Two-Tier Labor Market and Wage of Protected Entrants: Evidence from a Quasi-Natural Experiment

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Abstract

This study aims at investigating whether wage of protected entrants may be affected by the introduction of a two-tier labor market regime. By using micro-data, we implement double and triple differences estimator in a quasi-natural experimental setting ideally provided by the Italian labor market reform occurred in 2003. The results are robust and show that after the policy implementation workers entering positions entitled to labor market protection experience a reduction in earnings of about 5.0%. A corollary of this result is that two-tier reforms may actually raise competition among workers reducing wage of protected entrants.

Jel classification: J63, J64. Key Words: EPL, Flexibility-at-the-Margin, Difference-in-Differences.

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1 Introduction

This study aims at assessing whether the creation of a two-tier employment protection regime has an impact on wages of protected entrants. We investigate if the introduction of this institutional framework and the resulting change of turnover costs affect wage by modifying workers' bargaining power, firms' outside options and labor demand. Using Italian data on university graduate workers, we show that employees who enter positions entitled to labor market protection experience a reduction in earnings of about 5.0% after the creation of a two-tier labor market. This is consistent with a scenario wherein the presence of flexible jobs leads to an underbidding of entry wage of protected workers.

In the recent past many European countries have experienced in-depth deregulation of labor markets. In order to cope with high unemployment rates, many governments have made use of policy instruments targeted to obtain decentralization of the collective bargaining system and employment flexibility. As a consequence over the past fifteen years a substantial amount of research has been devoted to understanding the effects of the reform mainly focusing on the impact of labor market reforms on labor utilization and unemployment. Among others, 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 labor market deregulation 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. As things stand, the evaluation of the effects of such reforms on wage setting and wage differentials is still an open issue (OECD, 2007). Our aim is to provide evidence on this respect. We believe this topic is particularly relevant since it may contribute to the understanding of the determinants of wage inequality between temporary and permanent workers and to figure out to what extent a further flexibilization of the labor market may lead to a decrease of the existing wage gap.[1]

The empirical background is the following. In late 2003 Italy undertook a severe labor market deregulation. This policy introduced the so called flexibility at-the-margin since, while workers in permanent jobs fully maintain their protections, firms may create new temporary positions by using new contractual forms for fixed-term employment. Indeed, fixed-term contract were already in use in Italy in 2003 although they were characterized by the fact that they could not be renewed at will (in some cases they could be extended for a very short period) and at the end of the contractual period they could be either destroyed or converted into permanent jobs. The 2003 reform pushed further the idea of flexible labor by introducing a new type of fixed-term contract: *para-subordinate job*. Workers employed under this regime are not considered as standard temporary dependent employees and, consequently, they are not subject to standard norms and tutelages. In particular, *para-subordinate* contracts have to be *not* necessarily destroyed or converted into permanent jobs when they expire. Instead, they can be renewed at will. This new regime implies that these contracts can be used *de facto* to repeatedly hire the same worker into the same job eluding norms for standard subordinate positions.

[1] Recent studies include Elia (2010) and Picchio (2006) who find persistent wage differentials between permanent and temporary workers in Italy. Similar results have been found in Mertens *et al.* (2007) for Germany.

In Italy, workers employed under this regime are known as *precari*.[2] The creation of this peculiar institutional framework allows to assess whether wage earned by workers who enter fully protected jobs has been underbidden by the creation of this form of unprotected employment. This can be done by using a natural experiment setup relying on the fact that in Italy employment protection varies according to firm size. In particular dismissal constraints are not enforced in case of small units (plants with less than 15 employees). This normative setting generates an exogenous threshold of the existing employment protection regime which applies both *before* and *after* the 2003 reform. It is then possible to construct a control group - namely individuals entering firms with less than 15 employees - in order to apply difference-in-differences procedure (DD) to evaluate if the creation of a two-tier labor market affects wages of fully protected workers.

The paper is divided as follows. Section 2 discusses some existing studies focused on the labor market effect of employment protection legislation (EPL) and of two-tier reforms. The Italian institutional setting is briefly described along with the characteristics of the main implemented reforms. Section 3 presents our dataset and discusses the empirical model and the identification strategy. Section 4 contains the results of the main specification and presents several robustness and falsification tests. In Section 5 some concluding remarks are addressed.

[2]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.

2 Literature and Institutional Setting

2.1 The wage effect of a two-tier regime

Previous literature either theoretical or empirical mostly concentrates on the impact of EPL on employment flows while the analysis of wage formation in a two-tier regime has received a minor interest. In the empirical literature, most of the existing studies ascertain the effect of labor market reforms focusing on international comparisons and cross-country variation of EPL strictness indicators. A significant effect of EPL on unemployment inflows and outflows is reported while ambiguous findings concerning employment and unemployment levels and job turnover are detected (recent studies include Di Tella and MacCulloch, 2005; Nickell *et al.*, 2005; and Garibaldi and Violante, 2005). Nonetheless, it has been recognized that works based on international comparisons often fail to account for the share of workers on temporary contracts and, furthermore, they do not control for the interaction of EPL with wage setting institutions (Boeri, 2010). To cope with these concerns, some recent studies estimate the effects of EPL using *before-after* estimator *within* country implementing natural-experiment methodologies. Acemoglu and Angrist (2001) use US microdata to assess the effects of EPL relying on the impact of the Employment Disability Act. In this vein, Boeri and Jimeno (2005) compare pre- and post-reform labor market outcomes taking into account asymmetries in the enforcement of EPL with respect to firm size. Using Italian and Spanish data, these authors highlight that there is a significant discontinuity in the size distribution of firms in conditioning layoff and hiring probabilities for permanent workers. Similarly, Garibaldi *et al.* (2004) find significant evidence on the impact of firm size threshold on employment dynamics in Italy. Leonardi and

Pica (2007) evaluate the effect of EPL on wages considering a reform that in 1990 introduced unjust dismissal costs in Italy for firms below 15 employees finding no effect of the reform on entry wages although they find a significant decrease of returns to tenure. Theoretical papers show that in the absence of frictions, firing restrictions should not have any impact on employment since wage may be set taking into account the posting of a bond from the worker (Lazear, 1990). However, in the presence of market imperfections EPL may have a real effect and it may ambiguously affect employment levels (Bertola and Rogerson, 1997). As pointed out by Boeri (2010) it is important to single out the impact of EPL on wage of existing insiders and on that of entrants. 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. However, in a two-tier labor market protected workers may 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, wage differentials 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. Our paper sheds some light on this topic. Our natural experiment focuses on labor market deregulation and considers only workers at their early labor market experience. We show that the introduction of the so called flexibility at-the-margin actually raises competition among workers leading to an underbid of wage of protected entrants. Nevertheless, the understanding of the specific mechanisms through which the policy produces its effects remains an unresolved issue.

2.2 The Institutional setting and the 2003 Reform

The implementation of the reform through the legislative decree 276/2003, definitely in charge after December 2003, has become one of the most significant shock 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 for firms with more than 15 employees. In fact, since 1973, the most binding institutional constraint for individual dismissals is represented by the Article no. 18 of the Italian Labor Code. This norm allows for individual dismissal only if it is justified by a *just cause* rule. Workers have the right to appeal firm and the judge establishes whether the dismissal is unfair. The court 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 for a period that ranges between 2.5 and 6 months. Conversely, firms employing more than 15 workers have to entirely pay the forgone wages and, most importantly, they must re-hire the worker. It is noteworthy that 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 evaluate the threshold, apprentices and temporary workers with a tenure shorter than nine months are not considered, while part-time workers and all other temporary contracts are included. 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 providing incentives for part-time work and introducing temporary contracts that may be either destroyed or transformed into permanent contract when they expire. Efforts to increase labor market flexibility were taken forward with the 2003 reform. These norms further deregulated the use of atypical work arrangements, such as temporary agency (staff-leasing) and part-time, and introduced new forms of atypical work such as on-call jobs, job sharing and *para-subordinate* work (lavoro a progetto). According to a recent legal debate, *para-subordinate* jobs represent the core of the reform. These are occasional jobs that cannot be configurated as self-employment since they have no economic risk, they have to observe a strict timetable and they are rewarded with a 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 is noteworthy 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).

3 Data and Identification Strategy

3.1 The Samples

The empirical investigation 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.[3] We rely on these specific repeated cross-sections for four main reasons.

Firstly, these surveys allow for the implementation of an experimental design. In particular, consider individuals interviewed in 2001. These are all individuals whose labor market outcomes are recorded *before* the reform. Now, consider the 2007 survey. In this case, labor market outcomes are recorded for all individuals *after* the reform. These two samples cover a 10 years period (1998-2007) and would be sufficient to construct an experiment. However, we have additional information coming from the 2004 sample which contains workers employed both under the new and the old regime. As we discuss in details in the descriptive analysis presented in the next paragraph, since the reform is in charge from December 2003, and since we have information concerning the starting date (year and month) of the current job for all employed individuals, within this specific sample we can separate those graduates who have been employed after the reform from the others. We remark that most of the individuals interviewed in 2004 have been employed under the old regime so, albeit we separate individuals according to the regime in charge at the time of their labor contract, the reader may think of this survey as largely composed by individuals employed *before* the reform.

Secondly, for those workers who were actually employed at the time of the interview the survey reports many information concerning the job position and,

[3]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.

among them, it contains indication concerning the number of workers employed in the single plant where each graduate is employed. 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 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.

Thirdly, all surveys contain information concerning the type of labor contracts. This information is crucial since it makes possible to address an obvious *caveat* arising when comparing small and large firms, i.e., the presence of a possible confounding trend. Indeed, if different trends were at work, these could affect wages of workers in a different way according to firms' size and then we would confound the trend-effect with the reform-effect. The information concerning labor contracts makes possible to cope with this issue leading to the construction of an alternative control group, namely temporary workers in large plants, which in turn allows for the construction of a triple-differences setup (DDD) which make possible to disentangle trend-effects from employment-protection effects.

Finally, a further advantage of using these data sets relates to the main research question of this paper. By relying on young graduates at their early labor market experience we can untangle the impact (if any) that the creation of a two-tier regime has on wages of protected entrants, avoiding problems related to insiderness or rent exerting mechanisms related to tenure and membership's aspects. Indeed, if we consider the entire insiders category, the introduction of flexibility at-the-margin might go in two apposite directions: *i*) it may induce a reduction in wage of protected workers due to a loss of bargaining power and a decrease in labor demand since firms' outside options become less costly; *ii*) it may exacerbate harassing and non-cooperative behavior adopted by protected insiders who, consequently, my be able to exert some rents from new entrants (Lindbeck and Snower, 2001). Since these effects may offset each other, they should be disentangled in order to understand the policy transmission mechanisms. In this respect, the use of data on young workers at their early labor market experience, enables us to exclude that workers in our sample may already undertake rent exerting behavior. Therefore, by relying on these data we can assess if the introduction of flexibility at-the-margin affects the wage of protected employees through an increase in competition among entrants.

3.2 Preliminary Statistics

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) interviewed three years after their graduation.[4] 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. The data provide indications to determine if individuals are employed, unemployed or out of the labor force, their degree qualification, region of residence, and many other personal characteristics. For those individuals who are employed at the time of the interview, the survey records if they are dependent workers or self-employed and for the former it records the type of job contract (part-time/full-time temporary/permanent), plant dimension, industry sector, firm's ownership (private/public) and the date of job start (year and month). In addition, the survey records if workers are in job positions where the competencies acquired at university are actually needed, hence it is possible to control for possible effect of educational mismatch which proved to impose significant penalization to Italian graduates (Ordine and Rose, 2011). Moreover, these surveys give information on high school performance of individuals (final mark and type of school) and on their family background (parents' education). These variables are relevant in or-

[4]The 2007 survey explicitly separates those graduates who, after the 3+2 university reform implemented in 2001, enrolled at universities under the new regime. Indeed, since at that time the old regime was in charge along with the new one, the ISTAT survey collected two separated representative samples for both the old and the new regime. We use only the survey covering the old regime which is fully comparable with the previous ones (similar number of graduates, majors, years of education, etc.). Moreover the survey which refers to the new university-regime contains only graduates with a three-years degree since 5 years were not elapsed since the higher education reform.

der to reduce the effect of the impact of unobserved heterogeneity when estimating individual wage equations. Table A3 contains information concerning graduates employed at the time of the interview for all our surveys. We remark that the share of unemployed graduates reduced from 9.3% in 2001 to 7.0% in 2007. Interestingly, we notice that the share of individuals who are out of the labor force increased from 17.1% to 22.3% and this is probably due to an increase in postgraduate education. The type of job - in terms of permanent/temporary characteristics - also changed during the considered time period. Table A3 shows that amongst dependent workers the share of permanent contracts fell down from 75.5% in 2001 to 69.2% in 2007. It is remarkable that most of the decrease in permanent contracts is associated with a more intense use of new temporary contracts and in particular of para-subordinate jobs. In particular, in 2007 6.3% of dependent workers is employed as a para-subordinate worker while this percentage was about 0.4% in 2004 and, obviously, zero in 2001. These preliminary statistics show that there is variability across the three samples which goes exactly in the expected direction, i.e., the 2007 sample is characterized by workers that have been somehow affected by the reform.

3.3 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 in plants with more ore less than 15 employees. Some insights can be gathered by inspecting these series. At the outset, 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 insights come up if we split the sample before and after the labor market reform using 2004 as a rough threshold (in next section we fix the threshold in a more precise way using information concerning month of job start). In Figure 2 we compare temporary workers (unprotected employees) according to firm size. It is interesting to notice 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 reduce with respect to unprotected categories. 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 Section 4.

3.4 The Identification Strategy

The identification strategy presented in this study is funded on the exogenous threshold separating firms in terms of dismissal constraints. It is used the fact that the introduced flexibility - in the form of labor contracts that can be renewed at will without imposing dismissal constraints - should be less relevant for firms that are exempted from the EPL restrictions. By means of this threshold 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*. The peculiarity of this normative setting generates an exogenous threshold of the existing employment protection regime which applies both *before* and *after* the 2003 reform. We can then apply difference-in-differences procedure (DD) to assess whether the creation of a two-tier labor market affects the wage of fully protected workers.

3.5 Addressing some caveats

The approach highlighted in the previous paragraph is, however, not straightforward. A first problem arises since possible measurement errors may derive from firm dimension. This issue, while being of a minor relevance when separating very small and very large firms, could be problematic around the threshold. Furthermore, as already pointed out, the DD estimates may be biased because employment protection may apply for small plants too. This might induce an underestimation of the wage effect of turnover costs on protected workers' wage, hence we need to address this point. In order to check if measurement error can drive our result we adopt the following strategy. Firstly, we consider only individuals employed in plants with more than 50 employees for which employment protection always applies for permanent workers and - within this sub-sample - we construct an alternative control group, i.e., individuals not entitled to employment protections because employed with a temporary contract. Once again, our results prove to be robust and do not seem to be affected by the threshold-setting method or by the choice of the control group. Moreover the results are supported by falsification tests implemented by using small plants and different job categories. Secondly, we make use of an alternative control group that can be constructed with our data, i.e., self-employed individuals. Using this peculiar category of workers, we undertake additional robustness and falsification tests which all go in the same direction: after the reform only protected workers have been invested by a wage reduction.

On top of the issues highlighted so far, an additional (and crucial) *caveat* could undermine our causal interpretation of the results. Indeed, 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 beneficed 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 leading us to cast some doubts on our causal interpretation of the results. To deal with this issue, we estimate difference-in-difference-in-differences models (DDD) where we combine the control group available because of the 15 employees threshold and the control group available when considering individuals employed in large firms *not* entitled to protection because employed in temporary jobs. This methodology collapses all available information in a unique framework. The results make us fairly confident that the 2003 reform generated a negative externality for new protected entrants.

4 Check, Robustness and Falsification of the Identification Method

4.1 First Verification: A Double Difference Approach

4.1.1 First check: simple double differences

We start our analysis by considering the 2004 survey where we exploit information concerning the date of beginning of the employment contract. We separate those workers whose job started before the reform from those employed under the new regime and we estimate the 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 (December 2003) reform. The dependent variable is the logarithm of monthly wage earned by individual i. The sample considers only full time permanent dependent workers and in this case it consists of about 7,500 individuals. In the RHS of eq. (1), $\mathbf{X}_i$ indicates a set of 20 control variables (age dummies, gender, marital status, 4 major dummies, university leaving grade, a dummy indicating the time to degree (degree on time), high school leaving grade by 5 types of high school, parents' education, a multilevel firm size dummy, a dummy for the public sector, a multilevel dummy for industries, and a dummy for educational mismatch) plus 19 regional dummies. $EP = \{0, 1\}$ 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 for permanent workers in large plants after the reform compared to permanent workers in small firms. Table 1 contains the results obtained by clustering standard errors at the plant dimension level in order to take into account the issue of serial correlation (Bertrand *et al.* 2004). In column (1), we detect a significant positive value for δ_2. At a first sight, this finding would be consistent with an interpretation of the effect of the two-tier reform which goes in the direction of an increase of bargaining power of protected workers (Picchio, 2006). However, the interpretation of this parameter must be really cautious, even if we are using a DD estimator. In fact, it is not possible to draw causal inference on the effect of the reform since there can be endogenous selection of workers in large firms driven by unobserved characteristics. Moreover, a causal interpretation of our finding would hinge on the assumption that the date at which the worker is employed is not related to firm size. Of course, we cast some doubts on this assumption since individuals may wait into unemployment to have better employment opportunities in large firms paying higher wages and providing employment protection. Hence, the two groups that we consider in our first evaluation exercise may actually be different in terms of relevant characteristics and the results may be driven by this heterogeneity. Put differently, the apparent effect of the reform may hide a bias due to endogenous treatment selection. In order to pinpoint our considerations, in column (2) and (3) of Table 1 we report the results of placebo effects estimated by using the 2001 and 2007 samples respectively. In these cases we use December 2000 and December 2006 as the (false) before-after cutoffs to define t. It is interesting to remark that a positive and significant parameter also characterizes the 2001 and 2007 samples and this supports the groups' heterogeneity hypothesis. In the light of these results we proceed by pooling our samples and using them simultaneously in order to overcome dangerous drawbacks.

4.1.2 Second Check: double differences across sub-samples

We estimate eq. (1) by using the 2001, 2004 and the 2007 samples in the following ways. We start by considering individuals employed before the reform from the 2004 survey. In this case we consider all individuals employed before December 2003. We compare them with the 'same' individuals from the 2007 survey whose occupation started no later than December 2006. Hence, we use two sub-samples of the 2004 and 2007 datasets and, in terms of eq. (1), $t = 1$ only for workers from the 2007 sample. In this way, we compare individuals that are identical across the two cross-sections so that problems of group heterogeneity and endogenous selection should be overcome. In column (4) of Table 1, our DD estimate detects a significant value for δ_2 with a point estimate of about -3.8% which highlights a wage penalization for protected workers employed after the reform. At this stage, it is interesting to present some falsification and robustness tests. A first test is derived by using simultaneously two sub-samples of the 2001 and 2004 dataset. In particular we still consider individuals employed before December 2003 in the 2004 sample but we compare them with individuals from the 2001 sample employed before December 2000. In this case, a falsification test is implemented by considering as treated only individuals from the 2004 survey. Results are reported in column (5) of Table 1. Interestingly, no significant value for δ_2 is found and this was expected since both groups are characterized by individuals whose job started before the reform. As a next step we replicate *mutatis mutandis* this exercise using the 2001 and 2007 samples. In column (6) of Table 1 we report estimates of eq. (1) obtained by using only individuals employed before December 2000 and before December 2006 and by imposing $t = 1$ only for the latter. In this case the estimated point value of δ_2 is about -9.6% and it is strongly significant, confirming our previous finding.

4.1.3 Third check: double differences across additional sub-samples

In this part of the study, we compare all remaining sub-samples characterizing our datasets, i.e., we make use of individuals employed after December 2000, December 2003 and December 2007 from the 2001, 2004 and 2007 survey respectively. Results are reported in Table 5. We start by comparing individuals from the 2001 and the 2007 samples setting $t = 1$ only for the latter since only this group has been exposed to the reform. In this case, as reported in column (1) of Table 2, we detect a value of δ_2 of about -8.9% which is significantly different from zero and in line with our previous findings. Further, if we compare individuals from the 2001 and the 2004 sample and we set $t = 1$ for the latter (who have been actually exposed to the reform) we do not find a significant value for δ_2 (column (2) of Table 2). Indeed, this was actually expected since we are considering as treated those individuals who have been employed immediately after the reform. In this case, it is well possible that the mechanisms of the reform were not yet completely at work (on this argument see Lee, 2004). Conversely, if we use individuals from the 2004 and the 2007 samples considering the latter as treated, we actually do detect a significant and negative value of about -7.8% (column (3) of Table 2). This result is consistent with a view in which the effects of the reform do not show in the very short run, but they seem to have determined differences between individuals employed immediately after the reforms and those employed later on. However, these preliminary findings call for a more in-depth investigation.

4.1.4 Fourth Check: double difference across samples

In order to complete the analysis undertaken so far, in this section we estimate our DD model in eq. (1) by still carrying out pair(s) comparison but, differently from before, we make use of the entire samples. We start by comparing the 2001 and the 2007 samples and we set $t = 1$ for the latter implementing a pure before-after methodology. As reported in column (4) of Table 2, the estimated value for δ_2 is statistically significant and is about -9.4% which is in line with the estimates reported previously. Furthermore, in the same table, column (5) shows the DD parameter obtained by comparing the 2004 and 2007 individuals with $t = 1$ only for the latter. In this case we find an overall negative effect of about -3.7% which is statistically significant. Finally we estimate our model using the 2001 and the 2004. In this case we set $t = 1$ for 2004 individuals and we do not find significant value for δ_2 as reported in column (6). Albeit these results are interesting, some weakness of the presented estimation strategy should be remarked at this stage. Firstly, since we consider all individuals from the 2004 sample as 'untreated' (while some of them could be actually treated) some of our estimates could be biased. Secondly, since pair comparisons have been implemented, our estimates are not efficient. In the next paragraph we address these specific concerns.

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

4.2.1 A complete DD framework

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 according to the following framework:

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

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 are two groups fixed effects for workers in plants with more or less than 15 employees. Only permanent dependent 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 the 19 regional dummy variables plus the 20 control variables as described in paragraph 4.1.1. Variable $(EP * January01\_December03)_{isj}$ is a dummy taking the value 1 if the individual is entitled for employment protection and has found a job in the period January 2001-December 2003. $(EP * January04\_December07)_{isj}$ is a dummy variable taking the value of 1 if the i individual is entitled for employment protection and has found a job after December 2003. Therefore, the reference dummy variable considers protected individuals whose occupation starts between January 1998 and December 2000. As in the previous paragraph the coefficient of main interest is δ_2. According to our previous results, this coefficient should range between -3.0% and -10.0%. It is worth noting that the introduction of the variable $(EP * January01\_December03)_{isj}$ allows us to test the common time trend assumption, i.e., prior to the reform there should be no significant differences in the evolution of wage for both workers with and without employment protection. As before, standard errors are clustered at the plant dimension level. Table 3 presents the results. In column (1) δ_2 is equal to -6.9% and it is statistically significant. This means that entrants entitled to employment protection had a wage loss after December 2003 compared to those employed in 1998-2000. The common time effect assumption is verified being δ_1 not statistically different from zero, as reported in column (1) of Table 3.

4.3 Third Verification: Using Alternative Control Groups

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

In this part of the paper we address concerns arising from our assessment of plants' dimension based on worker's indication. Indeed, while it is reasonable to think that the worker is able to evaluate the number of employees working in the plant where he/she is employed, there can be co-workers that are employed part-time. Whether these individuals have been accounted as full time employees, we may incur measurement errors that may bias our results. This issue comes along with another *caveat* related to plant dimension: since a small plant may be part of a larger firm operating in the province with multiple plants employing overall more than 60 workers, an additional source of bias may arise. In this case, we may consider as 'untreated individuals' workers that actually have been exposed to the treatment. In order to deal with these problems we rely on the following strategy. We make use of an alternative control group that can be constructed in our sample, i.e., temporary workers employed in large plants. In particular we consider only those workers who declared to be employed in plants with more than 50 employees. In this case, we are considering only plants that are constrained by employment protection for permanent workers. Then, within these employees we separate two groups: permanent (full protected) and temporary (unprotected) workers and we estimate the same framework of eq. (2) where EP_{isj} is a dichotomous variable taking the value 1 if the individual is employed with a permanent contract. In column (2) of Table 3 we report estimates for δ_1 and δ_2. Interestingly, our main results are entirely confirmed being δ_1 not statistically different from zero while δ_2 is negative and significant indicating a penalization for permanent workers employed after the reform of about -2.7%. It is important to note that, although we have strongly modified our data by using temporary workers (previously excluded) as a reference category, the results goes in the expected direction: an overall decrease in terms of wages for protected workers after the 2003 reform has occurred.

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

In order to complete 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 wages of temporary and permanent workers. 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. All coefficients are not statistically different from zero.

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

A further check is carried out using additional observations available in our sample, referred to self-employed individuals. They are about 8,000 (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 workers employed in plants with more than 15 employees. We estimate the same setup of eq. (2) where EP is dummy variable equal to 1 only for dependent workers with a permanent contract. In column (1) of Table 4 we report the results, which are exactly 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 with 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.

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

An final falsification exercise is presented at this stage. Column (2) of Table 4 contains the results obtained by restricting the sample to self-employed workers and dependent employees in small plants. 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: A Triple Difference Approach

4.4.1 Assessing triple differences

A key concern arises at this stage. Albeit the highlighted results appear to be robust according to many 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 a larger spillover effect 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. This type of problem still holds when self-employed individuals are compared with workers employed in large plants. To deal with this concern, in this part of the paper we make use of an additional control group already highlighted, namely temporary workers employed in large firms. In order to control for possible confounding trends we apply the following procedure. First, we separate workers according to plant dimension (15 employees). Second, within these two groups, we separate between workers with a temporary or a permanent contract. In this way 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 DDD estimate of the causal effect of the 2003 reform on the wage of workers entitled to employment protections.

4.4.2 Results

Preliminary results are reported in column (1) of Table 5. 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 $(EP * January04\_December07) * (Permanent)_{isj}$. This coefficient measures the relative variation after December 2003 of the wage of permanent workers minus that of temporary workers in large plants with respect to the wage of permanent workers minus that of temporary workers in small plants. This coefficient 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 wage of permanent workers in large plants more than that of workers employed in small plants.

In column (2) of Table 5 we present additional estimates derived including among regressors year fixed effects (9) instead of survey fixed effect (3). 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 according to this additional specification too. Finally, in column (3) we report more robust estimates obtained after including among our regressors time varying large plant specific effects (9). 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 triple difference 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 associated to $(EP * January04\_December07) * (Permanent)_{isj}$ is statistically significant with a point estimate of -5.3%. These results make us fairly confident about the negative effect that the 2003 Italian labor market reform had on wages of protected entrants.

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 and increase the rents of outsiders with respect to those of protected entrants. Hence, the flexibilization of the labor market could raise competition among workers leading to a decrease of the entry wage of insiders. This effect may arise through both a worsening of the bargaining position of workers and a change in the employment strategies of firms which eventually lower labor demand of protected employees. To test this hypothesis we make use of Italian data exploiting a quasi-natural experiment provided by the creation of a new form of unprotected employment after the labor market reform undertaken in 2003. Using data on graduate workers we show that in the presence of a two-tier regime those who enter positions entitled to labor market protection experience a reduction in earnings of about 5.0%. This result is corroborated by a series of robustness checks and falsification tests carried out on different surveys, a large time span and various workers categories. The analysis presented in this work may be useful for policy since the evaluation of the determinants of wage inequality between temporary and permanent workers may contribute to figure out to what extent a further flexibilization of the labor market may lead to a decrease of the existing wage gap. However, it is crucial to remark that although the reported evidence points to a reduction in entry level disparities among workers, our findings can be consistent with different theoretical explanations which have very different implications for welfare and policy. It would be then relevant to evaluate how the entry wage has been affected by bargaining issues or by changes in labor demand. The comprehension of the exact contribution of these mechanisms to wage setting would be important to ascertain whether our empirical results mirror efficient outcomes or just an income redistribution in favor of entrepreneurs which might substantially deviate from the walrasian competitive allocation mechanism. These are challenges for future research.

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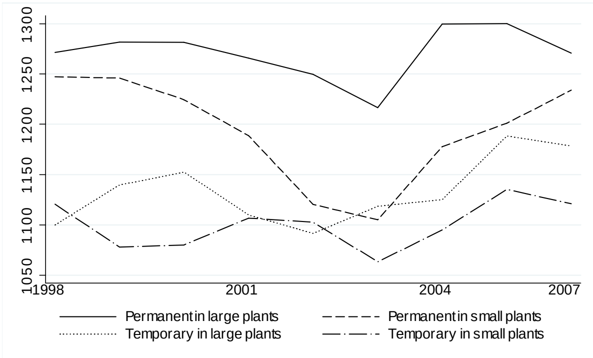

Figure 1: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) and plant dimension (more or less than 15 employees) over the period 1998-2007 in Italy.

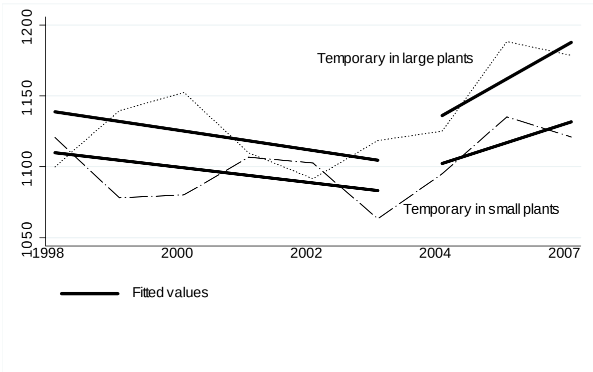

Figure 2: Average monthly wage (in Euros) of temporary workers according to plant dimension (more or less than 15 employees) over the period 1998-2007.

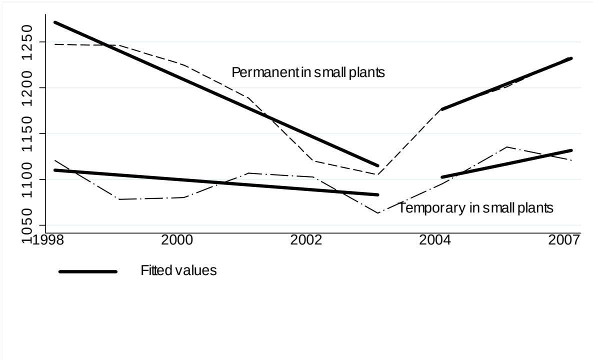

Figure 3: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) in plants with less than 15 employees over the period 1998-2007.

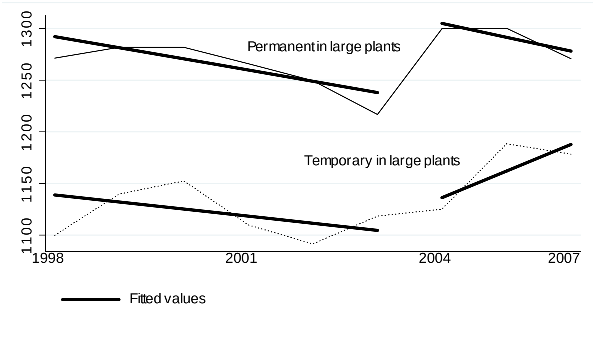

Figure 4: Average monthly wage (in Euros) according to type of job contract (permanent and temporary) in plants with more than 15 employees over the period 1998-2007.

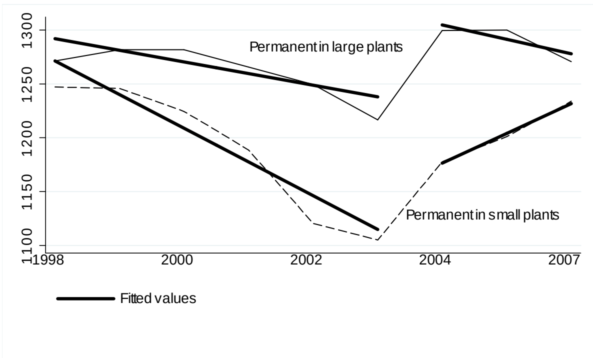

Figure 5: Average monthly wage (in Euros) of permanent workers according to plant dimension (more or less than 15 employees) over the period 1998-2007.

Table 1: Difference in Differences Estimates. First and Second Check.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	*Logarithm of monthly wage*					
Method	DD ('04)	DD ('01)	DD ('07)	DD ('04/'07) Sub-samples	DD ('01/'04) Sub-samples	DD ('01/'07) Sub-samples
Coeff.						
$t \cdot (EP)$	$.106^{***}$ (.001)	$.043^{**}$ (.047)	$.072^{**}$ (.012)	$-.038^{**}$ (.028)	$-.061$ (.102)	$-.096^{**}$ (.026)
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes	Yes	Yes	Yes
Regional Dumm. (20)	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	7,556	5,893	6,906	10,978	11,302	11,648
R^2	.26	.14	.22	.22	.16	.16

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. $EP = 1$ if an individual is employed with a permanent contract in a plant with more than 15 employees. In column (1) the sample is restricted to individuals employed with a permanent contract from the 2004 sample and $t = 1$ for individuals employed after December 2003; in column (2) the sample is restricted to individuals employed with a permanent contract from the 2001 sample and $t = 1$ for individuals employed after December 2000; in column (3) the sample is restricted to individuals employed with a permanent contract from the 2007 sample and $t = 1$ for individuals employed after December 2006. In column (4) the sample is restricted to individuals with a permanent contract from the 2004 and 2007 sample employed before December 2003 and December 2006 respectively, $t = 1$ for individuals from the 2007 survey. In column (5) the sample is restricted to individuals with a permanent contract from the 2001 and 2004 sample employed before December 2000 and December 2003 respectively, $t = 1$ for individuals from the 2004 survey. In column (6) the sample is restricted to individuals with a permanent contract from the 2001 and 2007 sample employed before December 2000 and December 2006 respectively, $t = 1$ for individuals from the 2007 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 educational mismatch) are included.

Table 2: Difference in Differences Estimates. Third and Fourth Check.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	*Logarithm of monthly wage*					
Method	DD ('01/'07) Sub-samples	DD ('01/'04) Sub-samples	DD ('04/'07) Sub-samples	DD ('01/'07) Entire samples	DD ('04/'07) Entire samples	DD ('01/'04) Entire samples
Coeff.						
$t \cdot (EP)$	$-.089^{**}$ (.043)	$-.016$ (.601)	$-.078^{**}$ (.011)	$-.094^{**}$ (.028)	$-.037^{***}$ (.025)	$-.060$ (.011)
Clustered S.E.	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.	2,814	2,147	1,821	14,462	12,799	13,449
R^2	.25	.23	.31	.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 employed after December 2000 and December 2006 respectively, $t = 1$ for individuals from the 2007 survey. In column (2) the sample is restricted to individuals from the 2001 and 2004 sample employed after December 2000 and December 2003 respectively, $t = 1$ for individuals from the 2004 survey. In column (3) the sample is restricted to individuals from the 2004 and 2007 sample employed after December 2003 and December 2006 respectively, $t = 1$ for individuals from the 2007 survey. In column (4) the sample is restricted to individuals from the 2001 and 2007 sample and $t = 1$ for individuals from the 2007 survey. In column (5) the sample is restricted to individuals from the 2004 and 2007 sample and $t = 1$ for individuals from the 2007 survey. In column (6) 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 educational mismatch) are included.

Table 3: Difference-in-Differences Estimates with Multiple Periods and Aternative Control Group (temporary workers)

	(1)	(2)	(3)
Dependent variable	*Logarithm of monthly wage*		
Method	DD (all datasets) Entire Samples	DD (all datasets) Plants > 50 Employees	DD (all datasets) Plants < 15 Employees
Coeff.			
$(EP * January01\_December03)$	$-.020$ (.431)	.007 (.244)	$-.028$ (.157)
$(EP * January04\_December07)$	$-.069^{**}$ (.044)	$-.027^{**}$ (.020)	$-.024$ (.111)
Firm size Fixed Effects (2)	Yes	No	No
Sample-year Fixed effects (3)	Yes	Yes	Yes
Clustered S.E.	Yes	Yes	Yes
Control Var. (20)	Yes	Yes	Yes
Regional Dumm. (19)	Yes	Yes	Yes
Obs.	20,355	20,446	5,825
R^2	0.20	0.23	0.17

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) 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) t-statistics clustered at the contract-type level, sample restricted to individuals employed in plants with more than 50 employees with either a permanent or a temporary contract. $EP = 1$ if the employee has a permanent contract. In column (3) t-statistics clustered at the contract-type level, sample restricted to individuals employed in plants with less than 15 employees with either 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 as well as 3 sample-year specific effects.

Table 4: Difference in Differences Estimates with Multiple Periods and Aternative Control Group (self-employed)

	(1)	(2)
Dependent variable	*Logarithm of monthly wage*	
Method	DD (all datasets) Protected *vs.* Self-empl.	DD (all datasets) Unprotected *vs.* Self-empl.
Coeff.		
$(EP * January01\_December03)$	$-.060$ (0.189)	$-.073$ (.228)
$(EP * January04\_December07)$	$-.048^{**}$ (.116)	.084 (.116)
Firm size Fixed Effects (2)	Yes	No
Sample-year Fixed effects (3)	Yes	Yes
Clustered S.E.	Yes	Yes
Control Var. (20)	Yes	Yes
Regional Dumm. (19)	Yes	Yes
Obs.	21,264	10,300
R^2	0.16	0.14

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) 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 (2) 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.

Table 5: Triple Differences Estimates with Multple Periods and Groups

	(1)	(2)	(3)
Dependent variable	*Logarithm of monthly wage*		
Method	DDD (2001/2004/2007) Entire Samples	DDD (2001/2004/2007) Entire Samples	DDD (2001/2004/2007) 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
Sample-year 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.	39,954	39,954	39,954
R^2	0.22	0.23	0.23

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 (2001, 2004 and 2007) 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 Sample-year 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.

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.
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%
Single	14,642	70.3%	18,360	71.0%	19,187	72.2%
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	2212	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	