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ABSTRACT

Employment Protection Legislation and Mismatch: Evidence from a Reform*

Liberalization of temporary contracts has been a hallmark of labor market reforms during the last decades. More recently, factors like the sovereign debt crisis pushed the most indebted countries to unprecedented reductions of employment protection legislation (EPL) also on open-ended contracts. These policies are justified under the assumption that EPL harms the allocation of workers on the jobs where they are most productive. How EPL affects the quality of job matches is nonetheless an underexplored issue. In this paper, we provide new evidence that exploits exactly one of these recent reforms, the so-called Fornero Law, introduced in Italy in 2012 in the background of austerity reforms. Results show that good matches have increased. Further, the reduction in EP favored labor reallocation. Eventually, it was also followed by an increase in productivity, albeit small. While the results are consistent with the economic theory that informed deregulation, we highlight caveats and limitations.

JEL Classification: J24, J63

Keywords: employment protection legislation, turnover, mismatch, productivity, Fornero Law, difference-in-differences

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1. Introduction

During the nineties labor market deregulation emerged internationally as a key policy recommendation (Imf, 1999; Oecd, 1994a), partly owing to the good economic performance of some low-protection countries (Elmsekov et al. 1998; Grubb and Wells 1993; Heckman and Pagés 2000; Lazear 1990). In most Oecd countries, this piece of advice has been for long understood in terms of *partial* labor market reforms and of liberalization of temporary contracts (Boeri, 2010). In more recent years, the sovereign debt crisis, and the pressure on public finance that followed, have created the conditions to proceed to a further weakening of Employment Protection Legislations (EPL), in particular for workers with open-ended contracts (Hastings and Heyes, 2016). These reforms are generally justified under the economic argument of *inefficient retention* (Bassanini and Ernst, 2002; Bierhanzl, 2005): high firing costs ought to be reduced as they are likely to prevent firms from separating from insufficiently productive matches. At the same time, low workers' turnover – a consequence of high EPL – would harm the ability of a labor market to match efficiently the right workers to the right jobs (Rogerson, 1987). The implication - it is argued - is that average productivity falls and the economic system becomes less competitive. In turn, poor competitiveness weakens economic growth and, ultimately, employment.

This article aims at testing whether changes in EPL may actually be able to trigger a key economic mechanism that supposedly enhance a country's economic performance, i.e. to improve the quality of job matches through a faster workers' turnover, and to study its implications in terms of productivity. To do so, we provide econometric evidence based on a difference-in-differences (DD) identification strategy and Italian micro-data on workers and firms. The DD setup is offered by a recent Italian reform, the so-called "Fornero Law" (after the name of the Italian Labor Minister under the Monti government), which changed EPL for open-ended contracts differently for companies of different sizes. While much of the literature has focused on the employment and welfare effects of changes in the legal dispositions for temporary contracts (e.g., Berton et al., 2012; Barbieri et al., 2016; Cappellari et al. 2012), less is known about reforms affecting mandated job protection provisions for open-ended contracts, and very little about their impact on the quality of job matches. Our paper intends to contribute to fill such literature gap.

Despite providing weaker external validity, a case study based on the empirical strategy we discuss below can offer complementary insights with respect to comparative analyses, and has a number of distinctive strengths. First, due to the unparalleled identification conditions that its design provides with, the Italian 2012 reform allows to grant a high internal validity of results relying upon microdata. Second, case studies are less vulnerable from confounding factors related to international differences in labor market institutions, including welfare programs and their functional equivalents, which can be difficult to control for in cross-country empirical studies. Our DD estimates hold constant any institutional specificity, and are also robust to any institutional changes that have broadly interested all workers and firms in the country. Third, Italy – along with Spain, where an analogous reform has been introduced in the same year – represents an unprecedented example of deregulation of open-ended contracts (Meardi, 2014; Picot and Tassinari, 2017). Fourth, Italy is also one of the most indebted countries of the Euro area (Perez and Rhodes, 2014) and a learning example of labor market reforms under austerity (Picot and Tassinari, 2017; Sacchi, 2015).

This paper proceeds as follows. In section 2, we provide an account of the reform under scrutiny, highlighting why its design is particularly apt to measure the impact of EPL reduction. In section 3, we first discuss the theoretical background of labor market deregulation and of its potential implications in terms of job match quality and human capital accumulation. We then provide a summary report of the related measurement issues, and of how we operationalize the quality of the match between firms and workers. We also identify the literature gap that we aim to fill with the present contribution and introduce our hypotheses. In section 4, we present the data and the empirical model; results are discussed in section 5. Section 6 describes the results about our mediating hypothesis – that the reform enhanced worker turnover. Sections 7 and 8 respectively discuss the results in terms of firm productivity, and draw some tentative concluding remarks.

2. Institutional background: the Fornero reform

Although this view is not unchallenged (Contini and Revelli, 1992; Contini and Trivellato, 2005; Del Conte et al., 2004), Italy has for long represented the epitome of labor market rigidity (Oecd, 1994b). This argument largely relied upon the Italian regulations on individual layoffs that have been in force since 1970. According to Laws 604/1966 and 300/1970, an employer is legitimated to dismiss a worker if a *just cause* exists (damage of equipment, fight or violence) 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 it impossible the continuation of the employment relationship. The consequences of unlawful dismissals depend then upon the firm’s size. In those employing more than fifteen workers, an illegitimate layoff is deprived of any legal effect, gives the dismissed worker the option to be reinstated to her former position, and leads to the compensation of all foregone salaries and social security contributions since the layoff date. In firms below the 15-employee threshold, 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.[1]

With the installment of the Monti government in November 2011, the new Labor Minister Elsa Fornero quickly succeeded to reduce employment protection in firms above the fifteen-employee threshold. Namely, Law 92/2012 – in force since July 18th – rules that in cases of layoffs motivated under a disciplinary

[1] See Berton et al. (2012) and Sacchi (2015) for a full account of labor market reforms in Italy since the late XIX century.

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 according to the relevant collective agreement, it should have been punished otherwise. 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 to 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. Hence, in firms above the fifteen-employee threshold, the Fornero reform (*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; (*iv*) reduced uncertainty about the duration and expenses of litigations (Ichino and Pinotti, 2012).

Two features of the reform make it a particularly apt case to study empirically the implications of EPL reforms at the micro level. First, the compensation scheme for unlawful dismissals has been changed for a subset only of firms. This has generated a quasi-experimental situation with treated and control units that can be studied with a DD approach. In other words, the reform provides variation both across firms and time that we exploit for identification. In addition, Law 92/2012 is a clear example of a top-down reform, with little involvement of local social partners and under the co-ordination of the EU within the realm of austerity reforms (Picot and Tassinari, 2017; Sacchi, 2015). This further contributes to make Law 92/2012 largely exogenous to the Italian pre-existing dynamics, preventing issues of reverse causality.

3. Skill allocation and skill development: theories and measurement issues

3.1 Theoretical background

Labor markets are characterized by continuous flows of workers in and out of unemployment, and from one job to another. This process is generally labelled *workers' turnover* and it is well-established in both the theoretical (Bentolila and Bertola, 1990; Bertola, 1990) and empirical literature (Cazes, 2013; Noelke, 2011; Oecd, 2004) that it is negatively affected by EPL. In this perspective, reducing EPL in order to increase workers' turnover may be desirable inasmuch as a higher worker reallocation should also entail that workers move more easily to the positions where their skills are more productive (Rogerson, 1987).

This view, however, only considers how EPL affects the *allocation* of given skills upon jobs, but neglects that EPL also influences skill *development*, i.e. the fact that skill levels and composition change and evolve over time (also) as a consequence of the employment protection regime they are subject to. Acemoglu and Pischke (1998; 1999) are among a number of authors suggesting that – when employment protection is higher, the allocation of workers to jobs is less efficient, and employment relationships last longer – workers and firms invest more in specific skills. Within this standard economic view, specific knowledge emerges from inefficient labor markets, in turn related to the existence of matching frictions, of which EPL provisions are a leading example.

On the contrary, institutional political economy highlights that once labor market imperfections are embedded into their "dense network of political and socioeconomic institutions" (Busemayer and Trampusch, 2012, p. 7), EPL - instead of being a given constraint - emerges as an endogenous feature of real labor markets. Following the transaction cost theory by Williamson (1981), Iversen and Soskice (2001) and, more in general, the literature on Varieties of Capitalisms (e.g. Estevez-Abe et al., 2001; Hall and Soskice, 2001; Thelen, 2004) suggest that the degree of EPL that characterizes a labor market follows – jointly with its functional complements like unions, income-maintenance schemes, internal labor markets, but also technological paradigms (Harcourt and Wood, 2007) – from an efficient process in which employers and workers cooperate to raise the system's productivity. Within this literature – and compared to the two ideal-types of Coordinated (CMEs) and Liberal Market Economies (LMEs) – Italy emerges, along with France and other Southern European countries, as a "mixed case", sharing some of the features of CMEs (Hall and Soskice, 2001) and, in particular, a high degree of EPL (Estevez-Abe, 2008). Alternatives to the VoC view – although suggesting that the relationship between strictness of EPL and skill specificity may go the other way around (Emmenegger, 2009; Goldthorpe, 2000) or that, far from coordination, EPL regimes emerge from different power relationships between political groups (Saint-Paul, 2000; Streeck, 2012) – still maintain that not only EPL affects skill allocation and development, but is also in turn subject to preferences that depend on workers' skills and on production technologies.

This essential literature overview suggests therefore that the relationships between EPL and skills is significative, complex, and circular. It is influenced by labor market transitions and entails at least three steps: first, workers' turnover – as directly affected by EPL – determines the allocation of existing workers' skills upon jobs. Second, once workers and jobs are matched, those given skills develop in a way that depends on the expected duration of the employment relationship, which, again, is a consequence (also) of EPL. Third, skills – because of their evolution during the employment relationship – shape preferences over EPL. While the empirical analysis of this article is essentially directed at the first one of these steps, we will also provide discussion and analyses touching, albeit more indirectly, upon the second step of skill development and the overall productivity effects of EPL.

So, "*how does EPL affect the allocation of existing skills upon jobs?"* and "*Does it actually improve the quality of matches as suggested by economic theory?*" are questions that pose a non-trivial measurement issue: we need to study the relationship between EPL and job-match quality in a way that prevents the issue of circular causality put forward above. This is exactly the problem solved by the design of the Fornero reform.

3.2 Measurement issues

Despite the central role for recent labor market reforms of the relationship between EPL and human capital, related empirical evidence has mostly focused on the "reduced form" of the underlying theoretical model, trying to assess directly the impact of labor market deregulation on employment and unemployment (Cazes, 2013; Noelke, 2015; Oecd, 2004). The reason has been fourfold. First, right or wrong, knowing about the consequences of labor market institutions on employment performance was regarded 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, as already discussed above, the causality of this relationship is circular; hence, credible identification of the effect of EPL on match quality hinges on the availability 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). The European Commission's communications such as "New skills for new jobs" (EC, 2008), "Agenda for new skills and jobs" (EC, 2010) and the latest "A New skills agenda for Europe" (EC, 2016) all witness how the 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 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 survey-based micro data, the availability of Longitudinal Matched Employer-Employee Databases and, most notably, to the realization of dedicated surveys like the Oecd's Survey of Adult Skills (Oecd, 2016).

Instead, more concerns exist 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 refers instead to the distance between skill demand and the amount and type of human capital possessed by the workers (skill supply). 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 feature strengths and weaknesses. 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 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 the most widely used metrics of skill mismatch.

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), and fails to account for skill accumulation and depreciation occurring on the job and after entry in the labor market. For these reasons, efforts have recently been made in order to distinguish the educational dimension in strict sense from other dimensions. 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 (Pellizzari and Fichen, 2013). Approaches more strictly related to the dynamics of employment relationships are those proposed by Ghignoni (2001) – who considers sector-specific individual experience – and by Estevez-Abe et al. (2001), who use job turnover.

The fourth reason why the empirical relationship between employment protection and match quality has been underexplored, is that such relationship is prone to simultaneity and reverse causality issues. As explained above, unless the various branches of the circular relation are credibly isolated, this circularity is likely to cause biases in the estimated impact of EPL in any empirical model regressing match quality on aggregate EPL measures or on indicators of job (in)security at the individual level. This is exactly where our paper mainly contributes to the debate.

3.3 Hypotheses and their operationalization

We are now in a position to state our main empirical hypothesis: ***HP1****: the Fornero reform has improved the probability that a worker in open-ended positions is well-matched to her job*. When testing HP1, the unit of observation is the individual worker, and our main outcome variable is match quality. Three reasons led us to assess match quality in terms of educational attainment. First, education is a certified formal skill that over an individual's career varies only by natural decay, and is hence particularly apt to capture skill allocation with no contamination in terms of skill development. Second, education is widely used and allows international comparisons. Third, alternatives like PIAAC lack a proper longitudinal dimension, hence compromising the key possibility to use the Fornero reform as an identification device.

From the metrics standpoint, we have chosen the objective approach, by defining a well-matched worker as one whose educational attainment is equal to the median educational attainment of her reference professional group. The objective approach circumvent the data availability problem that affects the normative approach, and recall and framing biases related to the subjective one. Similarly to Iversen and Soskice (2001) and Lazear (2009), then, we define the reference professional groups in terms of sectors and occupations. Namely, sectors are identified by the broad categories of the Italian ATECO2007 classification (manufacturing, construction, trade and other services, while agriculture is excluded from the analysis) whereas for the occupations we have used the categories of the national statistical office's one-digit CP2011 classification.[2] A word of caution is in order to prevent possible misunderstandings of our statement. We are not testing the hypothesis that the match quality of a given worker improves after the Fornero reform; we are aware that a worker may even lose her job after the reform. Instead, we want to check whether the *average* match quality has improved, and we do it using individual data.

The proper theoretical argument says that workers' turnover – and not the Fornero reform – is beneficial to workers' allocation. For HP1 to be the right empirical counterpart of allocation theory, we need therefore to test a second statement, i.e. our *mediating hypothesis*: ***HP2**: the Fornero reform has improved workers' turnover*. We operationalize workers' turnover at the firm level in terms of (i) the share of separations and associations over total employment, (ii) the share of associations over total employment, (iii) the share of separations over total employment, also (iv) purged by retirements.

In the economic policy perspective, knowing that a higher turnover leads to better matches would not be of great interest unless this in turn enhances the overall system's competitiveness, i.e. its productivity. However, as argued above, the final effect of EPL upon productivity does not only depend upon skills *allocation*, but also on their *development*. Both are influenced by EPL, and the overall balance cannot be decided a priori based on theoretical arguments. No clear hypothesis can hence be put forward about the impact of the Fornero reform upon productivity, irrespective on whether or not HP1 and HP2 hold. Nonetheless, we study empirically the relationship between the Fornero reform and productivity, and cast the related results in the light of the evidence about HP1 and HP2.

[2] ATECO 2007 is the Italian version of NACE Rev. 2; CP2011 is very similar to ISCO08.

In the next section, we begin by describing the data used for testing our main hypothesis, HP1. Testing HP2 requires a different data source that will be described in section six. Our exploration of the impact of the Fornero reform on productivity is based on yet another data source, described in section seven.

4. Data, sample selection and specification issues

The effect of the reform on match quality is here carried out using individual-level quarterly data from the Italian Labour Force Survey (LFS). We pool the various cross-sectional surveys for the period that goes from the 1Q of 2011 until the 3Q of 2014 (included), excluding though the 3Q of 2012 in which 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 activities and of occupations. 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 a potential trade-off between the capability to control for unobserved heterogeneity and the risk of contamination between treated and control units. Ideally, one would narrow the sample as much as possible around the fifteen-employee threshold. Doing so should reduce the concern that unobserved firm-level components, which may have an effect on the quality of matches and be correlated with the reform, introduces an estimate bias in the analysis. In the Italian LFS data, firm size is only available as an (ordered) categorical variable, and this poses limits to the way we can select our sample. In practice, one possibility is that we retain only workers from firms sized between eleven and nineteen employees, and compare workers in firms with 11-15 employees (below the threshold) and workers in firms with 16-19 employees (above the threshold). 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 (Garibaldi et al., 2004; Torrini, 2008). 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 threshold and before the reform. To prevent contamination, we should hence instead *avoid* to include firms too close to the reform threshold. In terms of the available LFS classification for firm size, a second possibility is to 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, for most of the analysis, we proceed with the second sample-selection option (tough we conduct robustness checks for this choice). 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 – and hence $Y_{ijkt} = 1$ – 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. 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. These controls are allowed to be time-varying. 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 recognize that workers' turnover is not immediate due to search frictions – 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:

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

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. Both models are estimated with the logit method.

Identification of causal effects through the above-described approach relies on the assumption that, had the reform not been introduced – the trends of the dependent variable would be parallel between treated and untreated units during the reform period. This hypothesis cannot of course be tested, but – following Heckman and Hotz (1989) – we can get some indications by regressing 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, 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 is 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 idea that trends were parallel in 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.

5. Results

5.1 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. Hence, HP1 is supported by our data. In fact, Table 2 shows that, as an effect of the Fornero law, there was a statistically significant increase in the odds of being well matched: the relative risk ratio (RRR) increased by 9.5% in companies affected by the reform. Consistently with our expectations, the impact of this reform was not immediate: 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 well-matched raised by almost 16%. This is consistent with the idea that workers' turnover – although relatively fast – is not immediate.

5.2 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 (but with the advantage of reducing the room for unobserved heterogeneity), 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). Second, 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). Third, another source of potential bias is measurement error. As widely described in section two, there exists a lively ongoing debate on how 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. Finally, 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).

5.3 Extensions

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 (sometimes) the substitution with younger better matches. Notice that, by virtue of the lower EPL for open-ended contracts, firms may in principle start reducing their use of temporary contracts, with an ambiguous effect on the mismatch of a firm's total workforce. We believe this substitution effect is, at best, second order, unable to overturn the effect on open-ended contracts, which represents the vast majority of a firm's workforce. Nevertheless, we control for the share of temporary contracts in all our regressions. Finally, Panels C and D eventually suggest that the bulk of the effect was carried out by the service sector and in northern regions.[3]

6. The effect of the reform on worker turnover

As emphasized by the existing literature and our earlier discussion, a major channel driving the effect of the Fornero Law on improved qualification mismatch lies in the alleged increase in worker reallocation. While

[3] We will have more to say on sectoral differences when discussing the productivity effects. As for the geographical differences, southern regions were traditionally characterized by intense turnover even before the reform, related to high natality/mortality of small firms, and to the diffusion of semi-legal personnel practices (Contini and Trivellato, 2005). This might partly explain the absence of any detectable effect of the reform.

there is some empirical evidence suggesting that EPL may curb firms' firing decisions, thereby also negatively affecting hiring plans and overall worker turnover more generally (Schivardi and Torrini, 2008), there is less work directly testing that reforms reducing EPL have indeed produced such effects. For instance, Kugler and Pica (2008) study the effect of an Italian reform of 1990 that *increased* EPL for *small* firms, while leaving it unchanged for larger firms. They show that worker turnover was negatively affected by this earlier reform. In this section, we aim at providing a preliminary assessment in the case of the Fornero Law that, to the best of our knowledge, has never been attempted before.

Such an empirical investigation requires data that the LFS used earlier cannot offer. Specifically, we need to compare the hiring and firing rates of firms below and above the threshold, before and after the Fornero Law. A *worker* level dataset such as the LFS does not allow us to compute such rates. Hence, we need to resort to alternative *firm*-level data. The Employer and Employee Survey (RIL) conducted by INAPP is ideal for the objective of this section. RIL is a nationally representative sample of firms with *any* number of employees, including the very small ones, operating in the non-agricultural private sector; the data that we use has been collected for the years 2010 and 2014. The surveys collect a rich set of information on personnel, organization, firm's recruiting activities and other workplace characteristics. In particular, the RIL asks each firm in the sample about (i) the number of hires during previous year and (ii) the number of separations, distinguished by reason (quits, lay-offs, retirement, and other reasons).

For each firm *j* in the data, and for each time period *t*, we can then measure worker turnover as the sum of accessions plus separations during the relevant survey year, divided by the firm's total number of employees at the beginning of the year. We can also compute a firm's accession rate and separation rates as, respectively, the fraction of accessions and separations on firm's total employees. We then run OLS regressions similar to equation (1), where essentially we compare the turnover rates for firms below and above the threshold, before and after the 2012 reform. The results are collected in table 5. In all specifications, we control for regional dummies, 1-digit sectors, a cubic polynomial in firm size, and a dummy for incorporated business.

The results are broadly consistent with the expectation that the Fornero Law induced treated firms to increase their turnover rates. The effect is positive in column (1), where we look at an overall turnover rate, and is also positive when we focus, separately, on the hiring (column 2) and the separation margin (column 3), albeit not statistically significant in the former case.

Clearly, there are a number of limitations in such an analysis. To begin with, it is well-known that a firm-level survey is not exempt from measurement errors (e.g., recall bias), and that this might be a concern in particular for the computation of firm-level variables, which require individual-level knowledge on the relevant events (hires and separations). Another limitations lies in our inability to test whether, consistently with the results of the analyses of the previous sections, the impact of the reform only emerges after some time. With only the 2010 RIL surveys to serve as the "before" period and the 2014 survey for the "after" period, we lack the necessary data to distinguish between the immediate and the longer-run effects. Another concern is related to the use of total separations, as in column (3). In fact, some of these refer to (workers') voluntary separations and ought to be excluded. Similarly, since other separations originate from retirement, the analysis would be more convincing if based on firm-level lay-offs only. This is what we pursue in column (4), which shows that the effect is again positive, and statistically significant (at the 10% level). We conclude this section by highlighting that, while our preliminary analysis largely supports HP2, thereby providing further empirical support to the small existing literature suggesting that EPL is detrimental to worker turnover, caution should be exercised in drawing final conclusions, partly reflecting the limitations discussed above.

7. Exploring the productivity effects of the reforms

The evidence produced in the previous sections goes in the direction to support the trigger of Rogerson's theory of workers reallocation, namely that turnover improves the quality of matches between the existing skill pool and jobs. One may expect this reallocation to be beneficial to productivity. Nonetheless, as discussed in section 3, one should distinguish between the productivity impact of shocks or policy interventions that simply alter the economy's allocation of given skills, from the broader effects arising from changes in the accumulation of skills. Hence, while reducing EPL may have a positive effect on productivity via the improved-allocation channel, the overall effect on productivity is not easily established if smaller EPL ultimately entails a deterioration in the economy's ability to accumulate the right amount and type of skills. Not surprisingly, the theoretical discussion on the effects of EPL on productivity is inconclusive, and so is the current state of the empirical literature. In this section we aim at providing some new empirical evidence taking advantage of the natural experiment provided by the Fornero reform. To the best of our knowledge, this is in fact one of the few existing evaluation exercises of the effect of EPL reforms on productivity. We show our results first and, in the final section, we discuss the implications of our findings in light of the theoretical arguments laid out in section 3 and of the limited available literature on the relationship between productivity and EPL.

As before, our analysis hinges on the estimation of an equation similar to equation (1). This time, though, the dependent variable is a measure of firm productivity. As in section 6, the unit of analysis is the firm. The first difficulty in carrying out such an evaluation is related to the need to find firm-level data containing accurate information on firm productivity. Neither the LFS (which is a worker level data) nor RIL (which lacks reliable measures of firm productivity) is suitable for our analysis. On the other hand, the AIDA data, distributed by Bureau Van Dijk, contain detailed information on official annual balance-sheet data for the universe of limited companies, as long as their turnover is over 100,000 Euros (a rather minimal threshold). One noticeable feature of the AIDA data, beyond the accuracy of the relevant indicators of firms' performance, lies in its large sample size: over 900,000 firms are included, covering the period 2006-2014, for a total of almost five million observations.

We have computed the productivity of each firm in any given year in two ways. First, as the log of the ratio between its value added and the number of its employees, thereby providing a measure of labor productivity. Second, as the residual in a OLS regression of the log of value added on the log of the firm's number of employees and the log of a firm fixed assets, what is referred to as total factor productivity (TFP).[4] In all specifications, estimated by OLS, we control for regional dummies, dummies for sector of activity (at 2-digit detail) and year dummies. One caveat of the AIDA data is that it only covers incorporated businesses. However, firms of all size are otherwise included. To make our analysis more directly comparable with the analysis of section three, and in light of the large sample size provided by the original AIDA data, we can afford to restrict the sample to firms with 10-15 employees and firms with 16-49 employees, for a total of over 600,000 observations. Notice, also, that firm size is measured as a continuous variable in AIDA. This allows us to control flexibly for firm size, through a cubic polynomial in the number of employees. Hence, our identification strategy combines the DD set up offered by the reform with a regression discontinuity (RDD) design (Imbens and Lemieux, 2008). In the RDD jargon, firm size is the *running variable*. Here identification is achieved by comparing, before and after the reform, firms just below and firms just above the 15-employee threshold, while allowing the dependent variable (here productivity) to depend flexibly on the running variable.

Table 6 collects the results. The table is organized to show a large number of separate regressions, obtained by estimating an equation similar to (1) on: (*i*) different samples, (*ii*) alternative productivity measures (value added per worker and TFP), and (*iii*) using different estimation methods (OLS or panel firm fixed-effect methods). In total, we run over 40 separate estimations, over the samples described by the rows in column (1). For instance, row 1 shows the effect obtained when considering firms of any size; row 2 instead restricts the sample to firms 10-49, while other rows further exclude firms in the 16-19 segment, or focus on different sectors, or exclude the indicated years. For ease of exposition, we only report the estimates of the coefficient of interest, the interaction $TREAT \times POST$, which gives the productivity impact of the reform.

A cursory look at column (3) of the table shows that the Fornero reform was followed by an increase in labor productivity. The effect is however relatively small in magnitude: when restricting attention to firms in the 10-49 segment, the increase in the *level* of labor productivity is always lower than 5%. The effects are however precisely estimated, also owing to the large sample sizes. The effect are still positive, with only a slightly smaller magnitude and with a similar level of statistical significance (at the 1% level) also in the case

[4] We also reconstructed the capital series using a version of the perpetual inventory method, as in Card et al., (2014). Experimentation with these two alternative measures of capital never produced any relevant differences in the estimated results.

of TFP, suggesting that the results are not driven by differential capital accumulation over the time periods by different firm types.

In columns (5) and (6) we take advantage of the longitudinal dimension of the AIDA data: individual firms can be followed over time thanks to the availability of unique identifiers (the firm's fiscal code). This allows us to add firm fixed effects to the estimated models, and hence rely only on the within-firm variation as a source of identification. Hence, the estimates in these columns also control for firm time-invariant *un*observed heterogeneity, which might be correlated with both match quality and a firm's selection into treatment status. Comparing columns (5) and (6) with corresponding estimates in column (3) and (4) shows that controlling for firm fixed effects results in even smaller productivity effects, but the effect remains positive and statistically significant in all rows (with the only exclusion of row 9, column 5).

We finally wonder whether the results hide some major heterogeneity across sectors of activity. In fact, the effect is still positive and statistically significant in both the manufacturing and the service sector samples. Moreover, the estimates do not in fact reveal a clear pattern of productivity impact across the two sectors. While the productivity impact of the reform is somewhat smaller in the service sector with the OLS methods, the relative impact is reversed in the estimates that control for firm fixed effects. We further elaborate on these sectoral differences in our next, concluding section.

8. Concluding remarks

This paper takes advantage of a recent Italian reform to study the relationship between EPL and the quality of the match between workers and firms. Using a DD approach and measuring the quality of matches in terms of dispersion around sector- and occupation-specific median educational attainment, we find that the reform led to an improvement in the probability of a good match by 9.5%. We also provide evidence that the reduction in EPL favored labor reallocation, a potential mechanism for the improved match quality. Consistently with some time lag for this reallocation to take place, the improved match quality is not visible during the first year after the law's enactment, but only during its second year. We also find evidence that the reduced mismatch took place mainly through a reduction of under-education among mature and old-age workers. Finally, we conducted a preliminary econometric analysis on the effect of the reform on firms' productivity, finding that the reduction in EPL was followed by an increase in productivity, albeit small in magnitude. Our results on mismatch and productivity are in line with Kampelmann and Rycx (2012), who report that higher under-education is detrimental for productivity in their sample of Belgian firms.

Essentially, this paper has provided new empirical evidence consistent with the idea that, when the constraints posed by EPL are relaxed, the given pool of skills and competencies can be more efficiently allocated, skill mismatch reduces, with a beneficial effect on productivity. While the results are broadly speaking in line with the economic theory that informed labor market deregulation during the last decades, it is important to highlight a few final remarks and draw attention to current caveats and limitations.

First, our data only allowed us to look at the quality of the match in terms of education, thereby neglecting the other dimensions of skill mismatch. While it is possible that the quality the match also improved along these other dimensions as a result of the EPL reform, future research should provide more direct evidence that this was indeed the case. On the one hand, we suspect that our finding that mismatch was not much reduced in the manufacturing sector, contrary to what happened in the service sector, while productivity increased in both sectors, provides an indirect indication that the skill reallocation was not confined to formal education, but interested at least partially various skills learned on the job, e.g., industry-specific or at least skills portable within the traditional industrial districts that characterize the Italian economy. On the other hand, the productivity effect of the EPL reform is estimated to be rather modest, suggesting that we should not expect large gains from overall reallocation. This is consistent with the views, expressed by some social scientists, that the Italian labor market was already more flexible, and mobility higher, than traditionally believed (e.g., Contini and Trivellato, 2005).

Second, our data do not allow us to say much about the way the reform affected skill formation. The curbed EPL may reduce the expected duration of the employment relationships. This may have adverse effects on the incentives of workers and firms to invest in specific skills, and be ultimately detrimental to productivity and competitiveness developments, particularly for CMEs, as neatly pointed out by Harcourt and Wood (2007). It is well possible that our productivity estimates are mostly capturing the immediate, one-off positive impact deriving from worker reallocation, but fail to incorporate the more long-run effects deriving from skill formation. If so, future research might detect that a dismantling of EPL provisions is followed by an even smaller, and possibly negative, effect on a country's competitiveness.

Third, we focused on a reform that reduced EPL for workers on open-ended contracts. In principle, the new environment may push firms to substitute temporary with permanent workers (e.g., Schivardi and Torrini, 2008). If so, productivity may arise, at least according to the few empirical papers that find that the share of temporary workers in the firm is associated with a smaller productivity (Dolado and Stucchi, 2008). However, this effect might be countervailed by the fact that permanent workers are, by virtue of the reform, now "less permanent", which may once again instill the negative productivity effects arising from lower accumulation of firm-specific skills. This is yet another issue that might benefit from continuing research using richer data, e.g. matched employer-employee data combining worker flows with firm financial information.

Forth, one may question the effectiveness of EPL reforms when this is not accompanied by complementary reforms of the labor market and of the social security system. With the Jobs Act of 2014, Italy has taken further steps towards the reduction of EPL that, in the spirit of "Flexicurity", were accompanied by significant improvements in the effectiveness and inclusiveness of its unemployment insurance (Picot and Tassinari, 2017). Progress on vocational training and active labor market policies, however, has lagged behind, which might limit the gains from the changes in the enacted EPL reforms. More generally, deregulation to easy worker reallocation is likely to be a “blunt weapon” unless undertaken within a healthily growing economy, in turn requiring a sound combination of structural reforms, industrial policies, as well as a stable political and macroeconomic environment.

Overall, we hope the results of our paper will contribute to the current debate about reforms under austerity, and will inform policy makers of other Mediterranean countries, such as France and Spain, considering or undergoing similar EPL reforms and to which our results potentially extend.

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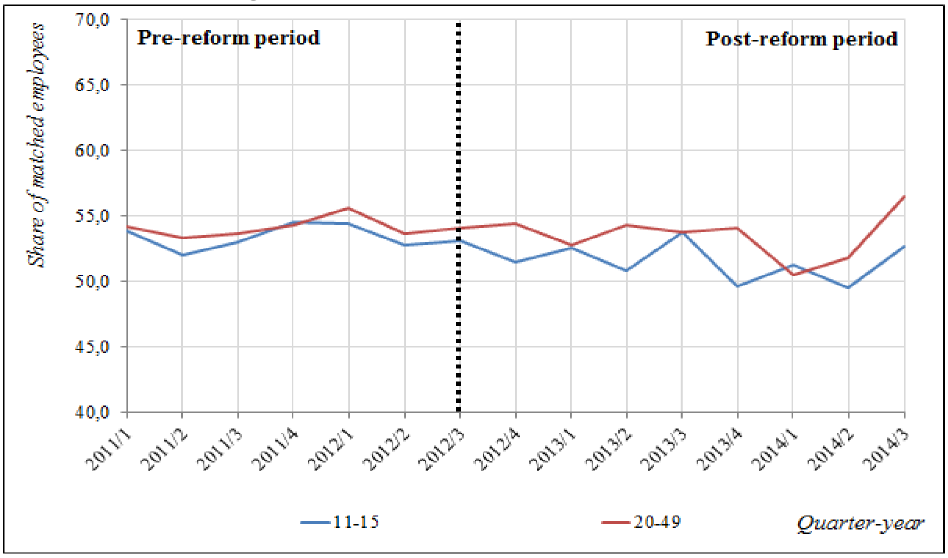
Tables and Figures

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

	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%; alternative specifications using the cell-shares of good matches and the logarithm of matches, do not affect the results.

TABLE 2 – Baseline results

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

Source: own computations on LFS data. Notes: robust standard errors; *** = 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	50,231
Firms 11-15/16-19, first four quarters	1.000	0.998	
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	59,692
Firms 11-15/20-49, first four quarters	1.041	0.431	
Firms 11-15/20-49, following four quarters	1.207***	0.001	
Panel C: measurement error			
Firms 11-15/20-49, overall effect	1.098**	0.014	81,130
Firms 11-15/20-49, first four quarters	1.073	0.103	
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	78,441
Firms 11-15/20-49, first four quarters	1.054	0.257	
Firms 11-15/20-49, following four quarters	1.166***	0.003	

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

TABLE 4 – Extensions

		RRR	p-value	Observations
Panel A: over- vs. under-education				
Over-education, overall effect		0.930	0.168	81,130
Under-education, overall effect		0.894**	0.014	
Over-education, first four quarters		0.968	0.582	
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	18,542
Firms 11-15/20-49, first four quarters		0.856	0.146	
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	62,588
Firms 11-15/20-49, first four quarters		1.083	0.120	
Firms 11-15/20-49, following four quarters		1.134**	0,030	
Panel C: sectors				
Manufacture	Overall effect	0.976	0.712	30,436
	First four quarters	0.962	0.613	
	Following four quarters	0.990	0.906	
Constructions	Overall effect	1.037	0.796	8,323
	First four quarters	1.040	0.807	
	Following four quarters	1.039	0.844	
Trade	Overall effect	1.094	0.411	13,283
	First four quarters	0.995	0.970	
	Following four quarters	1.207	0.182	
Other services	Overall effect	1.237***	0.002	29,087
	First four quarters	1.178**	0.033	
	Following four quarters	1.300***	0.003	
Panel D: geography				
North	Overall effect	1.122**	0.024	48,921
	First four quarters	1.050	0.409	
	Following four quarters	1.202***	0.005	
Center	Overall effect	0.975	0.777	15,352
	First four quarters	0.951	0.625	
	Following four quarters	1.001	0.993	
South	Overall effect	1.159*	0.099	16,857
	First four quarters	1.155	0.129	
	Following four quarters	1.162	0.233	

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

Table 5 – The effect of the Fornero Reform on worker turnover

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

Source**:** own computations on RIL data. Notes**:** OLS regressions. Standard errors in parenthesis. The worker turnover rate is measured at the firm level as the sum of accessions plus separations during the relevant survey year, divided by the firm's total number of employees at the beginning of the year. Accession rate (separation rates) are computed as the fraction of accessions (separations) on firm's total employees. Firing only is the ratio between the number of firings and a firm's employees. Standard errors in parenthesis. In all specifications, the controls list include: regional dummies, 1-digit sector of activity (Ateco classification) dummies, a cubic polynomial in firm size (no. of employees), dummy for incorporated business. ***, **, * significant at, respectively, 1%, 5% and 10%. (*) significant at the 12%.

Table 6 – The effect of the Fornero Reform on firm productivity

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

Source: own computations on AIDA data. Notes: OLS regressions. For each subsample, as described by column 1, entries refer to the estimated coefficient of the reform indicator (TREAT x POST); see equation (1) in the main text. Robust standard errors in parenthesis. In all specifications, the list of controls includes: a cubic polynomial in firm size (no. of employees), regional dummies, dummies for sector of activity (2-digit, Ateco classification), year dummies. TFP is defined as the residual in an OLS regression of the log of value added on the log of the firm's number of employees and the log of a firm fixed assets. ***, **, * significant at, respectively, 1%, 5% and 10%.