

Two-tier labor market reform and entry wage of protected workers: evidence from Italy

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Abstract This study aims at investigating whether wages of workers entering positions entitled to employment protection are affected by the introduction of a two-tier labor market regime. Difference-in-differences estimators—also combined with propensity score matching—are applied to repeated cross section microdata to evaluate the impact of a reform deregulating the Italian labor market in 2003. The results are robust and show that after the policy implementation protected entrants experienced a reduction in earnings of about -4.5% .

Keywords Wage · EPL · Flexibility at the margin · Difference-in-differences

JEL Classification J63 · J64

1 Introduction

In the recent past, many European countries experienced in-depth deregulation of labor markets. In order to cope with high unemployment rates, many governments made use of policy instruments targeted to obtain decentralization of the collective bargaining system and employment flexibility. Over the past fifteen years, a substantial amount of research has been devoted to the comprehension of the effects of these labor mar-

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ket policies mainly focusing on their impact on labor utilization and unemployment. Among others, [Boeri and Jimeno \(2005\)](#), [Boeri and Garibaldi \(2007\)](#), [Hijzen et al. \(2013\)](#) and [Nickell et al. \(2005\)](#) highlight the relevance of the issue for unemployment flows and unemployment duration. More recently, some authors look at the impact of flexibility on productivity, finding mixed results ([Autor et al. 2007](#); [Bassanini et al. 2009](#); [Jona Lasinio and Vallanti 2011](#); [Leonardi and Pica 2013](#)). [Boeri \(2010\)](#) points out that it might be important to separate the impact of EPL on wage of existing insiders from that of entrants. According to this author, two-tier reforms generate a widening of institutional asymmetries that may affect the bargaining position of insiders and increase the rents of outsiders. Hence, insider workers could experience a reduction in earnings due to a change in their bargaining power or to a downward shift of labor demand. Moreover, in case of a two-tier regime, entrants may have different contracts and it is relevant to distinguish between protected and unprotected positions. It has also been recognized that wage differentials across job contracts may reflect productivity gaps associated with firms' sorting behavior ([Berton and Garibaldi 2012](#)). Indeed, the impact of deregulation on both wage and productivity is in principle ambiguous, and it is not surprising that the empirical evidence is also inconclusive. As things stand, the evaluation of the effects of deregulation on wage setting and wage differentials is still an open issue. The purpose of this study is to provide evidence on this respect.

In this work we aim at assessing whether the creation of a two-tier employment protection regime has an impact on wage of protected entrants. This effect may arise through a change of turnover costs which affects wage by modifying workers' reservation wage, the availability of outside options for firms and, consequently, the bargaining outcome. This topic is particularly relevant since it may contribute to the understanding of the determinants of wage inequality among workers subject to different employment security regimes and to figure out the possible effects of a further deregulation of labor market.¹ This is a political and economic *vexata quaestio*.

The empirical background of the present study is the following. In late 2003, Italy undertook a severe labor market deregulation characterized by the so-called flexibility at the margin. In particular, after the reform, albeit workers in permanent jobs entirely maintained their protections, firms could create new temporary positions by using new contractual forms for fixed-term employment. Since in Italy employment protection varies according to firm's size, the availability of flexible contracts turns out to be particularly useful for firms constrained by restrictive job security provisions in order to circumvent turnover costs imposed by the law. This normative setting generates an exogenous threshold which can be used to construct a control group to apply difference-in-differences (DD) procedure intended to evaluate the impact of flexible contracts on wage of protected employees. However, this approach is not straightforward since several caveats may arise. In particular, the introduction of new types of fixed-term contracts could generate flows of workers across employment status and firm size, undermining the difference-in-differences analysis. To cope with this issue, we use proxies for individual ability and parental background and we also implement propensity score matching techniques coupled with difference-in-difference methodology as

¹ [Cappellari et al. \(2011\)](#) and [Picchio \(2006\)](#) find persistent wage differentials between permanent and temporary workers in Italy. Similar results have been found in [Mertens et al. \(2007\)](#) for Germany.

in [Blundell et al. \(2004\)](#). The empirical analysis is carried out on data covering recent university graduate workers. The results show that after the creation of a two-tier labor market, employees who entered positions entitled to legal protection experienced a reduction in earnings of about -4.5% . This is consistent with a scenario wherein the presence of flexible jobs leads to an underbidding of entry wage of protected workers.

The paper is divided as follows. In Sect. 2 the Italian institutional setting is briefly described along with the characteristics of the implemented reform. Section 3 presents our dataset and discusses the empirical model as well as the identification strategy. Section 4 contains the results and presents several robustness and falsification tests. In Sect. 5 concluding remarks are addressed.

2 The Italian labor market and the 2003 reform

The implementation of the legislative decree 276/2003, definitely in charge in December 2003, represented one of the most significant shocks to the Italian labor market. The reform aimed at regulating new temporary job contracts in order to bypass limits imposed by the Italian law to firms with more than 15 employees. In fact, since 1973, the Italian legislation allows for individual dismissal only if it is justified by a *just cause* rule. The courts' reports have established that only misconduct can be considered as *just cause*, while economic reasons cannot. If the dismissal is considered unfair, workers are entitled to a compensation which crucially varies according to firm size. While firms employing less than 15 employees must pay to the worker only a monthly forfeit, firms employing more than 15 workers have to entirely pay the forgone wages and, most importantly, they must re-hire the worker.² The labor market reform of 2003 comes after a previous attempt to deregulate the labor market that took place in late 1997 (Law 197/1997). This law increased flexibility by introducing temporary contracts. However, jobs created under these contractual forms must be either destroyed or transformed into permanent positions when they expire. Efforts to increase labor market flexibility have been then taken forward with the 2003 reform. The new norms further deregulated the use of atypical work arrangements and introduced *para-subordinate* jobs (*lavori a progetto*). These are occasional jobs that cannot be configured as self-employment since they have no economic risk and they are rewarded with pure wage compensations. The Italian labor market has been deeply transformed by the introduction of these types of occupations since, although *para-subordinate* jobs can be created only in the presence of a specific project that is somehow different from the main firm's activity, there is a wide consensus among legal experts concerning the fact that these contracts hide de facto subordinated jobs involved in primary business activities ([Ichino 2008](#)). This new regime can be used to repeatedly hire the same worker into the same job eluding norms for standard subor-

² The 15 employees' threshold is computed by considering the specific establishment rather than the whole firm. However, in case the single plant belongs to a firm employing more than 60 employees in the same province, the most binding employment protection applies independently of plant size. To fix the threshold, apprentices and temporary workers with tenure shorter than nine months are not considered, while part-time workers and all other temporary contracts are included.

dinate positions. In Italy, workers employed with these contractual arrangements are known as *precari*.³

It is apparent that the creation of such a two-tier regime has a different impact on firms according to existing legal constraints regulating hiring and firing costs. In particular, large firms could benefit from the new institutional setup since it enables them to use labor contracts that can continually be renewed without incurring turnover costs imposed by the law. In this new setting, permanent workers in large firms face a new bargaining situation since firms have gained credible outside options which can be used to improve their bargaining outcome lowering wages of protected workers (see Muthoo 1999). On top of that, it is crucial to remark that this form of outside option turns out to be credible only for new entrants since the bargaining power of existing insiders is secured by firing costs.

3 Strategy and data

3.1 The identification strategy

The empirical frame relies on repeated cross sections recording individual labor market outcomes. We separate workers whose job started before the 2003 reform from those employed under the new regime, and we apply difference-in-differences procedure (DD). The identification strategy is based on the exogenous threshold separating firms in terms of dismissal constraints. We build up a control group consisting of individuals employed in firms with less than 15 employees since turnover costs of them were not affected by the introduction of the reform. We compare wages of these workers with those of individuals employed in large plants, in order to establish whether the introduction of a brand new form of *unprotected entrants* has affected wages of *protected workers*.

Formally, we estimate the following wage equation:

$$w_i = \mathbf{X}_i \boldsymbol{\beta} + \delta_0 \text{post2003}_i + \delta_1 (\text{firm15})_i + \delta_2 \text{post2003}_i * (\text{firm15})_i + u_i \quad (1)$$

where i indicates the generic individual with a permanent contract and $\text{post2003} = \{0, 1\}$ is a dummy variable equal to 1 if the job started after the reform. The dependent variable is the logarithm of monthly real wage earned by individual i . In the RHS of Eq. (1), \mathbf{X} indicates a set of control variables, while $\text{firm15} = \{0, 1\}$ indicates the “treatment” and takes the value of 1 if individual i is employed in a plant with more than 15 employees. Our parameter of interest is δ_2 which measures the relative variation in wage of permanent workers in large plants after the reform compared to that of permanent workers in small ones. Equation (1) will be modified according to different specifications and tests we discuss in Sect. 4.

³ Interestingly, Blanchard and Landier (2002) use the French word *precarité* to define the fact that in France low productivity workers always move from one job to the other because their job position will never be converted into a permanent one. In Italy the idea of *precariato* is used in a different way: it defines workers who are in the same unstable job that, when expires, can be either destroyed or renewed.

3.2 The data

The empirical investigation is based on three repeated cross sections coming from surveys carried out by the Italian National Statistical Institute (ISTAT) on the labor market outcomes of representative samples of young skilled workers. These surveys, known as *Indagine sull'Inserimento Professionale dei Laureati*, cover only university graduates who entered the labor market in 1998, 2001 and 2004 and were interviewed three years later. Hence, data have been collected in 2001, 2004 and 2007, respectively.⁴ Workers in our samples are 73,088 individuals owning a university degree obtained after a 4/5 years course of study (basically B.Sc. plus M.Sc. degree).⁵ In Appendix, Table 8 defines our variables, while Table 9 and Table 10 contain some representative statistics of our samples in terms of academic/personal characteristics and labor market outcomes, respectively. Graduates in medicine have been excluded from our analysis since after graduation they are usually involved in a three-year postgraduate course; hence, they are likely not to be in the labor market at the time of the interview. We rely on these specific repeated cross sections for three main reasons.

Firstly, in these surveys the labor market outcomes of individuals interviewed in 2001 are recorded *before* the reform, while those of individuals interviewed in 2007 are recorded *after* the reform. These two samples cover a 10-year period (1998–2007) and would be sufficient to derive sensible results. Moreover, we have information from the 2004 sample which contains data on workers employed under both the new and the old regimes. Within this specific sample, we can separate those workers who have been employed after the reform from the others since we know the starting date (year and month) of the current job.

Secondly, for employed workers several characteristics of job position are described and, among them, indication concerning the number of workers employed in the single plant where each graduate works is provided. This is relevant in order to assess whether individuals are entitled to employment protection. We are aware of the potential error that may arise when evaluating the dimension of a single plant by relying on information derived from worker's answer instead of administrative data. Indeed, the main weakness of this assessment arises because interviewed workers may consider colleagues employed part-time as full-time workers, while, from a legal perspective, they should actually account proportionally to the hours they work in order to establish plant's dimension. On top of that, the 15 employees' threshold may turn out to be problematic because, whenever the single plant is part of a larger firm employing more than 60 employees in the same province where the plant is located, employment protection applies independently of the number of employees. Both of these aspects may induce a downward bias in our DD estimates since some treated individuals for which

⁴ From now on, we refer to these samples as 2001, 2004 and 2007. However, the reader should keep in mind that the date refers to the date of the interviews, while workers entered the labor market three years earlier.

⁵ The 2007 survey explicitly separates graduates who, after a university reform implemented in 2001, enrolled at universities under the new higher education system. Since the old system was in charge along with the new one, the ISTAT survey collected two separated representative samples for students for both systems. We use only the survey covering the old system which is fully comparable with the previous ones.

employment protection applies may actually end up in the control group. However, as we discuss in detail in Sect. 4, we implement many robustness checks, showing that our results do not hinge neither on possible measurement errors nor on the use of a biased control group.

Finally, all surveys report the type of labor contract. This information is crucial since it makes possible to separate worker categories and to construct alternative control groups, namely temporary workers and self-employed, rendering possible the implementation of several robustness and placebo tests.

3.3 Addressing some *Caveats*

The approach highlighted in paragraph 3.1 is not straightforward. At the outset, it should be recognized that during the period under investigation, the Euro currency was definitely introduced in Italy. Many would argue that large firms benefited from the adoption of the single currency more than the smallest ones in terms of foreign demand. This may have induced changes in relative employment and productivity differentials between large and small firms casting some doubts on the causal interpretation of the results. To tackle this issue, we make use of an alternative control group, i.e., temporary workers in large and small firms who are not entitled to employment protection. Furthermore, self-employed individuals represent a valuable alternative control group. Using these peculiar categories of workers, we undertake additional robustness and falsification tests. By using the above-mentioned control groups, we can also check for possible bias arising by misreporting of plant's dimension.

A further concern is related to possible anticipation effects deriving from the fact that the reform was announced in February 2003 and then introduced in September. Indeed, people could have changed their behavior before the reform so that our parameters could be biased. We tackle the issue by presenting a robustness check made by excluding from our analysis all workers employed during 2003 to avoid distortions related to prospective behavior anticipating the effect of the reform.

On top of that, it should be pointed out that the introduction of a new type of fixed-term contract could generate flows of workers across type of contracts, employment status and firm size. Moreover, the characteristics in terms of ability and productivity of workers hired in each reference group could also change after the reform. Although we use several controls and proxies for individual ability, we further address the issue by making use of propensity score matching techniques coupled with difference-in-differences methodology (PSDD) as in [Blundell et al. \(2004\)](#). Matching procedures ensure that each treated individual is compared only with his/her control groups' counterparts who are similar in terms of observable characteristics. Coupling a propensity score matching procedure—which is only able to deal with observable confounders—with a DD approach offers the scope for representing an unobserved determinant of individual exposure to treatment decomposed into group and time-specific components of the error terms. In our specific case of interest, starting from treated individuals after the reform, i.e., those employed with an open-ended contract in large plants after December 2003, by applying a selected matching procedure we construct three coun-

terfactuals (treated before the reform and controls before and after the reform), to implement the PSDD estimator.

Finally, we should take into account that wages are reported only for employed workers and selection into employment could be a potential source of bias for our results. As far as the reform affected employment probability, we could confound the effect of the policy on wages with that arising from changes of selection into employment due to the reform. To deal with this issue, we estimate simultaneously the wage equation along with a selection equation to control for the probability of being employed. In this way, we can control for possible effects of changes in the probability of being employed on wages.

3.4 Wage patterns and labor contracts

At this stage, it is interesting to show wage patterns for the period 1998–2007. In Fig. 1 we plot average monthly real wages of full-time dependent workers according to the date of job start. We consider only dependent workers classified in four categories, namely temporary and permanent employees in plants with more or less than 15 employees. Some insights can be gathered by inspecting these series. Differences across contracts and plants' dimension are as expected. Workers employed in large plants under permanent contracts are located at the top tail of the wage distribution, while, at the opposite, temporary workers in plants with less than 15 employees are located at the bottom. In order to give a picture of the wage behavior before and after the labor market reform of 2004, we report time trends allowing for a break in 2004. Albeit over the considered period real wages of graduate workers are slightly decreasing, we notice that all series seem to rise up from 2003 to 2004. The reason may be that these years have been characterized by a cyclical upturn. As remarked by the [Bank of Italy \(2005\)](#) annual report, in 2004 the Italian GDP showed a growth rate of 1.5% which turns out to be the largest growth rate of the entire considered period. The main causes have been widely identified in the positive expansion of

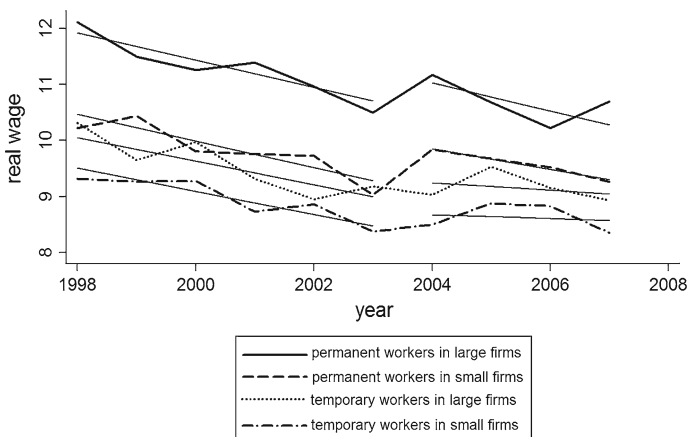


Fig. 1 Average monthly real wage of permanent and temporary workers according to plant dimension

the entire world economy boosted by real interest rate decrease as well as by the outgoing fiscal and monetary policies adopted in the USA after the terrorist attack of 2001. Furthermore, additional explanations may reside in the propagation of effects of the introduction of the Euro currency after 2002. Despite the overall wage increase in 2004, it seems that the relative advantage of permanent workers reduced after 2004, being the gap between their wages and those of other workers' categories lower than before. It should be outlined that apart from the sudden wage increase in 2004, generally all series present a decreasing trend. Indeed, permanent workers' series have similar trends independently of firm size both before and after the reform. This appears to be consistent with the presence of common time trend required in order to adopt the difference-in-differences methodology presented in Eq. (1). Wage series of temporary workers seem to be slightly flatter after the reform. However, considering confidence intervals, it is not possible to conclude that their trends are significantly different from that of other series. In the light of this preliminary investigation, in our econometric analysis we carry out several tests in order to support the common time trend assumption.

In Fig. 2 we report ratios of temporary over permanent workers by firm size over the period 1998–2007. It is interesting to note that albeit the relative use of temporary contracts appears to be slightly increasing during these years, changes are particularly smooth so that the share of temporary workers in 2000 is rather similar to that recorded in 2006 independently of firm size. Since temporary/permanent ratios behave in a very similar way in large and small firms, wage differentials by firm size should not be driven by composition effects associated with contract changes. We argue that the time variation of these ratios may in part depend on the use of temporary workers to respond to demand fluctuations in the very short run. It is also important to note that time variations are not only similar across firm size but also present before and

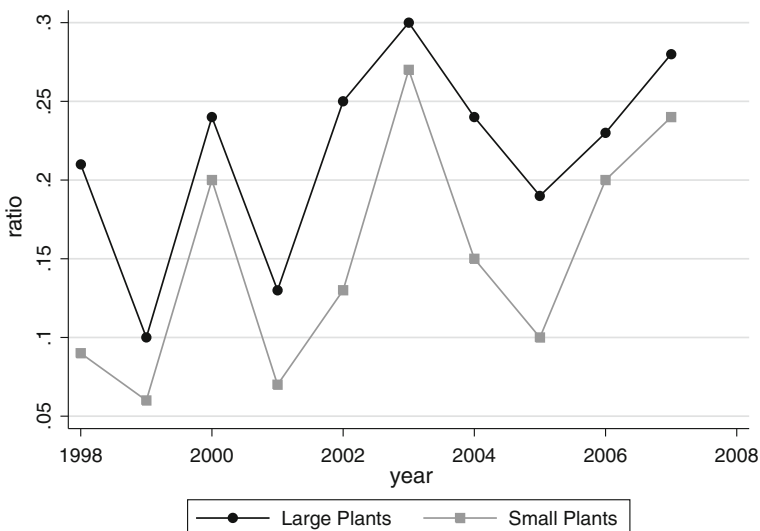


Fig. 2 Ratios of temporary over permanent employees by plant dimension (more or less than 15 employees), 1998–2007

after the reform so that no problematic differences in groups' composition seem to be present in the two sub-periods.

4 Results

4.1 First verification: double differences with multiple groups and time periods

We start our analysis by designing an empirical strategy to apply DD techniques using all available datasets simultaneously. We estimate an interest equation as follows:

$$\begin{aligned} w_{isj} = & \mathbf{X}_{isj}\boldsymbol{\beta} + t_s + \gamma_j + \delta_0 firm15_{isj} \\ & + \delta_1(firm15 * January01_December03)_{isj} \\ & + \delta_2(firm15 * January04_December07)_{isj} + u_{isj} \end{aligned} \quad (2)$$

where i corresponds to individuals and s to the time period (in year) in which the individual i has been interviewed and j indicates groups. t are dummies accounting for sample fixed effects (2001, 2004 and 2007). γ represents fixed effects for workers in plants with more or less than 15 employees. In the RHS of Eq. (1), \mathbf{X} includes 26 control variables (age, gender, marital status, parents' education, time to degree, university majors, university leaving grade, high school leaving grade by 5 types of high school, firm size, public sector, industry and 6 occupational dummy variables) and 19 regional dummy variables. It is important to note that the introduction of proxies for individual's unobserved ability such as high school and university leaving grade and the inclusion of family background aim at reducing the impact that unobserved components may have in determining allocation across different groups. At this stage only permanent workers are considered. $firm15$ is a dichotomous variable taking the value 1 if the individual is employed in a firm whose dimension entitles for employment protection. We separate the entire time span in three sub-periods: January 1998–December 2000, January 2001–December 2003, January 2004–December 2007 so that in Eq. (2) ($firm15 * January01_December03$) and ($firm15 * January04_December07$) are dummy variables taking the value 1 if the individual is subject to employment protection and he/she has found a job in the period January 2001–December 2003 or after December 2003, respectively. The reference dummy considers individuals whose occupation starts between January 1998 and December 2000. It is worth noting that the introduction of the interaction dummy ($firm15 * January01_December03$) allows us to test the common time trend assumption, i.e., to verify the absence of any significant difference in the evolution of wage for workers with and without employment protection during the whole observation period.

Table 1 presents the results obtained by clustering standard errors at plant dimension level in order to face the issue of serial correlation (Bertrand et al. 2004). The coefficient of main interest is δ_2 , reported in column (1), which is equal to -2.6% , and it is statistically significant. This means that entrants entitled to employment protection had a wage loss after December 2003 compared to their prereform peers. The common time trend assumption is verified being the estimated coefficient δ_1 not statistically different from zero. In column (2) of Table 1, we present additional esti-

Table 1 Difference-in-differences estimates with multiple periods

Dependent variable	(1)	(2)	(3)
	Logarithm of monthly real wage		
Method	DD	DD	DD
Coeff.	Only permanent workers	Only permanent workers	Only permanent workers
<i>(firm15 * January01_December03)</i>	-.015	-.016	-.016
	(.116)	(.110)	(.110)
<i>(firm15 * January04_December07)</i>	-.026**	-.046**	-.047**
	(.043)	(.037)	(.038)
<i>(Job start - year) * (firm15) fixed effects (9)</i>	No	No	Yes
<i>Job start-year fixed effects (9)</i>	No	Yes	Yes
<i>Firm size fixed effects (2)</i>	Yes	Yes	Yes
<i>Survey fixed effects (3)</i>	Yes	No	No
<i>Clustered S.E.</i>	Yes	Yes	Yes
<i>Control Var. (26)</i>	Yes	Yes	Yes
<i>Regional dummies (19)</i>	Yes	Yes	Yes
<i>Obs.</i>	18,555	18,555	18,555
<i>R²</i>	.22	.23	.23

OLS estimates. The dependent variable is the log of monthly real wage. Robust p values in parentheses. t statistics clustered at the firm size level, only workers employed with a permanent contract considered and $firm15 = 1$ if an individual is employed in a plant with more than 15 employees. *January04_December07* is a dummy variable equal to 1 if the individual has been employed after December 2003. *January01_December03* is a dummy variable equal to 1 if the individual has been employed from January 2001 to December 2003. In column (2) *Job start-year* fixed effects used instead of survey fixed effects. In column (3) the same specification of column (2) is estimated and firm size fixed effects for each *Job start-year* have been included. In all columns, 19 regional dummies and 26 control variables (age dummies, gender, marital status, 5 major dummies, university leaving grade, degree on time, high school leaving grade by 5 types of high school, parents' education, 4 firm size dummies, dummies for the public sector, industries and 6 occupational dummies) are included

mates derived including among regressors year fixed effects instead of survey fixed effects. In this case we are using information concerning the date of job start for each employed individual. Our results appear to be robust also according to this additional specification. Finally, in column (3) we report estimates obtained including time-varying large plant specific effects. This approach has the advantage of taking into account the concerns raised by Conley and Taber (2011) about the inconsistency of the difference-in-differences estimation when the treated group and the number of policy changes are small. Our approach accounting for time-varying large-plant-specific effects is perfectly in line with the solution proposed by these authors. As in the previous case, only the coefficient δ_2 is statistically significant showing a point estimate of -4.7% .

4.2 Second verification: addressing confounding trends

A key concern arises at this stage. Albeit previous results are robust according to several specifications, there can still be systematic differences between small and large firms. In particular, it is possible to argue that after the adoption of the single currency across Europe occurred in 2002, large firms benefited of larger spillover compared to smallest ones. As large firms do typically more business abroad, under the assumption that the single currency fostered somehow foreign demand and investments it is well possible that the introduction of the single currency induced changes in relative employment and productivity differentials between large and small firms. We could then confound the impact of the labor market reform with the Euro consequence.⁶ In order to control for possible confounding trends, we apply the following strategies.

4.2.1 Robustness 1: using temporary workers to construct triple differences

We make use of an alternative control group consisting of temporary workers. These are those who do not benefit from employment protection independently of firm size; hence, they represent a natural candidate to implement a triple differences strategy to check for the presence of confounding trends. To build up the empirical model, we proceed as follows. Firstly, we separate workers according to plant dimension. Secondly, we separate between workers with a temporary or a permanent contract. Then we construct the difference within temporary workers and the difference within permanent workers according to plant dimension. By differentiating out these two differences, we obtain the triple differences (DDD) estimate of the causal effect of the 2003 reform on the wage of workers entitled to employment protection. Preliminary results are reported in column (1) of Table 2. In this table, the dummy *Permanent* is equal to one if the individual is employed as a permanent worker. This dummy is interacted with ($firm15 * January01_December03$) and with ($firm15 * January04_December07$) where *firm15* indicates if the individual is employed in a plant with more than 15 employees. The coefficient of interest is that associated with the variable ($firm15 * January04_December07$) * (*Permanent*) since it measures the relative variation after December 2003 of the wage differential between permanent and temporary workers in large and small plants. This approach has the advantage of raising the sample size to about 30,000 observations. The estimated parameter is significantly negative and close to previous values, i.e., -4.6% . This confirms that the impact of the two-tier reform is in the direction of a reduction in the entry wage of permanent workers in large plants more than that of those employed in small ones. In column (2) of Table 2 we present additional estimates derived including among regressors year fixed effects. Our findings appear to be robust according to this additional specification too. Finally, in column (3) we report more robust estimates including among regressors time-varying large plant specific effects. As in previous cases, only the coefficient associated with

⁶ We remark that albeit in 2007 an important recession started in Europe, in Italy the effects of the downturn show only in 2008. As a consequence, issues related to the recession should not affect the labor market outcomes of individuals belonging to the final tail of last wave of our sample.

Table 2 Triple differences estimates with multiple periods and groups

Dependent variable	Logarithm of monthly real wage		
	(1)	(2)	(3)
Method	DDD	DDD	DDD
Coeff.	Permanent and temporary	Permanent and temporary	Permanent and temporary
<i>firm15 * January01_December03</i>	.001 (.400)	.005 (.452)	.005 (.426)
<i>firm15 * January04_December07</i>	.042 (.111)	.005 (.263)	.001 (0.778)
<i>(firm15) * (January01_December03) * (Permanent)</i>	-.025 (.160)	-.025 (.160)	-.024 (.163)
<i>(firm15) * (January04_December07) * (Permanent)</i>	-.046** (.045)	-.037** (.021)	-.045** (.019)
<i>(Job start - year) * (firm15)</i> fixed effects (9)	No	No	Yes
<i>Job start-year</i> fixed effects (9)	No	Yes	Yes
<i>Firm size</i> fixed effects (2)	Yes	Yes	Yes
Survey fixed effects (3)	Yes	No	No
Clustered S.E.	Yes	Yes	Yes
Control Var. (26)	Yes	Yes	Yes
Regional dummies (19)	Yes	Yes	Yes
Obs.	29,627	29,627	29,627
R^2	.24	.25	.25

OLS estimates. Robust p values in parentheses (t statistics clustered at the firm size level). The dependent variable is the log of monthly real wage. In all columns workers employed with either permanent or temporary contract are considered; $firm15 = 1$ if the individual is employed in a plant with more than 15 employees. $Permanent = 1$ if the individual is employed with a permanent contract. *January04_December07* is a dummy variable equal to 1 if the individual has been employed after December 2003. *January01_December03* is a dummy variable equal to 1 if the individual has been employed from January 2001 to December 2003. In column (2) *Job start-year* fixed effects used instead of survey fixed effects. In column (3) the same specification of column (2) is estimated and firm size fixed effects for each *Job start-year* have been included. In all columns, 19 regional dummies and 26 control variables (see note of Table 1) are included.

*(firm15 * January04_December07) * (Permanent)* is statistically significant with a point estimate of -4.5% .

4.2.2 Robustness 2: using self-employed as alternative control group

A further check is carried out using observations referred to self-employed individuals. They are about 8000 workers (Table 9), and they are not affected by the reform. By comparing affected and unaffected occupations according to firm's dimension, we can further assess whether the 2003 reform had a negative effect upon protected

Table 3 Difference-in-differences estimates: robustness and falsification using self-employed

Dependent variable	(1)	(2)	(3)	(4)
	Logarithm of monthly real wage			
Method	DD	DD	DD	DD
Coeff.	≥ 15 Empl. versus self-empl.	≥ 15 Empl. versus self-empl.	< 15 Empl. versus self-empl.	< 15 Empl. versus self-empl.
<i>firm15 * January01_</i> <i>December03</i>	-.126 (0.119)	-.123 (0.129)	-.103 (.161)	-.095 (.179)
<i>firm15 * January04_</i> <i>December07</i>	-.115* (.072)	-.115** (.050)	.086 (.104)	.086 (.101)
<i>Job start-year</i> fixed effects (9)	No	Yes	No	Yes
Survey fixed effects (3)	Yes	No	Yes	No
Clustered S.E.	Yes	Yes	Yes	Yes
Control Var. (26)	Yes	Yes	Yes	Yes
Regional dummies (19)	Yes	Yes	Yes	Yes
Obs.	20,692	20,692	10,151	10,151
R^2	.16	.17	.14	.15

OLS estimates. Robust p values in parentheses (t statistics clustered at the job-type level). The dependent variable is the log of monthly real wage. In columns (1) and (2) only workers employed with a permanent contract in plant with more than 15 employees and self-employed workers are considered; *January04_December07* is a dummy variable equal to 1 if the individual has been employed after December 2003. *January01_December03* is a dummy variable equal to 1 if the individual has been employed from January 2001 to December 2003. In columns (3) and (4) the sample is restricted to individuals employed in plants with less than 15 employees with a permanent contract and to self-employed. Twenty-six control variables and 19 regional dummies included in all specifications as well as 3 sample-year specific effects. In all columns $firm15 = 1$ if the individual is a dependent worker. In columns (2) and (4) *Job start-year* fixed effects used instead of survey fixed effects

individuals. We start by considering only self-employed and permanent employees in large plants. We estimate the same setup of Eq. (2), and in this case *firm15* is a dummy variable equal to 1 only for dependent workers with a permanent contract. Results reported in column (1) of Table 3 are as expected. The coefficient associated with (*firm15 * January04_December07*) is statistically significant. The point estimate is larger than previous ones (-11.0%) since reference categories are different. This means that after December 2003 permanent workers in plants with more than 15 employees earn less than in the period 1998–2000 compared to self-employed. This difference is not present in the period January 2001–December 2003 as δ_1 is not significantly different from zero; hence, the common time effects assumption is verified also in this case. In column (2) of Table 3 estimates for δ_1 and δ_2 obtained by using year fixed effects confirm these findings. A falsification exercise is also presented. Columns (3) and (4) of Table 3 contain the results obtained by restricting the sample to self-employed workers and dependent employees in small plants with a permanent contract. In this case the falsification is implemented considering as treated dependent

workers with a permanent contract. As expected, no coefficient is statistically different from zero.

4.3 Third verification: addressing anticipation effects

A matter of concern in the interpretation of our results may be related to the fact that the incoming reform was known before the date of its actual legal enforcement. Indeed, the reform was announced in February 2003 so that people may have changed their behavior before the reform was actually implemented. We address the issue by presenting a further check made by excluding from our analysis all workers employed during 2003, avoiding potential distortions arising from the presence of wages that could be the outcome of prereform anticipation effects. The use of this procedure should reassure us that the estimated coefficients are not biased by some behavior that preceded the introduction of the reform and could otherwise be confounded with the effect of it. In Table 4 we report the estimated coefficients of our main models excluding year 2003 from our sample. In particular, column (1) contains the DD comparison of protected workers in large and small firms, column (2) refers to the triple differences model, while column (3) compares protected workers in large firms and self-employed. Overall the table shows that previous results are confirmed.

4.4 Fourth verification: addressing composition changes within groups

The DD estimates we have presented so far rely on the crucial assumption of the absence of systematic composition changes within each group. However, in our case there could be reasons to cast some doubts on the validity of this assumption. The introduction of new fixed-term contracts usually seeks to reduce unemployment and, consequently, may alter employment flows. If this is the case, the characteristics of workers hired after the reform in large plants with open-ended contracts may differ from those of workers hired before. This may be the consequence of a sorting process adopted by firms since a larger menu of contracts becomes available. In this case our DD estimates would be inconsistent. Moreover, whenever firms offer permanent positions to workers who are “better” along measured or unmeasured attributes, we could underestimate the true effect of the reform on wage of protected workers. In Table 11 in “Appendix” we provide evidence concerning observed characteristics of workers hired before and after the reform by type of contract, including self-employed, and firm size. The reported statistics do not highlight any particular composition change affecting treated individuals differently from other groups and show an almost static picture across groups. However, composition changes occurring along unobserved characteristics, if present, may still undermine our estimates.

Even though we use several proxies for individual productivity such as high school leaving grade and university leaving grade as well as proxies for parental background which are likely to reduce the impact of personal characteristics on wage determination, we can further alleviate this concern by estimating a semi-parametric PSDD model as proposed by [Blundell et al. \(2004\)](#) among the first. This methodology applied to repeated cross sections allows to identify counterfactual cases across different samples

Table 4 DD and DDD models excluding workers employed during 2003

Dependent variable	(1)	(2)	(3)
	Logarithm of monthly real wage		
Method	DD	DDD	DD
Coeff.	Only permanent workers	Permanent and temporary	≥ 15 Empl. versus self-empl.
<i>firm15</i> * <i>January01_December03</i>	-.020 (.140)	-.014 (.199)	-.125 (.149)
<i>firm15</i> * <i>January04_December07</i>	-.026** (.035)	.010 (.105)	-.110* (0.066)
(<i>firm15</i>) * (<i>January01_December03</i>) * (<i>Permanent</i>)	–	–.008 (.473)	–
(<i>firm15</i>) * (<i>January04_December07</i>) * (<i>Permanent</i>)	–	–.047** (.049)	–
<i>Firm size</i> fixed effects (2)	Yes	No	No
Survey fixed effects (3)	Yes	Yes	Yes
Clustered S.E.	Yes	Yes	Yes
Control Var. (26)	Yes	Yes	Yes
Regional dummies (19)	Yes	Yes	Yes
Obs.	17,591	27,346	19,562
R^2	.22	.24	.14

OLS estimates. Robust p values in parentheses (t statistics clustered at the firm size level). In column (1) only workers employed with a permanent contract are considered and $firm15 = 1$ if an individual is employed in a plant with more than 15 employees. In column (2) workers employed with either permanent or temporary contract are considered; $firm15 = 1$ if the individual is employed in a plant with more than 15 employees. $Permanent = 1$ if the individual is employed with a permanent contract. In column (3) only workers employed with a permanent contract in plant with more than 15 employees and self-employed workers are considered; $firm15 = 1$ if the individual is a dependent worker. In all columns, individuals who found a job in 2003 are excluded. *January04_December07* is a dummy variable equal to 1 if the individual has been employed after December 2003. *January01_December03* is a dummy variable equal to 1 if the individual has been employed from January 2001 to December 2003

and to match together those which have similar predicted probabilities. Our strategy is the following. We take as treated permanent workers in large plants, and we evaluate their outcomes after the reform. Then, we select a control group, and we use propensity score and matching technique to construct three counterfactual groups (i) treated *before* the reform; (ii) untreated *before* the reform; and (iii) untreated *after* the reform. Given our repeated cross section structure, matching has to be repeated three times in order to find comparable individuals before and after treatment. The matching hypothesis is stated in terms of the before–after evolution instead of levels. It means that controls evolved from a pre- to a postreform period in the same way treatments would have done had they not been treated. We choose the appropriate weights to be assigned to the selected set of counterfactuals according to the nearest neighbor and the kernel (normal-type) method. Then, the DD estimator is applied. The propensity score is estimated by using a probit model and by including among regressors all relevant individual characteristics as well as regional dummy variables. Bias reduction obtained

under the nearest neighbor matching method for each of our estimated propensity score is reported in Table 5. We ensure common support by dropping treatment observations whose p-score is higher than the maximum or less than the minimum p-score of the controls.⁷

In Table 6 we present the results. In column (1) we apply PSDD to our main specification, i.e., we use as control group full-time permanent workers in small plants. In this case, independently on the use of either nearest neighbor or kernel method for matching, we detect a wage reduction for permanent workers in large firms after the 2003 reform. The point estimate of δ_2 is about -3.0% . The common time trend assumption is also verified since δ_1 is always not statistically different from zero. In column (2) we present the results of our PSDD strategy implemented using temporary workers in large plants as control group and, consequently, modifying the propensity score and the matched counterfactuals. Our previous results are confirmed also in this case since a wage loss for permanent workers in large firms after the reform is detected. This penalty ranges between -2.6% and -4.1% according to our alternative matching procedures. Even in this case δ_1 is statistically not significant. Finally, we present the PSDD estimates obtained by considering self-employed individuals as control group, and within this category we construct before/after counterfactuals for permanent workers in large plants. In this case, the wage reduction for protected employees is of about -6.0% , and it is robust according to both nearest neighbor and kernel matching procedures.

4.5 Final verification: addressing selection into employment

A final drawback that needs to be addressed may derive from the fact that wages are recorded only for employed workers. In practical terms, in Eq. (2) we observe the dependent variable w only if the individual is actually employed. According to numbers reported in Tables 9 and 11, it seems that selection into employment could be present in our data and, consequently, by ignoring this potential source of bias, we could confound the effect of the policy on employment probability with its effect on wage. To tackle this issue, we rely on the so-called averaged log-likelihood function accounting for the probability of being employed when estimating the wage equation. In this way, albeit we cannot untangle the point estimate of the effect of the reform on the probability of being employed (in the selection equation both employed and unemployed workers have been included), we can control for changes in this probability after the reform and we may control for its effects on wages. Identification problems are solved by means of exclusion restrictions related to variables that are likely to be correlated with employment and uncorrelated with wage such as the civil state and regional dummies. Control variables in the selection equation are those included in Eq. (2) but those related to job characteristics.

In Table 7 we report estimates obtained using DDD adding the employment selection equation. Since in this case unemployed individuals are also considered,

⁷ Since the results derived by imposing common support can be sensitive to the adopted procedure (Lechner 2008), we replicate our study by also dropping the percentage of the treatment observations at which the p-score density of the control observations is the lowest. Results are unaffected by the selected method.

Table 5 Propensity score(s) matching and bias reduction

	Bias	Reduction in bias (%)
<i>Permanent in large plants versus permanent in small plants</i>		
Propensity score 1		
Unmatched	97.4	
Matched	3.9	96.0
Propensity score 2		
Unmatched	98.4	
Matched	3.8	97.0
Propensity score 3		
Unmatched	98.2	
Matched	4.9	95.0
<i>Permanent versus temporary</i>		
Propensity score 1		
Unmatched	99.4	
Matched	4.1	97.1
Propensity score 2		
Unmatched	98.7	
Matched	3.9	96.5
Propensity score 3		
Unmatched	98.4	
Matched	3.5	96.0
<i>Permanent versus self-employed</i>		
Propensity score 1		
Unmatched	99.4	
Matched	4.9	90.3
Propensity score 2		
Unmatched	98.4	
Matched	5.9	88.0
Propensity score 3		
Unmatched	96.4	
Matched	5.9	89.1

Treated individuals are permanent workers in large plants employed after the reform. Nearest neighbor matching procedure (1:1 ratio) applied. Propensity score 1 compares treated with permanent workers in large plants employed before the reform. Propensity score 2 compares treated with individuals in the specific control group employed after the reform. Propensity score 3 compares treated with individuals in the specific control group employed before the reform

observations increase substantially by going from 29,627 to 42,263. The issue of sample selection seems to be relevant since a significant correlation between the residuals of the two equations is reported. Notwithstanding, estimates concerning the wage effect of the reform are not affected by the new specification, confirming that in Italy graduates who enter permanent positions in large plants have experienced a reduction in earnings compared to other workers' categories. This result appears to be robust to different exclusion restrictions.

Table 6 Propensity score difference-in-differences estimates

Dependent variable	Logarithm of monthly real wage		
	(1)	(2)	(3)
Method	PSDD	PSDD	PSDD
Coeff.	≥ 15 Empl. <i>N.N.</i> <i>Kernel</i>	Perm. versus Temp. <i>N.N.</i> <i>Kernel</i>	Perm. versus self-Empl. <i>N.N.</i> <i>Kernel</i>
<i>firm 15 * January01_December03</i>	-.001 (.900)	-.001 (.976)	.001 (.999)
<i>firm 15 * January04_December07</i>	-.026** (.034)	-.045*** (.000)	-.131*** (.000)
<i>(Job start - year) * (firm15) fixed effects (9)</i>	Yes	Yes	Yes
<i>Job start-year fixed effects (9)</i>	Yes	Yes	Yes
<i>Firm size fixed effects</i>	Yes	No	No
<i>Survey fixed effects</i>	Yes	Yes	Yes
Obs.	11,379	18,634	17,034
<i>Treated Before</i>	3,019	8,527	4,550
<i>Treated After</i>	6,070	6,070	6,070
<i>Control Before</i>	1,170	2,557	4,062
<i>Control After</i>	1,120	1,480	2,352

PSDD estimates. Bootstrap S.E. (1000 replications) used for propensity score. The dependent variable is the log of monthly real wage. In all columns *January04_December07* is a dummy variable equal to 1 if the individual has been employed after December 2003. *January01_December03* is a dummy variable equal to 1 if the individual is employed from January 2001 to December 2003. In column (1) only workers employed with a permanent contract are considered; *firm 15* = 1 if the individual is employed in a plant with more than 15 employees. In column (2) the sample is restricted to individuals employed in plants with more than 15 employees with either a permanent or a temporary contract. *firm 15* = 1 if the individual has a permanent contract. In column (3) the sample is restricted to individuals employed in plants with more than 15 employees with a permanent contract and to self-employed. *firm15* = 1 if the individual has a permanent contract in a plant with more than 15 employees. Twenty-six control variables and 19 regional dummies included in all specifications. The propensity score has been estimated by using a probit model on individual characteristics and regional dummy variables. *N.N.* and *Kernel* refer to the nearest neighbor and to the kernel (normal-type) matching estimator, respectively. Common support imposed by dropping treatment observations whose p-score is higher than the maximum or less than the minimum p-score of the controls. *N.N.* applied with 1:1 ratio

Table 7 Triple differences estimates with correction for sample selection with multiple periods and groups

Main equation	(1)	(2)
Dependent variable	Wage equation	
Method	Logarithm of monthly real wage	
	<i>DDD</i>	<i>DDD</i>
	Coeff.	Coeff.
<i>firm15 * January04_December07</i>	-.015 (.155)	-.016 (.131)
<i>(firm15) * (January01_December03) * (Permanent)</i>	.032 (.120)	.027 (.100)
<i>(firm15) * (January04_December07) * (Permanent)</i>	-.023** (.010)	-.029** (.011)
<i>Firm size</i> fixed effects (2)	Yes	No
<i>Survey</i> fixed effects (3)	Yes	Yes
<i>Job start-year</i> fixed effects (9)	Yes	Yes
Control Var. (25)	Yes	Yes
Married	No	Yes
Regional dummies (19)	Yes	No
Selection Equation	<i>Employment equation</i>	
	Coeff.	Coeff.
Survey fixed effects (3)	Yes	Yes
Control Var. (13)	Yes	Yes
Married	Yes	No
Regional dummies (19)	No	Yes
ρ	-.020*** (.000)	-.070*** (.000)
Obs.	42,263	42,263

Maximum likelihood estimates of the averaged log-likelihood functions (BHHH procedure applied). Robust p values in parentheses. For the main equation, see notes in Table 1. The selection equation is a probit model where the latent variable is equal to 1 in case of employment. In column (1) variable *Married* excluded from the main equation and included in the selection equation; in column (2) regional dummy excluded from the main equation and included in the selection equation. ρ is the estimated correlation index between residuals in the main and in the selection equations. Regressors in the selection equation are: age dummies, gender, 5 major dummies, university leaving grade, degree on time, high school leaving grade by 5 types of high school, parents' education

5 Concluding remarks

This paper is aimed at providing evidence on the impact of the introduction of a two-tier employment protection regime on entry wage of protected workers. We argue that the presence of institutional asymmetries may influence firms' outside options leading to a reduction in relative earnings of workers hired with open-ended contracts. To test this hypothesis, we exploit a policy reform introducing in Italy a new form of unprotected employment. Using data on recent graduate workers, we show that after the reform, those who entered positions entitled to labor market protection experienced a significant reduction in earnings. This result is corroborated by a series of robustness

checks and falsification tests carried out on a large time span and on various workers' categories.

The analysis presented in this work may be useful for policy since it highlights to what extent relative wages are sensitive to the normative institutional setting. In this vein, our study contributes to the evaluation of the determinants of wage inequality among workers employed under different protection regimes and to figure out the effects of a further flexibilization of the labor market. This is a burning issue for policy makers. However, two aspects should be remarked. The reported evidence only points to a reduction in the *entry level* disparities. The dynamics that may take place in the long run because of tenure or insidersness' matters—that could offset the initial reduction in wage disparities—has not been considered in our empirical framework. More importantly, it is crucial to recognize that our findings may be consistent with different theoretical explanations which have very different implications for welfare and policy. Therefore, it would be relevant to ascertain whether the reduction in entry level wage disparities mirrors an efficient outcome or just a redistribution of income in favor of entrepreneurs. These are challenges for future research.

Appendix

See Tables 8, 9, 10 and 11.

Table 8 Description of variables

<i>Individual and household</i>	
Female	Dummy variable indicating the respondent's sex, Female = 1, 0 otherwise
Age	Respondent's age in 4 classes. Class 1: age \leq 24; Class 2: 24 < age \leq 26; Class 3: 26 < age \leq 29; Class 4: age \geq 30
Wage	Monthly real wage of full-time workers. ISTAT consumer price index used
Parents education	Two dummy variables indicating if the respondent's parents have a university degree. Father education = 1 if the father has a university degree, 0 otherwise; Mother education = 1 if the mother has a university degree, 0 otherwise
Regional dummies	20 dummy variables indicating the respondent's region of residence
<i>Education</i>	
Degree subject	A vector of 6 0–1 dummy variables indicating degree subjects: (1) Science = 1 if mathematics, science, chemistry, pharmacy, geo-biology, agrarian; (2) Medicine = 1 if medicine; (3) Engineering = 1 if engineering, architecture; (4) Econ.&Law = 1 if political science, economics, statistics, law; (5) Humanities = 1 if humanities, linguistic, teaching, psychology; (6) Sport Science = 1 if sport science
High school grade	Final score (scale from 36 to 60) by type of high school: Lyceum; Teaching; Accountancy; Vocational
University grade	Final score (scale from 66 to 110)
Degree on time	Dummy variable indicating if the degree is completed on time (adjusted for course duration), Degree on time = 1, 0 otherwise
<i>Job</i>	
Permanent job	Dummy variable indicating if the respondent has a temporary or a permanent contract at the interview, Permanent job = 1, 0 otherwise

Table 8 continued

Occupation	6 Occupational dummy variables indicating the specific job grouped according to the ISCO-08 structure: Managers, Professionals, Technicians, Clerical workers, Craft and related trades workers, Elementary occupations
Para-subordinate job	Dummy variable indicating if the respondent has a para-subordinate temporary contract (<i>contratto a progetto</i>) at the interview, Para-subordinate job = 1 if yes, 0 otherwise
Self-employed	Dummy variable indicating if the individual is either self-employed or he has a subordinate/para-subordinate job; Self-employed = 1 if self-employed, 0 otherwise
Firm size	Multilevel dummy variable indicating plant size according to the number of employed worker. Firm size = 0 if employees ≤ 5 ; Firm size = 1 if $5 < \text{employees} < 15$; Firm size = 2 if $15 \leq \text{employees} < 50$; Firm size = 3 if $50 \leq \text{employees} < 100$; Firm size = 4 if employees ≥ 100
Industry	A multilevel dummy variable (6 levels) indicating the industry sector for employed individuals
Firm ownership	A dummy variable indicating if the firm ownership is public or private, Public = 1, 0 otherwise

Table 9 Frequency and average of variables in the samples: curricula and family background, 2001, 2004, 2007

	2001		2004		2007	
	Frequency	Percentage	Frequency	Percentage	Frequency	Percentage
<i>Individual features</i>						
Observations	20,844	100.0	25,674	100.0	26,570	100.0
Female	11,148	54.6	12,925	51.5	13,681	53.0
Male	9,273	45.4	12,152	48.5	12,139	47.0
Age	20,844	100.0	25,674	100.0	26,570	100.0
Mean age class	2.8		2.6		2.4	
Married	6,202	29.7	7,432	29.0	7,383	28.8
Father education	4,519	21.7	6,204	23.8	6,462	24.3
Mother education	2,632	12.6	3,944	15.2	4,868	18.3
University grade	20,576	99.0	25,674	100.0	26,570	100.0
Mean university grade	103.0		102.4		102.0	
High school grade	20,844	100.0	25,674	100.0	26,570	100.0
Mean high school grade	48.8		49.4		50.0	
<i>Majors</i>						
Science	4,037	19.4	4,904	15.7	4,018	15.1
Medicine	1,259	6.0	4,175	16.0	5,191	19.5
Humanities	4,696	23.8	4,110	18.8	4,492	16.9
Econ&Law	7,076	33.9	7,142	27.5	8,461	31.8
Engineering	3,509	16.8	5,036	19.5	4,408	16.6
Sport Science	–	–	659	2.5	7	0.1

The averages are sample averages. For final marks (high school and university), averages are with respect to the number of individuals in the group. Father and Mother education indicate the percentage of individuals whose father and mother have a university degree. Age classes defined in Table 8

Table 10 Frequency and average of labor market variables in the samples, 2001, 2004, 2007

	2001		2004		2007	
	Frequency	Percentage	Frequency	Percentage	Frequency	Percentage
<i>Whole sample</i>						
Obs.	20,844	100	25,674	100	26,570	100
Employed	15,334	73.6	18,165	70.6	17,928	67.5
Unemployed	1,933	9.3	1,688	6.6	1,873	7.0
Not in the labor force	3,577	17.1	5,040	19.7	5,981	22.5
Missing	–	–	781	3.1	788	3.0
Unemployment rate		11.2		8.5		9.4
<i>Employed individuals</i>						
Dependent workers	10,636	68.5	11,302	62.2	11,242	62.7
Self-employed	2,669	17.3	3,319	18.3	2,685	15.0
Atypical workers	2,212	14.2	3,500	19.2	2,869	16.0
Para-subordinate workers	–	–	44	0.2	1,132	6.3
<i>Dependent workers</i>						
Permanent	7,981	75.5	8,199	76.3	7,412	69.2
Temporary	2,586	24.5	2,542	23.6	3,292	31.8
Employed in <i>Firm</i> < 15 employees	2,316	22.5	1,661	16.0	1,858	17.7
Employed in $15 \leq \textit{Firm} < 100$ employees	3,845	36.1	3,722	35.6	3,715	35.3
Employed in <i>Firm</i> ≥ 100 employees	4,406	41.4	5,040	48.4	4,942	47.0
<i>Wage</i>						
Obs.	11,093	72.3	13,148	71.8	15,041	83.9
Mean nominal wage	1,026 Euro		1,113 Euro		1,180 Euro	

Table 11 Statistics for treated and control groups before and after the reform

	Treated			Control 1			Control 2			Control 3		
	Before	After	Diff.	Before	After	Diff.	Before	After	Diff.	Before	After	Diff.
Obs.	8,854	6,070	-32.6	2,590	1,529	-40.9	8,854	6,346	-28.3	3,895	2,393	-38.6
Male	56.6	58.7	+2.1	42.5	44.7	+2.2	44.8	47.3	+2.5	58.9	63.2	+4.3
Married	25.5	23.4	-1.9	26.3	24.4	-2.1	25.0	24.8	-0.2	25.9	23.8	-2.1
Father education	21.0	19.2	-1.8	19.3	17.0	-2.3	21.3	21.1	-0.2	26.3	24.9	-1.3
Mother education	13.9	16.9	+3.0	13.0	13.1	+0.1	14.8	17.2	+2.4	15.9	18.4	+2.5
University grade	105.9	102.1	-3.6	106.4	101.9	-4.2	106.0	103.0	-2.8	106.6	101.5	-4.8
High school grade	50.0	50.1	+0.2	48.8	49.1	+0.6	49.4	49.8	+0.8	48.3	48.3	0.0
Science	21.9	19.6	-2.3	26.8	25.3	-1.5	25.4	23.4	-2.0	17.1	15.4	-1.7
Humanities	12.8	11.1	-1.7	18.5	18.1	-0.4	26.3	21.4	-5.1	13.1	13.6	+0.5
Econ&Law	33.1	35.0	+1.9	38.8	38.7	-0.1	29.0	33.7	+4.7	32.3	33.2	+0.9
Engineering	32.2	34.3	+2.1	15.9	17.9	+2.0	19.3	21.5	+2.5	37.5	37.8	+0.3
Real wage	10.90	10.69	-0.21	9.59	9.48	-0.11	9.03	9.01	-0.02	10.68	10.49	-0.19

Apart from Observations, University grade and High school grade, all reported numbers are percentages and refer to the share in the respective group. "Treated" indicates full-time workers employed with a permanent contract in plants with more than 15 employees. "Control_1" indicates full-time workers employed with a permanent contract in plants with less than 15 employees. "Control_2" indicates full-time workers employed with a temporary contract in plants with more than 15 employees. "Control_3" indicates full-time workers self-employed. "Before" and "After" refer to workers employed before and after December 2003

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