Payroll Tax Reductions for Minimum Wage Workers: Relative Labor Cost or Cash Windfall Effects?*

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Abstract

This paper uses administrative employer-employee data to uncover the effects of a large payroll tax reduction for minimum-wage workers in France in the 1990s. Exploiting the change in labor costs both at the job level and at the firm level, I find that the number of minimum-wage jobs increases but that these additional jobs stem exclusively from firms which had previously very few or no minimum wage workers. On the other hand, firms which already employed workers at minimum-wage levels initially, and thus benefit *ex ante* from a cash windfall, increase employment irrespective of wage levels. These results show that not all firms react to changes in relative labor costs and highlight the importance of alleviating liquidity constraints for firm growth.

JEL codes: H22, H25, H32, J23

Keywords: Payroll taxes, Firm behavior, Rent sharing, Minimum wage.

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1 Introduction

The general public often perceives changes in firm taxation as borne by the firms—in particular, tax cuts are widely seen as corporate giveaways. On the other hand, economists tend to stress that taxes on firms are ultimately borne by individuals, be it workers, consumers or shareholders. Firms appear as transparent entities, which can be reduced to their production function and serve as a tax remitter and an incidence-maker.

In the case of payroll taxes, the added cost on labor that these taxes nominally impose on employers could be detrimental to minimum wage employment, as firms cannot pass it on to workers in the form of lower wages. Reducing this added cost could therefore prove beneficial to low-wage employment (Cahuc, 2003). In this perspective, many countries implemented payroll tax reductions targeting low-skilled, disadvantaged workers.[1] In 2005, the OECD advocated for "[reducing] direct taxes (social security contributions and income taxes) on those with low earnings where this would shift the structure of labor demand toward low-wage workers, while protecting their incomes" (Brandt et al., 2005). In France, payroll tax reductions for minimum wage workers have been implemented since the 1990s. Evaluations of the French payroll tax reductions have found important firm-level employment effects (Crépon and Desplatz, 2001; Bunel and L'Horty, 2012), stemming from a reduction of job destruction rather than improved employment prospects from the unemployed (Kramarz and Philippon, 2001). They did not generate low-pay traps (Aeberhardt and Sraer, 2009; Lhommeau and Rémy, 2009).

Recent empirical work has however shed light on unexpected effects of payroll tax cuts. In particular, Saez et al. (2019) and Carbonnier et al. (2020) show that policies reducing the labor cost of a specific group of workers, irrespective of their potential effect on employment for these workers, also induce a cash windfall for firms already relying intensively on this particular type of labor. In the case of the Swedish payroll tax cut targeted at young workers, youth-intensive firms shared the rent through increased wages and also expanded more (Saez et al., 2019). In contrast, a corporate tax credit lowering labor costs for low- and medium-wage workers did not affect employment and the additional cash was shared primarily with high-skilled workers (Carbonnier et al., 2020). Conversely, payroll tax rates that do not vary by labor type can affect the production process. Benzarti and Harju (2020) exploit a discrete change in Finnish payroll taxes according to firms' level of capital depreciation and find that firms facing a higher tax rate substitute away from low-skilled and

[1] In particular, some countries such as Colombia, South Africa or Sweden have implemented payroll tax reductions for young workers. In Finland, a payroll tax subsidy exists for older workers. Reductions of labor cost are sometimes also conditional on the wage level. In Finland, firms can apply for a grant to cover parts of the labor costs when they hire a previously unemployed individual. See Becerra (2017); Ebrahim and Pirttilä (2019); Huttunen et al. (2013); Kangasharju (2007); Saez et al. (2019).

manual labor.

In this paper, I analyze a major cut in payroll taxes in France which aimed at lowering the cost at the minimum wage. The 1995 and 1996 reforms reduced the minimum labor cost by 10%. These payroll tax cuts are targeted at very low wages—which is believed to be the most efficient—and with no other criteria regarding age, firm location, firm size or previous unemployment status. They provide a setting to understand the impacts of a change in the relative cost of different production factors: labor types that differ with respect to their productivity.

Using exhaustive linked employer-employee data as well as the firms' corporate tax forms, I take advantage of the variation of labor cost both at the job level—the labor cost at low wage levels decreases—and at the firm level—firms which already relied on minimum wage labor see their production costs exogenously reduced. Implementing a difference-in-differences strategy at the job level, I find that the tax cut substantially increased employment at the targeted wage levels. This increase stems exclusively from firms which had no, or very few, low-wage workers before the reform. On the other hand, firms which benefit from a cash windfall due to the reform have a very steady number of workers at low wage levels and appear to slightly increase the number of jobs at higher wage levels. A difference-in-differences estimation at the firm level with respect to direct exposure to cash windfalls shows that these firms that benefited from a windfall grow faster than others.

By using a variety of lenses to examine a cut in payroll taxes, this paper sheds new light on the mechanisms through which labor costs affect firm behavior. While the distortion of the wage distribution overall shows a reaction along the relative-cost parameter, I find that this effect is dominated by a cash windfall effect. Firms that already resorted intensely to minimum wage labor see their production costs mechanically reduced. These firms use the additional cash to (i) hire and retain slightly more high-wage workers, (ii) increase firm-level employment overall, and (iii) increase low wages above the tax reduction threshold. I show that these firms are able to grow more precisely because they are less cash constrained.

On the other hand, all of the increase in the number of minimum wage workers thus comes from firms which do not benefit from a cash windfall *ex ante*. To profit from the payroll tax reduction, they retain more minimum wage workers than they used to and tend to trap more low-wage workers below the threshold of the payroll tax cut. In other words, firms with no *ex ante* benefits from the reform adapt their production process and their wage policy to profit from the tax cut, while firms with *ex ante* benefits use the cash windfall to increase their production capacities, in a context of increasing demand.

These results also suggest that targeting cash-constrained firms, rather than groups of workers that are disadvantaged on the labor market, can be relevant in order to increase employment. Targeting minimum-wage workers makes them more employable, but giving cash to small, low-skill, liquidity-constrained firms enables them to increase employment overall. According to the policy goal with respect to unemployment, my findings show that it may be more pertinent to target firms rather than workers.

From a methodological viewpoint, these results also stress that defining an intention to treat at the firm level, based on the extent to which a firm is directly targeted by a policy—as is often done in the literature—can be misleading. Any firm is "treated" by a change in relative labor cost, and not only firms who see their costs directly affected without any change in their inputs. I show that firms that employed too few minimum wage workers to see their costs mechanically reduced by the policy account for all of the change at the low end of the wage distribution. Looking solely at firm-level effects of this direct "treatment" hides the complexity of firm's behavior in reaction to reduced labor costs. This advocates for analyzing the effects of a change of labor costs at various levels, as opposed to solely defining a "treatment" at the firm level.

Related literature. This paper contributes to several strands of literature.

First, the paper provides additional evidence of the firm-level employment effects of payroll tax cuts. I find that the more a firm benefits *ex ante* from the payroll tax cut, the more it increases its employment, relatively to firms with no *ex ante* change in their labor costs. At the same time, firms that see their costs increase due to other concomitant changes in payroll taxes grow more slowly. These results are in line with previous studies of firm-level effects of worker-level payroll tax cuts (Bunel and L'Horty, 2012; Crépon and Desplatz, 2001; Daunfeldt et al., 2019; Saez et al., 2019) and confirm that more generally, a sudden increase, or decrease, of liquidities in the firm affects firm-level employment (Benzarti and Harju, 2020; Ku et al., 2020; Melcangi, 2018). There is little evidence on the duration of these firm-level employment effects. Ku et al. (2020) find that abolishing geographically-defined payroll tax rates in Norway had lasting negative effects in regions which saw their labor costs increase. I find that in the case of the French payroll tax reduction, the employment effects are only temporary, "boosting" employment of cash-receiving firms which appear to anticipate a growth trajectory that they would have otherwise more gradually.

While I complement the evidence on firm-level effects of payroll tax cuts designed at the worker level, my results highlight the importance of diving into individual-level effects. Most papers studying the effects of payroll taxes on employment solely define a "treatment" at the firm level, according to a measure of its exposure to the policy relying in computed *ex ante* total changes in costs (Bunel and L'Horty, 2012; Daunfeldt et al., 2019; Kaunitz and

Egebark, 2017; Goos and Konings, 2007). To the best of my knowledge, few papers combine a firm-level perspective, which makes it possible to measure scale and cash windfall effects, with an individual-level perspective. Combining both levels of analysis enables to measure the potential benefits to the targeted workers in terms of wages and employment prospects. Saez et al. (2019) reveal that the payroll tax cut for young workers in Sweden did not lead to an increase in wages for eligible workers, but did improve their employment rates. Carbonnier et al. (2020) establish that firms with a cash windfall due to a reduction of their corporate income tax, proportional to the amount of the wage bill below 2.5 times the minimum wage, did not increase the number of "eligible" workers or create a wage trap. My paper shows that not only is it crucial to look at the effects at different levels, but that combining these perspectives, by looking at individual effects according to the firm's exposure status, is necessary. In particular, results of this combining approach suggest that targeting low-wage workers and (indirectly) targeting cash-constrained firms leads to effects on employment of a different nature: the former increases low-wage labor, while the latter boosts employment growth. In the case of the French payroll tax cut studied here, these effects coexist, but a different targeting of workers could lead to indirectly target firms which are not those with the greatest need in liquidities.

Heterogeneous effects on job dynamics according to the level of exposure of firms suggest a cash windfall channel. Calculating precise indicators of liquidity constraints and of credit constraints, I find supporting evidence to this mechanism by showing that more cash-constrained firms demonstrate larger employment responses to a cash windfall. By bringing to light distributional effects of a cash windfall, this paper contributes to the literature examining rent-sharing behavior of firms (Carbonnier et al., 2020; Howell and Brown, 2020; Kline et al., 2019; Saez et al., 2019). As in Carbonnier et al. (2020) and Saez et al. (2019), the rent is generated by an exogenous reduction in labor costs. As opposed to the papers in this strand of the literature which look at how the rent is passed on to the average wages of workers, I investigate its effect on the complete distribution of wages. I highlight that firms with a positive cash windfall retain more above-threshold workers and are more prone to increase wages of below-threshold workers to the point that these workers are not entitled to the payroll tax reduction anymore. I find suggestive evidence that they also hire more high-wage workers than they did before.

Finally, a payroll tax cut at the minimum wage is akin to a reduction of the minimum wage as it reduces the minimum labor cost for the employer. My paper therefore addresses the canonical question of the effect of the minimum wage on employment through a novel channel: by exploring the effects of a *reduction* of the minimum labor cost. My results can be interpreted as a positive effect of reducing the minimum labor cost on firm-level employment. Provided that increases and decreases of the minimum wage would have symmetric effects—which is not self-evident, partly because there could be hysteresis effects—this result is in line with other studies finding that increasing the minimum wage decreases firm-level employment (see for instance Harasztosi and Lindner, 2019; Jardim and van Inwegen, 2019), and in contradiction with other studies finding no effect or a positive impact on employment (for instance Hirsch et al., 2015; Card and Krueger, 1994). However, I argue that in this setting, the cash windfall channel could counteract or hide a "pure" minimum wage effect which would be driven by the absence or presence of frictions on the labor market.

Organization of the paper The paper is organized as follows. Section 2 describes the nature and levels of payroll taxes in France in the 1990s and the changes induced by the 1995 and 1996 reforms. Section 3 presents the data. In section 4, I present the job-level employment effects of the payroll tax cut. Firm-level effects of the reform are presented in section 5. Section 6 discusses those results with respect to the previous literature. Section 7 concludes.

2 Institutional context

2.1 Social Security Contributions

In France, payroll taxes represent 18.5% of GDP, which ranks them as the major source of public revenue. Payroll taxes encompass contributions paid by employers and employees which fund specific areas of Social Security—pensions, health, unemployment benefits and child benefits, among others—as well as less contributory sources of revenue taxed on payroll.

Payroll taxes are calculated by applying a rate to gross wages. Labor cost is the sum of gross wages and payroll taxes, and gross wages net of employee Social Security contributions constitute the net wages. The payroll tax rate for an individual worker depends on the worker's hourly gross wages: while some payroll taxes are calculated by applying a unique rate to total wage, other contributions, such as pensions and unemployment Social Security contributions, display a decreasing marginal tax rate.

In 1995, without taking into account pre-existing payroll tax cuts for low-wage workers, the average rate of payroll taxes was 38% for a minimum wage worker and for any non-executive employee with an hourly wage less than 2.15 times the minimum wage. This rate fell to 30% for an employee paid between 2.15 and 6.45 minimum wage, 27% between 6.45 and 8.6 minimum wage, and finally to 21% for wages above. For the second and third wage groups, payroll taxes are higher if the employee is an executive.

2.2 Reforms

Payroll tax cuts targeted at low wages exist since 1993. In their first version, these tax cuts were quite modest: firms were exonerated of paying family Social Security contributions on workers paid between 1 and 1.1 minimum wage (hereafter MW), and paid half the rate of this contribution for workers with wages between 1.1 and 1.2 MW. The reduction amounted to 5.4% of the gross wage for the first group of workers, and 2.7% for the second. This meant that the effective payroll tax rate was equal to 32 and 35%, instead of the legal total rate of 38%. In July 1994, the threshold was extended to 1.3 MW (see Figure 1a). In January 1995, total exoneration of family Social Security contributions was extended to wages up to 1.2 MW (Figure 1b).

The *Juppé* reforms considerably increased these cuts in payroll taxes. The first reform in September 1995 implemented another payroll tax cut, representing 12.8% of gross wages at the minimum wage and continuously decreasing up to 1.2 MW (blue line in Figure 1b). The second reform in October 1996 unified both schedules to create a single un-notched tax cut with a maximum tax cut of 18.2% of gross wages and a slightly higher threshold, equal to 1.33 MW (Figure 1c).

These reforms induced a large drop in low-wage labor cost. The first reform made minimum labor cost drop by an additional 10%. Labor cost for wages equal to 1.1 MW fell by 4.4%. All wages below 1.33 MW, with the exception of the $[1.27, 1.3]$ wage range,[2] benefited from the two *Juppé* reforms.[3]

At the same time, other payroll tax reforms affect labor cost. Employer Social Security contributions for unemployment are slightly reduced for all workers in January 1997, from 4.18% of gross wages to 3.97%. Between 1995 and 1997, the tax rate for employers' Social Security contributions for complementary pensions increased from 3% to 3.75% for all non-executives (on the fraction of their wages below 3 "Social Security Thresholds", which in January 1995 corresponded to wages below 6.45 MW), and from 10% to 11.25% for executives on the fraction of their wages between 1 and 8 "Social Security Thresholds"—i.e., between 2.15 to 17.2 MW.

[2] As you can see from Figure 1c, smoothing the payroll tax cut's schedule led to a less favorable reduction of labor cost for this small range of wages.

[3] The real effect of the *Juppé* reforms on labor cost for those wage levels is however smaller due to two hikes in the minimum wage value in July 1995 and July 1997: these years, the minimum wage increase, which was often partly discretionary in this period, was higher than the raises in other years of the 1993-1999 period. These increases were yet well below the annual raises in the previous years.

3 Data

I present the administrative data that I use, and then describe the datasets constructed for the different analyses.

3.1 Administrative data sources

I combine administrative data,[4] with varying levels of observation, coverage and time horizons.

The primary source is the exhaustive linked employer-employee data for French firms of the private sector (*DADS Postes*). This data stems from the quarterly or annual declarations that firms do for the payment of Social Security contributions. These forms contain precise information on each worker in the firm: annual wages, number of hours worked and job contract period, but also occupation, executive/non-executive status, gender, age and city of residence. Firms also fill in the number of employees and the firm identifier. Each annual *DADS Postes* database for year t contains the job-level information for the year N - 1, allowing a two-year panel at the job level. The data starts in 1993, but some variables such as the number of hours worked are poorly filled in in the first years. I use this data primarily over the period 1995-1999.

To gain more hindsight on the evolution of the distribution of jobs, I use the panel-, sample-version of the *DADS Postes*, called *DADS Panel*, which traces back to 1967 and benefits from better data quality checks than the early years of *DADS Postes*. It contains information on a random sample of 1/25th of private sector workers. However, the absence or poor quality of the reported number of hours worked before 1995 imposes to restrict the sample to workers identified as working full time, in order to be able to compute their hourly wage.

Forms filled in by firms for the corporate income taxes contain the complete balance sheets of firms. I use this firm-level data specifically to retrieve a full-year equivalent measure of the workforce and to compute indicators of liquidity constraints and access to credit.[5] This data exists since 1994.

[4] Access to this confidential data was granted by the *Comité du secret statistique* by decisions ME27 of October 2, 2013, ME56 of June 2, 2014 and ME91 of June 6, 2015.

[5] To measure the size of the workforce, I also calculate the number of full-time equivalent workers in the firm using the information on the number of hours worked and contract duration of each job in the DADS. In the early years of the *DADS Postes* however, this measure displays poor consistency. I use the self-reported variable carried by the fiscal data instead.

3.2 Simulation, restrictions and samples

Firm-level data. I construct firm-level data for the firm-level analysis. This data is also matched with the job- and worker-level data (see below) to ensure similar scopes of analysis.

First, I calculate the gross wage for each observation in the *DADS Postes*, for each year between 1994 and 1999. The employer-employee data contains annual net wages, as well as a so-called "gross" remuneration which does not correspond to the posted wage. I use the *TAXIPP* microsimulation model[6] to derive gross wages from the net wages, applying the employee Social Security contributions' rates and other taxes. I also calculate all components of payroll taxes and obtain the labor cost for each work contract.

Then, to get a measure of how the payroll tax reforms affect workers and firms differently, I calculate for the 1995 data, for each job, what the labor cost would have been had the 1997 payroll tax schedule applied. This counterfactual labor cost is lower than the actual labor cost for workers targeted by the payroll tax cuts, and it is higher for high-wage workers with increased complementary pension Social Security contributions .

I aggregate this comprehensive employer-employee data at the firm level, after discarding observations corresponding to hourly wages below 0.8 times the minimum wage as well as jobs which do not fall within the main Social Security scheme (*régime général*).

This data is merged on the firm identifier with the fiscal data. I discard firms in the financial, agricultural or energy production sectors, and firms that are not observed in both data sources.

In the unbalanced panel, I keep firms which are at least in the 1995 annual data. This is necessary to have a measure of exposure to payroll tax reductions for all firms. In a balanced panel, I keep firms observed each year of the 1994-1999 period.

In the largest sample, I keep firms with more than 2 full-year equivalent workers in 1995. I use this large sample to analyze the changes in the aggregate wage distribution. For the firm-level analysis, I restrict to firms with more than 10 full-year equivalent workers in 1995:

[6] These calculations are made using the microsimulation model TAXIPP which enables precise calculations of Social Security contributions based on the information in the data, on all relevant parameters of the legislation which are detailed in the *Tax and benefit tables*, and on the functions associating job and firm characteristics to the amounts of Social Security contributions (and payroll taxes in particular) using the relevant parameters. Useful information in the data for Social Security contributions calculations are for the most part wages, number of hours, status (executive or non-executive), number of employees in the firm and location (Alsace-Moselle or rest of France). The microsimulation model calculates all Social Security contributions relevant for private firms, i.e., unemployment, pensions (including supplementary pensions), family, health but also all other contributions such as *CSG*, *versement transport* or contribution to the housing funds, as well as eventual reductions in payroll taxes based on wage levels (*allègements généraux*) or number of hours worked (*Robien*, *Aubry I*, *Aubry II*).

very small firms display more sporadic employment behavior. I use the large sample, as well as the unbalanced sample, for robustness checks. The size of the samples are given in Table 1.

Job-level and worker-level data. I use data at the job level to analyze the changes in the distribution of wages. In the exhaustive data linking jobs and firms, the *DADS Postes*, the two-year panel enables for each year t to identify whether a job is a new job (observed in t but not in t - 1), a destroyed job (observed in N - 1 but not in t) or a continuing job (observed both years).[7]

To compensate for the lack of historical perspective, I alternatively use the *DADS Panel*. The data does not contain information on the number of hours worked, so I restrict the sample to workers identified as full-time workers to adequately compute hourly gross wages as in the comprehensive data. I do not decompose the total number of jobs per bin between new, old and continuing jobs, because restricting to full-time jobs would bias these measures and because the data features an odd hike of observations in 1994 and 1995 in the private sector, which is not reflected in the exhaustive data. I restrict the period to 1992-1999, using the data from 1991 to characterize jobs in 1992.[8]

I match each of these databases—the exhaustive 2-year panel and the sample long panel—with the large balanced firm-level data. Other than guaranteeing a comparable scope for the firm-level and job-level analyses, this allows to study whether the wage distribution evolves differently in groups of firms with different characteristics, as observed in baseline year 1995. Moreover, merging with the balanced sample of firms rules out the possibility for the evolution of the number of jobs to be driven by the attrition of firms observed in 1995.

4 Individual-level employment effects

In this section, I test whether the large increases of payroll tax cuts for wages around the minimum wage distorted the distribution of wages. I explore whether distortions stem from changes in hires, dismissals or retention of jobs. I present the empirical strategy in section 4.1, and the results in section 4.2. I show that the number of jobs at targeted low-wage levels increase, while remaining very stable at higher wages. Firms with different workforce

[7] Note that new jobs are not necessarily hires and destroyed jobs are not always separations: an employee could continue to work in a firm but with a new job contract, such that in the data the previous job would be identified as a "destroyed job", and the job with the new contract would be identified as a "new job". The *DADS Postes* data in the 1990s does not contain a worker identifier which would enable to measure real hires and separations.

[8] The data for 1990 is no longer available, which limits the period of analysis.

composition before the reform could react differently in terms of hires, dismissals and retention, at different wage levels; hence, I test whether the wage distribution is uniformly distorted in firms with different *ex ante* levels of low-pay workers. Defining a measure of firms' exposure to the changes in labor costs, I find that the wage distribution evolved very differently in firms which benefited *ex ante* from a cash windfall thanks to the reform, and in those which did not.

4.1 Empirical strategy

I conduct an analysis at the job level to analyze the effects of the 1995-1997 reforms on the distribution of wages. In particular, I test whether, and to what extent, these changes in labor costs distorted the distribution of wages in favor of low wages. I look at whether the changes in the wages distribution stems from changes in the retention of already existing jobs, or in net created jobs. I further investigate whether continuing jobs display large wage increases.

Notations. I denote $N_{b,t}$, $H_{b,t}$, $D_{b,t}$, $C_{b,t}$ and $S_{b,t}$ respectively the number of jobs, hires, destroyed jobs, net created jobs and continuing jobs (or stayers), in wage bin b in year t. "Hires" are jobs observed in year t which did not exist in *t* - 1; "destroyed jobs" on the contrary are jobs observed in *t* - 1 but not in *t*; and "net created jobs" is the difference between those two aggregates. "Continuing jobs" or "stayers" are jobs that are observed in my data for two consecutive years. For destroyed jobs, the wage bin is the one observed in *t* - 1; for all other jobs, including stayers, the wage bin is that observed in t. I further decompose between stayers who move towards a higher wage bin (of width equal to 0.1 MW) and those that don't.

I consider the ratio $\frac{X_{b,t}}{N_{b,95}}$ for $X \in \{N, H, D, C, S\}$: I normalize raw numbers by the number of jobs in 1995 in the wage bin, for every bin b and year t.

Simple differences. In the baseline specification, I regress $\frac{X_{b,t}}{N_{b,95}}$, $X \in \{N, H, D, C, S\}$ on wage bin dummies and their interaction with year dummies:

$$\frac{X_{b,t}}{N_{b,95}} = \sum_{i} \gamma_i \ B_{i,b} + \sum_{k \neq 1995} \ \sum_{i} \beta_{i,k} \ B_{i,b} \ Year_{k,t} + \epsilon \tag{1}$$

where $B_{i,b} = 1$ if b is bin i, and $Year_{k,t} = 1$ if year t is equal to k. Wages range from 1 to 5 MW. The first wage bin comprises wages between 1 and 1.3 MW (treated wages), while other wage bins span 0.4 MW each. For $X \in \{N, H, C, S\}$, the wage bin considered is that of the wage observed in t. For $X = D$, the wage bin is naturally that of the wage observed in t - 1. For $X = S$, I present results where the wage bin is defined in t - 1 when looking specifically at stayers with an upward wage mobility. This allows to study both the destination wages and the past wages of stayers. The period covered either starts in 1992 (using the panel version of the DADS, for variable N only) or in 1995. The estimated OLS $\hat{\beta}_{i,k}$ is the simple difference in the normalized number of jobs X (for example, hires) in wage bin i in year k relatively to 1995.

Difference in differences. I complement this simple-difference approach by a difference-in-differences strategy, whereby I define as "treated" the wages between 1 and 1.1 MW ($Treat_b$ = 1), and as "controls" wages between 1.3 and 1.5 MW. Alternatively, I consider as treated all wages below 1.3 MW. With $Year_{k,t}$ a dummy equal to 1 if year t is equal to k, I estimate:

$$\frac{N_{b,t}}{N_{b,95}} = \alpha + \gamma \, Treat_b + \sum_{k \neq 1995} \delta_k \, Year_{k,t} + \sum_{k \neq 1995} \beta_k \, Year_{k,t} \, Treat_b + \epsilon \tag{2}$$

OLS-estimated $\hat{\beta}_k$ gives the differential evolution of the number of jobs in treated wages between 1995 and t, compared to the control group. The β estimates for the years before 1995 provide a test of the parallel trends assumption. Note that the difference-in-differences strategy only applies for $X = N$, because the panel data required to have pre-1995 data imperfectly measures hires, job destruction and continuing jobs.

This strategy is similar to Becerra (2017), which also estimates the effect of a targeted payroll tax cut on a normalized count measure of jobs. I introduce multiple pre- and post-reform periods.

Measuring firms' exposure to the policy. While payroll tax cuts are defined with respect to the level of wages, and therefore appear as a "treatment" at the individual level, they nonetheless affect employers differently according to the composition of their workforce. All employers are affected by the policy, in that the *Juppé* reforms modify the relative cost of labor. However, these reforms also have a direct, mechanical effect on firms that already employed minimum wage workers: the additional payroll tax reductions generate an exogenous cash windfall, and the size of this additional liquidity depends on the proportion low-wage workers represent in the firm and their exact wages.

For each firm in the data aggregated at the firm level, I calculate the *ex ante* change in labor cost induced by the changes in the payroll tax schedules between 1995 and 1997. This represents the change in labor cost due to exogenous change in payroll taxes, had the labor composition of the firm remained constant between 1995 and 1997. Denoting LC the labor cost for a firm, which depends roughly on the firm and workforce characteristics W (wage distribution, executive or non-executive status, number of employees, *etc.*) and on the payroll tax schedule Sch, I define the *ex ante* change in labor cost due to the 1995-1997 reforms as:

$$\Delta LC = \frac{LC(W_{1995},\, Sch_{\,\mathbf{1997}}) \;-\; LC(W_{1995},\, Sch_{\,1995})}{LC(W_{1995},\, Sch_{\,1995})} \tag{3}$$

Based on this continuous measure of direct exposure to the changes in payroll taxes, I group firms into 5 groups of equal total number of workers in 1995, hereafter referred to as weighted quintiles and denoted Q1 to Q5. Firms in the first quintile have the lowest, or most negative, *ex ante* variation of labor cost: the changes in the payroll tax schedule between 1995 and 1997 strongly decrease their labor cost. Conversely, firms in the top quintile are those most affected by the change in payroll tax rates which increase labor cost *ex ante*.

Figure 2a plots the distribution of *ex ante* change in labor costs. Labor cost can mechanically decrease in proportions as high as 8%.[9] Most firms' variation of labor costs take negative values, but these firms are also smaller on average: when grouping firms into five groups of equal total full-year employment in 1995, the group with the biggest *ex ante* decrease in labor costs (Q1) is composed of 40% of all firms in the balanced sample. Firms in Q1 have an average *ex ante* decrease of labor costs of about 2%. *Ex ante* increases in labor costs are comparatively small: for each weighted quintile with positive variation of labor cost, the average value is below 0.5% (Figure 2b).

4.2 Results

Impact on total employment. Figure 3 plots the $\hat{\beta}$ coefficients of the simple differences regression for the total number of jobs, for a small selection of wage bins: wages between 1 and 1.3 MW ("treated" wages) and wages just above, between 1.3 and 1.7 MW, using the sampled data on the 1992-1999 period. The number of jobs in both wage bins evolve quite similarly before 1995: it slightly increases between 1994 and 1995 after having stagnated the two years before. However, their paths diverge after 1995: the number of jobs increases more at treated wages than above. The number of jobs with wages below 1.3 MW increase

[9] Note that a firm with only minimum wage workers would see its labor costs reduced *ex ante* by 9.5%. This is easily calculated, using the fact that the denominator, i.e., the labor cost with 1995 payroll taxes, is 1.326 (1.38-0.054) times the total gross wages, and that the numerator is equal to -0.128 total gross wages (difference in payroll tax cuts at the minimum wage as expressed as a percentage of gross wages).

by 20%[10] between 1995 and 1998. By contrast, the number of jobs in the 1.3-1.7 wage bin stagnates until 1997 and very slightly increases afterwards.

The differential post-1995 trends for treated wage bins with respect to other wage bins is confirmed by the whole set of the simple differences coefficients (Figure 4a). While the total number of jobs significantly increases for wages between 1 and 1.3 MW as early as 1996, the distribution of jobs above 1.3 MW is essentially undisrupted during the whole 1992-1999 period. The post-1995 coefficients obtained using the exhaustive—but period-constrained—version of the data are very similar (Figure 5).

Taking into account the small difference in trends in the numbers of jobs, the difference-in-differences estimates do confirm an increased bunching at treated wage levels. The number of jobs paid between 1 and 1.1 MW increases by 10 percentage points more than the number of jobs at higher wage levels as early as 1996, and by 10 additional percentage points in 1998 (Figure 4b). Considering as "treated" all wages below 1.3 MW, the pre-treatment coefficients are smaller in absolute values, and while the point estimates in the post-1995 period are slightly lower, the narrative remains similar (Figure A1).

This increased bunching could stem from an increase in net job creation around the minimum wage and/or from higher retention of jobs at these wages. The following paragraphs investigate the sources of this evolution.

Impact on hires, job destruction and job retention. To decompose jobs between hires, destroyed jobs and continuing jobs, I use the exhaustive version of the employer-employee data. This is done for two reasons, detailed in section 3: the restriction to full-time jobs which distorts the measure of new, destroyed and continuing jobs (for instance, a job identified as "new" in year t could have been part-time in N - 1); and some odd features of the sampled data which are not mimicked by the exhaustive data. The fact that both data feature very similar results when looking at the evolution of the number of jobs post-1995 (see Figure 5) gives confidence in assuming that in the exhaustive data as well, for all wage bins, the number of jobs would have remained very similar before 1995, had the data allowed such pre-treatment analysis.

The simple difference estimates for net job creation in the 1995-1999 period show that the added bunching at the minimum wage does not stem from net job creation (Figure 6a). While "hires" increase for the treated range of wages, job destruction increases roughly in

[10] Coefficients can be interpreted simply for the regressions of the total number of jobs, because the normalized number of jobs in bin b in year t is $\frac{N_{b,t}}{N_{b,95}}$. Therefore, by construction, in equation (1), $\hat{\alpha} + \hat{\gamma}_b$ is equal to 1 for every bin b, and $\hat{\beta}_{b,t}$ is interpreted as an increment with respect to this value, so as a multiple of the total number of jobs in bin b in 1995.

the same proportion (Figures 6c and 6d). On the contrary, the added bunching at the minimum wage appears to originate from an increase in job retention in the treated wages range (Figure 6b). The differential increase in the 1-1.3 MW wage bin between 1995 and 1999 is equal to 0.1, in units of the normalized measure of continuing jobs.

This result is consistent with Kramarz and Philippon (2001) which analyzes worker-level employment responses to increases and decreases in minimum labor cost in the 1990s in France. For the 1992-1999 period of minimum labor cost decreases, the authors identify workers paid in year t between the new and the $t-1$ minimum labor cost, i.e., workers who, at $t-1$, would have represented a sub-minimum labor cost for this year's legal standards, and compare their probabilities of coming from non-employment with those of workers paid at wages just above. The authors find that workers paid at the minimum wage in year t do not come more often from non-employment. This implies that any differential change in total employment at minimum wage levels with respect to slightly higher wages (which the authors do not investigate) should come from differential changes in job retention, which is indeed what I find.

I further decompose job retention between continuing jobs with a significant upward mobility, and other continuing jobs. I define having a significant upward wage mobility as going from one, 0.1-wide wage bin to one that is higher up the distribution. I am interested in their "arrival" wage bin: the wage bin in which they are observed in year N, after having been in a lower wage bin in year $N-1$. I find that below the 1.3 minimum wage threshold, the number of continuing jobs with no such upward wage mobility increases during the period (Figure 7a). At higher wage levels however, this is not the case. This could be interpreted as a wage trap: workers paid less than 1.3 MW are more likely to be "stuck" at wage levels that give right to a payroll tax reduction for their employer, in post-treatment years as compared to 1995.

Nonetheless, the probability of having an upward wage mobility also increases when this upward mobility translates into achieving a wage (still) below the 1.3 MW threshold (Figure 7c). Logically, looking at the N - 1 wage bin of jobs with an upward wage mobility in year t, I find that the number of jobs with N - 1 wages below 1.3 MW increases (Figure 7b). In other words, lowering the labor cost below 1.3 MW also resulted in firms being more prone to increasing the wages of minimum wage workers. Their wage can move upwards to another wage bin below 1.3 MW, as stated above, keeping workers "trapped" below the tax cut threshold, but they can also move higher up the distribution: the number of upward-moving jobs paid between 1.3 and 1.7 MW also increases throughout the period (Figure 7c). These results confirm the worker-level analysis conducted by Aeberhardt and Sraer (2009) which showcases a higher wage mobility for workers paid below the 1.3 MW compared to workers paid above. Additionally, these results explain the positive coefficients in the 1.3-1.7 wage bin for the most recent years, when looking at the evolution of the number of jobs (Figure 5) and of stayers in particular (Figure 6b).

Heterogeneity with respect to firms' exposure to the policy. Results so far show that, on the large population of employees of firms in the private sector, the number of jobs opening entitlement to payroll tax reductions largely increases, compared to the number of jobs at higher wages which remains roughly constant. I explore whether this increased bunching at the minimum wage is observed in all firms, or differs according to the direct, or indirect, exposure of firms to the change in payroll tax cuts.

For better statistical power, the heterogeneity analysis for the simple differences specification—including the regressions with the total number of jobs as the dependent variable—is based on the exhaustive employer-employee data, meaning that the regressions are run on the 1995-1999 period, thereby preventing a formal test of pre-trends. However, as for the analysis ran on the whole sample, the sign and order of magnitude of the post-1995 coefficients for the total number of jobs per bin are in line with those obtained on the panel, sample, long-run version of the data.

The results of the simple differences regression on the total number of jobs per bin, estimated separately for each of the five weighted quintiles of firms, are striking. All sub-samples display an increased bunching at the minimum wage *except* for the sample of jobs in firms in the first quintile, i.e., firms with negative *ex ante* variation of labor costs (Figures 8a to 8e). The distribution of wages for firms in Q1 is very stable at low wage levels, with signs of a slight increase of the number of jobs at higher wages. In all other firm groups, the number of low-wage jobs expands and the number of jobs at higher wage levels remains very stable. This additional bunching at the minimum wage increases with the level of *ex ante* change in labor costs. Moreover, the bigger the change in firms' labor costs, the earlier the bunching increases. In firms of Q5, the number of jobs paid between 1 and 1.3 MW increased by around 40% between 1995 and 1996. By 1999, it had increased by 130%.

This pattern for the total number of jobs emerges from three facts. First, in Q1, the stability of the number of low-wage jobs is not due to the compensation of hires by job destruction, or by a decrease in the number of continuing jobs balanced by an increase in net created jobs: neither hires nor job destruction or retention of workers change in these firms (Figure 9). In firms of Q2 and Q3, hires *and* job destruction slightly increase but in total, there is no additional net created jobs. The additional bunching at low wages stems exclusively from an increase of the number of continuing jobs (Figures 10 and 11). In firms of Q4 and Q5, however, all flows contribute to the increase in the number of jobs below 1.3 MW: new jobs increase more than the destruction of jobs, and retention increases (Figures 12 and 13).

The pattern of upwards wage mobility also differs. In Q1, the number of continuing jobs with upwards wage mobility with a "destination" wage above the 1.3 MW threshold increases as early as 1996, while it remains very stable in all other groups of firms (Figure 14). Conversely, in firms without a cash windfall, and especially in Q4 and Q5, the number of continuing jobs with increasing wages with a destination wage *below* the 1.3 MW threshold increases. Overall, firms in Q1 appear to use the cash windfall partly to increase wages of workers, while other firms seem to increase wages only if the destination wages still give right to a reduction in payroll taxes.

Overall, firms with a cash windfall appear to react very differently to the change in relative labor cost than firms with no change, or a positive change, in labor costs *ex ante*. In total, they do not hire, destroy jobs or retain low-wage workers differently in the 1996-1999 period than they did in 1995. However, there appears to be slightly more hires and continuing jobs at higher wages, although estimated coefficients are not always significantly different from zero. On the other hand, in firms which did not benefit from a cash windfall, the change in the distribution of jobs seems to reflect the decrease in the relative labor cost of low-wage workers, with increased retention of these workers, combined with a wage mobility trap, and, for firms in Q4 and Q5, more net job creation.

Heterogeneity with respect to job characteristics. I investigate whether these dynamics benefit certain types of jobs relatively to others, by replicating the analysis on subsamples of jobs. Figures A3, A4 and A5 present the 1998 β coefficients of equation 1 estimated on these subsamples.

I find that new hires increase at the minimum wage more among part-time and part-year jobs, than among full-time or full-year jobs (Figure A3b). This is coherent with the large increase of part-time jobs at this period in France. This also suggests that while choosing to hire more low-wage labor, firms resist to providing them the attributes of long-term jobs, preferring more flexible work contracts.

The 1990s are also years of expansion of temporary work. Temporary work contracts bind a worker with a temporary work agency, which is the worker's legal employer. The agency then "rents" its workforce to other firms to perform short-term tasks, such as construction work, forklift jobs or one-day inventory counts in supermarkets. I find that the number of temporary work contracts largely increased below the payroll tax cut threshold (Figure A4, right column).

Finally, the services industry appears as the main provider of additional low-wage jobs (Figure A5). Only manufacturing and construction firms which are likely to have a high share of high-wage workers (groups Q4 and Q5) increase their number of low-wage jobs.

5 Firm-level effects

Results at the job level show that while firms as a whole appear to be taking advantage of the change in the relative cost of labor, the wage distribution of jobs in firms directly benefiting from the reform seems roughly unchanged. While the effect on the number of hires and on job retention at the minimum wage level is strikingly null, there seems to be a positive, yet not statistically significant, effect on hires and continuing jobs at higher wage levels, which indicates that benefiting from a cash windfall could participate in reshaping the firms' production function. At the firm level, benefiting from an exogenous reduction in production costs could matter for growth if firms are cash constrained. In this section, I explore whether the tax windfall affects firm-level overall employment decisions—in other words, whether firms use the additional cash to grow.

5.1 Empirical strategy

Based on the definition of firm-level direct exposure to changes in labor costs using *ex ante* variation of labor costs (Section 4.1), I implement a difference-in-difference approach with multiple groups and multiple time periods. I regress the growth rate of a firm-level outcome with respect to 1995 levels on an interaction of time and exposure group dummies, taking as the time reference the pre-treatment year (1995), and as the reference group the second weighted quintile, Q2, which is the group of firms having close to zero *ex ante* variation in labor cost (Figure 2b). This discrete measure of firm exposure allows to explore non-linearities of the treatment effect—as in Bunel and L'Horty (2012) or Saez et al. (2019) for instance—and in particular to distinguish firms with a negative change in labor costs from firms with a small increase *ex ante*.

Denoting $Y_{i,t}$ the outcome for firm i in year t, $Q_{j,i}$ a dummy equal to 1 if firm i belongs to quintile j of *ex ante* variation of labor cost, $Year_{k,t}$ a dummy equal to 1 if year t = k, the baseline specification is:

$$\Delta Y_{i,t} = \alpha_i + \gamma_t + \sum_{\substack{j=1 \\ j\neq 2}}^{5} \sum_{\substack{k=1994 \\ k\neq 1995}}^{1999} \beta_{j,k}\, Q_{j,i}\, Year_{k,t} + \delta_{s,t} + \epsilon_{i,t} \tag{4}$$

$\Delta Y_{i,t}$ is the growth rate of $Y_{i,t}$ relatively to the year 1995. α and γ are the firm and year fixed effects. δ is a sector $\times$ year fixed effect to account for potentially different economic trends. If well identified, the coefficient $\hat{\beta}_{j,k}$ is the effect of being in exposure quintile Qj—compared to being in Q2—in year k on the 1995-to-k growth rate of the outcome. In the reduced form estimations, $Y_{i,t}$ is full-year employment. As a first stage analysis, I estimate (4) with $Y_{i,t}$ the share of "social charges" in the firm's total labor costs, as observed in the firms' profit and loss accounts. Social charges aggregate payroll taxes and some other costs attributed to labor inputs, such as work council fees. If firms in Q1 (resp. Q3 to Q5) have indeed a stronger (resp. weaker) decrease of the share of social charges within their labor costs, then *ex ante* variation of labor costs is a good proxy for firms' exposure to the reform

Using treatment groups, rather than a continuous measure of treatment (unlike Crépon and Desplatz, 2001), allows non-linear effects of changes in the labor cost. In particular, it allows the firm-level effects of an *ex ante* increase in labor costs to be different from that of an *ex ante* reduction.[11]

The dummies for each year of the 1996-1999 period enables to distinguish short-term and long-term effects. Coefficients associated to the year 1994 provide a direct test of the parallel trends assumption for the 1994-1995 period.

Alternative specifications. To account for possible different growth trends for firms with different initial workforce sizes, especially as firms in the first deciles are also smaller, I control for an interaction of initial size group (ranking firms into size quintiles according to their size in 1995) and year. I also estimate equation (4) on the unbalanced sample, and on a larger sample of firms with more than 2 full-year equivalent employees in 1995.

5.2 Results

First stage. Figure 15a presents the estimated β coefficients of equation (4) with the growth rate of the share of social charges in total labor costs as the dependent variable. The $\beta_{j,k}$ coefficient interprets as the additional cumulative growth, between 1995 and year k, for a firm in Qj relatively to Q2. I plot those estimated coefficients around the (unconditional) average cumulative growth for firms in Q2. The share of social charges in total labor costs decreases more for firms with *ex ante* decreases of labor costs (Q1) than in firms with little *ex ante* change in their labor costs (Q2). By contrast, this ratio grows slightly for firms with positive *ex ante* variation of labor costs (Q3–Q5). In other words, *ex ante* variation in labor

[11] Note that the different effects of increases *vs.* decreases of labor costs will not be distinguishable from differential effects of labor cost changes on low-wage *vs.* high-wage workers.

costs due to the change in payroll tax schedules is a predictor of variation in the burden of social charges. Firms that mechanically benefit from payroll tax cuts benefit indeed more from these reductions than other firms, meaning that the workforce composition does not change radically. Conversely, even though the distribution of wages is on agregate slightly shifted towards low wages in groups Q3 to Q5 (see Section 4), this shift is not large enough to make the share of social charges drop between 1995 and 1997 in these groups.

Impact of the cash windfall on employment. Figure 15b shows a clear correlation between "exposure" quintiles and the average growth rate of employment in the first two years after the reform. Between 1995 and 1997, full-year equivalent employment increases more in firms which directly benefited from payroll tax cuts, and less in firms with mechanical increase of labor costs due to the uncapping of Social Security contributions at very high wages. While the unconditional average growth of employment for firms in Q2 between 1995 and 1997 is around 5%, the (conditional) average growth among firms in Q1 is more than 4 percentage points higher than (conditional) average growth in Q2. Conversely, by 1997, firms in Q3, Q4 and Q5 have grown less than firms in Q2, conditionally on firm, year and year-sector fixed effects. These differential evolutions contrast with the very similar growth trends between 1994 and 1995. Reductions and increases in payroll taxes seem to matter for firm-level employment growth.

However, this effect appears temporary as employment growth rates roughly converge in 1998: between 1995 and 1998, firms in all groups experience *overall* very similar cumulative growth rates, when controlling for firm, year and sector-year fixed effects. This could be driven partly by the fact that the difference in *ex ante* labor costs evolution between quintiles diminishes by 1998, as payroll tax cuts, which benefit more to firms in Q1, are slightly reduced (Figure 1d). However, the gap between 1995-1997 and 1995-1998 growth rates still appears too big to be only explained by this slight change in *ex ante* variation of labor costs: in absolute value, the first stage 1998 coefficient for Q1 is much smaller than the 1997 coefficient, but as expected, it is not null, meaning that the change in payroll tax cuts in 1998 is not major and that there is no radical change in workforce composition in Q1 between 1997 and 1998. Overall, the cash windfall induced by the payroll tax cut appears to have *boosted* firm growth but did not increase it, relatively to other firms, in the long term.

There could be other possible explanations of the temporary nature of the effect on employment. It could be that economic growth in 1998 and 1999 is less beneficial to firms in Q1 than to others, in a way that the year-sector fixed effects do not capture. Another mechanism could be that the cash windfall enables firms to *anticipate* growth, rather than trigger it in the long term: this could be the case if firms delayed strategic choices, such as increasing firm size or hiring a manager, due to insufficient liquidities. I test this mechanism in section 5.4.

Finally, the short-term effect of *ex ante* change in labor costs on firm-level employment may seem at odds with the job-level results. Analyzing the effects of the reform on the wage distribution revealed no hike in low-wage jobs in firms with a strong cash windfall, contrasting with evidence of such a hike in other groups of firms. However, employment at higher wage levels does appear to increase in firms in Q1 (Figure 8a). Moreover, the firm-level employment outcome in Figure 15b is calculated as the growth rate of full-year equivalent employment in the firm. As firms in Q1 are also smaller on average than firms in Q2, their growth rates are higher for a similar absolute increase in labor.

5.3 Robustness of the employment effect

Accounting for differences in initial firm size. Differing initial firm size could be a potential confounder for the reduced form estimation. Indeed, firms in Q1 are on average smaller than firms in other groups, and it has been shown that smaller firms display higher employment growth (Moscarini and Postel-Vinay, 2012).[12]

To separate any effect of initial firm size on firm growth, I add size–year fixed effects in equation (4), defining "size" as the firm size quintile group in 1995. I find that the higher employment growth for Q1 firms with respect to Q2 is about as strong when accounting for differential firm growth according to initial firm size (Figure A7c): controlling for firm, year, year–sector and initial size–year fixed effects, firms in Q1 grow by 3.6 percentage points more by 1997 than firms in Q1, in terms of full-year employment, compared to a coefficient of 4.4 obtained with the baseline specification (4).

Using a larger sample, including smaller firms. I also test whether results are robust to using the larger sample comprised of all firms of at least 2 full-year equivalent employment in 1995. This is the wide sample used to analyze the effects on the wage distribution in Section 4.

Figure A7a shows that the pattern for 1995-1997 growth is similar on this larger sample, to the difference that the unconditionnal average for Q2 is higher on this sample (the 1995-1997 average growth in Q2 firms in 9%, compared to 5% in the baseline sample). The estimated coefficient $\beta_{2,1997}$ is exactly the same (4.4 percentage points) in both samples, such that in

[12]Recent papers have shown that this effect of firm size on cyclical job flows is in fact entirely driven by an effect of firm age (Haltiwanger et al., 2013; Fort et al., 2013; Colciago et al., 2019).

relative terms, the effect of being in Q1 relatively to being in Q2 on the short-term employment growth is smaller when we include very small firms in the sample. However, the short-term employment effect practically disappears when we control for firms' initial size (Figure A7b). This suggests that initial size drives the results when considering very small firms,[13] while this effect is of minor importance when restricting to more mature firms.

Very small firms also display specific behavior in 1998: on the large sample, the coefficient for Q1 in 1998 is negative, and even more so when accounting for firms' initial size (Figures A7a and A7b). This reflects an actual drop in average employment between 1997 and 1998 for firms in Q1. As this drop is not observed in the baseline sample, this suggests that very small firms, with an initial size smaller than 10 full-year equivalent employment in 1995, are affected differently by the conjoncture. Moreover, including very small firms in the sample brings noisier, more sporadic employment growth patterns. This is an expected effect, as for small firms, a small change in the number of employees in absolute values translates into a large percentage increase or decrease in employment.

Unbalanced panel of firms. Facing *ex ante* increases or decreases of payroll taxes may correlate with the probability of a firm to exit the market. Differential patterns of firm exits may in turn inflate or deflate employment effects of exposure to changes in labor costs.

Figure A6b shows that when controlling for initial firm size, firms from Q1 face slightly higher risks of attrition. This is true for the whole period, including before 1995, which suggests that it is at least partly a feature of this type of firms rather than an effect of the reform. This does not allow to draw a causal interpretation from the post-1995 attrition estimates.

In any event, this differential attrition warns against the robustness of our employment growth estimates on the balanced panel, as surviving firms are likely to be selected and survival could be correlated to employment growth. I find, however, that the coefficients for Q1 in 1996 and 1997 estimated on the unbalanced sample are very similar to the ones obtained for the baseline specification and sample (Figure A6c).

Even though selected attrition is not a confounder for the policy exposure measure, taking into account firm exits would give a more complete picture of employment dynamics. When a firm ceases to exits, all its jobs are *de facto* destroyed. In Figure A6d, I plot the coefficients estimated after attributing a -100% growth rate in year k to any firm absent of the data in year k. As expected, the unconditional average growth rate of employment relatively

[13]Note that very small firms represent a considerable share of the large sample, as the number of firms in the sample varies from 91 855 to 336 428 when we include firms with 2 to 10 full-year equivalent employment in 1995 (Table 1).

to 1995 is in an inverted U shape, with a peak in 1995, because any firm in the sample is necessarily observed in 1995 and employment growth would need to be extremely dynamic to compensate for attrition.[14] Estimating equation (4) for this corrected employment growth outcome yields positive estimates for Q1: in 1996 and 1997, corrected employment growth is around 3 percentage points less negative for firms in Q1 relatively to Q2. Negative or insignificant estimates for years 1998 and 1999 come as a result of both increased attrition for Q1 firms compared to other groups, and convergence of employment growth trends in 1998 (Figure A6c).

5.4 Testing the cash constraints hypothesis

The temporary nature of the effect of a cash windfall on employment growth (Figure 15b) suggests that liquidity constraints may be a mechanism by which the cash windfall translates into higher employment growth. Indeed, if firms benefiting from a cash windfall face liquidity constraints, the additional cash provided by the change in labor costs could be used to implement strategic decisions that they would not have possibly made without it, by lack of (access to) cash.

In the years 1996 and 1997, firms in the manufacturing sector face an increase in demand, driven by exports. This growth propagated to other industries.[15] The baseline specification (4) includes controls at the sector–year level and therefore accounts for different exposure to the demand shock. However, a possible effect of the cash windfall is that firms are able to adjust more brutally their inputs to the increasing demand, while they would have been constrained to increase gradually their employment growth otherwise. The underlying model is that in a world without frictions on the capital market, input growth would react more abruptly to changes in the demand for outputs.

Building cash constraints indicators. To test whether firms more or less constrained in their access to cash exhibit different growth patterns, I build several proxies of cash constraints using the detailed firm corporate income tax data. Usual cash constraints indicators can be divided into two categories: proxies of access to credit and proxies of liquidity detention.

If there is no friction in a firm's access to credit, the firm would get immediate access to the cash it needs for profitable projects. This is not the case if information imperfection makes access to credit dependent on the firm's financial history and past performance indicators.

[14] Figure A6b shows that by 1997, 10% of firms observed in 1995 are no longer in the data.
[15] *Insee conjoncture*, June 1998.

Leverage is the ratio of debt to assets; if it is high, it can indicate difficulties for the firm to contract more debt. *Credit risk* is the ratio of interests expense to EBITDA. A high credit risk ratio indicates a risk that the firm may not be able to repay its loans and its interests.

If a firm's activity generates enough liquid assets, it can use its own cash to undertake investments or hires. A more or less large fraction of a firm's sales can be made on credit, in which case this generates assets which are not liquid. Furthermore, a firm may use a more or less big share of its assets to fund everyday activity. The *share of liquid assets* among total assets is a straightforward measure of how much cash a firm possesses. Other more refined measures account for a firm's everyday cash use. Working capital represents liquidities that are in effect available for the firm. The *working capital ratio* is the ratio of current assets to current liabilities. It indicates a firm's ability to meet its short-term costs and debt obligations, and can be used as a performance indicator, but it also gives an indication on the amount of cash available. The *accounts receivables to sales ratio* is the ratio of the amount of sales occurring on credit to total sales. A high ratio indicates a short-term liquidity risk, as the firm might not have enough cash to cushion economic difficulties. *Excess operating cash* is the difference between cash receipts and payments. It is calculated as EBITDA minus changes in working capital. It measures a firm's self-financing capacity. *Free cash flow* is the available cash of a firm once capital expenditures have been accounted for. It can be calculated as the excess cash minus business taxes and investments. A negative free cash flow is the sign of growing endebtment, and conversely, a positive free cash flow means that the firm generated more cash than what is needed to cover for the cash outflows. These last two indicators are raw measures, which I therefore normalize by total assets. To indicate cash constraints instead of access to cash, I take the inverse of each of these four liquidity indicators.

Heterogeneity analysis. For each cash constraints measure, I identify as being most cash constrained firms ranking among the top 25% in 1995. For each proxy of cash constraints, I run equation (4) separately on the subsample of firms with high cash constraints, and on the subsample of firms with low to medium cash constraints. Figure 16 plots the 1996 coefficients for group Q1 estimated on subsamples with respect to three variables of cash constraints.

I find that firms with a high leverage ratio drive most of the employment boost for firms with a cash windfall. However, firms with a higher or lower credit risk indicator don't seem to react differently to the cash windfall, which suggests that a firm's assets, rather than income, is the focal point for access to credit. Heterogeneity regarding the proportion of sales occurring on credit (*receivables*) is scarce.

Additionally,[16] I find that generating more or less cash is a good predictor of firms' response to the cash windfall. Firms with a low share of liquid assets seem to put the extra cash to use by boosting their employment growth. Accounting for a firm's regular use of these liquid assets, using the inverse of the working capital ratio as a proxy for cash constraints, yields the same results. On the other hand, liquidity constraints as measured by excess operating cash and free cash flow do not seem to be a driving force of employment effects. These measures account for, respectively, changes in working capital, which are imperfectly measured using the firm tax data, and the amounts of investments and the corporate income tax, which are one-off payments and not everyday, regular expenses. These variables could be good proxies of firm performance and financial health, but may be more imperfect measures of firm cash than the most simple indicators that are based on regular inflows and outflows of cash.

Overall, I find that the temporary effect of the cash windfall on firm employment growth is stronger for firms that are initially indebted, and therefore could have difficulty contracting new loans, and for firms with low (liquid) revenues, who do not have spare cash allowing them to undertake profitable strategies without having to contract new debts. These results confirm that cash windfall is a driving force of firm-level responses. Moreover, they give more strength to the interpretation according to which giving firms financial lee-way enables them to react in due time to changes in their economic environment. While firms in Q1 do not react to the relative costs channel, by employing more low-wage labor, they make use of the additional cash generated by the reform by enhancing their production capacities.

6 Discussion

I find that a large payroll tax cut targeted at the minimum wage succeeded in globally increasing the number of jobs at the low end of the distribution, but that all of this increase stems from firms which had too few minimum wage workers previously to be entitled to a cash windfall from this policy. Minimum wage employment in firms with a substantial *ex ante* cash windfall remains extremely stable, while overall employment in those firms grows more than in other firms. As these results may appear surprising, both individually and taken together, I discuss how they compare to previous literature and what they suggest of firm labor demand behavior.

[16] The corresponding estimates are not yet included in Figure 16.

6.1 Effects of the minimum wage on employment

Most work on the effects of the minimum wage on employment has focused on particular populations, such as teenagers, or on specific industries, in particular fast-food restaurants (as in the seminal work of Card and Krueger, 1994) or the retail industry. However, focusing on populations or industries assumed as "low-wage" is suspected of producing downward-biased estimates, because part of these populations or of these industries' employees are paid above the minimum wage. Estimating employment or wage effects on these samples likely dilutes the effect of the minimum wage, which would be stronger on low-wage employees. Moreover, these analyses raise concerns about the representativeness of these populations or sectors. For instance, teenagers have a weaker attachment to the labor market than other minimum wage workers working full time, with longer and continuous employment spells.

More recent papers compare firms with different "bites" of the minimum wage (Harasztosi and Lindner, 2019; Hirsch et al., 2015; Huang et al., 2014; Jardim and van Inwegen, 2019). Yet the policy debate around the minimum wage is in general more focused on the number of jobs destroyed (and created) than on the effects on firms—including firm-level employment.

To my knowledge, only two recent papers approach the minimum wage question through this angle: Jardim et al. (2017) and Cengiz et al. (2019). They investigate the effects of changes in the minimum wage on the number of low-pay jobs (Jardim et al., 2017) or on the wage distribution as a whole (Cengiz et al., 2019). In particular, Cengiz et al. (2019) apply a bunching estimator to a differences in differences strategy using a large set of US State-level minimum wage increases and find a zero net effect on employment. Indeed, they find that the destruction of below-minimum wage jobs is compensated by a hike in the bunching of the distribution at the new minimum wage. In this paper, I find that lowering the minimum labor cost has overall a positive effect on the number of jobs at the minimum wage. These results are however not contradictory: raising the minimum labor cost can increase the number of jobs at the minimum wage if a large number of workers with productivities below the new minimum wage were already employed, and decreasing the minimum labor cost allows for a higher number of individuals with productivities below the old minimum labor cost to be employed. Moreover, in the setting I study, the pre-reform minimum labor cost was high as a ratio to median labor cost, and both Cengiz et al. (2019) and Jardim et al. (2017) find that job destruction increases with the level of pre-reform minimum-to-median wage ratio. Symmetrically, we could expect job creation to increase with the pre-reform bite of the minimum wage, when minimum labor cost is reduced.

Related to the minimum wage literature, my results also highlight the drawbacks of identifying minimum-wage employment effects by comparing firm-level employment of firms with more or less low-wage workers prior to the reform. Analyses using firm-level bite as a measure of treatment identify the effect of an increase (or decrease) of low-wage labor cost on firm behavior, but on employment as a whole. While I find that firms with more minimum wage workers increase firm-level employment when the minimum labor cost decreases, my paper confirms the potential worries that "non-treated" firms with no or little *ex ante* change in labor costs due to changes in the minimum labor cost may change their labor composition, due to the distortion of the relative cost of labor. Firms with little or no low-wage workforce retain and hire more minimum wage labor. In a setting where the minimum labor cost increases, firms with no minimum wage labor initially might increase new hires' or incumbents' wages to restore wage differentiation with respect to skills or occupations.

Moreover, at the firm level, employment effects may not be interpreted as (only) effects of the minimum wage: the cash windfall effect may confound a "pure" minimum wage effect which would be driven by the absence or presence of frictions on the labor market. Besides, the negative firm-level employment effects of *ex ante* increases of labor costs mimic the negative firm-level employment effects of increases of the minimum wage (see for instance Harasztosi and Lindner, 2019; Jardim and van Inwegen, 2019), which stresses that the wage level concerned by a change in labor cost may not matter. Previous firm-level studies of minimum wage effects highlight a liquidity channel, which appears to be fundamental to understand any firm-level employment effect: Draca et al. (2011), Hirsch et al. (2015) and Harasztosi and Lindner (2019) all show that firms' profits decrease with the firms' exposure to minimum wage hikes.

Moreover, at the firm level, employment effects may not be interpreted as (only) effects of the minimum wage. In the case of a payroll tax cut for low-wage workers, I find that firm-level employment effects are largely driven by the cash windfall the policy generated. These results are in line with firm-level studies of minimum wage effects which highlight a liquidity channelt: Draca et al. (2011), Hirsch et al. (2015) and Harasztosi and Lindner (2019) all show that firms' profits decrease with their exposure to minimum wage hikes. Finally, I find that firms that suffer from a cash loss due to increases in payroll taxes further up the wage distribution react in a symmetric manner to those benefiting from a cash windfall: they increase employment less. This mimics the negative firm-level employment effects of increases of the minimum wage (see for instance Harasztosi and Lindner, 2019; Jardim and van Inwegen, 2019), which suggests that the wage level concerned by a change in labor cost may not matter—at the firm level, it is the cash windfall, or cash loss, that matters.

6.2 How do firms react to a cash windfall?

My results reveal a cash windfall mechanism: the targeted reduction in payroll taxes affects employment differently according to the amount of cash windfall, or cash loss, of firms, and cash constraints appear to be a strong mechanism.

Recent papers address the rent-sharing behavior of firms receiving unexpected cash. They highlight the effects on the average wages of employees, showing that part of the windfall is redistributed through higher wages to all, or a fraction, of employees (Carbonnier et al., 2020; Howell and Brown, 2020; Kline et al., 2019; Saez et al., 2019). I take a different approach. In the case of these payroll tax reductions which target low-wage employees, firms which benefit from a cash windfall and firms that don't largely differ in terms of workforce composition, in pre-treatment year but also likely in trends. As a result, average wage evolutions are not comparable between groups. Rather, I investigate whether at a broader level (large groups of firms defined with respect to policy exposure), potentially benefiting or not from a cash windfall affects the distribution of wages—bearing in mind that the policy distorts the relative cost of labor. I find that in firms benefiting from a cash windfall, overall, the wage distribution does not react at all to the change in relative labor cost. If anything, the number of jobs at higher wage levels increase.

This surprising result reminisces Daunfeldt et al. (2019): the authors evaluate the firm-level effects of the payroll tax cut targeting the young in Sweden and find that firms benefiting from large labor cost savings due to this tax reduction slightly increased employment of older individuals. However, contrary to my findings—I find that the number of low-wage jobs does not increase in firms with the highest cash windfall—they find that these effects on the non-targeted population are in fact much smaller than those on the targeted (young) individuals. Other studies find "spillover" effects of exogenous labor cost reductions, but these spillovers affect the workers' wages rather than their employment prospects (Carbonnier et al., 2020; Saez et al., 2019).

Looking at how firm-level employment changes with firms' exposure to labor cost changes, I find that firms with a cash windfall grow faster than firms with virtual no *ex ante* change in costs, and firms facing a cost increase due to the uncapping of Social Security contributions on high wages grow slower. These results confirm what has been shown by the literature tackling firm-level employment effects of changes in payroll taxes. Crépon and Desplatz (2001) find a strong firm-level employment effect of the *Juppé* reforms. Although the overall sign matches my results, I find that defining firm exposure solely with respect to changes in payroll tax reductions (and not overall payroll tax changes) produces biased estimates, by confounding the effects of payroll tax cuts on low-wage jobs and of payroll tax increases on high wages (see Figure A2). I also nuance their results by showing that the firm-level effects of the payroll tax cut are short term. Bunel and L'Horty (2012) highlight a strong reaction of full-time equivalent employment to changes in labor costs in France between 2003 and 2005. Similarly, Saez et al. (2019) find that the reduction in payroll taxes on the wages of the young increased growth in firms with a high share of young workers prior to the reform, relatively to firms with a lower share.[17] Symmetrically, Ku et al. (2020) find that an exogenous increase in payroll taxes in some regions in Norway impulsed a significant drop in local employment, and Benzarti and Harju (2020) show that a higher payroll tax at the firm level implies (downward) employment responses, relatively to firms subject to a lower rate. More generally, my paper adds to the evidence that firms respond to positive or negative cash shocks by adjusting employment, suggesting binding financial constraints (Melcangi, 2018).

This paper contributes to this literature by pinpointing the cash-constraints mechanism. Saez et al. (2019) show that among firms benefiting from a cash windfall, younger firms, firms with low sales and firms with a low share of liquid assets grow more in reponse to the payroll tax cut. However, these differences in growth are rarely statistically significant. Using rich firm data, I can compute variables that are more likely to be related to cash constraints (Section 5.4). Moreover, robustness specifications show that initial employment size—which correlates strongly with age (Fort et al., 2013; Haltiwanger et al., 2013; Colciago et al., 2019)—is a strong predictor of employment growth (Section 5.3), but the existing literature has shown that this is a feature of firm growth—even in the absence of cash-relieving policies (Moscarini and Postel-Vinay, 2012). Finally, running a split-sample strategy on this richer set of cash constraints indicators, I find evidence that liquidity constraints relating to every-day cash flows, more than credit constraints, might be more determinant.

7 Conclusion

This paper studies a large payroll tax cut on minimum wage workers. Results show that neither changing relative labor costs for different labor types, or wages, nor the cash windfall that results from the policy, may fully explain firm behavior and the changes in the job

[17] An exception is provided by Carbonnier et al. (2020) that shows that a tax cut proportional to the total payroll of employees paid less than 2.5 times the minimum wage had no effect on employment. The setting is however very different from these payroll tax cuts on two points: the tax cut is calculated based on the proportion of workers paid less than 2.5 times the minimum wage—a much higher threshold than that of the payroll tax cut studied here—and the tax cut materializes with at least a one-year delay, being a tax credit. The first feature implies that the firms benefiting from a cash windfall are very different in our settings, and in particular could differ in terms of liquidity constraints, and the demand for this higher-paid labor is suspected to be less sensitive to its cost; the second feature could further explain that firms do not take into account in year t a reduction in labor cost effective one, two or three years later.

destribution.

I find that the payroll tax cut triggered an increase in the number of jobs at the minimum wage, as was expected by the reform. However, this stems only from firms with too few low-wage workers to benefit *ex ante* from a reduction in labor costs thanks to the reform. In these firms, the retention rate of low-wage workers increases, as well as the number of hires in firms with no minimum wage workers prior to the reform. Results suggest that this increase of the number of low-wage jobs is coupled with a wage trap.

Firms which already employed minimum-wage workers prior to the reform, and therefore stand to benefit from an exogeneous drop in their production costs, appear not to react to the change in relative costs: on the contrary, the number of jobs at higher wages increase, through more retention of higher-wage workers and more wage increases for low-wage jobs. These firms pocket the cash windfall but do not seek to increase the benefits of this payroll tax reduction. With a firm-level analysis, I find that these firms use the cash windfall to boost their size growth, with initial cash constraints driving much of this effect.

From a methodological viewpoint, this paper shows the limits of focusing solely on firm-level or job-level "treatment". Mixing both levels of analysis enables to see that both individual-level treatment and firm scale effects matter, and moreover shows that relative-costs effects may differ according to the cash windfall firms receive.

In terms of policy, this paper shows that reducing the cost of minimum-wage labor succeeds in fostering employment at the minimum wage, but also in—indirectly—addressing liquidity constraints of small low-wage firms which can anticipate hiring decisions. Efficient labor cost-reducing policies should be designed in such a way that they take in account the mechanical cash windfall effect, which can help liquidity-constrained firms but can also be pocketed by less-constrained, bigger firms, and their employees (as shown by Carbonnier et al., 2020).

Future research should investigate the conditions in which a cash windfall is used to hire and/or to increase wages of the less-skilled, low-wage workers, and those in which the cash windfall serves profits and the wages of skilled workers and managers. In particular, comparing the effects of different types of cuts in production costs, targeting different types of firms, could help to disentangle fairness motives, the importance of bargaining power of different types of workers, and the role of liquidity constraints.

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Table 1: Number of firms

Number of full-year equivalent workers in 1995	Sample type	
	Balanced	Unbalanced
At least 2	336 428	545 720
At least 10	91 855	130 683

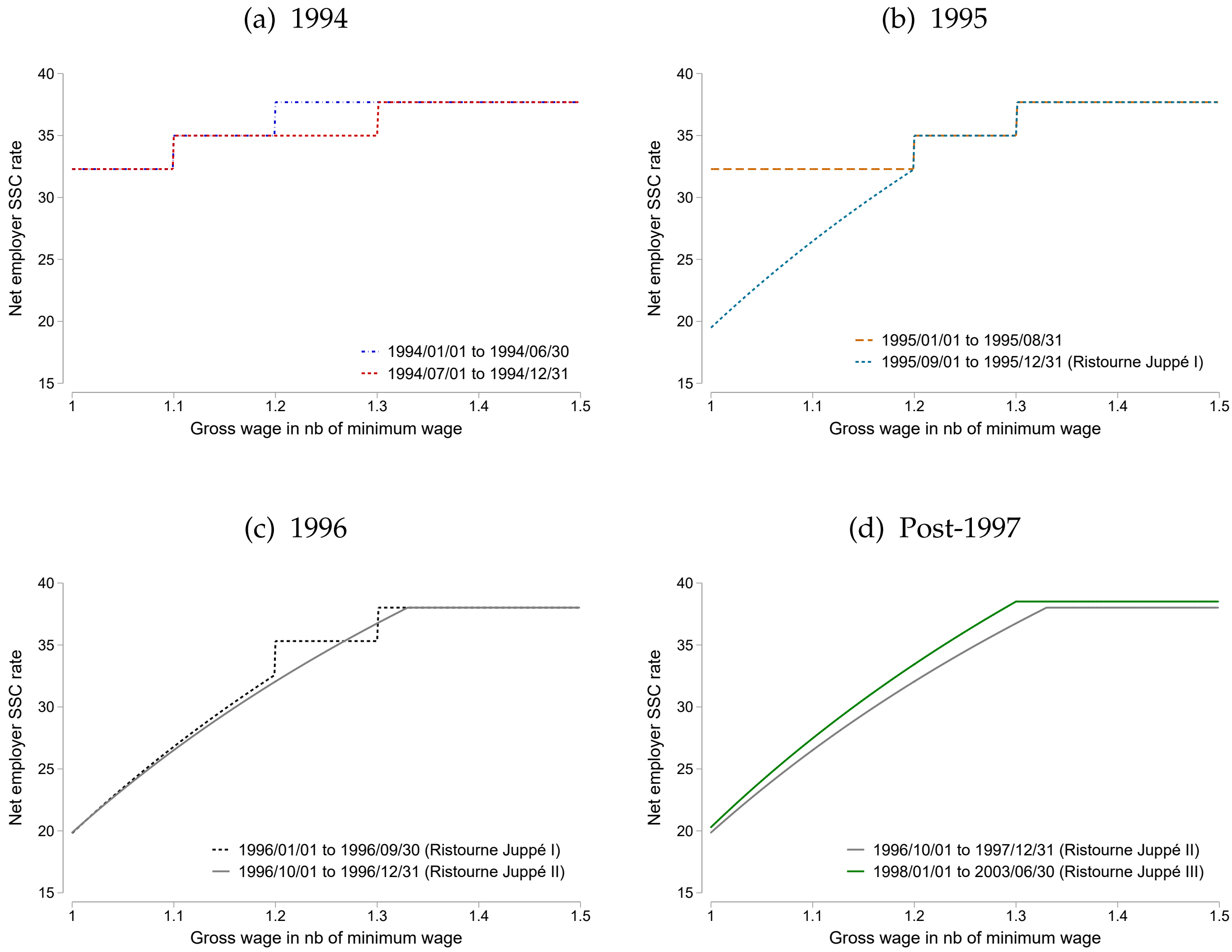
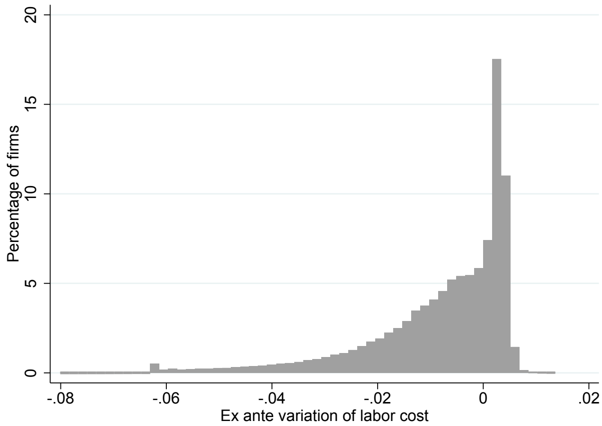


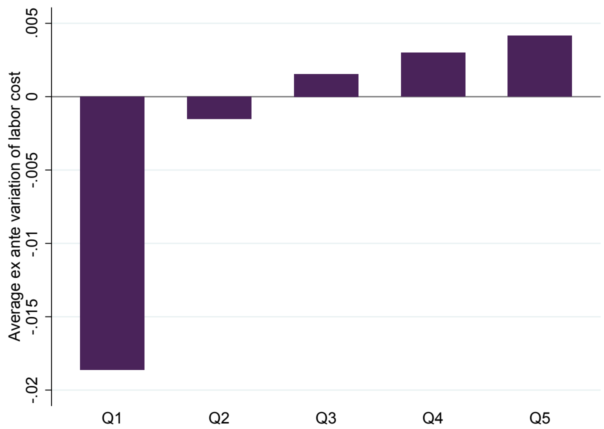
Figure 1: Employer SSCs net of payroll tax cuts

Note: Reductions in employer SSCs are deducted from the sum of all employer SSCs (pension, unemployment and non-contributory SSCs). SSC rates may differ with the employee status; the sum of employer SSCs is calculated for a non-executive employee.
Source: IPP *Tax and benefit tables*, author's calculations.

(a) Distribution of *ex ante* variation of labor cost



(b) Average *ex ante* variation of labor cost by weighted quintiles of firms



(c) Average *ex ante* variation of labor cost by weighted deciles of firms

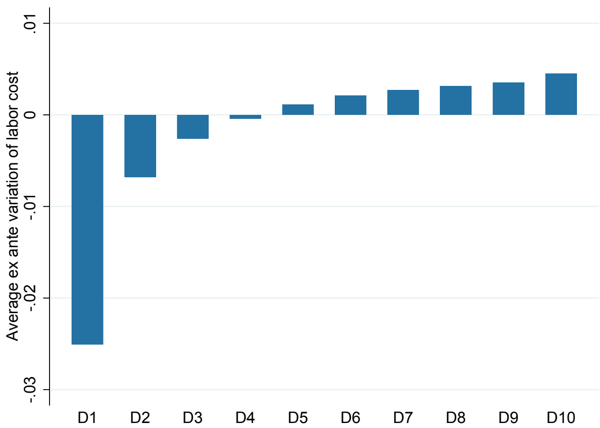


Figure 2: *Ex ante* variation of labor cost

Note: Figure 2a represents the distribution of *ex ante* variation of labor cost. Figures 2b and 2c plot the average values of *ex ante* variation of labor cost by subgroups of firms.
Source: Fiscal and employer-employee data (FICUS, DADS Postes). Author's microsimulation of changes in labor costs.

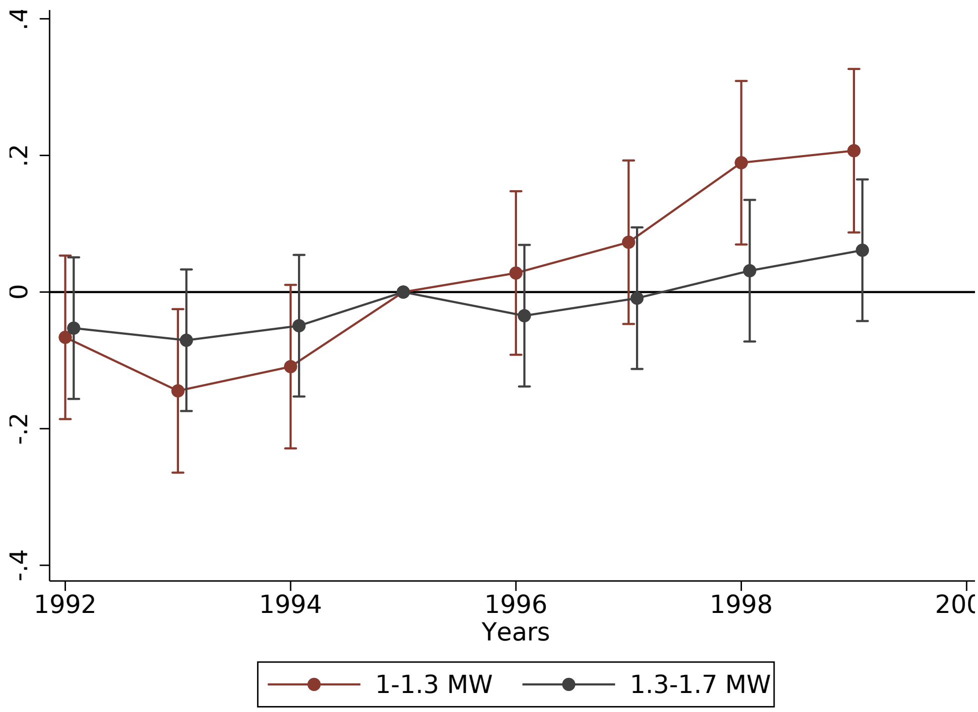


Figure 3: Simple differences: All jobs, selected wage bins

Note: This figure plots the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for two wage bins and each year t, with a normalized measure of the number of all jobs in a given wage bin and a given year as the dependent variable.
Source: Sample version of the employer-employee data (*DADS Panel*).

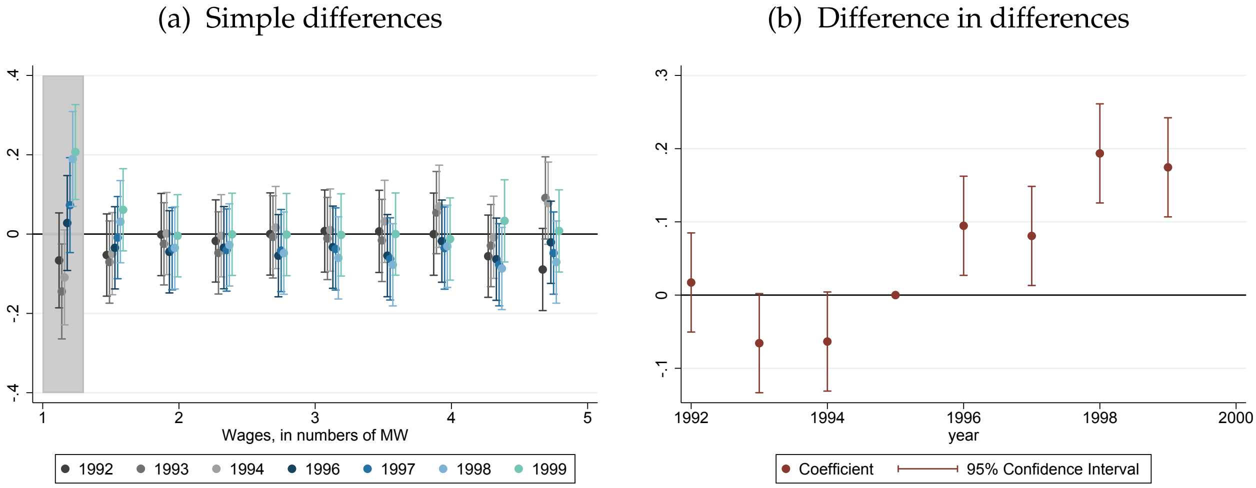


Figure 4: Simple difference and Difference in differences estimates

Note: Figure 4a plots the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of all jobs in a given wage bin and a given year as the dependent variable. Figure 4b plots the difference-in-differences estimates of equation 2.
Source: Sample version of the employer-employee data (*DADS Panel*).

(a) Sample data (*DADS Panel*) (b) Exhaustive data (*DADS Postes*)

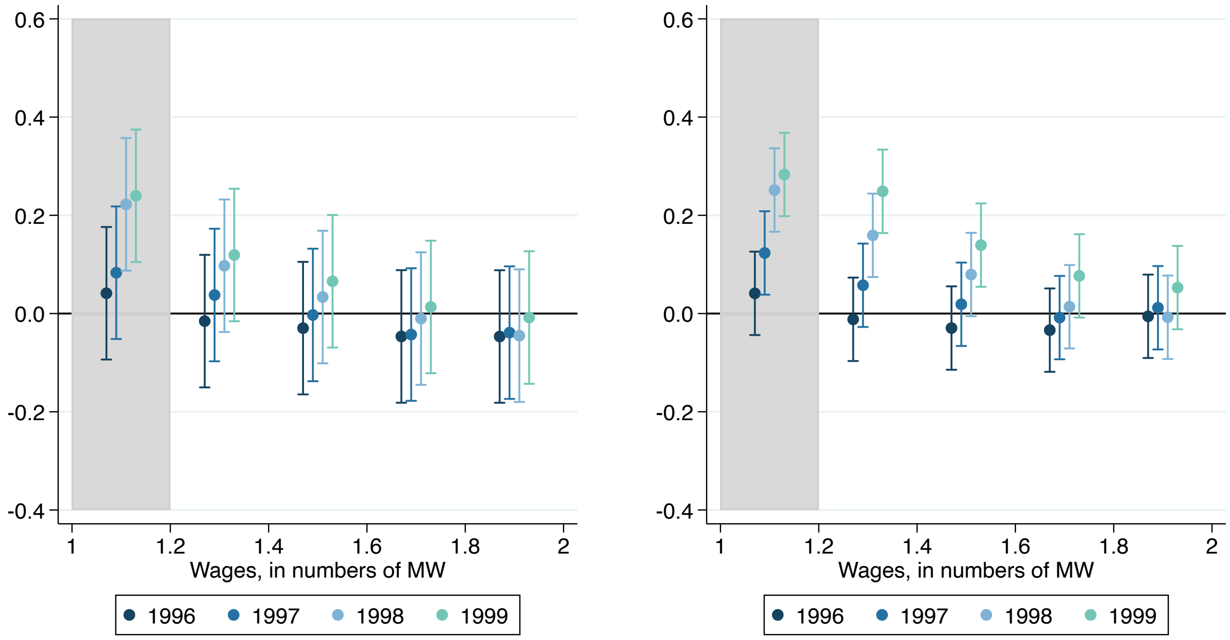


Figure 5: Simple difference estimates: All jobs, comparing sample and exhaustive data

Note: These figures plot a subset of the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for bins b of width 0.2 and each year t post-1995, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable.
Source: Sample version (panel 5a) and exhaustive (panel 5b) employer-employee data.

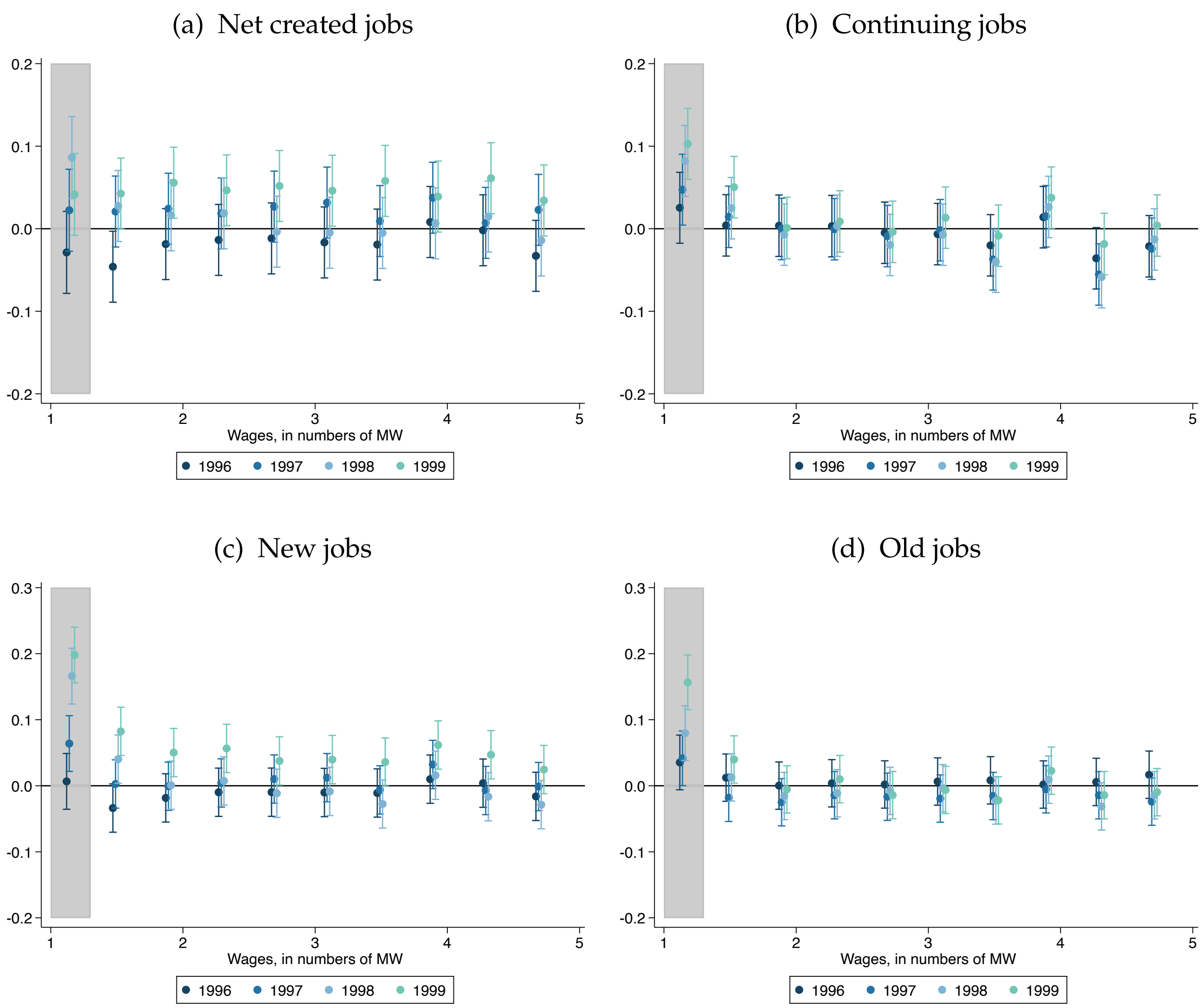


Figure 6: Simple difference estimates: Decomposing between job types

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*.
Source: Exhaustive employer-employee data (*DADS Postes*).

(a) No upward wage mobility

(b) Upward wage mobility: wage bin in *N-1*

(c) Upward wage mobility: wage bin in *N*

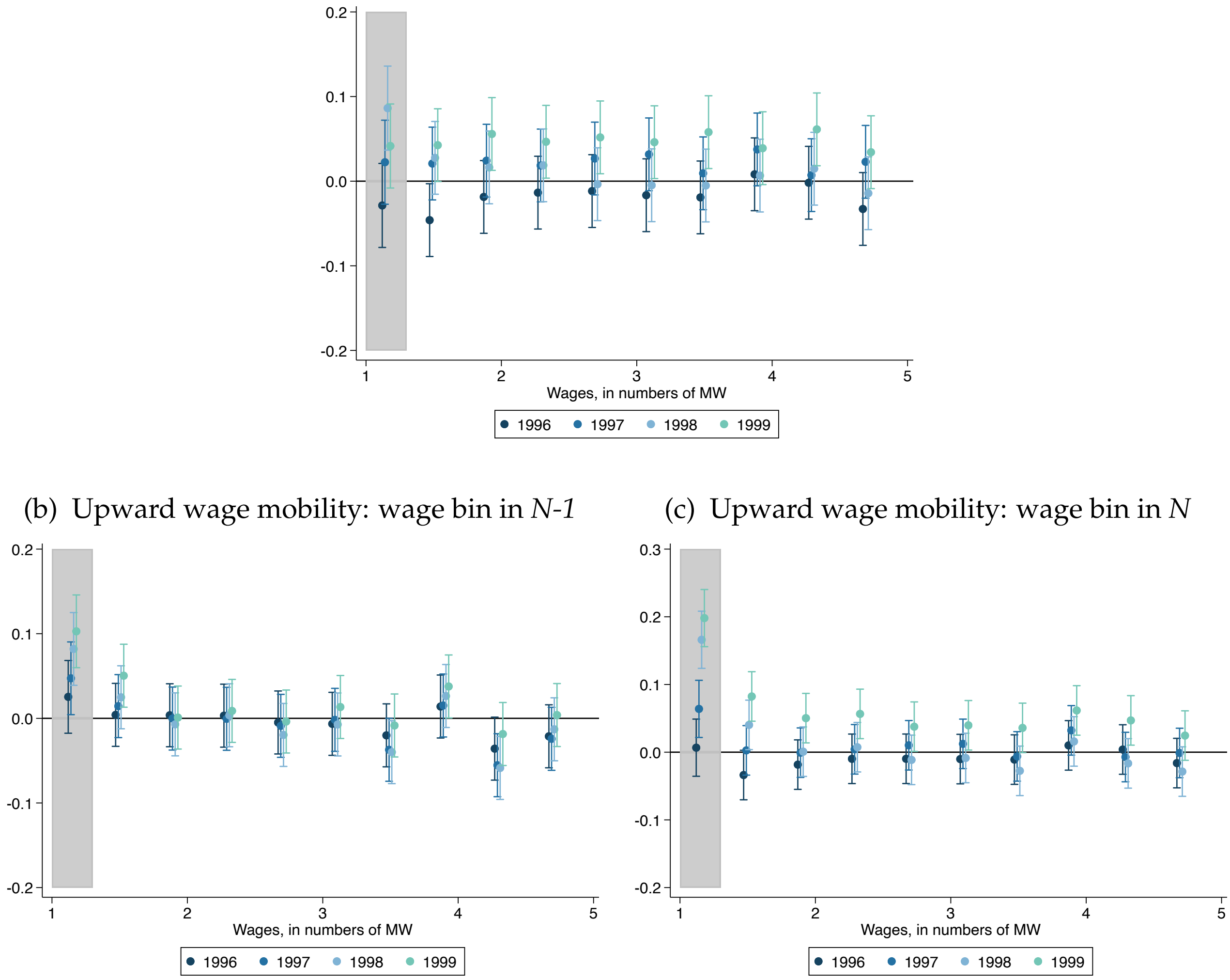


Figure 7: Simple difference estimates: Continuing jobs with or without significant upward wage mobility

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of continuing jobs in a given wage bin and a given year as the dependent variable. Continuing jobs are jobs that are observed both in N and in *N-1*. In Figure 7a, the dependent variable is the number of continuing jobs with no upward wage mobility. In Figures 7b and 7c, the dependent variable is the number of continuing jobs with no upward wage mobility. In Figure 7b, the wage bin is that of the wage observed in *N-1* (*origin*) whereas in Figure 7c, the wage bin is that of the wage observed in N (*destination*). Upward wage mobility is defined as a change of wage bin.
Source: Exhaustive employer-employee data (*DADS Postes*).

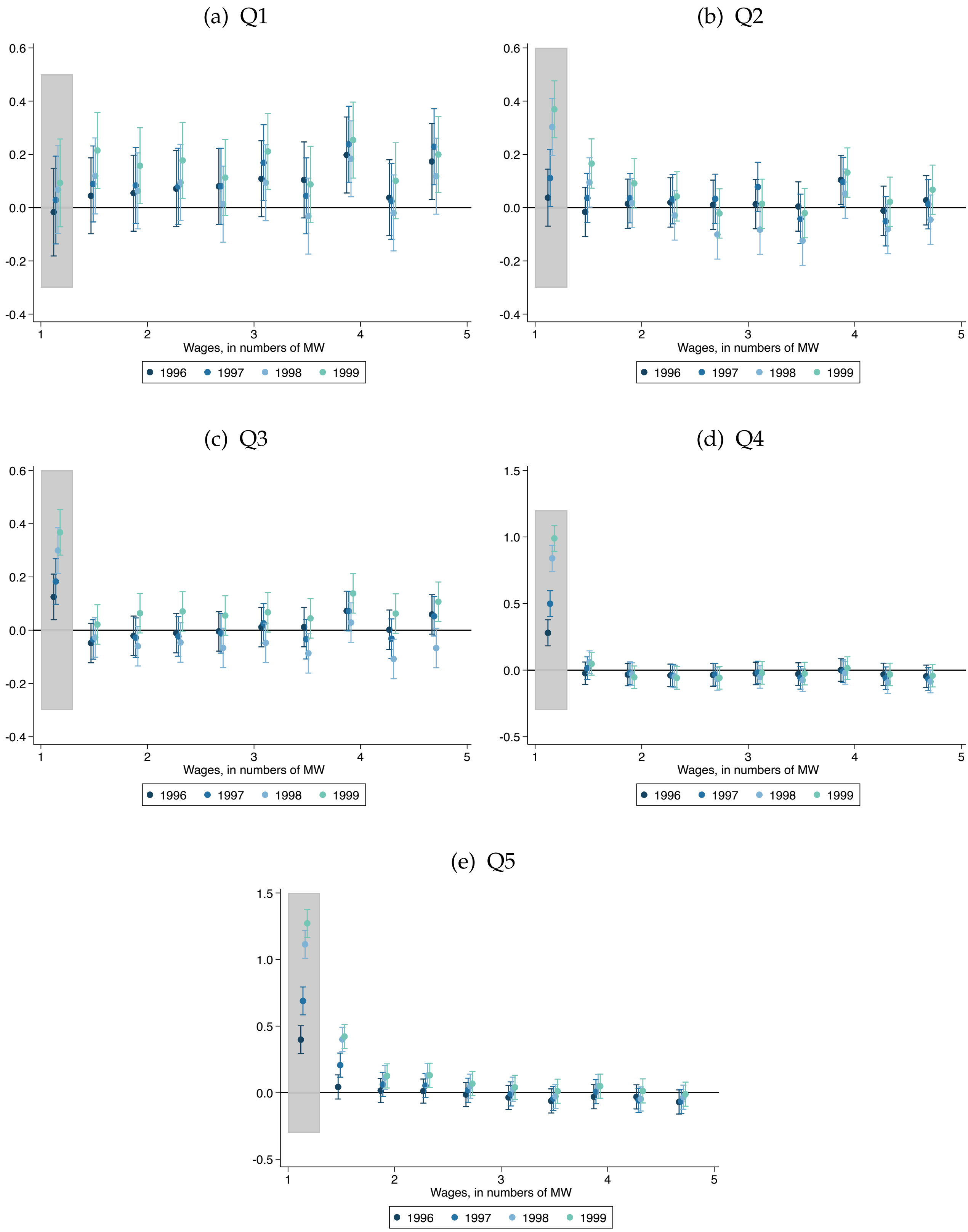


Figure 8: Simple difference estimates: All jobs, by firms' quintile

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of all jobs in a given wage bin and a given year as the dependent variable. The simple differences is estimated on the sample of firms belonging to a specific quintile of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

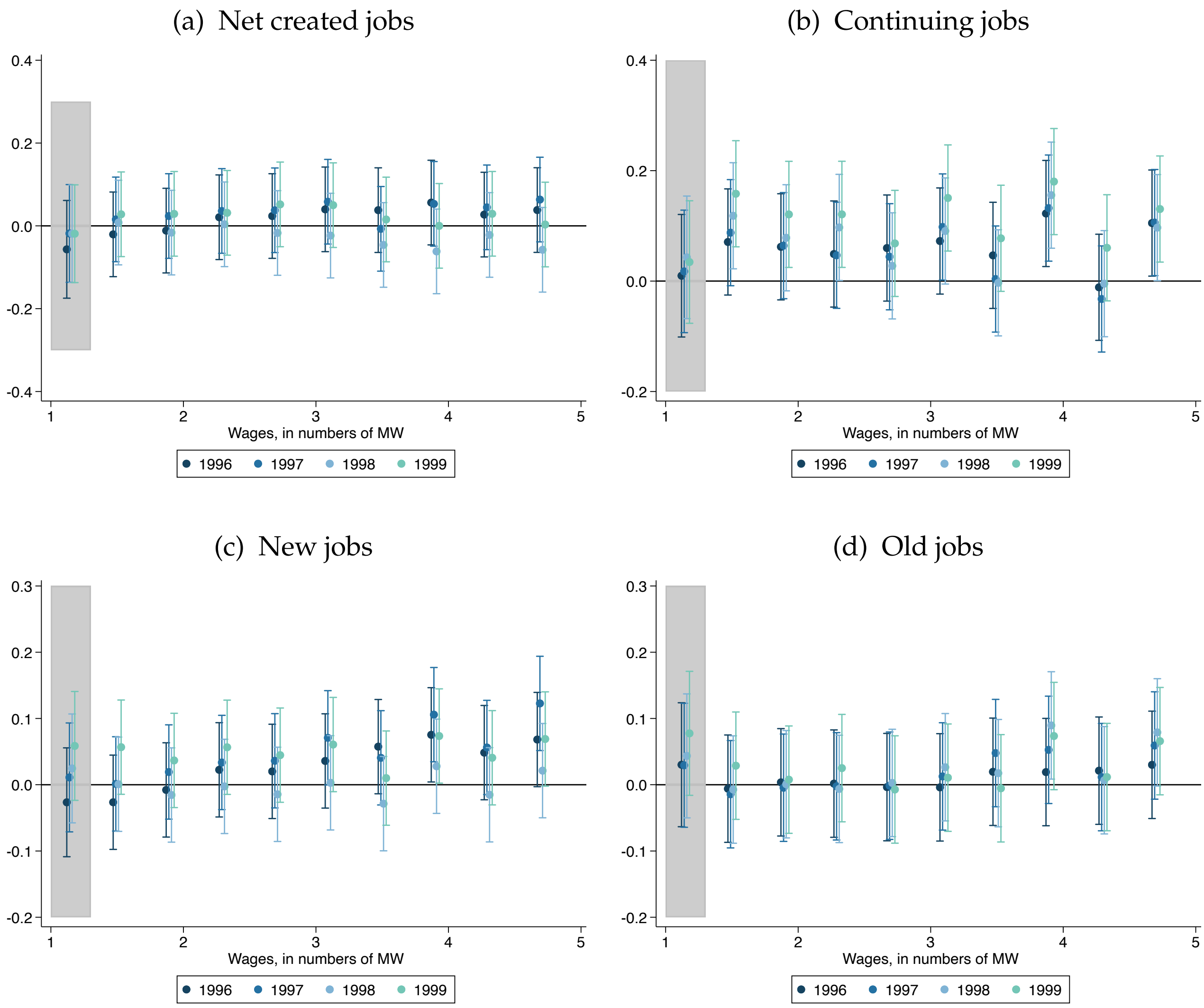


Figure 9: Simple difference estimates: Decomposing between job types, Q1

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*. The simple differences is estimated on the sample of firms belonging to quintile Q1 of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

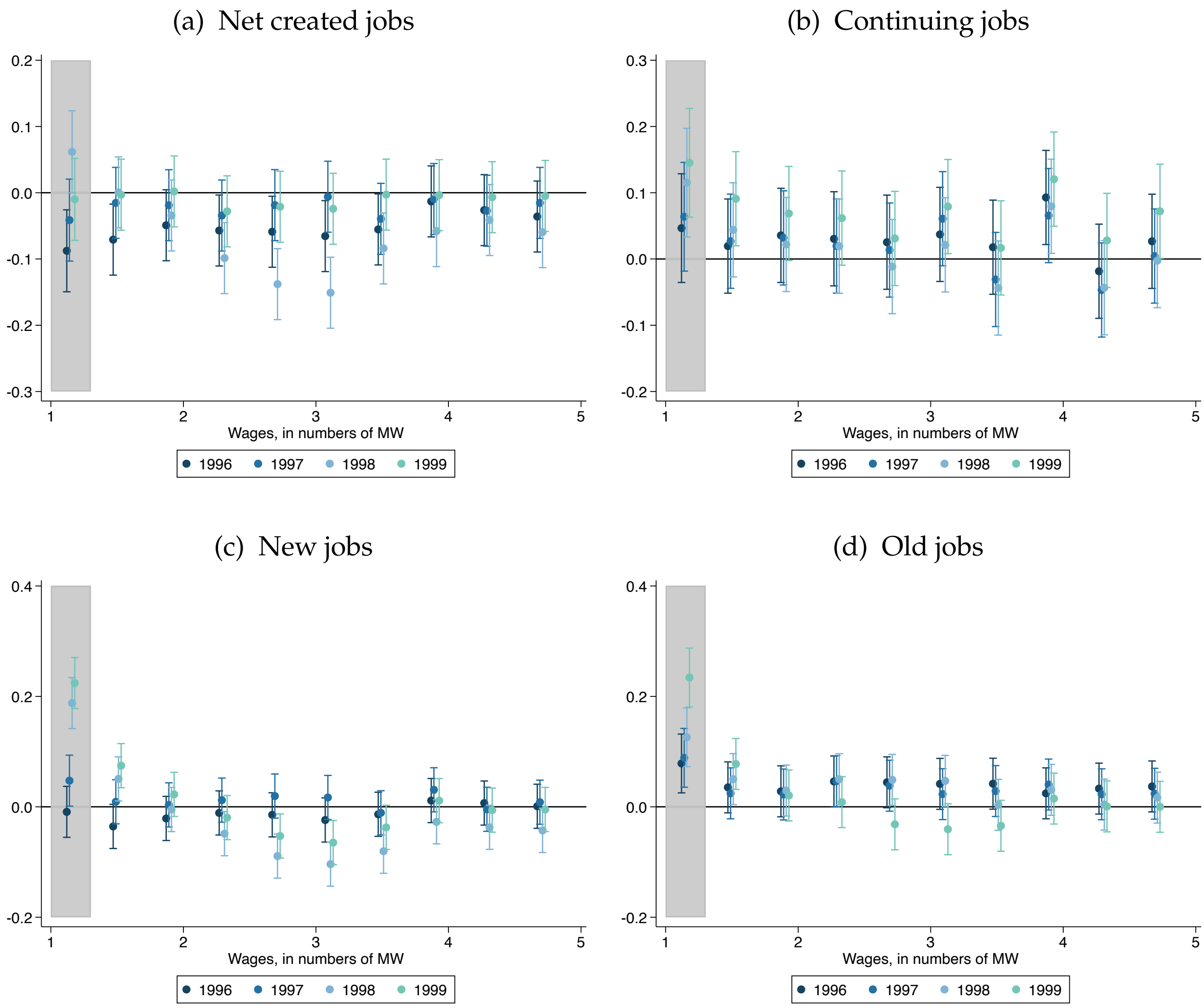


Figure 10: Simple difference estimates: Decomposing between job types, Q2

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*. The simple differences is estimated on the sample of firms belonging to quintile Q2 of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

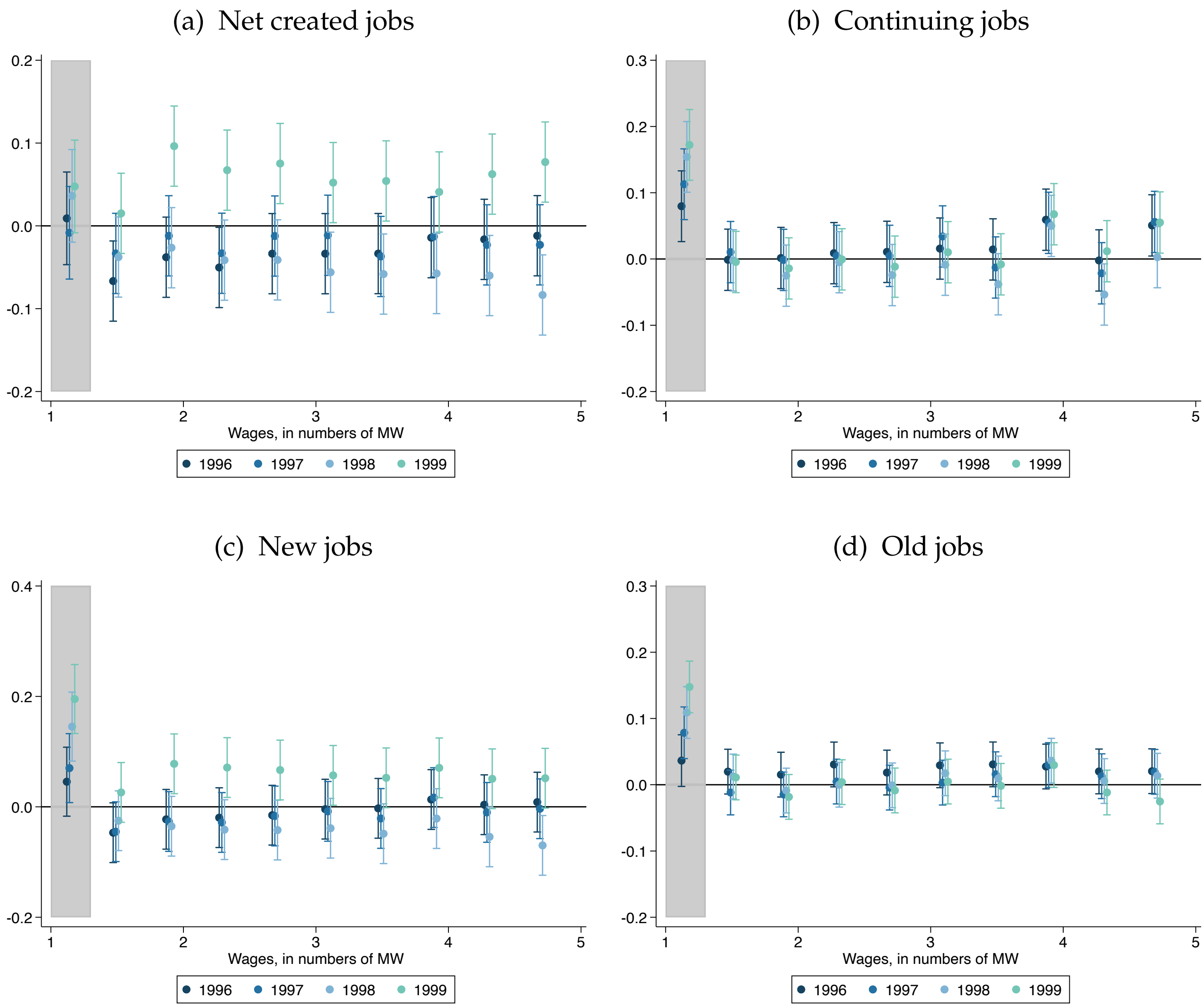


Figure 11: Simple difference estimates: Decomposing between job types, Q3

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*. The simple differences is estimated on the sample of firms belonging to quintile Q3 of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

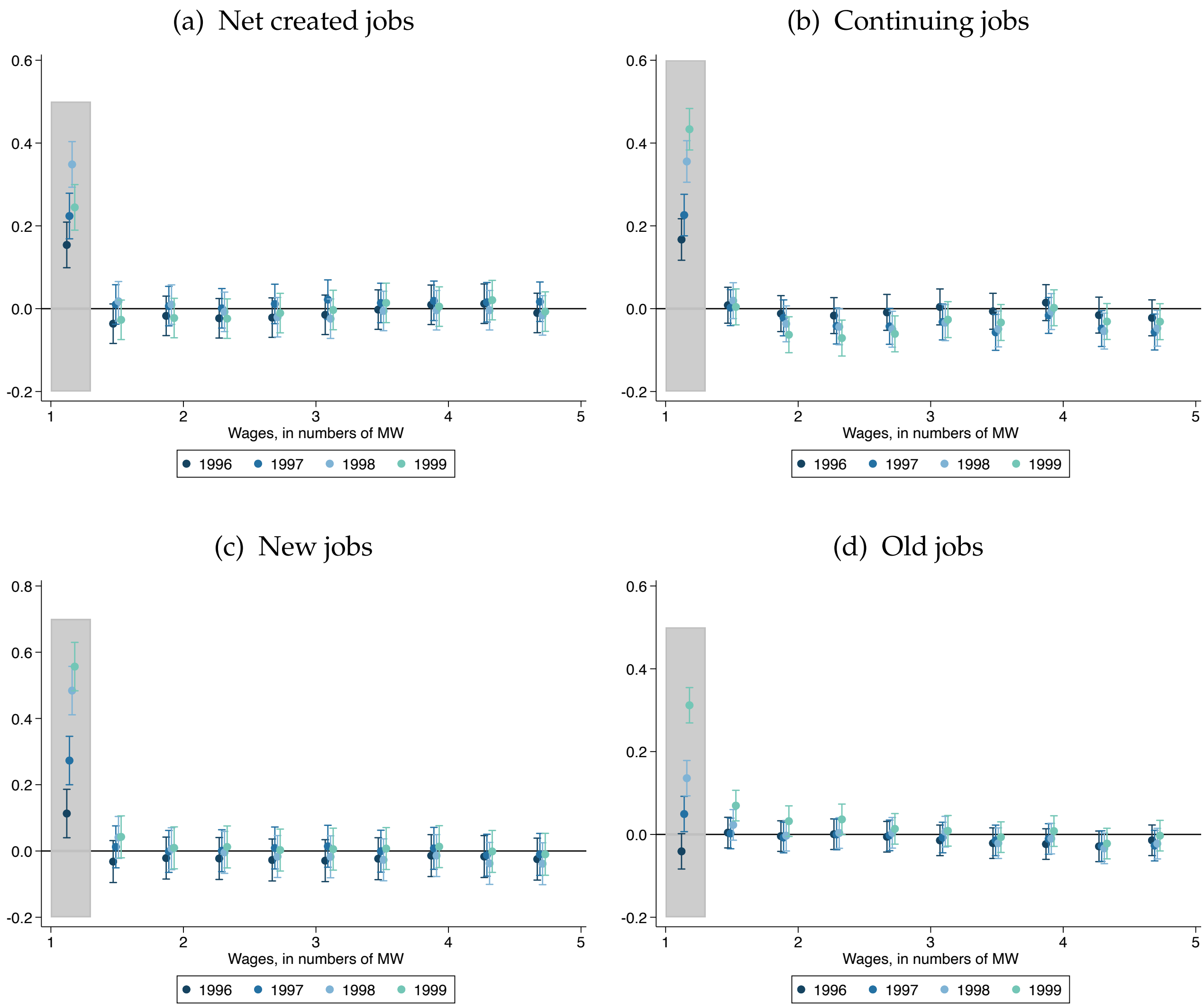


Figure 12: Simple difference estimates: Decomposing between job types, Q4

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*. The simple differences is estimated on the sample of firms belonging to quintile Q4 of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

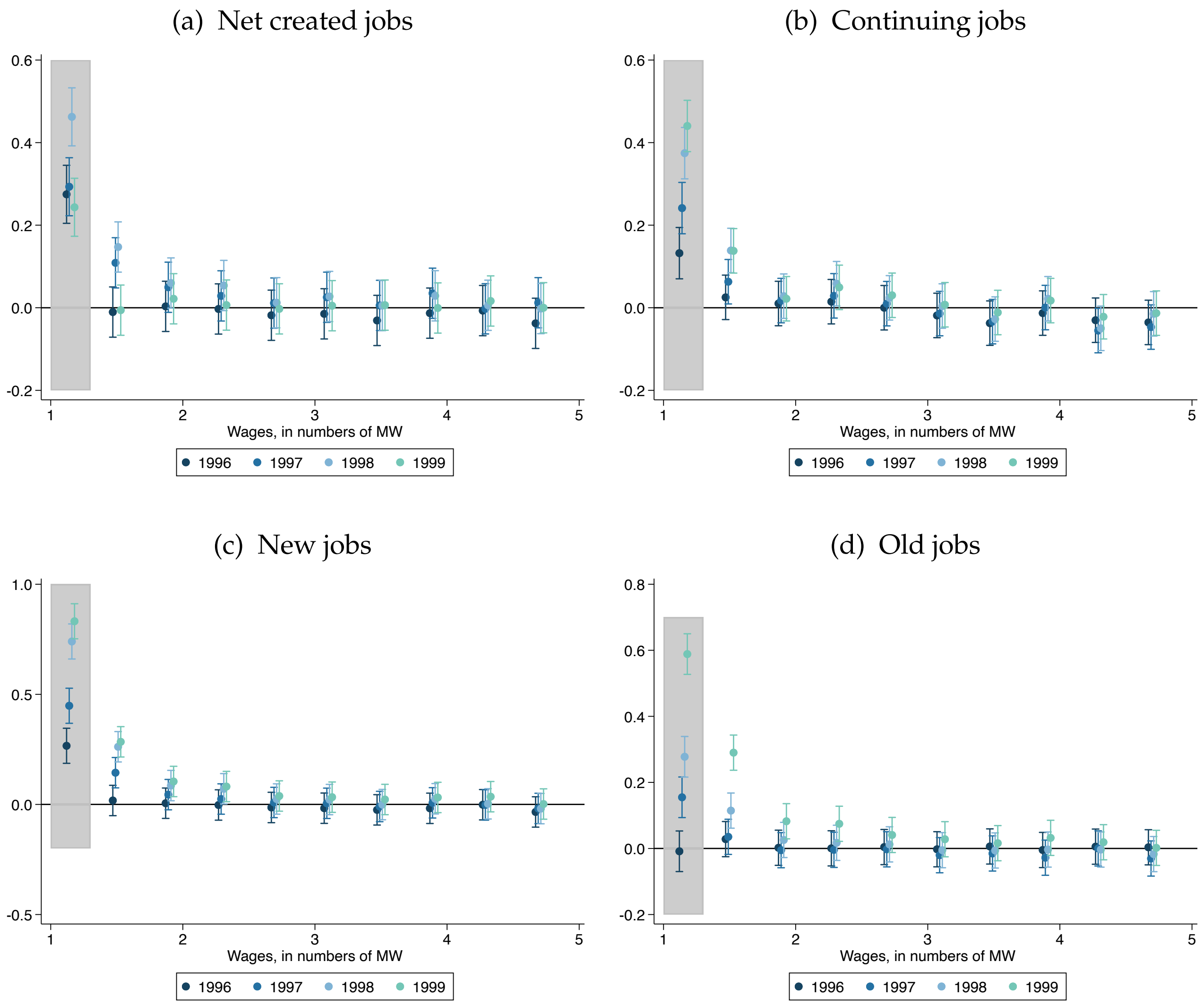
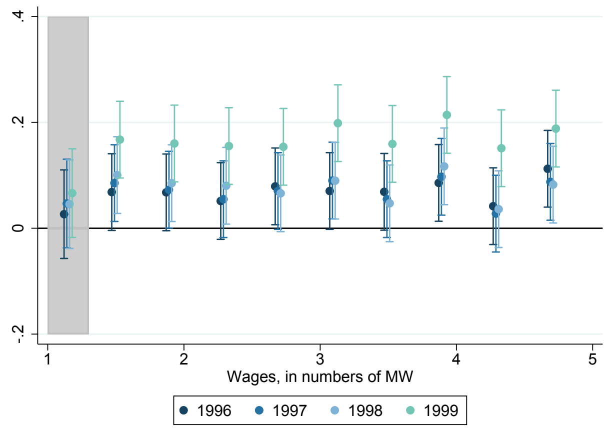
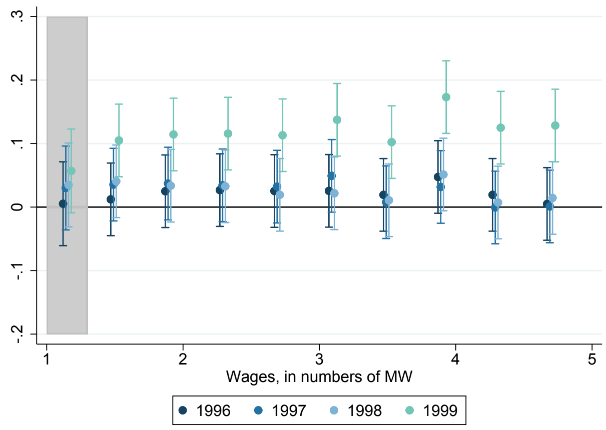


Figure 13: Simple difference estimates: Decomposing between job types, Q5

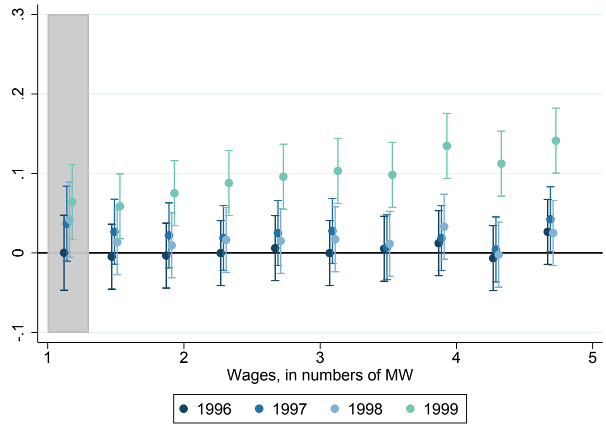
Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Net created jobs is the difference between new jobs and old jobs. New jobs are jobs that are observed in *N* but not in *N-1*; old jobs are jobs that are observed in *N-1* but not in *N*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*. The simple differences is estimated on the sample of firms belonging to quintile Q5 of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).



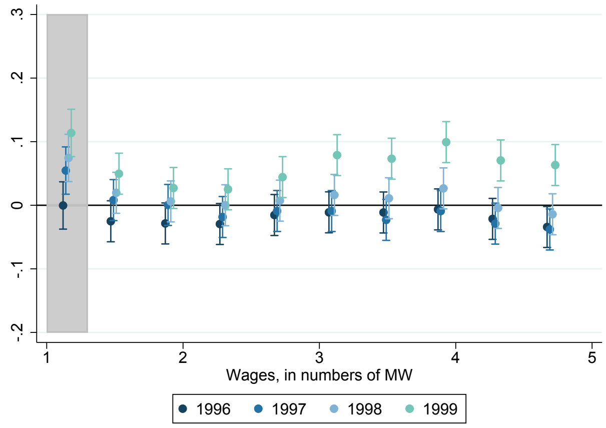
(a) Q1



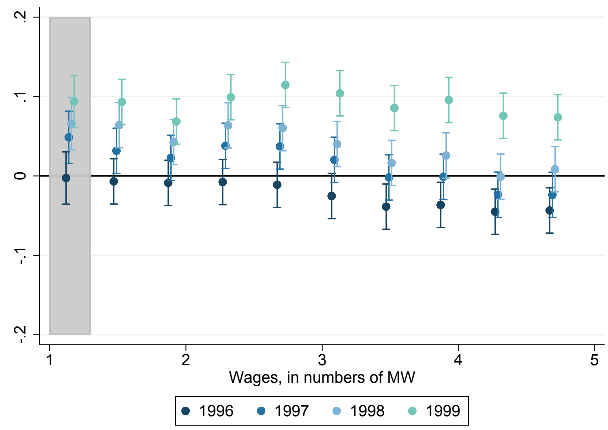
(b) Q2



(c) Q3



(d) Q4

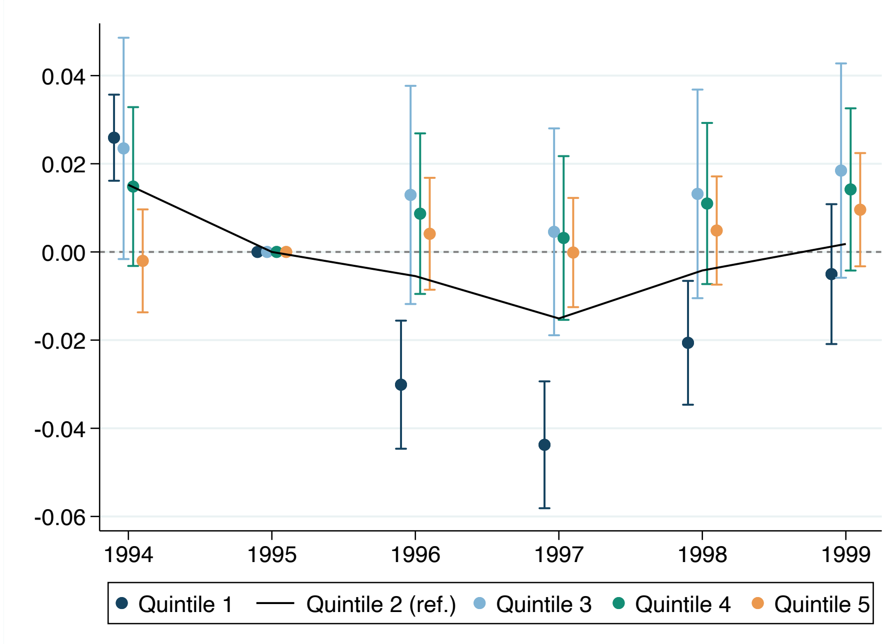


(e) Q5

Figure 14: Simple difference estimates: Upwards wage mobility, destination wage bin, by firms' quintile

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t. The dependent variable is the number of jobs in a given wage bin b ("destination") and a given year N which were in a lower wage bin $a < b$ in $N-1$. The simple differences is estimated on the sample of firms belonging to a specific quintile of exposure to the SSC reforms, with Q1 the weighted quintile of firms benefiting ex ante from a cash windfall and Q5 the weighted quintile of firms suffering the most from an ex ante increase in labor cost.
Source: Exhaustive employer-employee data (*DADS Postes*).

(a) Differential growth rate of the ratio of social charges in total labor costs



(b) Differential growth rate of full-year equivalent employment

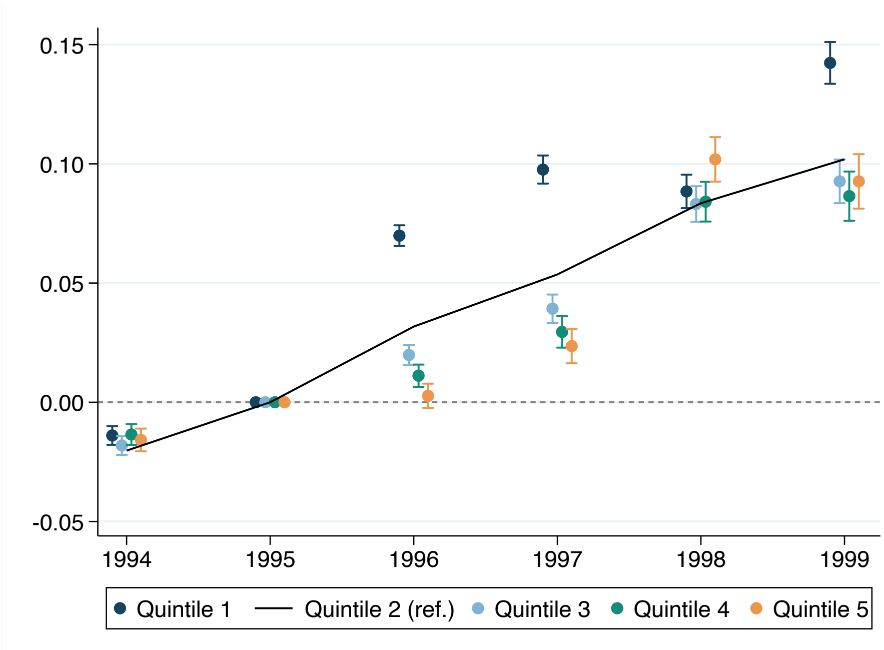


Figure 15: Firm-level effects of changes in payroll taxes

Note: These figures represent the β coefficients estimated for the regression of equation 4 for two dependent variables: the cumulative growth rate of the ratio of social charges in total labor costs (Figure 15a) and of full-year equivalent employment (Figure 15b). For each year, these graphs plot the sum of the β coefficients and the average of the dependent variable for the reference group (Quintile 2) for this year. The unconditional average growth rate of full-year employment in firms of Quintile 2 between 1995 and 1997 is 5%, and controlling for year-sector fixed effects, year and firm fixed effects, the 1995-1997 growth rate for firms in Quintile 1 is 4.4 percentage points higher compared to Quintile 2. The regression is estimated on the balanced panel of firms with at least 10 full-year equivalent employment in 1995 (N = 96862 and 96654, respectively). Standard errors are clustered at the firm level. The vertical bars represent the 95% confidence interval.
Source: Fiscal and employer-employee data (FICUS, DADS Postes).

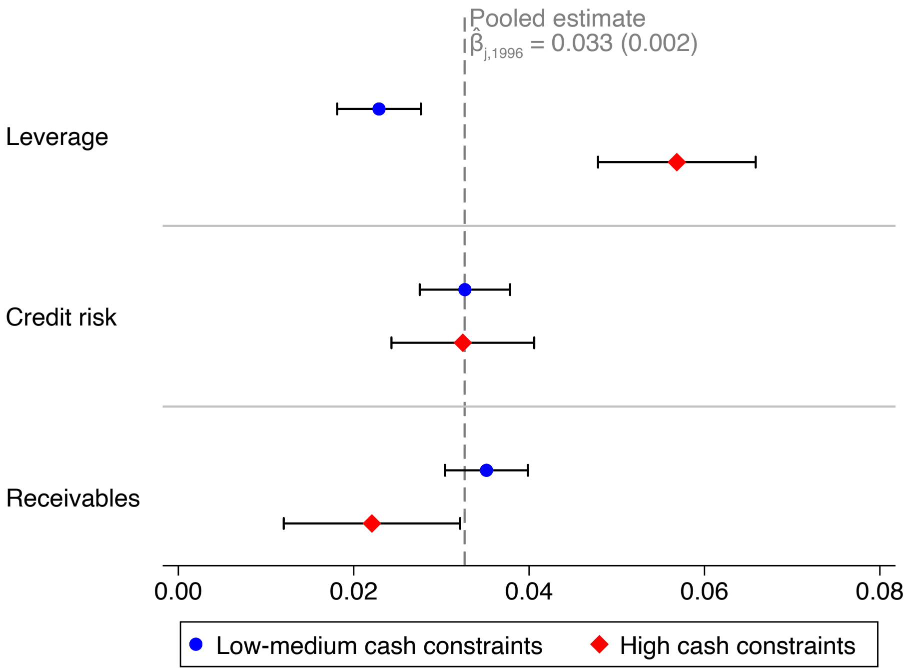


Figure 16: Heterogeneity of Q1 firm-level effects with respect to cash constraints: 1996 coefficients

Note: This figure plots the $\beta_{1,1996}$ coefficients of equation (4), with additional size–year fixed effects, estimated on sub-samples of firms with respect to their level of cash constraints. Firms with high cash constraints rank in the top quartile of the cash constraints proxy variable. Firms with low to medium cash constraints rank in the first three quartiles. Standard errors are clustered at the firm level. The vertical bars represent the 95% confidence intervals. In 1996, the employment effect on firms in Q1 relatively to Q2 is 0.056 on the subsample of firms with the highest cash constraints as measured by leverage, compared to 0.022 for the subsample of firms with low to medium cash constraints according to this measure. On the whole sample, this coefficient is estimated to be 0.033.
Source: Fiscal and employer-employee data (FICUS, DADS Postes).

Additional figures

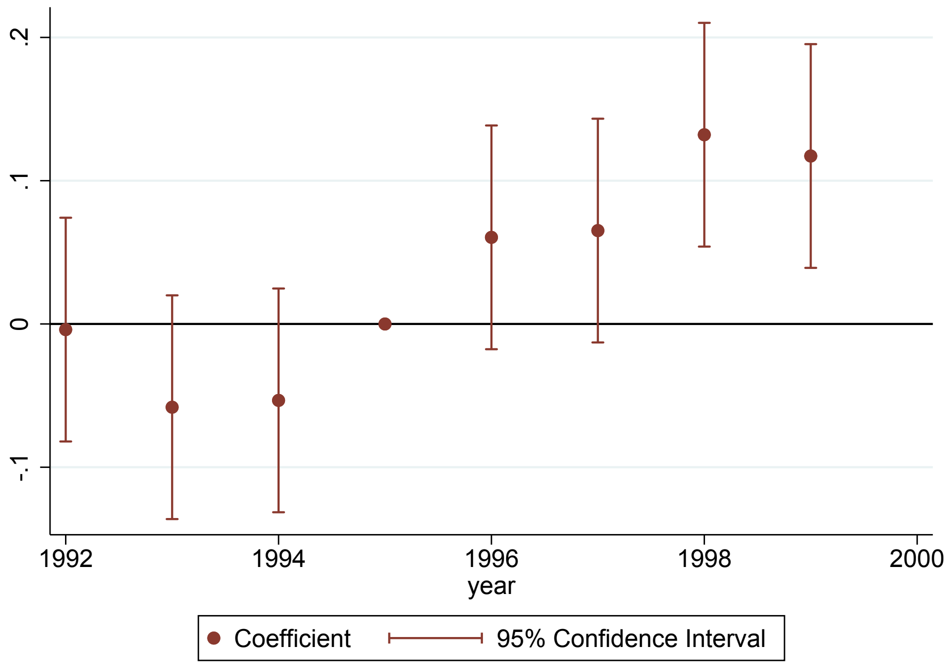
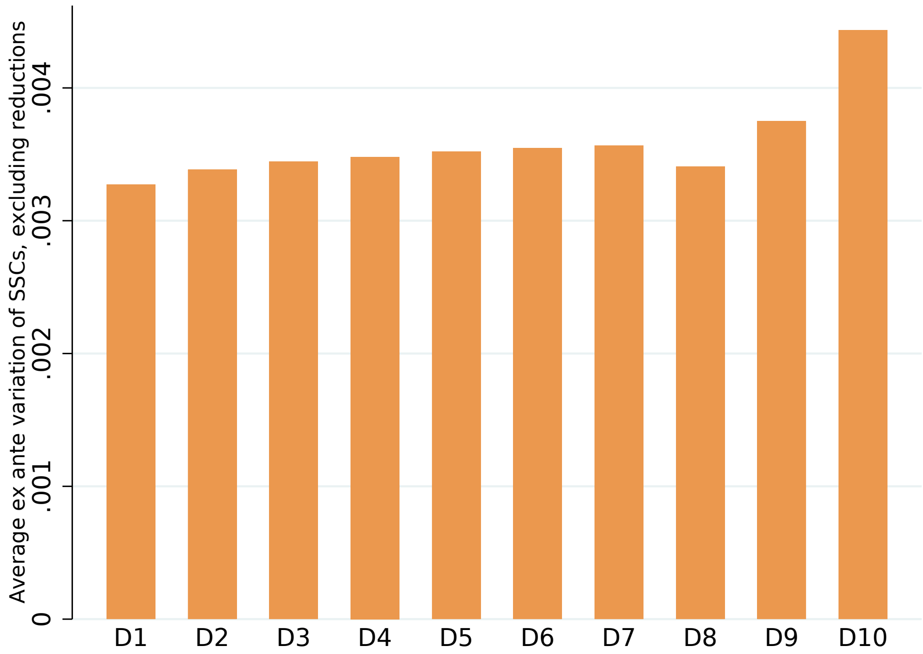


Figure A1: Difference in differences: a larger treatment group

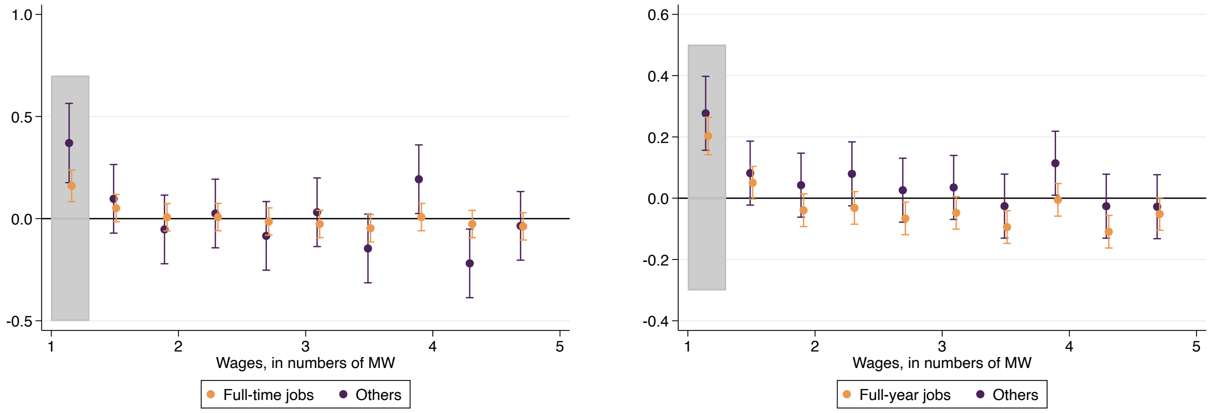
Note: This figure plots the difference-in-differences estimates of equation 2 using a larger treatment group.
Source: Sample version of the employer-employee data (*DADS Panel*).

Figure A2: Average change in employer SSCs, excluding reductions on low wages, by weighted deciles of ex ante change in labor cost

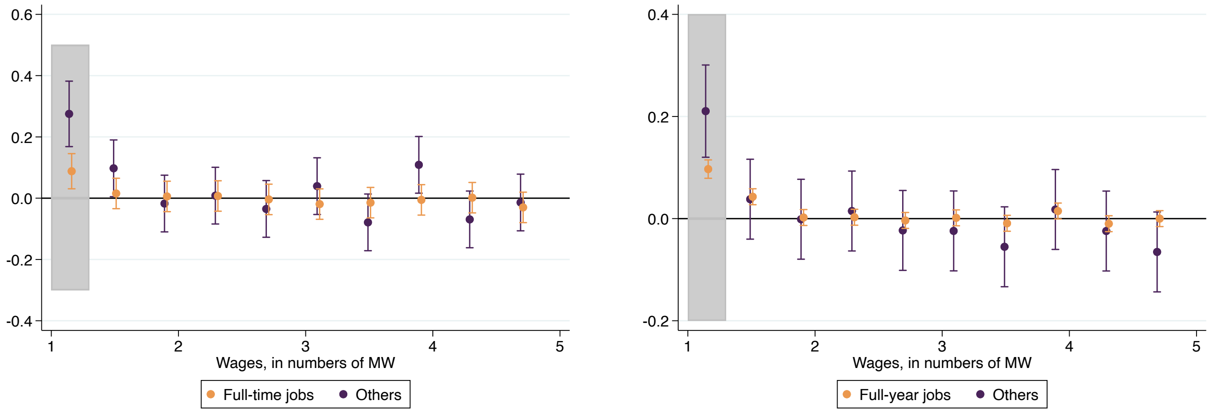


Note: The variable described is the average ex ante change in employer SSCs, applying the 1997 SSC schedule to the 1995 characteristics of the firms in the sample, to the exclusion of potential SSC reductions. Firms are ranked by their value of ex ante change in labor cost. Deciles are weighted by total full-year equivalent employment in 1995. The sample described is the 1994-1999 balanced sample of firms with at least 2 full-year equivalent employment in 1995.

(a) Total number of jobs



(b) New jobs



(c) Continuing jobs

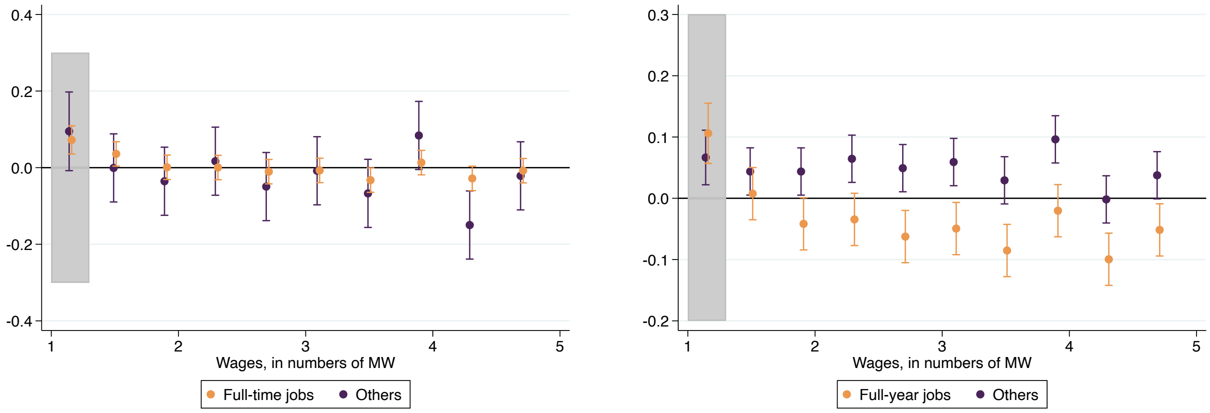
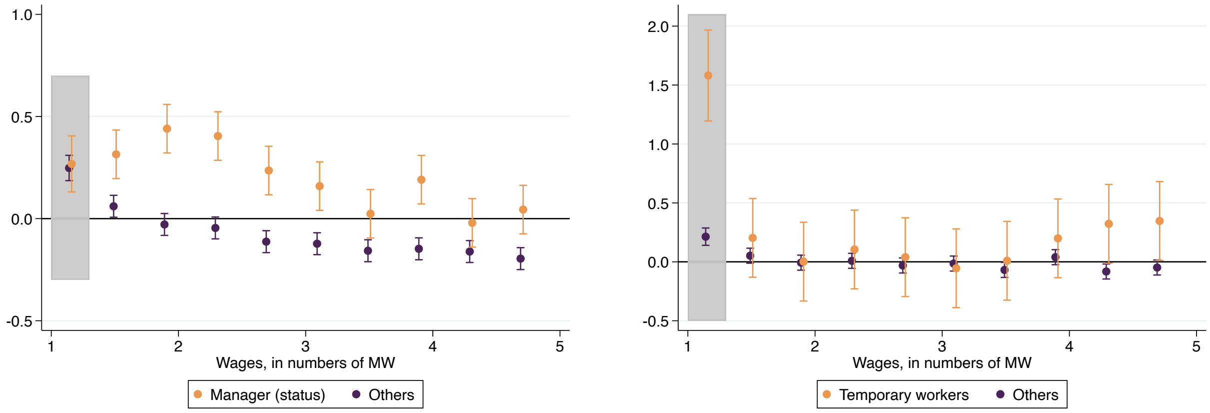


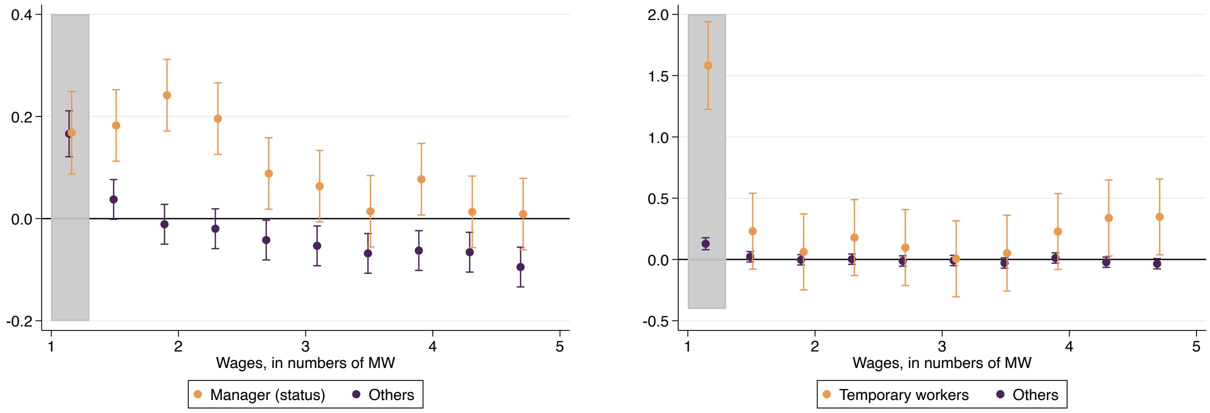
Figure A3: Simple difference estimates, heterogeneity: full-time and full-year jobs

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Regressions are run separately on four subsamples: full-time jobs *vs.* part-time jobs, full-year jobs *vs.* part-year jobs. The numbers of jobs in each bin are normalized by the number of jobs in this bin in 1995 in the particular subsample they relate to. Only coefficients corresponding to the year 1998 are plotted. New jobs are jobs that are observed in *N* but not in *N-1*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*.
Source: Exhaustive employer-employee data (*DADS Postes*).

(a) Total number of jobs



(b) New jobs



(c) Continuing jobs

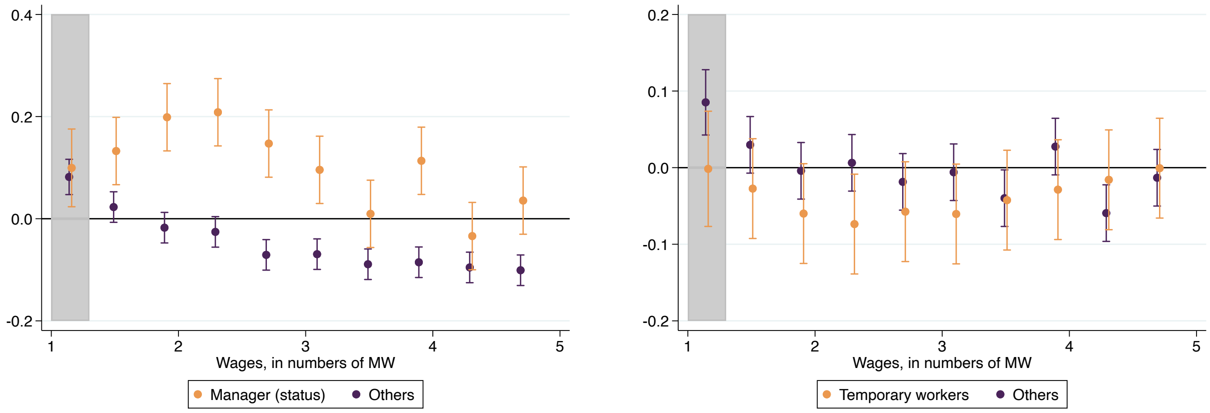


Figure A4: Simple difference estimates, heterogeneity: manager status and temporary workers

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. Regressions are run separately on four subsamples: jobs with *vs.* without a "manager" status, and temporary *vs.* regular workers. The numbers of jobs in each bin are normalized by the number of jobs in this bin in 1995 in the particular subsample they relate to. Only coefficients corresponding to the year 1998 are plotted. New jobs are jobs that are observed in *N* but not in *N-1*. Continuing jobs are jobs that are observed both in *N* and in *N-1*. The wage bin is that of the wage observed in *N*.
Source: Exhaustive employer-employee data (*DADS Postes*).

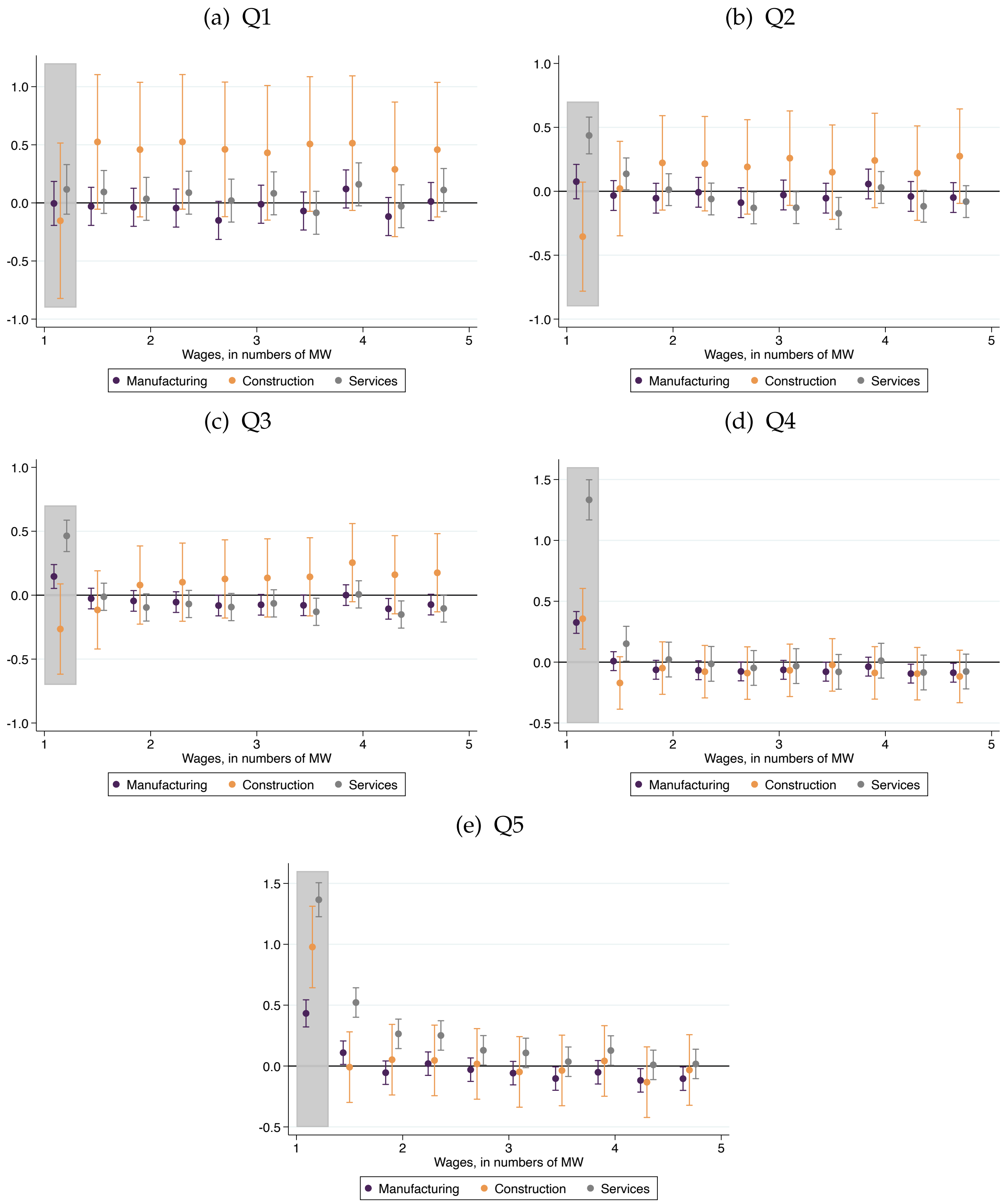
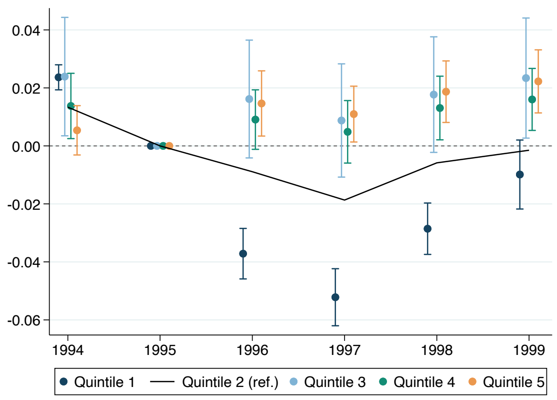


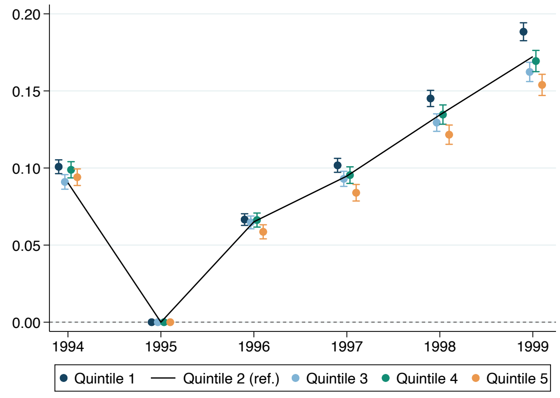
Figure A5: Simple difference estimates: All jobs, by firms' quintile, decomposing by sectors

Note: These figures plot the $\beta_{b,t}$ estimates of the simple differences regression given by equation 1 for each bin b and each year t, with a normalized measure of the number of jobs in a given wage bin and a given year as the dependent variable. For each quintile, regressions are run separately on three subsamples: the manufacturing industry, services and the construction sector. The numbers of jobs in each bin are normalized by the number of jobs in this bin in 1995 in the particular subsample they relate to. Only coefficients corresponding to the year 1998 are plotted.
Source: Exhaustive employer-employee data (*DADS Postes*).

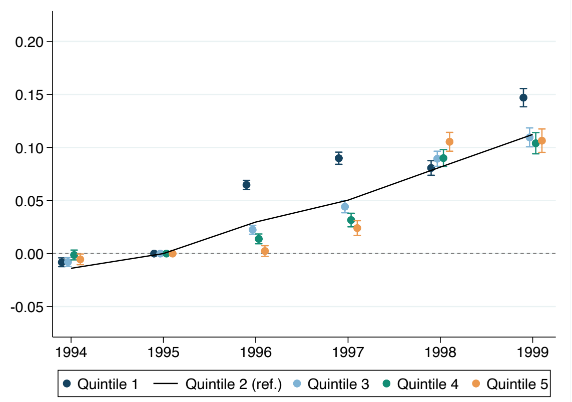
(a) Differential growth rate of the ratio of social charges in total labor costs



(b) Differential probability of attrition



(c) Differential growth rate of full-year equivalent employment



(d) Differential growth rate of full-year equivalent employment accounting for attrition

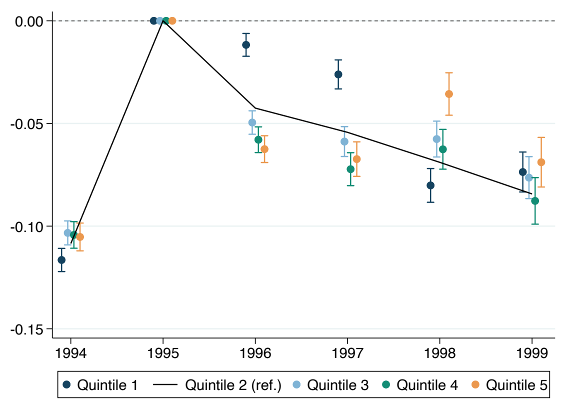
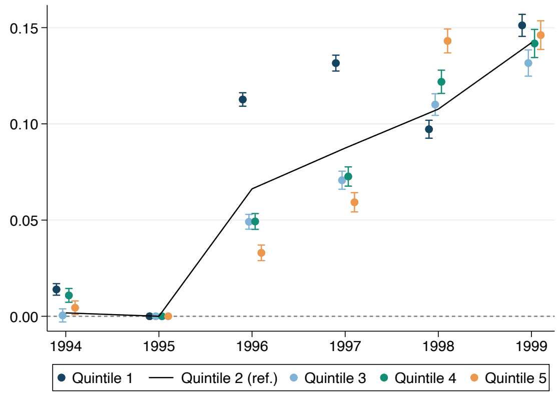


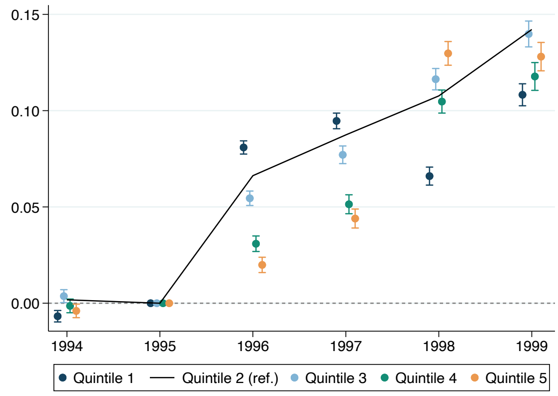
Figure A6: Firm-level effects of changes in payroll taxes: Using an unbalanced sample

Note: These figures represent the β coefficients estimated for the regression of equation 4 for three dependent variables: the cumulative growth rate of the ratio of social charges in total labor costs (Figure A6a), attrition (Figure A6b), and the cumulative growth rate of full-year equivalent employment (Figures A6c and A6d). In Figure A6d), full-year equivalent employment is corrected for attrition, assuming a -100% growth rate when the firm is not observed in the data. For each year, these graphs plot the sum of the β coefficients and the average of the dependent variable for the reference group (Quintile 2) for this year. In Figure A6c, the unconditional average growth rate of full-year employment in firms of Quintile 2 between 1995 and 1997 is 5%, and controlling for year-sector fixed effects, year, firm and size-year fixed effects, the 1995-1997 growth rate for firms in Quintile 1 is 4.0 percentage points higher compared to Quintile 2. The regression is estimated on the unbalanced panel of firms with at least 10 full-year equivalent employment in 1995 (N = 137853, 138316, 137471 and 138316, respectively). Standard errors are clustered at the firm level. The vertical bars represent the 95% confidence interval.
Source: Fiscal and employer-employee data (FICUS, DADS Postes).

(a) Differential growth rate of full-year equivalent employment: Firms with more than 2 full-year equivalent employment in 1995



(b) Differential growth rate of full-year equivalent employment: Firms with more than 2 full-year equivalent employment in 1995, adding size-year fixed effects



(c) Differential growth rate of full-year equivalent employment: Firms with more than 10 full-year equivalent employment in 1995, adding size-year fixed effects

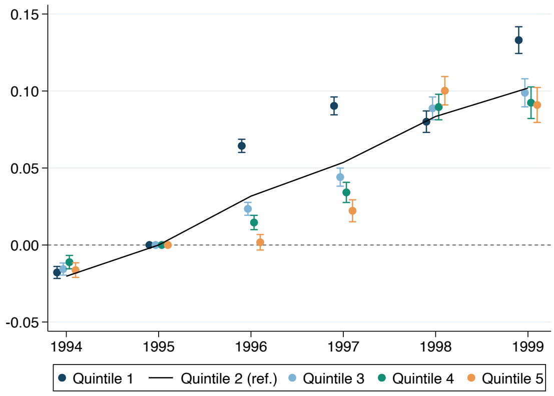


Figure A7: Firm-level effects of changes in payroll taxes: Accounting for initial firm size

Note: These figures represent the β coefficients estimated for the regression of equation 4, with the cumulative growth rate of full-year equivalent employment as the dependent variable. In Figures A7b and A7c, I control for the interaction of firms' initial size group and year. In Figures A7a and A7b, the regression is estimated on the balanced panel of firms with at least 2 full-year equivalent employment in 1995 (N = 350352 and 350352). In Figure A7c, the sample is restricted to firms with more than 10 full-year equivalent employment in 1995 (N = 96654). For each year, these graphs plot the sum of the β coefficients and the average of the dependent variable for the reference group (Quintile 2) for this year. In Figure A7b, the unconditional average growth rate of full-year employment in firms of Quintile 2 between 1995 and 1997 is 9%, and controlling for year-sector fixed effects, year, firm and size-year fixed effects, the 1995-1997 growth rate for firms in Quintile 1 is 0.7 percentage points higher compared to Quintile 2. Standard errors are clustered at the firm level. The vertical bars represent the 95% confidence interval.
Source: Fiscal and employer-employee data (FICUS, DADS Postes).

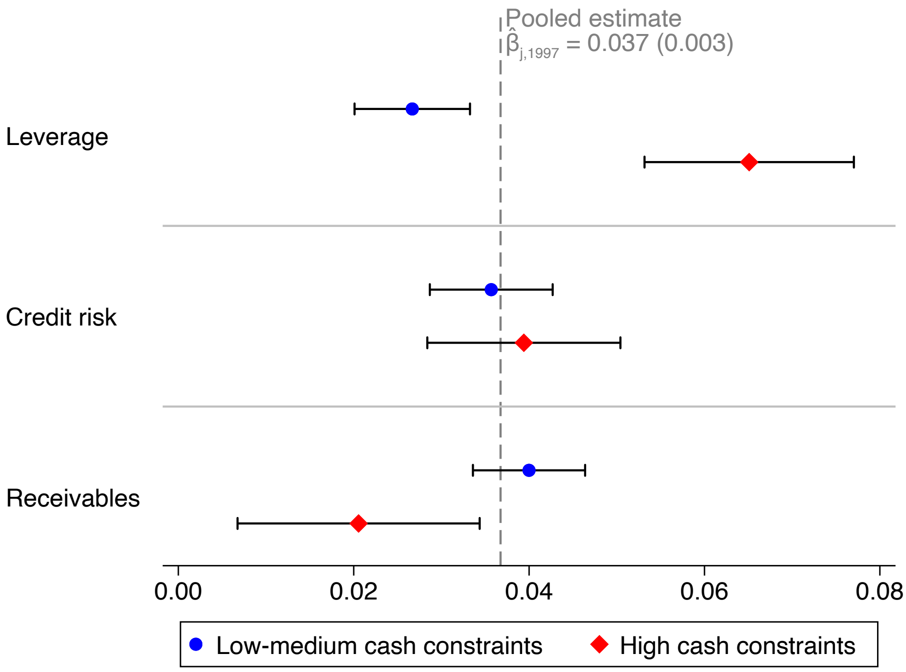


Figure A8: Heterogeneity of Q1 firm-level effects with respect to cash constraints: 1997 coefficients

Note: This figure plots the $\beta_{1,1997}$ coefficients of equation (4), with additional size–year fixed effects, estimated on sub-samples of firms with respect to their level of cash constraints. Firms with high cash constraints rank in the top quartile of the cash constraints proxy variable. Firms with low to medium cash constraints are those that rank in the first three quartiles. Standard errors are clustered at the firm level. The vertical bars represent the 95% confidence intervals. In 1997, the employment effect on firms in Q1 relatively to Q2 is 0.065 on the subsample of firms with the highest cash constraints as measured by leverage, compared to 0.026 for the subsample of firms with low to medium cash constraints according to this measure. On the whole sample, this coefficient is estimated to be 0.037.
Source: Fiscal and employer-employee data (FICUS, DADS Postes).