

Employment Protection Legislation and Mismatch: Evidence from a Reform

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Background

- During the last three decades, **labor market deregulation** has been largely prescribed to overcome poor labor market and growth performances [Imf 1999; Oecd 1994]
- Both theoretical [Bertola 1990] and empirical [Cazes 2013] literature suggest that **EPL reduces worker turnover** but does not directly affect employment
- However EPL:
 - **favors inefficient retention** [Bassanini and Ernst, 2002]
 - **harms the allocation** of workers to the jobs where they would be more productive [Rogerson, 1987]

Aim and outline of presentation

- The aim is to test empirically whether **reducing EPL improves workers' allocation**, understood as job match quality
- We do this by taking advantage of Law 92/12 (the “**Fornero Law**”), that allow the application of a DiD approach
- Outline:
 - Institutional details
 - Theoretical background and measurement issues
 - Data, sample selection and model specification
 - Main results
 - Discussion

Institutional details: pre-treatment

- Individual layoffs in Italy are legitimized by disciplinary and economic reasons. If a layoff is ruled unlawful:
 - In firms employing **16 workers or more**, the employer is required to pay all foregone wages and social security contributions. In addition, **the worker** can choose:
 - **Reinstatement**, or...
 - ...a **monetary reparation** of 15 monthly salaries
 - In smaller firms **the employer** chooses between
 - **Re-hiring** the worker, or...
 - ...paying a **compensation** of 2,5 to 6 monthly salaries (up to 14 for most tenured workers)

Institutional details: post-treatment

- Law 92/12 changes the rules for larger firms only:
 - Unlawful **disciplinary** layoffs lead to reinstatement (plus a compensation of no more than 12 salaries) only if the judge ascertains that no just cause or subjective reason actually exists, or if it should be punished otherwise according to collective agreements. In the other cases only a compensation ranging between 12 and 24 salaries is due
 - Unlawful **economic** layoffs lead to reinstatement (plus a compensation of no more than 12 salaries) only if the judge ascertains that no objective reason actually exists. In the other cases only a compensation ranging between 12 and 24 salaries is due

Institutional details: main changes

In firms with more than 15 workers, the Fornero Law hence:

- **Deprived** the workers of the option to choose between reinstatement and monetary reparation
- **Limited** the room for reinstatement to well-defined cases
- **Reduced** the amount of total compensation
- **Reduced** uncertainty on duration and expenses of litigation

This process was de-facto imposed by international actors with no involvement of social partners [Picot and Tassinari, 2017; Sacchi, 2015]. Its **exogeneity** opens the possibility of a **diff-in-diffs analysis** around the 15-employee threshold

Operationalization of match quality

- Job match quality is a **multidimensional object**: we understand it in terms of **skills**
- Workers' skills are measured in many ways (Quintini 2011):
 - The most traditional is **education** (Freeman 1976)
 - Dedicated surveys on **literacy and numeracy**, like PIAAC (Pellizzari and Fichen 2013)
 - Actual work **experience** (Ghignoni 2001)
- Consistently with a large literature, we use education (McGuinness et al. 2016)
- Education is also more consistent with the idea of observing **skill allocation rather than skill creation**

Data and sample selection

We use pooled cross-sections from the **Italian LFS**

- Only employees with **open-ended contracts** in the private non-agricultural sector
- Observed period is limited to **2011-2014** due to time inconsistencies in the definitions of sectors and occupations (relevant to our definition of mismatch)
- Pre-treatment period is **2011:Q1 to 2012:Q2** (the reform was enforced during 2012:Q3)
- Post-treatment period is **2012:Q4 to 2014:Q3** (in 2014:Q4 another major reform – the *Jobs Act* under the Renzi government – was introduced)

Data and sample selection

- Ideally, one should narrow the firms size as much as possible **around the 15-employee threshold**, in order to minimize the room for unobserved heterogeneity
- However, this sampling strategy would be prone to a high **contamination risk** as – due to the reform – firms may grow over the threshold
- Hence, we compare match quality of workers in firms sized 11-15 (controls) with those in firms sized 20-49 (treated)
- We pay a price in terms of u.h. (but LFS has a large amount of observables), but we run robustness checks

Model specification

- Baseline model is estimated through a logit specification, where $Y_{ijkt} = 1$ if individual i is well-matched

$$Y_{ijkt} = \beta_0 + \beta_1 TREAT + \beta_2 POST + \beta_3 TREAT \times POST + \beta_4 X_{ijkt} + \gamma_t + \delta_t + \varepsilon_{ijkt}$$

- Individual i is considered as well-matched if her educational attainment at quarter t is equal to the **median** educational level observed on all employees within the same economic activity j and occupation k at the same point in time
- As turnover may take a while to have allocation effects, we also separate first- and second-year effects of the reform

Estimation results

According to our model, **the reform has improved job-match quality** in treated units (the odds of a good match grows by 9.5%). **It takes one year** for workers' turnover to affect job-match quality.

	RRR	p-value	Observations
Firms 11-15/20-49, overall effect	1.096**	0.021	
Firms 11-15/20-49, first four quarters	1.040	0.386	81,130
Firms 11-15/20-49, following four quarters	1.156***	0.005	

Source: own computations on LFS data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%.

Corresponding to an AME of + **1.7 p.p.**

Corresponding to an AME of + **2.7 p.p.**

Robustness checks

	Odds ratios	p-value	Observations
<i>Panel A: unobserved heterogeneity</i>			
Firms 11-15/16-19, overall effect	1.085	0.126	
Firms 11-15/16-19, first four quarters	1.000	0.998	50,231
Firms 11-15/16-19, following four quarters	1.185**	0.014	
<i>Panel B: measurement error</i>			
Firms 11-15/20-49, overall effect	1.098**	0.014	
Firms 11-15/20-49, first four quarters	1.073	0.103	81,130
Firms 11-15/20-49, following four quarters	1.125**	0.019	
<i>Panel C: changes in median education</i>			
Firms 11-15/20-49, overall effect	1.088**	0.011	
Firms 11-15/20-49, first four quarters	1.029	0.535	81,130
Firms 11-15/20-49, following four quarters	1.153***	0.006	
<i>Panel D: uncompleted education</i>			
Firms 11-15/20-49, overall effect	1.107**	0.011	
Firms 11-15/20-49, first four quarters	1.054	0.257	78,441
Firms 11-15/20-49, following four quarters	1.166***	0.003	

Placebo tests and parallel-trend assumption

	Odds ratios	p-value	Observations
<i>Panel A: longitudinal placebo test</i>			
Firms 11-15/20-49, overall effect	0.980	0.745	32,764
<i>Panel B: sectional placebo test</i>			
Firms 16-19/20-49, overall effect	1.009	0.858	
Firms 16-19/20-49, first four quarters	1.039	0.503	63,661
Firms 16-19/20-49, following four quarters	0.977	0.719	

Source: own computations on LFS data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%.

OLS regression of the quarterly change in the number of good matches in cells defined by sector*occupation*firm size*quarter on a dummy for treated units, using pre-treatment quarters only



Test of the parallel trend assumption

	Coefficient Estimate	p-value	Observations
No controls	-396.99	0.504	317
Year and quarter fixed effects	-400.59	0.499	

Source: own computations on LFS data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%

Business cycle

Control strategy	Odds Ratios		
	Overall effect	First four quarters	Following four quarters
<i>Fixed effects (Panel A)</i>			
(1) Sector + Region + Year + Quarter (baseline specification)	1.096**	1.040	1.156***
(2) Sector + Region + Year*Quarter	1.096**	1.039	1.158***
(3) Sector + Region*Year*Quarter	1.099**	1.048	1.153***
(4) Region + Sector*Year*Quarter	1.094**	1.044	1.147***
(5) Sector*Region*Year*Quarter	1.098**	1.046	1.155***
<i>Business cycle aggregate indicators (Panel B)</i>			
(6) Quarterly national GDP growth + Region + Sector fixed effects	1.095**	1.039	1.155***
(7) Yearly regional GDP growth + Quarter + Sector fixed effects	1.095**	1.039	1.155***
(8) Quarterly regional unemployment rate + sector fixed effects	1.096**	1.040	1.157***
<i>Business cycle aggregate indicators differentiated by firm size (10-15 vs. 20-49) (Panel C)</i>			
(9) Quarterly national GDP growth + Region + Sector fixed effects	1.082**	1.037	1.142**
(10) Yearly regional GDP growth + Quarter + Sector fixed effects	1.097**	1.029	1.153***
(11) Quarterly regional unemployment rate + sector fixed effects	1.089**	1.036	1.150***

Source: own computations on LFS data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%

Firm performance

Log of value added per worker	Overall effect	1.096**	0.020	
	First four quarters	1.038	0.405	
	Following four quarters	1.154***	0.006	
Total Factor Productivity	Overall effect	1.097**	0.019	81,130
	First four quarters	1.040	0.390	
	Following four quarters	1.157***	0.005	
Profits over sales	Overall effect	1.100**	0.017	
	First four quarters	1.042	0.360	
	Following four quarters	1.161***	0.004	

Source: own computations on LFS and balance-sheet data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%

Contamination from other LM institutions

		Odds ratios	p-value	Observations
Probability of being a temp (any type)	Overall effect	0.955	0.393	93,172
	First four quarters	1.004	0.951	
	Following four quarters	0.907	0.174	
Probability of being an apprentice	Overall effect	1.143	0.418	92,780
	First four quarters	1.184	0.372	
	Following four quarters	1.102	0.664	
Controlling for the use of STW	Overall effect	1.096**	0.020	81,130
	First four quarters	1.040	0.385	
	Following four quarters	1.156***	0.005	

Source: own computations on LFS data. Notes: robust standard errors; *** = 1%; ** = 5%; * = 10%

Workers' turnover

- A few doubts that EPL reforms affect turnover (for Italy: Kugler and Pica 2008; Boeri and Garibaldi 2018; Sestito and Viviano 2018; Chiara&Fabio&Lia in this session)
- However, the turnover effects of Law 92/12 have not been tested
- We use 2010 and 2014 waves of **RIL data** (as Massimiliano&Maurizio&Giovanni) to estimate a comparable **DD model of workers' turnover measures**
- **Controls:** regional dummies, 1-digit sectors, firm size, incorporated business. **Selection:** private, non-agricultural

Workers' turnover

- Results support the view that **the reform enhanced turnover**

	Worker turnover rate	Worker accession rate	Worker separations rate	Firing only
variables				
Post-reform period	-0.659*** (0.118)	-0.300*** (0.063)	-0.401*** (0.065)	-0.086*** (0.019)
Firms above art. 18 threshold	-0.790*** (0.257)	-0.411*** (0.138)	-0.537*** (0.143)	-0.147*** (0.038)
Reform	0.671* (0.388)	0.322(*) (0.208)	0.414* (0.210)	0.101* (0.061)
controls	yes	yes	yes	yes
No. observations	45092	44836	44836	40821

Productivity

- The effect of EPL reforms on **productivity** is what is ultimately relevant to test the usefulness of workers' reallocation
- It depends both on skill **allocation** and on skill **development**
- Empirical strategy: **DD with RDD** of per-worker **log of value added** and of **TFP** measured at firm level using balance-sheet data (**AIDA**)
- **Sample selection:** years 2006-2014 and firms sized 10-15 (control units) or 19-49 (treated units)
- **Controls:** firm size, 2-digit sector, year and regional dummies

Productivity

The effect of the Fornero Law upon productivity is economically small, but **positive** and very precisely estimated

Robustness: control for a linear time trend and GDP growth, both interacted with the treatment. Lower magnitude, but still significant

row	Sample	VA per worker	TFP	VA per worker	TFP
1	all firms	0.114 (0.002)	0.107 (0.002)	0.106 (0.002)	0.074 (0.002)
2	firms size 10-49	0.043 (0.003)	0.038 (0.003)	0.025 (0.002)	0.014 (0.002)
3	as in row2, but excluding size 16-19	0.048 (0.004)	0.044 (0.004)	0.028 (0.002)	0.018 (0.002)
4	manufacturing, size 10-49	0.049 (0.005)	0.042 (0.005)	0.019 (0.003)	0.013 (0.003)
5	services, size 10-49	0.036 (0.005)	0.034 (0.005)	0.034 (0.003)	0.021 (0.003)
6	as in row4, but excluding size 16-19	0.056 (0.005)	0.050 (0.005)	0.024 (0.004)	0.018 (0.004)
7	as in row5, but excluding size 16-19	0.037 (0.006)	0.037 (0.006)	0.036 (0.004)	0.036 (0.004)
8	as in row2, but period 2010-2014	0.018 (0.004)	0.012 (0.004)	0.015 (0.002)	0.008 (0.002)
9	as in row4, but period 2010-2014	0.023 (0.006)	0.021 (0.005)	0.009 (0.003)	0.006 (0.003)
10	as in row5, but period 2010-2014	0.016 (0.006)	0.008 (0.006)	0.019 (0.004)	0.015 (0.003)
	control variables	yes	yes	yes	Yes
	firm fixed effects	no	no	yes	Yes

Discussion and concluding remarks

- An EPL reduction has improved job match quality in Italy. The likely channel was increased turnover, and led to higher productivity in the short run
- **Limitations:**
 - Education captures only a share of match quality
 - Positive allocation effects may be countervailed by negative accumulation ones in the longer run
 - EPL may have effected productivity through higher workers' effort rather than better allocation
 - EPL reforms may drive a substitution between temps and perms