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# The effects of hiring credits on firm dynamics: a synthetic difference-in-differences evaluation

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## Abstract

Despite hiring credits being a long-standing labor market policy, evidence of their impact at the firm level is limited. This paper contributes to the existing literature by providing an empirical analysis of an untargeted hiring credits policy introduced in Italy in 2015. We implement the Synthetic Control Difference in Differences method using administrative employer-employee data. The results show an increase in employment but at the same time a reduction in capital intensity, labor productivity and workforce quality. Furthermore, the reduction in labor costs for involved firms is not associated with an increase in their profitability.

**Keywords:** hiring credits, synthetic difference-in-differences, firm performance.

**JEL:** J21, J08, D22.

**Total words:** 8194

## 1 Introduction

Hiring credits are an active labor market policy (ALMP) that has been widely used after the Great Recession to ease labor market conditions. The empirical research concerning this policy has mainly focused on evaluating the effects on employment (Crépon and Van Den Berg, 2016) and has proved that hiring credits produce positive effects on this outcome (Card et al., 2010; Cahuc et al., 2019; Vooren et al., 2019). However, evidence on the effects of hiring credits on subsidized firms is scant. This paper bridges this gap by considering an economy-wide employer-borne payroll tax cut designed for permanent hirings that occurred in Italy in 2015 (Law 190/2014) and evaluating the effects of this policy on firm performance. The empirical analysis is based on high-quality administrative data provided by the National Social Security Institute (INPS), which contains the universe of non-agricultural firms with at least one employee.

This study brings two contributions to the existing literature. First of all, we exploit the Synthetic Control Difference in Differences (SDID) originally proposed by Arkhangelsky et al. (2021) that combines two methods: Synthetic Control (SC) and Difference in Differences (DID). To the best of our knowledge, this is the first time that this methodology has been implemented with microdata in a causal inference framework. Secondly, by using a high number of firm-level outcomes related to

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input utilization, profitability, productivity, and workforce composition, we provide new evidence on the impact of economy-wide hiring credits on subsidized firms.

Theoretically, the potential effects of hiring credits on the firms involved in the program are ambiguous: while a reduction of labor costs may boost employment (Batut, 2021), the effect on firm performance is less clear. On one side, together with its direct positive effect, the cost reduction might have further positive indirect effects, e.g., it could be redirected towards improving firms' screening activities, enhancing the match quality, and consequently increasing productivity (Faccini, 2014). Conversely, firms might hire workers whose productivity would not justify the match without the subsidy, potentially resulting in a decline in overall productivity (Boone and Van Ours, 2004). The impact of hiring credits on firm capital intensity is also ambiguous: the tax cut provides firms with a cash flow that might be channeled into capital accumulation, yet the dynamics of the capital-to-labor ratio also depend on the substitutability of inputs in the specific production function of the firm (Benzarti and Harju, 2021a). Eventually, the ambiguity surrounding the effects on firms' performance underscores the significance of our empirical analysis at the firm level.

We investigate firm dynamics by using the INPS employer-employee dataset, which allows us to identify the subsidized firms, i.e. those that have registered at least one subsidized contract in 2015, and provides information on the workforce composition and wage dynamics at the firm level. We merge these data with balance sheet data on the universe of limited liability companies in Italy provided by the CERVED archive. This is a subsample of the Italian firms that excludes sole proprietorships and represents the main sample of our empirical analysis.

The policy does not provide any exogenous variation to identify its effects on firms, as it simply offers an unconditional tax cut to employers for permanent hirings. Thus, we exploit the panel dimension of our data and a data-driven matching procedure provided by the SDID method developed by Arkhangelsky et al. (2021). This method combines SC and DID by using a weighting procedure originally developed by Abadie and Gardeazabal (2003) for his SC, which selects units in the control group, but also allows unit-level shifts in the outcome variables, i.e. unit fixed effects, as DID does.

As for the overall effects of the incentive, in line with the literature, the expected increase in employment and the reduction in labor cost per worker resulting from the incentive are respectively an average of 8.6% and 2.6%. However, this does not positively impact on firm profitability. To disentangle the mechanisms behind this set of results, we investigate firms' input utilization and productivity. We show that the subsidized firms experience a reduction of 4.5% in capitalization after the usage of the policy. We also estimate a decrease of 3.6% in terms of labor productivity (value added (VA) per worker) in the subsidized firms, with respect to the pre-treatment levels. Furthermore, we show a decrease in terms of workforce quality that may be related to the decrease in labor productivity. All our results are robust to alternative samples and specifications.

This paper is related to the emerging strand of the literature that evaluates the effects of ALMPs on firm-level outcomes. Lechner et al. (2013) show that the ALMPs mix provided by the German local employment offices has mild if not negative effects on firm performance, a result in line with Blasco and Pertold-Gebicka (2013) who estimate firm-level effects of monitoring and counseling in Denmark. More closely related to our paper are the evaluations of the effects of hiring credits on the performance of the recruiting firms. Lombardi et al. (2018) study the hiring credits targeted to long-term unemployed in Sweden and show that the policy design is crucial in determining positive or negative results. Saez et al. (2019) estimate with a DID approach how an automatic payroll tax cut targeted to young workers in Sweden cast an increase in firm size, productivity and profits. Following a similar approach, Rubolino (2022) estimates positive effects on firm performance of the women-targeted hiring credits introduced in Italy in 2012. Depalo and Viviano (2024) evaluate the effect of the same policy that we study on firm size, capital-to-labor ratio and VA per worker and find a reduction in the productivity and capital deepening of participating firms. They register a negative effect on productivity and the intensity of capital. Our study differs from the existing

literature in two ways. First, we apply an innovative methodology, the SDID method proposed by Arkhangelsky et al. (2021) and, secondly, we use an extended set of outcomes related to firm performance which encompasses firm size, labor cost, input capitalization, productivity, profitability and workforce quality.

The paper is organized as follows: Section 2 presents the institutional background; Section 3 describes our data and research strategy; Section 4 provides our main results; Section 5.1 reports the results from robustness checks on different samples and specifications of the treatment; Section 6 concludes.

## 2 Institutional background

Despite the Italian labor market has been historically characterized by the predominance of permanent contracts (OECD, 2019), two major waves of reform have been implemented in the last decades. The first one occurred between the late '90s and early '00s with the aim of favoring the spread of fixed-term contracts and staff-leasing work (Tassinari, 2022). As a result, by the end of the '00s, fixed-term contracts comprised approximately 60% of new hires (Darulich et al., 2023). The second wave was aimed at reducing the duality of the labor market represented by the co-existence of a stock of open-ended contracts, characterized by strong employment protection, and a growing stock of flexible contracts (Boeri and Garibaldi, 2019). Among these interventions, the most notable was the Jobs Act (Law 183/2014) which was effective from March 2015 onwards. This reform introduced a new open-ended contract based on graded security while also amending the employment protection legislation. More specifically, it reduced the firing costs for firms with more than 15 employees by eliminating the possibility of judicial reinstatement after "unfair dismissal."<sup>1</sup>

This process was not accompanied by a restructuring of ALMPs. Hiring credits remained a prevalent practice, as highlighted by Vergari (2016), but Law 190/2014 in 2015 introduced a measure that was unprecedented for Italy, i.e. an unconditional and broad-based hiring credit policy for permanent positions. Our empirical analysis focuses on this measure. Note that the Italian legislation traditionally followed a targeted approach, granting hiring incentives to promote the employment of workers in disadvantaged groups (such as young individuals, women, or the long-term unemployed) or aimed at bolstering employment opportunities in the less developed Southern regions. These initiatives align with the well-documented regional and demographic disparities in Italy (Boeri and Garibaldi, 2007; Fernández-Villaverde et al., 2023).

Law 190/2014 diverged from the traditional approach because it aimed at an economy-wide creation of permanent positions or conversion of fixed-term contracts into permanent ones. A side objective of this policy was to counter the further deregulation of the labor market introduced with the Jobs Act (Ardito et al., 2023). Access to the credits was straightforward, as firms could request them directly through the online UNIEMENS form that employers are required to complete to declare social contributions for their employees. For employees eligible for credits under Law 190/2014, employers simply needed to fill in a designated section of the UNIEMENS form using a specific alphanumeric code provided by INPS. This process is standard for all hiring credits managed by INPS. The eligibility for these incentives was primarily based on the criterion that the worker had not held a permanent contract in the six months preceding the new subsidized contract. In 2015, the employers that were involved in the policy enjoyed a three-year exemption from social contribution costs (capped at €8,060 per year).<sup>2</sup> In 2016, it was introduced a new measure referred to the contracts initiated in that year with the same criterion but its duration was reduced to two years and the subsidy was reduced to 40% of social contributions (capped at €3,250 per year). During

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<sup>1</sup>In this case, workers will receive a reimbursement from their employers depending on their tenure in the firm instead of being restored to their position.

<sup>2</sup>In Italy, payroll taxes borne by employers amount to slightly more than 23% of wages and they mainly refer to social security contributions paid to INPS.

2015 and 2016, these measures contributed to the creation of 1.5 million and 600,000 permanent contracts, respectively, constituting 57% and 35% of total permanent hirings or conversions in those years (INPS, 2017).

### 3 Data and methodology

In this section, we describe the data and the strategy that we employ in our analysis. In Subsection 3.1, we give insights on the employee-level data and in Subsection 3.2 on the firm-level data. We use anonymous tax identifiers to merge data from several sources. In Subsection 3.3 we describe the sample; lastly, in Subsection 3.4 we describe our econometric strategy.

#### 3.1 Matched employer-employee data

INPS collects social contribution taxes from both employers and employees from all private sector firms in Italy. The VisitINPS program provides the employer-employee dataset that represents our main data source. The information provided by the firm allows us to retrieve details about both the job and the individual who holds it. Data include information on annual gross earnings, number of weeks worked in a given year, occupation (blue-collar worker, white-collar worker, middle manager, executive), gender, year of birth, and first year as an employee. There is no information on the number of hours worked but INPS measures full-time equivalent (FTE) weeks and this allows us to collect comparable information on weekly wages for full-time and part-time workers.

This dataset allows us to identify those workers who have benefitted from a certain set of policies, like hiring credits. This detail is crucial to us in identifying those contracts (and firms) involved in the hiring credit scheme introduced by Law 190/2014. We will refer to firms that registered at least one subsidized contract as *subsidized firms*. We select the primary job of each individual in a given year by considering the job that is permanent if the individual has more than one job. If the worker has two jobs of the same type, we select the highest-paying job. We collapse data at the worker level and retrieve information on firms including the following variables: firm size, permanent contracts share, blue-collar workers share, white-collar workers share, and worker ability. Worker ability is estimated through a standard two-way fixed effects model *à la* Abowd et al. (1999, AKM henceforth).<sup>3</sup> In the correspondent literature, worker-fixed effects are interpreted as proxies for their individual ability (Card et al., 2016, 2018). We defined quartiles on the distribution of workers' ability and then calculated the share of high-skilled workers (those in the 4th quartile of the ability distribution) and low-skilled workers (those in the 1st quartile of the ability distribution)

#### 3.2 Firm data

The employer-employee dataset includes firm "demographics" such as firm identifier, birth date, closing date, industry, province and legal status. Furthermore, thanks to a unique tax firm identifier, we can merge the INPS data with the data extracted from the CERVED archive which collects information on firms' balance sheets, such as sales, VA, labor costs, assets and profitability measures, for the universe of Italian limited liability companies. The CERVED dataset is constructed on the basis of standardized reports that employers have to file annually. We exploit the CERVED data to retrieve information on the capital-to-labor ratio, where capital is measured as the total fixed and intangible assets (K/L henceforth), and labor productivity (log VA per worker). Then we use as a measure of profitability the EBITDA-to-revenues ratio.

Note that the CERVED dataset does not include private partnerships and sole proprietorships. To provide evidence on the performance of the firms involved in the policy we need to merge INPS

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<sup>3</sup>See Appendix A for details on the AKM specification.

and CERVED data. Thus, our analysis is focused on the CERVED subsample of the population of Italian firms.<sup>4</sup>

### 3.3 Sample restrictions and descriptive statistics

To implement our strategy, we construct a balanced panel of firms that span from 2005 to 2019. We restrict the sample to this period for two reasons: to evaluate the effects of the policy after the tax rebate ends (i.e., 2018) and to avoid interactions between the policy and the COVID-19 effects.<sup>5</sup>

Furthermore, we exclude firms in the agriculture, public sector and extraterritorial bodies. Finally, we produce our benchmark estimates on the sample of firms with at least 5 employees each year because of the extreme computational burden that the bootstrap structure of the standard errors that our empirical strategy imposes.

The variables that we selected for our main analysis are represented in Table 1 whereas Table 2 presents summary statistics for the benchmark balanced panel of firms registered in CERVED-INPS archives between 2005 and 2019.

We split the sample between firms that received the subsidy and firms that did not. As shown in Table 2, the pre-intervention means of the variables in the two groups show small differences and this may reflect the fact that the CERVED dataset represents a quite homogenous sample. However, the presence of these differences is also the first evidence of the need for some data trimming which SDID performs through its weighting scheme. Speaking of the magnitude of such divergences it seems that the distance between the two groups is not suggestive of a "high" level mismatch between the two groups. This is important for SDID estimates of weights and it may be the case because CERVED firms are quite a homogenous sample as described in section 3.2 and 3.3.

In Figures B.1 and B.2 in Appendix B, we outline the distribution of firms among regions and sectors. The CERVED sample includes firms represented by limited liability companies, that are larger than the average (ISTAT, 2021), thus firms are concentrated in the more advanced regions of Central and Northern Italy and in manufacturing, wholesale and retail trade and construction sectors.

### 3.4 Estimation strategy

Our paper aims to investigate how the economy-wide hiring credits introduced in 2015 affected the performance of the subsidized firms in subsequent years up to 2019.

The policy does not offer any exogenous variation to identify its effects on firms since its design simply provides an unconditional tax cut to employers for permanent hirings.

In this context, the SDID methodology developed by Arkhangelsky et al. (2021) is useful because similarly to the SC method Abadie and Gardeazabal (2003), it is a weighting estimator that aims to match pre-intervention trends between treated units and (synthetic) control units providing a data-driven matching procedure between the two groups, in cases where more treated units exist (as in ours). However, like DID, this method allows for the presence of unit-level shifts (i.e. unit fixed effects) and it also allows for large-panel inference.

Let us consider a balanced panel of firms  $N$  and  $T$  periods, where we will denote  $Y_{it}$  as the outcome of unit  $i$  and we have a binary exposure variable  $W_{it}$  which takes the value 1 if the unit is treated and zero otherwise.<sup>6</sup> Thus,  $W_{it}$  compares those firms that have used at least once the

<sup>4</sup>Please, note that the matching rate between INPS and CERVED data on balance sheets improves markedly for larger firms (Card et al., 2014).

<sup>5</sup>Note also that the CERVED data on balance sheets are available until 2018.

<sup>6</sup>We illustrate the estimator with a balanced panel given that we work with the SDID package in Stata (Clarke et al., 2023) which allows us to work with a balanced panel only.

**Table 1:** Firm-level variables selected for the analysis

Variables	Source	Content
Log firm size	INPS	Number of workers at a certain firm in a given year.
EBITDA-to-revenues ratio	CERVED	Earnings before interest, taxes, depreciation, and amortization in 2015 euros on yearly revenues.
Log labor costs per worker	CERVED	Gross salaries + specific aspects such as training courses and the provision of personal protective equipment - labor ratio.
Log tangible + intangible assets-to-labor ratio (K/L)	CERVED	Annual value of machines equipment and buildings + R&D + brands and other intangible assets - labor ratio.
Log total assets-to-labor ratio	CERVED	Annual value of machines equipment and buildings + R&D + brands and other intangible + financial assets - labor ratio.
Log VA per worker	CERVED	Value of production – net purchases + variations of raw material stock – service and third-party asset costs in 2015 euros.
Share of blue-collar workers	INPS	Share of blue-collar workers at a firm in a given year
Share of white-collar workers	INPS	Share of white-collar workers at a firm in a given year
Share of low-ability workers	INPS	Share of workers in the first quartile of the ability distribution as recorded with the AKM model estimated between 2005-2019.
Share of high-ability workers	INPS	Share of workers in the fourth quartile of the ability distribution as recorded with the AKM model estimated between 2005-2019.



**Table 2:** Firm-level variables: means for the pre-intervention period.

Outcomes	Subsidized firms (A)	Controls (B)
Log firm size	31.8	31.5
% open-ended contracts	0.875	0.893
Within -firm log wages	6.214	6.216
% blue-collar workers	0.544	0.545
% white-collar workers	0.390	0.394
% low-ability workers	0.065	0.070
% high-ability workers	0.442	0.446
SD within-firm wages	0.276	0.264
Log K	6.226	5.931
Log K/L	3.298	3.232
Log total assets/L	5.13	5.12
Log VA per worker	4.026	4.001
Log total assets	8.063	7.820
Log labor cost/L	3.640	3.637
EBITDA - revenues ratio	.092	.090
EBITDA per worker	24.99	24.79
N. firms	17.759	15.980

Notes: The means are calculated on the pre-intervention period 2005-2014 using the balanced panel of firms with at least 5 employees registered for the years 2005-2019.

subsidy provided by Law 190/2014 against those that have never used it.<sup>7</sup> Differently from the case of SC, we have  $N_{tr}=N - N_{co}$  treated units with  $N_{tr} \geq 1$ , where  $N_{co}$  is the number of control units. Furthermore, SDID estimates the ATT with a two-way fixed effects regression (Arkhangelsky et al., 2021):<sup>8</sup>

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg \min_{\tau, \mu, \alpha, \beta} \left\{ \sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \omega_i^{sdid} \lambda_t^{sdid} \right\} \quad (1)$$

In this equation,  $\tau$  represents the parameter that estimates the average treatment effect on the treated (ATT), i.e. the causal effect of the policy. The other parameters of the equation are  $\beta_t$ ,  $\alpha_i$  and  $\mu$ . They are time-fixed effects, unit-fixed effects, and the intercept, respectively. In equation (1), there are some features that SDID adds to standard DID and SC. Indeed, similarly to SC we use weights that are unit specific ( $\omega_i^{sdid}$ ) but the SDID estimator also allows for unit-specific fixed effects. Given that this procedure absorbs any constant difference between units, the unit weights in equation (1) will be calculated with more flexibility than with a standard SC.

Intuitively, it means that more units may be selected in the synthetic group than what would happen in a traditional SC. This happens because with SDID the synthetic control is requested to exhibit parallel trends with respect to the treated group and not a perfect alignment. This adds flexibility and robustness to the estimator which can be used even in the case that the outcome systematically differs among the two groups.

On the one hand, the introduction of unit fixed effects in equation (1) improves the precision of the estimation since they usually explain a lot of the outcome variability. On the other hand, this

<sup>7</sup>To be more specific, the control group in our specification is formed by those firms that have never used both the subsidies introduced in 2015 and 2016.

<sup>8</sup>For the sake of clarity, we underline that the discussion in this section refers to how SC and SDID may be explained with a two-way fixed effects regression but, as Arkhangelsky et al. (2021) point out, SDID may be used in contexts where the data generating process is more sophisticated and treatment effects are heterogeneous over time.



will result in a less sparse synthetic control than standard SC and that is why Arkhangelsky et al. (2021) impose a ridge penalty in the estimation of the unit weights.

Arkhangelsky et al. (2021) also introduce the time weights,  $\lambda_t$ , that are positive and sum up to one. Positive values are associated with those pre-treatment periods that have the most predictive power over post-treatment ones in terms of outcome (typically, the most recent periods before treatment). These weights are calculated by minimizing the distance between the outcome before and after the intervention for the control units. In the estimation of equation (1), the use of these weights helps to both remove bias and improve precision because they reduce the influence of pre-treatment periods that are very different from the post-treatment ones. This feature is a novelty for SC methods and it is partly due to the double-differencing structure of the SDID estimator.<sup>9</sup> Arkhangelsky et al. (2021) show the good bias properties and asymptotic normality of the estimator.

Our main inference procedure for  $\hat{\tau}^{sdid}$  is a clustered bootstrap at the firm level as suggested by Arkhangelsky et al. (2021).

We estimate the effect of the policy using equation (1). However, to allow for the heterogeneity of the treatment effect over time, we follow the dynamic specification of the equation that has been proposed by Arkhangelsky et al. (2021) and implemented in Stata by Clarke et al. (2023). The main results will be described with the event-study plots. Note that, while the reference period of the standard dynamic two-way fixed effect regression is the period before the intervention (Borusyak et al., 2024), in the case of the double-differencing structure of the SDID estimator, the reference period is selected with the  $\lambda$ s of equation (1). Thus differently from any standard dynamic DID specification, the coefficients shown in the event-study plots in the next section will represent the treatment effect dynamics calculated as the differences in the outcomes between the treatment and the (synthetic) control group in each period benchmarked against the same differences calculated in a reference period which is selected by the  $\lambda$ s of equation (1). The plotted coefficients represent deviations from this baseline and significant deviations before the treatment indicate a violation of the parallel trends whereas deviations after the intervention signal the policy effect.<sup>10</sup> This representation of the treatment effect allows us to detect its dynamics and the presence of parallel trends between the two groups too.

## 4 Main Results

In this section, we present the results of our estimation of the effect of the policy on the subsidized firms. For each outcome, we outline both the average ATT as well as its dynamics over time exploiting the event-study plots, following Clarke et al. (2023). The section is organized as follows: in Subsection 4.1 we present the results on firm size, labor cost and firm profitability; in Subsection 4.2 we report the estimates on input utilization and labor productivity; in Subsection 4.3 we present the results on workforce composition.

### 4.1 Firm size, labor cost and profitability

Given that the policy was aimed at increasing employment, we start this section by presenting the results on (log) firm size. We present these estimates in Figure 1 (top-left panel). The ATT is statistically significant and it states that the policy induced an 8.6% firm size increase in the subsidized firms compared to the control ones.

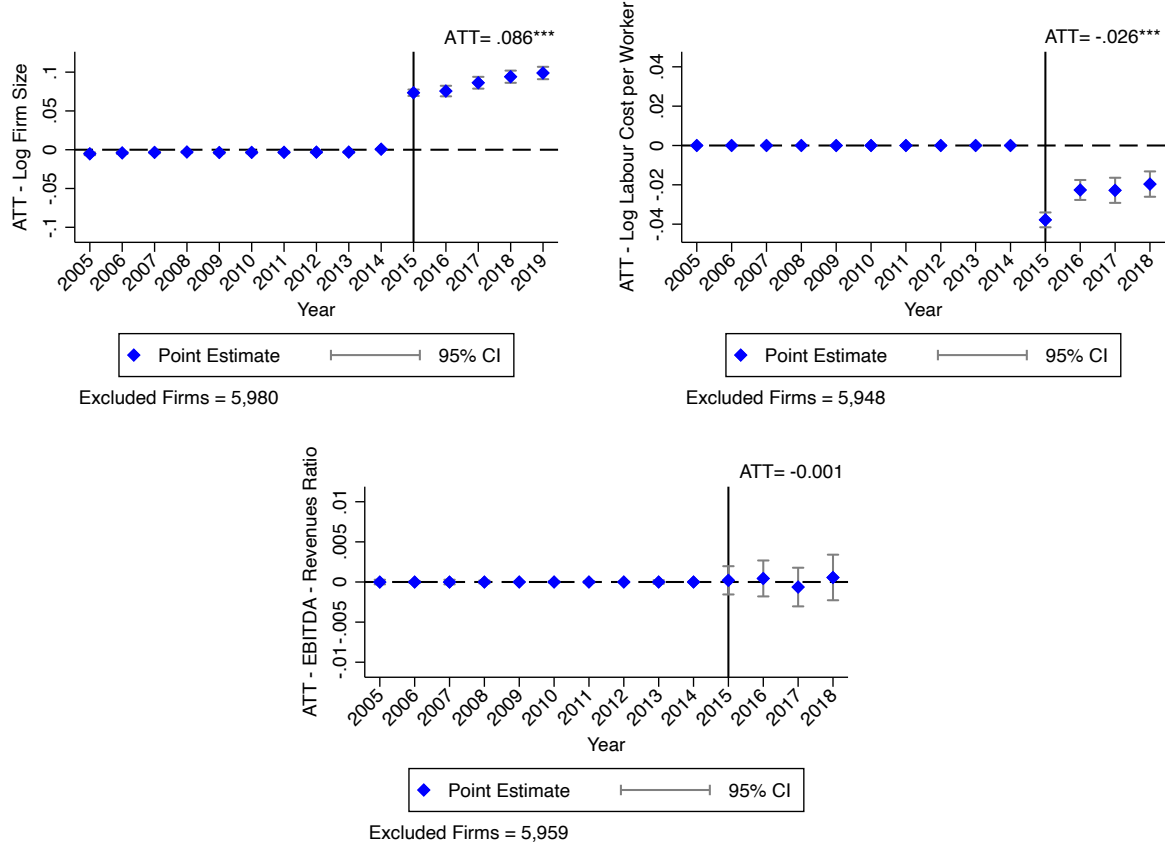
The top-right panel of Figure 1 shows that labor costs are reduced by 2.6 % in subsidized firms with respect to control ones as a result of the subsidy. The bottom panel of Figure 1 reports no significant effect of the policy on the ratio of EBITDA-to-total revenues, which is a crucial measure of firm profitability. This finding shows that, notwithstanding the reduction in labor costs, firm

<sup>9</sup>In Appendix C we provide further details on how the weights are estimated and on the SDID estimation procedure.

<sup>10</sup>For more information on the dynamic specification of SDID, see Appendix C.

profitability did not improve in the four years after the treatment. In the forthcoming subsections, we will provide some evidence of the possible reasons behind such evidence. As previously discussed, among the direct effects concerning the characteristics of the new hirings, and the indirect effects on firm-level decisions and organization, the direction of the overall effect is not predetermined. We start with the analysis of the dynamics of capital-to-labor ratio and productivity which are related to the technical substitution effects due to the labor cost reduction. Subsequently, we consider the effect on the firm's workforce in terms of both type of occupation and workforce quality.

**Figure 1:** SDID estimate of ATT - Firm size, labor cost and profitability



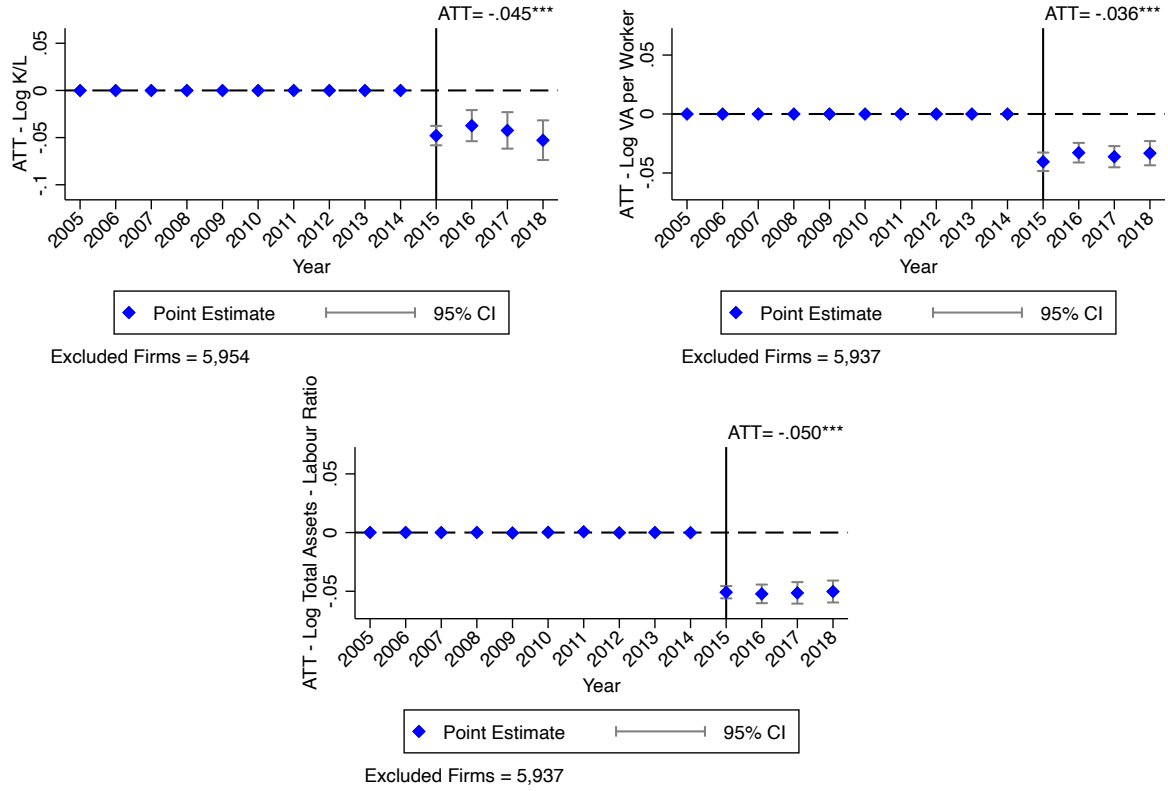
Notes: a) log firm size (top-left); b) log labor cost per worker (top-right); c) EBITDA-to-revenues ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$

## 4.2 Input utilization and productivity

In this subsection, we report the estimates related to input utilization and firm productivity.

The overall effect on log K/L amounts to -4.5% and this effect is persistent in the following years, evidencing that the subsidized firms did not match the additional hirings with new investments. This picture may be explained by several channels. First of all, it may reflect a broader tendency of firms to substitute labor for capital as this factor becomes relatively cheaper. This evidence is coherent with [Benzarti and Harju \(2021b\)](#), who analyze the effect of a variation in the employer portion of payroll tax rates on firm-level input use. The results on the capital-to-labor ratio are robust to the use of a different measure of capital, i.e. total assets, as the bottom panel of Figure 2 shows. The increase in labor intensity is consistent with the effect of the policy on labor productivity (top-right

**Figure 2:** SDID estimate of ATT - Input utilization and productivity



Notes: a) log tangible + intangible assets - labor ratio (top-left); b) log VA per worker (top-right); c) log total assets - labor ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$

panel in Figure 2 which is slightly negative. The overall effect on log VA per worker amounts to -3.6% and it is persistent over time even beyond the duration of the incentive.

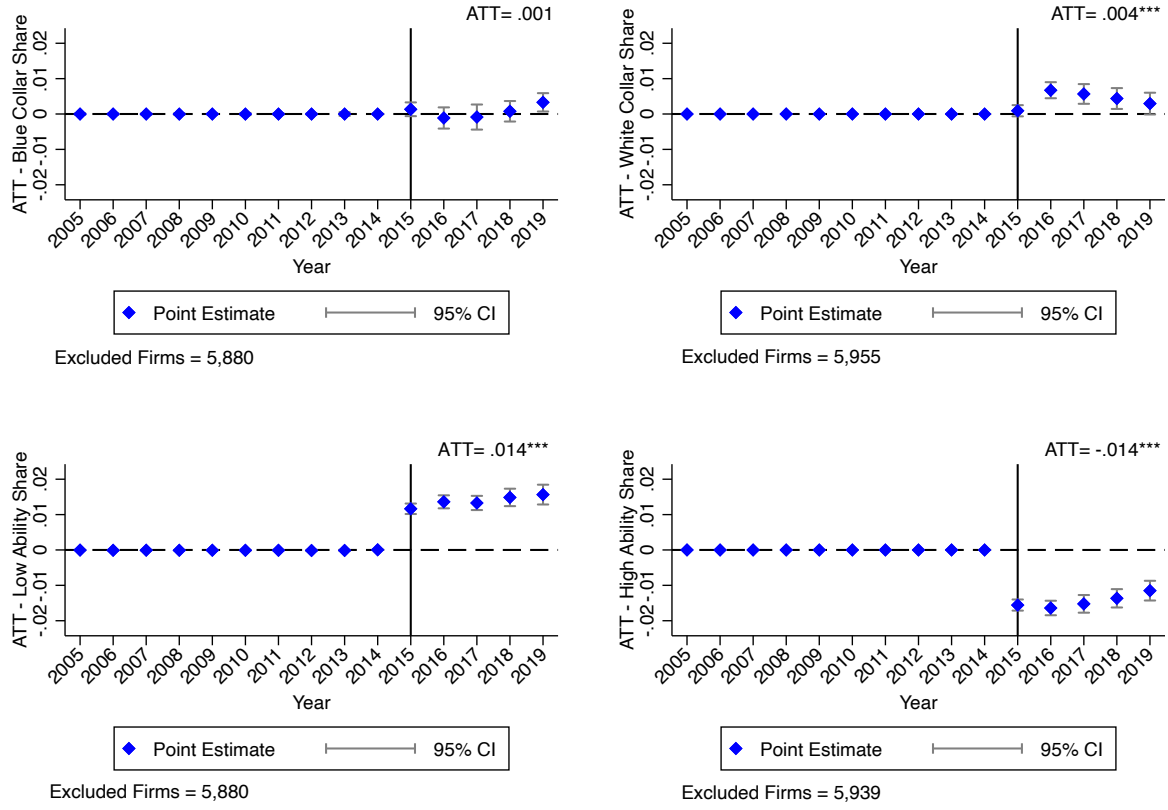
### 4.3 Workforce composition

Given the quality of our data, we can investigate how the composition of firms' labor force evolved from 2015 onwards.

The top-left panel of Figure 3 plots the effect of the intervention on the share of blue-collar workers at the firm level. The effect is small and not significant. Furthermore, the dynamic of the effect is quite stable across the period apart from 2019, when the share of blue-collar workers in the subsidized firms slightly increased by 0.01% with respect to the synthetic control firms. Figure 3, top-right panel, shows the effect of the policy on the share of white-collar workers. The effect is positive with a magnitude of 0.4 %. At the same time, the event-study tells us that the effect on this outcome has a peak in 2016 and then slowly returned to zero, indicating that employment adjustments still occur beyond the incentive duration.

In the previous section, we identified a decrease in capital intensity that was accompanied by a negative effect on the VA per worker. An effect in the same direction on firm performance could also be generated by a change in the quality of its employees. We estimate an index of workers' individual productivity by relying on the two-way-fixed effects AKM methodology (Abowd et al.,

**Figure 3:** SDID estimate of ATT - Workforce composition



Notes: SDID estimate of ATT of Law 190/2014 on: a) share of blue-collar workers (top-left); b) share of white-collar workers (top-right); c) share of low-ability workers (bottom-left); d) share of high-ability workers. Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$

1999). In this way, we observe how the quality of employment changes, thus considering another channel, besides the lack of complementary investments, that can explain the negative effect of the incentive on firm performance.

The impact on workforce ability is reported in the bottom panels of Figure 3. The ATTs clearly show that the quality of the workforce decreased in treated firms as the low-ability workers share increased (by 1.4%) while the high-ability workers share decreased (by 1.4%). Differently from the case of the (slight) effect on workforce occupation, and similarly to the measures of performance analyzed in the previous subsections, this effect persists over time.

Overall, these findings suggest that the subsidized firms did not experience an improvement in their profitability and this fact can be related to a permanent reduction in capital intensity, labor productivity and worker ability.

## 5 Additional results for different samples

In this section, we present three different robustness on three different samples. To be more specific, as mentioned in Section 2, a structural reform of permanent contracts was introduced in 2015 and it made *unfair* firings easier for firms with at least 15 employees. Thus, we split the sample by firm size to give insights into how our results may be affected by this reform.

**Table 3:** SDID ATT for Law 190/2014, different samples.

Outcomes	ATT	ATT ≥ 15 employees	ATT ≤ 13 employees	ATT (2010-2019)
Log firm Size	0.086***	0.080***	0.123***	0.080***
Log labor cost/L	-0.026***	-0.020***	-0.045***	-0.30***
EBITDA - revenues	0.001	-0.001	0.001	-.010**
Log VA per worker	-0.036***	-0.021***	-0.064***	-0.021***
Log K/L	-0.045***	-0.013	-0.088***	-0.67***
Log total assets/L	-0.051***	-0.027***	-0.100***	-0.050***
% blue-collar workers	0.001	0.001	0.004**	0.001
% white-collar workers	0.004***	0.002*	0.002	0.003*
% low-ability workers	0.014***	0.012***	0.024***	0.014***
% high-ability workers	-0.014***	-0.017***	-0.022***	-0.010***
N. firms	33,739	10,029	23,382	49,320

Notes: the table reports the estimated ATT for Law 190/2014 with SDID on the panel of CERVED firms in 2005-2019. In the first column are reported the ATTs for all firms with at least 5 employees. The second column reports estimates for firms with at least 15 employees. The third column reports the ATTs for firms with 13 or fewer employees. The fourth column reports the ATTs for all firms with at least 5 employees in a shorter panel restricted to the years 2010-2019. Bootstrap standard errors are calculated with 100 repetitions. \*\*\*p ≤ 0.01

In Subsection 5.1, we present the results for firms with at least 15 employees. A drawback of re-scaling the sample is that it reduces the number of observations as we end up with 10,029 firms, of which 63% are subsidized. In Subsection 5.2, we show the estimates related to firms with 13 or fewer employees, where we have 23,382 firms of which 33% are subsidized. This helps us to exclude firms that strategically remained under the 15 employees threshold (Boeri and Garibaldi, 2019). Finally, in Subsection 5.3 we present the estimates for firms with at least 5 employees using a shorter time span to enlarge our balanced sample.

For demonstration purposes, we report and comment on the aggregate ATT for each outcome in Table 3. This helps us to compare the results for our main sample (first column of the table) with the ones related to each robustness check. For the graphical representation of the estimates, please refer to Appendix D for the results in Subsection 5.1, Appendix E for the results in Subsection 5.2 and Appendix F for the results in Subsection 5.3.

## 5.1 Results for firms with at least 15 employees

The results of our estimation for firms with at least 15 employees are summarized in the second column of Table 3. The table shows that the ATT for log firm size (top-left panel) is 0.080 and it is statistically significant. In addition to this, Table 3 reports a negative effect of the policy on labor costs per worker of 2%. Furthermore, it depicts no significant effect on profitability. Therefore, the evidence in Table 3 reproduces the same mechanism seen in Figure 1 for the main sample: after the intervention, the firm size increased but firm profitability did not improve.

As for the main sample, we briefly summarise the estimates on balance sheet outcomes for firms with at least 15 employees. Looking at Table

3, a negative effect on log tangible and intangible assets to labor ratio is registered but is no longer significant. However, firms experienced a reduction in the total assets to labor ratio by 2,7%. Furthermore, the estimated ATT for log VA per worker is somehow reduced with respect to the main results and it amounts -2.1%. Again, this confirms the fact that the firms involved did not carry out complementary investments along with the increase in employment, and this could explain why productivity decreased. The smaller coefficients with respect to the main results may reflect the fact that larger firms are less cash-constrained and may have more options to carry out capital

investments.

Speaking of the workforce composition, no effect emerges for blue-collar workers. There is a mild increase in the share of white-collar workers at the beginning of the treatment period but it gradually fades out.

We also analyze the impact on workforce quality. Again, we witness a decrease in the workforce's average quality in the firms involved in the policy, with coefficients that replicate the ones described for the main sample. The low-ability share increased by 1.1% and the high-ability share decreased by 1.7%. As mentioned in Subsection 4.3, also the change in the workforce quality may explain the labor productivity dynamics.

These findings show that the results in Section 4 are robust to a larger firm size. However, as expected, the effects of the subsidies on larger firms are somehow attenuated as new subsidized hirings represent a lower share of the whole labor cost and of total employees at the firm level, compared to smaller subsidized firms.

## 5.2 Results for firms with 13 or fewer employees

In this subsection, we build confirming evidence of our main results by looking at firms with 13 or fewer employees. The results are summarised in the third column of Table 3.

Firstly, Table 3 reports an ATT for log firm size that is 12%, which is statistically significant. Additionally, Table 3 shows a negative effect on labor costs per worker, which corresponds to a 4.5% reduction with respect to the synthetic control group. Table 3 indicates no significant impact on profitability. Thus, the evidence we gathered for this sample mirrors the mechanism observed in Figure 1 for the main sample: firm size increased, firms saved on hiring costs but they did not improve their profitability. Regarding inputs utilization, the third column of Table 3 indicates a negative and significant effect on the tangible and intangible assets to labor ratio, which amounts to an 8.8% reduction with respect to the control group. There is also a reduction in the total assets to labor ratio, which amounts to 10% and a negative ATT for log VA per worker, which is quantified in a 6.4% reduction. Thus, similarly to larger firms, small firms did not carry out capital investments to match the increased employment, and this may explain the reduced productivity.

Speaking of workforce composition, for the blue-collar share there is no observed effect. There is a slight increase in the share of white-collar workers at the beginning of the treatment period but it is not significant. In the third column of Table 3, we also re-examine the impact on workforce ability. Again, we observe a decline in the average quality of the workforce in the involved firms, with coefficients of -2.4% for high-ability workers and 2.2% for low-ability workers. As mentioned before, these results on workforce ability can be linked to the reduction in labor productivity. As expected, the coefficients for smaller firms are bigger reflecting

a more pronounced impact deriving from the employment increase due to hiring credits and the fact that small firms may be more cash-constrained (Saez et al., 2019).

Overall, these findings corroborate the robustness of the results in Section 4 even when we consider firms of different sizes. Actually, the analysis on differently sized firms also allows us to speculate on how the JA, which reduced the firing costs for larger firms, may have influenced our results (Boeri and Garibaldi, 2019). The literature that studied the combination of these two reforms shows that the effect of the JA on employment growth is negligible while most of the employment dynamics may be attributed to the hiring subsidies (Cirillo et al., 2017; Sestito and Viviano, 2018). This evidence combined with the fact that our robustness check did not change the scale and sign of the coefficients found in Section 4, is a reasonable piece of evidence confirming that the firms' dynamics were induced by the hiring credits.

### 5.3 Additional results for a shorter panel

This section proposes confirming evidence of the results shown so far by narrowing the panel to the years 2010-2019 and selecting firms with at least 5 employees. By changing the years on which we balance our panel, we relax the main restriction imposed by SDID.

Restricting to years 2010-2019 enlarges a bit the size of the sample which is now composed of 49,320 firms. However, looking at the fourth column of Table 3, it is clear that our main results presented in Section 4 are confirmed both qualitatively and quantitatively even in this smaller panel. Again, the firms that have exploited the tax cuts show an increase in their size and experience a reduction in labor costs per worker whereas their profitability did not improve. Actually, in this sample, a significant reduction in profitability is registered. This finding is associated with a reduction in capital utilization, labor productivity and the quality of the workforce, as for the main sample.

Eventually, the robustness checks we have built have confirmed the evidence in Section 4. Indeed, even if the effects are somewhat different in magnitude for different-sized firms, our findings show an increase in employment but at the same time a reduction in capital intensity, labor productivity and workforce quality. Furthermore, the reduction in labor costs for involved firms is not associated with an increase in their profitability.

## 6 Conclusion

Hiring credits are an important component of active labor market policies, both in Italy and globally (Card et al., 2010; OECD, 2019). In this study, we applied a Synthetic Control Difference-in-Differences (SDID) methodology to an employer-employee panel dataset for the first time to analyze the impact of such credits on firm performance and workforce characteristics. SDID allows us to combine the best of synthetic control and difference-in-differences, and overcome the fact that the policy that we analyze did not offer us an exogenous variation to identify the treatment effect on firms. We leveraged high-quality administrative data provided by INPS, integrating it with balance sheet information and utilizing a double fixed effects methodology to measure workforce ability. This comprehensive approach allowed us to assess the impact of the untargeted hiring credit introduced in Italy in 2015 on a wide array of firm-level characteristics.

The policy aimed at increasing employment, and the results show an 8.5% increase in firm size. Labor costs were reduced by 2.6% in subsidized firms. However, there was no significant effect on the EBITDA-to-revenues ratio, suggesting that the reduction in labor costs did not translate into improved profitability over the four years following the intervention. The overall effect on the capital-to-labor ratio and VA per worker amounted, respectively, to a 4.5% and 3.6% decrease. Furthermore, the policy led to a 1.4% increase in the share of low-ability workers in treated firms and a corresponding decrease in the share of high-ability workers. The results are robust to different specifications of the variables and variations in the pre-treatment periods. Additionally, we conducted analyses on firms of different sizes to isolate the effects of institutional changes around the 15-employee threshold.

These findings raise several questions on the use of this type of hiring credits policy. First, the absence of profitability gains in subsidized firms can be attributed to their shift towards a high-labor intensity and low-productivity paradigm. Indeed, while hiring at a lower cost, firms did not carry out complementary investments to improve their technology. Secondly, firms focused on cutting labor costs rather than improving labor quality through their new hirings, and this approach resulted in a less productive and less skilled workforce. These facts are consistent with both the null effect on profitability and the decrease in the VA per worker.

The evidence provided underscores the distortionary effects of labor market policies such as untargeted hiring credits. While they provide short-term financial relief to firms, they may also drive



counterproductive firm behavior in the long run. Notably, the adverse effects on capital investments and workforce quality persisted even after the hiring credits ended, indicating lasting impacts on firm dynamics. Policymakers should consider these consequences when designing and implementing labor market interventions, and balance the immediate benefits of employment subsidies with the need to foster productivity and profitability growth.

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## **Declaration of Generative AI and AI-assisted technologies in the writing process**

During the preparation of this work the authors used Grammarly in order to improve readability and language. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

# Appendices

## A AKM estimation

Here we present the estimation of workers’ ability using the AKM method [Abowd et al. \(1999\)](#). To characterize our specification in this first step, we rely on well-defined evidence of the fact that firms pay similar workers differently ([Card et al., 2013, 2016, 2018](#); [Macis and Schivardi, 2016](#); [Song et al., 2019](#); [Casarico and Lattanzio, 2019](#)).

Using yearly data from 2005-2019, we estimate firm wage premiums from the equation:

$$w_{ijt} = \theta_i + \psi_{j(it)} + X_{it}\beta + \epsilon_{ijt} \quad (2)$$

The dependent variable represents the log real weekly wage for individual  $i$  at firm  $j$  at time  $t$ ;<sup>11</sup>  $\theta_i$  being worker fixed effects that proxy worker ability;  $\psi_{j(it)}$  represents the firm fixed effect or firm wage premium being paid by firm  $j$  with respect to a randomly chosen firm in the sample.

$X_{it}$  contains a cubic polynomial in age (normalized at 40), a set of dummies for occupations interacted with a cubic polynomial in experience (current year minus year of the first job as an employee) and a full set of time dummies. We exclude the linear term in age and in experience to avoid collinearity with time and  $\epsilon_{ijt}$  represents an error term.

To estimate equation (2), we use a panel of workers that spans from 2005 to 2019. Furthermore, since workers’ mobility is crucial to identifying firm fixed effects, we select the firms-workers largest connected set ([Abowd et al., 2002](#)). Restricting the analysis to this set means that we focus on 99% of observations in our panel.

Once estimated equation (2), we end up with the so-called firm and individual fixed effects. Firm fixed effects are interpreted by the literature as firm wage premiums that are representative of the wage policies([Card et al., 2013](#)) practiced by firms. Such premiums may be flexibly explained by firm market power, efficiency wage, or strategic wage posting behavior ([Mortensen, 1998](#); [Cahuc et al., 2014](#)) or by time-invariant factors which may reflect the surplus produced by the firm and rent-sharing wage-setting policies ([Card et al., 2016](#)) or compensating wage differentials for less desirable working conditions ([Sorkin, 2018](#); [Bana et al., 2023](#)).

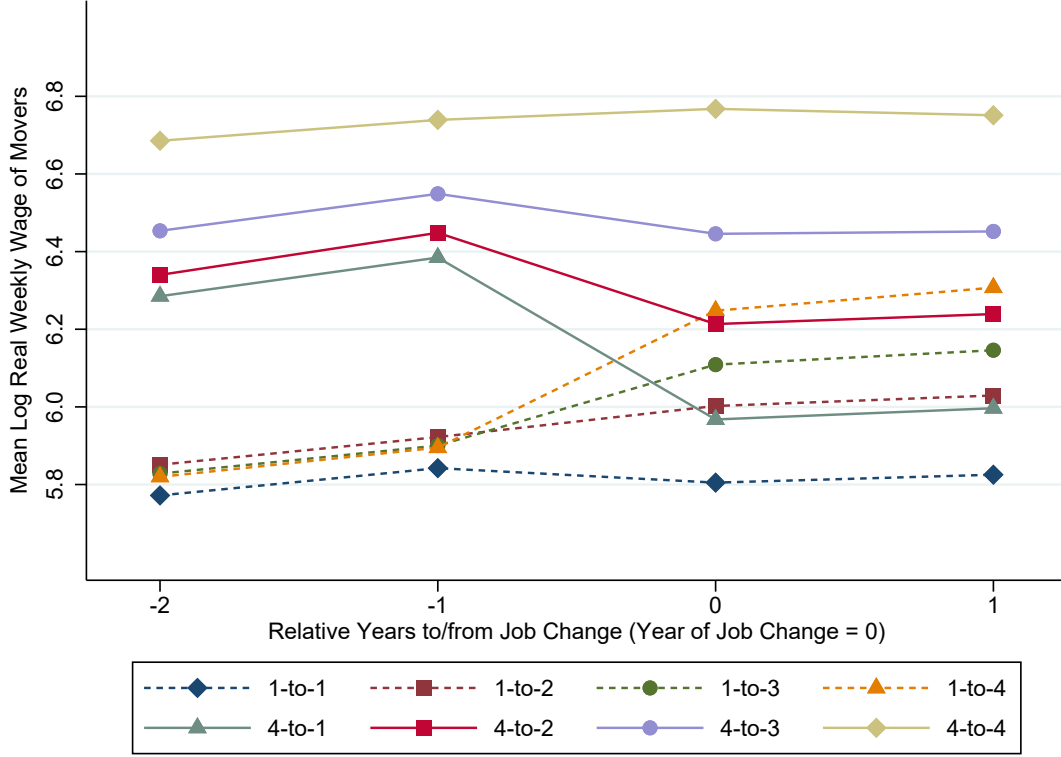
In order to have unbiased estimates, the main assumption behind AKM models is the so-called exogenous mobility. To be more specific, workers may move between firms following some pattern but what is important is that mobility is not related to components of the error term of equation 2. For example, if an idiosyncratic ”match effect” has driven worker mobility, and a worker-specific surplus occurred from the match with a certain firm, we would be mistakingly attributing this effect to a firm-specific wage premium common to all workers employed at that firm.

To test this assumption, we will follow a routine developed in [Card et al. \(2013, 2016\)](#). First, we calculate the mean wage of coworkers for individuals who change jobs in a certain year. Then, we define the average wage of movers up to two years prior to and after a move and we rank these average wages based on the quartile of origin and destination of one’s average coworker wage. Thus, we end up with 16 cells formed as a combination of each quartile of the mean coworker wage in the old and new firms. For clarity, we report on Figure [A.1](#) the mean wage of those who start from the first or the last quartile of the distribution of the coworkers’ wage.

Looking at Figure [A.1](#), it is reasonable to state that the exogenous mobility assumption may be accepted. In fact, if there were match effects like the ones defined, for instance, by dynamic matching models ([Eeckhout and Kircher, 2011](#)), the difference in wage before and after a firm switch (here proxied by the coworker wages) would include this effect beside the firm wage premium. If this were the case, the estimates would be biased and the additive specification strongly disputable. However, looking at the symmetry of wage trajectories before and after a move, it seems that there is no

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<sup>11</sup>We have winsorized values of log wages that are above(below) 99th(1st) percentile of log wages distribution over the period.



**Figure A.1:** Mean weekly wage of movers across quartiles of average coworker weekly wage. Data relate to the period 2005-2018.

general premium depending on the firm switch. Furthermore, we do not see a sudden drop in wages before the switch and a rise afterward: this should mean that we do not have unobservable negative shocks at the firm level which could lead workers to move to other firms. The same reasoning could apply to shocks in individuals' productivity which could be correlated with mobility and wages.

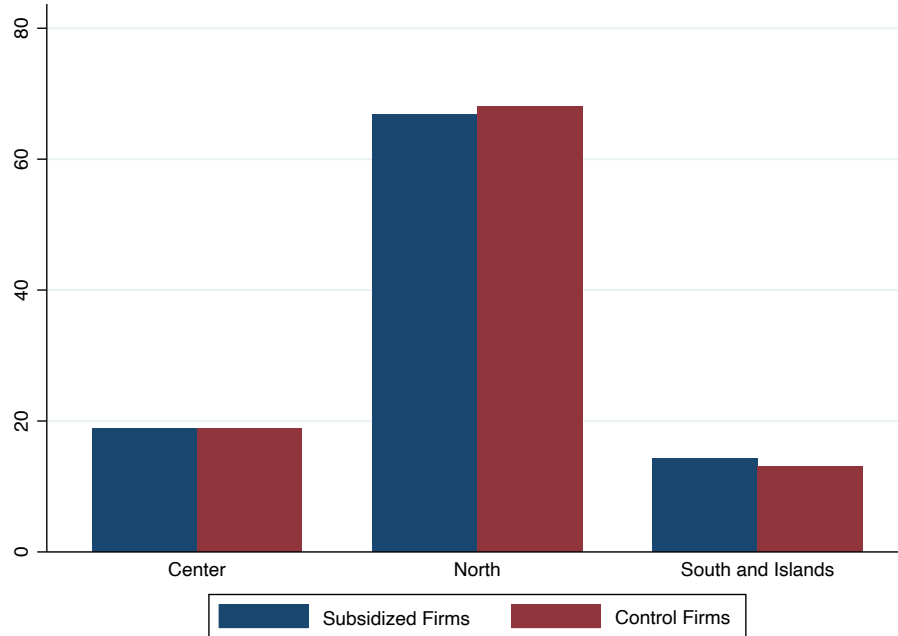
Given that our data pass the test on exogenous mobility, we use  $\theta_i$ s to define the ability of the individual and calculate the quality of the workforce of the firms in our sample.

**Table A.1:** Summary statistics for AKM model estimates

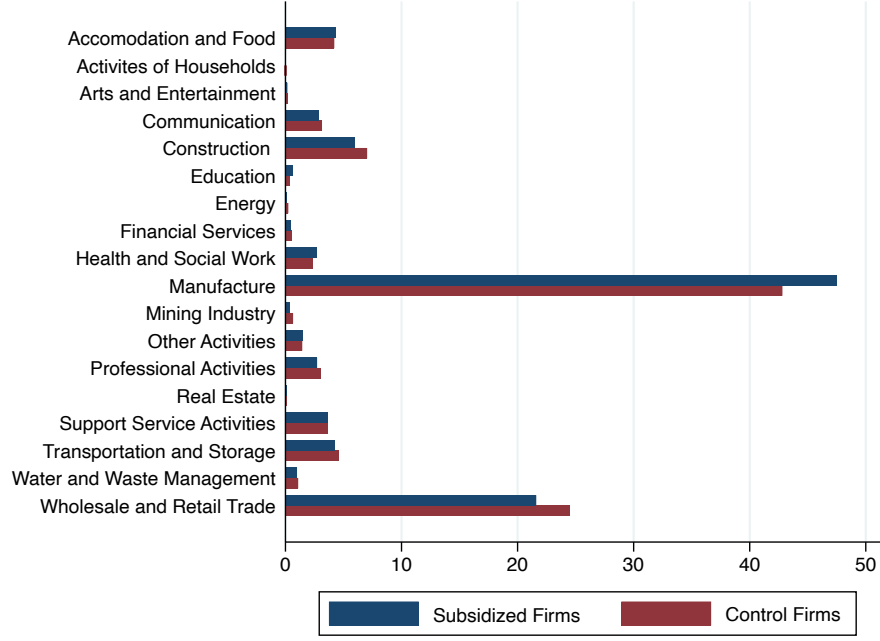
	All sample	Largest Connected set
<b>Sample size</b>		
Workers	22,499.319	21,953.547
Firms	3,630.666	3,209.560
<b>Summary Statistics</b>		
Observations	178,275.185	174,976.095
Mean log wages	6.137	6.140
Standard deviation of log wage	.439	.440
<b>Summary of estimates</b>		
Standard deviation of firm effect		.217
Standard deviation of worker effect		.313
Correlation of worker/firm effects		.193
RMSE of AKM residuals		.169
Adjusted R <sup>2</sup>		.828

Notes: Table reports summary statistics for model (2).

## B Additional tables and figures

**Figure B.1:** Regional distribution of firms.





**Figure B.2:** Sectoral distribution of firms.

## C Addendum on SDID

In this section, we give more insights into the assumptions and the mechanisms behind SDID. To explain the formal aspects of the method, [Arkhangelsky et al. \(2021\)](#) rely on a latent factor model:

$$\mathbf{Y} = \mathbf{L} + \tau\mathbf{W} + \mathbf{E} \quad (3)$$

In this equation, the latent factors  $\mathbf{L} = \Gamma\Upsilon^T$  are such that equation (3) can be understood as an "interactive fixed effects model". We can write it as:

$$Y_{it} = \gamma_i v_t^T + \tau W_{it} + \epsilon_{it} \quad (4)$$

Therefore, in this latent factor model, we are interacting both time and unit fixed effects (or factor loadings, in latent factor model notation). The first assumption that SDID takes into account (as SC does) is that the conditional expectation of the idiosyncratic component of equation (3), given the binary treatment assignment matrix  $\mathbf{W}$  and the (unobserved) systematic component  $\mathbf{L}$ , is zero. Therefore, there is a "selection on unobservables" assumption behind SDID ([Ferman and Pinto, 2021](#)). In other words, according to equation (3), the treatment assignment does not depend on the idiosyncratic component once we condition on  $\mathbf{L}$ . Overall, such latent factor models are generalizations of standard two-way fixed effects models because if we gave an additive form to  $\mathbf{L}$  such as  $L_{it} = \alpha_i + \beta_t$ , we would come back to contexts where  $\tau$  could be estimated with a DID regression. Formal discussions in [Arkhangelsky et al. \(2021\)](#) and in this section are carried out generalizing  $\mathbf{L}$  as having an additive component and a multiplicative component, in matrix terms  $\mathbf{L} = \Gamma\Upsilon^T$ . Furthermore, [Arkhangelsky et al. \(2021\)](#) allows for correlation across time within units of the idiosyncratic component.

Having in mind this model, our aim is to estimate  $\tau$  which is the treatment effect for the treated units. A confounding bias has to be addressed as  $\mathbf{L}$  is important for the treatment assignment but is at the same time unobservable, making equation (3) subject to endogeneity. Synthetic control methods, such as SDID, address this issue without estimating  $\mathbf{L}$  also because it could mean that estimation of equation (3) should rely on very strong assumptions ([Abadie, 2021](#); [Arkhangelsky](#)

et al., 2021). Indeed, SC recovers weights that replicate the factor structure of the treated units such that any divergence between the treated unit and its estimated counterfactual is on average the treatment effect (Ferman and Pinto, 2021). However, SC has no double-differencing structure (differently from DID and SDID) and the procedure is based on the assumption (and empirical assessment) of perfect alignment between treatment and synthetic control series. This is the reason why time weights are not as important as in SDID. Intuitively, once (almost) perfect alignment between treated unit and synthetic control outcomes is achieved, we are in a context where SC weights  $w^{sc}$  are able to reproduce the factor structure of the treated units such that the synthetic control is the counterfactual of interest and the treatment effect can be estimated by subtracting the treated unit outcome from the synthetic control outcome in the post-intervention period. This is why SC is more interested in balancing out the factor loading  $\Gamma$  in equation (3).

As mentioned in Subsection 3.4, SDID adds to SC unit fixed effects in the estimation of  $\tau$ . This means that the synthetic series that SDID provides will not be necessarily aligned with the treatment one. Therefore, when estimating  $\tau$  this difference has to be considered and this is the case when the double-differencing structure of SDID needs to be taken into account. In this context, SDID time weights become relevant since they give more importance to the pre-intervention periods that are more related (or say predictive) to post-treatment ones. In this way, SDID time weights  $\lambda$  balance out time common factors  $\Upsilon$  when SDID estimates the ATT in a double-differencing fashion. The combination of unit weights and time weights provides a specific double-robustness property to SDID that is, if one of the weighting schemes is effective, the confounding bias is removed. The structure of the weights and the use of latent factor models make the SDID estimator robust to additive shocks to the factor structure of equation (3) (Arkhangelsky et al., 2021).

Further assumptions that are needed for SDID to remove the bias referred to  $\mathbf{L}$  and consistently estimate  $\tau$  is that the structure of  $\mathbf{E}$  does not embed too much correlation within the units and that the factor loading structure  $\Gamma$  is stable over the period of analysis, which means that a balanced panel is preferred when applying SDID.

Here we define the SDID weights as in Arkhangelsky et al. (2021):

$$(\hat{w}_0, \hat{w}^{sdid}) = \arg \min \sum_{t=1}^{T_0} \left( w_0 + \sum_{i=1}^{N_{co}} w_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^N Y_{it} \right)^2 + \zeta^2 T_{pre} \|w\|_2^2 \quad (5)$$

$$\Omega = \left\{ w \in \mathbb{R}_+^N : \sum_{i=1}^{N_{co}} w_i = 1, w_i = N_{tr}^{-1}, \forall i : i = N_{co} + 1, \dots, N \right\}$$

The regularization parameter  $\zeta$  is set as:

$$\zeta = (N_{tr} T_{post}^{1/4}) \hat{\sigma} \quad \text{where}$$

$$\hat{\sigma}^2 = \frac{1}{N_{co}(T_{pre} - 1)} \sum_{i=1}^{N_{co}} \sum_{t=1}^{T_{pre}-1} (\Delta_{it} - \bar{\Delta})^2 \quad (6)$$

$$\bar{\Delta} = \frac{1}{N_{co}(T_{pre} - 1)} \sum_{i=1}^{N_{co}} \sum_{t=1}^{T_{pre}-1} \Delta_{it}$$

As one can see from the first row of equation (5), an intercept is inserted in the optimization problem which delivers the unit weights.<sup>12</sup> By this,  $w_i$ s make outcome patterns for the control units parallel to the treated ones in the pre-treatment period. As already mentioned, this adds flexibility to SDID with respect to SC, where there is no intercept. It is worth noting that equation (5) is looking for a synthetic control for the average value of the outcome of treated units in the pre-treatment period (as if there was an aggregate of the treated units, i.e. their average). The

<sup>12</sup>This is parallel to the insertion of unit-fixed effects in the estimating equation of  $\tau$  in equation 1.

regularization parameter  $\zeta$  ensures a unique solution. With this formalization of the weights, we end up with  $w_i$ s that are less sparse than weights in standard SC with no intercept.

Time weights are calculated as:

$$(\hat{\lambda}_0, \hat{\lambda}^{sdid}) = \arg \min \sum_{i=1}^{N_{co}} \left( \lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t Y_{it} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^T Y_{it} \right)^2 + \zeta^2 N_{co} \|\lambda\|^2$$

$$\Lambda = \left\{ \lambda \in \mathbb{R}_+^T : \sum_{t=1}^{T_{pre}} \lambda_t = 1, \lambda_t = T_{post}^{-1}, \forall t \in \{T_{pre} + 1, \dots, T\} \right\}$$
(7)

The regularization parameter is now:

$$\zeta = 10^{-6} \hat{\sigma} \quad \text{where}$$

$$\hat{\sigma}^2 = \frac{1}{N_{co}(T_{pre} - 1)} \sum_{i=1}^{N_{co}} \sum_{t=1}^{T_{pre}-1} (\Delta_{it} - \bar{\Delta})^2$$

$$\bar{\Delta} = \frac{1}{N_{co}(T_{pre} - 1)} \sum_{i=1}^{N_{co}} \sum_{t=1}^{T_{pre}-1} \Delta_{it}$$
(8)

As already mentioned, time weights help in balancing out common factors from equation (3) when running double-differencing estimation as in SDID. Time weights are calculated minimizing the distance between pre-treatment outcomes and post-treatment outcomes for the control units. By this, positive weights will be assigned to the pre-intervention periods that are more predictive of the post-treatment. Intuitively, when we estimate both unit and time weights, equation (1) ends up being a two-way fixed effects regression which is actually local in that it compares periods and units that are more similar to each other.

To better grasp the functioning of these weights and how SDID in practice calculates the ATT from equation (1), we compare calculations for the ATT from DID, SC and SDID. We start by reporting as Arkhangelsky et al. (2021) define ATT in their notation:

$$\hat{\tau} = \hat{\delta}_{tr} - \sum_{i=1}^{N_{co}} \hat{w}_i \hat{\delta}_i \quad \text{where} \quad \hat{\delta}_{tr} = \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^N \hat{\delta}_i$$
(9)

According to what we stated so far in this section and in section (3.4), the three methods will deliver different estimations of  $\tau$  because the way they calculate  $\hat{\delta}_i$  is different:

$$\hat{\delta}_i^{sdid} = \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^T Y_{it} - \sum_{t=1}^{T_{pre}} \lambda^{sdid} Y_{it}$$

$$\hat{\delta}_i^{did} = \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^T Y_{it} - \sum_{t=1}^{T_{pre}} Y_{it}$$

$$\hat{\delta}_i^{sc} = \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^T Y_{it}$$
(10)

In a nutshell, according to equations (9) and equations (10), DID runs a double-differencing estimation of ATT but gives equal weights to both time periods and units and relies on (raw) parallel trends assumption. SC weights control units so that control and treated units trends are almost perfectly aligned and then any difference (on average) will be assigned to the treatment effect. SDID applies a weighting scheme that can make the trends parallel but at the same time when estimating the ATT applies time weights  $\lambda$  that make the estimation more robust and able

to balance out both common factor and unit-specific factor loadings (see equation (3)) in a doubly-robust fashion (Arkhangelsky et al., 2021). Furthermore, as Clarke et al. (2023) report, one can use the estimates produced by the SDID estimator composed of the outcomes of equations (1), (5) and (7) to replicate an event-study plot as in a standard dynamic difference-in-differences model (Callaway and Sant’Anna, 2021). In the case of a typical event-study plot, each coefficient in each period (in our case, the yearly coefficient from 2005 to 2019) may be represented as follows:

$$\left(\bar{Y}_t^{Tr} - \bar{Y}_t^{Co}\right) - \left(\bar{Y}_{baseline}^{Tr} - \bar{Y}_{baseline}^{Co}\right) \quad (11)$$

Broadly speaking, event-study coefficients represent difference-in-differences between treated and control units with respect to a baseline period which is typically the period before the treatment starts (2014, in our analysis). In the case of SDID, the baseline period averages are not referred solely to the period before the treatment but they are averages of the pre-treatment outcomes for both treatment and control groups, weighted by time weights estimated in (7).

$$\bar{Y}_{baseline}^{Tr} = \sum_{t=1}^{T_{pre}} \lambda Y_t^{Tr} \text{ and } \bar{Y}_{baseline}^{Co} = \sum_{t=1}^{T_{pre}} \lambda Y_t^{Co} \quad (12)$$

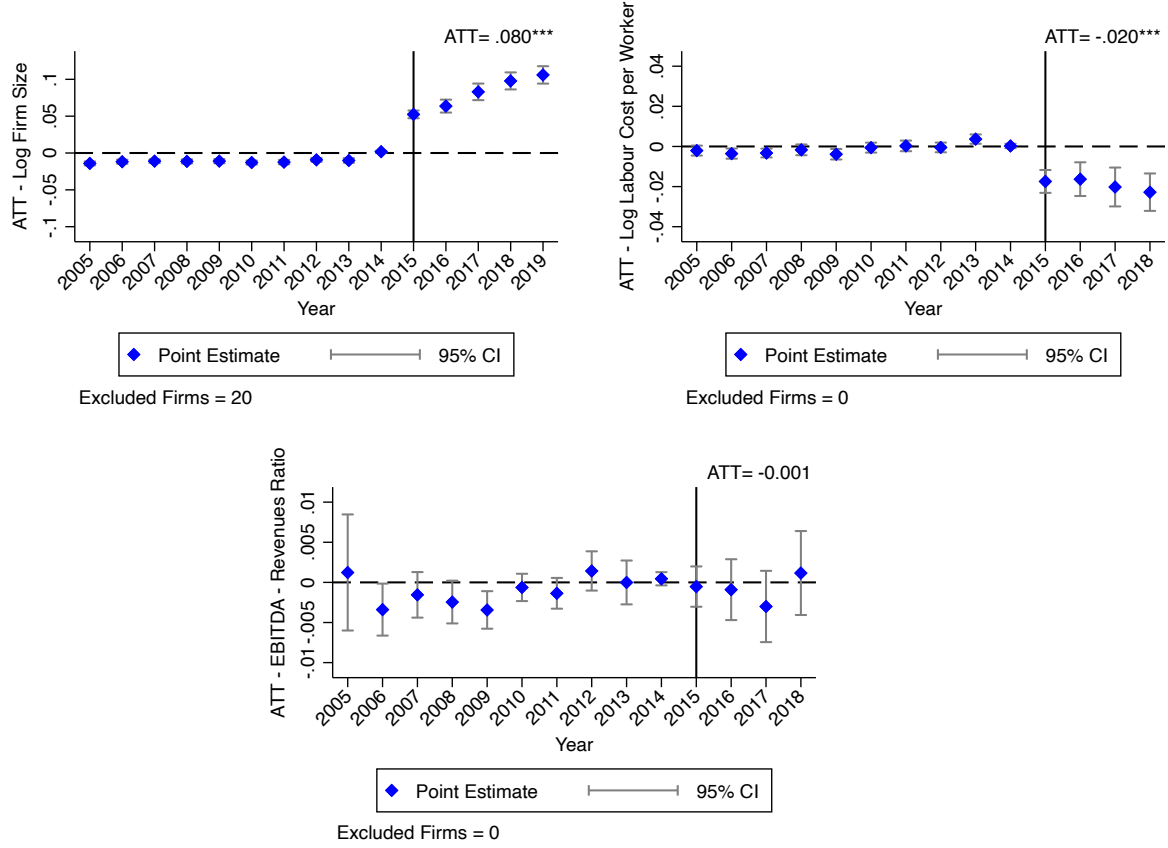
In Section 4, we will combine (11) and (12) to study the dynamics of the effect of the hiring credits introduced in 2015 based on the weighting structure estimated by SDID and using our panel of firms.

To replicate the factor structure, it is common in the recent literature to use the pre-treatment outcomes to build the synthetic controls both for SC and SDID (Doudchenko and Imbens, 2016; Kellogg et al., 2021; Ferman and Pinto, 2021) because of the higher predictive power of these variables with respect to other potential covariates. However, Abadie (2021) warns that the usage of the pre-treatment outcomes and not other covariates (in combination with pre-treatment outcomes) may induce more estimation errors as the dimension of the matrix  $\mathbf{L}$  increases and more unobservable factors have to be proxied through the pre-treatment outcomes. In our case, it is not straightforward to say what is a covariate and not an outcome. As we are focusing on the usage of the hiring credits that mechanically change the value of the inputs inside the production function of firms, potentially any characteristic of the firms could be involved, e.g., production and sales as well as profits. Labor productivity and the composition of the workforce could be affected as well by the hiring process as the capital-to-labor ratio could change too. This reasoning led us not to consider covariates when estimating SDID.

## D SDID results for firms with at least 15 employees

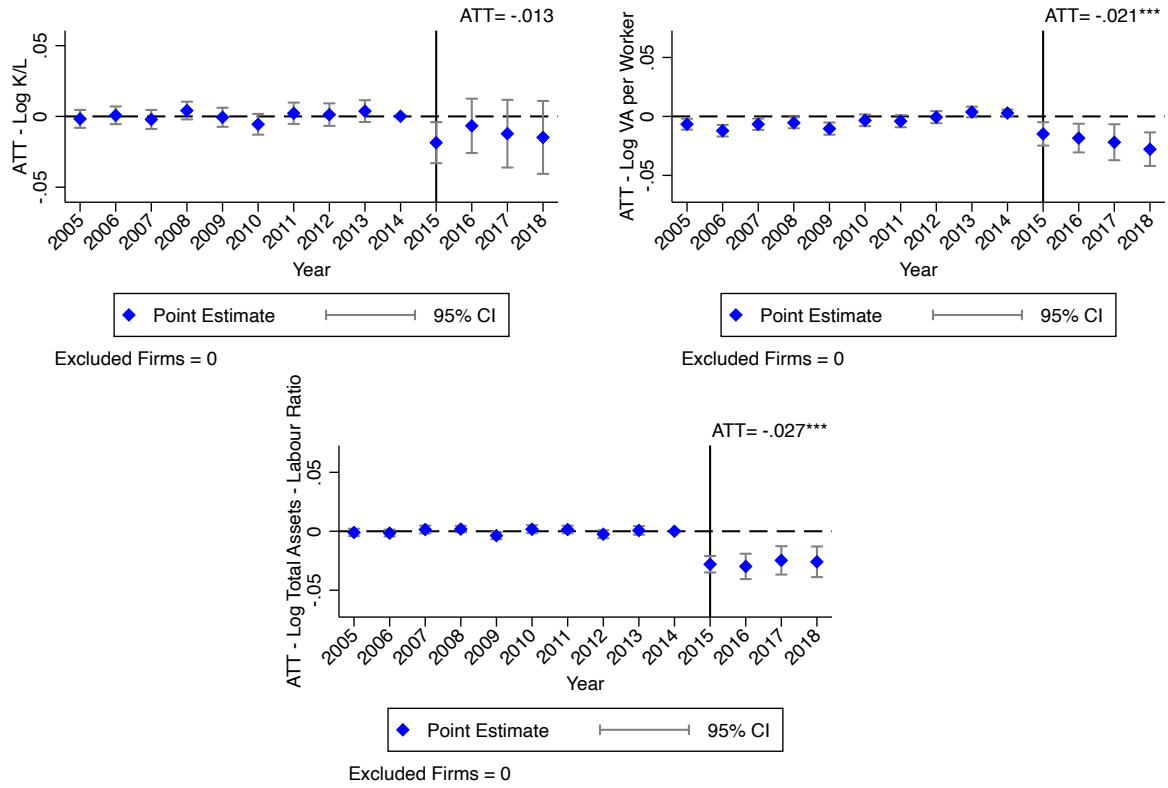
In this subsection, we present the results summarized in the second column of Table 3. The estimates depicted in this section are the results of SDID estimation on a balanced panel of firms with at least 15 employees recorded in the CERVED archives.

**Figure D.1:** SDID estimate of ATT - Firm size, labor cost and profitability



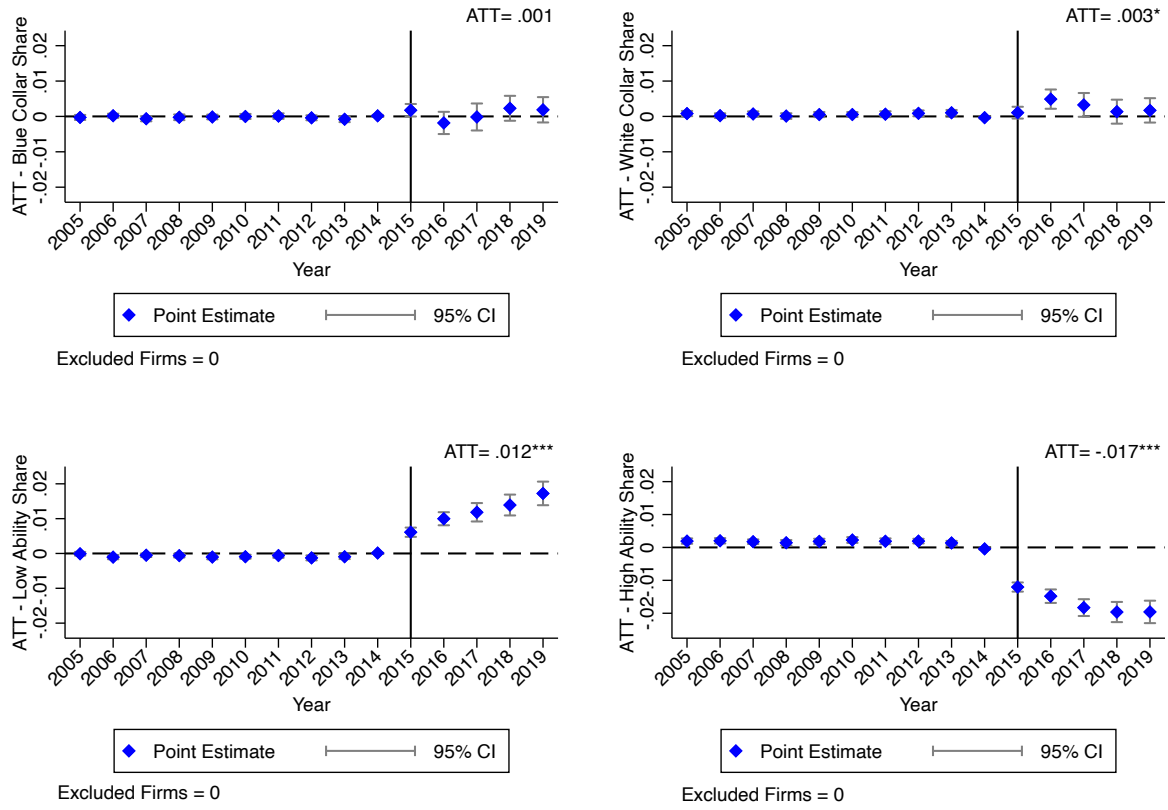
Notes: a) log firm size (top-left); b) log labor cost per worker (top-right); c) EBITDA-to-revenues ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 15 employees are considered. \*\*\*  $p \leq 0.01$

**Figure D.2:** SDID estimate of ATT - Input utilization and productivity



Notes: a) log tangible + intangible assets - labor ratio (top-left); b) log VA per worker (top-right); c) log total assets - labor ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 15 employees are considered. \*\*\*  $p \leq 0.01$

**Figure D.3:** SDID estimate of ATT - Workforce composition



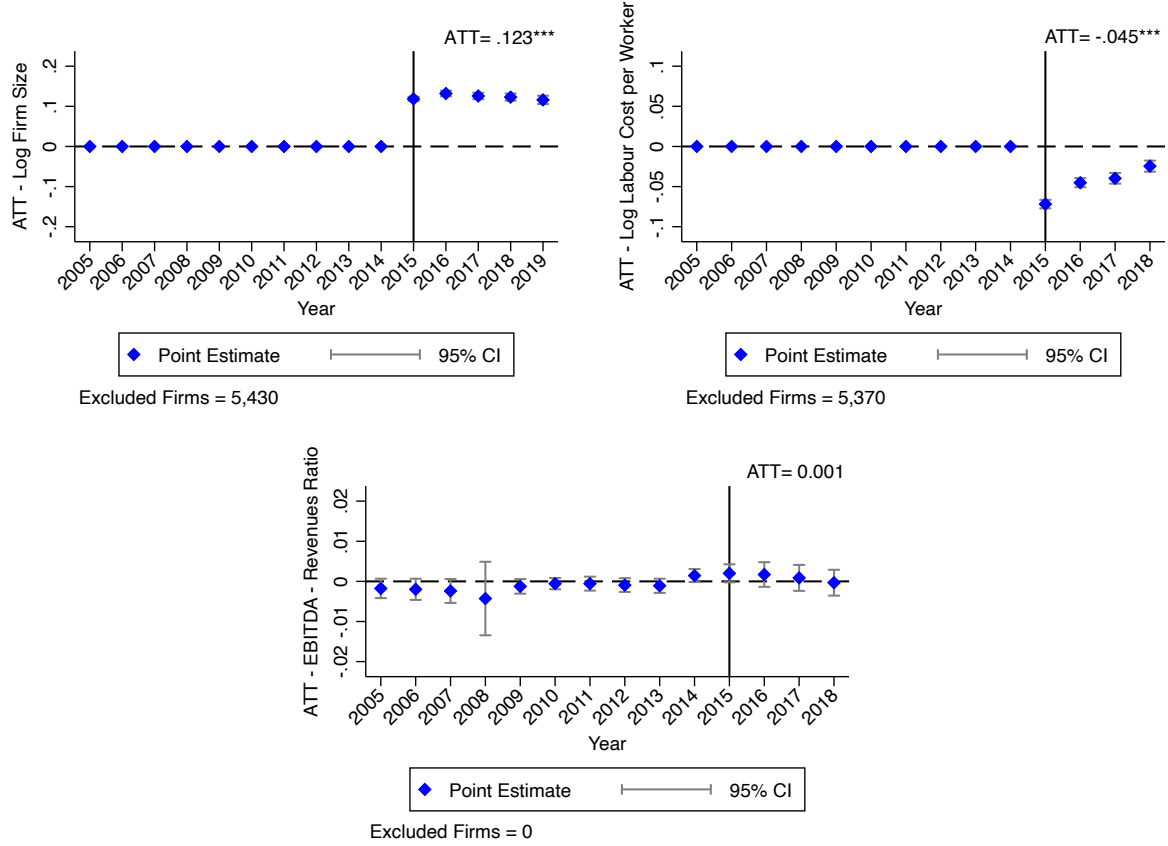
Notes: a) share of blue-collar workers (top-left); b) share of white-collar workers (top-right); c) share of low-ability workers (bottom-left); d) share of high-ability workers. Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 15 employees are considered. \*\*\*  $p \leq 0.01$



## E SDID results for firms with 13 or fewer employees

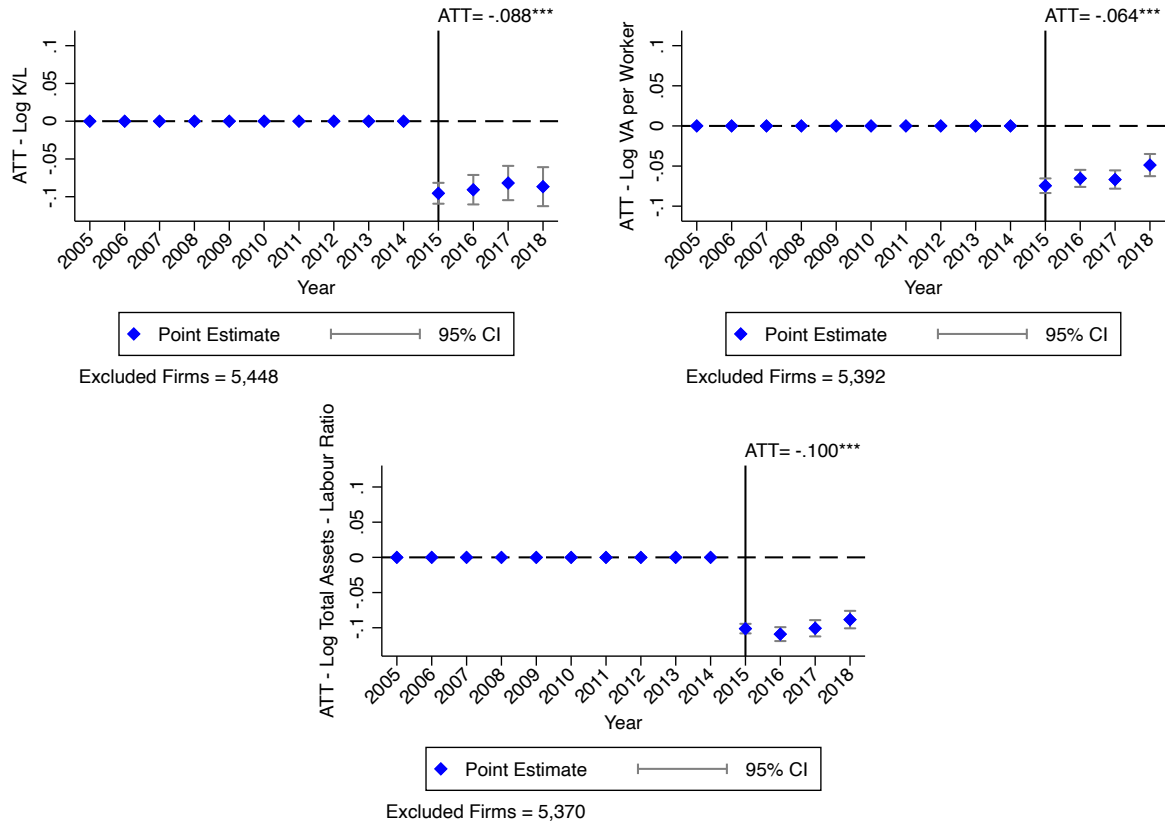
In this subsection, we present the results summarised in the third column of Table 3. The estimates reported in this section are the results of SDID estimation on a balanced panel of firms with 13 or fewer employees recorded in the CERVED archives.

**Figure E.1:** SDID estimate of ATT - Firm size, labor cost and profitability



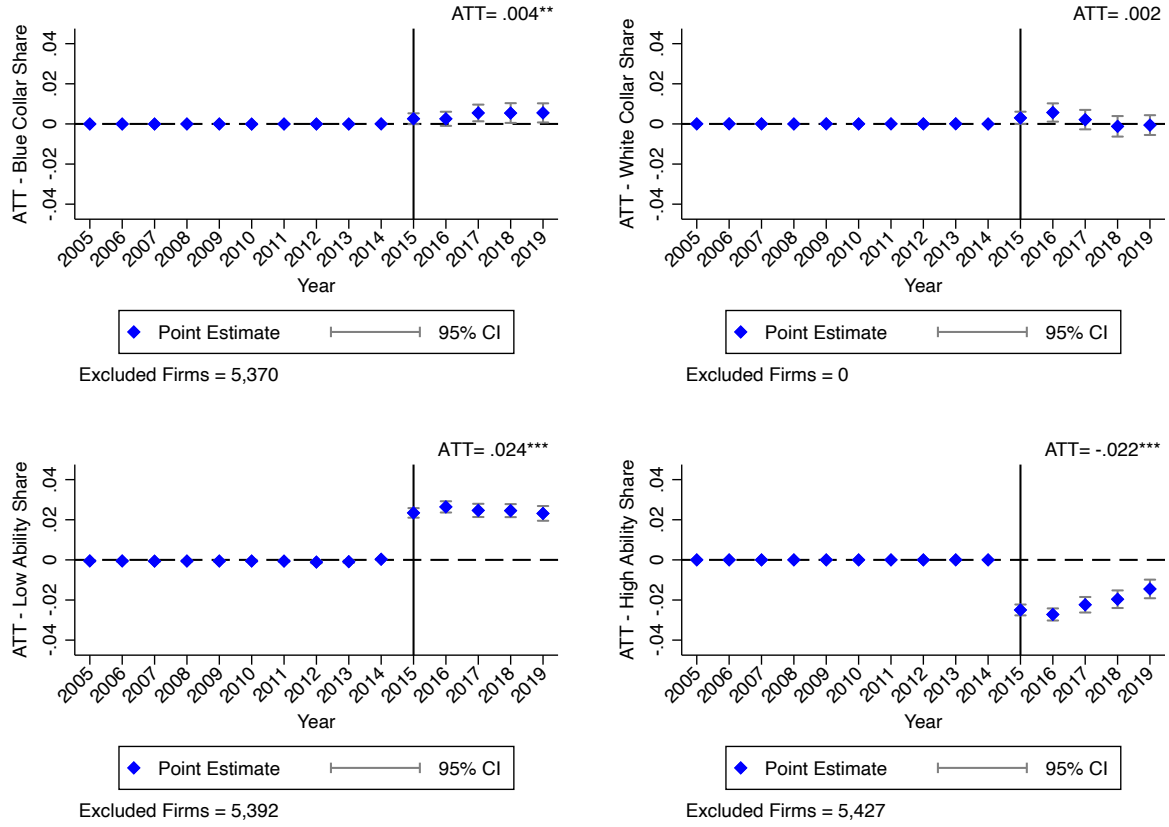
Notes: a) log firm size (top-left); b) log labor cost per worker (top-right); c) EBITDA-to-revenues ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with 13 or fewer employees are considered. \*\*\*  $p \leq 0.01$

**Figure E.2:** SDID estimate of ATT - Input utilization and productivity



Notes: a) log tangible + intangible assets - labor ratio (top-left); b) log VA per worker (top-right); c) log total assets - labor ratio (bottom). Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with 13 or fewer employees are considered. \*\*\*  $p \leq 0.01$

**Figure E.3:** SDID estimate of ATT - Workforce composition

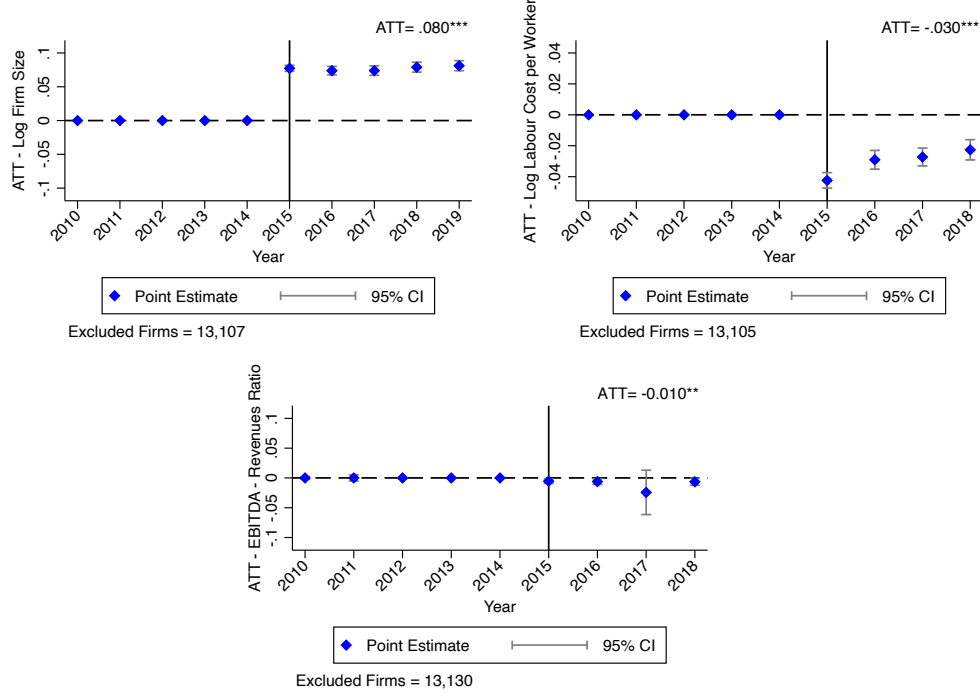


Notes: a) share of blue-collar workers (top-left); b) share of white-collar workers (top-right); c) share of low-ability workers (bottom-left); d) share of high-ability workers. Overall ATT in the top-right of each panel. Bootstrap standard errors are calculated with 100 repetitions. Firms with 13 or fewer employees are considered. \*\*\*  $p \leq 0.01$

## F SDID results for a shorter time span

In this subsection, we present another robustness for the estimates proposed in Section 4. We focus again on a balanced panel of firms with at least 5 employees while restricting to years 2010-2019. The results are confirmed both qualitatively and quantitatively even if the sample (both in terms of donor pool and treatment group) slightly changes due to the different selection.<sup>13</sup>

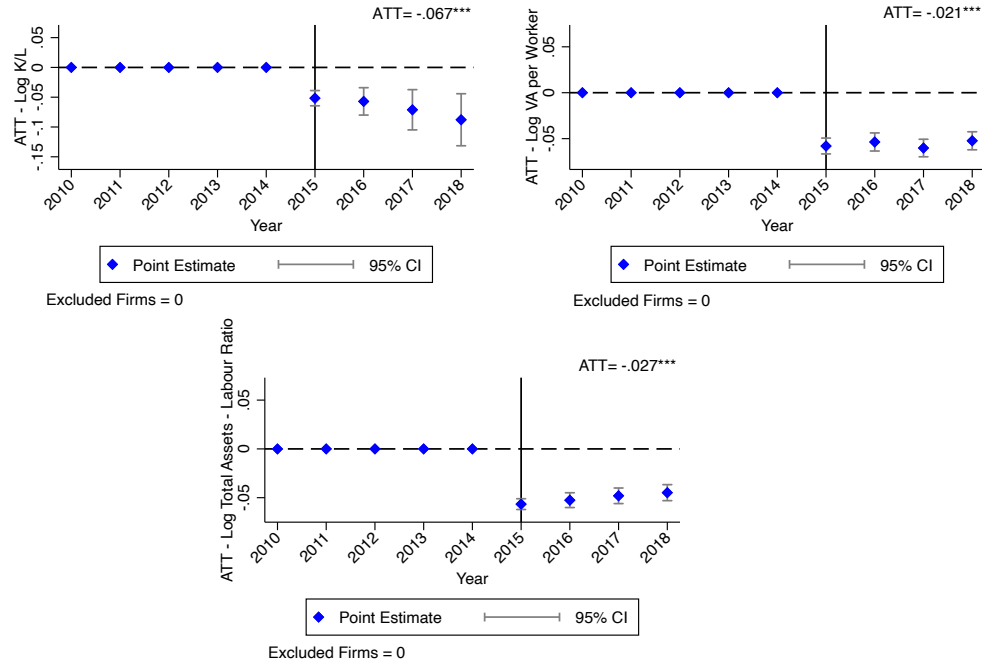
**Figure F.1:** SDID estimate of ATT - Firm size, labor cost and profitability



Notes: a) log firm size (top-left); b) log labor cost per worker (top-right); c) EBITDA-to-revenues ratio (bottom). Overall ATT in the top-right of each panel. Years 2010-2019 are considered. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$

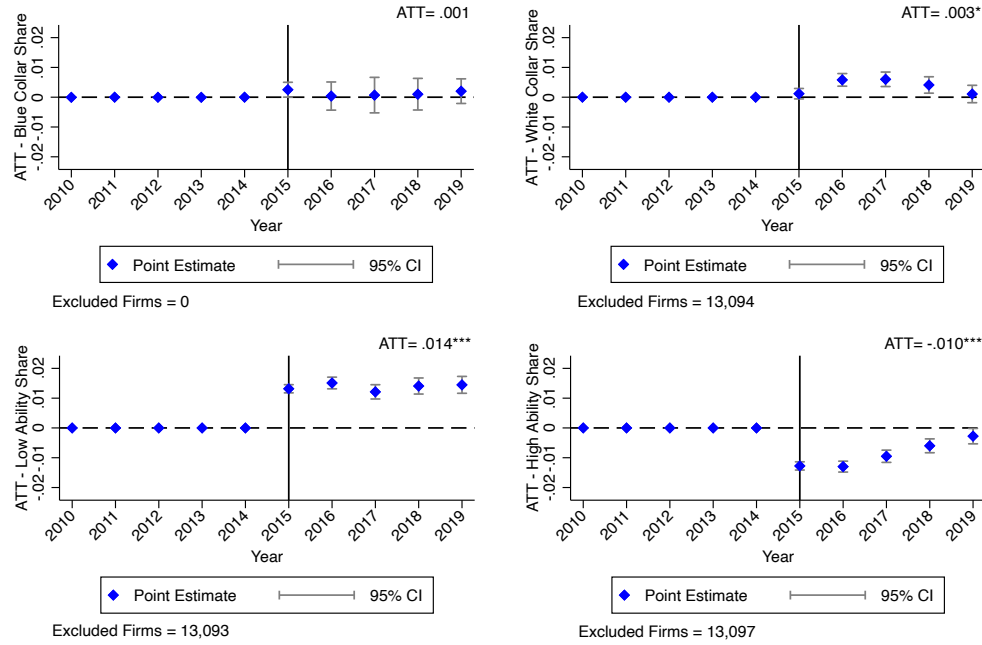
<sup>13</sup>In this sample, we have 49,320 firms of which 53% are subsidized.

**Figure F.2:** SDID estimate of ATT - Input utilization and productivity



Notes: a) log tangible + intangible assets - labor ratio (top-left); b) log VA per worker (top-right); c) log total assets - labor ratio (bottom). Overall ATT in the top-right of each panel. Years 2010-2019 are considered. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$

**Figure F.3:** SDID estimate of ATT - Workforce composition



Notes: a) share of blue-collar workers (top-left); b) share of white-collar workers (top-right); c) share of low-ability workers (bottom-left); d) share of high-ability workers. Overall ATT in the top-right of each panel. Years 2010-2019 are considered. Bootstrap standard errors are calculated with 100 repetitions. Firms with at least 5 employees are considered. \*\*\*  $p \leq 0.01$