

What Happens to Workers at Firms that Automate?*

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Abstract

We estimate the impact of firm-level automation on individual worker outcomes by combining Dutch micro-data with a direct measure of automation expenditures covering all private non-financial sector firms. Using a novel difference-in-differences event-study design leveraging lumpy investment, we find that automation increases the probability of incumbent workers separating from their employers. Workers experience a 5-year cumulative wage income loss of 9 percent of one year's earnings, driven by decreases in days worked. These adverse impacts of automation are larger in smaller firms, and for older and middle-educated workers. By contrast, no such losses are found for firms' investments in computers.

Keywords: Firm-level automation, Worker displacement

JEL: D24, J23, J62, O33

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1 Introduction

Advancing technologies are increasingly able to fully or partially automate job tasks. These technologies range from robotics to machine learning and other forms of artificial intelligence, and are being adopted across many sectors of the economy. Applications include selecting job applicants for interviews, picking orders in a warehouse, interpreting X-rays to diagnose disease, and automated customer service. These developments have raised concerns that many workers are being displaced because their firms adopt automation technology (Eurobarometer 2017; Pew 2017).

An emerging literature studies firms adopting automation technology. The literature focuses predominantly on robotics or other manufacturing technology, and measures adoption through imports, surveys, or electricity usage (Doms et al., 1997; Dunne and Troske, 2005; Dinlersoz and Wolf, 2018; Cheng et al., 2019; Dixon et al., 2019; Koch et al., 2021; Acemoglu et al., 2020; Aghion et al., 2020; Humlum, 2021; Bonfiglioli et al., 2021; Hirvonen et al., 2021).¹ Compared to non-adopters, firms adopting automation technology are generally found to experience faster employment, revenue, and productivity growth, and either declining or stable labor shares. However, Bonfiglioli et al. (2021) find that, when accounting for firm-level product demand shocks that generate a positive correlation between robot imports and employment, exogenous exposure to automation imports leads to a decline in firm-level employment.

There are as yet no empirical studies that directly examine what happens to individual workers when their firm decides to automate.² However, studying this adjustment process

¹A related literature studies aggregate adoption of robotics, using industry and/or regional variation (Graetz and Michaels, 2018; Aghion et al., 2019; Dauth et al., 2021; Acemoglu and Restrepo, 2020).

²An exception is Humlum (2021) who finds that a firm’s wage bill declines for production workers and increases for tech workers after automation. Other related studies on worker adjustments have used more aggregate sources of variation and do not always focus on causal effects. In particular, Cortes (2016) finds that workers switching out of routine-intensive occupations experience faster wage growth relative to those who stay; Dauth

is critical to our understanding of how labor markets are impacted by the automation of work. While the theoretical literature models automation as displacing humans from certain tasks (see Acemoglu and Restrepo 2018), this need not translate to worker-level displacement if productivity increases are sufficiently strong. Further, firm- and sector-level impacts do not inform about the adjustment process that workers go through: even positive aggregate changes may be accompanied by worker churn (including across firms within sectors) and economic consequences for individual workers directly affected by automation. Any such adjustments are also of first-order importance for policymakers aiming to diminish adverse impacts out of distributional concerns.

In studying the impacts of firms' automation activity on individual workers, our paper makes three contributions to the literature. First, we measure automation at the firm-level across all private non-financial sectors. We use data from an annual Dutch firm survey on automation costs. These automation costs refer to an official bookkeeping entry on firms' profit and loss account, and are defined as expenditures on third-party automation services provided by specialist integrators across a wide range of automation technologies. This means our measure complements the literature which has so far focused on the adoption of machinery (and particularly robots) by manufacturing firms.

Second, we develop a novel empirical approach based on evidence that firms' automation expenditures occur in discrete episodes of lumpy investment. In particular, we leverage spikes in automation cost shares to define automation events. We exploit the *timing* of these events in a difference-in-differences event-study methodology for identifying causal effects on firms' incumbent workers. This approach is made possible by the relatively high frequency of automation events in our data. The advantage of this approach is that it allows for more credible identification of worker-level effects: this is important because the literature has shown that automating and non-automating firms are on different employment trends, a finding we replicate in our data.

et al. (2021) correlate regional variation in robot exposure with worker outcomes; while Edin et al. (2019) show that workers have worse labor market outcomes when their occupation is experiencing long-term decline.

Third, by linking our annual firm survey to administrative firm and worker data, we can follow individual workers over time and measure a rich set of outcomes for workers in the years surrounding an automation event. These outcomes include annual wage income, daily wages, firm separation, days spent in non-employment, self-employment, early retirement, unemployment benefits, and welfare receipts. To our knowledge, our paper provides the first estimates of the impacts of firm-level automation on these outcomes. Our data cover the years 2000–2016 and we observe 35,580 firms, employing close to 5 million unique workers per year on average.

We find that automation at the firm increases the probability of incumbent workers (with at least three years of firm tenure) separating from their employer. On average, these workers experience a 5-year cumulative wage income loss of 9% of an annual wage. These losses are driven by increases in non-employment: we do not find evidence of wage scarring on average for workers impacted by automation. Lost wage earnings from non-employment spells are only partially offset by various benefits systems and workers are more likely to enter early retirement. Turning to effect heterogeneity, we find that these adverse impacts from automation are larger in smaller firms and for older and middle-educated workers. Finally, we show that, unlike firm investments in automation, firm investments in computers do not result in displacement effects.

This paper is structured as follows. In section 2, we introduce our data, and compare our measure of automation costs to other measures of firm-level technology use. Section 3 explains how the lumpiness of firms’ automation expenditures is used to identify automation events, and documents these events in our data. Section 4 analyzes the relationship between automation and firm-level employment and wage outcomes. Section 5 reports our main results on the effects of automation on worker-level outcomes using our difference-in-differences event-study design. In section 6 we directly compare the worker-level impacts of automation to those of computerization. The final section concludes.

2 Data

2.1 Administrative data on workers and firms

We use Dutch data provided by Statistics Netherlands. In particular, we link an annual firm survey to administrative firm and worker databases covering the universe of firms and workers in the Netherlands. The firm survey is called “Production Statistics” and includes a direct question on automation costs – it covers all non-financial private firms with more than 50 employees, and samples a subset of smaller non-financial private firms.³ This survey can be matched to administrative company and worker records.

Our data cover the years 2000–2016, and we retain 35,580 unique firms with at least 3 years of automation cost data – together, these firms employ around 5 million unique workers annually on average. We use a worker’s total annual gross earnings in all jobs as the main measure of wage income. Since we observe the number of days but not hours worked per year, we use daily wages as a measure of wage rates. We remove firms where Statistics Netherlands indicate that the data are (partly) imputed. We further remove workers enrolled in full-time studies, and those earning either less than 5,000 euros per year or less than 10 euros per day, as well as workers earning more than half a million euros per year or more than 2,000 euros on average per day. For workers observed in multiple jobs simultaneously, we only retain the job providing the main source of income in each year when matching workers to firms.

We further observe workers’ gender, age, and nationality. A downside to these data is that we do not observe workers’ occupations, and only have information on the level of education for a small and selected subset of workers (with availability skewed toward younger and high-educated workers). We further match worker-level data to administrative records on receipts from unemployment, welfare, disability, and retirement benefits.

³Firms are legally obliged to respond to the survey when sampled. However, the sampling design implies our data underrepresent smaller firms: we also examine effect heterogeneity across firm size classes to consider how this sample selection affects our overall findings.

We can track workers across firms on a daily basis, allowing us to construct indicators for firm separation and days spent in non-employment.

2.2 Automation costs

An important advantage of our data is the availability of a direct measure of automation at the firm level. In particular, automation costs are an official bookkeeping entry defined as expenditures on third-party automation services. While the disadvantage of this measure is that we do not know the exact automation technology being used by the firm, it does capture all automation technologies rather than focusing on a single one. Further, we measure it at the level of the firm rather than the industry, and across all private non-financial sectors.

Table 1 shows summary statistics on annual automation costs for firms, both in levels, per worker, and as a percentage of total costs (excluding automation costs). This highlights several things. First, almost one-third of firm-year observations has zero automation expenditures. Second, the average automation cost share is 0.44%, corresponding to an outlay of around 211,000 euros annually, or 1,045 euros per worker. Third, this distribution is highly right-skewed as the median is only 0.16% – this skewness persists even when removing observations with zero automation costs.

The top panel of Table 2 further shows how these automation costs and cost shares differ by broad (one-digit) sector. Our comprehensive measure of automation technologies indicates that all sectors have automation expenditures, though there is substantial variation at the firm level both between and within each of these sectors. Average expenditures at the sectoral level range from 244 to 1,789 euros per worker. The highest mean automation expenditures per worker are observed in Information & communication, followed by Professional, scientific & technical activities, Wholesale & retail, and Manufacturing. Conversely, Accommodation & food serving has the lowest expenditure per worker, followed by Construction, Administrative & support activities, and Transportation & storage. However, there is much variation between firms in the same sector, as shown by the standard deviations of the automation cost share in total (other) costs.

While we do not use either this sectoral or between-firm variation in our empirical identification strategy, we will consider effect heterogeneity across sectors since the nature of automation technologies may be sector-specific.

Table 2 also reports the same statistics but separately by firm size class, grouped into 6 classes used by Statistics Netherlands: the smallest firms have up to 19 employees whereas the largest have more than 500. Unsurprisingly, automation cost levels rise with firm size: firms with fewer than 20 employees spend around 12,000 euros annually on automation services, whereas the largest firms spend close to 3.2 million euros. Less obviously, this table also reveals that automation cost shares increase with firm size, particularly at the very top: the smallest firms have average automation cost shares of around 0.41%, whereas firms with between 100 to 200 employees have a cost share of 0.44%. This increases to 0.51% for firms between 200 and 500 workers, and 0.76% for firms with more than 500 workers. This is consistent with the literature’s findings that more productive and therefore larger firms are more likely to automate. However, there is substantial variation within size classes, also.⁴

2.3 What do automation costs capture?

Since our automation cost measure does not specify the automation technology being implemented, we compare firm-level automation costs with other, specific, technologies obtained from a firm-level survey provided by Statistics Netherlands. These measures are self-reported indicators (unlike automation costs, which are part of firms’ official bookkeeping), and the questions differ by survey year.⁵

We obtain correlations between firms’ (standardized) automation cost shares and their self-reported implementation with self- or externally developed process, product, and

⁴Appendix A.1 further illustrates how the distribution of automation cost expenditures per worker and automation cost shares change over time. It shows that both the means and variances of automation costs per worker and automation cost shares have increased over time.

⁵We take firms’ most recent reported technology use.

organizational innovations (all three measured as dummies), controlling for sector fixed effects and firm size.⁶ Automation cost shares are positively correlated with all three self-reported innovation types, indicating that automating firms are more innovative across the board. However, the coefficient for process innovation is more than twice as large as those for product and organizational innovations, highlighting that this innovation margin is differentially more important for firms with high automation cost shares. Firms with a one standard deviation higher automation cost share are 20.5 percentage points more likely to report implementing process innovations. This suggests that automation cost shares differentially measure process automation technology rather than innovations more broadly.

We also estimate the same models for a host of self-reported uses of specific technologies, all of which are given as dummy variables in the data.⁷ First of all, this shows that automation cost shares are positively associated with the general indicator of using electronic data suited to automated processing. Automation cost shares are also positively correlated with several well-known technologies, such as Customer Relationship Management (CRM) software used for inventory and distribution and Enterprise Resource Planning software. Further, value chain integration itself is not predictive of automation cost shares, but the use of automated records in such integration is.

Further, automation cost shares are correlated with the use of big data analysis, and with the use of cloud services for both customer management and accounting and financial management. Firms with high automation cost shares are more likely to receive their own orders through Electronic Data Interchange (EDI) but less likely to order through EDI at other firms— the latter would of course require these trading partners to have EDI technology. Finally, automation cost shares are higher for firms using sales software.

Importantly, not all technologies are associated with firms' automation cost shares: the use of radio-frequency identification, Local Area Networks, or using internet for financial transactions or worker training are not predictive of a firm's automation cost share.

⁶Results are reported in Appendix Table A.1.

⁷Results are reported in Appendix Table A.2.

These results can be seen as a type of placebo result, since we would not expect these specific technologies to be associated with substantial automation at the firm. All in all, these descriptives show that firms' automation costs are most strongly related to process innovations, and meaningfully associated with the usage of various specific technologies, particularly those which make use of electronic data suited to automated processing.⁸

Another measure of firm-level technology adoption used in the literature are firm imports of robots and other automating technologies. We compare this to our automation cost measure by leveraging data on automation imports, which are available from 2010 onward.⁹ Automation imports are defined as imports of intermediates classified by Acemoglu and Restrepo (2021) as automatically controlled machines, automatic transfer machines, automatic welding machines, numerically controlled machines, and (other) industrial robots. We find that average automation expenditures are substantially higher than average automation imports since few firms are importers, and that automation costs are observed across a wide range of sectors whereas automation imports are largely limited to firms in Manufacturing, Wholesale & retail, and Transportation & storage.

Automation imports and automation expenditures are correlated between, but not within firms: that is, firms are not more likely to have higher automation costs when they (net) import more automation technology. This comparison shows that the firm-level import measures used in the literature likely capture a different dimension of technology adoption. One reason could be that our measure of automation costs captures that the firm relies on an integrator to install the machinery whereas in the case of automation imports the firm is purchasing the automation machinery directly. However, a limitation

⁸Appendix A.3 further reports the sectoral prevalence of the technologies which are significantly positively correlated with automation cost shares: this gives insight into which specific automation technologies are being used in these industries. The main finding is that different sectors do implement these technologies at different rates, but the overall use of data for automated processing is relatively common in all sectors, reflecting a general characteristic of automation.

⁹Results are reported in Appendix A.4.

to our comparison is that Dutch firms also export automation technology at high rates—in fact, the Netherlands is a net exporter of automation technology. This could indicate that, in the Netherlands, firm-level automation imports need not correspond to actual deployment of the imported technology within the firm.

3 Automation events

3.1 Defining automation events

We identify automation events using what we call automation spikes. We define an automation spike as follows. Firm j has an automation cost spike in year τ if its real automation costs $AC_{j\tau}$ relative to real total operating costs (excluding automation costs) averaged across all years t , \overline{TC}_j , are at least thrice the average firm-level cost share excluding year τ :

$$spike_{j\tau} = \mathbb{1} \left\{ \frac{AC_{j\tau}}{\overline{TC}_j} \geq 3 \times \frac{1}{T-1} \sum_{t \neq \tau}^T \left(\frac{AC_{jt}}{\overline{TC}_j} \right) \right\} \quad (1)$$

where $\mathbb{1}\{\dots\}$ denotes the indicator function. As such, a firm that has automation costs around one percent of all other operating costs for year $t \neq \tau$ will be classified as having an automation spike in $t = \tau$ if its automation costs in τ exceed three percent of average operating costs over years t . Finally, we define an automation event as the year when the firm has its first increase in automation cost share that qualifies as a spike.^{10,11}

¹⁰Firms without an automation event do not necessarily have zero automation costs, but their automation expenditures do not vary much relative to total costs, implying they do not undergo automation events as we define them. Firms without such events do have lower automation costs also on average, however.

¹¹Note that automation costs capture the use of third-party automation services: this raises the question whether these events capture outsourcing at the firm-level. However, for outsourcing, we would expect to see a sustained increase of costs for the firm (reflecting a flow of wages to these outsourced workers) rather than a temporary spike.

We develop a theoretical model to show that the existence of automation events is consistent with monopolistic competition between profit-maximizing firms that choose their optimal timing to incur a firm-specific fixed cost of automation.¹² Assuming technological progress such that a firm’s technology ages and its relative output price increases if it does not automate, the model predicts spikes in automation cost shares, i.e. a one-period increase in automation expenditures relative to other costs preceded and followed by periods in which the firm does not automate. Moreover, firms will automate at different points in time if they have different fixed costs of automation.

3.2 Summary statistics of automation events

Out of the total number of 35,580 firms with at least 3 years of automation cost data, there are 10,425 firms that have at least one spike in their automation cost share between 2000 and 2016.¹³ That is, 29% of the firms in our sample spike at least once over the 17 years of observation.¹⁴ Out of the firms that have at least one automation cost spike, the large majority spikes only once over 2000–2016, although some spike twice and up to five times at most.

Figure 1 shows the evolution of automation costs shares around automation events. This is constructed by redefining time τ as event time: the difference between the actual calendar year and the calendar year of the spike, such that all automation events line up in $\tau = 0$. Figure 1 uses the sample of 10,425 firms, showing a clear one-period increase

¹²This model is related to Bonfiglioli et al. (2021) and Humlum (2021) and extends task-based frameworks of automation in Acemoglu and Restrepo (2018, 2019) by endogenizing firm-level automation as a lumpy investment following Haltiwanger et al. (1999). It is provided in Appendix B.

¹³Shown in Appendix Table C.1.

¹⁴We also find that this percentage varies relatively little across sectors (ranging from 25% in Construction to 39% in Information & communication) and firm sizes, although the smallest firms are least likely to have automation events. See Appendix C.2 for details.

in the automation cost share when a firm has its automation event.¹⁵

We also assess the ‘lumpiness’ of automation costs. We find that while only 5% of firm-year observations are automation spikes, these make up 11% of total investment in automation across all firms that we observe at least three times in the automation cost data. Further, among firms that have an automation cost spike, on average 54% of all observed automation costs occurs during such spikes (with a median of 52%): if automation costs were evenly distributed within firms over time, any one year would only correspond to 13% of the total outlay. This suggests that while firms also have outlays on automation that do not fall within our strict automation event definition, automation costs are lumpy.

4 Firm-level analyses

Following the recent literature examining changes in firm-level outcomes from firm-level automation that was summarized in the introduction, this section briefly discusses how our automation events correlate with firm-level outcomes, in particular firm-level employment and the firm’s average daily wage. Section 4.1 compares firms with an automation event to firms without an automation event, showing that these firms are on substantially different labor demand trajectories. Section 4.2 therefore presents an analysis of the impacts of automation only using the sample of firms with an automation event.

4.1 Comparing automating to non-automating firms

We first ask how firms with an automation event differ from those without an automation event: as outcomes, we consider growth in firm-level employment and in the firm’s average daily wage. In particular, we estimate variants of the following model:

$$\Delta \ln Y_{jt} = \beta \times A_j + D_t + \gamma \times X_j + \varepsilon_{jt}, \quad (2)$$

¹⁵Appendix C.3 shows this increase is driven by automation costs rising, not total costs decreasing.

where the dependent variables are annual log changes in employment, in the average daily wage, and in the wage bill for firm j in calendar year t . A_j is a dummy for the firm having an automation event over the 2000–2016 period, D_t are calendar year fixed effects, and X_j additional controls consisting of two-digit sector dummies and baseline firm-level characteristics.¹⁶ The term ε_{jt} is an error term and standard errors are clustered at the firm-level. The coefficient of interest, β , tells us whether automating firms experience different employment, mean daily wage, and wage bill trajectories.

Table 3 shows that automating firms have 1.5 to 1.9% higher employment growth but not higher daily wage growth compared to non-automating firms. Automating firms’ wage bills therefore grow faster compared to non-automating firms—Dunne and Troske (2005) find similar patterns for U.S. manufacturing firms that are adopting information technology. In Bessen et al. (2020), we show that these associations are not significantly different for manufacturing firms compared to non-manufacturing ones. In sum, these results show that, on average, automating firms have faster employment growth¹⁷ but not faster wage growth.¹⁸

4.2 Difference-in-differences only using automating firms

The employment growth for automating firms relative to non-automating ones does not rule out that automation at the firm level can be labor-saving when it occurs; such labor-saving effects would matter for individual workers employed in these automating

¹⁶Appendix D.1 estimates a linear probability model where the dependent variable is a dummy for the firm having an automation event over 2000–2016, showing that larger firms and firms paying higher wages are more likely to automate. We include initial-year values for these variables as additional controls to capture convergence dynamics.

¹⁷Appendix D.2 shows similar results when using a balanced panel of firms.

¹⁸Appendix D.4 shows that similar effects are found for importers of automation technology compared to non-importers, although the employment growth and wage bill differences are more than twice as large, reflecting that automation importers are much more positively selected.

firms. We therefore also consider the evolution of firm outcomes around an automation event, looking at the sub-sample of automating firms only and exploiting the timing of automation events for identification.

We set up our data in a stacked difference-in-differences design as in e.g. Cengiz et al. (2019).¹⁹ More specifically, we create separate datasets for each cohort of firms that have their first automation event in year c , with $c \in \{2003, \dots, 2011\}$. As before, τ is event time, i.e. calendar year t minus the calendar year c in which the firm has an automation event ($\tau \equiv t - c$). In each dataset we keep $\tau \in \{-3, \dots, 4\}$ as our event window. Then for each dataset, we add observations for the same calendar years on all firms that have an automation event in year $c + 5$ or later as controls. For example, our first cohort of treated firms are firms that have their first automation event in 2003. The event window surrounding this event contains the calendar years from 2000 to 2007. All control firms for the 2003-cohort are those firms that we observe over the same event window (2000 to 2007), and that have their first automation event later than 2007. These firms are “clean controls” in the sense that they do not have an automation event during the event window. 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 $\tau \in \{-3, \dots, 4\}$.²⁰

The by now well-documented problems related to two-way fixed effects in an event-study design arise from staggered treatment timing (de Chaisemartin and D’Haultfoeuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2020). 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

¹⁹For other recent papers using this setup, see for example Goldschmidt and Schmieder (2017); Deshpande and Li (2019); Clemens and Strain (2021); Baker et al. (2021).

²⁰To ensure that our results are not driven by selective survival of firms, we also ensure that our treatment firms survive until at least 5 years after the automation event. This does not affect our results.

effects in the case of staggered timing, just as the Callaway and Sant’Anna (2020) and Sun and Abraham (2021) approaches do.

Using this stacked dataset, we estimate the following difference-in-differences specification:

$$\ln Y_{jt} = \alpha + \sum_{\tau \neq -1; \tau = -3}^4 \beta_{\tau} \times I_{\tau} + \sum_{\tau \neq -1; \tau = -3}^4 \delta_{\tau} \times I_{\tau} \times treat_j + \eta_j + \theta_t + \varepsilon_{jt}, \quad (3)$$

where j indexes firms by cohort.²¹ I_t are leads and lags in event time, with $\tau = -1$ as the reference category. Treatment is indicated by the dummy variable $treat_j$ which turns on for firms that have an automation event in the window. Y_{jt} is j ’s employment or average daily wage and η_j are a set of cohort by firm fixed effects²² and θ_t a set of calendar year fixed effects.²³

We require two assumptions for a causal interpretation of δ_{τ} (e.g. Callaway and Sant’Anna, 2020; Borusyak et al., 2021).²⁴ First, we need that treated and control firms follow parallel trends in absence of treatment. We provide evidence for this assumption by showing that pre-event trends are mostly similar for firms that have an automation event now compared to those that have an automation event later. In addition, as we saw above, firms that do not have an automation event are on different trends than firms that do have an automation event. By relying only on differences in event timing, rather than also on event incidence, we can be more confident that we are comparing firms on similar trends.²⁵ Second, we need that firms do not anticipate an automation event, i.e. there should not be an effect of treatment in the future on current outcomes (e.g. Ab-

²¹This is because individual firms can appear multiple times in the stacked data.

²²We account for the fact that control firms can appear multiple times in the stacked dataset by including cohort by firm fixed effects (Baker et al., 2021).

²³Adding a full set of cohort by year fixed effects does not affect our findings.

²⁴Also see Appendix B for a more formal discussion in the context of our theoretical model.

²⁵Appendix D.3 shows results using non-automating firms as control group: we find diverging pre-trends in employment in this comparison. This is consistent with the

bring and Van Den Berg, 2003). This “no-anticipation” assumption is more difficult to maintain at the firm than worker level, because firms that decide to automate are more able to anticipate their own decision and this might affect other decisions they make in anticipation of the automation event. For this reason we will interpret these firm-level results as descriptive.

Results are shown in Figure 2 for two groups of firms: those with fewer than 500 workers, and firms with 500 workers or more (where firm size is determined at $\tau = -1$). First, we find that in firms with fewer than 500 workers, employment contracts following an automation event by about 20% compared to firms that have an automation event later. In contrast to employment, daily wages remain stable around these events. In the largest firms, wages actually grow by about 5% relative to the control group, but there is no significant change in employment. All in all, wage bills fall after the automation event in the smaller but not the larger firms. Bessen et al. (2020) show that while these effects are quantitatively larger (albeit more imprecisely estimated) in manufacturing firms, they are observed in non-manufacturing firms as well.

We also find that employment, wage, and wage bill trajectories for the subsample of automation importers are similar to those of the largest firms in Figure 2, with even more sizable positive impacts.²⁶ This is because there is a strong positive correlation between being an importer of automation technology and firm size²⁷, suggesting that using imports to measure automation involves important selection effects, as also argued by Bonfiglioli et al. (2021). Our main analyses, by contrast, rely on our measures of automation costs that are observed across different firm sizes.

evidence presented in section 4.1 and suggests that non-automating firms are not a good control group for automating firms in our case.

²⁶See Appendix D.4 for details.

²⁷Firms with automation cost spikes employ around 8.5% more workers than do firms without such spikes; by contrast, firms with automation imports are 131% larger than non-importers.

5 The impact of automation on individual workers

This section presents our main results: the impact of firm-level automation on individual workers. Section 5.1 outlines our difference-in-differences event-study design for identification of causal effects. In section 5.2 we present main results. Section 5.3 discusses effect heterogeneity, and in section 5.4 we present robustness tests.

5.1 Difference-in-differences event-study design

For the worker-level analysis we exploit our linked employer-employee data and follow a similar stacked difference-in-differences setup as in our firm-level analysis. Again, we create separate datasets for each cohort of firms that have an automation event in year c . We subsequently merge all workers who are employed at this firm in $c - 1$. These are our sets of treated workers. Similar to the firm-level analysis, we keep observations for these workers over the event window $\tau \in \{-3, \dots, 4\}$, with τ defined as event time ($\tau \equiv t - c$). Then, for each cohort c and the same set of calendar years, we add all workers employed in $c - 1$ at firms that have their first automation event in year $c + 5$ or later as control workers. For example, we observe the cohort of workers treated in 2005 over the event window 2002 to 2009. We pick our control workers such that we observe them over the same window from 2002 to 2009, and that in 2004 they are employed at a firm that has an automation event later than 2009.²⁸ We repeat this procedure for each cohort $c \in \{2003, \dots, 2011\}$ and stack the resulting datasets so that they line up in event time. We restrict our baseline analysis to incumbent workers: those workers with at least 3 years of firm tenure in $c - 1$.²⁹

²⁸We only require control group workers to be at a firm j in year $c - 1$. Hence, control group workers do not have to be employed at firm j when firm j actually automates in year $c + 5$ or later.

²⁹Appendices E.1 and E.2 provide further details on sample construction and summary statistics for workers.

We use the following difference-in-differences specification:

$$Y_{ijt} = \alpha + \beta \times treat_i + \sum_{\tau \neq -1; \tau = -3}^4 \gamma_{\tau} \times I_{\tau} + \sum_{\tau \neq -1; \tau = -3}^4 \delta_{\tau} \times I_{\tau} \times treat_i + \lambda X_{ijt} + \varepsilon_{ijt}, \quad (4)$$

where i indexes individuals by cohort, j firms by cohort, and $\tau \in \{-3, \dots, 4\}$ is event time.³⁰ Y_{ijt} is an individual-level outcome for worker i who must be employed at firm j in $\tau \in \{-3, \dots, -1\}$. Firm j can be a treatment group firm if j has an automation event in $\tau = 0$, or can be a control group firm if it has an automation event in $t + 5$ or later.

Turning to the right-hand side of equation (4), $treat_i$ is a treatment indicator for worker i if her firm j has an automation event at $\tau = 0$. Further, I_t are event-time indicators, with $\tau = -1$ as the reference category. Lastly, X_{ijt} are controls: these are a set of worker (age and age squared, gender, and nationality) and firm (sector and firm-level employment at $\tau = -1$) characteristics as well as year fixed effects. In our baseline specification, we replace $\beta \times treat_i$ with individual by cohort³¹ fixed effects which also absorb the non-time varying controls in X (gender, nationality, firm sector and employment at $\tau = -1$).³² We cluster standard errors at the level where treatment occurs: that is, all workers employed at the same firm in $t - 1$ are one cluster.³³

In equation (4), the parameters of interest are δ_t for $t \geq 0$: these estimate the period $t \geq 0$ treatment effect relative to pre-treatment period $\tau = -1$ (given that I_{-1} is the reference category). For example, if automation leads to an immediate decrease in wage income that equals 1% of annual labor earnings in $\tau = -1$, we have that $\delta_0 = -0.01$. Similarly, if automation leads to an annual wage income loss in $\tau = 4$ that equals 3% of annual wage income in $\tau = -1$, we have that $\delta_4 = -0.03$. The figures in the next section

³⁰Index i is for an individual worker within a cohort c since individual workers can appear multiple times in the data. The same holds for any firm j .

³¹We account for the fact that individual control workers can appear multiple times in the stacked dataset by using cohort by individual fixed effects (Baker et al., 2021).

³²Except when we estimate the hazard rate of a worker leaving the firm.

³³Adding a full set of cohort by year fixed effects does not affect our findings.

plot estimates of δ_τ for $\tau \in \{-3, \dots, 4\}$ for several worker-level outcomes: annual wage earnings, the hazard rate of leaving the firm, annual days in non-employment, annual benefit income, and the probability to retire early or to become self-employed.

Estimates of δ_τ can be interpreted as causal effects under the identifying assumptions of (i) parallel trends in the absence of automation events, and (ii) no anticipation of the automation event by workers. Our empirical approach directly supports these assumptions in several ways.

First, our specification strictly exploits differences in event timing rather than also using event incidence for identification. Only exploiting event timing across automating firms is important if firms without automation events are on different labor demand trajectories, as we have shown in the previous section: the outcomes for workers employed at firms without an automation event are not an appropriate counterfactual. Effectively, we are matching workers on the firm-level outcome of having an automation event at some point in time. Our use of timing differences across firms is in the spirit of a recent literature exploiting event timing differences in other contexts (see e.g. Duggan et al. 2016; Fadlon and Nielsen 2021; Miller 2017; Lafortune et al. 2018).³⁴

Second, our specification only considers incumbent workers, defined as those with at least 3 years of firm tenure in $\tau = -1$. On average across firms in our data, 64% of workers are incumbents (where the median is 68%). This captures workers who have a stable working relation with their firm.³⁵ This is important because identification requires that workers are not selected into the firm in anticipation of an automation event occurring in the near future. In this respect our worker-level analysis differs most clearly from the firm-level analysis: the no anticipation assumption is much more likely to hold for incumbent workers who are in a stable working relationships with the firm. This

³⁴In support of our approach, Appendix E.3 shows that the timing of automation events cannot be easily predicted from observed firm-level characteristics in our data.

³⁵Dutch labor law during almost our entire data period ensures temporary contracts are of a maximum duration of 3 years, implying that workers with 3 years of tenure are likely to have open-ended contracts.

reasoning is similar to the focus on incumbent workers in the mass lay-off literature (e.g. see Jacobson et al. 1993; Couch and Placzek 2010; Davis and Von Wachter 2011), where such lay-off events are assumed not to be anticipated for the firms' incumbent workers.

Third, we further strengthen the assumption of parallel trends by matching on worker and firm observables to ensure that $\delta_\tau = 0$ for all $\tau < 0$ (Azoulay et al. 2010). In our baseline specification, we match treated and control group workers on pre-treatment annual real wage income, separately by sector and calendar year. While the match is exact for calendar year and sector, we use coarsened exact matching (CEM, see Iacus et al. 2012; Blackwell et al. 2009) for pre-treatment income. To this end, we construct separate strata for deciles of real wage income, as well as separate bins for the 99th and 99.9th percentiles, in each of the three pre-treatment years $\tau = -3, -2, -1$. We then match treated workers to control group workers for each of these income bins, while additionally requiring them to be observed in the same calendar year and work in the same sector one year prior to treatment. We include calendar year and sector matching to ensure we are not capturing sector-specific business cycle effects, or other unobserved time-varying shocks affecting workers based on their original sector of employment. As such, each treated worker is matched to a set of controls from the same calendar year and sector and belongs to the same pre-treatment earnings bin.

Finally, parallel trends in the absence of treatment requires that the results we find are not driven by concurrent events unrelated to automation. In section 5.4 below, we therefore check the robustness of our results by eliminating firms with other events occurring inside the event-window, including take-overs, acquisitions, firm splits, and restructuring.

5.2 What happens to workers at firms that automate?

We now turn to our main findings. First, we discuss the impact of automation on incumbents' average annual wage income and its components: the probability to leave the firm, days in non-employment, and daily wages conditional on being employed. We then discuss the impact of automation on benefit income, on the probability to retire early,

and on the probability to become self-employed.

5.2.1 Annual wage income, firm separation, non-employment, and daily wages

We begin by estimating the impact of firm-level automation on individual workers' real annual wage earnings, scaled by wage earnings levels in $\tau = -1$ to obtain relative impacts. These estimates of parameters δ_t in equation (4) are shown in panel (a) in Figure 3, multiplied by 100 to capture percentage changes in annual real wage earnings relative to their $\tau = -1$ level.

The estimates highlight that incumbent workers lose wage income as a result of the automation event. Indeed, the average incumbent worker loses about 1% of annual wage earnings in $\tau = 0$; 1.6% (of annual wage earnings in $\tau = -1$) in $\tau = 1$; 1.9% in $\tau = 2$; 2.4% in $\tau = 3$; and 2.4% in $\tau = 4$. Overall, automation decreases annual wage earnings for an incumbent worker by 9.3% ($=1+1.6+1.9+2.4+2.4$) cumulatively of her annual wage earnings in $\tau = -1$ after 5 years. Given that annual wage earnings grow by 1.6% annually on average, this reflects a non-negligible loss compared to usual wage earnings trajectories. In euros, this 9% annual earnings loss corresponds to a cumulative real earnings loss of around 3,700 euros for the average incumbent worker over the 5 years following her firm's decision to automate.

These losses in annual earnings from work may be driven by changes in days worked following firm separation, changes in daily wages if employed, or a combination of both. To study the importance of firm separation, panel (b) in Figure 3 presents estimates from equation (4) where the dependent variable is the worker's hazard rate of separating from her pre-treatment employer. All coefficients have been multiplied by 100, such that the effects are in percentage points. The panel shows that automation leads to some incumbent workers leaving the firm: after 5 years, incumbent workers at automating firms have a statistically significant 1.7 percentage point higher hazard rate of firm exit. This is a non-negligible increase, given that the corresponding hazard among control group workers is 8.4%.

It is noteworthy that worker displacement does not occur instantly: rather, displace-

ment effects arise over time. There are various (and non-mutually exclusive) possible explanations for this. For one, these patterns are consistent with incumbent workers having open-ended contracts, making it costly and time-consuming to fire them. Further, these gradual changes could in part also result from a time delay in the effective implementation of automation technologies and work process changes relative to the cost outlay, or because it takes time for workers and firms to learn about changes to their match quality under the new technology. Gradual displacement following an automation event is in contrast to the well-documented phenomenon of mass lay-off events, where at least 30% of the firm’s incumbent workforce is laid-off at once (see Davis and Von Wachter (2011) for an overview).

Although our results so far show that automation leads to an increase in firm separation, this need not translate to losses in annual wage income if displaced workers find re-employment quickly (and at similar wage rates): we now turn to impacts on non-employment. Results are shown in panel (c) in Figure 3, where we define the dependent variable in equation (4) as the annual number of days spent in non-employment. Starting in the automation event year ($\tau = 0$), non-employed days for treated workers gradually increase relative to control group workers. In particular, non-employment increases by 1 day in the automation event year (although this estimate is not statistically significant), which increases to around 5.3 days annually after 5 years, with a total cumulative increase in non-employment of around 18 days compared to the control group. By comparison, in the event year, matched control group incumbents spend around 11 days in non-employment on average, suggesting automation produces an average increase of 9% in non-employment days in the automation year itself. The cumulative five-year impact corresponds to a 12% average increase relative to the five-year cumulative non-employment duration of 145 days experienced by control group incumbents.

We do not find strong evidence to support that automation affects incumbents’ wages conditional on employment, as shown in the final panel of Figure 3. Recall that we do not observe daily hours worked in our data: changes in daily wages can therefore result from

changes in hourly wages and/or changes in daily hours worked.³⁶ The absence of strong daily wage effects implies that the decrease in annual wage income for incumbent workers when their firm automates is largely driven by the observed rise in non-employment spells. This absence of wage scarring effects is in contrast to displacement effects from mass lay-offs or firm closures (Jacobson et al., 1993; Couch and Placzek, 2010; Davis and Von Wachter, 2011), which have been found in the Netherlands as well.³⁷

5.2.2 Annual benefit income, early retirement, and self-employment

Panel (a) in Figure 4 considers the impact of automation on incumbent workers' real annual benefit income (in euros), comprised of unemployment benefits, disability benefits, and welfare payments. We find that incumbent workers receive more benefit income following an automation event: after 5 years, the cumulative total amount received is 344 euros on average. Given that the average annual wage income loss cumulates to about 3,700 euros after 5 years (see above), this implies that less than 10% of the wage earnings loss from automation is offset by benefit payments. This finding is comparable to that for other worker displacement events, where typically only a small part of the average negative impact on earnings is compensated by social security (Hardoy and Schøne 2014).

Panel (a) in Figure 4 further shows that all of the additional benefit payments arise from unemployment insurance: this is expected, as unemployment benefit eligibility is very high among workers with at least three years of firm tenure.³⁸ Consistent with high

³⁶As shown in Appendix E.4, we do not find statistically significant impacts on log hourly wages for the subperiod where these are observed, suggesting our finding is not driven by mismeasurement.

³⁷See Deelen et al. (2018); Mooi-Reci and Ganzeboom (2015) who find evidence of substantial wage scarring after mass lay-offs in the Dutch context, using the same administrative data as we do here.

³⁸From 2000 to 2015, eligible workers in the Netherlands were entitled to up to 38 months of unemployment benefits following job loss. Since 2016, maximum eligibility is 24 months.

unemployment benefit eligibility, we do not see any increase in welfare payments. Lastly, the uptake of disability benefits is actually slightly decreasing for treated versus control group workers over time.

Panel (b) in Figure 4 examines whether automation also has an effect on early retirement, defined as the receipt of retirement benefits prior to reaching the legal retirement age. In particular, 5 years after the automation event, treated incumbent workers are 0.7 percentage points more likely to be observed in early retirement. While this effect might seem small in size, the average probability of early retirement among control-group incumbents in $\tau = 4$ is around 1.73%. As such, the treatment effect represents a 40% increase in the incidence of early retirement. Besides early retirement, the figure also examines the possibility that displaced workers enter self-employment. We do not find any effects there: estimates are very close to zero and statistically insignificant, highlighting that self-employment is not a compensating income source.

5.3 Effect heterogeneity

First, we consider effect heterogeneity by incumbent worker characteristics: firm size, worker age, and education level.³⁹ We find substantial heterogeneity in the effects of firm-level automation for incumbent workers employed in firms of different sizes, as measured by their number of workers in $\tau = -1$.

Annual treatment effects (averaged over the post-treatment period $\tau = 0$ through $\tau = 4$) are reported for six firm size classes in panel A of Table 4. Several results emerge. First, while the average incumbent worker experiences losses in earnings across firm size classes, these losses are generally larger for smaller firms: in firms with up to 50 employees, workers lose around 3.2% earnings annually over the four years following the automation event, whereas workers lose only 0.8 to 2.4% annually in larger firms. These differences are only partly driven by differences in firm separation and non-employment: automation leads to declining log daily wages (conditional on employment) for workers in smaller but not larger firms. Overall, this is consistent with the firm-level analyses re-

³⁹See Appendix E.5 for details.

ported in Section 4.2, where we found stronger wage growth and no employment losses for larger firms following automation events.⁴⁰ Since losses are higher for workers at smaller firms, we would probably find somewhat higher average wage losses from automation if our data were more representative in terms of firm size. Theory highlights that effects on incumbent workers are less negative in firms where productivity increases from automation are larger. Viewed through the lens of this model, our results are consistent with larger firms more effectively substituting capital for labor. This could for example arise if larger firms are better able to relocate workers across jobs within the firm.

Further, we find that workers aged 50 and older are most negatively affected by automation events, as shown in panel B of Table 4: differences with younger age groups are statistically significant, implying all other groups experience smaller or even negligible income losses. This is the result of older workers separating from the automating firm at higher rates, and experiencing larger increases in non-employment duration. Unsurprisingly (and not reported here), the early retirement effects we found are entirely driven by the oldest age group. Taken together, older workers appear to face substantially higher adjustment costs from automation than do younger workers.

Wage losses from automation also differ by workers' education level. We only observe education for a subset of workers, reducing statistical power. However, the estimates show losses are highest for middle-educated workers, and lowest for high-educated workers. This is consistent with a long literature which has documented that jobs that can at present be automated are predominantly concentrated in the middle of the wage distribution (see Acemoglu and Autor (2011) for an overview), although effects on aggregate skill demand may largely play out between rather than within firms as automating firms gain employment share (Doms et al., 1997; Dunne and Troske, 2005).⁴¹

⁴⁰Appendix E.5 shows that effects for incumbent workers employed at firms which import automation technology are very similar to the effects for larger firms.

⁴¹Appendix E.5 reports further effect heterogeneity results. We find a similar pattern as for workers' education level when using within-firm age-specific wage quartiles as our skill measure.

We find that effects are quite pervasive across sectors: this suggests that displacement effects are not limited to robotics or process innovations in manufacturing. However, as we do not observe the specific technology being adopted or work process being changed, our estimates—even within sectors—may capture an average across different automation technologies which individually may have more positive or negative effects on workers.

Lastly, we compare our results for incumbent workers to estimates for recent hires, defined as those with less than three years of firm tenure prior to the automation event.⁴² Unlike for incumbent workers, we find no income losses from automation for the average recent hire. This could be because recent hires have built up less firm-specific human capital, and therefore are more able to adapt to new job tasks either within the same firm or when moving to a new employer. However, it may also be that recent hires do not lose income because these workers are in part hired in anticipation of the automation event – in this case their outcomes are endogenous to the event.

5.4 Robustness tests

Our estimates can be interpreted as causal effects only under the identifying assumption of parallel trends in the absence of automation events and no anticipation of the automation event. A simple falsification test for parallel trends is to see whether $\delta_\tau = 0$ for $\tau < 0$, as seems to be the case in Figures 3 and 4. To go further, we can additionally match workers on their firms’ pre-treatment employment trends. This implies we now ensure that treated and control workers are not only employed at firms that experience an automation event at some point in time, but where pre-treatment employment growth is similar.

Another important concern is that automation events could coincide with other firm-level events. Our data include administrative information on several other important firm-level events, namely mergers, take-overs, acquisitions, firm splits, and restructuring. As a second robustness check, we therefore eliminate firms that experience such events anywhere in the event window. As a third robustness check, we remove outlier firms in terms of employment changes (those experiencing an employment change exceeding 90

⁴²See Appendix E.6 for details.

percent in any one year), both in the event window and outside of it. The removal of these outliers is intended to capture any firm-level events which are not formally documented in our administrative records. Fourth, we remove firms where there was a new worker among the firm’s top-decile annual wage income earners⁴³ in the three years prior to the automation event. This is intended to capture automation events coinciding with managerial change, which may bring changes in personnel policy unrelated to automation.

For all these robustness checks, estimates are very similar, though effects are somewhat smaller when eliminating firms with (suspected) management change: this suggests that automation may sometimes be the result of a new manager changing business practices. Overall, however, our findings are very robust, showing that firm-level events other than automation are unlikely to be the driving force behind the worker impacts we find.⁴⁴

As a further robustness check, we construct events for firm-level investments which we expect to be neutral from the perspective of incumbent workers: spikes in “other material fixed assets”. This is a residual category comprising items like furniture, shelving, silos, containers, and pellets. It excludes buildings, land, means of transportation, machines, communication equipment, and computers; as well as immaterial assets such as software and licenses. We construct spikes in other material fixed assets analogously to automation costs—these expenditures are also lumpy, allowing for the construction of spikes.⁴⁵ We estimate worker-level difference-in-differences models for the overlapping sample of firms where we observe both automation events and at least three years of data on other fixed material assets. In contrast to automation events, events based on other fixed material assets do not have any impact on firms’ incumbent workers.⁴⁶ This shows that the worker impacts we find for automation events are not a mechanical outcome of

⁴³Conditional on this worker earning at least 150 euros a day, i.e. 40,000 euros a year.

⁴⁴Appendix Figure E.4a in Appendix E.7.2 summarizes our estimates for relative annual wage income for all four robustness checks pertaining to firm-level pre-trends and events.

⁴⁵See Appendix E.7.1 for details.

⁴⁶Results are shown in Appendix Figure E.4b.

constructing firm-level events the way we do.⁴⁷

6 Automation versus computerization events

We have found that automation displaces incumbent workers: this raises the question whether this effect is specific to automation technologies or occurs with investment in new technology more generally.

Statistics Netherlands conducts a separate and partially overlapping firm survey on investments, including computer investments.⁴⁸ This item is called ‘computers’ or ‘computers and other hardware’ and consistently defined as follows: “All data-processing electronic equipment insofar as they can be freely programmed by the user, including all supporting appliances. Do not include software.” All investment within the company counts towards the expenditures, also if the equipment is second-hand, leased or rented, or produced within the company. It excludes investments in plants that are located abroad or resulting from take-overs of other organizations whose operations are continued without change.

We analyze the effects of computer investments in a similar way to that of automation events, and directly contrast it to the impacts of automation in the part of the sample where we have overlapping data. This serves two purposes. First, we can consider to what extent spikes in automation costs have different effects on workers than spikes in computer investment. Second, if automation cost expenditures and computer investments are correlated at the firm level, we can remove firms which have computer investment

⁴⁷Appendices E.7.3, E.7.4, and E.7.5 report results from three further robustness tests: 1) using alternative definitions of automation events, including different spike thresholds, 2) changing the model specification, and 3) performing a randomization test. Also here, we find that our results are robust.

⁴⁸We only consider computer investments because investments in software and communication equipment are only observed from 2012 onward. In 2012, software investments are of a similar magnitude as computer investments.

spikes within our estimation window to rule out that our automation event is partially capturing investment in computers. Conversely, we also estimate the effects of computer investments in isolation, that is, excluding any events where automation events occur within the estimation window.⁴⁹

Automation costs are higher than computer investments across the distribution, both in total and per worker⁵⁰, though of course both can come with other unmeasured correlated costs, such as software for computers, and machinery for automation. In order to compare automation to computerization events, we construct computer investment spikes in the same way we have for automation, but using computer investment per worker.⁵¹ We use the same threshold, assigning firms a computer investment spike if their computer investment per worker exceeds three times their usual level.⁵² Compared to automation spikes, computer investment spikes are more frequent. However, similar to automation events, there is a clear one-period increase in computer investments per worker when a firm has its computerization event: in the event year, treated firms spend around 2,500 euros per worker, compared to around 400 euros in the years before and after.

After restricting the overlapping sample further to firms that exist in all years in their computerization event window (as we also did for automation events), we construct four different datasets. First, we consider automation and computerization events in isolation, identifying treated and control group workers for one type of event while ignoring the

⁴⁹Throughout our analyses, we consider the overlapping sample of firms where we observe both automation events and at least three years of computer investment data. This means our dataset consists of 25,118 instead of 35,580 firms, and is more skewed towards larger firms as these are most likely to be sampled in both surveys.

⁵⁰As shown in Appendix F, along with further details of computer investments by sector and firm size.

⁵¹We use computer investments per worker because, unlike automation expenditures, computer investments are not part of total costs; and because total investments cannot serve as a denominator because they are inconsistently defined over our sample period.

⁵²The resulting distribution of computer investment spikes is reported in Appendix F.

other. This allows us to estimate our difference-in-differences event-study for automation and computerization separately. However, these two events are correlated across firms over time: firms that have recently had one type of event are more likely to also experience the other sometime soon, even in the same year. This implies any estimated impact of automation may be contaminated by computerization, and vice versa. We therefore construct two additional samples of events which occur in isolation: we only retain those automation (computerization) events where there is no computerization (automation) event occurring in the estimation window for either treated or control group firms. For each of the four samples, we then estimate Equation (4) and report results in Figure 5.

This comparison leads to several findings. First and foremost, computerization does not lead to wage earnings losses for incumbent workers: estimates are small and never statistically significant. This is in contrast to automation, which does lead to income losses for the average incumbent worker. Our findings are robust to considering automation events without concurrent computerization and vice versa. Consistent with these results, we do not find any increase in firm separation or non-employment duration for workers impacted by computerization. This means that automation is a more labor-displacing force than computerization from the perspective of a firm’s incumbent workers. This could be due to various factors, which we cannot disentangle here. First, computer technology could be less worker-displacing than automation overall because it more strongly complements worker capabilities. Second, firms’ investments in computer technology may be less displacing from workers’ perspective if these investments replace older vintages of computers (and computerization events reflect replacement costs), as this technology has already reached higher adoption levels. In that case, computerization effects reflect capital deepening, which, in contrast to automation, does not lead to worker displacement (Acemoglu and Restrepo, 2018).

7 Conclusion

We estimate the impacts of firms’ automation activities on individual workers, using firm-level data on automation expenditures across all non-financial private sectors in the Netherlands over 2000–2016. Leveraging a novel difference-in-differences event-study design exploiting spikes in automation costs, we show that automation at the firm significantly increases incumbent workers’ hazard of separating from their employers. This finding of course does not imply that automation destroys jobs on net in automating firms or in the economy as a whole. However, we show that automation can be disruptive for firms’ incumbent workers, leading to job churn and non-negligible adjustment costs.

Specifically, on average, these workers experience a 5-year cumulative wage income loss of 9 percent of one year’s earnings, driven by decreases in days worked. While we do not find evidence of substantial wage scarring, wage income losses are only partially offset by various benefits systems, and older workers are substantially more likely to enter early retirement. We document that these impacts are quite pervasive across different sectors of the economy, though income losses are larger for older workers, workers with medium levels of education, and workers employed at smaller firms. In contrast, we do not find evidence that incumbent workers face similar job and income losses from firms’ investments in computer technology. This suggests that, from the perspective of incumbent workers, automation is (at present) a more labor-displacing force.

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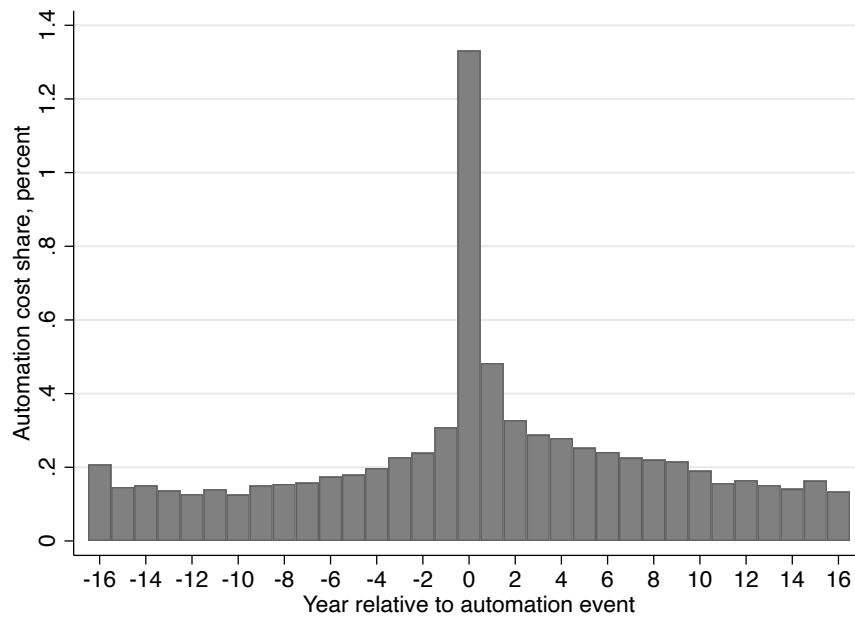
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Figures

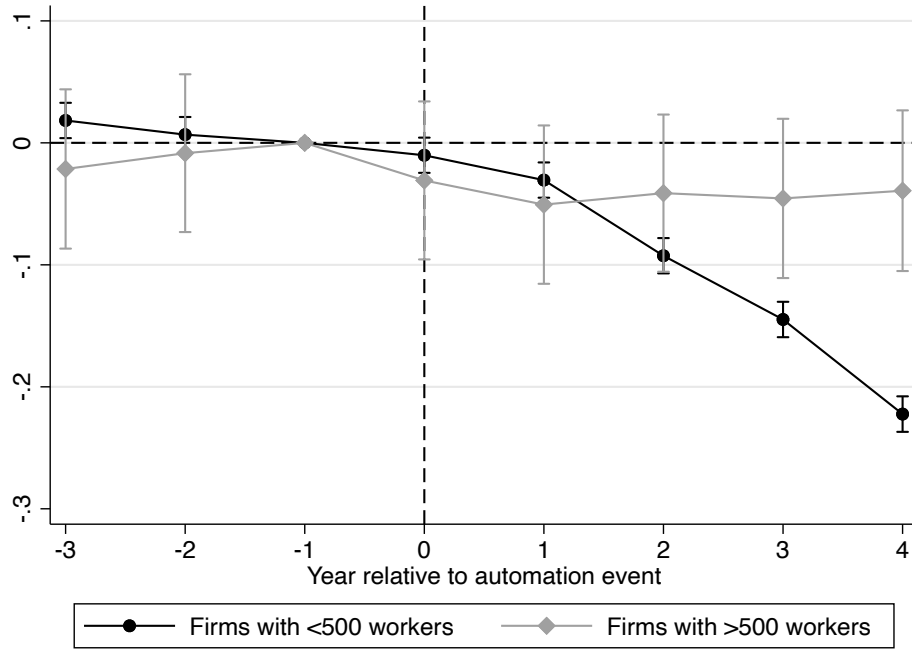
Figure 1. Automation cost shares around automation events



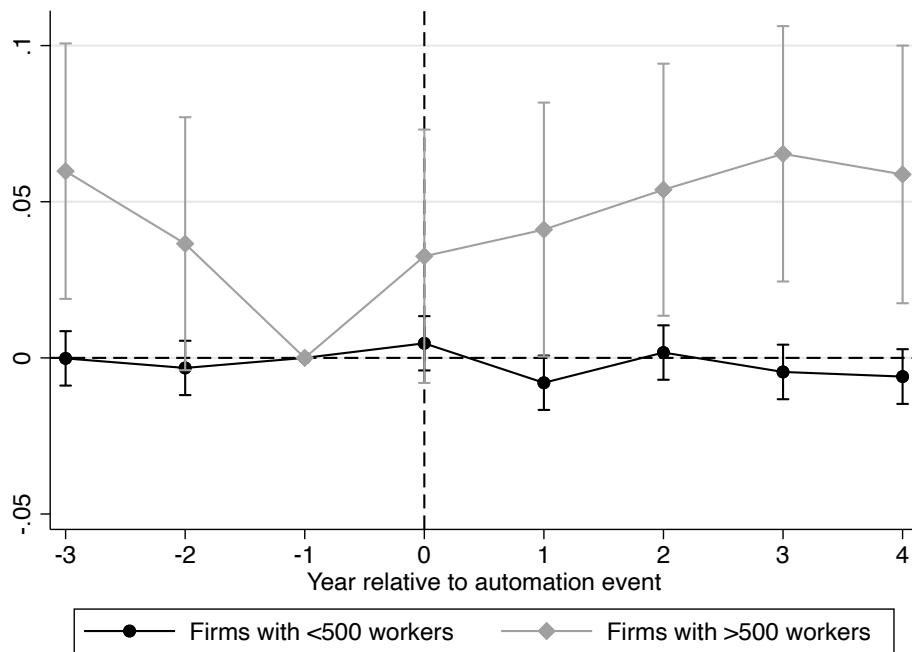
Notes: $N = 10,425$ in $\tau = 0$.

Figure 2. Firm-level outcomes for automating firms using event timing

(a) Log employment change

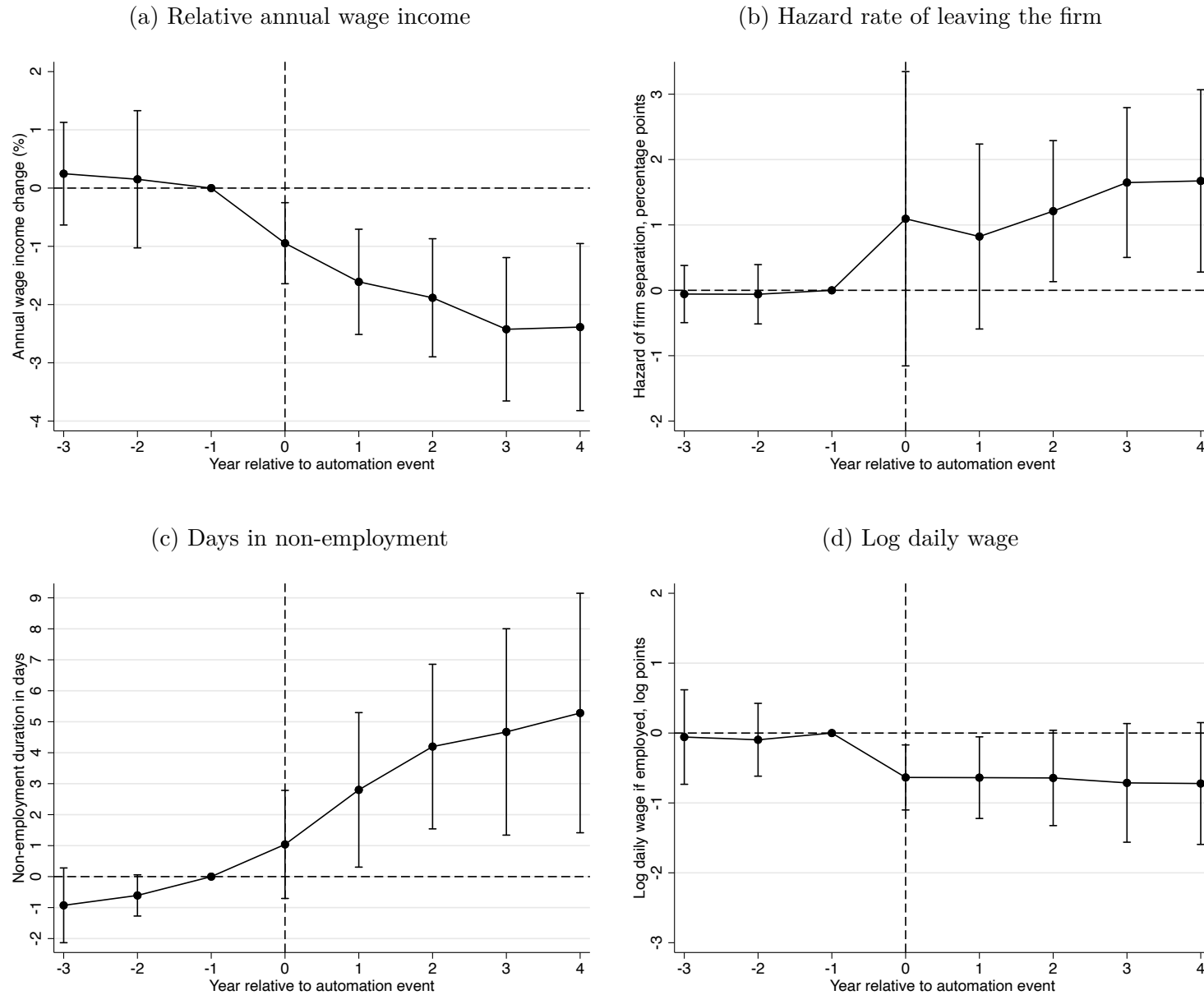


(b) Log daily wage change



Notes: All automating firms that exist in all years $\tau \in \{-3, \dots, 4\}$. Both models are weighted by firm-level employment size in $\tau = -1$. Whiskers reflect 95% confidence intervals.

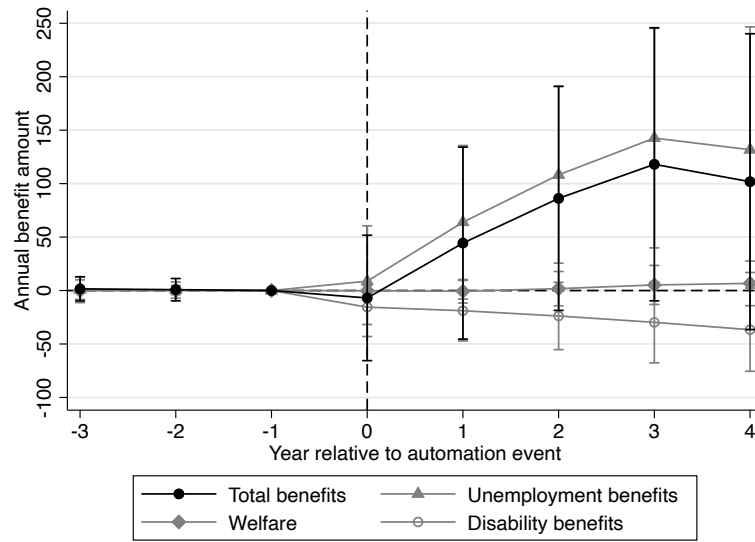
Figure 3. What happens to workers at firms that automate?



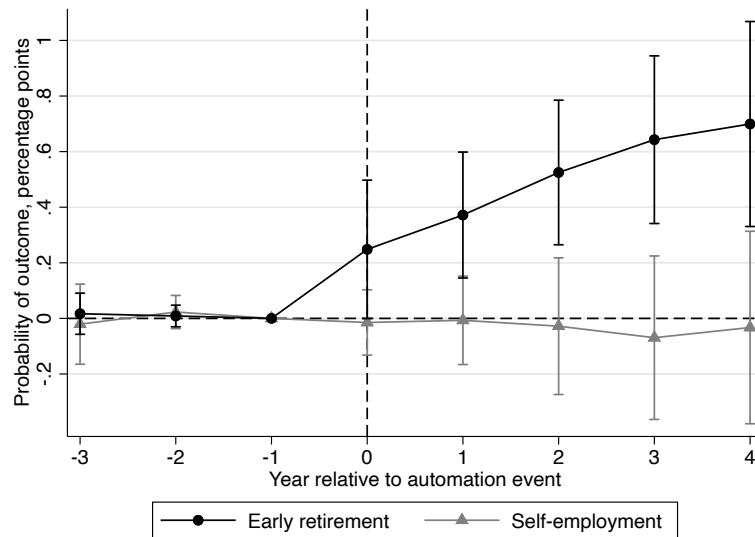
Notes: $N = 8,791,392$ for annual real wage income and days in non-employment, $N = 8,021,759$ for hazard of firm separation, $N = 8,429,129$ for log daily wages. Whiskers represent 95 percent confidence intervals.

Figure 4. Effect of automation on annual benefit income and early retirement

(a) Annual benefit income

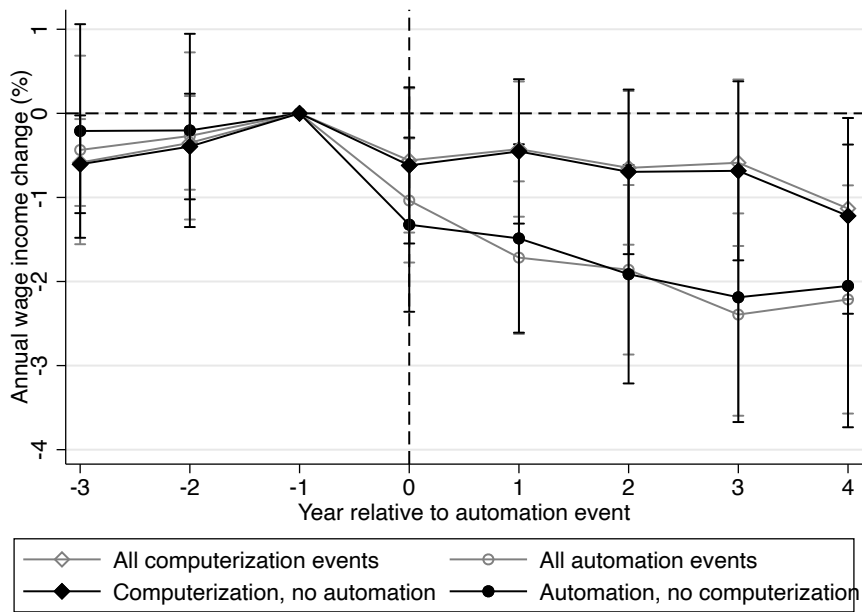


(b) Early retirement and self-employment



Notes: $N = 8,791,392$. Whiskers represent 95 percent confidence intervals.

Figure 5. Relative annual wage income effects of automation and computerization



Notes: All estimates are for the overlapping sample where we observe data on both automation costs and computer investments. $N = 10,022,288$ for all computerization; $N = 8,525,056$ for all computerization excluding automation; $N = 8,499,112$ for all automation; and $N = 5,167,464$ for automation excluding computerization.

Tables

Table 1. Automation costs

| | All observations | | | Automation costs >0 | | |
|------------------|------------------|-----------------|----------------|---------------------|-----------------|----------------|
| | Cost level | Cost per worker | Cost share (%) | Cost level | Cost per worker | Cost share (%) |
| p5 | 0 | 0 | 0.00 | 2,211 | 59 | 0.04 |
| p10 | 0 | 0 | 0.00 | 3,987 | 101 | 0.06 |
| p25 | 0 | 0 | 0.00 | 10,487 | 256 | 0.14 |
| p50 | 11,736 | 283 | 0.16 | 30,000 | 641 | 0.32 |
| p75 | 52,824 | 986 | 0.47 | 93,711 | 1,446 | 0.68 |
| p90 | 192,458 | 2,256 | 1.06 | 305,290 | 2,949 | 1.37 |
| p95 | 453,582 | 3,625 | 1.69 | 713,180 | 4,591 | 2.13 |
| <i>mean</i> | <i>211,307</i> | <i>1,045</i> | <i>0.44</i> | <i>307,794</i> | <i>1,522</i> | <i>0.64</i> |
| N firms × years | | 238,713 | | | 163,881 | |
| Pct with 0 costs | | 31% | | | 0% | |

Notes: Automation cost level and per worker are reported in 2015 euros, automation cost share is calculated as a percentage of total costs, excluding automation costs. The number of observations is the number of firms times the number of years.

Table 2. Automation costs by sector and firm size class

| | Total cost <i>Mean</i> | Cost per worker <i>Mean</i> <i>SD</i> | | Cost share (%) <i>Mean</i> <i>SD</i> | | Nr of obs <i>Firms</i> <i>Firms × yrs</i> | |
|-------------------------------------------|---------------------------|------------------------------------------|--------|-----------------------------------------|------|----------------------------------------------|--------|
| A. Sector | | | | | | | |
| Manufacturing | 430,125 | 1,078 | 7,065 | 0.36 | 0.58 | 5,513 | 44,375 |
| Construction | 78,225 | 457 | 1,463 | 0.20 | 0.36 | 4,429 | 28,182 |
| Wholesale & retail trade | 116,252 | 1,174 | 4,176 | 0.31 | 0.80 | 10,918 | 75,277 |
| Transportation & storage | 280,151 | 909 | 4,962 | 0.41 | 1.07 | 3,124 | 21,270 |
| Accommodation & food serving | 55,624 | 244 | 896 | 0.30 | 0.50 | 1,184 | 6,545 |
| Information & communication | 447,852 | 1,789 | 25,852 | 0.85 | 2.93 | 2,627 | 16,801 |
| Prof'l, scientific & technical activities | 149,069 | 1,283 | 5,490 | 1.02 | 1.75 | 3,977 | 23,581 |
| Administrative & support activities | 133,887 | 839 | 18,825 | 0.50 | 1.18 | 3,808 | 22,682 |
| B. Firm size class | | | | | | | |
| 1-19 employees | 12,269 | 921 | 14,568 | 0.40 | 1.30 | 9,499 | 48,073 |
| 20-49 employees | 27,689 | 893 | 4,547 | 0.42 | 1.34 | 13,424 | 86,553 |
| 50-99 employees | 61,601 | 954 | 4,345 | 0.42 | 0.96 | 6,192 | 47,072 |
| 100-199 employees | 144,891 | 1,135 | 5,812 | 0.44 | 0.94 | 3,413 | 28,666 |
| 200-499 employees | 406,461 | 1,573 | 21,305 | 0.51 | 1.11 | 1,943 | 17,868 |
| ≥500 employees | 3,161,867 | 2,124 | 14,294 | 0.76 | 1.60 | 1,109 | 10,481 |

Notes: Automation costs in 2015 euros, automation cost shares as a percentage of total costs, excluding automation costs. Total N firms is 35,580; Total N firms × years is 238,713.

Table 3. Firm-level outcomes for automating vs non-automating firms

| | Δ log employment | | Δ log daily wage | | Δ log wage bill | |
|---------------------|-------------------------|---------------------|-------------------------|-------------------|------------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Automating | 0.019*** (0.006) | 0.015*** (0.006) | -0.002 (0.003) | -0.002 (0.002) | 0.016*** (0.005) | 0.013*** (0.005) |
| Additional controls | No | Yes | No | Yes | No | Yes |

Notes: N = 168,091 firm-year observations, where 10,425 out of 35,580 unique firms automate. All models include calendar year fixed effects. Additional controls are two-digit sector dummies and initial-year values for log employment and log mean daily wage. All models are weighted by the inverse of the number of firm-level observations multiplied by average firm-level employment size. Standard errors are clustered at the firm-level. *p<0.10, **p<0.05, ***p<0.01.

Table 4. Heterogeneity in incumbent worker impacts

| | Annual wage income | Separation hazard | Days non- employed | Log daily wage |
|---------------------------------------------|--------------------------|----------------------|-----------------------|--------------------|
| <i>A. Firm size</i> | | | | |
| 1–19 employees (reference) | -3.17*** (0.76) | 1.83*** (0.37) | 4.25*** (1.39) | -2.21*** (0.48) |
| <i>Deviations from reference group for:</i> | | | | |
| 20–49 employees | 0.23 (0.91) | -0.13 (0.46) | -0.19 (1.64) | 0.87 (0.57) |
| 50–99 employees | 2.42** (0.96) | -0.47 (0.57) | -3.26* (1.78) | 1.80*** (0.62) |
| 100–199 employees | 1.35 (1.11) | -1.38** (0.69) | -1.93 (2.09) | 1.00 (0.69) |
| 200–499 employees | 2.21* (1.16) | 0.07 (0.90) | -1.21 (2.38) | 2.54*** (0.74) |
| ≥500 employees | 0.77 (1.51) | -0.17 (2.57) | 1.25 (3.19) | 1.77* (0.94) |
| N | 8,791,392 | 8,021,759 | 8,791,392 | 8,429,129 |
| <i>B. Worker age</i> | | | | |
| Age ≥50 (reference) | -3.97*** (1.25) | 2.82*** (0.92) | 10.14*** (2.28) | -0.45 (0.56) |
| <i>Deviations from reference group for:</i> | | | | |
| Age 40–49 | 2.62* (1.36) | -1.17** (0.58) | -7.56*** (2.64) | -0.15 (0.49) |
| Age 30–39 | 2.25* (1.27) | -1.63** (0.74) | -7.39*** (2.68) | -0.55 (0.66) |
| Age 20–29 | 3.20* (1.71) | -1.54 (1.07) | -9.15*** (2.70) | 0.40 (0.91) |
| N | 8,791,392 | 8,021,759 | 8,791,392 | 8,429,129 |
| <i>C. Worker education level</i> | | | | |
| Medium education (reference) | -2.54*** (0.77) | 2.81 (1.97) | 5.25** (2.21) | -1.13*** (0.41) |
| <i>Deviations from reference group for:</i> | | | | |
| Low education | 0.86 (1.47) | -1.42 (1.33) | -0.78 (2.34) | 0.76 (0.90) |
| High education | 1.21* (0.70) | 0.30 (1.31) | -2.15 (1.64) | 0.78 (0.70) |
| N | 2,511,583 | 2,177,563 | 2,511,583 | 2,413,426 |

Notes: All coefficients are average annual effects over the post-treatment period ($\tau = 0$ through $\tau = 4$). Coefficients for wage income, separation hazard, and log daily wages have been multiplied by 100 to reflect percentages. Education is classified using ISCED-2011: low is up to and including lower secondary education; medium is upper secondary or post-secondary education excl. tertiary education; and high education is tertiary education. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.