The Effect of Labour Cost Reduction on Employment of Vulnerable Groups — Evaluation of the Hungarian Job Protection Act

Svraka, András

Ministry of Finance, Hungary

2 August 2018

The Effect of Labour Cost Reduction on Employment of Vulnerable Groups — Evaluation of the Hungarian Job Protection Act*

András Svraka†

August 2018

Abstract

In 2013 Hungary introduced large scale targeted employers' social security contribution cuts for the young, old, low-skilled, and other marginally attached workforce, called the Job Protection Act (JPA). In this paper I estimate the employment effects of the programme for the main target groups using the discontinuities in the JPA's design in a differences in differences framework on administrative datasources. My estimates show robust and economically significant employment effects for the JPA, a total 1.2% point increase in employment rate three years after the introduction. The JPA was highly effective in the young and low-skilled target groups, with high self-financing ratios, while it was only marginally effective in the old target group. The results suggests that targeted tax incentives can be a cost-efficient way of increasing employment in vulnerable groups.

Keywords: Job Protection Act, targeted tax incentives, differences in differences
JEL codes: H24, J21, J23

*I am grateful to Viktor Hudecz, Tibor Keresztély, Endre Morvay, Benedek Nobilis, Dóra Novák, Péter Tóth and the participants of the 2017/18 Korea-Hungary Knowledge Sharing Program for their comments and suggestions, and to the staff at COAEPS and NTCA for providing access to the unemployment and tax databases and for their help in preparing the data for analysis. Endre Morvay also shared his code for cleaning the unemployment data with me. All remaining errors are the author's. The views expressed herein are those of the author and do not necessarily reflect the views of the Ministry of Finance.

†`andras.svraka@pm.gov.hu`, senior researcher, Ministry of Finance, Hungary

1 Introduction

Historically, Hungarian employment and participation rates were low compared to other member states of the European Union. These differences can largely be attributed the low participation of certain groups in the labour market. For the pre-crisis period Kátay (2009) identified four main groups that can explain most of the difference between Hungarian and EU15 participation rates: employees with primary education only, the cohorts below 25, above 50, and women of childbearing age.

In recent years several policy measures were aimed at increasing the employment rates in these groups. The retirement age was gradually raised and early retirement schemes were abolished. Significant tax reforms were also enacted. The progressive tax schedule on labour income was repealed and replaced with a flat rate system in several steps between 2011 and 2013, supplemented by a child tax allowance. In 2013 an additional new measure was introduced, called the Job Protection Act (JPA) with the aim to boost employment for certain vulnerable groups.

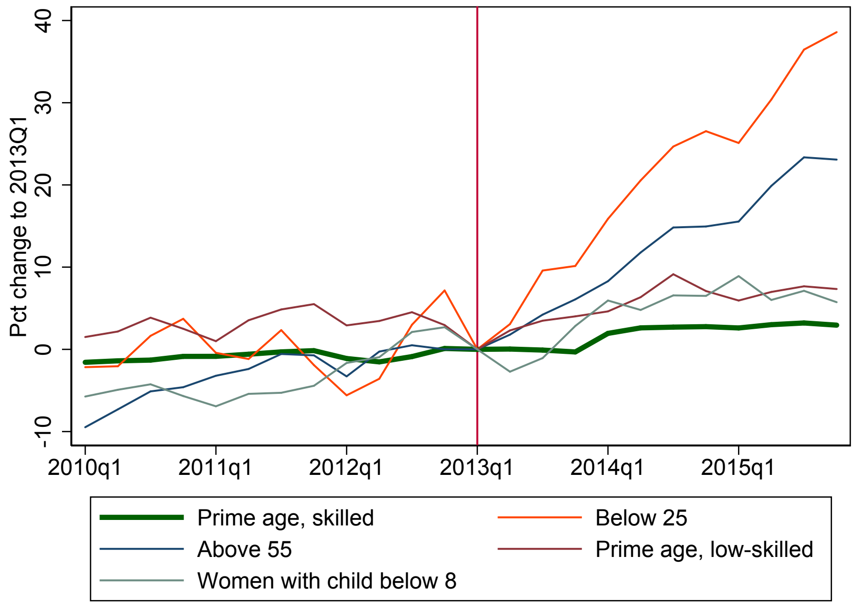
The JPA is a tax credit that reduces employers' social security contributions in groups where the Hungarian participation and employment rates are low: permanently for all employees aged below 25, or above 55; employees working in low-skilled jobs that don't require any vocational training; and temporarily (for three years) for employees returning to work after a child-care leave, and newly hired long term unemployed and career starters. The target groups cover around 900 thousand and in 2015 the programme's annual fiscal cost was HUF 130 bn (0.4% of GDP).

In the recent years a strong labour market recovery started during which employment among these vulnerable groups also increased. Figure 1 shows changes in the employment rates for the major JPA target groups on the primary,[1] domestic[2] labour market in the recent years based on the Hungarian Labour

[1]The public works programmes were significantly expanded after the crisis. Standard statistical definitions count these government supported jobs as regular employment. Here, I will focus on employment in the primary labour market, where the JPA available. Therefore I excluded participation in the public works programmes, which would distort the employment rates for the low-skilled. Between 2013 and 2015 the monthly participation in the public works programmes was 150–250 thousand.

[2]Migration of Hungarian workers to other EU countries increased in the recent years. The LFS covers households in Hungary but nevertheless, many of these workers can be found in the LFS sample. Some of them commute daily for the their jobs across the border (e.g. to Austria, or Slovakia), but the LFS can also include those who work more or less permanently abroad but still have family in Hungary (see Blaskó and Fazekas 2016).

Figure 1: Changes in employment rate in the main JPA target groups



Source: HCSO LFS, own calculations
Note: Employment in this figure is defined as the standard ILO definition in the LFS but without people employed in the public works programmes and without people working abroad. Low-skilled is defined as employees without a high school diploma.

Force Survey.[3]

We can see a large increase in employment rates of younger and older cohorts, while employment rates of low-skilled prime age workers and mothers with young children also increased slightly after the introduction of the JPA in 2013. The employment rate for prime age skilled workers also started increasing in 2014 with the recovery. However, besides the pension and tax reforms, and factors related to the business cycle, several other trends could be driving these. In 2014 the child care benefits were made more flexible, which could help mothers with young children return to the labour market, while the technological changes in the economy and changing skill composition of the labour force could influence the the demand for low-skilled labour.

In this paper I will estimate the causal employment effects of the JPA using a quasi-experimental setup. The JPA was introduced in a single step for all target

[3]Career starters and long term unemployed according to the JPA eligibility criteria cannot be identified separately in the data. Career starters are included in the below 25 group.

groups. The eligibility is defined using simple rules, which will result in some groups covered by the JPA, while others — who are similar to the treated group in many regards — not covered. These discontinuities can be used to construct counterfactuals to identify the true effects of the JPA. In order to separate most confounding factors, like the expansion of the public works schemes, I will use administrative micro data to identify the treated and control groups of the JPA target groups and use a differences in differences estimator to get a causal estimate for the employment effects for the JPA.

My results show robust, statistically and economically significant employment effects for the programme. The JPA increased employment by around 1.2 percentage points three years after its introduction. The effects were heterogeneous across the target groups, the employment rate in the below 25 group increased by 2.6%, in the low-skilled group by 2.2% but only by 0.8% in the above 55 group, and the JPA increased exits to employment from long term unemployment by around 0.7%. Due to limitations in the available data, I was not able to estimate the effects of the JPA among mothers with young children. The results suggest, the JPA led to some substitution between eligible and non-eligible low-skilled workers but the overall substitution effect was small.

Based on these results I will do a rough cost-benefit analysis that shows an overall self-financing ratio (the amount of tax collected from the higher employment divided by the total tax expenditure of the JPA) of around 40%. For the low-skilled it was as high as 70% but for the above 55 groups as low as 14%. This partial equilibrium analysis ignores potential second round, or wage effects, therefore they can be considered as a lower bound but they should capture the main channel of adjustment.

These results show that targeted tax incentives can be a cost-effective way for boosting employment of vulnerable groups in Hungary.

Ex post studies of the JPA were not done previously. Benedek, Kátay and Kiss (2013) used ex ante simulations, based on a dynamic labour supply microsimulation model embedded in a general equilibrium macro model to predict the JPA's labour market and macroeconomic effects. According to their results, the programme was expected to increase the employment rate by 1 percentage point in the long run, after adjustments in the supply of labour and capital. My ex-post analysis suggests slightly higher employment effects.

The JPA is quite unique with its broad coverage. It was not only novel in Hungary, but there aren't many examples for similar tax incentives form other countries either. Temporary hiring credits for long term unemployed, child care returnees, or career starters are common in many countries. However, large scale targeted tax incentives for vulnerable groups — which are general and unconditional in the sense that eligibility doesn't depend on the level of income — using some form of tagging are rare. Sweden introduced similar schemes in 2007 in employers' social security contributions for young (below 25) and old (above 65) employees but unlike the JPA, these tax credits were not capped. The tax cut for young workers was analysed by Egebark and Kaunitz (2017) who found small positive employment effects and no wage effects using a quasi-experimental differences in differences method. Low Hungarian youth participation rate might explain why the JPA was much more effective than the Swedish tax incentive. The tax cut for older workers was analysed by Laun (2012) who found some positive effects on employment of men, with effects higher than for the JPA. The two programmes are also different, the Swedish tax credit targeted employees close to the official retirement age, while the JPA targeted a broader group. My analysis is not able to identify the effects of the JPA for older workers but it is possible that the JPA was also more effective in raising employment among workers closer to retirement. However, the pension reforms introduced around the same time make the identification of these effects difficult.

Targeted tax incentives for long term unemployed and other inactive groups existed in Hungary before the JPA. These voucher based system called Start had mostly narrow target groups but offered higher subsidies than the JPA. Several Start programmes were studied using quasi-experimental methods. Cseres-Gergely, Scharle and Földessy (2015) analysed programmes targeting low-skilled, older workers and found significant effects for older men with vocational training. According to their analysis, the programme was cost-effective even though it was only effective in raising the employment of men. Szabó-Morvai (2015) analysed the Start programme for mothers returning after a child care leave. The programme had an overall small employment effect but it significantly raised employment among skilled mothers with multiple children. My analysis showed high effects on exits to a job from long term unemployment under the JPA. However, it is uncertain how permanent these effects were.

2 The Job Protection Act

Taxation of labour changed significantly in Hungary since 2010. The progressive personal income tax was replaced with a flat rate system and a family tax allowance. In 2013 a large scale employers' tax incentive, called Job Protection Act (JPA) was introduced with the aim of boosting labour force participation of certain disadvantaged groups whose participation rates were low in Hungary compared to either the European average, or to regional peers.

The JPA consists of six different types of tax credits in the employers' social security contributions (27% at the time of the introduction[4]) and payroll taxes (1.5%). There are two major types of credits. The permanent cuts, which cover the largest target groups can be claimed by employers as long the as the employees fall under the following categories, regardless of when the job started. The amount of the JPA in these categories is 14 percentage points of the gross wage[5] capped at HUF 100,000 in each month.[6]

Below 25 Employees below the age of 25 are entitled for the tax credit until and including the month of their 25th birthday.

Above 55 Employees above the age of 55 are entitled for the tax credit beginning in the month of their 55th birthday.

Elementary occupations Any employee working in an elementary occupation defined as main category 9 according to the Hungarian Central Statistical Office's HSCO-08 classification system[7] is eligible for the JPA. These occupations don't require formal qualifications and consist of simple tasks. This group includes basic service sector jobs like cleaning and fast food workers, simple industry jobs like warehouse workers and some assembly line workers. Category 9 of the HSCO covers some elementary agricultural jobs but not the majority. Employees are eligible as long as their occupation is in category 9 of the HSCO.

The other major types of tax cuts are temporary. Only employees starting a job after the introduction of the JPA are eligible, and in general, the cuts expire after three years. The amount of the JPA in these categories is 28.5 percentage points of the gross wage (which means a full exemption from social security tax and the vocational contribution) during the first two years after hiring, and 14

[4]Beginning in 2017 the employers' SSC was cut several times and the amount of the JPA credit was adjusted. The target groups were also expanded with low-skilled agricultural occupations and the length of the tax credit for mothers with at least three children was extended to five years in 2015. The period covered by this analysis is not affect by these changes.

[5]At the time of the introduction of the JPA the average tax wedge was 49% according to the OECD methodology. However, this this figure doesn't include targeted tax cuts like the JPA at any income level (OECD 2017).

[6]HUF 100,000 was 102 percent of the full time minimum wage at the time of the introduction of the JPA. Due to minimum wage raises the ratio decreased to 98.5 percent in 2014 and to 95.2 percent in 2015.

[7]See `https://www.ksh.hu/feor_eng_menu`. The classification is broadly comparable to the ILO's ISCO-08 classification, although there are small differences.

percentage point in the third year. The amount is capped at HUF 100,000 in each month. The following categories were introduced.

Long term unemployed Unemployed people, who registered as unemployed at a local employment agency, and find a job after a long spell of unemployment are eligible for this SSC cut if at the start of the job they had been unemployed for at least 6 months in the previous 9 months. For the calculation of the unemployment spells certain atypical work arrangements are not considered as employment (e.g. simplified employment, which is a form of temporary work) while the time of participation in the government financed public works programmes is not counted in either the 6 month, or the 9 month period. That is, for people enrolled in the public works scheme, the eligibility has to determined by adding up multiple spells of non-employed periods.[8] In order to claim the credit, the newly hired employees have to provide their employers a certificate issued by the employment agency about their eligibility.

Childcare returnees The JPA credit is available for employees who start working (either by returning to their previous job, or starting a new job) after they stop receiving childcare benefits, or employees who start working while still receiving childcare benefits.

Career starters In addition the the permanent SSC reduction for the below 25 group, a higher SSC cut is available for career starters. Employees below the age of 25 with a maximum of 180 days of paid work earlier in their lives are eligible for a temporary SSC reduction.

Employers can choose which SSC cuts they claim if an employee is eligible for several JPA types. However, only one type can by claimed at once. For all categories, the JPA is available only for private sector employers and only for employees in standard employment contracts (it is not available for atypical forms, e.g. temporary, or public works, and self-employment).

3 Data

I use two anonymous administrative data sources for the analysis. Tax returns from the National Tax and Customs Agency (NTCA) cover the entire popula-

[8]The rule was changed in 2016, and since that public works spells count as non-employment for the purposes of the JPA.

tion of individual taxpayers and the dataset from the Central Office for Administrative and Electronic Public Services (COAEPS) covers the entire population of people who were registered as job-seekers by the National Employment Agency.

3.1 Tax returns

The main data source is a panel covering the period between 2009 and 2015, built from linked, anonymous datasets of employers' monthly social security and tax filings, and individuals' annual tax returns. The monthly filings contain detailed data on employment but they are only available for the month of May in each year. Linking these datasets across the years gives a fully balanced panel of the population of Hungarian taxpayers: those who were employed, or received some form of taxable benefit at least once during the month of May in these years, or filed a tax return between 2009 and 2015.

In the raw database a single observation from the annual tax returns is the detailed income declaration of an individual taxpayer across the various taxable income sources. In the employers' filings a single observation describes the the income earned during a particular period by an individual from a specific employer. It also has information on the income earned, the type and length of the contract, and occupation. An individual can appear several times in a single month (e.g. switching jobs during the month, having a second job, or due to accounting revisions), therefore the data needed to be aggregated. During the cleaning process contracts not in May in each year were omitted, data was harmonized across the years, and from the remaining contracts it was determined whether an individual had at least one day of paid work during the month at an employer who is eligible for the JPA (i.e. excluding the public sector) in a regular labour contract. Participation in the public works programmes was considered as non-employment for the same reason. Finally, the data was extended with age, gender and occupation, and linked with information about the employers.

This dataset covers the whole taxpaying population but effects in terms of employment to population ratios are easier to interpret and they also control for demographic changes. Therefore population statistics by age and gender were used to impute observation needed to cover the entire population aged 20 to 59. The final dataset covers around 6.2 million people in this age bracket[9] between 2010 and 2015. Data for 2009 was only used to construct a lagged employment

[9]This might overstate the size of the active population, as recently emigrated people, who were employed in Hungary during the period will be counted. This might bias employment rates downwards but it won't bias the estimated total employment gains.

variable, otherwise observations for 2009 were omitted. The descriptive statistics are shown in Table 13.

3.2 Unemployment registry

The JPA credit for the long-term unemployed is conditional on the length of unemployment and non-employment at the time of starting a new job. As the tax returns have only one observation per individual and per year about the type and length of employment, they are not sufficient to identify those who are eligible for the long-term unemployed tax credit. A second data source, the unemployment registry is used for this target group.

The unemployed in Hungary are required to register at the employment agencies in order to claim unemployment benefits, participate in ALMPs, or public works programmes. The registry is a complete database of all entries and exits to and from official unemployment along with the reason for exit (e.g. found a job, participated in public works employment, retirement). The employment agencies record additional information about the jobseekers, of which age, gender, educational attainment and residence are available.

The registry covers around 2 million people, who were unemployed at least once between January 2011 and June 2015. One observation in the database is the record of an entry, or an exit with its date. Exits can be permanent (e.g. finding a job, going into retirement, or other form of inactivity), or temporary (participating public works programmes, finding certain types of temporary jobs). Correspondingly, an entry can be a new entry (the person was not in contact with the job office), or a re-entry form a temporary exit. This data structure was transformed into a fully balanced panel of all covered individuals that shows how many days an individual spent in unemployment, or in public works programmes in a particular month. Using the recorded reason for the exit, it was determined whether someone successfully applied for a job not in the public works programmes and a job that is not subsidized in other labour market programmes.[10] After successful exits the employment agency does some monitoring using data from employers about the type of the new job. This was used to determine whether the new job is in the public sector (where the JPA is not available).

As the eligibility for the JPA credit at the time of starting a job can change even day-by-day, the outcome variable has to describe a relatively short period

[10] Note, that the JPA, or other tax incentives are not considered as subsidies here. This refers to direct wage subsidies in various employment programmes, that often cover 100%, or more of the labour costs.

of time. I will use monthly exit rates and eligibility at the end of the month. Estimates for the major JPA groups from the tax database will be available for May only, thus I will only estimate the long term unemployed JPA credit's effects on successful exits for each May using data from 2011 and 2015.[11] I will use three and twelve month survival rates as additional outcome variables to measure long term impact but data on employment survival is fairly unreliable.

Finally, the number of days spent in unemployment and in public works programmes was used to calculate the length of unemployment spells according to the eligibility criteria of the long term unemployed. Eligibility requires at least six months of non-employment, therefore the final panel spans from 2012 to 2015. The descriptive statistics are shown in Table 14.

4 Empirical strategy

As described in the previous section, the JPA has clear eligibility criteria for all target groups which is observable in the data. Therefore, it is possible to find different sets of individuals who are similar to individuals in each JPA target group but who are not eligible for JPA tax credits. These groups can be used as controls to construct a counterfactuals and identify the employment effect of the JPA tax cuts.

The JPA was introduced in one step, in January 2013. This lends itself to a differences in differences estimator, in which the employment probabilities of treated (JPA) and non-treated (non-JPA) individuals is compared pre-treatment and post-treatment.

In general, the following equation is estimated for each JPA target group:

$$y_{it} = \beta_1 + \beta_2 JPA_i + \beta_{3t} + \beta_{4t} JPA_{it} + \gamma X_i + \varepsilon_{it} \tag{1}$$

where y_{it} shows is whether individual i is employed in period t at an employer who is eligible for the JPA, and in a labour contract that is also eligible for the JPA. JPA_i indicates whether the individual is in the JPA target group, β_{3t} are time fixed effects for several periods before and after the introduction of the JPA, and JPA_{it} is the interaction between treatment and period. Therefore β_{4t} are the variables of interest in the estimation, showing the changes in employment

[11] Entries into unemployment might be relevant for the assessment of the JPA but the unemployment registry only covers the population of those who were registered as unemployed at least once, and the COAEPS data can't be linked to other sources, thus flows into unemployment can't be analysed.

probabilities due to the JPA in each period after introduction. The JPA was announced in August 2012. As the final data cover only May in each year, possible anticipatory effects can be ruled out, and all years after that are considered as post-JPA.

The differences in differences method requires a common pre-treatment trend in employment between the treated and control groups. This might not hold but using further control variables X_i, including age, gender and lagged labour market status[12] can increase the reliability of the estimates by reducing the heterogeneity of the treated and control groups. The estimates can still suffer from omitted variable bias, as many other individual specific factors can influence the probability of working. A possible solution would be to use a fix effects estimator. The drawback of this approach is that defining treatment and control groups based on age will omit some individuals in some years (see Subsection 4.1), which itself can introduce bias in the estimations. Instead, I will use past employment as a control variable which correlates highly with current employment.

I will estimate Equation (1) for the three major JPA groups using linear probability models (LPM). The goal is to estimate the marginal effects of β_{4t} for all periods. These tend to be very similar for LPMs and for non-linear functional forms, like the logistic, or probit regressions in case of outcome probabilities that are not close to zero, or one. Here, the less strict conditions and easier interpretation of LPMs offer an advantage. However, in case of the low average exit rates for the long term unemployed the marginal rates can differ between LPMs and non-linear models, therefore in these cases I will estimate logistic regressions.

This differences in differences method can have several potential drawbacks. In order to get internally valid estimates, the treated and control groups have to be similar to each other. Since the target groups are quite broad, entire target groups might not be usable as treated groups in the estimations. However, a narrower treatment group used in the analysis could lead to a loss in external validity, as estimates will be valid only for a subgroup of the eligible population.

Additionally, this method measures changes in employment probabilities of the JPA target groups relative to similar groups. At the same time, the JPA also changed the relative wages of employees in these groups. Holding everything else constant, this could lead to a decreased demand for workers who are not eligible for the JPA credits relative to those who are eligible. If employers do

[12] I will use a broader definition of employment for the lagged variable that includes self-employment, temporary work and employment in the public sector but still excludes participation in the public works programmes. The aim is to control for past labour market status in general, while the outcome variable should only count employment in the JPA.

such substitution in their hiring, the employment gains from these estimations would be upward biased.

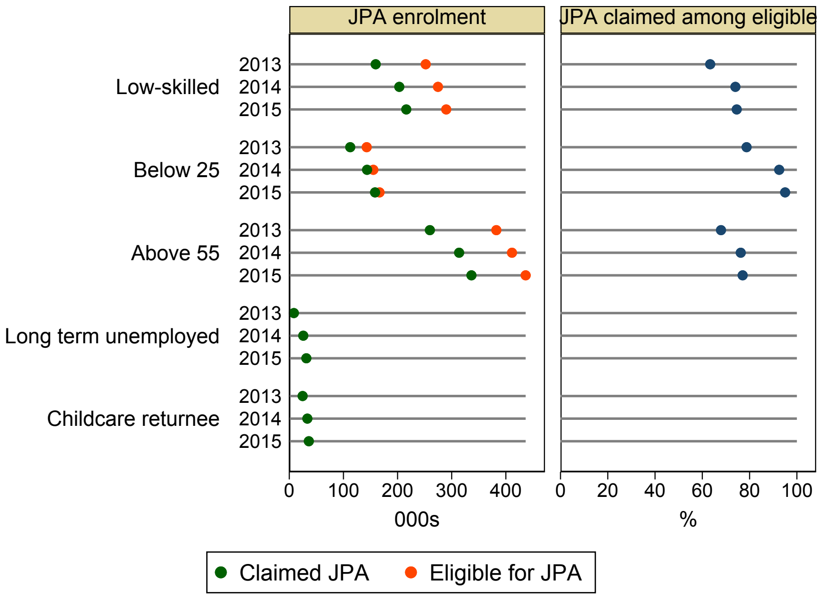
4.1 Defining treatment and control groups

The heterogeneous effects of the JPA across the various target groups are in themselves of interest when evaluating the programme but the different labour market situation in the target groups also require separate analyses, as different considerations have to made when selecting the population to identify the JPA's effects.

In general, I will analyse intention of treatment. Eligibility for the JPA doesn't necessary mean an employer will also claim the credit. If take-up rate is low and the JPA has an effect on employment, the estimated parameter will be biased downward, as it will include all the potential gains in employment in the treated population. For the purposes of estimating the extra employment due to the JPA, and calculating the tax cut's cost efficiency this it is not a limitation. Figure 2 shows the absolute numbers of eligible employees and the number of claimants, and the take-up ratio. The take-up was was already fairly high in May 2013 — five months after introduction — and it increased by next year in all major target groups. The take-up is highest among the young employees, while in the older and low-skilled groups it is somewhat lower. The long term unemployed and child care returnee target groups are much smaller. The number of eligible employees is not available directly for last two categories but estimates based on other data sources suggest a take-up rate of around 40 and 20 percent respectively.

Below 25 Similarly to Egebark and Kaunitz (2017), using the age of 25 as a cut-off value is a straightforward approach for this target group. The choice of the control group is limited, people several years older than 25 have different employment prospects, therefore I will use the 25–27 year old cohorts as control group. All cohorts below 25 are eligible for the JPA but the individual cohorts face different labour market conditions that could violate the parallel trends condition. A narrower age bracket sacrifices some external validity but using only the 22–24 year old cohorts as treatment group can increase the reliability of the estimates. Nevertheless, the effects of the JPA on the younger cohorts is relevant from a policy perspective. Employment rate of university graduates in Hungary is close to the EU average but the employment rate of those without a tertiary degree is significantly lower. One can expect the JPA to have a positive effect

Figure 2: Enrolment in the JPA target groups



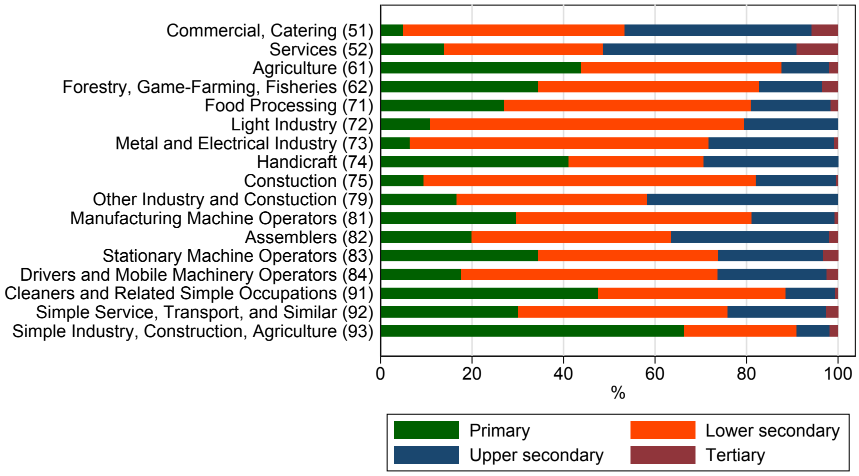
Source: NTCA database, own calculations

on employment prospects in the 19–23 year old cohorts among those who left school by this age, either by dropping out, or never attending university but this analysis will not provide separate estimates for this group.

In this analysis I will not estimate the effect of the separate JPA credit for career starters because the available data sources don't have information of total days worked. Since only few employers claim this JPA credit, and the main effect of the programme can be expected from the general credit targeting young people, this is not a major limitation when evaluating the JPA.

Above 55 Age-based cut-offs can be used here similar to the below 25 target group with similar trade-offs. In this case a wider age bracket could identify the JPA's effect on early retirements (see e.g. Laun 2012). However, there were major changes in the Hungarian pension system since 2012 that aimed at raising labour force participation in cohorts, where there is a lot of overlap with the JPA. The eligibility for the various early retirement schemes is not observable in the available data, which limits the list of cohorts for possible treatment groups. In general, people below 57 are not eligible for early retirement, therefore I will use the 55–57 year old cohorts as the treated group and the 52–54 year old cohorts as

Figure 3: Educational attainment across occupations by two-digit HSCO-08 codes for manual workers in 2012



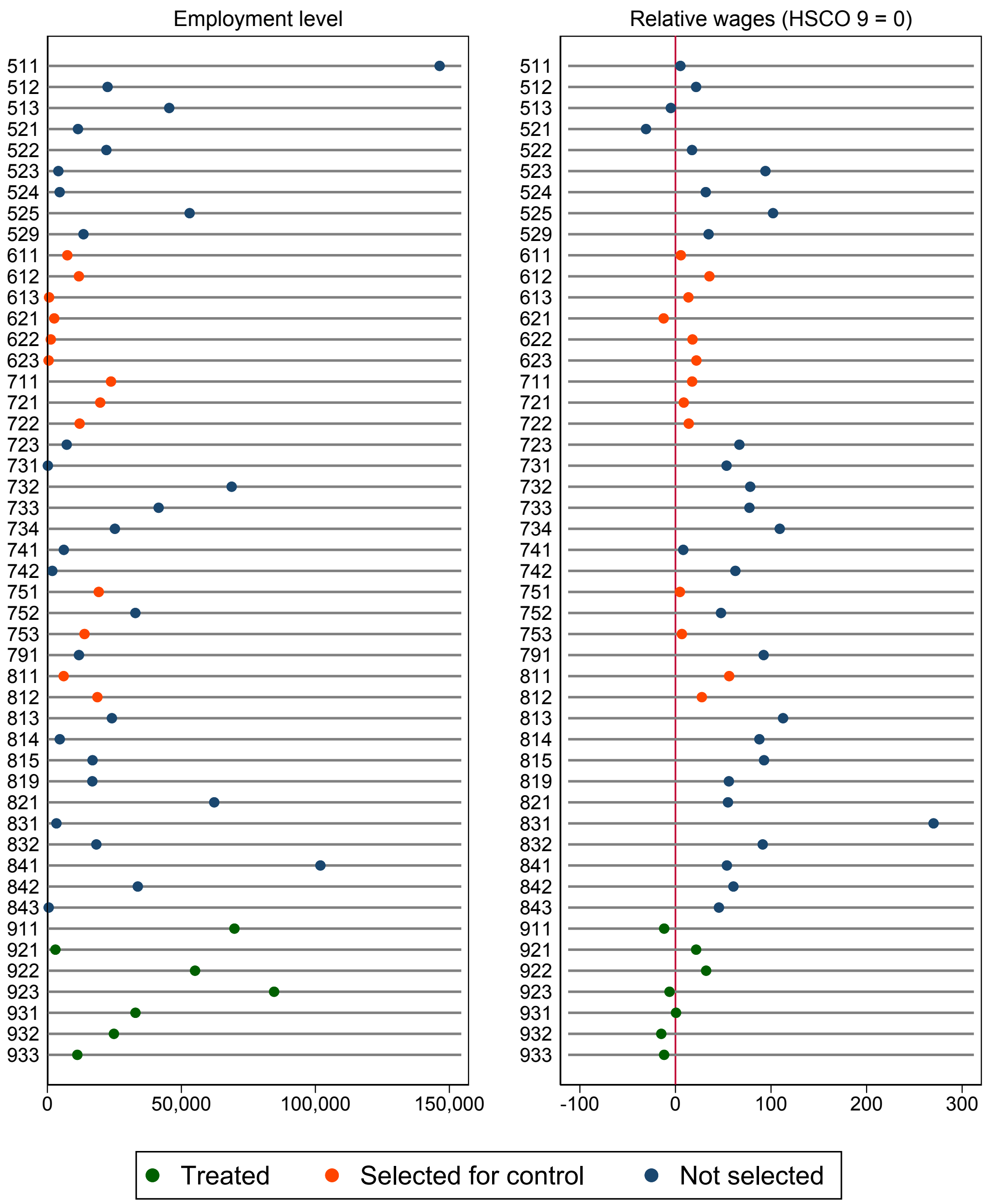
Source: HCSO LFS
Note: Definition of employment is the same as in Figure 1. The chart shows educational attainment only for prime age employees (between 25 and 54).

the controls. The regular retirement age was at least 62 during the years relevant for this analysis. However, since 2012 women are eligible for retirement after 40 years of service which lowers the effective retirement age for some women (high school degree, or less; while continuously employed, or cared for children) but in general this shouldn't affect the cohorts selected for the analysis. Previous early retirement schemes usually were not effective in these age brackets either.

Low-skilled Unlike the previous two groups, eligibility in this case is not observable for the non-employed. Occupations in category 9 of the HSCO-08 consist of low-skilled jobs in a variety of economic sectors (industry, services, agriculture) and Figure 3 shows that there are other occupations in all these sectors that employ mainly low-skilled workers without an upper secondary degree. The share of these is particularly high in agriculture but it is also high in food processing and construction, while many retail occupations — which employ a large number of people — don't require post-secondary degrees (see left panel in Figure 4). Low wages, which are close to the wages attainable in the least skilled category 9 occupations also reflect on the low productivity of these workers (right panel in Figure 4).

Past occupations are observable in the available data which allows the con-

Figure 4: Employment and wage levels by three-digit HSCO-08 occupations for manual workers in 2012



Source: NTCA database, own calculations
Note: See Table 15 for description of the occupations.

struction of a proxy measure of skill-level. People change occupations across these categories (which shows that these jobs are substitutes in some sense) but switching rates are not high. Therefore people who held a job in category 9 can be considered as treated in the JPA and people who held a job in an occupation similar to category 9 but didn't hold in category 9 can be considered as controls.[13] Similarity between occupations is based on the average wages, as seen in Figure 4. Occupations where educational levels are higher were excluded from the control groups.

As Figures 3 and 4 show, there are many low-skilled workers employed in the retail sector, where average wages are also low. However, retail sector is more heterogeneous, more workers have upper secondary degrees. Due to the large number of retail employees, I will add commercial occupations (ISCO-08 511) to the control group in an alternative specification.

Employers are free to choose which JPA credit they take for the young, or old low-skilled workers, and the amount of the credit is the same for all three groups. To keep the estimates of the different groups easily interpretable, estimations of the low-skilled target group will only cover the 25–54 age group, while estimations for the below 25 and above 55 groups will include all skill levels.

Long term unemployed People who had been unemployed since the same date can have different eligibility status for the long term unemployed JPA credit if they participated in the public works scheme during their unemployment spell. An unemployed person, who registered as unemployed six months before hiring was only eligible if she didn't participate in the public works schemes. This can be used as the identification strategy because people who are identical in every other regard but who were not employed in the primary labour market for the same amount of time have different eligibility status.

Data on unemployment spells is available from the unemployment registry (see Section 3.2). A successful outcome will be an exit to a non-subsidized job in Equation (1), with the JPA status based on eligibility described in the previous paragraph, controlling for the length of the unemployment spell, including time spent in the public works scheme.

Participation in the public works programme is observable in the data but other active labour market programmes (ALMPs) are not. Ideally the analysis should control for the participation in ALMPs, as these, along with the public

[13]Only occupations in labour contracts eligible for the JPA were taken into account. HSCO-08 has been in force since January 2011, and there is no direct, one-to-one correspondence with the previous version of the occupational classification. Therefore, only occupations held since 2011 were taken into account.

works programme could influence the probability of successfully leaving unemployment, as unemployed people can gain skills, or experience in these.

The changes in the tax system during the period covered in this analysis could affect the work or hiring incentives in either the JPA target groups, or the control groups. For the three major target groups these changes didn't have different affects on the treated and control groups. However, for the long term unemployed there could be an issue due to certain employers' tax incentives that were in place before the JPA. Until 2013 there were several narrowly targeted, voucher-based tax incentives called Start. Under the Start programmes employers could claim tax credits when hiring long term unemployed. The Start eligibility criteria were slightly different than the JPA criteria. One Start programme — Bónusz — was in force in 2012 and 2013. This programme was available for people who were unemployed continuously for at least six months. There were other, smaller scale programmes for career starters, low-skilled, or old unemployed, and mothers with young children. Therefore, I will estimate another model, only including jobseekers outside the target group of the major Start programmes: men aged between 30 and 50 with at least a lower secondary degree.

Data is only available for one year in the pre-treatment period, which makes the estimations more uncertain. However, this is not a major issue for this target group, as the method of identification uses the different treatment of participation in the public works programmes, where treatment status didn't change for the whole target group at once. Therefore we can use the variance between individuals over time.

I will estimate two models for this target group. First, using all exits of unemployed workers who had been in the registry unemployed, or in public works schemes for at least six months. Second, the same model with those, who were not eligible for the Start subsidies: unemployed men, between the ages of 30 and 50, with at least lower secondary education.

Mothers with young children A possible approach for identification is to compare the employment probabilities of mothers of children of different age, similarly to Szabó-Morvai (2015). The potential effects of the Start programmes have to be considered in the selection of the treated and control groups in this case too. Some data on the number and age of children is available in the tax returns through the family allowance, and the duration of childcare leave can also be identified. However, neither of these are sufficient to create treated and control groups without major biases, or omissions. Therefore, I will not estimate the ef-

Table 1: Differences in differences coefficients for the main regression results

	Below 25	Above 55	Low-skilled
JPA × 2010	−0.001	0.002	
	(0.002)	(0.002)	
JPA × 2011	0.004*	−0.002	−0.029***
	(0.002)	(0.001)	(0.001)
JPA × 2013	0.011***	0.005***	0.013***
	(0.002)	(0.001)	(0.001)
JPA × 2014	0.019***	0.008***	0.013***
	(0.002)	(0.002)	(0.001)
JPA × 2015	0.026***	0.008***	0.022***
	(0.002)	(0.002)	(0.001)

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level. For the full tables see Models (4) from Tables 5 and 6, and Model (5) from Table 8.

fect of the JPA in this target group. Further work and additional data is needed to extend this research.

5 Regression results

The results for the three major target groups are summarized in Table 1 with the differences in differences (DiD) coefficients from the full models including demographic controls and lagged employment. The detailed results for these target groups can be found in Tables 5, 6, and 8. The tables present four models: (1) a simple DiD model; (2) a DiD with demographic controls; (3) a DiD with lagged employment; and (4) the full model, a DiD with demographic controls and lagged employment. As anticipated, the basic differences in differences model suffers from omitted variable bias. The DiD coefficients are significant at the conventional levels. The demographic controls and the lagged dependent variables are also significant, the coefficient of lagged employment is large, and their inclusion decreases the DiD coefficients for all three target groups.

The estimated effects are increasing in time, which is expected, as both employers and employees need time to adjust to the tax incentive. The JPA already had economically significant effect on the employment probabilities of these three target groups even in the first year, five months after the introduction.

Pre-treatment DiD coefficients also become insignificant with lagged employment for the below 25 and above 55 groups but not for low-skilled. This suggest that the pre-treatment parallel trends assumptions are likely to hold, and the method is able to identify the true effect of the JPA for the below 25 and above 55 groups. For the low-skilled group a possible explanation for negative pre-treatment effects could be the long term decline in the employment of the low-skilled but these results suggest that the conditions for the DiD estimates might not hold.

Another explanation can be that the last occupation held has a major impact on what kind of new job an employee will take. The proxy measure used as treatment variable for skill level is based on all previous private sector occupations. Model (5) in Table 8 includes additional fixed effects for past occupation. While this model still has the same issue with a significant pre-treatment DiD effect, due to the significant and large coefficients for past occupation, and their impact in the DiD coefficients, this is the preferred model for the cost-benefit analyses. A further robustness check is shown for the low-skilled in Table 9 using an alternative definition for the control group, including retail workers. The estimated effects in this model are lower. Overall, these results suggest that the estimates for the low-skilled group are possibly the most uncertain.

Table 7 shows estimations for the above 55 group broken down by gender. The DiD coefficients for men are not significant, while for women they are higher than the average effect for the whole cohort. One explanation why the JPA was ineffective in raising the employment rates of men could be that such a tax cut only affects employment probabilities for people close to the retirement age (see Laun 2012; Albanese and Cockx 2015). The method is based on comparing the 52–53 year old with 56–57 the old. For men, these cohorts are far form either the effective, or the statutory the retirement age but for women they are close to the effective retirement age due to the early retirement scheme after 40 years of service. There were no significant gender differences for the JPA's effects in the two other target groups.

Table 2 shows the estimated marginal effects from the preferred model for the long term unemployed with the full results of the second specification using only non-Start jobseekers in Table 11. The results for the first specification using all eligible jobseekers are in Table 10. I used logistic models, as the exit probabilities from the unemployment registry are very low (between 2 and 4%), the marginal effects from an LPM can be very different from marginal effects in a non-linear model.

Although the average exit probabilities are decreasing in time, the estimated DiD effects with individual-specific factors are increasing. The JPA increased

Table 2: Marginal effects of the JPA for the long term unemployed from Table 11

	Exit to job (1 month survival)	Exit to job (3 month survival)	Exit to job (12 month survival)
2013	0.000	0.000	−0.000
2014	0.005	0.004	0.002
2015	0.007		

one month exit rates by 0.7 percentage points to an observed 1.6%. As the data spans to exits in June 2015, three and twelve month exit rates are only available for 2013 and 2014. The JPA's effect on three and twelve month survival rates are not significant. While this could indicate that the credit had no long term effect, the data available after leaving the unemployment registry is limited, which makes the measurement of these outcomes more uncertain.

5.1 Substitution effects

The design of the JPA limits the incentive to substitution within target groups, because employees in the three major target groups are eligible regardless when they were hired. However employers could still substitute employees not in the JPA target groups for those who are eligible. To check whether such substitution occurred, Equation (1) with the full set of controls can be estimated with the control groups from the previous section as "treated" groups (i.e. whose relative labour cost went up compared to the JPA target groups) and groups completely outside the scope of the JPA as controls. This means people in their early 30s for the young control group, people in their late 40s for the old control group and the those prime age manual workers who were not selected in either the treated, or control groups for the low-skilled regressions.

Table 12 shows the results. There are no signs of substitution for the non-eligible younger and older cohorts, the DiD coefficients are non-significant. The employment probabilities for the low-skilled but non-JPA workers decreased compared to other prime age blue collar employees (categories 5–8 in HSCO-08) using both the main and the alternative control group specifications. However, as discussed in Subsection 6.1, the overall effects of this substitution on employment levels is low.

6 Discussion and policy analysis

The results described in Section 5 show the effects attributed to the JPA but due to the methodological limitations discussed in Subsection 4.1 only for the treated and control groups used in the regression analysis which are narrower than the entire JPA target groups. To analyse the overall effect of the programme, to calculate labour demand elasticities, and to do a cost-benefit analysis, these results have to be extrapolated for the whole treated population.

6.1 Employment effects

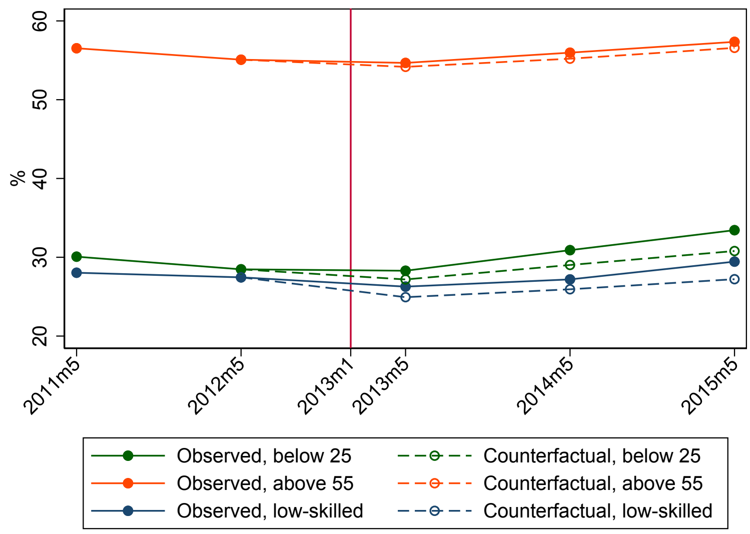
Figure 5 shows the JPA's impact in the three major target groups, extrapolating the regression results to the broader target groups. As discussed earlier, a tax subsidy for the youngest cohort — just after leaving school — and the oldest cohorts — close to retirement — could have very different effects than the estimates identified here comparing the cohorts around the JPA's age cutoff. Therefore, the below 25 groups consist of the 20–24 cohorts, the above 55 of the 55–59 cohorts,[14] and the low-skilled group consist of everyone identified as low-skilled according the proxy measure for skill in the estimation control groups. Employment ratios are calculated for the whole labour market, including the public sector, where the JPA is not available.

The counterfactuals are calculated as a percentage point difference of the regression results for each year in Table 1 from the observed employment ratios. The counterfactual lines show that the employment rates of all three target groups would have increased even without the JPA during the labour market recovery starting in 2013, but the JPA significantly increased employment rates for the young (from 30.8% to 33.4% by 2015) and the low-skilled (from 27.2% to 29.4%by 2015).

Figure 6 translates these results into number of jobs gained due to the JPA, and also corrects for the substitution effect found for the low-skilled target group. The JPA significantly increased the employment probabilities of the low-skilled workers, and due to the relatively large size of this target group (see Figure 2), most of the gains in employment levels — approximately 30 thousand by 2015 — came from the low-skilled workforce. The young employees' relatively small target group leads to only a 16 thousand increase in employment, despite the JPA's strong effects in this group. Employees above 55 form the largest target group, which means even with the JPA's low impact on their employment probabilities,

[14] Note, that the data was prepared only for the 20–59 cohorts.

Figure 5: Observed and counterfactual employment rates for the main JPA target groups



Note: The vertical line shows the introduction of the JPA in January 2013. Counterfactuals calculated from Models (4) from Tables 5 and 6, and Model (5) from Table 8.

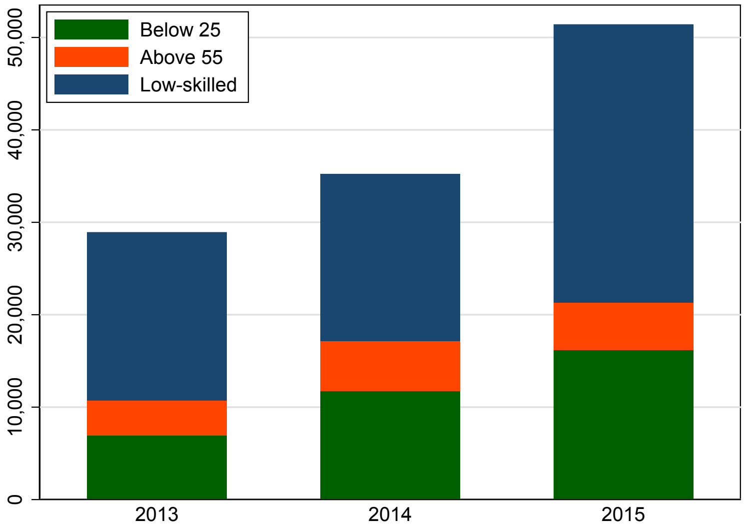
the JPA raised employment levels by around 5,000.[15]

The substitution effect amounts to only 2,600 in the low-skilled group control group, which was subtracted from the low-skilled figures in the chart.

The estimated effects for the long term unemployed credit are in terms of exit rates, that can't be translated directly into employment figures. On one hand, there is uncertainty around the three and twelve month survival rates. On the other hand, monthly employment estimates for the other JPA groups can be interpreted as a good approximation for the annual effects but monthly exit rates for the long term unemployed can differ from the annual exit rates. Nevertheless, we can give an upper bound by looking at the number of people in a particular year who were registered as unemployed, or as public works participants and had an at least six months long unemployment spell (that might span across two years) and counting how many of them left the registry by December of that year. In 2014 this group was around 460 thousand, of whom 44 thousand had a successful exit, a 8.6% exit rate. If the long term unemployed JPA increased this exit rate by 0.7% (see Table 2), the total effect would be around 3.5 thou-

[15]Note, that in this paper the low-skilled group always refers to prime age employees only, low-skilled young and old workers are counted in young and old target groups respectively.

Figure 6: Net employment gains caused by JPA in the main target groups



Note: Employment effects calculated from (4) from Tables 5 and 6, and Model (5) from Table 8, substitution effects from Table 12.

sand. In May 2015 there were 31 thousand employees covered by the long-term unemployed JPA credit, and half of them had been in their jobs for less than a year. This back-of-the-envelope calculation shows that the effect of the JPA for the long term unemployed was small, it increased employment by a few thousand. Adding these to the figures for the three major groups would increase the total employment gains by around 7%. However, compared to the actual number of newly hired claimants this effect is fairly substantial.

6.2 Budgetary effects

These net employment gains are also associated with general equilibrium effects, e.g. more consumption by the newly hired employees, which could increase output by more than just the higher employment through the JPA. Potential savings can also come from lower expenditure on unemployment benefits, pensions, or on labour market programmes, including employment in the public works programmes, which were not estimated in this papers. Simulations by Benedek, Kátay and Kiss (2013) based on the general equilibrium microsimulation model by Benczúr, Kátay and Kiss (2012) showed that the main effects, in particular the extra budgetary revenues of labour taxation reforms come from the direct behavioural changes in labour supply and labour demand. Therefore I will focus on the higher taxes and social security contributions coming from the net employment gains while noting, these should be considered as a lower bound.

To calculate the budgetary effect, a few assumptions have to be made. Tax rates stayed constant during the period of the analysis. Hungary has a completely flat PIT system with a statutory rate of 16%. The only major deduction is the family allowance for dependent children, claimed by around a quarter of the taxpayers. According to figures by the NTCA, the average effective tax rate was 14%. Social security contributions are also flat, except for the JPA credits. Employees pay 18.5% SSCs, and employers pay 28.5% SSC. All new employees are eligible for the standard JPA tax credit, which amounts to HUF 14,500/month.[16] Due to the flat tax system, these rates can be applied to the average private sector wages excluding public works programme participants for each year and each target group.

The estimated employment effects refer to monthly gains for the month of May in each year. The methodology cannot identify whether this led to permanent employment for the newly hired. Considering that May tends to be an "average" month in terms of employment levels, we can assume the same effect for all year, and multiply the May budgetary estimate by 12 for an annual estimate.

Finally, we can calculate self-financing ratios, by dividing the above estimates for the budgetary gains with the total budgetary expenditure on the JPA. The results are shown in Table 3.

The uncertainty in calculating the employment effects of the long term unemployed credit makes the estimation of budgetary effects also difficult. The amount of tax revenue gained through entering employment heavily depends on how long the newly hired could stay in their jobs but the data sources used in the paper don't provide sufficient guidance for this.

6.3 Labour demand elasticities

The quasi-experimental setup used is this paper doesn't allow the separation of demand and supply effects but it is still useful to describe the the results in terms of elasticities. Based on employment rates in Figure 5 and the regression coefficients, we can calculate elasticities η_{it} — which can be interpreted mainly

[16] The JPA cap is reduced for part time employees. Due to the low share of part time employment, I will ignore this rule.

Table 3: Estimated budgetary effect of the JPA

Year	Target group	Total fiscal cost (HUF bn)	Tax revenues from behavioural effects (HUF bn)	Self-financing ratio (%)
2013	Below 25	22	5	24
2014	Below 25	29	9	31
2015	Below 25	32	14	42
2013	Above 55	41	5	12
2014	Above 55	47	7	16
2015	Above 55	50	7	14
2013	Low-skilled	25	12	50
2014	Low-skilled	31	13	41
2015	Low-skilled	32	23	70
2013	Total	87	23	26
2014	Total	108	29	27
2015	Total	115	43	38

Note: Budgetary expenditures are only available in aggregated form, for each target group. Some employers claim the low-skilled tax credit for some of their young, or old employees, while these groups were separated for the regression analysis (see Subsection 4.1). Using the unadjusted budgetary figures would lead to upward biased self-financing ratios for the below 25 and above 55 groups, and a downward biased ratio for the low-skilled group. The total costs were adjusted by re-weighting the amount of tax expenditure claimed with the enrolment figures for the months of May from the micro data for each target group. After the re-weighting the cost for the career starters' tax credit was added to the below 25 group's cost.

as labour demand elasticities — for each target group i and year t as

$$\eta_{it} = \frac{\frac{\beta_{it}}{emp_{it}-\beta_{it}}}{\frac{1.285 \cdot wage_{it}-14500}{1.285 \cdot wage_{it}} - 1}$$

where β_{it} refers to β_{4t} from Equation (1) for each target group, and emp_{it} and $wage_{it}$ are the average employment rates and wages for each year and target group. Similarly to the budgetary estimates, both the employment rate and average wage exclude participation in the public works programmes. The results are shown in Table 4.

A recent meta analysis of own-wage labour demand elasticities by Lichter, Peichl and Siegloch (2014) shows an average elasticity of −0.25. However, they found large heterogeneity in the estimates. According to their results, Central

Table 4: Estimated labour demand elasticities for the major JPA target groups

	2013	2014	2015
Below 25	−0.45	−0.75	−1.06
Above 55	−0.17	−0.26	−0.26
Low-skilled	−0.56	−0.52	−0.92

Eastern European economies show a higher labour demand elasticity and the elasticity for low-skilled labour demand is higher. The predicted value from their model of a reduced form estimate for the total own-wage elasticity for low-skilled workers based on administrative panel data for Hungary ranges from −0.78 to −0.90 for short-run to long-run elasticities. The elasticity of −1 in this paper — which can be considered as an intermediate-term elasticity, where firms have adjusted their labour demand, yet the capital stock has not fully adjusted — is higher. This difference could be explained by the fairly large shock to labour costs (close to 9% reduction in labour cost on average), or the timing of the tax reform, which was during a period of economic recovery after the Great Financial Crisis.

Krrikyan (2013) has recent structural estimates for Hungarian labour demand elasticities for 2009 with a short-run elasticity for low-skilled at −0.27, and long-run elasticity at −1.83 for low-skilled workers.

Elasticity estimates for a comparable tax reform and methodology can be found in Egebark and Kaunitz (2017). They estimate an elasticity of −0.32 using a reduced form model for young employees in a targeted cut in Sweden. They estimated wage effects as well, which would result in a somewhat higher elasticity, compared to the method used in this paper. Nevertheless, there is a large difference between the two estimates. The higher Hungarian elasticity is in line with other studies (e.g. Lichter, Peichl and Siegloch 2014) and it could also be explained by the differences in the Swedish and Hungarian labour market situation. Sweden has very high participation rates, while in Hungary some groups, including the young workers have low participation, especially high rates of people not in employment, education, or training (NEET). While Egebark and Kaunitz (2017) found no major effect among unemployed (as opposed to the total affected cohorts) either but the large pool of potential workforce among the Hungarian youth and the different skill distributions in the two countries can explain the higher effects for the JPA.

7 Conclusions

I estimated the employment effects of a recent Hungarian targeted tax incentive scheme called the Job Protection Act (JPA) of 2013. It reduced employers' social security contributions of several groups that had low labour market participation, like the young, the old, the low-skilled, and long term unemployed. I used a quasi-experimental setup, exploiting the discontinuities of the JPA eligibility criteria with a differences in differences estimator using administrative micro data sources to identify the effects of the tax cuts.

The estimates show robust, statistically and economically significant effects for the programme. Employers already adjusted their labour demand in the first year of the introduction of the JPA, and by 2015 — after being in force for three years — the programme had significant positive effects on employment. It contributed significantly to the higher employment rates of young and low-skilled workers but it only marginally increased employment for older workers. Employment rates for the young increased by 2.6%, for the low-skilled by 2.2%, and for the old only by 0.8%. The change in the employment rate among the old was driven by the higher employment of women. There were no gender differences among the other groups. I found some evidence of employment churn, where employers substituted employees eligible under the JPA for similar but not eligible workforce. However, the magnitude of this effect was small, it reduced the net employment gains by less than 3,000 among the the low-skilled. Overall the JPA led to a net employment gain of around 50,000 which amounts to 1.2% of the labour force. Higher employment increased tax and social security revenues as well. Self-financing ratios — the ratio of the extra revenue from newly hired employees and the total fiscal cost of the programme — were as high as 70% in the low-skilled target group and 40% in the young target group, but only 14% in the old target group.

The JPA credit for the long term unemployed increased exit rates from unemployment by around 0.7%. It is a substantial increase but due to the low take-up rate and small target groups this raised employment by 3,500 at most. The JPA also reduced the employers' social security contributions of employees returning after a child-care leave but due to the limitations of the available data I couldn't analyse the programme's effect in this target group.

Employment effects are not the only possible channels through which employers and employees can react to the JPA. A possible extension of this analysis could look at effects on wages, or employer performance, like sales, or profits. Saez, Schoefer and Seim (2017) showed that a Swedish tax incentive targeting young employees similar to the JPA increased the employment for the targeted population but it had no direct effect on young employees' wages (as showed previously by Egebark and Kaunitz 2017 for this reform). However, firms that had a high share of young employees prior the tax reform increased their sales, profits and wages for all of their workers relative to other firms. This suggests that firm level wage rigidities — perhaps equity concerns — limit the pass-through of the tax cuts in wages. The high estimated labour demand elasticities suggest the potential wage effects could be low but analysing these adjustments can be a potential extension of this paper.

The results in this paper show that the targeted tax cuts of the Job Protection Act successfully contributed to the labour market recovery in Hungary at a relatively low fiscal cost. However, there might be some scope to refine the programme by focusing on those groups, where the labour demand elasticity is higher.

Another interesting finding is the low take-up rate for the long-term unemployed in child-care returnee groups. This might be explained by the complex administration required from both the job-seekers and the employers. Take-up in the three major target groups — below 24, above 55, low-skilled — is higher, and employers can easily claim the tax credit on their monthly tax filings without any need for further proof, as eligibility can be checked using available data. According to my results the JPA substantially raised the chances of exiting unemployment, therefore encouraging participation could be a cost effective way of helping the long-term unemployed. This could be achieved by providing better information about the JPA to employers, or by easing the administrative burden of the programme in this target group.

References

Albanese, Andrea and Bart Cockx (2015). *Permanent Wage Cost Subsidies for Older Workers: An Effective Tool for Increasing Working Time and Postponing Early Retirement?* IZA Discussion Papers 8988. Institute for the Study of Labor (IZA). URL: `http://ftp.iza.org/dp8988.pdf`.

Benczúr, Péter, Gábor Kátay and Áron Kiss (2012). *Assessing changes of the Hungarian tax and transfer system: A general-equilibrium microsimulation approach*. MNB Working Paper 2012/7. Magyar Nemzeti Bank (the central bank of Hungary). URL: `https://ideas.repec.org/p/mnb/wpaper/2012-7.html`.

Benedek, Dóra, Gábor Kátay and Áron Kiss (2013). "Microsimulation as a tool for assessing the impact of tax changes". In: *The Hungarian Labour Market 2013*. Ed. by Károly Fazekas, Péter Benczúr and Álmos Telegdy. Centre for

Economic and Regional Studies, Hungarian Academy of Sciences & National Employment Non-profit Public Company Ltd. URL: http://econ.core.hu/file/download/HLM2013/TheHungarianLabourMarket_2013_onefile.pdf.

Blaskó, Zsuzsa and Károly Fazekas, eds. (2016). *The Hungarian Labour Market 2016*. Centre for Economic and Regional Studies, Hungarian Academy of Sciences & National Employment Non-profit Public Company Ltd. URL: http://www.econ.core.hu/file/download/HLM2016/TheHungarianLabourMarket_2016_onefile.pdf.

Cseres-Gergely, Zsombor, Ágota Scharle and Árpád Földessy (2015). *Evaluating the impact of a well-targeted wage subsidy using administrative data*. Budapest Working Papers on the Labour Market 1503. Institute of Economics, Centre for Economic and Regional Studies, Hungarian Academy of Sciences. URL: https://ideas.repec.org/p/has/bworkp/1503.html.

Egebark, Johan and Niklas Kaunitz (2017). *Do Payroll Tax Cuts Raise Youth Employment?* Working Paper Series 1001. Research Institute of Industrial Economics. URL: https://www.ifn.se/eng/publications/wp/2014/1001.

Kátay, Gábor (2009). *Az alacsony aktivitás és foglalkoztatottság okai és következményei Magyarországon*. MNB-tanulmányok 79. Magyar Nemzeti Bank. URL: https://www.mnb.hu/letoltes/mt-79.pdf.

Krrikyan, Vahe (2013). "Estimating the demand for heterogeneous labor in Hungary during the pre-crisis and crisis periods". MA thesis. Central European University. URL: http://www.etd.ceu.hu/2013/krrikyan_vahe.pdf.

Laun, Lisa (2012). *The effect of age-targeted tax credits on retirement behavior*. Working Paper Series 2012:18. IFAU - Institute for Evaluation of Labour Market and Education Policy. URL: https://ideas.repec.org/p/hhs/ifauwp/2012_018.html.

Lichter, Andreas, Andreas Peichl and Sebastian Siegloch (2014). *The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis*. IZA Discussion Papers 7958. Institute for the Study of Labor (IZA). URL: http://ftp.iza.org/dp7958.pdf.

OECD (2017). *Taxing Wages 2017*. OECD Publishing. DOI: 10.1787/tax_wages-2017-en.

Saez, Emmanuel, Benjamin Schoefer and David Seim (2017). *Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden*. Working Paper 23976. National Bureau of Economic Research. DOI: 10.3386/w23976.

Szabó-Morvai, Ágnes Szobonyáné (2015). "Essays on maternal employment policies". PhD thesis. Central European University. DOI: 10.14754/CEU.2015.05.

A Tables

Table 5: Regression results for the below 25 target group

	(1)	(2)	(3)	(4)
JPA	−0.198***	−0.029***	−0.061***	−0.016***
	(0.001)	(0.002)	(0.001)	(0.002)
2010	0.016***	0.016***	0.037***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)
2011	0.022***	0.023***	0.023***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)
2013	−0.016***	−0.016***	−0.007***	−0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
2014	−0.011***	−0.011***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
2015	−0.002	−0.002	0.010***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2010	0.011***	0.010***	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2011	0.007***	0.006***	0.004*	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2013	0.013***	0.013***	0.011***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2014	0.030***	0.029***	0.019***	0.019***
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2015	0.044***	0.043***	0.026***	0.026***
	(0.002)	(0.002)	(0.002)	(0.002)
Female		−0.082***		−0.057***
		(0.001)		(0.001)
Age		0.042***		0.012***
		(0.000)		(0.000)
Employment (lagged)			0.447***	0.443***
			(0.001)	(0.001)
Constant	0.457***	−0.627***	0.204***	−0.075***
	(0.001)	(0.010)	(0.001)	(0.012)
N	2,988,244	2,988,244	2,988,244	2,988,244

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 6: Regression results for the above 55 target group

	(1)	(2)	(3)	(4)
JPA	-0.050^{***}	0.005^{**}	-0.028^{***}	0.002
	(0.001)	(0.002)	(0.001)	(0.002)
2010	-0.002	-0.001	0.032^{***}	0.033^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
2011	0.007^{***}	0.008^{***}	0.011^{***}	0.011^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
2013	-0.004^{***}	-0.004^{***}	-0.002	-0.002^{*}
	(0.001)	(0.001)	(0.001)	(0.001)
2014	0.004^{**}	0.004^{*}	0.013^{***}	0.013^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
2015	0.012^{***}	0.010^{***}	0.018^{***}	0.016^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2010	0.011^{***}	0.010^{***}	0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2011	-0.001	-0.001	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2013	0.004^{**}	0.004^{**}	0.005^{***}	0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2014	0.009^{***}	0.009^{***}	0.008^{***}	0.008^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
JPA × 2015	0.012^{***}	0.013^{***}	0.007^{***}	0.008^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Female		-0.095^{***}		-0.101^{***}
		(0.001)		(0.001)
Age		-0.013^{***}		-0.007^{***}
		(0.000)		(0.000)
Employment (lagged)			0.487^{***}	0.488^{***}
			(0.001)	(0.001)
Constant	0.416^{***}	1.170^{***}	0.093^{***}	0.523^{***}
	(0.001)	(0.016)	(0.001)	(0.017)
N	3,282,580	3,282,580	3,282,580	3,282,580

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 7: Regression results for the above 55 target group by gender

	Men	Women
JPA	0.001	0.004
	(0.003)	(0.002)
2010	0.021***	0.040***
	(0.002)	(0.002)
2011	0.014***	0.009***
	(0.002)	(0.001)
2013	−0.000	−0.003*
	(0.002)	(0.001)
2014	0.022***	0.004*
	(0.002)	(0.002)
2015	0.030***	0.004*
	(0.002)	(0.002)
JPA × 2010	−0.007**	0.010***
	(0.002)	(0.002)
JPA × 2011	−0.006**	0.002
	(0.002)	(0.002)
JPA × 2013	0.003	0.006**
	(0.002)	(0.002)
JPA × 2014	0.004	0.011***
	(0.002)	(0.002)
JPA × 2015	0.003	0.013***
	(0.003)	(0.003)
Age	−0.005***	−0.009***
	(0.001)	(0.000)
Employment (lagged)	0.547***	0.434***
	(0.001)	(0.001)
Constant	0.378***	0.569***
	(0.027)	(0.023)
N	1,561,333	1,721,247

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 8: Regression results for the low-skilled target group (control group without retail workers)

	(1)	(2)	(3)	(4)	(5)
JPA	-0.463^{***}	-0.457^{***}	-0.295^{***}	-0.291^{***}	-0.284^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2011	0.044^{***}	0.044^{***}	0.060^{***}	0.060^{***}	0.066^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2013	-0.032^{***}	-0.032^{***}	-0.016^{***}	-0.016^{***}	-0.032^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2014	-0.023^{***}	-0.024^{***}	0.005^{***}	0.005^{***}	-0.025^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2015	-0.019^{***}	-0.019^{***}	0.003^{*}	0.003^{*}	-0.032^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2011	-0.032^{***}	-0.032^{***}	-0.041^{***}	-0.041^{***}	-0.029^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2013	0.025^{***}	0.025^{***}	0.012^{***}	0.012^{***}	0.013^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2014	0.027^{***}	0.027^{***}	0.015^{***}	0.015^{***}	0.013^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2015	0.043^{***}	0.044^{***}	0.031^{***}	0.031^{***}	0.022^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		-0.040^{***}		-0.028^{***}	0.002^{***}
		(0.001)		(0.000)	(0.000)
Age		0.000^{***}		-0.000^{***}	-0.001^{***}
		(0.000)		(0.000)	(0.000)
Employment (lagged)			0.440^{***}	0.439^{***}	0.201^{***}
			(0.000)	(0.000)	(0.000)
Constant	0.690^{***}	0.697^{***}	0.353^{***}	0.367^{***}	0.230^{***}
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Last occupation					Yes
N	8,323,472	8,323,472	8,323,472	8,323,472	8,323,472

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 9: Regression results for the low-skilled target group (control group including retail workers)

	(1)	(2)	(3)	(4)	(5)
JPA	−0.502***	−0.504***	−0.318***	−0.319***	−0.344***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2011	0.035***	0.035***	0.050***	0.050***	0.054***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2013	−0.027***	−0.027***	−0.016***	−0.015***	−0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2014	−0.019***	−0.019***	0.002**	0.002**	−0.022***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2015	−0.015***	−0.015***	−0.002*	−0.002*	−0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2011	−0.023***	−0.023***	−0.033***	−0.033***	−0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2013	0.021***	0.021***	0.011***	0.011***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2014	0.022***	0.022***	0.018***	0.018***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2015	0.040***	0.040***	0.036***	0.035***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		−0.023***		−0.013***	0.008***
		(0.001)		(0.000)	(0.000)
Age		0.000*		−0.000***	−0.001***
		(0.000)		(0.000)	(0.000)
Employment (lagged)			0.445***	0.445***	0.212***
			(0.000)	(0.000)	(0.000)
Constant	0.734***	0.744***	0.381***	0.402***	0.267***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Last occupation					Yes
N	9,158,669	9,158,669	9,158,669	9,158,669	9,158,669

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 10: Logit estimates for the long term unemployed target group, all eligible jobseekers

	Exit to job (1 month survival)	Exit to job (3 month survival)	Exit to job (12 month survival)
JPA	-0.291^{***}	-0.289^{***}	-0.207^{***}
	(0.028)	(0.028)	(0.032)
2013	-0.197^{***}	-0.203^{***}	-0.121^{**}
	(0.031)	(0.032)	(0.038)
2014	-0.157^{***}	-0.176^{***}	-0.110^{**}
	(0.037)	(0.036)	(0.039)
2015	-0.615^{***}		
	(0.039)		
JPA × 2013	0.060	0.046	−0.031
	(0.035)	(0.036)	(0.044)
JPA × 2014	0.191^{***}	0.191^{***}	0.131^{***}
	(0.041)	(0.040)	(0.039)
JPA × 2015	0.373^{***}		
	(0.053)		
Unemployment spell length	-0.039^{***}	-0.045^{***}	-0.046^{***}
	(0.001)	(0.001)	(0.001)
Female	-0.155^{***}	-0.141^{***}	-0.118^{***}
	(0.030)	(0.030)	(0.021)
Age	-0.008^{***}	-0.006^{***}	-0.009^{***}
	(0.001)	(0.001)	(0.001)
Educational attainment (ref. Lower secondary)			
Primary	-0.549^{***}	-0.602^{***}	-0.816^{***}
	(0.024)	(0.024)	(0.029)
Upper secondary	0.017	0.025	0.165^{***}
	(0.018)	(0.018)	(0.022)
Tertiary	-0.086^{*}	−0.052	0.225^{***}
	(0.034)	(0.035)	(0.039)
Constant	-2.104^{***}	-2.173^{***}	-2.654^{***}
	(0.049)	(0.052)	(0.066)
N	2,022,271	1,538,634	1,538,634

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the district level.

Table 11: Logit estimates for the long term unemployed target group, only job-seekers not eligible for the major Start programmes

	Exit to job (1 month survival)	Exit to job (3 month survival)	Exit to job (12 month survival)
JPA	-0.308^{***}	-0.319^{***}	-0.181^{***}
	(0.043)	(0.044)	(0.054)
2013	-0.237^{***}	-0.254^{***}	-0.183^{**}
	(0.054)	(0.056)	(0.065)
2014	-0.278^{***}	-0.295^{***}	-0.233^{**}
	(0.064)	(0.064)	(0.074)
2015	-0.733^{***}		
	(0.070)		
JPA × 2013	0.016	0.020	-0.033
	(0.063)	(0.066)	(0.078)
JPA × 2014	0.206^{**}	0.198^{*}	0.164
	(0.075)	(0.077)	(0.091)
JPA × 2015	0.448^{***}		
	(0.080)		
Unemployment spell length	-0.044^{***}	-0.050^{***}	-0.055^{***}
	(0.001)	(0.002)	(0.002)
Age	-0.018^{***}	-0.017^{***}	-0.020^{***}
	(0.002)	(0.003)	(0.003)
Educational attainment (ref. Lower secondary)			
Upper secondary	-0.017	-0.011	0.169^{***}
	(0.032)	(0.033)	(0.036)
Tertiary	-0.102^{*}	-0.075	0.189^{**}
	(0.051)	(0.056)	(0.068)
Constant	-1.417^{***}	-1.444^{***}	-1.899^{***}
	(0.097)	(0.112)	(0.118)
N	236,149	186,598	186,598

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the district level.

Table 12: Regression results for substitution effects in the main target groups

	26–27 vs. 28–30	50–51 vs. 52–53	Low-skilled excl. retail vs. Manual workers	Low-skilled incl. retail vs. Manual workers
JPA	−0.000	0.000	−0.037***	0.078***
	(0.001)	(0.001)	(0.001)	(0.001)
2010	0.035***	0.036***		
	(0.001)	(0.001)		
2011	0.018***	0.012***	0.061***	0.062***
	(0.001)	(0.001)	(0.000)	(0.000)
2013	−0.006***	−0.004***	−0.031***	−0.033***
	(0.001)	(0.001)	(0.000)	(0.000)
2014	0.003*	0.013***	−0.035***	−0.038***
	(0.001)	(0.001)	(0.000)	(0.000)
2015	0.005***	0.017***	−0.039***	−0.044***
	(0.001)	(0.001)	(0.001)	(0.001)
JPA × 2010	0.003	−0.003		
	(0.002)	(0.002)		
JPA × 2011	0.005*	−0.001	0.035***	0.020***
	(0.002)	(0.002)	(0.001)	(0.001)
JPA × 2013	−0.001	0.002	−0.009***	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
JPA × 2014	0.004*	−0.001	−0.005***	−0.000
	(0.002)	(0.002)	(0.001)	(0.001)
JPA × 2015	0.005**	−0.001	−0.012***	−0.006***
	(0.002)	(0.002)	(0.001)	(0.001)
Employment (lagged)	0.463***	0.488***	0.194***	0.202***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	−0.084***	−0.103***	0.017***	0.021***
	(0.001)	(0.001)	(0.001)	(0.001)
Age	−0.004***	−0.004***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.331***	0.337***	−0.152***	−0.171***
	(0.008)	(0.018)	(0.001)	(0.001)
Last occupation			Yes	Yes
N	3,010,064	2,951,425	5,689,057	6,670,663

Note: *, if $p < 0.05$, **, if $p < 0.01$, ***, if $p < 0.001$. Standard errors in parentheses are adjusted for clustering at the individual level.

Table 13: Descriptive statistics of the tax returns panel

	count	*mean*	*min*	*max*
2010				
Employment	5,574,215	0.419	0	1
Age	5,574,215	39.679	20	59
Female	5,574,215	0.505	0	1
Employment (lagged)	5,574,215	0.533	0	1
HSCO-08 occupation	3,447,749	5203.699	110	9,220
2011				
Employment	5,567,835	0.427	0	1
Age	5,567,835	39.726	20	59
Female	5,567,835	0.503	0	1
Employment (lagged)	5,567,835	0.590	0	1
HSCO-08 occupation	3,392,774	5190.466	110	9,332
2012				
Employment	5,557,169	0.416	0	1
Age	5,557,169	39.789	20	59
Female	5,557,169	0.502	0	1
Employment (lagged)	5,557,169	0.595	0	1
HSCO-08 occupation	3,354,003	5198.716	110	9,332
2013				
Employment	5,525,704	0.411	0	1
Age	5,525,704	39.803	20	59
Female	5,525,704	0.500	0	1
Employment (lagged)	5,525,704	0.586	0	1
HSCO-08 occupation	3,334,979	5230.431	110	9,332
2014				
Employment	5,476,501	0.420	0	1
Age	5,476,501	39.760	20	59
Female	5,476,501	0.499	0	1
Employment (lagged)	5,476,501	0.573	0	1
HSCO-08 occupation	3,341,327	5223.313	110	9,332
2015				
Employment	5,429,450	0.430	0	1
Age	5,429,450	39.739	20	59
Female	5,429,450	0.498	0	1
Employment (lagged)	5,429,450	0.585	0	1
HSCO-08 occupation	3,456,253	5314.335	110	9,332

Note: Employment refers to the definition used throughout the paper: private sector employment in contracts that are eligible for the JPA. The definition used for the lagged employment is broader, it includes employment in the public sector, temporary worker, self-employed, but excludes public works participation.

Table 14: Descriptive statistics of the unemployment registry

	count	*mean*	*min*	*max*
2012				
Exit to job	507,400	0.023	0	1
Exit to job (min 3 months)	507,400	0.021	0	1
Exit to job (min 12 months)	507,400	0.012	0	1
JPA	507,400	0.807	0	1
Unemployment spell length	507,400	29.489	6	191
Female	507,400	0.489	0	1
Age	507,400	40.056	18	62
Education	507,400	1.858	1	4
2013				
Exit to job	524,259	0.019	0	1
Exit to job (min 3 months)	524,259	0.016	0	1
Exit to job (min 12 months)	524,259	0.010	0	1
JPA	524,259	0.831	0	1
Unemployment spell length	524,259	32.716	6	203
Female	524,259	0.485	0	1
Age	524,259	40.512	18	62
Education	524,259	1.847	1	4
2014				
Exit to job	506,975	0.020	0	1
Exit to job (min 3 months)	506,975	0.017	0	1
Exit to job (min 12 months)	506,975	0.011	0	1
JPA	506,975	0.798	0	1
Unemployment spell length	506,975	36.667	6	215
Female	506,975	0.491	0	1
Age	506,975	40.829	18	62
Education	506,975	1.822	1	4
2015				
Exit to job	483,637	0.013	0	1
Exit to job (min 3 months)	0	.	.	.
Exit to job (min 12 months)	0	.	.	.
JPA	483,637	0.792	0	1
Unemployment spell length	483,637	40.497	6	217
Female	483,637	0.507	0	1
Age	483,637	41.184	18	62
Education	483,637	1.811	1	4

Table 15: List and descriptive statistics of HSCO-08 occupations for 2012, and selection into control groups for the low-skilled estimations

HSCO-08 3-digit code		Wage			Employed	Treated/ Control
		Avg. (HUF)	Median (HUF)	Rel. to HSCO cat. 9 (%)		
511	Commercial occupations	111,606	108,000	5.2	146,365	None
512	Other commercial occupations	129,026	117,133	21.6	22,464	None
513	Catering industry occupations	100,960	108,000	−4.9	45,467	None
521	Personal service workers	73,445	59,500	−30.8	11,412	None
522	Personal care workers	124,479	113,000	17.3	22,010	None
523	Stewards, attendants	206,038	196,136	94.2	4,099	None
524	Building maintenance occupations	139,754	120,000	31.7	4,576	None
525	Life and property protection occupations	214,451	194,721	102.1	53,068	None
529	Other services occupations	142,880	127,910	34.7	13,474	None
611	Plant cultivation occupations	112,053	108,000	5.6	7,430	Control
612	Animal producing occupations	143,820	138,310	35.5	11,761	Control
613	Mixed crop and animal producers	120,515	108,000	13.6	636	Control
621	Forestry workers	92,997	92,999	−12.4	2,562	Control
622	Game-farming occupations	125,047	111,000	17.8	1,265	Control
623	Fish-farming occupations	129,350	120,543	21.9	459	Control
711	Food producing, processing and preservation occupations	124,631	120,783	17.5	23,744	Control
721	Garment and leather industry workers	115,348	108,001	8.7	19,722	Control
722	Wood industry occupations	120,858	108,800	13.9	12,039	Control
723	Printing trades workers	177,062	144,095	66.9	7,210	None
731	Metallurgical occupations	162,879	153,474	53.5	183	None
732	Metal working occupations	189,107	162,400	78.2	68,758	None
733	Maintenance and repair mechanics of machines and equipment	188,229	164,200	77.4	41,519	None
734	Technicians and mechanics of electrical equipment	221,907	193,300	109.1	25,193	None

Continued on next page

Table 15 — Continued from previous page

HSCO-08 3-digit code		Wage: Avg. (HUF)	Wage: Median (HUF)	Wage: Rel. to HSCO cat. 9 (%)	Employed	Treated/ Control
741	Handicraft workers	114,795	93,000	8.2	6,128	None
742	Precision instrument mechanics	172,633	155,000	62.7	1,827	None
751	Master builders' occupations	111,028	108,000	4.6	19,161	Control
752	Construction, assembly occupations	156,675	135,000	47.7	32,831	None
753	Specialized construction industry occupations	113,257	108,000	6.7	13,857	Control
791	Other industry and construction industry occupations	204,035	161,221	92.3	11,787	None
811	Food, beverage and tobacco products machine operators	165,758	146,952	56.2	6,070	Control
812	Light industry machine operators and production-line workers	135,429	121,702	27.6	18,656	Control
813	Basic chemicals and chemical products manufacturers machine operators	225,592	187,402	112.6	24,065	None
814	Base materials products machine operators	199,268	182,318	87.8	4,620	None
815	Metal processing and finishing plant operators	204,466	177,000	92.7	16,853	None
819	Other manufacturing machine operators	165,406	147,909	55.9	16,762	None
821	Assemblers	164,408	153,416	54.9	62,257	None
831	Mining plant operators	392,653	370,560	270.0	3,377	None
832	Other stationary machine operators	202,960	168,347	91.3	18,266	None
841	Drivers of vehicles and related occupations	163,207	137,235	53.8	101,881	None
842	Mobile machinery operators	170,469	156,500	60.7	33,731	None
843	Shipping occupations	154,382	140,589	45.5	491	None
911	Cleaners and helpers	93,554	94,603	−11.8	69,797	Treated
921	Garbage collectors and similar occupations	129,054	122,418	21.6	3,008	Treated
922	Transport and storage labourers	140,150	130,000	32.1	55,102	Treated
923	Other simple service and transport occupations	99,522	94,700	−6.2	84,588	Treated
931	Simple industry occupations	106,746	100,000	0.6	32,863	Treated
932	Simple construction industry occupations	90,350	93,000	−14.9	24,824	Treated
933	Simple agricultural, forestry, hunting, fishery labourers	93,538	93,000	−11.8	11,174	Treated