Employment protection legislation and mismatch: evidence from a reform[1]

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Abstract. The aim of this paper is to assess the impact that the so-called Fornero Law had on the quality of the educational match contributing, thus, to a general debate on the effects of deregulation on the labor market and on skill mismatch in particular. The specific scope of the Fornero Law – limited only to companies of a certain dimension – allowed us to apply a DiD model using the Italian LFS pooled cross-section data over 14 yearly quarters. Our results show that, as an effect of the reform, the odds of educational match in companies with more than 15 employees has increased. This is consistent with economic theory that informed labor market deregulation during the last decades. However, these effects are not visible during the first year after the law's enactment, but only during its second year, a likely consequence of workers' turnover time. Robustness tests support our findings.

Keywords: Skill mismatch, Employment Protection Legislation, Fornero Law, Difference-in-differences

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1. Introduction and literature

Economic theory dating to back to the late eighties and early nineties suggests that labor market deregulation is of key importance to competitiveness, growth and employment (Bertola, 1990; Rogerson, 1987). As firms are left free to hire and fire at low costs, it is argued, they can improve the average quality of matches between workers and jobs, move to most promising specializations, increase their productivity and profits, boost their innovation activity, and hence improve the overall competitiveness of the economic system, its growth potential and employment score. Building also on the good economic performance of the US under Ronald Reagan and of the UK under Margaret Thatcher during the eighties, labor market deregulation was the prescription to heal "inflexible Europe" (Imf, 1999; Oecd, 1994) from its low growth and high unemployment disease. Following this piece of advice, in the last decades it quickly became also a policy actually implemented in a huge variety of countries around the world (Berton et al. 2012), if not *the* employment policy *par excellence*. Key to this way of reasoning is the (unproved) assumption that workers' turnover is beneficial to the quality of matches between workers and firms.

Related empirical evidence nonetheless mostly focused on the "reduced form" of the underlying theoretical model, i.e. on trying to assess directly the impact of labor market deregulation on employment and unemployment (e.g. the literature surveyed in Cazes, 2013; Noelke, 2015; Oecd, 2004), paying little attention on how employment protection legislation affects the quality of job-worker matches. The reason has been fourfold. First, knowing about the consequences of labor market institutions on employment performance was (and probably still is) of much higher policy relevance. Second, the relationship between employment protection and the quality of job-worker matches is more micro in nature and raises issues of data availability. Third, measuring the quality of a match is not trivial and requires information that is not commonly collected in labor market databases. Fourth, the causality of this relationship is circular and its identification requires the emergence of quasi-experimental conditions.

Today a renewed interest for knowledge-based economy, human capital accumulation and active ageing has circumvented the first limitation (policy relevance), in particular inasmuch as job match quality is assessed in terms of skills. The European Commission's communication "New skills for new jobs" (European Commission, 2008) and the Europe 2020 initiative "Agenda for new skills and jobs" (European Commission 2010), witness that skill formation plays a primary role within the European strategy towards Europe 2020. The European Center for the Development of Vocational Training (Cedefop 2009) has recognized this issue through the identification of five priorities for future research: (i) improve measurement of skills and skills mismatch; (ii) examine the persistence of skill mismatch and its impacts; (iii) improve understanding of skill mismatch processes, its dynamics and the consequences of skill mismatch; (iv) focus on skill mismatch for vulnerable groups on the labor market; and (v) improve data availability and use.

Data availability and use – to put it in Cedefop's terms and to move to the second limitation listed above – has already improved a lot thanks to the collection of large sets of micro data (of which the European Labor Force Survey and EU-SILC are just two easy examples), the availability of Longitudinal Matched Employer-Employee Databases (LMEED, more often administrative in nature: see for instance Desai, 2008), the realization of dedicated surveys (like the Oecd's Survey of Adult Skills: Oecd, 2016) and to software and hardware advancements, now allowing the analysis of large amount of information within a limited time.

Instead, more concern exists upon *measuring* the quality of a match, even when its assessment narrows to that of skill mismatch. In theory, the ideas of skill demand and of skill mismatch are well defined: the former refers to the amount and type of human capital that an employer deems ideal to carry out the job for which the related vacancy was opened. The latter, to the distance between skill demand and skill supply, i.e. the amount and type of human capital possessed by the workers. Empirically, however, the operationalization of these ideas is not as easy. From the metrics standpoint, the literature (Flisi et al., 2014; ILO, 2014; Johansen and Gatelli, 2012; Quintini, 2011; Sala, 2010) highlights three different approaches; all of them suffer from some limitation. Under the *normative* approach, groups of experts are interviewed after the realization of the relevant matches about the skills that presumably the employers were looking for when they posted the relative vacancies. This approach carries the advantage of identifying separately skill demand and supply, but is extremely costly – what limits the size of the resulting data – and strongly country-specific; for these reasons it is seldom used in the empirical literature. Under the *subjective* approach whether skill supply fits with skill demand is directly asked to the employed workers, sometimes in conjunction with their employers. This procedure is less costly and can easily be integrated within existing large labor market surveys, but is openly prone to self-assessment bias. In the *objective* approach, eventually, the distribution of employed workers' (or of *realized matches'*, to use this literature's terms) skills within each occupation is described in terms of its mean (or median) and dispersion: workers whose skills lay within a given range (usually once or twice the standard deviation) from the reference distribution point (mean, median) are considered well-matched. Although this approach does not really measure the distance between skill demand and supply – as instead it captures the dispersion of the realized market equilibrium, which is likely to suffer from some rationing on the supply side – it is probably the most widely used metrics of skill mismatch. For comparability reasons, in our empirical exercise we will hence move along these same lines.

If *how* the skill mismatch should be measured is all but commonly agreed, *what* should be measured is possibly even more discussed. Match quality is traditionally defined and measured in terms of distance between workers' education and the level of education typically required in their occupations (Freeman, 1976). However, this makes the issue of mismatch equivalent to that of over- or undereducation (Büchel et al., 2003; Leuven and Oosterbeek, 2011). For this reason, efforts have recently been made in

order to distinguish the educational dimension in strict sense from that of work-related skills. The most prominent of these examples is represented by the Oecd Program for the International Assessment of Adult Competencies (PIAAC), which collects data about numeracy and literacy at the individual level in more than forty countries in the world (see e.g. Pellizzari and Fichen, 2013). Although representing a clear advancement in the direction of recognizing the multi-dimensional nature of skill mismatch (Nedelkoska et al., 2015), PIAAC-like approaches still appear very much tied to the idea – common to education-based measures – that skills pertain to the realm of those formal competencies that can be transferred from one job to another. The current state of the art of measuring individual skills, in other words, is rather close to Becker's idea (1964) of *general human capital*, and neglects the obvious fact that individual skills accumulate and evolve in a way which is specific to one's working career[2]. PIAAC itself, moreover, suffers from another limitation that harms its potential in terms of causal identification, i.e. at the moment it lacks a longitudinal dimension. For this reason, in order to fully exploit the quasi-experimental situation described below, in our empirical exercise we will make use of less innovative data, and measure skills in terms of education.

What our paper mainly contributes to, indeed, is on the identification of the causal relationship between employment protection legislation and skill mismatch, i.e. exactly on the theoretical foundations of labor market deregulation. The issue is not trivial, and is deeply exposed to the risk of reverse causality. To what extent, indeed, does workers' mobility shape mismatch, and to which one, instead, is mismatch a determinant of workers' mobility? Search-type models (e.g. Mortensen and Pissarides, 1994) predict that wrong matches are the first to separate, but shorter-lived employment relationships (a likely consequence of deregulation) are in turn not neutral to the accumulation of skills (Acemoglu and Pischke, 1999; Berton and Garibaldi, 2012; Lazear, 2009) and hence potentially to job match quality. In order to prevent this circularity, in this paper we take advantage of a recent labor market reform – namely Law 92/2012, better known as "Fornero Law" after the name of the Minister of Labor under the Monti government – that suddenly relaxed EPL in Italy for a subset only of firms, leaving individual layoff conditions unaffected for the others. This has created the ideal conditions to apply a conditional difference-in-differences identification strategy.

This paper proceeds as follows: in section two, we describe the Italian institutional framework concerning individual layoffs before and after the introduction of the reform; in section three, we describe the data and motivate sample selection and model specification. While section four presents our empirical results and the robustness checks, in section five we draw some tentative concluding remarks.

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[2] An exception to this is represented by Ghignoni (2001), in which the role of individual experience is openly recognized.

2. The institutional framework

The label of *employment protection legislation* pertains to all the rules governing the hiring and firing processes existing in a labor market. It therefore also includes the regulations on collective dismissals and all the limitations concerning the use of temporary contracts. The reform we are exploiting in this empirical exercise to identify the causal effect of EPL on skill mismatch, however, focuses on individual layoffs.

Before Law 92/2012 was enforced – what happened on July 18th, 2012 – individual layoffs in Italy were regulated under Laws 604/1966 and 300/1970, the latter being better known as the "workers' statute"; these laws intervened on a situation of no limitations to individual layoffs. Indeed, under the civil code of 1865, open-ended contracts were explicitly forbidden. Recognizing an existing *de-facto* situation in which many employers aimed at integrating their laborers permanently within the production process, the fascist civil code enforced in 1942 introduced the possibility to stipulate open-ended employment contracts; nonetheless, it also granted the complete freedom to dismiss workers without any justification. Law 604/1966 represented a turning point with respect to this principle, inasmuch as it introduced the idea that individual layoffs must be justified in order to prevent abuses from the employers. In particular, an employer is legitimated to dismiss a worker if a *just cause* exists – damage of equipment, fight or violence towards other colleagues – or in case of a *justified reason*, that can be either *subjective* – major breaches of contract obligations – or *objective*, when the organization of the production process would make impossible the continuation of the employment relationship. In case the dismissed employee deems illegitimate the layoff, she has to bring the case to a labor court. Under Law 604/1966, the labor judge has the authority to ascertain whether the dismissal was legitimated by a just cause or a justified reason. In case it was not, the employer is obliged to choose between starting a new employment relationship with the dismissed worker, or to compensate her with a sum ranging from 2.5 to 14 monthly salaries, depending on firm size and worker's seniority. In no case a severance pay is present. Law 300/1970, with its well-known article no. 18, introduced a more generous compensation system for illegitimate dismissals in firms employing more than fifteen workers in the same production unit or municipality[3]. According to its provisions, an illegitimate layoff is deprived of any legal effect, leads to reinstating the dismissed worker to her former position, and to the compensation of all foregone salaries and social security contributions since the layoff date. As for Law 604/1966 – which keeps applicable to small firms – no severance pay is present for legitimate dismissals, while layoffs that are deemed discriminatory are null[4].

Based on the above-described provisions included in Law 300/1970, Italy has usually been presented as an exemplary case of rigid labor markets. This view was far from being unchallenged (Contini

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[3] The threshold falls to five workers in the farming sector. In any case, having more than sixty employees all over Italy suffices to apply article 18 of Law 300/1970.

[4] For more details about the Italian institutional framework until the Monti government, see Berton et al. (2012).

and Revelli, 1992; Contini and Trivellato, 2005; Del Conte et al., 2004), but what is relevant to our purposes is that since the early nineties the institutional setting defined by Laws 604/1966 and 300/1970 started to be considered questionable. Nonetheless, after a decade of partial labor market reforms that led to an almost complete liberalization of the use of temporary contracts (Davidsson, 2011), it was only the economic crisis in 2008 that created the necessary political capital and institutional context to proceed to a revision of Law 300/1970. Following a process that Sacchi (2015) has defined *conditionality by other means*, the European Union required Italy to proceed to structural labor market reforms – in particular to revise the employment protection system concerning workers in medium and large firms – in order to receive financial support in the aftermath of the sovereign debt crisis. The fourth Berlusconi government made two attempts to fulfill this request through the introduction of contractual derogations to the labor law, but in both cases it proved unsuccessful. In 2010 it tried to circumvent article no. 18 by allowing employers to stipulate open-ended contracts with a provision to move the settlement of labor disputes (including those about dismissals) from labor courts – which decide according to the law – to arbitration boards, which instead decide according to equity principles. The government eventually withdrew this provision, after the President of the Republic deemed that it might be unconstitutional. In September 2011 a new attempt was made with Law 148/2011. Its article no. 8 provided that plant- and local-level collective agreements had the possibility to derogate to labor law, thereby including the norms concerning individual layoffs. This kind of collective agreements had to be signed by the most representative unions at the national or local level, a condition that could give rise to a massive number of lawsuits from national union representative questioning the representativeness of local signers. For this reason, the labor market provisions of Law 148/2011 was considered not sufficient by the European Commission, the European Central Bank and the International Monetary Fund, that withdrew their support to the Berlusconi government and made pressure for the instatement of a new "technical" government. Berlusconi resigned in November 2011, when the former European Commissioner Mario Monti became the new Prime Minister, with Elsa Fornero as Labor Minister.

Law 92/2012 eventually succeeded in revising article no. 18 of Law 300/1970. In cases of layoffs motivated under a disciplinary reason that a labor court rules illegitimate, reinstatement is possible only if the judge deems that the supposed *just cause* or *justified subjective reason* simply did not exist, or that the relevant collective agreement decided to punish in a different way. Moreover, the dismissed worker is entitled of a compensation ranging from five to twelve monthly salaries, on top of all foregone social security contributions. Instead, in the other cases in which a disciplinary layoff is judged illegitimate, the dismissed workers are only entitled to a monetary compensation ranging from twelve to twenty-four monthly salaries. For layoffs motivated by an economic reason, instead, reinstatement is possible only if no *justified objective reason* actually existed; in those cases, laid-off workers are also entitled of a monetary compensation ranging from five to twelve monthly salaries. In all the remaining situations of unlawful

economic dismissals, workers are only entitled to a monetary compensation ranging from twelve to twenty-four monthly salaries. No severance pay – to be understood as firing cost to be paid to the dismissed worker in case of a lawful dismissal – has been introduced.

The Fornero reform, hence, has changed the compensation scheme for unlawful dismissals for subset only of firms, namely for those employing more than fifteen workers, leaving regulations unchanged for the others. In particular, in firms above the fifteen-employee threshold it i) deprived the workers of the option to choose between reinstatement and monetary reparation; ii) limited the room for reinstatement to a list of well-defined cases; iii) reduced the amount of total compensation[5]; iv) reduced uncertainty about the duration and expenses of litigations. This provision has hence generated a quasi-experimental condition that can be exploited to identify the effects of EPL changes through a conditional difference-in-differences model the specification of which is described in next section.

3. Data, sample selection and specification issues

The analysis was carried out using the Italian Labour Force Survey (LFS) pooled cross-sectional data for the period that goes from the 1Q of 2011 until the 3Q of 2014 (included), excluding though the 3Q of 2012 covering the months prior to and after the Fornero Law was enacted. Setting the 1Q of 2011 as the starting point of our analysis is determined by a non-comparability with previous rounds of LFS which adopt a different classification of economic activity – namely ATECO 2002 – and occupation – namely CP2001. Building time-consistent definitions of sectors and occupations would require a level of aggregation not compatible with the related literature on skill mismatch. On the other hand, the ending point of our study is determined by the introduction of a new law, the so called "Jobs Act", during the 4Q of 2014.

Sample selection implies then a potential trade-off between the capability to control for unobserved heterogeneity, and the risk of contamination between treated and control units. Ideally, indeed – in order to avoid that unobserved firm-level components that have an effect on the quality of matches and that may be correlated with the reform, could introduce an estimate bias – one should narrow the sample as much as possible around the fifteen-employee threshold. In terms of the Italian LFS data, this means retaining only workers from firms sized between eleven and nineteen employees. However, this strategy is prone to the risk of contamination. Imagine for instance that firm *j* with 15 workers before the reform grows to 16 exactly because EPL above the threshold is now less binding[6]. After the reform firm *j* contributes to the match quality observed above the threshold with all of its employees, and not only with its new hire. As in LFS data we cannot identify flows but only stocks, when computing above-threshold match quality we would mistakenly include workers whose match quality was determined below the

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[5] Anecdotal evidence from Milan labor court suggests that, on top of foregone wages, workers were able to get twenty-four monthly salaries.
[6] This possibility, although small, is well documented by Garibaldi et al. (2004).

threshold and before the reform. The opposite case – i.e. of firms falling from above to below the threshold across the introduction of the reform – can emerge as a simple consequence of the ongoing economic crisis. To prevent contamination, we should hence instead *avoid* to include firms too close to the reform threshold. In terms of the LFS classifications, we should compare firms in the 11-15 bracket (the control group), with those in the 20-49 one (treated group). Since the LFS is quite abundant of both firm- and worker-level observables, we deem the risk of omitted variable bias a minor one, and proceed for the second sample-selection option. After having further restricted our sample to the private nonagricultural sector, we remain with a final sample of 81,130 open-ended workers.

In order to identify the impact of the reform, we apply a difference-in-differences strategy according to the following specification:

$$Y_{ijkt} = \beta_0 + \beta_1 TREAT + \beta_2 POST + \beta_3 TREAT \times POST + \beta_4 X_{ijkt} + \gamma_t + \delta_t + \varepsilon_{ijkt} \quad [1]$$

An individual *i* is considered as well-matched if her educational attainment at time *t* (measured in quarters) is equal to the median attainment of all employees within the same economic activity *j* and occupation *k* at the same point in time[7]. In specific, in terms of categorization of economic activity we have used the five broad categories of the Italian ATECO2007[8], i.e. manufacturing, construction, trade and other services while dropping out the agricultural sector; whereas in relation to the occupation categories we have applied the national statistical office's one-digit CP2011 classification. Our baseline specification includes a wide range of controls X_{ijkt} accounting for individual demographic (sex, age, education, citizenship, region of residence, marital status and household type) and job (sector of economic activity, occupation type, share of temporary workers and full-time workers within the same sector and occupation) characteristics as major determinants of the level of educational (mis)match. The model is then saturated with year- (γ_t) and quarter-level (δ_t) fixed effects. Regional, sector, occupation, year and quarter fixed effects aim at controlling for the business cycle that may have differently affected firms below and above the threshold. The remaining demographic characteristics control for the labor supply composition that, again, may vary in small and medium-size firms.

In addition to this specification – and in order to test whether companies required a certain amount of time to be informed about reform's legal provisions before taking any action, or simply to recognize that workers' turnover is not immediate due to legal constraints – we also introduce a second specification that separates the effects of the reform during the first year of its enactment and separately during its second year:

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[7] Lazear (2009) suggests that combining sectors and occupations is the best strategy to capture the job skill mix.
[8] ATECO 2007 is the Italian version of NACE Rev. 2.

$$Y_{ijkt} = \beta_0 + \beta_1 TREAT + \beta_2 POST1 + \beta_3 POST2 + \beta_4 TREAT \times POST1 + \beta_5 TREAT \times POST2 + \beta_6 X_{ijkt} + \gamma_t + \delta_t + \varepsilon_{ijkt} \qquad [2]$$

where *POST*1 = 1 if we are during the first year of implementation of the reform (i.e. from 2012:Q4 to 2013:Q3) and *POST*2 = 1 if during the second (i.e. from 2013:Q4 to 2014:Q3). This specification still allows to identify separately year and quarter fixed effects. In our analyses, the outcome of both equations is a dummy equal to one if the individual is well-matched, and zero vice versa, therefore all regressions rely on a logit method.

Identification of casual effects through the above-described approach relies on the assumption that the trends of the dependent variable are parallel between treated and untreated units. In order to test this hypothesis, and following Heckman and Hotz (1989), we have regressed the quarterly variation in the number of good matches recorded in cells defined by sector, occupation and class of firm size during the pre-treatment period only, on a dummy variable taking the value of one for firms above the threshold, possibly complemented with year and quarter fixed effects. In symbols:

$$\Delta Y_{jkst} = \alpha + \beta TREAT_{jkst} + \gamma_t + \delta_t + \mu_{jkst}$$

If pre-reform trends are actually parallel, $\hat{\beta}$ should not be statistically different from zero; Table 1 shows that this actually the case. Figure 1 then plots the share of good matches in small (11-15) and large (20-49) firms over the period under scrutiny. The evidence further supports the parallel trend assumption for the pre-reform period, and puts forward an overall slight gap in favor of larger firms. This gap seems actually to widen after the introduction of the reform.

4. Results

4.1 Baseline model

The results obtained with both models [1] and [2] confirm the hypothesis that deregulation has contributed to improve the quality of matches in the Italian labor market[9]. In fact, as the Table 2 shows, in overall the effect of the Fornero law was significant and it increased by 9.5% the odds of being matched in companies affected by the reform. Consistently with our expectations, the impact of this reform was not immediate and over the first year since the law's enactment, no statistically significant change is visible. It is only during its second year of application that the entire effect was exerted with odds of being matched raised by almost 16%; this is consistent with the idea that workers' turnover – although fast – is not immediate.

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[9] The estimated coefficients for control variables in all specifications are available upon request.

4.2 Robustness

A series of robustness checks further supports our findings (Table 3). First, at the price of introducing some contamination between treated and untreated units, we focus upon firms sized around the reform threshold, i.e. on those with 11-15 employees as the control group (as above) compared to those with 16-19 employees as the new treated group. While the overall effect persists but becomes non-significant at conventional levels, once we separate first- and second-year effects our baseline findings are confirmed, with the reform improving the odds of a good match by 18.5% during its second year of implementation (Panel A). Contamination may nonetheless occur also at the worker level if, for instance, treated firms grow by poaching good matches to untreated units. This a likely situation within tight labor markets. To circumvent this possibility we have re-estimated our model within the ten (out of twenty) Italian regions where the level of over-education is highest. The rationale is that we do not expect the supply of good matches to be rationed within those regions. Again, results are robust (Panel B). Another source of potential bias is measurement error. As widely described in the introduction, there exist a lively ongoing debate on how skill or even educational mismatch should be measured. This means that any choice is potentially prone to criticism. As a robustness, we have hence tried to redefine the sector- and occupation-specific educational reference point in terms of the mode (instead of the median) of the distribution of educational attainment. Panel C proves that results are not affected by this change. Eventually, we consider the possibility that – having included in the sample workers aged fifteen or more – some workers may still be at school. After restricting the sample to individuals aged no less than twenty-five, again, results are confirmed (Panel D).

4.3 Extensions

Having proved the robustness of our results, we can now proceed to study them into more depth. A major issue is to understand whether educational mismatch has improved through a reduction of over-education, of under-education, or both. In Table 4, Panel A, we transform our baseline model into a multinomial one, with three possible outcomes: good matches (the reference outcome), over-education or under-education. Estimates clearly show that the main driver has been a reduction in under-education. These results should, in our view, be read jointly with those in Panel B, where we split the sample between workers aged until 34, and those aged 35 or more. The effect of the reform is fully carried by the latter. This means that the quality of matches has improved thanks to a reduction in under-education that occurred mainly among mature and old-age workers. A likely interpretation of this evidence is that across the reform period the quality of matches has improved through the dismissal of undereducated older workers and the substitution with younger better matches; a comparable reading of the Italian labor market is proposed

also in Berton et al. (forthcoming). Panels C and D eventually suggest that the bulk of the effect was carried out by the service sector and in northern regions, i.e. where mismatch was highest before the reform.

5. Discussion and concluding remarks

This paper takes advantage of an exogenous variation of employment protection legislation occurred in Italy in 2012 to test the theoretical prediction that workers' turnover improves the quality of job matches. Using a difference-in-differences approach and measuring the quality of matches in terms of dispersion around sector- and occupation-specific median educational attainment, we find indeed that the reform under scrutiny is responsible for having improved the probability of a good match by 9.5%. Consistently with the idea that, due to legal constraints, the reform took a minimum of time before it could affect workers' turnover, we find that the bulk of the effect occurred during the second year of implementation of the reform. Moreover, we also find evidence that this effect took place mainly through a reduction of under-education among mature and old-age workers, and that where under-education was highest before the reform – i.e. in Northern regions and in the service sector – the effect has been stronger. Results survive to a number of robustness checks.

This piece of evidence goes hence in the direction to support the trigger of Rogerson's theory of workers reallocation, namely that turnover improves the quality of matches. Nonetheless, this does not imply that the full theory is supported, ad in particular that this process leads to more productivity, competitiveness and then employment. The evidence surveyed by Cazes (2013), Noelke (2015) and the Oecd (2004) does not find any clear relationship between EPL regimes and employment levels. This means that the virtuous circle portrayed in Rogerson's theory gets jammed somewhere. A deeper understanding of the scope of our results may help to solve this puzzle. What we find is that firms took advantage of the reduced EPL regime to improve the fit of their employees in terms of the human capital they get *off* the job. However, EPL is not neutral to the amount and the quality of skills one accrues *on* the job: Bassanini et al. (2007) and Berton and Garibaldi (2012) suggest that more flexible regimes create an incentive to reduce the investment in workplace training. In addition, Acemoglu and Pischke (1999), Berton et al. (2016) and Lazear (2009) argue that they also change the employed workers' mix of skills from specific to general. We may hence have two countervailing effects of EPL reforms: turnover improves the allocation of off-the-job skills, which is likely to have a positive effect upon productivity. Nonetheless, turnover also reduce the incentive to invest in workplace training and in specific skills, what instead may reduce productivity. Unfortunately, the literature relating EPL regimes and productivity is still rather scarce: Dolado and Stucchi (2008), for instance, find that temporary workers are less productive due a reduced work effort. Moreover, such literature does not consider general equilibrium effects, and namely that structural EPL reforms may lead to a substitution of temporary workers with – even if less protected – open-ended workers, and hence

to an average higher productivity. The theories that informed the major labor market changes during the last decades appear hence empirically still ill grounded with respect to many of their key results.

Tables and Figures

FIGURE 1 – Share of good matches in treated and untreated units
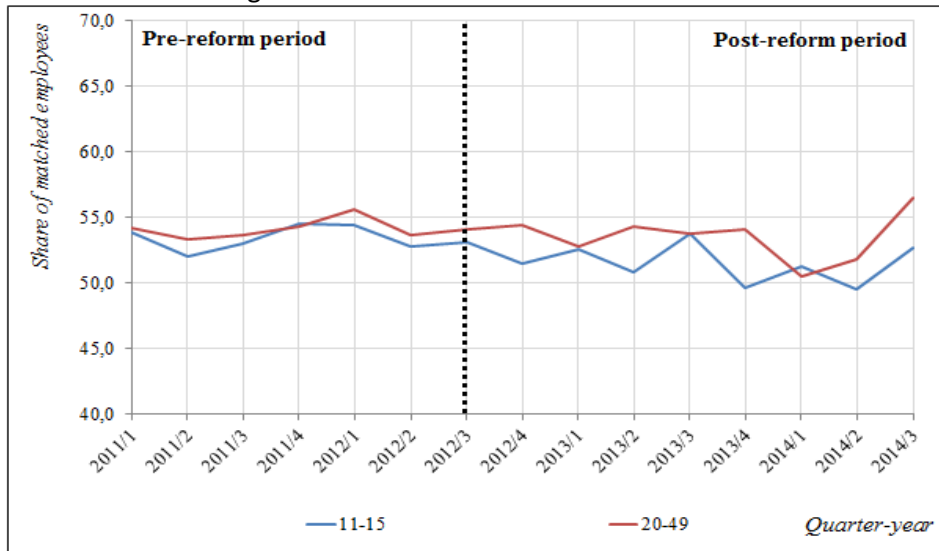
Source: own computations on LFS data.

TABLE 1 – Test of the parallel trend assumption

	RRR	p-value	Observations
No controls	1.016	0.535	40,762
Year and quarter fixed effects	1.016	0.536	

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

TABLE 2 – Baseline results

	RRR	p-value	Observations
Firms 11-15/20-49, overall effect	1.095**	0.021	81,130
Firms 11-15/20-49, first four quarters	1.040	0.386	
Firms 11-15/20-49, following four quarters	1.157***	0.005	

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

TABLE 3 – Robustness checks

	RRR	p-value	Observations
Panel A: unobserved heterogeneity			
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	
Panel B: contamination at worker level			
Firms 11-15/20-49, overall effect	1.120**	0.011	
Firms 11-15/20-49, first four quarters	1.041	0.431	59,692
Firms 11-15/20-49, following four quarters	1.207***	0.001	
Panel C: measurement error			
Firms 11-15/20-49, overall effect	1.099**	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	
Panel D: uncompleted education			
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	

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

TABLE 4 – Extensions

	RRR	p-value	Observations
Panel A: over- vs. under-education			
Over-education, overall effect	0.930	0.168	
Under-education, overall effect	0.894**	0.014	
Over-education, first four quarters	0.968	0.582	81,130
Over-education, following four quarters	0.892*	0.096	
Under-education, first four quarters	0.916*	0.089	
Under-education, following four quarters	0.872**	0.021	
Panel B: age class			
15-34 years old			
Firms 11-15/20-49, overall effect	0.972	0.754	
Firms 11-15/20-49, first four quarters	0.856	0.146	18,542
Firms 11-15/20-49, following four quarters	1.112	0.385	
35 or more			
Firms 11-15/20-49, overall effect	1.108**	0.023	
Firms 11-15/20-49, first four quarters	1.083	0.120	62,588
Firms 11-15/20-49, following four quarters	1.134**	0,030	
Panel C: sectors			
Manufacture — First four quarters	0.962	0.613	30,436
Manufacture — Following four quarters	0.990	0.906	
Constructions — First four quarters	1.040	0.807	8,323
Constructions — Following four quarters	1.039	0.844	
Trade — First four quarters	0.995	0.970	13,283
Trade — Following four quarters	1.207	0.182	
Other services — First four quarters	1.177**	0.033	29,087
Other services — Following four quarters	1.300***	0.003	
Panel D: geography			
North — Overall effect	1.122**	0.024	48,921
North — First four quarters	1.050	0.409	
North — Following four quarters	1.202***	0.005	
Center — Overall effect	0.975	0.777	15,352
Center — First four quarters	0.951	0.625	
Center — Following four quarters	1.000	0.993	
South — Overall effect	1.158*	0.099	16,857
South — First four quarters	1.155	0.129	
South — Following four quarters	1.162	0.233	

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

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