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IZA DP No. 13008

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# ABSTRACT

## Buy Flexible, Pay More: The Role of Temporary Contracts on Wage Inequality*

We investigate the role of temporary contracts in shaping wage inequality in a dual labour market. Based on Italian individual-level administrative data, our analysis focuses on new hires in temporary and open-ended contracts for the period of 2005–2015. To estimate the presence of differentials over the daily wage distribution, we follow Firpo (2007) and implement an inverse probability estimator, which allows us to control for labour market history, including lagged outcomes, over the last 16 years. Our results show the existence of a premium for temporary contracts over the full distribution of daily remuneration at entry, confirming the economic theory of equalizing differences. The wage premium is greater when permanent contracts are more valuable, such as for 'marginalised' categories like female, young, and low-paid temporary workers, and during the years of the economic crisis. The gap remains substantial after taking into account differences in working hours between workers.

**JEL Classification:** J31, J41, C31, J21

**Keywords:** temporary work, wage inequality, unconditional quantile treatment effect, inverse probability weighting

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# 1 Introduction

Wage inequality has always existed. As each person differs in terms of skills, attitude, opportunities, and background, it must not be surprising whether two or more individuals earn different levels of salary. Thereby, wage inequality may not represent an issue by itself, but some of its determinants do. Understanding what drives wage inequality is very important considering it has grown constantly over the last decades (Lemieux, 2006; Felbermayr et al., 2018; Devicienti et al., 2019).

Another contemporaneous phenomenon in the labour market of several countries is the increasing diffusion of temporary contracts. From constituting only a small minority of jobs, they have become the main channel of entry into the labour markets of several southern European countries characterised by a strong duality. While these contracts have been introduced with the idea of providing flexible labour to employers and facilitating the occupational activation of 'marginalised' categories, their diffusion might have also contributed to a widening wage inequality and partly explains its recent increasing trend.

The existence of a wage gap between temporary and permanent workers is predicted by economic theory. According to Rosen (1986), temporary workers should receive higher wages as 'compensation' for their less favourable job conditions. However, most empirical research has found that temporary workers receive a wage penalty rather than a 'wage premium' (e.g. Blanchard and Landier, 2002; Booth et al., 2002; Boeri, 2011; Gebel, 2010; Kahn, 2016). According to these studies, the wage penalty reported by temporary workers may be due to their lower levels of bargaining power, training and labour productivity, as well as expectations of conversion into a higher-paid permanent contract in the future.

In this study, we aim to provide further insight on the role of temporary contracts in shaping wage inequality. Identifying the influence of temporary contracts on wage inequality is challenging, however, due to several confounding factors. First, the composition of workers hired into the two types of contracts is likely to differ due to more selective hiring processes for permanent jobs. Second, even if assignment in the two contracts was as good as random, selective attrition along the job spell is likely to modify the composition of the two groups in a different way. For example, we may expect the more highly skilled temporary workers may be 'promoted' to permanent jobs (Elia, 2010), which changes the skill composition of the two groups in favour of permanent jobs along the job spell. Both selections would bias the estimates in the direction of lower wages for temporary contracts.

Our contribution to the literature is threefold. First, the previous literature relied on stock samples of workers, whose composition may be affected by dynamic sorting. To avoid this issue, we focus on an inflow sample of new hires and compare their gross daily wages at hiring.

Second, while most of the previous literature has used survey data, we rely on administrative data due to the richer longitudinal dimension, larger sample size, and lower risk of misreporting. We draw a sample of more than 3 million new hires during the period of 2005–2015 from Italian administrative registries (LoSai INPS), which cover about 6.5% of employees in the salaried private sector. We focus on Italy as an interesting case study because the share of temporary workers out of the total number of employees substantially increased in recent decades, such that they represent the majority of new hires each year (67.6% in the fourth quarter of 2018, Ministry of Labor and Social Policies, 2019). By exploiting the rich panel structure of the data, we control for selection at hiring in the two contracts taking into account occupational history over the last 16 years, which importantly includes lagged wages and contracts. We implement a flexible inverse probability weighting estimator to estimate the average treatment effect on the treated (ATT), where the estimand of interest is the wage earned by individuals hired into permanent jobs, conditional on a similar employment history to those hired in temporary ones.

Third, we gain further insight into the potential mechanisms by taking advantage of the large sample. First of all, since wage inequality is partially related to differences in terms of working hours among employees (Vaughan-Whitehead and Vazquez-Alvarez, 2018; Checchi et al., 2018; Ciani and Torrini, 2019), we run estimates both for all hires and the ones in full-time jobs only. Moreover, we estimate the unconditional quantile treatment effect on the treated (QTT), as proposed by (Firpo, 2007), to determine whether wage inequality changes over the distribution. Finally, we estimate heterogeneous effects by individual characteristics (i.e. gender, age, and macro-region of residence), year of hiring, sector, and type of temporary contract. Indeed, differences in wage inequality are expected over different categories of individuals. For example, on the one hand, 'marginalised' categories such as female, young, and low-skilled workers may be negatively affected by temporary contracts given their lower bargaining power, unlike other workers who may receive a wage premium to accept a flexible contract. On the other hand, the intrinsic value of an open-ended contract may be higher for 'marginalised' individuals, who might be willing to work in a permanent job for a lower wage.

The descriptive evidence is in line with previous empirical research showing a wage penalty for full-time temporary contracts. However, we reach different conclusions after controlling for positive selection into permanent contracts. We estimate an average premium at entry for temporary contracts of about 11.3 percentages points (pp). The differences are smaller for full-time jobs (9.5 pp), which indicates the presence of inequality in working hours in favour of temporary employment. However, in contrast with the previous literature, our evidence corroborates the theory of equalizing differences (Rosen, 1986), which states that an individual may prefer a temporary contract over an open-ended one only in the presence of a wage premium, due to the intrinsic value of permanent jobs. This interpretation is also supported by estimations on the quantiles and of heterogeneity, which show a larger premium for low-skilled workers, women, youths, and during the years of the economic crisis. The premium is therefore larger for categories of workers for whom this intrinsic value is probably higher. Results are robust to multiple sensitivity analyses, such as a placebo test on the lagged dependent variable (Imbens and Wooldridge, 2009) and the relatively large confounder that would be needed to invalidate our results (Rosenbaum, 2002). A second estimation approach based on a fixed-effect estimator exploiting the entry into different contracts by the same individuals also confirms an average premium for temporary contracts at hiring.

The remainder of the paper is organised as follows. Section 2 contains a review of studies focusing on wage inequality and temporary contracts. Section 3 presents the institutional setting. Section 4 describes the administrative data and sample selection. The descriptive evidence is presented in Section 5, while Section 6 explains the empirical strategy we use to estimate the wage gap between temporary and open-ended contracts. Section 7 shows results of the econometric analysis. The last section offers some concluding remarks.

# 2 Literature Review

Much theoretical and empirical research in the economic literature has tried to identify numerous sources of wage inequality, such as discrimination related to individual characteristics, e.g. gender, ethnicity (Barth et al., 2012; Goldin et al., 2017; Barth et al., 2017); globalisation (Helpman, 2017); collective bargaining and unionisation (Lemieux, 2008; Devicienti et al., 2019); education and labour productivity (Iranzo et al., 2008; Faggio et al., 2010; Barth et al., 2016); price levels (Autor et al., 2005; Boeri et al., 2019); and working hours (Checchi et al., 2018; Ciani and Torrini, 2019). A further source of wage inequality may be related to the heterogeneity of job contracts existing in a labour market (Cazes and Laiglesia, 2015), and in particular, to the duality between temporary and permanent workers.

Economic theory suggests that open-ended contracts have a higher intrinsic value for workers thanks to the longer expected duration and lower likelihood of future unemployment spells. Rosen (1986) was one of the first economic studies to theorize a positive wage gap in favour of temporary workers. According to his theory of equalizing differences, a wage premium for temporary jobs is possible, even at the same level of competence, because of less favourable conditions. Wage profiles may also differ. Individuals in temporary contracts may earn a higher wage at entry since their shorter expected job duration might leave less room for future wage increases through deferred compensation schemes and seniority rules than permanent jobs.

Nonetheless, the literature tends to find evidence that temporary workers receive a wage penalty rather than a ‘wage premium’. In Table 1, we present a collection of empirical studies focusing on EU and OECD countries. The empirical literature is quite cohesive in detecting that temporary jobs pay significantly less than permanent ones, regardless of the estimation methodology. The gap seems larger for lower-wage workers (Mertens et al., 2007; Barbieri and Cutuli, 2010; Comi and Grasseni, 2012; Bosio, 2014; Regoli et al., 2019). There are, however, a few exceptions. For instance, a wage premium is reported in Japan and Norway (Brown and Sessions, 2005) and for highly educated individuals (Brown and Sessions, 2005; Raitano and Fana, 2019). Similarly, Laß and Wooden (2019) found that in Australia, higher-paid casual and temporary agency workers receive a wage premium, in contrast to the workers below the first decile of the wage distribution, who show a significant gap. No differences were found between fixed-term and open-ended contracts.

An aspect that may explain the existence of a negative wage gap is the lower level of training received by temporary workers during a job spell. For instance, Arulampalam and Booth (1998) and Booth et al. (2002) show that fixed-term employees in the United Kingdom have a lower probability of being involved in training, and especially those who also have 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 may represent another important factor to consider when interpreting the existing wage gap during a job spell (Barbieri and Cutuli, 2010; Comi and Grasseni, 2012). According to Picchio (2006) and Bosio (2014), the negative wage gap reported by temporary workers may also be due to the fact that some individuals choose these jobs as probationary periods and accept lower wages since they anticipate being renewed with a high-paid permanent contract afterwards. The potential of temporary contracts to increase the chances of obtaining a permanent job is also a controversial issue,[1] although a large part of the empirical literature finds positive 'stepping stone effects'.[2] Overall, the presence of a wage penalty for temporary contracts may have consequences for the earning instability of individuals and, more generally, for the inequality of national wage levels, as highlighted by studies such as Brandolini et al. (2002), Mertens et al. (2007), Cappellari and Leonardi (2016), and Laß and Wooden (2019).

[1]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 also be used to improve human capital and social contacts, and to acquire information about vacancies.

[2]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 section of the literature finds a negligible effect, as in France (Magnac, 2000), Spain (Güell and Petrongolo, 2007), and 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).

Table 1: Literature review on wage gap between permanent and temporary workers

| **Paper** | **Country** | **Data** | **Methodology** | **Results (temporary vs permanent workers)** |
|---|---|---|---|---|
| Blanchard and Landier (2002) | France | Enquêtes Emploi (1983-2000) survey | OLS | -20% monthly wage |
| Booth et al. (2002) | UK | BHPS (1991-1997) survey | OLS, individual fixed-effects | -13/17% (OLS), -7/11% (FE) hourly wage |
| Hagen (2002) | West Germany | GSOEP (1999) survey | OLS, matching and control function | -6% (matching), -10% (OLS), -23% (control function) hourly wage |
| Brown and Sessions (2003) | UK | BSAS (1997) survey | OLS and IV | -13% hourly wage |
| Brown and Sessions (2005) | 13 OECD countries | BSAS (1997), ISSP (1997) surveys | Heckman selection | Gap in all countries apart from JP and NO (not significant in US, IT, DK and CH). Premium in case of tertiary education |
| Gash and McGinnity (2007) | West Germany and France | ECHP (1994-2001) survey | Nearest-neighbour matching | -9/10% hourly wage |
| Mertens et al. (2007) | Germany and Spain | GSOEP (1995-2000), ECHP (1995-2000) surveys | OLS, individual fixed-effects, quantile regression | -18% (OLS), -4/7% (FE), lower penalty for the high-earning workers (QR) |
| Elia (2010) | Italy | SHIW (2002-2006) survey | DiD: fixed-term vs permanent contracts before-after Biagi 2003 reform | -8/10% monthly wage |
| Barbieri and Cutuli (2010) | Italy | ECHP (1995-2001), SHIW (2004-2008), IT-SILC (2004-2006) surveys | OLS, matching, quantile regression, individual fixed-effects | -8/12% (OLS), -8/10% (FE), -9% (matching), lower gap for high-wage workers (QR) |
| Gebel (2010) | UK, Germany | BHPS & GSOEP (1991-2007) surveys | Matching | -10/21% monthly wage decreasing during the career |
| Boeri (2011) | 15 EU countries | EU-SILC, ECHP (2004-2007) surveys | OLS | -7/45% monthly wage |
| Comi and Grasseni (2012) | 9 EU countries | EU-SILC (2006) survey | OLS, quantile regression | -7/21% hourly wage (OLS), larger for low-wage workers (QR) |
| Bosio (2014) | Italy | SHIW (2002-2008) survey | RIF regression, IVQTE (regional & sectoral exposition to labour market reform) | -7/14% hourly wage at the median, insignificant penalty for high-wage workers |
| Kahn (2016) | 13 EU countries | ECHP (1995-2001) survey | Individual fixed-effects | -1.5/3% hourly wage |
| Duman (2019) | Turkey | HLFS (2004-2015) survey | Quantile regression, Heckman selection, Blinder-Oaxaca decomposition | -10% hourly wage at the mean and U-shaped penalty along distribution |
| Regoli et al. (2019) | Italy, France and Germany | EU-SILC (2009) survey | RIF regression | Significant decreasing wage penalty along distribution |
| Raitano and Fana (2019) | Italy | EU-SILC (2004-2012) survey integrated with INPS administrative data | OLS | (Not-significant) wage premium for graduated new-entrants with a temporary job |
| Laß and Wooden (2019) | Australia | HILDA (2001-2015) survey | RIF regression with individual fixed-effects | (Gap) premium for (very low-) high-paid casual & agency workers |

Two considerations regarding the results found in the literature are, however, worthy of mention. First, all studies on temporary contracts and wage inequality rely on survey data, with the sole exception of Raitano and Fana (2019), who uses a sample from EU-SILC merged with information from administrative registries (INPS). Survey data, however, may suffer from small numbers of observations, misreporting, and limited information on the employment history of the individuals (see Section 4). Second, the empirical literature has mostly analysed stock samples of existing jobs, which can make the identification of causal effects quite challenging.[3] Indeed, selective attrition along the job spell is likely to modify the composition of the two groups in a different way, biasing estimates in the direction of a wage penalty. For example, higher-skilled temporary workers are more likely to be 'promoted' to permanent jobs (Elia, 2010), which would change the skill composition in favour of permanent jobs. Our choice of using administrative data and focusing on the wage level of new hires therefore represents a novelty in the literature.

# 3 Institutional Framework

Temporary employment contracts were introduced in Italy in the sixties, but for a long time, they remained a clear minority in the Italian labour 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 into the labour market (67.6% in the fourth quarter of 2018 – Ministry of Labor and Social Policies, 2019) and 17.1% of total employees in 2018, three points above the EU28 average (Eurostat, 2019). The main reason for this sudden rise can be found in the reforms of the Italian labour market starting in the 1990s. Since then, the international labour market has known deep change in terms of legislation and socio-economic features in order to cope with growing needs related to economic globalization. Moreover, an OECD (1994) study emphasised that the loss of competitiveness, the growth slowdown, and the increase in unemployment from the 1970s to the 1990s, especially in some countries such as Italy, were due to policies that did not favour flexibility in the labour market. Therefore, the aim of the above-mentioned reforms consisted of boosting temporary employment and reducing the rigidity of the Italian labour market overall.[4]

Two legislative interventions particularly encouraged the use of temporary contracts: the Treu Package (1997) and the Biagi Law (2003). The first relaxed the rules for apprenticeships and introduced new types of temporary contracts (e.g. temporary agency workers). As for the second, it further both incentivised the use of temporary and apprenticeship contracts and enlarged their supply (e.g. introducing collaborator contracts - the so-called *co.co.co*). In

[3]Only one study focuses on hirings: Raitano and Fana (2019). This paper, however, analyses only fresh graduates entering the labour market.

[4]Having the opportunity to quickly adequate the number of employees in case of demand variations should indeed incentivise employers to hire more (Cahuc and Postel-Vinay, 2002; Boeri and Garibaldi, 2007).

recent years, however, a route change seems to have taken place in Italy, as the last two reforms of the labour market (the Fornero reform and the Jobs Act) tried to favour more stable work relations. To improve 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) encouraged the creation of permanent work contracts through massive fiscal benefits to employers and lower firing costs.

Another imporant aspect of the Italian labour market is the relevant role of collective agreements. Italy does not have a national minimum wage but salaries and work conditions are negotiated in a two-tier structure. The first tier is at the industry level, where the representatives of employers and workers negotiate issues such as minimum wage, working hours, organisation, and disciplinary dispositions. In 2014, more than 500 collective bargaining agreements (so-called *Contratti Collettivi Nazionali di Lavoro*, or CCNL) existed in Italy Lucifora and Vigani (2019). While there is no formal extension to employers and employees not associated with an employers' organisation, the wage minima set in the CCNL are *de facto* binding as they are frequently used by the work courts to determine the fair level of compensation for a job (Article 36 of the Italian Constitution). The regulations set by the CCNL apply in the same way to both temporary and open-ended workers, including minimum remuneration. A differentiation of salaries may instead occur at the second tier level since employers can offer wages exceeding the CCNL minima.

# 4 Data and Sample Selection

Most of above-mentioned studies investigating the wage gap between temporary and permanent workers rely on survey data (Table 1), such as data from the European Union Statistics on Income and Living Conditions (EU-SILC, e.g. Boeri, 2011; Comi and Grasseni, 2012; Regoli et al., 2019), the British Household Panel Survey (BHPS, e.g. Booth et al., 2002; Gebel, 2010), and the Household Income and Labour Dynamics in Australia survey (HILDA, e.g. Laß and Wooden, 2019). However, survey data present several issues for this type of analysis.

First, survey data are generally characterized by a limited number of sample observations, which may increase the discrepancy around the true value of the phenomenon under analysis (Deaton, 1997).[5] Furthermore, non-response bias might also affect the estimates, especially for 'hard-to-survey' populations (Tourangeau et al., 2014) such as the youth, the less educated, low-income people, the residentially mobile, and those living in single adult households (Michaud et al., 2011; Frankel and Hillygus, 2014; Jenkins and Van Kerm, 2017). The latter represents a particular concern for studies analysing income or wage distribution since the non-response

[5]For instance, the BHPS contains about 5,000 households, compared to a total population of 27 million households in the UK. Similarly, the HILDA survey involves more than 17,000 respondents 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 out of 25 million households living in Italy.

bias may change along the distribution itself. Finally, survey data may suffer biases related to the misreporting and recalling of respondents. As for misreporting, it may be associated with perceived social stigma or systematic under-reporting behaviours, and it is more common among highly educated individuals, the self-employed, and wealthier households (Cannari and D'Alessio, 1993; Hurst et al., 2013; Greene et al., 2017). As for recall bias, it depends on the fact that, as is 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). This bias may affect the credibility of analyses reconstructing the labour market history of the individuals.

To avoid all of these issues, differently from most of studies in Table 1, we use administrative data from social security registers of the Italian Social Security Institute (LoSai INPS). LoSai's overall sample available for research purposes has a longitudinal structure from 1985 up to 2015 and covers 6.5% of all salaried or semi-subordinate employees working in the salaried non-agricultural private sector. The data contains individual employment histories since 1985, unemployment benefit receipts from 1999 onwards, and other information on assimilated working weeks (e.g. sickness, maternity leave, military service, short-term compensation). It also provides firm characteristics such as dimension and sector and worker characteristics such as gender and year of birth. Thanks to the rich longitudinal dimension, we can reliably reconstruct the labour market history of individuals, which we use to control for selection into permanent and temporary contracts (see Section 6). As a further advantage, the large number of observations is ideal to provide reliable estimates of the wage gap related to temporary contracts along quantiles of the wage distribution.

Our sample is composed of 3,453,413 new hires between 2005 and 2015. As such, we are able to estimate the wage premium during different economic conditions and institutional periods such as the reforms in 2012 (Fornero Law) and 2015 (Jobs Act). Following much empirical research in the literature (Baker and Solon, 2003; Blundell et al., 2015; Hospido, 2015; Cappellari and Leonardi, 2016), we set an age restriction on the sample. Specifically, we focus on individuals between the ages of 15 and 65.[6] To obtain a more robust estimation of the average differences, we also exclude extreme values from our sample, trimming the data at the $0.1^{\text{th}}$ and $99.9^{\text{th}}$ 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 and wage) or covariates, for a final sample consisting of 3,346,560 observations (new jobs) or 1,214,642 individuals. Overall, about 38% of the jobs created in the period are permanent contracts (1,273,764), 52% are temporary contracts (1,738,980), 4% are seasonal jobs (144,347), and 6% are apprenticeships (189,469). Temporary contracts are further subdivided into fixed-term contracts or *Contratti a tempo determinato* (1,145,800), agency

[6] In contrast to the common methodological choice to drop female workers to minimize selection issues, we decide not to restrict the sample to males only; however, we show results by males and females separately in Section 7.

contracts (274,941), on-call contracts (73,407), and other temporary or subsidized job contracts (244,832).

The outcome of our analysis is the individual gross daily wage at the moment of hiring, which is calculated as the ratio between gross total remuneration and working days in the initial part of the spell. This wage definition allows us to take into account differences in terms of working days among workers; however, it does not consider the further working hours inequality potentially existing among employees (Vaughan-Whitehead and Vazquez-Alvarez, 2018; Checchi et al., 2018; Ciani and Torrini, 2019). As the LoSai dataset does not contain precise information on working hours but only on part-time or full-time work status, we develop the econometric analyses first looking at all jobs, and then, similarly to other empirical research on the topic (e.g. Gottschalk et al., 1994; Brandolini et al., 2002; Dustmann and Meghir, 2005), we consider full-time contracts only. In the first case, differences in working hours are thus considered as a component of the overall observed remuneration gap between temporary and open-ended contracts. Finally, since the analysis involves a long period of time (2005–2015), daily wages are inflation-adjusted (base 2016=100) using the national consumer price index provided by the Italian Institute of Statistics (Istat).

# 5 Descriptive Evidence

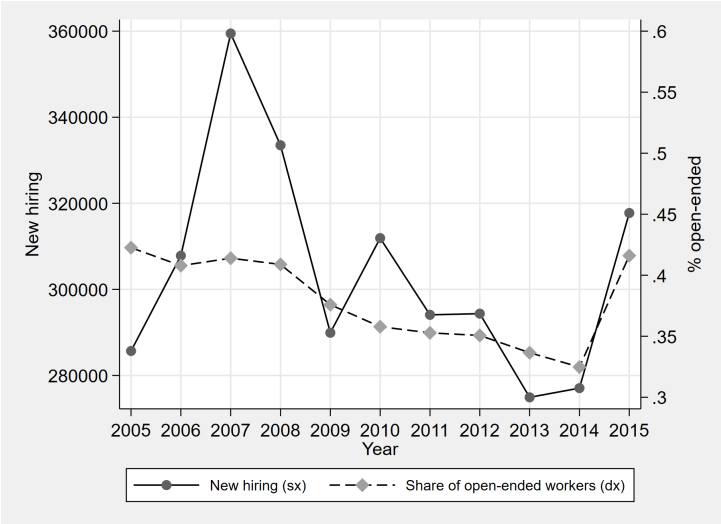
In this section, we show some descriptive evidence on the evolution of temporary contracts and differences in daily salary at hiring based on our final sample of administrative data. First, we look at the evolution of hiring in the salaried private sector during the analysed period (i.e. annual inflow of observations in the sample, Figure 1). The number of new hires rises from 286 to 360 thousand per year in the 2005–2007 period, which represents 4.3 and 5.5 million people in the full Italian population. The new hires greatly decrease from 2007 until 2014 and increase again in the last year of analysis (i.e. 2015). The first increase and the subsequent collapse in the number of new hires are linked to the macroeconomic cycle observed at the national level in the same period, especially regarding the negative effects produced by the economic crises in 2008–2009 and 2013.[7] As for the rise shown in 2015, this might be related to the economic recovery as well as the last reform of the Italian labour market (i.e. the Jobs Act) and its fiscal benefits to employers.

Figure 1 also highlights a constant decrease in the use of permanent employment contracts by Italian firms over time. In fact, the share of permanent workers among the new hires was more than 40% in 2005 and about 33% in 2014. However, the trend reverses in 2015, when the Jobs Act reform is implemented, given the reform objective of discouraging temporary contracts in favour of open-ended ones. Through the relative variations in new hires by job

[7]This is confirmed by the fact that the coefficient of correlation between the annual number of new hires 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.

contract reported in Figure 2, it is possible to better understand why the share of permanent contracts decreased so much among new hires in the Italian labour market. The reasons for this are mainly that, on the one hand, the number of temporary workers increased more than others in the 2005–2008 period, and on the other hand, the number of open-ended workers strongly decreased from 2009 onwards, except for the upturn observed in 2015.

Figure 1: New hires and share of open-ended workers




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

Beyond the two main categories of job contracts (i.e. temporary and open-ended), there is an additional type of worker that represents a small part of our sample (about 6% of new hires): apprentices.[8] This job contract, however, has peculiar features, such as an age restriction and employee income regulation. For this reason, we exclude it from the main analysis reported in Section 7 but return to this job contract at a later time. Figure 2 shows that the number of apprentices among new hires increased slightly until 2007, but then a large drop is reported in the subsequent eight years.

Comparing the distribution of the daily wages of temporary workers to that of open-ended workers in the reference period (2005–2015), the former appears more concentrated around the central peak, whereas the latter features more extended wings and thus a greater variance (Figure 3).[9] Therefore, open-ended workers in our sample are more likely to report both the lowest and highest levels of daily wage with respect to temporary employees. When we restrict the sample to full-time jobs (Figure 3b) to control for working hours inequality among workers,

[8] Another small minority of workers is composed of those having a seasonal contract. These job contracts generally have a very short length, and they can only be adopted in a limited range of economic activity sectors (e.g. tourism, restaurants, agriculture).

[9] Figure 3a illustrates a noticeable hump in the left part of both distributions, which is probably related to the minimum level of taxable contributions for Italian employees working in the private sector (7.15 euros per working hour in 2015).

the difference in the left side of the distribution largely disappears, which might be related to the hourly minima set by the collective bargaining agreements, covering both temporary and open-ended contracts in the same way.

Figure 2: Relative variation in new hires by job contract (base 2005=100)

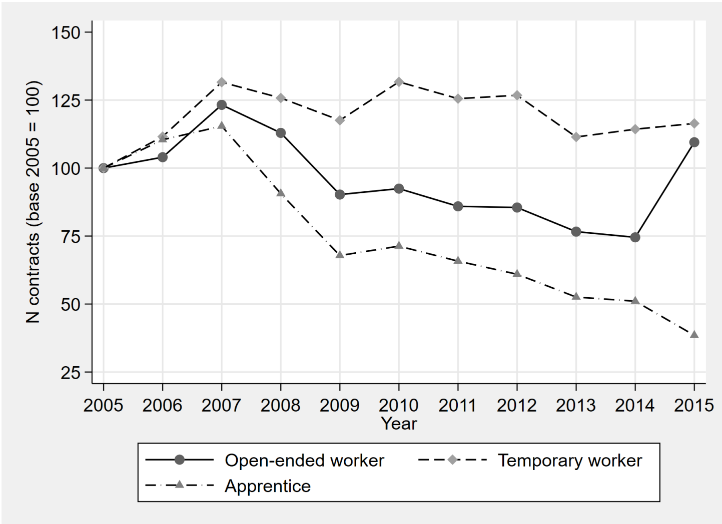
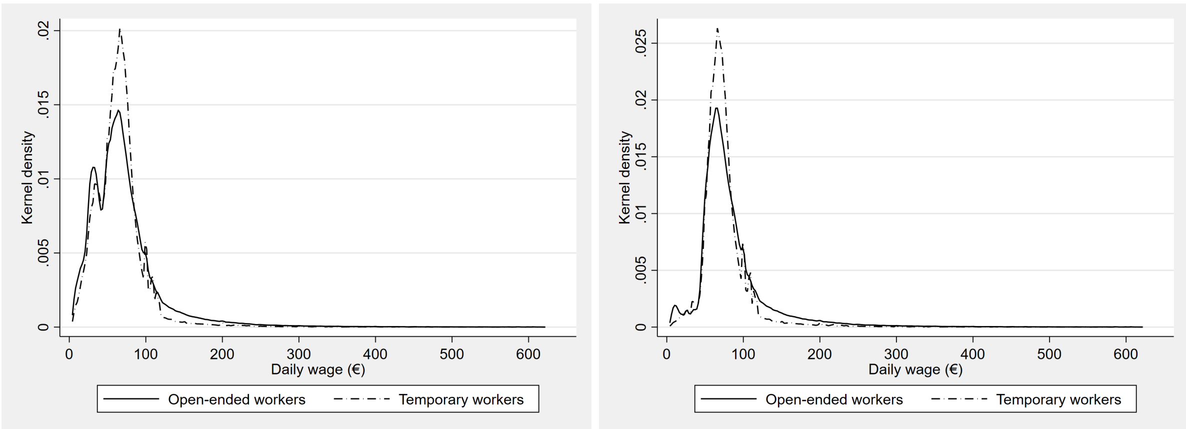



Figure 3: Kernel density estimates for daily wage by job contract

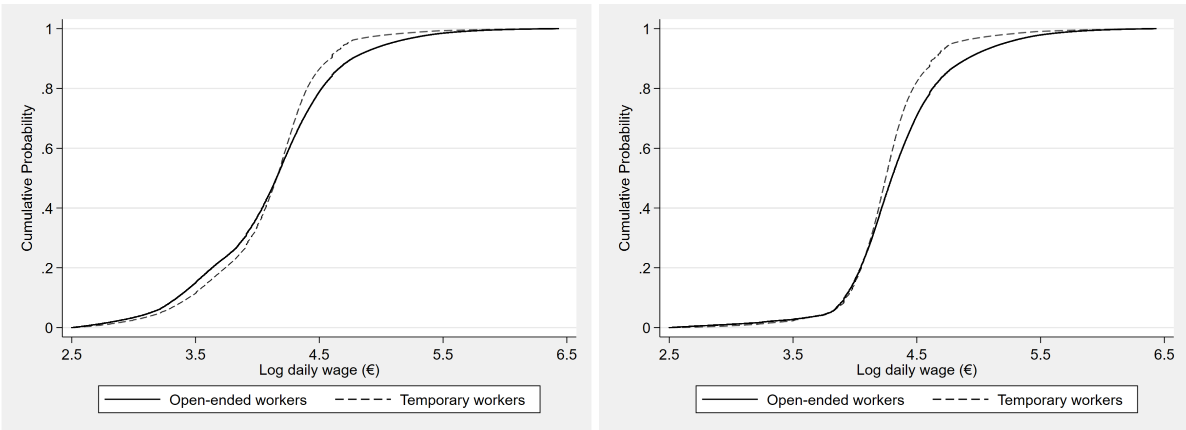



(a) Any job (b) Full-time jobs

Looking at the mean remuneration by contract type during the period of 2005–2015, we observe that open-ended jobs earn, on average, a daily salary that is 8% higher (71.4 versus 66.2 euros). This difference reaches 12% if we consider only full-time jobs (86.0 versus 76.5 euros per day), which is explained by the fact that temporary workers tend to work less as part-time employees (28.6% compared to 30.1%). Cumulative distribution functions provided in Figure 4 give further and clearer evidence of the overall differences between the two job contracts under analysis. Figure 4a shows that temporary workers tend to have a greater (smaller) daily wage than open-ended workers until (after) the median. Figure 4b, however, highlights a partially different conclusion when we consider full-time jobs only. In this case, open-ended jobs seem to grant a premium along the full wage distribution.

This descriptive analysis highlights the importance of looking at the gap over the full distribution and not just at the average wage. However, this evidence does not take into account potential differences in the composition of workers hired with different contracts in terms of demographic characteristics, skills, and previous work experience. Indeed, employers generally put greater effort into selection procedures (i.e. to choose the best candidate possible) when they hire through a permanent contract. At the same time, these factors are also likely to have a significant role in explaining the daily wage. Therefore, descriptive evidence of a wage penalty for temporary job may be due to the fact that open-ended workers tend to be more expert and high-skilled than temporary ones at hiring. In the following sections, we try to take into account these confounders.

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




(a) Any job (b) Full-time jobs

# 6 Identification Strategy

In this section, we define the empirical strategy used to estimate the presence of a premium or penalty on daily wages at entry for temporary contracts. We consider as treated new hires in any temporary contract, whereas the control group is composed of new hires in open-ended contracts. Later, in Section 7.3, we distinguish between different types of temporary contracts present in the Italian legislation.

Our identification strategy is based on unconfoundedness or the conditional independence assumption (CIA), which assumes that once controlled for the observable characteristics, the potential salary of the individuals in the different contracts is independent of the actual type of contract. Our goal is first to estimate the average gap for the temporary contracts (i.e. the so-called average treatment effect on the treated – ATT). Second, we estimate the quantile treatment effect on the treated (QTT), which represents the effect on the distribution of the outcomes of the temporary contracts. To flexibly control for potential confounders, we implement an inverse probability weighting estimator (IPW – see e.g. Firpo, 2007; Busso et al., 2014). Compared to the ATT, the estimation of QTTs provides further insights into wage inequality. Specifically, estimated QTTs highlight whether the wage differential between temporary and open-ended contracts is stable over the distribution or whether it differs, for instance, from low-paid to high-paid workers.

Clearly, the set of conditioning variables is crucial to assess the credibility of the 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. In order to exploit the full information set of the dataset and capture different trends in the salaries of the two groups of workers, the list of covariates we control for is divided between: i) old history (between 16 and 11 years before treatment); ii) less recent history (10–6 years before treatment); iii) more recent history (5–2 years before treatment); and iv) the last year.

We select a long list of variables that may affect the potential salary and the probability of selection into a temporary or a permanent contract either directly or indirectly. We therefore include average daily remuneration (lagged outcomes), percentage of working weeks by contract (lagged treatments), qualification (blue collar, white collar, or apprentice), firm size (0–15, 16–200, 201+), and macrosectors. In addition, we include total weeks worked (with a specific dummy if zero), total remuneration as collaborator, number of years receiving unemployment benefits (also with the total cumulated days), total hours of temporary layoffs (the so-called '*Cassa Integrazione Guadagni*', CIG) and ever worked part-time. For the last year, the control variables are more detailed on the main job and also include a proxy for percentage of part-time work,[10] 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.[11] Finally, as information regarding the current job is simultaneous with the treatment and, therefore, endogenous, we include it only in a sensitivity analysis and when we condition the sample on job characteristics (e.g. full-time jobs only, in Section 7.1). More specifically, simultaneous characteristics cover qualification (i.e. blue collar, white collar, apprenticeship, senior staff, director, and other), firm position (i.e. group, single, and mother), firm size (14 dummies), microsector (i.e. NACE 2002 at the 2-digit level), part-time status (i.e. mixed, vertical, and horizontal), and having already worked

[10]This is obtained by dividing the working weeks 'useful' for social security purposes by the total working weeks. We also use this proxy to build an adjusted version of daily wages and add them to the set of control variables.

[11]Note that the region of residence is measured in 2015 due to data availability.

in the same firm and the total remuneration received.[12] A detailed list of the predetermined and exogenous covariates used in the benchmark analysis can be found in Table A.1 in Appendix A.

We stress that among the conditioning variables, we also include detailed information on past wages over the last 16 years. These lagged outcomes allow us to control for unobserved heterogeneity that is invariant over time, like a fixed-effects panel data estimator. Indeed, if the 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 been manifested in the previous wages of individuals (Imbens and Wooldridge, 2009). Besides, as we observe past remuneration in multiple lags, we can control for differential trends in the outcomes between the two groups. As we aim to estimate the gap over the full outcome distribution, we include higher terms of these lagged dependent variables up to the third order.

Several random factors, on top of our covariates, may drive the entry of an individual in a temporary or permanent job. This source of exogeneity is what we indirectly exploit for identification. For example, as vacancies are posted in a heterogeneous way over time and across locations, at the moment when the individual is looking for a job there may be only one vacancy that pays a sufficient compensation (which includes job security), taking into account also the specific commuting costs. This randomness in the opening of vacancies over time and locations is likely unrelated to the potential salary of the individuals. However, it may determine the entry into one of the two types of contracts and, therefore, be a source of exogeneity of the treatment selection that is left out from our set of conditioning variables. Nonetheless, even though we control for a rich set of worker characteristics that are likely to affect both outcome and treatment selection, we cannot avoid that other unobservable confounding factors may remain. Therefore, in a sensitivity analysis in Section 7.5, we estimate the size of a confounder that would be required to invalidate our results (Rosenbaum, 2002). More details are shown in Appendix B.[13]

Finally, note that our focus on the wage at hiring is justified by the stronger internal validity of the estimates, which avoid dynamic sorting along the spell. Indeed, even if the assignment in the two contracts would be as good as random, selective attrition along the job spell is likely to modify the composition of the two groups in a different way. For example, higher skilled temporary workers are more likely to be "promoted" to permanent jobs, which would change the skill composition in favour of permanent jobs. As temporary jobs are meant to expire after a certain date, this dynamic selection is particularly challenging the further we measure the outcome from the moment of hiring.

[12]About 14.1% of individuals in temporary jobs had already worked in the same firm compared to 5.4% of individuals in permanent jobs.

[13]Even if the CIA holds, the QTT 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 QTT can be interpreted as the quantiles of the treatment effect.

# 7 Results

## 7.1 A Wage Premium for Temporary Jobs

We first estimate the propensity score of entering in a temporary contract by implementing a logistic regression on our covariates. Selection into treatment given the vector of covariates is moderate as the pseudo R-squared is 0.148. Afterwards, we check common support of the estimated propensity score. To restrict the role of outliers and remove the thinnest part of the propensity score distribution, we drop from the sample the treated with a propensity score above the 99.9$^{\text{th}}$ percentile of the control units (Lechner and Strittmatter, 2019). The common support of the propensity score is shown in Figure A.2. Second, since some failure in the specification of the propensity score model might result in unbalanced covariates and biased estimates, balancing tests are performed. The results of these tests, reported in the Appendix (Tables A.1 and A.2), show that the IPW overall balances covariates characterizing treated and control workers, as the mean and the maximum standardized bias shrink from 9.2 and 62.1 to 1.9 and 7.8, respectively. In a sensitivity analysis, we also control for remaining unbalanced covariates by adding a regression adjustment in the ATT estimation (see Section 7.5).

The estimation of both the ATT and QTT on the logarithm of daily remuneration at hiring shows results that substantially differ from the descriptive evidence and previous empirical research (Table 1).[14] These are, however, in line with economic theory. As suggested by Rosen (1986) and his theory of equalizing differences, the results show a wage premium in favour 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 the moment of hiring. Such a premium on the daily wage is also explained by differences in working hours. As shown in Section 5, temporary workers tend to more often report full-time status than permanent workers. Once we re-weight the permanent jobs by the IPW weight, the share of part-time work among permanent jobs jumps from 30% to 35% (compared to 29% for temporary jobs). Therefore, temporary workers seem to be granted a premium in terms of working hours. After removing part-time jobs[15], the premium at the mean is reduced by a few percentage points. As shown in Table 2, since the ATT decreases by about 16% (from $11.27\%$ to $9.51\%$), we may state that about one sixth of the wage premium for temporary jobs is related to working hours differences between temporary and permanent jobs.[16]

Interesting insights emerge when estimating heterogeneous 'effects' of temporary contracts

[14] In Figure A.1 in Appendix A, we report the QTT estimates before and after controlling for differences in composition between the two types of contracts.

[15] We remain with 2,128,369 jobs or 893,942 individuals.

[16] Selecting full-time jobs only cannot control for differences in overtime, which can also contribute to the daily remuneration. According to the 2015 Labour Force Survey (LFS), full-time permanent workers in Italy reported being paid for overtime work during the reference week more frequently than those with temporary contracts (3.0% vs 2.2%). This means that if we could take into account differences in overtime, we would probably find an even larger premium for temporary jobs.

according to some important individual characteristics. In particular, panel A in Table 2 illustrate wage gap heterogeneity by gender, age group (younger or older than 35 years of age), and macro-region of residence across Italy (North, Center, and South). For the estimation of heterogeneous effects, we fully re-estimate the IPW weights for each group of workers. The wage premium related to temporary contracts is significantly greater among female workers with respect to male ones and among the young compared to adult workers. Therefore, the wage premium for temporary employment seems to be more in favour of those groups of the working age population that have more difficulties in accessing the Italian labour market (Pacifico et al., 2018). Conversely, differences in the wage premium are quite small between the three macro-regions. 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 centre of Italy, respectively.

The exclusion of part-time jobs from the analysed sample also determines some differences in the wage premium observed by subgroups. Panel B in Table 2 show an overall reduction of ATTs, but some categories of workers reported a greater decrease in the wage premium than others. In particular, considering full-time jobs only leads to a small variation in estimates of the average wage premium for temporary jobs in the case of female workers and those living in southern regions. Conversely, a large reduction is revealed by male and young workers, as well as by those resident in the north of Italy. Finally, differences in terms of the wage premium for temporary contracts among categories of Italian workers increase when distinguishing by gender and, especially, by macro-area of residence, whereas they narrow slightly between young and adult workers.

Table 2: Estimation of the daily remuneration gap at the mean (ATT)

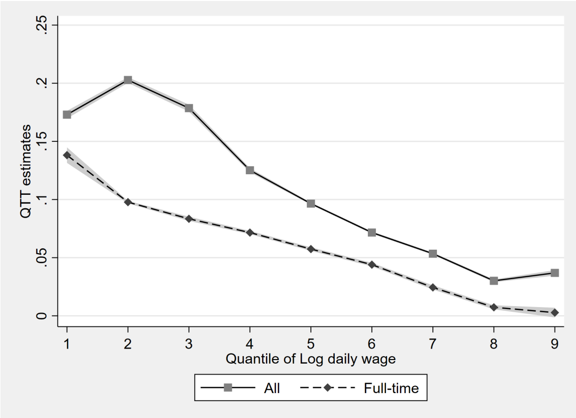
| Any jobs (A) | All (1) | Women (2) | Men (3) | Young (4) | Adult (5) | North (6) | Center (7) | South (8) |
|---|---|---|---|---|---|---|---|---|
| ATT | 11.27 | 13.93 | 9.76 | 13.11 | 9.64 | 11.56 | 12.88 | 11.31 |
| Std.Err. | (0.14) | (0.10) | (0.09) | (0.07) | (0.09) | (0.10) | (0.15) | (0.11) |
| Observations | 3,012,744 | 1,163,941 | 1,848,803 | 1,403,772 | 1,608,972 | 1,508,726 | 681,654 | 822,364 |
| N individuals | 1,152,057 | 468,318 | 683,739 | 599,320 | 658,152 | 574,287 | 266,133 | 311,637 |
| **Full-time jobs (B)** | **All (1)** | **Women (2)** | **Men (3)** | **Young (4)** | **Adult (5)** | **North (6)** | **Center (7)** | **South (8)** |
| ATT | 9.51 | 12.73 | 5.96 | 10.43 | 8.44 | 8.72 | 10.93 | 10.89 |
| Std.Err. | (0.09) | (0.20) | (0.10) | (0.12) | (0.15) | (0.11) | (0.25) | (0.18) |
| Observations | 2,128,369 | 643,293 | 1,485,076 | 966,109 | 1,162,260 | 1,090,505 | 457,338 | 580,526 |
| N individuals | 894,349 | 302,626 | 591,723 | 459,821 | 506,202 | 462,704 | 199,869 | 231,776 |

*Notes:* Estimates of ATT are based on the standard IPW method. Panel (A) retains all jobs, while panel (B) keeps full-time jobs only. In each column, we retain only individuals belonging to a certain group: all individuals (1), women (2), men (3), individuals younger (4) or older (5) than 35 years old, individuals living in the North (6), Center (7) or South (8). For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Standard errors (in parentheses) are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

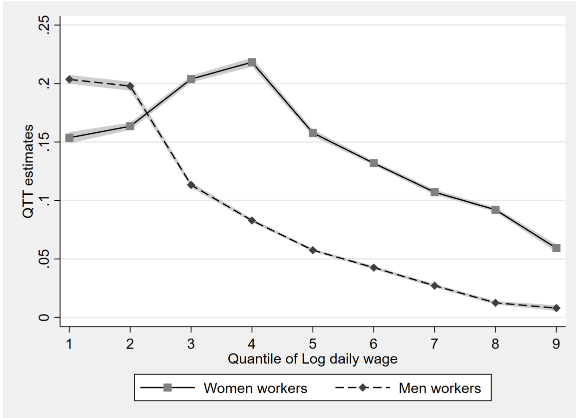
Finally, we look at the remuneration gap along the distribution. Differently from the previous literature, the wage premium appears to be greater among low-paid workers and almost null in the top part of the distribution, among high-paid workers. The estimated QTT for the whole sample (Figure 5a) is +$17.3\%$ at the first decile, +$9.6\%$ at the median, and +$3.6\%$ at the ninth decile. Once we condition on full-time jobs, the premium only slightly decreases over the full distribution (Figure 5a). As expected, the reduction in the observed wage premium especially involves the lowest part of the wage distribution, where part-time jobs are concentrated. We also observe some differences in the wage penalty along the wage distribution given individual characteristics.

First, Figure 5b indicates that the positive wage gap in favour of temporary workers is greater among male workers until the second decile, while it is greater among females from the third decile onwards. Second, and in contrast, differences by age groups of workers appear stable along the entire wage distribution since young workers always report a greater premium with respect to adults (Figure 5c). Third, Figure 5d shows that the wage premium is greater among workers living in the northern regions until the second decile, but it becomes almost zero in the highest part of the distribution for the same group. Estimates for full-time jobs only are shown in Figure A.3 in Appendix A. Overall, in line with evidence for the total sample, all subsamples of workers analysed show a greater premium for temporary jobs in the bottom part of the wage distribution and a lower or even not significant premium at the top.

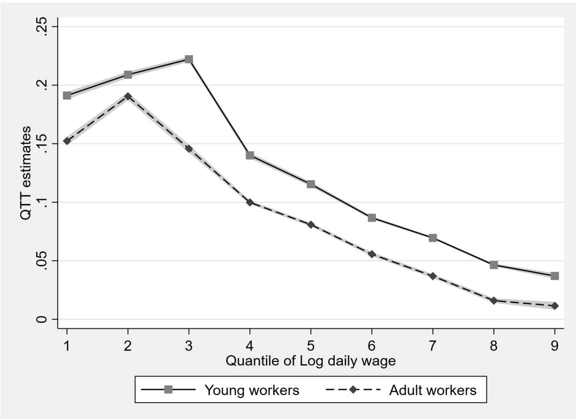
Figure 5: Estimation of the wage gap at the quantiles (QTT)



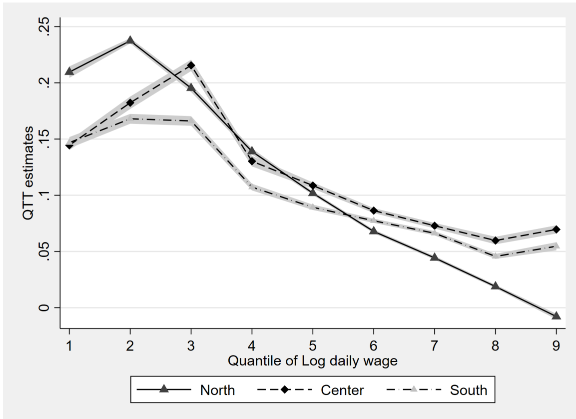

(a) Estimates for total sample




(b) Estimates by gender




(c) Estimates by age




(d) Estimates by macroregion

*Notes:* Estimates of QTT are based on the standard IPW method. For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

To sum up, our estimate shows a wage premium for temporary jobs, which is suggestive of a sort of compensation for the shorter expected job tenure and the poorer job security. This might indicate that individuals are willing to accept a lower salary when offered a permanent job, due to its higher intrinsic value. The fact that the premium is higher for workers expecting longer periods out of the labour market to find a new job confirms this interpretation. However, as our estimates refer to the wage at hiring, it is also possible that the wage difference might revert along the job spell. Indeed, the salary of a permanent contract is expected to have more room for increase over time given the longer expected duration of the job spell thanks to seniority rules and deferred compensation schemes. Due to stronger confounding factors in the estimation of wage inequality along the job spell, we limit our analysis to wage inequality at entry.

## 7.2 The Role of Reforms and the Economic Crisis

The reference period of our analysis is far from empty of exogenous shocks and featured significant events such as an economic crisis and reforms of the Italian labour market. Two different interventions particularly deserve mention: i) the Fornero reform (2012) and ii) the Jobs Act (2015). These interventions are likely to have an impact on the estimated wage premium because they changed the regulation of open-ended contracts in order to encourage their use. In particular, we expect that making open-ended contracts more attractive for firms and simultaneously less attractive for individuals because of the greater ease of dismissal may have led employers to reduce the wage premium for temporary contracts. It should be noted that, at least in the first part of our reference period, long-lasting effects related to the staggered implementation of the Biagi Law (2003) – which instead incentivised temporary and apprenticeship contracts (see Figure 2) – may also play a role in the wage premium. Moreover, the Italian economy suffered negative effects of the Great Recession during the reference period, especially in 2009 and 2013. If Rosen's (1986) theory of equalizing differences holds and considering the greater difficulty of finding a job during a recession, we expect that during the economic crisis open-ended contracts became even more valuable for workers, increasing the wage premium for temporary jobs. For the same reason, it is likely that the wage premium decreases during periods of economic growth.

Table 3 shows that the wage premium in favour of temporary contracts experienced some fluctuations during the 2005–2015 period. Specifically, it reports three different peaks (in 2005–2006, 2009, and 2013) and then collapses in 2015. The downward trend is interrupted by the 2009 and 2013 economic crises, when there was a slowdown in the number of new hires (see Figure 1). Finally, the wage premium collapse in 2015 appears to be clearly connected with the Jobs Act, which reduced employment protection legislation for permanent contracts and, therefore, may have decreased their relative attractiveness for employees. At the same time, the fiscal benefits to employers for hiring workers on open-ended contracts related to the same reform should also have increased the supply of these contracts, thus increasing the compensation.

Table 3: The wage gap at the mean over the period of 2005–2015

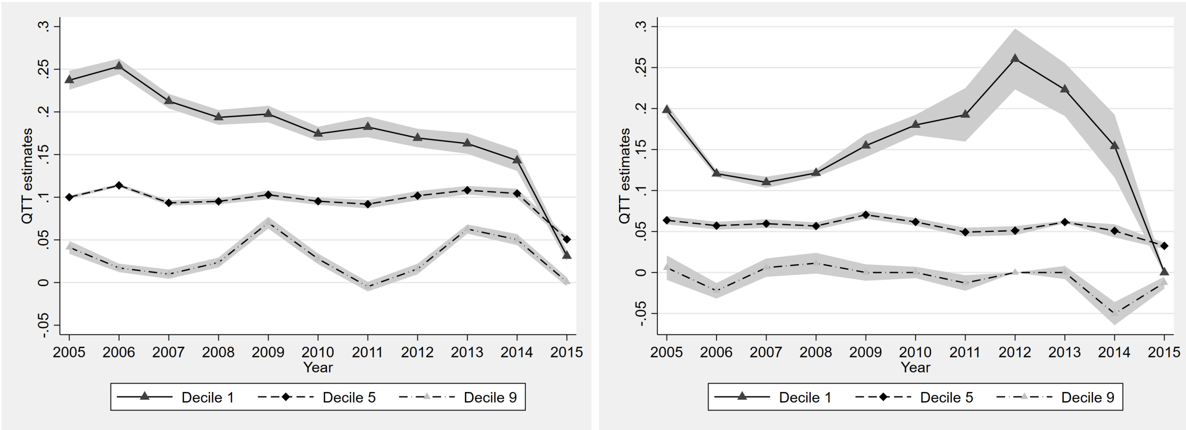
| | Any jobs (A) | | | | Full-time jobs (B) | | | |
|---|---|---|---|---|---|---|---|---|
| Year | ATT | Std.Err. | Observations | N individuals | ATT | Std.Err. | Observations | N individuals |
| 2005 | 13.06 | (0.20) | 253,211 | 202,442 | 10.27 | (0.28) | 196,596 | 158,467 |
| 2006 | 13.13 | (0.17) | 273,377 | 217,818 | 7.92 | (0.30) | 211,086 | 169,750 |
| 2007 | 11.22 | (0.18) | 323,125 | 257,118 | 8.10 | (0.25) | 242,266 | 195,214 |
| 2008 | 11.61 | (0.17) | 302,983 | 245,273 | 7.53 | (0.23) | 221,001 | 182,354 |
| 2009 | 13.01 | (0.20) | 264,740 | 217,559 | 9.92 | (0.28) | 187,793 | 157,419 |
| 2010 | 11.42 | (0.18) | 286,101 | 235,022 | 9.15 | (0.20) | 204,116 | 171,137 |
| 2011 | 10.47 | (0.23) | 270,048 | 224,212 | 9.02 | (0.25) | 191,536 | 161,446 |
| 2012 | 11.13 | (0.23) | 271,176 | 222,238 | 9.43 | (0.36) | 183,248 | 153,469 |
| 2013 | 12.35 | (0.20) | 240,137 | 197,280 | 11.43 | (0.34) | 154,178 | 130,245 |
| 2014 | 11.36 | (0.21) | 241,406 | 197,860 | 8.48 | (0.44) | 152,523 | 128,513 |
| 2015 | 4.26 | (0.14) | 286,440 | 234,177 | 5.09 | (0.20) | 184,026 | 154,440 |
| All | 11.27 | (0.14) | 3,012,744 | 1,152,057 | 9.51 | (0.09) | 2,128,369 | 894,349 |

*Notes:* Estimates of ATT are based on the standard IPW method. Panel (A) retains all jobs, while panel (B) keeps full-time jobs only. For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Standard errors (in parentheses) are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

The wage premium trend turns out to be quite heterogeneous among workers in different daily wage quantiles. Figure 6 shows how QTT estimates change over time for three quantiles: the first decile, the median, and the ninth decile. The results highlight that changes over time primarily involve low-paid workers, given that the estimated wage premium at the first decile moves from 0.24 to 0.14 during the 2005–2014 period, and even to 0.03 in 2015 (Figure 6a). In contrast, QTT estimates are overall stable around 0.10 for workers at the median (except for the downturn in 2015) and slightly above zero but time-variant for those in the highest decile. In particular, in the last case, two important increases are reported in 2009 and 2013, suggesting that changes related to the highest decile of daily wage may have mainly led to the peaks of ATT values observed in the same years.

Interestingly, when taking into account full-time jobs only, the trend of the wage premium for temporary workers at the lowest decile does not decrease anymore but rather is hump shaped: it decreases from 2005 to 2007; then it increases from 0.12 in 2008 to 0.26 in 2012; and finally, it collapses to 0 in 2015 (Figure 6b). In addition, considering only full-time jobs mostly shifts the wage premium downward both at the median and the $9^{\text{th}}$ decile. In the highest decile, the premium for temporary jobs becomes insignificant overall at the 5 percent level, except for a few years where it is small but negative.

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




(a) All jobs

(b) Full-time jobs

*Notes:* Estimates of QTT are based on the standard IPW method. As for estimates involving only full-time jobs, conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

## 7.3 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 of the daily wage at hiring as well. Specifically, temporary contracts can be subdivided into fixed-term contracts (or *'Contratti a tempo determinato'*), agency, on call, and other temporary or subsidized job contracts.[17] As a further treatment of temporary employment, we also analyse apprenticeship contracts, despite them not formally being included among temporary contracts in Italy. In order to estimate the ATT and QTT for each type of temporary contract, we now define four new treatment variables: fixed-term workers, agency workers, on-call workers, and apprentices.[18]

Table 4 shows that not all temporary contracts involve a daily wage premium. The most common contracts among temporary workers (i.e. fixed-term contracts) show an ATT equals to +9.1%, and workers hired with an agency contract benefit from an ever greater premium at the mean (+19.6%). In contrast, on-call workers receive a significant wage penalty (−5.7%) with respect to open-ended ones with similar demographic characteristics and occupational histories, while apprentices do not show any significant daily remuneration gap at the mean. When only full-time jobs are considered (right panel of Table 4), the wage premium reported by workers hired on a fixed term or an agency contract is lower than before, whereas the wage

[17] Given that the latter represents a residual category, it contains multiple contracts that are very different in terms of features and time application (some of them are effective in specific years only). For this reason, we decided not to estimate ATT and QTT for this type of temporary contract.

[18] Since according to national regulations apprenticeship contracts apply only to individuals less than 30 years of age, we restrict the sample to this subgroup.

penalty suffered by apprentices is much higher (−15%) as 77% of apprentices work full-time (compared to 67% of fixed-term contract workers and 63% of the re-weighted open-ended contracts).

Table 4: The wage gap at the mean by temporary contract

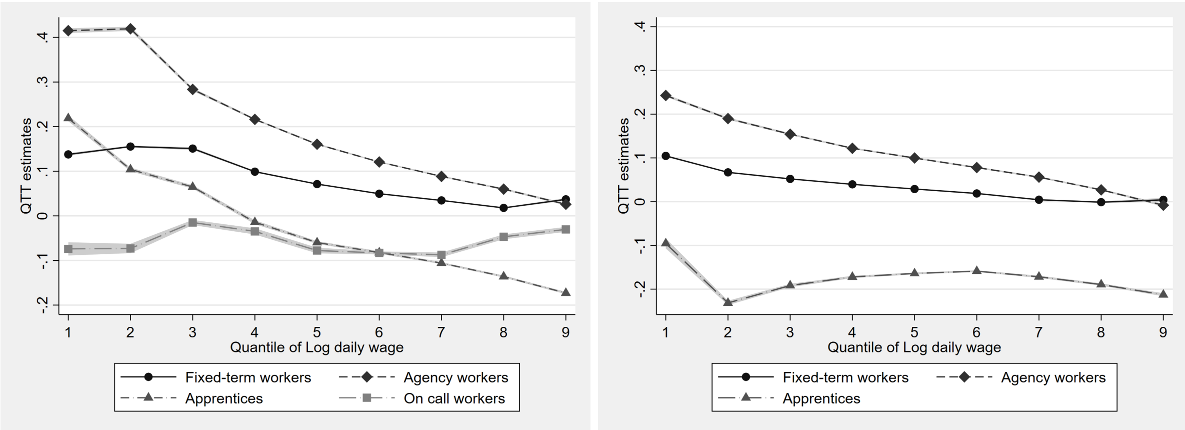
| | Any jobs (A) | | | | Full-time jobs (B) | | |
|---|---|---|---|---|---|---|---|
| | Fixed-term (1) | Agency (2) | On call (3) | Apprentice-ship (4) | Fixed-term (1) | Agency (2) | Apprentice-ship (4) |
| ATT | 9.13 | 19.62 | -5.73 | -0.05 | 7.17 | 14.68 | -14.71 |
| Std.Err. | (0.05) | (0.10) | (0.21) | (0.12) | (0.07) | (0.09) | (0.15) |
| Observations | 2,419,564 | 1,551,004 | 1,235,399 | 539,584 | 1,654,047 | 1,093,589 | 378,912 |
| N individuals | 1,062,306 | 827,606 | 737,552 | 338,748 | 793,964 | 617,590 | 254,240 |

*Notes:* Estimates of ATT are based on the standard IPW method. Panel (A) retains all jobs, while panel (B) keeps full-time jobs only. In each column, we retain only the treated individuals hired in a specific temporary contract: fixed-term (1), agency work (2), on call (3), and apprenticeship (4). For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. On-call jobs are not retained for full-time employment as no information on their working time is provided. Standard errors (in parentheses) are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

Estimates for the wage gap across the daily wage distribution by temporary contract reported in Figure 7 overall confirm what can be seen in Figure 5a (i.e. a decreasing wage premium across the distribution), except for on-call workers. This type of job contract indeed suffers a wage penalty fluctuating around −6% across the entire distribution. This gap may be explained by the few hours worked by this type of contract or the lower bargaining power of these workers. Similarly to other temporary contracts, apprentices also show a decreasing wage gap along the distribution with respect to open-ended workers, which however results in a null difference at the mean.

The picture changes considerably for several contracts when we refer to full-time jobs only. While fixed-term and agency workers keep the same shape overall along the distribution (becoming statistically insignificant at the $8^{th}$ and $9^{th}$ decile, respectively), that of apprentices totally changes. In fact, Figure 7b shows that the wage gap between apprentices and open-ended workers is now stable and negative across the entire distribution. This wage penalty is likely related to the wage regulation of the apprenticeship and the fact that this type of worker receives part of her total employee income as training (Albanese et al., 2019).

Figure 7: The wage gap across the wage distribution by temporary contract




(a) All jobs

(b) Full-time jobs

*Notes:* Estimates of QTT are based on the standard IPW method. On-call jobs are not retained for full-time employment as no information on their working hours is provided. For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

## 7.4 Differences between Economic Sectors

Along with the individual characteristics of employees, the economic sector in which they work may affect both the extent and direction of the observed wage gap due to different minimum wage levels defined by collective bargaining agreements. Starting from the National Institute of Statistics' classification of economic activity, we distinguish Italian workers in the following sectors: manufacturing, retail or trade, business services (e.g. real estate, financial, insurance, or consulting activities), other services (e.g. IT activities, health, or other social services), transport, tourism, and construction.

As a contextual preference for specific types of temporary contracts exists between economic sectors in Italy (e.g. on-call contracts are adopted more in the tourism sector) and considering important differences in the observed wage gap by type of contract (highlighted in Section 7.3), this heterogeneity may represent an issue for the analysis we want to develop here. To isolate the influence of sector on the type of temporary contract, we retain only the 'standard' fixed-term contracts. Consequently, estimates presented in this section consider as treatment variable the one defined in the previous section (Section 7.3) for this type of temporary contract, namely, fixed-term contracts.

Table 5 shows that the wage premium significantly differs between economic sectors. The premium is high in the 'other services' sector (25%), followed by the transport and tourism sectors (16% and 12%, respectively), whereas it is much lower in the manufacturing, retail, and business services sectors, ranging between 3% and 6%. The wage gap between temporary and open-ended contracts is almost zero in the construction sector.

Estimates at the mean narrow when we consider full-time jobs only. It is particularly important to notice two changes in this case. First, there is no wage premium for full-time fixed-term workers in the retail, construction, and manufacturing sectors. Second, the tourism and business services sectors show a raise in wage premium, especially due to its increase among low-paid workers (see Figure A.3d in Appendix A).

Table 5: The wage gap at the mean by economic sector

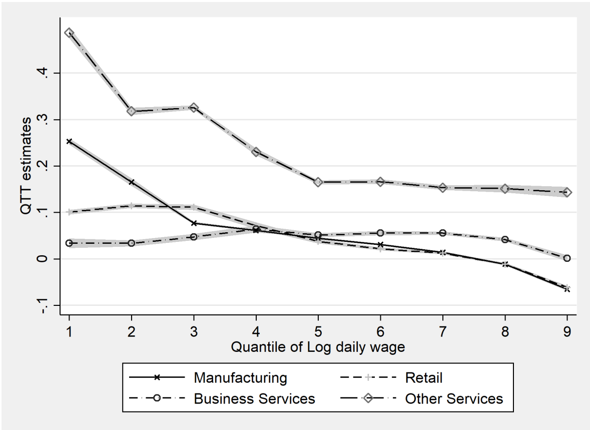
| | All (1) | Manufact-uring (2) | Retail (3) | Business Services(4) | Other Services(5) | Transport (6) | Tourism (7) | Construction (8) |
|---|---|---|---|---|---|---|---|---|
| **Any jobs (A)** | | | | | | | | |
| ATT | 9.13 | 6.09 | 3.86 | 3.50 | 25.44 | 15.76 | 11.96 | -0.62 |
| Std.Err. | (0.05) | (0.10) | (0.14) | (0.17) | (0.27) | (0.27) | (0.15) | (0.07) |
| Observations | 2,419,564 | 474,323 | 297,462 | 338,394 | 376,645 | 196,353 | 345,534 | 370,726 |
| N individuals | 1,062,306 | 296,449 | 201,996 | 219,668 | 231,173 | 101,900 | 176,194 | 165,214 |
| **Full-time jobs (B)** | | | | | | | | |
| ATT | 7.17 | 0.25 | -0.68 | 8.26 | 15.15 | 11.43 | 13.85 | -1.20 |
| Std.Err. | (0.07) | (0.09) | (0.12) | (0.18) | (0.56) | (0.33) | (0.17) | (0.09) |
| Observations | 1,654,047 | 391,115 | 170,925 | 174,065 | 242,377 | 155,676 | 175,697 | 329,979 |
| N individuals | 793,964 | 251,153 | 125,074 | 127,869 | 154,119 | 80,274 | 95,623 | 148,680 |

*Notes:* Estimates of ATT are based on the standard IPW method considering in the treated group one type of temporary contract, namely, the fixed-term contracts (or *'contratti a tempo determinato'*). Panel (A) retains all jobs, while panel (B) keeps full-time jobs only. In each column, we retain only individuals hired in a specific sector: all individuals (1), manufacturing (2), retail (3), business services (4), other services (5), transport (6), tourism (7), and construction (8). For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Standard errors (in parentheses) are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

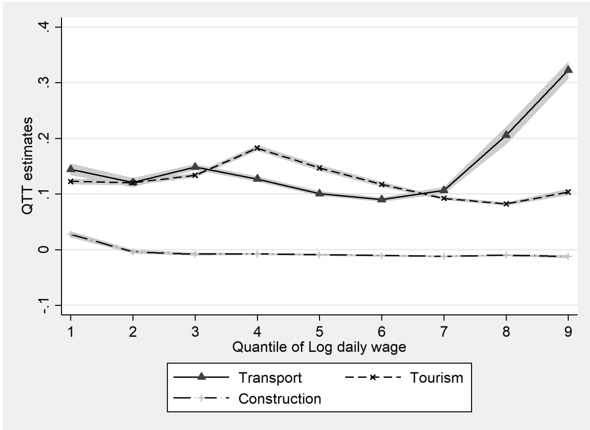
Fixed-term workers clearly report a decreasing premium along the daily wage distribution in three economic sectors (i.e. manufacturing, retail, and other services), while it is pretty stable in the business services, tourism, and construction sectors, and even increasing in the transport sector from the seventh decile onwards (up to 0.32) (Figure 8). Interestingly, fixed-term workers in the construction sector show estimates always close to 0. Conversely, those working in the manufacturing and retail sectors receive a premium, except for the last two deciles of daily remuneration (the business services sector shows an insignificant QTT in the ninth decile).[19]

[19] When we focus on full-time jobs only, fixed-term workers in the manufacturing and retail sectors start to report a daily wage penalty earlier with respect to open-ended workers: from the sixth and fifth deciles, respectively (Figure A.3d). Furthermore, low-paid workers in the other services sector show a much smaller premium for temporary jobs, whereas it is much greater among those in the tourism sector (Figure A.3e).

Figure 8: Estimation of QTTs by economic sector




(a) Estimates by sector (1)




(b) Estimates by sector (2)

*Notes:* Estimates of QTT are based on the standard IPW method, considering in the treated group one type of temporary contract, namely, the fixed-term contracts (or *'contratti a tempo determinato'*). Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

## 7.5 Robustness Tests

In this section, we implement several validation tests and sensitivity analyses on our main results (more details are available upon request).

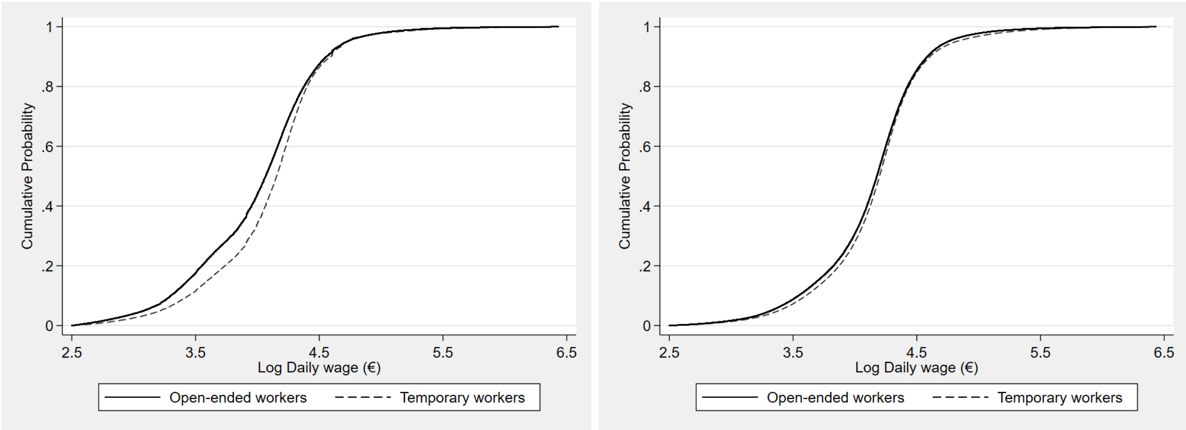
First, we assess the credibility of the CIA as proposed by Imbens and Wooldridge (2009). In particular, we test the presence of a 'placebo effect' on the daily wage in the year before hiring. In this exercise, we focus on a subset of the same individuals who were also hired in the previous year (t−1), keeping the definition of treatment as in the current year (t).[20] First, we observe that an individual entering a temporary contract in year t is much more likely to have also worked in a temporary job in year t−1 (+39.0 percentage points, pp). However, after implementing the IPW estimator using predetermined $Xs$, the difference becomes negligible (−0.9 pp). This first placebo test suggests that after conditioning on the $X$s, the assignment rule of year t does not determine treatment in year t-1.

Then, we look at the difference in the outcome in year t−1. Figure 9 illustrates a comparison of cumulative distribution functions for open-ended and temporary contracts between those deriving from the main results and those from the placebo test. The difference in the daily wage mean goes from 11.3% to 3.7%, while Figure 9b also shows that the distribution of outcomes for the two groups of workers is much more similar with respect to the baseline estimates (Figure 9a).[21] Although some small bias seems to remain, results of the placebo test overall confirm that the observables used to implement the IPW estimator make the treatment assignment in year t quite randomly related to the outcome of the previous year t-1.

[20]Note that the measurement of all covariates is shifted by one year so as to keep them as predetermined.

[21]This also holds when only considering full-time jobs (Figures A.4a and A.4b in Appendix A).

Figure 9: Placebo test: cumulative distribution of daily wage logarithm—All jobs




(a) All jobs in year t

(b) All jobs in year t−1

Second, we verify whether evidence of a wage premium for temporary jobs is led by very short temporary contracts. Therefore, we replicate the main results considering only contracts that had an effective duration of at least three months. As shown in Figure A.5, point estimates are slightly larger.

Third, we include contemporary variables, which are potentially endogenous, to the benchmark analysis including all jobs. As shown in Figure A.6, the results are fairly robust, which suggests that conditioning on our rich predetermined work experience variables is enough to also capture the influence of simultaneous characteristics on the treatment.

Fourth, as the credibility of our identifying assumption depends on having a rich set of observable characteristics, we focus on a subsample of individuals with richer information about work experience. We retain only individuals who during the previous 10 years had accumulated at least 5 years of work experience (260 weeks). This selection retains about one-third of the sample or 1,090,584 jobs, of which 51% are in temporary contracts and 49% in permanent ones.[22] The ATT is reduced from 11.3 to 6.2 pp, and point estimates of the QTTs are smaller. However, as shown in Figure A.7, the qualitative picture is still similar, showing a statistically significant positive effect up to the 7$^{th}$ decile, while becoming slightly negative for the 8$^{th}$ and the 9$^{th}$ deciles. A smaller premium for individuals with greater work experience might also be explained by the larger premium estimated for disadvantaged workers.

Fifth, we run some sensitivity tests on the semi-parametric estimator: 1) we modify the trimming rule so as to remove only the treated with a propensity score above the maximum of the controls; 2) we implement a doubly robust estimator and parametrically account for control variables in a least squares weighted by the IPW weights; and 3) we implement a nearest-neighbour estimator. As shown in Table A.3, estimates are robust to the different specifications.

[22] About 38% of the excluded 1,922,160 jobs with less work experience are in permanent jobs, while the remaining 62% are in temporary ones.

Sixth, we test the sensitivity of our ATT estimates to potential failures of the CIA. Although we have a rich set of observable characteristics, we cannot exclude the possibility of other unobservable factors driving the selection of individuals into the two groups of contracts. We follow Rosenbaum (2002) and estimate the magnitude of an unobserved confounding factor, on top of our covariates, that would invalidate our results. This bias is a worst-case scenario, as the relation between this confounder and the daily salary is assumed to determine perfectly whether individuals in temporary jobs would earn a larger salary than those in matched permanent jobs.[23] We estimate that this worst-case confounding factor should have increased the odds of the treated entering into a temporary contract instead of an open-ended job by 43% (see Table B.1). Finally, we obtain more insight into the relative importance of this bias compared to our control variables. We follow DiPrete and Gangl (2004) and estimate an equivalent change in the odds of treatment by varying an observed control variable such as total work experience in the last 16 years.[24] We estimate that to increase the odds of treatment as required by the 'worst-case' confounding factor, we would need to change the difference of total weeks of experience in the last 16 years in favour of individuals in permanent contracts by 386 weeks.[25] Overall, this evidence seems to indicate the robustness of our results since to undermine our estimates, we would need a sizeable worst-case confounding factor on top of our covariates.

Finally, we verify whether the gap at the mean between the two types of contracts holds if we rely on alternative identification assumptions. In particular, we directly exploit the panel dimension of the dataset and test whether differences in means are observed for the same individuals when hired in different contracts. We therefore implement a fixed-effect estimator for the daily remuneration of individuals through a dummy indicating whether an individual was hired in a temporary job (rather than a permanent one) and parametrically control for the other explanatory variables used in the IPW estimator. As shown in Table A.3, estimates are smaller but still positive and statistically significant, which confirms the existence of a premium when the individual works in a temporary job. While the discrepancy in the point estimates might be due to the different identifying assumptions, we also have to keep in mind that they refer to different populations. Indeed, the coefficient from the fixed-effect estimator refers only to individuals hired both in a permanent and a temporary job during our period of observation, whereas the gap estimated by the IPW estimator refers to all temporary contracts. Since about 71% of individuals are observed in only one of the two contracts, point estimates clearly refer to different sub-populations.[26]

[23] As in Rosenbaum (2002), we implement this test on the nearest neighbour matching estimator, which, as shown in Table A.3, estimates very similar results to the IPW estimator.

[24] We estimate through a logit model the influence of total work experience in the last 16 years on the probability of assignment into treatment. To determine the overall influence of this variable, we only control for age, gender, year, and region of residence and let the other experience-related variables vary with it. The equivalent bias is obtained by dividing the log of the required bias by the coefficient of the independent variable.

[25] As shown in Table B.2, for full-time jobs the required bias is 35%, which can be obtained by increasing the working weeks in favour of permanent contracts by 320 weeks.

[26] Contract transformations are, in principle, not included in our inflow sample of new hires, which may partly

## 7.6 Discussion

Despite being in line with the economic theory, our results differ from previous studies, which showed a large penalty for temporary jobs (Section 2). This is likely the result of several methodological advancements we were able to carry out thanks to the administrative data.

First, the rich set of covariates, which includes labour market history over the last 16 years, seems to play an important role in controlling for selection at the start of the contract. Indeed, the wage gap observed in the raw data is reverted once we implement the IPW estimator. Second, in contrast to the previous literature, our administrative data allows us to rely on an inflow sample and focus on the wage at entry, which is not affected by dynamic selection (see Section 6). To check the importance of this aspect, we re-estimate the ATT by relying on a stock of contracts existing at a specific date of each year. We alternatively select the existing jobs on the 1st of April or 1st of October and end up with about 8 million jobs in each analysis. We then estimate the average gap for the daily salary earned during that part of the spell. Results in Table A.4 in Appendix A show a large penalty for the stock of temporary workers: about 19% for all jobs and 16% for full-time jobs only.[27] Finally, we provide further estimates on the wage gap dynamics over time for our inflow sample, restricting the maximum year of entry to 2010. Therefore, we estimate the average wage gap at 1, 3, and 5 calendar-year distances since entry in our inflow sample for the individuals remaining in the same type of contract or also firm. The IPW estimates are shown in Table A.5 in Appendix A and indicate that the wage premium for temporary jobs is reabsorbed the later we measure the outcome. All these findings go in the direction of what was shown in the previous empirical literature.

However, we have to be careful in interpreting these results as evidence of a decreasing premium over the job duration. Indeed, as shown in Table A.5, there is a high level of attrition, which is larger for individuals in temporary contracts due to the transient nature of their job.[28] This attrition can affect the composition of the two groups and invalidate the interpretation of the estimates as the effect of the temporary contracts rather than just the result of dynamic sorting. Indeed, due to the positive selection into permanent positions, we may expect that the better temporary workers will move to permanent positions over time, whereas the individuals with a lower potential wage may be 'trapped' in temporary jobs. Conversely, the 'worse' permanent workers may drop out of the pool of permanent jobs. This dynamic selection could bias the estimates in the direction of a wage penalty for temporary contracts. In our analysis, we isolated the bias coming from dynamic selection by focusing on the entry wage. The higher internal validity of our estimates comes, however, at the cost of restricting the insights of our

explain why only 29% of individuals are observed in both contract groups.

[27] For this exercise, we only use a subset of control variables (age, gender, year of hiring, and region) since experience variables are endogenous as the job might have already started in the previous calendar years. If we use this subset of covariates on our inflow sample, we find an average premium of 5% (–2% for full-time jobs).

[28] If we look at 1, 3, and 5 calendar-year distances, we see that the share of entries remaining in a permanent (temporary) contract goes from 84% (71%) to 66% (40%) and then to 58% (30%). The retention is even lower if we focus on the same firm and type of contract: 70% (45%), 36% (9%), and 24% (4%).

analysis only to the beginning of the job spell.

# 8 Conclusions

From 1980s onwards, income inequality dramatically rose throughout developed countries (Roine and Waldenström, 2015), and several studies have highlighted the role of wage inequality in explaining this trend (Gradín, 2016; Felbermayr et al., 2018; Devicienti et al., 2019). In the same period, temporary contracts gained considerable importance in the labour markets of several countries. In this paper, we try to understand whether temporary contracts may have contributed to increases in wage inequality in dual labour markets.

In contrast to most of the previous literature, we rely on administrative data to study wage at hiring. A large inflow sample of more than 3 million new hires during the period of 2005–2015 was drawn from Italian social security registers (LoSai INPS), covering 6.5% of jobs in the private sector. We consider Italy an interesting case study because firms rely heavily on temporary contracts, such that permanent jobs depict a minority among new hires (Ministry of Labor and Social Policies, 2019). We compare the gross daily wage between the two groups of contracts at the mean and the deciles of the distribution. Differently from the previous literature, which mostly relied on stock samples of workers, we focus on the daily wage earned at entry in an inflow sample to avoid problems of selective attrition for the two contracts along the job spell. To take into account compositional differences between hires in the two types of contracts, we implement an inverse probability weighting estimator (IPW), as proposed by Firpo (2007). Thanks to the longitudinal dimension of the administrative registries, we can control for individual characteristics and occupational history over the last 16 years, including multiple lagged contracts and wages.

The descriptive evidence is in line with the previous literature, showing a gap in the mean and along the distribution of full-time jobs (e.g. Blanchard and Landier, 2002; Booth et al., 2002; Boeri, 2011; Gebel, 2010; Kahn, 2016. However, different conclusions are reached after we correct for compositional differences and positive selection into permanent contracts. Our results highlight the existence of a premium in favour of temporary contracts at the mean (+11.3%) and over the full distribution of daily remuneration, with a stronger effect for the lowest deciles. The daily wage premium in favour of temporary workers diminishes but remains important (+9.5%) when we only consider full-time jobs. The results appear to be robust to multiple sensitivity analyses such as the relatively large confounder that would be required to invalidate the results (Rosenbaum, 2002), placebo tests on lagged outcomes (Imbens and Wooldridge, 2009), and controlling for an individual fixed effect.

Despite our evidence contrasts with the previous empirical literature, which tends to find a wage gap for stocks of existing jobs, it is in line with Rosen's (1986) economic theory of equalizing differences. This is confirmed by the fact that the wage premium at entry is greater when permanent jobs are more valuable, such as for 'marginalised' categories (e.g. females, youths, low-paid workers) or in years of economic crisis. Higher entry wages for temporary contracts might also be explained by the different wage increase expectations for the two groups. While the daily wage of a permanent worker is expected to increase throughout the employment relationship due to seniority rules, the salary of a temporary worker has less room for expansion given the shorter expected job duration.

Our estimates show a strong decrease in 2015, when an important labour market reform was implemented: the Jobs Act. This reform made the dismissal regulations of open-ended contracts much more flexible and introduced a large subsidy for hiring permanent workers. These changes made open-ended contracts relatively less (more) attractive for employees (employers) and, therefore, might have reduced the wage premium for temporary contracts. Considering, however, that fiscal benefits stopped in 2017 and the dismissal regulations were reverted to the previous legislation by the Constitutional Court in 2018, it is not clear whether this effect will persist over time.

In conclusion, our study highlights that temporary contracts contribute to increasing wage inequality in a dual labour market. However, in contrast to other forms of wage inequality, this wage premium may be seen as a positive form of inequality since it compensates workers for the shorter expected job duration and future unemployment spells. Although wage inequalities at an equal level of competence represent a concern for society, a premium for temporary contracts is actually a welcome compensation for the flexible labour supplied by workers to their employers. Nonetheless, we cannot determine whether the estimated wage premium is 'optimal' for the workers. Indeed, even if we found that full-time temporary workers gain, on average, a 9.5% higher daily wage at the moment of hiring with respect to open-ended ones, we cannot state whether this adequately covers their unfavourable employment conditions after taking into account the workers' preferences and risk aversion. It is difficult to provide a policy recommendation to effectively deal with the labour market dualism in the absence of this information. For this reason, we believe that the estimation of the optimal wage premium for temporary jobs should be the objective of future research on this topic.

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# Appendix

## A Tables and Figures

Table A.1: Control variables and balancing tests

| Years (lags before hiring) | Variable | Weights | Temporary jobs | Open-ended jobs | %bias | %bias reduction |
|---|---|---|---|---|---|---|
| 16-11 | No work experience | RAW | 0.61 | 0.55 | 12.3 | |
| | | IPW | 0.61 | 0.62 | -1.1 | 91.3 |
| 16-11 | Total working weeks | RAW | 56.63 | 77.55 | -19.6 | |
| | | IPW | 56.65 | 55.34 | 1.2 | 93.8 |
| 16-11 | Daily salary | RAW | 25.80 | 32.04 | -15.3 | |
| | | IPW | 25.50 | 24.60 | 2.2 | 85.6 |
| 16-11 | Daily salary (quadratic) | RAW | 2,217 | 2,811 | -7.0 | |
| | | IPW | 2,104 | 1,951 | 1.8 | 74.1 |
| 16-11 | Daily salary (cubic) | RAW | 320,000 | 340,000 | -0.4 | |
| | | IPW | 270,000 | 240,000 | 0.8 | -109.0 |
| 16-11 | Daily salary (INPS weeks adjustment) | RAW | 27.21 | 33.59 | -15.1 | |
| | | IPW | 26.90 | 25.99 | 2.2 | 85.8 |
| 16-11 | Daily salary (INPS weeks adjustment) (quadratic) | RAW | 2,411 | 3,013 | -6.8 | |
| | | IPW | 2,294 | 2,141 | 1.7 | 74.5 |
| 16-11 | Daily salary (INPS weeks adjustment) (cubic) | RAW | 360,000 | 370,000 | -0.3 | |
| | | IPW | 300,000 | 270,000 | 0.8 | -140.0 |
| 16-11 | % as blue collar | RAW | 0.26 | 0.29 | -7.6 | |
| | | IPW | 0.26 | 0.26 | -0.7 | 91.0 |
| 10-6 | Total working weeks | RAW | 74.45 | 96.05 | -21.8 | |
| | | IPW | 74.46 | 73.32 | 1.1 | 94.7 |
| 10-6 | Daily salary | RAW | 38.02 | 44.61 | -14.7 | |
| | | IPW | 37.69 | 36.49 | 2.7 | 81.8 |
| 10-6 | Daily salary (quadratic) | RAW | 3,264 | 4,195 | -8.6 | |
| | | IPW | 3,161 | 2,890 | 2.5 | 70.9 |
| 10-6 | Daily salary (cubic) | RAW | 470,000 | 590,000 | -2.7 | |
| | | IPW | 420,000 | 350,000 | 1.4 | 48.0 |
| 10-6 | Daily salary (INPS weeks adjustment) | RAW | 41.37 | 47.93 | -14.0 | |
| | | IPW | 41.01 | 39.95 | 2.3 | 83.9 |
| 10-6 | Daily salary (INPS weeks adjustment) (quadratic) | RAW | 3,745 | 4,643 | -7.8 | |
| | | IPW | 3,637 | 3,391 | 2.1 | 72.6 |
| 10-6 | Daily salary (INPS weeks adjustment) (cubic) | RAW | 540,000 | 660,000 | -2.3 | |
| | | IPW | 500,000 | 440,000 | 1.2 | 48.0 |
| 10-6 | Years in part-time | RAW | 0.40 | 0.41 | -1.8 | |
| | | IPW | 0.39 | 0.39 | -0.2 | 88.4 |
| 10-6 | % as blue collar | RAW | 0.38 | 0.41 | -6.8 | |
| | | IPW | 0.38 | 0.39 | -2.4 | 64.6 |
| 10-6 | % as apprentice | RAW | 0.06 | 0.05 | 7.1 | |
| | | IPW | 0.06 | 0.07 | -2.4 | 66.4 |
| 10-6 | Sector: % secondary primary construction | RAW | 0.23 | 0.28 | -10.3 | |
| | | IPW | 0.24 | 0.25 | -2.3 | 77.9 |
| 10-6 | Sector: % trade tourism | RAW | 0.15 | 0.13 | 4.5 | |
| | | IPW | 0.15 | 0.15 | -2.6 | 43.7 |
| 10-6 | Sector: % services | RAW | 0.19 | 0.21 | -3.3 | |
| | | IPW | 0.19 | 0.17 | 5.2 | -54.9 |
| 5-2 | Total working weeks | RAW | 86.56 | 106.76 | -25.7 | |
| | | IPW | 86.24 | 85.90 | 0.4 | 98.3 |
| 5-2 | Daily salary | RAW | 53.16 | 58.16 | -11.1 | |
| | | IPW | 52.74 | 51.43 | 2.9 | 73.9 |
| 5-2 | Daily salary (quadratic) | RAW | 4,520 | 5,757 | -9.7 | |
| | | IPW | 4,421 | 4,036 | 3.0 | 68.9 |
| 5-2 | Daily salary (cubic) | RAW | 600,000 | 900,000 | -5.2 | |
| | | IPW | 560,000 | 470,000 | 1.7 | 67.0 |
| 5-2 | Daily salary (INPS weeks adjustment) | RAW | 60.52 | 64.62 | -8.6 | |
| | | IPW | 60.12 | 59.69 | 0.9 | 89.5 |
| 5-2 | Daily salary (INPS weeks adjustment) (quadratic) | RAW | 5,668 | 6,675 | -7.2 | |
| | | IPW | 5,566 | 5,357 | 1.5 | 79.3 |
| 5-2 | Daily salary (INPS weeks adjustment) (cubic) | RAW | 800,000 | 1,000,000 | -3.9 | |
| | | IPW | 760,000 | 710,000 | 0.8 | 78.0 |
| 5-2 | Years in part-time | RAW | 0.66 | 0.63 | 2.4 | |
| | | IPW | 0.66 | 0.70 | -3.3 | -37.6 |
| 5-2 | % as blue collar | RAW | 0.53 | 0.55 | -5.5 | |
| | | IPW | 0.53 | 0.56 | -6.2 | -12.7 |
| 5-2 | % as apprentice | RAW | 0.06 | 0.04 | 9.8 | |
| | | IPW | 0.06 | 0.06 | 0.1 | 98.9 |
| 5-2 | Contract: % temporary | RAW | 0.42 | 0.19 | 59.0 | |
| | | IPW | 0.41 | 0.43 | -3.7 | 93.7 |
| 5-2 | Contract: % seasonal | RAW | 0.02 | 0.01 | 13.1 | |
| | | IPW | 0.02 | 0.02 | -0.8 | 93.6 |
| 5-2 | Firm size: % in 1-15 | RAW | 0.36 | 0.37 | -0.5 | |
| | | IPW | 0.36 | 0.39 | -5.5 - | 1086.8 |
| 5-2 | Firm size: % in 16-200 | RAW | 0.25 | 0.25 | 0.5 | |
| | | IPW | 0.25 | 0.26 | -2.0 | -349.6 |
| 5-2 | Firm size: % in 201+ | RAW | 0.18 | 0.18 | -0.8 | |
| | | IPW | 0.18 | 0.16 | 6.7 | -750.8 |
| 5-2 | Sector: % secondary primary construction | RAW | 0.27 | 0.34 | -15.5 | |
| | | IPW | 0.27 | 0.29 | -3.0 | 80.6 |
| 5-2 | Sector: % trade tourism | RAW | 0.21 | 0.17 | 10.1 | |
| | | IPW | 0.21 | 0.23 | -4.8 | 52.6 |
| 4-2 | Total remuneration as collaborator | RAW | 920.88 | 1,321.10 | -4.6 | |
| | | IPW | 929.99 | 932.40 | 0.0 | 99.4 |
| 4-2 | Years with unemployment benefits | RAW | 0.48 | 0.24 | 30.5 | |
| | | IPW | 0.46 | 0.45 | 1.1 | 96.4 |
| 4-2 | Total days in unemployment benefits | RAW | 54.34 | 28.23 | 26.0 | |
| | | IPW | 51.76 | 50.39 | 1.4 | 94.7 |
| 4-2 | Total hours in temporary layoff (CIG) | RAW | 19.81 | 12.27 | 4.1 | |
| | | IPW | 19.78 | 20.29 | -0.3 | 93.3 |
| 1 | Total working weeks | RAW | 22.40 | 27.88 | -25.1 | |
| | | IPW | 22.26 | 22.36 | -0.5 | 98.2 |
| 1 | Daily salary | RAW | 47.39 | 52.95 | -10.4 | |
| | | IPW | 46.74 | 45.42 | 2.5 | 76.3 |
| 1 | Daily salary (quadratic) | RAW | 4,709 | 6,012 | -7.5 | |
| | | IPW | 4,551 | 4,161 | 2.2 | 70.1 |

# Table A.1: Control variables and balancing tests

| Years (lags before hiring) | Variable | Weights | Temporary jobs | Open-ended jobs | %bias | %bias reduction |
|---|---|---|---|---|---|---|
| 1 | Daily salary (cubic) | RAW | 850,000 | 1,100,000 | -3.2 | |
| | | IPW | 780,000 | 700,000 | 0.9 | 72.0 |
| 1 | Daily salary (INPS weeks adjustment) | RAW | 54.44 | 59.10 | -8.1 | |
| | | IPW | 53.79 | 53.63 | 0.3 | 96.6 |
| 1 | Daily salary (INPS weeks adjustment) (quadratic) | RAW | 6,069 | 7,093 | -5.1 | |
| | | IPW | 5,900 | 5,780 | 0.6 | 88.3 |
| 1 | Daily salary (INPS weeks adjustment) (cubic) | RAW | 1,200,000 | 1,400,000 | -2.0 | |
| | | IPW | 1,100,000 | 1,100,000 | 0.0 | 99.0 |
| 1 | Total remuneration as collaborator | RAW | 337.03 | 521.72 | -5.4 | |
| | | IPW | 342.46 | 333.05 | 0.3 | 94.9 |
| 1 | Number firms: 1 | RAW | 0.30 | 0.38 | -16.7 | |
| | | IPW | 0.30 | 0.31 | -2.1 | 87.2 |
| 1 | Number firms: 2 | RAW | 0.15 | 0.15 | 1.4 | |
| | | IPW | 0.15 | 0.15 | -0.6 | 56.7 |
| 1 | Number firms: 3 | RAW | 0.10 | 0.08 | 5.8 | |
| | | IPW | 0.10 | 0.11 | -2.1 | 63.2 |
| 1 | Number firms: 4+ | RAW | 0.11 | 0.07 | 16.1 | |
| | | IPW | 0.10 | 0.10 | 1.9 | 88.4 |
| 1 | Total hours in temporary layoff (CIG) | RAW | 11.05 | 5.73 | 5.5 | |
| | | IPW | 10.98 | 11.42 | -0.5 | 91.7 |
| 1 | Received unemployment benefits | RAW | 0.25 | 0.11 | 36.2 | |
| | | IPW | 0.24 | 0.25 | -3.5 | 90.2 |
| 1 | Total days in unemployment benefits | RAW | 30.77 | 14.03 | 28.2 | |
| | | IPW | 29.21 | 30.98 | -3.0 | 89.4 |
| 1 | Ever in part-time | RAW | 0.20 | 0.18 | 5.3 | |
| | | IPW | 0.20 | 0.22 | -6.0 | -12.7 |
| 1 (Main Job) | Working hours: proxy of % part-time | RAW | 0.59 | 0.60 | -2.2 | |
| | | IPW | 0.59 | 0.59 | -0.7 | 70.2 |
| 1 (Main Job) | Working hours: part-time | RAW | 0.14 | 0.15 | -2.2 | |
| | | IPW | 0.14 | 0.16 | -4.4 | -96.0 |
| 1 (Main Job) | Firm group: son | RAW | 0.04 | 0.05 | -5.4 | |
| | | IPW | 0.04 | 0.04 | 2.2 | 59.2 |
| 1 (Main Job) | Firm group: mother | RAW | 0.16 | 0.15 | 4.1 | |
| | | IPW | 0.16 | 0.14 | 5.5 | -33.6 |
| 1 (Main Job) | White collar | RAW | 0.19 | 0.17 | 4.7 | |
| | | IPW | 0.18 | 0.16 | 5.7 | -22.1 |
| 1 (Main Job) | Blue collar | RAW | 0.44 | 0.47 | -4.4 | |
| | | IPW | 0.45 | 0.48 | -7.3 | -67.1 |
| 1 (Main Job) | Firm size: 6-10 | RAW | 0.08 | 0.08 | 0.6 | |
| | | IPW | 0.08 | 0.08 | -3.3 | -442.2 |
| 1 (Main Job) | Firm size: 11-15 | RAW | 0.05 | 0.05 | 1.6 | |
| | | IPW | 0.05 | 0.05 | -2.4 | -56.4 |
| 1 (Main Job) | Firm size: 11-25 | RAW | 0.05 | 0.05 | 2.3 | |
| | | IPW | 0.05 | 0.06 | -1.8 | 21.8 |
| 1 (Main Job) | Firm size: 26-50 | RAW | 0.06 | 0.06 | -0.2 | |
| | | IPW | 0.06 | 0.07 | -2.1 | -1018.6 |
| 1 (Main Job) | Firm size: 51-100 | RAW | 0.05 | 0.06 | -3.2 | |
| | | IPW | 0.05 | 0.05 | -0.2 | 93.0 |
| 1 (Main Job) | Firm size: 101-200 | RAW | 0.04 | 0.05 | -6.0 | |
| | | IPW | 0.04 | 0.04 | 1.0 | 83.5 |
| 1 (Main Job) | Firm size: 201-500 | RAW | 0.04 | 0.05 | -8.3 | |
| | | IPW | 0.04 | 0.03 | 2.2 | 73.9 |
| 1 (Main Job) | Firm size: 501+ | RAW | 0.14 | 0.12 | 6.5 | |
| | | IPW | 0.14 | 0.12 | 7.8 | -20.5 |
| 1 (Main Job) | Contract: open-ended | RAW | 0.24 | 0.53 | -62.1 | |
| | | IPW | 0.24 | 0.24 | 1.8 | 97.1 |
| 1 (Main Job) | Contract: seasonal | RAW | 0.02 | 0.01 | 10.0 | |
| | | IPW | 0.02 | 0.02 | -0.8 | 92.0 |
| 1 (Main Job) | Contract: other | RAW | 0.03 | 0.02 | 7.2 | |
| | | IPW | 0.03 | 0.03 | 0.2 | 97.4 |
| 1 (Main Job) | Sector: secondary primary | RAW | 0.13 | 0.16 | -9.9 | |
| | | IPW | 0.13 | 0.13 | -1.7 | 83.0 |
| 1 (Main Job) | Sector: construction | RAW | 0.07 | 0.11 | -12.8 | |
| | | IPW | 0.07 | 0.09 | -4.0 | 68.8 |
| 1 (Main Job) | Sector: retail | RAW | 0.07 | 0.07 | -2.8 | |
| | | IPW | 0.07 | 0.07 | -2.1 | 25.2 |
| 1 (Main Job) | Sector: tourism | RAW | 0.10 | 0.06 | 13.0 | |
| | | IPW | 0.10 | 0.11 | -5.0 | 61.9 |
| 1 (Main Job) | Sector: transport | RAW | 0.04 | 0.06 | -7.9 | |
| | | IPW | 0.04 | 0.04 | 0.0 | 99.8 |
| 1 (Main Job) | Sector: education or services | RAW | 0.13 | 0.08 | 18.3 | |
| | | IPW | 0.12 | 0.11 | 5.2 | 71.5 |
| 0 and 1 | Job-to-job: employed 60 days before | RAW | 0.38 | 0.55 | -33.9 | |
| | | IPW | 0.39 | 0.39 | 0.4 | 98.8 |
| 0 | Year of hiring: 2005 | RAW | 0.08 | 0.09 | -6.7 | |
| | | IPW | 0.08 | 0.07 | 0.7 | 88.8 |
| 0 | Year of hiring: 2006 | RAW | 0.09 | 0.10 | -4.7 | |
| | | IPW | 0.09 | 0.08 | 0.6 | 87.5 |
| 0 | Year of hiring: 2007 | RAW | 0.10 | 0.12 | -5.3 | |
| | | IPW | 0.10 | 0.10 | 0.5 | 91.0 |
| 0 | Year of hiring: 2008 | RAW | 0.10 | 0.11 | -3.7 | |
| | | IPW | 0.10 | 0.09 | 0.7 | 80.8 |
| 0 | Year of hiring: 2009 | RAW | 0.09 | 0.09 | 1.4 | |
| | | IPW | 0.09 | 0.09 | 0.5 | 65.7 |
| 0 | Year of hiring: 2010 | RAW | 0.10 | 0.09 | 4.4 | |
| | | IPW | 0.10 | 0.10 | 0.5 | 87.7 |
| 0 | Year of hiring: 2011 | RAW | 0.10 | 0.08 | 5.0 | |
| | | IPW | 0.09 | 0.09 | 0.7 | 86.7 |
| 0 | Year of hiring: 2012 | RAW | 0.10 | 0.08 | 5.5 | |
| | | IPW | 0.10 | 0.09 | 1.0 | 82.2 |
| 0 | Year of hiring: 2013 | RAW | 0.08 | 0.07 | 4.6 | |
| | | IPW | 0.08 | 0.09 | -0.8 | 81.4 |
| 0 | Year of hiring: 2014 | RAW | 0.09 | 0.07 | 6.1 | |
| | | IPW | 0.09 | 0.09 | 0.2 | 97.5 |
| 0 | Year of hiring: 2015 | RAW | 0.09 | 0.10 | -5.1 | |
| | | IPW | 0.09 | 0.10 | -4.5 | 13.0 |
| 0 | Women | RAW | 0.43 | 0.33 | 20.0 | |
| | | IPW | 0.42 | 0.42 | 0.5 | 97.5 |
| 0 | Age | RAW | 35.67 | 37.91 | -21.2 | |
| | | IPW | 35.65 | 35.28 | 3.4 | 83.8 |
| 2015 | Region of residence: Molise/Abruzzo | RAW | 0.03 | 0.03 | 0.7 | |
| | | IPW | 0.03 | 0.03 | -0.7 | 5.2 |
| 2015 | Region of residence: Basilicata | RAW | 0.03 | 0.04 | -3.2 | |

## Table A.1: Control variables and balancing tests

| Years (lags before hiring) | Variable | Weights | Temporary jobs | Open-ended jobs | %bias | %bias reduction |
|---|---|---|---|---|---|---|
| | | IPW | 0.03 | 0.03 | 1.1 | 63.8 |
| 2015 | Region of residence: Campania | RAW | 0.07 | 0.10 | -11.5 | |
| | | IPW | 0.07 | 0.07 | 1.8 | 84.0 |
| 2015 | Region of residence: Emilia Romagna | RAW | 0.09 | 0.07 | 7.2 | |
| | | IPW | 0.09 | 0.09 | -0.8 | 88.8 |
| 2015 | Region of residence: Friuli Venezia Giulia | RAW | 0.02 | 0.02 | 5.1 | |
| | | IPW | 0.02 | 0.02 | -1.1 | 78.2 |
| 2015 | Region of residence: Lazio | RAW | 0.09 | 0.10 | -2.6 | |
| | | IPW | 0.09 | 0.08 | 2.4 | 9.0 |
| 2015 | Region of residence: Liguria | RAW | 0.03 | 0.02 | 3.5 | |
| | | IPW | 0.03 | 0.03 | -0.9 | 74.9 |
| 2015 | Region of residence: Lombardia | RAW | 0.17 | 0.20 | -7.6 | |
| | | IPW | 0.17 | 0.17 | 0.4 | 95.4 |
| 2015 | Region of residence: Marche | RAW | 0.03 | 0.02 | 6.9 | |
| | | IPW | 0.03 | 0.03 | -1.8 | 73.5 |
| 2015 | Region of residence: Puglia | RAW | 0.06 | 0.06 | -0.7 | |
| | | IPW | 0.06 | 0.06 | -0.6 | 6.3 |
| 2015 | Region of residence: Sardinia | RAW | 0.03 | 0.02 | 6.4 | |
| | | IPW | 0.03 | 0.03 | -1.9 | 70.1 |
| 2015 | Region of residence: Sicily | RAW | 0.06 | 0.08 | -7.1 | |
| | | IPW | 0.06 | 0.06 | 1.9 | 73.5 |
| 2015 | Region of residence: Tuscany | RAW | 0.07 | 0.06 | 4.0 | |
| | | IPW | 0.07 | 0.07 | -0.4 | 90.9 |
| 2015 | Region of residence: Trentino Alto Adige | RAW | 0.02 | 0.02 | 4.5 | |
| | | IPW | 0.02 | 0.02 | -1.5 | 66.6 |
| 2015 | Umbria | RAW | 0.01 | 0.01 | 2.4 | |
| | | IPW | 0.01 | 0.01 | -1.1 | 56.2 |
| 2015 | Region of residence: Veneto | RAW | 0.09 | 0.08 | 3.3 | |
| | | IPW | 0.09 | 0.09 | -0.6 | 81.3 |
| | | | Pseudo R2 of logit model | Mean bias | Median bias | |
| | All variables | RAW | 0.148 | 9.2 | 6.1 | |
| | | IPW | 0.007 | 2.0 | 1.6 | |

*Notes:* Variables with missing information (e.g. daily salary when not working) have a value of zero

## Table A.2: Summary of the distribution of the absolute standardized bias

BEFORE IPW

| | Percentiles | Smallest | | |
|---|---|---|---|---|
| 1% | 0.3198 | 0.1852 | | |
| 5% | 0.6152 | 0.3198 | | |
| 10% | 1.5608 | 0.3916 | N covariates | 112 |
| 25% | 3.4086 | 0.4528 | | |
| 50% | 6.0501 | | Mean | 9.1607 |
| | | Largest | Std. Dev. | 10.1250 |
| 75% | 10.3960 | 33.8670 | | |
| 90% | 19.9954 | 36.2333 | Variance | 102.5152 |
| 95% | 28.1904 | 59.0302 | Skewness | 2.8919 |
| 99% | 59.0302 | 62.0879 | Kurtosis | 13.5746 |

AFTER IPW

| | Percentiles | Smallest | | |
|---|---|---|---|---|
| 1% | 0.0139 | 0.0107 | | |
| 5% | 0.1866 | 0.0139 | | |
| 10% | 0.3531 | 0.0277 | N covariates | 112 |
| 25% | 0.6700 | 0.1116 | | |
| 50% | 1.6022 | | Mean | 1.9587 |
| | | Largest | Std. Dev. | 1.7254 |
| 75% | 2.4604 | 6.2282 | | |
| 90% | 4.7805 | 6.6688 | Variance | 2.9771 |
| 95% | 5.7469 | 7.3247 | Skewness | 1.3711 |
| 99% | 7.3247 | 7.7756 | Kurtosis | 4.4165 |

Table A.3: Sensitivity analysis: estimation of the daily remuneration gap at the mean—All jobs

| | Baseline IPW (1) | IPW trimming max (2) | Doubly robust (3) | Nearest Neighbour (4) | Individual fixed-effect (5) |
|---|---|---|---|---|---|
| ATT | 11.27 | 11.43 | 9.46 | 11.98 | 4.18 |
| Std.Err. | (0.11) | (0.08) | (0.10) | (0.10) | (0.08) |
| Observations | 3,012,744 | 3,012,744 | 3,012,744 | 3,012,744 | 3,012,744 |
| N individuals | 1,152,057 | 1,152,057 | 1,152,057 | 1,152,057 | 1,152,057 |

*Notes:* Estimates of differences at the mean for different specifications of the semi-parametric estimator on the observables (columns 1-4) or controlling for individual fixed effects and the other control variables (column 5 – see Section 7.5). Robust standard errors (in parentheses) take into account the clustering by individual.

Table A.4: Additional analysis: stock vs inflow sample

| | Stock sample of April (1) | | Stock sample of October (2) | | Inflow sample (3) | |
|---|---|---|---|---|---|---|
| | Any jobs (A) | Full-time jobs (B) | Any jobs (A) | Full-time jobs (B) | Any jobs (A) | Full-time jobs (B) |
| ATT | -19.52 | -15.94 | -18.94 | -16.24 | 4.65 | -2.47 |
| Std.Err. | (0.09) | (0.08) | (0.09) | (0.08) | (0.07) | (0.12) |
| Observations | 8,146,018 | 6,408,372 | 8,231,654 | 6,452,312 | 3,012,744 | 2,128,369 |
| N individuals | 1,285,865 | 1,048,341 | 1,297,210 | 1,056,056 | 1,152,057 | 894,349 |

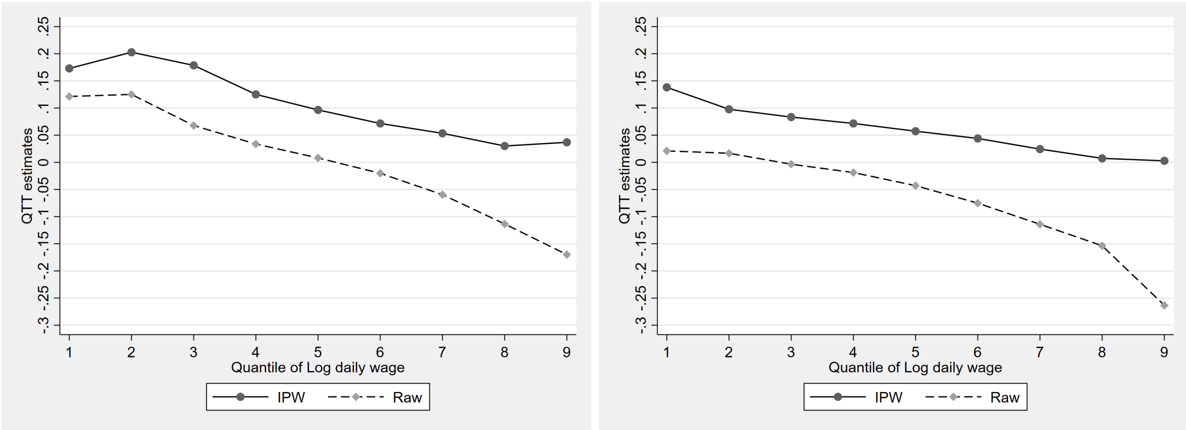
*Notes:* Estimates of ATT are based on the standard IPW method. (A) columns retain all jobs, while (B) columns keep full-time jobs only. Estimates of (1) columns are based on a stock sample of jobs existing on the 1st of April, while (2) columns on those existing on the 1st of October. Estimates of (3) columns retains only an inflow sample of new hiring (baseline inflow sample). In all the analyses, we only condition on a subset of control variables (age, region of residence, year of hiring, gender) to avoid issues of endogeneity. Standard errors (in parentheses) are robust to heteroskedasticity, and therefore conservative, and take into account the clustering by individual.

Table A.5: Wage gap dynamics of the inflow sample hired during 2005-2010

| | Any jobs (A) | | Full-time jobs (B) | |
|---|---|---|---|---|
| | Same contract (1) | Same contract & firm (2) | Same contract (1) | Same contract & firm (2) |
| ATT at entry | 12.21 | 12.21 | 9.13 | 9.13 |
| Std.Err. | (0.12) | (0.12) | (0.17) | (0.17) |
| N treated | 951,476 | 951,476 | 708,977 | 708,977 |
| N controls | 752,061 | 752,061 | 553,788 | 553,788 |
| ATT at 1 year | 11.38 | 10.34 | 6.05 | 6.69 |
| Std.Err. | (0.13) | (0.15) | (0.20) | (0.22) |
| N treated at 1 year | 678,366 | 427,564 | 513,454 | 314,064 |
| Treated retention rate at 1 year | 71% | 45% | 72% | 44% |
| N controls at 1 year | 630,772 | 522,829 | 467,895 | 385,320 |
| Controls retention rate at 1 year | 84% | 70% | 84% | 70% |
| ATT at 3 years | 6.89 | 5.78 | 0.23 | 1.68 |
| Std.Err. | (0.16) | (0.25) | (0.27) | (0.40) |
| N treated at 3 years | 380,957 | 88,693 | 295,875 | 67,300 |
| Treated retention rate at 3 years | 40% | 9% | 42% | 9% |
| N controls at 3 years | 497,855 | 269,497 | 373,354 | 202,355 |
| Controls retention rate at 3 years | 66% | 36% | 67% | 37% |
| ATT at 5 years | 1.86 | 2.18 | -5.22 | -2.23 |
| Std.Err. | (0.18) | (0.36) | (0.29) | (0.52) |
| N treated at 5 years | 284,523 | 35,880 | 223,251 | 27,860 |
| Treated retention rate at 5 years | 30% | 4% | 31% | 4% |
| N controls at 5 years | 435,445 | 178,812 | 327,637 | 136,564 |
| Controls retention rate at 5 years | 58% | 24% | 59% | 25% |

*Notes:* Estimates of ATT based on the standard IPW method for the wage gap at entry and 1, 3, and 5 calendar years after entry. Inflow sample for entries between 2005 and 2010. (A) columns retain all jobs, while (B) columns keep full-time jobs only. (1) columns focus on individuals remaining in the same type of contract (permanent or temporary), (2) columns consider individuals remaining in the same type of contract and firm. The retention rates by treatment group are calculated as the number of jobs at entry divided by the number of jobs still existing in that contract (1) or also firm (2). In all the analyses, we add 11 dummies for the month of entry as further conditioning variables in the IPW estimator. Standard errors (in parentheses) are robust to heteroskedasticity, and therefore conservative, and take into account the clustering by individual.

Figure A.1: QTT: before and after IPW weights




(a) Any jobs

(b) Full-time jobs

Figure A.2: Common support (all jobs)

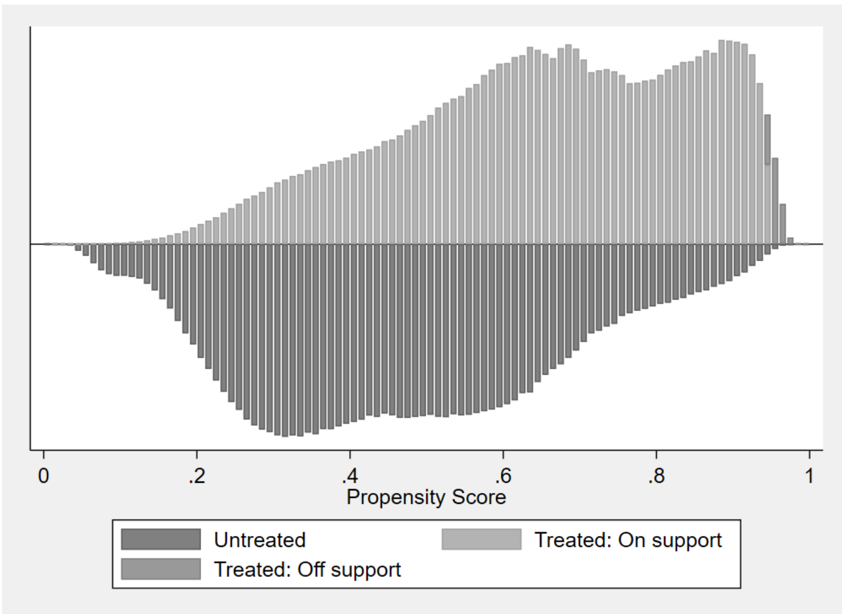
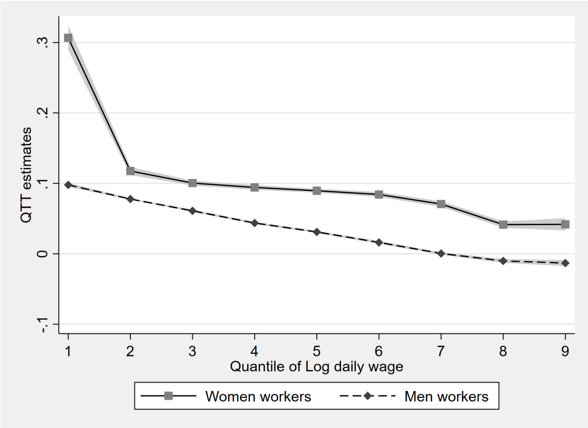
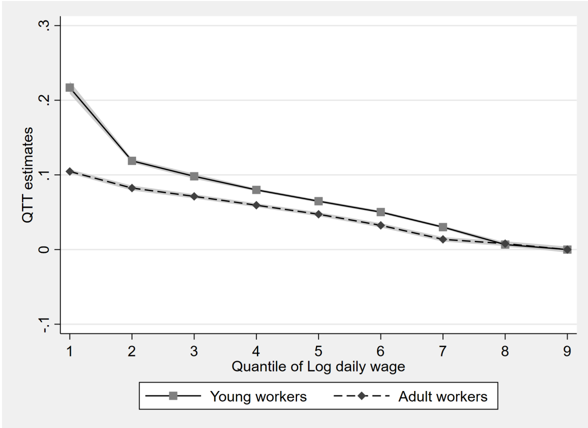


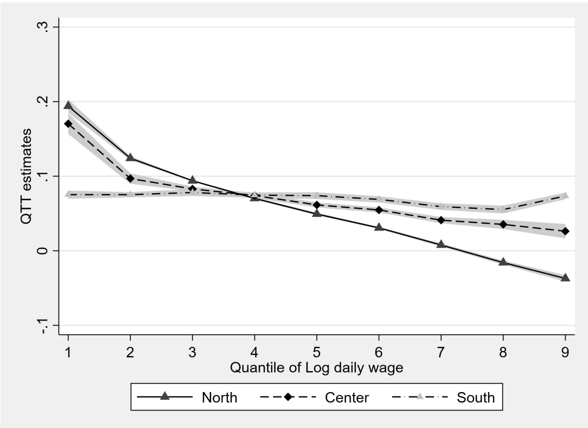
Figure A.3: Heterogeneous wage gaps at the quantiles (QTT)—Full-time jobs



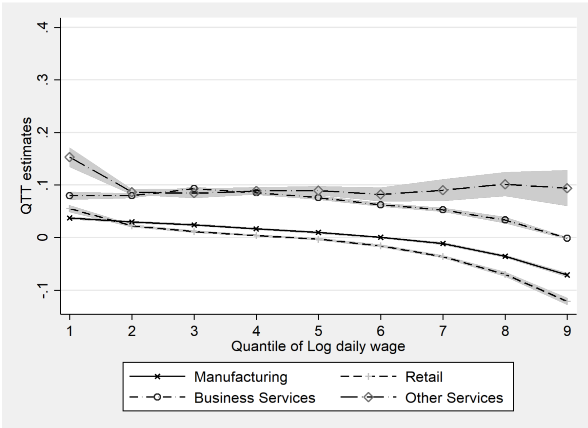

(a) Estimates by gender



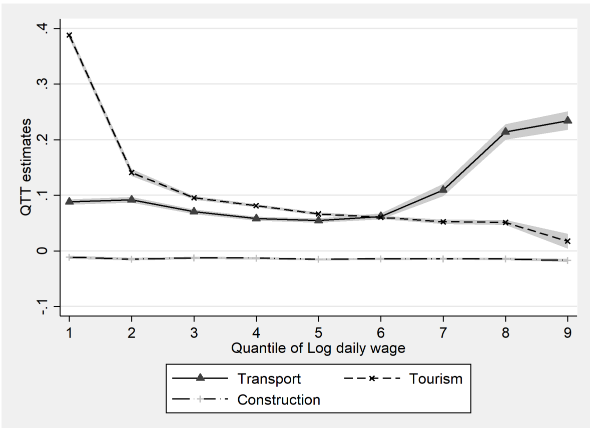

(b) Estimates by age




(c) Estimates by macroregion



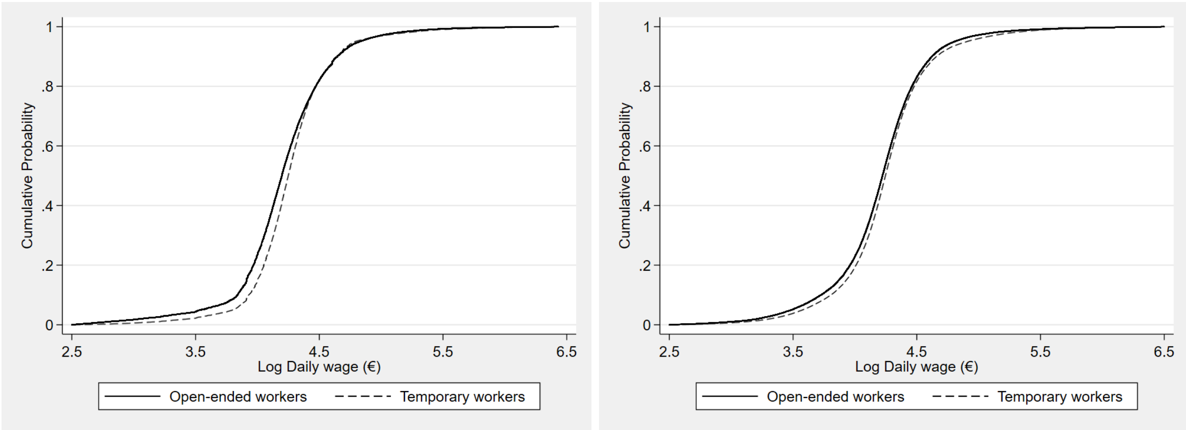

(d) Estimates by sector (1)




(e) Estimates by sector (2)

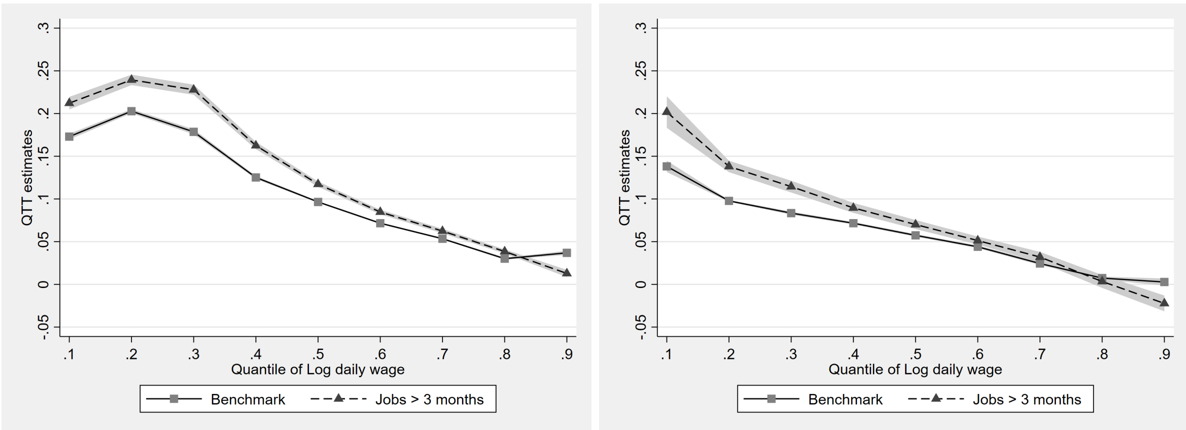
*Notes:* Estimates of QTT are based on the standard IPW method considering only full-time jobs. Estimates by sector consider in the treated group one type of temporary contract, namely, the fixed-term contracts (or *'contratti a tempo determinato'*). Conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

Figure A.4: Placebo test: cumulative distribution of daily wage logarithm—Full-time jobs



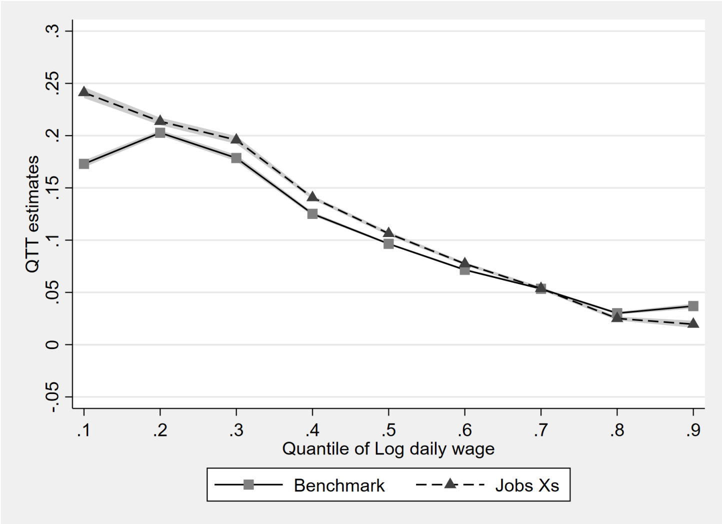

(a) Full-time jobs in year t (b) Full-time jobs in year t−1

Figure A.5: Sensitivity analysis—Only jobs with an effective duration > 3 months. All jobs (left), full-time jobs (right)



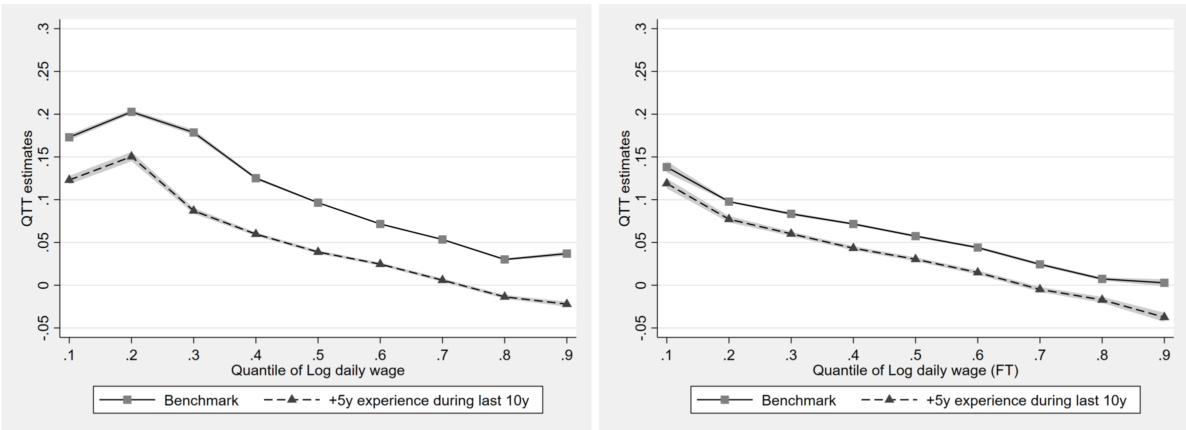

*Notes:* Estimates of QTT are based on the standard IPW method. For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

Figure A.6: Sensitivity analysis—Including simultaneous job characteristics (all jobs)




*Notes:* Estimates of QTT are based on the standard IPW method. Results on full-time jobs are not reported since contemporary variables are already conditioned in the benchmark analysis. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

Figure A.7: Sensitivity analysis—Individuals with experience > 5 years (260 weeks) during the last 10 years. All jobs (left), full-time jobs (right)




*Notes:* Estimates of QTT are based on the standard IPW method. For estimates retaining only full-time jobs, conditioning variables also include simultaneous job characteristics. Shadowed areas show 95% confidence intervals. Standard errors are obtained by bootstrap (199 repetitions), taking into account the clustering by individual.

## B Rosenbaum sensitivity test

The sensitivity analysis proposed by Rosenbaum (2002) assumes that the estimated ATT is due to an unobserved confounding factor $u$ correlated to the outcome $Y$ and the treatment $D$. The odds ratio of differential treatment assignment given $u$ and the covariates $X$ is defined as Γ,

$$\Gamma = \frac{p_i(X_i, u_i) * (1 - p_j(X_j, u_j))}{p_j(X_j, u_j) * (1 - p_i(X_i, u_i)} = \frac{exp(\beta * X_i + \gamma u_i)}{exp(\beta * X_j + \gamma u_j)}, \tag{SENS}$$

where $i$ and $j$ indicate treated and control units, $p(X, u)$ is the propensity score estimated by a logistic regression for the probability of being treated given the $X$ covariates and $u$ unobserved confounding factors, whose coefficients are $\beta$ and $\gamma$. For matched units ($X_j = X_i$), Γ is equal to 1 only if $u$ is not correlated to the treatment ($\gamma = 0$) or unobserved factors for the two groups are the same ($u_i = u_j$). The confounding factor $u$ is defined as a 'worst-case scenario' since it is assumed to perfectly determine whether $Y$ of the treated would be larger or smaller than $Y$ of the matched controls. Thanks to this sensitivity analysis, one can estimate the magnitude of the bias Γ that would make the treatment effect equal to zero. For example, a Γ = 1.5 suggests that the presence of a confounding factor $u$ that makes treated individuals 50% more likely to be assigned to the treatment may undermine the analysis. Finally, to assess the relative magnitude of the bias, DiPrete and Gangl (2004) proposed estimating an equivalent bias for the odds of treatment by varying an observed control variable. This can be obtained by dividing the log of the required bias by its coefficient in the propensity score model.

Table B.1: Rosenbaum sensitivity analysis—All jobs

| Gamma | sig+ | sig- | t-hat+ | t-hat- | CI+ | CI- |
|---|---|---|---|---|---|---|
| 1.00 | 0.000 | 0.000 | 0.1073 | 0.1073 | 0.1063 | 0.1083 |
| 1.01 | 0.000 | 0.000 | 0.1043 | 0.1103 | 0.1033 | 0.1113 |
| 1.02 | 0.000 | 0.000 | 0.1014 | 0.1132 | 0.1003 | 0.1142 |
| 1.03 | 0.000 | 0.000 | 0.0985 | 0.1161 | 0.0974 | 0.1172 |
| 1.04 | 0.000 | 0.000 | 0.0956 | 0.1190 | 0.0945 | 0.1201 |
| 1.05 | 0.000 | 0.000 | 0.0927 | 0.1219 | 0.0917 | 0.1229 |
| 1.06 | 0.000 | 0.000 | 0.0899 | 0.1247 | 0.0889 | 0.1258 |
| 1.07 | 0.000 | 0.000 | 0.0871 | 0.1276 | 0.0861 | 0.1286 |
| 1.08 | 0.000 | 0.000 | 0.0843 | 0.1304 | 0.0833 | 0.1314 |
| 1.09 | 0.000 | 0.000 | 0.0816 | 0.1331 | 0.0805 | 0.1342 |
| 1.10 | 0.000 | 0.000 | 0.0788 | 0.1359 | 0.0778 | 0.1369 |
| 1.11 | 0.000 | 0.000 | 0.0762 | 0.1386 | 0.0751 | 0.1396 |
| 1.12 | 0.000 | 0.000 | 0.0735 | 0.1413 | 0.0725 | 0.1423 |
| 1.13 | 0.000 | 0.000 | 0.0708 | 0.1440 | 0.0698 | 0.1450 |
| 1.14 | 0.000 | 0.000 | 0.0682 | 0.1466 | 0.0672 | 0.1477 |
| 1.15 | 0.000 | 0.000 | 0.0656 | 0.1493 | 0.0646 | 0.1503 |
| 1.16 | 0.000 | 0.000 | 0.0631 | 0.1519 | 0.0620 | 0.1529 |
| 1.17 | 0.000 | 0.000 | 0.0605 | 0.1545 | 0.0595 | 0.1555 |
| 1.18 | 0.000 | 0.000 | 0.0580 | 0.1570 | 0.0570 | 0.1581 |
| 1.19 | 0.000 | 0.000 | 0.0555 | 0.1596 | 0.0545 | 0.1606 |
| 1.20 | 0.000 | 0.000 | 0.0530 | 0.1621 | 0.0520 | 0.1632 |
| 1.21 | 0.000 | 0.000 | 0.0506 | 0.1646 | 0.0495 | 0.1657 |
| 1.22 | 0.000 | 0.000 | 0.0481 | 0.1671 | 0.0471 | 0.1682 |
| 1.23 | 0.000 | 0.000 | 0.0457 | 0.1696 | 0.0447 | 0.1706 |
| 1.24 | 0.000 | 0.000 | 0.0433 | 0.1721 | 0.0423 | 0.1731 |
| 1.25 | 0.000 | 0.000 | 0.0409 | 0.1745 | 0.0399 | 0.1755 |
| 1.26 | 0.000 | 0.000 | 0.0386 | 0.1769 | 0.0376 | 0.1780 |
| 1.27 | 0.000 | 0.000 | 0.0362 | 0.1793 | 0.0352 | 0.1804 |
| 1.28 | 0.000 | 0.000 | 0.0339 | 0.1817 | 0.0329 | 0.1827 |
| 1.29 | 0.000 | 0.000 | 0.0316 | 0.1840 | 0.0306 | 0.1851 |
| 1.30 | 0.000 | 0.000 | 0.0293 | 0.1864 | 0.0283 | 0.1874 |
| 1.31 | 0.000 | 0.000 | 0.0271 | 0.1887 | 0.0261 | 0.1898 |
| 1.32 | 0.000 | 0.000 | 0.0248 | 0.1910 | 0.0238 | 0.1921 |
| 1.33 | 0.000 | 0.000 | 0.0226 | 0.1933 | 0.0216 | 0.1944 |
| 1.34 | 0.000 | 0.000 | 0.0204 | 0.1956 | 0.0194 | 0.1967 |
| 1.35 | 0.000 | 0.000 | 0.0182 | 0.1979 | 0.0172 | 0.1989 |
| 1.36 | 0.000 | 0.000 | 0.0160 | 0.2001 | 0.0150 | 0.2012 |
| 1.37 | 0.000 | 0.000 | 0.0139 | 0.2024 | 0.0128 | 0.2034 |
| 1.38 | 0.000 | 0.000 | 0.0117 | 0.2046 | 0.0107 | 0.2056 |
| 1.39 | 0.000 | 0.000 | 0.0096 | 0.2068 | 0.0086 | 0.2078 |
| 1.40 | 0.000 | 0.000 | 0.0075 | 0.2090 | 0.0065 | 0.2100 |
| 1.41 | 0.000 | 0.000 | 0.0054 | 0.2111 | 0.0044 | 0.2122 |
| 1.42 | 0.000 | 0.000 | 0.0033 | 0.2133 | 0.0023 | 0.2144 |
| 1.43 | 0.009 | 0.000 | 0.0012 | 0.2154 | 0.0002 | 0.2165 |
| 1.44 | 0.937 | 0.000 | -0.0008 | 0.2176 | -0.0018 | 0.2186 |
| 1.45 | 1.000 | 0.000 | -0.0028 | 0.2197 | -0.0039 | 0.2208 |
| 1.46 | 1.000 | 0.000 | -0.0049 | 0.2218 | -0.0059 | 0.2229 |
| 1.47 | 1.000 | 0.000 | -0.0069 | 0.2239 | -0.0079 | 0.2249 |
| 1.48 | 1.000 | 0.000 | -0.0089 | 0.2259 | -0.0099 | 0.2270 |
| 1.49 | 1.000 | 0.000 | -0.0108 | 0.2280 | -0.0119 | 0.2291 |
| 1.50 | 1.000 | 0.000 | -0.0128 | 0.2301 | -0.0138 | 0.2311 |

*Notes:* Results obtained by using rbounds Stata routine after nearest neighbour one-to-one matching, retaining all jobs. Gamma: log odds of differential assignment due to unobserved factors; sig+: upper bound significance level (assumption: overestimation of treatment effect); sig-: lower bound significance level (assumption: underestimation of treatment effect); t-hat+: upper bound Hodges–Lehmann point estimate; t-hat-: lower bound Hodges–Lehmann point estimate; CI+: upper bound confidence interval at 95%; CI-: lower bound confidence interval at 95%

Table B.2: Rosenbaum sensitivity analysis—Full-time jobs only

| Gamma | sig+ | sig- | t-hat+ | t-hat- | CI+ | CI- |
|---|---|---|---|---|---|---|
| 1.00 | 0.000 | 0.000 | 0.0633 | 0.0633 | 0.0624 | 0.0641 |
| 1.01 | 0.000 | 0.000 | 0.0612 | 0.0654 | 0.0603 | 0.0662 |
| 1.02 | 0.000 | 0.000 | 0.0591 | 0.0674 | 0.0583 | 0.0683 |
| 1.03 | 0.000 | 0.000 | 0.0571 | 0.0695 | 0.0562 | 0.0704 |
| 1.04 | 0.000 | 0.000 | 0.0551 | 0.0715 | 0.0542 | 0.0724 |
| 1.05 | 0.000 | 0.000 | 0.0530 | 0.0736 | 0.0522 | 0.0744 |
| 1.06 | 0.000 | 0.000 | 0.0511 | 0.0755 | 0.0502 | 0.0764 |
| 1.07 | 0.000 | 0.000 | 0.0491 | 0.0775 | 0.0482 | 0.0784 |
| 1.08 | 0.000 | 0.000 | 0.0471 | 0.0795 | 0.0463 | 0.0804 |
| 1.09 | 0.000 | 0.000 | 0.0452 | 0.0814 | 0.0444 | 0.0823 |
| 1.10 | 0.000 | 0.000 | 0.0433 | 0.0834 | 0.0424 | 0.0842 |
| 1.11 | 0.000 | 0.000 | 0.0414 | 0.0853 | 0.0406 | 0.0861 |
| 1.12 | 0.000 | 0.000 | 0.0395 | 0.0872 | 0.0387 | 0.0880 |
| 1.13 | 0.000 | 0.000 | 0.0377 | 0.0891 | 0.0368 | 0.0899 |
| 1.14 | 0.000 | 0.000 | 0.0358 | 0.0909 | 0.0350 | 0.0918 |
| 1.15 | 0.000 | 0.000 | 0.0340 | 0.0928 | 0.0332 | 0.0936 |
| 1.16 | 0.000 | 0.000 | 0.0322 | 0.0946 | 0.0313 | 0.0955 |
| 1.17 | 0.000 | 0.000 | 0.0304 | 0.0964 | 0.0296 | 0.0973 |
| 1.18 | 0.000 | 0.000 | 0.0286 | 0.0982 | 0.0278 | 0.0991 |
| 1.19 | 0.000 | 0.000 | 0.0269 | 0.1000 | 0.0260 | 0.1009 |
| 1.20 | 0.000 | 0.000 | 0.0251 | 0.1018 | 0.0243 | 0.1027 |
| 1.21 | 0.000 | 0.000 | 0.0234 | 0.1036 | 0.0225 | 0.1044 |
| 1.22 | 0.000 | 0.000 | 0.0217 | 0.1053 | 0.0208 | 0.1062 |
| 1.23 | 0.000 | 0.000 | 0.0200 | 0.1071 | 0.0191 | 0.1079 |
| 1.24 | 0.000 | 0.000 | 0.0183 | 0.1088 | 0.0174 | 0.1097 |
| 1.25 | 0.000 | 0.000 | 0.0166 | 0.1105 | 0.0158 | 0.1114 |
| 1.26 | 0.000 | 0.000 | 0.0149 | 0.1122 | 0.0141 | 0.1131 |
| 1.27 | 0.000 | 0.000 | 0.0133 | 0.1139 | 0.0124 | 0.1147 |
| 1.28 | 0.000 | 0.000 | 0.0117 | 0.1155 | 0.0108 | 0.1164 |
| 1.29 | 0.000 | 0.000 | 0.0100 | 0.1172 | 0.0092 | 0.1181 |
| 1.30 | 0.000 | 0.000 | 0.0084 | 0.1189 | 0.0076 | 0.1197 |
| 1.31 | 0.000 | 0.000 | 0.0068 | 0.1205 | 0.0060 | 0.1214 |
| 1.32 | 0.000 | 0.000 | 0.0053 | 0.1221 | 0.0044 | 0.1230 |
| 1.33 | 0.000 | 0.000 | 0.0037 | 0.1237 | 0.0028 | 0.1246 |
| 1.34 | 0.000 | 0.000 | 0.0021 | 0.1254 | 0.0013 | 0.1262 |
| 1.35 | 0.088 | 0.000 | 0.0006 | 0.1269 | -0.0003 | 0.1278 |
| 1.36 | 0.985 | 0.000 | -0.0009 | 0.1285 | -0.0018 | 0.1294 |
| 1.37 | 1.000 | 0.000 | -0.0025 | 0.1301 | -0.0033 | 0.1310 |
| 1.38 | 1.000 | 0.000 | -0.0040 | 0.1317 | -0.0048 | 0.1325 |
| 1.39 | 1.000 | 0.000 | -0.0055 | 0.1332 | -0.0063 | 0.1341 |
| 1.40 | 1.000 | 0.000 | -0.0070 | 0.1348 | -0.0078 | 0.1356 |

*Notes:* Results obtained by using rbounds Stata routine after nearest neighbour one-to-one matching, retaining only full-time jobs. Gamma: log odds of differential assignment due to unobserved factors; sig+: upper bound significance level (assumption: overestimation of treatment effect); sig-: lower bound significance level (assumption: underestimation of treatment effect); t-hat+: upper bound Hodges–Lehmann point estimate; t-hat-: lower bound Hodges–Lehmann point estimate; CI+: upper bound confidence interval at 95%; CI-: lower bound confidence interval at 95%