

Does Employment Protection Affect Qualification Mismatch?

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Abstract

This article investigates the effect of the relaxation of the rigidity of employment protection legislation in dual economies on qualification mismatch, measured as the sum of over-education and under-education. Using a unique matched employer–employee flow dataset, the effect of two reforms, which significantly modified the rules for the utilization of temporary contracts in Italy, is studied. By increasing the relative flow of workers on short-term contracts, the short-term contract reform is found to have reduced the relative flow of over-educated workers. However, this result holds only for older male workers, while no effect is found for female and younger workers. The apprenticeship reform instead had no major effects on the relative flow of mismatched workers.

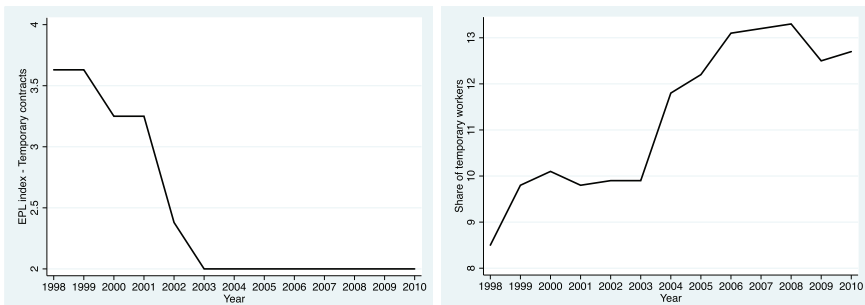
1. Introduction

Mismatched workers in OECD countries account for approximately 25 percent of the total workforce (CEDEFOP 2014). Mismatch is particularly severe in Mediterranean countries, which are characterized by more segmented labour markets (European Commission 2013). In these ‘dual economies’ (Saint-Paul 2002), labour market flexibility has been introduced at the margin, with the creation of two tiers. In the upper tier, workers who are usually hired on open-ended contracts characterized by stringent employment legislation (EPL) enjoy higher wages and good benefits. In the lower tier workers are typically hired on low-EPL temporary contracts and experience lower wages, higher turnover and job insecurity (Cahuc *et al.* 2016; Dolado *et al.* 2002). In this context, the role of temporary contracts in affecting mismatch is ambiguous. On one side, temporary contracts are used by new entrants in the labour market as a stepping stone, while still searching for better matches (Groot 1996; Groot and Maassen Van Den Brink 2000): the so-called ‘waiting room effect’ (Dekker *et al.* 2002). Even though data show

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FIGURE 1

EPL Index Associated with Temporary Contracts and Share of Temporary Employees. [Colour figure can be viewed at wileyonlinelibrary.com]



(a) EPL Index associated with temporary contracts.

(b) Temporary employees (% of total employment).

Source: OECD.

that approximately 23 per cent of UK graduates (McGuinness and Sloane 2011) and 20 per cent of Italian graduates (Iammarino and Marinelli 2015) at their first job experience are over-educated, this is a temporary phenomenon, as the increased flexibility facilitates job-to-job transitions and provides scope for improving the quality of the matches. On the other side, temporary jobs serve as an instrument to integrate into the labour market weaker segments of the populations, with lower productivity and bargaining power. These workers are more willing to take up temporary jobs which demand lower qualifications than the ones they possess, because they are happy to find a job after searching for a long time. Over-education in this segment of the labour market is a more persistent phenomenon due to the assumed 'dead-end' character of these contracts, which may be strengthened by the stigmatization of these workers (Dekker *et al.* 2002).

This article investigates the way the reform of two types of temporary contracts in Italy during the early 2000s affected qualification mismatch. The focus is on the Italian region Veneto for a number of reasons. First, the implementation of several labour market reforms in Italy (1997, 2001 and 2003) reduced the average EPL strictness of temporary contracts (Figure 1(a)). As a result, the share of temporary contracts increased significantly (Figure 1(b)). Second, Italy is among the OECD countries with the highest rates of mismatch (Verhaest and van der Velden 2013) and ranks number 1 for under-education (McGowan and Andrews 2015). Third, Veneto is one of the leading Italian industrial regional economies: in 2016, the nominal GDP accounted for 9.25 per cent of the Italian GDP, the GDP per capita was approximately equal to 111 per cent of the EU average (European Commission 2019) and the highly specialized manufacturing base (see the online Appendix) makes its economy comparable to the German one (Devicienti *et al.* 2019).

To pursue this objective, the longitudinal dataset PLANET, which provides information on the universe of workers' flows in the private sector in Veneto, is merged with the AIDA dataset, which provides firms' balance sheets information. Due to its longitudinal feature, the time period covered (1998–2011) and the detailed information on workers' education and occupation, the final dataset is unique.

Workers are defined as mismatched if their level of education is higher (or lower) compared to the mode of workers in the same age group, within the same occupation who enter the market in a specific year. A difference-in-differences model is proposed, in which two institutional reforms occurred in 2001 and 2003 are analysed to study the way qualification mismatch among temporary workers has changed after the reforms. The 2001 reform liberalized the utilization of short-term contracts, but its actual implementation had to wait for the approval of collective bargaining, which took place in different sectors at different times. The 2003 reform, which modified a number of features of the apprenticeship contract in terms of eligibility, duration, minimum wage and training content, required regional governments or industry-specific collective agreements to issue implementation guidelines, and therefore, the actual implementation happened in different regions and sectors at different times. The identification strategy is borrowed from Cappellari *et al.* (2012), and the variation in the implementation of the two reforms is exploited to identify their effects on workers' mismatch.

The results show that the short-term contract reform had a significant positive effect on the relative flow of workers hired on short-term contracts, which lead to a significant reduction of the relative flow of over-educated workers. These findings suggest that by enabling workers to escape from relatively poor matches and by providing tools to firms to substitute workers with different skills, the short-term contract reform has been effective in improving the quality of the matches. However, these results are short-term and hold only for older male workers, while there is no evidence of beneficial effects among women and younger workers. Although the apprenticeship reform introduced multiple changes to the regulations of the apprenticeship contract, which could have affected the flow of mismatched workers, no significant effect is found neither in terms of over-education, nor in terms of under-education.

This article is organized as follows. Section 2 reviews the literature on the relationship between EPL rigidity and mismatch, while Section 3 describes the Italian institutional background, while Section 4 illustrates the methodology. Section 5 describes the data, Section 6 explains the validity of the approach and Section 7 illustrates the results. Finally, Section 8 concludes the article.

2. Literature review

In the literature, there is no clear-cut evidence on the relationship between the rigidity of EPL and mismatch. Di Pietro (2002) discusses a number of reasons

why rigid markets with stringent EPL are expected to be associated with higher mismatch. First, stringent EPL on the firing of permanent workers makes it more difficult for firms to adapt their workforce to shocks and to address gaps between the demand and supply of skills. Hence, less stringent EPL reduces mismatch by decreasing the waiting time until workers get a better match. Second, firing restrictions may prevent firms from immediately taking advantage of upward changes in skilled workforce availability, discouraging them from adopting new technologies and reducing the number of vacancies that can be filled with high-skilled workers. Third, rigid EPL tends to lock highly educated workers into relatively poor matches by making it more difficult for them to obtain a new position, leading indirectly to a higher incidence of over-education by pushing the economy towards a 'low-skill, low-technology trap'. In an influential work, Lazear (1998) shows that when firms face barriers to laying off due to legal or other institutional impediments (e.g. powerful trade unions), the relative value of risky workers, whose productivity is unknown, is reduced, with negative effects on the average quality of the workers. Brunello *et al.* (2007) also argue that as more rigid EPL makes it harder for individuals to obtain their first job and for firms to reduce staff, the use of temporary contracts may help reduce mismatch. Sicherman (1991) states that, under a strict EPL regime, firms face longer employment relationships and, if experience complements education, employers prefer to hire highly educated young workers without experience to allow for future internal promotions, thus increasing the share of over-educated workers (Croce and Ghignoni 2012). On a different perspective, Gangl (2004) claims that stringent EPL countries may experience lower over-education because employers may be more risk-averse in recruiting, with positive effects on the quality of the matches.

Empirically, Ortiz (2010) finds a positive correlation between being hired on a temporary contract and being over-educated. Daly *et al.* (2000), Verhaest and van der Velden (2013) and Croce and Ghignoni (2012) fail to find a significant correlation between EPL and mismatch. Davia *et al.* (2017) instead find that countries with more rigid EPL are associated with lower shares of over-educated workers.

Another related strand of the literature focuses on the effect of minimum wage on employment (Neumark and Wascher 2008; Dube *et al.* 2010), specifically on teen employment (Allegretto *et al.* 2017; Giuliano 2013; Manning 2016; Portugal and Cardoso 2006), which remains a controversial topic among economists. Although there is no evidence of the direct effect of minimum wage on qualification mismatch in the literature, by making the apprenticeship contract more expensive firms might become more selective when hiring an apprentice, which should lead to the recruitment of better educated workers. Nevertheless, when minimum wage is increased, the retention rate of workers is higher (Portugal and Cardoso 2006), and this might lead instead firms to select more under-educated workers, with the intent of providing specific training (Berton *et al.* 2017).

3. Institutional background

The typical labour contract in Italy is the permanent contract, which is open-ended and associated with stringent employment protection regulations (EPL). The short-term contract shares all the features of the permanent contract except for the limited duration and zero firing costs at termination. A short-term contract can only be renewed once and its total duration cannot be longer than three years. When hiring a worker on a short-term contract, employers need to justify why they offer a short-term instead of a permanent contract, choosing among a list of specific circumstances, such as the replacement of workers on leave or a sudden increase in production. If either the length or the motivation is not truly reported, the worker can sue the employer to get her contract converted to a permanent one. Firms can also use apprenticeship contracts to hire young workers for a maximum duration of four years, and are required to provide on-the-job training to be performed by local authorities or accredited training institutions. At the end of the training period, the acquired qualifications are asserted by a formal certification. To compensate for the training costs incurred by the firms, labour costs amount to one-third of the costs paid for permanent and short-term contracts.

In Italy and in the Veneto region, in 2014, permanent contracts accounted for approximately 87–88 per cent of total employment, followed by short-term contracts (7–9 per cent), while apprenticeship contracts accounted for approximately 1.5–2.5 per cent of total employment.

In the time period considered (1998–2007), two major labour market reforms were implemented: the short-term contract reform (Law 368/2001) and the apprenticeship reform (Law 30/2003).

The Short-Term Contract Reform

The 2001 reform (Law 368/2001) introduced a major novelty in the short-term contract regulation: the possibility to use the contract for any reason of a technical, organizational, production or replacement nature, largely broadening the scope for utilization. Because of the strong deregulation of the short-term contract, this reform was instrumental in establishing a dual labour market (Boeri 2011).

The relaxation of this rule, however, created confusion among employers regarding the requirements for adoption (Aimo 2006): it was not clear whether employers could use short-term contracts also for activities not of temporary nature. Moreover, in case of court disputes, the applicability relied too much on the interpretations of the judges, causing delays and high uncertainty on the potential outcome. The risk incurred by firms of being obliged to convert short-term contracts in permanent contracts after a court appeal created strong disincentives for their adoption (Venn 2009). According to the new legislation, collective bargaining was responsible for setting the maximum percentage of fixed-term employees in firms' total employment. For this reason, to be effective in a given industry, the new law had to wait for

TABLE 1
Short-Term Contract Reform by Year and Sector

<i>Sector</i>	<i>Year</i>
Chemicals, Constructions, Food Products, Printing, Retail Trade, Textiles, Transportation, Wood Products	2005
Energy, Telecommunication	2006
Real Estate, Water and Gas	2007

Note. The list refers to sectors/years in which the collective agreements have been renewed and the rules for the new short-term contract approved.

Source: ISFOL (2010) and *Consiglio Nazionale dell'Economia e del Lavoro* (CNEL).

the renewal of the specific collective agreements. In practice, all collective agreements confirmed the maximum share of short-term contracts set in the previous bargaining rounds, so no differences were introduced by the reform on employment flexibility across industries.

Nevertheless, only industries with contracts negotiated after the law was implemented could utilize the new short-term contract. After 2001, the renegotiation of collective bargaining agreements at sectoral level only occurred in some industries in 2005, 2006 and 2007 (Table 1). Few sectors of the economy, such as metal manufacturing and banking renewed the collective agreements during the period but postponed the implementation of the 'new' short-term contract to a later normative agreement, which took place after the period considered (see the online Appendix). This variation across sectors is exploited in the identification strategy.

The Apprenticeship Reform

The 2003 reform (Law 30/2003) significantly modified a number of features of the apprenticeship contract, in terms of eligibility, duration, minimum wage and training content. First, the limit for eligibility was raised from 25 to 29 years old; second, the duration of the contract was extended from varying between 18 months and four years (five in the craft sector) to vary between two and six years; third, a minimum wage at least two levels below the wage of a similarly qualified worker (i.e. the position reached by the end of the contract) was instituted, and finally, the option for firms to supply training at the workplace as a substitute for external training was established (Albanese *et al.* 2017). While the latter was an attempt to tackle the inefficiencies of local authorities, it also made it harder to monitor compliance.¹

The national law was enacted in 2003 and it required regional governments to issue the necessary regulations concerning the training content of the new apprenticeship contract. All regions acted slowly, including Veneto, which issued the regulations in 2009. However, additional institutional variations in the adoption of the new apprenticeship contract were offered by Law 80/2005, which provided that in the absence of regional regulations, industry-specific collective agreements could define the rules for the training content of the apprenticeship contract. Starting from 2005, collective agreements which

TABLE 2
Apprenticeship Reform by Year and Sector

<i>Sector</i>	<i>Year</i>
Banking	2005
Chemicals, Construction, Energy, Food Products, Metal Manufacturing, Printing, Retail Trade, Textiles, Transportation, Wood products	2006
Water and Gas, Rubber and Plastic, Tourism, Radio and TV	2007

Note. The listed sectors are the ones in which the specific collective agreements have been renewed and the new apprenticeship contract has been regulated.

Source: ISFOL (2010) and ISFOL (2007).

enabled the utilization of the new apprenticeship contract were signed in specific sectors (Table 2). This variation across sectors is exploited in the identification strategy.

Data show that the share of apprentices among the 15–29 employed workers in the Veneto region increased from 16.7 per cent in 2005 to 19.9 per cent in 2007. However, while 16.6 per cent of apprentices were in training in 2005, only 12.1 per cent were in training in 2007. In addition, the average duration range marginally increased compared to the period before the reform, with an average minimum and maximum length across sectors of 33 and 56 months, respectively (see the online Appendix).

4. Methodology

The goal is to study the effect of the implementation of two labour market reforms which significantly modified key features of temporary contracts on mismatch. An employed person may experience two forms of mismatch: qualification and skills mismatch (see the online Appendix). The focus here is on qualification mismatch, which refers to a situation in which a person holds a job whose qualification requirements do not correspond to the level and/or type of qualification she has (ILO 2018). In the literature, three different measures of qualification mismatch (Leuven and Oosterbeek 2011) have been proposed: objective (or normative), subjective (self-assessed), and statistical (Hartog 2000). While the first approach is based on classifications, which quickly become outdated (Chevalier 2003; ILO 2018), the second approach heavily relies on the opinion of the respondent and the wording of the survey question (Alba-Ramirez 1993; Allen and van der Velden 2001; Chevalier and Lindley 2009; Mavromaras *et al.* 2010). The statistical measure, which estimates the level of qualification mismatch by comparing the level of education attained with the mean (Verdugo and Verdugo 1989) or the modal (Bauer 2002; Kiker *et al.* 1997; Mendes de Oliveira *et al.* 2000) value of the level of education within each occupational category, is the one used in this study. The main reason for selecting this approach is the availability of a unique employer–employee flow dataset, which provides detailed information

on firms and workers for the period of the reforms. The mode is chosen (over the mean), like in most of the literature (Battu and Sloane 2004; Chiswick and Miller 2009; Kampelmann and Rycx 2012) as it is the measure least affected by the shape of the distribution of education (Mavromaras *et al.* 2012). To take into account the dynamic nature of the required education due to changes in technology and educational quality, cells of individuals are defined according to age group, occupation and year of entrance in the labour market (Quinn and Rubb 2006). Workers are mismatched if they are over- (under-) educated, that is, their level of education is higher (lower) compared to the mode of workers in the same age group, within the same occupation who enter the market in a specific year. This definition based on flows rather than on stocks allows to better capture the evolution of the required education for a specific type of job over time. Moreover, the focus on flows into and out of temporary contracts (of limited duration) helps overcome the potential drawback of this approach of having the occupational mode driven by the majority of incumbent older workers with longer tenure.

The staggered implementation of the two labour market reforms gives rise to a quasi-experimental setting in which adopting and non-adopting sectors within the Veneto region are observed over a sufficient period of time. The identification strategy borrows from Cappellari *et al.* (2012) and the model is specified as follows:

$$Y_{j,t} = c + \alpha(S_{Reform})_{j,t} + \beta(A_{Reform})_{j,t} + \gamma(SA_{Reform})_{j,t} + \zeta_t + \phi_j + \epsilon_{j,t}, \quad (1)$$

where j refers to the firm and t to the time. Four groups of firms are identified: those not affected by the reforms, those affected only by the first reform, those affected only by the second reform and those affected by both reforms. $(S_{Reform})_{j,t}$ is the dummy variable which takes value 1 for firms with an active business in years and sectors affected only by the short-term contract reform, $(A_{Reform})_{j,t}$ is the dummy variable which takes value 1 for firms with an active business in years and sectors affected only by the apprenticeship reform, and $(SA_{Reform})_{j,t}$ is the dummy which takes value 1 for firms with an active business in years and sectors affected by both reforms.² All estimates include year dummies, ζ_t , which controls for shift in the dependent variable common to all sectors and firm fixed effects ϕ_j . The coefficients of interest are α , β , and γ , which can be interpreted as the causal effects of the short-term contract reform (α), the apprenticeship reform (β) and the combination of both reforms (γ) on outcome Y . Data are collapsed at firm level and observations are weighted by firm size; hence, per each firm, one observation is retained in each year considered. Standard errors are corrected for heteroscedasticity and clustered at sector level (Cameron and Miller 2015).

Although in the literature, there is no clear theoretical insight about timing, the effect on mismatch is expected to be contemporaneous to the reforms. Given that the workers flows are analysed and the implementation of the reforms had already been significantly delayed (the laws were approved in 2001

and 2003, respectively), it is envisioned that firms would react promptly to take advantage of the new rules.

As a first step, the flow of a specific category of workers X is defined as the difference between the entries and the exits of those workers in firm j at time t , that is, $(X_{jt}^{entries} - X_{jt}^{exits})$. Second, the effect of the two reforms on the contribution of the flow of each contract type to the total employment flow in firm j at time t is computed to investigate whether each reform contributed to increase proportionally more the utilization of specific types of contracts. To do so, equation (1) is estimated using as outcomes the ratio of the flows of short-term contracts and apprenticeships and the total employment flow:

$$(St-rel)_{j,t} = (Short-term_{flow})_{j,t} / (Employment_{flow})_{j,t}, \quad (2)$$

$$(App-rel)_{j,t} = (Apprentices_{flow})_{j,t} / (Employment_{flow})_{j,t}. \quad (3)$$

Next, outcomes of interest such as total mismatch and its two components over- and under-education, computed as the flow of workers in firm j at time t who are mismatched, over-educated and under-educated respectively, over the total employment flow are considered:

$$Mismatch-rel_{j,t} = (Mismatched_{flow})_{j,t} / (Employment_{flow})_{j,t}, \quad (4)$$

$$Over-rel_{j,t} = (Over-educated_{flow})_{j,t} / (Employment_{flow})_{j,t}, \quad (5)$$

$$Under-rel_{j,t} = (Under-educated_{flow})_{j,t} / (Employment_{flow})_{j,t}. \quad (6)$$

Since in the literature-specific groups of individuals are found to be more frequently hired on temporary contracts and more exposed to mismatch (Dekker *et al.* 2002; Groot and Maassen Van Den Brink 2000), potential asymmetric effects are explored. Accordingly, a triple difference-in-differences estimation is performed, where an additional source of variation is included, by focusing on the relative flows of specific groups of workers within the benchmark model (equation (1)). Specifically, gender and age represent a third dimension along which the treatment varies (Goodman-Bacon 2018), as in the following model:

$$Y_{g,j,t} = c + \alpha(S_{Reform})_{j,t} + \beta(A_{Reform})_{j,t} + \gamma(SA_{Reform})_{j,t} + \delta(S_{Reform})_{j,t}G + \lambda(A_{Reform})_{j,t}G + \theta(SA_{Reform})_{j,t}G + \zeta_t G + G + \zeta_t + \phi_j + \epsilon_{j,t}, \quad (7)$$

where G is a dummy variable, which takes value 1 for specific groups of workers, such as women and young individuals in the age groups 15–24. Data are collapsed at firm level and category of individuals (by age and gender) and observations are weighted by firm size. The interest is in the causal effects of the reforms for the group of workers G on outcome Y , which are represented by the coefficients $(\alpha + \delta)$ for the short-term contract reform, $(\beta + \lambda)$ for the apprenticeship reform and $(\gamma + \theta)$ for the combination of both reforms. As

workers eligible to be hired on an apprenticeship contract are those below the age of 29, an additional analysis is performed by estimating model 1 on a restricted sample of firms which hire workers in the age group 15–29.

5. Data and descriptive statistics

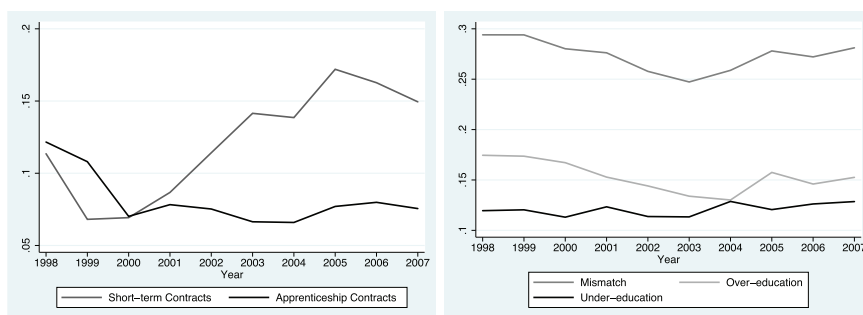
An employer–employee flow dataset from the Italian Veneto region (PLANET) is used to perform the analysis. The dataset includes all workers who experienced a mobility episode, that is, hiring, firing, transformations, resignations, contract expiration and retirements. Once entered in the panel, each worker is followed for the entire career, unless she moves outside the Veneto region.³ A valuable feature of this dataset is the inclusion of detailed information on occupation (four digits), education (eight categories), different types of labour contracts and firms' characteristics, such as industry and sector (three digits); however, the stock of workers is not observable. The firms' national tax number (*codice fiscale*) is used to merge this dataset with the AIDA dataset, which provides balance sheets information inferred by the standardized reports that firms file annually with the Chamber of Commerce.⁴ This dataset is used to extract information on the firms size and the analysis is restricted to observations for which the correlation between the flows of workers in AIDA and in PLANET is approximately 90 per cent. This matched employer–employee dataset provides all flows of workers into and out of firms for the period 1998–2007.

Due to the poor reliability of the education level of foreign workers, firms whose average flows of foreign workers during the period considered are above 4 per cent (approximately 10 per cent of the firms) are removed from the sample. The tails of the flows distribution are cut below 1 per cent and above 99 per cent to avoid extreme values and occupations with less than 50 observations are removed (approximately 0.004 per cent of the total number of observations). The analysis is focused on individuals between 15 and 64 years old.

In Figure 2, the evolution of the relative flows of temporary workers and qualification mismatch is reported. An increase in the relative flow of short-term employees and apprentices is observed in 2005, followed by a decline. While over-education increases in 2005 and decreases afterwards, under-education decreases in 2005 and remains stable in 2006 and 2007.

Table 3 reports descriptive statistics for the whole sample of workers and for the selected sample. Characteristics such as age, education and qualification mismatch are quite similar between the two groups. Approximately 50 per cent of individuals in the sample have a junior high school degree and more than 30 per cent have a high school degree, with only 6 per cent of individuals having a tertiary level of education. Approximately 30 per cent of workers are hired on a short-term contract, while approximately 8 per cent are hired on an apprenticeship contract. Short-term contracts are more common among women (37 per cent compared to 25 per cent) and older workers.

FIGURE 2
Relative Employment and Mismatch Flows (1998-2007). [Colour figure can be viewed at wileyonlinelibrary.com]



(a) Relative flows of short-term and apprenticeship workers. (b) Relative flows of mismatched, over-educated and under-educated workers.

Note: The variables are defined as in equations (2)–(6) and weighted by firm size.

Over-education affects 23 per cent of the sampled individuals, mostly young males in the 15–24 and 25–34 age groups. Approximately 13 per cent of the sampled workers are under-educated, mainly females and older workers.

Two dummy variables which identify the treated and the control groups are created per each treatment/reform. Exposure to the treatment is defined based on the sector each firm belongs. Treated firms with respect to the short-term contract reform are those firms which operate in sectors whose collective agreements, signed after the implementation of the 2001 law, passed the new legislation. In a similar fashion, treated firms with respect to the apprenticeship reform are those firms which operate in sectors, whose industry-specific collective agreements defined the rules for the training content of the apprenticeship contract.

Table 4 reports descriptive statistics of the firms included in the final sample. In column 1, the characteristics of all firms are reported; in column 2, the features of the subset of firms not affected by the reforms are shown. In columns 3 and 4, the characteristics of the subset of firms, which were exposed to only one of the two reforms are reported, respectively. Finally, column 5 describes the characteristics of the subset of firms, which were exposed to both reforms. No firms were subject to any treatment between 1998 and 2004, most firms exposed to the first reform were treated in 2005, while most firms exposed to the apprenticeship reform were treated in 2006. The characteristics of the firms are quite similar across groups, except for a slightly lower employment flow and a slightly higher relative flow of short-term contracts in firms exposed to the short-term contract reform. In terms of qualification mismatch, all the variables reported are similar across groups.

TABLE 3
Descriptive Statistics — Workers

	<i>Selected sample</i>		<i>Whole sample</i>	
	<i>Mean</i>	<i>Std. Dev.</i>	<i>Mean</i>	<i>Std. Dev.</i>
Female	0.363	0.481	0.407	0.491
Age 15–24	0.303	0.460	0.285	0.452
Age 25–34	0.362	0.480	0.366	0.482
Age 35–44	0.209	0.407	0.217	0.412
Age 45–54	0.097	0.296	0.102	0.302
Age 55–64	0.028	0.165	0.029	0.168
No education	0.042	0.201	0.048	0.215
Elementary	0.067	0.250	0.075	0.263
Junior high school	0.509	0.499	0.507	0.491
High school	0.319	0.466	0.304	0.460
Bachelor's degree	0.060	0.237	0.062	0.241
Postgraduate degree	0.003	0.051	0.003	0.052
PhD	0.0002	0.013	0.0002	0.014
Short term	0.297	0.457	0.342	0.472
Short term (female)	0.372	0.483	0.410	0.491
Short term (male)	0.255	0.436	0.287	0.452
Short term (Age 15–24)	0.230	0.421	0.276	0.447
Short term (Age 25–34)	0.285	0.451	0.319	0.466
Short term (Age 35–44)	0.328	0.469	0.369	0.482
Short term (Age 45–54)	0.419	0.494	0.447	0.497
Short term (Age 55–64)	0.521	0.499	0.549	0.497
Apprenticeship	0.080	0.271	0.063	0.242
Apprenticeship (female)	0.071	0.256	0.055	0.228
Apprenticeship (male)	0.085	0.279	0.067	0.251
Apprenticeship (Age 15–24)	0.252	0.433	0.209	0.407
Apprenticeship (Age 25–34)	0.010	0.097	0.007	0.087
Over-education	0.237	0.426	0.248	0.432
Over-education (female)	0.21	0.415	0.235	0.424
Over-education (male)	0.248	0.431	0.259	0.438
Over-education (Age 15–24)	0.266	0.442	0.282	0.450
Over-education (Age 25–34)	0.258	0.437	0.272	0.445
Over-education (Age 35–44)	0.168	0.374	0.169	0.374
Over-education (Age 45–54)	0.200	0.400	0.218	0.413
Over-education (Age 55–64)	0.222	0.416	0.233	0.423
Under-education	0.132	0.339	0.132	0.339
Under-education (female)	0.143	0.350	0.140	0.347
Under-education (male)	0.125	0.332	0.125	0.331
Under-education (Age 15–24)	0.102	0.302	0.106	0.308
Under-education (Age 25–34)	0.116	0.321	0.115	0.319
Under-education (Age 35–44)	0.158	0.365	0.156	0.362
Under-education (Age 45–54)	0.233	0.423	0.227	0.418
Under-education (Age 55–64)	0.166	0.372	0.145	0.351
No. of observations		614,204		1,731,679
No. of firms		12,823		16,563

Note. Whole sample: all workers, entering and exiting the firms in the merged AIDA-PLANET dataset. Selected sample: all workers, entering and exiting the selected firms in the final dataset, where the focus is on the age range 16–64, firms whose average flows of foreign workers during the period considered is above 4% are removed, the tails of the flows distribution below 1% and above 99% are cut and occupations with less than 50 observations are removed.

TABLE 4
Descriptive Statistics — Firms

	<i>Full sample</i>	<i>Never treated</i>	<i>Short term</i>	<i>Apprenticeship</i>	<i>Both reforms</i>	
No. of firms	12,823	5,314	355	1,914	5,240	5,240
Observations	59,259	45,270	1,950	3,206	8,833	117,646
1998	4,758	4,758	0	0	0	10,442
1999	5,167	5,167	0	0	0	11,385
2000	5,694	5,694	0	0	0	12,330
2001	5,405	5,405	0	0	0	12,466
2002	7,151	7,151	0	0	0	12,421
2003	6,702	6,702	0	0	0	12,255
2004	5,794	5,794	0	0	0	12,061
2005	6,188	2,522	1,943	112	1,611	11,458
2006	6,283	1,180	3	1,461	3,639	11,453
2007	6,117	897	4	1,633	3,583	11,375
Agriculture	0.87	1.14	0	0	0	1.11
Extraction	0.33	0.44	0	0	0	0.29
Manufacturing	48.49	49.50	59.85	88.90	26.15	48.32
Energy	0.12	0.14	0	0.12	0.07	0.15
Construction	7.51	6.66	24.67	0	10.81	7.79
Commerce	27.50	25.16	0	0	55.55	24.98
Hotels and Restaurants	1.71	1.50	0	10.39	0	2.26
Transport and Commun.	4.23	3.71	14.62	0	6.16	4.57
Finance	0.33	0.39	0	0.59	0	0.38
Real estate	6.45	8.27	0	0	0.89	7.33
Public sector	0.02	0.02	0	0	0	0.01
Education	0.21	0.27	0	0	0	0.23
Health	0.72	0.94	0	0	0	0.89
Others	1.51	1.86	0.87	0	0.36	1.65
Firm size	30.86	34.87	43.63	53.32	32.21	45.11
Employment flow	2.59	2.81	1.58	2.65	1.75	3.01
Share of short-term flow	0.13	0.12	0.19	0.17	0.17	0.12
Share of apprenticeship flow	0.08	0.08	0.07	0.07	0.08	0.08
Share of mismatch flow	0.28	0.28	0.26	0.28	0.30	0.28
Share of over-education flow	0.17	0.17	0.15	0.19	0.17	0.16
Share of under-education flow	0.12	0.11	0.11	0.09	0.13	0.13

Note. Characteristics of firms according to different samples: Column 1 refers to all firms in the sample; column 2 refers to firms never affected by the reforms (collective agreements not renewed); column 3 refers to firms affected only by the short-term contract reform (collective agreements only approved the new short-term contract).

Column 4 refers to firms affected only by the apprenticeship reform (collective agreements only approved the new apprenticeship contract); column 5 refers to firms affected by both reforms (collective agreements approved both new contracts).

6. Validity of this approach

The validity of this approach is based on three assumptions: first, the proposed measure of mismatch is not endogenously affected by the reforms; second, the evolution of the outcomes of interest in treated and control sectors would not be different without the reforms; and finally, there is no casual relationship between the outcomes of interest and the adoption of the reforms.

The proposed definition of mismatch relies on the mode of education being an accurate measure of the education level within a certain occupation. In

TABLE 5
Effect of the Reforms on Mean and Mode Levels of Education

	<i>Mean</i>	<i>Mode</i>	<i>Mode</i>
<i>SReform</i>	0.0089 (1.06)	-0.0225 (-1.14)	0.0031 (0.21)
<i>AReform</i>	-0.0120 (-1.04)	-0.0098 (-0.64)	-0.0130 (-1.23)
<i>SAReform</i>	0.0050 (0.53)	0.0147 (0.67)	0.0068 (0.47)
<i>N</i>	59,259	59,259	59,259
<i>R</i> ²	0.0065	0.0033	0.0049

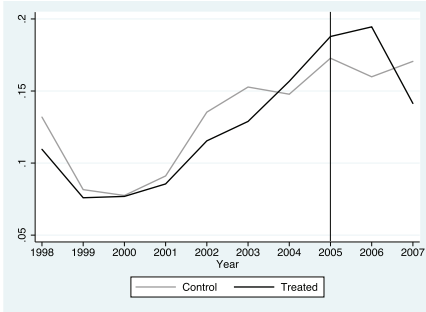
Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variables are: in column 1, the mean of education by occupation and age at firm level; and in column 2, the mode of education by occupation and age at firm level.

addition, being the mode a time-varying variable, it may be endogenously affected by the reforms. To rule out the hypothesis that the reforms affected the mode (mean) of education and, in turn, had an impact on the proposed measures of mismatch, equation (1) is estimated using as outcomes the mode and the mean of education computed at firm level (Table 5). The coefficients of the reforms not significantly different from zero are interpreted as evidence that the labour market reforms under consideration did not have an impact on the proposed measure of mismatch. Additionally, a robustness analysis in which a three-year lagged mode is used, with the advantage of avoiding measuring the mode during a treatment period, thus ensuring exogeneity of the treatment, provides results fully consistent with the findings obtained using the current mode (see the online Appendix).

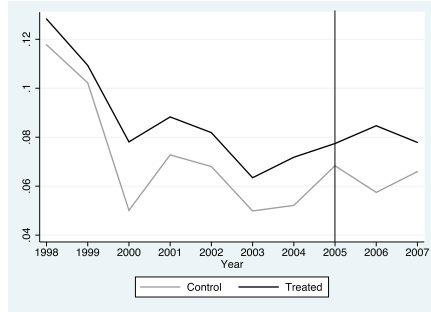
The validity of the difference-in-differences estimation strategy is also based on the assumption that the evolution of the outcomes of interest for the adopting and non-adopting sectors would not be systematically different in the absence of the intervention. The main concern is indeed that sectors which adopted first the new contracts are those in which the relative flows of temporary and mismatched workers were already growing faster, violating the assumption of parallel trend before the treatment. To address this issue, the relative flows of short-term workers, apprentices and mismatched workers in sectors affected and non-affected by the short-term contract reform (Figure 3) and by the apprenticeship reform (Figure 4) are compared. Panels 3a and 3b compare the relative flows of short-term workers and apprentices in treated and control sectors with respect to the short-term contract reform. Panel 3c depicts the relative flow of mismatch in adopting and non-adopting sectors, while Panels 3d and 3e show the relative flows of over-educated and under-educated workers, respectively. The same exercise is reported for firms in treated and control sectors with respect to the apprenticeship reform (Figure 4). Although the figures reveal a similarity in the trends of the two series before the reforms, formal placebo tests are provided. The main model is estimated by including per each reform placebo dummies for the treated in

FIGURE 3

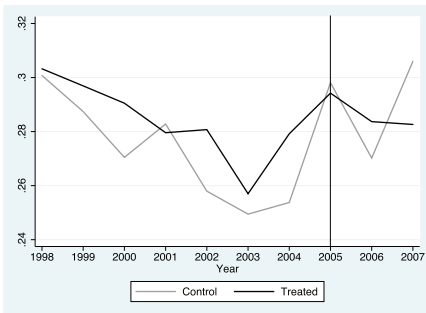
Relative Short-Term Workers and Apprenticeship Workers Flows and Relative Over-Educated and Under-Educated Workers Flows in Treated and Control Groups with Respect to the Short-Term Contract Reform. [Colour figure can be viewed at wileyonlinelibrary.com]



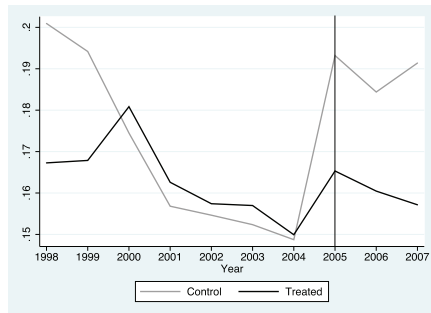
(a) Short-term workers.



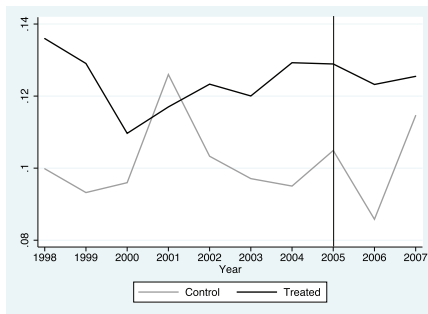
(b) Apprenticeship workers.



(c) Mismatched workers.



(d) Over-educated workers.

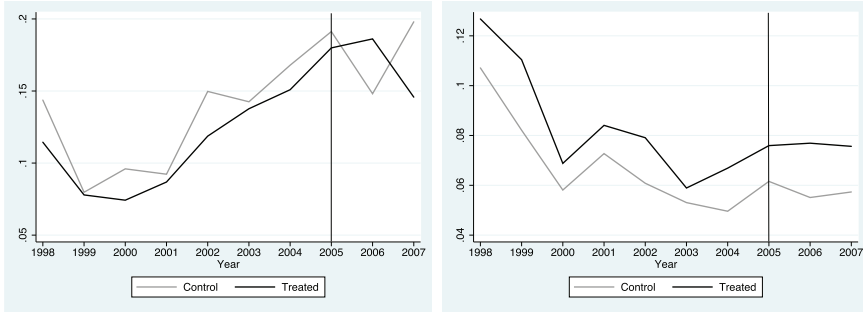


(e) Under-educated workers.

Note: Each variable is computed as the ratio with respect to the employment flow, as defined in equations (2)–(6). The treated group includes all firms in sectors in which the collective agreements have approved the rules for the new short-term contract.

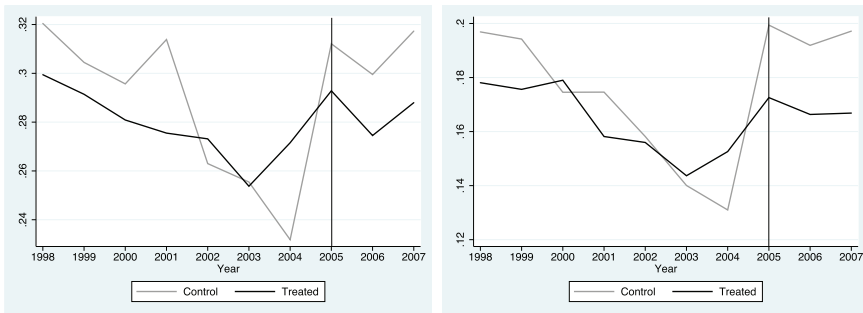
FIGURE 4

Relative Short-Term Workers and Apprenticeship Workers Flows and Relative Over-Educated and Under-Educated Workers Flows in Treated and Control Groups with respect to the Apprenticeship Reform. [Colour figure can be viewed at wileyonlinelibrary.com]



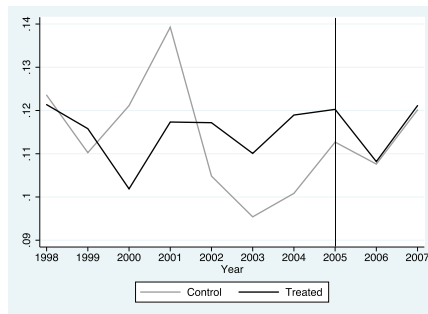
(a) Short-term workers.

(b) Apprenticeship workers.



(c) Mismatched workers.

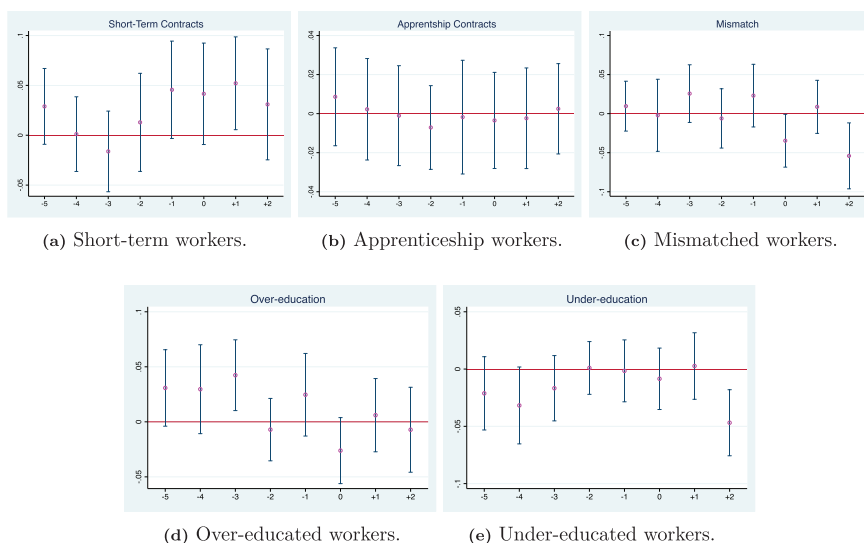
(d) Over-educated workers.



(e) Under-educated workers.

Note: Each variable is computed as the ratio with respect to the employment flow, as defined in equations (2)–(6). The treated group includes all firms in sectors in which the collective agreements have approved the rules for the new apprenticeship contract.

FIGURE 5
Event Study for the Short-Term Contract Reform. [Colour figure can be viewed at wileyonlinelibrary.com]



Note: The dependent variable is the net flow of workers over the net employment flow. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size. Ninety-five per cent confidence intervals are reported.

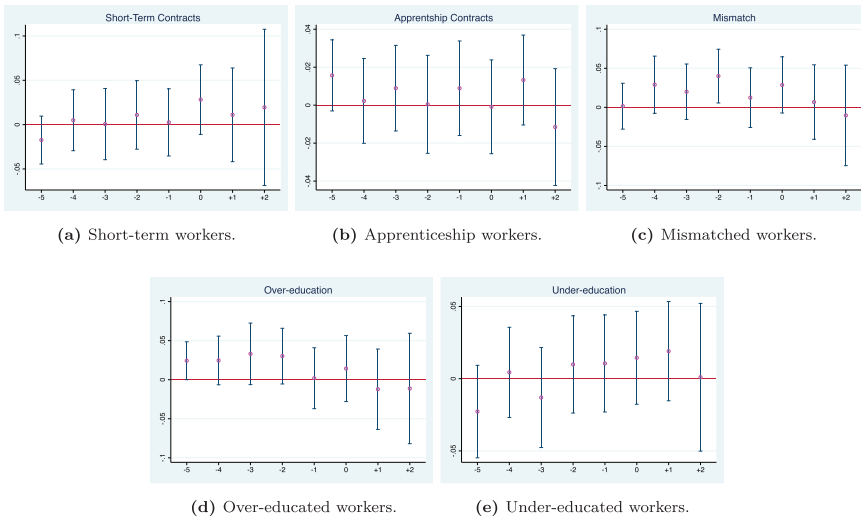
the years before and after the reform, where $k \in \{S, A\}$:

$$Y_{j,t} = c + \sum_{p=-5}^{+2} \alpha_p(k_{Reform})_{j,t+p} + \zeta_t + \phi_j + \epsilon_{j,t}.$$

Coefficients per each year per each outcome variable, together with the 95 per cent confidence intervals, are reported in Figures 5 and 6. Although some coefficients turned out to be occasionally different from zero in few years before the treatments, overall no systematic pattern of pre-trend effects is found across all variables considered. We also observe a one-year anticipation effect of the short-term contract reform on the relative share of short-term employees, which could be ascribable to the firms and workers' expectations of the industry collective agreement renewals (Giannelli et al. 2012). Event studies for the triple difference in differences, accounting for gender and age, are reported in the online Appendix.

Finally, to demonstrate the exogenous nature of the reforms, the dummies, which take value 1 for the year in which the reforms were implemented and 0 otherwise, where $k \in \{S, A\}$, are regressed on the lags of the relative flows of temporary and mismatched workers (Cappellari et al. 2012), as defined in

FIGURE 6
Event Study for the Apprenticeship Reform. [Colour figure can be viewed at
wileyonlinelibrary.com]



Note: The dependent variable is the net flow of workers over the net employment flow. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size. Ninety-five per cent confidence intervals are reported.

equations (2) and (3) and equations (5) and (6), respectively:

$$\text{k-Reform}_{j,t} = c + \sum_{q=1}^5 \text{St-rel}_{t-q} + \varepsilon_{j,t}, \quad (8)$$

$$\text{k-Reform}_{j,t} = c + \sum_{q=1}^5 \text{App-rel}_{t-q} + \varepsilon_{j,t}, \quad (9)$$

$$\text{k-Reform}_{j,t} = c + \sum_{q=1}^5 \text{Over-rel}_{t-q} + \varepsilon_{j,t}, \quad (10)$$

$$\text{k-Reform}_{j,t} = c + \sum_{q=1}^5 \text{Under-rel}_{t-q} + \varepsilon_{j,t}. \quad (11)$$

The employment flows are averaged across three-digit sectors and provinces. If the coefficients of the lag variables happen to be systematically statistically significant, this may reveal the presence of a causal relationship between the relative flows of temporary and mismatched workers and the adoption of the reformed contracts. The results (Tables 6 and 7) show no systematic relationship between the lags of temporary employment and mismatch flows and the adoption of the reforms, reassuring about the exogeneity of the reforms.

TABLE 6
Trends in Temporary Employment

	S_{Reform}	A_{Reform}		S_{Reform}	A_{Reform}
Short term $t - 1$	0.102 (0.70)	0.089 (0.60)	Apprenticeship $t - 1$	0.139 (0.47)	0.151 (0.51)
Short term $t - 2$	0.03 (0.01)	-0.004 (-0.02)	Apprenticeship $t - 2$	-0.303 (-1.09)	-0.323 (-1.19)
Short term $t - 3$	0.017 (0.08)	0.023 (0.11)	Apprenticeship $t - 3$	-0.128 (-0.42)	-0.155 (-0.50)
Short term $t - 4$	0.243 (1.21)	0.208 (1.02)	Apprenticeship $t - 4$	0.012 (0.04)	-0.028 (-0.10)
Short term $t - 5$	-0.265 (-1.10)	-0.247 (-1.01)	Apprenticeship $t - 5$	0.384 (1.09)	0.367 (1.05)
Constant	0.716*** (9.85)	0.716*** (9.83)	Constant	0.729*** (12.24)	0.733*** (12.31)
N	1,737	1,737	N	1,737	1,737
R^2	0.0234	0.0199	R^2	0.0303	0.0325

Note. t -Statistics in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable takes value 1 in the year of the reform and 0 otherwise.

TABLE 7
Trends in Over-Education and Under-Education

	S_{Reform}	A_{Reform}		S_{Reform}	A_{Reform}
Over-education $t - 1$	0.300 (0.98)	0.308 (1.01)	Under-education $t - 1$	-0.196 (-0.72)	-0.187 (-0.69)
Over-education $t - 2$	0.064 (0.19)	0.019 (0.06)	Under-education $t - 2$	0.019 (0.09)	0.034 (0.15)
Over-education $t - 3$	0.160 (0.72)	0.162 (0.73)	Under-education $t - 3$	0.301 (1.21)	0.293 (1.18)
over-education $t - 4$	-0.073 (-0.40)	-0.062 (-0.34)	Under-education $t - 4$	-0.583* (-1.85)	-0.572* (-1.80)
Over-education $t - 5$	-0.091 (-0.38)	-0.072 (-0.30)	Under-education $t - 5$	-0.284 (-0.88)	-0.313* (-0.97)
Constant	0.769*** (8.17)	0.675*** (8.10)	Constant	0.798*** (10.38)	0.794*** (10.32)
N	1,737	1,737	N	1,737	1,737
R^2	0.0232	0.0218	R^2	0.0375	0.0367

Note. t -Statistics in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable takes value 1 in the year of the reform and 0 otherwise.

7. Results

General Findings

The main findings are reported in Table 8. First, the effect of the reforms on the relative flows of workers on temporary contracts (columns 1 and 2 of Table 8), as defined in equations (2) and (3), is shown. If the reforms promoted the utilization of certain types of temporary contracts, a change in the relative flow of workers hired on those contracts on the total employment flow is expected. The relative contribution of short-term contracts to the total flow of employment is found to be higher by approximately 5 percentage points due to

TABLE 8
Relative Employment and Mismatch Flows

	<i>Short term</i>	<i>Apprenticeship</i>	<i>Mismatch</i>	<i>Over-education</i>	<i>Under-education</i>	
<i>S</i> _{Reform}	0.0497** (2.12)	0.0023 (0.22)	-0.0573*** (-3.12)	-0.0464*** (-2.71)	-0.0108 (-0.70)	
<i>A</i> _{Reform}	0.0188 (0.90)	-0.0028 (-0.30)	0.0192 (1.28)	-0.0002 (-0.00)	0.0192 (1.40)	
<i>SA</i> _{Reform}	-0.0307 (-1.11)	-0.0029 (-0.27)	0.0222 (1.16)	0.0263 (1.37)	-0.0041 (-0.23)	
Constant	0.1163*** (6.06)	0.0495*** (8.00)	0.3031*** (24.03)	0.1962*** (13.23)	0.1069*** (7.98)	
<i>N</i>	59,259	59,259	59,259	59,259	59,259	59,259
<i>R</i> ²	0.0065	0.0026	0.0026	0.0008	0.0013	0.0008

Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is computed as the net flow of workers over the net employment flow. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size.

the implementation of the short-term contract reform, in line with the results of Cappellari *et al.* (2012).⁵ However, the same reform had no effect on the relative flow of workers hired on apprenticeship. According to the estimates, the apprenticeship reform had no significant effect neither on the relative flow of short-term workers neither on the relative flow of apprentices. Also, in firms exposed to both reforms, which is an environment of high flexibility, the relative flow of temporary workers (both short-term and apprentices) did not change. These results point to the fact that the increased flexibility brought up by the short-term contract reform, through the extension of the scope for hiring on a temporary basis, was positively received in the labour market. As a consequence, the relative flow of workers hired on short-term contracts increased, with no substitution effects between apprenticeship and short-term contracts.

Then, the impact of the reforms on the relative flows of mismatched, over-educated and under-educated workers (columns 3–5 of Table 8), as defined in equations (4)–(6), is analysed. The short-term contract reform significantly decreased the relative flow of over-educated workers by approximately 4.6 percentage points, while no effect was found with regard to under-education. In relation to the apprenticeship reform, no effect on the relative flows of over-educated and under-educated workers is found. No effect is also found on the relative flows of over-educated and under-educated workers in firms exposed to both reforms. These findings suggest that by fostering the diffusion of short-term contracts, the short-term contract reform was effective in significantly reducing the relative flow of over-educated workers. This evidence supports the theory that lifting the restrictions in the utilization of short-term contracts may facilitate the hiring decisions of firms, increase the number of vacancies and favour the firms' adoption of new technologies, leading to better quality matches (Brunello *et al.* 2007; Di Pietro 2002). A robustness test, in which the effect of the reforms is allowed to be time-varying for each year after the

TABLE 9
Relative Employment and Mismatch Flows by Gender

	Short term	Apprenticeship	Mismatch	Over- education	Under- education	
<i>S</i> Reform	0.0754** (2.34)	0.0088 (0.59)	-0.0801*** (-3.23)	-0.0525*** (-2.52)	-0.0276 (-1.42)	
<i>A</i> Reform	-0.0089 (-0.33)	-0.0029 (-0.29)	-0.0015 (-0.05)	-0.0159 (-0.49)	0.0143 (0.94)	
<i>SA</i> Reform	-0.0380 (-0.96)	-0.0159 (-1.15)	0.0493* (1.82)	0.0354 (1.48)	0.0138 (0.65)	
Female	0.0377 (0.89)	-0.0306 (1.54)	-0.0441 (-0.82)	-0.0448 (-0.89)	0.0006 (0.03)	
<i>S</i> Reform Female	-0.0854 (-1.29)	0.0729 (-0.52)	0.0535 (1.38)	0.0302 (0.42)	0.0540 (1.55)	
<i>A</i> Reform Female	0.0722 (1.35)	-0.0580 (-0.31)	0.0795 (1.30)	0.0548 (0.89)	0.0246 (0.62)	
<i>SA</i> Reform Female	0.0288 (0.36)	0.0427 (1.33)	-0.1048* (-1.79)	-0.0395 (-0.70)	-0.0653 (-1.29)	
Constant	0.1183*** (5.03)	0.0527*** (6.36)	0.3191*** (22.08)	0.2094*** (14.64)	0.109*** (10.34)	
<i>N</i>	59,259	59,259	<i>N</i>	59,259	59,259	
<i>R</i> ²	0.0108	0.0029	<i>R</i> ²	0.0006	0.0016	
<i>S</i> Reform Female ($\alpha + \delta$)	-0.0099 (-0.20)	-0.0063 (-0.29)	<i>R</i> ²	-0.0072 (-0.18)	-0.0336 (-0.93)	0.0264 (0.95)
<i>A</i> Reform Female ($\beta + \lambda$)	0.0632 (1.51)	-0.0087 (-0.50)	<i>R</i> ²	0.0779 (1.65)	0.0389 (0.95)	0.0390 (1.11)
<i>SA</i> Reform Female ($\gamma + \theta$)	-0.0092 (-0.16)	0.0267 (1.03)	<i>R</i> ²	-0.0554 (-1.12)	-0.0040 (-0.09)	-0.0514 (-1.19)

Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is computed as the net flow of workers over the net employment flow. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size. The last three rows report the coefficients and *t*-statistics for the group of female workers.

implementation of the reform and in which a trend variable and interacted time and treated group dummies for all the periods before and after the treatments are added, confirms the main results. This test shows a significant effect for the year of the treatment, informing about the simultaneous impact of the reform. Although a significant coefficient on mismatch is also observed two years after the reform, the corresponding coefficient on over-education is negative, but not statistically significant. Overall, these results point to a robust short-term effect of the reform, while persistence in the medium term is revealed to be weak (Table 11 in the online Appendix).

Heterogeneous Effects

(a) Males and females

Following recent research (Davia *et al.* 2017) which suggests that over-education is more common among females as a consequence of the workers' decisions to occupationally downgrade in order to achieve an improved work-life balance, asymmetric effects by gender are explored (Table 9). While

the apprenticeship reform did not have any effect on the relative flows of temporary and mismatched workers, independent of gender, interesting asymmetric effects are found for the short-term contract reform. First, an increase in the relative flow of workers on short-term contracts ascribable to the short-term contract reform is observed only among males, while no effect of the reform on the relative flows of temporary female workers is found. Second, in terms of mismatch, the short-term contract reform significantly reduced the relative flow of over-educated workers by 5.2 percentage points only among men, with no effects among women. Hence, the increased flexibility brought by the short-term contract reform was effective in improving the quality of the matches, but among male workers only. As explained by Engellandt and Riphahn (2005), this may be due to the fact that women seek temporary employment for different reasons compared to men. Women may more frequently self-select in non-screening types of temporary employment compared to men because this better matches their higher propensity to move on to non-market employment. Therefore, this hypothesis could suggest that women in temporary employment *ceteris paribus* may provide less effort than men, who are instead more likely to seek career advancement.

(b) Young and older workers

Asymmetries among workers in the age group 15–24 (Table 10), which according to the literature are disproportionately more likely to be hired on temporary contracts (García-Pérez and Muñoz-Bullón 2011), are then explored. On average, the relative flow of 15–24 young workers hired on both short-term and apprenticeship contracts is higher by approximately 8.6 and 12.5 percentage points, compared to older workers. However, while the short-term contract reform did not have any significant effect on the relative flows of 15–24 years old workers hired on short-term or apprenticeship contracts, it increased the relative flow of older workers hired on short-term contracts by 5.8 percentage points. There is weak evidence of the apprenticeship reform reducing the relative flow of 15–24 years old workers hired on short-term contracts by 7.6 percentage points, while increasing the same flow among older workers by 4.2 percentage points. In firms exposed to both reforms, the relative flow of older workers hired on short-term contracts decreased by approximately 5.6 percentage points, while no effect was found for the 15–24 years old cohort.

In terms of mismatch, the coefficients of the change in mismatch flows ascribable to the reforms are all statistically insignificant for the 15–24 years old category. Nevertheless, the relative flow of over-educated older workers is lower by 4.8 percentage points as a consequence of the short-term contract reform, which confirms the general results reported in Table 8. Our findings seem to suggest that the increased flexibility brought by the short-term contract reform was beneficial for older workers in terms of reduced over-education; however, younger workers were not significantly affected. Hence, no evidence is found in support of the ‘waiting room effect’ hypothesis.

TABLE 10
Relative Employment and Mismatch Flows by Age Groups (15–24)

	Short term	Apprenticeship	Mismatch	Over-education	Under-education
<i>SReform</i>	0.0589** (2.11)	0.0068 (0.50)	-0.0574** (-2.39)	-0.0488** (-2.27)	-0.0085 (-0.49)
<i>AReform</i>	0.0420* (1.65)	0.0040 (0.35)	0.0099 (0.60)	-0.0084 (-0.33)	0.0183 (1.21)
<i>SAReform</i>	-0.0565* (-1.78)	-0.0190 (-1.35)	0.0277 (1.30)	0.0304 (1.31)	-0.0027 (-0.13)
Age 15–24	0.0868* (1.64)	0.1248*** (5.26)	-0.0376 (-1.31)	-0.0490 (-1.04)	0.0167 (0.53)
<i>SReform</i> Age 15–24	-0.0521 (-1.00)	0.0170 (0.58)	0.0210 (0.48)	0.0798 (0.73)	-0.0100 (-0.29)
<i>AReform</i> Age 15–24	-0.1186** (-2.21)	-0.0062 (-0.25)	0.0387 (0.94)	0.0395 (0.70)	-0.0008 (-0.03)
<i>SAReform</i> Age 15–24	0.1327** (2.34)	0.0202 (0.55)	-0.0434 (-0.86)	-0.0381 (-0.66)	-0.0053 (-0.13)
Constant	0.0612*** (4.46)	-0.0398*** (-5.49)	0.3100*** (21.22)	0.1585*** (14.49)	0.1044*** (7.67)
<i>N</i>	59,259	59,259	59,259	59,259	59,259
<i>R</i> ²	0.0085	0.1811	0.0016	0.0034	0.0011
<i>SReform</i> Age 15–24 ($\alpha + \delta$)	0.0067 (0.16)	0.0239 (1.00)	-0.0363 (-1.04)	-0.0177 (-0.55)	-0.0185 (-0.62)
<i>AReform</i> Age 15–24 ($\beta + \lambda$)	-0.0765* (-1.74)	-0.0022 (-0.11)	0.0486 (1.35)	0.0311 (0.70)	0.0174 (0.62)
<i>SAReform</i> Age 15–24 ($\gamma + \theta$)	0.0762 (1.53)	0.0012 (0.04)	-0.0156 (-0.37)	-0.0076 (-0.16)	-0.0080 (-0.23)

Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is computed as the net flow of workers over the net employment flow. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size. The last three rows report the coefficients and *t*-statistics for the group of workers in the age group 15–24.

Moreover, no evidence is found of increased over-education imputable to the role of short-term contracts as an instrument to absorb weaker and older segments of the working population (Dekker *et al.* 2002).

The changes to the legislation brought by the apprenticeship reform were multiple. In fact, as a consequence of the reform, workers are protected by a minimum wage, which has been shown to reduce job turnover rates (Portugal and Cardoso 2006) and, by making the apprenticeship contract more expensive, could have pushed firms to become more selective. Moreover, the age eligibility extension could have also lead older workers (25–29 years old) to be hired as apprentices, potentially increasing the relative flow of over-educated workers. To further investigate these effects, an additional analysis is carried out by restricting the sample to firms which hire only workers below the age of 29. Results show that the apprenticeship contract reform did not have any significant effect neither on the relative flows of 15–29 workers on temporary contracts nor on the relative flows of 15–29 mismatched workers (Table 11). Moreover, when a triple difference-in-differences estimation is performed to isolate the effects of the reforms on workers in the age category

TABLE 11
Relative Employment and Mismatch Flows (Sample of Firms Hiring Workers in the Age Range 15–29)

	<i>Short term</i>	<i>Apprenticeship</i>	<i>Mismatch</i>	<i>Over-education</i>	<i>Under-education</i>
<i>SReform</i>	0.0364** (1.98)	0.0024 (0.21)	-0.0176 (-0.17)	-0.0159 (-1.02)	-0.0017 (-0.02)
<i>AReform</i>	0.0234 (1.44)	-0.0045 (-0.44)	0.0730 (0.86)	0.0220 (1.25)	0.0510 (0.64)
<i>SAReform</i>	-0.0426* (-1.88)	-0.0035 (-0.30)	-0.0067 (-0.65)	0.0057 (0.30)	-0.0730 (-0.74)
Constant	0.0281*** (4.14)	-0.0440*** (6.50)	-0.0510*** (-1.50)	0.1001*** (12.38)	-0.1512*** (-5.03)
<i>N</i>	48,280	48,280	48,280	48,280	48,280
<i>R</i> ²	0.0012	0.0028	0.0003	0.0015	0.0004

Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is computed as the net flow of workers over the net employment flow. The sample includes all firms which hire workers with age below 29. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size.

15–24, all coefficients are statistically insignificant (Table 12). Among the 25–29 age group, the apprenticeship reform increased the relative flow of workers hired on short-term contracts, with no effects on mismatch. Also, in an environment of high flexibility, although the relative flow of 25–29 workers hired on short-term contracts decreased, no effects are found on mismatch.

8. Conclusions

In this article, new light is shed on the relationship between EPL rigidity and mismatch in dual labour economies, by focusing on the impact of two labour market reforms approved in Italy in the early 2000s, which significantly modified the rules for the utilization of short-term and apprenticeship contracts. This research is important from a policy perspective as it provides new insights on the role of temporary contracts in a domain which has been so far little explored, such as qualification mismatch. The findings are mixed, as important positive effects are found as a consequence of the short-term contract reform, but no effect is found as a result of the apprenticeship contract. The lack of any effect of the latter is potentially ascribable to the opposite direction of the amendments: while the age eligibility extension and the increased opportunities for the provision of training increased the flexibility of the contract, the introduction of a minimum wage added an additional restriction to the firms' hiring decision. On the other hand, the short-term contract reform which clearly increased the flexibility of the contract lead to an improvement of the quality of the firm–worker matches. The effect is found to be limited to older male workers, who have higher chances to search for better jobs even when employed, increasing their likelihood to find a perfect match. Women and younger workers, instead, who

TABLE 12
Relative Employment and Mismatch Flows by Age Groups (Sample of Firms Hiring Workers in the Age Range 15–29)

	Short term	Apprenticeship	Mismatch	Over-education	Under-education
<i>SReform</i>	0.0787 (1.62)	0.0093 (0.62)	0.0434 (0.34)	-0.0003 (-0.02)	0.0437 (0.34)
<i>AReform</i>	0.0431** (2.21)	0.0030 (0.26)	0.0513 (0.74)	0.0318 (1.56)	0.0195 (0.30)
<i>SAReform</i>	-0.0712** (-2.48)	-0.0221 (-1.44)	-0.0758 (-0.64)	-0.0162 (-0.74)	-0.0596 (-0.52)
Age 15–24	0.0787*** (3.13)	0.2525*** (13.30)	0.1765*** (2.54)	0.0948*** (4.79)	0.0817 (1.18)
<i>SReform</i> Age 15–24	-0.0070 (-0.12)	0.0093 (0.31)	-0.2120 (-1.01)	-0.0362 (-0.82)	-0.1758 (-0.87)
<i>AReform</i> Age 15–24	-0.0907** (-2.09)	-0.0093 (-0.37)	0.1188 (0.59)	-0.0412 (-0.89)	0.1600 (0.85)
<i>SAReform</i> Age 15–24	0.1014 (1.47)	0.0274 (0.74)	-0.0492 (-0.19)	0.0578 (1.11)	-0.1070 (-0.43)
Constant	-0.0050 (-0.49)	-0.0274*** (-5.48)	-0.1883*** (-3.51)	0.0624*** (5.98)	-0.2508*** (-5.08)
<i>N</i>	48,280	48,280	48,280	48,280	48,280
<i>R</i> ²	0.0141	0.1728	0.0080	0.0320	0.0029
<i>SReform</i> Age 15–24 ($\alpha + \delta$)	0.0353 (0.84)	0.0186 (0.79)	-0.1686 (-1.04)	-0.0365 (-1.05)	-0.1321 (-0.86)
<i>AReform</i> Age 15–24 ($\beta + \lambda$)	-0.0476 (-1.29)	-0.0063 (-0.30)	0.1701 (0.79)	-0.0093 (-0.24)	0.1795 (0.88)
<i>SAReform</i> Age 15–24 ($\gamma + \theta$)	0.0302 (0.54)	0.0053 (0.18)	-0.1250 (-0.54)	0.0416 (0.94)	-0.1666 (-0.77)

Note. *t*-Statistics in parentheses; significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is computed as the net flow of workers over the net employment flow. The sample includes all firms which hire workers with age below 29. All regressions include time and firm fixed effects. Standard errors are clustered at sector level. Regressions are weighted by firm size. The last three rows report the coefficients and *t*-statistics for the group of workers in the age group 15–24.

are the weakest segments of the population, were not significantly affected by the enhanced labour market flexibility, potentially due to the different rationale in their self-selection into temporary employment. This result comes with the caveat of the effect being contemporaneous to the reform, with weak evidence of persistence in the medium run.

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Notes

1. The law also introduced two additional forms of apprenticeship. Students between the age of 15 and 18 can absolve their obligation in regular schools, by participating to full-time vocational courses, or through apprenticeships (type I). In addition, the ‘high-apprenticeship’ allows young individuals (age 18–29) to obtain a secondary- or tertiary-level education degree (bachelor’s or master’s degree) through apprenticeship (type III). The first type of apprenticeship was implemented in Veneto after 2007 (ISFOL 2009). The ‘high apprenticeship’ was experimentally implemented in Veneto in the same sectors in which the collective agreements were renewed. Four masters’ degrees were instituted, of which two terminated in 2007 and two in 2008 (ISFOL 2010). Due to the small number of apprentices involved (37 students), the focus is on the vocational apprenticeship contract.
2. The classification of treated and control sectors may be subject to a misclassification problem, as the renewal of agreements has been implemented at micro-sector level and it is difficult to collect information about all the collective agreements in the period considered. However, the classification of Cappellari *et al.* (2012) and Albanese *et al.* (2017) is used, the identification strategy has been discussed with the authors and the same sources have been used. The misclassification error should therefore be minimal and confined to a limited number of observations.
3. The size of the attrition is difficult to compute as the workers who move away from Veneto are not observed and the institution which provides the data does not release such information. However, using the administrative employer–employee Veneto Worker History dataset (Card *et al.* 2014), which covers the universe of firms and workers in Veneto until 2001, the attrition rate is computed to be 3 per cent in 1998–2001.
4. These data are distributed by Bureau van Dijk, and are available from 1995 onwards for all corporate (non-financial) firms with annual turnover above € 500,000.
5. A direct comparison with the results of Cappellari *et al.* (2012) is not possible: first, data representative of a single Italian region are used, while their results are based on the Italian firms population; second, the identification strategy used in this article cannot take advantage of the variation in the implementation of the apprenticeship reform at regional level; third, the definition of turnover in this article is not computed using the stocks of workers.

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Additional supporting information may be found online in the Supporting Information section at the end of the article.

Supporting Information