Another look at the "stepping stone *vs.* dead end" issue about the impact of temporary employment. What can we learn from recurrent Labour Force Surveys?

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Abstract

Over the last ten years the Italian labour market experienced several changes in the regulation of temporary contracts. Using short panels from the Italian Labour Force Survey (LFS) we identify the causal effect of experiencing a spell of temporary employment *vs.* a spell of unemployment on short-term labour market outcomes. The parameter of interest is recovered by imposing that, conditional on a suitable set of observable characteristics of the workers, of their households as well as of the local labour market, the treatment status is ignorable for the outcome. We carry out the analyses over three two-year periods characterized by increasingly open arrangements about the use of temporary contracts.

We exploit the features of the LFS rotating sampling scheme to build a test for the ignorability assumption. It turns out that for the 1995/96 and 2000/01 samples, based on the old quarterly LFS, ignorability is rejected, while for the 2005/06 sample, based on the new continuous LFS, the matched treated and control groups turn out statistically balanced.

As for the estimate of the impact, experiencing a spell of temporary work takes to a 30% higher employment rate one year later for men, 35% for women. Most of this impact is due to temporary and unsatisfactory jobs, though. When we look at the impact on the probability of experiencing a transition to a permanent job, the effect is not significant for men, and just marginally significant for women. As for the impact on the probability to get a satisfactory job, it is significant and as large as 9.5% both for men and women.

Finally, there is a sizeable heterogeneity of the effects across areas for men: for permanent employment there is a positive effect in the North, while it is negative in the South; for satisfactory employment the effect is about 15% in the North, while it is nil in the South.

Keywords: Temporary employment, Programme evaluation, Propensity score matching, Over-identification test

JEL codes: C31, J41, J63

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1. *Introduction*[*]

Over the last decade the Italian legislation on temporary employment contracts – *i.e.*, fixed-term contracts, apprenticeship and other training-related contracts, temporary work agency jobs – and employment protection has experienced a series of changes, basically towards more flexible arrangements. Indeed, according to Brandt, Burniaux and Duval (2005) Italy has been the country with the largest drop in the OECD's Employment Protection Legislation sub-index for temporary employment since the mid-1990s. The question we address in this paper is about the causal effect of experiencing a spell of temporary employment *vs.* a spell of unemployment on short-term labour market outcomes.

The issue is by no means peculiar to Italy. Indeed, the legislation on employment contracts followed a similar pattern in many European countries, starting from Spain. To date, however, empirical knowledge of the role played by temporary contracts on individual employment histories remains controversial. The wider context of our question is the debate whether temporary jobs can be regarded as 'stepping stones' – *i.e.*, ports of entry into permanent employment in a particular company or into the labour market as a whole, or rather as 'dead ends' – traps in worker's employment biographies, which foster a division between 'core' and 'peripheral' workers in the labour market[1].

Quite often specific temporary work schemes have been examined. For instance, Larsson, Lindqvist and Nordström Skans (2005) consider a scheme providing temporary replacement contracts to people registered as unemployed to the local Labour Exchanges in Sweden. This is largely the case for Italy too, where most research has focused on specific training-related contracts (Contini *et al*. 2002; Barbieri and Scherer, 2007), temporary work agency jobs (Ichino, Mealli and Nannicini 2005, 2008), the so-called 'quasi subordinate' contracts also featuring a flexible termination clause (Berton, Devicienti and Pacelli 2008)[2].

Besides, part of the literature looking at the subsequent labour market outcomes for the generality of the many different schemes of temporary employment in Italy aims at documenting and analysing its dynamics (see, among many others, Barbieri and Scherer

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[1] A large number of papers addressed this question. We refer just to some of them. Alba-Ramirez (1998) and Güell and Petrongolo (2007) deal with the Spanish case, and D'Addio and Rosholm (2005) consider the European Union as a whole: they mostly conclude that temporary employment is not an effective route for entry into permanent positions, specially for women and workers with low qualifications. By contrast, studies for the UK (Booth, Francesconi and Frank 2002), the Netherlands (De Graaf-Zijl, Van den Berg and Heyma 2004), and Germany (Hagen 2003), give support to the notion of a springboard role of fixed-term contracts. Summary reviews of that literature are in Barbieri and Sestito (2008), Boockmann and Hagen (2008) and Portugal and Varejão (2009). The last two papers focus on fixed-term contracts as sorting mechanisms, and may at least to some part be understood as prolonged probationary periods.

[2] Note that 'quasi subordinate' workers legally are not employees, although they share many characteristics of (temporary) employees: being often engaged by just one firm, working on the firm's premises, etc.. Besides, their identification and counting from statistical surveys or administrative sources is somewhat problematic, at least up to 2003.

2005, 2009; Trivellato 2008b: Section 2 – with contributions by Rustichelli, Centra, Leombruni, Anastasia and Maurizio; Berton, Richiardi and Sacchi 2009).

Papers dealing with a proper impact evaluation of temporary contracts on individual employment histories are quite few[3]: among them Gagliarducci (2005), Berton, Devicienti and Pacelli (2008), and Barbieri and Sestito (2008). They differ with respect to the main question they address to, the data-sets used for their empirical analyses, and the impact evaluation strategy. Perhaps not surprisingly, the available evidence is only partial and not conclusive.

Gagliarducci (2005) uses the ILFI survey (*Indagine Longitudinale sulle Famiglie Italiane*, 1997 interview), looks at the sequences of (possibly interrupted) spells of employment in a long time span, and applies multi-spell duration techniques. He finds that «*the probability of moving from a temporary to a permanent job increases with the duration of the contract, but decreases with repeated temporary jobs and especially with interruptions. This suggests that it is not temporary employment per se but the intermittence associated with it that is detrimental to employment prospects*» (Gagliarducci 2005: 429). Given the moderate sample size of the ILFI survey, and the small sub-sample appropriately selected for the empirical analyses, observed heterogeneity is forcibly handled within a mixed proportional hazard model.

Berton, Devicienti and Pacelli (2008) study the port of entry *vs.* trap-effect hypotheses on the Italian labour market in the medium term within a multi-state framework. They use the Work Histories Italian Panel (WHIP), a large dataset based on the administrative archives of the National Institute for Social Security. Their main results are that, after accounting for the substantial role played by individual heterogeneity, «*whatever the initial state of a worker, retaining the same contract is always the most likely destination, even after individual heterogeneity is controlled for. Despite this evidence of persistence, […] the port of entry hypothesis cannot be denied in Italy, in the sense that the transition to open-ended employment is more likely for individuals holding any type of temporary contract than for unemployed individuals. However, some temporary contracts are better than others in providing access to open ended employment: our results suggest that training contracts are the best port of entry to open ended employment, while quasi subordinate work is the worst*» (Berton, Devicienti and Pacelli 2008: 3-4). These results have to be taken with due caution, because WHIP does not comprise all employment: in fact, it essentially covers (regular) employees in the industrial and service sector[4].

A quite different route is taken by Barbieri and Sestito (2008) – henceforth B&S. They look at the subsequent short-term labour market outcomes for the generality of the schemes of temporary employment, in the context of the entire labour force flows. They develop a simple, yet convincing approach «*that fully exploits the peculiar longitudinal features of the Labour Force Survey and that, besides its informativeness for the Italian case, might be replicated in similar survey contexts*». Their main finding is that

[3] A review of impact evaluation studies on Italian labour market policies – among which interventions in regulation – and incentives to firms targeted to an occupational increase is in Trivellato (2008a: Part 2).

[4] More on that in Bison, Rettore and Schizzerotto (2009: 6-7).

«*individuals' heterogeneity explains a good amount of the raw differences in the subsequent labour market status of temporary workers and the comparison group. Yet there appears to be a sizable net gain from experiencing a temporary work*» (B&S 2008: 129 and 127).

Moving from the contribution of B&S, in this paper we pose two questions to which we offer clear evidence. These questions are: (i) what is the potential of Italian Labour Force Survey (LFS) – and presumably of LFSs carried out in several other countries with a similar design that includes a panel component – to recurrently shed light to the stepping stone *vs*. dead end debate on fixed-term contracts?; (ii) under the evaluation strategy used – matching on the whole set of sensible observables, how can we test the ignorability of treatment status, and thus corroborate (or falsify) our causal conclusions?

The paper improves on B&S in several respects. First, we get larger panels from the Italian Labour Force Survey (LFS), by pooling series of subsequent short panels. This will allow us to get more robust results. In addition, we will be able to carry out the analyses for three two-year periods when different regulations about temporary contracts were in place. Second, we further exploit the LFS datasets, both by using data on several household's characteristics associated to the individuals, and, starting from 2004, benefitting from the richer information on work experience and the actual job collected with the new, continuous LFS (Istat, 2004). Third, and most important, we offer an over-identification test, which we will call 'backward test', to assess whether, conditional on the set of observable characteristics of the individuals, the treatment status is ignorable for the outcomes[5]. We will use that test extensively to gauge the periods and groups for which we can draw credible inferences on the causal effect of temporary contracts.

Following B&S, we use a partial equilibrium analysis to address the issue of the effect of experiencing a spell of temporary work *vs*. a spell of unemployment on short-term labour market outcomes. We carry out the analysis within the period 1995-2006. More precisely, we consider three two-year periods characterized by different, increasingly open arrangements about the use of temporary contracts:

(a) 1995/96: taken as the 'pre-reform' period, where just training-related contracts (apprenticeship and the *Contratto di Formazione e Lavoro*) were available, in addition to the open-ended contract;
(b) 2000/01: after the approval of the so-called 'pacchetto Treu' (1997, operational from 1998-99), that chiefly introduced temporary work agency contracts;
(c) 2005/06: after a noticeable enlargement of the use of fixed-term contracts (2001) and the so-called 'legge Biagi' (2003), that extended the opportunities for temporary work agency jobs and apprenticeship, and introduced an additional training-related contract (*contratto di inserimento*) along with some other contractual schemes of marginal relevance[6].

[5] On tests for the ignorability assumption see Rosenbaum (1984), Heckman and Hotz (1989) and Lee (2008).

[6] For the sake of brevity, we refer to B&S (Section 2 and 3) and Berton, Richiardi and Sacchi (2009: Chapters 1 and 4) for a detailed presentation of the main changes in the regulation of labour contracts and an analysis of the evolution of temporary and total employment in the 1990's and early 2000's.

As anticipated, the evaluation study makes use of short panels from the quarterly LFS. The main features of the evaluation strategy are as follows: the reference population is the stock of individuals not employed at time *t*0; the two alternative treatments are a spell of temporary work *vs*. a spell of unemployment at a subsequent time *t*1; the outcome is the labour market state – or a perceived labour condition by the worker – at time *t*2.

By exploiting the longitudinal feature of the LFS, each individual aged 15 years or more in a sampled household is interviewed four times within a 15 months window. For each individual in the sample we consider the first, second and fourth wave. Thus, the distance between *t*0 and *t*1 is 3 months, *t*1 ≡ *t*0+1 quarter: within a quarter we observe almost all the inflows into temporary employment (except for extremely short spells of temporary work). The outcome is measured one year after *t*1, *i.e.* at *t*2 ≡ *t*1+4. Actually, the length of the available panels does not allow to identify effects beyond 12 months after *t*1.

The parameter of interest is recovered by assuming that, conditional on a suitable set of observable characteristics of the workers, the treatment status is ignorable for the outcome. We correct for the selection bias by propensity score matching techniques, exploiting information in *t*0 about individual demographics, employment state and search activities, previous working experience, household characteristics (about age, education, employment, etc.), and summary indicators of local labour demand conditions measured at the province level.

The LFS panels allow us to test whether, conditional on the available observables, the treatment status is ignorable. We exploit one feature of the rotating sample scheme of LFS (and of several labour force surveys in other countries), which allows us to observe the labour market state in a pre-treatment period. We test the ignorability condition by comparing some 'backward' outcomes on matched samples obtained by the same matching strategy we use for impact estimates.

Section 2 describes our evaluation strategy and the backward test. In Section 3 we illustrate the data together with some definitions of interest. Section 4 presents descriptive evidence on sample sizes and outcomes for the three periods considered. Section 5 presents the main results about the impact of temporary job experiences for the 2005/06 sample, for which we get credible results (Appendix A lists the covariates used for the propensity score in the 2005/06 sample, along with some summary statistics). It is worth adding that the 2005/06 period is also the most interesting, as at that time all incremental reforms extending the opportunities for temporary contracts were in operation. Less detailed results for the previous two periods are outlined in Section 6: as it will be seen, they are rather dubious. In Section 7 we elucidate which additional variables from the 2005/06 continuous LFS sample are crucial for the specification test to pass, and draw indications about the direction of biases affecting the results for the previous two periods. Finally, Section 8 provides a brief summary and some discussion on the results and their interpretation.

2. *The evaluation strategy*

The evaluation exercise makes use of short panels from subsequent waves of LFS.

The pattern of the rotating sampling scheme of the survey is in Table 1, with reference to the period 2005/06. As the sampling scheme did not change over time, Table 1 straightforwardly applies to the other periods as well.

To illustrate our evaluation strategy consider as an example the E sample. Its first wave occurs at the last quarter of 2005: the second wave is in the first quarter of 2006; then there is no interview for the two subsequent quarters; the third and fourth waves occur at the last quarter of 2006 and the first of 2007, respectively.

The reference population we consider is made up of all individuals who were not at work at the first interview ($t0$). Out of this population we consider two sub-groups made up of those individuals who three months later ($t1$) experience one of two alternative events: the treatment group is made up of individuals experiencing a spell of temporary employment, while the comparison group is made up of individuals experiencing a spell of unemployment:

$$I = \begin{cases} 1 & \text{temporary employment in } t1 \\ 0 & \text{unemployment in } t1 \end{cases}.$$

We consider as outcome variable a suitably defined labour force state or perceived condition Y (see below) at the fourth interview ($t2$), thus one year after the time with respect to which the treatment status is defined. Let Y^T and Y^{NT} be the potential outcomes a specific individual would experience being exposed to, and denied, the treatment, respectively. The average impact of the treatment on the treated group (ATT) is:

$$E[\alpha \mid I=1] = E[Y^T - Y^{NT} \mid I=1] = E[Y^T \mid I=1] - E[Y^{NT} \mid I=1]. \qquad (1)$$

Table 1: *Rotating sample scheme of the Italian* L*FS* [(a)]

Year	*Quarter*	*Rotation groups*							
2004	4	***A***							
2005	1	A	B						
	2		B	C					
	3			C	D				
	4	***A***			D	**E**			
2006	1	***A***	B			**E**	F		
	2		B	C			F	G	
	3			C	D			G	H
	4				D	E			H
2007	1					**E**	F		
	2						F	G	
	3							G	H
	4								H

[(a)] As an example, in rotation group E the three waves used for impact analysis are in bold; in rotation group A the three waves used for the backward test are in italics bold (see section 2.2).

2.1. *The fundamental problem of causal inference and propensity score matching*

The last term in equation (1) is unobservable by construction, since the outcome Y^{NT} is never observed on those undergoing the treatment. We do observe the mean value of Y^{NT} but only on the comparison group. By contrasting it to the mean outcome experienced by the treated group we get the well-known identity:

$$E[Y^{T} \mid I=1]-E[Y^{NT} \mid I=0]=E[\alpha \mid I=1]+\left(E[Y^{NT} \mid I=1]-E[Y^{NT} \mid I=0]\right). \qquad (2)$$

This clarifies that the observed difference between treated workers and controls includes the so-called selection bias, namely the difference we would have observed had the treated been denied the treatment. In our case, it is very likely that individuals who actually got a temporary job in *t*1 are different from those who didn't with respect to characteristics relevant for the labour market state in any time period including *t*2.

A popular strategy to solve the selection bias problem is balancing the two groups with respect to a suitable set of observable characteristics. The unbiasedness of the resulting estimator for the ATT crucially rests on the so-called ignorability condition:

$$Y^{NT} \perp I \mid X. \qquad (3)$$

As for the computational aspects, as usual to ease calculations we match controls to treated workers on the propensity score (Rosenbaum and Rubin 1983):

$$e(X)=\Pr(I=1 \mid X).$$

2.2. *A backward test for the ignorability condition*

In order to test the assumption (3) we should be able to show that, after balancing with respect to *X*, the two groups are distributed the same way with respect to Y^{NT}. As, by definition, before the treatment we observe Y^{NT} for both groups, one sensible way to test (3) would be to use Y^{NT} observed before the treatment and compare the results on the two matched samples. If differences turned out to be significant, the evidence would be against (3); otherwise the evidence would favour (3).

In the evaluation strategy outlined so far, we exploit the whole LFS sampling window as well as all the pre-treatment available sensible characteristics to balance the two groups[7]. Thus, there is nothing left to implement the test. Still, the LFS sampling scheme outlined in Table 1 allows us to test (3) on an independent sample from the same population, as the one we exploit to identify the causal effect.

To exemplify, stick to the E sample. The A sample represents the very same population as the E one in the last quarter of 2005 (*t*0) and the first quarter of 2006 (*t*1). If

[7] It is worth stressing that the matching variables should be suitable to the problem at hand, that is it should be sensible to assume for them to have some causal relationship to both treatment and outcome. See Pearl (2009) for a clarifying comment on the exchange of letters among D.B. Shrier, D.B. Rubin, A. Sjölander, and J. Pearl in *Statistics in Medicine*, 2008 and 2009, about the inclusion of covariates in propensity score methods.

we apply the same matching strategy to the last two waves of the A sample as we do for the first two waves of the E sample, we end up with two couples of treatment and control groups alike up to sampling variability. Then:

- to evaluate the causal parameter of interest, we collect X in the first wave, observe the treatment status I in the second wave, compare the outcomes across the matched groups in the fourth wave;
- for the backward test, we collect X in the third wave, observe the treatment status I in the fourth wave, compare the outcomes across the matched groups in the first wave. A graphical representation of the time design of the backward test is in Figure 1.

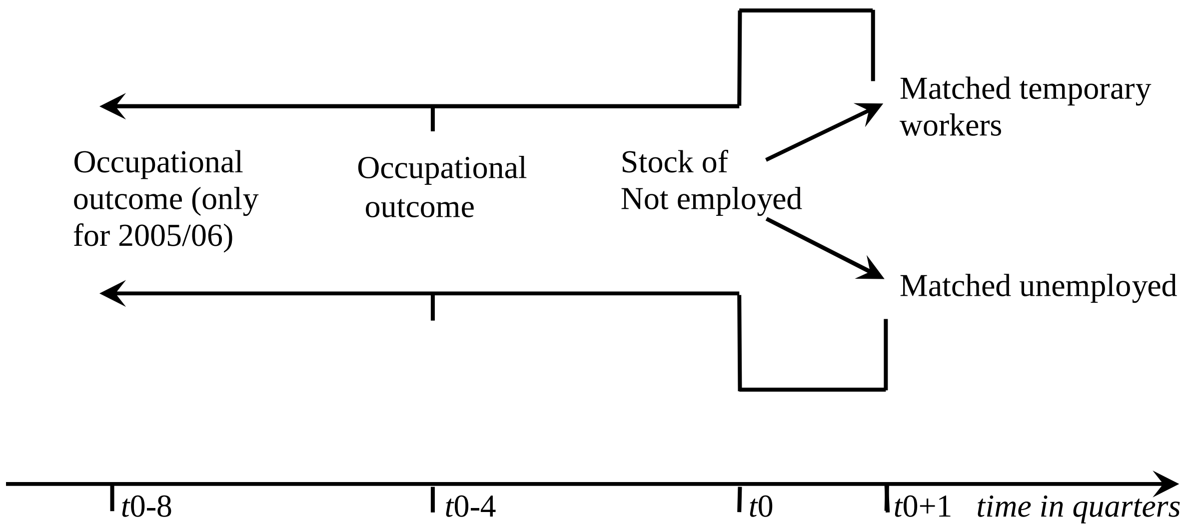
As in our data we have 8 samples for each period, we could potentially use samples A to D for the backward test of samples E to H. An alternative strategy, which allows us to exploit the whole dataset, is to use each sample for the backward test related to the sample itself[8]. In the sequel we use the whole dataset A to H in order to gain precision for our estimates and power for the tests.

3. *Data and definitions*

We consider three samples, defined as inflows in the treatment over a two-year period, *i.e.*, 8 non-overlapping panels for each sample: 1995/96, 2000/01 and 2005/06. The regulations on temporary jobs became progressively less stringent over the three periods we consider.

As the inflow period is defined as *t*1, for each quarter of inflow we need information on the previous quarter (*t*0) and on the last wave after one year (*t*2).

Figure 1: *The time design of the backward test*



[8] Some preliminary analyses on the samples E to H showed similar results when compared to the original strategy. We estimated the outcomes for the backward test on the A to D samples and compared them to the same strategy applied to the E to H samples. The results were statistically equivalent.

For the first two periods we use data from the old quarterly survey, *Rilevazione Trimestrale delle Forze di Lavoro* (RTFL), from 1994-IV to 1997-IV and from 1999-IV to 2002-IV, respectively. On the contrary, the 2005/06 inflows are from the new continuous survey – still within a quarterly frame, *Rilevazione Continua delle Forze di Lavoro* (RCFL), from October 2004 to December 2007[9].

It is worth noting that the LFS presents quite frequent minor changes and some periodic major innovations in questionnaire, definitions, classifications and several other features of the survey process. Fortunately enough, preliminary analyses showed that these changes have no impact on the comparability of the 8 panels *within* each of the three two-year periods. Moreover, also the comparison *between* the two periods covered by the RTFL, 1995/96 and 2000/01, is not affected by those changes.

On the contrary, the comparison between the two periods covered by the RTFL and the 2005/06 period covered by the RCFL is strongly affected by the important changes that occurred when passing from the quarterly to the continuous survey. In the sequel, we will point out all instances in which the comparisons need special caution, and will try to gauge the direction of possible biases. As the RCFL offers more information in many stages of our evaluation strategy, and this information is crucial in order to obtain more reliable estimates of the effect of interest, our analysis will focus on the 2005/06 sample.

For each of the three periods, we merge the 8 longitudinal samples in order to obtain a reasonably large pooled sample for the analyses. The population of interest is first defined with reference to the state in *t*0: potential treated and controls are those in the working age (15-64) who are not working in the first wave. Table 2 shows the sample size and the state in *t*0 for the three samples. We consider 'at risk' of treatment all non-employed in *t*0[10]: 106,151 in 1995/96, 98,797 in 2000/01, 72,139 in 2005/06.

3.1. Definition of the treatment and control groups

According to B&S (2008: 129), we define the treatment as «*the generality of the many different schemes of temporary employment existing in Italy*».

Table 2: *Sample size (15-64) and labour force state at t*0*, by two-year period*

*State in t*0	*1995/96*		*2000/01*		*2005/06*	
	No.	%	No.	%	No.	%
Total	213,093	*100.0*	207,759	*100.0*	163,900	*100.0*
Employed	106,942	*50.2*	108,962	*52.5*	91,761	*56.0*
Unemployed	14,454	*6.8*	13,148	*6.3*	7,140	*4.4*
Out of the labour force	91,697	*43.0*	85,649	*41.2*	64,999	*39.6*

[9] Due to the lack of an individual longitudinal code in the RTFL, the longitudinal samples were produced using the probabilistic record linkage procedure by Paggiaro and Torelli (1999). The RCFL introduced a reliable personal identification code, so that a deterministic exact record linkage is possible.

[10] We will take into account the differences between unemployed and out-of-the labour force people in *t*0 as covariates within the matching procedure.

Actually, data from the RTFL do not allow us to specify further the kind of temporary job one holds. The only question related to temporary employment is about the type of contract for employees: in this question, they are requested to classify themselves as permanent or temporary workers, and in the latter case they are asked why they hold a temporary position. As B&S (2008: 132) point out, «*the relevant filter is the respondent's perception. So it is possible that the workers identified as temps in the LFS include people with permanent arrangements who feel their job is unstable. Symmetrically, the permanents may include people with temporary arrangements who perceive their work position as permanent, this being the common understanding by the worker and the firm*».

The situation is definitely different with the new RCFL, which provides a breakdown by type of temporary contract: fixed-term contract, apprenticeship and other training-related schemes, temporary work agency contract, etc.. Thus, in principle it could be possible to compare the effects of every type of temporary contract separately, by considering each of them as a specific treatment. However, empirically the only sub-group which might be analysed separately is fixed-term contracts, the others being too small and affected by some missing values.

Finally, in the RCFL it is also possible to identify temporary contracts among workers who are formally self-employed (the so-called 'quasi subordinate' workers – *collaborazioni coordinate e continuative* and *collaborazioni occasionali*). However, we will exclude these workers from the treatment group, as preliminary analyses and backward tests showed that they are much different from the bulk of temporary employees in terms of past and future outcomes. Moreover, this allows us to compare basically the same cluster of contracts – temporary contracts for employees – over the three time periods.

Turning to controls, there are different measurements of unemployment, depending on the operational specification of labour market attachment chiefly in terms of job search intensity and elapsed time from the last search (Battistin, Rettore and Trivellato, 2007). We checked the sensitivity of our results to different definitions of unemployment, and found that the control group closer to the treatment one in terms of observable characteristics is made up of the unemployed defined according to the Eurostat guidelines (not at work, immediately available for work and actively searching during the last month). Thus, we stick on the official definition of unemployment by Istat (the Italian statistical agency).

3.2. Definition of the outcome variables

Largely following B&S, the short-term outcomes we consider, one year after entering the treatment/control status, are three binary variables:

- whether at work or not: we call it "Employment";
- whether holding a permanent job or not (self-employment included[11]): we call it "Permanent employment";

[11] The main reason for that inclusion is that the job duration of self-employment is potentially permanent (in

- whether holding a satisfactory job: we call it "Satisfactory employment". Operationally, moving from the answers to the LFS questionnaire a worker is considered as not satisfied either in the case s/he is searching for another job and/or in the case s/he holds a temporary job because s/he «could not find a permanent job» and/or in case s/he holds a part-time job because she «could not find a full-time job». The complement provides us with workers with satisfactory employment.

With reference to these outcome variables two comments are in order. First, the comparability of results over time is weakened by the fact that almost all variables involved in the definition of the outcomes went through changes when the new RCFL was introduced. As a prominent example for "Satisfactory employment", «*since 2004 involuntariness is investigated separately from the contractual arrangements (and other features) breakdown. This has led to a discontinuity in the share of people who are involuntarily temporary employees*» (B&S 2008: 163).

Second, as the time lag between the measurement of the outcome and the inflow into temporary employment is one year, or slightly more, there might be some overlap between treatment and outcome. This is relevant mainly when the temporary contract is longer than one year[12]. One should pay attention to this feature, intrinsic to the dataset used, when interpreting the results.

4. Descriptive evidence

Table 3 presents the average outcomes at time $t2$ by treatment status at time $t1$ and by gender, separately for the three periods. The first common evidence is that both the group size and the average outcomes are similar across the two samples from the RTFL (1995/96 and 2000/01), while large differences emerge with respect to the RCFL sample (2005/06). Caution is needed to disentangle how much these differences are due to changes in the survey design and instruments and how much to the actual dynamics of the phenomenon.

As for the size of the treatment and control groups, the number of treated in each sample is over 1,300 (more than four times larger than in B&S), large enough to allow stratification by gender and area. As regards controls, there are about 9 unemployed for each temporary employee in 1995/96 and 2000/01 (close to the ratio found by B&S), while in 2005/06 the ratio drops to approximately 3 controls for each treated – an indirect, but clear sign of the growth of temporary employment[13].

As regards the average outcomes at time $t2$ and their differences across the two treatment arms, let us look first at the "Employment" rate. The main evidence is as

any case, we have no information about its termination). Nevertheless, as in the RCFL we identify 'quasi subordinate' workers, we will exclude them from the permanent employment outcome. They are about 2% for all groups, and never affect the main results.

[12] Note, however, that the main evidence reported here remains qualitatively the same when dropping from the treated samples contracts lasting more than one year.

[13] There is a strong increase in the absolute number of inflows into temporary contracts in 2005/06. It is even stronger in relative terms if we consider that the overall sample size is smaller.

Table 3: *Sample size and average outcomes (%) at t2 by year and treatment status at t1, and by gender*

	Outcome	*1995/96*		*2000/01*		*2005/06*	
		Treatment status		Treatment status		Treatment status	
		Temp.	Unempl.	Temp.	Unempl.	Temp.	Unempl.
All	No.	1,380	13,006	1,362	11,680	1,845	5,447
	Employment rate	65.1	18.5	69.4	17.3	65.1	27.3
	Perm. empl. rate	28.0	12.5	27.2	11.0	17.3	14.2
	Satisf. empl. rate	36.7	11.6	39.3	11.3	22.8	11.2
Men	No.	699	6,042	623	5,300	835	2,477
	Employment rate	66.5	22.5	70.1	20.8	67.9	32.5
	Perm. empl. rate	32.6	16.0	27.6	14.0	20.1	18.8
	Satisf. empl. rate	38.5	14.7	39.5	14.4	26.2	15.1
Women	No.	681	6,964	739	6,376	1,010	2,970
	Employment rate	63.6	14.9	68.7	14.4	62.8	23.0
	Perm. empl. rate	23.3	9.5	26.9	8.5	15.1	10.4
	Satisf. empl. rate	34.8	8.8	39.1	8.7	19.9	7.9

follows:

- For the treated – temporary employees – the employment rate is 65% in 1995/96 and 2005/06, and 69% in 2000/01. Differences across gender are from 2 to 5 percentage points (p.ps) in the three periods.
- For the controls – unemployed – the corresponding rates are much lower: about 18% in the two RTFL samples, they rise to 27% in the RCFL sample; this happens consistently by gender. Differences across gender are much higher, and rise from 6-7 to almost 10 p.ps.
- Differences between treated and controls are very heterogeneous among the sub-groups. They go from 35% for men in 2005/06 to 54% for women in 2000/01. Overall, in 2005/06 the increase for the controls, and a sort of parallel decrease for the treated, takes to lower differences in all sub-groups.

Turning to the "Permanent employment" rate, we get the following main findings:

- It is always much lower than the corresponding Employment rate, *i.e.* a large fraction of the employed at time *t*2 are temporary in both treatment arms. The difference between the Permanent employment and Employment rates is particularly striking for the treated; to some extent possibly because a fraction of temporary employees are 'locked in' by contracts longer than one year.
- In the RTFL samples the rates for the treated are about 28%, while in the 2005/06 sample they drop to 17%[14]. For the controls the evidence is the other way, as the rate slightly grows from 11% in 2000/01 to 14% in 2005/06.

[14] The exclusion of 'quasi subordinate' workers, about 2%, from the RCFL outcomes has comparatively small consequences.

- As a consequence, differences between treated and controls are much lower in the RCFL sample. The overall difference is 16% in 1995/96 and 2000/01, while it drops to 3% in 2005/06.

Finally, looking at the "Satisfactory employment" rate, the key evidence can be summarised as follows:

- The rates for the treated drop from about 38% in the RTFL samples to 23% in RCFL[15]. The corresponding rates for controls are about 11% in all samples. This differential trend is consistent both for men and women.
- As a consequence, the differences between treated and controls are much lower in the 2005/06 sample: from 23-28 to 12 p.ps, with no relevant differences across gender.

5. Estimation of the causal effect for the 2005/06 sample

Results in Section 4 are about the rough comparison of average outcomes between treated and controls. Now we turn to the estimation of causal effects using the strategy outlined in Section 2. To get rid of the selection bias problem, we balance the two treatment arms with respect to a set of observable characteristics of the worker, of his/her household as well as of the local labour market, which on *a priori* grounds should affect both treatment and outcomes. Characteristics we use for the 2005/06 sample are in Tables A1 to A3 in Appendix A. All variables refer to $t0$, unless otherwise stated.

Here is a summary list (gender is excluded, as we will use it throughout as a stratification variable):

- Age and marital status;
- Level of education (plus if s/he is student or not);
- Labour market state (plus details about job search actions and intensity, and entitlement to unemployment benefits);
- Previous working experience, whether recent or not, whether temporary or not;
- Household characteristics: size, age composition, educational levels, number of employed and unemployed (additional variables from the RTFL, with respect to B&S, for 1995/96 and 2000/01);
- Labour force state (four dummies: employed, temporary, quasi subordinate, unemployed) one year before $t0$, a dummy if an unemployed receives unemployment insurance or income support from *Liste di mobilità*[16], a dummy if a person left school during the last five years (additional variables provided by the RCFL for 2005/06);
- Activity rate, unemployment rate, and temporary/total employed rate by province (to proxy the conditions of the local labour market),

[15] This is partly related to changes in the definition of "Satisfactory employment", specifically to the different way undesired temporary jobs are detected.

[16] *Liste di mobilità* is a programme designed to handle redundancies in the labour market. It includes a 'passive' component' – monetary benefits with a high replacement rate, varying according to the worker's age – only for workers who have been dismissed by firms larger than 15 employees. See Paggiaro, Rettore and Trivellato (2009).

- Area of residence (interacted with the interview's year in order to capture local trends).

Tables A1 and A2 also present the distribution of most variables by treatment status and gender. Most of the differences across the two treatment arms are statistically significant at the 5% level.

As for the matching procedure, we used the *Psmatch2* Stata programme (Leuven and Sianesi 2003) trying with different matching methods for a sensitivity analysis. Results presented here use kernel matching with Epanechnikov kernel and bandwidth .01, keeping into the analysis only units within the common support[17].

5.1. Backward test

Table 4 shows the results for the backward test described in Section 2.2. The three outcomes we consider are those defined in Section 3.2, except that here they refer to periods *before t*0, *t*0 |– *t*0+1 being the quarter in which subjects entered one or the other of the two treatment arms.

Here we take the quarter *t*0-4 as the reference period for the test (see Figure 1). That is, we look at the current labour market state recorded at the interview one year before *t*0. In addition, we consider also the labour market state at quarter *t*0-8, since it is retrospectively recorded at each wave, thus also at the interview one year before *t*0[18].

The results for the 'unmatched' samples show that in the pre-treatment quarters *t*0-4 and *t*0-8 treated were quite a lot different from controls with respect to their labour market state; this is apparent both for men and women. As an example, about 29% of both men and women in the treatment group were employed one year before, while the corresponding rates drop to 21% for men and to 14% for women in the control group. These differences point to a major unbalance between the two treatment arms with respect to characteristics relevant for the subsequent employment history.

When we turn to the 'matched' samples all pre-treatment differences disappear. That is, balancing the two treatment arms with respect to the available characteristics appears to be enough to solve the selection bias problem. Note however that differences between the two matched treatment arms are larger, albeit not statistically significant, for women than for men, pointing to a possible residual selection bias problem for the former ones.

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[17] This option excludes the treated units whose *p*-score is outside the range estimated for controls. The exclusion of a few treated units accounts for the small differences in the average outcomes which will be found between matched and unmatched treated units.

[18] As just pointed out, this information was not currently collected by the RTFL.

Table 4: *Backward test, whole sample 2005/06, by gender* [(a)]

Outcome	*Sample*	*Temp.*	*Unempl.*	*Diff.*	*St. error*	*t-stat.*	*Signif.*
			Men				
Employment rate at *t*0-4	Unmatched	29.85	21.43	8.42	1.72	4.88	***
	Matched	29.51	28.19	1.32	2.14	0.62	
Perm. empl. rate at *t*0-4	Unmatched	15.80	15.21	0.59	1.46	0.40	
	Matched	15.94	16.33	-0.39	1.79	-0.22	
Satisf. empl. rate at *t*0-4	Unmatched	13.47	10.93	2.54	1.30	1.95	*
	Matched	13.34	13.16	0.18	1.61	0.12	
Employment rate at *t*0-8	Unmatched	29.04	24.88	4.16	1.78	2.33	**
	Matched	28.92	29.46	-0.54	2.19	-0.25	
Temp. empl. rate at *t*0-8	Unmatched	11.73	6.12	5.61	1.08	5.18	***
	Matched	11.45	10.88	0.57	1.39	0.41	
Unempl. rate at *t*0-8	Unmatched	34.03	47.86	-13.83	2.00	-6.92	***
	Matched	33.65	33.81	-0.16	2.40	-0.07	
No.		861	2,058				
			Women				
Employment rate at *t*0-4	Unmatched	28.95	13.93	15.03	1.38	10.85	***
	Matched	28.73	26.95	1.78	2.03	0.87	
Perm. empl. rate at *t*0-4	Unmatched	11.56	8.16	3.40	1.05	3.25	***
	Matched	11.72	12.44	-0.72	1.53	-0.47	
Satisf. empl. rate at *t*0-4	Unmatched	11.75	5.49	6.26	0.94	6.64	***
	Matched	11.82	10.11	1.71	1.39	1.23	
Employment rate at *t*0-8	Unmatched	24.23	14.08	10.15	1.36	7.48	***
	Matched	24.39	25.29	-0.90	1.99	-0.45	
Temp. empl. rate at *t*0-8	Unmatched	11.47	4.51	6.96	0.89	7.79	***
	Matched	11.53	11.04	0.49	1.32	0.37	
Unempl. rate at *t*0-8	Unmatched	30.53	39.47	-8.94	1.74	-5.12	***
	Matched	30.15	28.97	1.18	2.51	0.47	
No.		1,081	2,549				

[(a)] Significance level: *** 1%; ** 5%; *10%.

5.2. Estimates of the causal effects

Table 5 presents the results by gender. The overall evidence is that matching nearly always reduces the differences between the two treatment arms, in some cases by a large extent. Nevertheless, the differences remain all positive and significant at the 5% level, with just one exception. The experience of a temporary employment spell *vs.* a spell of unemployment at time *t*1 takes to a 30 p.ps higher Employment rate for men, and 35 p.ps for women, one year later. The causal effect on the probability to hold a Permanent employment one year later is not statistically significant for men, while it is as large as 4 p.ps for women. Finally, the causal effect on the probability to hold a Satisfactory employment one year later is about 9.5 p.ps both for men and women.

Table 5: *Estimates of the causal effects, whole sample 2005/06, by gender* [(a)]

Outcome	*Sample*	*Temp.*	*Unempl.*	*Diff.*	*St. error*	*t-stat.*	*Signif.*
			Men				
Employment rate at *t*1+4	Unmatched	67.90	32.45	35.45	1.87	18.93	***
	Matched	67.80	38.11	29.69	2.36	12.57	***
Perm. empl. rate at *t*1+4	Unmatched	20.12	18.77	1.35	1.57	0.86	
	Matched	20.17	19.14	1.03	1.99	0.52	
Satisf. empl. rate at *t*1+4	Unmatched	26.23	15.06	11.17	1.52	7.34	***
	Matched	26.00	16.56	9.44	2.01	4.69	***
No.		835	2,477				
			Women				
Employment rate at *t*1+4	Unmatched	62.77	22.99	39.78	1.59	24.95	***
	Matched	62.24	27.63	34.61	2.32	14.87	***
Perm. empl. rate at *t*1+4	Unmatched	15.05	10.37	4.68	1.16	4.03	***
	Matched	15.33	11.37	3.96	1.71	2.32	**
Satisf. empl. rate at *t*1+4	Unmatched	19.90	7.95	11.95	1.12	10.64	***
	Matched	19.55	9.81	9.74	1.69	5.76	***
No.		1,010	2,970				

[(a)] Significance level: *** 1%; ** 5%; *10%.

5.3. The heterogeneity of the causal effects

To understand whether the causal effect of temporary contracts is heterogeneous across different sub-groups of the population, we carried out extensive analyses by stratifying our samples with respect to many characteristics observed in *t*0, *i.e.* before the treatment. Here we present the main evidence[19].

The only characteristic for which we found significant heterogeneity is geographical area. Table 6 summarises the estimated impacts with reference to a binary stratification: Centre-North and South (Islands included). The upper part of the table documents the results of the backward test. It leads to accepting the null hypothesis of no unbalance left after balancing on the observable characteristics for the Centre-North, while in the South there are some significant differences at the 10% level, and one of them – women, Employment as the outcome – is significant also at the 5%.

Taking this into account, the lower part of the table presents the estimates of the causal effects. For men they vary a lot across areas. The impact of temporary employment on the probability to hold a Permanent job one year later is positive in the Centre-North and negative in the South, with about the same absolute size of 6 p.ps. The impact on the probability to hold a Satisfactory job is 15 p.ps in the Centre-North, statistically zero in the South. On the contrary, differences across areas are negligible for women[20].

[19] Some further results are available from the authors on request.

[20] Note that, compared to the whole sample, the point estimates for the effect on Permanent employment are

Table 6: *Backward test and estimates of the causal effects, 2005/06 sample, by gender and area* [(a)]

Outcome	*Sample*	*Men North*		*Men South*		*Women North*		*Women South*	
		Diff.	Sign.	Diff.	Sign.	Diff.	Sign.	Diff.	Sign.
Backward test									
Employment rate at *t*0-4	Unmatched	3.73		10.04	***	8.87	***	19.36	***
	Matched	3.67		4.58	*	2.77		6.78	**
Perm. empl. rate at *t*0-4	Unmatched	-3.25		1.97		1.23		4.06	***
	Matched	0.94		1.54		-0.68		3.99	*
Satisf. empl. Rate at *t*0-4	Unmatched	0.00		2.92	*	4.06	***	7.14	***
	Matched	2.80		1.00		2.52		2.58	
Employment rate at *t*0-8	Unmatched	-1.36		6.52	***	5.51	**	11.15	***
	Matched	2.21		3.91		-2.02		1.40	
Temp. empl. rate at *t*0-8	Unmatched	2.91		7.36	***	4.12	***	9.31	***
	Matched	0.70		3.27	*	0.07		1.75	
Unempl. rate at *t*0-8	Unmatched	-10.77	***	-10.34	***	-10.30	***	-1.07	
	Matched	-0.70		-2.83		-0.75		-1.07	
No. treated		393		468		594		487	
No. controls		665		1,393		965		1,578	
Estimate of the causal effects									
Employment rate at *t*1+4	Unmatched	31.40	***	35.12	***	35.79	***	38.50	***
	Matched	26.99	***	27.24	***	29.98	***	33.53	**
Perm. empl. rate at *t*1+4	Unmatched	3.67		-1.29		3.13	*	4.28	***
	Matched	5.57	*	-6.55	**	3.15		3.81	
Satisf. empl. rate at *t*1+4	Unmatched	15.06	***	6.35	***	11.77	***	9.84	***
	Matched	14.57	***	0.15		9.20	***	7.64	***
No. treated		376		459		568		442	
No. controls		711		1,766		1,168		1,802	

[(a)] Significance level: *** 1%; ** 5%; *10%.

6. Impact evaluation for the 1995/96 and 2000/01 samples

The information available from the RTFL is definitely less rich than for the RCFL. Thus, for the 1995/96 and 2000/01 samples we face some important restrictions. The main one is that there is no information on the labour market state one year before the interview: patently, it cannot be used as a matching variable in the selection of the controls nor in the backward test to validate the identification strategy. Moreover, information on when the worker left school is also missing, and the details on the job search actions and intensity at *t*0 are poorer.

similar, but become non significant due to the smaller sample sizes.

Table 7 reports the results of the backward test for the 1995/96 and 2000/01 samples, separately for men and women. As regards overall Employment, the observed difference between treated and controls is about 15% in 1995/96 and 17% in 2000/01.

These figures are close to those observed in 2005/06 for women, but much higher for men. Anyhow, the crucial evidence is that after matching the differences between the two treatment arms do not disappear. On the other hand, matching is effective in leading to

Table 7: *Backward test, whole samples 1995/96 and 2000/01, by gender* [(a)]

Outcome	*Sample*	*Temp.*	*Unempl.*	*Diff.*	*St. error*	*t-stat.*	*Signif.*
			Men 1995/96				
Employment	Unmatched	26.78	11.16	15.62	1.30	12.00	***
rate at *t*0-4	Matched	27.20	20.68	6.52	1.85	3.53	***
Perm. empl.	Unmatched	12.50	8.46	4.04	1.12	3.61	***
rate at *t*0-4	Matched	12.69	15.05	-2.36	1.43	-1.65	
Satisf. empl.	Unmatched	12.91	7.42	5.49	1.07	5.15	***
Rate at *t*0-4	Matched	13.11	14.21	-1.10	1.43	-0.77	
No.		728	5,886				
			Women 1995/96				
Employment	Unmatched	22.37	7.78	14.59	1.09	13.37	***
rate at *t*0-4	Matched	22.57	17.70	4.87	1.68	2.90	***
Perm. empl.	Unmatched	9.88	4.94	4.94	0.86	5.73	***
rate at *t*0-4	Matched	9.97	9.98	-0.01	1.24	-0.01	
Satisf. empl.	Unmatched	10.40	4.16	6.24	0.81	7.70	***
Rate at *t*0-4	Matched	10.50	9.23	1.27	1.24	1.03	
No.		769	6,774				
			Men 2000/01				
Employment	Unmatched	27.43	10.18	17.25	1.32	13.05	***
rate at *t*0-4	Matched	27.54	22.49	5.05	1.94	2.61	***
Perm. empl.	Unmatched	12.24	7.07	5.17	1.09	4.75	***
rate at *t*0-4	Matched	12.28	13.52	-1.25	1.47	-0.85	
Satisf. empl.	Unmatched	14.45	6.53	7.92	1.07	7.39	***
Rate at *t*0-4	Matched	14.52	13.91	0.61	1.54	0.40	
No.		678	4,990				
			Women 2000/01				
Employment	Unmatched	24.81	7.15	17.66	1.09	16.22	***
rate at *t*0-4	Matched	24.80	18.15	6.65	1.75	3.80	***
Perm. empl.	Unmatched	8.88	4.39	4.49	0.83	5.43	***
rate at *t*0-4	Matched	8.84	10.21	-1.37	1.20	-1.14	
Satisf. empl.	Unmatched	10.97	3.84	7.13	0.81	8.85	***
Rate at *t*0-4	Matched	10.82	9.88	0.94	1.27	0.74	
No.		766	5,862				

[(a)] Significance level: *** 1%; ** 5%; *10%.

not significant differences with respect to the two other outcomes, Permanent and Satisfactory employment.

Overall, the backward test shows that after matching some differences remain between the two treatment arms relevant for their pre-treatment working histories. As the difference is not significant for Permanent (and Satisfactory) jobs, this means that temporary employees in *t*1 were also more likely to be temporary one year before. Thus, the impact estimates for 1995/96 and 2000/01 have definitely to be taken with caution.

Table 8: *Estimates of the causal effects, whole samples 1995/96 and 2000/01, by gender* [(a)]

Outcome	*Sample*	*Temp.*	*Unempl.*	*Diff.*	*St. error*	*t-stat.*	*Signif.*
			Men 1995/96				
Employment rate at *t*1+4	Unmatched	66.52	22.54	43.98	1.69	25.97	***
	Matched	66.33	31.78	34.55	2.04	16.97	***
Perm. empl. rate at *t*1+4	Unmatched	32.62	15.97	16.65	1.51	11.01	***
	Matched	32.94	20.69	12.25	1.98	6.20	***
Satisf. empl. rate at *t*1+4	Unmatched	38.49	14.70	23.79	1.48	16.09	***
	Matched	38.49	20.17	18.32	2.02	9.06	***
No.		699	6,042				
			Women 1995/96				
Employment rate at *t*1+4	Unmatched	63.58	14.93	48.65	1.48	32.81	***
	Matched	63.06	23.09	39.97	2.07	19.35	***
Perm. empl. rate at *t*1+4	Unmatched	23.35	9.48	13.87	1.23	11.26	***
	Matched	23.72	13.80	9.92	1.80	5.51	***
Satisf. empl. rate at *t*1+4	Unmatched	34.80	8.83	25.97	1.23	21.15	***
	Matched	33.93	13.43	20.50	1.96	10.44	***
No.		681	6,964				
			Men 2000/01				
Employment rate at *t*1+4	Unmatched	70.14	20.83	49.31	1.74	28.27	***
	Matched	70.41	31.84	38.57	2.15	17.90	***
Perm. empl. rate at *t*1+4	Unmatched	27.61	13.98	13.63	1.52	8.97	***
	Matched	27.97	19.73	8.24	2.05	4.03	***
Satisf. empl. rate at *t*1+4	Unmatched	39.49	14.44	25.05	1.56	16.06	***
	Matched	39.51	22.31	17.20	2.20	7.83	***
No.		623	5,300				
			Women 2000/01				
Employment rate at *t*1+4	Unmatched	68.74	14.43	54.31	1.42	38.32	***
	Matched	68.84	26.54	42.30	1.95	21.66	***
Perm. empl. rate at *t*1+4	Unmatched	26.93	8.53	18.40	1.17	15.75	***
	Matched	27.08	14.50	12.58	1.80	6.97	***
Satisf. empl. rate at *t*1+4	Unmatched	39.11	8.72	30.39	1.20	25.23	***
	Matched	39.32	17.26	22.06	1.96	11.28	***
No.		739	6,376				

[(a)] Significance level: *** 1%; ** 5%; *10%.

Table 8 shows the impact estimates for these two samples, separately for men and women. The main result is that in both periods all estimated impacts are highly significant and much larger than those estimated for the 2005/06 period (see Table 5). The critical point, however, is that it is not possible to establish how much these differences are true causal effects and how much they are due to the selection bias left behind by matching detected by the backward test.

7. The role of additional matching variables for the 2005/06 sample

Summing up, a crucial piece of evidence from the previous two sections is that the matching estimator passes the backward test for the 2005/06 period, thanks to the richer information provided by the RCFL, while it fails to pass it for 1995/96 and 2000/01. For these two periods the set of variables available for matching from the RTFL is not enough to solve the selection bias problem.

Thus, it would be interesting to explore which additional variables from the RCFL matter for the different performance of propensity score matching. To get a clue to that issue, we simulate a matching with (approximately) the same variables from the RTFL for 2005/06, and compare the results to those obtained using the whole set of variables from the RCFL.

Table 9 exhibits the results for the women sample. It is apparent that the additional variables available just from the RCFL, indeed essentially the four dummies for 'Labour force state one year before *t*0', are crucial for the specification test to pass.

Table 9: *Specification test with different sets of matching variables, 2005/06 sample, women* [(a)]

Outcome	*Sample*	*Temp.*	*Unemp.*	*Diff.*	*t-stat.*	*Signif.*
Empl. rate at *t*0-4	Unmatched	28.95	13.93	15.03	10.85	
	Matched ALL	28.73	26.95	1.78	0.87	
	Matched RTFL	28.89	23.62	5.27	2.64	**
	ALL – LabourSt_{-4}	28.64	22.96	5.68	2.86	***
	ALL – Ub	28.88	25.80	3.08	1.54	
	ALL – Stud5y	28.93	26.04	2.89	1.46	

Legenda: Matched ALL: matching with all variables available from the RCFL (as above).
Matched RTFL: matching with the set of variables mimicking those from the RTFL (i.e., excluding the variables available just from the RCFL).
ALL – LabourSt_{-4}: matching excluding only the four dummies on 'Labour force state one year before *t*0'.
ALL – Ub: matching excluding only the dummy 'Receives unemployment insurance or income support from *Liste di mobilità*'.
ALL – Stud5y: matching excluding only the dummy 'Left school during the last five years'.

(a) Significance level: *** 1%; ** 5%.

The result is in line with expectations, as there is abundant theoretical and empirical evidence that previous work history is a quite good predictor of subsequent labour market outcomes. By the way the result is not trivial, as the dummies for 'Labour force state one year before *t*0' refer to a retrospective, self-declared state. Thus, they not rarely differ from the actual state reported at *t*0-4, because of the different format of collecting the relevant information and the presence of recall errors[21].

Besides, it is worth stressing that results in Tables 4 and 6 show that balancing with RCFL data is effective also for time *t*0-8, for which there is no covariate available at *t*0.

8. Summary and conclusions

We moved from the approach proposed by B&S (2008) to estimate the causal effect of a spell of temporary employment *vs.* a spell of unemployment on short-term labour market histories. We exploited the longitudinal features of the Italian LFS and used propensity score matching to compare those who enter a temporary job to the unemployed with respect to their labour market state, and its subjective assessment, one year later. We extended B&S's approach and results in various directions.

As the first important methodological contribution, we show how to exploit the LFS rotating sampling scheme to obtain a backward test for the ignorability condition, on which the identification strategy crucially relies. As many national labour force surveys feature this sampling scheme – or a similar one, our test should straightforwardly apply to many other countries.

We evaluate the causal effect in three different periods characterized by different degrees of regulation of temporary employment. By applying our backward test to the 1995/96 and 2000/01 samples, based on the old RTFL, we find that the comparison of the treatment group to the matched control group is potentially affected by selection bias (indeed, this should apply also to B&S's results). On the contrary, the test does not point to the existence of selection bias for the 2005/06 sample, which exploits a much richer set of information made available by the new RCFL. This is in line with what B&S (2008: 153) expected: «*the better quality of the new LFS conducted since 2004 seems promising in order to better deal with the heterogeneity between the temps and the people who remain unemployed*».

Finally, the use of many pooled samples allows us to obtain larger sample sizes than B&S, with more precise estimates and the possibility of stratifying the analysis by gender and area.

As regards the estimate of the causal effects, overall the effects we find are much smaller than those found by B&S. When compared to matched unemployed, being temporary at time *t*1 takes to a 30% higher Employment rate for men, 35% for women. But this difference is mostly due to temporary and unsatisfactory jobs. When we look at the causal effect on the probability to hold a Permanent job one year later we find no effect for

[21] See Bernard *et al.* (1984), Bound, Brown and Mathiowetz (2001), and Tourangeau, Rips and Rasinski (2000), among others.

men, while the causal effect for women is as large as 4 p.ps. On looking at the causal effect on the probability to hold a Satisfactory job one year later, we find a figure as large as 9.5 p.ps both for men and women, still much smaller than the one found by B&S.

We also find evidence of heterogeneity of the causal effects across areas. The null effect on Permanent employment we find for men at the aggregate level results from a positive effect in the Centre-North and a negative one in the South; as regards Satisfactory employment, the effect is about 15 p.ps in the North, while it is nil in the South. This evidence is consistent with B&S, who found that the effect is larger in better performing labour markets[22].

Finally, as B&S (2008: 141) point out, it is important to keep in mind that «*the presence (and broad changes) of temporary arrangements is likely to have systemic effects upon job matching, supply of jobs, wage bargaining, etc.*». Thus, the partial equilibrium frame of our impact analysis implies that the results should be regarded as an interesting contribution, but still fall short from a conclusive answer to the issue of the overall effects of temporary employment – general equilibrium ones included.

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[22] As for impact heterogeneity, the analysis is severely limited by the treatment groups size. A tentative "Partially" Interacted Linear Model does not add any significant variables or interaction terms to gender and area. Results are available from the authors on request.

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Appendix A: Covariates for the propensity score in the 2005/06 sample

Table A1: *Distribution of personal characteristics by gender and by treatment status at time t1*

Variable	*Description*	*Men*		*Women*	
		Treated	Controls	Treated	Controls
Age1	$15 \leq$ age ≤ 24 (baseline)	.35	.30	.25	.23
Age2	$25 \leq$ age ≤ 34	.27	.30	.27	.32
Age3	$35 \leq$ age ≤ 44	.19	.19	.25	.30
Age4	$45 \leq$ age ≤ 54	.13	.14	.18	.13
Age5	$55 \leq$ age ≤ 64	.06	.07	.05	.02
Reg1	North-West (baseline)	.17	.10	.20	.16
Reg2	North-East	.16	.07	.22	.11
Reg3	Centre	.12	.10	.14	.12
Reg4	South	.35	.47	.33	.42
Reg5	Islands	.20	.24	.11	.19
Single	1 if never married (baseline)	.63	.65	.44	.48
Married	1 if married in *t*0	.34	.31	.50	.44
Mar_Past	1 if divorced or widowed	.03	.04	.06	.08
Stud	1 if student in *t*0	.16	.11	.15	.09
Stud5y	1 if left the school during the last 5 years	.22	.22	.15	.20
Grad	1 if college graduated	.07	.07	.13	.12
Hsch	1 if high school graduated	.40	.38	.42	.45
Unemp	1 if unemployed (Eurostat definition) in *t*0	.44	.57	.40	.50
Unemplt	1 if long-term unemployed	.19	.30	.19	.26
Avail0	1 if available to work in the next 2 weeks	.61	.82	.52	.77
Srctemp	1 if searching for a temporary job	.02	.01	.02	.02
Srcacct	1 if would accept a temporary job	.30	.49	.27	.46
Srcft	1 if searching for a full-time job only	.25	.37	.16	.22
Srcmove	1 if available to move to find a job	.09	.15	.04	.07
Srcpriv	1 if actively searching in the private sector	.36	.55	.29	.50
Srcexch	1 if searching in public labour exchanges	.13	.18	.11	.18
Ub	1 if receives unemployment benefits	.14	.05	.14	.03
Exp	1 if has previous working experience	.74	.65	.76	.60
Exprec	1 if experience in the last 2 years	.64	.42	.63	.30
Exptemp	1 if last job was temporary	.44	.23	.49	.19
Lyemp	1 if employed 1 year before *t*0	.22	.18	.18	.10
Lytd	1 if temporary 1 year before *t*0	.11	.06	.10	.04
Lycoco	1 if parasubordinate 1 year before *t*0	.01	.01	.01	.01
Lyunemp	1 if unemployed 1 year before *t*0	.47	.62	.41	.50
Pr_Part	Participation rate in the province	.60	.57	.62	.59
Pr_Unemp	Unemployment rate in the province	.10	.12	.08	.10
Pr_Temp	Temp./total employed rate in the province	.11	.12	.11	.11

Table A2: *Distribution of household characteristics by gender and by treatment status at time t1*

Variable	*Description*	*Men*		*Women*	
		Treated	Contr.s	Treated	Contr.s
Son	1 if son of the head of household	.57	.58	.39	.43
Famkids	1 if there are kids under 15 in the household	.32	.28	.37	.39
Famold	1 if there are elders over 64 in the h.	.12	.16	.12	.12
Famemp1	1 if there is 1 employed in the h.	.35	.39	.53	.57
Famemp2	1 if there are 2 or more employed in the h.	.25	.16	.21	.16
Famtemp	1 if there are temporary workers in the h.	.12	.11	.13	.11
Famunemp	1 if unempl. in the h. (other than the resp.)	.11	.19	.11	.15
Famgrad	1 if graduates in the h. (other than the resp.)	.09	.08	.11	.08
Famhsch	1 if hs degree in the h. (other than the resp.)	.46	.45	.50	.45
Ntot	Number of members of the household	3.62	3.54	3.50	3.52

Table A3: *Other variables included in the propensity score*

Variable	*Description*
Qrt1-Qrt8	Quarter of the sample (7 dummies)
Src*	Other details about job search intensity
Inact5-Inact7	Different classifications for inactive job search
Y06reg2-Y06reg5	Interactions between regions and years
Fs_*	Interactions between son and other family variables