

Alternative Work Arrangements and Worker Outcomes: Evidence from Payrolling*

Maarten Goos
Utrecht University

Alan Manning
London School of Economics

Anna Salomons
Utrecht University

Bas Scheer
CPB

Wiljan van den Berge
Utrecht University & CPB

February 2022

Abstract

The rising incidence of alternative work arrangements raises questions about worker outcomes in non-standard labor contracts. We study this question in the Netherlands, a country which has seen a rapid rise in flexible labor contracts, using administrative employer-employee data over 2006–2019. To identify the impact of alternative work arrangements, we exploit a legal work arrangement called “payrolling”, whereby workers hired by one firm can be put on the payroll of another firm while continuing their job duties at the original firm. We find that workers on payrolling contracts experience worse labor market outcomes compared to a matched control group following their switch to a payrolling contract, including lower hourly wages, lower employment probability, and lower pension contributions. This suggests alternative work arrangements reduce employment protection and job quality for workers.

Keywords: Alternative work arrangements, Outsourcing, Labor contracts

JEL: J31, J32, J41, J42

*We thank Egbert Jongen, Pierre Koning, Ioana Marinescu and seminar participants at ASSA and CPB for helpful comments. Goos, Salomons, and Van den Berge gratefully acknowledge support from Instituut Gak. Scheer and Van den Berge gratefully acknowledge funding from the Dutch Ministry of Social Affairs and Employment.

1 Introduction

Modern-day labor markets are witnessing a growing incidence of non-standard contracts, including the use of outsourced labor hired through (sub)contractors and temporary help agencies. This rise of alternative work arrangements raises important questions about worker outcomes in such contracts (Katz and Krueger, 2019).

However, the causal impacts of alternative work arrangements on workers have proven difficult to study due to both data limitations and concerns about exogeneity. Measuring alternative work arrangements in survey data is challenging because surveys do not accurately capture several key aspects of alternative work arrangements, including smaller jobs, multiple-job holding, working as a contractor, and work on online platforms (Abraham and Amaya, 2019; Abraham et al., 2018; Katz and Krueger, 2019). The advantage of administrative matched employer-employee datasets is that small and multiple jobs are typically also measured. However, administrative datasets typically do not register worker-firm matches for workers hired as outsourced or contracted labor. As such, one cannot be sure that workers moving from a regular contract to an alternative work arrangement (e.g. to a temporary help agency or outsourced to a business service firm) remain effectively employed at the original employer. One important exception is Drenik et al. (2020), who measure at the firm-level which workers are hired through temp agencies. Goldschmidt and Schmieder (2017) were the first to address this issue by considering groups of workers within the same occupation (e.g. cleaners) moving to a business service firm in the same location as the originating firm. But even in this case there is no guarantee that workers continue to work for the same firm after being outsourced. A final important concern is that workers on alternative work arrangements could be negatively selected. Indeed, Drenik et al. (2020) show that workers at temp agencies have lower *worker* fixed effects, as measured by Abowd et al. (1999)-style models (henceforth AKM). Additionally, Katz and Krueger (2019) find that after controlling

for observables, the wage penalty associated with temp agency work declines.

We study alternative work arrangements in the Netherlands, a country which has seen a rapid rise in flexible labor contracts: as a result, the Netherlands has one of the highest incidences of temporary work in Europe (along with Poland, Portugal, and Spain), comprising over 20 percent of the working age population. This makes the Netherlands an interesting setting to study worker outcomes in alternative work arrangements.

To overcome the measurement and identification issues outlined above, we exploit a legal work arrangement called “payrolling”, whereby workers hired by a firm (‘firm A’) can be put on the payroll of another firm (‘firm B’). This payroller (firm B) takes over the labor contract while the worker continues to work at their original employer, firm A. The payroller becomes the formal employer of the worker and not only takes care of the payrolling administration, but is also responsible for pension contributions, sickness benefits, and layoffs. Crucially, payrolling firms operate under weaker labor law regulations than regular firms, such that firms can save on costs related to layoffs or pension contributions by moving workers to a payrolling contract. Payrolling firms are covered by the same labor law regulations as are temporary help agencies. However, unlike temporary help agencies, the payrolling firm does not offer worker recruitment or training: it merely takes over preexisting employer-employee relationships while affording the employer more contractual flexibility.

We use administrative employer-employee data for each quarter from 2006–2019, where we observe about 90,000 workers in total switching from a regular labor contract to a payrolling one. We construct a matched control group of non-payrolled workers employed at firms that also use payrolling, but at some later point in time. This allows us to circumvent potential selection into the use of payrolling by firms, and establish a credible counterfactual for workers moving to an alternative work arrangement.

We find that upon being moved to a payrolling firm, workers suffer a decline in total wage earnings of around 6.5% relative to the control group. These losses are long-lasting, as it

takes payrolled workers around three years to attain parity again. Part of these income losses are driven by non-employment: payrolled workers are about 3 percentage points more likely to end up in non-employment and be reliant upon unemployment benefits. This likely reflects the lower employment protection for payrolled workers. Among those who remain employed, hourly earnings decline by some 2% compared to the control group, and it takes about six quarters before they catch up. Payrolling firms have lower pay premiums as measured by the AKM fixed effects, and as a result payrolled workers receive only 70% of the pay premium that non-payrolled workers earn. The strongest impacts for payrolled workers are on less visible dimensions of the employment relationship. First, even after three years they are 10 percentage points less likely than the control group to be on a permanent contract that offers strong employment protection and job security. Second, payrolled workers receive much lower pension contributions, with a decline of around 90% in the quarter after payrolling and taking three years to catch up to the control group. The evidence suggests that payrolled workers catch up to the control group mostly through job switching, rather than through conditions improving at the payrolling firm.

We contribute to the literature studying alternative work arrangements in several ways. First, in contrast to other papers studying alternative work arrangements (e.g. [Drenik et al. \(2020\)](#); [Goldschmidt and Schmieder \(2017\)](#)), because of legal restrictions on payrolling contracts, we can be certain that workers put on a payrolling contract continue to perform the same duties before and after payrolling. Second, each firm in the Netherlands has access to payrolling, so our measure of alternative work arrangements in principle covers the entire labor market. Therefore, we can study alternative work arrangements in a broader context than earlier studies looking at specific occupations such as cleaners or security guards ([Dube and Kaplan, 2010](#); [Felix and Wong, 2021](#); [Goldschmidt and Schmieder, 2017](#)). In practice we find payrolling predominantly occurs in the low-wage labor market, such as in retail and food services. Third, we apply a staggered difference-in-differences design exploiting the timing

of payrolling events at the firm-level: this is important for identification if firms that make use of alternative work arrangements are selected along unobservable dimensions.

Our study relates to a growing body of evidence on the importance of monopsony power in modern labor markets (Manning, 2003, 2021). Recent studies measure monopsony power through labor supply elasticities to the firm (Dal Bó et al., 2013; Dube et al., 2020, 2018; Webber, 2015), or (regional) labor market concentration of firms (Arnold, 2020; Azar et al., 2019, 2020; Marinescu et al., 2021). Our study contributes a different perspective to this literature by showing the impacts of institutional features of the labor market on monopsony power. By allowing firms to circumvent the regular labor contracts and its protections for workers, that were largely put in place precisely to protect workers from firms' presumed bargaining power, legal work arrangements – like payrolling – erode worker's bargaining power. In this sense our work relates to papers studying other features of employment contracts that increase firms' monopsony power, including non-compete clauses (Lipsitz and Starr, 2021). Just as contracts with non-compete clauses, workers sign a payrolling contract voluntarily. However, they might not be fully aware of the consequences of signing the contract, or they might feel there is no reasonable alternative available to them.

The paper proceeds as follows. Section 2 outlines our data, including the Dutch institutional context of payrolling, and presents worker- and firm-level descriptive statistics. Section 3 explains our empirical approach, and section 4 presents our findings on the effect of payrolling on individual workers. Section 5 concludes.

2 Data and measurement

2.1 Institutional context

A dual labor market. Compared to other OECD countries, including France and Germany, the Netherlands has strong employment protection legislation for standard (“open-ended”) labor contracts yet weaker employment protection for temporary (i.e. ‘fixed-term’) contracts, as shown in Figures 1a and 1b. This protection includes protection against dismissal. Firms can only lay off workers on permanent contracts if the public employment office agrees that there is reasonable cause for the dismissal, such as long-term loss of work for the firm or inadequate performance of the worker. Firms are required to pay severance pay to workers on permanent contracts. Another option for firms is to request consent of the worker, which usually involves a higher severance payment. Firms also have a strong obligation for sickness payments: if workers become ill for a longer period, firms are required to pay 70% of the wage for two years before workers can be laid off. Further, Figure 2 shows that the incidence of temporary contracts is high at around 20% of employment (compared to around 12% for the OECD on average and 4% for the US in 2017), and has increased substantially over the past decades. This makes the Dutch labor market an interesting case for studying non-standard working arrangements.

Payrolling. One non-standard working arrangement specific to the Dutch setting offers an interesting case for empirical research: payrolling. Payrolling firms act as an intermediary on the labor market, and are very similar to temporary help agencies. They act as the formal (*de jure*) employer of workers, while workers perform their job duties at another firm (the *de facto* employer) that has hired the payrolling firm.

In terms of employment protection, payrolling is identical to temporary agency work in the period we study: both sectors fall under the same labor law and collective bargaining agreements during the period we study. Under these agreements, employers are allowed to

offer workers more back-to-back temporary contracts and with a longer cumulative length – up to 5.5 years in total, as compared to 3 years (pre-2015) and 2 years (post-2015) for standard employment contracts.¹ Further, workers can be dismissed without severance pay when the hiring firm decides the worker is no longer needed. This contrasts with employment protection in standard contracts as outlined in the previous paragraph.

However, unlike temporary help agencies, payrolling companies do not match workers and firms (Zwemmer, 2016): they take as given the pre-existing worker-firm match but place the worker on their payroll, instead of the original firm's. This resolves a common identification problem in the literature studying alternative work arrangements such as temporary help agency work and outsourcing, namely that workers in these arrangements cannot be linked to the firm where they actually do their work. In the case of payrolling, by contrast, the worker legally has to remain employed and work only at the original firm, implying that we can observe the worker, the firm that effectively employs them, as well the payrolling firm which is providing the employment contract.

These aspects of payrolling firms are reflected in the way they advertise themselves. For example, one of the largest payrolling companies in the Netherlands stated on their website in 2015 (cited in Zwemmer (2016)):

“With Payroll Services, you delegate the personnel and wage administration to Payroll Select. Your employees are on our wage list, with all of the benefits this brings.

- *Fewer employer risks: With payrolling, your workers are employed by Payroll Select. Because we are formally the employer, you face significantly fewer risks under employment law in relation to employment contracts, continued wage payments, probationary periods, illness etc.*

¹The change in 2015 reflects the introduction of the “Wet Werk en Zekerheid” (WWZ), which introduced a lower maximum number of consecutive temporary contracts. At the same it reduced the maximum length that workers could receive unemployment benefits.

- *Dealing flexibly with personnel: Payrolling your staff via Payroll Select means that you are not tied down to employment contracts. This gives you the freedom to respond to market developments with greater flexibility*²

2.2 Data sources

We use administrative employer-employee records from Statistics Netherlands, covering the universe of workers and firms in the Netherlands over the period 2006–2019.³ The raw data measure all jobs at a monthly level, where a job is defined as a worker-firm observation. We collapse the monthly data to the quarterly level. For workers observed in multiple jobs simultaneously, we only retain the job providing the main source of income in each quarter. We use total earnings across all jobs as the main measure of wage income. All earnings are deflated using the CPI relative to 2015. Based on the main employer’s sector code, workers are grouped in payrolling, temporary agency work and other sectors.⁴ For each worker we then merge administrative data on demographics from municipal registrations, social security benefits, education level⁵ and enrollment in education.

²Another large payrolling company mentions on their website that “*Payroll is possible for every company. As a company you recruit and select your own workers, but you outsource both the payroll administration and risks associated with being an employer. With payrolling you combine low costs, no risks in the case of illness and flexibility.*” (www.payrollplaats.nl, accessed on 9-12-2021)

³The data start in 2006, but 2009 is the first year in which payrolling firms can be separately identified in the data using a sector code.

⁴The sector code we use is the so-called “*Standaard Bedrijfsindeling 2008*” (SBI 2008) from Statistics Netherlands. The first 4 digits fully correspond to NACE Rev.2, and the first 2 digits fully correspond to ISIC Rev.4. The sector code for payrolling is 7830; the sector code for temporary help agencies is 7820.

⁵Due to limitations in the registrations we do not observe education level for everyone. In particular, older and/or lower-educated workers are less likely to be observed in the education-level administration.

2.3 The payrolling sector

Figure 3 shows the number of workers employed by payrolling firms over our data period, 2009–2019. While employment in payrolling firms is small overall, there is a striking rise over the past decade: the number of workers employed in payrolling arrangements as a share of all employment rises from 0.6% to 1.5% and the total number of workers on payrolling contracts has risen to about 140,000. Over the entire period, the number of workers employed in payrolling firms is around 15% of the number of workers employed in temporary help agencies.

Descriptives on payrolled workers. Table 1 describes the characteristics of workers employed in payrolling firms, as well as those employed in temporary help agency firms— a more familiar firm type which is a relevant comparison group—, and all other firms. This highlights that workers employed in payrolling firms represent the bottom of the labor market: their average hourly wages are 12.22 euros, substantially lower than the average hourly wage of 21.63 euros, and similar to the 12.82 euro hourly wage of workers employed by temporary help agencies. The lower hourly wages are consistent with negative AKM firm fixed effects for both payrolling and temporary help agencies. Workers in payrolling earn about 16% less than comparable workers at other firms. Workers in payrolling firms work a lower number of annual hours, working 154 fewer hours annually than temporary help agency workers, and slightly more than half those of other workers. Close to 90% of workers in payrolling firms and in temporary help agencies are employed in temporary contracts (with similar firm tenures), as opposed to 28% for all other workers. Workers in payrolling firms are lower-educated than workers employed outside of temporary help agency firms. Lastly, workers in payrolling arrangements are younger, but more likely to be female and less likely to have a migration background as compared to temporary help agency workers.

Descriptives on payrolling firms. Table 2 compares the characteristics of payrolling

firms to those of temporary help agency firms and all other firms. Consistent with the worker-level descriptives, payrolling firms pay lower annual and hourly wages on average. This is also reflected in their lower AKM firm fixed effect. The most striking difference is that payrolling firms typically employ substantially more workers (480 on average) than do temporary help agency firms (141 on average) and other firms (18 on average).

Where do payrolled workers come from? Table A1 shows the sectors payrolled workers originate from, as well as the share payrolled workers make up in total sectoral employment. Sectors are ranked by this share, such that the ones where payrolling is more common are at the top of the table. This highlights that a broad range of firms payroll their workers, but it is most frequent in Food and beverage establishments, Hotels and other accommodations, and Other business services. In terms of the absolute number of workers, Retail, except automobiles is the second-largest contributor: Table A2 provides further sectoral detail within this broad category of retail, revealing that supermarkets and department stores are the main tributary for payrolled workers. Table A1 also shows many payrolled workers originate in facility management, which includes catering, cleaning and landscaping services.⁶

3 Empirical approach

We estimate the impact of payrolling on individual worker's outcomes using a stacked difference-in-differences design. In this section we describe our approach and identifying assumptions.

⁶Using a smaller sample from the Dutch Labor Force Survey (about 1% of the Dutch Labor Force in a rotating panel) from 2012 and later, we can also consider the occupations of payrolling workers. Workers in payrolling are mostly employed as services and sales workers (41.6%), where occupations in restaurants and bars such as waiters are most common. Other large occupation groups are elementary occupations (16.1%) and professionals (13.1%).

Treatment group. We measure a payrolling event as a move of a worker from employment in a non-payrolling firm in quarter t to employment in a payrolling firm in quarter $t + 1$. Dutch labor law ensures that workers who are moved to a payrolling firm in this way continue to perform the same tasks at the firm they came from, but are now formally employed by the payrolling company. To exclude cases where workers were first laid off or voluntarily left the non-payrolling firm, and then found employment at a payrolling firm, we apply some additional restrictions. We exclude workers who receive unemployment benefits around the event, who have more than a one-month non-employment gap in between the two contracts, or who have more than 3 months' overlap between origin and payrolling firm jobs. These workers are our treated sample. We have 45,238 treated workers.

Control group. The control group consists of matched workers employed in t in firms that also payroll at some point in the future, but are not payrolled themselves in t . This setup is similar to the displaced worker literature (e.g. [Jacobson et al., 1993](#)) where workers displaced in t are compared to a control group of workers not displaced in t , except that we also impose that the control group workers can only come from firms that use payrolling at some point to control for potential unobservables associated with using payrolling as a firm.

Stacked difference-in-differences. We set up our data in a stacked difference-in-differences design as in e.g. [Cengiz et al. \(2019\)](#).⁷ More specifically, we create separate datasets for each cohort of workers that have their first payrolling event in quarter t , with $t \in \{2009Q1, \dots, 2016Q4\}$. Define τ as event time, i.e. calendar quarter (t) minus the calendar quarter c in which the worker is payrolled ($\tau \equiv t - c$). In each dataset we keep $\tau \in \{-11, \dots, 13\}$ as our event window. Then for each dataset, we add observations for the same calendar quarter on all workers that work in quarter t at a firm that also payrolls at some point in the future, but are not payrolled themselves. For example, our first cohort of treated workers

⁷For other recent papers using this setup, see for example [Baker et al. \(2021\)](#); [Bessen et al. \(2019\)](#); [Clemens and Strain \(2021\)](#); [Deshpande and Li \(2019\)](#); [Goldschmidt and Schmieder \(2017\)](#)

are workers that are payrolled in the first quarter of 2009. The event window surrounding this event contains the calendar quarters from the first quarter of 2006 to the first quarter of 2012. All potential control workers for the 2009Q1-cohort are those workers that we observe over the same event window (2006Q1 to 2012Q1) and that are not payrolled in 2009Q1 or earlier. We repeat this procedure for each cohort of firms. Finally, we stack the cohort-specific datasets so that they line up in terms of event time $\tau \in \{-11, \dots, 13\}$. We ensure that workers can only be included as treated or control once (namely, the first time they are observed), and are not used as control after being treated.

Note that the by now well-documented problems related to two-way fixed effects in an event study design arise from staggered treatment timing (Callaway and Sant’Anna, 2020; de Chaisemartin and D’Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). By creating a balanced panel in event time, we have effectively eliminated the staggered timing in the data, and hence do not suffer from the issues that staggered timing may create. Most notably, we do not use “already-treated” units as control units. Baker et al. (2021) show that a stacked difference-in-differences setup recovers the true treatment effects in the case of staggered timing, just as the Callaway and Sant’Anna (2020); Sun and Abraham (2021) approaches do.⁸

Matching. We have many potential control workers, so we apply matching to find the set of workers who are most comparable to our treated workers. Specifically, within each cohort c , we match treated and control group workers on average hourly wage and average quarterly hours worked over the past three years using coarsened exact matching, gender, 1-digit sector of origin and tenure at the origin firm.⁹ We restrict each treated worker to

⁸As a robustness check we also setup our data for the Callaway and Sant’Anna (2020) approach and find similar results.

⁹For hourly wage we use deciles separately per year and a separate bin for 0 earnings and a separate bin for the 99th percentile and above. For quarterly hours we use four groups per year (0 hours, 1-200 hours, 201-400 hours and 401-800 hours per quarter). For tenure we use 3 groups: 0 - 3 quarters, 4 - 7 quarters

have at most 5 matches, and weight each matched control by $\frac{1}{n_i}$, with n_i being the number of controls for treated worker i . On average we can find at least one match for 82% of treated workers.¹⁰ After matching we have 37,083 treated and 170,668 control workers originating from, respectively, 19,450 and 39,241 firms. We have 320 distinct payrolling firms that employ our treated workers after the payrolling event. Figure A1 shows for each event quarter the number of payrolled workers, origin firms and payrolling firms involved in the events we use for estimation. Worker-level descriptives on the matched sample are reported in Table 3.

Estimating equation. On the matched sample we estimate the following difference-in-differences model:

$$Y_{it} = \alpha + \sum_{\tau \neq -1; \tau = -11}^{13} \beta_{\tau} \times I_{\tau} + \sum_{\tau \neq -1; \tau = -11}^{13} \delta_{\tau} \times I_{\tau} \times \text{payrolled}_i + \eta_i + \theta_t + \varepsilon_{it}, \quad (1)$$

where i subscripts individual workers, t denotes calendar time in quarters, and τ is event-time in quarters relative to the payrolling event. I_{τ} are event time indicator dummies, treat_i is a treatment dummy that equals 1 if a worker is payrolled, η_i are individual fixed effects, and θ_t are calendar quarter fixed effects. Standard errors are clustered at the level of the firm the worker is employed at the quarter before treatment to account for within-firm correlation.

We define the first quarter a worker is observed as having the main job at a payrolling firm as $\tau = 1$. Workers still have their main job at the origin firm at $\tau = 0$ and are also required to have at least one more quarter of tenure at their origin firm. This means that the event typically already happens somewhere between $\tau = 0$ and $\tau = 1$. We therefore take $\tau = 0$ as the first quarter of treatment and $\tau = -1$ as the reference quarter.

Identifying assumptions. We require two assumptions for a causal interpretation of

and more than 7 quarters. This reflects that most of our sample consist of relatively young workers with relatively short tenure.

¹⁰If we apply less restrictive matching criteria to increase the share of matched treated workers, the quality of the match becomes poorer, but the results do not qualitatively change.

δ_τ (e.g. [Borusyak et al., 2021](#); [Callaway and Sant’Anna, 2020](#)). First, we need that treated and control workers follow parallel trends in absence of treatment. We provide evidence for this assumption by showing that pre-event trends are similar for workers that are payrolled compared to matched workers that are not payrolled. Second, we need that workers do not anticipate the payrolling event, i.e. there should not be an effect of treatment in the future on current outcomes (e.g. [Abbring and Van Den Berg, 2003](#)). By focusing on workers that were already employed for at least one quarter at the firm before being payrolled, we ensure that workers are not hired with the immediate intention of putting them on a payrolling contract. Anticipation is therefore less likely. In a heterogeneity analysis we distinguish workers by tenure at the originating firm and find that most of the results are very similar across different lengths of tenure.

4 Payrolling and worker outcomes

We start our analysis by looking at the impacts of payrolling on labor income and employment. In a second step we turn to secondary employment conditions, such as pension contributions and contract type, which proxies for employment protection and insurance against long-term sickness or disability.

4.1 Impacts on income and employment

Figure 4 shows that payrolled workers experience a decline in quarterly total wage earnings compared to the control group of around 150 euros in the event quarter. In subsequent quarters, their income slowly converges back to the same level as that of the control group, but it does not attain parity even after three years. These wage income losses are non-negligible since these workers earn around 3,300 euros per quarter prior to moving to a payrolling contract.

These income losses could come from various sources: a decline in employment (at the extensive and/or intensive margins), as well as a decline in hourly wages. Figure 5 shows that payrolled workers are 3 percentage points less likely to be employed than control group workers: these effects are sizable given that the control group non-employment rate is around 15% after three years, as well as long-lasting. These lower employment probabilities are also reflected in a higher unemployment benefit receipt, as shown in Figure 6. These extensive margin effects emerge relatively quickly after the payrolling event, and peak after 8 to 10 quarters afterwards.¹¹

Hours worked also decline, but not as strongly nor as persistently as employment probabilities: after 4 quarters, there is no longer a statistically significant difference with the control group, as shown in Figure 7b. This implies that as a result of being payrolled, workers who remain employed increase hours worked— an intensive margin effect. This is confirmed by the estimates for log hours worked presented in Figure 8b: after an initial fall of around 8% relative to the treatment group, hours worked among those who are employed are 5% higher for payrolled than control group workers three years after the payrolling event.

Finally, payrolling leads to a persistent decline in hourly wages of around 0.5 euros, seen in Figure 9. Note that this estimate includes zeros: this persistence is in fact driven by the long-time negative effect on employment status we have established above. However, even among those who remain employed, payrolling leads to an immediate decline in hourly wages of 2% compared to the control group, and it takes employed payrolled workers around a year and a half to catch up to their counterparts who were not payrolled. This contrasts with [Goldschmidt and Schmieder \(2017\)](#) who focus on longer-tenured workers outsourced to business service firms in Germany and find persistent negative effects on wages of about 10% lasting up to 10 years after the event.

¹¹The impacts we find are not driven by firm downsizing or changes in worker composition at the firm around the payrolling event (see Figures A2 and A3).

The negative impacts on hourly wages are reflected in impacts on AKM firm fixed effects (Figure 11). Workers who are payrolled suffer an immediate decline in their AKM fixed effect of about 4 percentage points. This means that payrolled workers on average get about 70% of the firm wage premium that other workers at the firm get. This is in line with recent findings by [Drenik et al. \(2020\)](#) on temp agencies, who find that temp agency workers in Argentina get about half of the firm wage premium that other workers get, and [Goldschmidt and Schmieder \(2017\)](#) on business service firms. Since AKM fixed effects are constant within firms, the gradual catching up we observe occurs through job mobility. This highlights that job mobility plays an important role in recovering from the losses workers suffer when being payrolled.

All in all, these results suggest that workers pay a substantial price for this alternative work arrangement: payrolling leads to a lasting higher incidence of non-employment, and at least in the short term, employed workers are working more hours for a lower hourly wage. Job mobility appears to be a channel through which workers catch up to control group workers.

4.2 Impacts on secondary employment conditions

Besides immediate financial consequences through wages and employment, alternative work arrangements may also impact secondary employment conditions because workers have less employment protection or insurance against sickness, or through lower pension payments.

We find that payrolled workers have a much lower chance of being employed in a permanent contract: Figure 12 shows this difference is more than 30 percentage points in the event quarter and remains a statistically significant and sizable 10 percentage difference after three years. This means that payrolled workers are facing a long-lasting decline in employment protection, as this is largely provided by permanent contracts in our setting.

Payrolled workers also accrue lower pension contributions from their employer: this is the case because payrolling firms are not required to make these contributions in the first 6 months of the contract and are only required to pay pension contributions for workers 21 years and older. Differences in pension contributions between payrolled workers and the control group decrease only slowly over time, equalizing after three years.

These striking adverse impacts on secondary employment conditions suggest workers either may not be fully aware of the consequences of signing the payrolling contract, or may feel there is no reasonable alternative available to them. Either way, it highlights how alternative work arrangements such as payrolling can erode worker bargaining power.

4.3 Effect heterogeneity

Finally we consider heterogeneity in impacts of payrolling by worker characteristics. For each subgroup we estimate the following simplified version of our baseline model where we summarize the total impact in one coefficient γ :

$$Y_{it} = \alpha + \gamma \text{post}_\tau \times \text{payrolled}_i + \eta_i + \theta_t + \varepsilon_{it}, \quad (2)$$

with post_τ a dummy that equals 1 if $\tau \geq 0$ and 0 otherwise and the other terms as defined before. Our coefficient of interest is γ , which gives the difference-in-differences estimate of being payrolled for a subgroup.

Figure 14 shows that the impacts on total earnings – including zeros – are largest for older workers and those with high education. Presumably these impacts are at least partly driven by the fact that these workers had the highest level of earnings before being payrolled. In contract, impacts on employment status not markedly different across subgroups (Figure 15).

The estimated overall impacts mask large heterogeneity between groups when we consider

outcomes conditional on having a job. First, the impact on log of total earnings is sharply negative for older workers, students, and those previously on a temporary contract, with declines up to 10% on average (Figure 16). In contrast, for minorities and workers previously on a permanent contract, the impacts are positive, up to about 5%. Strikingly, workers with a middle-level education (usually vocational education) seem to suffer stronger losses than workers with low or high levels of education. These patterns are partly driven by declines in hours worked conditional on having a job (Figure 17) and partly by declines in hourly earnings (Figure 18).

Finally, impacts on secondary employment conditions are also largest for workers on temporary contracts, students, those with middle-level education, and for older workers, with pension contributions up to 40% lower over the three years after payrolling for some groups (Figure 19).

5 Conclusion

Alternative work arrangements are increasingly common, including in the Netherlands, which has one of the highest incidences of flexible labor contracts in the OECD. To identify the impact of alternative work arrangements on worker outcomes we study “payrolling”, a legal work arrangement whereby workers hired by one firm are hired by a payrolling firm while continuing their original job duties. Like temporary help agencies, payrolling firms can offer labor contracts with increased flexibility, while ensuring the originally observed firm-worker match remains in place.

Combining rich administrative employer-employee data over 2006–2019 with a stacked difference-in-differences design exploiting the timing of payrolling events, we find that workers moving to payrolling contracts experience worse labor market outcomes compared to a matched control group, including lower hourly wages, lower hours worked, and lower pen-

sion contributions. Impacts appear to be largest for students, older workers, and workers previously on a temporary contract. This suggests alternative work arrangements reduce employment protection and job quality for workers. In a future version of the paper we will develop a theoretical model to understand under what conditions workers would consent to being payrolled, and when firms may find it profitable to do so.

References

- Abbring, J. H. and Van Den Berg, G. J. (2003). The nonparametric identification of treatment effects in duration models. *Econometrica*, 71(5):1491–1517.
- Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High Wage Workers and High Wage Firms. *Econometrica*, 67(2):251–333.
- Abraham, K. G. and Amaya, A. (2019). Probing for informal work activity. Working Paper 24880, National Bureau of Economic Research.
- Abraham, K. G., Haltiwanger, J. C., Sandusky, K., and Spletzer, J. R. (2018). Measuring the gig economy: Current knowledge and open issues. Working Paper 24950, National Bureau of Economic Research.
- Arnold, D. (2020). Mergers and acquisitions, local labor market concentration, and worker outcomes. Working paper.
- Azar, J., Marinescu, I., and Steinbaum, M. (2019). Measuring labor market power two ways. In *AEA Papers and Proceedings*, volume 109, pages 317–21.
- Azar, J., Marinescu, I., and Steinbaum, M. (2020). Labor market concentration. *Journal of Human Resources*, pages 1218–9914R1. Publisher: University of Wisconsin Press.

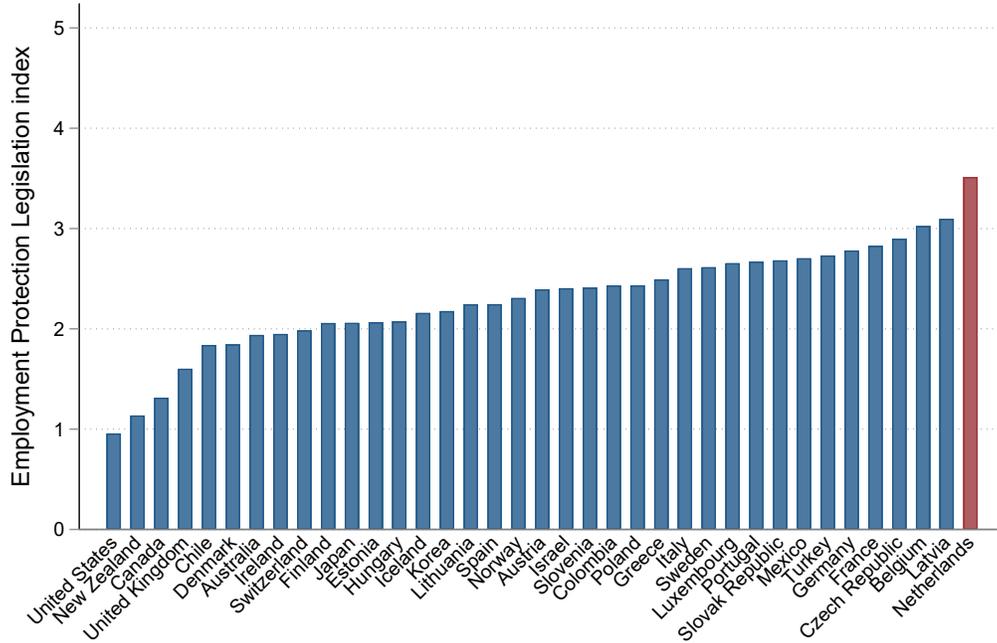
- Baker, A., Larcker, D. F., and Wang, C. C. Y. (2021). How much should we trust staggered difference-in-differences estimates? Technical report.
- Bessen, J., Goos, M., Salomons, A., and van den Berge, W. (2019). What happens to workers at firms that automate? Working paper, Boston University School of Law.
- Borusyak, K., Jaravel, X., and Spiess, J. (2021). Revisiting Event Study Designs: Robust and Efficient Estimation. Working paper.
- Callaway, B. and Sant’Anna, P. H. (2020). Difference-in-differences with multiple time periods. *Journal of Econometrics*.
- Cengiz, D., Dube, A., Lindner, A., and Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454.
- Clemens, J. and Strain, M. R. (2021). The heterogeneous effects of large and small minimum wage changes: Evidence over the short and medium run using a pre-analysis plan. Working Paper 29264, National Bureau of Economic Research.
- Dal Bó, E., Finan, F., and Rossi, M. A. (2013). Strengthening state capabilities: The role of financial incentives in the call to public service. *The Quarterly Journal of Economics*, 128(3):1169–1218.
- de Chaisemartin, C. and D’Haultfoeulle, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.
- Deshpande, M. and Li, Y. (2019). Who is screened out? application costs and the targeting of disability programs. *American Economic Journal: Economic Policy*, 11(4):213–48.
- Drenik, A., Jäger, S., Plotkin, M. P., and Schoefer, B. (2020). Paying outsourced labor: Direct evidence from linked temp agency-worker-client data. Working Paper 26891, National Bureau of Economic Research.

- Dube, A., Jacobs, J., Naidu, S., and Suri, S. (2020). Monopsony in online labor markets. *American Economic Review: Insights*, 2(1):33–46.
- Dube, A. and Kaplan, E. (2010). Does outsourcing reduce wages in the low-wage service occupations? evidence from janitors and guards. *ILR Review*, 63(2):287–306.
- Dube, A., Manning, A., and Naidu, S. (2018). Monopsony and employer mis-optimization explain why wages bunch at round numbers. Technical report, National Bureau of Economic Research.
- Felix, M. and Wong, M. B. (2021). Labor market consequences of domestic outsourcing: Evidence from legalization in Brazil. Technical report.
- Goldschmidt, D. and Schmieder, J. F. (2017). The rise of domestic outsourcing and the evolution of the German wage structure. *The Quarterly Journal of Economics*, 132(3):1165–1217.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277. Themed Issue: Treatment Effect 1.
- Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings Losses of Displaced Workers. *The American Economic Review*, 83(4):685–709.
- Katz, L. F. and Krueger, A. B. (2019). The rise and nature of alternative work arrangements in the United States, 1995–2015. *ILR Review*, 72(2):382–416.
- Lipsitz, M. and Starr, E. (2021). Low-wage workers and the enforceability of noncompete agreements. *Management Science*, 0(0):null.
- Manning, A. (2003). *Monopsony in Motion: Imperfect Competition in Labor Markets*. Princeton University Press.

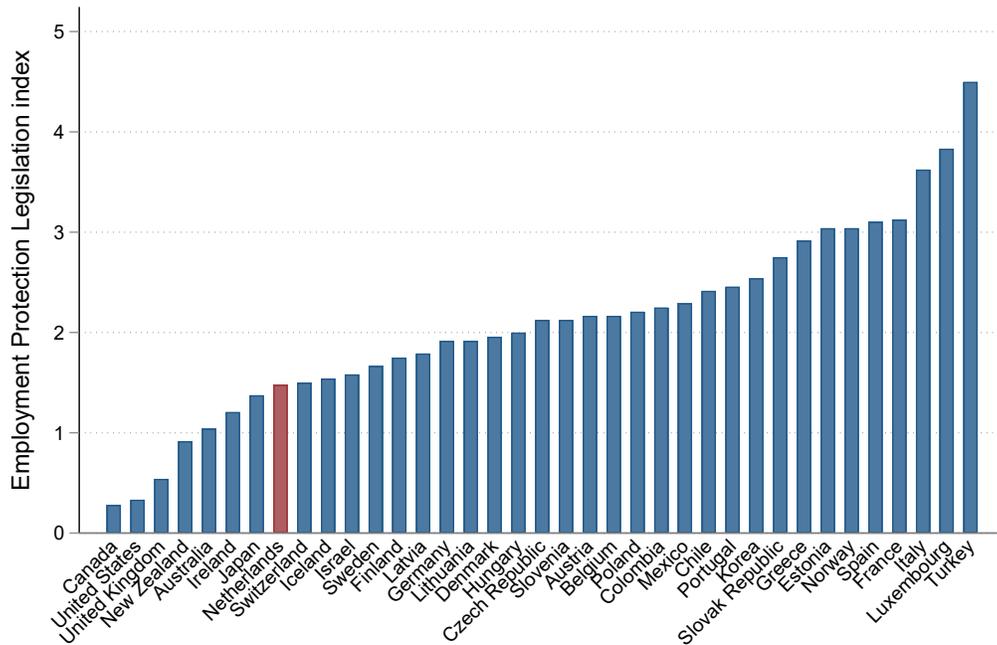
- Manning, A. (2021). Monopsony in labor markets: A review. *ILR Review*, 74(1):3–26.
- Marinescu, I., Ouss, I., and Pape, L.-D. (2021). Wages, hires, and labor market concentration. *Journal of Economic Behavior Organization*, 184:506–605.
- OECD (2019). Strictness of employment protection — individual and collective dismissals. data retrieved from OECD Statline, https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV.
- OECD (2021). Share of temporary employment. data retrieved from OECD Statline, https://stats.oecd.org/Index.aspx?DataSetCode=TEMP_I.
- Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199. Themed Issue: Treatment Effect 1.
- Webber, D. A. (2015). Firm market power and the earnings distribution. *Labour Economics*, 35:123–134.
- Zwemmer, J. (2016). *Payrolling in the Netherlands*, volume 94 of *Bulletin of comparative labour relations*, chapter 9, pages 117–127. Wolters Kluwer.

Figure 1: EPL in the Netherlands is very strong for regular contracts, but not for temporary contracts.

(a) Regular contracts

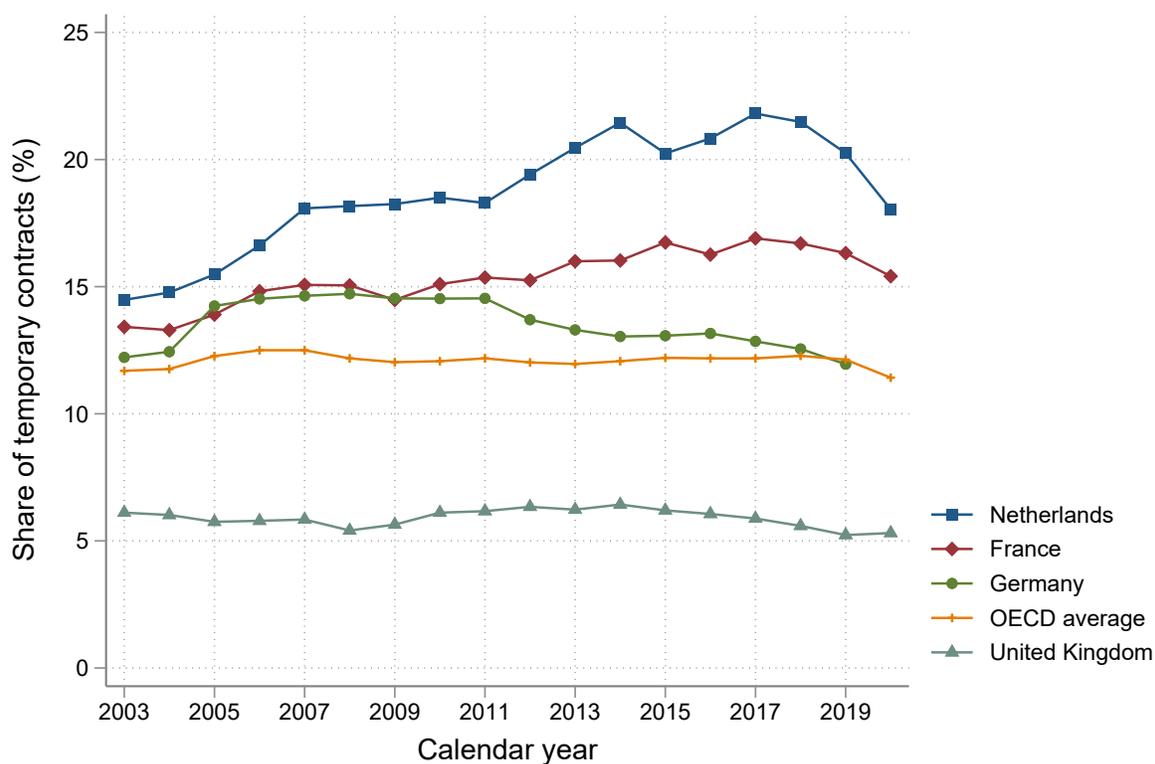


(b) Temporary contracts



Source: OECD (2019)

Figure 2: The share of temporary contracts in the Netherlands is high and has risen over the last 15 years.



Source: OECD (2021)

Notes: Temporary contracts are defined as contracts that end after an objective criterion has been reached, such as an end date or finishing an assignment. Also includes temp agency workers, seasonal work and training contracts. For the United States the OECD only reports statistics for a few years. In 2017 the share of temporary contracts was 4 percent.

Figure 3: Rising number of employees in payrolling firms, 2009–2019

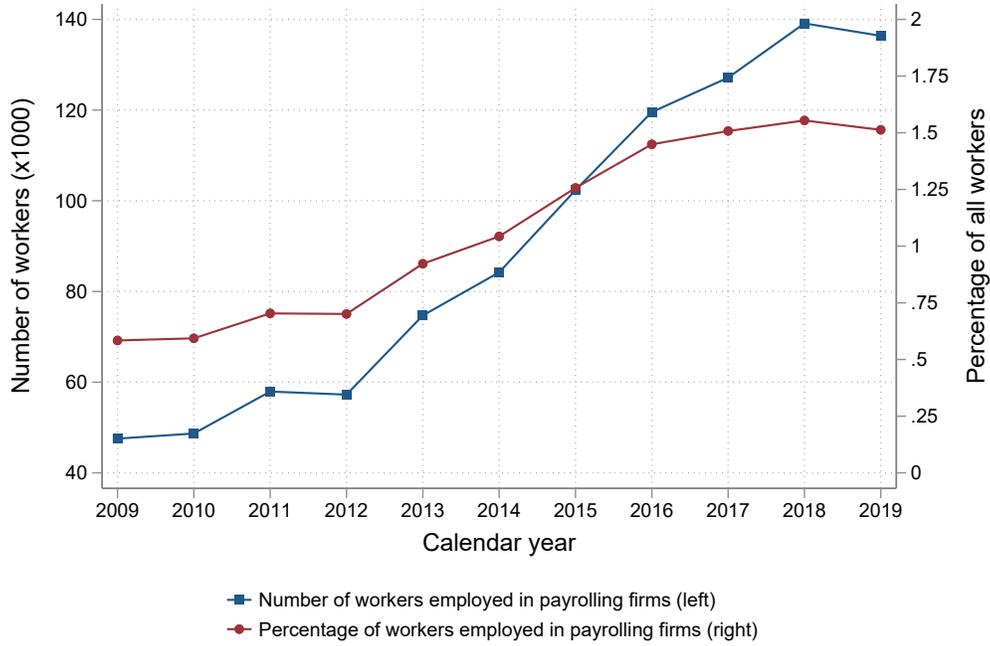
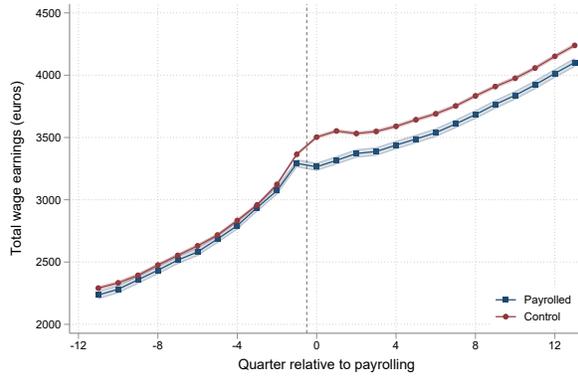


Figure 4: Total wage earnings (real euros)

(a) Descriptive



(b) DiD estimate

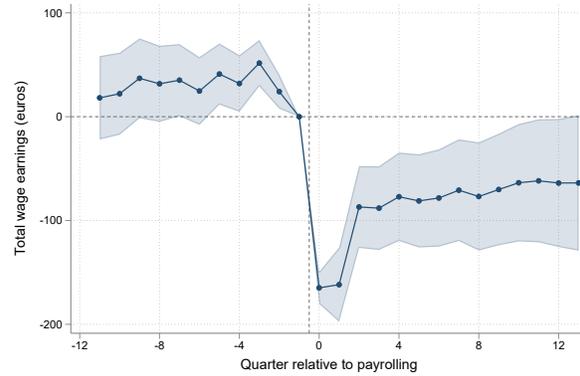


Figure 5: Employment status

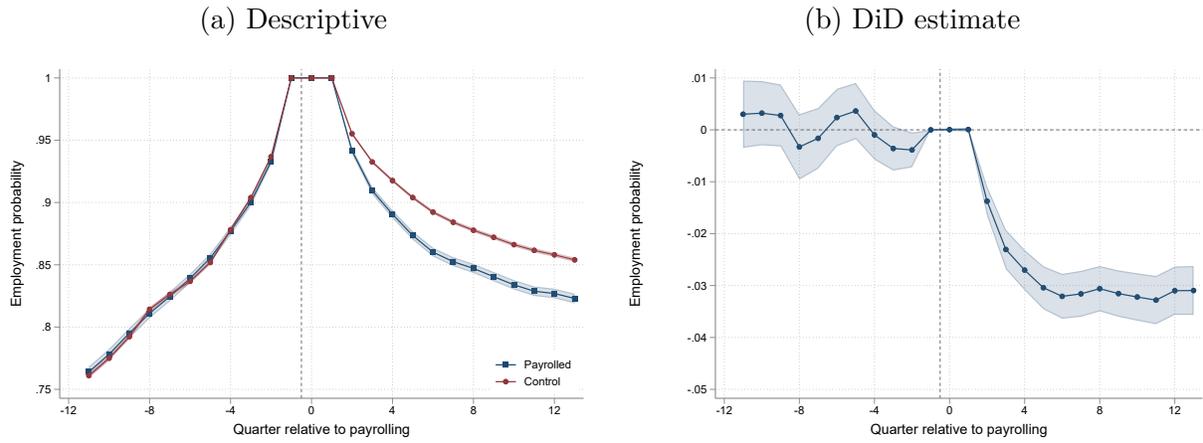


Figure 6: Receiving unemployment benefits

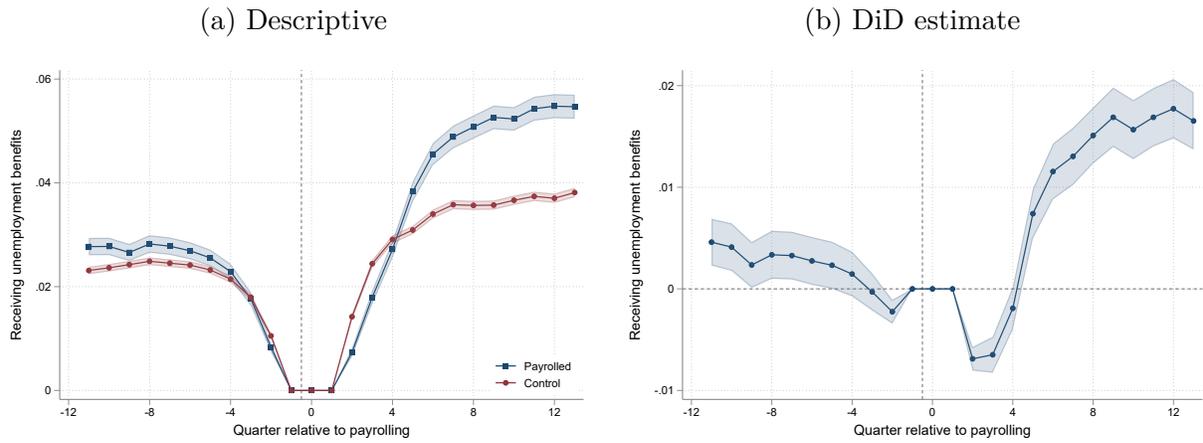


Figure 7: Total hours worked

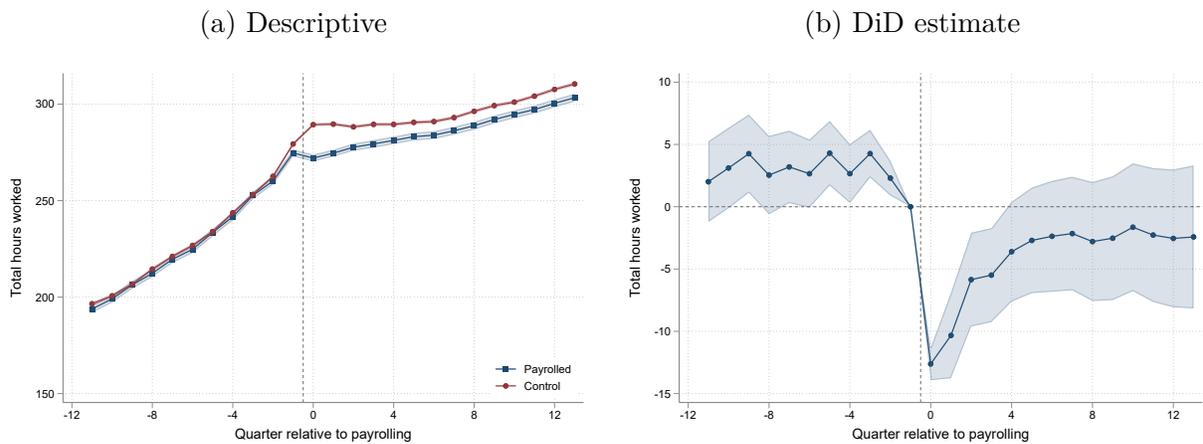


Figure 8: Log total hours worked

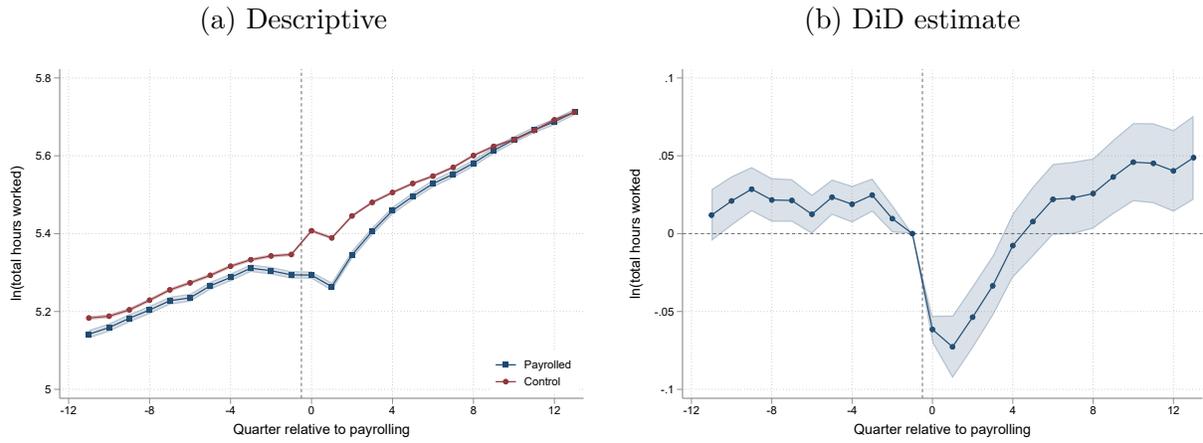


Figure 9: Hourly earnings (real euros)

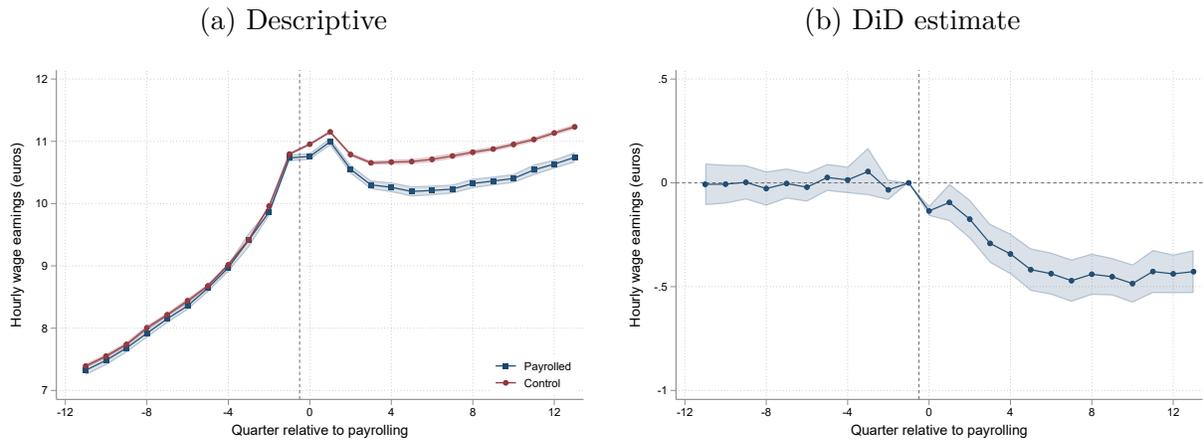


Figure 10: Log hourly earnings

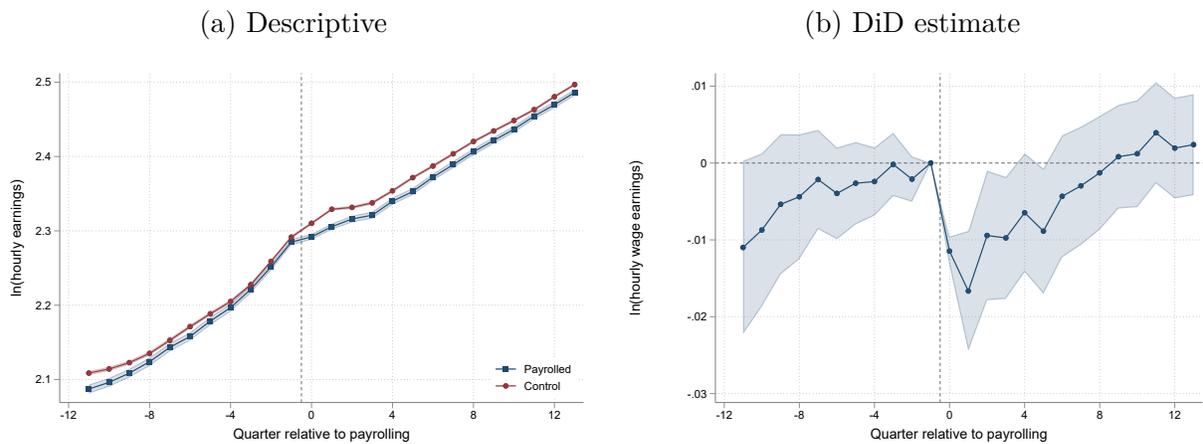


Figure 11: AKM firm fixed effect

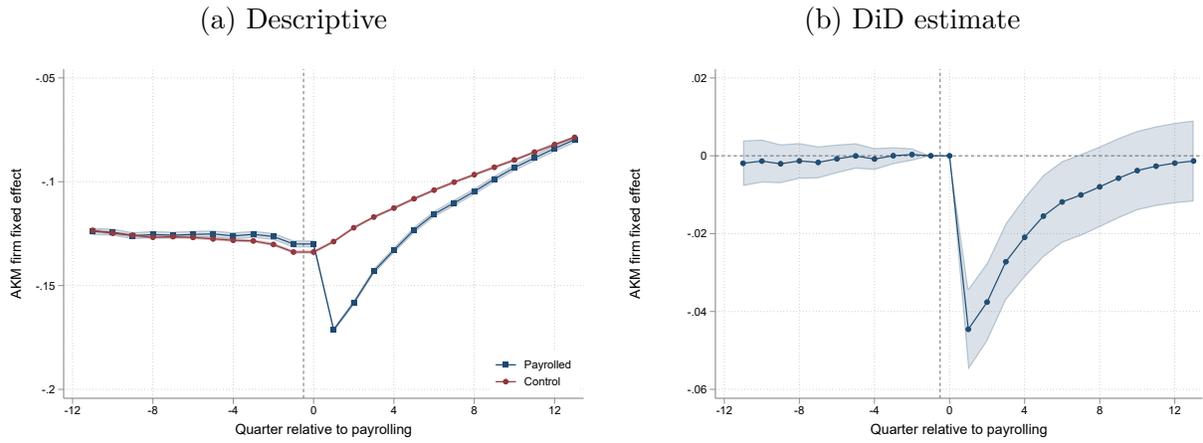


Figure 12: Permanent contract incidence

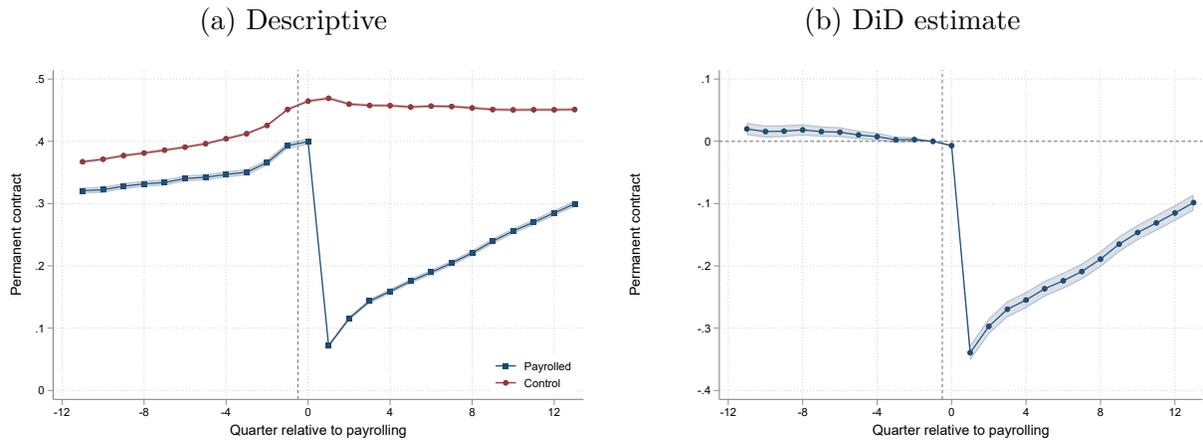


Figure 13: Log pension contributions

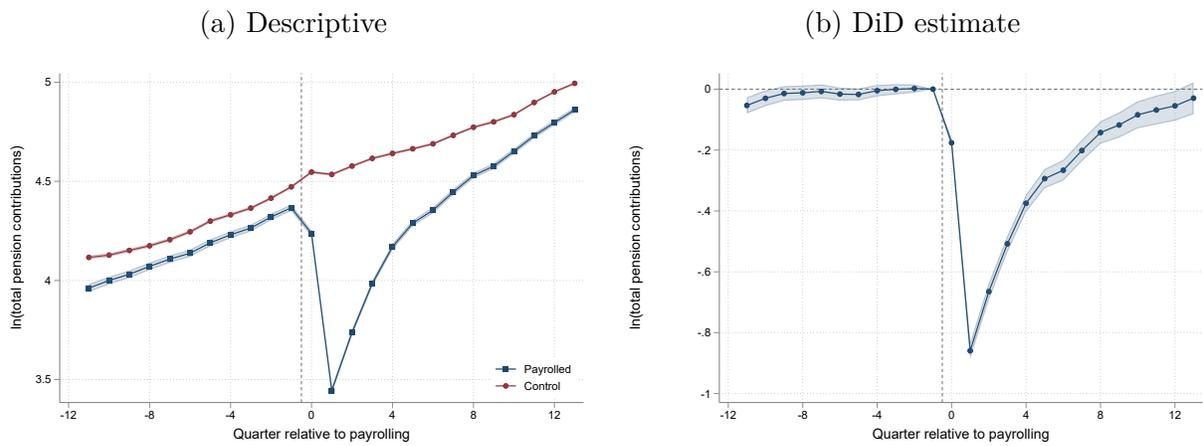


Figure 14: Heterogeneity in effects of payrolling on total earnings (euros)

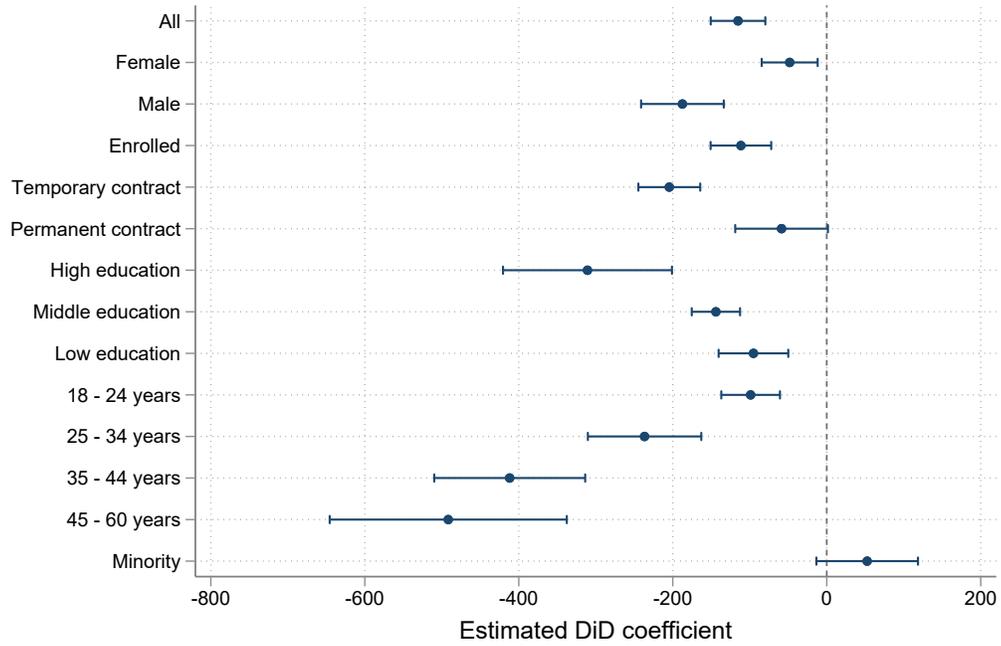


Figure 15: Heterogeneity in effects of payrolling on employment status

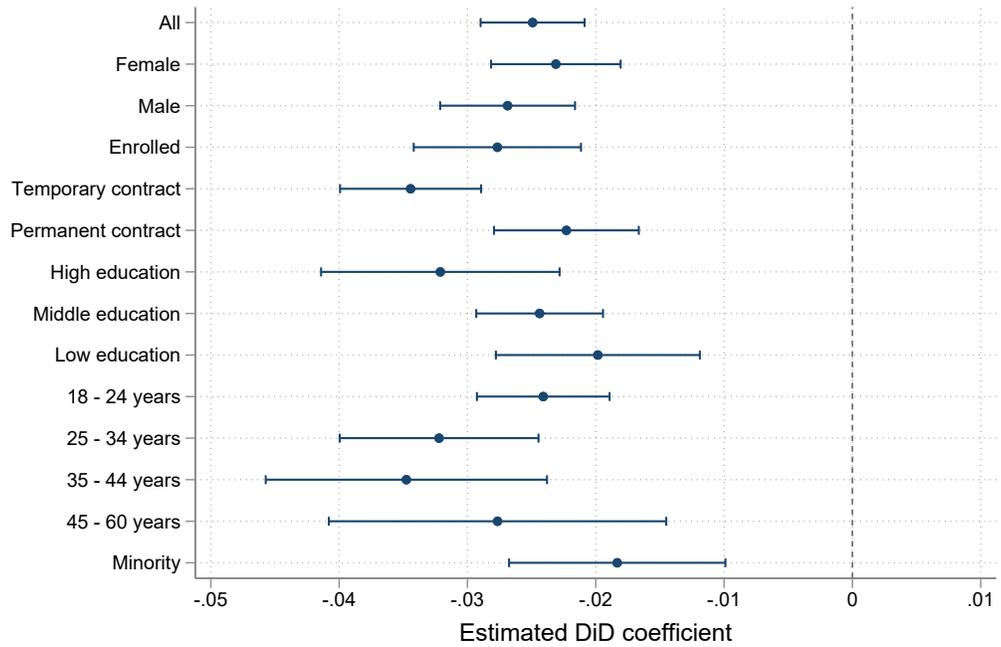


Figure 16: Heterogeneity in effects of payrolling on log of total earnings

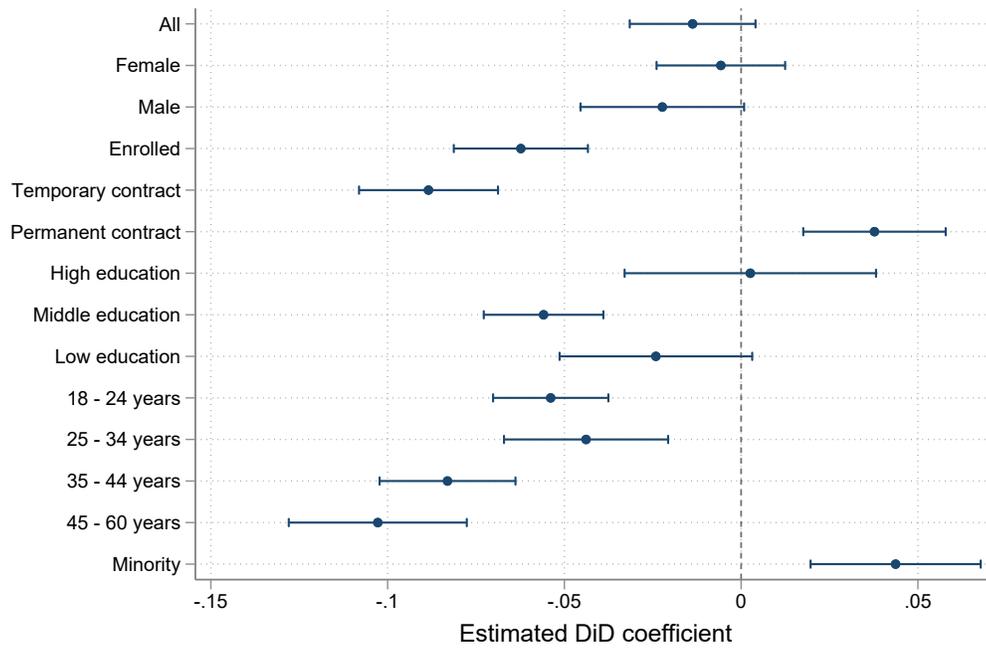


Figure 17: Heterogeneity in effects of payrolling on log of total hours

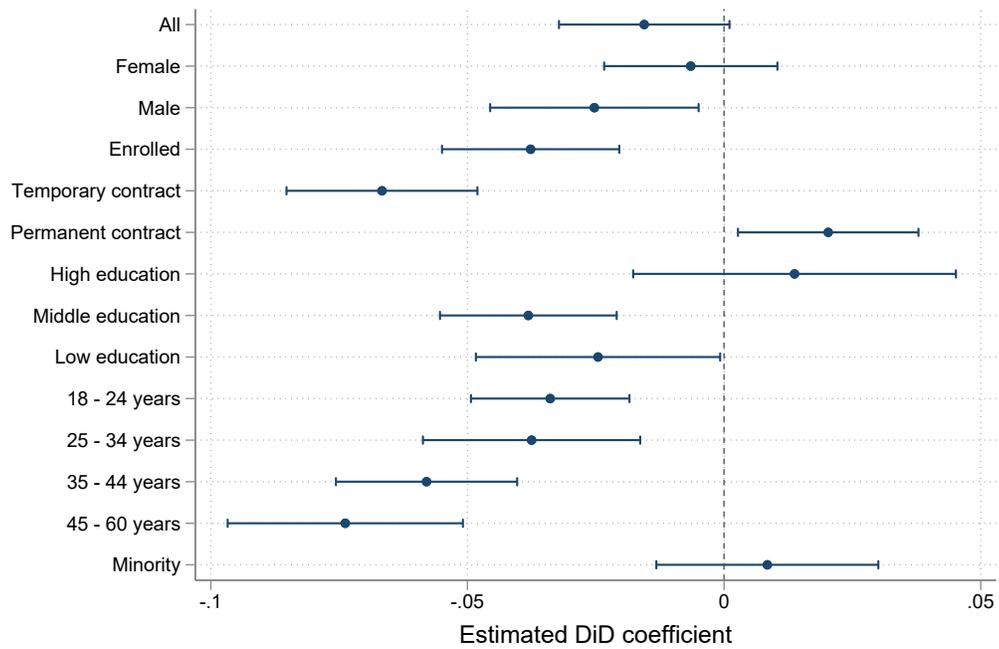


Figure 18: Heterogeneity in effects of payrolling on log of hourly earnings

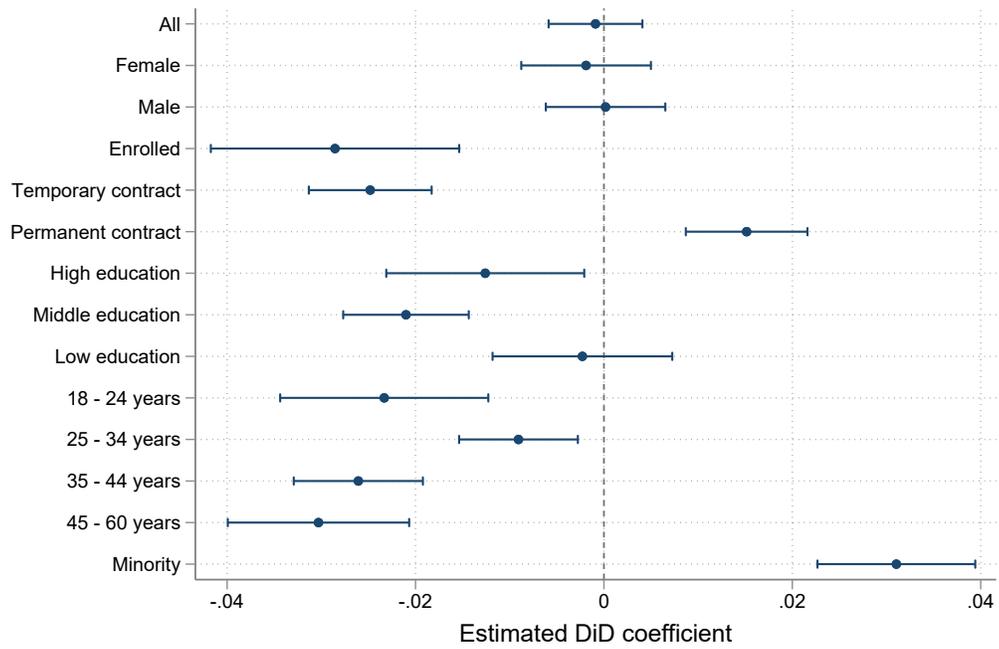


Figure 19: Heterogeneity in effects of payrolling on log of total pension contributions

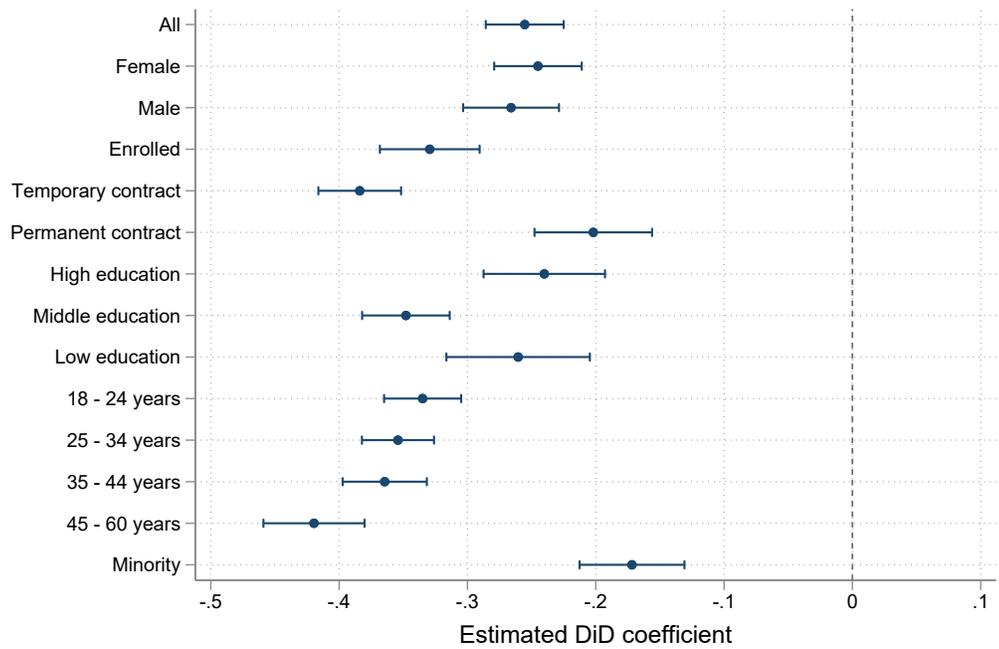


Table 1: Worker descriptives by firm type

	<i>Firm type</i>		
	Payrolling	Temporary help agency	All other
Annual earnings (euros)	11,159	13,743	34,389
Hourly earnings (euros)	12.22	12.82	21.62
Annual hours worked	864	1,018	1,507
AKM firm fixed effect	-0.16	-0.10	0.01
Employment type			
Permanent	0.12	0.10	0.69
Temporary	0.88	0.90	0.28
No data	0.00	0.00	0.03
Firm tenure in days	402	395	1,230
Education			
Low	0.17	0.17	0.10
Middle	0.48	0.33	0.28
High	0.17	0.12	0.23
No data	0.19	0.39	0.38
Female	0.47	0.37	0.47
Age	29.69	33.88	39.92
Migration background			
Native	0.65	0.44	0.79
First-generation migrant	0.21	0.45	0.13
Second-generation migrant	0.14	0.11	0.08
Number of employed worker \times year observations	881,153	6,295,603	80,778,038

Notes: Individual employed worker observations, averages over 2009–2019. The columns for payrolling and temporary help agency cover all workers who have a main job at a firm with the corresponding sector code. The column for “All other” covers workers with main jobs at all other firms. All earnings are deflated using the CPI with 2015 = 100.

Table 2: Firm descriptives by firm type

	<i>Firm type</i>		
	Payrolling	Temporary help agency	All other
Annual earnings			
Mean	15,431	18,160	32,317
Standard deviation (within firms)	10,994	10,347	15,765
Hourly earnings			
Mean	16.25	16.43	23.33
Standard deviation (within firms)	7.10	5.37	8.18
Number of workers			
Median	49	13	2
Mean	480	141	18
Standard deviation (between firms)	1,865	2,144	284
Mean AKM firm fixed effect	-0.13	-0.10	-0.09
Number of firms	622	11,165	1,103,279
Number of firm \times year observations	1,850	36,520	4,924,450

Notes: Individual firm observations, averages over 2009–2019. The columns for payrolling and temporary help agency cover all firms classified in these respective sector codes. The column for “All other” covers all firms with other sector codes. All earnings are deflated using the CPI with 2015 = 100.

Table 3: Matched worker descriptives by treatment group status

	Payrolled	Control
Quarterly earnings (euros)	3,292	3,365
Quarterly hours worked	275	279
Hourly earnings (euros)	10.73	10.80
Employment type		
Temporary contract	0.60	0.55
Permanent contract	0.40	0.45
Firm tenure in days	626	686
Education level		
Low	0.18	0.20
Middle	0.53	0.49
High	0.14	0.12
No data	0.14	0.20
Female	0.52	0.52
Enrollment in education	0.39	0.39
Age	26.8	28.8
Immigration background		
Native	0.77	0.75
First generation immigrant	0.10	0.14
Second generation immigrant	0.13	0.11
Firm characteristics		
Average hourly wage (euros)	13.89	14.18
Firm size	3,681	845
AKM firm fixed effect	-0.13	-0.13
Number of workers	37,083	170,668

Notes: Matched worker sample. Averages in $\tau = -1$, the quarter used for matching and weighted by matching weights.

Appendix

This supplementary appendix contains more details on data construction and descriptives.

A Descriptives on payrolling events

Figure A1: Number of payrolled workers, origin firms and payrolling firms (right axis) involved in events used for estimation of effects.

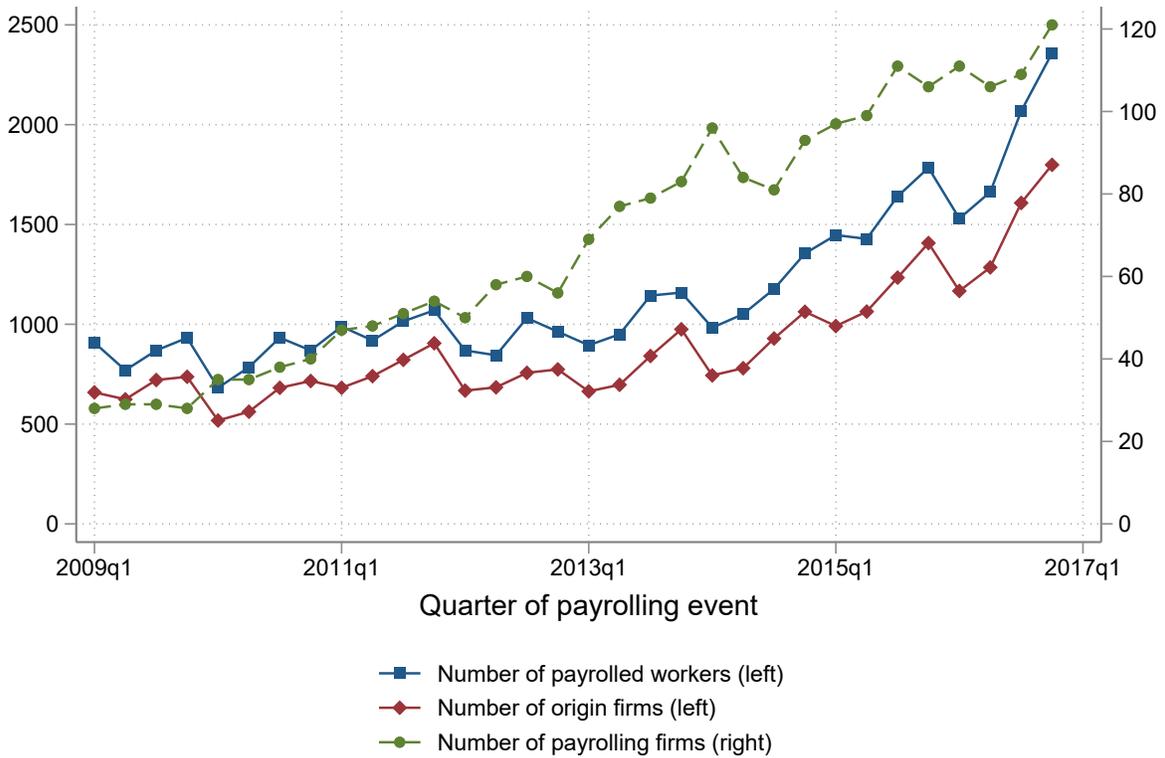


Figure A2: Firm size of firms that use payrolling in $t = 0$ compared to firms control group workers are at.

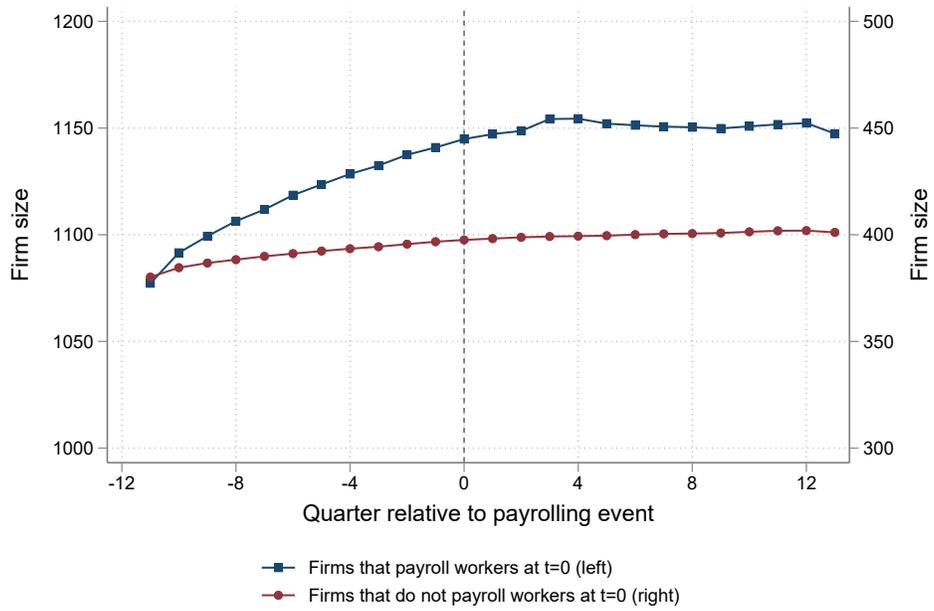


Figure A3: Average hourly wage of firms that use payrolling in $t = 0$ compared to firms control group workers are at.

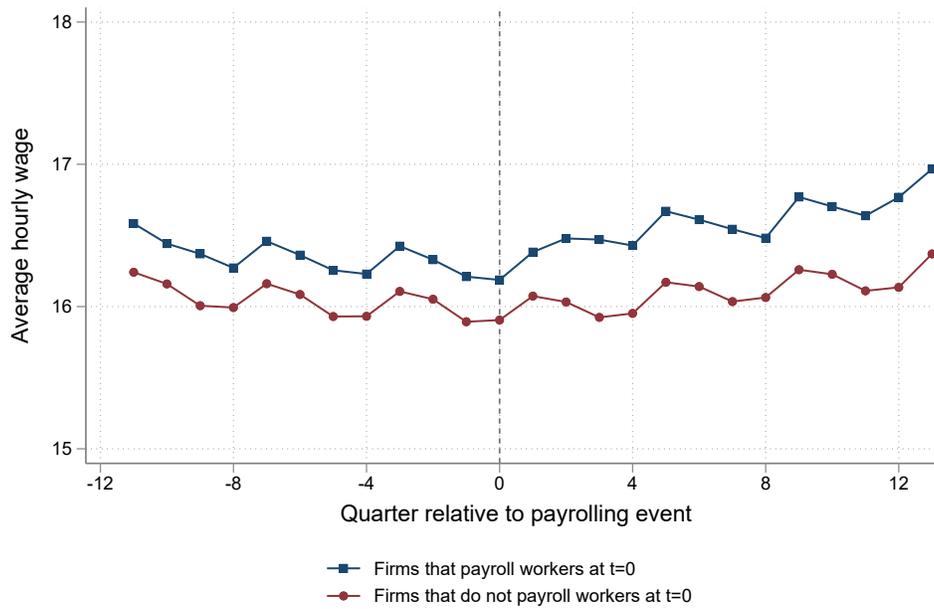


Table A1: Payrolling events by sector

Sector	Nr of payrolled workers	Payrolled workers as % of all workers in sector	As % of all payrolled workers in sample
Food and beverage establishments	8,044	2.77	21.7
Hotels and other accommodations	1,376	1.92	3.7
Other business services	624	1.57	1.7
Arts	410	1.55	1.1
Industrial design, photography, translation, and other consultancy	363	1.44	1.0
Production and distribution of audio and video	196	1.34	0.5
Advertising and market research	569	1.20	1.5
Sports and recreation	839	1.09	2.3
Retail, except automobiles	7,431	0.98	20.0
Radio and television broadcasting	75	0.96	0.2
Mail and couriers	657	0.90	1.8
Wellness and other services; funeral activities	491	0.79	1.3
Renting and leasing of motor vehicles, consumer goods, machines and other tangible goods	236	0.77	0.6
Facility management, cleaning and landscaping	1,215	0.72	3.3
Security	246	0.70	0.7
Repair of computers and consumer products	43	0.69	0.1
Agriculture and related service activities	665	0.64	1.8
Travel and tourism agencies	123	0.54	0.3
Water transport	87	0.54	0.2
Non-financial holdings and management advising	864	0.54	2.3

Notes: Sectors are classified with two-digit SBI-2008 codes, which correspond to NACE rev 2 and ISIC rev 4. The sectors are sorted by the share of payrolled workers as a % of all workers in a sector and only contains the top 20 sectors.

Table A2: Payrolling events for detailed retail sectors

Detailed retail sector	Nr of payrolled workers
Supermarkets, department stores and similar non-specialised stores	2,818
Shops selling outerwear and clothing accessories (non-specialized)	591
Shops selling meat and meat products	309
Shops selling ladies' wear	225
Department stores	210
Shops selling footwear	205
Builder's merchants and other shops selling various building materials	204
Gas stations	190
Drugstores	167
Shops selling other household equipment	165
Shops selling electrical household appliances and parts of it	147
Shops selling sports goods (not for water sports)	131
Shops selling flowers, plants, seeds and garden material	114
Garden centres	109
Non-specialised stores with non-food (no department stores)	92
Specialised shops selling other food n.e.c.	91
Shops selling newspapers, magazines and stationery	85
Shops selling furniture, articles for lighting and other household articles n.e.c	85
Bakeries	79
Shops selling furniture	78

Notes: Sectors are classified with five-digit SBI-2008 codes, which correspond to NACE revision 2.