek is defined as a 1997 share of full-time jobs in the top 30%, while low exposure to the tax cut is defined as a predicted tax cut, based on the 1997 wage distribution, in the bottom 30%. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure 12: Decomposing the Effects of the Mandate: Workweek Reduction vs. Tax Cut

(a) Log Employment



(b) Log Hourly Labor Cost

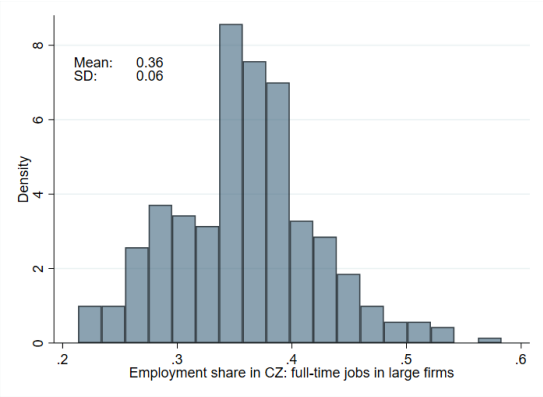


Notes: This figure decomposes the intent-to-treat effect of the legal mandate on firm-level employment (Panel (a)) and hourly labor cost (Panel (b)), following Equation (18). To the left of the vertical dashed line, each pair of bars reports two components: $\hat{\beta}^{H35}$ and $\text{DiD}^{F\times H35}$ for the workweek reduction, $\hat{\beta}^{TC}$ and $\text{DiD}^{P\times TC}$ for the tax cut, and $\hat{\gamma}$ and $\text{DiD}^{F\times P\times TC}$ for their interaction. The product of each pair, reported above the bars, gives that term's contribution to the intent-to-treat effect. The sum of the three contributions gives the intent-to-treat effect implied by the decomposition. To the right of the dashed line, we report both this sum and the intent-to-treat effect estimated directly from Equation (6). All estimates compare outcomes between 1997 and 2001. Whiskers denote 95% confidence intervals. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

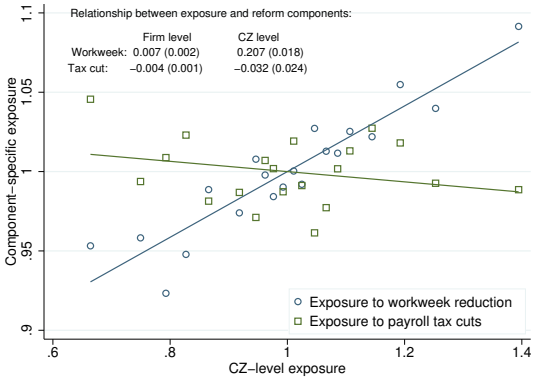
Reading: Operating under the 35-hour regime reduces employment by 6.3%. The mandate increases exposure-weighted adoption by 66.1 percentage points, implying a 4.1% reduction in firm employment attributable to the workweek reduction component.

Figure 13: Commuting Zone-Level Analysis

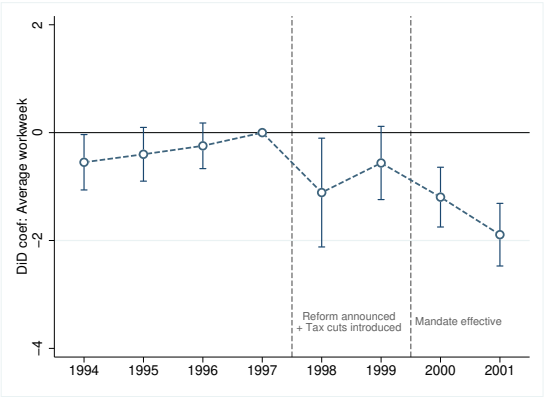
(a) Distribution of CZ-Level Exposure



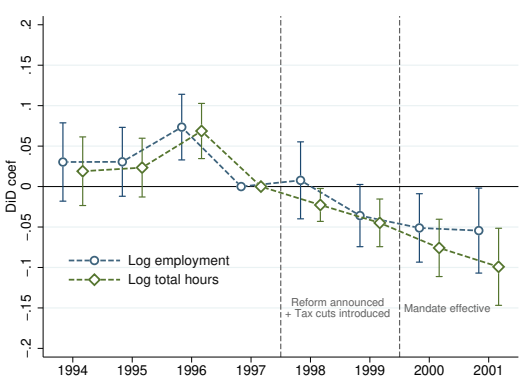
(b) CZ Exposure and Reform Components



(c) Average Weekly Hours



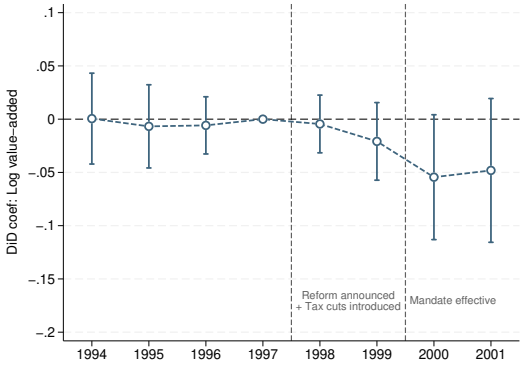
(d) Log Employment and Total Hours



(e) Log Employment by Skill Group



(f) Log Value Added



Notes: This figure presents commuting-zone (CZ) level estimates of the effects of the 35-hour reform. Panel (a) shows the distribution of CZ-level exposure, defined as the 1997 employment share in full-time jobs at firms with more than 20 employees. Exposure is normalized by its mean throughout Panels (b)–(f). Panel (b) relates normalized CZ exposure to average exposure to each reform component, measured by the 1997 share of full-time jobs and the predicted payroll tax cut based on 1997 wages. The slopes from regressions of each component-specific exposure on overall CZ exposure are reported with robust standard errors in parentheses. For comparison, we also report the corresponding slopes using firm-level mandate status, the source of variation underlying the firm-level ITT estimates. Panels (c)–(f) report estimates of ψ_τ from Equation (20). Whiskers denote 95% confidence intervals. CZ-level outcomes are constructed from private-sector establishments in 304 commuting zones in metropolitan France, with no restriction on firm size. Because value added is observed only at the firm level, we exclude multi-region firms for this outcome.

Main Tables

Table 1: Firm-Level Summary Statistics, 1997

	Mandated	Non-mandated	Economy-wide
Workforce:			
Share female	0.32	0.32	0.34
Age < 35	0.44	0.43	0.44
Age 35–50	0.41	0.41	0.40
Age > 50	0.16	0.16	0.16
Average weekly hours	36.65	36.38	36.24
Share > 38.5 hours	0.64	0.64	0.63
Mean hourly wage	10.96	10.83	10.89
Share < 1.73 × min. wage	0.66	0.67	0.66
Skill composition:			
Managers and Professionals	0.10	0.12	0.13
Middle-skill	0.18	0.17	0.18
Lower-skill	0.72	0.71	0.69
Industry composition:			
Manufacturing	0.34	0.26	0.26
Construction	0.12	0.16	0.14
Services	0.54	0.58	0.60
Sample size:			
Total nb. of workers	897,046	498,065	10,713,496
Total nb. of firms	25,161	33,450	233,808

Notes: This table presents firm-level summary statistics in 1997 for three groups: mandated firms with 22–48 employees, non-mandated firms with 10–18 employees, and firms of any size. All columns are restricted to balanced panels of private-sector firms in metropolitan France observed in every year from 1994 to 2001. Variables are measured at the firm level and constructed from matched employer–employee records. Skill groups are defined using workers' occupation codes. Middle-skill occupations include supervisors and technicians, while low-skill occupations include clerks and blue-collar workers; in the main analysis, these two groups are combined. Industry shares refer to the firm's primary sector of activity. The numbers of workers and firms are reported at the bottom of the table.

Table 2: Local Average Treatment Effects of Operating under the 35-Hour Regime (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First stage	Tax cut claim	Workweek	Hourly wage	Hourly labor cost	Total hours	Employment	High-skill employment	Lower-skill employment	Value added	TFP
After x Mandated	0.6301***										
	(0.0037)										
$H35$		0.4740***	-3.1526***	0.0277***	0.0109***	-0.0991***	-0.0024	0.0143***	-0.0151**	-0.0175***	0.0282***
		(0.0057)	(0.0700)	(0.0023)	(0.0023)	(0.0057)	(0.0057)	(0.0016)	(0.0062)	(0.0056)	(0.0053)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cragg Donald F stat		54,392	54,392	54,385	54,385	54,385	54,392	54,392	54,392	52,646	34,419
Kleibergen Paap F stat		28,917	28,917	28,916	28,916	28,916	28,917	28,917	28,917	27,849	17,156
Observations	117,132	117,132	117,132	117,108	117,108	117,108	117,132	117,132	117,132	111,798	63,716

Notes: This table presents two-stage least squares estimates from Equation (11). Column (1) reports the first stage: the effect of the mandate on adoption of the 35-hour regime, $H35$. Columns (2)–(11) report the second-stage effects of adoption on firm-level outcomes: tax-cut take-up, TC (2); average weekly hours (3); log hourly wage (4); log hourly labor cost (5); log total hours (6); log employment (7); high-skill employment as a share of total 1997 employment (8); lower-skill employment as a share of total 1997 employment (9); log value added (10); and Total Factor Productivity (11). Columns (10) and (11) focus on firms observed in the balance sheet data with positive value added. Column (11) further restricts to firms with positive inputs (employment in both skill groups, capital, and intermediates). Estimates contrast 2001 with 1997. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size. Hours are not observed consistently for 12 firms of the sample, which are excluded in columns (4)-(6). Standard errors are clustered at the firm level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Decomposition of Local Average Treatment Effects: Workweek Reduction, Tax Cut, and Interaction

	Employment			Hourly wage			Hourly labor cost			Value added			TFP		
	OLS	2SLS Cont.	2SLS Binned	OLS	2SLS Cont.	2SLS Binned	OLS	2SLS Cont.	2SLS Binned	OLS	2SLS Cont.	2SLS Binned	OLS	2SLS Cont.	2SLS Binned
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
$\widehat{\beta}^{H35}$	0.048***	-0.063***	-0.098***	0.025***	0.030***	0.034***	0.027***	0.032***	0.038***	0.024***	-0.024	-0.042	0.021***	0.050***	0.036
	(0.003)	(0.018)	(0.027)	(0.001)	(0.007)	(0.010)	(0.001)	(0.007)	(0.010)	(0.003)	(0.017)	(0.026)	(0.003)	(0.017)	(0.027)
$\widehat{\beta}^{TC}$	0.122***	0.133***	0.202***	0.018***	-0.011	-0.015	-0.011***	-0.047***	-0.055**	0.040***	0.027	0.043	-0.005	-0.055*	-0.042
	(0.004)	(0.037)	(0.065)	(0.002)	(0.014)	(0.024)	(0.002)	(0.014)	(0.024)	(0.004)	(0.035)	(0.062)	(0.004)	(0.033)	(0.063)
$\widehat{\gamma}$	0.032	0.130	0.353**	-0.005	-0.073**	-0.064	0.008	-0.041	-0.061	0.027	0.168*	0.204	-0.001	-0.101	-0.123
	(0.020)	(0.093)	(0.173)	(0.007)	(0.034)	(0.063)	(0.008)	(0.035)	(0.065)	(0.018)	(0.086)	(0.162)	(0.019)	(0.082)	(0.165)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$(F_i, P_i, F_iP_i)'\mathbb{1}[t \geq 2000]$	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Kleibergen–Paap rk Wald F	—	169.27	34.49	—	169.20	34.30	—	169.20	34.30	—	150.29	30.04	—	111.60	17.81
Hansen J-test (p-value)	—	0.4325	0.9447	—	0.5441	0.6383	—	0.6506	0.5637	—	0.8938	0.7541	—	0.9650	0.2615
Observations	117,132	117,132	43,732	117,108	117,108	43,712	117,108	117,108	43,712	98,238	98,238	36,394	63,062	63,062	22,720

Notes: This table reports estimates from Equation (17), decomposing the local average treatment effect of 35-hour regime into three components: the effect of 35-hour regime absent the subsidy, the additional effect of the subsidy, and their interaction. We report results for the firm-level outcomes listed in the first row: log employment, log hourly wage, log hourly labor cost, log value added, and TFP. For each outcome, the first sub-column reports OLS estimates, while the second and third report two-stage least squares estimates. The second sub-column uses continuous instruments based on the 1997 full-time share, the predicted tax cut based on 1997 wages, and their interaction. The third uses binned instruments based on the top and bottom 30% of the corresponding exposure distributions. The 2SLS specifications control for interactions between the exposure measures and the post-reform indicator, using continuous exposure measures with the continuous instruments and the corresponding indicators with the binned instruments. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size. The value added and TFP columns are restricted to firms observed in the balance sheet data with positive value added. The TFP columns further require positive inputs (employment in both skill groups, capital, and intermediates). Hours are not observed consistently for 12 firms of the sample, which are excluded in columns "Hourly wage" and "Hourly labor cost". Estimation is based on years 1997 and 2001. Standard errors are clustered at the firm level and reported in parentheses. Kleibergen–Paap F statistics and p-values for the J tests are reported at the bottom of the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

The Effects of a Shorter Workweek on Employment, Output, and Productivity

Pauline Carry, Claire Montialoux, Elio Nimier-David,
Alexandra Roulet, Nina Roussille

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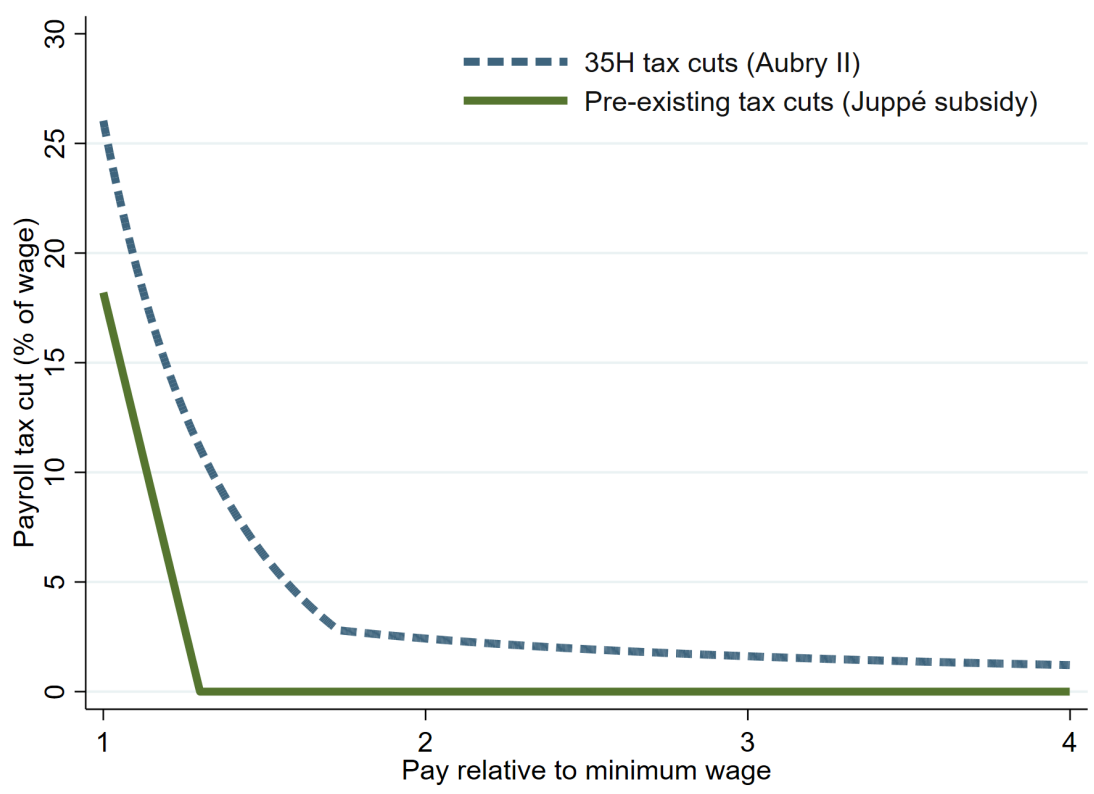
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A Institutional Appendix

A.1 Payroll Tax Reductions

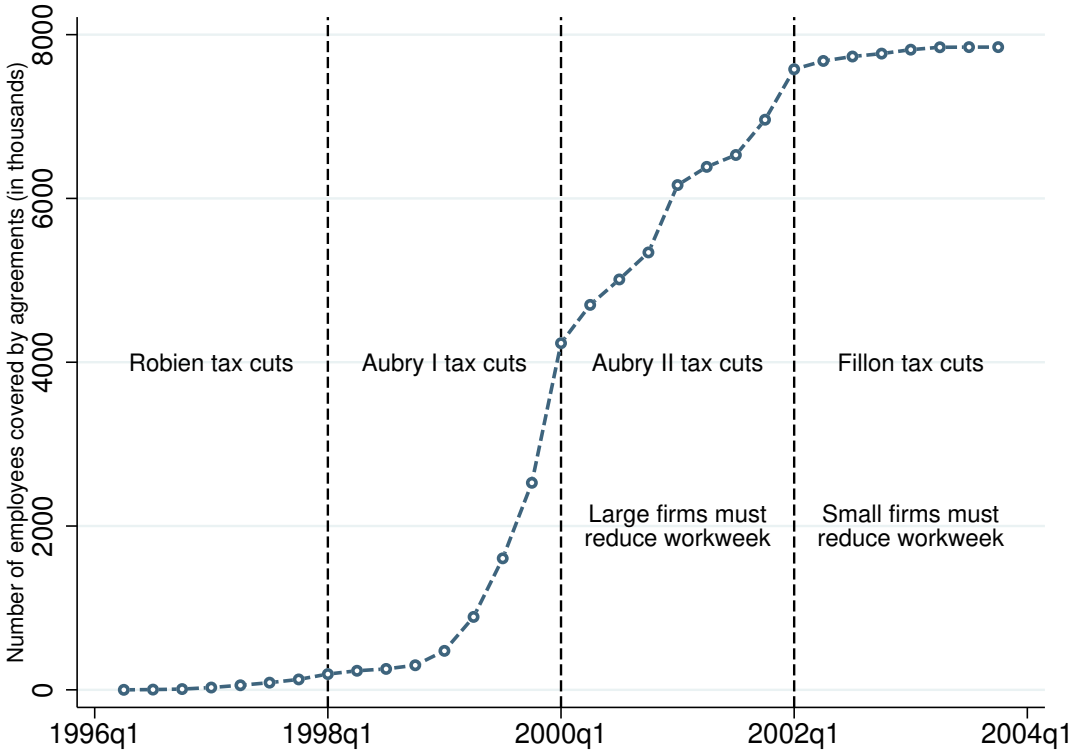
Figure A.1: Payroll Tax Cut Schedule Pre- and Post-Reform



Notes: This figure shows the payroll tax cut schedule associated with the reform. The horizontal axis is the worker's full-time-equivalent wage, normalized by the full-time minimum wage. The vertical axis reports the tax cut as a percentage of the wage. The solid line shows the pre-existing tax cut in place in 1997, which was independent of working time and available to all firms. The dashed line shows the Aubry II tax cut schedule, conditional on adoption of the 35-hour workweek. Figure 1 reports the difference between the two.

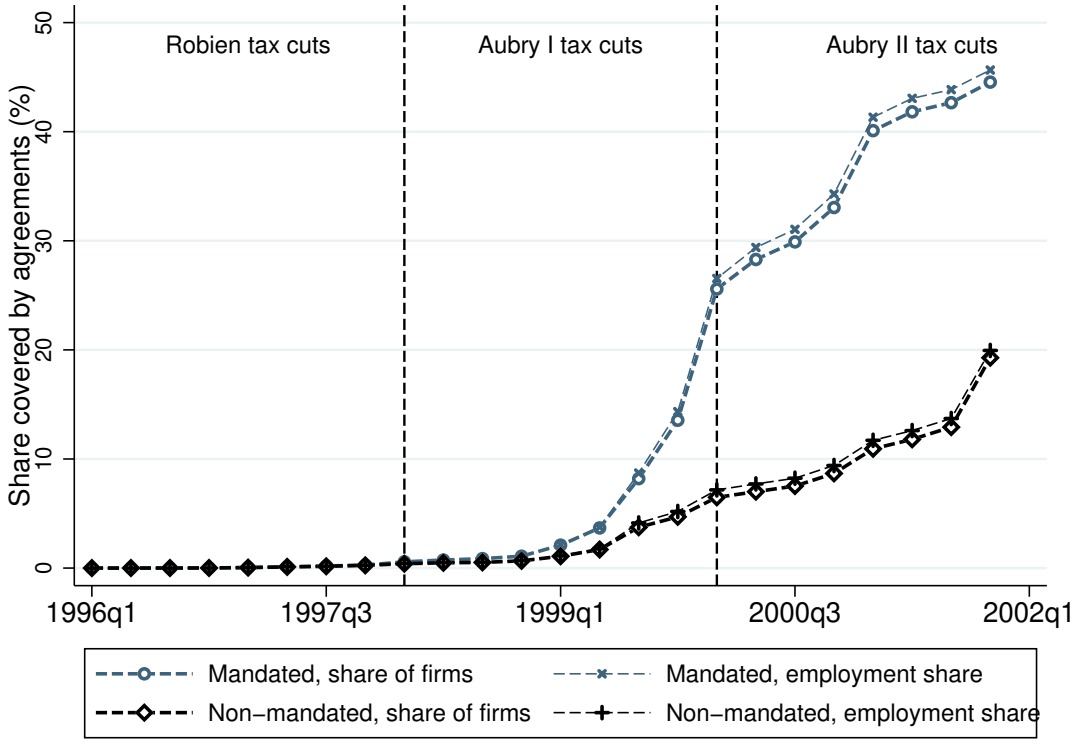
A.2 Workweek Reduction Agreements

Figure A.2: Workweek Reduction Agreements Used to Claim Tax Cuts



Notes: This figure shows the cumulative number of employees covered by firm-level workweek reduction agreements used to claim the associated payroll tax cuts, by implementation date, computed from administrative records. Vertical dashed lines indicate key legislative milestones under the Robien, Aubry, and Fillon reforms, including the introduction of payroll tax incentives and the phased compliance deadlines for large and small firms.

Figure A.3: Workweek-Reduction Agreements in Our Estimation Sample



Notes: This figure shows the cumulative shares of firms and employees covered by firm-level workweek reduction agreements over time in the estimation sample. Results are reported separately for mandated and non-mandated firms. Vertical dashed lines indicate the introduction of the Robien, Aubry I, and Aubry II payroll tax cuts. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

A.3 International Comparisons

Table A.1: Cross-country Comparison of Workweek Reduction Policies

Country	Workweek	Same pay	Payroll tax cuts
Australia (1983)	40→38h	Yes	No
Portugal (1996)	44→40h	Yes	No
Italy (1997)	48→40h	Yes	No
France (1998)	**39→35h**	**Yes**	**Yes**
Belgium (2001)	40→38h	Yes	Yes
South Korea (2003)	44→40h	Yes	No
Venezuela (2012)	44→40h	Yes	No
Israel (2018)	43→42h	Yes	No
Colombia (2021)	48→42h	Yes	No
Chile (2023)	45→40h	Yes	No
Mexico (2026)	48→40h	Yes	No

Notes: This table summarizes several workweek reduction reforms implemented across countries. The first column reports the year of legislation; implementation may occur later or be phased in over time. The second column reports the change in statutory working time, the third indicates whether pay preservation is required, and the final column indicates whether the reform is accompanied by a fiscal policy aimed at reducing employer costs.

A.4 Computation of Employer Payroll Taxes

This appendix documents how we compute job-level employer payroll taxes.[39] We build on the tax microsimulation code of Bozio et al. (2025), which computes employer and employee payroll contributions and associated tax relief from 1925 to 2018, using observed characteristics such as wages and social security regime. Because the workweek reduction reform is not their focus, their code makes simplifying assumptions about the subsidies attached to it. We extend the code to compute these subsidies, exploiting the fact that we observe actual tax cut claims in the administrative data on workweek reduction agreements. Our main additions are (i) the Robien subsidy of 1996, (ii) the Juppé tax cut from October 1996 to end-1997, (iii) the Aubry I payroll tax cuts, and (iv) the Aubry II payroll tax cuts.

A.4.1 Tax Formulas Over Our Sample Period

Balladur Scheme (July 1993–1996). Employers benefited from a reduction in family social security contributions targeted at low wages: a full exemption of 5.4 percentage points of the gross wage for very low wages, and a partial exemption equal to half of that amount for low wages, with eligibility thresholds changing over time. Let w denote the gross wage, MW the annual minimum wage, and pr the full-time equivalent factor, and define the wage relative to the minimum wage as $z = \frac{w}{pr \cdot MW}$. In

[39] We thank Antoine Bozio for sharing the microsimulation codes ahead of their publication and Jade Sillère for research support on this appendix and the corresponding code to compute payroll taxes.

1994, for example, the reduction $S^F(w)$ is

$$S^F(w,z) = \begin{cases} \tau_1 \cdot w & \text{if } z < 1.1, \\ \tau_2 \cdot w & \text{if } 1.1 \le z \le 1.2, \\ 0 & \text{if } z > 1.2, \end{cases} \tag{21}$$

where τ_1 is the full exemption and $\tau_2 = \frac{1}{2} \cdot \tau_1$ the partial exemption. On 1 January 1995, the middle bracket was shifted from $[1.1, 1.2]$ to $[1.2, 1.3]$ times the minimum wage.

Juppé Scheme (September 1995–June 2003). From 1 September 1995 to the end of September 1996, the Juppé scheme introduced a linearly degressive reduction in health insurance contributions for wages up to 1.2x the monthly minimum wage, with a maximum reduction of 12.8 percentage points at the minimum wage. From 1 October 1996 to 31 December 1997, the Balladur family allowance exemption and the Juppé scheme were merged into a single linearly degressive reduction (the final Juppé scheme), equal to 18.2% of contributions at the minimum wage and phasing out at 1.33x the minimum wage; unlike before, the reduction was no longer prorated for part-time work. From 1 January 1998 to 30 June 2003, the same scheme applied with minor modifications, the phase-out threshold being lowered to 1.3x the minimum wage, until it was replaced by the Fillon tax cuts in 2003. In 1995, for example, the Juppé reduction is

$$S^F(w, MW, pr) = \begin{cases} \tau_{J,1} \dfrac{1.2 \cdot pr \cdot \mathrm{MW} - w}{0.2 \cdot pr \cdot \mathrm{MW}} w & \text{if } pr\,\mathrm{MW} \le w \le 1.2 \cdot pr \cdot \mathrm{MW}, \\ 0 & \text{if } w > 1.2 \cdot pr \cdot \mathrm{MW}, \end{cases}$$

where MW is the full-time monthly minimum wage, pr the prorata factor reflecting part-time intensity ($pr = 1$ for full-time workers), and $\tau_{J,1} = 12.8\%$ the statutory reduction rate.

Robien Scheme (1996). The Loi Robien introduced a temporary reduction in employer social security contributions for firms voluntarily reducing working time by at least 10% for all or part of their workforce, either to create jobs (*offensive* agreements, with a hiring commitment of 10% or 15% of the workforce) or to avoid layoffs (*defensive* agreements). It was granted for seven years, with a higher rate in the first year. For a worker with gross wage w and employer social security contributions $C(w)$, the reduction in year t of participation is $S^F(w, \Delta h, t) = \tau(\Delta h, t)\, C(w)$, where Δh is the proportional reduction in hours and $\tau(\Delta h, t)$ the rate in Table A.2: 40% in the first year and 30% thereafter for a 10–15% reduction, 50% and 40% for a reduction of 15% or more.

Aubry I (June 1998). Aubry I provided a temporary lump-sum subsidy, in the form of a flat-rate reduction in employer contributions, to firms that adopted the 35-hour week early by signing a collective

Table A.2: Robien Payroll Tax Cut

Workweek reduction	Rate in Year 1	Rate in Years 2–7
$0.10 \leq \Delta h < 0.15$	0.40	0.30
$\Delta h \geq 0.15$	0.50	0.40

agreement before their statutory deadline. The annual amount per employee, denoted $S^F(t)$ (in French francs), depended on the signing date t and the type of agreement. It was granted for five years from the effective date of the workweek reduction. Table A.3 reports the first-year amounts; in each of the following four years, the amount fell by 1,000 FRF. The aid was prorated by the worker's full-time equivalent share. *Basic* agreements involved a 6% job creation commitment, *Ambitious* agreements a 9% commitment.

Table A.3: Aubry I: First-Year Temporary Subsidy

Agreement signing period	Basic	Ambitious
$t <$ 1 Jul 1999	9,000	13,000
1 Jul 1999 $\leq t <$ 1 Jan 2001	7,000	11,000
1 Jan 2001 $\leq t <$ 1 Jan 2002	6,000	10,000

Note: Nominal statutory parameters in force during each period, in French francs.

Aubry II (January 2000). Aubry II introduced a permanent subsidy, again as payroll tax cuts, for firms implementing the statutory 35-hour week under a collective agreement. It combined a lump-sum per-employee subsidy, independent of the wage, with an additional reduction targeted at low wage workers, applying up to 1.73x the minimum wage and degressive in the wage, with its maximum at the minimum wage. The annual payroll tax cut is

$$S^F(MW, w, a, b, c, pr) = \max\left(a \cdot pr \cdot \frac{MW \cdot pr}{w} - b \cdot pr,\ c \cdot pr\right), \tag{22}$$

where a, b, and c are policy parameters governing the level, slope, and floor of the schedule. Their values are reported in Table A.4.

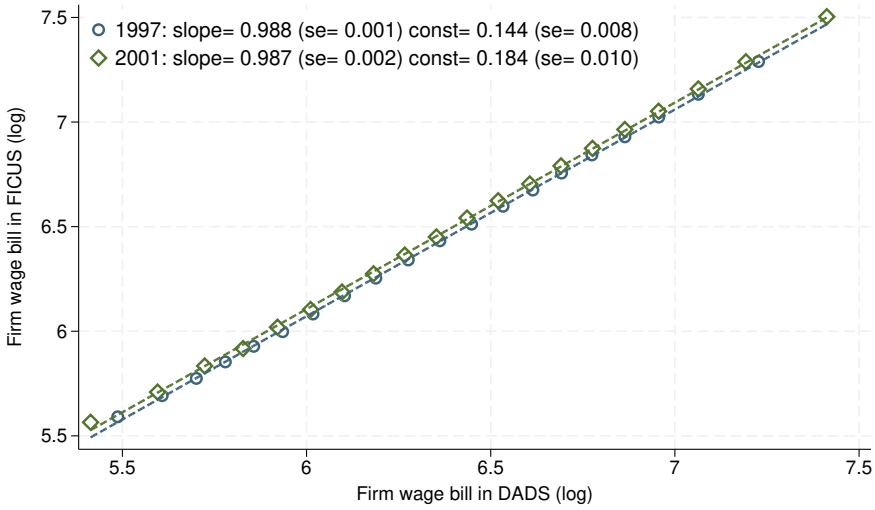
Table A.4: Parameters for the Aubry II Permanent Annual Subsidy (Nominal Amounts)

Period applicable	Unit	Minimum wage	*a*	*b*	*c*
1 Feb 2000–30 Jun 2000	FRF	6 881.68	41 500.0	20 000.0	4 000.0
1 Jul 2000–30 Jun 2001	FRF	7 101.38	42 824.9	20 638.5	4 127.7
1 Jul 2001–30 Jun 2002	FRF	7 388.68	44 557.1	21 473.4	4 294.7

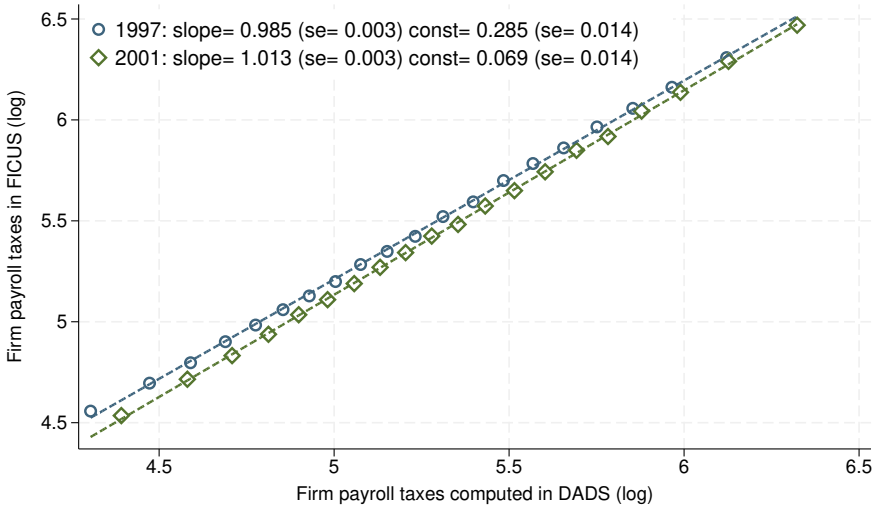
Note: Nominal statutory parameters in force during each period, in French francs.

Figure A.4: Verification of Employer Payroll Tax Calculations

(a) Wage Bill



(b) Employer Payroll Taxes



Notes: This figure compares firm-level wage bills and employer payroll taxes across the matched employer–employee data (DADS) and firm balance-sheet data (FICUS), before (1997) and after (2001) the reform. Panel (a) compares log total wage bills, which are observed in both datasets. Panel (b) compares log employer payroll taxes observed in FICUS with those we compute using the method described in Appendix A.4, combining DADS with information on workweek reduction agreements. Dashed lines show OLS fits of the FICUS measure on the corresponding DADS-based measure. Estimated slopes and intercepts, with standard errors, are reported in the legend.

A.5 Translated Excerpts from Workweek Reduction Agreements

This appendix reproduces the provisions on part-time employees contained in a selection of collective agreements publicly available implementing the 35-hour workweek. Each excerpt is shown in the original French, followed by our English translation. "RTT" (*réduction du temps de travail*) refers to the working-time reduction.

4 May 1999

Original :

Les salariés à temps partiel bénéficient du régime de la réduction du temps de travail dans les mêmes conditions que les salariés à temps complet. Les salariés à temps partiel auront le choix entre :

1. le maintien de leur contrat de travail avec une augmentation de salaire en référence au nouveau taux horaire ;
2. une diminution de 10 % de leur temps de travail sans changement de rémunération ;
3. la revalorisation de leur contrat de travail si la situation de l'entreprise le permet.

Translation:

Part-time employees benefit from the working-time reduction scheme under the same conditions as full-time employees. Part-time employees will have the choice between:

1. keeping their employment contract with a pay increase based on the new hourly rate;
2. a 10% reduction in their working time with no change in pay;
3. an upward revision of their employment contract if the company's situation allows.

1 December 1999

Original :

Afin de garantir l'égalité de traitement entre les salariés bénéficiant d'une réduction de temps de travail, la transposition du maintien du salaire brut base 39 heures se traduit :

- soit par une augmentation correspondante du salaire du titulaire d'un contrat de travail à temps partiel en cas de maintien de la durée contractuelle, ce qui correspond à une création d'heures ;
- soit par la réduction de la durée contractuelle du travail par accord entre l'employeur et le salarié, ce qui contribue à favoriser l'emploi ;
- soit par une solution mixte équivalente.

Translation:

In order to guarantee equal treatment among employees benefiting from a working-time reduction, the maintenance of gross pay on a 39-hour basis is transposed:

- either through a corresponding increase in the pay of the holder of a part-time contract if contractual hours are maintained, which amounts to the creation of hours;
- or through a reduction in contractual working hours by agreement between the employer and the employee, which helps promote employment;
- or through an equivalent mixed solution.

8 November 2000

Original :

Cet accord s'applique à l'ensemble des salariés des entreprises et des établissements de la branche, quels que soient leurs contrats de travail et leur ancienneté, y compris les cadres et les salariés à temps partiel. Les salariés titulaires d'un contrat de travail à temps partiel bénéficient des droits reconnus aux salariés à temps complet par la loi, le présent accord, les conventions et accords collectifs de la branche, d'entreprise ou d'établissement.

Translation:

This agreement applies to all employees of the companies and establishments of the industry, whatever their employment contract or seniority, including managerial staff and part-time employees. Employees holding a part-time employment contract benefit from the rights granted to full-time employees by law, by this agreement, and by industry-, company- or establishment-level collective agreements.

20 April 2000

Original :

Cependant, que ce soit par accord d'entreprise ou en l'absence d'accord, le salarié demeurant à temps partiel sans modification d'horaires bénéficie d'une majoration de sa rémunération afin de tenir compte des modifications apportées au statut du personnel à temps plein.

Translation:

However, whether under a company-level agreement or in the absence of one, an employee who remains part-time without any change in hours receives a pay increase to take account of the changes made to the status of full-time staff.

25 January 2001

Original :

Parmi les diverses formes d'organisation du temps de travail, le temps partiel a été choisi par de nombreux salariés. Ces personnes travaillent souvent déjà moins de 35 heures effectives, en moyenne, par semaine. Conformément à l'accord-cadre, les parties soussignées conviennent qu'elles bénéficient d'une réduction du temps de travail au même titre que les salariés à temps plein. Les personnes à temps partiel disposent d'un contrat individuel qui précise les conditions de ressources et les modalités d'exercice de leur activité pour une durée inscrite audit contrat. Ces personnes se verront proposer une réduction de leur durée du travail proportionnelle à la réduction du temps de travail bénéficiant aux salariés à temps plein, à situation initiale identique et suivant les mêmes régimes de travail. Cette réduction sera compensée dans les mêmes conditions que celles pratiquées pour les salariés à temps plein. En accord avec leur hiérarchie, ces personnes pourront choisir de conserver leur horaire de travail à temps partiel. Dans ce cas, leur rémunération sera adaptée proportionnellement, conformément aux dispositions applicables par ailleurs.

Translation:

Among the various forms of working-time organization, part-time work has been chosen by many employees. These employees often already work fewer than 35 actual hours per week on average. In accordance with the framework agreement, the signatory parties agree that they benefit from a working-time reduction on the same basis as full-time employees. Part-time employees have an individual contract specifying their pay conditions and the terms under which they carry out their activity, for a duration set out in that contract. They will be offered a reduction in their working hours proportional to the working-time reduction granted to full-time employees, for an identical initial situation and under the same working arrangements. This reduction will be compensated under the same conditions as those applied to full-time employees. In agreement with their line management, they may choose to keep their part-time working hours. In that case, their pay will be adjusted proportionally, in accordance with the provisions applicable elsewhere.

15 June 1999

Original :

Sont considérés comme salariés à temps partiel les salariés dont l'horaire contractuel est inférieur à 80 % du temps plein. Afin de garantir l'égalité de traitement entre les salariés bénéficiant d'une réduction de temps de travail, la transposition du maintien du salaire contractuel brut base 39 heures se traduit :

- soit par une augmentation correspondante du salaire du titulaire d'un contrat de travail à temps partiel en cas de maintien de la durée contractuelle, ce qui correspond à une création d'heures ;
- soit par la réduction de la durée contractuelle du travail par accord entre l'employeur et le salarié, ce qui contribue à favoriser l'emploi ;
- soit par une solution mixte équivalente.

Translation:

Employees whose contractual hours are below 80% of full-time hours are considered part-time employees. In order to guarantee equal treatment among employees benefiting from a working-time reduction, the maintenance of contractual gross pay on a 39-hour basis is transposed:

- either through a corresponding increase in the pay of the holder of a part-time contract if contractual hours are maintained, which amounts to the creation of hours;
- or through a reduction in contractual working hours by agreement between the employer and the employee, which helps promote employment;
- or through an equivalent mixed solution.

23 January 2001

Original :

Les salariés à temps partiel ont le choix entre les deux situations suivantes :

- leur temps de travail est réduit proportionnellement à celui des salariés à temps complet, base 37 heures passant à 35 heures ;
- les salariés à temps partiel (hors salariés en préretraite progressive) base 35 heures qui souhaiteraient maintenir leur horaire contractuel verraient alors leur nouveau salaire de base revalorisé en conséquence.

[…] Par ailleurs, tous les salariés à temps partiel bénéficient de l'octroi d'une semaine de congés en RTT.

Translation:

Part-time employees may choose between the following two situations:

- their working time is reduced in proportion to that of full-time employees, whose basis moves from 37 to 35 hours;
- part-time employees (excluding those in phased early retirement) on a 35-hour basis who wish to keep their contractual hours would then see their new base pay revised upward accordingly.

[…] In addition, all part-time employees are granted one week of leave as RTT.

13 September 2000

Original :

Les salariés travaillant à temps partiel au moment de l'entrée en vigueur du présent accord conservent leur pourcentage d'activité par rapport à la nouvelle durée légale du travail et le type de répartition de leur temps partiel (hebdomadaire, mensuel, annuel). Ils bénéficient d'une réduction du temps de travail proportionnelle à celle accordée aux collaborateurs à temps plein avec maintien de leur rémunération. Cette RTT prend la forme de 8 jours de repos à leur disposition (au-delà des jours de congés annuels, des jours fériés et des jours au titre des repos collectifs sous forme de « ponts ») et d'une réduction complémentaire de 5,5 %, celle-ci appelant une modification de la répartition du temps de travail prévue par l'avenant à leur contrat de travail.

Translation:

Employees working part-time when this agreement comes into force keep their percentage of activity relative to the new statutory working time, as well as the type of distribution of their part-time hours (weekly, monthly, annual). They benefit from a working-time reduction proportional to that granted to full-time staff, with their pay maintained. This RTT takes the form of 8 rest days at their disposal (in addition to annual leave, public holidays and collective rest days in the form of "bridge days") and of an additional reduction of 5.5%, the latter requiring a change in the distribution of working time set out in the amendment to their employment contract.

18 January 2001

Original :

Les salariés à temps partiel bénéficient du régime de la réduction du temps de travail dans les mêmes proportions et selon les mêmes conditions que les salariés à temps complet.

Translation:

Part-time employees benefit from the working-time reduction scheme in the same proportions and under the same conditions as full-time employees.

8 January 2001

Original :

Les salariés à temps partiel se verront proposer une réduction proportionnelle et un réaménagement de leur temps de travail contractuel. Ils bénéficieront d'un maintien de rémunération dans la même proportion et selon les mêmes modalités que les salariés à temps plein. Ils pourront également opter pour un maintien de leur durée actuelle de travail accompagné d'une revalorisation proportionnelle de leur rémunération.

Translation:

Part-time employees will be offered a proportional reduction and a reorganization of their contractual working time. Their pay will be maintained in the same proportion and under the same terms as for full-time employees. They may also opt to keep their current working hours, together with a proportional upward revision of their pay.

12 October 2000

Original :

Les présentes mesures de réduction du temps de travail (horaires et annuelles) seront appliquées aux salariés à temps partiel dans les mêmes conditions que celles appliquées aux salariés à temps plein de leur unité d'activité, service, guichet, avec un prorata correspondant à leur régime de travail à temps partiel.

Translation:

The present working-time reduction measures (hourly and annual) will be applied to part-time employees under the same conditions as those applied to the full-time employees of their business unit, department or counter, prorated according to their part-time working arrangements.

13 December 2000

Original :

Les salariés à temps partiel au moment de la mise en vigueur de cet accord doivent opter pour l'une ou l'autre des solutions présentées ci-après :

- passer à temps complet, dans les mêmes conditions que les autres salariés à temps complet de leur catégorie ;
- réduire leur temps de travail dans la même proportion que les salariés à temps plein avec maintien de la rémunération, en optant pour la formule la plus proche du tableau en annexe.

Translation:

Employees working part-time when this agreement comes into force must choose one of the following solutions:

- switch to full-time, under the same conditions as the other full-time employees in their category;
- reduce their working time in the same proportion as full-time employees, with pay maintained, by opting for the closest formula in the appended table.

Date unknown

Original :

Les collaborateurs à temps partiel sont a priori concernés par l'une des deux solutions suivantes :

- diminuer proportionnellement leur temps de travail, à salaire constant. Cela peut se traduire par l'obtention d'un prorata du nombre de jours disponibles si le collaborateur relève des modalités de réalisation de missions, ou par une réduction du temps de travail journalier pour un collaborateur relevant des modalités standard ;
- augmenter proportionnellement leur salaire à durée de travail constante.

Translation:

Part-time staff are, in principle, concerned by one of the following two solutions:

- reduce their working time proportionally, at constant pay. This may take the form of a prorated number of available days for staff covered by the project-based arrangements, or of a reduction in daily working time for staff covered by the standard arrangements;
- increase their pay proportionally, at constant working time.

Date unknown

Original :

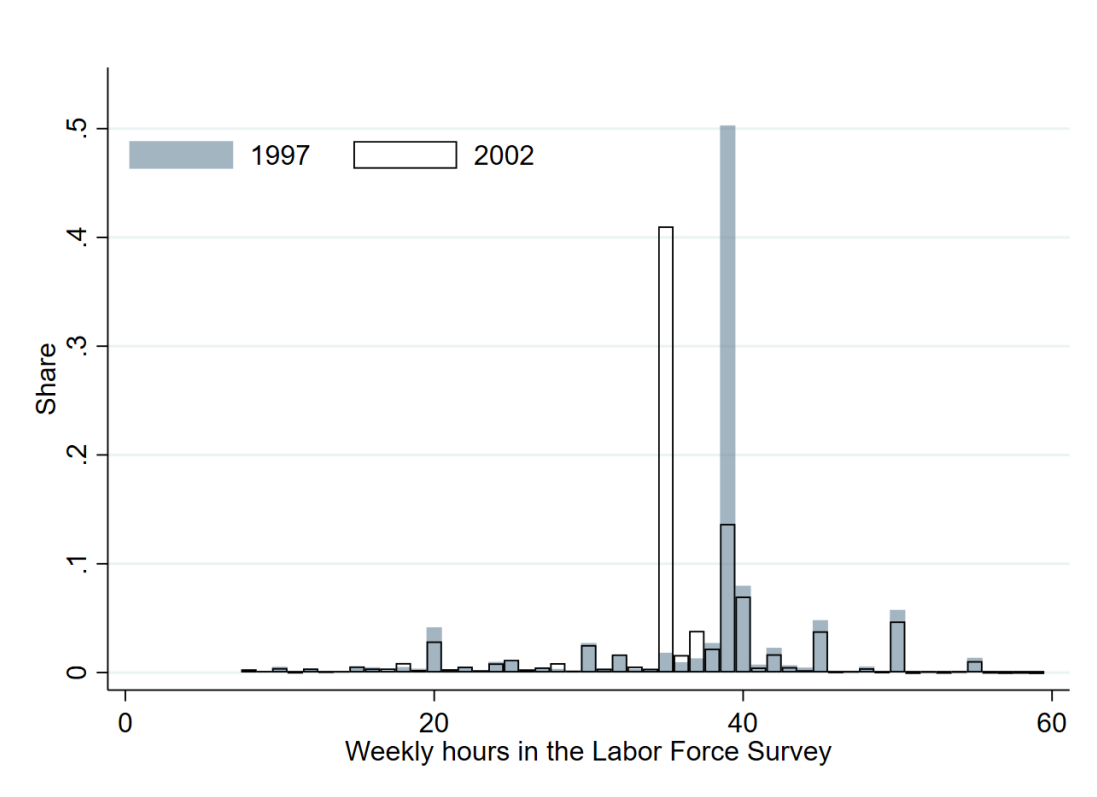
Dans ce cas, les salariés à temps partiel se créent proportionnellement des droits à RTT. Ils bénéficieront également du maintien de la rémunération de base actuelle, ancienneté comprise, prévu pour les salariés à temps plein.

Translation:

In that case, part-time employees accrue RTT rights proportionally. They will also benefit from the maintenance of current base pay, including seniority, provided for full-time employees.

A.6 Additional Descriptive Evidence on the Workweek Reduction

Figure A.5: Distribution of Weekly Hours in Labor Force Survey



Notes: This figure shows the distribution of weekly hours using worker reports from the French Labor Force Survey (LFS) in 1997 and 2002. It provides the worker-reported counterpart to Figure 2, which uses employer-reported hours. We use 2002 rather than 2003 for comparability because the LFS underwent a major redesign in 2003. The sample includes private-sector workers aged 18–60.

B Technical Appendix

This appendix states formally the assumptions and proves the identification results discussed in Section 4.

B.1 Assumptions

We restate (A1)–(A10) formally and add three auxiliary conditions, (A1^H), (A3^H), and (A3^{TC}), which apply the no-anticipation and parallel-trends assumptions to adoption and take-up rather than to outcomes.

Assumption A1 (No anticipation). $Y_{i0}(1) = Y_{i0}(0)$ almost surely.

Assumption A1^H (No anticipation in adoption). $H35_{i0}(1) = H35_{i0}(0)$ almost surely.

Assumption A2 (SUTVA). Potential outcomes and potential treatments of firm i depend on its own mandate status only.

Assumption A3 (Parallel trends). $\mathbb{E}[Y_{it}(0) - Y_{i0}(0) \mid S_i = 1] = \mathbb{E}[Y_{it}(0) - Y_{i0}(0) \mid S_i = 0]$.

Assumption A3^H (Parallel trends in adoption). $\mathbb{E}[H35_{it}(0) - H35_{i0} \mid S_i = 1] = \mathbb{E}[H35_{it}(0) - H35_{i0} \mid S_i = 0]$.

Assumption A3^{TC} (Parallel counterfactual take-up). $\mathbb{E}\big[TC_{it}\big(H35_{it}(0)\big) \mid S_i = 1\big] = \mathbb{E}\big[TC_{it}\big(H35_{it}(0)\big) \mid S_i = 0\big]$.

Because $TC_{i0} = 0$ for every firm, the trend condition reduces to equality of counterfactual take-up levels under no mandate. Voluntary adoption and take-up may rise in both groups after the reform, since the fiscal incentive makes adoption more attractive; these assumptions require only that the responses be common across groups.

Assumption A4 (Relevance). $\pi_t^C > 0$.

Assumption A5 (Exclusion). Almost surely, for all $t' \in \{0, t\}$ and $s \in \{0, 1\}$,

$$Y_{it'}(s) = Y_{it'}\big(H35_{it'}(s),\, TC_{it'}(H35_{it'}(s))\big).$$

Assumption A6 (Monotonicity). $H35_{it}(1) \geq H35_{it}(0)$ almost surely.

Assumption A7 (Mandate-invariance of tax-cut take-up). Potential tax cut take-up depends on mandate status only through 35-hour regime: for each $h35 \in \{0, 1\}$, $TC_{it}(h35)$ is the same whether or not firm i is mandated.

Let $X_i = (F_i, P_i)$ denote the predetermined exposure measures of Section 4.4, with support $\mathcal{X}$, and let $B_i = (B_i^F, B_i^P) \in \{0, 1\}^2$ denote the binned exposure groups.

Assumption A8 (Conditional parallel trends). (A3), (A3^H), and (A3^{TC}) hold conditional on $X_i = x$, for every $x \in \mathcal{X}$.

Assumption A9 (Overlap and conditional relevance). For every $x \in \mathcal{X}$, $0 < \mathbb{P}(S_i = 1 \mid X_i = x) < 1$ and $\pi_t^C(x) \equiv \mathbb{P}(C_{it} = 1 \mid S_i = 1, X_i = x) > 0$.

When (A8) and (A9) are invoked with B_i in place of X_i, the conditioning event is $B_i = b$.

Assumption A10 (Linear exposure effects). For all $x = (f, p) \in \mathcal{X}$,

$$\mathbb{E}\big[\tau_{it}^{H35} \mid X_i = x,\ S_i = 1,\ C_{it} = 1\big] = \beta_t^{H35}\, f,$$
$$\mathbb{E}\big[\tau_{it}^{TC} \mid X_i = x,\ S_i = 1,\ C_{it} = 1,\ TC_{it}(1) = 1\big] = \big(\beta_t^{TC} + \gamma_t f\big)\, p,$$

where $\tau_{it}^{H35} = Y_{it}(1,0) - Y_{it}(0,0)$ and $\tau_{it}^{TC} = Y_{it}(1,1) - Y_{it}(1,0)$ are defined in (8).

B.2 Object 1: The Intent-to-Treat Effect of the Mandate

Proposition B.1. *Under (A1)–(A3),* $\mathrm{DiD}_t^Y = \Delta_t^{ITT}$.

Proof. By (A2), $Y_{it} = Y_{it}(S_i)$, so (5) reads

$$\mathrm{DiD}_t^Y = \big(\mathbb{E}[Y_{it}(1) \mid S_i = 1] - \mathbb{E}[Y_{i0}(1) \mid S_i = 1]\big) - \big(\mathbb{E}[Y_{it}(0) \mid S_i = 0] - \mathbb{E}[Y_{i0}(0) \mid S_i = 0]\big).$$

By (A1), $\mathbb{E}[Y_{i0}(1) \mid S_i = 1] = \mathbb{E}[Y_{i0}(0) \mid S_i = 1]$. This implies

$$\mathrm{DiD}_t^Y = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid S_i = 1] + \Big(\mathbb{E}[Y_{it}(0) - Y_{i0}(0) \mid S_i = 1] - \mathbb{E}[Y_{it}(0) - Y_{i0}(0) \mid S_i = 0]\Big).$$

The first term is Δ_t^{ITT} by (4); the second is zero by (A3). □

B.3 Object 2: The Effect of Operating under the 35-Hour Regime

Lemma B.1. *Under (A4)–(A7):*

(i) $Y_{it}(1) - Y_{it}(0) = C_{it}\,\big[Y_{it}(1, TC_{it}(1)) - Y_{it}(0,0)\big]$ *almost surely;*

(ii) $\Delta_t^{ITT} = \pi_t^C\, \theta_t^{LATE}$;

(iii) $\theta_t^{LATE} = \mathbb{E}\big[\tau_{it}^{H35} \mid S_i = 1, C_{it} = 1\big] + s_t^{TC}\, \mathbb{E}\big[\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1, TC_{it}(1) = 1\big]$, *which is* (9).

Proof. (i) By (A5), $Y_{it}(s) = Y_{it}\big(H35_{it}(s), TC_{it}(H35_{it}(s))\big)$. Partition firms by $(H35_{it}(0), H35_{it}(1))$ into never-takers $(0,0)$, compliers $(0,1)$, always-takers $(1,1)$, and defiers $(1,0)$; (A6) assigns probability zero to defiers. For never-takers, both bundles are $(0,0)$ by (2), so the difference is zero. For always-takers, both bundles are $(1, TC_{it}(1))$ by (A7), so the difference is zero. For compliers, the bundle is $(1, TC_{it}(1))$ under $s = 1$ and $(0,0)$ under $s = 0$. Since $C_{it} = 0$ for never- and always-takers and $C_{it} = 1$ for compliers, the identity follows.

(ii) Take expectations of (i) conditional on $S_i = 1$ and apply the law of iterated expectations over C_{it}: $\mathbb{E}\big[C_{it}\{Y_{it}(1, TC_{it}(1)) - Y_{it}(0,0)\} \mid S_i = 1\big] = \mathbb{P}(C_{it} = 1 \mid S_i = 1)\,\mathbb{E}\big[Y_{it}(1, TC_{it}(1)) - Y_{it}(0,0) \mid S_i = 1, C_{it} = 1\big] = \pi_t^C\,\theta_t^{LATE}$. Assumption (A4) ensures that the complier population has positive probability ($\pi_t^C > 0$), so that the conditional expectations defining θ_t^{LATE} and s_t^{TC} are well defined.

(iii) Equation (8) is an identity, verified by cases: if $TC_{it}(1) = 0$ both sides equal $Y_{it}(1,0) - Y_{it}(0,0)$, and if $TC_{it}(1) = 1$ both sides equal $Y_{it}(1,1) - Y_{it}(0,0)$. Taking expectations over compliers of mandated firms and applying iterated expectations to the second term, $\mathbb{E}\big[TC_{it}(1)\,\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1\big] = s_t^{TC}\,\mathbb{E}\big[\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1, TC_{it}(1) = 1\big]$. □

Proposition B.2 (Wald-DiD)**.** *Under (A1)–(A7), (A1^H), and (A3^H):*

(i) $\mathrm{DiD}_t^Y = \Delta_t^{ITT}$;

(ii) $\mathrm{DiD}_t^{H35} = \mathbb{E}\big[H35_{it}(1) - H35_{it}(0) \mid S_i = 1\big] = \pi_t^C > 0$;

(iii) $\mathrm{DiD}_t^Y \,/\, \mathrm{DiD}_t^{H35} = \theta_t^{LATE}$.

Proof. (i) This is Proposition B.1. In fact, (A1) is implied by the other assumptions: by (A5) at $t = 0$ and (1), $Y_{i0}(s) = Y_{i0}\big(H35_{i0}(s), 0\big)$, which does not depend on s by (A1^H).

(ii) By (A2), $H35_{it} = H35_{it}(S_i)$, so the argument of Proposition B.1 applies with $H35$ in place of Y, (A1^H) in place of (A1), and (A3^H) in place of (A3), giving $\mathrm{DiD}_t^{H35} = \mathbb{E}[H35_{it}(1) - H35_{it}(0) \mid S_i = 1]$. By (A6), $H35_{it}(1) - H35_{it}(0) = C_{it}$, so the right-hand side equals π_t^C, which is positive by (A4).

(iii) By (i), (ii), and Lemma B.1(ii), $\mathrm{DiD}_t^Y / \mathrm{DiD}_t^{H35} = \pi_t^C \theta_t^{LATE} / \pi_t^C = \theta_t^{LATE}$. □

Proposition B.3 (Take-up rate among compliers)**.** *Under the assumptions of Proposition B.2 and (A3^{TC}),* $\mathrm{DiD}_t^{TC} = \pi_t^C\, s_t^{TC}$. *Hence* $s_t^{TC} = \mathrm{DiD}_t^{TC} / \mathrm{DiD}_t^{H35}$, *and the inequalities* $0 \le \mathrm{DiD}_t^{TC} \le \mathrm{DiD}_t^{H35}$ *are a testable implication.*

Proof. By (1), $TC_{i0} = 0$ for every firm, so

$$\begin{aligned}\mathrm{DiD}_t^{TC} &= \mathbb{E}[TC_{it} \mid S_i = 1] - \mathbb{E}[TC_{it} \mid S_i = 0] \\ &= \mathbb{E}\big[TC_{it}(H35_{it}(1)) \mid S_i = 1\big] - \mathbb{E}\big[TC_{it}(H35_{it}(0)) \mid S_i = 0\big].\end{aligned}$$

By (A3^{TC}), the second term equals $\mathbb{E}[TC_{it}(H35_{it}(0)) \mid S_i = 1]$, so $\mathrm{DiD}_t^{TC} = \mathbb{E}\big[TC_{it}(H35_{it}(1)) - TC_{it}(H35_{it}(0)) \mid S_i = 1\big]$. Partition by compliance type as in Lemma B.1: the difference is zero for never-takers by (2) and for always-takers by (A7), and equals $TC_{it}(1)$ for compliers. Hence $\mathrm{DiD}_t^{TC} = \pi_t^C\,\mathbb{E}[TC_{it}(1) \mid S_i = 1, C_{it} = 1] = \pi_t^C\, s_t^{TC}$. The ratio follows from Proposition B.2(ii), and the inequalities from $s_t^{TC} \in [0,1]$. □

If the mandate raised take-up among firms whose adoption it leaves unchanged, in violation of (A7), DiD_t^{TC} could exceed the complier share and the estimated take-up rate could exceed one. Section 4.3 reports $\widehat{s}_{2001}^{TC} = 0.47$.

B.4 Object 3: Separating the Two Policy Components

For $x \in \mathcal{X}$, define the conditional objects

$$\begin{aligned}
\Delta_t^{ITT}(x) &= \mathbb{E}\big[Y_{it}(1) - Y_{it}(0) \mid S_i = 1, X_i = x\big], \\
\pi_t^{C}(x) &= \mathbb{P}\big(C_{it} = 1 \mid S_i = 1, X_i = x\big), \\
\theta_t^{LATE}(x) &= \mathbb{E}\big[Y_{it}(1, TC_{it}(1)) - Y_{it}(0,0) \mid S_i = 1, C_{it} = 1, X_i = x\big], \\
s_t^{TC}(x) &= \mathbb{P}\big(TC_{it}(1) = 1 \mid S_i = 1, C_{it} = 1, X_i = x\big),
\end{aligned}$$

and the conditional component effects

$$\begin{aligned}
\beta_t^{H35}(x) &= \mathbb{E}\big[\tau_{it}^{H35} \mid S_i = 1, C_{it} = 1, X_i = x\big], \\
\beta_t^{TC}(x) &= \mathbb{E}\big[\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1, TC_{it}(1) = 1, X_i = x\big].
\end{aligned}$$

Lemma B.2 (Conditional loading)**.** *Under (A5)–(A7) and (A9), for every $x \in \mathcal{X}$,*

$$\Delta_t^{ITT}(x) = \pi_t^{C}(x)\, \theta_t^{LATE}(x), \qquad \theta_t^{LATE}(x) = \beta_t^{H35}(x) + s_t^{TC}(x)\, \beta_t^{TC}(x).$$

Proof. (A5)–(A7) are restrictions on firm-level potential outcomes and take-up and therefore hold conditional on $X_i = x$. By (A9), $\mathbb{P}(S_i = 1 \mid X_i = x) > 0$ and $\pi_t^C(x) > 0$, so the conditional objects $\Delta_t^{ITT}(x)$, $\theta_t^{LATE}(x)$, and $s_t^{TC}(x)$ are well defined. The identity in Lemma B.1(i) holds pointwise; taking expectations conditional on $\{S_i = 1, X_i = x\}$ and repeating the argument of Lemma B.1(ii)–(iii) on this event yields both equalities. □

Proposition B.4 (Conditional identification)**.** *Under ($A1^H$), (A2), and (A5)–(A9), for every $x \in \mathcal{X}$,*

$$\mathrm{DiD}_t^{Y}(x) = \Delta_t^{ITT}(x), \qquad \mathrm{DiD}_t^{H35}(x) = \pi_t^{C}(x), \qquad \mathrm{DiD}_t^{TC}(x) = \pi_t^{C}(x)\, s_t^{TC}(x),$$

so that $\theta_t^{LATE}(x) = \mathrm{DiD}_t^{Y}(x)/\mathrm{DiD}_t^{H35}(x)$ and $s_t^{TC}(x) = \mathrm{DiD}_t^{TC}(x)/\mathrm{DiD}_t^{H35}(x)$.

Proof. All conditional expectations are well defined by (A9). Assumptions (A2), (A5)–(A7), and ($A1^H$) are restrictions on firm-level potential outcomes and treatments and therefore hold conditional on $X_i = x$. The proofs of Propositions B.1, B.2, and B.3 go through conditional on $X_i = x$, with the conditional versions of (A3), ($A3^H$), and ($A3^{TC}$) in (A8) in place of the unconditional ones, and the conditional relevance part of (A9) in place of (A4). The ratio statements then follow from Lemma B.2. □

Proposition B.5 (Partial identification of the component effects)**.** *Under the assumptions of Proposition B.4, the identified set for $\big(\beta_t^{H35}(x), \beta_t^{TC}(x)\big)$ at any $x \in \mathcal{X}$ is the line*

$$\Big\{\big(\theta_t^{LATE}(x) - s_t^{TC}(x)\, b,\ b\big) :\ b \in \mathbb{R}\Big\}.$$

Proof. By Lemma B.2, every pair in the identified set satisfies $\theta_t^{LATE}(x) = \beta_t^{H35}(x) + s_t^{TC}(x)\,\beta_t^{TC}(x)$, so the identified set is contained in the line. □

Proposition B.6 (Saturated binned specification)**.** *Let* $\Delta_{b,t}^{ITT} = \mathbb{E}\big[Y_{it}(1) - Y_{it}(0) \mid S_i = 1, B_i = b\big]$ *for* $b \in \{0,1\}^2$*, as in* (13)*. Under (A1), (A2), and the parallel-trends condition for* Y *in (A8) and the overlap condition in (A9), both stated with* B_i *in place of* X_i*,* $\mathrm{DiD}_t^Y(b) = \Delta_{b,t}^{ITT}$ *for every* b*. Consequently, in the population regression of* $Y_{it'}$*,* $t' \in \{0, t\}$*, on firm fixed effects, a period effect interacted with* $(1, B_i^F, B_i^P, B_i^F B_i^P)$*, and* $S_i \times \mathbb{1}[t' = t]$ *interacted with* $(1, B_i^F, B_i^P, B_i^F B_i^P)$*, the coefficients on the last four regressors equal*

$$\Delta_{00,t}^{ITT}, \qquad \Delta_{10,t}^{ITT} - \Delta_{00,t}^{ITT}, \qquad \Delta_{01,t}^{ITT} - \Delta_{00,t}^{ITT}, \qquad \big(\Delta_{11,t}^{ITT} - \Delta_{01,t}^{ITT}\big) - \big(\Delta_{10,t}^{ITT} - \Delta_{00,t}^{ITT}\big).$$

Proof. The first statement is Proposition B.1 conditional on $B_i = b$. The regression is saturated in $\big(S_i, B_i, \mathbb{1}[t' = t]\big)$: within each mandate group and period, expected outcomes are unrestricted functions of b, and the firm fixed effects together with the group-specific period effects absorb all components common to the two mandate groups. The coefficients on $S_i \times \mathbb{1}[t' = t]$ and its interactions are therefore the saturated parameterization of the map $b \mapsto \mathrm{DiD}_t^Y(b)$: its level at $b = (0,0)$, the two simple differences, and the cross-difference. □

The event-study version used in Section 6.1 applies this result period by period. Because the specification is saturated in the exposure groups, it imposes no functional-form restriction: it identifies the four group ITTs, each of which averages over the compliance and take-up the mandate induces in that group, and it does not attribute differences across groups to either policy component. Attributing cross-group differences to the policy components requires (A10).

Proposition B.7 (Linear exposure model)**.** *Under the assumptions of Proposition B.4 and (A10):*

(i) For every $x = (f, p) \in \mathcal{X}$*,*

$$\mathrm{DiD}_t^Y(x) = \beta_t^{H35}\, f\, \mathrm{DiD}_t^{H35}(x) + \beta_t^{TC}\, p\, \mathrm{DiD}_t^{TC}(x) + \gamma_t\, f p\, \mathrm{DiD}_t^{TC}(x). \tag{23}$$

Let $m_t(x) = \big(f\,\mathrm{DiD}_t^{H35}(x),\ p\,\mathrm{DiD}_t^{TC}(x),\ f p\,\mathrm{DiD}_t^{TC}(x)\big)$*. The vector* $(\beta_t^{H35}, \beta_t^{TC}, \gamma_t)$ *is point identified if and only if* $\{m_t(x) : x \in \mathcal{X}\}$ *spans* $\mathbb{R}^3$*. When* $\mathcal{X}$ *contains more than three points,* (23) *is overidentified and its excess restrictions are testable.*

(ii) Let $D_{it} = (F_i H35_{it},\ P_i TC_{it},\ F_i P_i TC_{it})'$ *and consider* (17) *on the pre-reform period and period* t*, so that first-differencing removes* μ_i *and* ρ_{rt}, ϕ_{ct} *become industry and county indicators. Write* Δ *for the first difference,* $V_i = (X_i, r_i, c_i)$*,* W_i *for the differenced controls (a constant,* F_i*,* P_i*,* $F_i P_i$*, and the industry and county indicators),* $Z_i = S_i(1, F_i, P_i, F_i P_i)'$*, and* $\widetilde{Z}_i = Z_i - \Pi_W Z_i$ *for the residual of* Z_i *from its population linear projection on* W_i*. Let* $R_{it} = \Delta Y_{it} - \beta_t' \Delta D_{it}$*. Suppose that*

(a) $\mathbb{E}[R_{it} \mid S_i, V_i] = g_t(V_i)$ *for some function* g_t*;*

(b) either $g_t(V_i)$ *lies in the linear span of* W_i*, or* $\mathbb{E}[\widetilde{Z}_i \mid V_i] = 0$*;*

(c) rank $\mathbb{E}[\widetilde{Z}_i \, \Delta D'_{it}] = 3$.

Then the population 2SLS coefficients on D_{it} *in* (17) *equal* $(\beta_t^{H35}, \beta_t^{TC}, \gamma_t)$.

Proof. (i) By (A10), $\beta_t^{H35}(x) = \beta_t^{H35} f$ and $\beta_t^{TC}(x) = (\beta_t^{TC} + \gamma_t f)\, p$. Substituting into Lemma B.2 and multiplying by $\pi_t^C(x)$,

$$\Delta_t^{ITT}(x) = \beta_t^{H35} \, f \, \pi_t^C(x) + \left(\beta_t^{TC} + \gamma_t f\right) p \, \pi_t^C(x) \, s_t^{TC}(x),$$

and Proposition B.4 replaces each object by its identified counterpart, giving (23): a system of linear equations in three unknowns, one per $x \in \mathcal{X}$, with identified coefficient vectors $m_t(x)$ and identified left-hand sides. It has a unique solution if and only if the $m_t(x)$ span $\mathbb{R}^3$. With more than three equations, the additional ones restrict the identified quantities.

(ii) By construction, $\widetilde{Z}_i$ is a function of (S_i, V_i) and $\mathbb{E}[\widetilde{Z}_i W'_i] = 0$. By (a), $\mathbb{E}[\widetilde{Z}_i R_{it}] = \mathbb{E}[\widetilde{Z}_i \, g_t(V_i)]$. Under the first alternative in (b), $g_t(V_i) = W'_i \lambda_t$ for some λ_t, so this expectation is zero by ortogonality to the controls; under the second, it equals $\mathbb{E}\big[\mathbb{E}[\widetilde{Z}_i \mid V_i] \, g_t(V_i)\big] = 0$. Hence $\mathbb{E}[\widetilde{Z}_i \, \Delta Y_{it}] = \mathbb{E}[\widetilde{Z}_i \, \Delta D'_{it}] \, \beta_t$, four equations in three unknowns, which (c) solves uniquely. By the Frisch–Waugh–Lovell theorem, the population 2SLS coefficients on D_{it} in (17) solve this system, so they equal β_t.

□

Since $\pi_t^C(x) > 0$, each $m_t(x)$ is a positive multiple of $\big(f, \; p \, s_t^{TC}(x), \; fp \, s_t^{TC}(x)\big)$, so the rank condition in (i) requires these three functions of x to be linearly independent over $\mathcal{X}$. The condition fails if P_i were exactly proportional to F_i and take-up constant across firms. Separate identification therefore requires sufficient variation in exposure and conditional take-up to make the three loading functions linearly independent. Variation in tax exposure at a given full-time share is one source of such variation; Figure C.4 documents substantial variation in our sample. Part (i) places no restriction on how the common no-mandate trend varies with exposure. Part (ii) is the regression counterpart. Condition (a) restates the parallel-trends assumptions (A8) and (A10) conditional on industry and county as well as exposure, since (17) includes both sets of fixed effects. Condition (b) makes the instruments valid: either the common residual trend is additive in industry, county, and the exposure terms, so that the controls absorb it, or the instruments are balanced with respect to (X_i, r_i, c_i) after residualization on those controls, as when mandate status is independent of exposure, industry, and county. We rely on the first; Figure C.4 supports balance of exposure across mandate groups unconditionally but does not establish it after residualization. Condition (c) is the rank condition on the instrument–regressor moments, the finite-moment analogue of the spanning condition in (i); it is assessed in Section 6.2 with the conditional F-statistics of Sanderson and Windmeijer (2016).

Corollary B.1 (Decomposition of the LATE)**.** *Under the assumptions of Proposition B.7,*

$$\theta_t^{LATE} = \beta_t^{H35}\,\mathbb{E}\big[F_i \mid S_i = 1, C_{it} = 1\big] + \beta_t^{TC}\,\mathbb{E}\big[P_i\,TC_{it}(1) \mid S_i = 1, C_{it} = 1\big] + \gamma_t\,\mathbb{E}\big[F_i P_i\,TC_{it}(1) \mid S_i = 1, C_{it} = 1\big].$$

Proof. By (8), $\theta_t^{LATE} = \mathbb{E}[\tau_{it}^{H35} \mid S_i = 1, C_{it} = 1] + \mathbb{E}[TC_{it}(1)\,\tau_{it}^{TC} \mid S_i = 1, C_{it} = 1]$. For the first term, iterated expectations over X_i and (A10) give $\mathbb{E}[\beta_t^{H35} F_i \mid S_i = 1, C_{it} = 1]$. For the second, condition on $(X_i, TC_{it}(1))$: the term vanishes when $TC_{it}(1) = 0$, and when $TC_{it}(1) = 1$ the conditional mean of τ_{it}^{TC} is $(\beta_t^{TC} + \gamma_t F_i)P_i$ by (A10), so the term equals $\mathbb{E}\big[TC_{it}(1)\,(\beta_t^{TC} + \gamma_t F_i)P_i \mid S_i = 1, C_{it} = 1\big]$. Separating the level and interaction parts yields the result. □

Corollary B.2 (Decomposition of the ITT and accounting identity)**.** *Define the mandate-induced changes in treatment*

$$\Delta H35_{it} = H35_{it}(1) - H35_{it}(0), \qquad \Delta TC_{it} = TC_{it}\big(H35_{it}(1)\big) - TC_{it}\big(H35_{it}(0)\big).$$

Under the assumptions of Proposition B.7:

(i) $\Delta H35_{it} = C_{it}$ *and* $\Delta TC_{it} = C_{it}\,TC_{it}(1)$ *almost surely, and*

$$\Delta_t^{ITT} = \beta_t^{H35}\,\mathbb{E}\big[F_i\,\Delta H35_{it} \mid S_i = 1\big] + \beta_t^{TC}\,\mathbb{E}\big[P_i\,\Delta TC_{it} \mid S_i = 1\big] + \gamma_t\,\mathbb{E}\big[F_i P_i\,\Delta TC_{it} \mid S_i = 1\big],$$

which is (16)*;*

(ii) the three doses are identified from conditional contrasts:

$$\mathbb{E}\big[F_i\,\Delta H35_{it} \mid S_i = 1\big] = \mathbb{E}\big[F_i\,\mathrm{DiD}_t^{H35}(X_i) \mid S_i = 1\big],$$
$$\mathbb{E}\big[P_i\,\Delta TC_{it} \mid S_i = 1\big] = \mathbb{E}\big[P_i\,\mathrm{DiD}_t^{TC}(X_i) \mid S_i = 1\big],$$
$$\mathbb{E}\big[F_i P_i\,\Delta TC_{it} \mid S_i = 1\big] = \mathbb{E}\big[F_i P_i\,\mathrm{DiD}_t^{TC}(X_i) \mid S_i = 1\big],$$

(iii) if, in addition, (A3) holds and (A3^H) and (A3^{TC}) hold with the exposure-weighted treatments $F_i H35_{it}$, $P_i TC_{it}$, and $F_i P_i TC_{it}$ in place of $H35_{it}$ and TC_{it}—for which a sufficient condition is (A8) together with $X_i \perp S_i$—then the doses equal the unconditional contrasts $\mathrm{DiD}_t^{F\times H35}$, $\mathrm{DiD}_t^{P\times TC}$, *and* $\mathrm{DiD}_t^{F\times P\times TC}$, *and*

$$\mathrm{DiD}_t^{Y} = \beta_t^{H35}\,\mathrm{DiD}_t^{F\times H35} + \beta_t^{TC}\,\mathrm{DiD}_t^{P\times TC} + \gamma_t\,\mathrm{DiD}_t^{F\times P\times TC}. \tag{24}$$

Proof. (i) The first claim is (A6) for adoption and, for take-up, the type-by-type computation in the

proof of Proposition B.3. By Lemma B.1(i) and (8),

$$\Delta_t^{ITT} = \mathbb{E}\big[C_{it}\,\tau_{it}^{H35} \mid S_i = 1\big] + \mathbb{E}\big[C_{it}\,TC_{it}(1)\,\tau_{it}^{TC} \mid S_i = 1\big].$$

Condition the first term on (X_i, C_{it}) and the second on $(X_i, C_{it}, TC_{it}(1))$, and apply (A10) on the events $\{C_{it} = 1\}$ and $\{C_{it} = 1, TC_{it}(1) = 1\}$, respectively:

$$\Delta_t^{ITT} = \mathbb{E}\big[C_{it}\,\beta_t^{H35} F_i \mid S_i = 1\big] + \mathbb{E}\big[C_{it}\,TC_{it}(1)\,(\beta_t^{TC} + \gamma_t F_i) P_i \mid S_i = 1\big],$$

which is the stated expression once C_{it} and $C_{it}TC_{it}(1)$ are written as $\Delta H35_{it}$ and ΔTC_{it}.

(ii) By iterated expectations over X_i, $\mathbb{E}[F_i C_{it} \mid S_i = 1] = \mathbb{E}[F_i\,\pi_t^C(X_i) \mid S_i = 1]$ and $\mathbb{E}[P_i C_{it} TC_{it}(1) \mid S_i = 1] = \mathbb{E}[P_i\,\pi_t^C(X_i)\,s_t^{TC}(X_i) \mid S_i = 1]$, and Proposition B.4 identifies $\pi_t^C(x)$ and $\pi_t^C(x)s_t^{TC}(x)$. The interaction dose is identical with F_iP_i in place of P_i.

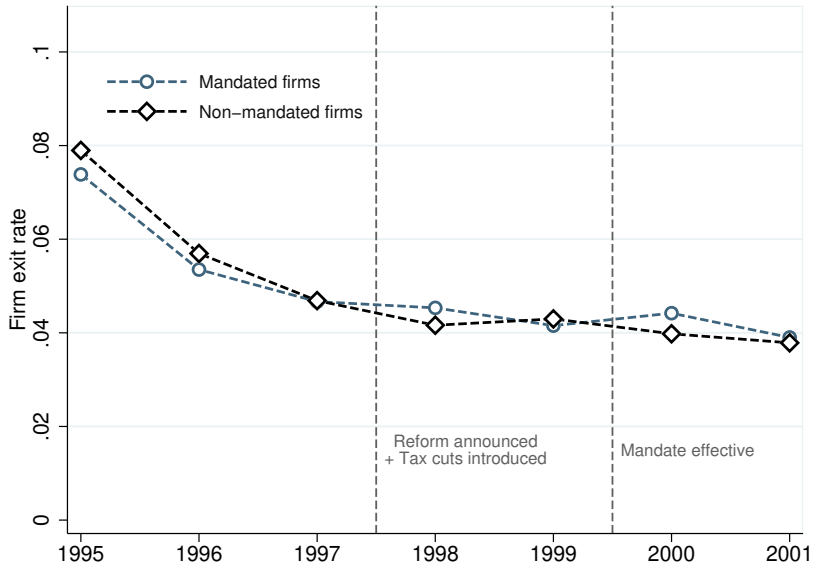
(iii) Because F_i and P_i are predetermined, $F_iH35_{it} = F_iH35_{it}(S_i)$, and the argument of Proposition B.1 applies to $R_{it} = F_iH35_{it}$: no anticipation holds by (A1^H) and parallel trends by the stated condition, so $\mathrm{DiD}_t^{F\times H35} = \mathbb{E}[F_i(H35_{it}(1) - H35_{it}(0)) \mid S_i = 1] = \mathbb{E}[F_i\,\Delta H35_{it} \mid S_i = 1]$. For $R_{it} = P_iTC_{it}$, $TC_{i0} = 0$ gives $\mathrm{DiD}_t^{P\times TC} = \mathbb{E}[P_iTC_{it}(H35_{it}(1)) \mid S_i = 1] - \mathbb{E}[P_iTC_{it}(H35_{it}(0)) \mid S_i = 0] = \mathbb{E}[P_i\,\Delta TC_{it} \mid S_i = 1]$ under the stated condition, and likewise for $F_iP_iTC_{it}$. Finally, $\mathrm{DiD}_t^Y = \Delta_t^{ITT}$ by Proposition B.1 (its assumptions hold by Proposition B.2(i) and (A3)), and substituting the doses into (i) gives (24). For the sufficient condition: if $X_i \perp S_i$, integrating each conditional parallel-trends statement in (A8) against the common distribution of X_i yields its unconditional counterpart for Y_{it} and, since F_i and P_i are functions of X_i, for each exposure-weighted treatment. □

Each dose in (ii) has a direct interpretation: $\mathbb{E}[F_i\,\Delta H35_{it} \mid S_i = 1] = \mathbb{E}[F_iC_{it} \mid S_i = 1]$ is the average full-time exposure activated by the mandate, and the tax-cut doses are the take-up-realized doses of predicted exposure among compliers. Each term of the decomposition is thus a per-unit component effect times the dose of that component induced by the mandate, and the doses are identified and reportable independently of the effect estimates (Figure F.2).

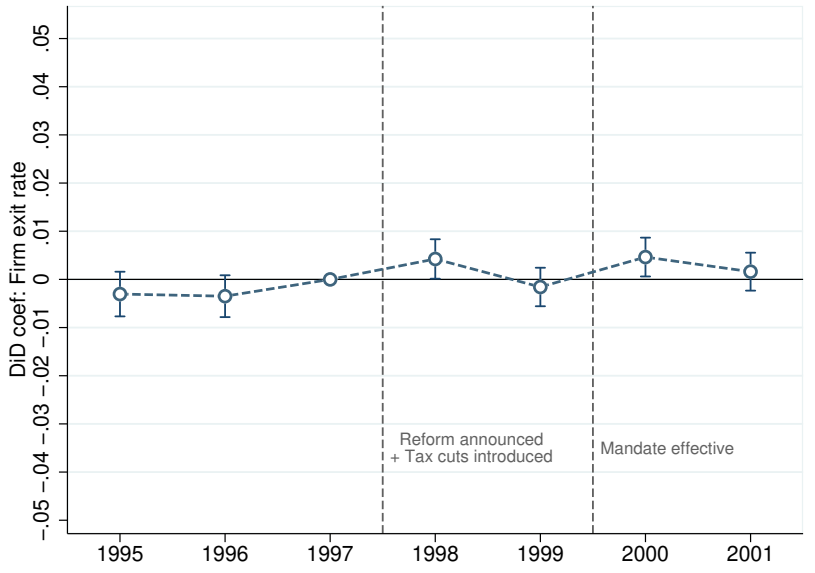
C Evidence Supporting the Empirical Design

Figure C.1: Effect of the Mandate on Firm Exit

(a) Raw Avg.: Firm Exit Rate



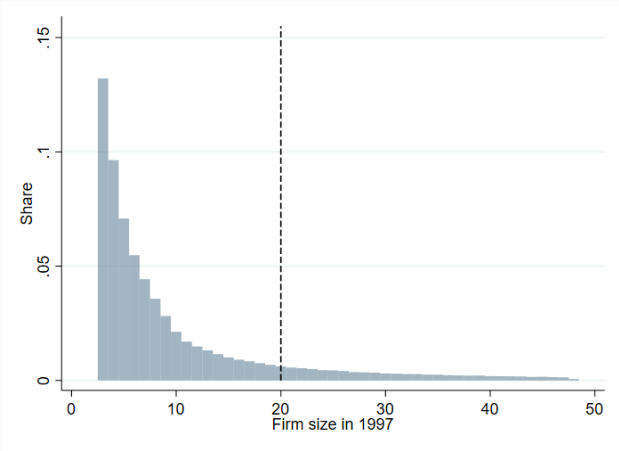
(b) DiD: Firm Exit Probability



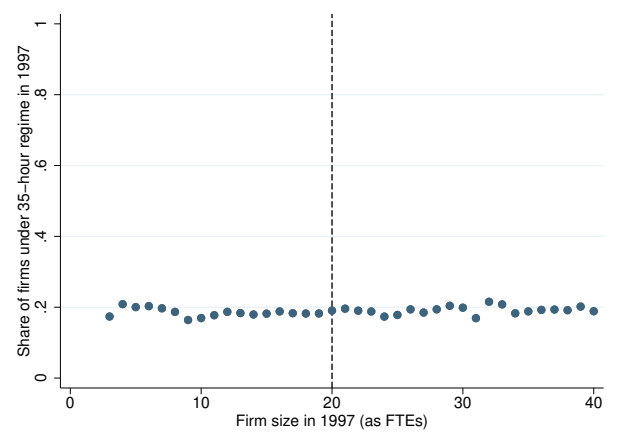
Notes: This figure reports intent-to-treat effects of the mandate on firm exit. The sample consists of firms observed in 1994 with 10–18 employees (non-mandated) or 22–48 employees (mandated). Panel (a) reports annual exit rates separately by mandate status, while Panel (b) reports the corresponding estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms.

Figure C.2: Firm Size and Reform Implementation

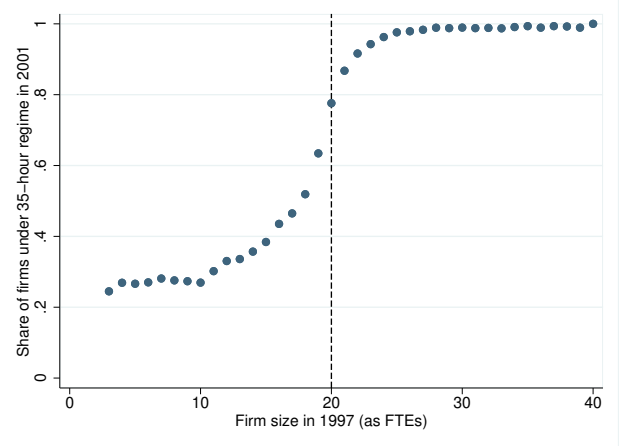
(a) Distribution of Firm Size, 1997



(b) Share of Firms under 35-Hour Regime, 1997



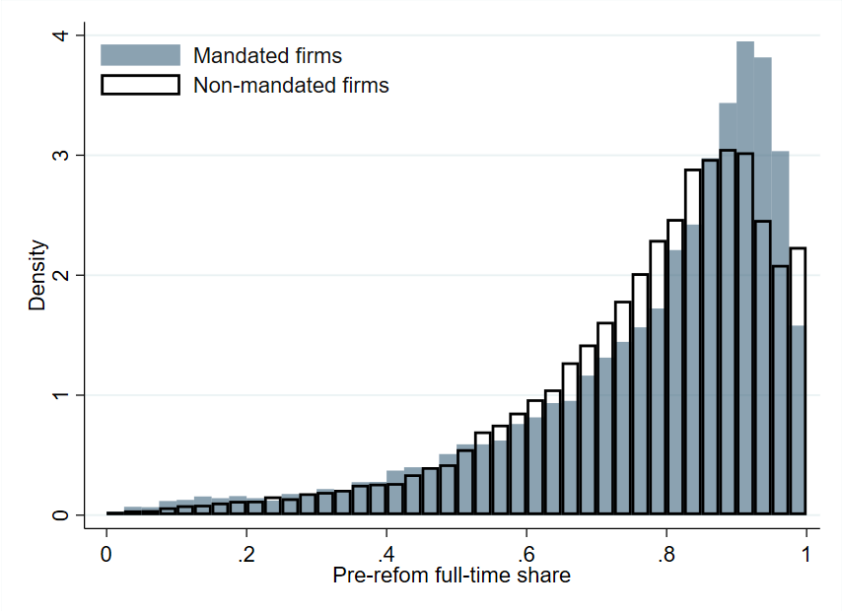
(c) Share of Firms under 35-Hour Regime, 2001



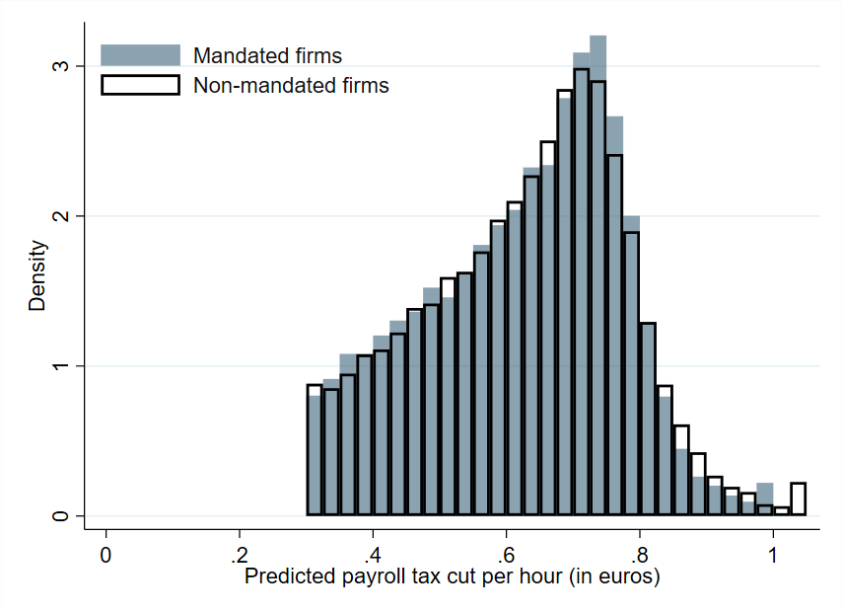
Notes: This figure examines the firm-size measure used for identification. Panel (a) shows the distribution of firm size in 1997 in the estimation sample. Firm size is measured in full-time equivalents. The vertical line marks the 20-employee threshold determining the mandate. Panels (b)–(c) show the relationship between 1997 firm size and the fraction of firms operating under the 35-hour regime, $H35$, in 1997 and 2001 respectively.

Figure C.3: Sources of Variation Used in the Decomposition

(a) Share of Full-Time Workers in 1997



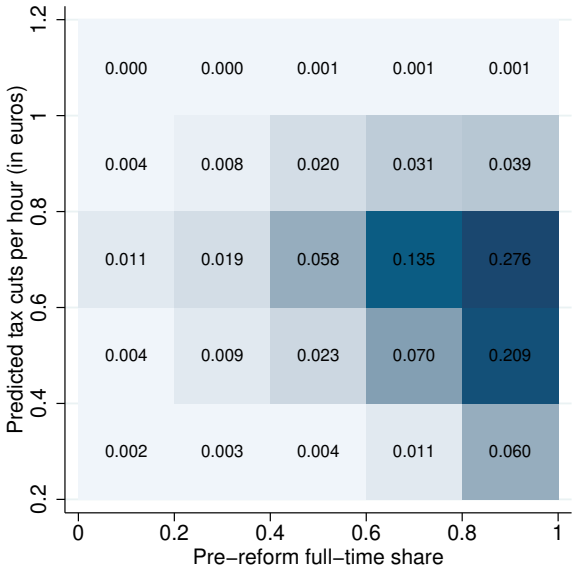
(b) Predicted Payroll Tax Cuts Based on 1997 Wages



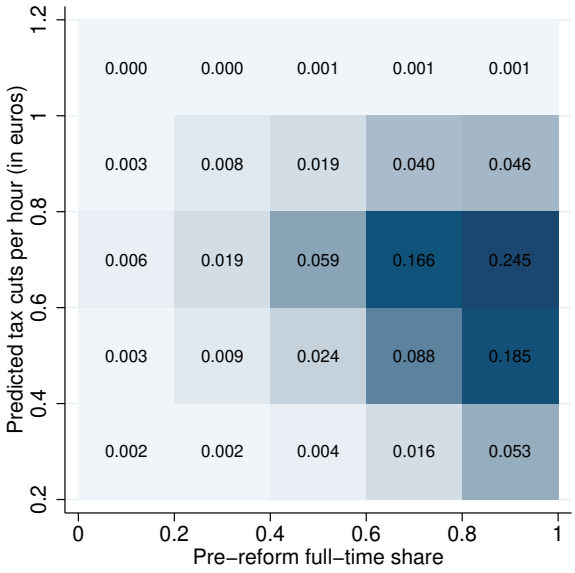
Notes: This figure shows the distribution across firms of the sources of variation used in the decomposition analysis. Panel (a) reports the distribution of the 1997 share of jobs with more than 35 hours per week in the firm, separately for mandated (shaded bars) and non-mandated (outlined bars) firms. This measure captures differential exposure to the workweek reduction. Panel (b) reports the distribution of predicted payroll tax cuts per hour by mandate status, capturing differential exposure to the fiscal component of the reform. Predicted payroll tax cuts are constructed from firms' 1997 wage distributions and the Aubry II tax schedule. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure C.4: Joint Distribution of Exposure to 35-Hour and to Tax Cut (*F*,*P*)

(a) Mandated Firms

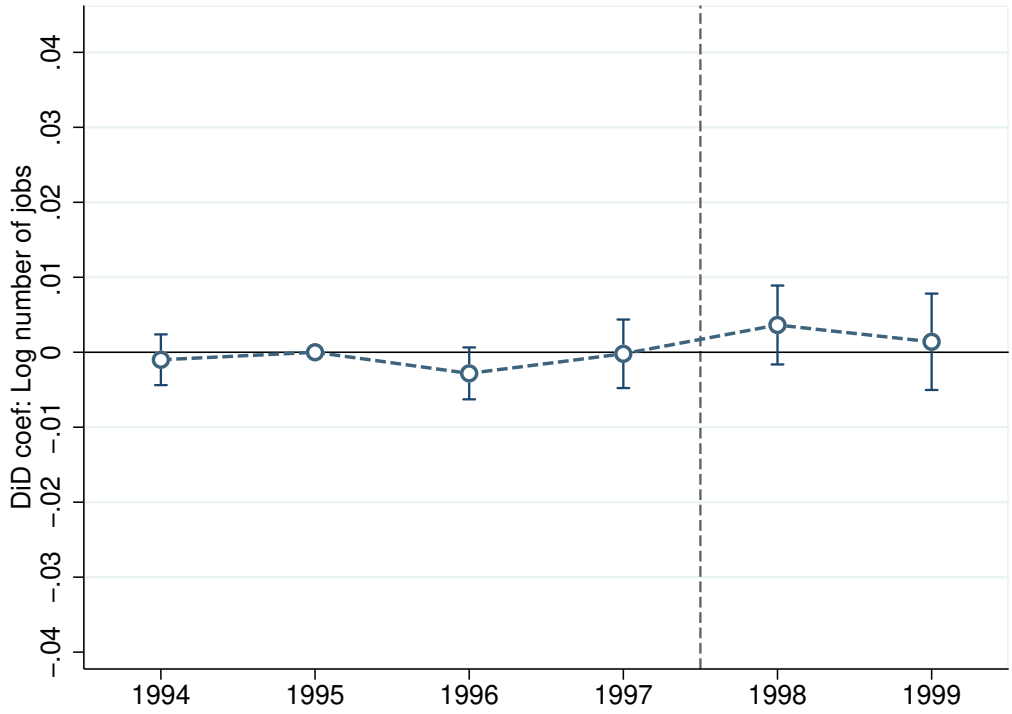


(b) Non-Mandated Firms



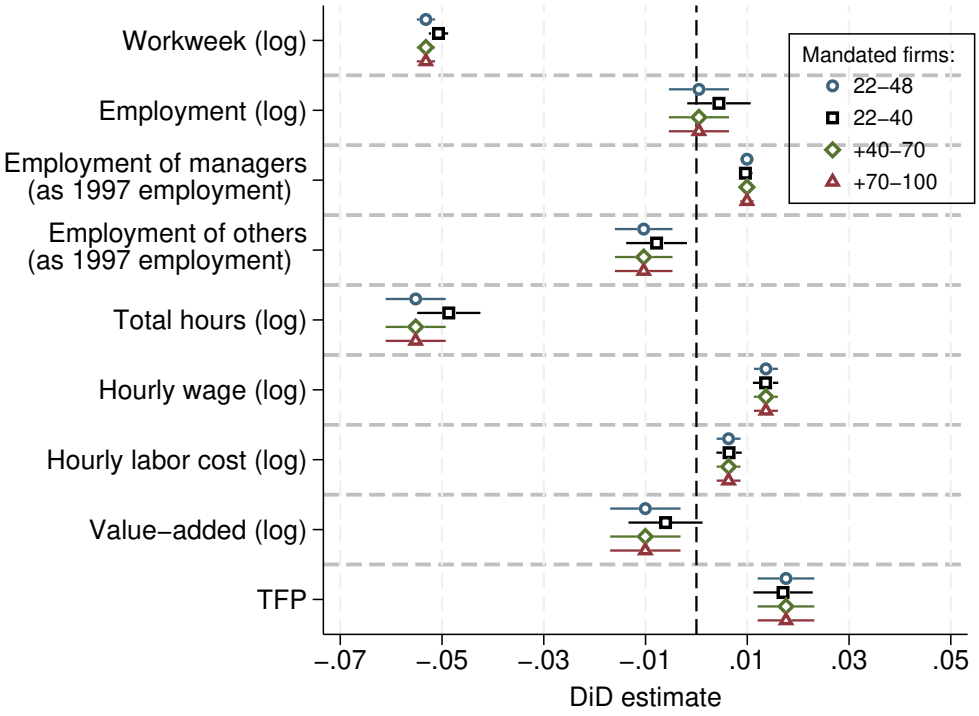
Notes: This figure shows the joint distribution of the two sources of variation used in the decomposition analysis. The x-axis reports firms' pre-reform share of full-time jobs, while the y-axis reports predicted payroll tax cuts based on 1997 wages. Each cell reports the share of firms in the corresponding full-time-share × predicted-tax-cut bin. Panel (a) shows the distribution for mandated firms and Panel (b) for non-mandated firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure C.5: Placebo Test of Mean Reversion



Notes: This figure presents a placebo event study for firm log employment. We assign placebo mandate status based on firms' 1995 size and estimate our baseline specification over the subsequent four years, matching the post-treatment horizon used in the analysis. The omitted year is 1995, while the 1994 estimate provides a pre-assignment placebo. The vertical dashed line marks the placebo mandate date. Whiskers denote 95% confidence intervals. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1995 size.

Figure C.6: Robustness to Alternative Samples of Mandated Firms



Notes: This figure reports difference-in-differences estimates between 1997 and 2001 for our main outcomes of interest. Each marker corresponds to a different sample of mandated firms. Blue circles show estimates for our baseline sample of firms with 22–48 employees in 1997. We then narrow the range to 22–40 employees (black squares) and widen it to 22–70 employees (green diamonds) and 22–100 employees (red triangles). Whiskers denote 95% confidence intervals. All specifications are estimated on balanced panels of firms.

D Worker-Level Analysis

This appendix presents the worker-level analysis, based on the DADS Panel, which covers 4% of workers.

D.1 Specifications

Our first strategy adapts Equation (6) to the worker level:

$$y_{it} = \sum_{\substack{\tau=1996 \\ \tau\neq 1997}}^{2001} \alpha_\tau S_{f(i)} \times \mathbf{1}[t=\tau] + \mu_i + \rho_{rt} + \phi_{ct} + u_{it}, \tag{25}$$

where $f(i)$ is the employer of worker i in 1997, and $S_{f(i)}$ equals one if this employer had 22–48 FTE employees in 1997 and zero if it had 10–18. We include worker, industry-by-year, and county-by-year fixed effects, where industry r and county c are those of the 1997 employer. Standard errors are clustered at the firm level. Because the panel covers only a sample of workers, we start in 1996 to limit the loss of observations when balancing the panel. Figure D.1 reports the results on hourly wages and labor cost; Figures D.3 and D.4 document the effects of the mandate on earnings, hourly wage and labor cost by skill group.

Our second strategy estimates effects by initial working time. We compare workers in mandated and non-mandated firms within bins of initial hours, and benchmark the estimates against a pre-reform placebo window. For each outcome q, and separately for the post-reform window (1999–2001) and the placebo window (1996–1998), we estimate

$$Y^q_{it} - Y^q_{it-2} = \sum_{b=1}^{4} \gamma^q_{bt} B_{ib,t-2} + \sum_{b=1}^{4} \beta^q_{bt} S_{i,t-2} B_{ib,t-2} + u^q_{it}, \tag{26}$$

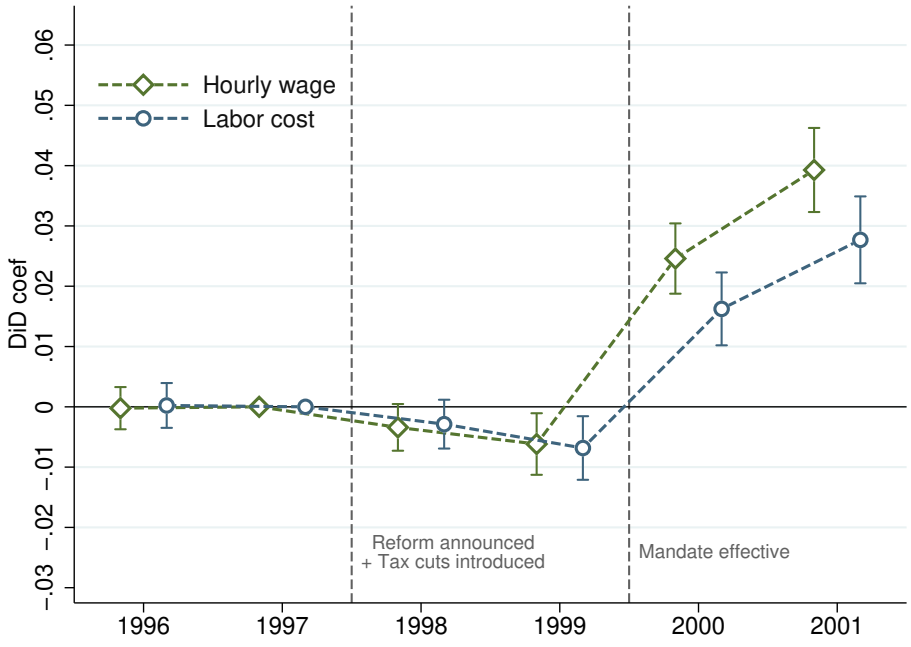
where $t \in \{1998, 2001\}$ is the end of the window, $B_{ib,t-2}$ indicates that the worker's weekly hours at the start of the window fall in bin b (5–30, 30–35, 35–39, or 39–48 hours), and $S_{i,t-2}$ indicates that the initial employer is mandated, based on firm size in 1997 (1994 for the placebo window).[40] The outcomes are employment in t, the change in weekly hours $h_{it} - h_{i,t-2}$, and the growth in the hourly wage $(w_{it} - w_{i,t-2})/w_{i,t-2}$.

The intercepts γ^q_{bt} absorb bin-specific dynamics such as mean reversion, as long as these are common to mandated and non-mandated firms, which the placebo window tests. The coefficients β^q_{bt} measure the differential change in mandated firms within each bin. The employment regression includes all workers employed at the start of the window, and the hours and wage regressions those who remain with the same employer. Figure D.2 reports $\hat{\beta}^q_{bt}$ with 95% confidence intervals for each window.

[40] The bins are $[5, 30)$, $[30, 35]$, $(35, 38.5)$, and $[38.5, 48)$ hours per week. Workers with initial hours below 5 or at or above 48 are excluded. Hours and hourly wages are winsorized at the 1st and 99th percentiles within year before constructing changes, and the changes are winsorized at the same percentiles within each window.

D.2 Results

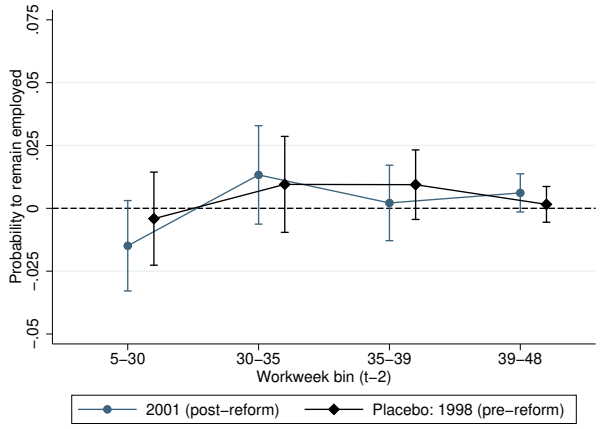
Figure D.1: Worker-Level: Hourly Wage and Hourly Labor Cost Effects of the Mandate



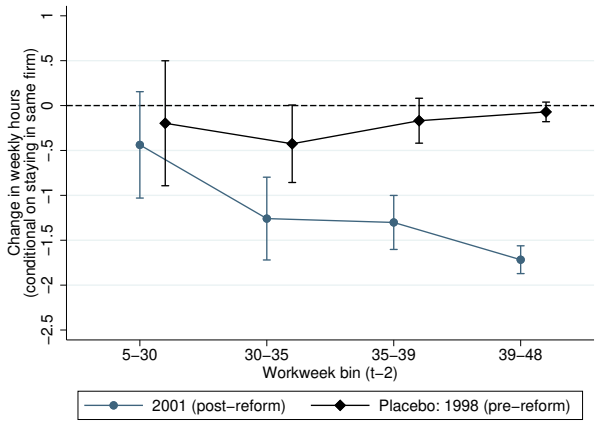
Notes: This figure reports worker-level difference-in-differences estimates of α_τ from Equation 25. The outcomes are log hourly wages and log hourly labor costs for full-time workers employed in mandated relative to non-mandated firms in 1997. Estimates are conditional on remaining employed. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample consists of workers employed in 1997 in private-sector firms with 10–18 or 22–48 employees.

Figure D.2: Worker-Level Results by Previous Workweek: Employment, Hours, and Wage

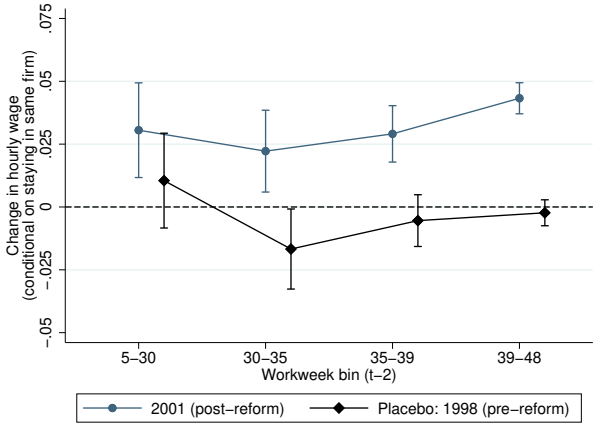
(a) Job Retention Probability



(b) Weekly Hours



(c) Hourly Wages



Notes: This figure reports estimates of β_{bt}^{q} from Equation 26. The outcomes are the probability of employment (Panel (a)), weekly hours conditional on employment (Panel (b)), and hourly wages conditional on employment (Panel (c)). In all panels, estimates are reported separately for the post-reform year 2001 and the pre-reform comparison year 1998. Weekly-hours bins are defined using hours worked two years before the outcome year. The coefficients capture the change in each outcome between $t-2$ and t in mandated relative to non-mandated firms. Thus, for the 2001 estimates, they measure changes between 1999 and 2001. Whiskers denote 95% confidence intervals. The sample consists of workers employed in private-sector firms with 10–18 or 22–48 employees in 1997.

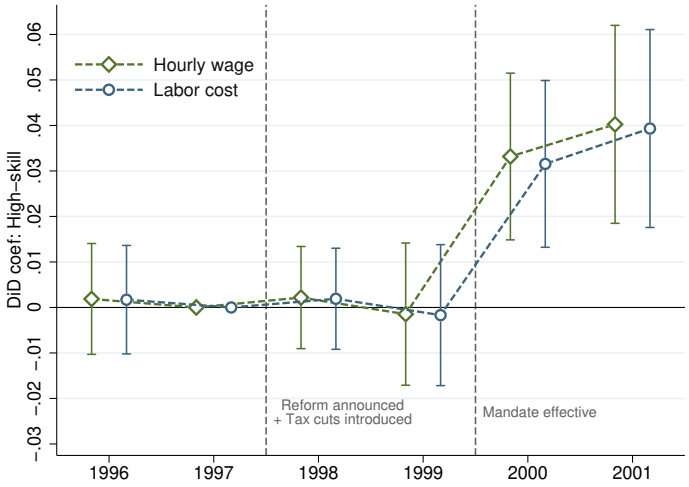
Figure D.3: Worker-Level Earnings Across All Jobs



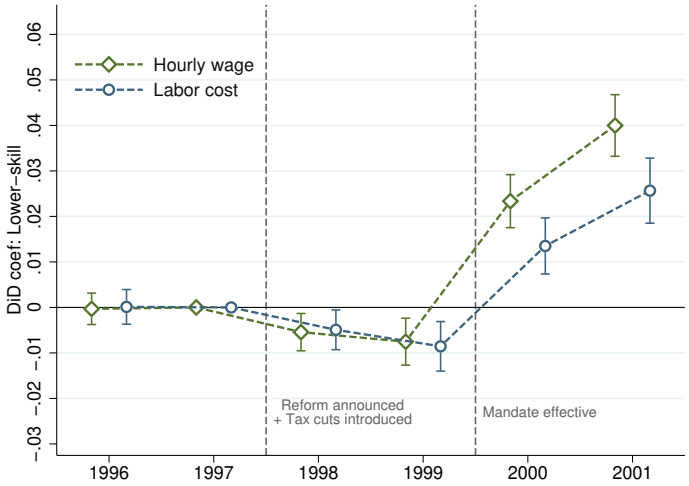
Notes: This figure reports worker-level difference-in-differences estimates of α_τ from Equation 25. The outcome is log total annual earnings across all jobs in a given year. As a comparison, Panel (d) of Figure 9 focuses on earnings in the main job. Estimates are conditional on remaining employed. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample consists of workers employed in 1997 in private-sector firms with 10–18 or 22–48 employees.

Figure D.4: Worker-Level: Hourly Wage and Hourly Labor Cost Effects of the Mandate, by Skill Group

(a) Managers and Professionals



(b) Other Employees

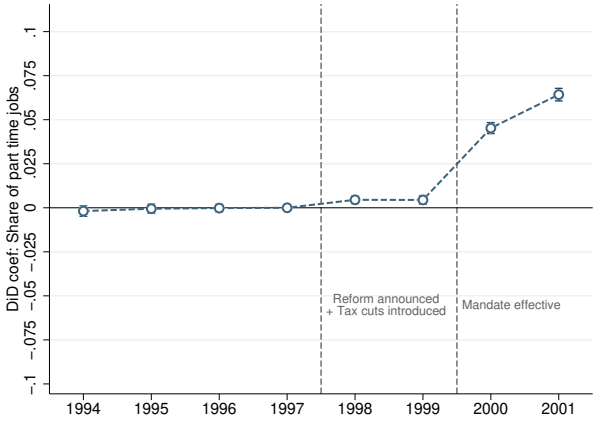


Notes: This figure reports worker-level difference-in-differences estimates of α_τ from Equation 25. Results are reported separately for managers and high-skill professionals (Panel (a)) and other employees (Panel (b)) based on the occupation in 1997. The outcomes are log hourly wages and log hourly labor costs for full-time workers employed in mandated relative to non-mandated firms. Estimates are conditional on remaining employed. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample consists of workers employed in 1997 in private-sector firms with 10–18 or 22–48 employees.

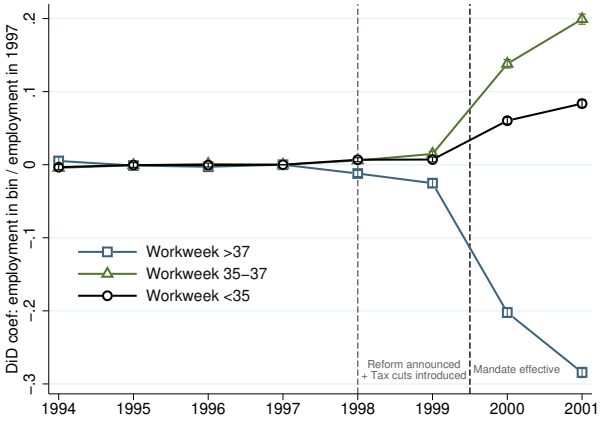
E Effect of the Mandate: Additional Results

Figure E.1: Shift Toward Part-Time Employment

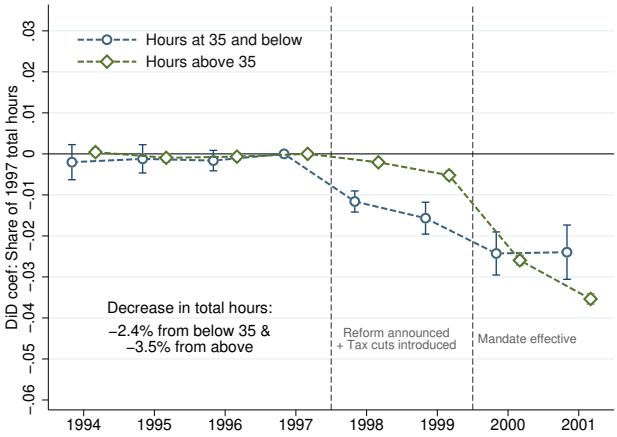
(a) Part-Time Employment Share



(b) Employment by Workweek



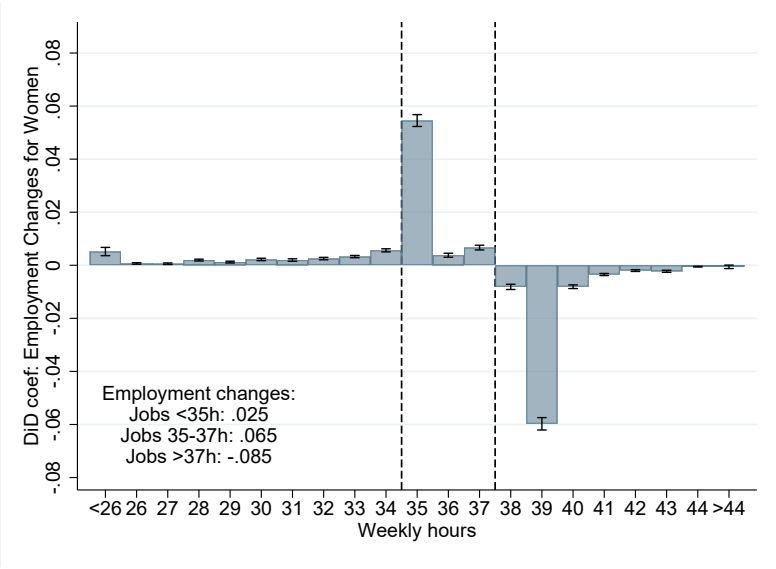
(c) Decomposition Into Hours Above vs. At-or-Below 35



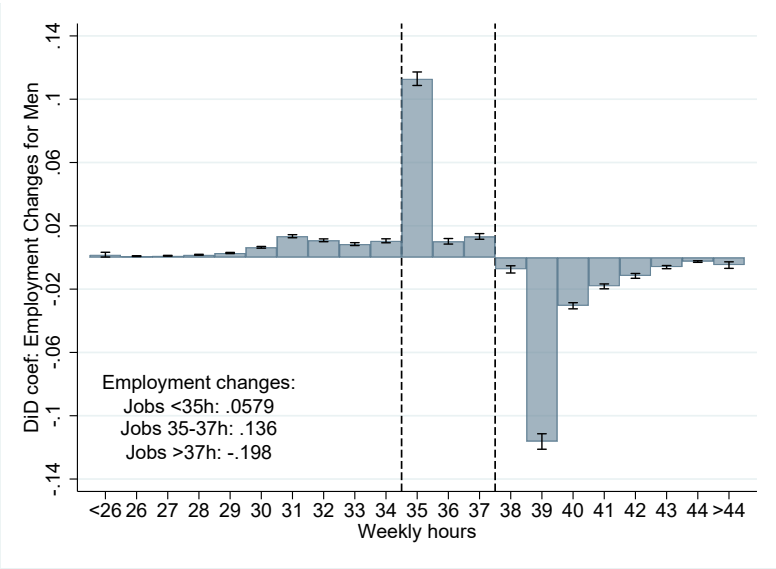
Notes: This figure reports intent-to-treat effects of the legal mandate on firm-level outcomes: the share of part-time jobs (Panel (a)), employment in each weekly-hours bin as a share of total 1997 employment (Panel (b)), and total hours worked in each hour bin as a share of total 1997 hours (Panel (c)). Panels report the corresponding difference-in-differences estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure E.2: Employment Effects Across Weekly Hours Bins by Gender

(a) Women



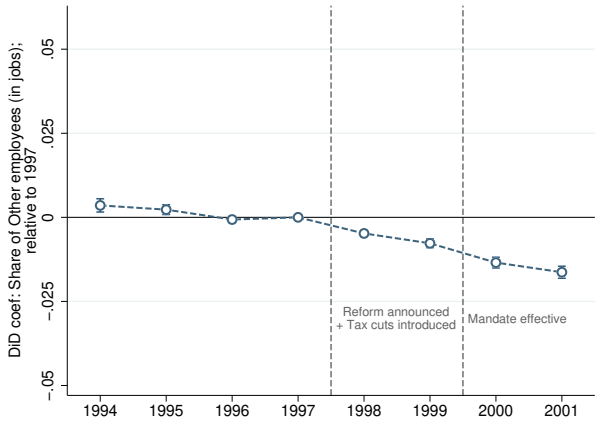
(b) Men



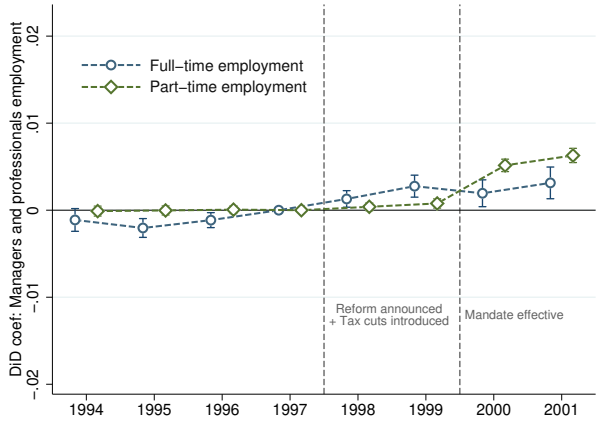
Notes: This figure reports firm-level difference-in-differences estimates of λ_k from Equation 19. Each bar shows the estimated change in employment in mandated relative to non-mandated firms within a given weekly-hours bin, by gender, between 1997 and 2001, expressed as a share of total firm employment in 1997. The estimates are therefore on a common scale across bins (and across both panels) and sum to the aggregate employment effect. Panel (a) reports the estimates for female employment, and Panel (b) for male employment. Whiskers denote 95% confidence intervals. Dashed vertical lines mark 35 and 37 hours. The figure also reports aggregate changes for jobs below 35 hours, between 35 and 37 hours, and above 37 hours, obtained by summing the corresponding bin-level estimates. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure E.3: Full-Time and Part-Time Employment by Skill Group

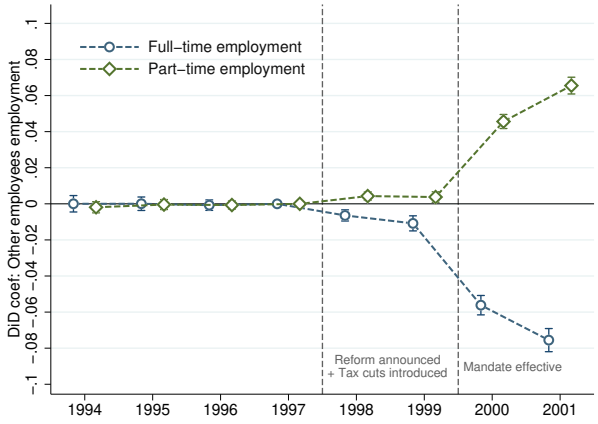
(a) Share of Lower-Skill Employees



(b) Manager/Professionals: Full-Time and Part-Time Employment

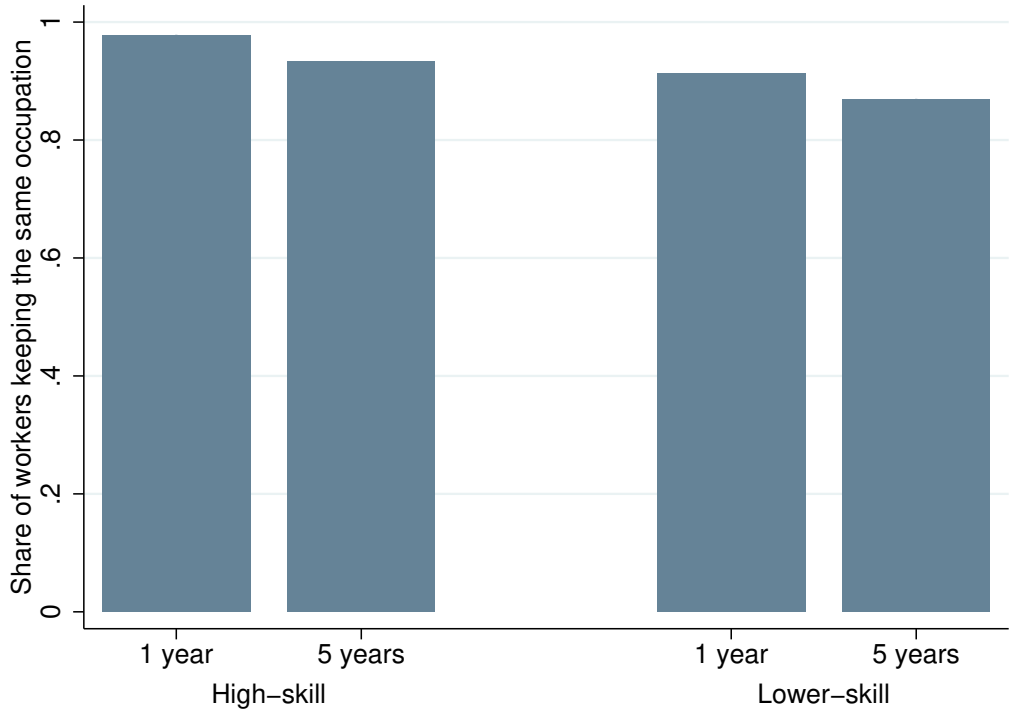


(c) Other Employees: Full-Time and Part-Time Employment



Notes: This figure reports intent-to-treat effects of the legal mandate on firm-level outcomes: the share of employees who are neither managers nor professionals (Panel (a)); part-time and full-time employment among managers and professionals, each expressed as a share of total 1997 employment (Panel (b)); and part-time and full-time employment among other employees, also expressed as a share of total 1997 employment (Panel (c)). Panels report the corresponding difference-in-differences estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

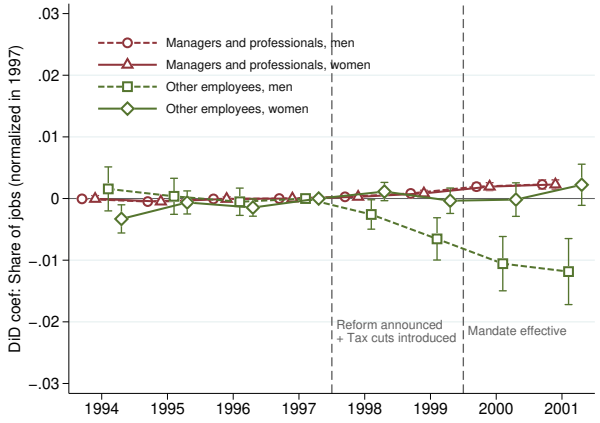
Figure E.4: Probability to Remain in Same Skill Group



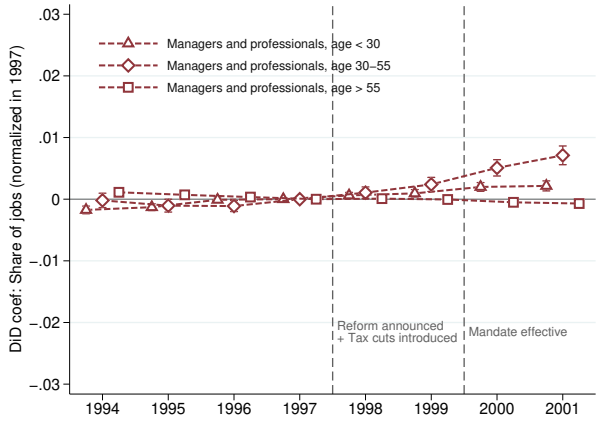
Notes: This figure reports the share of workers in a given occupational group in year t who remain in the same group in $t+1$ and $t+5$. Results are based on the 4% worker panel and computed over 1994–2001. High-skill workers include executives, managers, and engineers, while lower-skill workers include clerks, blue-collar workers, supervisors, and technicians.

Figure E.5: Employment by Skill Group, Gender, and Age

(a) By Gender



(b) Managers/Professionals, by Age



(c) Other Employees, by Age



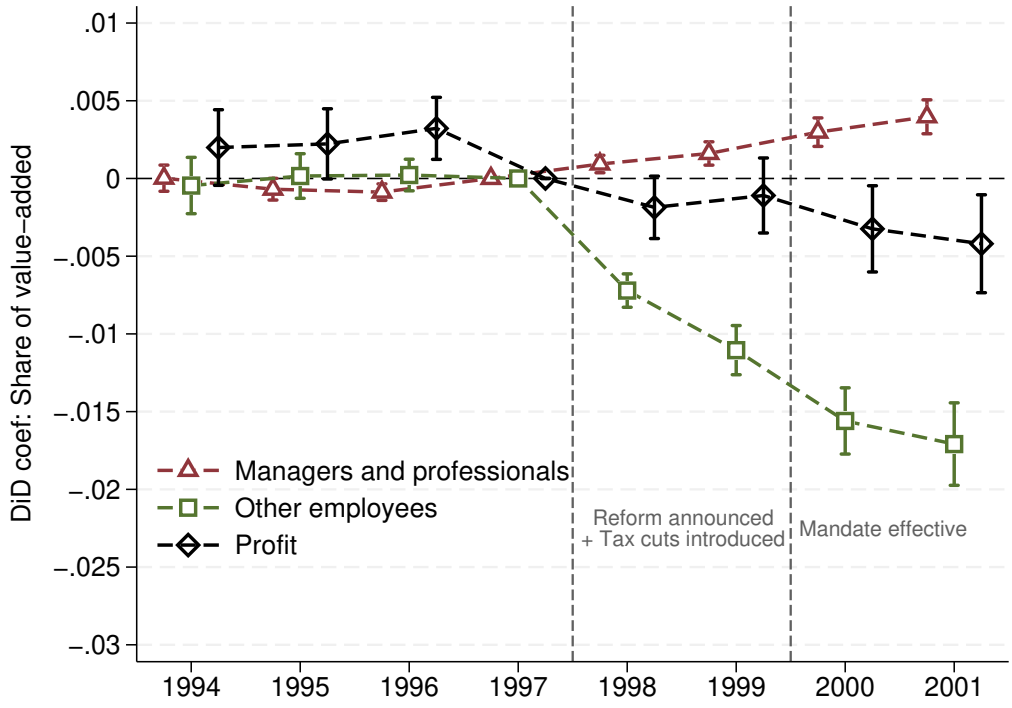
Notes: This figure reports intent-to-treat effects of the legal mandate on firm-level employment by worker type, expressed as a share of total firm employment in 1997. Panel (a) reports effects by gender and occupation, while Panels (b)–(c) report effects by age group and occupation. We report the corresponding difference-in-differences estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure E.6: Worker FE by Skill Group



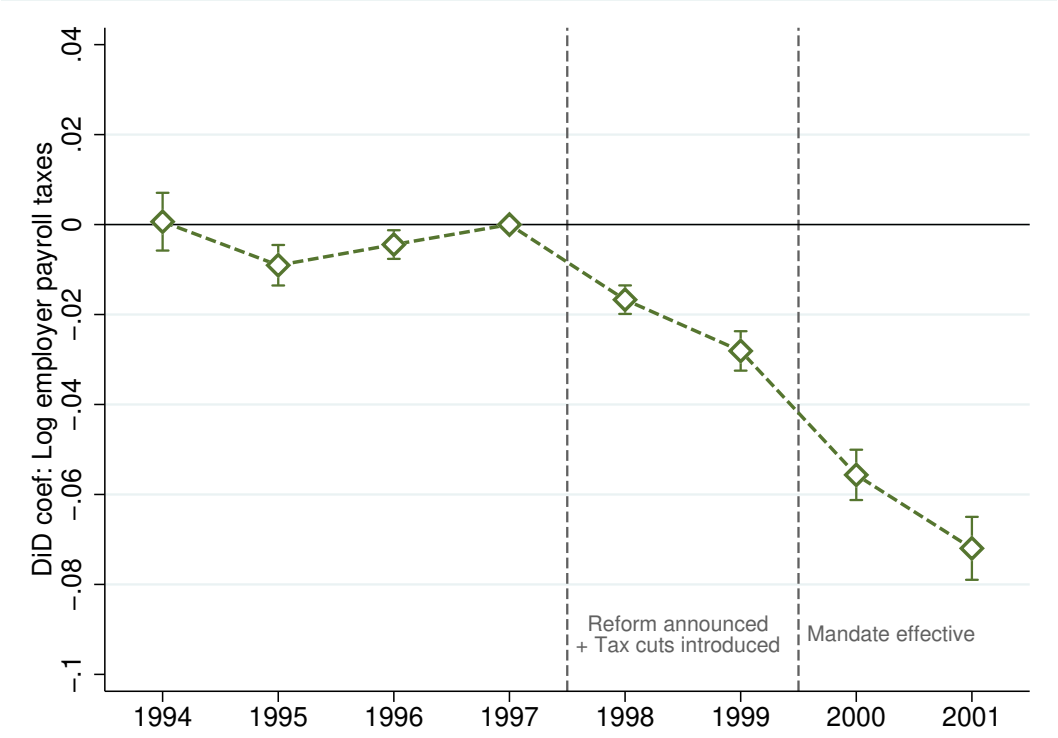
Notes: This figure reports average worker fixed effects among workers employed in mandated and non-mandated firms, separately for managers and professionals and for other employees. Mandated firms are shown by solid lines with circles and non-mandated firms by dashed lines with squares; managers and professionals are shown in dark red and other employees in green. Worker fixed effects are estimated from a two-way fixed-effects regression of wages on worker, firm, and year fixed effects, using the largest connected set of workers and firms in the matched employer–employee data over 1994–1997. We use the 4% worker panel. We then average the estimated worker effects within each firm group and year, weighting by days worked in the firm-year. Because worker fixed effects are constant over time by construction, changes in these averages capture changes in the composition of workers employed by each group of firms. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms.

Figure E.7: Decomposition of Value Added



Notes: This figure reports intent-to-treat effects of the legal mandate on the firm-level high-skill wage bill, lower-skill wage bill, and profits, all as a share of pre-reform value added. We report the corresponding difference-in-differences estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure E.8: Log Total Employer Payroll Taxes



Notes: This figure reports intent-to-treat effects of the legal mandate on firm-level log employer payroll taxes. We report the corresponding difference-in-differences estimates of α_τ from Equation 6. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

F Decomposition of the Reform: Additional Results

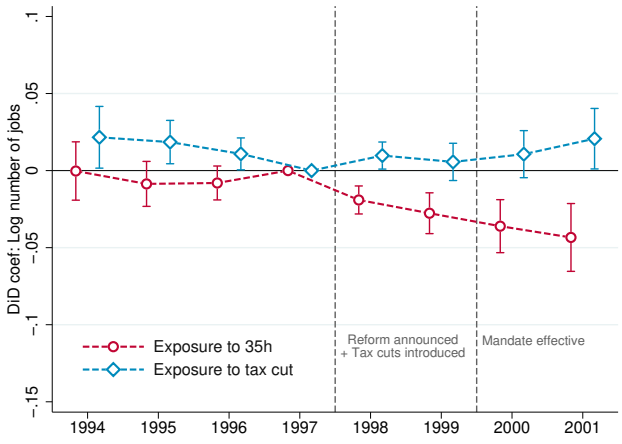
Table F.1: Conditional LATE and ITT

	(1)	(2)	(3)	(4)	(5)
	All	High 35h exposure	Low PTC exposure	High 35h × Low PTC	Low 35h × Low PTC
Panel A. Intent-to-treat (DiD)					
$\Delta_Y^{\text{ITT}}(x)$: DiD on employment	-0.001	-0.012*	-0.020***	-0.039***	0.001
	(0.004)	(0.007)	(0.007)	(0.012)	(0.017)
$\Delta_{H35}^{ITT}(x)$: DiD on 35h indicator	0.630***	0.740***	0.640***	0.723***	0.431***
	(0.004)	(0.005)	(0.007)	(0.009)	(0.017)
$\Delta_{TC}^{ITT}(x)$: DiD on tax cut claim	0.299***	0.266***	0.242***	0.241***	0.235***
	(0.003)	(0.006)	(0.006)	(0.009)	(0.014)
Panel B. LATE (Wald-DiD)					
$s^{TC}(x)$: 2SLS on tax cut claim	0.474***	0.360***	0.378***	0.333***	0.547***
	(0.006)	(0.008)	(0.010)	(0.012)	(0.036)
$\theta^{LATE}(x)$: 2SLS on employment	-0.002	-0.017*	-0.032***	-0.054***	0.002
	(0.006)	(0.009)	(0.011)	(0.016)	(0.038)
Observations	117,220	35,166	35,164	14,114	7,418

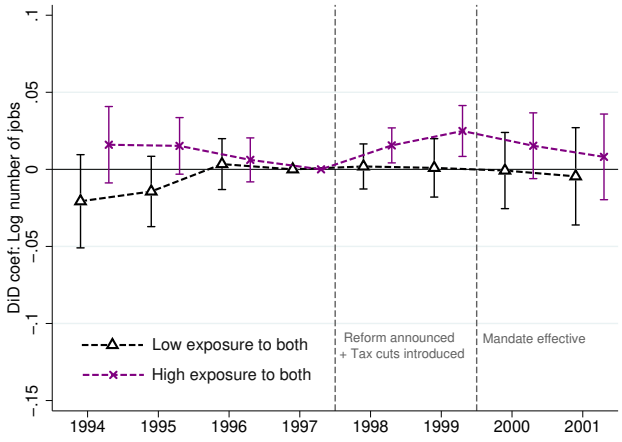
Notes: This table reports intent-to-treat effects of the mandate (Δ^{ITT}) and local average treatment effects of operating under the 35-hour regime (θ^{LATE}) across subsamples defined by firms' exposure to the two components of the reform. Exposure to the workweek reduction is measured by the 1997 share of full-time workers, F_i, and exposure to the payroll tax cut by the tax cut predicted from the firm's 1997 wage distribution, P_i. High and low exposure refer to the top and bottom 30% of the corresponding distributions. Column (1) reports estimates for the full sample. Columns (2) and (3) restrict to firms with high exposure to the workweek reduction and low exposure to the payroll tax cut, respectively. PTC stands for payroll tax cut. Columns (4) and (5) further restrict the low-tax-cut-exposure sample to firms with, respectively, high and low exposure to the workweek reduction. Panel A reports difference-in-differences estimates from Equation (6): $\Delta_Y^{ITT}(x)$ is the effect of the mandate on log employment, $\Delta_{H35}^{ITT}(x)$ its effect operating under the 35-hour workweek, and $\Delta_{TC}^{ITT}(x)$ its effect on payroll tax-cut take-up. Panel B reports two-stage least squares estimates from Equation (11), instrumenting the 35-hour regime with mandate status: $s^{TC}(x)$ is the effect on payroll tax-cut take-up and $\theta^{LATE}(x)$ its effect on log employment. All estimates compare 2001 with 1997. All specifications include firm fixed effects, industry-by-year fixed effects, and county-by-year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure F.1: Decomposition of Mandate Effects: Reduced-Form Estimates by Exposure Group

(a) High Exposure to One Component, Low Exposure to the Other



(b) High or Low Exposure to Both Components



Notes: This figure reports intent-to-treat effects of the legal mandate by exposure group, estimated from a saturated specification interacting mandate status, year indicators, and binned measures of exposure to the two components of the reform. Exposure to the workweek reduction is measured by the 1997 share of full-time workers, while exposure to the payroll tax cut is measured by the tax cut predicted from the 1997 wage distribution. High and low exposure correspond to the top and bottom 30% of the respective distributions. All coefficients in Panels (a) and (b) are estimated jointly. Panel (a) reports estimates for firms with high exposure to the workweek reduction and low exposure to the tax cut, and for firms with low exposure to the workweek reduction and high exposure to the tax cut. Panel (b) reports estimates for firms with low exposure to both components and for firms with high exposure to both. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

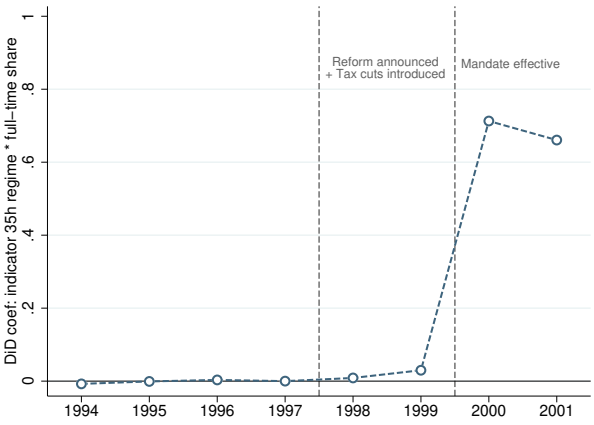
Table F.2: LATE Decomposition: First-Stage Results

	$F/\bar{F} \times H35$	$P/\bar{P} \times TC$	$(F-\bar{F}) \times P \times TC$
After × Mandated	-0.413***	-0.407***	0.218***
	(0.051)	(0.063)	(0.031)
After × Mandated × $F/\bar{F}$	1.035***	0.236***	-0.201***
	(0.051)	(0.058)	(0.029)
After × Mandated × $P/\bar{P}$	0.038**	0.478***	-0.021***
	(0.016)	(0.015)	(0.003)
After × Mandated × $(F-\bar{F}) \times P$	-0.104*	-0.377***	0.577***
	(0.061)	(0.081)	(0.040)
After × $F/\bar{F}$	0.366***	0.036	-0.069***
	(0.039)	(0.037)	(0.019)
After × $P/\bar{P}$	0.097***	0.110***	-0.006**
	(0.013)	(0.010)	(0.002)
After × $(F-\bar{F}) \times P$	-0.069	-0.159***	0.242***
	(0.047)	(0.050)	(0.026)
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Sanderson–Windmeijer F	523.11	404.28	510.91
Observations	117,132	117,220	117,220

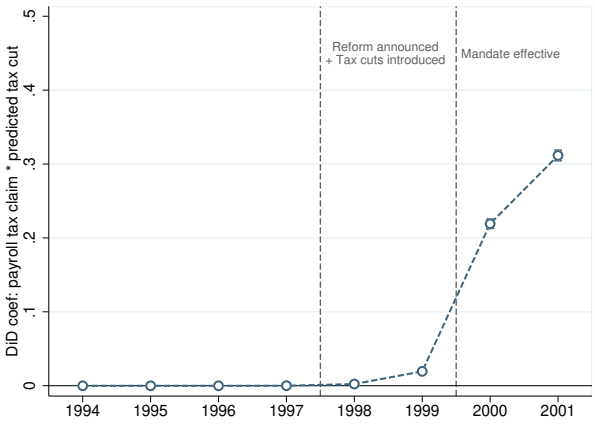
Notes: This table reports the first-stage estimates for equation (17). The three endogenous regressors are shown in the column headings. The four excluded instruments are reported in the first four rows; the remaining regressors are controls. F denotes the share of full-time jobs in the firm in 1997, and $\bar{F}$ its sample mean. P denotes the predicted payroll tax cut per hour based on 1997 wages, and $\bar{P}$ its sample mean. $H35$ is an indicator for operating under 35-hour regime, and TC an indicator for tax-cut take-up. The estimation uses observations from 1997 and 2001, with *After* indicating 2001. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size. Standard errors clustered at the firm level are reported in parentheses. The joint Kleibergen–Paap rk Wald F-statistic is 169.27. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure F.2: First Stage: Exposure-Weighted Take-Up

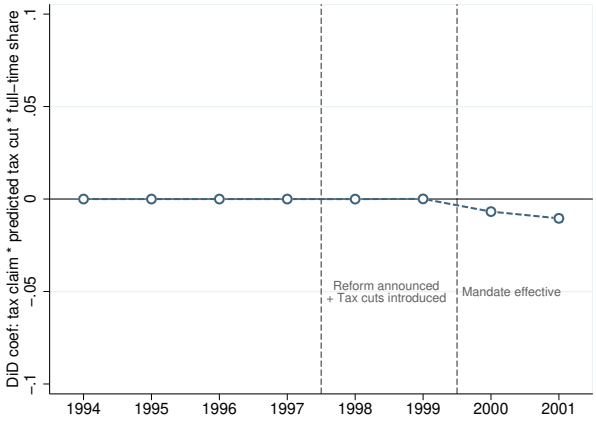
(a) DiD$^{F\times H35}$



(b) DiD$^{P\times TC}$



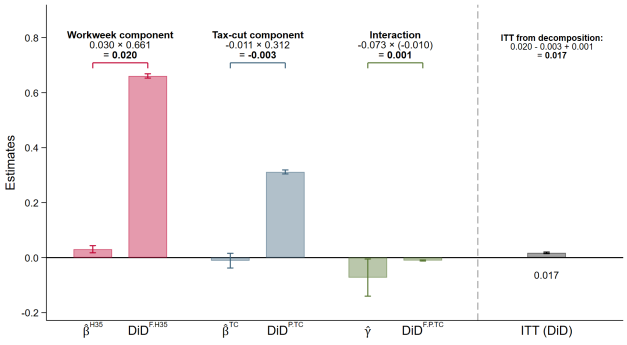
(c) DiD$^{F\times P\times TC}$



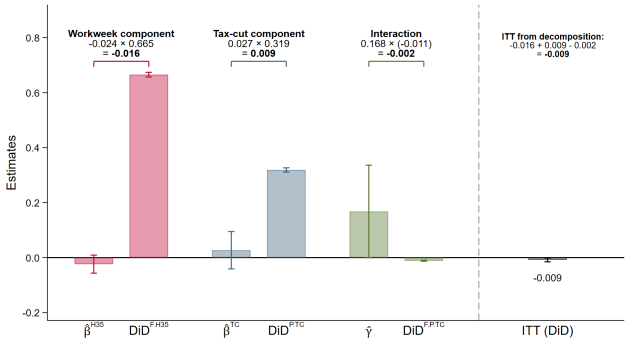
Notes: This figure reports the intent-to-treat effects of the legal mandate on the three treatment components entering the decomposition in specification 18. The estimates correspond to the difference-in-differences coefficients α_τ from Equation (6). In Panel (a), the outcome is 35-hour regime ($H35$) multiplied by exposure to the workweek reduction, measured by the firm's 1997 full-time share normalized by its sample mean. In Panel (b), the outcome is tax-cut take-up (TC) multiplied by exposure to the tax cut, measured by the predicted tax cut based on 1997 wages normalized by its sample mean. In Panel (c), the outcome is tax-cut take-up multiplied by both exposure measures; in this interaction, the full-time share is centered around its sample mean. Whiskers denote 95% confidence intervals. The two vertical dashed lines indicate the reform announcement and the implementation deadline for large firms. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

Figure F.3: Decomposition of ITT

(a) Hourly Wage



(b) Value Added



(c) TFP



Notes: This figure decomposes the intent-to-treat effect of the legal mandate on firm-level hourly wage (Panel (a)), value added (Panel (b)), and TFP (Panel (c)) following Equation 18. To the left of the vertical dashed line, each pair of bars reports two components: $\hat{\beta}^{H35}$ and $\text{DiD}^{F\times H35}$ for the workweek reduction, $\hat{\beta}^{TC}$ and $\text{DiD}^{P\times TC}$ for the tax cut, and $\hat{\gamma}$ and $\text{DiD}^{F\times P\times TC}$ for their interaction. The product of each pair, reported above the bars, gives that term's contribution to the intent-to-treat effect. The sum of the three contributions gives the intent-to-treat effect implied by the decomposition. To the right of the dashed line, we report both this sum and the intent-to-treat effect estimated directly from Equation 6. All estimates compare outcomes between 1997 and 2001. Whiskers denote 95% confidence intervals. The sample is our main balanced panel of private-sector firms with 10–18 or 22–48 employees, based on their 1997 size.

G Longer-Run Evidence

G.1 Empirical Strategy

The estimates in Sections 5 and 6 capture the effects of operating under the 35-hour regime up to two years after adoption. This appendix extends the analysis to a longer horizon.

From January 2002, all firms were formally subject to the 35-hour regime. However, some firms never implemented the reduction for institutional reasons (see Section 8.1). This has two implications for our empirical strategy.

First, we can no longer rely on $H35$, which incorporates legal mandate status and exhibits little variation after 2002, by construction. Instead, we define a measure of revealed adoption, $Adoption_{it}$, based solely on the within-firm distribution of working hours. Specifically, $Adoption_{it} = 1$ if the modal workweek is below 38 hours. Figure G.1 reports the reduced-form relationship between mandate status and revealed adoption, estimated using the event-study specification in Equation (6) on the balanced 1994–2003 panel. Mandate status continues to predict adoption after 2001, while the 1994–1996 placebo coefficients remain close to zero.

Second, a Wald-DiD ratio estimated in 2002 or 2003 would compare firms with different durations of exposure to the shorter workweek. We therefore instrument time since adoption, rather than adoption itself, to estimate effects at different horizons.

Sample and Definitions. We keep the firm-size bands of the baseline, with mandate status defined by 1997 FTE employment, and extend the panel to a balanced sample of firms present in all years from 1994 to 2003. Because adoption must be dated, we restrict attention to firms that did not operate under a 35-hour workweek before 2000. For these firms, let $a_i \geq 2000$ denote the first year in which $Adoptionit = 1$, and define the horizon $E_{it} = t - a_i + 1$ for $t \geq a_i$ and $E_{it} = 0$ otherwise, so that the adoption year counts as the first year under the 35-hour workweek. We group horizons into two bins,

$$D_{it}^{1\text{–}2} = \mathbb{1}[E_{it} \in \{1, 2\}], \qquad D_{it}^{3\text{–}4} = \mathbb{1}[E_{it} \in \{3, 4\}].$$

Since $a_i \geq 2000$ and the panel ends in 2003, no firm is observed beyond its fourth year, so the two indicators and $E_{it} = 0$ partition all firm-years. Mirroring the two-period design of Equation (11), the estimation sample consists of the reference year 1997 and the post-mandate years 2000–2003.

Specification. Let $P_1 = \{2000, 2001\}$ and $P_2 = \{2002, 2003\}$. We estimate by two-stage least squares

$$y_{it} = \theta_{1\text{–}2}\, D_{it}^{1\text{–}2} + \theta_{3\text{–}4}\, D_{it}^{3\text{–}4} + \mu_i + \rho_{rt} + \phi_{ct} + \varepsilon_{it}, \tag{27}$$

$$D_{it}^{k} = \gamma_1^k\, S_i \times \mathbb{1}[t \in P_1] + \gamma_2^k\, S_i \times \mathbb{1}[t \in P_2] + \alpha_i^k + \eta_{rt}^k + \psi_{ct}^k + \upsilon_{it}^k, \qquad k \in \{1\text{–}2,\ 3\text{–}4\}. \tag{28}$$

The two endogenous horizon indicators are instrumented by mandate status interacted with the two post-mandate periods, so the system is exactly identified. As in Equation (6), μ_i is a firm fixed effect and ρ_{rt} and ϕ_{ct} are industry-by-year and county-by-year fixed effects; standard errors are clustered at the firm level. The coefficient $\theta_{1\text{–}2}$ is the effect of having adopted the 35-hour workweek for one to two years and $\theta_{3\text{–}4}$ the effect after three to four years; as in Section 4.3, both include the firm's associated decision to claim the payroll tax cut.

Identification. For any variable X, let $DiD^X_{P_p}$ denote the average over $t \in P_p$ of the contrast DiD^X_t defined in Equation (5), taken net of the fixed effects. Because the model is exactly identified, the 2SLS estimates solve the two moment conditions

$$DiD^Y_{P_p} = \theta_{1\text{–}2}\, DiD^{D^{1\text{–}2}}_{P_p} + \theta_{3\text{–}4}\, DiD^{D^{3\text{–}4}}_{P_p}, \qquad p = 1, 2. \tag{29}$$

This system is triangular. In P_1, no firm can have reached its third year, so $DiD^{D^{3\text{–}4}}_{P_1} = 0$ and the first condition identifies $\theta_{1\text{–}2}$ as the Wald-DiD ratio of Section 4.3 pooled over 2000 and 2001: the effect for mandated compliers during their first two years under the 35-hour workweek. In P_2, mandated firms that adopted at the 2000 deadline are in their third or fourth year, whereas non-mandated firms that adopted at their own 2002 deadline are in their first or second year: the mandate raises the share of firms in $D^{3\text{–}4}$ and lowers the share in $D^{1\text{–}2}$. The second condition therefore identifies

$$\theta_{3\text{–}4} = \frac{DiD^Y_{P_2} - \theta_{1\text{–}2}\, DiD^{D^{1\text{–}2}}_{P_2}}{DiD^{D^{3\text{–}4}}_{P_2}}. \tag{30}$$

The 2002–2003 contrast is purged of the short-horizon effect accruing to late adopters, valued at the $\theta_{1\text{–}2}$ recovered from the earlier period, and the remainder is attributed to the longer horizon. This is the sense in which the estimates are chained across cohorts.

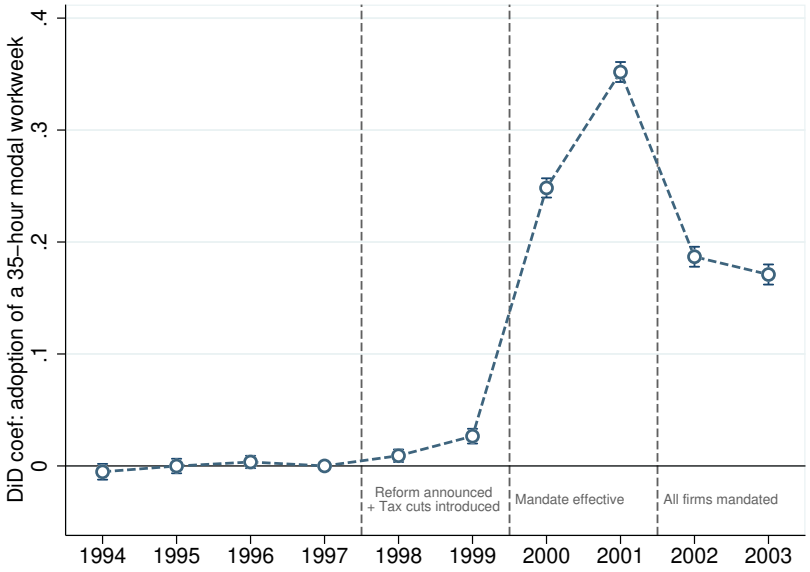
Identification requires assumptions (A1)–(A7) of Section 4 to hold through 2003 on the balanced 1994–2003 panel, together with two additional restrictions. First, *cohort-invariant horizon effects*: conditional on the fixed effects, the effect of having operated under the 35-hour workweek for a given number of years is the same for firms scheduled to adopt in 2000 (mandated) and in 2002 (non-mandated). This is what links the two cohorts in Equation (30). Second, the exclusion restriction extends to the second instrument: after 2001, mandate status affects outcomes only through the timing of adoption. This is plausible because, from 2002, the statutory workweek was the same for all firms and, from 2003, the Fillon payroll tax reductions depended on neither mandate status nor adoption. Monotonicity correspondingly applies to timing: the mandate never delays adoption.

The timing of the Fillon reductions matters for interpretation. $\theta_{3\text{–}4}$ is identified from outcomes in 2002 and 2003, when the adoption-contingent subsidies of the Aubry laws were being replaced by unconditional ones. Whereas $\theta_{1\text{–}2}$ bundles the workweek reduction with the tax cut, as does the

LATE of Section 5.5, $\theta_{3\text{–}4}$ progressively strips out the tax-cut component. It therefore provides an independent benchmark for the decomposition of Section 6, which isolates the effect of the workweek reduction from cross-sectional variation in exposure rather than from the withdrawal of the subsidy.

G.2 Results

Figure G.1: Four Years After Reform: Adoption of 35-Hour Workweek based on Modal Hours



Notes: This figure reports the intent-to-treat effects of the legal mandate on adoption of the 35-hour workweek. We report the corresponding difference-in-differences estimates of α_τ from Equation (6), extending the specification through 2003. The *Adoption* indicator is defined using the mode of observed working time within the firm. Whiskers denote 95% confidence intervals. The three vertical dashed lines indicate the reform announcement, the implementation deadline for large firms, and the implementation deadline for small firms. The sample is a balanced panel of private-sector firms observed from 1994 to 2003, with 10–18 or 22–48 employees based on their 1997 size.

Table G.1: Effects by Time Since Adoption

	(1) Workweek	(2) Hourly wage	(3) Hourly labor cost	(4) Total hours	(5) Employment	(6) Value added	(7) Value added per hour	(8) TFP
Adoption 1–2 years ago	-5.4378***	0.0331***	0.0097**	-0.1760***	-0.0111	-0.0388***	0.1176***	0.0876***
	(0.1399)	(0.0045)	(0.0045)	(0.0105)	(0.0103)	(0.0103)	(0.0079)	(0.0084)
Adoption 3–4 years ago	-4.4260***	0.0461***	0.0134**	-0.1632***	-0.0309*	-0.0519***	0.0679***	0.0512***
	(0.1709)	(0.0055)	(0.0056)	(0.0166)	(0.0166)	(0.0163)	(0.0107)	(0.0122)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen–Paap F stat	3,853	3,853	3,853	3,853	3,853	3,499	3,499	3,038
Observations	187,930	187,930	187,930	187,930	187,930	157,320	157,320	135,305

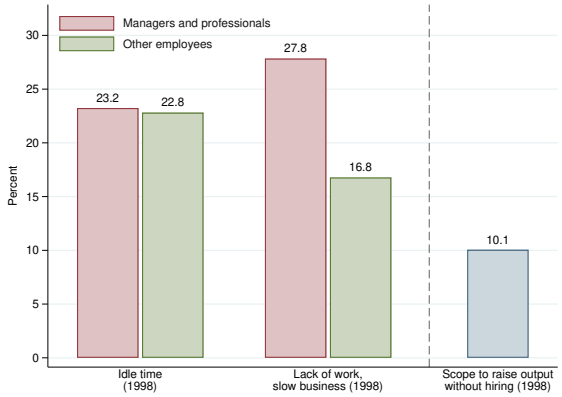
Notes: This table reports 2SLS estimates of the effects of time since adoption of the 35-hour workweek from Equation (27). The first row reports the effect after one to two years under the 35-hour workweek, and the second row the effect after three to four years. The two horizon indicators are instrumented by mandate status interacted with indicators for 2000–2001 and 2002–2003, respectively. The sample is a balanced panel of private-sector firms observed from 1994 to 2003, restricted to firms that had not adopted the 35-hour workweek before 2000; the estimation uses 1997 and 2000–2003. All specifications include firm, industry-by-year, and county-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

H Survey Evidence on the Mechanisms

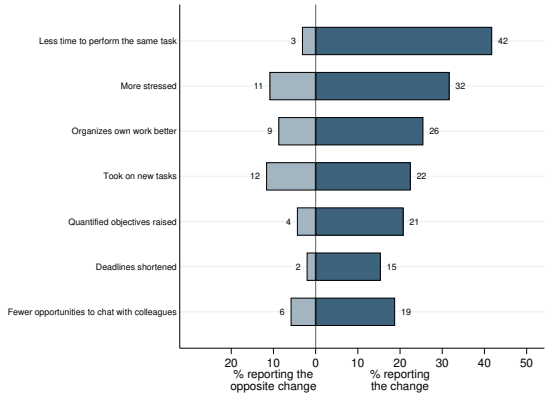
H.1 Organizational Changes

Figure H.1: Evidence on Organizational Changes

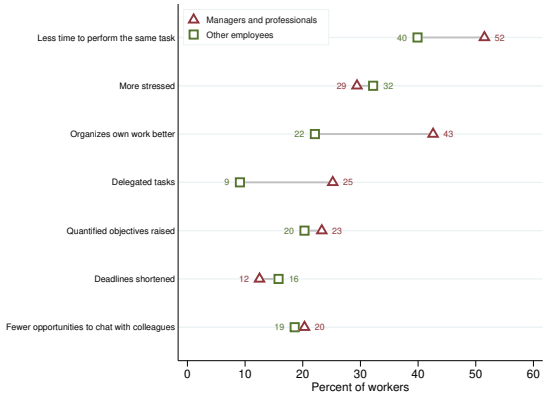
(a) Idle Time Pre-reform



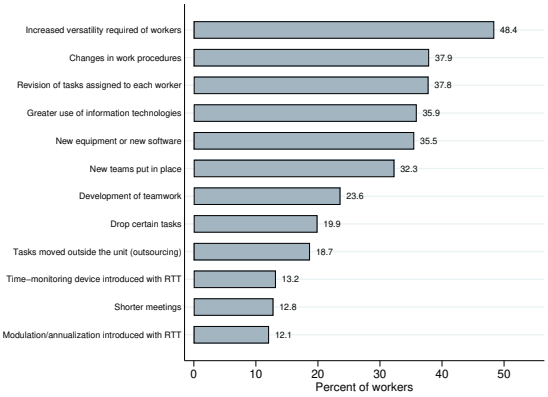
(b) Worker-Level Evidence on Intensification



(c) Heterogeneity in Intensification by Skill



(d) Organizational Changes

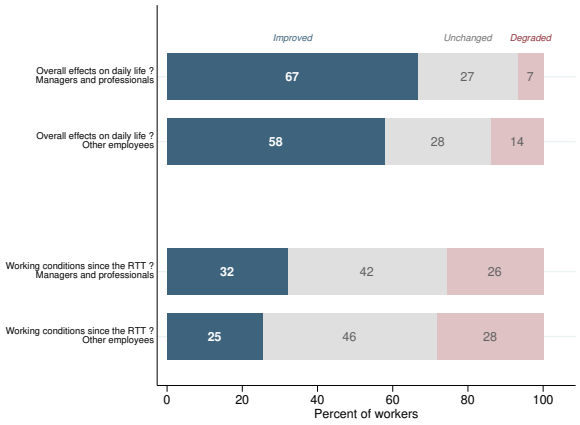


Notes: This figure presents evidence on mechanisms from several surveys described in Section 3.1. Panel (a) shows, by skill group, the share of workers reporting periods of idle time at work and the share reporting sometimes having too little work because of slow business, using the 1998 Working Conditions Survey. On the right, it also reports the average increase in output that manufacturing employers state they could achieve without additional hiring, from the 1998 INSEE *Enquête de conjoncture dans l'industrie*. Panels (b)–(d) use the DARES survey on the effects of the reform. Panel (b) reports, for each dimension of work, the share of workers reporting the change indicated by the row label (right of zero) and the share reporting the opposite change offered in the same question (left of zero). Responses are mutually exclusive: workers select one of the two changes or "no change," so the two bars need not sum to 100; the residual corresponds to no change. Two items are asked only of restricted subsamples: quantified objectives, for workers subject to such objectives (143 managers and professionals, 696 other employees), and shortened deadlines, for workers subject to production norms or same-day deadlines (98 managers and professionals, 770 other employees). Panel (c) reports the same items as Panel (b), separately for managers and professionals and for other employees; the row on delegated tasks replaces the new-tasks response shown in Panel (b). Panel (d) reports the share of workers reporting each organizational change in their unit since the workweek reduction.

H.2 Worker Satisfaction

Figure H.2: Worker Satisfaction

(a) Effects on Daily Life



(b) Effects on Hours Satisfaction



Notes: This figure reports workers' assessments of the effects of the 35-hour reform. Panel (a) reports, by skill group, the share of workers stating that the reform improved, left unchanged, or worsened their overall daily life and, separately, their working conditions, using the DARES survey on the reform. Panel (b) reports, by skill group, the share of workers satisfied with their working time, separately for workers in firms that had implemented a workweek reduction and those in firms that had not, using the 2001 supplement to the Labor Force Survey.

Discussion on Welfare Effects. We discuss which parameters of a utility function could rationalize positive welfare effects for lower-skill workers. For managers and professionals, employment increases and earnings are unchanged at lower hours, so the effect is non-negative under any preference in which utility decreases in hours.[41] We discuss the welfare effects for other employees. Let us assume that workers have MaCurdy (1981) preferences over consumption c and weekly hours h,

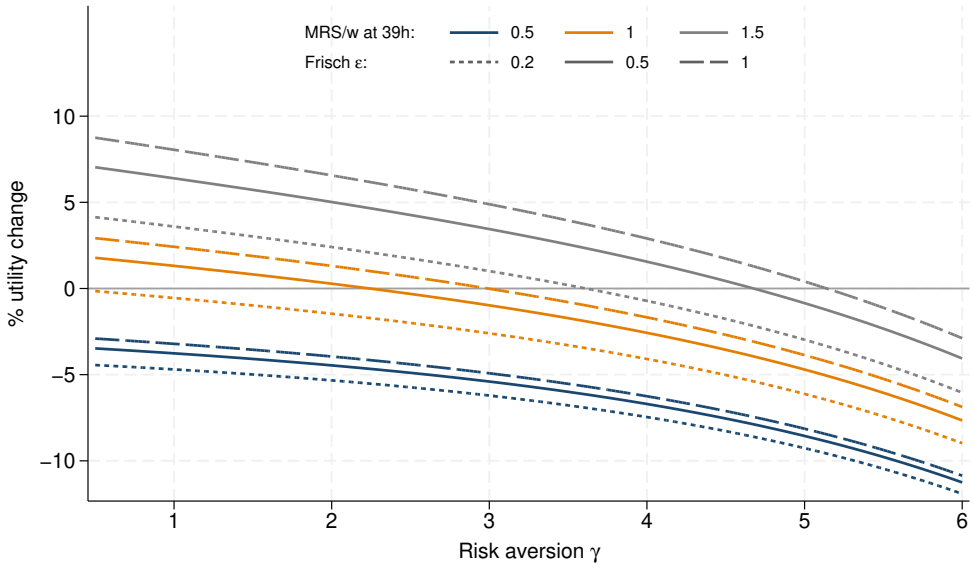
$$u(c,h) = \frac{c^{1-\gamma}}{1-\gamma} - \kappa \frac{h^{1+1/\varepsilon}}{1+1/\varepsilon}, \tag{31}$$

where γ is the curvature of utility over consumption (risk aversion), ε is the Frisch elasticity of labor supply, and κ governs the disutility of work. Consumption equals earnings: an employed worker consumes wh, and an unemployed worker consumes a fraction b of the earnings of a worker at post-reform hours and supplies no hours.

Before the reform, a worker is employed with probability p at hours $h_0 = 39$ and wage w_0. The reform lowers employment by 4.1%, so the employment probability becomes $0.959\,p$, and moves incumbents to $h_1 = 35$ hours at an hourly wage $w_1 = 1.04\,w_0$ (Figure D.4). The change in expected

[41] This assumes that we are holding work intensity fixed or that its effects on welfare are second-order. This is consistent with evidence of increased satisfaction for many managers in Section 8.3.

Figure H.3: Simulations: Welfare Effects



Notes: This figure reports simulated welfare effects of the workweek reduction for workers who are neither managers nor professionals using the preferences and expected-utility calculation described above. The horizontal axis varies the curvature of utility over consumption, γ. Lines correspond to combinations of the Frisch elasticity $\varepsilon \in \{0.2, 0.5, 1\}$ and the pre-reform marginal rate of substitution relative to the wage, $MRS/w \in \{0.5, 1, 1.5\}$. The simulation assumes hours fall from 39 to 35, hourly wages rise by 4%, employment falls by 4.1%, and the unemployment-benefit replacement rate is 0.6. Welfare changes are expressed per initially employed worker and, under the normalization described in the text, can be read as a percentage of post-reform earnings.

utility is

$$\Delta U = 0.959\, p\, u(w_1 h_1, h_1) + (1 - 0.959\, p)\, u(b\, w_1 h_1, 0) - \Big[p\, u(w_0 h_0, h_0) + (1-p)\, u(b\, w_1 h_1, 0)\Big], \quad (32)$$

which simplifies to

$$\Delta U = p\Big\{\underbrace{u(w_1 h_1, h_1) - u(w_0 h_0, h_0)}_{\text{gain of stayers}} - 0.041\ \underbrace{\big[u(w_1 h_1, h_1) - u(b\, w_1 h_1, 0)\big]}_{\text{cost of job loss}}\Big\}. \quad (33)$$

We plot $\Delta U/p$, the change per initially employed worker, with consumption normalized so that post-reform earnings equal one; since $u_c(1) = 1$ for every γ, the vertical axis reads as a percentage of earnings. We set $b = 0.6$ (replacement rate of UI). In Figure H.3, we vary γ on the horizontal axis. Rather than fixing κ, we index it by the marginal rate of substitution between leisure and consumption relative to the wage at the pre-reform allocation, $MRS/w = \kappa h_0^{1/\varepsilon}(w_0 h_0)^{\gamma}/w_0$: a value of 1 means workers were at their preferred hours at 39, a value below 1 that they wanted to work more, above 1 that they wanted to work less. Lines differ by $MRS/w \in \{0.5, 1, 1.5\}$ and $\varepsilon \in \{0.2, 0.5, 1\}$.