Does a Wage Premium for Temporariness Exist? Evidence from Italy *

Andrea Albanese †
Luxembourg Institute of Socio-Economic Research (LISER)

Giovanni Gallo ‡
National Institute for the Analysis of Public Policies (INAPP)

Preliminary Version, please do not quote

January 21, 2019

Abstract

This paper wants to investigate whether a wage premium for temporary workers exists in Italy. Based on administrative individual level data, our analysis focuses on new-entry employees for the period 2005-2015 in temporary and open-ended contracts. To estimate the presence of a wage premium over the remuneration distribution, we follow Firpo (2007) and implement an inverse probability estimator on the observables. We assume unconfoundedness and exploit the long panel dimension of our data, which allows us to control for labour market history, including lagged outcomes, during the last 16 years. Our results show the presence of premium for temporariness over the full distribution of daily remuneration. There is also evidence of a different level of premium depending the type of temporary contract considered.

Keywords: Temporary work, Wage inequality, Unconditional Quantile Treatment Effect, Inverse probability weighting

JEL codes: J31, J41, C31, J21

*We thank the Ministry of Labour for granting access to administrative individual level data from social security registers of the Italian Social Security Institute (LoSai INPS). The usual disclaimer applies.

†Luxembourg Institute of Socio-Economic Research (LISER), Labor Market Department; Ghent University (SHERPPA). E-mail: andrea.albanese@liser.lu. Address: Portes des Sciences 11, Esch-sur-Alzette, Luxembourg.

‡Corresponding author. National Institute for the Analysis of Public Policies (INAPP), CAPP. E-mail: ext.gi.gallo@inapp.org. Address: INAPP, Corso d'Italia 33, Rome, Italy.

1 Introduction

The temporary contracts of employment were introduced in Italy in the Sixties, but for a long time they remain a clear minority in the Italian labor market. Indeed, temporary workers represented 5% of total Italian employees until 1993 (ILO, 2016). However their share hugely increased in the last decades, so that they represent most of new entrants to the labor market (Ministry of Labor and Social Policies, 2017) and 15.5% of total employees in 2017, 2.2 points above the EU28 average (Eurostat, 2018). The main reason of this sudden rise can be found in the reforms of Italian labor market performed starting from the 1990s. In that period the international labor market has known a deep change in terms of legislation and socio-economic features in order to cope with the growing needs related to the economic globalization. Moreover, an OECD (1994)'s study emphasized that the loss of competitiveness, the growth slowdown, and the increase in unemployment from 1970s to 1990s, especially in some countries such as Italy, were due to policies which did not favor flexibility in the labor market. Therefore, the aim of the above-mentioned reforms consisted of incentivizing the temporary employment and overall reducing the rigidity of the Italian labor market.

Two legislative interventions particularly encouraged the use of temporary contracts: the Treu Package (1997) and the Biagi Law (2003). The first one relaxed the rules for the apprenticeship use and introduced new types of temporary contracts (e.g. temporary agency workers). As for the second, it furtherly both incentivized the use of temporary contracts and enlarged their supply (e.g. introducing the casual work contracts). In the recent years, however, a route change seems to have taken place in Italy since the last two reforms of the labor market (the Fornero reform and the Jobs Act) tried to favor more stable work relations. To improve the national employment under both a quantitative and qualitative point of view, the Fornero reform (2012) limited the use of non-standard temporary contracts and the Jobs Act (2014-2015) incentivized the creation of permanent work contracts through massive fiscal benefits to the employers. The important role played by temporary contracts in Italy leads to investigate on the *bonus* and *malus* related to this particular employment relationship, especially on those regarding the wage.

This paper focuses on the effect of temporary contracts on the wage among new entrants to the private sector of the Italian labor market. The objective is to understand to what extent employees hired in temporary contracts receive a negative or positive wage premium keeping workers and firms observed characteristics constant. In addition, we want to investigate how the differential wage of temporary contracts changes along the wage distribution in order to catch any potential heterogeneity. Indeed, different forces might be operating at a different level of salaries. For example, lower skill workers may be damaged by temporary contracts, given the lower bargaining power attached to them, unlike high skilled workers, who might receive a wage premium to accept a flexible contract. To do that, we use administrative data from social security registers of the Italian Social Security Institute (*LoSai INPS*).

The novelty of the paper is twofold. First, we estimate differences in salaries over the full distribution of the contracts. We follow Lehmann (1974) and Doksum (1974) and estimate the unconditional Quantile Treatment Effect (QTE). To make the quantiles comparable in terms of composition, we adopt the methodology of Firpo (2007) and control for all the occupational history of employees over the last 16 years, which importantly includes lagged contracts and wages. Second, we differentiate between different types of temporary contracts to understand how the wage gap changes according to the feature of the contract.

The remainder of the paper is organised as follows. Section 2 contains a review of studies focusing on economic effects produced by temporary contracts. Section 3 describes the administrative data and sample selection. The descriptive evidences are presented in Section 4, while Section 5 explains the empirical strategy we use to estimate the effect of temporary contracts on the received wage. Section 6 shows results of the econometric analysis. Last section concludes with a discussion on policy implications.

2 Literature review

The literature presents many empirical studies which try to estimate the effect of fixed-term employment on the employees' wage. Rosen (1986) was one of the first economic studies to theorize a wage gap between temporary workers and permanent ones. According to his theory of equalizing differences, a wage gap is possible even at the same level of competence because of the less favorable conditions of temporary workers. In fact, they should be compensated with higher wages (i.e. wage premium) given the shorter job tenure and the worse job security. Nonetheless, several empirical researches found evidence that temporary workers receive a wage penalty rather than a 'wage premium' (Booth et al., 2002; Picchio, 2006; Cappellari and Jenkins, 2014; Cappellari and Leonardi, 2016). Only high-paid temporary workers appear able to gain a wage premium since they are generally highly skilled, and so they have a high bargaining power (Comi and Grasseni, 2012). At the opposite, low-paid temporary workers tend to persist in the same quintile group of employee income (Uhlendorff, 2006; Stewart, 2007; Cappellari and Jenkins, 2008). These two evidences have clear consequences on the earning instability and more in general on the inequality of national wage level, as highlighted by studies such as Brandolini et al. (2001), Mertens et al. (2007), Cappellari and Leonardi (2016), and Lass and Wooden (2017).

An aspect which may explain the existence of a negative wage gap is the lower level of training that might be received by temporary workers. For instance, Arulampalam and Booth (1998) and Booth et al. (2002) show that fixed-term employees have a lower probability to be involved in any type of training in the United Kingdom, especially those who have also a part-time contract or are not union members. The shorter job tenure and the consequent disincentive for employers to invest in training lead temporary employees to be less productive than open-ended ones (Booth et al., 2002; Draca and Green, 2004; Nienhuser and Matiaske, 2006), and thus to the risk of a persistent wage gap over time. Similarly, the lower bargaining power of temporary workers, generally younger and more low-skilled than the permanent ones, may represent another important factor to consider in order to understand the existing wage gap (Comi and Grasseni, 2012).

Another controversial topic associated to temporary contracts is their capability to be stepping stones into good permanent jobs. The core idea is that such contracts allow for a reduction of information asymmetries between employers and employees since the latter can signal their skills. Temporary jobs may be also used to improve human capital and social contacts, and to acquire information about vacancies. Nonetheless, there is no clear consensus in the literature on their impact though large part of the empirical literature finds positive "stepping stone" effects. This is the case of Italy (Gagliarducci, 2005; Ichino et al., 2008; Picchio, 2008; Berton et al., 2011), the UK (Booth et al., 2002), the US (Addison and Surfield, 2009), Sweden (Hartman et al., 2010) and Belgium (Cockx and Picchio, 2012). Another part of the literature find negligible effect such as in France (Magnac, 2000), Spain (Güell and Petrongolo, 2007) and in the Netherlands (de Graaf-Zijl et al., 2011), whereas a few studies find negative "dead-end" effects such as in the US (Autor and Houseman, 2010), Spain (Amuedo-Dorantes, 2000), and Japan (Esteban-Pretel et al., 2011). Finally, given that the flexibility was initially suggested to reduce unemployment and stimulate the economic growth, the potential increase of vacancies triggered by temporary contracts is also source of interest. Having the opportunity to quickly adequate the number of employees in case of demand variations should indeed incentivize employers to hire more (Boeri and Garibaldi, 2007).

3 Data and Sample Selection

Most of above-mentioned studies investigating on the wage gap between temporary and permanent workers rely on survey data, such as data from the European Union Statistics on Income and Living Conditions (e.g. Comi and Grasseni, 2012), the British Household Panel Survey, and the Household (e.g. Booth et al., 2002; and Cappellari and Jenkins, 2014), Income and Labour Dynamics in Australia (e.g. Lass and Wooden, 2017). Nonetheless, survey data highlight several issues for this type of analysis. First, survey data are generally characterized by a limited number of sample observations. To avoid huge costs of time and money, only a small part of households living in a country are indeed interviewed.[1] But, a small size of survey sample increases the discrepancy around the true value of the phenomenon under analysis (Deaton, 1997). It is due to the fact that the survey participation probability varies across the population according to some demographic and economic characteristics. Furthermore, non-response bias might also affect the estimates especially for the "hard-to-survey" populations (Tourangeau et al., 2014) such as the youth, the less educated, the low income people, residentially mobile, and to live in single adult households (Michaud et al., 2011; Frankel and Hillygus, 2014; Jenkins and Van Kerm, 2017). Finally, survey data may suffer biases related to the misreporting and recalling of respondents. As for the misreporting, it may be associated with perceived social stigma or systematic underreporting behaviors, and it is more common among high-educated individuals, self-employed, and wealthier households (Cannari and D'Alessio, 1993; Hurst et al., 2013; Greene et al., 2017). As for the recall bias, it depends on the fact that, as well known, people forget past events and details, so reported values tend to be less and less accurate the longer the recall period (Scott and Amenuvegbe, 1990; Stull et al., 2009).

To avoid all these issues, following Cappellari and Leonardi (2016), we use administrative data from social security registers of the Italian Social Security Institute (*LoSai INPS*). The *LoSai*'s overall sample available for research purposes has a longitudinal structure up to 2015 and covers 6.5% of all salaried or semi-subordinate employees working in the private sector. The data contains individual employment histories since 1985, unemployment benefit receipts from 1999, and other information on assimilated working weeks (e.g. sickness, maternity leave, military service, short-time compensation). It also provides firm characteristics such as dimension and sector and worker characteristics such as gender and year of birth. We use all this information to reconstruct the labour market history of the individuals.

Our sample is composed of 3,453,413 new jobs started between 2005 until 2015. The outcome of the analysis is the daily salary at the moment of hiring. As such, we are able to estimate the wage premium during different economic conjunctures and institutional periods such as the reforms in 2012 (Fornero Law) and 2015 (Jobs act). Following many empirical researches in the literature (Baker and Solon, 2003; Blundell et al., 2015; Hospido, 2015; Cappellari and Leonardi, 2016), we set an age restriction to the sample. Specifically, we focus on individuals between the ages of 15 and 65. [2]. To obtain a more robust estimation on the average differences, we also exclude

[1] For instance, the BHPS contains about 5,000 households (`http://www.iser.essex.ac.uk/bhps/faqs/sample`) compared to a total population of 27 million households in UK. Similarly, the HILDA survey involves more than 17,000 respondents (`https://melbourneinstitute.unimelb.edu.au/hilda`) over a total population of 24 million inhabitants in Australia, while the Italian component of the EU-SILC sample consists of about 29,000 households (`https://www.istat.it/it/archivio/5663`) out of 25 million households living in Italy.

[2] At the opposite, in contrast with the common methodological choice to drop female workers to minimize selection issues and potential endogeneity, we decide not to restrict the sample to males only. In fact, it is likely that focusing

extreme values from our sample trimming the data at the 0.1th and 99.9th percentiles of the daily wage distribution (4.1 and 621.5 euros respectively). Finally, we drop all observations with missing values in the variables of interest (i.e. type of employment contract, wage) or covariates, for a final sample consisting of 3,346,560 observations (new jobs) or 1,214,642 individuals. As for the daily wage definition, we refer to the individual daily wage calculated as the ratio between gross total remuneration and working days in the first part of the spell.[3] Since the analysis regards a long period of time (2005-2015), daily wages were inflation-adjusted (base 2016=100) using the national consumer price index provided by the Italian Institute of Statistics (Istat).

4 Descriptive evidence

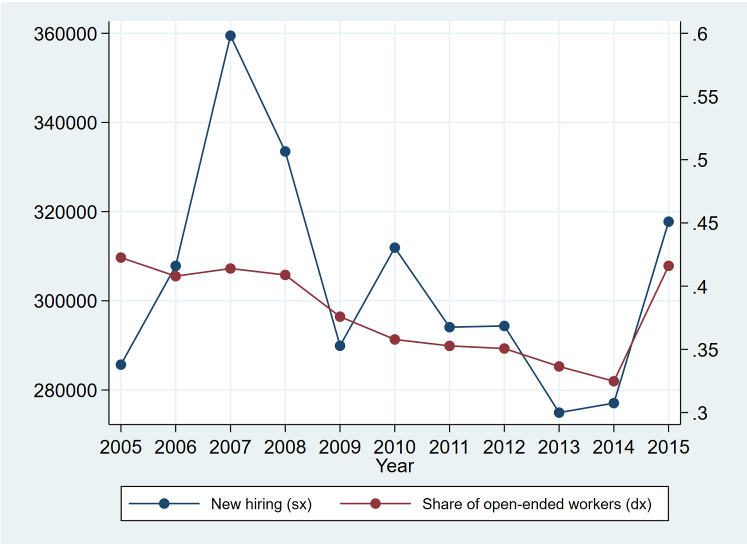
Preliminary evidences from our final sample of administrative data show that the trend of new hiring (i.e. annual inflow of observations in the sample) in the Italian labour market is not straightforward but fluctuating over the reference period (Figure 1). The number of new hiring rises from 280 to 360 thousand per year in the 2005-2007 period, which represents 4.2 and 5.4 million people in the full Italian population. The new hiring hugely decreases until 2014, and it increases again in the last year of analysis (i.e. 2015). The first increase and the subsequent collapse in the number of new hiring are probably due to the macroeconomic cycle observed at national level in the same period, especially regarding the negative effects produced by the economic crisis in 2009 and 2013.[4] As for the rise shown from 2014 to 2015, instead, it might be related to the last reform of the Italian labor market (i.e. the Jobs Act) and, in particular, to its fiscal benefits to the employers.

on new-entry workers and controlling for their occupational history over the last 16 years already reduce the risk of these issues. However, we show results by males and females separately in Section 6

[3]Beyond the length of the employment contract, a further difference between temporary and open-ended workers may be on average the number of working hours per day. An alternative approach consists of using full-time daily wages by adjusting it for the hours worked, but we use it in a sensitivity analysis only because of possible measurement errors (Section 6.4).

[4]This is confirmed by the fact that the coefficient of correlation between the annual number of new hiring and the annual GDP at market prices (chain linked volumes, index 2010=100) provided by Eurostat (`http://ec.europa.eu/eurostat/data/database`) is above 0.8 for the 2005-2014 period.

Figure 1: New hiring and share of temporary workers

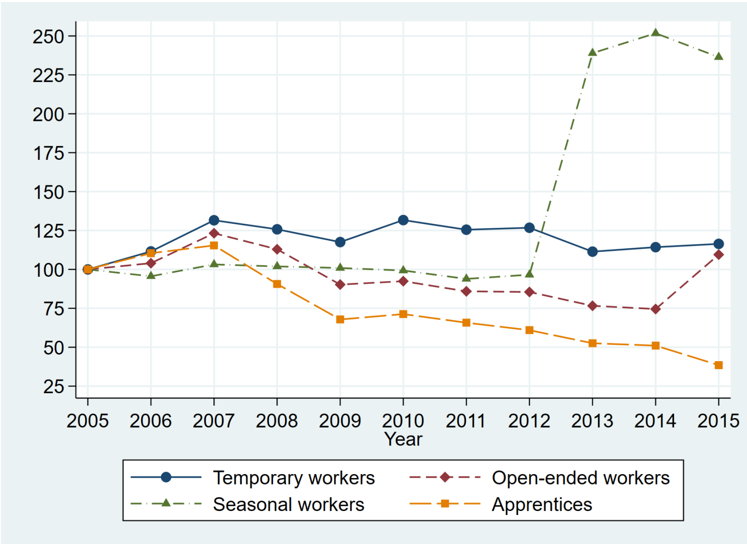


Notes: New hiring represent the annual inflow of observations in the sample.

Figure 1 also highlights a noteworthy decrease in the usage of permanent employment contracts by Italian firms over time. In fact, the share of permanent workers among the new hiring was more than 40% in 2005 and it is about 33% in 2014. However, the decline stops in our sample and it is even reversed in 2015. This turns out to be the likely effect of a reform of the Italian labor market, i.e. the Jobs Act, having as main objective the discouragement of temporary job contracts in favor of open-ended ones. Through the relative variations in new hiring by job contract reported in Figure 2, it is possible to better understand why the share of permanent contracts decreased so much among new-entries in the Italian labor market. The reasons of that are mainly two: on the one hand, the number of temporary workers increased more than the others in the 2005-2008 period and, on the other hand, open-ended workers strongly diminished from 2009 onwards except for the upturn observed in 2015.

Beyond the two main categories of job contracts we analyzed so far, there are two additional types of workers who jointly represent a small part of our sample (about 10% of new hiring): the seasonal workers and apprentices. In terms of employment contract length, seasonal workers should belong to the temporary ones, while apprentices are considered as permanent workers by Italian labor law. However, these job contracts have very peculiar features, such as the economic activity sector where they can be adopted, the employee income regulation, and the contract length. For this reason, we exclude them from the main analysis reported in Section 6, except returning to these job contracts at a later time. Figure 2 shows that the number of apprentices among new hiring slightly increased until 2007, but then it reports a large drop in the subsequent eight years. At the opposite, after an overall stationary trend from 2005 to 2012, the number of seasonal workers denotes a huge and sudden growth in 2013 probably due to a fiscal benefit introduced by the Fornero reform.[5]

Figure 2: Relative variation in new hiring by job contract

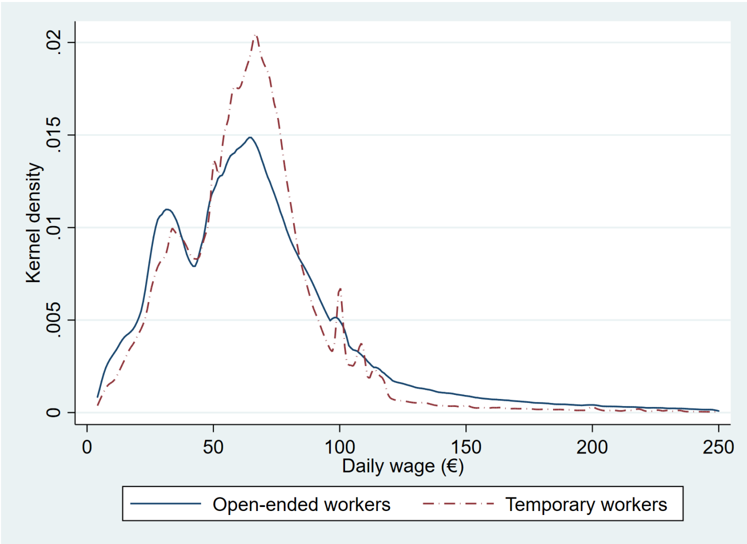


The new important role adopted by temporary contracts in the labor market in recent years denotes an increasing source of concern for the potential wage gap these job contracts may determine in the country. As reported by part of the economic literature mentioned in Section 2, temporary workers might tend to decrease the average wage at national level because of a possible wage penalty. Comparing the distribution of daily wages among temporary workers to the one shown by the open-ended workers in the reference period (2005-2015), the former appears more concentrated around the central peak, whereas the latter is featured by more extended wings and thus a greater variance (Figure 3).[6] Therefore, open-ended workers in our sample are more likely to report both the lowest and highest levels of daily wage with respect to the temporary ones.

[5]To encourage Italian employers to hire more through seasonal contracts, the Fornero reform excluded this type of workers from an additional contribution for the employment social insurance.

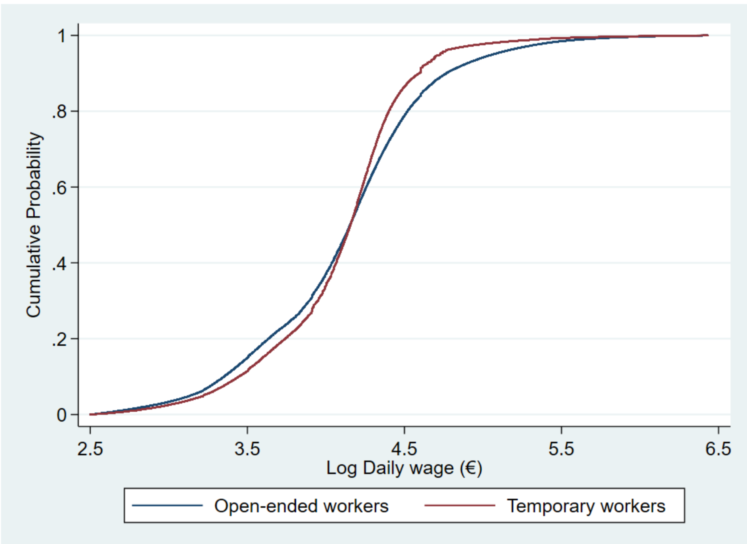
[6]Figure 3 illustrates a noticeable hump in the left-part of both distributions. It is related to collective bargaining agreements which affect Italian employees working in specific economic sectors (e.g. services, trade, transport, tourism) ensuring them a sort of minimum wage.

Figure 3: Kernel density estimates for daily wage by job contract



Cumulative distribution functions provided in Figure 4 give a further and clearer evidence on the overall wage differences between the two job contracts under analysis. Specifically, considering the whole 2005-2015 period, Figure 4 shows that temporary workers are likely to have a greater daily wage than the others until the logarithm of daily wage is equal to 4.2 (i.e. a daily wage equals to about 67 euros, while open-ended workers have a higher probability to report greater wages from 67 euros onwards.

Figure 4: Sample cumulative distribution function of daily wage logarithm by job contract



In other words, descriptive evidences from our sample seem to point out that temporary contracts ensure a better remuneration in the bottom part of the wage distribution, and the opposite in the top part. Interestingly, at the median of the cumulative distributions there is no evidence of a wage gap since the daily wage is equal to about 63 euros for both open-ended and temporary contracts. At the opposite, the mean daily wage is about 10% higher for open-ended contracts (68 versus 64 euros) due to the skewed distribution to the right.

This descriptive analysis highlights the importance of looking at the gap on the full distribution and not just on the average salary. Also, the previous analysis does not allow to control for demographic characteristics of workers, as well as for main characteristics of their contracts and jobs, which have instead a significant role in explaining the potential wage gap. The analyses illustrated in the next Sections try to take into account these relevant aspects.

5 Identification Strategy

In this section we aim at defining the empirical strategy to estimate the presence of a premium or penalty on daily wage for temporary contracts, once controlled for relevant characteristics of both the workers and the firms. At first, we consider as treated any temporary contract but seasonal ones ($D = 1$) and as comparison group the open-ended contracts except for apprenticeship ($D = 0$). We exclude seasonal workers from the treated due to the fact that, according to the Italian legislation, they can be employed only in specific economic sectors and this may represent a source of bias for our estimates. Similarly, we exclude the apprentices from the open-ended workers, because they are traditionally paid less as a result of a non-monetary transfer: the training. Afterwards, we will distinguish between different types of temporary contracts present in the Italian legislation, such as workers on call and agency ones.

In this analysis, we estimate differences on the logarithm of daily remuneration at hiring (i.e. the outcome variable Y) between treated and control group workers both at the mean and along the distribution. In fact, we also aim at understanding whether the potential gap is heterogeneous along the wage distribution. To estimate the wage difference at the mean, we regress an OLS model, whereas to assess the distributional differences of temporary contracts on the outcome variable, we estimate the Quantile Treatment Effect (QTE) as originally proposed by Lehmann (1974) and Doksum (1974). As shown in Koenker and Bassett (1978), a quantile q_τ of an outcome distribution Y can be estimated by finding the quantile q minimizing a sum of check functions ρ_τ.

$$q_\tau = argmin_q \sum_{i=1}^{N} \rho_\tau 1(Y_i - q) \quad (1)$$

Where N is the total sample size of the i individuals and the check function $\rho_\tau = (Y_i - q) * (\tau -$

$1(Y_j - q \leq 0))$.

To obtain the QTE we need to estimate the potential quantiles q_τ in presence or absence of treatment. If a treatment would be randomly assigned, the (unconditional) QTE for a τ quantile can be estimated as the differences between quantiles of the marginal distributions of the potential outcomes under treatment ($q_{1,\tau}$) and no treatment ($q_{0,\tau}$). If the selection into a temporary contract is not randomly related to the outcome Y, as it is probably our case, we can aim at attenuating differences in potential outcomes between the two groups of workers by conditioning on a set of observable characteristics. In this paper we follow this route, which is known as selection on observables, and implement an inverse probability weighting estimator (IPW) as proposed by Firpo (2007).

Similarly to estimators on the average, our estimator relies on the unconfoundedness (or conditional independence - CIA) and common support assumption.[7] While the latter assumes that treatment status is not certain (eq. CS) , unconfoundedness states that once controlled for the covariates the potential outcomes of the individuals (Y_0, Y_1) are independent of the actual treatment assignment D (eq. CIA). The two assumptions are formalized as follows.

$$(Y_0, Y_1) \perp\!\!\!\perp D|X \tag{CIA}$$

$$0 < P_i(D = 1|X) < 1 \tag{CS}$$

In our analysis we focus on the quantile treatment effect on the treated (QTT), which rely on the weaker conditional independence and common support assumptions[8] at the cost of estimating the effect only on the treated population. To estimate the QTT we need to retrieve the unobserved quantile of the treated in the absence of treatment ($\hat{q}_{0,\tau|D=1}$). As demonstrated in Firpo (2007), under the weak unconfoundedness and common support, this can be estimated by reweighting the check functions ρ_τ of the quantile of the control group.

$$QTT_\tau = q_{1,\tau|D=1} - \hat{q}_{0,\tau|D=1} = q_{1,\tau|D=1} - argmin_q \sum_{j=1}^{N_c} \hat{\omega}_j * \rho_\tau 1(Y_j - q) \tag{2}$$

Where N_c is the total size of the control individuals j , $\hat{\omega}_j$ are the estimated weights of the units in the control group and ρ_τ is the check function for the τ quantile of the control group $q_{\tau|D=0}$. The weights are estimated as in the standard IPW estimator for the ATT, $\hat{\omega}_j = \frac{\hat{p}(X_j)(1-D)}{1-\hat{p}(X_j)N_c}$. In our application we estimate the propensity score $\hat{p}(X_j)$ by using a logistic model. Standard errors are

[7] An additional assumption of the QTE is the uniqueness of quantile, which states that the potential outcomes have to be continuous and not flat at the τ percentile.

[8] Only the potential outcome in the absence of treatment has to be independent on the actual treatment status $(Y_0, \perp\!\!\!\perp D|X)$ and assignment in the actual treatment should not be certain i.e. $P_i(D = 1|X) < 1$

obtained by boostrapping the estimates.

Clearly the set of conditioning variables is crucial to assess the credibility of the weaker unconfoundedness assumption. We argue that the rich information contained in *LoSai* allows us to considerably reduce the role of unobserved heterogeneity between the two groups of workers. However, as unobserved factors might still be present we refrain from interpreting estimates as a *causal* effect of the temporary contract on the wage of individuals, but rather as the gap observed between the two contracts after flexibly controlling for the labour market experience of individuals over the last 16 years.[9]

In order to exploit the full information set of the dataset and capture different trends between the two groups of workers, the list of covariates we control for is divided between: i) old history (between 16 and 11 years before the treatment); ii) less recent history (10-6 years before the treatment); iii) more recent history (5-2 years before); and iv) the last year. We select a long list of variables which may affect the current salaries Y and the probability of selection into a temporary contract D either directly or indirectly. We therefore include average daily remuneration (lagged outcomes), percentage of working time by contract (lagged treatments), qualification (blue collar, white collar or apprentice), firm size (0-15, 16-200, 201+) and macro-sectors. In addition, we include total weeks worked (with a specific dummy if zero), total remuneration as collaborator, number of years receiving unemployment benefits (with also total cumulated days), total hours in temporary layoffs (the so-called '*Cassa Integrazione Guadagni*' - CIG) and ever worked as part-time. For the last year the control variables are more detailed on the main job and also includes the percentage of part-time, more detailed firm information (9 dummies for dimension, 7 dummies for sector and 3 for firm position in the group), number of different employers in the year (1, 2, 3, 4+) and job-to-job transition (proxied by a dummy equal to one if the worker had another job 60 days before starting the current job). Additional individual information is included such as age, gender, year of hiring and region of residence.[10] Finally, as information on the current job is simultaneous with the treatment and therefore endogenous, we include it only in a sensitivity analysis (Section 6.4). The detailed list of covariates can be found in Table **??** in the Appendix.

We stress that among the conditioning variables we also include detailed information on the past level of wages during the last 16 years. These lagged outcomes allow us to control for unobserved heterogeneity which is invariant over time as in a fixed-effect panel data estimator. Indeed, if the

[9] Even if the CIA holds, the QTE cannot be interpreted as an individual level treatment effect without the additional 'rank preservation' assumption. This assumption states that an individual should maintain her position in the distribution with or without treatment. If this assumption holds, then the QTE can be interpreted as the quantiles of the treatment effect. Since for our treatment there is no compelling reason to believe in the rank preservation, we just interpret the QTE as the effect on the outcome distribution of temporary contracts.

[10] Note that the region of residence is measured in 2015.

two groups differ for some unobserved variables not included in our list of covariates, such as level of education, the effect of these variables on wages is likely to have already manifested in the previous wage of the individuals (Imbens and Wooldridge, 2009). Furthermore, since we observe the past remuneration in multiple lags, we can also control for differential trends in the outcomes between the two groups. Finally, as we aim at estimating the gap on the full outcome distribution, we include higher terms of these lagged dependent variables up to the third order.

As proposed in the literature we check the common support and trim the treated units with too few control units in the same area. To restrict the role of outliers and remove the thinnest part of the propensity score distribution, we remove the treated with a propensity score above the 99.9 percentile of the control units (Lechner and Strittmatter, 2017). Second, as failure in the specification of the propensity score model might results in unbalanced covariates and biased estimates, balancing tests are performed. Finally, the past level of remuneration is also used to implement a placebo test as proposed in Imbens and Wooldridge (2009). In particular, we test the presence of a (placebo) effect on the daily wage in the year before hiring. For this placebo test, we retain only the sample employed in that year and the set of conditioning variables is changed as to be predetermined to the year considered.[11]

6 Results

The estimation of both ATT and QTT on the logarithm of daily remuneration at hiring shows results which are opposite with respect to other empirical researches on the wage gap between temporary and open-ended contracts (Booth et al., 2002; Picchio, 2006; Cappellari and Jenkins, 2014; Cappellari and Leonardi, 2016), but in line the economic theory (Table 1 and Figure 5). In fact, as suggested by Rosen (1986) and his theory of equalizing differences, results report a wage premium in favor of temporary workers at the mean and along the wage distribution. Specifically, having a temporary contract in Italy determines on average a 11.3% higher daily wage at hiring, as a likely compensation for the shorter job tenure and the worse job security (column (1) of Table 1). Looking at the remuneration gap along the distribution, in sharply opposition with Comi and Grasseni (2012), the wage premium appears to be greater among low-paid workers and almost null in the top part of the distribution. The estimated QTT for the whole sample (Figure 5a) is +0.24 at the first ventile, +0.10 at the median, and +0.01 at the nineteenth ventile.

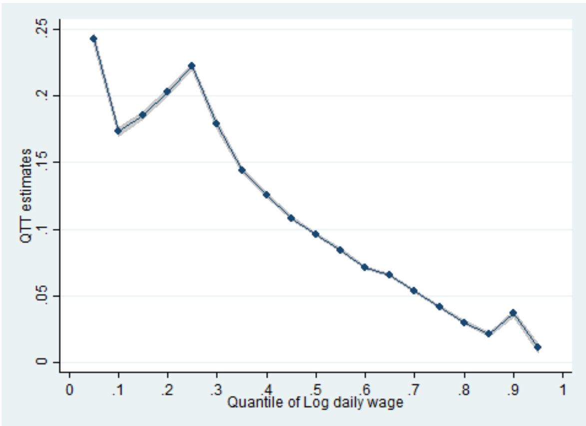
[11] The variables measured in the last year before treatment are moved to the last year before the placebo treatment (i.e. two years before treatment). Similarly, the variables in the most recent history stop 3 years before treatment.

Table 1: Estimation of the wage gap at the mean (ATT)

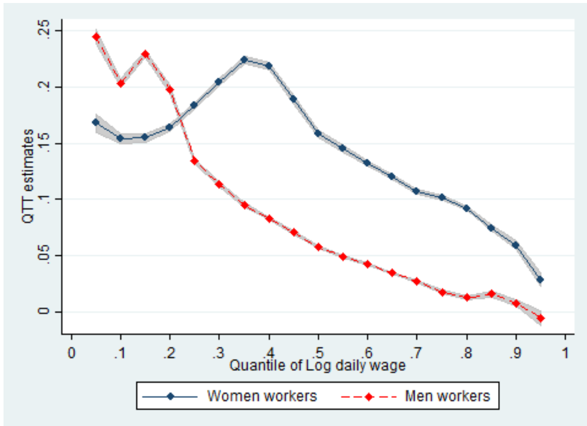
var	(1) All	(2) Women	(3) Men	(4) Young	(5) Adult	(6) North	(7) Center	(8) South
ATT	11.27	13.93	9.76	13.11	9.65	11.56	12.89	11.31
Std.Err.	(0.10)	(0.18)	(0.14)	(0.12)	(0.17)	(0.15)	(0.28)	(0.21)
Observations	2,981,690	1,143,246	1,839,050	1,394,154	1,585,096	1,496,755	673,203	806,832
N individuals	1,151,372	467,598	683,686	599,127	657,168	574,162	265,956	311,242

Notes: Standard errors clustered by individual id in parentheses. Estimates of ATT are based on the standard IPW method.

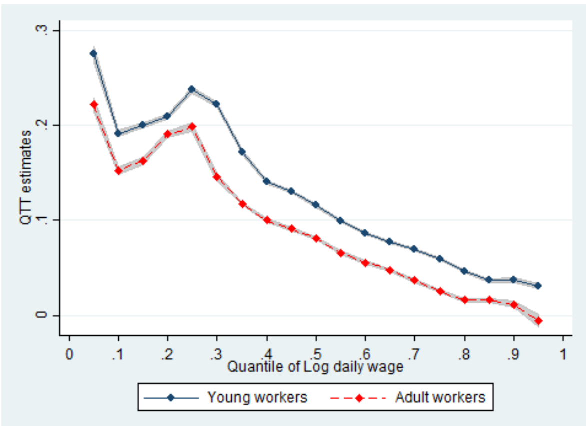
Figure 5: Estimation of the wage gap at the quantiles (QTT)



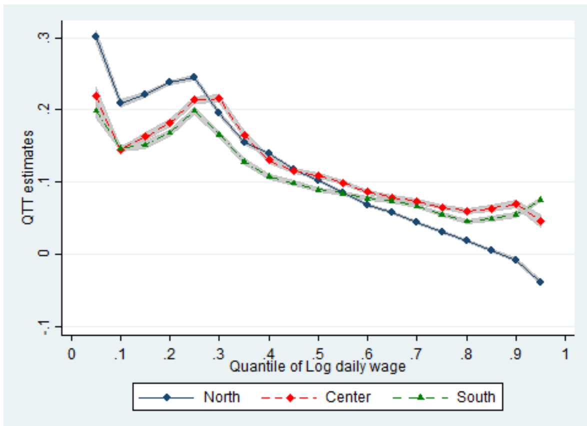
(a) Estimates for total sample



(b) Estimates by gender



(c) Estimates by age



(d) Estimates by macro-region

Notes: Estimates of QTT are based on the standard IPW method. Shadowed areas show 95% confidence intervals based on bootstrapped standard errors with 199 repetitions.

Beside the wage gap for the whole sample of workers, interesting insights emerge when observing heterogeneous "effects" of temporary contracts on the outcome by some important individual characteristics. In particular, columns (2)-(8) of Table 1 illustrate the wage gap heterogeneity by gender, age group (young if less than 35 years old, and adult otherwise), and macro-region of residence across Italy (North, Center, and South).[12] The wage premium related to the temporary contract is significantly greater among female workers with respect to male ones, and among the young compared to the adult workers. Therefore, the wage premium for a temporary employment seems more in favor of those categories of the working age population having more difficulties to access to the Italian labor market (Browne and Pacifico, 2016). At the opposite, differences in the wage premium are quite small between the three macro-regions. In fact, having a temporary contract determines a 11.3% higher daily wage at hiring than an open-ended one in Southern regions, whereas wage premiums for temporary workers are +11.6% and +12.9% in the North and Center of Italy respectively.

As for estimates of QTT by the analysed individual characteristics, also in these cases we highlight a greater premium in the bottom part of wage distribution and a lower or even insignificant premium at the top part (Figure 5). Nonetheless, it is possible to observe different trends along the wage distribution when comparing estimated QTTs by subsample of workers. For instance, the wage premium for temporary workers is greater among male workers until the fourth ventile, while it is greater among female ones from the fifth ventile onwards. Similarly, the premium is greater among workers living in the Northern regions until the fifth ventile, while the highest wage premiums are reported among those living in the Center of Italy from the median onwards but the 0.95 quantile. Finally, the premium related to the temporary contract is greater for the young workers with respect to the adult ones along the entire wage distribution.

6.1 The role of reforms and the economic crisis

The reference period of our analysis is far to be empty of exogenous shocks, given that the Italian labour market was reformed several times from 2005 to 2015. Two different interventions particularly deserve to be mentioned: i) the Fornero reform (2012); ii) the Jobs Act (2014-2015). These interventions are likely to have an impact on the estimated wage premium because they changed the regulation of temporary contracts in order to disincentivize them. In particular, we expect that disincentives to temporary contracts (i.e. making open-ended contracts less relatively expensive) lead employers to grant a lower premium. It should be noted that, at least in the first part of our reference period, long lasting effects related to the Biagi Law (2003) - which has instead incentivized temporary contracts - may also have a role in the wage premium. Moreover, the Italian economy suffered negative effects of the Great recession during the reference period,

[12] The sum of subsamples observations does not coincide with the all sample observations because of trimming procedures on the treated group.

especially in 2009 and 2013 according to Eurostat statistics.[13] If the Rosen (1986)'s theory of equalizing differences holds, considering the greater difficulty to find a job during a recession, then we expect that the economic crisis overall increases the wage premium as a higher compensation for temporary workers. For the same reason, it is likely that the wage premium decreases during a period of economic growth.

Table 2 shows that the wage premium in favor of temporary contracts was not stable over the 2005-2015 period. Specifically, it reports three different peaks (in 2005-2006, 2009, and 2013), and finally it collapses in 2015. High levels of the wage premium in 2005 and 2006 are likely due to incentives for new types of temporary contract by the Biagi Law, while the subsequent decrease may be associated to the fact that the law firstly incentives low paid temporary contracts such as jobs on call and apprenticeships (see Section 6.2). As for the peaks observed in 2009 and 2013, they seem to coincide with the two drops in the national GDP growth which determined a consequent slowdown in the number of new hiring (see Figure 1). Therefore, this evidence may somehow confirm the Rosen (1986)'s theory of a wage premium as a compensation for temporary contracts because that rises when the probability to find a job is higher. Finally, the wage premium collapse in 2015 appears clearly connected with the Jobs Act and in particular with fiscal benefits for open-ended contracts introduced by the same, which made temporary contracts relatively more expansive and significantly increased the new hiring in Italy.

Table 2: The wage gap at the mean over the period 2005-2015

Year	ATT	Std.Err.	Observations	N individuals
2005	13.06	(0.34)	245,528	199,979
2006	13.13	(0.28)	267,264	215,623
2007	11.22	(0.27)	318,147	255,048
2008	11.61	(0.30)	299,473	243,507
2009	13.01	(0.34)	261,415	215,928
2010	11.42	(0.31)	281,946	233,011
2011	10.47	(0.33)	264,914	222,060
2012	11.13	(0.37)	265,144	220,115
2013	12.36	(0.37)	238,941	196,460
2014	11.38	(0.37)	240,204	197,062
2015	4.26	(0.23)	285,371	233,450
All	11.27	(0.10)	2,981,690	1,151,372

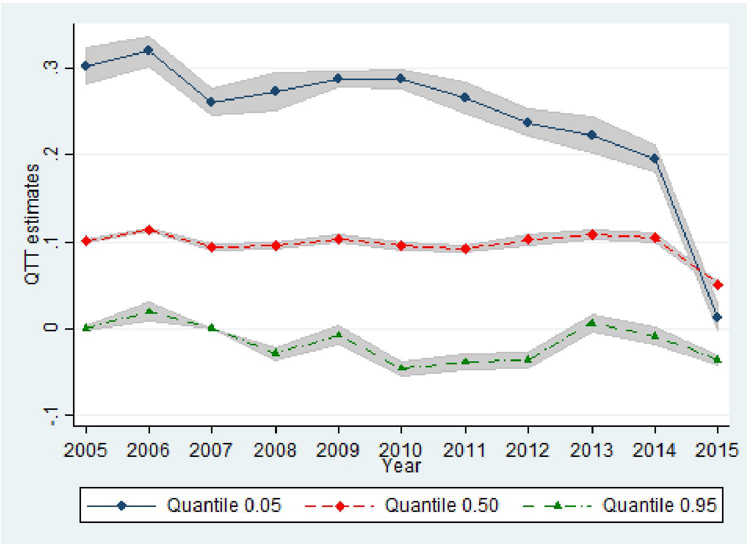
Notes: Standard errors clustered by individual id in parentheses. Estimates of ATT are based on the standard IPW method.

The trend of the wage gap related to temporary contracts turns out to be quite heterogeneous among workers belonging to different daily wage quantiles. Figure 6 shows how QTT estimates change over time for three quantiles only: the first ventile, the median, and the nineteenth ventile.

[13]Link: https://ec.europa.eu/eurostat/data/database.

Results highlight that changes over time primarily regard low-paid workers, given that the estimated wage premium at the quantile 0.05 performs a decreasing trend during the 2005-2014 period (from 0.3 to 0.2) and it becomes statistically insignificant at 5 percent in 2015. At the opposite, QTT estimates are overall stable around 0.10 for workers at the median (except for the downturn in 2015) and approximately equal to 0 for those belonging to the highest ventile. Interestingly, high-paid temporary workers are the only suffering a wage penalty with respect to the open-ended ones, especially during the recession period.

Figure 6: The wage gap across distribution over the period 2005-2015



Notes: Estimates of QTT are based on the standard IPW method. Shadowed areas show 95% confidence intervals based on bootstrapped standard errors with 199 repetitions.

6.2 Wage gap by type of contract

Up to this point we have considered temporary contracts as a whole, however different types of temporary contracts are present in the Italian legislation, and thus potentially different treatments on the daily wage at hiring. In order to estimate ATT and QTT for each type of temporary contract, we define now four new treatment variables D_i:

- $D_1 = 1$ for agency workers, and $D_1 = 0$ for open-ended workers;
- $D_2 = 1$ for on call workers, and $D_2 = 0$ for open-ended workers;
- $D_3 = 1$ for seasonal workers, and $D_3 = 0$ for open-ended workers;
- $D_4 = 1$ for apprentices, and $D_4 = 0$ for open-ended workers.[14]

[14]The apprenticeship is not formally considered a temporary employment contract in Italy, because it automatically

Table 3 shows that not all temporary contracts determine a wage premium. On call workers report significant wage penalty (-5.7%) at the daily wage mean with respect to open-ended workers with similar demographic characteristics and occupational history. The category of agency workers shows instead a large wage premium (+19% on average), as well as the one of seasonal workers. Finally, apprentices do not highlight any significant wage gap at the mean compared to open-ended workers.

Table 3: The wage gap at the mean by temporary contract

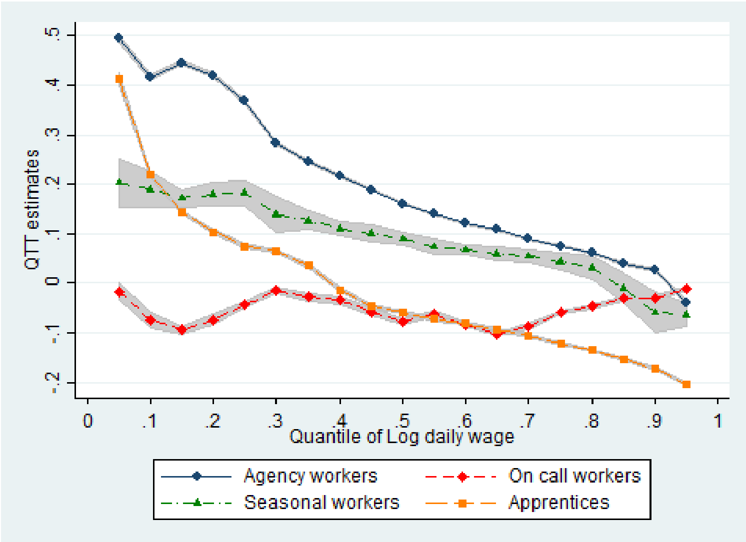
	Agency workers	On call workers	Seasonal workers	Apprentices
ATT	19.62	-5.73	9.27	-.05
Std.Err.	(0.15)	(0.37)	(1.29)	(0.21)
Observations	1,541,243	1,229,236	1,391,898	536,089
N individuals	827,353	736,550	791,678	338,329

Notes: Standard errors clustered by individual id in parentheses. Estimates of ATT are based on the standard IPW method.

Estimates for the wage gap across the daily wage distribution by temporary contract reported in Figure 7 overall confirm what seen in Figure 5 (i.e. a decreasing wage premium across the distribution) for agency and seasonal workers only. On call workers indeed suffer a wage penalty fluctuating around -0.05 across the entire distribution, whereas apprentices show a significant wage premium until the seventh ventile and a negative wage gap with respect to open-ended workers after. The latter trend may explain why the wage gap between apprentices and open-ended workers is null at the mean of daily wage.

becomes an open-ended contract after three years unless the final assessment is particularly negative. Nevertheless, we include this category of workers in this analysis by contract because the defined length and some of its features (e.g. training) make it more similar to temporary contracts than permanent ones. Moreover, since apprenticeship contracts apply to individual that are less than 30 years old only according to the national regulation, this variable is missing for those being 30 or more.

Figure 7: The wage gap across distribution by temporary contract



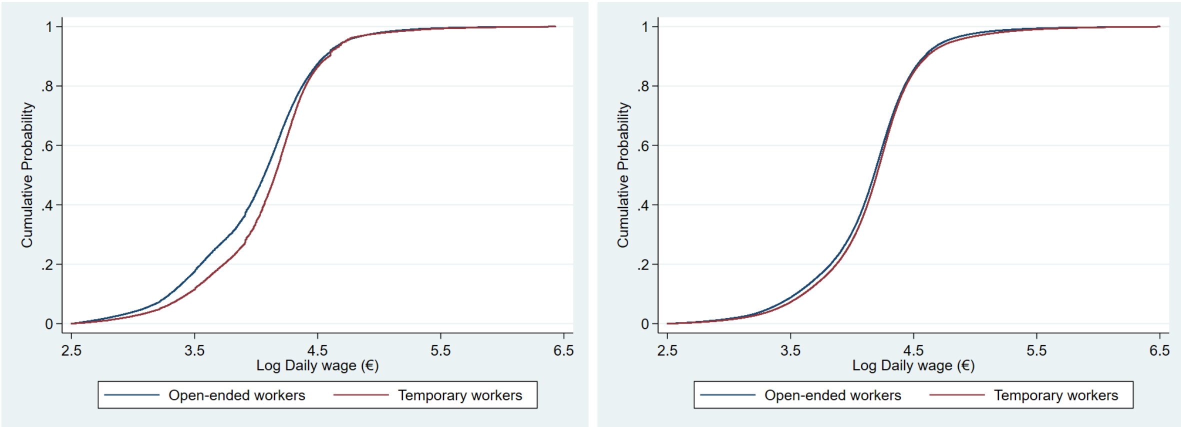
Notes: Estimates of QTT are based on the standard IPW method. Shadowed areas show 95% confidence intervals based on bootstrapped standard errors with 199 repetitions.

6.3 Placebo test

The estimation of the (unconditional) QTE for a quantile is unbiased only if the treatment is randomly assigned. Since our treatment (i.e. having a temporary contract) is likely not be randomly related to the outcome (i.e. logarithm of daily wage at hiring), we do a selection on observables and implement an IPW estimator to ensure a correct estimation of QTEs on the treated. A common method to verify whether the selection on observables really makes estimates unbiased is the placebo test proposed by Imbens and Wooldridge (2009). In particular, as explained in Section 5, we test the presence of a "placebo effect" on the daily wage in the year before hiring.

Figure 8 illustrates a comparison of cumulative distribution functions for open-ended and temporary contracts between those deriving from main results reported in column (1) of Table 1 and functions based on the placebo test. Figure 8b shows that the distribution of outcomes for the two groups of workers is much more similar with respect to the ones based on the IPW method (Figure 8a). Although some small bias seems to be left even after the matching, results of the placebo test overall confirm that observables used to implement the IPW estimator make the treatment quite randomly related to the outcome.

Figure 8: Placebo test results



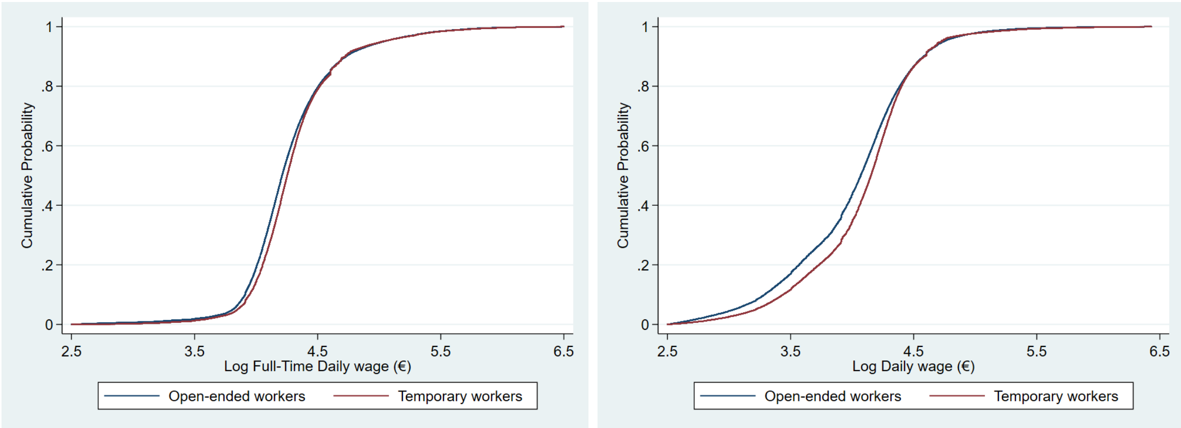
(a) Estimates based on IPW method (b) Estimates based on placebo test

6.4 Sensitivity analysis

In this Section we perform two sensitivity analysis in order to both exploring alternative methodological approaches and testing the robustness of our main results. The first one consists of using full-time adjustment of daily wages. The estimation analysis shown in Section 6 is simply based on daily wage, but temporary and open-ended workers may be different on average the number of working hours per day. This approach makes temporary and open-ended workers the same in terms of hours worked. Nonetheless, our data set does not contain direct information on hours worked, so they are indirectly calculated referring to the number of hours remunerated to the actual working weeks as defined by the social security. The second sensitivity analysis relies on the addition in the list of covariates of information on the current job. This information allows to make deeper comparison between temporary and open-ended workers in terms of wage, even though it is simultaneous with the treatment and therefore endogenous. More in details, this covers: qualification (blue, white, apprenticeship, senior staff, directors and others), firm position (group, single, mother), dimension (14 dummies), micro-sector (NACE 2002 at 2 digit level), part-time (mixed, vertical, horizontal), and having already worked in the same firm.

Figure 9 shows cumulative distribution functions resulting from both sensitivity analysis. As expected, estimates based on a full-time adjustment of daily wages (Figure 9a) report that open-ended and temporary contracts are much more similar than those deriving from main results (Figure 8a), and consequently wage premium in favor of temporary workers are lower. For instance, the wage gap is here equal to +5.0% at the mean and +4.5% at the meadian only. However, as the measurement error in this variable is probably large, and considering that the existence of a wage premium is still confirmed, we decide to show estimation results based on full-time adjusted daily wages as robustness check only.

Figure 9: Sensitivity analysis results



(a) Full-time adjustment of daily wages

(b) Estimates with endogenous variables

Similarly, with regard to estimates with endogenous variables (Figure 9b), they are also overall the same of those from main results, as well as the estimated wage premium for temporary contracts (11.8% and 9.4% at the mean and the median respectively). Nonetheless, given the potential risk of endogeneity related to information on the current job, we decide to show also these results as robustness check only.

[To be completed]

References

Addison, J. T. and C. J. Surfield (2009). Does atypical work help the jobless? evidence from a caeas/cps cohort analysis. *Applied Economics 41*(9), 1077–1087.

Amuedo-Dorantes, C. (2000). Work Transitions into and out of Involuntary Temporary Employment in a Segmented Market: Evidence from Spain. *Industrial and Labor Relations Review 53*(2), 309–325.

Arulampalam, W. and A. L. Booth (1998). Training and Labour Market Flexibility: Is There a Trade-off? *British Journal of Industrial Relations 36*(4), 521–536.

Autor, D. H. and S. N. Houseman (2010). Do Temporary-Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from "Work First". *American Economic Journal: Applied Economics 2*(3), 96–128.

Baker, M. and G. Solon (2003). Earnings Dynamics and Inequality among Canadian Men, 1976–1992: Evidence from Longitudinal Income Tax Records. *Journal of Labor Economics 21*(2), 289–321.

Berton, F., F. Devicienti, and L. Pacelli (2011, November). Are temporary jobs a port of entry into permanent employment?: Evidence from matched employer-employee. *International Journal of Manpower 32*(8), 879–899.

Blundell, R., M. Graber, and M. Mogstad (2015, July). Labor income dynamics and the insurance from taxes, transfers, and the family. *Journal of Public Economics 127*, 58–73.

Boeri, T. and P. Garibaldi (2007). Two tier reforms of employment protection: a honeymoon effect?*. *The Economic Journal 117*(521), F357–F385.

Booth, A. L., M. Francesconi, and J. Frank (2002). Temporary jobs: stepping stones or dead ends? *The Economic Journal 112*(480), 189–213.

Brandolini, A., P. Cipollone, and P. Sestito (2001). Earnings Dispersion, Low Pay and Household Poverty in Italy, 1977-1998. Temi di discussione (Economic working papers) 427, Bank of Italy.

Browne, J. and D. Pacifico (2016, October). Faces of joblessness in italy. anatomy of employment barriers.

Cannari, L. and G. D'Alessio (1993). Non-Reporting and Under-Reporting Behaviour in the Bank of Italy's Survey of Household Income and Wealth. *Bulletin of the International Statistics Institute 55*(3), 395–412.

Cappellari, L. and S. P. Jenkins (2008). The Dynamics of Social Assistance Receipt: Measurement and Modelling Issues, with an Application to Britain. SSRN Scholarly Paper ID 1426053, Social Science Research Network, Rochester, NY.

Cappellari, L. and S. P. Jenkins (2014). Earnings and labour market volatility in Britain, with a transatlantic comparison. *Labour Economics 30*, 201–211.

Cappellari, L. and M. Leonardi (2016). Earnings Instability and Tenure. *The Scandinavian Journal of Economics 118*(2), 202–234.

Cockx, B. and M. Picchio (2012). Are short-lived jobs stepping stones to long-lasting jobs?*. *Oxford Bulletin of Economics and Statistics 74*(5), 646–675.

Comi, S. and M. Grasseni (2012). Are temporary workers discriminated against? evidence from europe. *The Manchester School 80*(1), 28–50.

de Graaf-Zijl, M., G. van den Berg, and A. Heyma (2011). Stepping stones for the unemployed: the effect of temporary jobs on the duration until (regular) work. *Journal of Population Economics 24*(1), 107–139.

Deaton, A. (1997, July). The analysis of household surveys : a microeconometric approach to development policy. Technical Report 17140, The World Bank, Washington D.C.

Doksum, K. (1974). Empirical probability plots and statistical inference for nonlinear models in the two-sample case. *The Annals of Statistics 2*(2), 267–277.

Draca, M. and C. Green (2004). The Incidence and Intensity of Employer Funded Training: Australian Evidence on the Impact of Flexible Work. *Scottish Journal of Political Economy 51*(5), 609–625.

Esteban-Pretel, J., R. Nakajima, and R. Tanaka (2011, August). Are contingent jobs dead ends or stepping stones to regular jobs? Evidence from a structural estimation. *Labour Economics 18*(4), 513–526.

Eurostat (2018). Part-time employment and temporary contracts - annual data.

Firpo, S. (2007). Efficient semiparametric estimation of quantile treatment effects. *Econometrica 75*(1), 259–276.

Frankel, L. L. and D. S. Hillygus (2014). Looking Beyond Demographics: Panel Attrition in the ANES and GSS. *Political Analysis 22*(3), 336–353.

Gagliarducci, S. (2005, August). The dynamics of repeated temporary jobs. *Labour Economics 12*(4), 429–448.

Greene, W., M. N. Harris, P. Srivastava, and X. Zhao (2017). Misreporting and econometric modelling of zeros in survey data on social bads: An application to cannabis consumption. *Health Economics 27*(2), 372–389.

Güell, M. and B. Petrongolo (2007). How binding are legal limits? transitions from temporary to permanent work in spain. *Labour Economics 14*(2), 153–183.

Hartman, L., L. Liljeberg, and O. N. Skans (2010, Jun). Stepping-stones, dead-ends, or both? an analysis of swedish replacement contracts. *Empirical Economics 38*(3), 645–668.

Hospido, L. (2015, April). Wage dynamics in the presence of unobserved individual and job heterogeneity. *Labour Economics 33*, 81–93.

Hurst, E., G. Li, and B. Pugsley (2013, April). Are Household Surveys Like Tax Forms? Evidence from Income Underreporting of the Self-Employed. *The Review of Economics and Statistics 96*(1), 19–33.

Ichino, A., F. Mealli, and T. Nannicini (2008). From temporary help jobs to permanent employment: what can we learn from matching estimators and their sensitivity? *Journal of Applied Econometrics 23*(3), 305–327.

ILO (2016). Non-standard employment around the world: Understanding challenges, shaping prospects. Report.

Imbens, G. W. and J. M. Wooldridge (2009, March). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature 47*(1), 5–86.

Jenkins, S. P. and P. Van Kerm (2017). How does attrition affect estimates of persistent poverty rates? The case of EU-SILC. In *Monitoring social inclusion in Europe - 2017 edition*, pp. 401–418. Luxembourg: Publications Office of the European Union.

Koenker, R. and G. Bassett (1978). Regression quantiles. *Econometrica 46*(1), 33–50.

Lass, I. and M. Wooden (2017). The Structure of the Wage Gap for Temporary Workers: Evidence from Australian Panel Data. Technical Report 8, Melbourne Institute of Applied Economic & Social Research.

Lechner, M. and A. Strittmatter (2017). Practical procedures to deal with common support problems in matching estimation. *Econometric Reviews 0*(0), 1–15.

Lehmann, E. (1974). Nonparametrics: Statistical methods based on ranks. *Journal of Applied Mathematics and Mechanics 57*(9), 562–562.

Magnac, T. (2000). Subsidised training and youth employment: Distinguishing unobserved heterogeneity from state dependence in labour market histories. *The Economic Journal 110*(466), 805–837.

Mertens, A., V. Gash, and F. McGinnity (2007). The Cost of Flexibility at the Margin. Comparing the Wage Penalty for Fixed-term Contracts in Germany and Spain using Quantile Regression. *Labour 21*(4-5), 637–666.

Michaud, P.-C., A. Kapteyn, J. P. Smith, and A. v. Soest (2011). Temporary and permanent unit non-response in follow-up interviews of the Health and Retirement Study. *Longitudinal and Life Course Studies 2*(2), 145–169.

Ministry of Labor and Social Policies (2017). *Rapporto annuale sulle Comunicazioni Obbligatorie 2017. Le dinamiche del mercato del lavoro dipendente e parasubordinato.* Roma: Ministry of Labor and Social Policies.

Nienhuser, W. and W. Matiaske (2006). Effects of the 'principle of non-discrimination' on temporary agency work: compensation and working conditions of temporary agency workers in 15 European countries. *Industrial Relations Journal 37*(1), 64–77.

OECD (1994). *The OECD jobs study: facts, analysis, strategies.* Paris: OECD.

Picchio, M. (2006). Wage Differentials between Temporary and Permanent Workers in Italy. Technical Report 257, Universita' Politecnica delle Marche, Dipartimento di Scienze Economiche e Sociali.

Picchio, M. (2008). Temporary contracts and transitions to stable jobs in italy. *LABOUR 22*(s1), 147–174.

Rosen, S. (1986). Chapter 12 The theory of equalizing differences. Volume 1 of *Handbook of Labor Economics*, pp. 641 – 692. Elsevier.

Scott, C. and B. Amenuvegbe (1990, May). Effect of recall duration on reporting of household expenditures : an experimental study in Ghana. Technical Report 8905, The World Bank.

Stewart, M. B. (2007). The interrelated dynamics of unemployment and low-wage employment. *Journal of Applied Econometrics 22*(3), 511–531.

Stull, D. E., N. K. Leidy, B. Parasuraman, and O. Chassany (2009, April). Optimal recall periods for patient-reported outcomes: challenges and potential solutions. *Current Medical Research and Opinion 25*(4), 929–942.

Tourangeau, R., B. Edwards, T. P. Johnson, K. M. Wolter, and N. Bates (Eds.) (2014). *Hard-to-Survey Populations*. Cambridge: Cambridge University Press.

Uhlendorff, A. (2006). From No Pay to Low Pay and Back Again? A Multi-State Model of Low Pay Dynamics. Technical Report 2482, Institute for the Study of Labor (IZA).