Robots and Workers: Evidence from the Netherlands

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Abstract — We estimate the effects of robot adoption on firm-level and worker-level outcomes in the Netherlands using a large employer-employee panel dataset spanning 2009-2020. Firm-level analyses confirm previous findings with positive effects on value added and hours worked for robot adopting firms and negative outcomes on competitors. Our worker-level results show that directly-affected workers (*e.g.*, bluecollar workers performing routine or replaceable tasks) face lower earnings and employability, while other workers indirectly gain from robot adoption. These results highlight the uneven effects of automation on the workforce.

Keywords — robots, workers, technology, productivity, the Netherlands

JEL codes — D63, E22, E23, E24, J24, O33

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1 Introduction

Industrial robots have spread rapidly in most advanced economies as well as in some emerging ones. Based on the International Federation of Robotics (IFR), in the US, the number of robots per 1000 industrial workers has increased from 0.35 in 1993 to 1.49 in 2014, while the same increase has been from 1.2 to 6.8 in the Netherlands. Although industrial robots have automated a variety of production tasks from painting to welding, sorting and assembling, and in many cases boosted productivity, their effects on workers are debated.

Firm-level studies on the effects of robot adoption paint a mixed picture. Most of these studies find that robot-adopting firms not only increase their productivity but also expand their employment (see, for example; Acemoglu et al. 2020 for France; Koch et al. 2021 for Spain; Dixon et al. 2021 for Canada; Humlum 2019 for Denmark; Acemoglu & Restrepo 2022 for the US). These firm-level outcomes reflect several forces, however. First, robot-adopting firms are typically more productive and often on a different trend than non-adopters (*e.g.* Koch et al. 2021, Acemoglu & Restrepo 2022). Second, adopters may be expanding at the expense of rivals in the same industry (Acemoglu et al. 2020). Because of this equilibrium effect of robots, overall industry or nation-wide employment could decline as non-adopting competitors significantly reduce employment. This is the pattern found by Acemoglu et al. (2020) and Koch et al. (2021) as well as by Bessen et al. (2020) for the Netherlands and Bonfiglioli et al. (2020) for France.[1]

Studies focusing on equilibrium implications of robots typically find negative effects on employment and wages. For example, Acemoglu & Restrepo (2020) estimate negative impacts on workers — especially low- and mid-skill workers and those in manufacturing and in bluecollar occupations — in US local labor markets who are more exposed to the spread of industrial robots. Dauth et al. (2021) estimate similar negative wage and employment impacts in manufacturing in Germany, but the negative employment effects are more muted than those in the US and are compensated by local expansion of non-manufacturing employment. Acemoglu & Restrepo (2022) estimate negative effects on wages and employment on demographic groups most exposed to automation, driven by robots and specialized software. Graetz & Michaels (2018) and Acemoglu & Restrepo (2020), Acemoglu et al. (2022) also report negative effects on the labor share at the industry level.

Nevertheless, we are far from a consensus on what types of workers are affected by robot adoption and what the impact of robotization is on individual workers. Acemoglu & Restrepo (2020), Acemoglu et al. (2020), Dauth et al. (2021) and Humlum (2019) estimate negative effects on production workers and Bonfiglioli et al. (2020) and Barth et al. (2020) estimate negative impacts on low-skilled workers. In contrast, Aghion et al. (2021) estimate positive employment effects, even for unskilled production workers in France, while Hirvonen et al. (2022) do not find negative effects for low-skilled workers in Finland.

We contribute to this emerging literature in two ways. First, we confirm several of the important firm-level and industry-level findings of the literature using high-quality Dutch employer-employee panel dataset on robots, firms and workers. Robot-adopting firms increase output by about 14.9%, increase employment (hours worked) by 4.3% and reduce the labor share by 4.6 percentage points,

[1] Aghion et al. (2021), on the other hand, find positive firm-level and industry-level effects, focusing on various proxies of equipment investment (rather than direct measures of robot adoption).

relative to comparable non-adopting firms. The quantitative magnitudes of these estimates are very similar to those from France and Spain. Also as in these countries, we estimate negative effects on non-adopting rivals in the same industry. For example, a non-adopting firm experiences a 6.2% decline in hours worked when the competition by robot adopters in the same four-digit industry increases by one standard deviation.

Second, we investigate the impact of robots adoption on workers. To shed light on the heterogeneous effects of robot adoption on different types of workers, we distinguish "*directly-affected*" workers from those that are "*indirectly-affected*". To motivate this distinction, recall that, as emphasized in Acemoglu & Restrepo (2020), robot adoption creates a negative *displacement effect* on workers whose tasks are being replaced, while also generating a positive *productivity effect*, indirectly benefiting workers as non-automated tasks expand. In line with this distinction, directly-affected workers are more likely to have been working in tasks that will be automated by robots and thus more prone to be displaced. We construct three alternative, though complementary, measures of directly-affected workers. The first is bluecollar workers employed in routine tasks (constructed using the routine task intensity index developed in Autor & Dorn 2013 and Koster & Ozgen 2021). Previous work has documented that these workers are more likely to perform tasks that can be more easily automated (see *e.g.* Autor & Dorn 2013, Oesch 2013) and have tended to be more adversely affected by the adoption of automation technologies at the aggregate level (see *e.g.* Acemoglu & Restrepo 2020, Bonfiglioli et al. 2020, Barth et al. 2020). The second measure is based on the replaceability index of Graetz & Michaels (2018) and, similarly, captures workers in occupations that can be more easily replaced by automation. The third measure simply focuses on the highest completed level of education by a worker.

Using all three measures, we find that workers who do not perform routine production tasks indirectly gain from robot adoption, while routine production workers, workers in replaceable occupations, or low-education workers lose out. These patterns are similar when we look at the effects of robot adoption on non-adopting rivals. The negative effects of robot adoption on workers employed in routine production work and replaceable occupations is predominantly through lower wages. The much smaller impacts on employment are broadly consistent with the idea that rigidities may be leading to slower or even muted quantity adjustments in the Dutch labor market.

Our discussion so far has already placed our work in the context of the recent literature. Here we only add that our paper is distinguished by the use of high-quality, longitudinal data on robot adoption matched to a panel of employer-employee administrative data and by the length of the period covered. We build our comprehensive measure of firm-level robot adoption by linking *International Trade Register* data to firm-level *Production Statistics*. In the Dutch context we are able to do this for the period covering 2009-2020, which gives us a longer sample than in Acemoglu et al. (2020) and, more importantly, we are able to study *worker* level outcomes. The use of actual, longitudinal robot data also distinguishes our paper from Aghion et al. (2021) for France and Bessen et al. (2020) for the Netherlands, which use proxies for automation; from Acemoglu et al. (2022) who use cross-sectional data on automation technologies and robots for the US; and from Hirvonen et al. (2022) for Finland, who focuses on a variety of advanced equipment, which includes other automation and non-automation technologies as well as robots.

This paper continues as follows. In Section 2 we outline the data construction and introduce our summary measures based on the task content of occupations. Section 3 analyses firm-level outcomes, followed by worker-level outcomes in Section 4. Section 5 concludes.

2 Data

2.1 Data description

This study benefits from a number of administrative datasets provided by the *Statistics Netherlands*. We combine a number of datasets namely: *Production Statistics*, *Tax Registers*, the *International Trade Register*, *Labor Force Surveys*, *Investment Statistics* and the *Firm Register*.

2.1.1 Firm-level data

Production Statistics constitute the core of our analysis on firms. They include very detailed firm-level information on firms' production input/outputs such as number of employees, value added, sales, total costs, personnel costs and total wage bill. The dataset contains all firms that have 50 employees and above, and a representative sample of firms smaller than 50 employees per year for the 2000-2020 period. We observe around 55 thousand unique firms per year. We focus on manufacturing firms but we use a broader definition of the manufacturing industry that includes manufacturing, energy, water and waste, construction, mining, and transportation.

We link *Production Statistics* to the *Tax Registers*, which is based on the employers' tax declarations. It includes employees that are employed at or work for formally registered firms. Hence, self-employed that do not work at formally registered firms are not included. We observe around 10 million employees per year, with detailed job histories and monthly wages and hours worked. By linking *Production Statistics* to *Tax Registers*, we construct a near universe employer-employee dataset (LEED) dataset on active firms in manufacturing industry and their employees over time.[2]

Following the literature we calculate the labor share as the total wage costs over gross value added (GVA). We set the labor share to missing if it is larger than one (which holds for about 4% of the cases).

Tax Registers include two main job related measures that are annual earnings before tax and hours worked in a year. From this we calculate hourly wages. In the analysis, to enable comparability we drop firms from the LEED dataset where at least one of the following variables; GVA, labor share, sales or total hours worked are coded as missing. Moreover, firms with labor share larger than 1 and firm size larger than 25 thousand are also dropped from the dataset. These selections decrease the number of observations by about 30%. This reduction is mainly caused by the limited coverage of *Production Statistics* of the small firms with less than 50 employees. However, we do not believe this is a major problem as we will show that essentially only large firms are robot adopters.

The *International Trade Register* (*ITR*) includes all trade transactions in the Netherlands with other countries at the firm level from 2009 onwards, yet makes a distinction between within-EU

[2] Production statistics covers population of firms with 50+ employees and a representative sample of firms that have fewer than 50 employees.

trade and non-EU trade. With respect to trade with non-EU countries, the information is gathered from the customs data. With respect to trade within the EU, Statistics Netherlands runs their own survey called *Intrastat*. Enterprises that import and/or export goods to the EU in excess of € 1.2 million in a year are required to specify which goods they traded and with which member state. Overall, the *ITR*, together with the *Intrastat* survey, roughly contain 80% of total Dutch imports and exports (in value) that can be attributed to a firm.

We can trace robot importing firms based on the specific commodity code, 847950, in line with the international trade codes of commodities. An important concern is if due to the threshold value of € 1.2 million we are missing out significant number of robot imports from the EU countries. One advantage of our data is that the threshold value applies to *total* imports value of a firm in a year from an EU country, meaning that the commodity code registration is not exclusively linked to the value of a single item imported. In other words, when a firm imports from *e.g.* France, for each item we would know the commodity code even as small as € 200 unless firm's total number of imports from France remains under less than € 1.2 million in a year. This would make it very unlikely that we will be missing out major robot imports from within the EU, as our dataset mostly consists of 50 employees or more, which easily trade more than € 1.2 million a year with EU countries. We then define robot-adopting firms that have cumulative imports of robots exceeding the median value of robot imports, which is € 2500, as imports below this value are unlikely to be referring to significant industrial robots that may be influential enough to change the course of production.[3] Moreover, robot production in the Netherlands is negligible, therefore we are not likely to miss out significant robot adopters in the country because we focusing on robot imports.

In Appendix A.1 we discuss two other datasets that we link to *Production Statistics* data, one on investments and another on the age of firms. By combining these seven datasets, we create a thorough picture of robot-adopting firms between 2009 and 2020.

2.1.2 Worker-level data

After constructing the LEED data, we link it to the *Demographic* register that contains the universe of population in the Netherlands, hence information on workers' age, gender, and whether a worker is born in the Netherlands or not. The resulting worker-level dataset contains almost the universe of employees in all sectors, though in our analysis we focus on the broader manufacturing sector where robot adoption is most prevalent. We then keep the working population by dropping workers that are younger than 18 or older than 67. We further drop all observations for an employee who earned more than half a million euros; worked more than $4,380$ hours; earned less than € 2.5 and more than € 500 hourly wages per annum. These selections correspond to around two standard deviations from the mean of each indicator. We further focus on workers that had a job at one employer in a given year. Our final data is a more or less balanced panel of 1.7 million workers that have been employed in manufacturing at least once between 2009 and 2020.

For each worker we have longitudinal information on hourly wages and hours worked when the worker is employed in a certain firm. This means we know whether a worker is employed in a

[3]We have tested different cut-off values based on the robot import value distribution, but this does not materially influence our results.

certain year. If a worker is not in employment in a certain year, we cannot associate her/him with firm characteristics. To be able to analyze the impact of robot adoption on the probability to be in employment for this worker, we assign firm characteristics of the last firm the worker has been employed.

An issue with the non-participation is that the *Tax Registers* do not include workers who are self-employed. Hence, instead of being unemployed, workers may for example have set up their own firm. In order to obtain information on whether a worker is in fact unemployed or not we merge in the so-called *Personal Income* data. These data include information on the total income and disaggregated income resources, such as rental income, of the universe of the population as well as the employment status. By combining our data with the *Personal Income* dataset, we are able to distinguish unemployed from those who are not participating in work due to other reasons such as retirement or study. Furthermore, we obtain the type of household, such as whether a worker lives with her/his partner or lives together with multiple adults on the same address.

2.1.3 Task content, education and the most affected workers

The effect of robot adoption is likely to be uneven across workers, especially when the skill levels and complexity of tasks workers perform are taken into account. In order to investigate the heterogeneous effects of robot adoption on workers, we define groups of workers that are potentially adversely affected by robots.

For this, we link our worker-level LEED dataset with observations in the year of observation or the 9 years preceding the year of observation to *Labor Force Survey*s (*LFS*s). We use a 10-year window in order to increase the number of observations of workers, because the LFS only contains about a couple of hundred thousand observations per year on the whole Dutch workforce. From the *LFS*s we obtain information on occupations and the education level for each worker. Because in the Netherlands, education and occupation levels of the employees can only be observed from the *LFS*, it provides us with the necessary information on education and occupations, although this comes at the expense of not analyzing the universe of employees.

We construct three measures describing the education level and task content of a job in a worker's occupation. When the workers fall in one of the three groups below, we label them *directly-affected* workers, denoted by a_{it}. We label all other workers *indirectly-affected* workers, as they are likely to experience the adoption of robot technology effects indirectly, for example through productivity, reorganization, reallocation to new tasks.

Bluecollar-routine workers. First, we focus on the effect of robot adoption on firm and worker outcomes when bluecollar workers perform highly routine tasks. These workers are expected to be impacted the most from robot adoption, since robots are particularly (at least for the time being) designed to accomplish routine tasks. Using the occupational codes of workers, we create a task complexity index, namely the routine task intensity index (RTI), following Autor & Dorn (2013) and Koster & Ozgen (2021). The RTI informs us on the task content of occupations workers perform. For the exact definition of the RTI, we refer to Appendix A.2. We use the definition by *O*NET Online* to define bluecollar occupations.

Bluecollar-routine workers are defined as follows:

$$a_{it} = \max_{\tau=-9,\ldots,t} \big(I(\mathcal{B}_{io\tau} = 1) \times I(\mathcal{RTI}_{io\tau} > 1)\big), \tag{1}$$

where $\mathcal{B}_{io\tau}$ is an indicator variable whether worker i is in a bluecollar occupation o in year τ. We use a 10-year window prior to the year of observation, to match the workers to the LFS on whether these workers are in bluecollar-routine occupations. Using a 10-year window also help to increase the number of observations[4]

Similarly, $I(\mathcal{RTI}_{io\tau} > 1)$ is an indicator function that equals one when the routine-task-intensity index exceeds 1 in τ. According to this definition, about 11% of the workers in the Dutch broader manufacturing industry during the study period are bluecollar-routine workers.

Replaceable workers. One may argue that not all routine-bluecollar workers are susceptible to robot adoption. Although some occupations require performance of highly routine tasks, they still need to be complemented by non-routine tasks which may require assessment and discretion, *e.g.* a call center agent, metal working machinist, wood cutting operator and metal driller.

To account for these differences, we construct a worker-level replaceability index at the 4-digit ISCO level. Our replaceability index is based on the description of robot applications by the *International Federation* of *Robots* (*IFR*), occupational classifications in the US Censuses, and the distribution of hours across occupations and industries for weighting as in Graetz & Michaels (2018). *IFR* distinguishes the applications that can be executed by robots on the basis of tasks such as welding, assembling and painting. If an occupational title includes one of there keywords we assign the value of 1 to that occupation to indicate that workers in that occupation is replaceable by robots. To apply this measure to the Dutch occupational classification, we use a crosswalk to concord the occupations from SOC to ISCO. Similar to the definition of bluecollar-routine workers we look at a worker's occupation within a 10 year window. Hence,

$$a_{it} = \max_{\tau=-9,\ldots,t} \big(I(\mathcal{V}_{io\tau} = 1)\big), \tag{2}$$

where $\mathcal{V}_{io\tau}$ is an indicator variable whether a worker performs a replaceable job in year τ.

Low-education workers. The final measure of workers likely to be adversely affected by robot adoption is based on education. We generate a measure of low-education workers by using the educational classification in the Dutch *LFS*. For this, we assign workers to have a low education when the highest level of obtained educational degree corresponds to secondary education. Hence, these workers would have in total a maximum of 10 years of primary and secondary education. Our measure is then:

$$a_{it} = \max_{\tau=-9,\ldots,t} \big(I(\mathcal{E}_{io\tau} = 1)\big), \tag{3}$$

[4]By adopting a 10-year window we assume that workers do not change occupations that often. We think this assumption is plausible as Visser et al. (2018) show that occupational mobility in the Netherlands is not common, particularly not for groups that are affected by robots. There is significant path-dependency in terms of job changes, and this trend is even stronger for occupational changes. Moreover, in the study period almost 70% of the employees have not experienced an earnings transition in consecutive years, even independently of occupational mobility (Bachmann et al. 2020) (an earnings transition is defined as a switch from one decile of the country- and year-specific earnings distribution to another decile). Therefore, potential occupational mobility is not likely to affect our results.

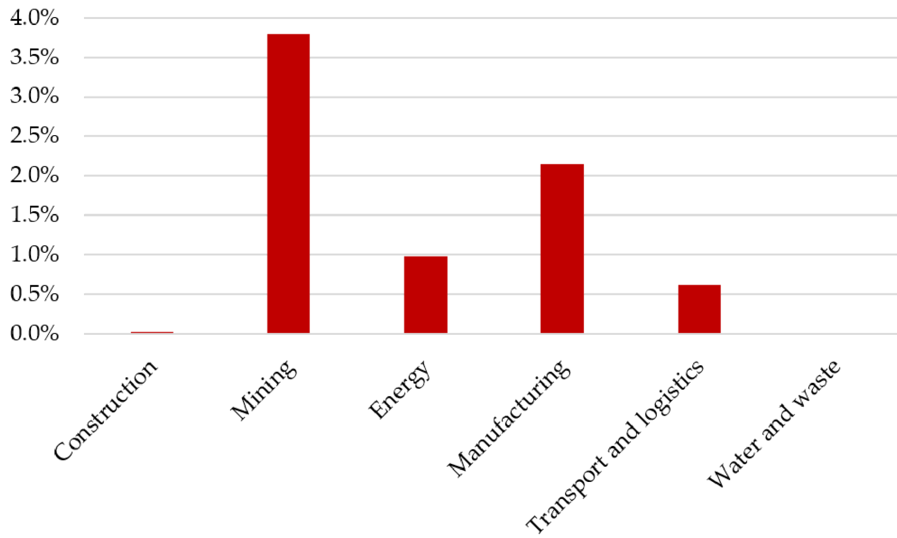


Figure 1 – Robot adoption by sector

where $\mathcal{E}_{io\tau}$ denotes the educational classification.

2.2 Descriptive statistics

2.2.1 Firm-level data

Our yearly unbalanced panel data spans 12 years and includes $162,220$ firm-year observations and $46,914$ thousand unique firms. We observe 218 unique robot-adopting firms (0.4%). Although only a small fraction of firms are adopting robots, they tend to be larger, and thus 6.8% of the workers in our sample are employed in a firm that adopt robots at some point during our time window.

As shown in Figure 1 robot adoption concentrates in the (narrowly-defined) manufacturing sector (2.1%). Other sectors with substantial robot adoption are mining (3.8%), energy (1.0%), and transport and logistics (0.6%). Figure 1 shows the percentage of robot purchased firms by sectors in 2020. There is a positive secular trend in robot adoption over the 12 years both at the sector and at the firm level, for instance the correlation between firms' import value of robots between t and $t-1$ is 0.76.

Because it takes time to observe the effects of robot adoption, especially in a highly-regulated labor market like the Netherlands where laying off workers is costly and time-consuming, effects at the firm level may take place with long and variable lags, and thus we also look at long-differences models, focusing on the years 2009 and 2020. The sample now includes 3,989 unique firms, 1.1% of which have adopted robots. Table A1 in Appendix A.3 reports descriptive statistics for the 2-wave balanced panel, indicating very similar values to those in Table 1.

Panel A of Table 1 presents descriptives of the main variables of interest for the unbalanced panel of firms between 2009 and 2020. This will be our main dataset throughout the paper from which we will make further selections depending on the type of the analysis. The mean hourly wage equals € 24. There are higher number of large firms in the dataset with average firm size 91 (in number of employees), while the median is considerably lower (*i.e.* only 31), reflecting the usual skewed

Table 1 – Summary statistics of 12-wave unbalanced panel 2009-2020

	mean	std. dev.	5^{th} perc.	Median	95^{th} perc.	N
Panel A: All firms	(1)	(2)	(3)	(4)	(5)	(6)
Gross value added in market prices *(in 1000 €)*	8,689	72,694	121.6	1,766	24,984	162,220
Hours worked	145,617	560,190	2,086	46,622	480,545	162,220
Number of workers	90.60	339.1	2	31	295	162,220
Labor share	0.553	0.189	0.199	0.574	0.841	162,220
Total wage bill *(in 1000s)*	3,681	18,536	43	965	12,209	162,220
Mean hourly wage *(in €)*	24.39	31.14	10.43	19.65	40.63	158.449
Sales per hour worked *(log)*	227.6	620.2	38.11	101.0	629.4	159,567
Robot adopter	0.00623	0.0787	0	0	0	162,220
Competition by robot adopters	0.0257	0.0993	0	0	0.148	161,694
Panel B: Robot-adopting firms	(1)	(2)	(3)	(4)	(5)	(6)
Gross value added in market prices *(in 1000 €)*	85,844	309,884	1,405	12,935	368,738	1,712
Hours worked	912,129	1,788,000	30,360	279,157	3,924,000	1,712
Number of workers	528.1	1,019	24	164	2,200	1,712
Labor share	0.528	0.183	0.210	0.535	0.819	1,712
Total wage bill *(in 1000s)*	30,927	76,213	765	6,588	137,633	1,712
Mean hourly wage *(in €)*	28.01	25.30	14.74	23.68	44.15	1,696
Sales per hour worked *(log)*	277.9	508.7	63.87	152.5	831.0	1,701
Robot adopter	0.591	0.492	0	1	1	1,712
Competition by robot adopters	0.117	0.239	0	0.00694	0.823	1,712

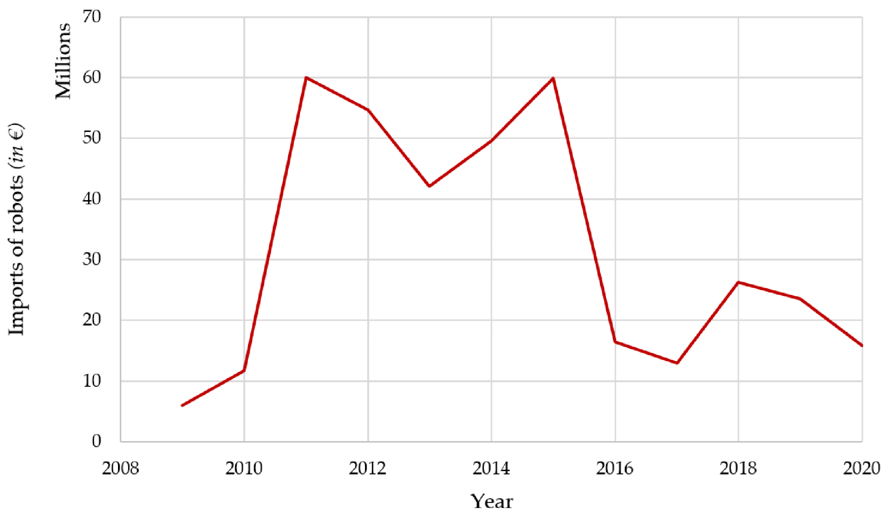
Notes: Panel A reports summary statistics for all firms in manufacturing sector. Panel B reports summary statistics for only robot adopting firms in manufacturing sector. For confidentiality reasons, the min and max values cannot be reported. Competition by robot adopters refers to the share of sales by robot adopting firms within the same 4-digit industry.

distribution of employment, with a few number of very large firms. The average gross value added is € 8.7 million, and average total hours of worked is 145 thousand. The histograms of main variables of interest are presented in Appendix Figure A2.

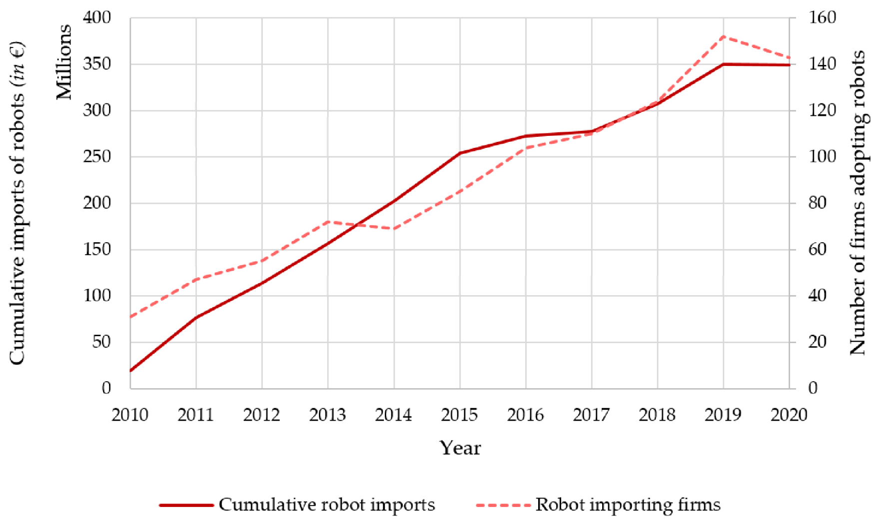
In Panel B, we present descriptives for firms that adopt robots sometime between 2009 and 2020. A comparison of robot-adopting firms with non-adopting firms indicates that, as expected, the former are, on average, larger, produce much higher value added, pay higher wages and have a larger workforce. It is shown that robot-adopting firms generate more than 10 times as much GVA than an average firm. The average number of workers of firms have will or have adopted robots is almost 6 times the size of the workforce of the average firm. Interestingly, we do not find large differences in the labor share.

Panel A of Figure 2 shows the value of robot imports over the period 2009-2020, while Panel B displays the cumulative imports of robots versus the number of firms adopting robots. Although the cumulative trend in all indicators is towards a steady increase over time, the imports value fluctuates significantly annually.

We explore the determinants of robot adoption more in Table 2, where we estimate simple exploratory regressions using data from 2009. The dependent variable is a dummy whether firms will adopt robots in the future. We first show the individual correlations between robot adoption and main firm level indicators, where we control for 4-digit industry and location fixed effects. Then, we augment the model by including all firm-level variables together. In column 6 we show that robot



(A) Annual robot adoption



(B) Cumulative robot adoption

Figure 2 – Robot adoption over time

adoption increases with GVA, while other measures of a firm's productivity are not statistically significant determinants of the robot adoption decision. This means that essentially only firm size is determining robot adoption.

Figure 3 shows that in 2020, more than 35% of all robots were adopted by firms in the top 2.5% of the distribution in terms of value added, confirming that mostly large firms are adopting robots.

2.2.2 Worker-level data

The descriptive statistics reported in Table 3 are for the firm data matched with worker-level data from the tax registers. We keep workers that appear at least once in an *LFS*-wave during our study period.

6.1% of the employees work in a robot-adopting firm. The mean hourly wage is €24.20. Strikingly,

Table 2 – Robot adoption by firms

Dependent variable:	*Robot adopter*					
	(1)	(2)	(3)	(4)	(5)	(6)
Gross value added *(log)*	0.008***					0.015***
	(0.001)					(0.005)
Gross value added per hour *(log)*		-0.000				
		(0.001)				
Labor share			-0.000			0.007
			(0.006)			(0.017)
Hourly wage *(log)*				0.004**		-0.009
				(0.002)		(0.006)
Hours worked *(log)*					0.006***	-0.006
					(0.001)	(0.005)
4-digit industry fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	6,784	6,784	6,784	6,593	6,784	6,593
R^2	0.207	0.195	0.195	0.200	0.205	0.213

Notes: The dependent variable takes the value of 1, when a firm adopts robots any time between 2009-2020. The regressions are estimated for the year 2009. All regressions include 4-digit industry and municipality fixed effects. We exclude gross value added per hour in column 6 as to avoid collinearity. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

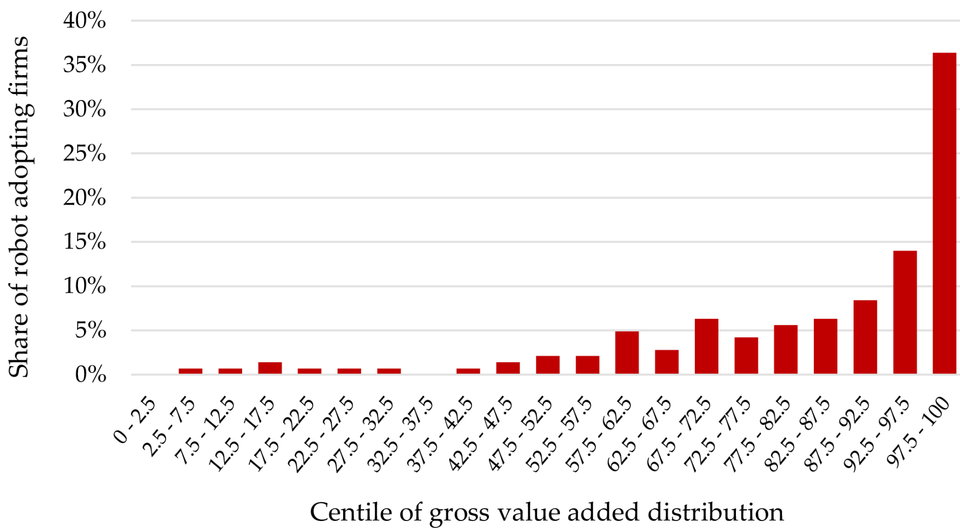


Figure 3 – Robot adoption by value added percentiles

annual earnings for employees in robot adopters are more than 30% higher. The employee characteristics are in general very similar between robot adopting and other firms, except for the share of low-education workers, which is considerably lower (about 50%) in adopters. The summary statistics seem to suggest that robot adopters are more productive, and also more productive employees sort themselves into robot adopters. Moreover, the difference in share of low-education workers in adopters and non-adopters may explain why hourly wages are considerably higher in robot adopters.

Average hours worked is about 1,800, which amounts to approximately a full-time contract. Moreover,

Table 3 – Summary statistics of matched LFS sample of workers 2009-2020

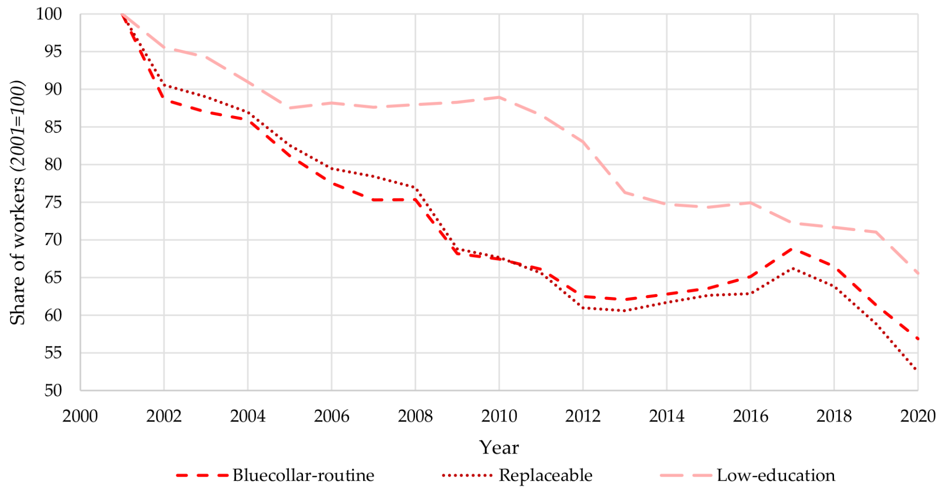
	mean	*std. dev.*	*5th perc.*	*median*	*95th perc.*	*N*
Panel A: All workers	(1)	(2)	(3)	(4)	(5)	(6)
Mean hourly wage *(in €)*	24.20	15.58	11.5	20.44	47.71	1,784,375
Hours worked	1,777	618.5	112.9	2,048	2,293	1,862,861
Employed	0.953	0.211	1	1	1	1,687,193
Personal income *(in €)*	49,411	32,492	16,935	43,138	98,849	1,739,296
Robot adopter	0.0417	0.200	0	0	0	1,861,005
Competition by robot adopters	0.0357	0.115	0	0	0.238	1,689,044
Bluecollar-routine worker	0.104	0.305	0	0	1	1,178,411
Replaceable worker	0.102	0.303	0	0	1	1,226,373
Low-education worker	0.345	0.475	0	0	1	1,313,107
Male	0.814	0.389	0	1	1	1,862,861
Age	45.79	11.57	25	47	63	1,862,861
Migrant	0.0871	0.282	0	0	1	1,862,861
2nd generation migrant	0.141	0.348	0	0	1	1,862,861
Household type – single	0.194	0.395	0	0	1	1,739,296
Household type – couple	0.800	0.400	0	1	1	1,739,296
Household type – other	0.00585	0.0763	0	0	0	1,739,296
Panel B: Workers in robot-adopting firms	(1)	(2)	(3)	(4)	(5)	(6)
Mean hourly wage *(in €)*	32.09	19.94	14.01	27.66	62.92	122,439
Hours worked	1.858	499.3	590	2,076	2,179	126,169
Employed	0.969	0.174	1	1	1	11,023
Personal income *(in €)*	65,841	42,924	25,055	56,898	131,139	120,885
Robot adopter	0.615	0.487	0	1	1	126,169
Competition by robot adopters	0.0906	0.175	0	0.000336	0.453	113,743
Bluecollar-routine worker	0.0923	0.289	0	0	1	74,943
Replaceable worker	0.0788	0.269	0	0	1	82,532
Low-education worker	0.188	0.390	0	0	1	88,789
Male	0.831	0.374	0	1	1	126,169
Age	46.03	10.64	27	47	62	126,169
Migrant	0.111	0.314	0	0	1	126,169
2nd generation migrant	0.166	0.372	0	0	1	126,169
Household type – single	0.174	0.379	0	0	1	120,885
Household type – couple	0.822	0.382	0	1	1	120,885
Household type – other	0.00342	0.0584	0	0	0	120,885

Notes: The data include workers that are in manufacturing and are appearing in an *LFS* wave at least once. Panel A reports summary statistics for all workers in firms in manufacturing sector. Panel B reports summary statistics for workers in robot adopting firms in manufacturing sector. Competition by robot adopters refers to the share of sales by robot adopters within the same 4-digit industry. For confidentiality reasons, the min and max values cannot be reported.

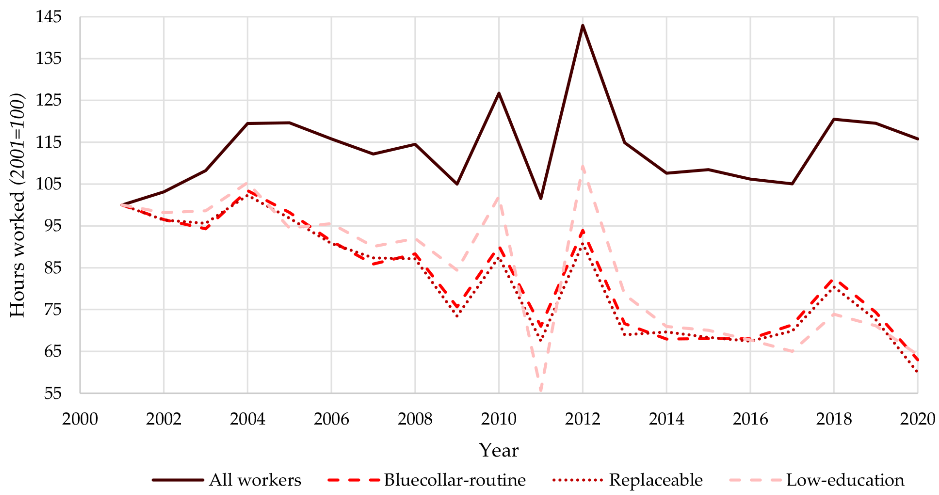
manufacturing has more full-time employees, for example as compared to the other sectors (such as healthcare or consumer services).

The (pre-tax) personal income is on average €49,411, while it is considerably higher €65,841 in robot adopters. Following workers over time within the study period, on average $1 - 0.953 = 4.7\%$ of the workers become unemployed. This rate is 3.1% that is about 25% lower for workers who were previously employed in these firms.

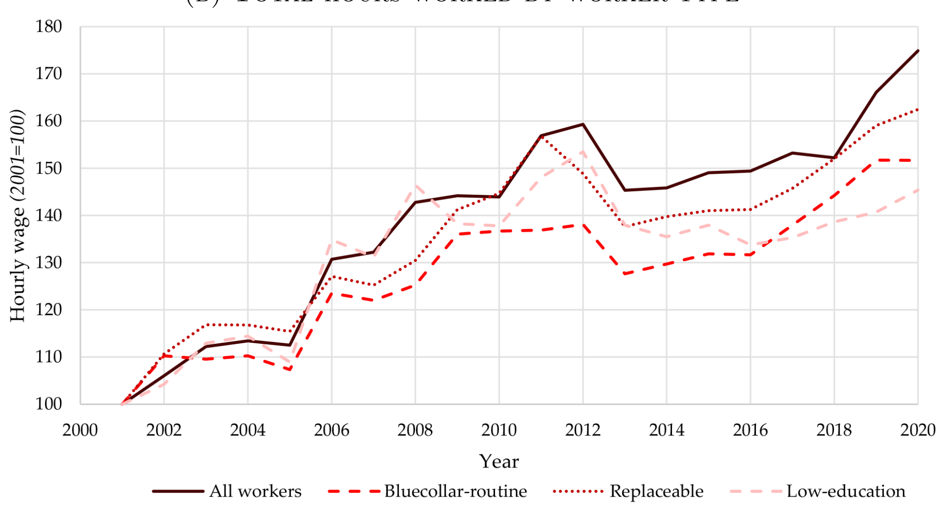
It it striking that about 80% of manufacturing workers is male. We do not find large differences between workers employed in robot-adopting firms and other firms in terms of gender, age, immigrant



(A) Share of workers by worker type



(B) Total hours worked by worker type



(C) Mean hourly wage by worker type

Figure 4 – Trends in hours worked and hourly wage by worker type

background, or household composition.

We further show that about 10% of the workers is classified as bluecollar-routine workers (*i.e.,* those who are in an occupation with an RTI value exceeding 1 and in a bluecollar occupation). The share of this type of workers is not very different between robot adopters and other firms (9.2% versus 10.4%). The share of replaceable workers follows a similar pattern. 10.2% of the workers are replaceable in our data while the share of replaceable workers in robot adopters is 7.9%. Low-education workers represent 34.5% of the workforce, but this share is 18.8% in robot adopters. The latter statistic indicates that the worker quality is higher in robot adopters and lower quality workers are sorting into non-adopting firms.

We now further motivate our three definitions of directly-affected workers, documenting that these workers are indeed more likely to be adversely impacted by robot adoption. We plot trends in the *total* hours worked and hourly wage of bluecollar-routine workers, replaceable workers, low-education workers, and other workers in Figure 4 by tapping into data from *LFS*s linked to *Tax Registers* from 2001 onwards. In Panel A we depict the share of workers by worker type in the last 20 years. There is clearly a substantial decrease in the share of all directly-affected worker types, with the share of replaceable workers and bluecollar-routine workers reduced the most by about 45% by 2020.

Figure 4b shows that there is an overall increase in total hours worked since 2005 for the average workers, which is about 25% higher in 2020 than in 2001. However, total hours worked for workers performing routine tasks in bluecollar occupations decreased by 40%, which is similar for replaceable and low-education workers.

Figure 4c reports the trends for (nominal) hourly wages. It indicates that average hourly wages have grown relatively fast, by about 75% between 2001 and 2020. This average masks significant heterogeneity, with slower growth for bluecollar-routine and replaceable workers than the rest. Wage growth is even slower for low-education workers. Our subsequent analysis sheds light on whether robot adoption has been a contributing factor to this slower wage growth in the Dutch economy.

3 Firm-level evidence on the effects of robot adoption

This section presents our baseline firm-level results. We focus on the effects of robot adoption on (gross) value added, the labor share, the hourly wage and and hours worked both for robot-adopting firms and their competitors. Section 3.1 outlines our econometric framework, Section 3.2 reports our main estimates for adopting firms, followed by a discussion on robustness of the results to relaxing various assumptions in Section 3.3. Section 3.4 turns to the effects of robot adoption on competitors.

3.1 Econometric framework

We present both long-differences regressions, focusing on 11-year changes, and panel data (fixed effects) estimates using the annual data. As in Graetz & Michaels (2018) and Acemoglu et al. (2020), the advantage of the long-differences specification is that it focuses on a time horizon during which most of the (potentially slow-acting) effects of robot adoption may be realized. In contrast, the fixed effects estimates use all of the available data and thus exploit all of the yearly variation in the sample. Hence, we find it useful to look at both sets of estimates.

Let y_{fmt} denote one of our four dependent variables (gross value added, the labor share, the hourly wage and and hours worked) for firm f located in municipality m in year t. Then, our long-differences estimation equation is:

$$\Delta y_{fmt} = \beta \Delta r_{fmt} + \zeta x_{fm\underline{t}} + \lambda_{f \in s} + \mu_m + \epsilon_{fmt}, \tag{4}$$

where Δ denotes the change between $\underline{t}$ and t, spanning the years of 2009 and 2020 and r_{fmt} indicates whether a firm is a robot adopter, as defined in Section 2.1. In addition, the $x_{fm\underline{t}}$'s are firm-level control variables in the first year of observation $\underline{t}$, including the log of number of workers and the log of value added per worker. $\lambda_{f \in s}$ are 4-digit industry fixed effects, and μ_m capture location fixed effects. Using the same notation, our panel data specification is:

$$y_{fmt} = \beta r_{fmt} + \zeta_t x_{fm\underline{t}} + \kappa_f + \lambda_{f \in s,t} + \mu_{mt} + \epsilon_{fmt}, \tag{5}$$

where $x_{fm\underline{t}}$ again denote beginning-of-sample control variables (which are not time-varying but we estimate time-varying coefficients ζ_t to allow for trends in $x_{fm\underline{t}}$), κ_f are firm fixed effects, $\lambda_{f \in s,t}$ are sector-by-year fixed effects and μ_{mt} are municipality-by-year fixed effects. Note that there are about 330 municipalities and 500 4-digit sectors in the SBI sector classification in the Netherlands. We include these controls and fixed effects to mitigate the issue that firms that adopt robots have underlying characteristics that are different and are therefore on different trends.[5]

The competition variable is defined on the basis of the share of sales in a given 4-digit industry accounted for by robot adopters (leaving out the sales of the own firm in question). Specifically, we define *robot adoption by competitors* as

$$r_{ft}^{\mathcal{C}} = \frac{\left(\sum_{f \in s} q_{ft} r_{ft}\right) - q_{ft} r_{ft}}{\left(\sum_{f \in s} q_{ft}\right) - q_{ft}}, \tag{6}$$

where r_{ft} is our usual robot adoption measure at the firm level and q_{ft} denotes firm sales.[6] Using this variable, we estimate analogues of equations (4) and (5), except with $r_{ft}^{\mathcal{C}}$ on the right-hand side and focusing on non-adapter firms. As in this case the identifying variation comes from the differences in competition *between* 4-digit industries we cannot include 4-digit industry-by-year fixed effects, and only include 2-digit industry-by-year fixed effects. Additionally, since robot adoption in an industry can be endogenous, we follow the strategy in Acemoglu & Restrepo (2020) and exploit the variation coming from a five-year lag of industry-level robot adoption in South Korea and Taiwan. These two countries are further ahead than the Netherlands in terms of adoption and are not directly competing with Dutch firms.[7] Specifically, using *IFR* data, we construct the

[5] We also perform several sensitivity analyses to investigate whether omitted variable bias is an issue. These analyses include the inclusion of leads and lags of robot adoption to equation (5). We also obtain Oster's (2019) bias-adjusted estimates.

[6] Note that we do not have detailed information on the product composition of different firms, and hence used coarser information to construct the competition variable than in Acemoglu et al. (2020).

[7] There are several reasons why robot adoption in an industry can be endogenous, motivating our instrumental-

following exposure variable as instrument:

$$r^{\mathcal{E}}_{\tilde{s}t} = \frac{\mathcal{R}_{\tilde{s},t-5} - \mathcal{R}_{\tilde{s},\underline{t}-5}}{n_{\tilde{s},t-5}}, \tag{7}$$

where $\tilde{s}$ refers to the *IFR* sector, $\mathcal{R}_{\tilde{s},t}$ is the total number of robots in Korea and Taiwan in sector $\tilde{s}$ in year t, and $n_{\tilde{s},t}$ is the total employment in sector $\tilde{s}$ in the Netherlands in t. Because $r^{\mathcal{E}}_{\tilde{s}t}$ has some extreme outliers we cap the instrument at its 99$^{\text{th}}$ percentile value.[8]

3.2 Firm-level impact of robot adoption

Table 4 presents our baseline firm-level results for manufacturing in the Netherlands. Panel A reports results for our long-differences specification from equation (4) on a balanced panel of firms. All regressions are weighted by total hours worked in the firm in 2009, and include the log of number of workers at time t as well as the log value added per worker at time t as controls.

The results are comparable to those in Acemoglu et al. (2020). Compared to France, we report somewhat larger effects of robot adoption on value added $(\exp(0.257) - 1) \times 100\% = 29\%$ vs 9% in France), on the labor share (-6.5 percentage points vs -2.7 percentage points), and on total hours worked (7.9% vs 5.5%). The decrease in labor share by 6.5 percentage point from robot adoption is also in line with Kehrig & Vincent (2020) who show a 5 percentage points decline of labor per decade in US manufacturing firms. We do not seem to detect any effects on wages, which in principle could either reflect the fact that firms are able to expand employment without putting upward pressure on wages.

In Panel B in Table 4 we report estimates from equation (5) using our unbalanced annual panel. Because we control for number of workers and gross value added per worker in 2009 we only include firms that we observe in 2009, therefore the number of observations vary from those in summary statistics table. Value added increases by $(\exp(0.139) - 1) \times 100\% = 15\%$. The labor share decreases by 4.6 percentage points, while hours worked increase by 4.3%. Since these estimates are close to the long-difference results, but are considerably more precise, in the rest of the paper we focus on these panel results.[9]

3.3 Firm-level impact of robot adoption — robustness

A central question is whether our estimates, and other similar ones in the literature, are due to robot adoption or other technologies that may be introduced at the same time as robots. Our *Investments*

variables strategy. First, robot adoption in an industry is likely to be correlated with other technological investments in the same industry, and if so, OLS estimates can be upward or downward biased (we investigate and find this concern not to be a major source of bias at the firm level, but this does not guarantee that the same issue is not important at the industry level). Second, our measure of robot adoption by competitors is imprecisely measured, especially since, as already noted, we do not have information on products-level sales. Our instrumentation strategy should purge some of this measurement error and imply that our IV estimates are likely (considerably) larger than the OLS estimates.

[8] The *IFR* data are from 2004-2014. Hence, for 2020 we would need data for 2015. We predict robots in 2015 by the linear trend of robot adoption in each sector in each country between 2010 and 2014. In Appendix B.8 we also provide estimations with non-extrapolated 2015 wave. Our results remain robust to baseline predictions.

[9] The similarity between the long-differences and panel estimates suggests that most of the effects of robot adoption are realized rather quickly. This conclusion is also backed up by the fact that leads and lags of robot adoption do not appear to be significant in panel regressions (see the Appendix).

Table 4 – Firm-level evidence for the effects of robot adoption

Dependent variable:	*ΔGVA (log)*	*ΔLabor share*	*ΔHourly wage (log)*	*ΔHours worked (log)*
Panel A: Long-differences	(1)	(2)	(3)	(4)
Robot adopter	0.257***	-0.065**	-0.002	0.076
	(0.085)	(0.031)	(0.049)	(0.062)
Firm-level control variables	✓	✓	✓	✓
4-digit industry fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Number of observations	4,298	4,298	4,227	4,298
R^2	0.513	0.503	0.444	0.0403

Dependent variable:	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*
Panel B: Fixed effects	(1)	(2)	(3)	(4)
Robot adopter	0.139***	-0.046***	0.011	0.042**
	(0.031)	(0.010)	(0.018)	(0.021)
Firm-level control variables	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	71,953	71,953	71,297	71,930
R^2	0.984	0.848	0.838	0.985

Notes: We weight all regressions by total hours worked in the firm in 2009. In Panel A reports the estimates based on 2009 & 2020 waves. We add the log of number of workers in t as well as the log value added per worker in t as controls. Panel B reports the estimates based on 2009-2020 and includes year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

data, described in Appendix A.1, allow us to separate investments in IT and other technologies and explore this issue. In Table 5 we estimate analogous specifications to those reported in Panel B of Table 4, augmented with controls for investments in computers and machinery. Similar to our robot adoption dummy, we define these investments by a dummy variables that equals one when a firm's investments in computers or machines in the past years were at least once among the top 5% of all investments in that year. These controls do not affect our estimates for the impact of robot adoption, though they tend to increase value added and employment.[10]

We also checked the robustness of our estimates to several modifications of our baseline specification in Appendix B. First, we verified that unweighted results are very similar to our baseline estimates that are weighted by firm size in 2009. Second, we obtained similar results when including non-manufacturing firms (meaning all firms in all sectors) in our sample as well. Third, we also obtained

[10]As an additional robustness check we also have estimated regressions where we control for the cumulative investments in computers and machines, which did not materially influence the results presented here. These results are available upon request.

Table 5 – Controlling for investments in computers and machines

Dependent variable:	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*
	(1)	(2)	(3)	(4)
Robot adopter	0.151***	-0.046***	0.010	0.053***
	(0.031)	(0.010)	(0.018)	(0.020)
Computer investment	0.067***	0.000	-0.011*	0.072***
	(0.010)	(0.003)	(0.006)	(0.007)
Machine investment	0.122***	-0.005	0.003	0.100***
	(0.012)	(0.0042)	(0.007)	(0.010)
Firm-level control variables	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	71,953	71,953	71,297	71,930
R^2	0.985	0.848	0.838	0.985

Notes: The computer/machine investment dummy equals one when a firm's investments in past years were at least once among the top 5% investments in computers/machines in that year. We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

similar results when we extend the sample back to 2004, assuming zero robot adoption before 2009. Fourth, as an omnibus measure against various omitted variable bias issues, we present bias-adjusted estimates, following Oster (2019), with and without detailed fixed effects.[11] These results, too, are very similar to our baseline estimates. Fifth, we include leads and lags of robot adoption. These leads and lags are mostly statistically insignificant and do not materially influence the coefficient of interest. Sixth, we investigate the robustness of our results to the the €1.2 million threshold value that allows Dutch firms trading within EU not to report commodity codes for the imports. In particular, we exclude imports from all countries that are below this threshold and verify that this has no effect on our results. Seventh, we checked the robustness of our results to the possible re-exporting of important robots. Finally, we estimated the effects of robot adoption separately for large and small firms.

3.4 Effects of robot adoption on competitors

In this subsection, we study the effects of robot adoption on competitors. As explained above, we limit the sample to non-adopting firms and look at the effects of robot adoption in their 4-digit industry, and we instrument this variable with exposure to robots in the same industries in Taiwan and South-Korea 5 years earlier. The first-stages, reported in Appendix B.6, are precisely estimated,

[11] Specifically, this approach corrects for any possible effects of unobservables not included in a regression. Building on Altonji et al. (2005), it looks at the relationship between the set of covariates included in regression and the coefficient estimate of interest, and presumes that adding covariates tends to reduce the degree of omitted variable bias. In addition, Oster's approach looks at not just how much the coefficient of interest moves, but also the change in the variance explained after adding the controls.

TABLE 6 – FIRM-LEVEL EVIDENCE FOR THE EFFECTS OF ROBOT COMPETITION, 2SLS ESTIMATES

Dependent variable:	Δ*GVA (log)*	Δ*Labor share*	Δ*Hourly wage (log)*	Δ*Hours worked (log)*
	(1)	(2)	(3)	(4)
Competition by robot adopters	-0.464	-0.139	0.0329	-0.623***
	(0.389)	(0.181)	(0.211)	(0.187)
Firm-level control variables	✓	✓	✓	✓
2-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	70,313	70,313	69,701	70,292
Kleibergen-Paap F-statistic	15.06	15.06	15.09	15.06

Notes: These estimations exclude the robot-adopting firms. Competition by robot adopters refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). We weight all regressions by total hours worked in the firm in 2009. We include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses and clustered at the IFR-industry×year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

and the Kleibergen-Paap F-statistic is about 15 in all specifications.

Our main results, reported in Table 6, indicate sizable negative effects from robot adoption on competitors, though these estimates are sometimes imprecise. In column 1, for example, a one standard deviation increase in robot adoption in the firm's 4-digit industry reduces value added by $(\exp(-0.464 \times 0.0993) - 1) \times 100\% = 4.5\%$. The imprecision may be partly due to the fact that our measures of competition are coarser than those in Acemoglu et al. (2020).

Column 2 shows an effect on the competitors' labor share, albeit statistically insignificant and small, with a one standard deviation increase in competitors' robot adoption reducing labor share by 1.38 percentage points Column 3 does not detect statistically significant effects on wages, although the standard error is too large to draw strong conclusions. Finally, we find more precise negative impacts on hours worked: a one standard deviation increase in competitors' robot adoption reduces hours worked by 6%.

Positive effects on adopting firms and negative effects on competitors combined imply that industry-level implications of robots are ambiguous in general. If we focus on the more precise estimates, we find that the overall effects are slightly negative, because the negative impacts on competitors are larger, and thus overall hours worked in the industry declined by about 2.7%. These negative effects are broadly consistent with past work, such as Graetz & Michaels (2018), Koch et al. (2021), Acemoglu & Restrepo (2020) andAcemoglu et al. (2020).

In Appendix B.8 we show robustness of these results with respect to the assumptions on the instrument. More specifically, we show that the results are robust to lagging robot values by 6 years instead of 5, as to avoid extrapolation of the data to 2015, although in this case we have to exclude 2009 because the *IFR* data are only available from 2004 onwards. Further, we show that results are robust to using values of the instrument at time t, in which case we extrapolate the data to 2020. Finally, we show that the results are essentially unaffected if we also include *IFR* data from Hong

Kong and Singapore to construct our instrument.

4 Worker-level analysis

We next turn to our main focus, and main contribution relative to past literature: the effects of robot adoption on workers. In addition to confirming the main outlines of our and other authors' firm-level results, our high-quality employer-employee data will enable us to investigate which types of workers are negatively impacted by robot adoption. Section 4.1 outlines the econometric framework we use for investigating worker-level effects. Then, Section 4.2 turns to heterogeneous effects of robot adoption on different types of workers, while Section 4.3 studies the heterogeneous effects of competitors' robot adoption.

4.1 Econometric framework

Let w_{ift} and h_{ift} denote, respectively, hourly wage and total hours for employee i working at firm f in year t. Then the main relationships of interest are:

$$\{\log w_{ift}, \log h_{ift}\} = \beta r_{ft} + \zeta z_{it} + \kappa_f + \lambda_{t,f\in s} + \mu_{f\in m,t} + \nu_i + \epsilon_{fmt}, \tag{8}$$

where z_{it} are worker characteristics such as age and immigration background. We further include firm fixed effects κ_f to control for the fact that more productive workers may sort themselves into more productive firms, which in turn are more likely to adopt robots. As in the firm-level analysis, equation (8) also includes 4-digit industry-by-year and municipality-by-year fixed effects to address the issue that more productive firms may be more likely to adopt robots.

Finally, given the nature of our data we can follow workers over time and include worker fixed effects, ν_i. Specifications that include worker fixed effects focus on the impact of robots adoption on the same worker and are particularly useful, since Table 2 provided evidence of endogenous sorting of workers across robot-adopting and non-adopting firms (with adopting firms having higher-skilled workers on average). In all specifications standard errors are clustered at the firm-year and worker levels.

Our main interest in this section, however, is not the overall impact of robot adoption on workers, captured by the parameter β, but heterogeneous effects. In particular, as explained above, we are interested in the differences between directly-affected workers (who suffered a displacement effect of robot adoption) and indirectly-affected workers (who should generally benefit from the indirect productivity effects, which induce additional hiring and non-automated tasks). We will use the three measures of directly-affected workers (based on workers performing bluecollar-routine tasks, performing replaceable tasks and having low education), as defined in equations (1), (2) and (3). The econometric specification in this case can be written as

$$\{\log w_{ift}, \log h_{ift}\} = \beta_1 r_{ft} a_{ift} + \beta_2 r_{ft}(1 - a_{ift}) + \delta a_{ift} + \zeta z_{it} + \kappa_f + \lambda_{t,f\in s} + \mu_{f\in m,t} + \nu_i + \epsilon_{fmt}, \tag{9}$$

where a_{it} is an indicator for whether the worker is directly affected. We control for the direct effects of a_{it}, firm fixed effects, industry-year fixed effects, and worker fixed effects. To further assuage concerns related to endogeneity, we estimate a version of (9) where we include firm-year fixed effects, which enables us to control for all direct effects of robot adoption on firms and focus on differential impacts on directly-affected workers *within* firms.

Beyond hours worked and wages, robot adoption may also change the employment status of these workers. To study the effects of robots on the probability of employment, we estimate the relationship between being employed, denoted by the dummy variable, e_{ift}, and firm-level robot adoption. Specifically, we estimate the following linear probability model:

$$e_{ift} = \beta_1 r_{ft} a_{it} + \beta_2 r_{ft}(1 - a_{it}) + \delta a_{it} + \zeta z_{it} + \kappa_f + \lambda_{t,f\in s} + \mu_{f\in m,t} + \nu_i + \epsilon_{fmt}. \tag{10}$$

As usual, the specification excludes the retired, self-employed and students. For workers who were previously employed but are currently unemployed, last firm's characteristics are assigned.

We use analogous models to study the impacts of competitors' robot adoption on directly-affected and indirectly-affected workers. Specifically, we estimate models of the following form:

$$\{\log w_{ift}, \log h_{ift}, e_{ift}\} = \gamma_1 r^{\mathcal{C}}_{ft} a_{it} + \gamma_2 r^{\mathcal{C}}_{ft}(1 - a_{it}) + \delta a_{it} + \zeta z_{it} + \kappa_f + \lambda_{t,f\in s} + \mu_{f\in m,t} + \nu_i + \epsilon_{fmt}, \tag{11}$$

where $r^{\mathcal{C}}_{ft}$ captures robot adoption in the same 4-digit industry. Further, let $\lambda_{t,f\in s}$ now denote 2-digit sector-by-year fixed effects. In line with the firm-level results, we instrument for $r^{\mathcal{C}}_{ft}$ using the robots exposure instrument as defined in equation (7). To address the issue that the instrument varies only at the IFR-industry level, we cluster our standard errors at the IFR-industry-by-year and worker levels.

4.2 The effects of robot adoption on workers

In Table 7 we first report the average effects of robot adoption on hourly wages, the employment probability and hours worked. Our main estimations focuses on workers matched with the *LFS* surveys, which provides occupation information used in our analysis of directly-affected workers. For consistency, we focus on this sample in Table 7 as well.

In columns 1-3 of Table 7 we estimate that robot adoption is associated with an average increase in hourly wages of 2.5%. Notably, this estimate is essentially the same regardless of whether a battery of worker characteristics (in particular, age, gender, migrant background, household type) are included, as we do in columns 2 and 3. When we additionally include worker fixed effects, the impact is smaller (hourly wages increase by 1.6%). We interpret this smaller effect to be indicative of the endogenous sorting of workers, whereby more productive workers tend to work for more productive firms, which are more likely to adopt robots, generating a slight overestimate of the positive effect of robot adoption on hourly wages.

Columns 4-6 turn to the impact of robot adoption on employment. Here, we do not detect consistent significant effects, which may again reflect the rigidities in the Dutch labor market, where laying workers off is difficult and quite slow. In columns 7-9, we look at hours worked, where we detect negative and fairly stable estimates. For example, without worker-level controls, hours worked declined by about 1.7%, and when detailed worker-level controls are added, this negative effect is about 1.3% (see column 8). With worker fixed effects in column 9, the impact is in the same ballpark but larger, *i.e.* -2.1%.

Table 7 – Worker-level evidence for the effects of robot adoption

Dependent variable:	*Hourly wage (log)*			*Employed*			*Hours worked (log)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter	0.025***	0.023***	0.016***	-0.005	-0.005	0.000	-0.017***	-0.013**	-0.021***
	(0.005)	(0.005)	(0.004)	(0.007)	(0.006)	(0.003)	(0.006)	(0.006)	(0.006)
Worker-level variables		✓	✓		✓	✓		✓	✓
Worker fixed effects			✓			✓			✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
4-digit industry×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	1,778,509	1,655,095	1,601,266	1,679,993	1,679,993	1,636,397	1,778,509	1,655,095	1,601,266
R^2	0.384	0.457	0.918	0.145	0.165	0.614	0.215	0.287	0.697

Notes: The table reports results from a regression of worker-level effects of firm-level robot adoption. Worker-level variables include age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults. Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The negative implications for hours worked, combined with positive wage impacts, already suggests that there may be heterogeneous effects from robot adoption — some workers getting pay increases, while others have their hours cut.

We next turn to the heart of our worker-level analysis: differential effects on directly-affected and indirectly-affected workers. In Table 8 we focus on hourly wages. Different columns focus on different controls and our three measures of directly-affected workers. The pattern is fairly clear and confirms our conjecture about the juxtaposition of negative hours effects and positive wage effects. For example, in columns 1-3, using the definition based on workers performing bluecollar-routine tasks, we find precisely-estimated and sizable positive impacts on indirectly-affected workers, and negative and equally precisely-estimated impacts on directly affected workers. Quantitatively, the estimate in column 1 implies that robot adoption increases hourly wages of indirectly-affected workers by $(\exp(0.034) - 1) \cdot 100\% = 3.5\%$. At the same time, directly-affected workers suffer hourly wage declines of about 5.5%. The patterns are similar in column 2 when we include worker fixed effects. In column 3, when we include firm-year fixed effects, we can only estimate the differential impact on directly-affected workers, which is found to be about 2.3% for directly-affected workers compared to other employees of the firm at the same time.

The results are quite similar in column 4-6 when we use the replaceable worker definition of Graetz & Michaels (2018), with the main exception being that in column 5, when we include worker fixed effects, the negative impact on the directly-affected workers becomes imprecise and is no longer statistically significant at the 5% level. The results are also broadly similar in columns 7-9, when we focus on low-education workers. In this case, too, there are precisely-estimated positive impacts for indirectly-affected workers, and significant and again fairly precisely-estimated negative implications for directly affected workers.[12]

Table 9 turns to the implications of robot adoption for employment and hours worked. In Panel A, we find small positive employment impacts on indirectly-affected workers and negative effects on directly-affected workers. For example, the estimates that control for worker fixed effects with the replaceable worker and low-education worker measures (columns 5 and 8) are positive for indirectly-affected workers, and of larger magnitude, though less precisely estimated for the directly-affected workers. With all three measures, when we include firm-year fixed effects (columns 3, 6 and 9) we estimate statistically significant differential impacts for directly-affected workers. We interpret the general imprecision of the results for employment to be in line with our conjecture that rigidities in the Dutch labor market slow down or prevent worker layoffs and also discourage or slow down hiring.

In Panel B of Table 9 we turn to effects on hours worked. In this case, the results are less clear-cut. In some specifications, we estimate negative impacts on both directly-affected and indirectly-affected workers, though in specifications with worker fixed effects, the magnitudes are larger for directly-affected workers. One reason for the less clear-cut nature of these results may be that our measures of who is directly affected may not fully capture which workers will be reallocated towards tasks with lower hours.

[12] We confirm in Appendix C.2 that, when we distinguish medium and low-education workers, the negative effects of robot adoption are more pronounced for the lowest-education category.

Table 8 – Worker-level evidence for the effects of robot adoption on hourly wage – heterogeneity

Dependent variable:	*Hourly wage (log)*								
	Bluecollar-routine workers			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter×	0.034***	0.015***		0.032***	0.014***		0.049***	0.018***	
indirectly-affected worker	(0.007)	(0.005)		(0.007)	(0.005)		(0.007)	(0.005)	
Robot adopter×	-0.054***	-0.008	-0.023***	-0.061***	-0.014	-0.026***	-0.069***	-0.013*	-0.034***
directly-affected worker	(0.015)	(0.008)	(0.008)	(0.014)	(0.009)	(0.009)	(0.012)	(0.007)	(0.008)
Directly-affected worker	-0.175***	-0.001	0.002	-0.178***	-0.007	-0.005	-0.189***	-0.015***	-0.013***
	(0.003)	(0.007)	(0.006)	(0.002)	(0.006)	(0.006)	(0.002)	(0.005)	(0.005)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm×year fixed effects			✓			✓			✓
Number of observations	771,681	731,290	702,987	806,390	764,276	736,273	793,550	753,123	724,914
R^2	0.476	0.937	0.948	0.477	0.937	0.948	0.498	0.937	0.948

Notes: The table reports the worker-level heterogeneous hourly wage effects of firm-level robot adoption. Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9 – Worker-level evidence for the effects of robot adoption on employment – heterogeneity

Dependent variable:	*Employment*								
Panel A: Employment	*Bluecollar-routine workers*			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter×	0.003	0.004		0.002	0.005*		0.003	0.007**	
indirectly-affected worker	(0.004)	(0.003)		(0.004)	(0.003)		(0.004)	(0.003)	
Robot adopter×	-0.005	-0.010	-0.014*	-0.003	-0.010	-0.016**	-0.006	-0.009	-0.016***
directly-affected worker	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)
Directly-affected worker	-0.005***	0.006	0.003	-0.004***	0.004	-0.004	-0.007***	-0.002	-0.000
	(0.001)	(0.008)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.004)	(0.004)
Control variables variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	771,016	736,034	708,116	805,725	769,235	741,566	792,772	757,842	730,012
R^2	0.161	0.596	0.629	0.158	0.595	0.627	0.159	0.596	0.628

Dependent variable:	*Hours worked (log)*								
Panel B: Hours worked	*Bluecollar-routine workers*			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter×	-0.024***	-0.015**		-0.022***	-0.012*		-0.019**	-0.009	
indirectly-affected worker	(0.008)	(0.007)		(0.007)	(0.007)		(0.007)	(0.007)	
Robot adopter×	-0.011	-0.027*	-0.019	-0.021*	-0.034*	-0.026	-0.037***	-0.032**	-0.023*
directly-affected worker	(0.011)	(0.014)	(0.015)	(0.012)	(0.017)	(0.018)	(0.011)	(0.013)	(0.014)
Directly-affected worker	0.003	-0.010	-0.020*	0.000	-0.005	-0.013	0.002	0.007	0.004
	(0.003)	(0.012)	(0.012)	(0.003)	(0.012)	(0.012)	(0.002)	(0.008)	(0.008)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	771,681	731,290	702,987	806,390	764,276	736,273	793,550	753,123	724,914
R^2	0.295	0.731	0.766	0.291	0.728	0.763	0.293	0.729	0.764

Notes: The table reports the worker-level heterogeneous employment effects of firm-level robot adoption. Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Finally, in Appendix C.3 we investigate the effects of robot adoption on a fourth measure, *personal income*, which combines information from all three of our measures (wage, extensive margin of employment captured by our employment dummy, and the intensive margin represented by hours worked). The results in this case confirm the pattern shown so far: robot adoption increases the personal income of indirectly-affected workers (by about 1.5% in our preferred specification with worker fixed effects) and reduces the income of directly-affected workers (by about 1.5% with worker fixed effects).

4.3 The effects of competitors' robot adoption on workers

Our results in the previous section suggest that the most negative effects of robot adoption may be on competitors, and hence at the worker level we may expect these results to fall on directly-affected workers employed in non-adopting firms whose competitors are intensively investing in robots. In this subsection, we investigate this issue further.

Table 10 reports estimates of the overall effects of robot adoption by competitors on hourly wage, employment status and hours worked of workers. Once again, we instrument for competitors' robot adoption, as in equation (7). The relevant first stages are reported in Appendix C.1 and continue to show a strong relationship, with Kleibergen-Paap F-statistics exceeding 10 in all specifications.

Going back to Table 10, the overall effect of competitors' robot adoption is quite similar to the effects of robot adoption by one's own firm. There are positive impacts on hourly wage, no effects on employment, and imprecise, though typically negative effects on hours worked. The increase in the hourly wage is somewhat unexpected. One possible explanation is that the increase in the hourly wage among adopting firms, shown in Table 7, puts upward pressure on the wages of the employees at non-adopting firms. Whether this is the case or not can be more easily understood once we look at heterogeneous effects, which we turn to next.

Table 11 explores heterogeneous effects. The patterns are consistent with our overall interpretation, though in some specifications somewhat imprecise. In sum, we find positive hourly wage impacts from competitors' robot adoption on indirectly-affected workers, and negative impacts for directly-affected workers. For example, in column 1, where we look at the measure based on bluecollar-routine work, we show that one standard deviation increase in competitors' robot adoption increases hourly wages by 3% for indirectly-affected workers. In contrast, the impact on the hourly wages of directly-affected workers is negative, even if imprecisely estimated. In column 3, when we include firm-year fixed effects, we estimate a fairly precise negative differential impact on the directly-affected workers. The pattern using the other two measures of who is directly affected are quite similar.

Overall, these patterns are in line with the interpretation we offered for the results in Table 10: firms whose competitors are investing in robots may be finding themselves in a double squeeze. The demand for workers employed in non-automated tasks goes up among their competitors, forcing them to increase wages, while they are also experiencing lower demand for their products, as their competitors expand at their expense.

Table 12 turns to the effects of competitors' robot adoption on employment and hours worked. These results are less precisely estimated. Almost in all of our specifications, the differential impact

Table 10 – Worker-level evidence for the effects of robot competition

Dependent variable:	*Hourly wage (log)*			*Employed*			*Hours worked (log)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Competition by robot adopters	0.211***	0.276***	0.285***	0.051	0.071	0.030	-0.045	0.001	-0.145
	(0.074)	(0.073)	(0.076)	(0.102)	(0.099)	(0.050)	(0.087)	(0.084)	(0.093)
Worker-level variables		✓	✓		✓	✓		✓	✓
Worker fixed effects			✓			✓			✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	1,504,477	1,399,702	1,347,437	1,426,511	1,426,511	1,384,170	1,504,477	1,399,702	1,347,437
Kleibergen-Paap F-statistic	67.03	66.35	57.11	68.14	68.14	56.04	67.03	66.35	57.11

Notes: Competition by robot adoption refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). Worker-level variables include age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults. Standard errors are clustered at the IFR-industry×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 11 – Worker-level evidence for the effects of robot competition on hourly wage – heterogeneity

Dependent variable:	*Hourly wage (log)*								
	Bluecollar-routine workers			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Competition by robot adopters× indirectly-affected worker	0.258**	0.572***		0.286**	0.536***		0.396***	0.666***	
	(0.117)	(0.175)		(0.116)	(0.170)		(0.120)	(0.184)	
Competition by robot adopters× directly-affected worker	-0.109	0.165	-0.353***	-0.113	0.058	-0.433***	-0.176	0.149	-0.449***
	(0.136)	(0.185)	(0.097)	(0.142)	(0.202)	(0.101)	(0.136)	(0.185)	(0.098)
Directly-affected worker	-0.161***	0.015**	0.014**	-0.162***	0.010	0.011	-0.170***	-0.003	-0.004
	(0.004)	(0.007)	(0.007)	(0.004)	(0.008)	(0.007)	(0.003)	(0.006)	(0.005)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	668,181	628,689	603,115	694,639	653,510	628,200	683,591	644,062	618,596
Kleibergen-Paap F-statistic	29.04	12.25	62.29	28.26	11.42	54.63	28.31	11.10	83.88

Notes: These estimations exclude the robot-adopting firms. Competition by robot adoption refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the IFR-industry×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 12 – Worker-level evidence for the effects of robot competition on employment – heterogeneity

Dependent variable:	*Employment*								
Panel A: Employment	*Bluecollar-routine workers*			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Competition by robot adopters×	0.107	0.015		0.101	-0.007		0.076	0.003	
indirectly-affected worker	(0.092)	(0.080)		(0.091)	(0.082)		(0.090)	(0.084)	
Competition by robot adopters×	0.160	-0.122	-0.092	0.114	-0.155	-0.089	0.109	-0.135	-0.055
directly-affected worker	(0.100)	(0.112)	(0.072)	(0.102)	(0.126)	(0.071)	(0.092)	(0.101)	(0.051)
Directly-affected worker	-0.006***	0.013	0.008	-0.005**	0.011	0.004	-0.009***	0.002	0.001
	(0.002)	(0.009)	(0.008)	(0.002)	(0.008)	(0.008)	(0.001)	(0.005)	(0.005)
Control variables variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	668,448	634,200	608,988	694,976	659,299	634,309	683,804	649,606	624,500
Kleibergen-Paap F-statistic	28.33	11.72	55.49	27.86	10.88	48.82	27.83	10.60	76.22

Dependent variable:	*Hours worked (log)*								
Panel B: Hours worked	*Bluecollar-routine workers*			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Competition by robot adopters×	-0.057	-0.251		-0.029	-0.267		-0.025	-0.306	
indirectly-affected worker	(0.118)	(0.214)		(0.116)	(0.212)		(0.120)	(0.220)	
Competition by robot adopters×	0.130	-0.345	-0.003	0.090	-0.340	-0.084	-0.052	-0.287	0.122
directly-affected worker	(0.137)	(0.247)	(0.167)	(0.139)	(0.269)	(0.162)	(0.125)	(0.251)	(0.168)
Directly-affected worker	-0.004	-0.002	-0.014	-0.0031	0.005	0.001	0.002	0.007	0.002
	(0.004)	(0.013)	(0.013)	(0.004)	(0.013)	(0.014)	(0.003)	(0.009)	(0.009)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	668,181	628,689	603,115	694,639	653,510	628,200	683,591	644,062	618,596
Kleibergen-Paap F-statistic	29.04	12.25	62.29	28.26	11.42	54.63	28.31	11.10	83.88

Notes: These estimations exclude the robot-adopting firms. Competition by robot adoption refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the IFR-industry×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

on directly-affected workers are negative, though never significant at conventional levels in this table.

Finally, in Appendix C.3 we show that the results for personal income are very similar to our hourly wage results — positive impacts on indirectly-affected workers and negative effects for directly-affected workers. The differential effects are quite sizable. For example, a one standard deviation increase in competitors' robot adoption has a differential negative impact on personal incomes of directly-affected workers of about 4.6%.

5 Conclusions

Despite the rapid spread of robots in most industrialized nations and some emerging economies, there is still much controversy about their effects. Previous work has focused on either market- or industry-level outcomes, or on firm-level outcomes. Much of this work finds negative market-level effects from robots on employment and wages, but positive firm-level implications. These positive effects result, in part, from the ability of robot-adopting firms to expand at their competitors' expense (and hence are consistent with negative industry-level effects). They may also reflect the differential trends on which adopting and non-adopting firms are, even before robot adoption. This literature has not focused on worker-level outcomes, and particularly, on which types of workers are positively or negatively impacted by robot adoption.

In this paper, we investigate the worker-level implications of robot adoption using high-quality data on robot imports, spanning a longer time period than most other studies, combined with detailed linked employer-employee data from the Dutch manufacturing sector. The Dutch economy provides an interesting context, since it has invested in automation technologies rapidly, but at the same time is subject to various labor market regulations and rigidities that may protect workers even in the face of automation.

We first confirm that the firm-level effects of robot adoption are very similar in the Netherlands to those we observe in other similar economies. In particular, robot-adopting firms increase their value added and employment, and reduce their labor share. This overall pattern and the quantitative magnitudes of our estimates are very similar to those presented in Acemoglu et al. (2020) for France. Moreover, as in French and Spanish manufacturing, these positive effects on adopting-firms are associated with negative impacts on competitors. Similarly to the French case, our estimates suggest that the negative effects are somewhat larger than the positive ones, so overall industry employment declines following robot adoption.

Most originally, we estimate the effects of robot adoption on workers. Our detailed data enable us to construct several measures concerning which workers are likely to be more negatively impacted by robot adoption. Specifically, task-based frameworks imply that workers performing tasks that will be replaced by robots will suffer the displacement effects of adoption, while workers employed in complementary tasks may benefit, as higher productivity translates into greater demand for skills associated with these tasks. We use three measures of which types of workers are going to be more directly affected and thus likely to suffer the negative consequences of robot adoption (Acemoglu & Restrepo 2020, Acemoglu et al. 2022). These are: workers employed in bluecollar-routine tasks, those employed in replaceable tasks (as defined in Graetz & Michaels 2018), and workers with low education levels. Consistent with theoretical expectations, using all three measures we find that robot adoption either by own employer or by competitors has more negative effects on directly-affected workers. For example, robot adoption by one's own employer leads to higher hourly wages for indirectly-affected workers, but to lower hourly wages for directly-affected workers.

Several questions and areas call for future inquiry. One important set of issues relates to the role of labor market institutions. Although our estimates are similar to those from other countries, the Dutch labor market is more rigid than those of many other industrialized nations and restricts firms' ability to adjust both employment and wages. Investigating the role of labor market institutions in mediating the effects of automation technologies is an important and interesting area for future work. Secondly, more granular data on market structure and competition patterns would be very useful for understanding how the adoption of automation technologies (and more broadly other new technologies) affects employees currently working for competitors. Third, this paper did not delve into the inequality implications of robots and other automation technologies. Recent work by Acemoglu & Restrepo (2022) documents substantial inequality impacts from the adoption of automation technologies in the US labor market. It would be interesting to investigate how these effects may or may not be different under more rigid labor market institutions. Finally, an open area of inquiry is whether there are other technologies, such as those creating new tasks, which firms can adopt simultaneously with robots that might have more favorable implications for workers.

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Appendix A Data

A.1 Other datasets

We further link *Investments* data to the *Production Statistics*. This dataset is produced in the same way as *Production Statistics* and includes the population of firms for firm size 50+ workers, and a representative survey of firms for those with firm size lower than 50. This dataset allows us to observe firms' tangible and intangible investments from at least 2003 onwards. These investments are detailed by for example investments on machinery (installations, machines and devices), as well as computers and hardware (computer, data processing electronic equipment etc.). This dataset includes non-financial firms only.

Another dataset we use is the *Firm Register*, which is a register of the universe of firms in the Netherlands to identify the location and age of the firms at a very refined spatial scale corresponding to more or less street level; that is the 6-digit postal code.

A.2 The routine task intensity index

We construct a measure of routine task intensity (RTI) that concords Autor & Dorn (2013)'s SOC level RTI to Dutch occupations as in Koster & Ozgen (2021), at the highest possible resolution, which is 4-digit ISCO (ISCO'08). The RTI informs us on the task content of occupations workers perform, which varies within educational levels. We gather data from *LFS*s from 1996-2020. The mapping of SOC level to ISCO subdivisions enables us identifying routinization level of occupations at the lowest level of breakdown. We construct 5 categories of task groups based on their degree of routineness, namely: routine cognitive ($\mathcal{RC}$), routine manual ($\mathcal{RM}$), non-routine manual ($\mathcal{NRM}$), non-routine analytic ($\mathcal{NRA}$) and non-routine interactive ($\mathcal{NRI}$). Following Autor & Dorn (2013), let $\mathcal{RTI}_{ot}$ be the routine task intensity of an occupation o in year t:

$$\mathcal{RTI}_{ot} = \mathcal{RC}_{ot} + \mathcal{RM}_{ot} - \mathcal{NRM}_{ot} - \mathcal{NRA}_{ot} - \mathcal{NRI}_{ot}, \tag{A.1}$$

$\mathcal{RTI}_{ot}$ is normalized to have mean zero and unit standard deviation.

A.3 Firm-level descriptives

Table A1 presents the same statistics for the 2-wave balanced panel of firms. These are the firms in our dataset that could be observed over the 12 years. The descriptive statistics shows that the summary statistics of the variables in the long-differences panel are very similar to that of the year-to-year panel.

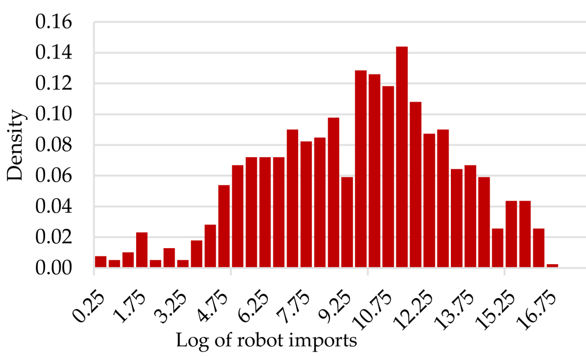
Figure A1 shows the distributions of robot imports for firms that are above and below the EU threshold of € 1.2 million. We find mild differences in the distribution of robot imports. In any case we will show robustness of the main results to excluding firms that are below the threshold in Appendix B.4.

In Figure A2 we report descriptive statistics of the main variables of interest. The distributions of value added and labor share are essentially normally distributed, while the distribution of firm size by the number of workers is double peaked. The reason is that there are many small firms with one

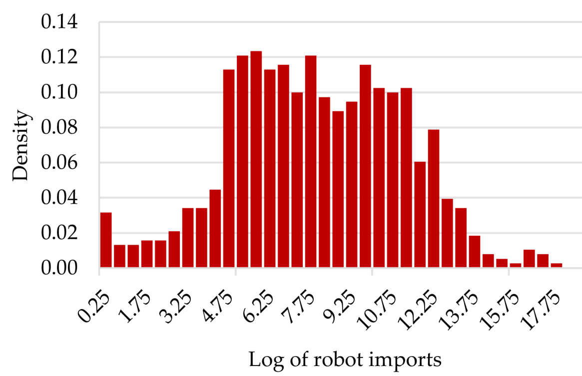
Table A1 – Summary statistics of 2-wave balanced panel 2009 and 2020

	mean	std. dev.	5th perc.	Median	95th perc.	N
Panel A: All firms	(1)	(2)	(3)	(4)	(5)	(6)
Gross value added in market prices *(in 1000 €)*	13,878	69,331	571.2	3,989	46,473	8,728
Hours worked	238,308	590,844	16,218	103,427	818,979	8,728
Number of workers	141.8	342.7	11	64	464	8,728
Labor share	0.555	0.168	0.255	0.569	0.814	8,728
Total wage bill *(in 1000s)*	5,932	16,578	306	2,180	21,168	8,728
Mean hourly wage *(in €)*	22.74	15.32	13.23	20.88	35.57	8,656
Robot adopter	0.011	0.106	0	0	0	8,728
Competition by robot adopters	0.025	0.878	0	0	0.129	8,709
Panel B: Robot-adopting firms	(1)	(2)	(3)	(4)	(5)	(6)
Gross value added in market prices *(in 1000 €)*	77,705	135,021	1,859	20,069	358,898	174
Hours worked	1,089,046	1,580,383	67,650	468,242	4,483,371	174
Number of workers	613.7	885.3	41	270.5	2,431	174
Labor share	0.557	0.184	0.236	0.569	0.874	174
Total wage bill *(in 1000s)*	33,334	54,380	1,340	10,755	143,993	174
Mean hourly wage *(in €)*	26.24	10.06	15.51	24.16	42.46	174
Robot adopter	0.569	0.497	0	1	1	174
Competition by robot adopters	0.089	0.203	0	0	0.594	174

Notes: Panel A reports summary statistics for all firms in manufacturing sector in 2009 & 2020. Panel B reports summary statistics for only robot adopting firms in manufacturing sector in 2009 & 2020. For confidentiality reasons, the min and max values cannot be reported. Competition by robot adopters refers to the share of sales by robot adopting firms within the same 4-digit industry.



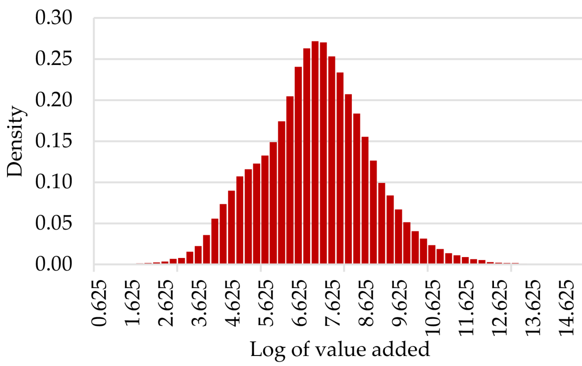
(a) Robot imports *(log)* of firms that are above the EU threshold



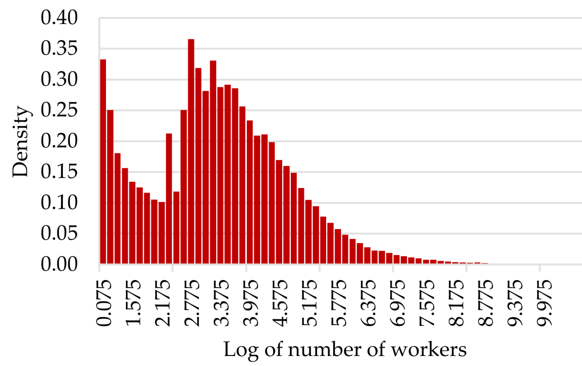
(b) Robot imports *(log)* of firms that are below the EU threshold

Figure A1 – Histograms – robot imports and the EU threshold

or two employees (note that $\exp(0.075) \approx 1$) as shown in the right top panel of Figure A2, as well as many medium-sized firms of about 50 workers. Further note that *all* firms with 50 employees or more are in the *Production Statistics* so smaller firms are under-sampled. While the distribution of hours worked is almost log-normally distributed, there is a spike at about 1600 hours, which is the full-time equivalent of one worker. Because firms often hire more than one full-time worker, the mean hours worked is obviously considerably larger.

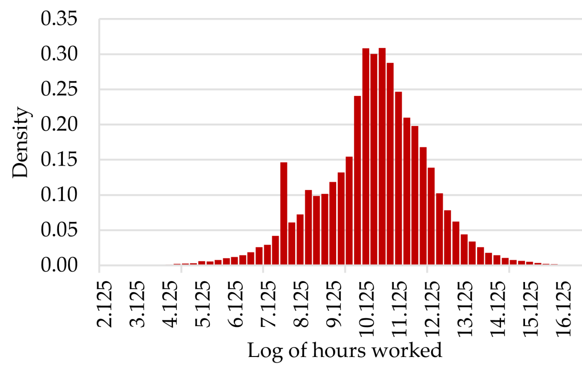


(A) Gross value added *(log)*



(B) Number of workers *(log)*

(C) Labor share



(D) Hours worked *(log)*

Figure A2 – Histograms of key variables

Appendix B Robustness of firm-level results

B.1 Sample selection and weighting

In columns 1-4 of Table B1 we report results where we do not weight the estimations with firm size. The estimates are barely affected. The effect of robot adoption on hours worked in column 4 is slightly stronger than the baseline estimate reported in Table 4, which further implies that the effect on labor share is somewhat smaller.

Column 5-8 include all firms above and beyond manufacturing firms. This means that the number of observations increase by 250%. However, as most of the variation in robot adoption comes from manufacturing firms, the estimates are very comparable. We find somewhat stronger effects on value added and hours worked, but the results are qualitatively similar.

In column 9-12 we investigate whether the choice of a relatively short time period of 12 years, due to data availability, affects the outcomes. For example, if the effects of robot adoption takes time, we may find underestimates of the impact of robot adoption. Therefore, in Table B1 we extend the panel of manufacturing firms to 2004 and assume that there are no robot adopters before 2009 (recall that the *ITR* data on trade transactions of robots is not available before 2009). We now weight by the number of hours worked in 2004 and control for value added per worker and number of workers in 2004. Because fewer firms are observed since 2004, the number of observations is reduced to just 15 thousand. Looking at the point estimates, they are very similar to the baseline estimates. However, because of the strong reduction in the number of observations, the coefficients capturing the impact of robots on value added and hours worked cease to be statistically significant.

B.2 Omitted variable bias

Here we investigate the concern that our estimates can be biased due to omitted variables that correlate to robot adoption. We apply Oster's (2019) bias-adjusted estimator. Essentially, the idea is to use coefficient movements together with changes in the R^2 after the inclusion of control variables to investigate whether omitted variable bias is important. Hence, coefficient movements alone are not a sufficient statistic to calculate the bias due to omitted variables, but the explained variance of the added control variables also matters. Oster (2019) then derives a GMM estimator to correct estimates for omitted variable bias, given the assumption that the relationship between the variables of interest and unobservables can be established from the relationship between the variables of interest and the observable control variables. In the current context, this makes sense because control variables that are added likely bear some relationship to unobservables. More specifically, we add the following firm characteristics: year-specific effects for the log of numbers of workers in 2009, the log of value added per worker in 2009, the log of cumulative investments in computers, machines, transport, and real estate and land, as well as the log of firm age.

There are two key input parameters that must be determined. First, a parameter must be chosen that determines the relative degree of selection on observed and unobserved variables, which is denoted by Π. Despite this parameter being fundamentally unknown, Altonji et al. (2005) and Oster (2019) show that $\Pi = 1$ is a reasonable upper-bound value. Second, there is the maximum R^2 from a hypothetical regression of the dependent variable of interest on robot adoption, and all

Table B1 – Robustness for the effects of robot adoption on firms: sample selection and weighting

	Unweighted results				*All firms*				*Manufacturing firms since 2004*			
Dependent variable:	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Robot adopter	0.136***	-0.029***	-0.024	0.097***	0.201***	-0.039***	0.019	0.107***	0.099	-0.045**	-0.005	0.062
	(0.026)	(0.010)	(0.015)	(0.023)	(0.026)	(0.089)	(0.015)	(0.021)	(0.076)	(0.022)	(0.031)	(0.052)
Firm-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	71,953	71,953	71,297	71,930	177,483	177,483	174,370	177,430	15,408	15,408	15,393	15,408
R^2	0.951	0.717	0.680	0.948	0.978	0.823	0.825	0.979	0.986	0.845	0.852	0.992

Notes: We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. For robustness checks, however, Column 1-4 presents unweighted results. Column 5-8 are weighted regressions and include all firms in all sectors. Column 9-12 are weighted regressions and include the active firms since 2004 to extend the time period. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

observable *and* unobservable controls. $R^2_{\text{max}} = 1$ is then again a reasonable upper bound value, assuming that measurement error is zero. Therefore, we expect that the bias-adjusted coefficients to be similar to our baseline estimates if the added controls in our regressions are correlated with the unobservables. Given these values, we display the results in Table B2.

In columns 1 and 2 of Table B2 we show two specifications, one without 4-digit industry×year and municipality×year fixed effects and one with these fixed effects included. Since Oster (2019)'s method critically depends on the number and type of controls included, we think it is important to show parsimonious specifications as well as specifications with a more elaborate set of controls.

The coefficient in column 1 is very similar to the comparable baseline coefficient in column 1 in Panel B, Table 4, but even somewhat higher because we do not include the detailed industry-year and municipality-year fixed effects. The estimate in column 2 is essentially the same, suggesting that omitted variables do not cause a major (upward) bias in the estimated effect.

We repeat the same set of regressions but now take labor share as the dependent variable. We find negative effects in the same order of magnitude as the baseline regressions, but the coefficients are somewhat imprecisely estimated, so the bias-adjusted estimates are not significantly lower than the baseline estimates. Since the bias-adjusted estimator is derived through GMM, estimates are less efficient than OLS, which implies that they are somewhat imprecisely estimated coefficients. In the baseline regressions we did not find any statistically significant effects on hourly wages, which is confirmed by the bias-adjusted estimates in columns 5 and 6.

For hours worked we find a strong and significant positive effect on hours worked of about 8.7% (see column 7). The estimate is about halved when we control for industry-year and municipality-year fixed effects in column 8. The point estimate is essentially the same as the baseline estimate. Unfortunately, the standard errors are somewhat larger.

All in all, these results do not give the impression that omitted variable bias is a serious problem, which is given the assumption that the included control variables bear some relationship with the unobservables. The point estimates are indeed fairly close to our baseline results and they are not significantly different than the baseline estimates.

B.3 Other identification issues

To further ascertain whether the robot adoption dummy captures a causal effect of robot adoption we undertake a series of checks to investigate whether our results are sensitive to the inclusion of control variables.

In the context of robot adoption, one may be willing to estimate event studies (see *e.g.* Humlum 2019). There are two reasons why we think in this setting undertaking event studies is not so useful. First of all, although the process of robot adoption can be one off, while for some firms adoption may take multiple years, the labor force may already adjust a few years before robot adoption takes place. For example, the firm may already start attracting skilled labor, or may provide trainings to existing staff. Also, it may take time after robot adoption before robots are used efficiently and before contracts of replaceable workers have been adjusted or terminated. Hence, despite the robot adoption may be a discrete shock, the response in, say, the labor share or hours worked once robots

Table B2 – Robustness for the effects of robot adoption on firms: bias-adjusted estimates

Dependent variable:	*GVA (log)*		*Labor share*		*Hourly wage (log)*		*Hours worked (log)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Robot adopter	0.186***	0.127***	-0.044	-0.017	-0.087	0.043	0.083**	0.042
	(0.046)	(0.037)	(0.030)	(0.022)	(0.058)	(0.060)	(0.040)	(0.035)
Firm-level control variables	✓	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
4-digit industry×year fixed effects		✓		✓		✓		✓
Municipality×year fixed effects		✓		✓		✓		✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	71,953	71,953	71,297	71,930	71,953	71,953	71,297	71,930
$R^2_{\max}$	1	1	1	1	1	1	1	1
δ	1	1	1	1	1	1	1	1

Notes: We weight all regressions by total hours worked in the firm in 2009. The controls are year-specific effects for the log of numbers of workers in 2009, the log of value added per worker in 2009, the log of cumulative investments in respectively computers, machines, transport, and real estate and land, as well as the log of firm age. Bootstrapped standard errors (100 replications) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B3 – Robustness for the effects of robot adoption on firms: leads and lags

	Leads and lags				*Only robot-adopting firms*			
Dependent variable:	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Robot adopter	0.152***	-0.048***	0.024	0.058**	0.242*	-0.075***	0.051*	0.022
	(0.046)	(0.015)	(0.024)	(0.028)	(0.133)	(0.021)	(0.027)	(0.088)
Robot adopter, $t-1$	0.047	-0.034**	0.011	-0.018				
	(0.050)	(0.015)	(0.027)	(0.030)				
Robot adopter, t	-0.067	-0.006	-0.029	-0.049				
	(0.041)	(0.012)	(0.023)	(0.030)				
Robot adopter, $t+1$	-0.052	-0.004	-0.034	-0.017				
	(0.037)	(0.013)	(0.022)	(0.025)				
Firm-level control variables	✓	✓	✓	✓				
Firm fixed effects	✓	✓	✓	✓				
2-digit industry×year fixed effects					✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓				
Municipality×year fixed effects	✓	✓	✓	✓				
Number of observations	67,020	67,020	66,377	66,997	1,145	1,145	1,144	1,145
R^2	0.985	0.854	0.841	0.986	0.412	0.385	0.488	0.411

Notes: We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

have been adopted may not be discrete.

Second, given that our panel covers just 12 years, event studies substantially restrict the number of observations (because, say, analyzing the robot adoption effects in a 6 years period, after 2015 we cannot observe whether a firm will adopt robots in, say, the next 5 years; and before 2014 we do not know whether firms have adopted robots more than 5 years ago). In other words the time span we have with our data is not long enough to observe pre and post adoption period changes with respect to robot adoption. We therefore take a simpler approach and add three variables: whether the firm will adopt robots in $t+1$, whether the firm has adopted robots in t, and whether the firm has adopted robots in $t-1$. We show in column 1-4 of Table B3 that the main effects are hardly affected. The coefficients of the leads and lags of robot adoption are mostly statistically insignificant and considerably smaller than the main effect. This suggests that robot adoption indeed implies a shock to value added and hours worked. Having said that, we find some evidence that the labor share already adjusts one year before robot adoption, although the coefficient of robot adoption in $t-1$ is smaller than the main effect.

Another approach would be to only focus on robot adopters because they are likely to be bigger and productive. We aim to address the issue that unobserved firm characteristics may be correlated with the timing of robot adoption by only exploiting variation in the *timing* of robot adoption. This limits our sample to just 133 firms and 1,145 observations, also because we weight the estimates by the value added per worker and the number of workers in 2009. Inevitably, this means that we cannot estimate the same specifications as before, as the fixed effects will absorb most of the identifying variation. Instead, we *only* include sector 2-digit-by-year fixed effects, implying that we control for sector-year trends, but do not control for firm effects. We think omitting firm fixed effects is acceptable because we just focus on robot adopters. The results are in line with what we have shown before. In Table B3 we find a strong effect on value added; robots seem to increase value added by 27%. The labor share is reduced by 7.5 percentage points, which is larger than the baseline estimate. We also find a small positive effect on hourly wages, but the effect on hours worked is very imprecise. All in all, this alternative identification strategy confirms our initial results.

B.4 Measurement error in robot adoption

Our definition of robot adoption may raise some concerns. The first issue is the threshold value within EU trade to be recorded, which could introduce a selection bias such that firms trading above the € 1.2 million threshold within the EU may have different characteristics, hence are differently impacted by robot adoption than those trading with non-EU countries. Therefore, in our analysis, we exclude all firms that have total import values below the EU threshold regardless of the origin of the trade partners. We show in columns 1-4 of Table B4 that using EU threshold values do not materially change the robot adoption effect on the dependent variables, except for hours worked where the effect ceases to be statistically significant. The effect on hours worked is also not different from that in our baseline results.

The second issue is selection among firms. One may be concerned that the comparison between firms that do not import and firms that do import is not correct. The reason is that firms, according to the data do not import, may still import goods, but in practice the value may be so low that it

Table B4 – Robustness for the effects of robot adoption on firms: measurement of robot adoption

	Remove values below 1.2 million threshold in ITR data				*Remove firms that do not import*				*Remove re-exporters of robots*			
Dependent variable:	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*	*GVA (log)*	*Labor share*	*Hourly wage (log)*	*Hours worked (log)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Robot adopter	0.143***	-0.053***	-0.022	0.077***	0.143***	-0.054***	-0.033	0.097***	0.132***	-0.060***	-0.013	0.033
	(0.044)	(0.013)	(0.020)	(0.024)	(0.048)	(0.014)	(0.021)	(0.026)	(0.038)	(0.012)	(0.022)	(0.025)
Firm-level control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	32,875	32,875	32,568	32,865	17,390	17,390	17,333	17,389	70,998	70,998	70,343	70,975
R-squared	0.886	0.886	0.993	0.985	0.991	0.899	0.906	0.994	0.983	0.842	0.822	0.984

Notes: Column 1-4 tests whether the minimum threshold requirement introduced by the Dutch government for the registration of imported goods creates a bias in our baseline estimates. To assess whether the importers are different than non-importers, or due to the threshold firms importing goods below the 1.2M threshold value are assigned as non-importers, column 5-8 remove the firms that do not import. Column 9-12 remove the re-exporting robot-adopting firms. We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

is not recorded, or they may import goods within the Netherlands. To circumvent this issue we exclude all firms that do not import in a certain year and are below the EU threshold of € 1.2 million. This reduces the number of observations by about 75%. We show in columns 5-9 of Table B4 that the estimations excluding firms that do not import display remarkably similar results for all dependent variables, despite the number of observations being reduced to just 17 thousand.

The third issue is that of re-exporting firms, which import robots and export robots to other countries. Those firms may not necessarily be robot adopters themselves, but just intermediaries. We therefore exclude the firms that ever (re-)exported robots in our sample period, which applies to about 25% of the robot adopters. The results reported in columns 9-12 in Table B4 are virtually the same.

B.5 Large and small firms

Here we distinguish between the effects of robot adoption on large and small firms so to assure that the effect we find is not just an effect that entirely applies to large firms. We define the large firms to be in the top 1% of firms with the largest workforce in 2009 and we define the rest of the firms as 'small'. Table B5 reports the results.

In column 1 we show that robot adoption increases value added of large firms and small firms, although the effect is almost twice as strong for large firms (20% versus 10%). The reduction in the labor share (see column 2) is also stronger for large firms, as the reduction is 5.5 percentage points for large firms, while it is only -3.7 percentage points for small firms.

The effects on hourly wage are also very different between large and small firms. While the average effect was close to zero and statistically insignificant, we find a strong positive effects of robot adoption on hourly wages for large firms (*i.e.* 7.4%), while it is negative and sizable for small firms (*i.e.* -4.9%). One possible interpretation is that within large firms, after robot adoption, the match between new technologies and workers' skills is likely to be better. Therefore, large firms do not need to hire new workers, but just reallocate existing workers to the new tasks, which results in higher wages. Smaller firms will need to hire workers that will be able to handle robots in production.

In column 4 we investigate the effects of robot adoption on hours worked. Here we only find statistically significant effects for small firms, which would be in line with the previous suggestion that reallocating replaceable workers within a firm is harder in smaller firms, therefore they seem to hire new workers.

In sum, these results confirm that robot adoption is not a phenomenon that only impact large firms, but also can affect the productivity of smaller firms. However, this suggestive evidence indicates that the robotization effects operate differently for these two groups of firms.

B.6 First-stage results for robot competition

In Table B6 we report first-stage estimates. We regress our measure of competition by robot-adopting firms on the robot exposure instrument as defined in equation (7). We find a strong and statistically significantly positive effect of industry-level exposure to robots on competition. The coefficient indicates that a standard deviation increase in robot exposure increases the robot competition by

Table B5 – Robustness for the effects of robot adoption on firms: large and small firms

Dependent variable:	GVA (log)	Labor share	Hourly wage (log)	Hours worked (log)
	(1)	(2)	(3)	(4)
Robot adopter×large firm	0.183***	-0.055***	0.071***	0.002
	(0.040)	(0.014)	(0.025)	(0.030)
Robot adopter×small firm	0.095***	-0.037***	-0.050**	0.083***
	(0.042)	(0.022)	(0.024)	(0.029)
Firm-level control variables	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	71,953	71,953	71,297	71,930
R^2	0.984	0.848	0.838	0.985

Notes: We define large firms to be in the top 1% of firms with the largest workforce in 2009. We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B6 – Firm-level evidence for the effects of robot competition: first-stage results

	(1)
Dependent variable:	Competition by robot adopters
Robots exposure	0.270***
	(0.065)
Firm-level control variables	✓
Firm fixed effects	✓
4-digit industry×year fixed effects	✓
Municipality×year fixed effects	✓
Number of observations	25,816
R^2	0.858

Notes: These estimations exclude the robot-adopting firms. We weight all regressions by total hours worked in the firm in 2009 and include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

$0.0943 \times 0.270 \times 100 = 2.6$ percentage points, which is about a quarter of a standard deviation. Hence, the effect is sizable.

B.7 WLS results for robot competition

We report the WLS competition results *without* instrumenting for robot competition in Table B7. We first show the main average effects for competition on firm level outputs in Panel A. The

Table B7 – Firm-level evidence for the effects of robot competition, WLS estimates

Dependent variable:	Δ*GVA (log)*	Δ*Labor share*	Δ*Hourly wage (log)*	Δ*Hours worked (log)*
	(1)	(2)	(3)	(4)
Competition by robot adopters	-0.075	0.009	0.019	-0.056
	(0.051)	(0.017)	(0.028)	(0.040)
Firm-level control variables	✓	✓	✓	✓
2-digit industry×year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Number of observations	71,145	71,145	70,503	71,124
R^2	0.981	0.810	0.800	0.982

Notes: These estimations exclude the robot-adopting firms. Competition by robot adopters refers to the share of sales by robot adopting firms within the same 4-digit industry. We weight all regressions by total hours worked in the firm in 2009. We include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

coefficient capturing competition effects has the opposite sign compared to robot adoption. The coefficients, however, are small and statistically insignificant.

As stressed earlier, one may be concerned that the effect of competition due to robot adoption is likely to be endogenous. For example, industrial sectors that are more productive are also more likely to adopt robots and invest in other automation technologies at the same time. Note that although this could explain why hours worked decrease, but this could not explain why we estimate a negative sign value added, though the estimation is imprecise (see columns 1 and 4). In any case, we will therefore instrument for robot competition in the specifications reported in Section 3.4.

B.8 Results for robot competition with alternative instruments

Here we provide additional robustness checks with respect to the instruments. For the instruments, we use data on the number of robots in each industry in South Korea and Taiwan between 2004 and 2014. Because we use the number of robots lagged by 5 years (see equation 7), we use extrapolated values in 2015 based on previous years. Alternatively, we lag the values by 6 years and exclude 2009. We show the results in Panel A in Table B8. We find similar outcomes as reported in Table 6. Hence, we think extrapolation of the *IFR* data with one year is not likely to change our findings.

Conversely, in Panel B we extrapolate the number of robots until 2020 and use the robot exposure based on the current year, t, instead of $t-5$. The results again confirm that our results are robust to this alternative instrument. The effect on hours worked is now somewhat stronger as compared to the estimate in Panel A, but still very close to the baseline estimate.

In Panel C in Table B8 we add the number of robots in Hong Kong and Singapore, which are arguably other good candidates for countries are are ahead in term of technological development, while at the same time are not important trading partners of the Netherlands. We show that the

Table B8 – The effects of robot competition, adjusting the instrument

Dependent variable:	*ΔGVA (log)*	*ΔLabor share*	*ΔHourly wage (log)*	*ΔHours worked (log)*
Panel A: Instrument based on $t-6$	(1)	(2)	(3)	(4)
Competition by robot adopters	-0.701*	-0.020	-0.160	-0.484**
	(0.412)	(0.135)	(0.216)	(0.229)
Firm-level control variables	✓	✓	✓	✓
2-digit industry×year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Number of observations	59,317	59,317	58,935	59,304
Kleibergen-Paap F-statistic	11.74	11.74	11.79	11.74
Panel B: Instrument based on t	(1)	(2)	(3)	(4)
Competition by robot adopters	-0.684	-0.149	-0.116	-0.711***
	(0.428)	(0.173)	(0.191)	(0.256)
Firm-level control variables	✓	✓	✓	✓
2-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	70,313	70,313	69,701	70,292
Kleibergen-Paap F-statistic	11.74	11.74	11.91	11.74
Panel C: Add Hong Kong and Singapore	(1)	(2)	(3)	(4)
Competition by robot adopters	-0.480	-0.146	0.032	-0.631***
	(0.398)	(0.185)	(0.218)	(0.192)
Firm-level control variables	✓	✓	✓	✓
2-digit industry×year fixed effects	✓	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓	✓
Number of observations	70,313	70,313	69,701	70,292
Kleibergen-Paap F-statistic	15.39	15.39	15.42	15.39

Notes: These estimations exclude the robot-adopting firms. Competition by robot adopters refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). We weight all regressions by total hours worked in the firm in 2009. We include year-specific coefficients for the log of numbers of workers in 2009 and the log of value added per worker in 2009. Robust standard errors are in parentheses and clustered at the IFR-industry×year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

results are very similar.

Appendix C Robustness of worker-level results

C.1 First-stage results for robot competition

In Table C1 we report first-stage results of the robot competition regressions for the worker-level analysis. Hence, we regress robot competition on robot exposure, like we did in Appendix B.6, yet the unit of analysis is now the worker.

In column 1 we only include firm, industry-year and municipality-year fixed effects. We find a somewhat stronger coefficient of robot exposure than at the firm level. One standard deviation increase in robot exposure increases competition by robot adopters by $0.0993 \times 0.213 \times 100 = 2.12$ percentage points. The coefficient is essentially the same once we include worker control variables in column 2 and is slightly lower when including worker fixed effects in column 3.

C.2 Robot adoption and the impact of low and medium-education workers

In line with the literature, *e.g.* Acemoglu & Restrepo (2020), Bonfiglioli et al. (2020) and Barth et al. (2020) who estimate negative impacts of robotization on low-skilled workers, one way to define 'directly-affected' workers is to focus on low-education workers. We define these workers as those whose highest degree of education is primary or secondary education. One may argue, however, that *medium-skilled* workers should be the ones who are more likely to be impacted by the broader automation effects (see Acemoglu & Autor 2011, Oesch 2013, Goos et al. 2014, Adermon & Gustavsson 2015, Autor et al. 2015). While this may true in general, in the Dutch context this may not be entirely applicable given that low-education workers are more likely to be in replaceable occupations. Similarly, most routine blue collar workers do have a low, rather than

Table C1 – Worker-level evidence for the effects of robot competition: first-stage results

Dependent variable:	*Competition by robot adopters*		
	(1)	(2)	(3)
Robot exposure	0.213***	0.212***	0.184***
	(0.051)	(0.052)	(0.051)
Worker-level variables		✓	✓
Worker fixed effects			✓
Firm fixed effects	✓	✓	✓
4-digit industry×year fixed effects	✓	✓	✓
Municipality×year fixed effects	✓	✓	✓
Number of observations	1,694,145	1,559,308	1,516,999
R^2	0.872	0.871	0.894

Notes: These estimations exclude the robot-adopting firms. Competition by robot adoption refers to the share of sales by robot adopting firms within the same 4-digit industry. Worker-level variables include age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults. Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

a medium, degree. More specifically, 56% of the replaceable workers have a low degree, 41% are medium-education workers, while 3% has a high degree. These numbers are approximately the same for bluecollar-routine workers.

In any case, we test for differential effects between the low and medium-education workers in Table C2 to ascertain our choice of low-education workers to be one of the directly-affected workers group from robot adoption. We find results that are in line with our previous findings. In column 1 we find that, without worker fixed effects, only high-education workers benefit from the robot adoption in terms of hourly wage, while low-education workers seem to lose. When we include the worker fixed effects in column 2 the results are comparable. Column 3 includes firm-year fixed effects, which implies that we identify the effects of robots *within* firms. Relative to the high-education workers, medium and low-education workers lose from robot adoption.

Columns 4-6 are less clear cut, in line with the results reported in Table 9. Columns 5 and 6 seem to confirm that the effects of robot adoption are increasing in education, as the probability to be employed within a firm is the most reduced for low-education workers, followed by medium-education workers.

The results in column 7-9 study employment effects at the intensive margin. Here again, we seem to find the strongest reductions in hours worked for the low-education workers, followed by the medium-education workers. Hence, all these results seem to point towards the idea that the low-education workers are indeed more affected by robot adoption.

C.3 Robot adoption and competition on personal income

From the *Personal Income* dataset we also obtained the pre-tax annual income. Personal income is a summarizing measure, which includes effects on hourly wages, hours worked and the probability to be employed. Hence, we think it is relevant to repeat our analysis, but take annual (personal) income as dependent variable. We start by studying the effects of robot adoption on annual income.

In columns 1-3 we consider directly-affected workers to be bluecollar-routine workers. We find that indirectly-affected workers gain from robot adoption, while directly-affected workers lose. In column 1, where we do no include worker fixed effects, we find that annual income increases by 2% for indirectly-affected workers when robots are adopted, while they decrease by -6.4% for directly-affected workers. Part of this effect is due to worker sorting, because when we control for worker fixed effects in column 2 we find smaller effects, but, still, we find that directly-affected workers experience negative income effects of robot adoption, while indirectly-affected workers gain. These results are confirmed, and even slightly more convincing, when we focus on the two other definitions of directly-affected workers: replaceability and whether a worker has a low education.

We further investigate the effects of robot competition in Table C4. In line with Tables 11 and 12, we do not find effects of robot competition when we do not include worker fixed effects. Still, the signs are in line with expectations with indirectly-affected workers benefiting, while directly-affected workers are losing (see columns 1, 4 and 7).

When we include worker fixed effects, the effects become more pronounced. For instance, in column 2, a standard deviation increase in competition increases incomes of indirectly-affected workers by

Table C2 – Worker-level evidence for the effects of robot adoption – low and medium-education workers

Dependent variable:	*Hourly wage (log)*			*Employed*			*Hours worked (log)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter×	-0.017*	-0.013*	-0.061***	-0.006	-0.009	-0.021***	-0.036***	-0.032**	-0.027
low-education worker	(0.010)	(0.007)	(0.010)	(0.007)	(0.006)	(0.007)	(0.011)	(0.013)	(0.018)
Robot adopter×	0.007	-0.004	-0.047***	-0.002	0.004	-0.010**	-0.025***	-0.011	-0.007
medium-education worker	(0.008)	(0.005)	(0.007)	(0.004)	(0.003)	(0.004)	(0.008)	(0.009)	(0.012)
Robot adopter×	0.046***	0.043***		0.007	0.010***		-0.013	-0.006	
high-education worker	(0.010)	(0.006)		(0.005)	(0.004)		(0.008)	(0.010)	
Low-education worker	-0.435***	-0.038***	-0.042***	-0.008***	-0.005	-0.005	-0.001	-0.012	-0.011
	(0.003)	(0.010)	(0.009)	(0.001)	(0.006)	(0.006)	(0.003)	(0.04)	(0.014)
Medium-education worker	-0.327***	-0.024***	-0.031***	-0.001*	-0.004	-0.004	-0.005**	-0.022*	-0.017
	(0.003)	(0.009)	(0.008)	(0.001)	(0.004)	(0.004)	(0.002)	(0.012)	(0.012)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	793,550	753,123	724,914	792,772	757,842	730,012	793,550	753,123	724,914
R^2	0.569	0.938	0.948	0.160	0.596	0.628	0.293	0.729	0.764

Notes: Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C3 – Worker-level evidence for the effects of robot adoption on income – heterogeneity

Dependent variable:	*Annual income (log)*								
	Bluecollar-routine workers			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robot adopter×	0.020**	0.012**		0.021***	0.012***		0.037***	0.018***	
indirectly-affected worker	(0.008)	(0.005)		(0.008)	(0.005)		(0.008)	(0.005)	
Robot adopter×	-0.065***	-0.014	-0.022***	-0.070***	-0.013	-0.022**	-0.087***	-0.020**	-0.04***
directly-affected worker	(0.019)	(0.009)	(0.009)	(0.016)	(0.009)	(0.009)	(0.014)	(0.008)	(0.009)
Directly-affected worker	-0.173***	0.005	0.002	-0.176***	-0.000	-0.008	-0.191***	-0.009	-0.010
	(0.003)	(0.008)	(0.009)	(0.003)	(0.008)	(0.009)	(0.002)	(0.007)	(0.007)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm×year fixed effects			✓			✓			✓
Number of observations	829,365	794,477	767,359	866,208	829,811	802,991	852,534	817,741	790,731
R^2	0.481	0.888	0.895	0.481	0.888	0.895	0.495	0.889	0.895

Notes: Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the firm×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C4 – Worker-level evidence for the effects of robot competition on income – heterogeneity

Dependent variable:	*Annual personal income (log)*								
	Bluecollar-routine workers			*Replaceable workers*			*Low-education workers*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Competition by robot adopters× indirectly-affected worker	0.122	0.459**		0.193	0.409**		0.260	0.468**	
	(0.183)	(0.207)		(0.182)	(0.191)		(0.187)	(0.208)	
Competition by robot adopters× directly-affected worker	-0.129	0.029	-0.410***	-0.230	-0.072	-0.506***	-0.285	0.141	-0.211**
	(0.202)	(0.200)	(0.114)	(0.215)	(0.211)	(0.123)	(0.198)	(0.205)	(0.102)
Directly-affected worker	-0.162***	0.023**	0.018	-0.159***	0.018	0.013	-0.174***	-0.000	-0.006
	(0.005)	(0.011)	(0.012)	(0.006)	(0.011)	(0.012)	(0.007)	(0.008)	(0.008)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker fixed effects		✓	✓		✓	✓		✓	✓
Firm fixed effects	✓	✓		✓	✓		✓	✓	
Firm×year fixed effects			✓			✓			✓
Number of observations	716,330	682,294	658,054	744,424	708,965	684,990	732,686	698,741	674,628
Kleibergen-Paap F-statistic	6.923	3.002	18.62	6.865	2.895	18.34	6.867	2.825	21.24

Notes: These estimations exclude the robot-adopting firms. Competition by robot adoption refers to the share of sales by robot adopting firms within the same 4-digit industry. Competition is instrumented by robot exposure as defined by equation (7). Control variables include worker-level variables (*i.e.* age dummies, as well as indicators for whether the worker is male, has a migrant background, and whether the worker is part of a couple or a household with multiple adults) and firm-level variables (*i.e.* 4-digit industry×year fixed effects and municipality×year fixed effects). Standard errors are clustered at the IFR-industry×year and worker levels and are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

$\exp(0.0993 \times 0.459) \times 100\% = 4.4\%$, which is a sizable effect. Hence, the increase in hourly wages apparently dominates the reductions in employment (see Tables 11 and 12). Columns 3, 6 and 9, where we include firm-year fixed effects, confirms that directly-affected workers are much worse of in relative terms compared to indirectly-affected workers when robot competition intensifies.