Working Time Reduction, Employment, and Productivity: Evidence from France's 35-Hour Reform

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January 31, 2026

Abstract

This paper estimates the causal effects of a large decrease in statutory work hours: France's 35-hour reform. The policy mandated a pay-preserving cut in the standard workweek from 39 to 35 hours, paired with payroll tax cuts for low-wage workers. We exploit the staggered implementation of the reform by firm size between 2000 and 2002 and combine matched employer–employee records, balance-sheet data, and worker surveys. At the firm level, total hours decreased by 5.9%, via shorter workweeks for full-time employees and a shift toward part-time jobs. Headcount remained unchanged, ruling out sizable work-sharing. This null employment effect masks unintended distributional consequences. For high-skilled workers, fewer hours were offset by a rise in headcount, while, for lower-skilled workers, the reform reduced both hours and employment. Analyses at the commuting zone level confirm these findings. Separating the effects of the hours mandate and the tax cuts reveals that the latter offset what would otherwise have been large employment losses. Finally, the reform led to a 1% decline in value added, partly mitigated by a 3% increase in total factor productivity.

JEL Codes: J08, J23, J41.
Keywords: Working time regulations, wage inequality, payroll taxes.

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1 Introduction

Statutory limits on working time are a core instrument of labor regulation in many advanced economies (Gethin & Saez, 2025). Over the past decades, several OECD countries have implemented sizable pay-preserving cuts in their standard workweek. For example, Portugal went from 44 to 40 hours in 1996, Italy from 48 to 40 hours in 1997, and France from 39 to 35 hours in 1998.[1] In 2025, Chile and Colombia legislated reductions of, respectively, 5 and 6 hours per week, while proposals for shorter weeks have faced strong resistance elsewhere—for instance, Spain's parliament rejected a bill to cut the standard workweek from 40 to 37.5 hours, and similar initiatives have stalled in Mexico and the United Kingdom.

These divergent trajectories, even among countries with similar income levels, reflect limited consensus on the effects of hours reductions. The debate, both among policymakers and economists, crystallizes around three main outcomes: employment, output and productivity. On employment and output, proponents argue that shorter hours promote work-sharing and increase employment by spreading workloads across more workers and, ultimately, keeping output unchanged. Critics counter that, as hourly labor costs rise, firms may instead cut output, intensify work, or substitute capital for labor, ultimately reducing employment. Productivity effects are equally contested: supporters point to gains in productivity per hour (e.g., via reduced fatigue, better time management, or organizational adaptation), whereas critics argue that if productivity exhibits increasing returns to hours worked (e.g., through task-specific learning), then dividing work across a greater number of shorter-hour jobs may reduce overall efficiency.

Our paper provides causal evidence on the employment, output and productivity effects of a large reduction in statutory work hours: France's landmark 35-hour workweek reform. Enacted in 1998, the reform reduced the statutory workweek from 39 to 35 hours, while requiring that workers' weekly pay remains unaffected. This implied an 11.4% nominal increase in the hourly wage for full-time incumbent workers transitioning from 39 to 35 hours. To mitigate the cost for employers, the reform included payroll tax reductions, targeted at low-wage workers. Firms could only access this fiscal compensation if they signed a collective agreement governing the terms of the workweek reduction.

We evaluate the joint impact of the statutory workweek reduction and the associated fiscal

[1] In the United States, Roosevelt introduced some of the earliest large-scale reforms: a 35-hour workweek under the President's Reemployment Agreement (PRA) between 1933 and 1935, followed by a 40-hour workweek established in the Fair Labor Standards Act of 1938 (Costa, 2000) As described in Goldin (1988), state laws mandating daily and weekly maximum hours of work appeared as early as 1848, and by 1921 all but four states had passed such legislation.

compensation using a difference-in-differences strategy that exploits the staggered rollout of the policy across firm sizes. Firms with more than 20 employees were required to comply by January 2000, whereas smaller firms faced a later deadline in January 2002. As a result, the analysis focuses on 2000–2001 as the post-reform period. We compare firms with 10–18 employees in 1997 (control group), to firms with 22–48 employees in the same year (treatment group). Apart from their size, these two groups are highly comparable in workforce composition, pay policies, and working time prior to the reform. Correspondingly, we verify that they exhibit similar pre-trends in all outcomes of interest.

Our analysis draws on multiple data sources. First, we rely on matched employer-employee registers. These comprehensive job-level data come from mandatory reports that all firms must file on their salaried workers. The data include information on annual hours, earnings, number of days of work, occupation code, and several demographic characteristics, such as age and gender. They also provide information on firm sizes, which we use to define our treatment group. Second, we leverage the panel version (1/25th random sample) of the matched employer-employee data. Together, these datasets allow us to study the employment and earnings effects of the reform along the hours and wage distribution, both at the firm and worker level. Third, we merge in data covering the universe of 35-hour workweek agreements signed between firms and union representatives. This dataset provides firm $\times$ year-level information on payroll tax deduction eligibility and allows us to estimate changes in effective labor costs. Finally, we use the annual labor force surveys to compare total hours reported by firms with hours reported by workers, and to examine more detailed measures of changes in workers' time allocation. Last, we use balance sheet data to assess the effect of the reform on firms' production, productivity and profitability.

We begin by documenting a strong first stage. Treated firms experienced a 2-hour decline in average weekly hours per worker relative to control firms, with a large shift in the distribution of these hours that mirrors the change in the statutory workweek: the pre-reform mass at 39 hours nearly disappears, while new concentrations emerge at 35 hours and among shorter, part-time jobs. Although the 2-hour decline is substantial, it is smaller than the 4-hour statutory change, reflecting the continued use of overtime among some full-time workers and the fact that a non-negligible share of control firms signed 35-hour agreements. Because monthly pay was legally preserved, this decrease in hours mechanically increased the hourly wages for full-time incumbents. However, the firms' average hourly wage only increased by 2.1% by 2001. This relatively modest increase reflects the combination of three factors. First, it averages across full-time and part-time workers, the latter not being directly targeted by the

reform. Second, we have an intent-to-treat design, in which some control firms grew and became subject to the new regulation and some voluntary adopted the 35h workweek, seeking access to payroll subsidies. Finally, we find evidence of wage moderation: firms offset part of this cost by slowing wage growth in the years between announcement, in 1998 and enforcement, in 2000.

Turning to employment, we find no evidence that the reform generated work-sharing. Headcount in treated and control firms follow identical paths both before and after the reform, with no time discontinuity around the reform. In our firm-level difference-in-differences, the point estimates for employment are close to zero, and we can rule out employment effects larger than $\pm$ 0.9%. Worker-level estimates confirm that the probability of remaining employed was unaffected. At the same time, total hours worked in treated firms fall by 5.9% relative to controls. Decomposing this decline, two-thirds come from the decrease in hours above 35 per week and a third from lower hours in jobs at or below 35 hours. In other words, the reform both truncates the right tail of the hours distribution and reduces hours even in the part-time range, reinforcing the conclusion that work-sharing did not materialize.

To disentangle the effects of the workweek reduction from the effects of the payroll tax cuts, we leverage two additional sources of variation: the predicted tax cuts based on pre-reform wage distribution and the pre-reform share of full-time workers. The intuition of our policy decomposition design is that firms with a higher share of full-time workers pre-reform were more exposed to the hour-reduction component, while firms with a higher share of low-wage workers were more exposed to the payroll tax reduction component. Empirically, we find that there is a large variation across firms in both dimensions of exposure. We exploit this heterogeneity by specifying a quadruple-difference that uses the full set of interactions between treatment status and both dimensions of exposure. We then use the estimated coefficients to construct counterfactual paths under "workweek reduction only" and "payroll tax cut only" scenarios. Using our decomposition design, we estimate that, absent the payroll tax cuts, headcount would have declined by about 5.3% and total hours worked by 11%. Together, these results highlight that the fiscal component played a critical mitigating role, even though it did not succeed in delivering job creation.

The null average employment effect masks substantial distributional consequences. High-skill workers experienced reductions in weekly hours while maintaining monthly pay. Their headcount increased slightly, leaving their firm-level total hours unchanged. By contrast, lower-skilled workers experienced declines in both hours and employment: their headcount fell by 0.9% of total employment, and hours declined by 6.1%. Despite the policy's pay-

preserving mandate, lower-skilled workers in treated firms experienced relative monthly earnings losses of 1.7%. This decrease reflects both a shift to part-time employment, as well as wage moderation.

We then consider mechanisms that can account for these skill-differential effects. We start by ruling out several potential explanations. First, payroll tax cuts were explicitly designed to subsidize low-wage jobs, and should therefore have cushioned, rather than amplified, employment and earnings losses among lower-skill workers. Second, a "different bite" of the reform on high- versus lower-skill positions does not fit the evidence: administrative data and the labor force survey both show that high-skill workers also experience sizeable reductions in weekly hours. In addition, high-skill–intensive firms do not differentially expand relative to other firms; most of the change in the skill mix occurs within firms rather than through compositional shifts between firms. We therefore favor explanations based on heterogeneous rents: if lower-skill jobs are closer to the profitability margin, the increase in hourly labor costs leads firms to shed these positions, consistent with stronger monopsony power and lower rents at the bottom of the skill distribution. This mechanism is particularly plausible in the French context, where the minimum-to-median wage ratio was the highest in the OECD in 1998 (above 60%). Task-specific productivity may also be at play: the relative productivity of high-skill tasks may have increased because these tasks can be reorganized more flexibly, and high-skill workers, including managers, are best positioned to lead the organizational changes required by the shift to a 35-hour workweek.

A natural concern is that our firm-level design might overstate job losses if lower-skill workers displaced from treated firms are absorbed by untreated firms in the same local labor markets. To address this, we complement the firm-level analysis with a commuting-zone (CZ) event-study that exploits variation in the pre-reform share of full-time jobs in large firms (above 20 employees). Going from 0 to 1 in this share captures the difference between local labor markets with little exposure to the 35-hour mandate and those where essentially all jobs were exposed. In such highly exposed CZs, we estimate a decline of 4 weekly hours per job—consistent with the statutory reduction—and large declines in both total hours (25%) and employment (15%) relative to unexposed CZs. The large negative employment effect in this design is consistent with the "workweek reduction only" counterfactual from our decomposition design, in which hours constraints bind but payroll subsidies are absent. Additionally, the CZ-level analysis replicates the skill-heterogeneous pattern: high-skill employment rises while lower-skill employment falls in more exposed local labor markets.

Last, we study the impact of the reform on value added, productivity, and profits. Value

added in treated firms falls modestly relative to controls, with event-study estimates indicating a reduction of 1% by 2001. Given that total hours fall by 5.9%, this modest reduction in value added translates into a sizeable increase in value added per hour—on the order of 4%—and composition-adjusted total factor productivity rises by 3%. These patterns suggest that firms responded to the higher cost of labor per hour not only by cutting hours, but also by reorganizing production, thus limiting the loss in output. However, from the firm's perspective, these efficiency gains were not enough to offset the cost shock: combining the decline in value added with the increase in labor costs yields a 10% reduction in the profit-to-assets ratio.

Our results hold important lessons for labor market regulations and the design of working time reduction policies. First, mandating shorter hours without corresponding demand-side policies leads to aggregate employment losses rather than work-sharing. While payroll tax cuts helped mitigate job losses in our setting, they were not sufficient to induce job creation and represent a significant budget cost. Second, the equity implications of such reforms are not easily anticipated. Despite tax reliefs targeted at low-wage earners, the reform disproportionately benefited higher-skilled workers. Further, implementation details matter: the mandate to preserve overall pay did not prevent the relative slowdown in wage growth, which eventually led to monthly wage losses, especially for low earners, even as hourly wages rose. while policymakers expected substantial productivity gains, we show that these gains only partially cushioned the decline in value added and profits.

Our study complements a growing literature studying workweek reduction reforms. A number of earlier studies of the 35-hour mandate use labor force survey data to assess the impact of the reform.[2] Estevão and Sá (2006) compare small and large firms in the labor force survey and find no employment effect, but report increased turnover and a rise in multiple job-holding. Chemin and Wasmer (2009) compare Alsace-Moselle, where the reform was less binding, to the rest of France. They also find no significant employment impact, though the underlying difference in stringency they exploit is modest (about two fewer workdays per year). Lepinteur (2019) complements this literature by focusing on subjective well-being: using European survey data, he finds that workers report higher satisfaction under shorter workweeks. Several other papers turn to the administrative matched employer-employee data to evaluate the reform's effects. Crépon, Leclair, and Roux (2004) compare firms that chose to implement the 35-hour week early to receive payroll tax subsidies to those that

[2] Askenazy (2013) provides a detailed institutional overview of the 35-hour reform and outlines directions for future research.

did not, using exposure to those subsidies as an instrument. They find that employment increased by 9.9%, while total factor productivity fell by 3.7%. Goux, Maurin, and Petrongolo (2014) study household labor supply, showing that husbands of treated women reduce their working hours, while women do not adjust their labor supply.

Several studies also look at similar workweek reduction reforms in other contexts. Pioneering this literature, Hunt (1999) exploits industry-level reductions in statutory workweek hours in West Germany during the 1980s. Despite a modest rise in employment, the total hours worked fell significantly, suggesting limited worksharing.[3] Crepon and Kramarz (2002) investigate an earlier French reform, finding that the 1982 reduction from a 40- to 39-hour workweek increased job separations. Batut, Garnero, and Tondini (2023) exploit sector-level variation in exposure to several European working-time reforms and find that while average hourly wages rose and hours worked declined, employment and value added per hour were largely unchanged. Fishback, Vickers, and Ziebarth (2024) study the U.S. 1933 President's Reemployment Agreement and find that employment rose by 24% in the month following the workweek regulation.[4] Finally, Raposo and van Ours (2010) and Asai, Lopes, and Tondini (2024) study Portugal's 1996 reduction in the statutory workweek from 44 to 40 hours. Both studies leverage difference in exposure to the reform, e.g., comparing firms that had opted into collective agreements already mandating a 40-hour week prior to the reform to firms that were still operating under a 44-hour contractual workweek. Asai, Lopes, and Tondini (2024) document decreases in employment and output while also observing improvements in hourly productivity, whereas Raposo and van Ours (2010) report reduced turnover and an increase in hourly wages. Taken together, the divergent findings across papers reflect the ongoing debate and the absence of agreement regarding the effects of workweek reductions.

Our paper contributes to this literature in several ways. First, to get precise, causal estimates of the effect of workweek reductions, we combine quasi-experimental variation in exposure to the reform (the firm size threshold) with administrative data. We then provide worker, firm and labor market level estimates, that allow us to account for potential reallo-

[3]This limited worksharing is consistent with Carry (2024) and Quach (2025) who document a lack of substitutability between hours and workers at the firm level.

[4]This paper also connects with the broader literature on short-time work (also known as short-time compensation), a public-policy mechanism designed to prevent layoffs during temporary economic downturns by subsidizing firms to reduce hours while providing income support to affected workers. Evaluations of these programs generally find they preserve employment during downturns. For instance, Cahuc and Carcillo (2011) and Tilly and Niedermayer (2017) show that short-time compensation schemes lower unemployment. Similarly, Giupponi and Landais (2023) use quasi-experimental variation in Italy's scheme during the Great Recession to demonstrate significantly reduced total hours per worker alongside increased headcount employment. However, the literature also highlights potential inefficiencies, such as adverse impacts on temporary workers, and dampened reallocation to more productive jobs (Cooper, Meyer, & Schott, 2017).

cation effects across firms. Second, our decomposition design allows us to disentangle the effects of the workweek reduction from the effect of payroll tax cuts, highlighting the importance of the payroll tax cuts in mitigating what otherwise would be negative employment effects. Third, we broaden the scope of outcomes studied simultaneously, looking at aggregate employment, employment along the hours distribution, wages, sales, and productivity. Finally, we emphasize the importance of examining heterogeneous effects across the skill distribution. Our results show that high-skill workers benefited from the reform while lower-skilled workers experienced declines in both hours and employment.

The second strand of literature we relate to examines the employment effects of legislated wage increases. Most of this work focuses on low-wage workers, particularly in the context of minimum wage policies. While early models predicted large disemployment effects, a growing body of empirical work finds that moderate increases in the minimum wage have small or zero effects on employment (see Dube and Lindner (2024) for a comprehensive survey of this literature). In contrast, the French workweek reform we study generated hourly wage increases for all incumbent full-time workers—regardless of initial wage—by mandating monthly earnings to be preserved while weekly hours were cut. This led to heterogeneous employment effects along the wage distribution: lower-wage workers experienced declines in both hours and employment, while high-skill workers saw small gains in employment and no decrease in their total hours at the firm-level. This heterogeneity is especially striking given that the accompanying payroll tax cuts were targeted at low-wage workers.

Lastly, our paper contributes to a large literature on the effects of payroll tax changes on labor market outcomes. Previous research has shown that payroll tax cuts are primarily capitalized into wages—especially where contributions are tightly linked to individual benefits—with broad-based reforms showing little effect on overall employment (see, e.g., Bozio et al., 2025; Saez, Matsaganis, and Tsakloglou, 2012). Targeted cuts to employer payroll taxes can generate at best modest employment gains for the treated groups (Kramarz & Philippon, 2001; Cahuc, Carcillo, & Le Barbanchon, 2019; Saez, Schoefer, & Seim, 2021). Our policy-decomposition design is consistent with this evidence: under the "payroll-tax-cut-only" counterfactual, we find no effect on employment. Our paper, however, provides an additional insight: when interacted with the workweek-reduction policy, payroll tax cuts play a quantitatively important mitigating role: absent the cuts, we estimate that the reform would have generated negative employment effects of 5 pp. These findings underscore the importance of policy design—specifically, how pairing regulatory mandates with targeted tax relief can help cushion adverse labor market effects.

2 Institutional Background

2.1 Historical and Political Context

Before the advent of the 35-hour workweek, France experimented with reducing working time through the *Loi Robien*, enacted in June 1996 under the conservative Labor Minister Gilles de Robien. The law granted firms incentives to voluntarily reduce working hours—typically by 10%, to around 35–36 hours per week—on the condition that they either hired additional workers or avoided layoffs, in exchange for reductions in employer social security contributions. In a January 1997 interview with *La Croix*, de Robien defended the approach: "this law works because it is incentive-based, it imposes nothing on the social partners, and they understood that everyone could gain". Despite this optimistic framing, the *Loi Robien* achieved only modest results. According to the government's Commission for Statistics and Studies (DARES), only a few thousand conventions (covering 175,000 workers) had been signed by June 1998.[5] Many pointed to its bureaucratic complexity, as well as concerns about the long-term costs and rigidity of reducing hours without corresponding productivity gains. Trade unions were ambivalent: while supportive of working time reduction in principle, they criticized the law's fragmented bargaining structure, fearing it would create uneven labor standards across firms and sectors.

The election of the Jospin government in 1997, supported by a left-wing coalition, ushered in a new phase in working-time policy. In June 1998, Minister of Labor Martine Aubry introduced two reforms that gradually established the mandatory 35-hour workweek. The first, "Aubry I", announced and enacted that same year, offered payroll tax deductions to firms that voluntarily shifted from a 39 to a 35-hour statutory workweek by 2000. While preserving the voluntary spirit of the *Loi Robien*, this reform saw far greater uptake.[6] The second, "Aubry II", announced in 1998 but enacted in 2000, marked the decisive legal shift to a mandatory framework: Firms with more than 20 employees had to comply by January 2000, while smaller firms were granted an additional two years.

The Aubry reforms rested on a dual rationale. First, they embodied a normative argument of social progress, seeking to improve quality of life, facilitate work–life balance, and reinvigorate labor dignity. Martine Aubry herself underscored this dimension with the admonition that "*travailler moins, c'est vivre mieux*" ("work less to live better").[7] Second, the laws were

[5] See Dares Note N° 37.2, Septembre 2000 - "La réduction du temps de travail d'un dispositif à l'autre - Une comparaison des conventions Robien et Aubry".

[6] We discuss our take-up measures in more details in Section 3.

[7] This phrase was later parodied by right-wing President Nicolas Sarkozy during his 2007 presidential cam-

advanced as economic policy tools for work-sharing, aimed at reducing the then-high unemployment rate—around 10.6 percent in 1997—by redistributing labor among more workers. Government projections predicted 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 Aubry reforms sparked a sharply polarized public debate. Trade unions largely embraced the policy, while employer associations warned about the policy's economic repercussions, arguing that statutory hours' reductions would increase labor costs, erode international competitiveness, and impose rigid constraints on firms' ability to adapt to fluctuating production needs.[8]

Following the 2002 presidential and legislative elections, the newly elected right-wing government enacted measures that significantly softened the Aubry reforms. In 2003, the government extended the payroll tax reductions, previously reserved for firms adopting 35-hour agreements, to all firms, including those that had retained the 39-hour workweek. At the same time, the *loi Fillon* raised the annual cap on overtime from 130 to 180 hours, effectively allowing firms to operate at 39 hours—with up to four hours of overtime per week across the year—subject to the standard overtime premium.

As evidenced in our literature review, even years after the reform, academics and policymakers remain divided on the actual effects of the reform. Reflecting on the reform's legacy, Askenazy, 2021 notes that, a quarter of a century later, 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."

2.2 Legal Changes to Wages, Overtime and Payroll Taxes

The Aubry laws required all firms to comply with the new working-time regulation by 2002. For identification, we restrict the treatment period to 1998-2001. This restriction ensures that smaller firms are not yet subject to the mandate, allowing us to isolate the causal effects of the reform from variation in exposure across firm sizes. Accordingly, the legal changes described in this Section refer only to the period from June 1998 to December 2001.

paign. In contrast to the work-sharing rationale of the 35-hour week, Sarkozy promoted a pro-overtime agenda with the slogan "*travailler plus pour gagner plus*" ("work more to earn more").

[8]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".

2.2.1 Hours, Wages and Overtime

As a preamble, it is important to note that the 35-hour reform did not impose a hard cap on weekly working hours. 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 now considered as overtime and paid at a premium. We now dive further into the rules that surrounded workers' wages and overtime.

When a firm adopted the 35-hour workweek (either voluntarily or following the statutory change), employees moving from a 39-hour to a 35-hour contractual schedule without a reduction in weekly pay effectively received an 11.4% increase in their hourly wage. Legally, the mandate to maintain weekly pay, and thus raise hourly wages, applied only to incumbent full-time workers earning up to 1.11 $\times$ the minimum wage. For higher paid workers, there was no statutory obligation. Nonetheless, official policy messaging emphasized "*maintien des salaires pour tous*" ("wage preservation for all"), and in practice most firms adhered to this guideline: according to the government's Commission for Statistics and Studies (DARES), over 90% of Aubry collective agreements explicitly included provisions to maintain weekly earnings for all full-time employees, regardless of wage level.

There was no legal requirement to raise wages for new hires, regardless of their hours, or for part-time workers, except when they were paid at the minimum wage, in which case they automatically benefited from a statutory upward adjustment following the reform (see Section 2.2.3). In some cases, new hires or part-time workers could also benefit if their contracts were deemed comparable to those of affected workers, though in practice such claims were difficult to enforce.

Crucially, the reform did not cap weekly working hours at 35. Instead, it gradually revised the overtime premium schedule. For firms with more than 20 employees (regardless of whether they signed a collective agreement), from January to December 2000, hours worked from the 36^{th} to the 39^{th} were subject to a 10% premium, hours between the 39^{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 from 10% to 25% while the rest of the schedule remained unchanged. For firms with fewer than 20 employees, the pre-reform overtime rules remained in effect, regardless of whether they had signed a workweek reduction agreement. Notably, this meant that small firms were not required to pay an overtime premium for hours between 35 and 39, even if they had adopted a 35-hour collective agreement.

Finally, it is worth recalling the distinction between the two waves of the reform. Under Aubry I, firms could voluntarily adopt a 35-hour contractual workweek through collective agreements, supported by payroll tax deductions. Under Aubry II, the 35-hour statutory workweek became mandatory for large firms first, then small firms, regardless of whether they had signed an agreement. The signature of an Aubry II agreement would however give firms access to a payroll tax subsidy, described in more details in the next subsection.

2.2.2 Payroll Tax Reductions

Aubry I. Under the Aubry I Law of June 13, 1998, firms that voluntarily adopted the 35-hour workweek and signed a collective agreement on working time reduction prior to January 2000 became eligible for a temporary subsidy. This subsidy was a flat, per-worker, per-year payroll tax deduction, granted for a period of five years from the date the reduced working time took effect. The level of aid declined over the five years following a given implementation date, and was (marginally) lower the later the signature of the agreement took place. The financial subsidy applied to all existing and newly hired workers, including part-time employees (on a prorated basis). To qualify for this subsidy, most firms were required to commit to job creation–typically at least a 6% increase in employment–and had to reduce the contractual workweek of full-time workers by at least 10%.[9] This feature is particularly important when interpreting our employment effects in 1998 and 1999, as take-up was tied to hiring commitments. Concretely, suppose a company signed a collective agreement in July 1998 and committed to a 10% reduction in the contractual workweek along with a 6% increase in hiring or equivalent employee preservation. From the moment the 35-hour contractual workweek was implemented, the firm received 9,000 F (this amounts to 2,450 USD in current dollars, or 5.4% of the average annual full-time wage in 1998) per employee for the first year. This subsidy then decreased by 1,000 F (this amounts to 271 USD in current dollars, or 0.6% of the average wage in 1998) per year over a five-year period.

Aubry II. By January 2000, all firms with more than 20 employees were legally required to adopt the 35-hour workweek, while firms with 20 or fewer employees were required to make this shift only by January 2002. Under the Aubry II Law, large firms that signed a collective agreement formalizing their transition to the shorter workweek became eligible for a permanent subsidy. This subsidy was called permanent to contrast with the temporary subsidy

[9] 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.

under Aubry I, which was only applicable for five years.[10] The permanent subsidy consisted of two components, both delivered as payroll tax deductions. The first was a flat subsidy of 4,000 francs per employee per year (this amounts to 1,066 USD in current dollars, or 2.3% of the average annual full-time wage in 2000). The second was a degressive subsidy targeted at low- and middle-wage workers: firms employing minimum-wage earners received a deduction of 17,500 francs per worker annually (this amounts to 4,663 USD in current dollars, or 10.2% of the median wage in 2000), which tapered progressively and fully phased out at 1.73 times the minimum wage. As with the temporary subsidy, amounts were prorated for part-time employees. Importantly, firms were not required to commit to either job creation or job preservation in order to benefit from the permanent subsidy.[11] Appendix Figure A.2 documents the schedule of employer payroll tax cuts before and after the 35-hour reform (Aubry II). Panel (a) plots, as a share of the worker's full-time equivalent wage (normalized by the minimum wage), the pre-existing Juppé subsidy and the new Aubry II schedule. Panel (b) reports the difference between these two curves. Panels (c) and (d), respectively, present the same information in annual 2024 euros. At the minimum wage, the reform raises the subsidy by 1,500 euros per worker per year ($\sim$ 8% of the wage). The incremental tax cut peaks slightly above the minimum wage, at close to 2,700 euros per year (around 11% of the wage), and then declines rapidly with the wage, flattening out at 900 euros per year for workers earning 1.73 the minimum wage or more. This pattern underscores the strong targeting of the reform toward low-wage jobs.

While the statutory workweek remained unchanged for small firms between January 2000 and December 2001, these firms still had the option to voluntarily sign a collective agreement in advance. If they did, they were eligible for the Aubry I temporary subsidy but, contrary to firms that switched before January 2000, they were not subject to any employment commitment, either in hiring or job preservation.

2.2.3 Other Features

Minimum Wage. The *Garanties Mensuelles de Rémunération* (GMR) were introduced under the Aubry II reform to ensure that workers earning the minimum wage (SMIC) or just above it—defined in 2000 as up to 1.11 times the SMIC—did not experience a fall in monthly pay

[10] Smaller firms that voluntarily adopted a 35-hour contractual workweek before 2002 remained eligible to the temporary subsidy, but not for the permanent one.

[11] Firms with more than 20 employees that were already receiving the temporary subsidy from Aubry I could, starting in January 2000, receive both the temporary and the degressive subsidy until the temporary subsidy expired. After that, they could receive the entirety of the permanent subsidy (i.e., both the degressive and the lump-sum subsidy).

when their firm signed a collective agreement reducing the workweek to 35 hours. This guarantee took the form of a differential supplement that topped up the new hourly-based wage so that monthly earnings remained at least equal to what they had been under the old 39-hour minimum wage system. It applied both to full-time workers and, pro-rated, to part-time ones. The GMRs were revalued annually, notably to track with consumer prices. Their level also depended on the date at which a company transitioned to the 35-hour week, leading to the coexistence of several distinct regimes.[12]

Annual Overtime Cap. The statutory annual overtime cap—i.e., the maximum number of hours a worker can legally work beyond the statutory workweek—remained unchanged at 130 hours per year. However, 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). For small firms, there was no change to the overtime cap or its 39 hours weekly threshold applicability during this period, regardless of whether they had signed an agreement.

Workweek Reduction Modalities. The French 35-hour reform allowed firms flexibility in how they reduced working time. Three main modality emerged. First, firms could shorten the length of each workday–typically by 45 minutes to an hour–or, alternatively, lengthen workdays while shifting to a four-day week. Second, some firms introduced regular time off, such as a fixed half-day each week (e.g., Wednesday afternoons, aligning with school schedules) or a full day off every two weeks. Third, the government created the "*Réduction du Temps de Travail*" (RTT) days, which converted hours worked beyond the 35-hour threshold into additional paid leave. In addition, a minority of firms experimented with other arrangements: (i) time-saving accounts ("*comptes épargne-temps*"), which allowed employees to bank extra

[12]Between 1 January 2000 and 31 December 2001, three such regimes were in force: GMR2, covering firms that reduced hours between July 1999 and June 2000 (valued at €1,114.3 as of July 2002); GMR3, covering firms that switched between July 2000 and June 2001 (valued at €1,133.5 as of July 2002); GMR4, covering firms that transitioned between July 2001 and June 2002 (valued at €1,147.5 as of July 2002).

hours for future use as deferred compensation or paid leave, (ii) early retirement programs, or (iii) annualization of working time, whereby redistributing hours across the year to match seasonal fluctuations in activity. We discuss in detail in Section 3 the empirical distribution of of these different modalities, in aggregate and across skill groups.

3 Data and Descriptive Evidence

3.1 Data

To quantify the effects of the workweek reduction on workers and firms, we combine linked employer-employee records with firms' balance sheet data, labor force surveys, and administrative records on workweek agreements over the period 1994-2001.

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 job $\times$ worker, the wage, number of hours worked, and length of the employment spell (in days). The dataset also includes 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 French workers and firms since 1994. Firms are consistently identified across years by their tax identifiers, but worker identifiers are reshuffled every two years in the version accessible to researchers. In complement to the cross-sectional data, we leverage a worker-level panel version (*DADS Panel*) which tracks a 1-in-25 random sample of individuals born in October of even-numbered years since 1976.

Workweek Agreements. A key data innovation in this paper, relative to prior studies on the French 35-hour reform, is the use of administrative records covering the universe of collective agreements signed by firms to qualify for payroll tax reductions under the reform. For each firm, we observe the exact implementation date of the reduced workweek, the applicable subsidy regime (Aubry I or Aubry II), and the conditions for eligibility (e.g., employment creation). Merging these data with the matched employer–employee records allows us to compute worker-year level estimates of payroll tax deductions associated with the signature of an Aubry I or Aubry II agreement in their firm. This information is critical to our empirical

strategy, detailed in Section 4, which exploits variation in the timing of adoption by firm size: firms with more than 20 employees were mandated to switch to a 35h statutory workweek by 2000, while smaller firms were only required to comply in 2002. Nevertheless, some small and large firms adopted the 35-hour workweek early under Aubry I, and some small firms voluntarily transitioned to 35 hours under Aubry II. The ability to observe exactly which firms—large and small—signed collective agreements enables us to precisely measure the magnitude of payroll tax changes introduced by the reform.

Firm-Level Balance Sheet and Income Statement Data. To analyze firm outcomes, we link the DADS with the FICUS (*Fichier Complet Unifié de Suse*) database using firm's unique tax identifiers. FICUS provides firm-year balance sheet and income statement data drawn from mandatory corporate tax filings for all firms registered in France. This linked dataset allows us to study output (value added and sales), inputs, and profits, and to construct measures of total factor productivity (TFP).

Payroll Tax Database. In France, social security is primarily financed through payroll taxes levied on labor income, which are divided into employee payroll taxes—deducted from workers' gross earnings—and employer payroll taxes—paid by firms on top of gross wages.[13] Employer-employee registers do not report employer payroll taxes, so microsimulation techniques are required to recover individual-level labor costs.

Following Bozio et al., 2025, we use the tax simulation model (TAXIPP) developed by the *Institut des Politiques Publiques* (IPP), and specifically its payroll tax module. We make additional adjustments specific to the 35 hours reform by merging in the workweek agreement dataset to identify whether and when a firm signed a 35-hour workweek agreement and to compute the corresponding tax deduction. The model takes as input the payroll tax schedule and DADS data on earnings and job spell. Finally, it computes, at the worker $\times$ year level, employer and employee payroll taxes, reductions in employer payroll taxes, flat-rate income tax (CSG and CRDS), and various other payroll taxes.

To assess the accuracy of our imputed payroll taxes, we aggregate them to the firm $\times$ year level then benchmark these estimates against firms' balance sheet data recordings of total employer payroll taxes. Appendix Figure A.3 verifies the close alignment between these two objects. In particular, Panel (a) plots log total wage bill in FICUS against log total wage

[13] As is well known in the public finance literature, the incidence of so-called employer payroll taxes may actually fall on workers. We retain this terminology but do not take a stand on the ultimate incidence of these contributions.

bill in DADS at the firm level for 1997 and 2001. Panel (b) plots log firm-level payroll taxes in FICUS against log firm-level payroll taxes inferred by merging workweek agreement data and the DADS for the same years. Dashed lines show the OLS fit of the FICUS variable on the corresponding DADS variable, with estimated slopes and intercepts (and standard errors) reported in the legend. The estimates being close to the 45-degree line indicates that imputation errors are of negligible magnitude and are unlikely to substantially affect our results.

Labor Force Survey. To supplement the administrative data, we use the French Labor Force Survey (LFS), a nationally representative snapshot of about 75,000 households per year. The LFS asks workers about their actual working time, which allows us to validate patterns observed in the administrative data. The 2001 wave further includes a unique supplement on workweek changes for 14,000 workers. This module asks whether a collective agreement to reduce hours was signed in the workers' firm, whether the worker's own hours were reduced by the reform, and, if so, how. We exploit these data to analyze heterogeneity in the implementation of the reform by workers' skill levels.

3.2 Sample and Variable Definitions

Sample. We restrict the sample to corporations with a valid identifying number 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 18-65. 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 and larger firms. We winsorize the top and bottom 0.5% of firm level outcomes (e.g., employment, wages, hours) in the DADS by treatment group, and the top and bottom 2.5% of ratios in the balance sheet data due to noisier measures.

Workweek. For each job spell, we compute the average weekly working time as the ratio of total hours worked over the year to the number of days employed, multiplied by 7. Hours are winsorized at 2,496, which corresponds to 52 weeks x 48 hours, the maximum workweek allowed under European Union regulations. This has two implications. First, it is not possible to distinguish between contractual and overtime hours in the linked employer-employee data, as firms report only total hours paid. Second, we do not observe within-year variation in working time.

Hourly Wage. Our measure of the hourly wage is defined as gross earnings divided by annual hours worked. To avoid implausibly values driven by extreme hours, we again cap annual hours at 2,496. We set the hourly wage to missing if it falls below 80% of the statutory hourly minimum wage measured in 1984. When computing firm-level averages, we weight individual hourly wages by the number of days worked within the firm-year. For individuals with multiple employers, we retain in our main analysis the spell and earnings at the main employer. We also have robustness checks where we instead measure total earnings, and the corresponding payroll taxes, by summing up earnings across all employers.

Profits. Profits are measured as accounting profits, defined as the difference between total revenues and total expenses. Our measure is net of corporate taxes and profit-sharing payments and, therefore, captures the residual income accruing to the entrepreneur. In other words, it corresponds to the profits that remain after deducting the portion of firm-level profits paid to the government through corporate taxation and to workers through profit-sharing schemes.

Total Factor Productivity. We measure total factor productivity (TFP) using the control-function approach of Ackerberg, Caves, and Frazer (2015). We estimate a value added production function and recover TFP as the associated firm-level productivity term. The estimation is carried out separately by industry. In the production function, we distinguish two types of labor inputs: high-skilled labor (managers, engineers, executives) and lower-skilled labor. For each skill group, we use the (log) number of hours worked in full-time-equivalent (FTE) units, where one FTE corresponds to 2,028 hours per year as of the pre-reform period. We restrict the sample to firms with strictly positive employment in both skill groups and include firm age as a control. Following Ackerberg, Caves, and Frazer (2015), we use materials as the proxy variable to control for firm-level unobserved productivity shocks and treat (log) tangible capital as the state variable. All monetary variables entering the production function and TFP computation are expressed in constant prices.

3.3 Aggregate Descriptive Evidence on the Adoption of the Reform

Figure 1 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). Before the reform, the distribution was tightly centered around 39 hours for full-time workers, with secondary mass points corresponding to part-time contracts. After the reform, the distribution shifts

leftward, with a new concentration emerging at 35 hours.

Appendix Figure A.5 displays the same distribution using self-reported hours from the Labor Force Survey (LFS). 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 LFS data similarly show a sharp decline in the fraction of full-time workers reporting 39-hour schedules and an increase in those reporting 35 hours.

Drawing on a special module of the 2001 Labor Force Survey, Appendix Figure A.6 documents the main modes of implementation of the reform by skill group. Panel (a) reports, separately for high- and lower-skill workers, the share reporting employment in a firm that implemented a workweek-reduction agreement and, conditional on this, the share whose own workweek was reduced. High-skill workers are somewhat more likely to be in a firm that implemented a workweek reduction (69% vs. 50%), consistent with high-skill workers being over-represented in larger firms. More importantly, conditional on being in a firm impacted by the hours reduction, the vast majority of both groups (respectively 87% and 93%) experienced a reduction in their own hours. Panel (b) shows, among workers covered by an agreement, the distribution of reported modes of working-time reduction. High-skill workers, such as executives and engineers, typically do not track weekly hours, so for them the reform was mainly implemented through additional RTT days or regular half-days off, rather than shorter workdays. For lower-skilled employees, such as technicians, clerks and blue-collar workers, implementation more often involved shorter daily hours or a half-day reduction per week. Finally, only a small minority of workers opted for the less common arrangements.

4 Empirical Strategies

Our empirical approach exploits the staggered implementation of the 35-hour reform by firm size. Firms with more than 20 employees (based on 1997–1999 headcount) were required to comply in 2000, while smaller firms had until 2002. We use this quasi-experimental variation to construct two complementary identification strategies: a difference-in-differences (DiD) framework and a policy decomposition design that separates the effects of the workweek reduction from the accompanying payroll tax cuts.

4.1 Difference-in-Differences Design

We implement a standard DiD design with two modifications. First, we define treatment status based on firm size in 1997, one year prior to the policy announcement. We therefore estimate an intent-to-treat effect. Second, we exclude firms whose size is close to the regulatory threshold, as firm size in this range has limited predictive power for whether the firm ultimately implemented the reform before or after 2002. Consequently, we define the treated group as firms with 22–48 full-time equivalent (FTE) employees and the control group as firms with 10–18 FTEs. Our baseline DiD specification is:

$$y_{jt} = \sum_{\tau=1994, \tau \neq 1997}^{2001} \alpha_\tau \times Treated_j \times \mathbb{1}[t = \tau] + \mu_j + \rho_{s(j)t} + \phi_{c(j)t} + u_{jt}$$

where y_{jt} is an outcome for firm (or worker) j in year t, μ_j is a firm (or worker) fixed effect, and $\rho_{s(j)t}$ and $\phi_{c(j)t}$ are time-varying fixed effects for sector and county, respectively. The μ_j term captures time-invariant characteristics of firms (or workers), while $\rho_{s(j)t}$ and $\phi_{c(j)t}$ account for shocks affecting all firms in a given industry or county. Our counties correspond to the 96 *Départements* present in metropolitan France. The α_τ coefficients trace the dynamic treatment effects relative to 1997. Standard errors are clustered at the firm (or worker) level. We run this regression on a balanced sample of firms for the period 1994-2001.

For interpretation, it is important to note that this specification captures the combined effect of the reform package (mandatory hours reduction and payroll tax subsidies). We later discuss the methodology we developed to isolate the effect of each policy component.

Table 1 provides summary statistics for treated and control firms in 1997. Prior to the reform, treated and control firms are balanced across most characteristics, including worker age, gender composition, average hours, and full-time shares. The only modest difference is that control firms are slightly less likely to be in manufacturing, and correspondingly more likely to be either in construction or services. We account for these differences using time-varying industry fixed effects in our main regression. The last column provides economy-wide summary statistics and demonstrates that our sample is in many ways representative of the broader labor market: while firms in our sample are on average smaller, other characteristics (such as gender composition, age, share full-time, wages and industry) are remarkably similar.

The validity of our DiD design relies on five main assumptions. While we verify the parallel trend assumption for each outcome in the next section, we focus here on the remaining four.

First, firm size in 1997 must be a valid proxy for treatment assignment under the reform's staggered implementation. Appendix Figure A.7 supports this claim. Panel (a) plots the share of firm-level hours above 35 against firm size in 1997.[14] There is no discontinuity at the 20-employee threshold, thus ruling out the presence of pre-existing policies affecting hours at this cutoff. In contrast, Panel (b), based on 2001 data, reveals a sharp drop in hours above 35 precisely at the regulatory threshold, consistent with differential exposure to the hours constraint. The difference is more pronounced when comparing firms with fewer than 18 FTEs to those with more than 22, further motivating our decision to exclude firms with 19–21 FTEs from the estimation sample.

Second, treatment assignment must be exogenous, that is firms should not manipulate their size. Our research design minimizes this risk by using firm size in 1997, one year prior to the announcement of the reform, to determine the treatment status. It is unlikely that firms manipulated their size in response to a yet-to-be-announced policy. Still, there could be other, pre-existing policies with the same size threshold that may have alterated the size incentives for firms. Panel (c) of Appendix Figure A.7 displays the 1997 firm size distribution, measured in full-time equivalent employees. There is no bunching below the 20 employee threshold, suggesting no manipulation of firm size.

Third, composition stability requires that there be no selective attrition, e.g., exit of firms from our sample, correlated with treatment. Appendix Figure A.8 Panel (a) plots firm exit rates for treated and control firms, showing similar levels across groups, pre and post reform. Panel (b) reports DiD estimates of exit rates, confirming no differential selection over time.

Finally, the Stable Unit Treatment Value Assumption (SUTVA) must hold: there should be no spillovers between treated and control firms and no general equilibrium effects. Although this assumption is not directly testable, several pieces of evidence suggest it is plausible. First, we systematically pair our difference in difference estimates with the raw averages of the variable of interest for both treatment and control. This allows us to show that there is no break in outcome trends for the control group at the time of the reform—something we would expect if SUTVA were violated. Second, in Section 5.4, we leverage CZ-level differences in the relative shares of firms' full-time workers and are able to replicate our firm-level estimates using CZ-level variation in exposure, which assuage concerns about potential between-firm spillovers within local labor markets.

[14] For a firm where all employees work 39 hours, this fraction is (39-35)/39 = 10%. It equals 0 if all employees work 35 hours or less.

4.2 Policy Decomposition Design

We want to disentangle the effects of the hours mandate from those of the payroll tax cuts. In an ideal design, we would observe firms that are randomly exposed only to the reduction in the statutory workweek (with no exposure to payroll tax cuts) and, conversely, firms that are exposed only to the payroll tax cuts (with no change in statutory hours). In the data, such firms do not exist, and even if they did, they might lie on idiosyncratic trajectories that limit external validity.

Instead, we exploit two dimensions of pre-reform heterogeneity across firms. First, we use the firm's share of full-time workers in 1997 as a proxy for its exposure to the change in statutory weekly hours. Intuitively, a firm employing only part-time workers is unaffected by the reduction in the full-time statutory workweek, whereas firms with a high share of full-time workers are strongly exposed. Second, we construct a measure of predicted payroll tax savings based on each firm's 1997 wage distribution. This measure captures the extent to which a firm can benefit from the degressive component of the permanent subsidy, which was targeted at lower-wage workers and phased out fully for wages above 1.73 times the minimum wage. For instance, a firm with only high-wage workers could only benefit from the modest flat subsidy of 4,000 francs per worker per year, whereas a firm employing low-wage workers could benefit from up to 17,5000 francs per worker per year, in addition to the flat subsidy.

Appendix Figure A.10 provides a conceptual illustration of this decomposition: exposure to the hours reduction is on the horizontal axis, and exposure to the payroll tax subsidy is on the vertical axis. Firms in the lower-right quadrant are mostly exposed to the working time reduction, while those in the upper-left quadrant are mostly exposed to payroll tax cuts. We use both sources of heterogeneity simultaneously, which allows us to account for their correlation as well as for the possibility that the two policy components interact.

The resulting policy decomposition design extends our main specification and includes interactions of treatment status and year with these two exposure variables in what is effectively a quadruple-difference design:

$$
\begin{aligned}
y_{jt} = & \sum_{\tau=1994,\tau\neq1997}^{2001} \alpha_\tau \times Treated_j \times \mathbb{1}[t=\tau] \\
& + \sum_{\tau=1994,\tau\neq1997}^{2001} \beta_\tau \times Treated_j \times \mathbb{1}[t=\tau] \times FTshare_j \\
& + \sum_{\tau=1994,\tau\neq1997}^{2001} \lambda_\tau \times Treated_j \times \mathbb{1}[t=\tau] \times PredTaxCuts_j \\
& + \sum_{\tau=1994,\tau\neq1997}^{2001} \delta_\tau \times Treated_j \times \mathbb{1}[t=\tau] \times FTshare_j \times PredTaxCuts_j \\
& + X_{jt}^{\top}\Gamma + \mu_j + \rho_{s(j)t} + \phi_{c(j)t} + u_{jt}
\end{aligned}
$$

where $PredTaxCuts_j$ is the predicted firm-level payroll tax cut per hour (in euros), computed from 1997 wages, and $FTshare_j$ is the firm's share of full-time workers in 1997. $X_{jt}^{\top}$ includes other interactions between the exposure measures and time dummies.

We estimate this model and use the estimated parameters to construct reduced-form counterfactuals. In particular, we can predict the effect of each component of the reform in isolation. For example, to obtain the effect of the workweek reduction absent any payroll tax cuts, we impute $PredTaxCuts_j = 0$, so that $\alpha_\tau + \beta_\tau$ estimate the effect of the workweek reduction *absent any payroll tax reduction*. Conversely, to isolate the effect of the payroll tax cuts holding working time constant, we impute $FTshare_j = 0$, so that $\alpha_\tau + \lambda_\tau$ captures the effect of payroll tax cuts *holding working time constant*.

For this decomposition to credibly identify the separate effects of the two components, several conditions should hold. First, the strategy requires sufficient empirical variation in exposure proxies. As shown in Figure 2, Panel (a) displays a wide distribution of full-time shares across firms, and Panel (b) shows a similarly broad distribution of predicted tax cuts. This heterogeneity provides the necessary variation to identify marginal effects along both policy dimensions. Second, for our quadruple-difference design to credibly isolate treatment effects, we need sufficient common support in the distributions between the control and the treated group. As shown in Figure 2, there is considerable overlap in the distributions between treated and control firms. Third, we must make a functional form assumption about how each exposure variable affects outcomes. Operationally, our specification allows for an interaction between the two exposure measures—so the marginal effect of one component is allowed to vary linearly with exposure to the other—but rules out more complex nonlinearities on either dimension or in their interaction. Finally, identification of the effect of one

component while holding the other fixed at zero relies on extrapolation beyond the observed support, especially for the "workweek reduction only" counterfactual. Whether this extrapolation is valid cannot directly be tested because the data lack support at the extremes of either dimension. For example, virtually no firms have a full-time share of zero, and none are allocated a predicted tax cut of zero, since the Aubry II policy includes a flat-rate subsidy independent of wages.[15]

5 Results

We present the effects of the 35-hour reform on hours worked, wages, employment, distributional outcomes across skill groups, and firm productivity. We begin with first-stage results, that is the effects on hours worked and wages in our DiD design.

5.1 First-Stage: Weekly Hours, Wages and Labor Cost

35h Workweek Indicator We construct an indicator $\mathbb{1}\{35\text{h workweek}\}_{jt}$ that equals one when firm j is classified as operating a 35-hour workweek in year t, and 0 otherwise. To do this, we compute for each firm-year the fraction of full-time workers (those working more than 30 hours per week) whose contractual hours are in the 30–38 hour range ("35h") and the fraction whose contractual hours are at least 38 hours ("39h"), weighting each worker by the number of days worked over the year. We set $\mathbb{1}\{35\text{h workweek}\}_{jt} = 1$ if the weighted share of "35h" workers exceeds the weighted share of "39h" workers in firm j at time t, and zero otherwise.We view this indicator as a parsimonious summary measure of the adoption of the 35-hour workweek. Figure 4, Panel (a), plots the average value of this indicator for treated and control firms between 1994 and 2001. In the pre-treatment period, the two groups track each other closely. They begin to diverge modestly in 1999 and much more sharply in 2000 and 2001. The DiD estimates in Panel (b) show that, by 2000, firms in the treated group were 25pp more likely to have adopted the 35-hour workweek, rising to 34pp in 2001.

Weekly Hours Figure 3 displays the weekly hours distribution in treated and control firms from 1997 to 2002. Panel (a) shows that, prior to the reform announcement, treated and control firms had nearly identical distributions, with a large concentration at 39 hours. Panels (b)

[15] In practice, many large firms receive no tax subsidies because they opt out of signing a collective agreement. However, firms' decisions to pursue these agreements—and thus claim the subsidies—are likely endogenous. Since this selection is plausibly correlated with the expected effects of the reform, we cannot use the observed distribution of actual tax subsidies claimed to measure exposure.

and (c) show minimal changes in these distributions, though slight shifts may reflect anticipatory adjustments by large firms in response to the Aubry I law, which signaled the coming shift to a 35-hour workweek. In contrast, Panels (d) and (e) reveal a pronounced reallocation toward 35 hours and part-time work among treated firms during the implementation period. Control firms also exhibit some reallocation—likely due to small firms voluntarily adopting the 35-hour workweek early to access tax incentives—but the change is markedly less pronounced. By 2002 (Panel f), when smaller firms are also required to comply, the hours distributions converge across groups, reflecting broad adoption of the reform. These patterns confirm a strong first-stage effect of the reform on working hours, and support the use of 2000–2001 as the relevant post-reform window in the analysis. Figure 4 Panel (c) plots average weekly hours for treated and control firms between 1994 and 2001. Prior to the reform, both groups follow parallel trends, with average weekly hours at around 36. Starting in 1998, the year the reform is announced, treated firms experience a decline in hours, likely due to anticipatory behavior. This decline becomes steeper by 2000, when average hours worked reach 34h in the treatment group, while control firms show only a modest decline to 35h. By 2001, average hours worked reach 33h in the treatment group, while control firms' average remains close to 35h. Panel (d) presents the corresponding DiD estimates, which remain close to zero through 1997, drop modestly in 1998 and 1999 (a 0.5 hours per week difference), then fall more dramatically through 2001—culminating in a reduction of 2 hours per week. This 2-hour decline, while substantial, is smaller than the 4-hour change in the statutory workweek—from 39 to 35 hours—mandated by the reform. There are several reasons for this. First, treated firms retained the option to schedule overtime beyond 35 hours, subject to premium pay and annual caps. As a result, some full-time workers continued working above the 35-hour threshold even after the reform. Second, some control firms voluntarily adopted 35-hour agreements to access associated payroll tax incentives. This likely induced a downward shift in hours among the control group as well. Finally, as Askenazy (2021) highlights, firms were given some flexibility in how working time post-reform was measured—for example, by excluding certain breaks from the calculation. As a result, in sectors such as retail, the actual reduction in working time ended up being closer to two and a half hours rather than four.

Wages Panel (e) of Figure 4 shows the evolution of firm-level average hourly wages for treated and control firms between 1994 and 2001, capturing both the pre-reform trajectory and post-reform adjustment. Prior to the reform, both groups exhibit similar wage growth. However,

beginning in 1998—after the policy was announced but before its implementation—wages in treated firms stagnate, while wages in control firms continue on their pre-existing upward trend. After implementation, wage growth in treated firms accelerates sharply, surpassing that of control firms by 2001. Panel (f) presents the corresponding DiD estimates, showing a small but statistically significant decline in hourly wages for treated firms relative to control firms during the 1998–2000 period. This is followed by a 1% increase in 2000 and 2.1% increase in 2001.

One likely explanation for the wage stagnation in treated firms prior to implementation is anticipatory adjustment: knowing that wages would effectively rise in the near future—due to the mandate to preserve monthly earnings despite shorter hours—employers may have withheld nominal wage increases in advance to manage labor costs. This intertemporal smoothing of wage adjustments is consistent with the sharp post-implementation rebound and supports the view that firms internalized the upcoming cost of the reform before it was enforced. Post-enforcement, the observed increase in average hourly wages at treated firms falls well short of the 11.4% nominal rise implied by the reform for full-time workers. Several factors help explain this gap. First, the mandate applies only to incumbent full-time workers: there is no required adjustment for part-time workers or new hires. This creates both a static effect—where the average wage includes many workers not subject to the mandate—and a dynamic incentive to substitute away from incumbent full-time employees. Firms may hire new workers or shift to part-time workers to contain labor costs. We explore these margins of adjustment in more detail in Section 5.1. Consistent with this, Appendix Figure A.11 documents that the wage increase is substantially larger for the subset of incumbent full-time workers, reaching 4% in 2001 relative to controls. Still, even this 4% increase remains far below the mandated 11.4%. Several additional explanations may account for this. First, wage growth was slower than usual between announcement and implementation as firms anticipated the coming rise in hourly wages. Second, firms might have found ways to offset or dilute the mandated increase even on incumbents—through reductions in overtime hours and associated wage premiums.

Labor Cost While all firms with more than 20 employees were mandated to switch to the 35h workweek by 2000, only the firms that signed a collective agreement benefited from payroll tax reductions. Additionally, while firms under 20 employees were free to remain at the 39h workweek until 2002, they could opt into the 35h workweek by signing a collective agreement. Both types of firms could sign a 35 hr workweek agreement prior to 2000 and

benefit from the Aubry I tax cuts. Thanks to the collective agreement database we are able to study whether and when firms signed these collective agreements, separately by treatment status. Appendix Figure A.9 Panel (a) shows that 15% of treated firms had signed an Aubry I agreement by December 1999, while only 7% of control firms did. By December 2000, 35% of treated firms had signed up for either an Aubry I or Aubry II tax cut, vs. 10% of control firms. By December 2001, these shares became 47.2% vs. 20.2%, respectively. For the interpretation of the reform, it is important to note that the 79.8% of small firms that did not sign an agreement by December 2001 had all remained at the 39h contractual workweek. Conversely, the 52.8% of large firms that did not sign an agreement by end of 2001 still shifted to the 35 hours statutory workweek starting in January 2000, they simply did not get the corresponding payroll tax cuts. Appendix Figure A.11 documents that, despite the payroll tax cuts, the labor cost increase is still substantial for the subset of incumbent full-time workers, reaching 3% in 2001 relative to controls. Panel (f) of Figure 4 also shows, this time at the firm level, that there is a more moderate increase of the labor cost, relative to wages. The DiD coefficient on labor cost reaches $\sim 1\%$ by 2001.

5.2 Employment and Hours Effects

Employment Figure 5 documents the impact of the reform on employment, measured as the log number of jobs (normalized to 1997 and weighted by the job spell length, such that if a job is held for six month in the year it counts as half a job). Panel (a) plots employment levels separately for treated and control firms over time. Both levels and trends are nearly identical prior to the reform. This confirms the validity of our control group. We further find no effect of the reform on the trend in employment in treated firms and no difference in post-reform employment levels between control and treated firms. The absence of any change in trajectory for either group also mitigates concerns about spillover effects—for example, if treated firms had poached workers from control firms, we would expect to see a decline in control group employment, which we do not observe.[16] Panel (b) presents the DiD estimates and confirms a precisely estimated null effect: we can rule out employment gains larger than 0.8% and losses greater than 0.9% at the 5% significance level. Figure 6 Panel (a) turns to the employment effects at the worker-level, using the panel dataset. The figure plots the DiD estimates for a worker's probability to remain employed, separately for a placebo (pre-reform) period and the actual post-reform period. The pre-reform coefficients capture DiD estimates of job

[16] We further test for spillover effects in Section 5.4, leveraging CZ-level differences in the relative shares of firms' exposure to the hours reduction component of the reform.

retention between 1995 and 1997, while the post-reform coefficients capture DiD estimates of retention between 1999—immediately following reform announcement—and 2001, the last year of differential exposure across firms. In both cases, we condition on employment two years earlier (t–2) and compare outcomes in t and t-2. We limit to the t vs. t-2 horizon because, given the limited length of the panel and the loss of observations when tracking workers over longer horizons, this shorter period (e.g. comparing 2001 to 1999 to evaluate the effects of the reform) preserves enough sample size to do the analysis separately by hours bin. Because the reform applied only to incumbent full-time workers, we expect employment effects to vary by pre-treatment working hours. We therefore estimate treatment effects separately for four groups: (i) part-time workers (5–30 hours/week), (ii) near-full-time workers (30–35 hours), (iii) workers just above the 35-hour threshold (35–39 hours), and (iv) overtime workers (39–48 hours). As expected, we find no significant treatment effects in the pre-reform period, validating the parallel trends assumption. In the post-reform period, we observe a small and statistically insignificant decline in the probability of remaining employed for part-time workers (approximately 1%), and no measurable effects for any other hours group. Together, the firm and worker-level results imply that the reform's stated goal of "work-sharing" did not materialize.

Employment along the Hours Distribution Despite little evidence of work-sharing in terms of job creations, there could be some reallocation of jobs throughout the hours distribution. To study the effects of the reform on the number of jobs in each bin of weekly hours worked, we implement a bunching DiD analysis adapted from the minimum wage literature, following Cengiz et al. (2019). We first construct a dataset at the firm $\times$ hours-bin $\times$ year level and estimate:

$$\frac{\text{\# jobs bin } i_{j,t}}{\text{\# jobs}_{j,1997}} = \sum_{k=25}^{45} \alpha_k \times Treated_j \times After_t \times \mathbb{1}[i = k] + \mu_{ij} + \rho_{ist} + \phi_{ict} + u_{ijt},$$

where # jobs bin $i_{j,t}$ is the number of jobs in weekly-hours bin i in firm j and year t, normalized by # jobs$_{j,1997}$, the aggregate employment in firm j in 1997. The index k denotes one-hour weekly-hours bins and ranges from 25 to 45: $k = 25$ corresponds to weekly hours worked of at most 25 hours (i.e., $[0, 25]$); $k = 26$ corresponds to weekly hours worked in $(25, 26]$, etc... and $k = 45$ corresponds to weekly hours strictly above 44 hours. $Treated_j$ is an indicator for treated firms and $After_t$ equals one in 2001 and zero in 1997. μ_{ij} are firm-by-hours-bin fixed effects; ρ_{ist} are hours-bin-by-sector-by-time fixed effects; and ϕ_{ict} are hours-bin-by-county-by-

time fixed effects. These fixed effects absorb firm-specific factors in the distribution of weekly hours as well as sectoral-level and county-level changes in the distribution of hours worked. We estimate this regression on a balanced panel of firms observed both in 1997 to 2001. Standard errors are clustered at the firm level.

The resulting coefficients α_k measure the DiD effect of the reform on employment in weekly-hours bin k. We plot these coefficients in Figure 7.[17] The figure shows that, not surprisingly, the main shift that happened in the weekly hours distribution is from 39 hours to 35 hours. One attractive feature of the bunching design is that we are able to document what happens to the rest of the distribution. Two striking findings emerge: first, jobs with more than 39 hours per week pre-reform also decreased following the reform. Second, we find that the decrease in jobs above 37 hours is only partly offset by an increase in the relative number of jobs in the 35 to 37 hours range. Netting out the employment losses in jobs above 37 hours are smaller but significant employment gains in jobs with hours between 25 and 33 hours/week. This reallocation pattern—from full- to part-time jobs—is confirmed in a complementary design. We use a DiD design that compares the evolution of the part-time employment share in treated and control firms over time. As shown in Appendix Figure A.12, Panel (b), the parallel trends assumption holds in the pre-reform period, and we observe a sharp and sustained increase in the part-time share in treated firms after the reform. By 2001, the share of part-time jobs in treated firms is 6 pp higher than in control firms.

Hours Since the reform mandated a reduction in the statutory weekly hours per worker and we observe no aggregate employment creation, total hours at the firm should decline. This is precisely what we document in Figure 8, which plots the impact of the reform on total hours worked at the firm. Panel (a) shows that both the levels and trends in total hours are nearly identical between treated and control firms prior to the reform, once more validating the choice of our control group. After 1998, however, the trajectories diverge: while control firms continue on a rising trend and accumulate an 8% increase in total hours by 2001 (relative to 1997), treated firms experience a marked slowdown, with total hours increasing by only 2% over the same period. Panel (b) presents the corresponding DiD estimates: in 1998–1999, the reform begins to exert anticipatory downward pressure on hours, and by 2001, we estimate a reduction in log total hours of 6% relative to the control group. Panel (c) decomposes this

[17] If there were relatively more jobs created between 1997 and year t in treated than control firms, the sum of α_k's across all hour bins will be above 0, below otherwise. Consistent with the zero employment effects from Figure 5, the α_k's add up to zero.

decline into hours worked above 35—those directly targeted by the reform—and at or below 35, which were not. We find that both components contributed to the reduction: by 2001, total hours above 35 declined by 3.5%, while hours at or below 35 fell by 2.4%. The former is somewhat expected, as the reform lowered the statutory workweek from 39 to 35 hours, but not entirely mechanical. Indeed, the new 35-hour statutory workweek did not strictly prohibit workers from exceeding 35 hours. Rather it altered the overtime incentives: firms had to pay a 25% overtime premium for additional hours and faced new annual caps on total overtime hours, effectively limiting average working time to around 39 hours per week (the previous full-time statutory workweek definition). More surprising is the reduction in total hours at or below 35, which suggests broader spillover effects beyond the directly treated population. Figure 6 Panel (b) turns to the hours effects at the worker-level. The figure presents DiD estimates of changes in weekly hours among job stayers, separately for a pre-reform (placebo) period and the actual post-reform period. Similar to the employment effects in Panel (a), we examine effects separately by baseline weekly hours, using four groups: 5–30, 30–35, 35–39, and 39–48 hours. The pre-reform coefficients capture DiD estimates of hours change between 1995 and 1997, while the post-reform coefficients capture DiD estimates of hours change between 1999—immediately following reform announcement—and 2001, the last year of differential exposure across firms. Reassuringly, for the placebo years, we observe no difference in the change in weekly hours by treatment status. In contrast, the change in weekly hours between 1999 and 2001 is larger in the treatment than in the control group for all four weekly hours groups. As expected, the largest drop—1.8 hours—is observed for workers at or above 39 hours in 1999, the group most directly targeted by the reform. But we also detect a significant decrease of ~ 1 hour for those working 30–35 hours pre-reform, and a smaller, marginally significant decline (0.5 hours) among workers in the 5–30 hours group. These results indicate that the reform triggered a broader reorganization of work within firms, beyond the targeted, full-time population. We also note that the reduction in weekly hours worked at or below 35 hours is consistent with the earlier finding of a reallocation towards part-time jobs: although more workers were employed in low-hours jobs, the average hours worked in those jobs was sufficiently low to generate a net decrease in hours. Thus, even as job counts rose below 35 hours, total labor input from part-time work continued to fall— further highlighting the lack of "work-sharing".

Wages along the Hours Distribution Panel (c) of Figure 6 re-examines the reform's impact on worker-level wage changes by further splitting workers into hours bins, and using the

same DiD framework as Panels (a) and (b). The results further reinforce the view that the reform triggered a broader reorganization within firms. As expected, and consistent with the results in the panel, incumbent full-time workers—those working 39 hours or more in 1999—experienced an average increase in hourly wages of around 5%. This is in line with the mandated preservation of monthly earnings despite reduced hours. More unexpectedly, part-time workers—those working at or below 35 hours in 1999—also experienced a wage increase of $\sim 2.5\%$. Several mechanisms could account for this finding. First, firms may have extended wage increases for reasons of internal equity (Giupponi & Machin, 2022; Hazell, Sarsons, & Taska, 2026).[18] Second, part-time workers may have benefited from a pass through of the payroll tax cuts, which were targeted at low-wage jobs, including part-time ones.

Policy Decomposition Design The DiD estimates discussed so far measure the effects of the 35h reform package, that is the effect of the joint implementation of the new statutory workweek definition and the payroll tax cuts offered to mediate its cost to the firms. Using the decomposition design described in Section 4.2, we now isolate the respective effects of the workweek reduction and the payroll tax cuts. Figure 9 presents estimates respectively of $\alpha_\tau + \beta_\tau$, the effect of the "workweek reduction" only (holding payroll tax rates constant), and $\alpha_\tau + \lambda_\tau$, the effect of the "payroll tax cut" only (holding the workweek constant). Panels (a) and (b) provide first-stage evidence that each specification indeed isolates the effect of only one component of the reform. Indeed, Panel (a) shows that, under the"payroll tax cut" only specification, there is no change in the share of hours above 35, while we estimate a 20% drop in the share of hours above 35 for the "workweek reduction" only specification. Panel (b) shows that, under"workweek reduction" only, there is, if anything, a rise in payroll taxes per worker per hour, which can be rationalized by the increase in hourly wages under the new statutory workweek. In contrast, a significant decline in payroll taxes per worker per hour is observed for the "payroll tax cut" only specification. Turning to our outcomes of interest, we

[18] A growing body of research suggests that fairness considerations play a meaningful role in how firms set wages and share rents with workers. Evidence from linked employer–employee data points to firm wage policies that appear consistent with internal pay equity norms (Goldschmidt & Schmieder, 2017; Song et al., 2018; Saez, Schoefer, & Seim, 2019). Complementing these findings, several studies show that unequal pay within firms can influence employee behavior—lowering morale, affecting retention, and dampening effort—highlighting the behavioral implications of perceived inequity (Card et al., 2012; Cohn, Fehr, & Goette, 2015; Breza, Kaur, & Shamdasani, 2017; Dube, Giuliano, & Leonard, 2019). Experimental and survey-based work further reveals that workers often view pay disparities as unfair, even when they reflect market-driven outcomes (Hvidberg, Kreiner, & Stantcheva, 2020). This behavioral and normative evidence is echoed in employer surveys, which consistently indicate that equity concerns—not just productivity or market forces—factor into wage-setting decisions (Blinder & Choi, 1990; Katz & Krueger, 1992; Agell & Lundborg, 1995; Campbell & Kamlani, 1997; Galuscak et al., 2012).

estimate that in the absence of the payroll tax cuts, employment would have declined by ∼2% (Panel (c)). In contrast, exposure to the payroll tax cuts only, has marginally positive effects on employment. Turning to the total hours worked at the firm in Panel (d), we observe a steady decline reaching 11% in 2001, for firms predicted to be solely exposed to the statutory workweek change. Meanwhile, there is no detectable change in total hours at firms predicted to be solely exposed to the tax cut. Taken together, these results highlight the critical role of fiscal offsets in avoiding job losses.

5.3 Distributional Effects

Average Weekly Hours by Skill Group Figure 10 Panel (a) shows the DiD estimates for the average weekly hours, separately for high-skill workers and lower-skilled workers. We observe that both groups experience a significant decrease in their average workweek, albeit the decrease is larger for lower-skilled workers: by 2001, the average weekly hours of high-skill workers at treated firms relative to control firms is down by 1.2 hours, and 2 hours for lower-skilled workers. The DADS pattern closely mirrors the evidence from the labor force survey, where workers directly report their hours, and from its dedicated supplement on the 35-hour workweek, discussed in Section 3.3. The consistency across employer-reported and worker-reported data alleviates any concern that managers' hours are not "truly" reduced and that the decrease in the DADS data could be driven by firms' misreporting for compliance reasons.

Total Hours and Employment, by Skill Group Figure 10 examines how the reform's effects varied across skill groups. Panel (b) highlights a striking divergence in total hours worked by each group, at the firm level: while lower-skilled workers experienced a significant decline—reaching -6% by 2001—total hours for high-skill workers remained effectively flat. Since both groups saw reductions in average weekly hours, the stability of total hours for high-skill workers implies an offsetting increase in their headcount. This is confirmed in Panel (c), which shows a small but clear rise in high-skill employment, suggesting that firms responded to the mandated hours cut by hiring more high-skill workers to maintain overall labor input.

In contrast, the decline in total hours for lower-skilled workers is driven by both shorter workweeks and a reduction in employment: Panel (c) shows a drop of $\sim 1\%$ in the share of lower-skilled workers employed in treated firms, consistent across both 2000 and 2001.

Earnings, by Skill Group Panel (e) turns to the effects on total monthly earnings. For this, we go to the panel and study incumbents workers of each skill type over time. One might

expect that the stability in employment for high-skill workers came at the cost of greater wage moderation. However, the data show the opposite pattern: high-skill workers preserved or even slightly increased their total monthly earnings, while lower-skilled workers experienced a 1.7% drop. This suggests that earnings losses were concentrated among more vulnerable workers—despite the fact that the payroll tax cuts were designed to protect them—while high-skill workers appear to have been insulated from these losses. Taken together, these results point to a regressive distribution of reform benefits and underscore the heterogeneity of firm responses across the skill spectrum.

Mechanism: Discussion This paragraph explores different explanations for the heterogeneity by skills. We first describe why some explanations are unlikely. In principle, payroll tax cuts were designed to subsidize low-wage jobs, so they should have cushioned, rather than amplified, job losses among lower-skill workers. Nor does a "different bite" of the reform on high- versus lower-skill positions fit the evidence: administrative data and the labor force survey both show that high-skill workers also experience sizeable reductions in weekly hours, and the main implementation channel—reallocating time across shorter days versus additional days off—appears to be available to lower-skill workers as well. In addition, high-skill–intensive firms do not differentially expand relative to other firms; most of the change in the skill mix occurs within firms rather than through compositional shifts between firms, which rules out a simple story in which only certain types of firms are affected. We then explore alternative mechanisms. One potential interpretation emphasizes heterogeneous rents: before the reform, lower-skill jobs would be more marginal from the firm's perspective so that the increase in hourly labor costs leads firms to shed these positions, consistent with differential monopsony power across skill groups. At the same time, there is it is possible that the relative productivity of high-skill tasks went up as these tasks can be reorganized more flexibly. Additionally, if the switch to 35h requires organizational changes, high-skill workers, including managers, are best positioned to lead these changes.

5.4 Market-level Evidence

Our evidence so far is based on DiD estimates that exploit variation in the timing of exposure between treated and control firms. A natural concern, however, is that these estimates might be biased by reallocation between treated and control firms within the same local labor market. For example, the employment losses for lower-skill workers in treated firms could be offset by expansions in untreated firms operating in the same geographic area. In this case,

our DiD estimates would overstate the true magnitude of job loss.

Our first piece of evidence against reallocation concerns is that, in the raw average graphs, outcomes for control firms display no discernible discontinuity around the reform date. We further complement this evidence with an aggregate design at the level of commuting zones (CZs). We construct a measure of local exposure to the 35-hour reform based on the pre-reform structure of employment in each CZ. Specifically, for each CZ c we compute the share of employment in 1997 accounted for by full-time jobs in firms with more than 20 employees. This share captures the prevalence of jobs that were directly subject to the reform in 2000, and thus provides a plausibly exogenous measure of the intensity with which the 35-hour workweek was imposed within each local labor market. We consider the 304 CZs in metropolitan France in our analysis.

Our CZ-level outcomes, y_{ct}, include log total hours, log total employment, and log employment by skill group. We estimate the following event-study specification:

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

where Exposure$_c$ denotes the pre-reform exposure measure defined above, μ_c are CZ fixed effects that absorb time-invariant differences across local labor markets, and ϕ_t are year fixed effects that capture aggregate shocks. The coefficients α_τ trace out how outcomes in CZs with full exposure evolve relative to CZs with no exposure before and after the reform.

Before turning to outcomes, we verify that the exposure variable varies sufficiently across CZs. Figure 11 Panel (a) shows the distribution of the exposure variable across the 304 CZs in metropolitan France. We find that exposure varies widely across CZs, reflecting substantial heterogeneity in the pre-reform importance of full-time jobs in large firms across local labor markets. Correspondingly, when we regress the average workweek per job at the CZ level on the exposure-by-year interactions, we find that high-exposure CZs experience a significantly larger reduction in weekly hours following the reform (Figure 11 Panel (b)). In particular, moving from zero to one in the exposure index corresponds to a decline of about four weekly hours if all jobs in the CZ were pre-reform full-time jobs above 39 hours in large firms, which is consistent with the statutory reduction embedded in the 35-hour workweek.

Figure 11, Panel (c), reports the estimated effects on total hours and employment at the CZ level. Relative to a CZ with no exposure, a CZ with full exposure experiences a 25% decline in hours and a 15% decline in employment. To interpret these magnitudes, it is useful to note two points. First, the coefficients capture a shift from one extreme (zero exposure) to

the other (full exposure). Second, although the CZ-level design does not explicitly separate the components of the reform, it is closer in spirit to the policy decomposition design than to the baseline DiD: the exposure index (the share of full-time jobs in large firms) highly correlates with exposure to the hours reduction component, and with higher-wage jobs, which are largely unaffected by the payroll subsidy component. Consequently, the larger negative employment effect in the CZ design is consistent with the 8% decline in employment under the "workweek reduction only" counterfactual in the policy decomposition design.

We then examine how these market-level responses vary by skill group. Figure 11, Panel (d), plots the event-study coefficients for employment, separately for high-skill and lower-skill workers. Consistent with our DiD results, we find that high-skill employment rises in more exposed CZs following the reform, whereas lower-skill employment falls.

In sum, the market-level evidence corroborates the main conclusions from the firm- and worker-level analysis: the 35-hour reform did not generate work-sharing, and it had adverse employment consequences for lower-skill workers that are visible at the level of aggregate local labor markets.

5.5 Output and Productivity

Figure 12 summarizes the dynamics of value added, total factor productivity (TFP), and profitability in treated and control firms, using the same DiD specification as in the previous subsections. Panels (a) and (b) show that total value added in treated firms begins to diverge from that of control firms around the implementation of the reform. The event-study coefficients for log value added are close to zero prior to 1998, indicating no pre-trends, and become modestly negative from 2000 onward. By 2001, treated firms' value added is about 1 percent lower than that of comparable control firms.

Value added goes down, but that could mean either that firms are producing fewer units of output or that they are using fewer inputs and becoming more efficient. To disentangle these possibilities, we look at labor productivity. First, we construct value added per hour of work and estimate the same event-study specification. Panels (a) and (b) of Appendix Figure A.13 show that the ratio of value added to hours rises in treated firms relative to control firms, with DiD estimates reaching about 4 percent by 2001 and no evidence of differential pre-trends. This result is consistent with firm-level hours and employment effects: given that total hours fall by about 6 percent and employment remains essentially unchanged, the modest 1 percent decline in value added implies that firms substantially increased output per hour worked. While informative, value added per hour conflates how efficiently firms

use labor with changes in other inputs, such as capital intensity or workforce composition: a firm can raise value added per hour simply by increasing capital per worker or upgrading its workforce, without necessarily becoming more productive in a technological sense. To move toward a broader measure of efficiency that disentangles these channels, we estimate TFP using an Ackerberg–Caves–Frazer production function. We then adjust this TFP measure for changes in the skill composition of employment, so that it reflects genuine within-firm efficiency gains rather than the fact that firms hire more high-skill workers after the reform. Panels (c) and (d) of Appendix Figure A.13 plot the evolution of this composition-adjusted TFP index for treated and control firms. The two groups follow similar trajectories before the reform but, after implementation, TFP in treated firms increases steadily relative to the control group. By 2001, the DiD estimates imply TFP gains of about 3 percent. The fact that TFP gains remain after controlling for the changing skill composition suggests that they are not mechanically driven by compositional shifts within observable skill groups, but instead reflect real improvements in within-firm efficiency, consistent with firms reorganizing production in response to the higher effective cost of hours induced by the reform.

Productivity gains therefore cushion, but do not fully offset, the small decline in value added. To understand how these remaining output losses are reflected in firms' income statements, we next examine profits. Panels (e) and (f) of Figure 12 report the trajectory of the profit-to-assets ratio. The average profit rate in 1997 is around 4.2 percent, and the post-reform DiD coefficients imply a decline of about 0.4pp, corresponding to a 10 percent reduction in profitability relative to the pre-reform level. To further shed light on the distribution of the output losses across factors, we decompose the decrease in value added into its components going to labor and profits. Panel (c) of Appendix Figure A.13 shows that the reform is associated with a rise in the high-skill labor share, alongside a persistent fall in the low-skill labor share and the profit share. Using the estimated DiD effects on value added, the wage bill, and profits, a simple accounting decomposition implies that 60 percent of the decline in value added is reflected in a decline in total labor compensation—driven by lower-skill workers—while the remaining 40 percent is borne by shareholders through lower profits.

Taken together, these results show that the mandated workweek reduction did not generate large output losses for treated firms: value added falls only slightly, despite a sizable contraction in hours. Increased labor productivity and higher TFP cushion the impact of shorter workweeks on output, consistent with firms responding by reorganizing production and reallocating toward more productive, high-skill workers. At the same time, these adjustments

are not costless. The combination of lower value added and a higher share of value added going to high-skill workers feeds into a substantial compression of profits, with a meaningful share of the burden of the reform ultimately borne by shareholders.

5.6 From ITT to IV estimates

The DiD estimates presented so far measure the *intention-to-treat* (ITT) effect of the reform: the coefficient on $After_t \times Treated_j$ captures the average impact of being in the treatment group, that is being a firm whose size is greater than 20 in 1997. In practice, firm size in 1997 is only a proxy for exposure to the 2000 35h mandate and implementation staggered, such that some treated firms never implement the 35-hour workweek, while others do so only late in the sample period. In addition, some small firms sign an Aubry agreement and switch to the 35h workweek, such that we do not have a "pure" control group either. As a result, the ITT estimates are attenuated. To recover the "treatment-on-the-treated" effect, we rescale the ITT estimates using an instrumental-variables strategy. Specifically, we use the interaction $After_t \times Treated_j$ as an instrument for an indicator $\mathbb{1}\{35\text{h workweek}\}_{jt}$ that equals one when firm j is classified as operating a 35-hour workweek in year t. To construct this indicator, we compute for each firm-year the fraction of full-time workers (those working more than 30 hours per week) whose contractual hours are in the 30–38 hour range ("35h") and the fraction whose contractual hours are at least 38 hours ("39h"), weighting each worker by the number of days worked over the year. We set $\mathbb{1}\{35\text{h workweek}\}_{jt} = 1$ if the weighted share of "35h" workers exceeds the weighted share of "39h" workers in firm j at time t, and zero otherwise.

$$\mathbb{1}\{35\text{h workweek}\}_{jt} = \gamma_0 + \gamma_1 After_t \times Treated_j + \mu_j + \rho_{s(j)}t + \phi_{c(j)}t + \nu_{jt}, \tag{1}$$

It quantifies how strongly firm size in 1997 predicts the actual implementation of the 35-hour workweek. The second-stage equation is;

$$y_{jt} = \beta_0 + \beta_1 \widehat{\mathbb{1}\{35\text{h workweek}\}}_{jt} + \mu_j + \rho_{s(j)}t + \phi_{c(j)}t + \varepsilon_{jt}, \tag{2}$$

The parameter of interest here is β_1, the causal effect of switching from the pre-reform workweek to a 35-hour workweek on outcome y_{jt} for firms whose adoption is induced by the reform.

Table 2 reports the results of this two-stage least squares procedure. Column (1) shows that the interaction $After_t \times Treated_j$ is a very strong predictor of adopting a 35-hour agreement: the reform increases the probability that a firm is on a 35-hour workweek by about

30pp, with very large first-stage F-statistics. Columns (2)–(8) present the corresponding IV estimates for weekly hours, wages, labor costs, total hours, employment, and the employment of high- and lower-skill workers. These coefficients can be interpreted as the effect of actually implementing a 35-hour workweek, thus providing a policy-relevant scaling of our baseline ITT results. Column (2) shows that adopting a 35-hour agreement reduces contractual weekly hours by almost six hours. This estimate is consistent with the ITT event-study coefficients, which display about a two-hour decline in weekly hours for treated firms relative to controls. Given that the first-stage effect in Column (1) is about 0.30, the fact that the IV estimate is about three times larger in magnitude than the ITT effect makes sense. Column (3) indicates that the 35-hour regime raises average wages by about 4.4%, and Column (4) shows that average labor costs increase by around 1.9%. Column (6) confirms that overall employment is essentially unchanged: the coefficient is small and statistically insignificant, in line with the ITT event-study estimates that show no clear break in firm employment around the reform. The adjustment margin is thus primarily intensive (hours per worker) rather than extensive (number of workers). Finally, Columns (7) and (8) decompose employment by skill group, showing a statistically significant and economically large increase in high-skill employment (about 3.1 pp) and a commensurate decline in lower-skill employment, implying a significant upgrading in the skill composition of the workforce.

6 Conclusion

This paper revisits France's landmark 35-hour workweek reform to evaluate key policy debates surrounding working time reductions—namely their effects on employment, output, and productivity. Leveraging matched employer–employee administrative data linked to firm-level balance sheets and payroll tax records, we exploit the reform's staggered implementation by firm size to estimate causal effects using a DiD strategy. We further develop a decomposition approach to separately identify the effects of the statutory hours reduction and the accompanying payroll tax relief.

We find no evidence of aggregate job creation for the reform, ruling out work-sharing effects. While employment headcount was not impacted, total hours were: the reform led to a 5.9% decrease in total hours worked at treated firms. Counterfactual estimates using our decomposition design imply that, absent payroll tax cuts, employment itself would have declined by 5%.

Although the policy included fiscal compensation targeted at low-wage workers, it failed

to prevent increases in intra-firm inequality. High-skill workers saw reductions in working time without earnings losses, while lower-skilled workers experienced reductions in both hours and pay. Further, high-skill workers employment share rose by 1pp, while that of lower-skill workers correspondingly decreased.

Finally, we document a modest drop in value added (–1%), partly offset by a substantial improvement in total factor productivity (+3%). These patterns suggest that firms responded to the higher cost of labor per hour not only by cutting hours, but also by reorganizing production, thus limiting the loss in output. However, from the firm's perspective, these efficiency gains were not enough to offset the cost shock: combining the decline in value added with the increase in labor costs yields a 10% reduction in the profit-to-assets ratio.

Our results hold important lessons for the design of working time reduction policies. First, mandating shorter hours without corresponding demand-side policies leads to aggregate employment losses, not gains as predicted by the work-sharing advocates. While payroll tax cuts helped mitigate job losses in our setting, they were not sufficient to induce job creation. Second, the equity implications of such reforms are not automatic. Despite tax reliefs targeted at low-wage earners, the reform disproportionately benefited higher-skilled workers. Finally, implementation details matter: the mandate to preserve overall pay did not prevent the relative slowdown in wage growth, which eventually led to monthly wage losses, especially for low earners, even as hourly wages rose.

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Main Tables

Table 1: Firm-Level Summary Statistics, 1997

	Treated	Controls	Economy-wide
Worker characteristics:			
Share female	0.33	0.33	0.34
Age < 35	0.44	0.44	0.44
Age 35–50	0.40	0.40	0.40
Age > 50	0.15	0.16	0.16
Weekly hours	36.65	36.38	36.24
Share full-time	0.64	0.64	0.63
Mean hourly wage	11.02	10.89	10.89
Share $< 1.73 \times$ min. wage	0.65	0.66	0.66
Skill groups:			
High-skill	0.11	0.12	0.13
Middle-skill	0.19	0.18	0.18
Low-skill	0.71	0.69	0.69
Industry composition:			
Manufacturing	0.33	0.26	0.26
Construction	0.12	0.15	0.14
Services	0.56	0.60	0.60
Sample size:			
Total nb. of workers	1,312,129	764,359	10,713,496
Total nb. of firms	36,667	51,053	233,808

Notes: This table presents firm-level summary statistics in 1997 for treated firms, control firms, and the economy as a whole. Treated firms are defined as firms with 22–48 full-time equivalent (FTEs) employees in 1997, while control firms have 10–18 FTEs; firms with 19–21 FTEs are excluded from the analysis. The last column reports economy-wide statistics computed over all private-sector firms in Metropolitan France present in all years between 1994 and 2001. All variables are measured at the firm level and are constructed using matched employer–employee records. Skill groups are categorized based on workers' occupation codes. In the analysis we group Middle-skill and Low-skill into the Lower-skill group. Industry shares correspond to the firm's primary sector of activity. The total numbers of workers and firms are reported at the bottom of the table.

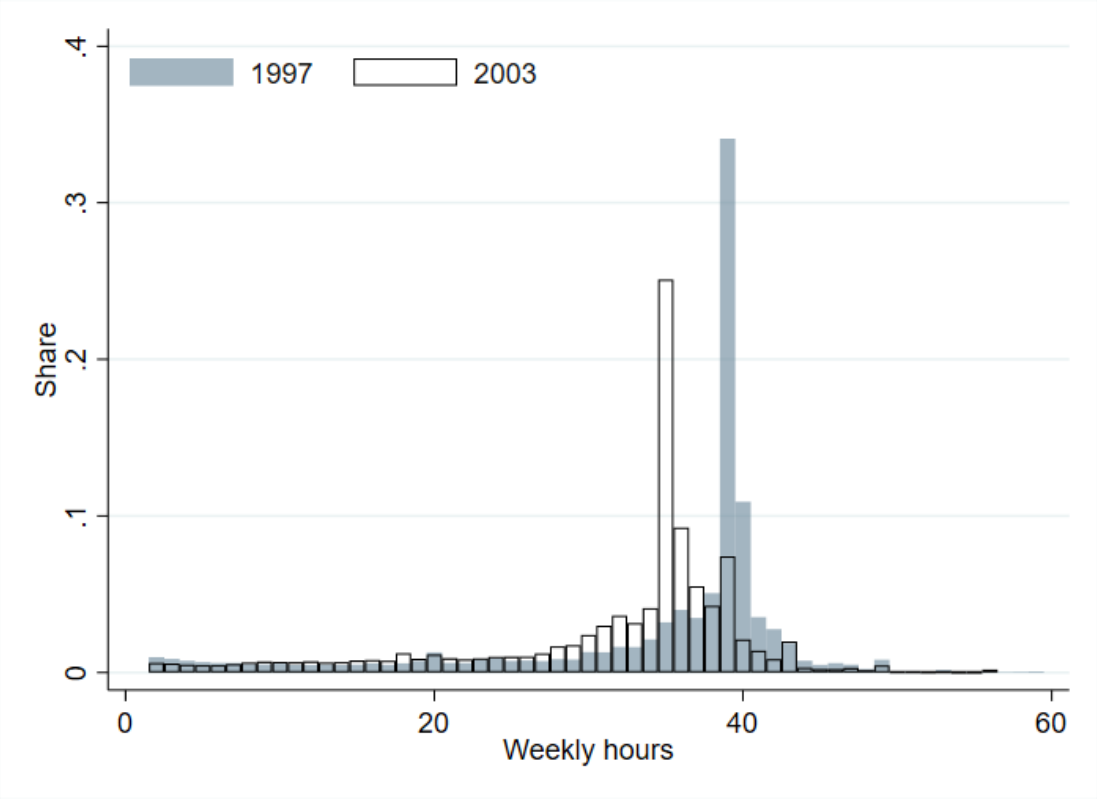
Table 2: Firm-Level Results: First-Stage and Reduced-Form Estimates

	(1) First stage	(2) Workweek	(3) Wage	(4) Labor cost	(5) Total hours	(6) Employment	(7) High-skill	(8) Lower-skill
After x Treated	0.2956***							
	(0.0036)							
$\mathbb{1}_{\{35\text{h workweek}\}}$		-5.9467***	0.0496***	0.0189***	-0.1877***	-0.0056	0.0311***	-0.0307***
		(0.1098)	(0.0130)	(0.0043)	(0.0120)	(0.0117)	(0.0031)	(0.0119)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cragg Donald F stat		19,038	19,038	19,038	19,038	19,038	19,038	19,038
Kleibergen Paap F stat		6,873	6,873	6,873	6,873	6,873	6,873	6,873
Observations	351,304	351,304	351,304	351,304	351,304	351,304	351,304	351,304

Notes: This table presents first-stage and reduced-form estimates of the effects of the 35-hour reform on firm-level outcomes. Column (1) reports the first-stage effect of treatment on the probability that a firm adopts a 35-hour workweek. Columns (2)–(8) report reduced-form effects of the reform on weekly hours, hourly wages, labor costs, total hours worked, overall employment, high-skill employment, and lower-skill employment, respectively. The indicator $\mathbb{1}\{35\text{h workweek}\}_{jt}$ indicates whether a firm operates under a 35-hour workweek. All specifications include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001. Standard errors are clustered at the firm level and reported in parentheses. Cragg–Donald and Kleibergen–Paap F statistics are reported at the bottom of the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Main Figures

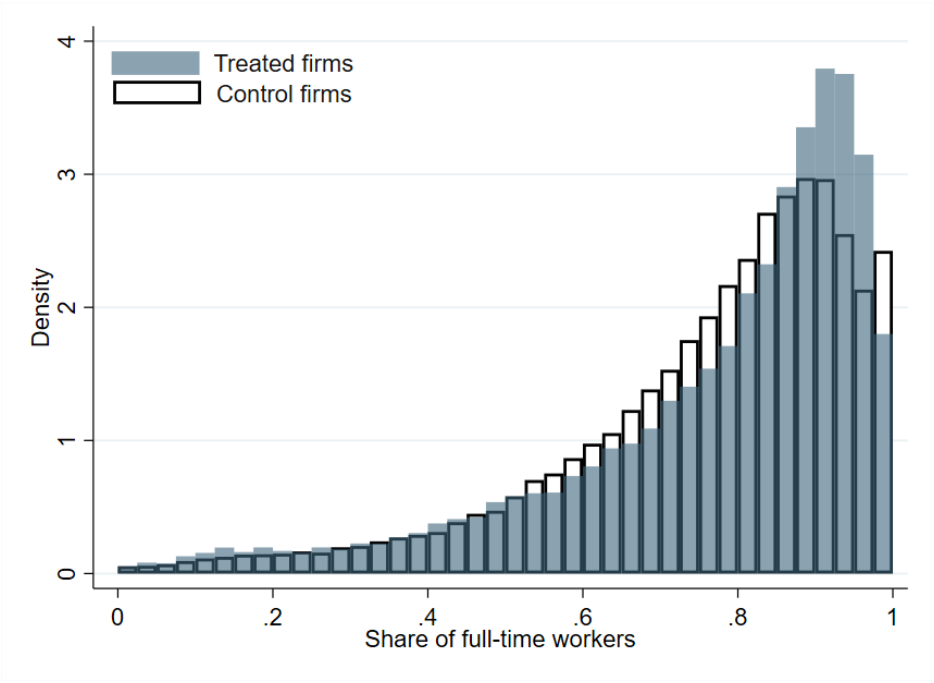
Figure 1: Distribution of Weekly Hours, Pre- and Post-Reform



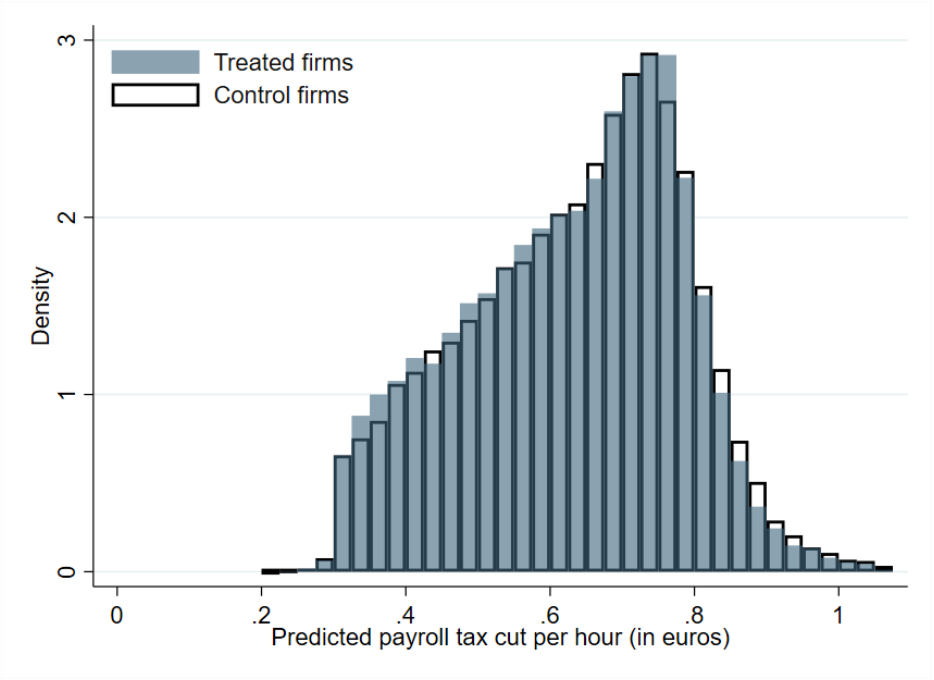
Notes: This figure shows the distribution of weekly hours worked in 1997 (shaded bars) and 2003 (outlined bars). Weekly hours are computed from annual hours reported in the matched employer–employee records, divided by the length of the employment spell and scaled to a weekly measure. The y-axis reports the share of workers in each one-hour bin. The sample consists of all private-sector workers in Metropolitan France. The distribution shifts toward 35 hours in 2003 relative to 1997.

Figure 2: Sources of Cross-Firm Variation Used in the Decomposition

(a) Pre-Reform Share of Full-Time Workers

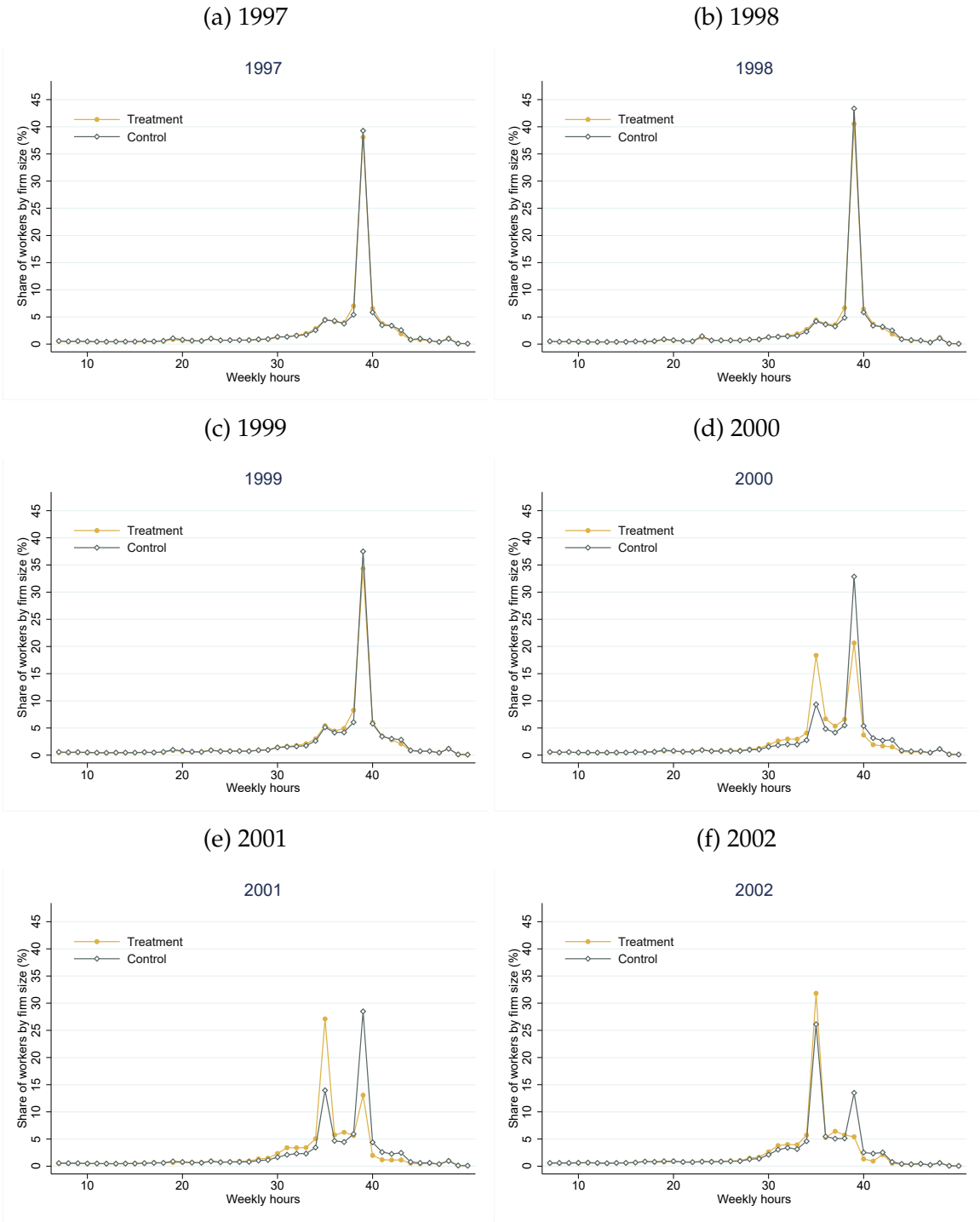


(b) Predicted Payroll Tax Cuts Based on 1997 Wages



Notes: This figure shows the sources of cross-firm variation used in the decomposition analysis. Panel (a) shows the distribution of firms' pre-reform share of full-time-equivalent (FTE) employees in 1997 separately for treated (shaded bars) and control (outlined bars) firms, which proxies differential exposure to the hours mandate. Panel (b) shows the distribution of predicted payroll tax cuts per hour, also separately for treated and control firms, which proxy exposure to the fiscal component of the reform. Predicted payroll tax cuts are constructed based on firms' 1997 wage distributions and the Aubry II tax incentives. The sample consists of private-sector firms with 10–18 or 22–48 FTEs in Metropolitan France, present in all years from 1994 to 2001.

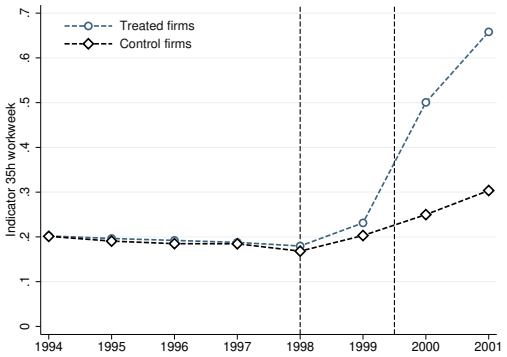
Figure 3: Distribution of Weekly Hours, 1997–2002



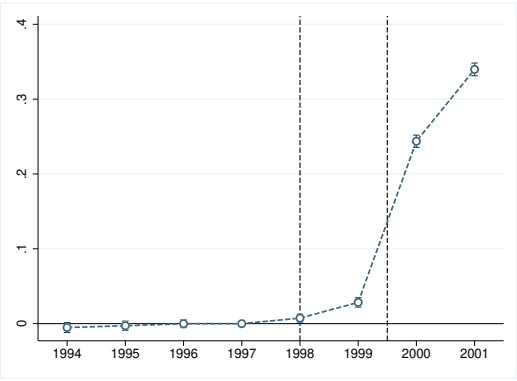
Notes: This figure shows the distribution of weekly hours in treated and control firms from 1997 to 2002. Each panel corresponds to a calendar year, with treated firms shown by the yellow line with solid marks and control firms shown by the gray line with outlined marks. We compute the average weekly working time as the ratio of total hours worked over the year to the number of days employed, multiplied by 7, all reported in the matched employer–employee records. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2002.

Figure 4: Firm-Level Effects of the Reform on Workweek, Hours, and Wage

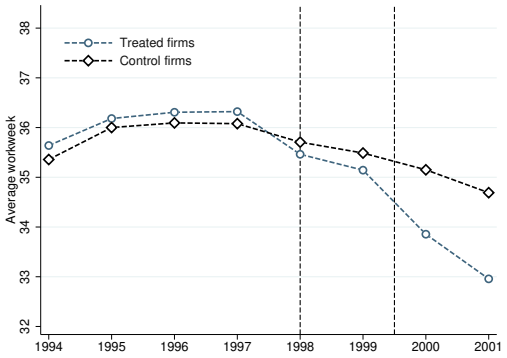
(a) Raw Avg.: 35h Workweek Indicator



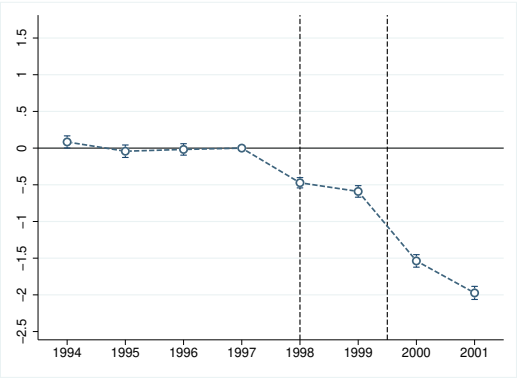
(b) DiD: 35h Workweek Indicator



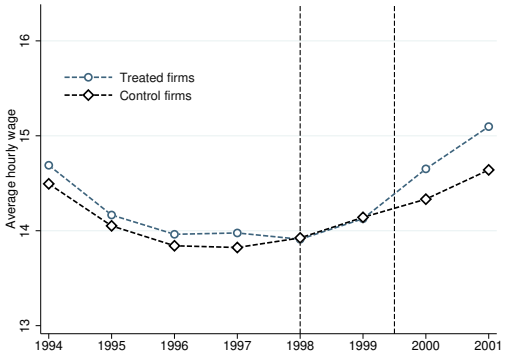
(c) Raw Avg.: Weekly Hours



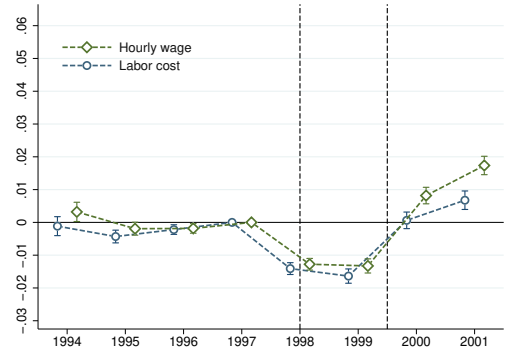
(d) DiD: Weekly Hours



(e) Raw Avg.: Hourly Wages



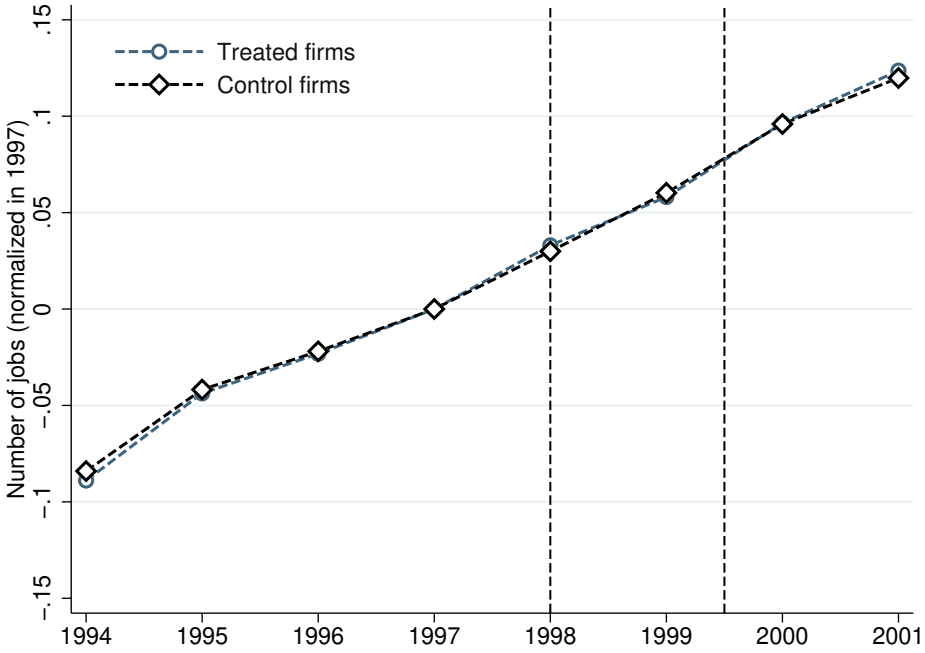
(f) DiD: Log Hourly Wages and Labor Cost



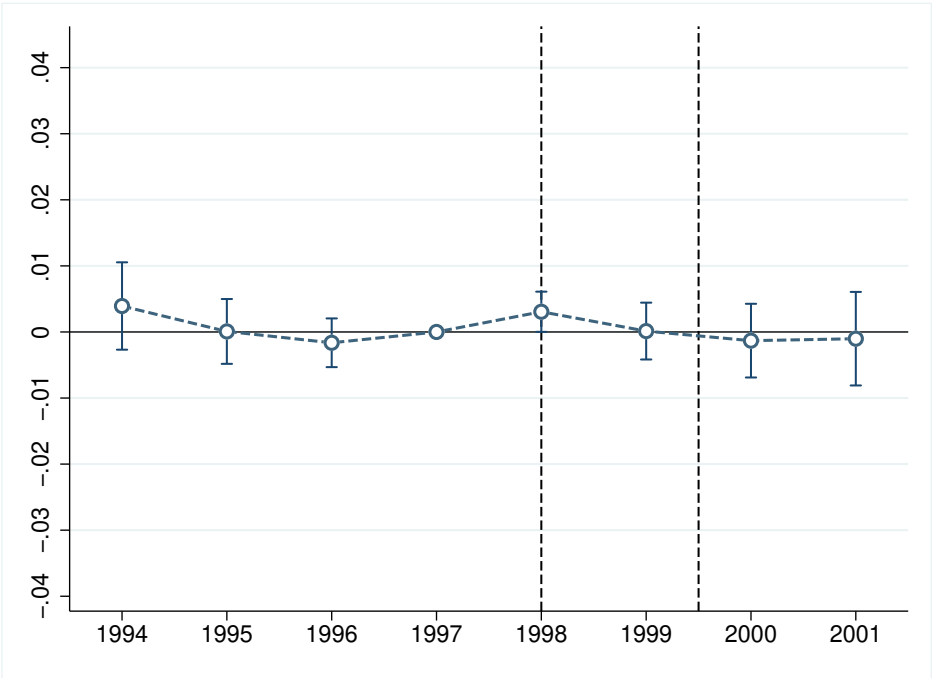
Notes: This figure shows raw trends and DiD estimates of the effects of the 35-hour reform on firms' workweek, hours, and wages. Panel (a) displays raw averages by treatment status for the indicator for operating under a 35-hour workweek. Panel (b) displays the corresponding DiD estimates. Panel (c) displays raw averages by treatment status for weekly hours, and Panel (d) displays the corresponding DiD estimates. Panel (e) displays raw averages by treatment status for hourly wages, and Panel (f) displays the corresponding DiD estimates for log hourly wages and log labor costs. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 5: Firm-Level Effects of the Reform on Employment

(a) Raw Avg.: Employment



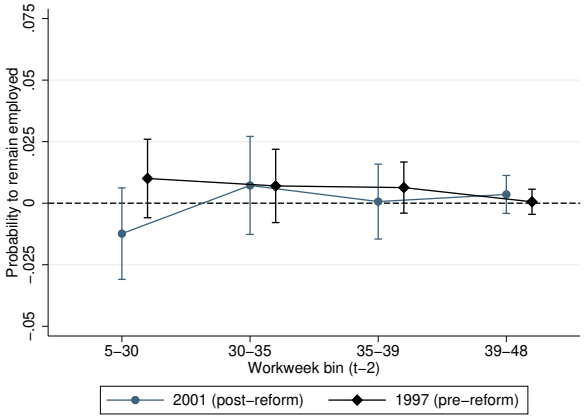
(b) DiD: Log Employment



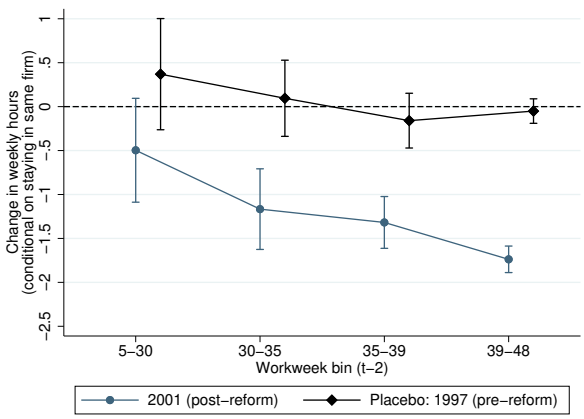
Notes: This figure shows firm-level employment outcomes in treated and control firms. Panel (a) displays raw averages for the number of jobs, by treatment status, normalized to zero in 1997. Panel (b) displays the corresponding DiD estimates for log employment. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 6: Worker-Level Effects on Employment, Hours, and Wage

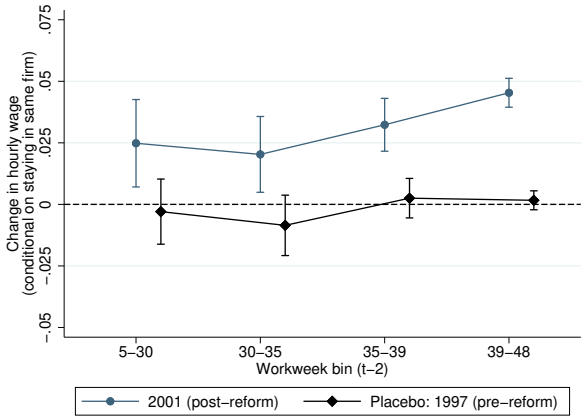
(a) Job Retention Probability



(b) Weekly Hours

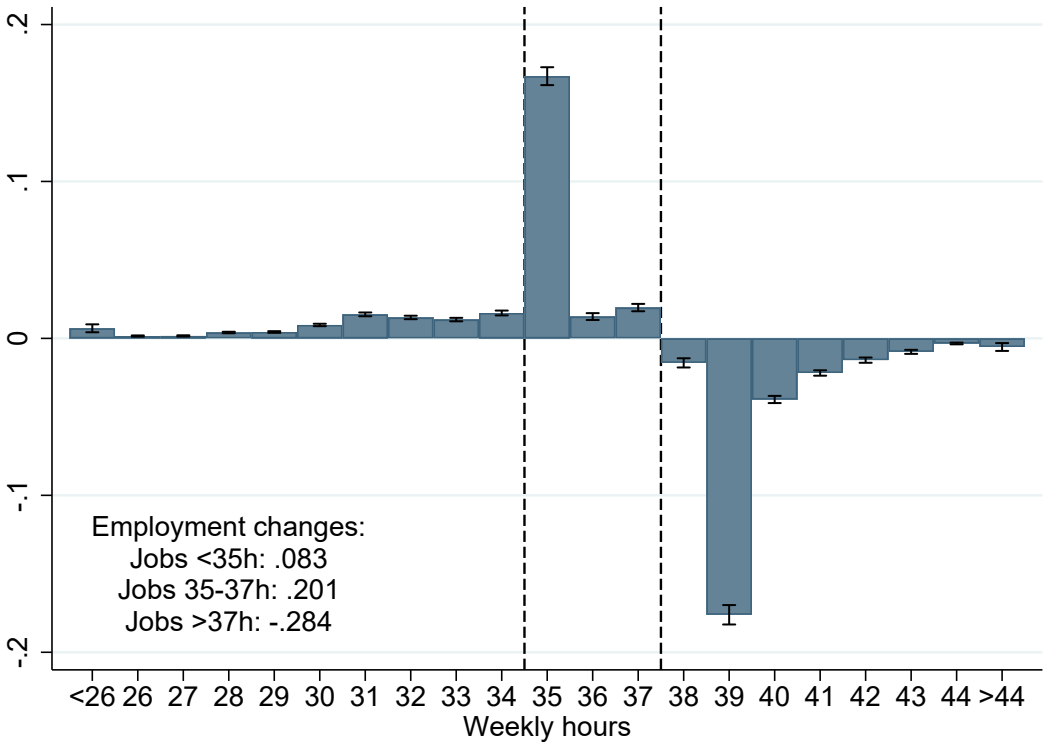


(c) Hourly Wages



Notes: This figure shows worker-level DiD estimates for employment, hours, and wages. Panel (a) displays DiD estimates for job retention probabilities across bins of weekly hours. Panel (b) displays DiD estimates of weekly hours worked. Panel (c) displays DiD estimates of hourly wages. In all panels, estimates are reported separately for the post-reform period (2001) and for a pre-reform comparison year (1997). Weekly hours bins are defined based on hours worked two years prior to the observation year. The coefficients plot the estimated change in the outcome variable between t-2 and t, in treated relative to control firms. For instance, for the 2001 (post-reform) estimates, the coefficients represent the change between 1999 and 2001, in treated relative to control firms. The sample consists of workers employed at private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, and observed in the panel both in t and t-2.

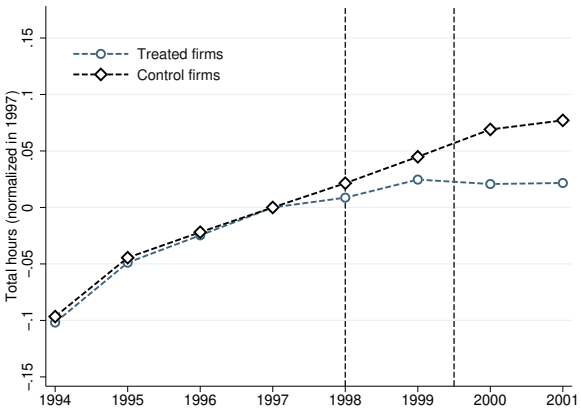
Figure 7: Employment Effects Along the Weekly Hours Distribution



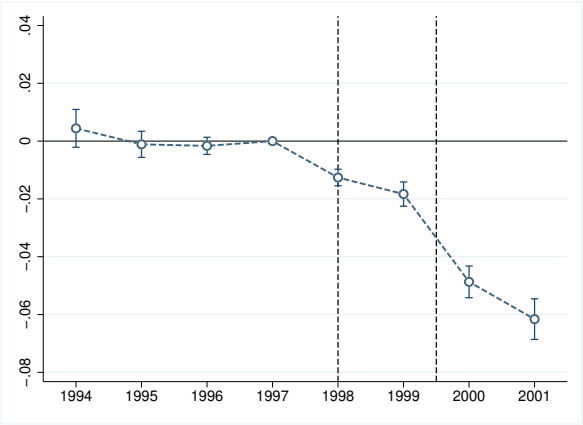
Notes: This figure shows bunching DiD estimates (ala Cengiz et al., 2019) of employment changes within each bin of weekly hours worked. Each bar reports the estimated change in employment associated with a given weekly-hours bin. Dashed vertical lines indicate the bounds of the 35–37 hours range. Aggregate employment changes for jobs below 35 hours, between 35 and 37 hours, and above 37 hours are reported as the sum of estimated changes across the corresponding bins. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 8: Reduction in Total Hours Worked and Its Decomposition

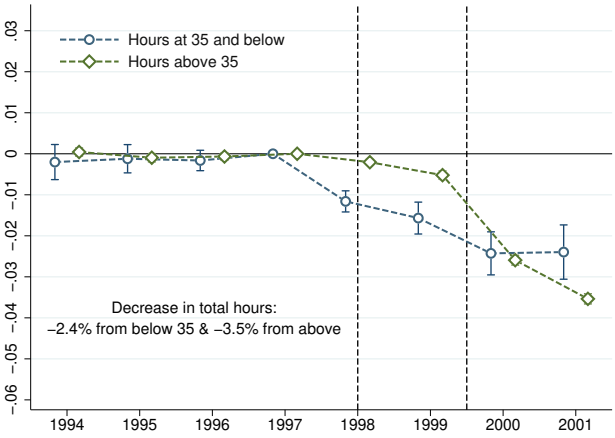
(a) Raw Avg.: Total Hours



(b) DiD: Log Total Hours



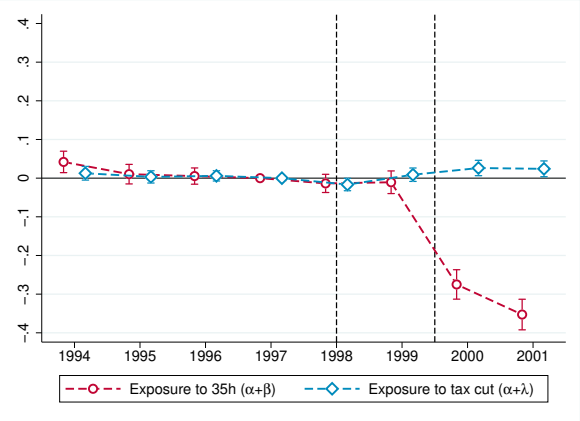
(c) Decomposition of the DiD Effect:
Hours $\leq$ vs. $>$ 35h



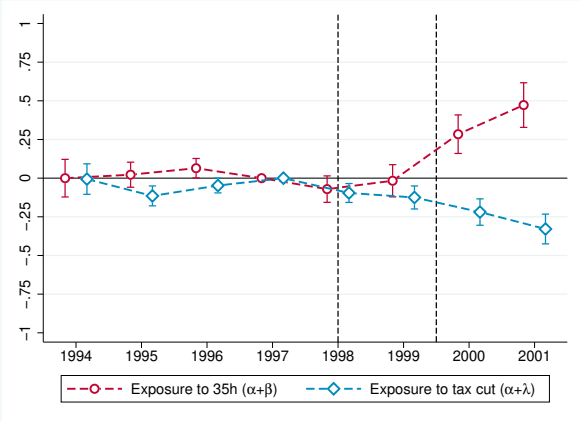
Notes: This figure shows firm-level reduction in total hours worked induced by the reform, and its decomposition. Panel (a) shows raw averages in total hours worked (normalized to zero in 1997) for treated and control firms. Panel (b) shows the corresponding DiD estimates of the effect of the reform on log total hours. Panel (c) decomposes the DiD effect on total hours into contributions from hours worked at or below 35 hours per week and hours worked above 35 hours per week. The reported percentages indicate the cumulative contribution of each margin to the overall reduction in total hours. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 9: Policy Decomposition Design

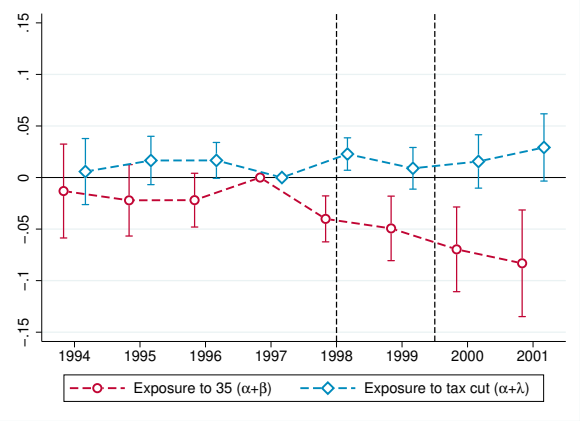
(a) First-Stage: Share of Hours > 35h



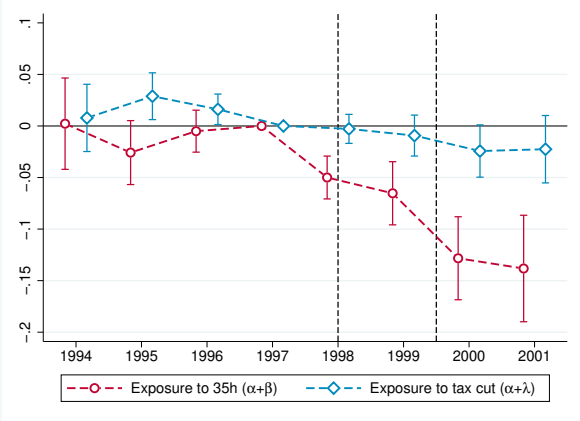
(b) First-Stage: Payroll Taxes per Hour



(c) Effect on Log Employment



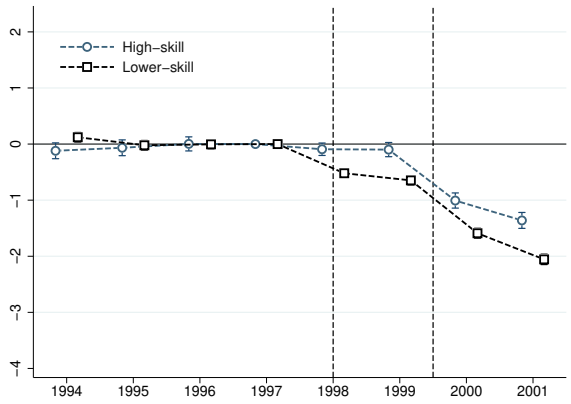
(d) Effect on Log Total Hours



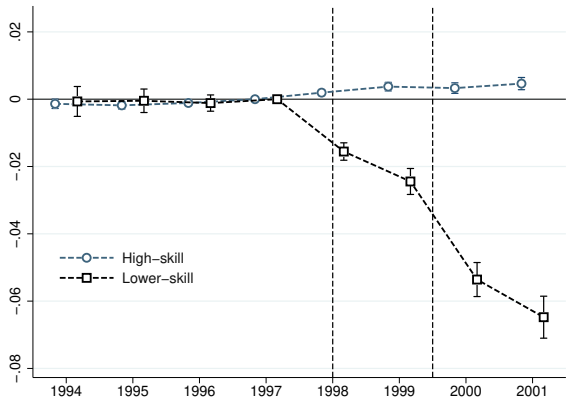
Notes: This figure shows the policy decomposition design, separating exposure to the workweek reduction mandate and to payroll tax cuts. Panel (a) shows first-stage effects on the share of hours worked above 35 hours, separately for exposure to the 35-hour workweek reduction and exposure to payroll tax cuts. Panel (b) shows first-stage effects on payroll taxes per hour under the same two exposure measures. Panel (c) shows estimated effects on employment under counterfactual scenarios in which firms are exposed only to the 35-hour workweek reduction component or only to the payroll tax cut component of the reform. Panel (d) shows corresponding effects on total hours worked under these counterfactual scenarios. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. Error bars represent 95 percent confidence intervals. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 10: Heterogeneity by Skill Group

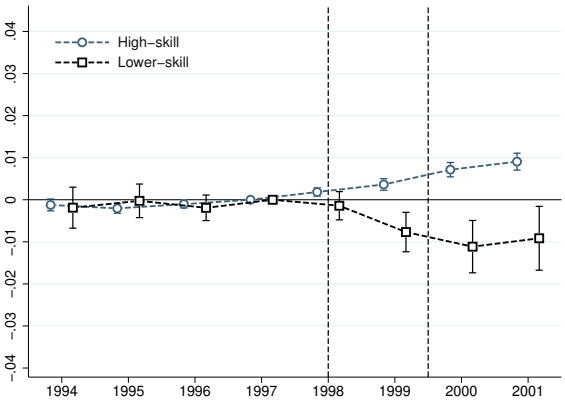
(a) Avg. Weekly Hours



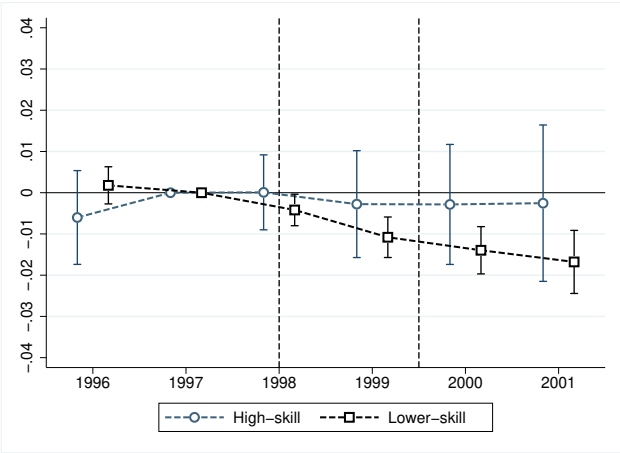
(b) Log Total Hours



(c) Employment Share



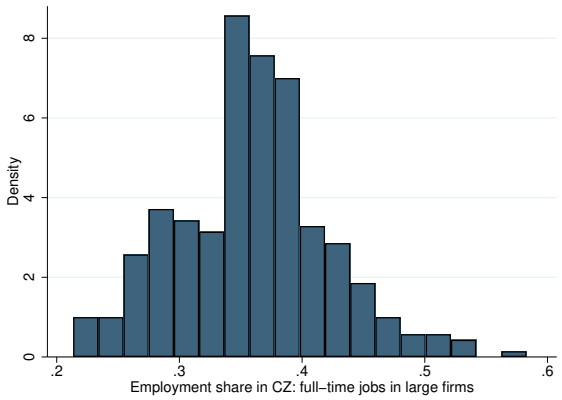
(d) Log Total Monthly Earnings



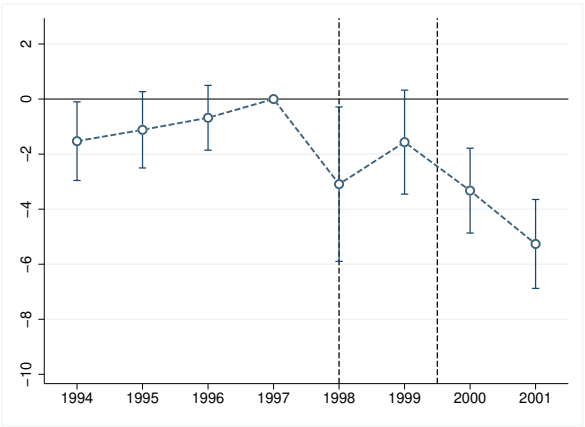
Notes: This figure presents DiD estimates of the effects of the 35-hour reform, separately by skill group. Panel (a) shows the evolution of average weekly hours, separately for high-skilled and lower-skilled workers. Panel (b) shows the evolution of total hours by skill group. Panel (c) shows the evolution of the share of each skill group in total employment, relative to its 1997 share. Panel (d) shows the evolution of log total monthly earnings by skill group. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure 11: Commuting Zone (CZ)-Level Analysis

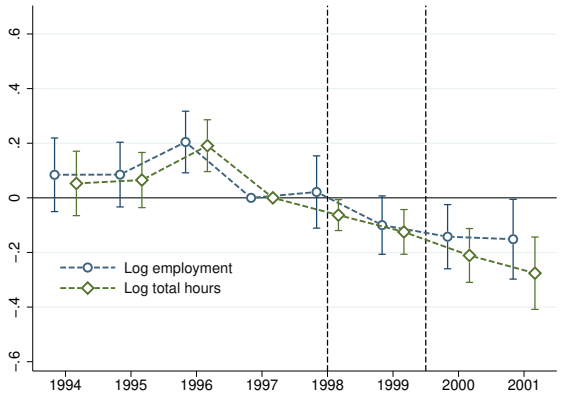
(a) Distribution of CZ Exposure Index



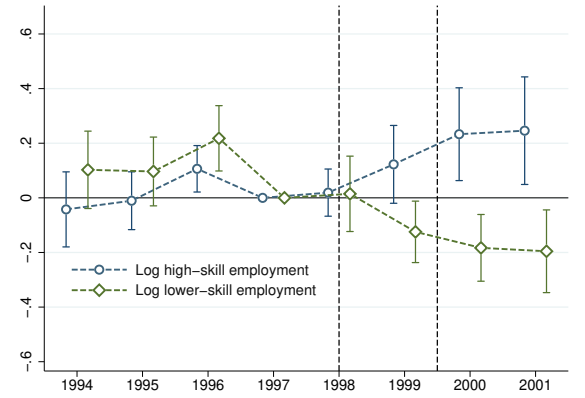
(b) First Stage: Average Weekly Hours



(c) Log Total Hours and Employment

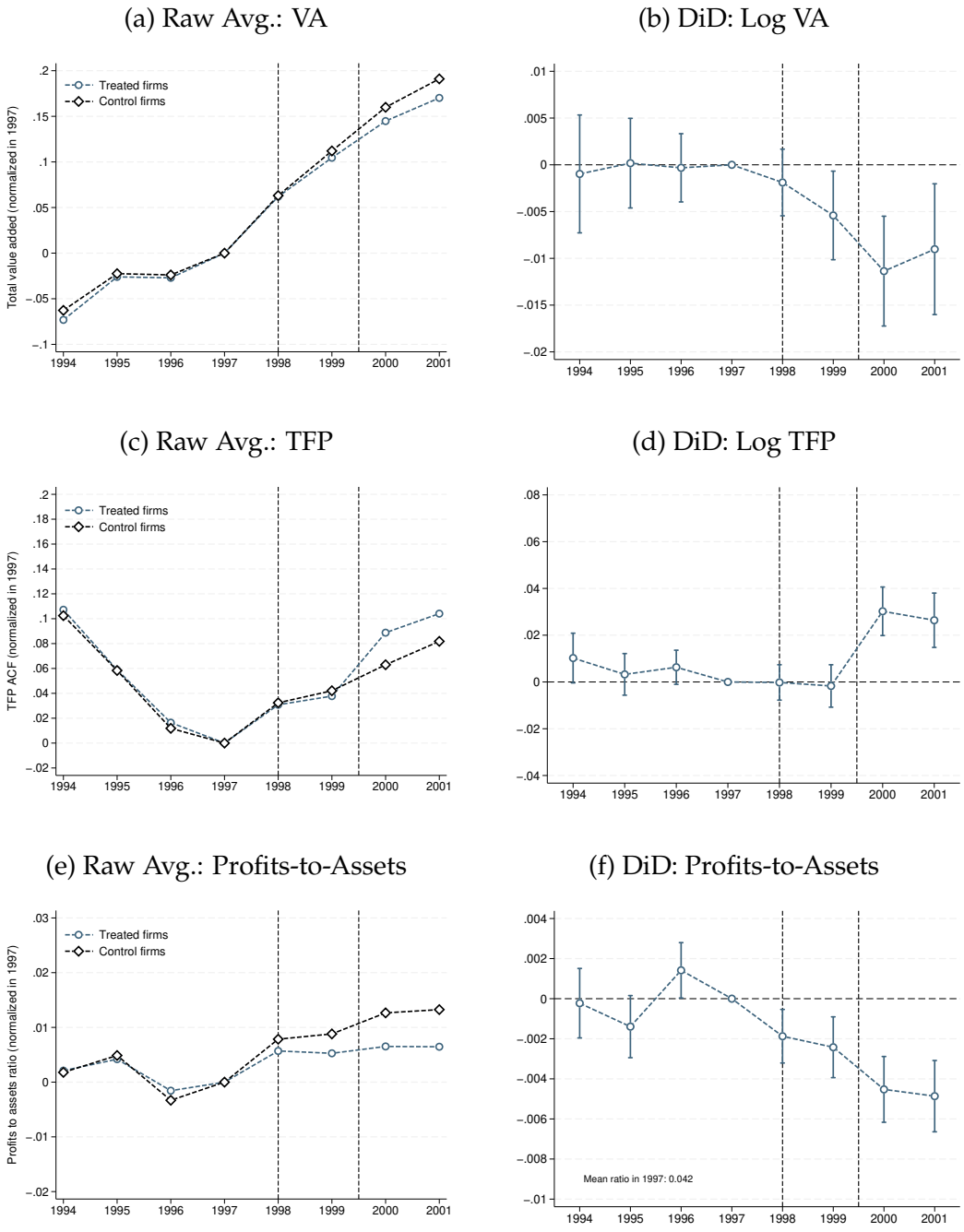


(d) Log Employment by Skill Group



Notes: This figure presents commuting zone (CZ)–level estimates of the impact of the 35-hour reform. Panel (a) shows the distribution of the commuting-zone (CZ) exposure index, defined as the 1997 employment share in full-time jobs in firms with more than 20 employees. Panel (b) shows first-stage estimates of the effect of exposure on average weekly hours. Panel (c) shows the effects of exposure on log total employment and log total hours. Panel (d) shows the effects of exposure on log employment by skill group. Panels (b)–(d) plot coefficients on interactions between the exposure index and year dummies from the event-study regressions. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. CZ-level outcomes are constructed from private-sector firms in Metropolitan France, restricting to firms with 10–18 or 22–48 full-time-equivalent (FTE) employees. Exposure is defined as the 1997 employment share in full-time jobs at firms with more than 20 employees. The analysis covers 304 commuting zones observed from 1994 to 2001.

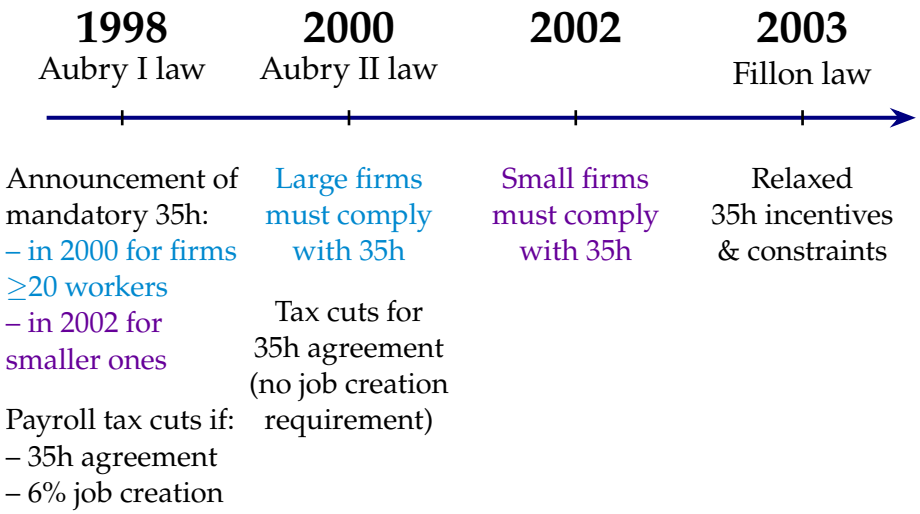
Figure 12: Firm-Level Effects of The Reform on VA, TFP, and Profits



Notes: This figure shows firm-level effects of the 35-hour reform on value added (VA), total factor productivity (TFP), and profits. Panel (a) displays raw averages by treatment status for (normalized) total value added. Panel (b) displays the corresponding DiD estimates for log value added. Panel (c) displays raw averages by treatment status for TFP. Panel (d) displays the corresponding DiD estimates for log TFP. Panel (e) displays raw averages by treatment status for profits-to-assets. Panel (f) displays the corresponding DiD estimates. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

A Appendix Figures

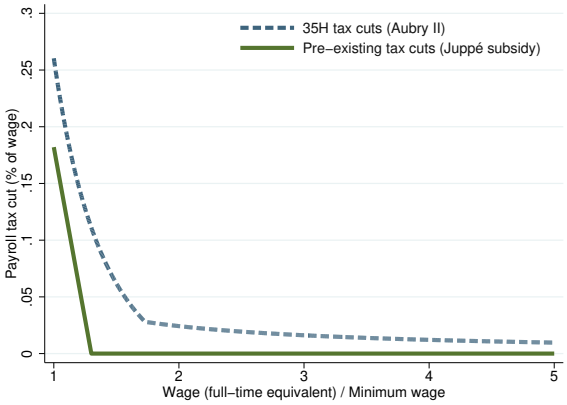
Figure A.1: Timeline of the 35-Hour Reform in France



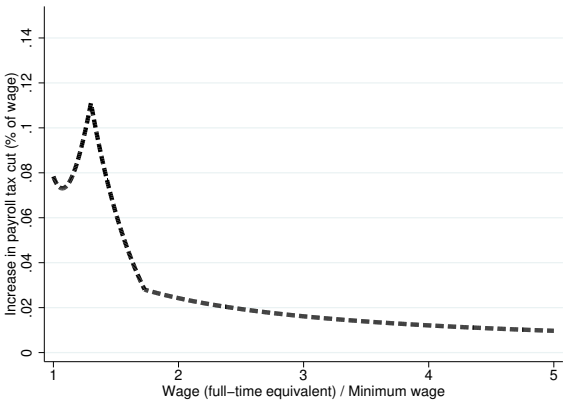
Notes: This figure shows the timeline of the implementation of the 35-hour workweek reform in France between 1998 and 2003. The figure highlights key legislative milestones. The figure also notes the relaxation of incentives and constraints under the Fillon reforms in 2003.

Figure A.2: Structure of Payroll Tax Cuts under The Reform

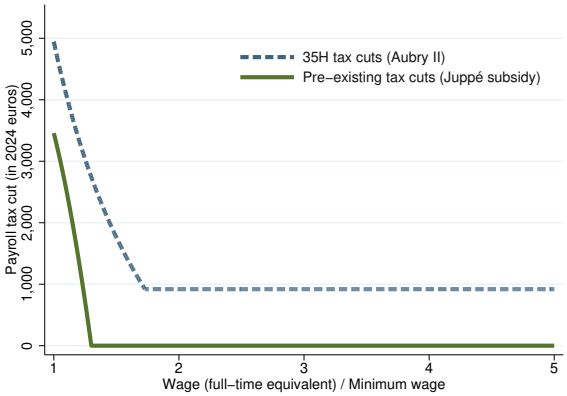
(a) Tax Cut Schedule Pre- and Post-Reform (Percent of Wage)



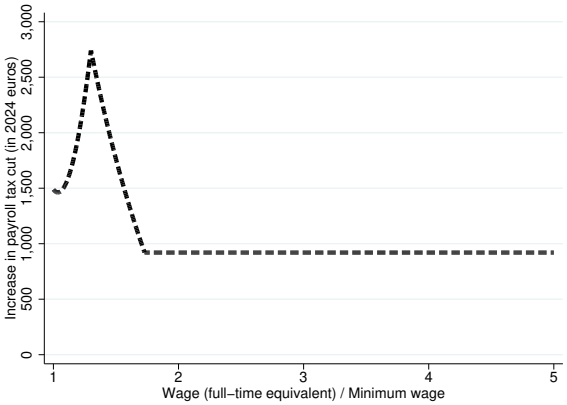
(b) Change in Tax Cuts Associated with 35h (Percent of Wage)



(c) Tax Cut Schedule Pre- and Post-Reform (in 2024 Euros)



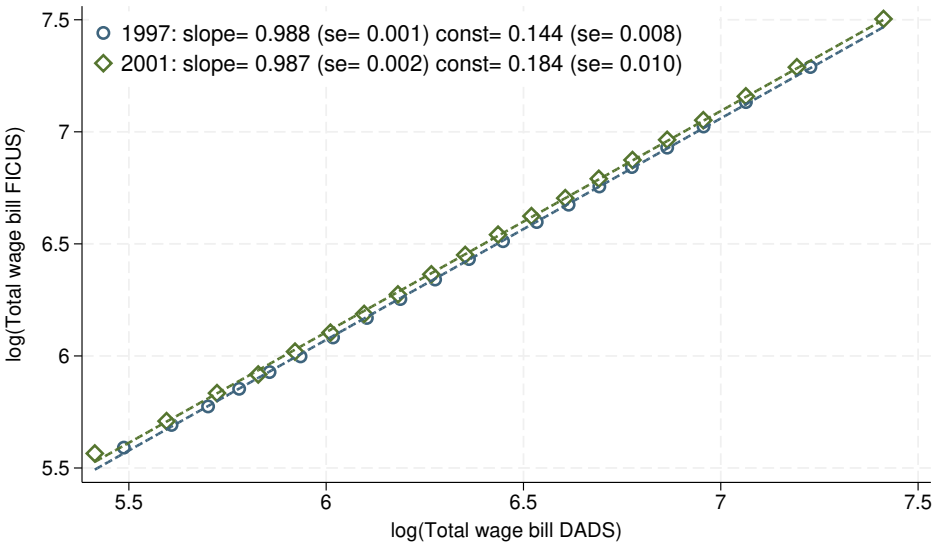
(d) Change in Tax Cuts Associated with 35h (in 2024 Euros)



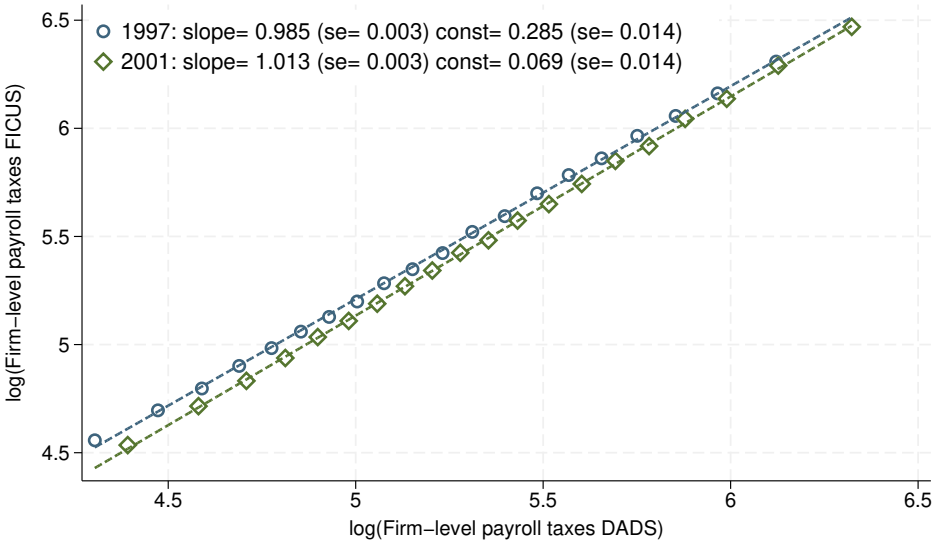
Notes: This figure shows the structure of employer payroll tax cuts associated with the 35-hour reform. Panel (a) plots the schedule of employer payroll tax cuts as a percentage of wages for full-time equivalent earnings, comparing the pre-existing Juppé subsidy with the tax cuts introduced under the Aubry II law. Panel (b) shows the change in payroll tax cuts induced by the 35-hour reform, again expressed as a percentage of wages. Panel (c) reports the same schedules as in panel (a), expressed in annual 2024 euros. Panel (d) shows the corresponding change in payroll tax cuts in 2024 euros. In all panels, the horizontal axis measures full-time equivalent wages relative to the statutory minimum wage.

Figure A.3: Validation of Payroll Tax Imputations

(a) Wage Bill

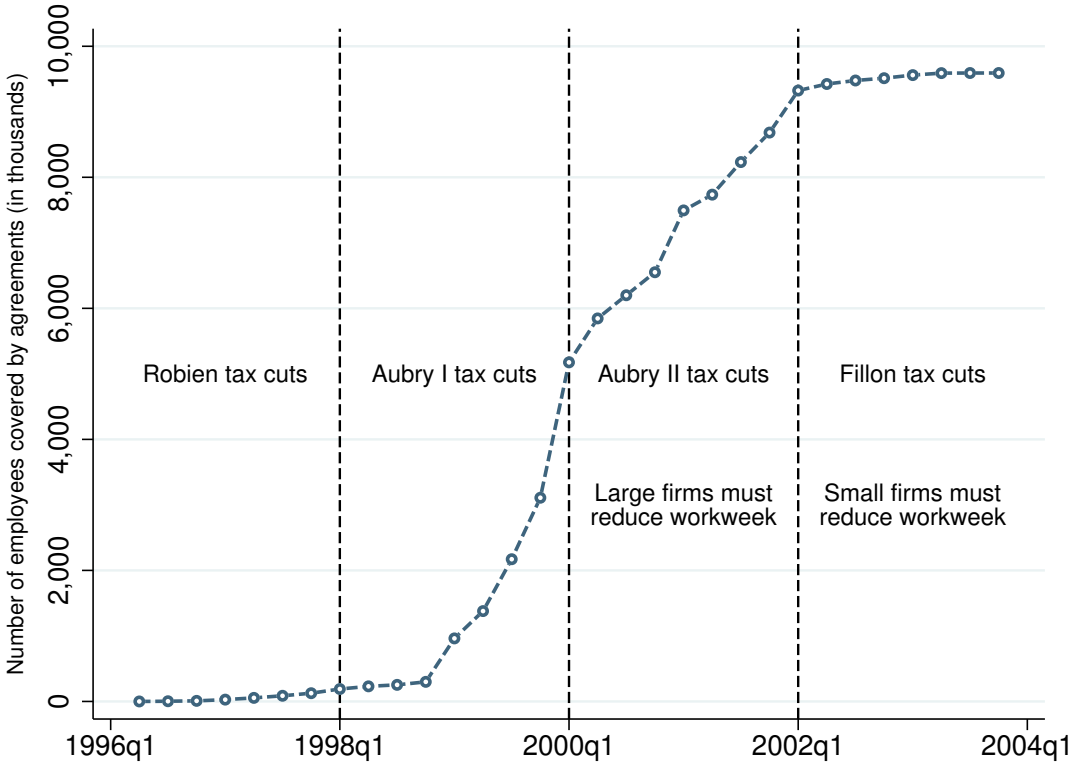


(b) Payroll Taxes



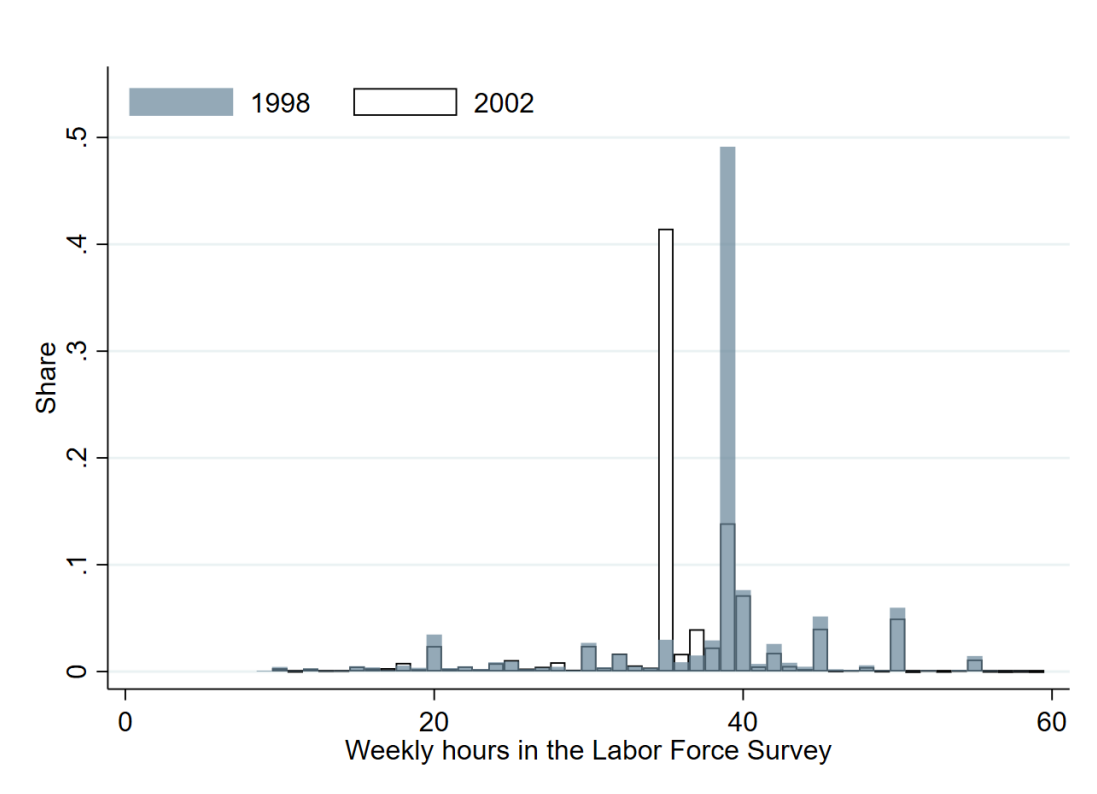
Notes: This figure compares firm-level wage bills and payroll taxes constructed from linked employer-employee data and balance sheet data. Panel (a) plots log total wage bill measured in FICUS against log total wage bill measured in DADS at the firm level for 1997 and 2001. Panel (b) plots log firm-level payroll taxes measured in FICUS against log firm-level payroll taxes inferred by combining DADS with workweek agreement information for the same years. Dashed lines report the OLS fit of the FICUS variable on the corresponding DADS-based variable. Estimated slopes and intercepts, along with standard errors, are reported in the legend. The 45-degree line corresponds to equality between the two measures.

Figure A.4: Timing and Coverage of Workweek Reduction Agreements



Notes: This figure shows the cumulative number of employees covered by signed workweek reduction agreements over time. Vertical dashed lines indicate key legislative milestones under the Aubry and Fillon reforms, including the introduction of payroll tax incentives and the phased compliance deadlines for large and small firms. The majority of agreements were signed between 1999 and 2001, aligning with the phased implementation deadlines and fiscal incentives under the Aubry laws.

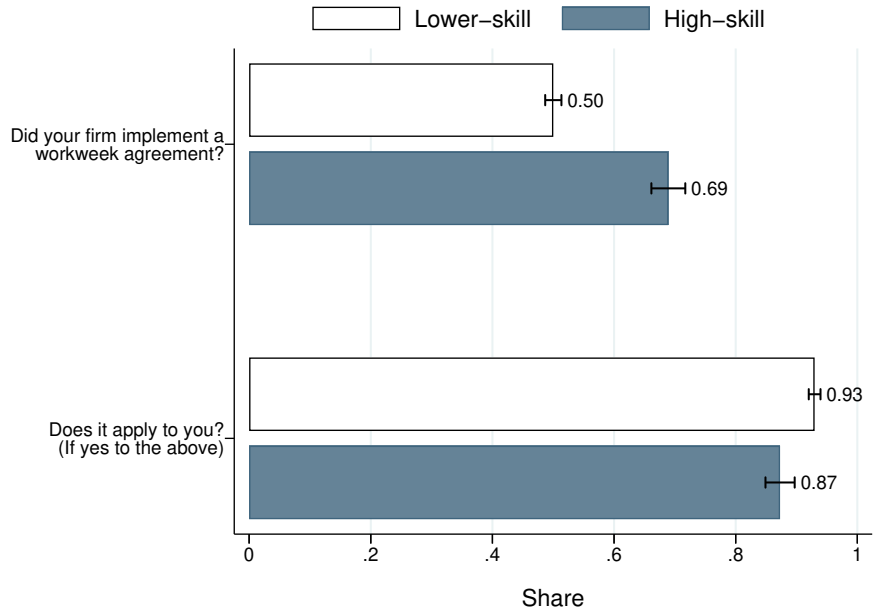
Figure A.5: Distribution of Weekly Hours (LFS), Pre- and Post-Reform



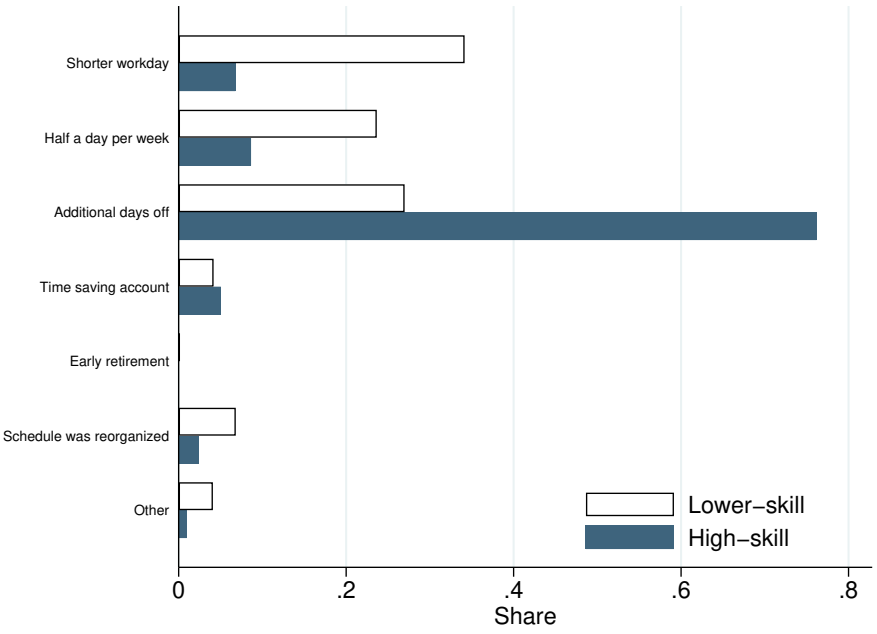
Notes: This figure shows the distribution of weekly hours using self-reported data from the French Labor Force Survey (LFS) in 1998 and 2002. We use 1998 rather than 1997 (as in Figure 1) because firm size information is not available in the LFS in 1997, and 2002 rather than 2003 because the LFS underwent a major redesign in 2003, switching from an annual cross-section to a quarterly panel and reformulating several questions. The figure shows a clear shift in mass from 39 to 35 weekly hours, consistent with the patterns observed in the administrative data.

Figure A.6: Workweek Reduction Modalities by Skill Group

(a) Worker-Reported Workweek Change, 2001



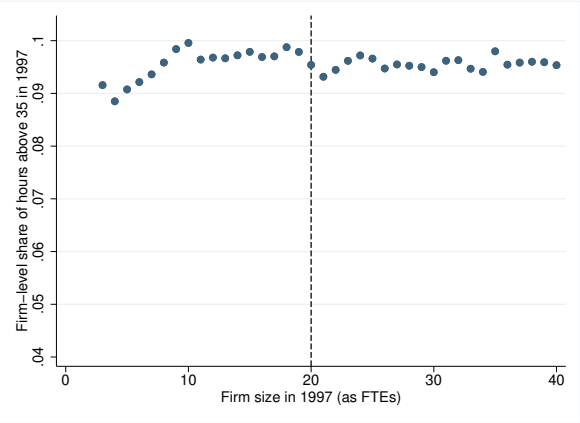
(b) Worker-Reported Modes of Working Time Reduction



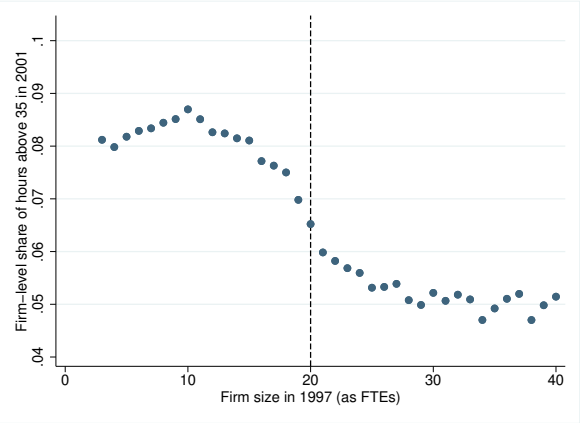
Notes: This figure shows survey-based evidence on the implementation and modalities of workweek reductions, using data from the French Labor Force Survey (LFS) in 2001. Panel (a) reports, separately for lower- and higher-skill workers, in the 2001 wave, the share reporting that their employer implemented a workweek-reduction agreement and, conditional on this, the share reporting that the agreement affected their own schedule (i.e., that their workweek was reduced). Panel (b) shows, among workers covered by a workweek-reduction agreement, the distribution of reported modes of working-time reduction, separately by skill group.

Figure A.7: Firm Size and Reform Implementation

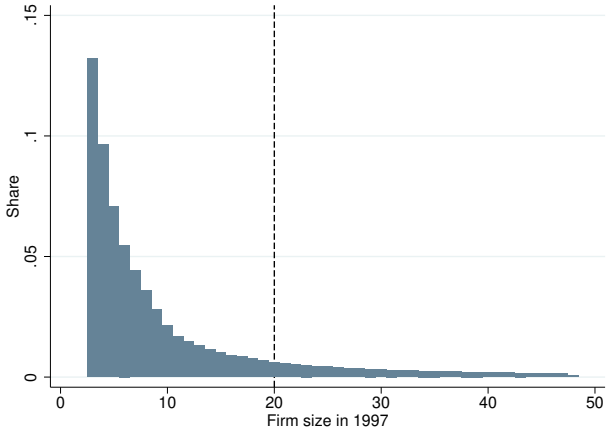
(a) Share of Firm-Level Hours > 35h, 1997



(b) Share of Firm-Level Hours > 35h, 2001



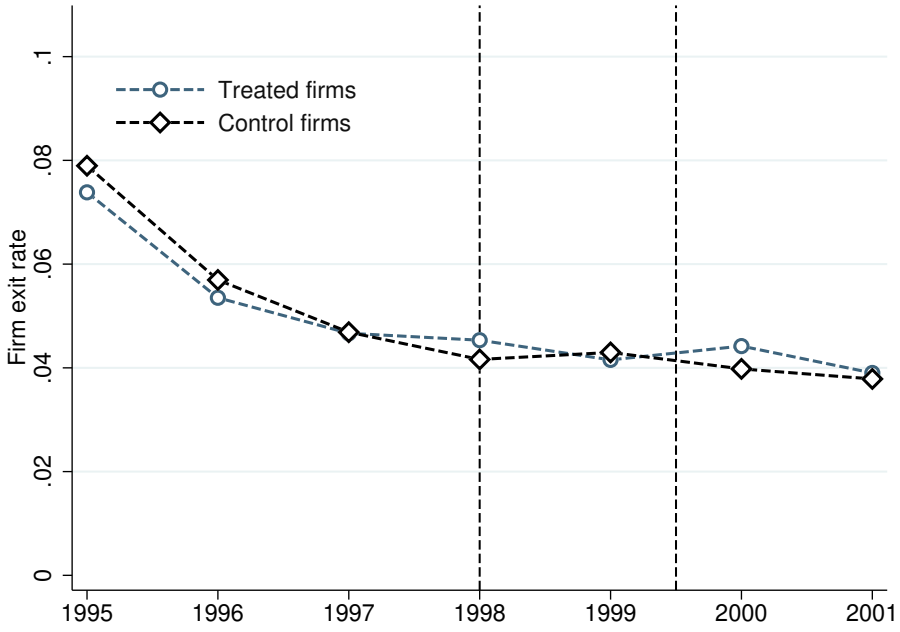
(c) Distribution of Firm Size, 1997



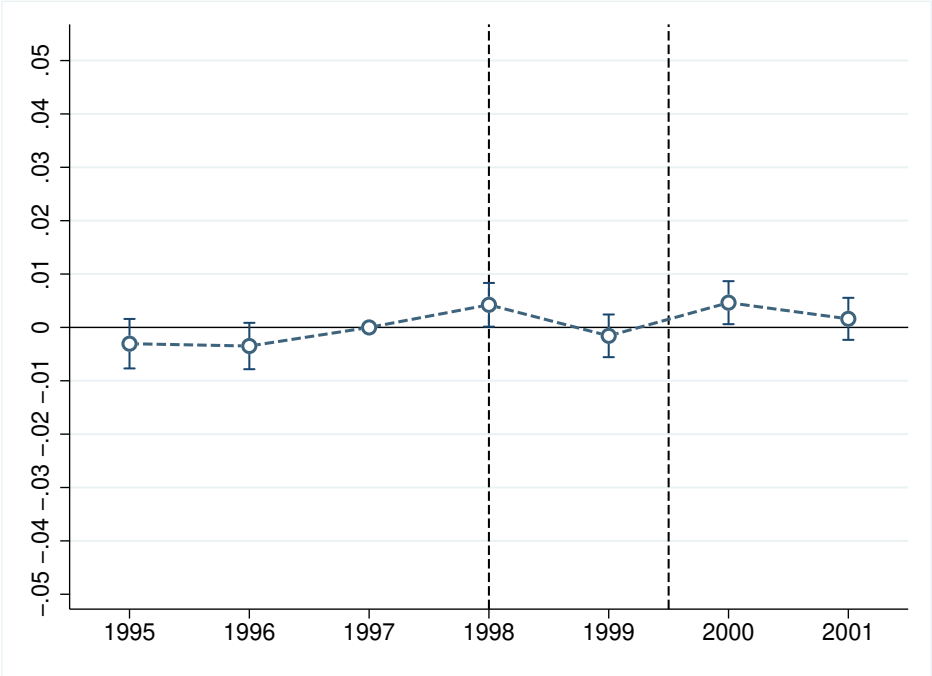
Notes: This figure examines the relationship between firm size and outcomes associated with the implementation of the 35-hour reform in the estimation sample. Panel (a) displays the share of firm-level hours above 35 per week against firms' full-time equivalent (FTE) employment in 1997, prior to the reform. Panel (b) shows the same relationship in 2001, after the reform had been implemented for large firms. The discrete drop in hours above 35 at the 20-employee threshold in 2001 indicates that firm size is a strong proxy for treatment assignment under the staggered implementation rules. Panel (c) displays the distribution of firm size in FTEs in 1997. The absence of bunching just below the 20-employee cutoff suggests limited manipulation of firm size around the threshold.

Figure A.8: Firm Survival, Pre- and Post-Reform

(a) Raw Avg.: Firm Exit Rates



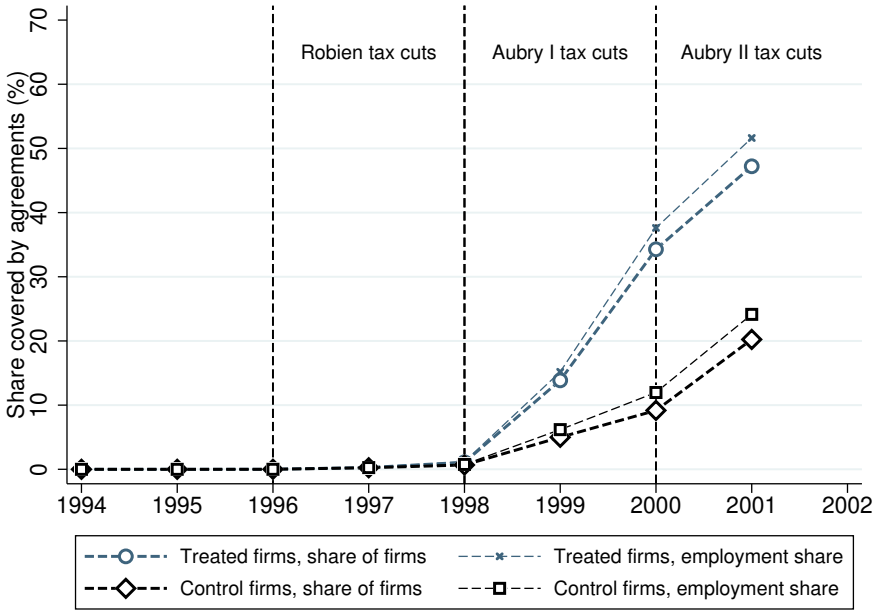
(b) DiD: Firm Exit Rates



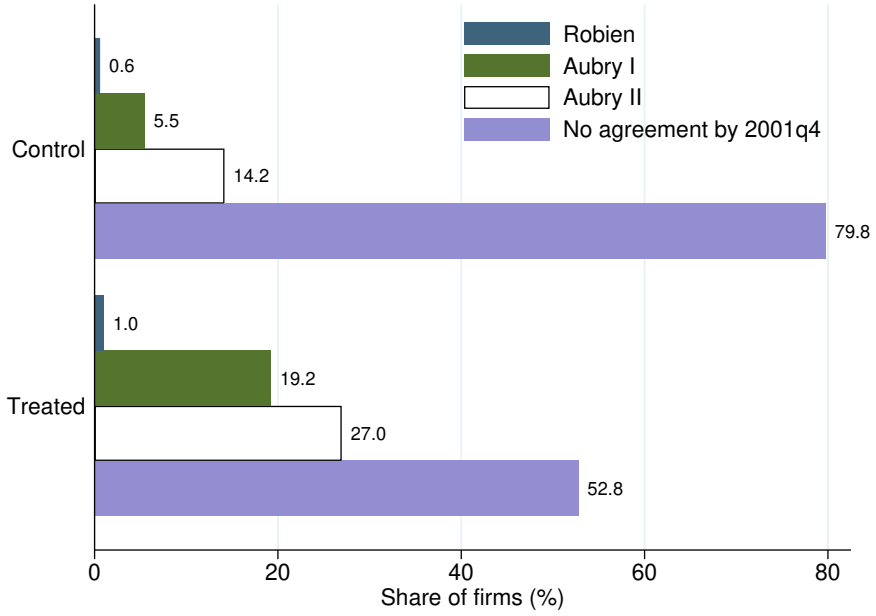
Notes: This figure examines firm survival before and after the 35-hour reform. Panel (a) reports annual firm exit rates separately for treated and control firms over time. Panel (b) reports DiD estimates of the effect of the reform on firm exit rates. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure A.9: Workweek-Reduction Agreements Signed in Our Estimation Sample

(a) Coverage by Any Workweek-Reduction Agreement

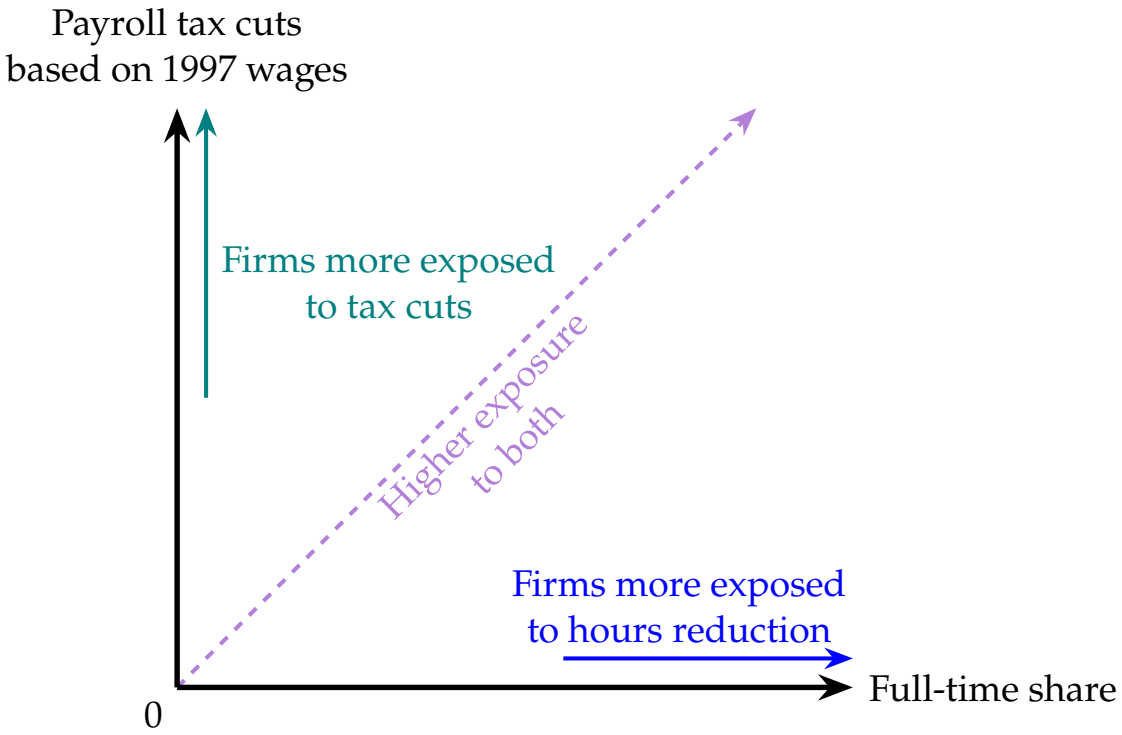


(b) Distribution of Agreement Types



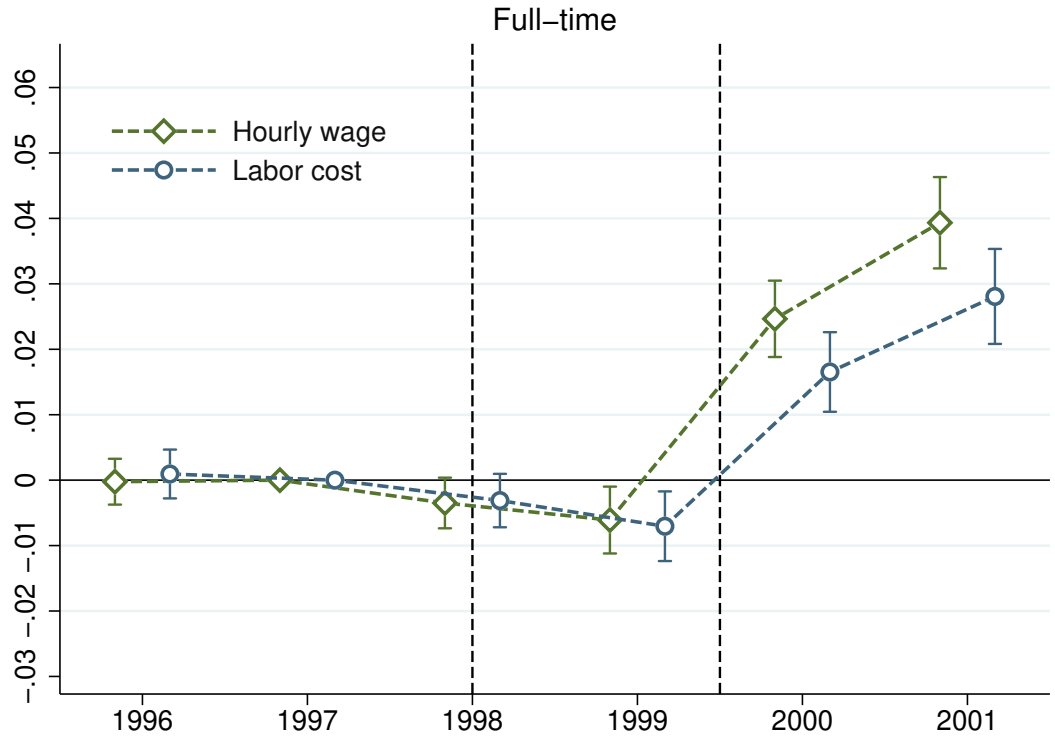
Notes: This figure shows the take-up of workweek-reduction agreements in the estimation sample. Panel (a) reports, over time, the share of firms and the share of employment in treated and control groups covered by any workweek-reduction agreement between 1994 and 2002. Vertical dashed lines indicate the introduction of the Robien, Aubry I, and Aubry II tax cuts. Panel (b) reports the distribution of agreement types (Robien, Aubry I, Aubry II) and the share of firms that had signed no agreement by the fourth quarter of 2001. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure A.10: Dimensions of Firm Exposure to Reform Components



Notes: This figure shows the two separate dimensions of firm exposure to the 35-hour reform. The horizontal axis represents exposure to the working-time constraint, measured by firms' pre-reform reliance on FTEs. The vertical axis represents exposure to payroll tax cuts, determined by firms' 1997 wage distribution and the structure of the subsidy schedule. For example, firms located further to the right are more exposed to hours reductions, firms higher on the vertical axis are more exposed to payroll tax cuts, and firms in the upper-right quadrant are highly exposed to both components. This variation in pre-reform characteristics underpins our empirical strategy to separately identify the effects of working-time constraints and payroll tax subsidies.

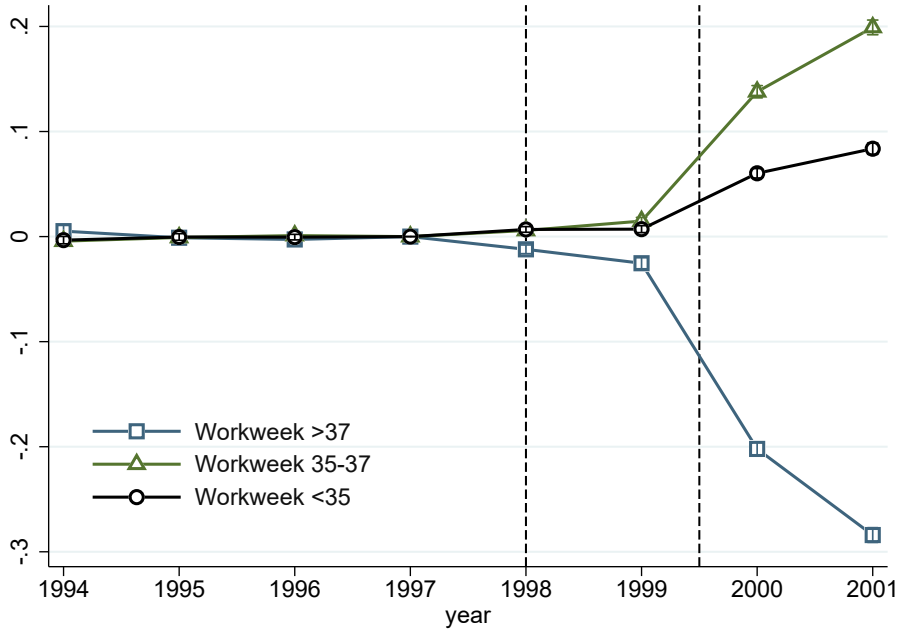
Figure A.11: Wage Effects of the Reform on Full-Time Workers



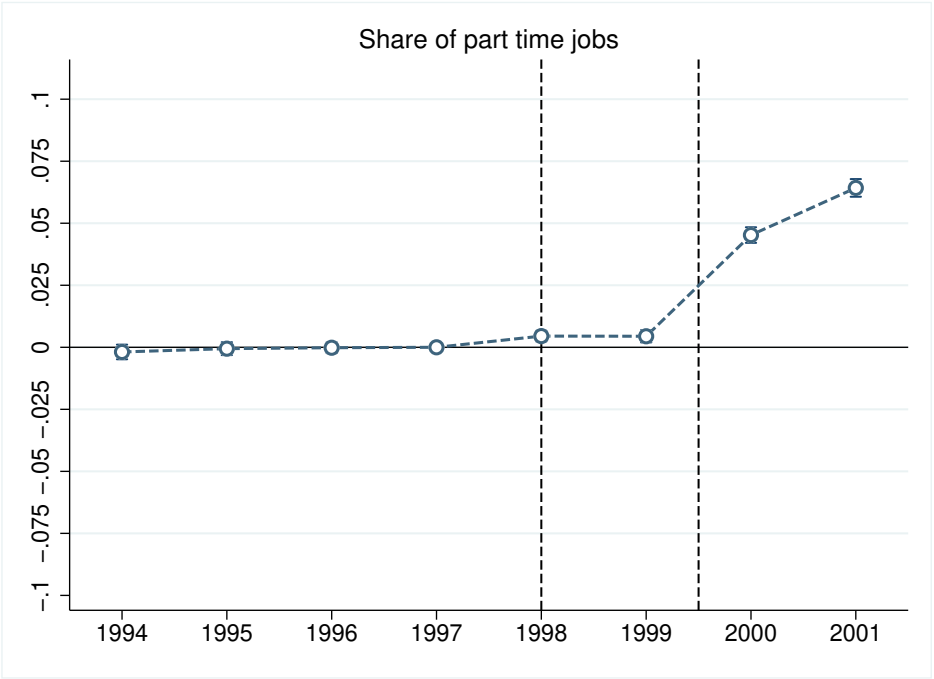
Notes: This figure shows the wage effects of the 35-hour reform for full-time incumbent workers. It plots event-study estimates of changes in hourly wages and total labor costs for full-time workers in treated firms relative to control firms. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of full-time incumbent workers employed in private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure A.12: Firm-Level Shift Toward Part-Time Employment

(a) Employment by Workweek



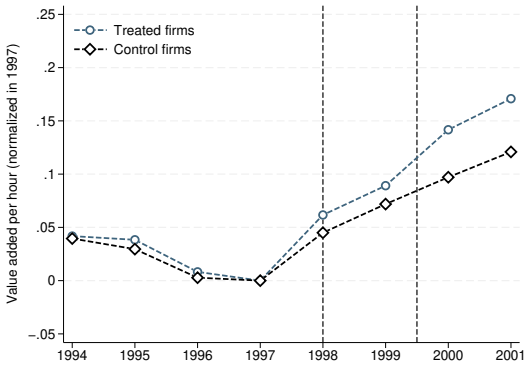
(b) Firm-Level Part-Time Employment Share



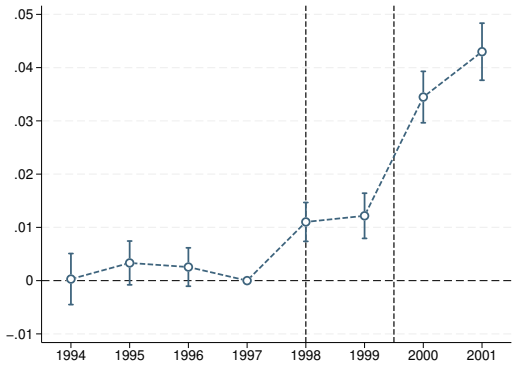
Notes: This figure shows firm-level adjustments in the distribution of employment across weekly hours categories following the 35-hour reform. Panel (a) reports changes in employment by workweek length, distinguishing jobs with weekly hours above 37, between 35 and 37 hours, and below 35 hours. Panel (b) reports DiD estimates of the effect of the reform on the firm-level share of part-time employment. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.

Figure A.13: Labor Productivity and Decomposition of VA

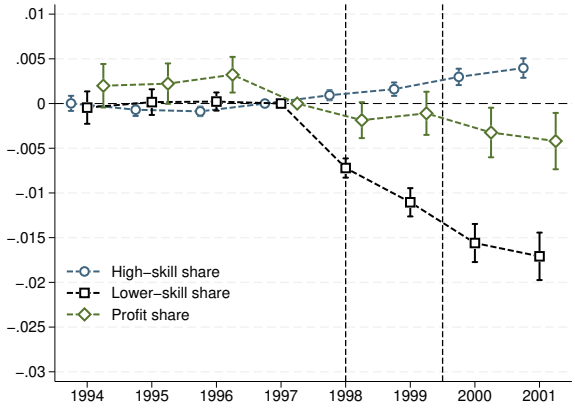
(a) Raw Avg.: VA per Hour



(b) DiD: VA per Hour



(c) Decomposition of the DiD Effect



Notes: This figure shows firm-level labor productivity and the decomposition of value added (VA) following the 35-hour reform. Panel (a) reports average value added (VA) per hour in treated and control firms, normalized to zero in 1997. Panel (b) reports the corresponding DiD estimates for value added per hour. Panel (c) decomposes changes in VA into components accruing to labor, separately for high- and low-skill workers, and to profits. The first and second vertical dashed lines indicate, respectively, the announcement of the Aubry reforms and the deadline for implementation in large firms. The sample consists of private-sector firms with 10–18 or 22–48 full-time-equivalent (FTE) employees in Metropolitan France, present in all years from 1994 to 2001.