Job protection deregulation, productivity and the distribution of income: Firm-level evidence from the Italian Jobs Act

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- **Productivity growth decline, especially in AEs (since before GFC)**
  - ✓ Slower innovation, reduced technological diffusion, weaker investment dynamics & capital and labor misallocation (Goldin et al., 2024)
- **Labor market rigidity** might reduce productivity-enhancing reallocation & firm investment in new technologies

Q1: *Can employment protection deregulation boost "within-firm" productivity?*

Q2: *What distributional effects between workers and capital owners?*

- Investigate effects of the 2014 Italian Jobs Act on firm productivity and distribution
- Firm-level panel data
- Difference-in-differences approach, focusing on firms around the 15-employee threshold
- **Key Findings**:
  - ✓ 1% gain in total factor productivity in treated firms
  - ✓ Labor share declining up to 0.7 p.p. five years post-reform

Theoretical channels: how employment protection impacts productivity

Allocative efficiency

- ✓ Deadweight loss: tax on job separations and hirings (*deadweight* loss) ↓
  - Lazear (1990), Hopenhayn and Rogerson (1993), Mortensen and Pissarides (1994)…

Technical efficiency

- ✓ Workforce composition: better screening ↑ or higher reliance on temporary contracts ↓
  - Lagos (2006), Autor et al. (2007)…
- ✓ Workers’ effort: moral hazard ↓ or higher investment in human capital ↑
  - Nickell (1999), Belot, Boone and Jan Van Ours (2007)…
- ✓ Capital deepening: reduction in adoption of risky technologies ↓ or increase because of stable matches ↑
  - Saint-Paul (2002), Koeniger (2005)…

Empirical evidence is mixed

Contrasting results from country-level studies

- Nickell & Layard (1999): weak positive relationship between EPL and TFP growth
- Bassanini et al. (2009) and Micco & Pages (2006): negative relationship between EPL and TFP growth
- Acharya et al. (2013): EPL leads to more patent applications

Firm-Level studies find negative effect on allocative efficiency…

- EPL slows job turnover (Boeri & Jimeno, 2005; Autor et al., 2007; Haltiwanger et al., 2008; Kugler and Pica, 2008; Messina and Vallanti, 2007; Cingano et al., 2010)

…effects on technical efficiency generally negative, but channels are unclear

- Cingano et al. (2010): EPL reduces labor productivity and capital deepening, especially in financially constrained firms
- Okudaira et al. (2013): greater EPL in Japan decreased TFP and labor productivity, no clear effects on capital deepening
- Cingano et al. (2016): Italian reform introducing EPL in small firms increased capital deepening but decreased TFP
- Autor et al. (2007): wrongful discharge protection in US increased capital deepening and labor productivity but lowered TFP

Only one firm-level study analyzing impact of *deregulation reform*

- Bjuggren (2018): easing last-in first-out dismissal rules in Sweden increased labor productivity by 2-3%, explained by TFP and capital deepening

The Italian Jobs Act: a controversial reform

- Referendum (failed) in June 2025 to abolish the reform

Institutional context

Before 2015

- **Dual regime by firm size,** with stricter dismissal rules for firms with > 15 employees
- **Above threshold**. Required just cause or justified reasons for dismissals; unjustified dismissals could lead to costly and lengthy court-ordered reinstatements, with employers liable for back pay for the entire period
- **Below threshold**. Compensation for wrongful dismissals instead of reinstatement

Fornero Reform in 2012: a first deregulation attempt

- Relaxation, but not abolishment, of reinstatement for economic dismissals. If not manifestly unfounded, compensation from 12 to 24 months' salary
- Restrictions on temporary contracts – 36 months limits

Jobs Act (2014-2015)

Law No. 183/2014 and Legislative Decree No. 23/2015

For firms above the 15 threshold and ***new hires***:

- **Eliminated reinstatements**, unless discriminatory dismissal
- **Compensation set** at two months' salary per year of service – min of 4 months, max of 24 months
- **Out-of-court settlement**: compensation of one month's salary per year of service, max 18 months

Constitutional court ruling

Judgment n. 194/ 2018

- Ruled the 'automatic' (tenure-based) criteria to compute compensation unconstitutional
- Increased compensation, but still no reinstatement

Data and stylized facts

- **Aida database.** Economic and financial information on Italian capital companies
  - ✓ Both listed & unlisted firms, manufacturing & services
- Comprehensive **data cleaning procedures** (as in Gal, 2013 & Kalemli-Ozcan et al., 2015)
  - ✓ After cleaning, sample accounts for 55% (50%) of aggregate employment (value added) reported in official statistics
- Total factor productivity as in Wooldridge (2009); labor productivity as value added per employee
- Limit sample to firms near 15-employee threshold in 2014:
  - ***Treated firms: 16 to 20 employees***
  - ***Control firms: 9-13 employees.*** Following Boeri and Garibaldi (2018), exclude firms with 14-15 employees.

Descriptive statistics: Mean values before and after the reform

	Treatment group		Control group		
	Pre-reform	Post-reform	Pre-reform	Post-reform	DiD
Number of employees	17	19	11	12	0,815
Log revenues	14,80	14,86	14,30	14,34	0,025
Log value added	13,53	13,64	13,01	13,09	0,025
Log TFP	10,39	10,44	10,25	10,29	0,010
Log LP	10,90	10,94	10,85	10,87	0,011
Log capital per employee	10,09	10,09	9,97	9,94	0,029
Log wage bill per employee	10,39	10,42	10,33	10,36	0,012
Wage bill as a share of value added	0,755	0,750	0,753	0,751	-0,002
Return on assets	0,021	0,029	0,020	0,028	0,000
Fixed assets to total assets ratio	0,288	0,280	0,275	0,265	0,002
Age	20	23	18	21	0,130

The reported statistics refer to the baseline estimating sample. All monetary values are expressed in 2015 euros.

More

Methodology: difference-in-differences

- **Quasi-natural experimental framework**:

$$Y_{i(s)t} = \beta_0 + \boldsymbol{\beta_1}(\boldsymbol{Treated_{i(s)}} * \boldsymbol{Post_t}) + \beta_2 X_{i(s),t-1} + \boldsymbol{\delta_i} + \boldsymbol{\delta_{st}} + \varepsilon_{i(s)t}$$

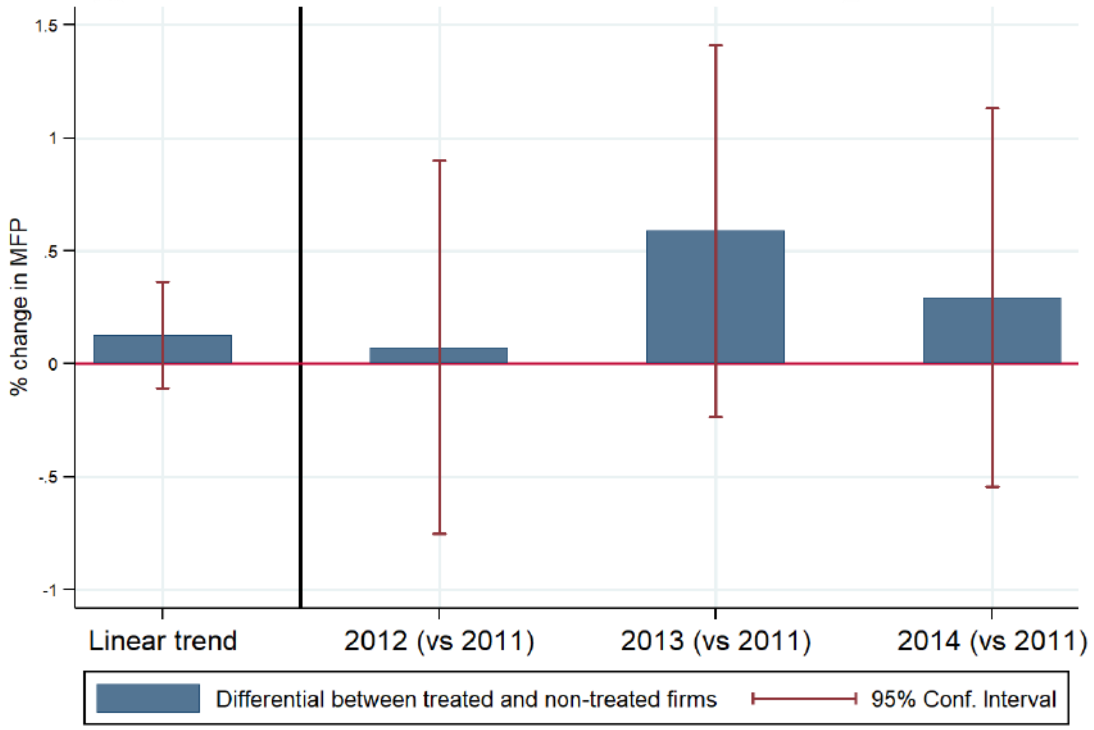
  - ✓ The subscripts *i, s,* t stand for firm, sector and year
  - ✓ $\boldsymbol{Y_{i(s)t}}$: log level of TFP in the baseline (log labor productivity, value of capital stock-to-employment, wage bill per employee, labor share in alternative specifications)
  - ✓ $\boldsymbol{Treated_{i(s)}}$: dummy distinguishing the treatment (firms with 16 to 20 employees in 2014) and the control group (9-13 employees)
  - ✓ $\boldsymbol{Post_t}$**:** either dummy for post-reform period or set of year dummies
  - ✓ $\boldsymbol{X}_{i(s),t-1}$: set of lagged firm level controls, including firms' size classes, age and profitability
  - ✓ $\boldsymbol{\delta_i}$: firm fixed effects, subsuming also region fixed effects
  - ✓ $\boldsymbol{\delta_{st}}$: interacted sector by year fixed effects to account for any sectoral shock

- Equation estimated through OLS, with standard errors clustered at the industry-year level

The parallel trends assumption holds

➢ In the absence of the policy, would the outcome for the treatment and control groups have evolved similarly over time?

Testing for pre-trends



➢ **No pre-trends:** similar trajectories for treated and control firms prior to the reform

- ✓ No linear trend
- ✓ No statistically significant difference in outcomes across years before 2014

➢ Focus on firms **close to the threshold** to minimize potential differences between the two groups

Baseline result: Positive effects on TFP

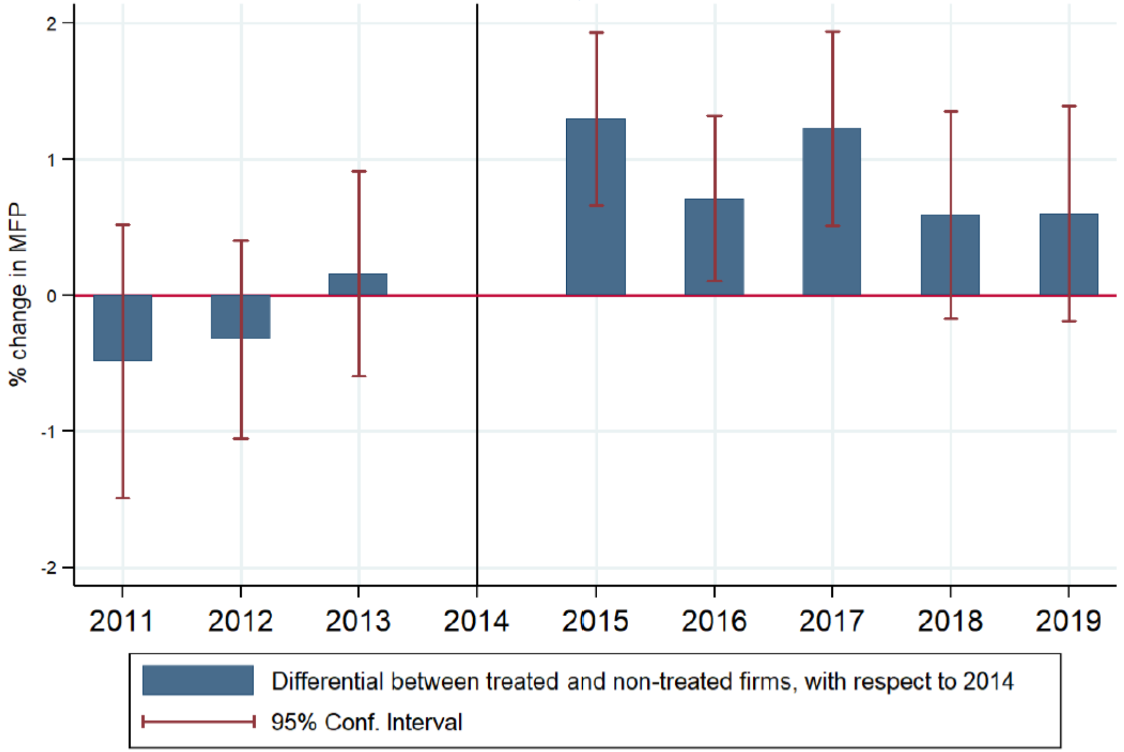
- The reform induced a 1% increase in TFP in treated firms relative to the control group
- The effect seems to slow down four to five years after the reform

Static model

	(1)	(2)	(3)
Post X Treated	0.007***	0.010***	0.009***
	(3.5)	(5.1)	(4.5)
Observations	478793	446141	444872
R^2	0.835	0.862	0.865
Firm FE	Yes	Yes	Yes
Sector by time FE	Yes	Yes	Sub.
Firm-level controls	No	Yes	Yes
Region by sector by time FE	No	No	Yes

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: *10%, ** 5%, *** 1%

Dynamic model



Robustness checks

➢ Sectoral heterogeneity

- ✓ The effect is larger in services, but holds also in manufacturing
- ✓ The effect is lower (higher) in more (less) capital-intensive sectors

➢ Sampling and clustering variations

- ✓ Clustering at the treatment and period level (Abadie et al., 2023)
- ✓ No restriction on firms included in sample (e.g. # years with data available)
- ✓ Treatment defined on the 2013-2014 average employment
- ✓ Adoption of different sampling bands around the 15-employee threshold:
  - Narrower bands: 16 to 18 vs 11 to 13 employees -- *even larger impact, and more persistent*
  - Larger bands: 16 to 24 vs 5 to 13 employees -- *same as the baseline*
  - Inclusion of firms with exactly 14 and 15 employees in the control group -- *more persistent*
  - Exclusion of more firms close to the threshold: 20 to 22 (or 20 to 25) vs 8 to 10 (or 5 to 10) – *lower at impact, but more persistent*

➢ Placebo analysis -- random assignment to treatment and control group

Details

Potential confounding factors: hiring subsidy

- Role of the concurrent implementation of a hiring subsidy -- not directly testable with our data, but three main arguments:

1. Subsidy applied ***uniformly*** to both treated and control firms
    - ✓ No reason to expect a discontinuity around the 15 employees threshold

2. If anything, we would expect control firms to benefit relatively more: **downward bias?**
    - ✓ The subsidy had a cap: smaller or «low wage» firms could achieve a larger reduction in the cost of labor for new hires
    - ✓ Gross job flow rates inversely related to size (Haltiwanger et al., 2008; Boeri and Garibaldi, 2018): higher uptake in smaller firms?
    - ✓ Our evidence aligns: the cost of labor increased more for treated firms compared to the control group

3. Our results hint that control firms did not bring forward planned hiring or over-hired to harness the benefits of the subsidy more than treated firms
    - ✓ Treated firms increased their headcount (and value added) relatively more than control firms
    - ✓ The probability of positive employment growth increased more for firms in the control group that were closer to the 15-employee threshold, while the same probability did not significantly change for firms further from the threshold

Details

Extension 1: Positive effect on labour productivity and capital deepening

Static and dynamic models

	Value added per employee		Value of the capital stock per employee	
	(1)	(2)	(3)	(4)
Post X Treated	0.015***		0.022***	
	(6.4)		(5.4)	
2011 X Treated		0.004		0.002
		(0.8)		(0.2)
2012 X Treated		0.005		0.011
		(1.1)		(1.3)
2013 X Treated		0.007		0.010
		(1.5)		(1.2)
2014 X Treated		0.000		0.000
		(.)		(.)
2015 X Treated		0.016***		0.012
		(3.8)		(1.5)
2016 X Treated		0.016***		0.024***
		(3.8)		(3.4)
2017 X Treated		0.024***		0.032***
		(5.3)		(4.3)
2018 X Treated		0.020***		0.035***
		(3.9)		(4.3)
2019 X Treated		0.020***		0.040***
		(3.7)		(3.7)
Observations	446141	446141	446141	446141
R^2	0.844	0.844	0.902	0.902
Firm-level controls	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*
Sector by time FE	*Yes*	*Yes*	*Yes*	*Yes*

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: *10%, ** 5%, *** 1%

- The **effect on LP is larger** (1.5% vs 1%) and more persistent than on TFP

- *Why?* **Capital deepening**: treated firms increased the capital-labor ratio by 2.2% more than control firms

  - ✓ The effect occurred with a lag and grew significantly over time
  - ✓ **Lower firing costs may unlock firms' productivity potential** by encouraging the adoption of more efficient but riskier production technologies (easier to restructure)

Extension 2: Productivity gains not equally shared

Static and dynamic models

	Wage bill per employee		Value added per employee		Wage bill as value added share	
	(1)	(2)	(3)	(4)	(5)	(6)
Post X Treated	0.009***		0.015***		−0.003***	
	(6.1)		(6.4)		(−3.4)	
2011 X Treated		0.003		0.004		−0.002
		(0.7)		(0.8)		(−1.0)
2012 X Treated		0.005		0.005		−0.002
		(1.3)		(1.1)		(−0.9)
2013 X Treated		0.005		0.007		−0.003
		(1.4)		(1.5)		(−1.4)
2014 X Treated		0.000		0.000		0.000
		(.)		(.)		(.)
2015 X Treated		0.013***		0.016***		−0.002
		(4.1)		(3.8)		(−1.3)
2016 X Treated		0.011***		0.016***		−0.004***
		(3.3)		(3.8)		(−2.9)
2017 X Treated		0.014***		0.024***		−0.007***
		(3.9)		(5.3)		(−3.7)
2018 X Treated		0.013***		0.020***		−0.005***
		(3.5)		(3.9)		(−3.1)
2019 X Treated		0.011***		0.020***		−0.007***
		(2.8)		(3.7)		(−3.7)
Observations	446141	446141	446141	446141	446141	446141
R^2	0.884	0.884	0.844	0.844	0.672	0.672
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector by time FE	Yes	Yes	Yes	Yes	Yes	Yes

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: *10%, ** 5%, *** 1%

- Average wage increases, but not as much as labor productivity: the **labor share decreases**
- Aligns with theoretical predictions:
  - ✓ Efficient bargaining model
  - ✓ Right-to-manage model & capital-labor complementarity
- Strikingly similar to Ciminelli, Duval and Furceri (2022)
  - ✓ Major EPL reform leads to 0.9 p.p. decline in labor share over medium term in panel of 26 OECD economies

Conclusion and policy implications

- First study examining productivity & distributional effects of EPL deregulation at firm-level

- **Improved technical efficiency & capital deepening**
  - ✓ Deregulation might spur adoption of more efficient but riskier technologies
  - ✓ Increase of permanent contracts & worker efforts other potential channels

- **Wages not keeping up with productivity** => income distribution shifting towards capital owners

- Very difficult to remove EPL once it is granted!
  - ✓ EMs where EPL is still low should design new legislation carefully
  - ✓ Complementary measures may be necessary to mitigate the negative effects of deregulation on income distribution

Grazie!

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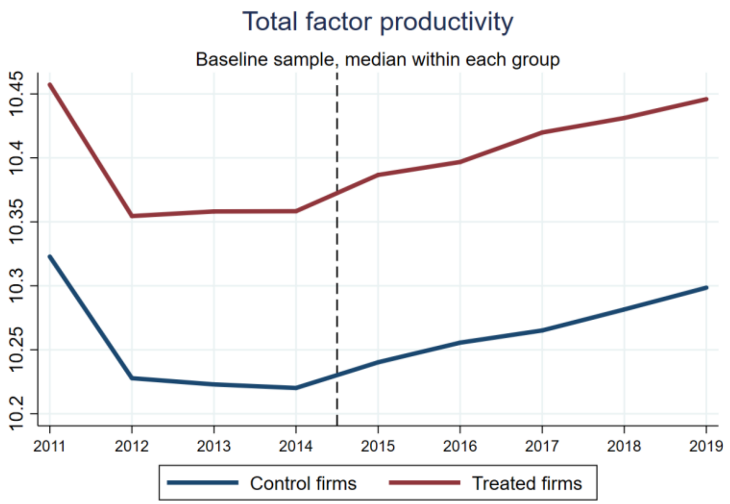
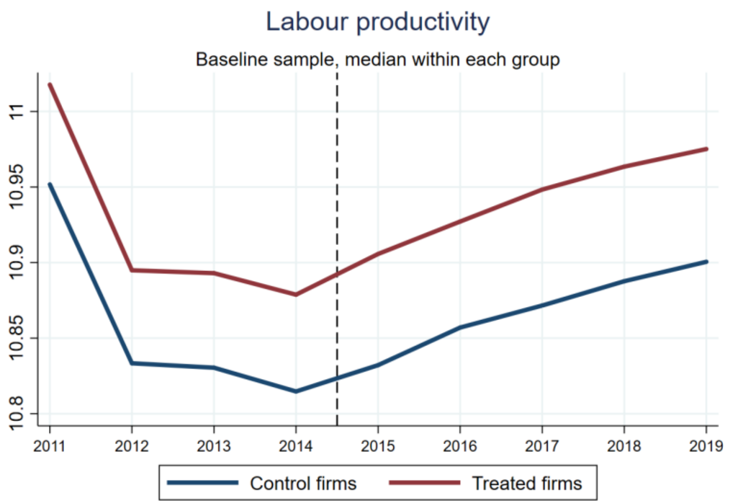
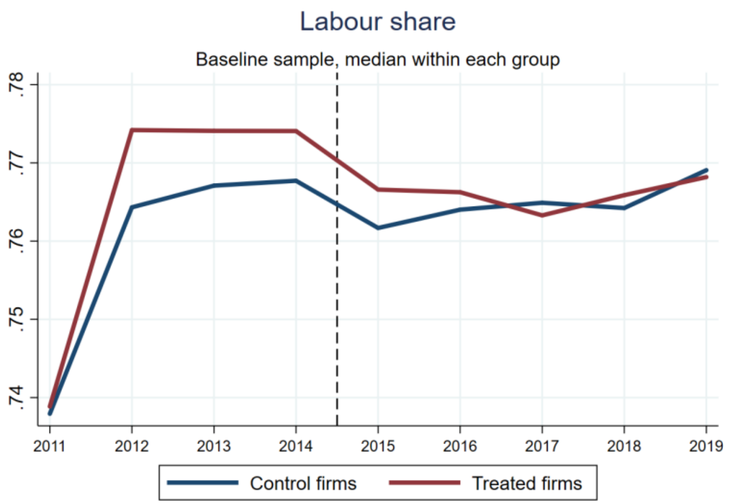
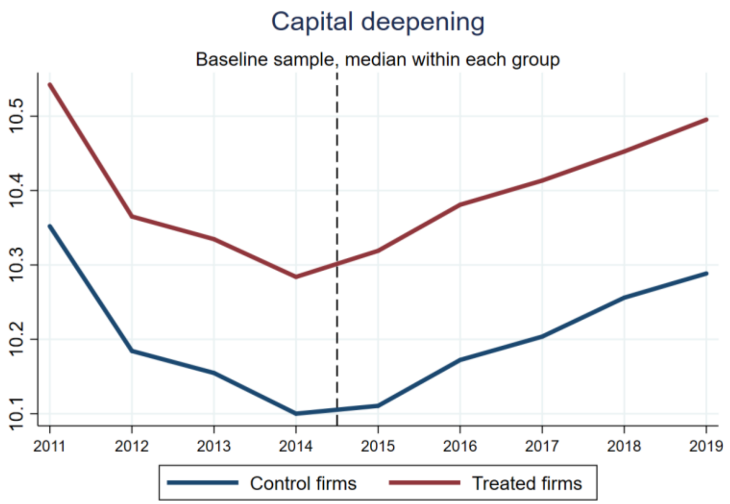
Annex

Raw data: depvar levels over time









BACK

Robustness checks: Estimates

Sectoral heterogeneity

	Industry	Services	Capital intensity
	(1)	(2)	(3)
Post X Treated	0.009***	0.015***	0.014***
	(3.1)	(5.2)	(6.4)
Post X Treated X Capital Intensity			−0.002***
			(−3.8)
Observations	168205	220046	446141
R^2	0.852	0.883	0.862
Firm-level controls	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*
Sector by time FE	*Yes*	*Yes*	*Yes*

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: *10%, ** 5%, *** 1%

Sensitivity analysis: Clustering and sampling

	Period by Treated clusters			Unbalanced panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Post X Treated	0.010**			0.010***		
	(4.4)			(4.7)		
2011 X Treated		−0.005	−0.004		−0.008	−0.007
		(−1.1)	(−0.9)		(−1.6)	(−1.3)
2012 X Treated		−0.003	−0.002		−0.006*	−0.004
		(−1.3)	(−0.8)		(−1.7)	(−1.1)
2013 X Treated		0.002	0.003		−0.002	−0.001
		(1.0)	(1.5)		(−0.5)	(−0.3)
2014 X Treated		0.000	0.000		0.000	0.000
		(.)	(.)		(.)	(.)
2015 X Treated		0.013***	0.014***		0.010***	0.011***
		(22.6)	(45.0)		(3.0)	(3.3)
2016 X Treated		0.007**	0.007**		0.006*	0.007*
		(5.1)	(5.0)		(1.8)	(1.9)
2017 X Treated		0.012***	0.012**		0.010**	0.009**
		(5.9)	(5.5)		(2.5)	(2.4)
2018 X Treated		0.006*	0.005*		0.004	0.003
		(3.1)	(2.5)		(1.1)	(0.8)
2019 X Treated		0.006	0.005		0.003	0.002
		(1.5)	(1.4)		(0.7)	(0.5)
Observations	446141	446141	444872	510677	510677	509444
R^2	0.862	0.862	0.865	0.856	0.856	0.859
Firm-level controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector by time FE	*Yes*	*Yes*	*Sub.*	*Yes*	*Yes*	*Sub.*
Region by sector by time FE	*No*	*No*	*Yes*	*No*	*No*	*Yes*

T-statistics in parentheses, standard errors clustered at the treatment-period levels in models 1 to 3 and at the industry-year level in models 4 to 6
Significance Level: *10%, ** 5%, *** 1%

BACK

Robustness checks: Estimates (cont'd)

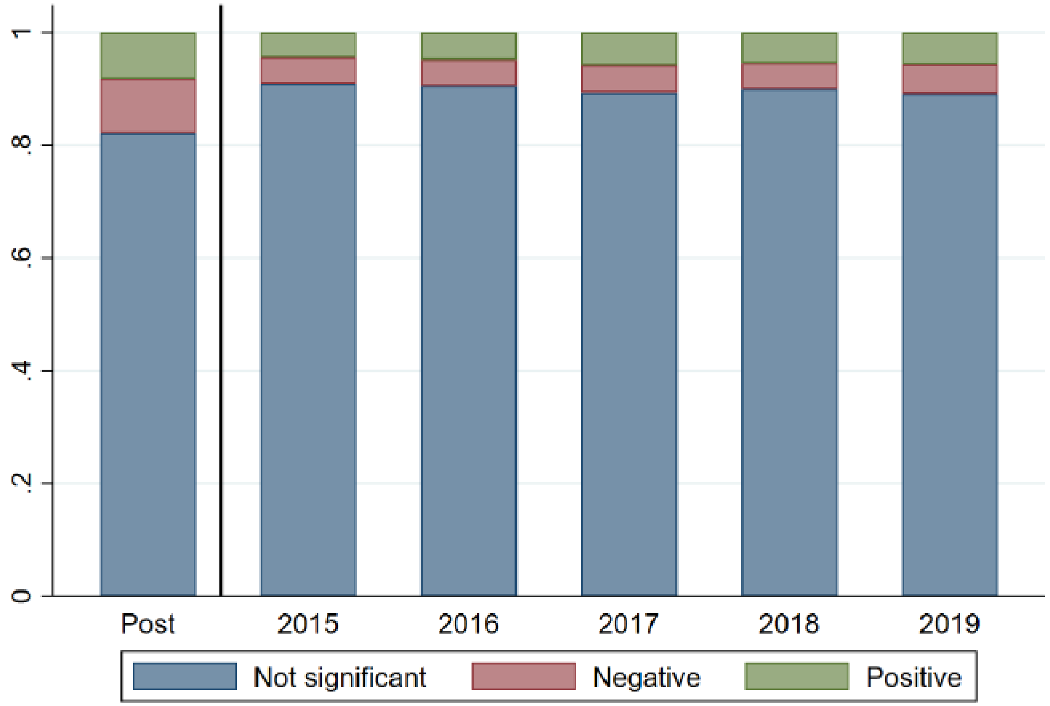
Sensitivity analysis: Sampling around the threshold

	11-13 vs 16-18		5-13 vs 16-24		10-15 vs 16-21	
	(1)	(2)	(3)	(4)	(5)	(6)
Post X Treated	0.016***		0.011***		0.010***	
	(7.1)		(5.2)		(5.9)	
2011 X Treated		−0.009		−0.009*		−0.001
		(−1.5)		(−1.9)		(−0.2)
2012 X Treated		−0.008*		−0.002		−0.002
		(−1.9)		(−0.5)		(−0.6)
2013 X Treated		−0.001		0.005		0.002
		(−0.3)		(1.1)		(0.6)
2014 X Treated		0.000		0.000		0.000
		(.)		(.)		(.)
2015 X Treated		0.028***		0.015***		0.012***
		(7.9)		(3.8)		(4.4)
2016 X Treated		0.010***		0.011***		0.010***
		(2.8)		(2.9)		(3.6)
2017 X Treated		0.011***		0.013***		0.012***
		(3.0)		(3.1)		(3.6)
2018 X Treated		0.010**		0.007		0.008**
		(2.3)		(1.5)		(2.4)
2019 X Treated		0.001		0.008*		0.008**
		(0.3)		(1.7)		(2.4)
Observations	254544	254544	947708	947708	474353	474353
R^2	0.865	0.865	0.858	0.858	0.866	0.866
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector by time FE	Yes	Yes	Yes	Yes	Yes	Yes

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: * 10%, ** 5%, *** 1%

Placebo analysis

Proportion of coefficients estimated for 500 random samplings of treatment and control group



BACK

Potential confounding factors: hiring subsidy

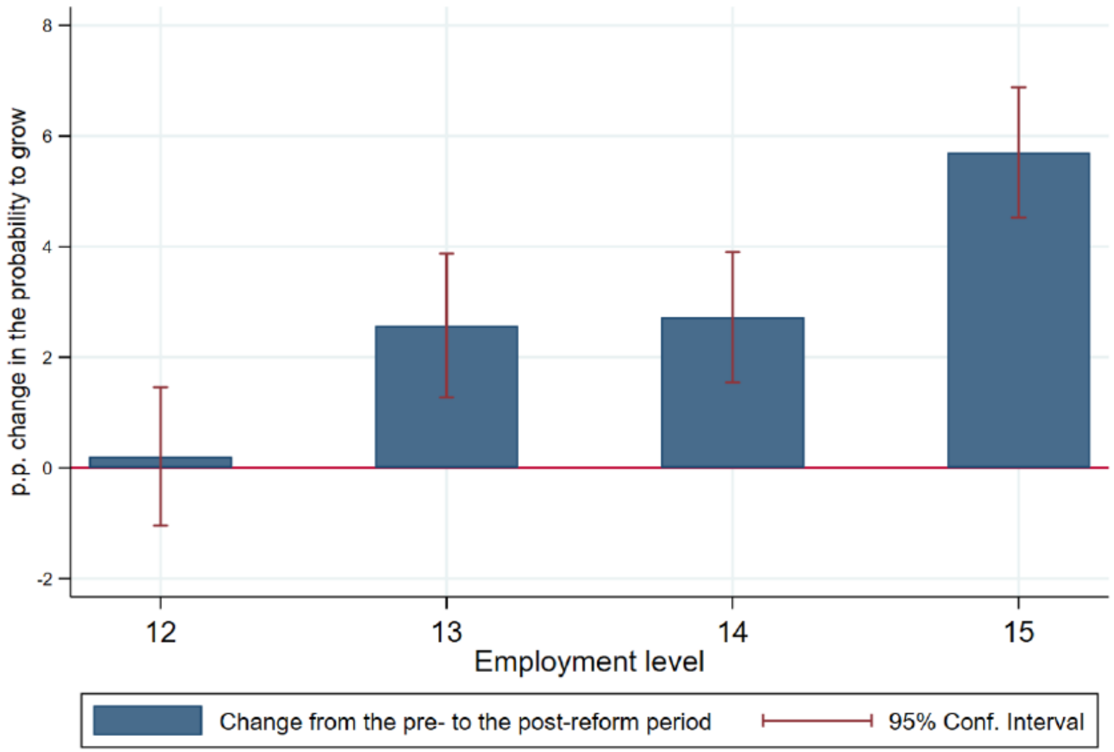
Employment and value added dynamics

	Employment	Value added
	(1)	(2)
Post X Treated	0.011***	0.014***
	(3.8)	(4.6)
Observations	446141	446141
R^2	0.647	0.866
Firm-level controls	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*
Sector by time FE	*Yes*	*Yes*

T-statistics in parentheses, standard errors clustered at the sector-year level
Significance Level: *10%, ** 5%, *** 1%

Note: The estimates are obtained using the same model of the baseline findings, but lagged size classes excluded from specification 1.

Probability to grow getting closer to the threshold



Note: Estimates following the methodology in Schivardi and Torrini (2008), where the probability of positive employment growth is regressed over a fourth degree polynomial in size (to account for the smooth relation between size and the probability of growing), a set of dummies with respect to employment levels, and firms' age and profitability as further controls.

BACK