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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 wage of workers entering positions entitled to employment protection may be affected by the introduction of a two-tier labor market regime. By using repeated cross-sections microdata, we apply difference-in-differences estimators - also combined with propensity score matching techniques - to evaluate the impact of the Italian labor market reform of 2003. The results are robust and show that after the policy implementation protected entrants experienced a reduction in earnings ranging between -3.0% and -6.0%.

Jel classification: J63, J64.

Key Words: EPL, Flexibility-at-the-Margin, Difference-in-Differences, Propensity Score, Matching Estimators.

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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 market policies mainly focusing on their impact on labor utilization and unemployment. Among others, Boeri and Jimeno (2005), Boeri and Garibaldi (2007) 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). 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. Boeri (2010) points out that it might be important to single out the impact of EPL on wage of existing insiders and on that of entrants. Indeed, in case of a two-tier regime, entrants may have different contracts and it is relevant to distinguish between protected and unprotected positions. According to this author, two-tier reforms generate a widening of institutional asymmetries so that they may affect the bargaining position of insiders and increase the rents of outsiders. Hence, protected workers could experience a reduction in earnings due to a change in their bargaining power or to a downward shift of labor demand. On top of that, it has been recognized that wage differentials across job contracts may reflect both productivity gaps associated to firms' sorting behavior and the presence of insider power owned by those workers

who actually qualify for labor market protection (Berton and Garibaldi, 2012). 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 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, this normative setting generates an exogenous threshold which can be used to construct a control group in order to apply difference-in-differences (DD) procedure. However, this approach is not straightforward since the introduction of new types of fixed-term contracts may

[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.

generate flows of workers across employment status and firm size, undermining the difference-in-differences analysis. To tackle this issue we implement propensity score matching techniques coupled with difference-in-difference methodology as in Blundell *et al.* (2004). The empirical setup is carried 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 labor market protection experienced a reduction in earnings that ranges between -3.0% and -6.0%. 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. The Italian institutional setting is briefly described along with the characteristics of the main implemented reforms in Section 2. Section 3 presents our dataset and discusses the empirical model and the identification strategy. Section 4 contains the results and presents several robustness and falsification tests. In Section 5 some concluding remarks are addressed.

2 The Italian Labor Market and the 2003 Reform

The implementation of the reform through the legislative decree 276/2003, definitely in charge in December 2003, represents one of the most significant shocks imposed to the Italian labor market. The reform aimed at regulating new temporary job contracts in order to by-pass 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. Firms employing less than 15 employees must pay to the worker a monthly forfeit. Conversely, 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 with the reform of 1998 (Law 197/1997). This law increased flexibility by introducing temporary contracts. Crucially, jobs created under these contractual forms must be either destroyed or transformed into permanent positions when they expire. Efforts to increase labor market flexibility were taken forward with the 2003 reform. The new norms further deregulated the use of atypical work arrangements and introduced *para-subordinate* work (*lavoro 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 compensation. The Italian labor market has been deeply transformed by the introduction of these types of occupations, mainly because they can be endlessly repeated. It should be reckoned that, despite *para-subordinate* jobs can be created only in the presence of a specific project that is somehow different from the main firm activity, there is a wide consensus among legal experts concerning the fact that these contracts hide *de facto* subordinated jobs involved in the main activities of firms (Ichino, 2008). This new regime

[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.

implies that these contracts can be used to repeatedly hire the same worker into the same job eluding norms for standard subordinate positions. In Italy, workers employed under this regime are known as precari.[3]

3 Strategy and Data

3.1 The Identification Strategy

The identification strategy presented in this study is funded on the exogenous threshold separating firms in terms of dismissal constraints. This threshold is provided by the normative setting which varies according to firm size. Indeed, after the reform firms with less than fifteen employees continued to be exempted from EPL restrictions while large firms were affected by the introduction of a new form of flexibility consisting in labor contracts that can be renewed at will. Therefore, we are able to build up a control group, i.e., individuals employed in firms with less than 15 employees, in order to establish if the introduction of a brand new form of *unprotected entrants* has affected wages of *protected workers*. We can then apply difference-in-differences procedure (DD) to assess whether the creation of a two-tier labor market affects the entry wage of fully protected workers.

The basic framework separates those workers whose job started before the reform from those employed under the new regime leading to the estimation of the

[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.

following wage equation:

$$w_i = \mathbf{X}_i\boldsymbol{\beta} + \delta_0 t_i + \delta_1 (EP)_i + \delta_2 t_i \cdot (EP)_i + u_i \tag{1}$$

where i indicates the generic individual and $t = \{0, 1\}$ is a dummy variable equal to 1 if the job started after the reform. The dependent variable is the logarithm of monthly wage earned by individual i. In the RHS of eq. (1), $\mathbf{X}_i$ indicates a set of control variables while $EP = \{0, 1\}$ indicates the 'treatment' and takes the value of 1 if individual i is employed as a permanent worker in a plant with more than 15 employees. Our parameter of interest is δ_2 which measures the relative variation in wage for permanent workers in large plants after the reform compared to permanent workers in small firms. Eq. (1) will be modified according to different specifications and tests we discuss in the next Sections.

3.2 The Data

The empirical investigation presented in this study is based on data from 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 are all university graduate workers who entered the labor market in 1998, 2001 and 2004 and were interviewed three years later. Hence the surveys have been collected in 2001, 2004 and 2007 respectively.[4] Workers in our samples are 73,088 individuals owning a university degree obtained

[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.

after a 4/5 years course of study (basically B.Sc. plus M.Sc. degree).[5] In the Appendix, Table A1 defines our variables while Table A2 and Table A3 contain some representative statistics of our samples in terms of academic/personal characteristics and labor market outcomes respectively. We rely on these specific repeated cross-sections for three main reasons.

Firstly, these surveys allow for the implementation of our econometric strategy. In particular, 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 years period (1998-2007) and would be sufficient to derive sensible results. However, we have additional information coming from the 2004 sample which contains data on workers employed under both the new and the old regime. Nevertheless, since we have information concerning the starting date (year and month) of the current job, within this specific sample we can separate those workers who have been employed after the reform from the others.

Secondly, for employed workers, the survey reports several information concerning the job position and it contains indication concerning the number of workers employed in the single plant where each graduate works. This information is crucial in order to assess if 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

[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.

using 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 these aspects may induce a downward bias in our DD estimates since some treated individuals for which employment protection applies may actually end up in the control group. However, as we discuss in details in Section 4, we implement many robustness checks showing that our results do not hinge either on possible measurement errors or on the use of a biased control group.

Finally, all surveys report the type of labor contracts. 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, however, not straightforward. At the outset, it should be recognized that almost at the same time of the labor market reform, 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 firms who are not entitled to employment protection. This analysis is also supported by several falsification tests implemented by using small plants and different workers' categories. Furthermore, self-employed individuals represent a valuable alternative control group. Using this peculiar category of workers, we undertake additional robustness and falsification tests. A word of warning should be also addressed to the occurrence of possible measurement errors that may derive from individual's evaluation of firm's dimension. This issue, while being of a minor relevance when separating very small and very large firms, could be problematic around the threshold. By using the above mentioned control groups we may overcome possible bias so that our results are not affected by misreporting of plant's dimension.

On top of that, it should be pointed out that the introduction of a new type of fixed-term contract may 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 may also change after the reform. Since these composition effects may affect our DD estimates, we make use of propensity score matching techniques coupled with difference-in-difference methodology (PSDD) as in Blundell *et al.* (2004). Matching procedures ensure that a treated individual is compared only to his/her counterparts in control groups 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., individuals employed with an open-ended contract in large plants after December 2003, by applying a selected matching procedure we construct three counterfactuals (treated before the reform and controls before and after the reform), to implement the PSDD estimator.

3.4 Wage Patterns

At this stage, it is interesting to show wage patterns arising from our dataset for the period 1998-2007. In Figure 1 we plot the average wage for full time dependent workers evaluated for each year using information concerning the date of job start. We consider only dependent workers classified in four categories, i.e., temporary and permanent employees in plants with more or less than 15 employees. Some insights can be gathered by inspecting these series. First of all, differences across contracts and plants' dimension are exactly as expected. Workers employed in large plant under permanent contract 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. Temporary workers appear to have a similar wage pattern independently on firm size, albeit those in large firms seem to be slightly better rewarded. Finally, if we look at permanent employees in small plants and we compare them with their peers in larger firms, we see that over the period 1998-2003 they experienced an increasing wage penalization which appears to have been almost recovered after 2003.

Some additional useful evidence comes up if we split the sample according to the date of the reform using year 2004 as a rough threshold. In Figure 2 we

compare temporary workers (unprotected employees) according to firm size. It is worth noting that wages of these two categories always move in the same direction, hence the wage gap between them remains almost constant over the considered time period. The same path arises from Figure 3, where we consider only plants with less than 15 employees (unprotected employees) separating temporary and permanent workers. Once again, the two series move in the same direction, i.e., downward till 2003 and upward after 2004. Conversely, we find a different scenario if we consider fully protected workers. In Figure 4 we show the series for plants with more than 15 employees and we compare permanent (protected) and temporary (unprotected) workers. In this case it is evident that after 2004 these two series do not move in a parallel way. A similar result is reported in Figure 5 where we consider permanent workers in large and small plants who differ in terms of employment protection. This implies that after 2004 the relative wage of protected workers seems to decrease with respect to that of unprotected ones. Whether this convergence between protected and unprotected workers is statistically significant and to what extent this wage gap reduction has been generated by the 2003 labor market reform will be evaluated in the next Section.

4 Results

4.1 First Verification: A Simple DD Approach

We start our analysis by carrying out pairwise comparisons. Firstly, we compare the 2001 and the 2007 samples and in terms of eq. (1) we set $t = 1$ for the year 2007, implementing a pure before-after methodology. In this case our sample

considers only full-time permanent workers and it consists of 14,462 individuals. In the RHS of eq. (1), $\mathbf{X}_i$ includes 20 control variables (age, gender, marital status, time to degree, educational mismatch, university majors, public sector, parents' education, university leaving grade, high school leaving grade by 5 types of high school, firm size, industry) and 19 regional dummy variables. EP 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. Table 1 contains the results obtained by clustering standard errors at plant dimension level in order to face the issue of serial correlation (Bertrand *et al.* 2004). In column (1), the estimated value for δ_2 is statistically significant and it is about -9.4%. This implies that there has been a relative reduction in wage of permanent workers in large plants after the reform with respect to that of employees in small firms. If we do a similar exercise comparing individuals from the 2004 and the 2007 survey, considering as post-reform workers observed in 2007, we find an overall negative effect of about -3.7% which is statistically significant (Table 1, column 2). Conversely, we do not find a significant δ_2 coefficient when we estimate our model comparing the 2001 and the 2004 surveys (Table 1, column 3). In order to correctly interpret these results, we should consider that observations in the 2004 survey are problematic since the reform occurred in the final year of it. Consequently, in the presented exercises we have not precisely separated pre- and post-reform individuals. Moreover, although these findings are interesting, we should remark that only pairwise comparisons are implemented while more efficient estimates can be obtained by using simultaneously all data sets. Next paragraph deals with these specific concerns.

4.2 Second Verification: Double Differences with Multiple Groups and Time Periods

In this Section we construct an empirical strategy in order to be able to apply DD techniques and, simultaneously, to use all available datasets. We apply a DD strategy estimating an interest equation as follows:

$$\begin{aligned} w_{isj} &= \mathbf{X}_{isj}\boldsymbol{\beta} + \alpha_s + \gamma_j + \delta_0 EP_{isj} + \\ &\delta_1(EP * January01\_December03)_{isj} + \delta_2(EP * January04\_December07)_{isj} + u_{isj} \end{aligned} \tag{2}$$

where i corresponds to individuals, s to the time period (in year) in which the individual i has been interviewed and j indicates groups. α_s are sample fixed effects (2001, 2004 and 2007). γ_j represents fixed effects for workers in plants with more or less than 15 employees. Only permanent workers are considered. EP_{isj} is a dichotomous variable taking the value 1 if the individual is employed in a firm whose dimension entitles for employment protection. $\mathbf{X}_{isj}$ contains control variables as described in previous paragraph. 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) $(EP*January01\_December03)_{isj}$ and $(EP * January04\_December07)_{isj}$ are dummy variables taking the value 1 if the individual is subject to employment protection and 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 these interaction dummies 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 2 presents the results. As in paragraph 4.1 the coefficient of main interest is δ_2, reported in column (1), which is equal to -7.1% and it is statistically significant. This means that entrants entitled to employment protection had a wage loss after December 2003 compared to their pre-reform peers. The common time trend assumption is verified being the estimated coefficient δ_1 not statistically different from zero. In column (2) of Table 2 we present additional estimates derived including among regressors year fixed effects instead of survey fixed effects. In this case we are using information provided by our dataset 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 after including among our regressors 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-plants 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 with a point estimate of -6.7%.

4.3 Third Verification: Addressing Confounding Trends

A key concern arises at this stage. Albeit the highlighted results appear to be robust according to several specifications, there can still be systematic differences between small and large firms. In particular, almost at the same time of the 2003

reform the Euro currency has been introduced in Italy. It is possible to argue that large firms may had larger spillover from the adoption of the single currency across Europe than the 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 would 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.

4.3.1 Robustness 1: temporary *vs.* permanent workers in large plants

We start by making use of an alternative control group consisting of temporary workers employed in large plants. In particular we consider only those workers who declared to be employed in plants with more than 15 employees. In this case, we analyze only plants that are constrained by employment protection for permanent workers. Then, within these employees we separate two groups: permanent (fully protected) and temporary (unprotected) workers and we evaluate the model presented in eq. (2) where, in this case, EP_{isj} is a dichotomous variable taking the value 1 if the individual is employed with a permanent contract. In column (1) and (2) of Table 3 we report estimates for δ_1 and δ_2 according to two different specifications. Our main results are entirely confirmed being δ_1 not statistically

[6] We remark that albeit in 2007 an important recession started in Europe, in Italy the effects of the downturn show only in 2008. In particular GDP growth rate in 2006 was +1.5% and in 2007 +1.7%. 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.

different from zero while δ_2 is negative and significant indicating a penalization of about -3.1% for permanent workers employed after the reform. It is important to note that, although we have strongly modified our data set by using temporary workers as a reference category and excluding individuals in small plants, all results go in the same direction.

4.3.2 Falsification 1: temporary *vs.* permanent workers in small plants

In order to support the analysis presented in the previous paragraph, some falsification exercise is undertaken. Column (3) in Table 3 shows the results obtained by restricting the sample to plants with less than 15 employees and comparing the evolution of wage of temporary and permanent workers within them. In this case, we evaluate our identification strategy by means of a falsification test implemented by considering as treated only workers with a permanent contract. Interestingly, all coefficients are not statistically different from zero and these results also hold for the specification reported in column (4) where year fixed effects are used instead of survey fixed effects.

4.3.3 Unifying Robustness 1 and Falsification 1: A triple differences approach

An additional robustness exercise is built up by simultaneously making use of the two control groups used so far and implementing a triple differences strategy. 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 protections. This approach has the advantage of raising the sample size to about 30,000 observations. Preliminary results are reported in column (1) of Table 4. The dummy $Permanent$ is equal to one 1 if the individual is employed as a permanent workers. This dummy is interacted with $(EP * January01\_December03)_{isj}$ and with $(EP * January04\_December07)_{isj}$ where EP indicates if the individual is employed in a plant with more than 15 employees. The coefficient of interest is that associated to the variable $(EP * January04\_December07) * (Permanent)_{isj}$ since it measures the relative variation after December 2003 of the wage differential between permanent and temporary workers in large and small plants. The estimated parameter is significantly negative and close to previous values, i.e., -5.5%. This confirms that the impact of the two-tier reform is in the direction of a reduction of the entry wage of permanent workers in large plants more than that of those employed in small ones. In column (2) of Table 4 we present additional estimates derived including among regressors year fixed effects. Our results appear to be robust according to this additional specification too. Finally, in column (3) we report more robust estimates arising after including among our regressors time varying large plant specific effects. As in previous cases, only the coefficient associated to $(EP * January04\_December07) * (Permanent)_{isj}$ is statistically significant with a point estimate of -5.3%.

4.3.4 Robustness 2: self-employed *vs.* protected workers

A further check is carried out using observations referred to self-employed individuals. They are about 8,000 workers (Table A2) and they are not affected by

the reform. By comparing affected and unaffected occupations according to firm's dimension we can further assess if the 2003 reform had a negative effect upon protected individuals. We start by considering only self-employed and permanent workers employed in large plants. We estimate the same setup of eq. (2) and in this case EP is a dummy variable equal to 1 only for dependent workers with a permanent contract. In column (1) of Table 5 we report the results, which are as expected. The coefficient associated to $(EP*January04\_December07)_{isj}$ is equal to -4.8% and it is statistically significant. 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 5 estimates for δ_1 and δ_2 obtained by using year fixed effects confirm these findings.

4.3.5 Falsification 2: self-employed *vs.* unprotected workers

A final falsification exercise is presented at this stage. Columns (3) and (4) of Table 5 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 by setting EP equal to 1 only for dependent workers with a permanent contract. As expected, no coefficient is statistically different from zero.

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 are 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 is 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 A4 in the Appendix we provide evidence concerning observed characteristics of workers hired before and after the reform by type of contracts, including self-employed, and firm size. The reported statistics do not highlight any particular composition change affecting treated individuals differently from the others and show an almost static picture across groups. However, composition changes occurred along unobserved characteristics, if present, may still undermine our estimates.

This concern may be alleviated by estimating a 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 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; *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 have evolved from a pre- to a post-reform 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 Logit model and by including among regressors all relevant individual characteristics as well as regional dummy variables.

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 reported. 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 matching procedures.

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 of relative earnings of workers hired with open-ended contracts. To test this hypothesis we make use of Italian data exploiting a policy reform introducing 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 various workers categories.

The analysis presented in this work may be useful for policy since it highlights that relative wages seem to be sensitive to the normative institutional setting. In this vein, our study may contribute 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 points 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 insiderness' aspects - that could offset the initial reduction of 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. Then, it would be relevant to ascertain if the reduction of entry level wage disparities mirrors an efficient outcome or just a redistribution of income in favor of entrepreneurs. These are challenges for future research.

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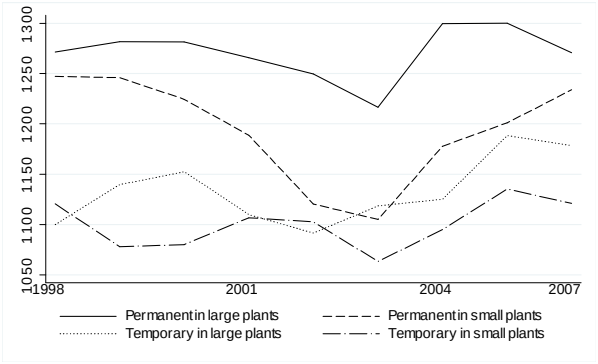

Figure 1: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) and plant dimension (more or less than 15 employees) over the period 1998-2007 in Italy.

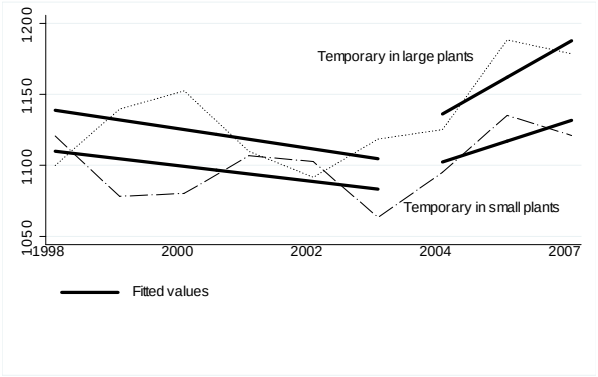

Figure 2: Average monthly wage (in Euros) of temporary workers according to plant dimension (more or less than 15 employees) over the period 1998-2007.

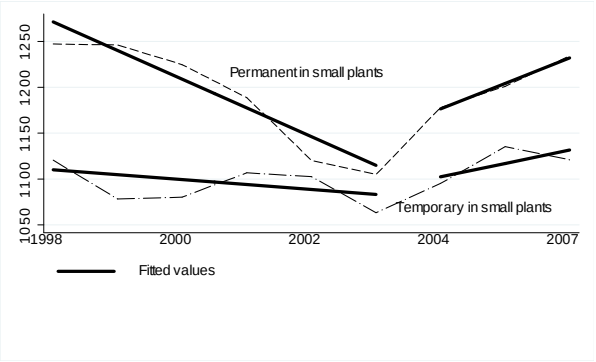

Figure 3: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) in plants with less than 15 employees over the period 1998-2007.

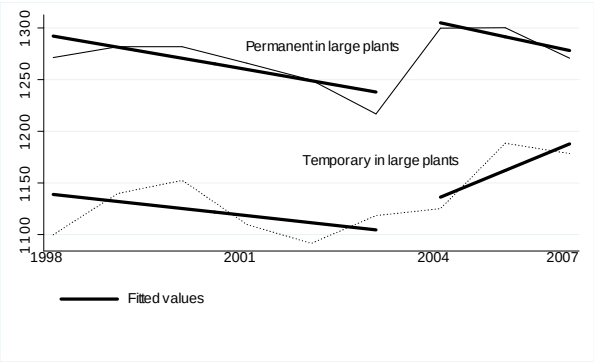

Figure 4: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) in plants with more than 15 employees over the period 1998-2007.

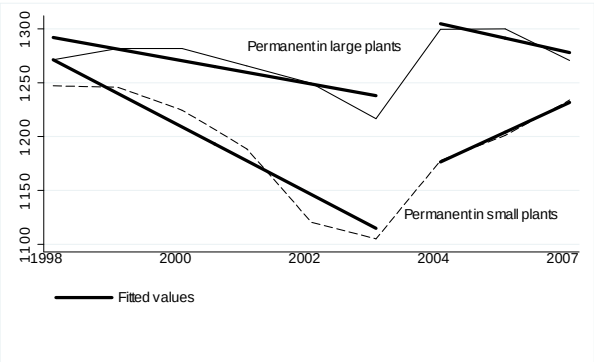

Figure 5: Average monthly wage (in Euros) of permanent workers according to plant dimension (more or less than 15 employees) over the period 1998-2007.

Table 1: Difference in Differences Estimates. First Verification.

	(1)	(2)	(3)
Dependent variable	*Logarithm of monthly wage*		
Method	DD (2001/2007) Entire samples	DD (2004/2007) Entire samples	DD (2001/2004) Entire samples
Coeff.			
$t \cdot (EP)$	$-.094^{**}$ (.028)	$-.037^{**}$ (.025)	$-.060$ (.097)
Clustered S.E.	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes
Obs.	14,462	12,799	13,449
R^2	.18	.24	.18

Notes: OLS estimates. Robust p-values in parentheses (t-statistics clustered at the firm dimension level). The dependent variable is the log of monthly wage. Only workers employed with a permanent contract considered and $EP = 1$ if an individual is employed in a plant with more than 15 employees. In column (1) the sample is restricted to individuals from the 2001 and 2007 sample and $t = 1$ for individuals from the 2007 survey. In column (2) the sample is restricted to individuals from the 2004 and 2007 sample and $t = 1$ for individuals from the 2007 survey. In column (3) the sample is restricted to individuals from the 2001 and 2004 sample and $t = 1$ for individuals from the 2004 survey. In all columns, 19 regional dummies and 20 control variables (age dummies, gender, marital status, 5 major dummies, university leaving grade, high school leaving grade by 5 types of high school, parents' education, 4 firm size dummies, 2 dummies for permanent and temporary labor contracts, dummies for the public sector, industries, degree on time and a dummy for educational mismatch) are included.

Table 2: Difference-in-Differences Estimates with Multiple Periods

	(1)	(2)	(3)
Dependent variable	*Logarithm of monthly wage*		
Method	DD (all datasets) Entire Samples	DD (all datasets) Entire Samples	DD (all datasets) Entire Samples
Coeff.			
$(EP * January01\_December03)$	$-.025$ (.373)	$-.020$ (.444)	$-.038$ (.219)
$(EP * January04\_December07)$	$-.071^{**}$ (.044)	$-.068^{**}$ (.043)	$-.067^{**}$ (.041)
$(Job\ start - year) * (EP)$ Fixed Effects (9)	No	No	Yes
$Job\ start - year$ Fixed Effects (9)	No	Yes	Yes
$Firm\ size$ Fixed Effects (2)	Yes	Yes	No
Survey Fixed Effects (3)	Yes	No	No
Clustered S.E.	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes
Obs.	20,355	20,355	20,355
R^2	.20	.20	.21

Notes: OLS estimates. The dependent variable is the log of monthly wage. Robust p-values in parentheses. All surveys (2001, 2004 and 2007) used, t-statistics clustered at the firm dimension level, only workers employed with a permanent contract considered and $EP = 1$ 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.

Table 3: Difference-in-Differences Estimates: Robustness 1 and Falsification 1

	(1)	(2)	(3)	(4)
Dependent variable	*Logarithm of monthly wage*			
Method	DD (all datasets)	DD (all datasets)	DD (all datasets)	DD (all datasets)
	+15 Employees	+15 Employees	-15 Employees	-15 Employees
Coeff.				
$EP * January01\_December03$	.007 (.166)	.007 (.254)	−.033 (.114)	−.028 (.157)
$EP * January04\_December07$	−.031** (.010)	−.027** (.019)	−.036 (.141)	−.024 (.112)
Job start − *year* Fixed Effects (9)	No	Yes	No	Yes
Survey Fixed Effects (3)	Yes	No	Yes	No
Clustered S.E.	Yes	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes	Yes
Obs.	24,129	24,129	5,825	5,825
R^2	.23	.24	.17	.18

Notes: OLS estimates. The dependent variable is the log of monthly wage. Robust p-values in parentheses. All surveys (2001, 2004 and 2007) used. In column (1) and (2) t-statistics clustered at the contract-type level, sample restricted to individuals employed in plants with more than 15 employees with a permanent or a temporary contract. $EP = 1$ if the employee has 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 (3) and (4) t-statistics clustered at the contract-type level, sample restricted to individuals employed in plants with less than 15 employees with a permanent or a temporary contract. $EP = 1$ if the employee has a permanent contract. 20 control variables and 19 regional dummies included in all specifications. In column (2) and (4) *Job start* − *year* Fixed Effects used instead of Survey Fixed Effects.

Table 4: Triple Differences Estimates with Multiple Periods and Groups

	(1)	(2)	(3)
Dependent variable	*Logarithm of monthly wage*		
Method	DDD (all datasets) Entire Samples	DDD (all datasets) Entire Samples	DDD (all datasets) Entire Samples
Coeff.			
$EP * January01\_December03$	$-.011$ (.800)	$-.009$ (.822)	$-.009$ (.882)
$EP * January04\_December07$	.038 (.426)	$-.032$ (.492)	$-.111$ (0.138)
$(EP) * (January01\_December03) * (Permanent)$	$-.023$ (.134)	$-.022$ (.114)	$-.031$ (.101)
$(EP) * (January04\_December07) * (Permanent)$	$-.055^{**}$ (.021)	$-.051^{**}$ (.023)	$-.053^{***}$ (.023)
$(Job\ start - year) * (EP)$ Fixed Effects (9)	No	No	Yes
$Job\ start - year$ Fixed Effects (9)	No	Yes	Yes
$Firm\ size$ Fixed Effects (2)	Yes	Yes	Yes
Survey Fixed Effects (3)	Yes	No	No
Clustered S.E.	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes
Obs.	29,954	29,954	29,954
R^2	.22	.23	.23

Notes: OLS estimates. Robust p-values in parentheses (t-statistics clustered at the firm size level). The dependent variable is the log of monthly wage. All surveys used. In all columns workers employed with either permanent or temporary contract are considered; $EP = 1$ 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.

Table 5: Difference in Differences Estimates: Robustness 2 and Falsification 2

	(1)	(2)	(3)	(4)
Dependent variable	*Logarithm of monthly wage*			
Method	DD (all datasets)	DD (all datasets)	DD (all datasets)	DD (all datasets)
	+15 Empl. vs Self-empl.	+15 Empl. vs Self-empl.	-15 Empl. vs Self-empl.	-15 Empl. vs Self-empl.
Coeff.				
$EP * January01\_December03$	$-.061$ (0.189)	$-.063$ (0.189)	$-.073$ (.228)	$-.063$ (.328)
$EP * January04\_December07$	$-.048^{**}$ (.016)	$-.049^{**}$ (.015)	.084 (.116)	.081 (.126)
Job start − year Fixed effects (9)	No	Yes	No	Yes
Firm size Fixed Effects (2)	Yes	No	Yes	No
Survey Fixed Effects (3)	Yes	Yes	Yes	Yes
Clustered S.E.	Yes	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes	Yes
Obs.	21,264	21,264	10,300	10,300
R^2	.16	.17	.14	.15

Notes: OLS estimates. Robust p-values in parentheses (t-statistics clustered at the job-type level). The dependent variable is the log of monthly wage. All surveys (2001, 2004 and 2007) used. In column (1) and (2) only workers employed with a permanent contract in plant with more than 15 employees and self-employed workers are considered; $EP = 1$ only if an individual is a dependent worker. $January04\_December07$ 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 (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. $EP = 1$ if the individual is a dependent worker. 20 control variables and 19 regional dummies included in all specifications as well as 3 sample-year specific effects. In column (2) and (4) *Job start − year* Fixed Effects used instead of Survey Fixed Effects.

Table 6: Propensity Score Difference in Differences Estimates

Dependent variable	*Logarithm of monthly wage*					
	(1)		(2)		(3)	
	PSDD +15 Empl. *vs.* -15 Empl.		PSDD +15 Empl. *vs.* Temp.		PSDD +15 Empl. vs Self-Empl.	
Method	N.N.	Kernel	N.N.	Kernel	N.N.	Kernel
Coeff.						
$EP * January01\_December03$	$-.012$ (.545)	.010 (.111)	$-.041$ (.876)	$-.008$ (.312)	.011 (.789)	.078 (0.119)
$EP * January04\_December07$	$-.029^{***}$ (.007)	$-.030^{**}$ (.048)	$-.041^{***}$ (.001)	$-.026^{**}$ (.027)	$-.061^{***}$ (.001)	$-.061^{***}$ (.006)
Firm size Fixed Effects	Yes	Yes	No	No	No	No
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	12,081	18,634	16,520	22,825	13,046	21,264

Notes: PSDD estimates. Bootstrap standard errors. The dependent variable is the log of monthly wage. All surveys (2001, 2004 and 2007) used. 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 has been employed from January 2001 to December 2003. In column (1) only workers employed with a permanent contract are considered; $EP = 1$ only if an 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. $EP = 1$ 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. $EP = 1$ if the individual has a permanent contract in a plant with more than 15 employees. 20 control variables and 19 regional dummies included in all specifications as well as 3 survey specific effects. The propensity score has been estimated by a Logit 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.

APPENDIX

Table A1: Description of Variables

Individual and Household	
Female	Dummy variable indicating the respondent's sex, Female=1, 0 otherwise.
Age	Respondent's age at the interview.
Employed	Dummy variable indicating if the respondent is working at the interview, Employed=1, 0 otherwise.
Wage	Monthly wage of full-time workers.
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 according to the ISTAT classification.
Education	
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: H.Sch.Gr. Lyceum; H.Sch.Gr. Teaching; H.Sch.Gr. Accountancy; H.Sch.Gr. 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.
Educational mismatch	Dummy variable for the answer to the question: "Is your degree a required qualification for your job?", Mismatch=1 if the answer is not, 0 otherwise.
Job	
Permanent job	Dummy variable indicating if the respondent has a temporary or a permanent contract at the interview, Permanent job=1, 0 otherwise.
Para-subordinate job	Dummy variable indicating if the respondent has a para-subordinate temporary contract (*contratto a progetto*) 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$\leq$ 5; Firm size=1 if 5 <employees< 15; Firm size=2 if 15 $\leq$employees< 50; Firm size=3 if 50 $\leq$employees< 100; Firm size=4 if employees$\geq$ 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 A2: Frequency and Average of variables in the samples: Curricula and Family Background, 2001, 2004, 2007.

	2001		2004		2007	
	Frequency	Percentage	Frequency	Percentage	Frequency	Percentage
Individual Features						
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	16,477	79.0%	20,733	82.6%	20,426	78.1%
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	
Majors						
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	23.83	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%

Note: The averages are sample averages. For final marks (high school and university) averages are with respect to the number of individuals in the group.

Table A3: Frequency and Average of Labor Market Variables in the Samples, 2001, 2004, 2007.

	2001		2004		2007	
	Frequency	Percentage	Frequency	Percentage	Frequency	Percentage
Whole sample						
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%
Employed Individuals						
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%
Dependent workers						
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 $Firm < 15$ employees	2,316	22.5%	1,661	16.0%	1,858	17.7%
Employed in $15 \leq Firm < 100$ employees	3,845	36.1%	3,722	35.6%	3,715	35.3%
Employed in $Firm \geq 100$ employees	4,406	41.4%	5,040	48.4%	4,942	47.0%
Wage						
Obs.	11,093	72.3%	13,148	71.8%	15,041	83.9%
Mean wage	1,026 Euro		1,113 Euro		1,180 Euro	

Table A4: Statistics for Treated and Control Groups Before and After the Reform

Obs. Before 25,058	Treated 37.4	Control_1 10,4	Control_2 34.7	Control_3 17.5
Obs. After 16,651	Treated 38.1	Control_1 9.2	Control_2 37.0	Control_3 15.7

	Treated			Control 1			Control 2			Control 3		
	Before	After	Diff.	Before	After	Diff.	Before	After	Diff.	Before	After	Diff.
Obs.	9,147	6,348	-30.6	2,615	1,534	-41.3	8,901	6,348	-28.7	4,395	2,421	-44.9
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	50.5	23.4	-27.1	55.1	24.8	-30.3	42.7	18.6	-24.1	54.7	24.0	-30.7
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	21.4	18.5	-2.9	26.8	25.3	-1.5	24.6	22.7	-1.9	17.1	15.1	-2.0
Medicine	0.5	0.1	-0.4	0.9	0.3	-0.6	4.0	1.0	-3.0	1.3	1.0	-0.3
Humanities	12.8	10.1	-2.7	17.9	13.3	-4.6	24.3	18.6	-5.7	13.1	8.6	-4.5
Econ&Law	33.1	37.0	+3.9	38.5	38.7	+0.2	27.4	33.7	+6.3	28.3	33.2	+4.9
Engineering	31.8	32.3	+0.5	15.3	17.9	+2.6	18.3	21.5	+3.2	36.7	37.8	+1.1

Notes: Apart from Observations, University grade and High school grade, all reported numbers are percentages and refer to the share in the respective group. In the first two lines percentages refer to the share of each group before and after the reform respectively. '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.