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Alternative Work Arrangements and Worker Outcomes: Evidence from Payrolling

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Maarten Goos, Alan Manning, Anna Salomons, Bas Scheer, Wiljan van den Berge

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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

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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 are placed on the payroll of another firm while continuing their job duties at the original firm. Following a switch to a payrolling contract, workers experience worse labor market outcomes compared to a matched control group, including lower hourly wage growth, a lower incidence of permanent contracts, lower employment probability, and lower pension contributions. This suggests alternative work arrangements may reduce employment protection and job quality for workers.

Keywords: Alternative work arrangements, Outsourcing, Labor contracts **JEL:** J31, J32, J41, J42

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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).

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 et al. 2018; Abraham and Amaya 2019; 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 which workers are hired through temp agencies at the firm level. 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. In the Netherlands, 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 quarterly administrative employer-employee data over 2006–2019, where we observe more than 40,000 workers switching from a regular labor contract to a payrolling contract. 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 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. Lastly, 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. Goldschmidt and Schmieder 2017; Drenik et al. 2020), we can be certain that workers placed on a payrolling contract continue to perform the same duties, because of legal restrictions on payrolling contracts. Second, every firm in the Netherlands has access to payrolling, such that our measure of alternative work arrangements in principle covers the entire labor market. This implies 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; Goldschmidt and Schmieder 2017; Felix and Wong 2021). We find that payrolling predominantly occurs in the low-wage labor market, across a broad range of service sectors such as retail, food services, accommodation, facility management, industrial design and photography, and postal and courier activities. Third, we apply a stacked 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; Webber 2015; Dube et al. 2018, 2020), or (regional) labor market concentration of firms (Azar et al. 2019, 2020; Arnold 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 regular labor contracts which include employee protections that were largely put in place precisely to protect workers from firms' presumed bargaining power, legal work arrangements – like payrolling – erode workers' 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). As is the case for contracts with non-compete clauses, workers sign a payrolling contract voluntarily. However, workers may not be fully aware of the consequences of signing this type of contract, or they might believe 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 ('openended') labor contracts yet weaker employment protection for temporary (i.e. 'fixed-term') contracts, as shown in Figure 1. This 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. An alternative is that the firm requests the worker's consent for ending the contract, 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. Payrolling is a non-standard working arrangement specific to the Dutch setting which offers an interesting case for empirical research. Payrolling firms are intermediaries on the labor market, acting 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. Under these agreements, employers are allowed to offer workers more back-toback 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. As such, employers could use payrolling to lower labor costs: this is likely to be especially relevant in sectors with low profit margins and/or with a high share of minimum wage workers.

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 perform their job duties. 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. Since employment protection legislation of payroll workers is identical to that of temporary help workers in our analysis period, our findings are also relevant for the temporary help sector, which covers more workers in the Netherlands and is also prevalent internationally.

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

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.

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.
- 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, to obtain more stable time series and more easily compare contracts with a monthly and four-weekly pay period. For workers observed in multiple jobs simultaneously, we only retain the job providing the main source of income in each quarter. We use total base earnings (i.e. without holiday payments, bonuses and other incidental payments) 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

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)

 $^{^{3}}$ The 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

worker we then merge in administrative data on demographics from municipal registrations, social security benefits, education level⁵ and enrollment in education.

2.3 The payrolling sector

The top panel of Figure 3 shows the number of workers employed by payrolling firms over 2009–2019. While employment in payrolling firms is modest overall, there is a striking rise over the past decade: the number of workers employed in payrolling contracts has risen from 40,000 to close to 120,000 workers annually. The bottom panel shows that the share of workers employed in payrolling arrangements as a share of all employment rises from 0.6% to 1.3%; and that this share is higher and rising more strongly for young workers (those aged between 18 and 24) and workers earning close to the minimum wage, to 3.8 and 5.1% respectively. Over the entire period, the number of workers employed in payrolling firms is around 14% of the number of workers employed in temporary help agencies.

Decriptives 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 21.62 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.⁶ Workers in payrolling earn about 16%

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.

⁶We estimate AKM (Abowd et al., 1999) fixed effects using the most-connected set of all firms and workers in our dataset. See Appendix B for more detail.

less than comparable workers at other firms. Workers in payrolling firms also 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 of a little over a year), as opposed to only 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, 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? Appendix 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 service activities, Accommodations, Arts, and Other business services. In terms of the absolute number of workers, Retail trade (not in motor vehicles) is the second-largest contributor after Food and beverage services: Appendix 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. Appendix Table A1 also shows many payrolled workers originate in Facility management, which includes catering, cleaning, and landscaping services.⁷

3 Empirical approach

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

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 workers who found employment at a payrolling firm after being laid off or voluntarily leaving the non-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. To exclude outliers, we also drop workers who earn less than 80% of the age-specific minimum wage or more than 200 euros per hour. The remaining 41,089 workers are our treated sample.

Control group. The control group consists of matched workers employed in t in firms that also payroll workers 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

⁷Using 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%).

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-indifferences design as in e.g. Cengiz et al. (2019).⁸ 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 $\tau \in \{-11, ..., 13\}$ as our event window. This ensures that the dataset is balanced in event time. 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 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 such that they line up in terms of event time $\tau \in \{-11, ..., 13\}$. 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.⁹

⁸For other recent papers using this setup, see for example Bessen et al. (2022); Goldschmidt and Schmieder (2017); Deshpande and Li (2019); Clemens and Strain (2021); Baker et al. (2021)

⁹We check whether a potential control worker has been included in earlier cohorts and excluding them from the set of potential controls if this is the case. We do this by iterating over the cohorts in chronological order: for example, for cohort t we check which control workers are present in earlier cohorts (t - 1) to t - c, where c is the number of cohorts that were matched before) and exclude them from the matching procedure.

A recent literature advises against using two-way fixed effects in an event study design with 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 approaches developed by Callaway and Sant'Anna (2020) and Sun and Abraham (2021) do.

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 use coarsened exact matching to match treated and control group workers on average quarterly hours worked over the past three years, average hourly wage over the past year, age group, gender, 1-digit sector of origin, contract type at the origin firm— permanent or temporary – and tenure at the origin firm.¹⁰ We restrict each treated worker to 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 93% of treated workers.¹¹ After matching we have 38,049 treated and 174,439 control workers originating from, respectively, 19,780 and 39,155 firms. We have 318 distinct payrolling firms that employ our treated workers after the payrolling event. Appendix Figure A1 shows for each event year the number of payrolled workers, origin firms, and payrolling firms involved in the

¹⁰For hourly wage we use deciles separately per year and a separate bin for 0 earnings. For quarterly hours we use four groups per year (0 hours, 1–200 hours, 201–400 hours and 401–800 hours per quarter). For age groups we use 18–24, 25–34, 35–44, 45–60 years. For tenure we use 3 groups: 0–3 quarters, 4–7 quarters and more than 7 quarters. This reflects that most of our sample consist of relatively young workers with relatively short tenure.

¹¹If we apply less restrictive matching criteria to increase the share of matched treated workers, the quality of the match decreases, but our results are qualitatively identical.

events we use for estimation. Worker-level descriptives on the matched sample are reported in Table 3, showing treated and control group workers are very similar as a result of our matching procedure.

Estimating equation. We estimate the following difference-in-differences model on the matched worker sample:

$$Y_{it} = \alpha + \sum_{\tau \neq -1; \tau = -11}^{13} \beta_{\tau} \times I_{\tau} + \sum_{\tau \neq -1; \tau = -11}^{13} \delta_{\tau} \times I_{\tau} \times \text{treat}_i + \eta_i + \theta_{ta} + \varepsilon_{it}, \tag{1}$$

where *i* subscripts individual workers, *t* denotes calendar time in quarters, and τ is eventtime 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_{ta} are calendar quarter by age group fixed effects.¹² 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 $\tau = 1$ as the first quarter a worker is observed in a payrolling firm as their main job. 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 δ_{τ} (e.g. see Callaway and Sant'Anna 2020; Borusyak et al. 2021). First, we need that treated and control workers follow parallel trends in absence of treatment. We provide evidence consistent with this assumption by showing that pre-event trends are similar for workers that are payrolled compared to matched workers that are not payrolled. Second, workers

 $^{^{12}}$ These age groups are 18–24, 25–34, 35–44 and 45–60 years old.

cannot anticipate the payrolling event, i.e. there should not be an effect of treatment in the future on current outcomes (e.g. see 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 they 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 impacts on employment 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. For all figures in this section, we report descriptive results for treated and control group workers in panel (a), and event study difference-in-differences estimates in panel (b).

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 almost 200 euros in the event quarter. In subsequent quarters, their income recovers somewhat, but remains around 100–200 euros lower than the control group income. These wage income losses are non-negligible at between 3% and 6% of a quarterly income 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 3.5 percentage points less likely to be employed than control group

workers: these effects are long-lasting, as well as sizable given that the control group nonemployment rate is around 15% after three years. 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 8 to 10 quarters afterwards.¹³

Hours worked also decline: initially by around 12 hours per quarter, with a partial recovery to 5–10 hours lower a year after payrolling, as shown in Figure 7b. This recovery is larger for the estimates of 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 no longer significantly different from the control group workers one year after the payrolling event. This difference in estimated effect can in part be explained by the fact that hours worked exhibits a positive trend in our estimation sample, as the descriptive Figure 7a shows. Since the difference in hours worked remains stable, the relative effect measured in logs diminishes over time.

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 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. Although the effect remains negative, it becomes statistically insignificant at that point. This contrasts with Goldschmidt and Schmieder (2017) who find persistent negative effects on wages of about 10% lasting up to 10 years after the outsourcing event for longer-tenured workers moving to business service firms in Germany.

¹³The impacts we find are not driven by firm downsizing or changes in worker composition at the firm around the payrolling event, as shown in Appendix Figures A2 and A3.

The negative impacts on hourly wages are reflected in impacts on AKM firm fixed effects, shown in Figure 11. Workers who are payrolled experience an immediate decline in their AKM fixed effect of around 4 percentage points. This means that payrolled workers on average receive about 70% of the firm wage premium than 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 compared to other workers, 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 work fewer 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 points difference after three years. This implies that payrolled workers are facing a long-lasting decline in employment protection, as this is largely provided by permanent contracts in the Dutch setting. Payrolled workers also accrue substantially lower pension contributions from their employer as demonstrated in Figure 13: 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 aged 21 years and older. Differences in pension contributions between payrolled workers and the control group are close to 90% in the event year and decrease only slowly over time: even three years after payrolling, there is still a 10% gap.

These striking adverse impacts on secondary employment conditions suggest workers may either not be fully aware of the consequences of signing the payrolling contract, or may believe 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. We estimate the following version of our baseline model for each grouping variable, where we summarize the total impact for each group g in one coefficient γ :

$$Y_{it} = \alpha + \sum_{g=1}^{G} \gamma_g \times I_g \times \text{post}_{\tau} \times \text{treat}_i + \eta_i + \theta_{ta} + \varepsilon_{it}, \qquad (2)$$

with G the number of groups, I_g a dummy that indicates that a worker is part of group g and post_{au} a dummy that equals 1 if $au \geq 0$ and 0 otherwise, and the other terms are as defined before. Our coefficient of interest is γ_g , which gives the difference-in-differences estimate of being payrolled for a subgroup.

Figure 14 shows that the impacts on total earnings in real euros— including zeros— are largest for older workers and those with high education. Presumably these impacts are at least in part driven by the fact that these workers had the highest level of earnings before being payrolled. In addition, we see that earlier treated cohorts are affected more than later treated cohorts. The smaller impact for later cohorts may be explained in different ways. First, firms who benefit more from payrolling (i.e. who are able to save more on wage costs) might decide to payroll earlier. Second, the Dutch labor market has improved during the post-treatment period: the Dutch unemployment rate peaked in 2014 in the wake of the European debt crisis. Third, in 2015 employment protection was tightened, with a specific focus on non-standard employment contracts. In contrast, impacts on employment status are 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 early treated cohorts, with declines up to 10% on average (Figure 16). In contrast, for first generation migrants, higher educated workers, and workers with a longer firm tenure, the impacts are smaller and insignificant. These patterns are partially driven by declines in hours worked conditional on having a job (Figure 17) and partially by declines in hourly earnings (Figure 18). Effects on hours worked generally exhibit the most heterogeneity across worker subgroups.

Finally, adverse consequences for secondary employment conditions are also largest for older workers and earlier treated cohorts, with pension contributions up to 40% lower over the three years after payrolling for some groups (Figure 19), compared to an overall estimate of 30%.

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 pension contributions. Impacts appear to be largest for older workers, for students, and for workers previously employed on a temporary contract. This suggests alternative work arrangements reduce employment protection and job quality for workers.

Our findings are based on payrolling as it existed prior to the 2020 enactment of new labor law in the Netherlands (*Wet Arbeidsmarkt in Balans, WAB*). This law equates the legal protection of employees on payrolling contracts to that of standard contracts, including pension accrual. Future research could consider whether adverse effects for individual employees have indeed been remedied since the introduction of this law.

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Source: OECD (2019). Higher values indicate higher protection.



Figure 2: High and rising share of temporary contracts in the Netherlands

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. This also includes temp agency workers, seasonal work, and training contracts.



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

Note: Minimum wage workers are defined as workers earning up to 120 percent of the statutory age-specific minimum hourly wage applicable in that year.



Figure 4: Total wage earnings (real euros)





Figure 6: Receiving unemployment benefits











Figure 10: Log hourly earnings











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

	Firm type		
	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

Table 1: Worker descriptives by firm type

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.

	Firm type		
	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

Table 2: Firm descriptives by firm type

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.

	Payrolled	Control
Quarterly earnings (euros)	3,293	3,361
Quarterly hours worked	274	279
Hourly earnings (euros)	10.83	10.84
Employment type		
Temporary contract	0.61	0.61
Open-ended contract	0.39	0.39
Firm tenure in days	620	651
Education level		
Low	0.18	0.19
Middle	0.53	0.52
High	0.15	0.13
No data	0.14	0.16
Female	0.52	0.52
Age	26.8	26.8
Enrolled in education	0.39	0.42
Immigration background		
Native	0.77	0.76
First generation immigrant	0.10	0.12
Second generation immigrant	0.13	0.12
Firm characteristics		
Average hourly wage (euros)	13.96	14.27
Firm size	3,944	830
AKM firm fixed effect	-0.12	-0.13
Number of workers	38,049	174,055

Table 3: Matched worker descriptives by treatment group status

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: Annual 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.



Sector	Nr of payrolled workers	Payrolled workers as % of all workers in sector	As % of all payrolled workers in sample
Food and beverage service activities	8165	2,82%	$21,\!46\%$
Accommodation	1379	1,93%	$3,\!62\%$
Arts	431	$1,\!63\%$	$1,\!13\%$
Other business services	645	$1,\!62\%$	1,70%
Industrial design, photography, translation and other consultancy	386	1,53%	1,01%
Motion picture and television programme production and distribution; sound recording and music publishing	222	1,52%	0,58%
Advertising and market research	593	1,25%	1,56%
Programming and broadcasting	90	$1,\!15\%$	$0,\!24\%$
Sports and recreation	872	1,14%	$2,\!29\%$
Retail trade (not in motor vehicles)	7664	1,01%	$20,\!14\%$
Postal and courier activities	655	0,90%	1,72%
Wellness and other services; funeral activities	495	$0,\!80\%$	$1,\!30\%$
Renting and leasing of motor vehicles, consumer goods, machines and other tangible goods	241	0,79%	$0,\!63\%$
Facility management	1220	0,73%	3,21%
Security and investigation	249	0,71%	$0,\!65\%$
Repair of computers and consumer goods	41	$0,\!66\%$	0,11%
Agriculture and related service activities	655	$0,\!63\%$	1,72%
Travel agencies, tour operators, tourist information and reservation services	127	0,56%	0,33%
Holding companies (not financial)	889	0,55%	2,34%
Water transport	82	0,51%	0,22%

Table A1: Payrolling events by sector

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.

Detailed retail sector	Nr of payrolled workers
Supermarkets, department stores and similar non-specialised stores	2936
Shops selling outerwear and clothing accessories (non-specialised)	611
Shops selling meat and meat products	316
Shops selling ladies' wear	236
Department stores	218
Shops selling footwear	216
Builder's merchants and other shops selling various building materials	211
Petrol stations	187
Drugstores	187
Non specialised shops selling household articles	161
Shops selling a combination of brown and white goods	151
Shops selling sports goods (not for water sports)	132
Garden centres	113
Shops selling flowers, plants, seeds and garden material	113
Specialised shops selling other food	103
Shops selling various home furnishings	96
Non-specialised stores with non-food (no department stores)	89
Shops selling newspapers, magazines and stationery	83
Shops selling furniture	80
Shops selling bread and pastry	77

Table A2: Payrolling events for detailed retail sectors

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

B Estimation of AKM fixed effects

A growing body of work exploits linked employer-employee data to estimate the worker and firm components to wages (e.g. Abowd et al., 1999; Card et al., 2013, 2015, 2018; Sorkin, 2018; Lachowska et al., 2020; Song et al., 2019). This literature has shown that the firm component is an important driver of wages. It can for example (partially) explain the increase of wage inequality (Card et al., 2013; Song et al., 2019) and gender differences in payments (Card et al., 2015).

The typical workhorse model is a simple two-way fixed effects model as in Abowd et al. (1999). Our data allow us to estimate firm and worker fixed effects using yearly data on workers and firms from 2006 to 2019. Contrary to most other papers estimating AKM effects, we can exploit data on hourly wages for our full sample. We estimate the following model

$$logY_{ijt} = \alpha_i + \psi_{j(i,t)} + X_{it} + \theta_t + u_{ijt}$$
(3)

where Y_{ijt} denotes hourly wages of worker *i* in firm *j* in year *t*, *i* are individual fixed effects and $\psi_{j(i,t)}$ are firm fixed effects. We control for a full set of year fixed effects θ_t and X_{it} contains worker age and age squared. Any other time-invariant differences between workers are absorbed by the worker fixed effects. The error term u_{ijt} may also contain firm-worker specific match effects.

The firm effects are identified off of workers switching from one firm to another. Hence, the sample only includes firms that are connected by a worker switching at some point between 2006 and 2019. This excludes 7.2% of firms in the full dataset.

The firm fixed effects can be interpreted as wage premiums paid by firms, for example due to firms having rents to share with workers. For our purposes it is enough to consider the firm fixed effects as time-invariant firm policies with regards to hourly wages. In effect, the fixed effects allow us to characterize some firms as "high wage" firms and some firms as "low wage" firms (conditional on worker characteristics).