Temporary Work Agencies in Italy: A Springboard Toward Permanent Employment?*

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Abstract

This paper measures to what extent Temporary Work Agency (TWA) employment creates a "springboard" toward permanent jobs, or a "trap" of endless precariousness. Applying Propensity Score matching in the presence of choice-based sampling, we estimate the causal effect of the treatment "TWA assignment" on the outcome "finding a permanent job after 18 months". The data come from Italy, where TWA employment was liberalized in 1997, and they were specifically collected for this evaluation study. We show that a TWA assignment increases the probability of finding a permanent job by 19 percentage points in Tuscany and by 11 percentage points in Sicily, although this second effect is only barely significant. These effects are large given that the observed baseline probabilities in the treated group are respectively 31% and 23% in the two regions. This treatment effect is highly heterogeneous with respect to observable characteristics such as age, education and firm's sector.

JEL Classification: C2, C8, J6.

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1 Introduction

Policy makers and labor market analysts are becoming increasingly concerned about the growth of temporary employment in Europe. According to OECD (1999), during the 90s there was a considerable continuity in the employment protection legislation of most countries, with one major exception: the deregulation of fixed-term contracts and temporary work agencies. Particularly in southern European countries, changes of labor market policy consisted mainly of measures aimed at introducing "flexibility at the margin", i.e., making the utilization of non-permanent contracts more loosely regulated while leaving the discipline of standard employment unchanged. In those countries where standard employment is subject to a very rigid legislation, the increasing flexibility at the margin had a strong effect on the diffusion of temporary contracts.[1]

The growing share of temporary employment in many European countries raised concerns over the risk of labor market "segmentation". Several studies have indicated the existence of a gap in the working conditions of permanent and temporary employees, particularly in terms of wages and working rights.[2] Triggered by this gap, public opinion and policy makers have stressed the importance of searching "an appropriate balance between flexibility and security" (European Commission, 2003). It is the so called "flexicurity" approach, which aims at squaring the circle of ensuring flexibility, job security and job quality, all at the same time.

While the evidence seems to suggest that "squaring the circle" is not an easy task in a cross-sectional sense, it could be that for most individuals

[1] Similarly, in the US, the recent growth of TWA employment appears to be related to the increasing strictness of unjust dismissal doctrine in many states (Autor, 2000).

[2] See the literature survey in OECD (2002).

"the circle is squared" in an intertemporal sense. This is because a temporary job may represent a costly investment that a young worker undertakes to increase the probability of finding a permanent job. Several theoretical arguments can be constructed to justify this intuition, mostly based around the idea that -in the presence of asymmetric information- a temporary contract is a costly signal that allows the worker to inform the market about her ability (Nannicini, 2005). However, different theoretical arguments can be raised to argue that temporary jobs depreciate human capital and lower worker's probability to jump into a stable position. Ultimately, it is an empirical question whether temporary jobs are an effective springboard toward permanent employment or a "trap" of endless precariousness.

This paper will attempt to answer this question with specific reference to Temporary Work Agency (TWA) employment. TWA employment represents a triangular contract, in which an agency hires a worker for the purpose of placing her at the disposal of a client firm for a temporary assignment. The analysis refers to Italy, where this kind of non-standard employment was liberalized in 1997. Specifically, the goal of our study is to evaluate whether the treatment consisting in a "TWA assignment" has a positive and significant causal effect on the outcome "finding a permanent job after 18 months". We will use a unique data set, which was collected precisely to perform this evaluation exercise. The data consist of the universe of TWA workers who went into an assignment during the first six months of 2001 (the "treated" group), which is then compared to a sample of workers who, at the beginning of this period, were unemployed or employed with a non-permanent contract (the "control" group). Interest lies in the average effect of treatment on the treated, i.e., in the difference between the outcome for the workers in the treated group with respect to the counterfactual unobservable outcome which would have prevailed for them in the absence of the TWA assignment. The estimation method of Propensity Score matching in the presence of choice-based sampling will be used to identify this effect.

The structure of the paper is as follows. Section 2 describes the take-off of TWA employment in Italy. Section 3 briefly mentions the possible determinants of the transition from temporary to permanent employment. Section 4 jointly discusses the methodological problems raised by our evaluation question, the implemented data collection strategy, and sample descriptive statistics. Section 5 presents the results of different Propensity Score matching estimations. Section 6 draws some conclusions.

2 Temporary work agencies in Italy

Italy is a good example of the trend toward flexibility at the margin which has characterized several European countries since the 90s. Undoubtedly, the major step toward the liberalization of non-standard contracts has been the so-called "Treu law" (law 196/1997), which legalized and regulated the supply of temporary workers by authorized agencies (against the law until then).[3] Afterwards, TWAs have roared and a "hot" policy debate over the consequences of this liberalization for firms and workers has begun.

The Treu law (including subsequent modifications) states that TWA employment is allowed in all but the following cases: replacement of workers on strike, firms that experienced collective dismissals in the previous 12 months, and jobs that require medical vigilance. The law does not set a maximum cumulated duration of assignments or legal motivations for using temporary work, leaving the provision of further regulation to collective bargaining. Col-

[3] On the introduction of this kind of non-standard employment in the Italian labor law, see Ichino (2000, Chapter VI, Section III).

lective agreements have typically stipulated that temporary workers cannot exceed 8-15% of normal employees (depending on the sector). Moreover they have constrained the allowed motivations for TWAs, which are: peak activity; one-off work and need of skills not available within the firm. Firms cannot extend an individual TWA contract more than four times or a cumulation period longer than 24 months.

On the whole, in the Italian labor market, firing costs for standard employment remain high[4] and TWA employment faces less regulatory restrictions than other short-term contracts. In this context, firms might decide to hire temps in situations that generate different kinds of non-standard relationships in other countries. It should also be noted that, from the firm's point of view, using TWA employment as a tool of personnel screening and selection is less associated with a negative "hire and fire" reputation than the utilization of other temporary contracts.

Following the Treu law, implemented in 1998, TWA employment has rapidly expanded, especially in the North of the country and in manufacturing sectors.[5] Nevertheless, in 2002 TWA employment still amounted to only 0.91% of total employment, far below the level observed in countries where it developed earlier. In 1999, in fact, the overall incidence was 4.5% in the Netherlands, 3.2% in the UK, 2.5% in France, and 2.5% in the US (CIETT, 2000). The average TWA utilization in the European Union was 1.5%.

It should be noticed, however, that TWA employment is still at a take-off stage in Italy. According to CIETT (2000), Italy will outmatch the 2% level by 2010. Moreover, this instantaneous stock measure captures the per capita incidence of this type of work with respect to total employment, but not its

[4]See OECD (1999) and Nicoletti et al. (2001).

[5]For an aggregate overview, see Ministero del lavoro (2001) and Nannicini (2004).

diffusion among workers. Since turnover is high, TWA employment affects a much larger number of workers than those who are actually observed in a TWA assignment at any given point in time. Thus, it may represent a springboard toward regular employment for a larger part of the labor force. Finally, the intensity of TWA employment utilization varies widely by industry, and in 2000 it was already over 3% in manufacturing sectors such as chemicals, machinery and electronics, and transportation manufacturing (Nannicini, 2004).

3 Springboard or trap?

From a theoretical point of view, there might be two broad reasons why temporary employment could represent a "springboard" into a stable job:

- *signaling*, i.e., more-able workers might signal their type by making themselves available for screening during temporary assignments;
- acquisition of human capital (general or specific), social contacts and information about permanent vacancies.

On the other end, temporary employment might be a "trap" of endless precariousness for the following reasons:

- a TWA experience is a "bad signal" as to the lack of alternatives (especially under the firm's belief that temps have already been screened by other employers);
- a TWA experience is associated with a limited acquisition of human capital because of the high turnover (in the presence of a positive externality connecting specific to general human capital).

Leaving the discussion of some of these different mechanisms to Nannicini (2005), it suffices here to say that there is no obvious reason to expect one mechanism to prevail. In the Italian labor market, which is characterized by a high rigidity of standard employment, firms appear to be interested in TWA employment not only for screening workers but also to deal with demand fluctuations. This second motivation is typically considered as the factor that transforms TWA employment into a trap. It is, however, not obvious that this is the case. For example, a firm might hire a temporary worker to face a non-permanent increase in market demand, and decide later to use the same worker (already screened during the short-term assignment) to fill a permanent vacancy. At the end of the day, whether TWA employment is a springboard or a trap is ultimately an empirical question.

Empirical studies in other countries have shown a wide set of results, depending on the institutional setting and the evaluation strategy. Also the descriptive evidence for the period 1996-1998 shows a large cross-country variation in the transformation rate of temporary contracts into permanent positions (see OECD, 2002): from 21% (France) to 56% (Austria) in one year, or from 34% (Spain) to 71% (Austria) in two years.

Booth et al. (2002) study the labor market prospects of temporary workers in the UK (where temps represent 7% of male employees and 10% of female employees). Their results show that temporary employment is associated with lower wages, less specific training and lower job satisfaction in respect to permanent employment. But, it is not associated with negative trajectories. In particular, women that go through a temporary job are able to completely catch up to women starting in permanent positions, in terms of wage and job satisfaction. Guell and Petrongolo (2003) investigate the transformation from temporary into permanent contracts in Spain. Estimating a duration model, their study shows that temporary contracts are used by Spanish firms both for flexibility and screening motivations. Malo and Munoz-Bullon (2002) perform an optimal matching analysis for Spain, and find that TWA employment characterizes labor market trajectories with a higher probability to end with stable jobs. Lechner at al. (1999) implement an econometric evaluation of the effects of subsidized non-profit TWAs in Germany. Their matching estimates suggest that this program is associated to a statistically significant additional re-employment effect of about 13 percentage points. Kvasnicka (2005) uses administrative data from the federal employment office in Germany, and applies matching techniques to estimate the stepping-stone function of TWAs for the unemployed. His analysis does not find any discernable effect of an initial TWA assignment on the future probability of being in standard employment. Zijl et al. (2004) investigate whether temporary work raises the transition rate to permanent employment in the Netherlands, using longitudinal survey data and estimating a multi-state duration model. Their results show that temporary jobs serve as stepping stones towards regular employment, since they shorten the duration of unemployment and substantially increase the future probability of being in standard employment.

Our study adds to this empirical literature in two ways. First, it is the first evaluation analysis that investigates the stepping-stone function of temporary contracts in the interesting environment represented by the Italian labor market (as discussed in Section 2). Second, it collects on-purpose data about both temporary workers and appropriate control subjects, and applies Propensity Score matching techniques with the aim to carefully identify the eventual "springboard" effect of TWA employment.

4 How to identify the "springboard" effect of a TWA assignment

4.1 Econometric framework and data collection

The aim of our analysis is to assess whether in Italy a TWA experience has a causal effect on the probability of finding a permanent job at a certain time in the future. Such a problem of causal inference involves "what if" statements and counterfactual outcomes. Hence, it can be translated into a treatment-control situation typical of the experimental framework (Rubin, 1974). In our case, the treatment consists in experiencing a TWA assignment, against the counterfactual situation of remaining unemployed (looking for a job) or going through other temporary jobs. The outcome of interest is the individual probability of finding a permanent job 18 months later.[6]

More formally, consider a set of I individuals, and denote each of them by subscript i: $i \in \{1, ..., I\}$. At time t_0, some of these subjects are "treated", i.e., they are in a TWA assignment, whereas the others, usually named "controls", are not. The treatment indicator is $T \in \{0, 1\}$. Interest lies in the binary outcome variable indicating permanent employment at time $t_1 > t_0$ (with $t_1 - t_0 \cong 18$ months): $Y \in \{0, 1\}$. We define the causal "springboard" effect of a TWA assignment as the difference between the two potential outcomes for the same unit i: $Y_1 =$ (the probability of being permanently employed at time t_1, if i *was* in a TWA assignment at time t_0), and $Y_0 =$ (the probability of being permanently employed at time t_1, if i *was not* in a TWA assignment at time t_0). Obviously, only one of these two potential outcomes can be observed, i.e., the one corresponding to the treatment the unit received, but the causal effect is defined by their comparison: $Y_1 - Y_0$.

[6] The exact time spell from the treatment to the outcome period derives from the sampling strategy, see below.

Our aim is to estimate a specific statistic of the distribution of $Y_1 - Y_0$, i.e., the average treatment effect for the subpopulation of temporary agency workers (*Average effect of Treatment on the Treated*, ATT):

$$E(Y_1 - Y_0 | T = 1). \tag{1}$$

The decision to have a TWA experience at time t can be represented, without loss of generality, as a process of utility maximization, V:

$$V = f(Z, U_v) \qquad\qquad T = I(V > 0), \tag{2}$$

where Z and U_v are observed and unobserved characteristics determining the individual selection into treatment, respectively. These sets of variables may contain both characteristics that are specific to the individual and represent her life history up to time t_0, and characteristics of the area (or labor market) where the individual lives.

Analogously, the two potential outcomes can be written as functions of observed (X) and unobserved (U) pre-treatment variables:[7]

$$Y_i = g_i(X, U), \tag{3}$$

with $i \in \{0, 1\}$.

If we estimate the ATT by means of the comparison of the average outcomes in the treated and control groups, we suffer the consequences of the common problem of self-selection, i.e., the potential association between some of the U and the treatment indicator T. An identifying assumption is needed, and we follow Rosenbaum and Rubin (1983) in assuming "unconfoundedness", which is a special case of ignorable missing mechanism and

[7] The two sets of variables X and Z may coincide or overlap to a certain extent.

the rationale behind common estimation strategies, such as regression modeling and matching. This assumption considers the whole *conditioning set* of pre-treatment variables $W = (X, Z)$ and assumes that:

$$(Y_1, Y_0) \perp T | W \tag{4}$$

$$0 < Pr(T = 1|W) < 1. \tag{5}$$

This means that, conditioning on observed covariates W, treatment assignment is independent of potential outcomes. Although very strong, the plausibility of this assumption heavily relies on the amount and quality of the information contained in W.

In the present setting, unconfoundedness might be violated because of both labor-supply and labor-demand motivations. Some of the characteristics of the area where the individual lives (e.g., the presence of high-pressure labor demand) might have attracted TWAs, making it easier for a worker to get a temporary job. These same area-specific characteristics might also ease the subsequent search for a permanent job.[8] Analogously, on the supply side, some individual unobserved characteristics might affect the propensity to accept a temporary job and, at the same time, facilitate (or hamper) the jump into a permanent job.

For the moment note that, under unconfoundedness, one can identify the "springboard" effect of TWA employment within subpopulations defined by the values of W,

$$\begin{aligned} E(Y_1 - Y_0|T = 1) &= E(E(Y_1 - Y_0|T = 1, W)) = \\ &= E(E(Y_1|T = 1, W) - E(Y_0|T = 0, W)|T = 1), \end{aligned} \tag{6}$$

[8]This is the reason why Section 5 uses the distance of each individual's home from the nearest agency in order to capture local labor market features not directly observed by the econometrician.

where the outer expectation is over the distribution of W in the subpopulation of temporary workers.

In the present study, we prefer to use matching estimation techniques, instead of regression modeling, since they allow for a careful and transparent check of *whom* we are comparing to *whom* when estimating average treatment effects. In principle, one would like to compare temporary workers and control individuals that have the same values of all covariates. Unless there is a substantial overlap on the two covariates distributions, with regression modeling, one relies heavily on model specification (i.e., on extrapolation) for the estimation of treatment effects. This is not the case with matching.

In practice, to solve the dimensionality problem produced by continuous variables in W, one can implement matching estimation exploiting the results by Rosenbaum and Rubin (1983) on the so-called "Propensity Score". The Propensity Score is the individual probability of being in a TWA assignment at t_0 given the observed covariates: $p(W) = P(T = 1|W)$. Under unconfoundedness, one can prove that: 1) T is independent of W given the Score $p(W)$; 2) Y_0 and Y_1 are independent of T given the Score. Because of these two properties, adjusting for the Propensity Score automatically controls for all observed covariates, at least in big samples. As a result, if $p(W_i)$ is known, the ATT can be estimated as follows:

$$\begin{aligned} \tau &\equiv E(Y_{1i} - Y_{0i}|T_i = 1) = \qquad (7) \\ &= E(E(Y_{1i} - Y_{0i}|p(W_i), T_i = 1)) = \\ &= E(E(Y_{1i}|p(W_i), T_i = 1) - E(Y_{0i}|p(W_i), T_i = 0)|T_i = 1) \end{aligned}$$

where the outer expectation is over the distribution of $(p(W_i)|T_i = 1)$.

However, the estimation of the Score is not enough to estimate the ATT using equation (7). In fact, the probability of observing two units with exactly the same value of the Score is in principle zero, since $p(W)$ is continuous. Various methods have been proposed in the literature to overcome this problem.[9] This evaluation study adopts two of them, *Nearest Neighbor Matching* and *Kernel Matching*. The first method matches each temporary worker with the control subject that displays the nearest value of the Propensity Score, and estimates the ATT as the average of all the outcome differences in the pairs of treated units and matched controls (unmatched controls are completely disregarded). In the Kernel matching, every temporary worker is matched with a weighted average of all controls, with weights that are inversely proportional to the distance between the treated and control units.[10]

It is clear from the above discussion that the credibility of this matching strategy to identify the "springboard" effect of a TWA assignment, which in turn relies on the unconfoundedness assumption, depends crucially on the quality and amount of observable pre-treatment characteristics we can control for. This is why, in our study, data collection is an important component of the overall evaluation framework.

We specifically collected data on TWA workers and appropriate control subjects. The analysis focused on a region at the center of Italy (Tuscany) and one in the south (Sicily), which were among the areas with incomplete penetration of TWAs in 2000. Five provinces with an agency (Livorno, Pisa, Lucca, Catania, Palermo) and four provinces without any agency (Grosseto, Massa, Messina, Trapani) were selected. This first step allowed us to exploit the variation in the home-to-agency distance of each subject, and use it as a proxy of local labor demand. In the econometric analysis performed in Section 5, we include this distance measure among the matching variables in

[9]See Becker and Ichino (2003) for further discussion.

[10]See again Becker and Ichino (2003) for a formal presentation of these two methods.

order to control for area-specific characteristics, under the assumption that -within every province- TWAs locate themselves in the areas with higher labor demand, making it easier to meet potential client firms.[11]

"Manpower Italia Spa" -a major company operating in the TWA sector with a national market share of about 25%- provided the dataset of the workers they hired. From this dataset, all workers who were on a TWA assignment during the first semester of 2001, and were living in one of the nine provinces mentioned above, were extracted. Hence, the first semester of 2001 was chosen as the "treatment" period, i.e., the period in which treated individuals went through their TWA experience. Data collection developed along the following two steps: 1) phone interviews to all temps who were resident in the nine provinces and were in a TWA assignment during the first semester of 2001; 2) phone interviews to a random sample of "controls" drawn from the population of the nine provinces, in order to match them with the treated units. Controls were chosen so as to have two characteristics: to be aged between 18 and 40 and not to have a stable job (either an open-ended contract or self-employment) on January 1, 2001.

In a sense, this first screening of potential controls might be interpreted as part of the matching strategy, aimed at identifying a common support for TWA workers and controls with respect to observable characteristics. Notice, moreover, that the control sample might include subjects who went through a TWA experience in a period different from the beginning of 2001. This is because the treatment coincides with "a TWA assignment during the first semester of 2001". If the outcome of some control units were affected by TWA in another period, our study would result in conservative estimates.

[11] See Altieri and Otieri (2004) for an analysis of the location choice by Italian TWAs, confirming the assumption that a similar decision is prevalently demand-driven.

For the treated units, the reference point in time is the date of the TWA assignment during the first semester of 2001. For the control units, it is January 2001. Information on the period before these reference points provided "pre-treatment" variables, while information on the date of the interview (November 2002) provided "outcome" or "post-treatment" variables. For both the treated and the control units, interviews followed an identical path, asking: a) demographic characteristics; b) family background; c) educational achievements; d) work experience before the treatment period; e) job characteristics during the treatment period; f) work experience from the treatment period to the end of 2002; g) job characteristics at the end of 2002.

After a first analysis of the data, control individuals who were out of the labor force in the treatment period (e.g. students) were dropped from the sample. In fact, these subjects showed characteristics that made them not easily comparable with the treatment units. Notice that this was a conservative choice with respect to the estimated treatment effects, since all these individuals had a very low probability of having a permanent job at the end of 2002. Dropping these observations is another step of the search for a common support for treated and control units. The final data set used for the empirical evaluation already contains control units who could be more meaningfully *matched* with the treated units.

At the end, the treated sample contains individuals who lived in the nine provinces and were on a TWA assignment through "Manpower" during the first semester of 2001; while the control sample contains residents in the nine provinces, aged 18-40, who belonged to the labor force but were not permanent workers as of January 1, 2001. As explained in the previous section, this choice of the control sample is associated to the counterfactual question: What would have been the outcome of temporary workers, had they chosen to keep looking for a stable job or accept another kind of non-standard contract in the first semester of 2001? This final dataset contains 2030 individuals: 511 treated (temps); 1519 controls.[12]

4.2 Methodological problems related to our sampling strategy

As described in Section 4.1, our data collection strategy combines *flow sampling* for the treated group and *stock sampling* for the control group. In the case of TWA workers, we preferred to use flow sampling (i.e., considering as treated every subject who had been in a TWA assignment during the first six months of 2001), since it was the only available solution to get a sufficient number of treated units. In the case of control subjects, we preferred to use stock sampling (i.e., selecting individuals with respect to their situation on January 1, 2001), since, on the contrary, we should have asked them a screening question referring to their employment contract in the "prevailing part of the first semester of 2001". This solution appeared problematic and potentially linked to measurement errors.

Of course, also our mixed sampling strategy might produce a bias in the results. With respect to the alternative strategy of using flow sampling for both treatment groups, we are incorrectly dropping from the control group subjects who were permanent employees on January 1, but -in the prevailing part of the first semester of 2001- were unemployed or employed with other non-standard contracts. However, as long as the transition probability from permanent employment to unemployment or non-permanent employment is very low, the group of individuals we are disregarding is very small, and there is no loss in relevant information from the control group when using stock

[12]See Ichino, Mealli and Nannicini (2004a) for further details on data collection.

sampling instead of flow sampling. Since these probabilities are supposed to be quite low in the Italian labor market, we believe that our sampling choice is better than any concrete alternative.

Another relevant methodological issue arising from our sampling strategy concerns choice-based sampling (Manski and Lerman, 1977). Our data collection scheme is a stratified sampling design, where one of the two stratifying variables is the province of residence, which is included in the pre-treatment set W, while the other is the treatment indicator T. One of the stratifying variables, T, is thus an *endogenous* variable with respect to the specification of the model for the Propensity Score, i.e., $Pr(T = 1|W)$. This type of sampling scheme is usually called "choice-based sampling" or, in general, "endogenous stratification".

As explained in Section 4.1, we adopted this sampling scheme in order to obtain information on an adequate number of temporary workers. With random sampling, this would have required a sample size in excess of the given budget, because of the relatively small proportion of this group in the population. In addition, since we intended to use an estimation strategy based on the matching of treated and control units, and because variables describing the geographical and economical context are, a priori, particularly relevant, the stratification by province allowed the selection of a number of controls that could guarantee an appropriate number of potential controls for each treated individual in every province.

Under unconfoundedness, regression analysis is robust with respect to choice-based sampling. With regression modeling, endogenous stratification can only affect efficiency. On the contrary, the application of estimation strategies based on the preliminary estimation of the Propensity Score is more problematic in the presence of choice-based sampling. Denoting with

A the variables that identify the province of residence, our sampling scheme allows a certain number of observations to be sampled at random from each of the strata defined by $A \times T$. Hence, every observation is characterized by the probability distribution $Pr(Y, W|A, T)$, with $Y = Y_1T + Y_0(1 - T)$. Sample data allow estimation of the distributions $Pr(W|A, T = 0)$ and $Pr(W|A, T = 1)$, whereas the Propensity Score is the conditional distribution $Pr(T = 1|W, A)$. Nevertheless, these distributions are linked to each other, via Bayes theorem, in the following way:

$$Pr(W|A, T = j)Pr(T = j|A)Pr(A) = Pr(T = j|W, A)Pr(W|A)Pr(A) \quad (8)$$

where $j = 0, 1$, so that:

$$\frac{Pr(W|A, T = 1)Pr(T = 1|A)}{Pr(W|A, T = 0)Pr(T = 0|A)} = \frac{Pr(T = 1|W, A)}{Pr(T = 0|W, A)} \quad (9)$$

$$\frac{\tilde{Pr}(T = 1|W, A)}{\tilde{Pr}(T = 0|W, A)} = \frac{Pr(T = 1|W, A)}{Pr(T = 0|W, A)} \frac{\tilde{Pr}(T = 1|A)}{\tilde{Pr}(T = 0|A)} \frac{Pr(T = 0|A)}{Pr(T = 1|A)} \quad (10)$$

where $\tilde{Pr}(T = 1|W, A)$ disregards choice-based sampling and $\tilde{Pr}(T = 1|A)$ is conditioned on the province in the choice-based sample. Hence, the odd of the misspecified (i.e., choice-based) Propensity Score can be used to implement matching *within* each province, because it is equal, up to a constant, to the odd of the *true* Propensity Score, which is itself a monotonic transformation of the Propensity Score (see Heckman and Todd, 1999).

On the basis of these results, in the present setting, we can estimate the province-specific ATTs by using the regional estimates of the Odd of the Propensity Score, in order to control for choice-based sampling. Then, we can obtain the regional ATT (either in Tuscany or in Sicily) as the weighted average of the province-specific ATTs, in order to control for geographical stratification. This is exactly what we do in Section 5.

4.3 The characteristics of treated and control units in the final sample

Table 1 reports the distribution of the observations across the nine provinces. The weighted proportion of each group (treated and controls) in the reference population (composed by unemployed and atypical workers aged between 18 and 40) is estimated by using "Manpower" and Istat data.[13] "Manpower" temps are 0.58% of this population in Tuscany and 0.15% in Sicily.[14] These small figures notwithstanding, it should be noted that in Tuscany 32% of the reference population declared to have contacted a TWA at least once, and 15% did the same in Sicily.[15]

Table 2 summarizes the relevant information available for all individuals in the sample. This table, as well as the following ones, presents the average characteristics of an important subsample of controls, dubbed the "matched controls". These control units are used as "nearest neighbors" of at least one treated unit in the Nearest Neighbor Propensity Score matching estimation.[16] Inasmuch as the treated units are more similar to the "matched controls" than to "all controls", the matching strategy has succeeded in improving the quality of the comparison used to estimate the "springboard" effect of TWA.

Treated individuals are prevalently young, male, single and without children. As far as education is concerned, there are not significant differences in years of schooling or educational attainment between treated and controls. Before the treatment period, a greater fraction of the treated was out

[13]The exact number of "Manpower" temps in each province in the first semester of 2001 is known. The population of unemployed and atypical workers aged between 18 and 40 in each province was estimated by combining Istat statistics and the answer rate of the first screening question in phone interviews. The ratio of the second to the first term is the province-specific weight.

[14]Note that "Manpower" declared a market share of 32% in Tuscany and 45% in Sicily.

[15]See Ichino, Mealli and Nannicini (2004a) for further data details.

[16]See Section 4.1 for a description of this estimator.

of the labor force. In 2001, by definition, all treated are employed. Among controls, in Tuscany 36% (Sicily 25%) had an atypical contract, while 64% (75%) were looking for a job. In 2002 -the "outcome" period- 31% of the treated had a permanent position in Tuscany, compared with 17% of the controls. In Sicily, the same comparison is 23% versus 13%.[17] Looking simply at this raw figures, in Tuscany TWA workers have a future probability of permanent employment that is higher by 14 percentage points than the one of the unemployed or other atypical workers. In Sicily, this difference lowers to 10 percentage points. Of course, these numbers are simple correlations that need to be cleansed from observable and unobservable influences, just as the analysis performed in Section 5 aims to do.

Table 3 reports additional characteristics on the treated and controls who were employed in the pre-treatment period. Among the treated, there is a greater fraction of individuals previously employed with an atypical contract and as blue-collar workers in manufacturing. One can also notice that the pre-treatment wage of the treated was lower on average than the wage of controls, while hours of work were greater (due to a lower utilization of part-time arrangements).[18] Table 4 reports additional characteristics on the treated (all) and the controls who were employed in the treatment period. The most relevant difference concerns the firm's sector: TWA workers are mainly used in the manufacturing sector (60% in Tuscany and 53% in Sicily),

[17]Incidentally, note that employers mention to 51% of TWA workers the possibility of hiring them on a permanent basis at the end of the assignment. Among these temps, 32% are effectively hired by the firm. But also among the others the percentage of direct-hiring is high: 20%. Among the treated who are employed in the outcome period, 38% (34% in Tuscany and 43% in Sicily) are working in the same firm of the TWA assignment. See Ichino, Mealli and Nannicini (2004a) for further data details.

[18]Another interesting element concerns wage mobility (even though the small sample size prevents us to use this information as an alternative outcome): 36.9% of the treated with wage below the median in 2000 had a wage above the median in 2002, compared with 15.1% of controls. See Ichino, Mealli and Nannicini (2004a) for further data details.

while the other atypical workers are prevalently employed in the service sector (68% in Tuscany and 74% in Sicily). The motivations behind the choice of atypical work, however, are quite similar. For instance, in Tuscany 59% of temps could not find permanent jobs (against 59% of the other atypical workers); 22% became temps to make up their mind on what they wanted to do (against 18%); 16% did it for personal flexibility needs (against 18%). Table 5 reports additional characteristics on the treated and controls who were employed at the end of 2002, i.e., in the outcome period. The discussed "manufacturing gap" is still there during this period.

The previous descriptive tables also provide information on matched controls, i.e., control units used in the Nearest Neighbor Propensity Score matching estimation. It is particularly informative to check whether (and to what extent) the treated-control gap in observable pre-treatment characteristics is reduced when considering only matched controls (again see Tables 2 and 3). Figure 1 does so in a graphic way by reporting the relative reduction of such a gap for Tuscany. For each variable, the difference between the averages of the treated and the averages of all controls is set equal to 100 and displayed as such. The figure also displays the difference between the average of the treated and matched controls as a fraction of the analogous difference between treated and all controls. Inasmuch as this relative difference is smaller than 100, the matching strategy has improved the quality of the comparison used for the estimation of the treatment effect. Figure 3 does the same for Sicily. Figure 2 reports instead a similar relative reduction in the "pre-treatment gap" for those variables that are available only for individuals who were employed in the pre-treatment period, i.e., the period of unemployment as a fraction of the transition from school to work, and the job characteristics in 2000. Figure 4 does the same for Sicily.

It is evident that the Nearest Neighbor algorithm for the choice of the control units to be compared with the treated units considerably reduces the "pre-treatment gap". This reduction is large, both in Tuscany and Sicily, confirming the quality of our data, which offer control subjects very similar to temps according to a wide and relevant set of observable characteristics. However, this reduction in the pre-treatment gap encounters some problems in the case of employment variables in Sicily (see Figure 4). This difficulty signals that in this region TWA workers have pre-treatment employment histories that are quite "peculiar" within the population of atypical workers and unemployed individuals. In other words, TWA employment segments more the Sicilian regional labor market than the one of Tuscany, playing possibly a different role in this environment. As shown in Section 5, in Sicily also the ATT has a different statistical significance than in Tuscany.

5 Estimated causal effects

Tables 6 and 7 contain the estimated ATTs for Tuscany and Sicily, respectively. For the reasons discussed in Section 4.2, each regional ATT is obtained as the weighted average of the province-specific ATTs. Matching variables include: gender, age, place of birth, nationality, marital status, number of children; years of schooling and prevalent job of the father, whether the father is living; educational level, grade in the last degree, post-school training; share of time without any occupation from school to the pre-treatment period; occupational status in the pre-treatment period, as well as type of contract, sector, profession, wage, working hours; province of residence and distance from the nearest temporary agency in the pre-treatment period.

Table 6 reports the results of both Nearest Neighbor and Kernel Propensity Score matching in Tuscany. With the first estimator, TWA employment has a significant and positive effect of 19 percentage points on the probability to be in a stable positions 18 months after the treatment. As a reference, note that in Tuscany the observed probability of finding a permanent job for controls is 17%, while the observed probability for the treated is 31%. Hence, for the treated, the estimated “counterfactual” probability to get a permanent job in the case of non-treatment is 12% (i.e., 31 minus 19). This estimated probability is even lower than the observed outcome of controls, meaning that controlling carefully for observed characteristics increases the estimated effect of TWA. In other words, TWA appears to be particularly effective for individuals with a lower probability to get a permanent job within the reference population (composed by unemployed and atypical workers). It is exactly for people with a lower expected probability of re-employment (conditioning on observable characteristics) that TWA represents an effective “springboard”. The results of Kernel Propensity Score matching show a similar picture, with an ATT equal to 18 percentage points. The fact that these two estimates are almost identical is a first evidence of their robustness.

Tables 7 reports the results of Nearest Neighbor and Kernel Propensity Score matching in Sicily. Both estimators find a lower and barely significant effect of TWA employment: 11 and 10 percentage points, respectively. As a reference, note that in Sicily the observed probability of finding a permanent job for controls is 13%, while the observed probability for the treated is 23%. For the treated, the estimated “counterfactual” probability to get a permanent job in the case of non-treatment is 12-13% (exactly the same of the observed outcome of controls). In this case, taking carefully into account the effect of observed characteristics does not change the overall picture.

The finding that in Sicily TWAs do not seem an effective springboard to permanent employment might be linked to the fact that, in this region, the public sector is the primary source of stable positions, and in this sector the recruitment channels are different from TWA employment (e.g., public selection procedures that do not include TWA experiences among the relevant "titles" used for the screening of candidates). As a consequence, human capital accumulation is the only channel at work behind the "springboard" effect of TWA employment. In Tuscany, on the contrary, the private sector is able to create a relevant number of stable positions that may be reached through the "signaling" effect of the TWA channel. The "springboard" effect is greater and more significant in this second context.

In Table 8, some sources of heterogeneity in the treatment effect are investigated. In all these cases, the ATT is estimated by means of Weighted Nearest Neighbor Propensity Score Matching. Analytical standard errors are reported. Bootstrapped standard errors have been calculated as well, but the analytical ones lead to more conservative (i.e., less significant) estimates. This heterogeneity analysis confirms non-significant results for Sicily. Only for a marginal minority of workers with university degrees does TWA employment have a strong and significant effect. In Tuscany, on the contrary, the heterogeneity analysis shows interesting results.

In the first row of Table 8, the ATT is estimated by dropping the unemployed from the control group. Consequently, in Tuscany, only the 228 controls who were employed with an atypical contract in the treatment period were considered.[19] In this case, the ATT looses much of its significance also in Tuscany. TWAs are a springboard to permanent employment, but such a springboard does not seem more effective than the ones offered by other

[19]These workers had the following contracts: 53% fixed-term, 14% co.co.co. (a particular Italian arrangement), 6% training contract, 18% irregular employment, 9% other occasional work. In Sicily: 44% fixed-term, 11% co.co.co., 2% training contract, 26% irregular employment, 17% other occasional work.

forms of temporary employment. Hence, the aggregate effect of the liberalization of TWAs on permanent employment depends on the magnitude of the possible "crowding-out" of other non-permanent contracts.

Again in Table 8, the ATTs for individuals under 30, over 30, with a university degree, and with or without a high-school degree are computed separately in these subsamples ("treatment-effect heterogeneity"). The ATTs in manufacturing or service sectors are computed by interacting the TWA experience with the sector of the using firm ("treatment heterogeneity"). These estimations show that the "springboard" effect of TWAs is greater for individuals over 30 years, with a university degree (even though they are a small minority) or in the service sector. The most surprising result is the one regarding age, which shows that young workers in the Italian labor market generally wait for quite a long period of time before finding a stable job.

6 Conclusions

This paper has investigated whether (and to what extent) TWA employment represents a "springboard" to a permanent job, or it is a "trap" of endless precariousness. Applying Propensity Score matching in the presence of choice-based sampling, the causal effect of the treatment "TWA assignment" on the outcome "finding a permanent job after 18 months" was estimated. The analysis referred to Italy, where TWAs were liberalized in 1997 and we had the opportunity to gather data appropriately collected for this evaluation study. Estimates find a positive effect of a TWA assignment on the probability to find a permanent job in Tuscany (19 percentage points) and a barely significant effect of about 11 percentage points in Sicily. These effects are large given that the observed probabilities in our treated group are respectively 31% and 23% in the two regions. Relevant heterogeneity in the treatment effect along observable characteristics such as age, education and firm's sector is also detected. In Tuscany (where the overall effect of TWAs is statistically significant), the estimated ATT is greater for individuals over 30 years, with an university degree (even though they are a small minority of temps) or in the service sector (rather than in manufacturing).

All the previous estimates rest on the plausibility of the identifying assumption of "unconfoundedness", which can be defended in our evaluation exercise thanks to the amount of information contained in the unique data set we built (see Section 4). However, it remains a questionable assumption that cannot be tested. In a parallel paper (Ichino, Mealli and Nannicini, 2004b), we develop a simulation-based sensitivity analysis aimed at assessing the robustness of matching estimates to specific deviations from this assumption. Applied to the present evaluation study, this sensitivity analysis finds that in Tuscany the estimated ATT is robust to meaningful deviations from unconfoundedness, while in Sicily the point estimate is not only barely significant but is rapidly driven to zero when the possible influence of an unobserved characteristic is simulated in the data.

From a policy perspective, this study finds that TWA employment has not been a "trap" of endless precariousness in Italy, but has been an effective "springboard" toward permanent employment. A similar springboard, however, is offered by other types of non-permanent labor contracts and it is not equally effective everywhere (e.g. it is in Tuscany, but not in Sicily) or for all workers (e.g. for workers in services, but not for workers in manufacturing sectors). It should be noted however, that precisely because TWA employment may allow workers to signal their (unobservable) ability to employers, it facilitates the emergence of a separating equilibrium in the labor market. Such a separating equilibrium benefits the workers who are better equipped to compete, while worsening the employment prospects of the weakest workers. The commendable attention that the Italian society (and unions in particular) devote to these weak workers, may appear to justify an opposition to TWA employment. However, banning the signaling possibilities offered by TWA employment would not help the weakest much, and would typically result in a less efficient outcome, not to mention the cost for the strongest workers. The correct way to help the weakest workers is to offer them the tools (e.g., training and better information) to compete effectively and send the right signals in the labor market.

Finally, from a methodological perspective, this study suggests that labor market programs in Italy should be increasingly evaluated with econometric methods specifically aimed at the identification of *causal effects*. Only in this way does the political debate have a chance to become more productive, being based on relevant empirical findings instead of ideological prejudices.

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Tables and Figures

Table 1: Province of residence before the treatment

	Agency	Distance	Treated	Controls	Tot.
Pisa	Yes	11.0	126	130	256
			(1.09)	(98.91)	(100)
Lucca	Yes	8.9	69	99	168
			(0.76)	(99.24)	(100)
Livorno	Yes	18.8	63	156	219
			(0.46)	(99.54)	(100)
Massa	No	39.9	10	130	140
			(0.15)	(99.85)	(100)
Grosseto	No	40.6	13	113	126
			(0.20)	(99.80)	(100)
TOSCANA	-	21.0	281	628	909
			(0.58)	(99.42)	(100)
Palermo	Yes	13.6	76	276	352
			(0.15)	(99.85)	(100)
Catania	Yes	15.4	112	195	307
			(0.22)	(99.78)	(100)
Messina	No	74.8	27	206	233
			(0.10)	(99.90)	(100)
Trapani	No	68.5	15	214	229
			(0.09)	(99.91)	(100)
SICILIA	-	38.0	230	891	1121
			(0.15)	(99.85)	100

Note: The variable "distance" measures the average distance from the nearest agency (in km), computed by means of postal codes. In brackets, the weighted proportion of each group (controls and treated) on the reference population. The weighted proportion of the treated refers to "Manpower" temps only.

Table 2: Characteristics of the whole sample

	TUSCANY			SICILY		
	Treated	Matched Controls	All Controls	Treated	Matched Controls	All Controls
Age	26.5	27.5	29.1	26.8	27.8	30.0
Male	0.56	0.41	0.29	0.67	0.57	0.29
Single	0.90	0.87	0.66	0.83	0.81	0.49
Children	0.09	0.16	0.45	0.20	0.23	0.86
Father school	9.3	9.2	8.6	8.7	9.2	7.6
Father blue	0.33	0.39	0.43	0.30	0.31	0.39
Father active	0.53	0.46	0.37	0.46	0.45	0.29
School	12.5	12.7	12.3	12.0	12.4	11.6
Grade	75.9	77.1	76.9	74.7	74.6	76.5
Training	0.32	0.30	0.28	0.42	0.42	0.34
Unemployment	0.38	0. 42	0.48	0.42	0.44	0.62
Employed 2000	0.35	0.36	0.42	0.34	0.35	0.30
Unemployed 2000	0.52	0.53	0.52	0.60	0.60	0.67
Out l.force 2000	0.13	0.10	0.05	0.06	0.05	0.03
Employed 2001	1.00	0.36	0.36	1.00	0.30	0.25
Unemployed 2001	0.00	0.64	0.64	0.00	0.70	0.75
Permanent 2002	0.31	0.16	0.17	0.23	0.14	0.13
Atypical 2002	0.42	0.36	0.31	0.39	0.17	0.18
Unemployed 2002	0.16	0.44	0.45	0.30	0.59	0.63
Out l.force 2002	0.11	0.04	0.07	0.07	0.09	0.07
N.individuals	281	135	628	230	128	891

Note: All variables except age, number of children, father's years of schooling, grade (expressed as a fraction of the highest mark), years of schooling and unemployment period (expressed as a fraction of the transition from school to work) are dummies. "Matched controls" are individuals who belong to the control sample and are used in the Nearest Neighbor Propensity Score matching estimation.

Table 3: Characteristics of the employed before the treatment

	TUSCANY			SICILY		
	Treated	Matched Controls	All Controls	Treated	Matched Controls	All Controls
Permanent	0.16	0.22	0.26	0.14	0.16	0.36
Atypical	0.84	0.78	0.74	0.86	0.84	0.64
Blue-collar	0.62	0.59	0.39	0.44	0.24	0.22
White-collar	0.36	0.41	0.54	0.54	0.71	0.67
Self-empl.	0.02	0.00	0.07	0.01	0.04	0.10
Manufact.	0.53	0.41	0.23	0.39	0.20	0.15
Service	0.39	0.45	0.67	0.49	0.67	0.70
Other	0.08	0.14	0.11	0.11	0.13	0.15
Wage	5.2	5.6	6.8	5.6	7.6	7.0
Hours	38.0	36.3	33.3	34.5	32.1	31.1
N.individuals	98	49	266	79	45	267

Note: All variables, except the hourly wage (expressed in Euros) and the weekly hours of work, are dummies. "Matched controls" are individuals who belong to the control sample and are used in the Nearest Neighbor Propensity Score matching estimation.

Table 4: Characteristics of the employed in the treatment period

	TUSCANY			SICILY		
	Treated	Matched Controls	All Controls	Treated	Matched Controls	All Controls
Manufact.	0.60	0.35	0.22	0.53	0.13	0.15
Service	0.36	0.56	0.68	0.42	0.79	0.74
Other	0.04	0.08	0.10	0.05	0.08	0.12
Wage	7.1	7.5	7.8	8.8	10.7	8.8
Hours	40.5	31.0	31.5	39.0	28.4	30.5
No stable job	0.59	0.69	0.59	0.70	0.61	0.55
Preferences	0.22	0.17	0.18	0.13	0.18	0.22
Flexibility	0.16	0.13	0.18	0.15	0.13	0.13
N.individuals	281	48	228	230	38	224

Note: All variables, except the hourly wage (expressed in Euros) and the weekly hours of work, are dummies. The last three dummies refer to the motivation by workers to choose an atypical contract in the treatment period: 1) because they could not find a stable job; 2) because they wanted to clear up their preferences; 3) because of flexibility needs. "Matched controls" are individuals who belong to the control sample and are used in the Nearest Neighbor Propensity Score matching estimation.

Table 5: Characteristics of the employed after the treatment

	TUSCANY			SICILY		
	Treated	Matched Controls	All Controls	Treated	Matched Controls	All Controls
Permanent	0.43	0.31	0.35	0.38	0.45	0.42
Atypical	0.57	0.69	0.65	0.63	0.55	0.58
Manufact.	0.47	0.37	0.26	0.42	0.07	0.14
Service	0.45	0.49	0.63	0.49	0.82	0.71
Other	0.09	0.14	0.11	0.08	0.10	0.16
Wage	6.2	7.2	7.3	6.6	7.9	7.3
Hours	37.4	34.8	32.8	36.3	29.1	30.5
N.individuals	206	70	299	144	40	268

Note: All variables, except the hourly wage (expressed in Euros) and the weekly hours of work, are dummies. "Matched controls" are individuals who belong to the control sample and are used in the Nearest Neighbor Propensity Score matching estimation.

Table 6: Effect of a TWA assignment on the probability to find a permanent job in Tuscany - *Nearest Neighbor and Kernel Propensity Score Matching*

	ATT_{NN}	N.treated	N.controls	ATT_K	N.treated	N.controls
Grosseto	0.31	13	11	0.23	13	85
	(0.19)			(0.18)		
Livorno	0.17	63	43	0.16	63	130
	(0.07)			(0.07)		
Lucca	0.16	69	28	0.14	69	78
	(0.07)			(0.07)		
Massa	0.10	10	8	0.18	10	105
	(0.26)			(0.16)		
Pisa	0.21	126	45	0.19	126	104
	(0.08)			(0.08)		
TUSCANY	0.19	281	135	0.18	281	502
	(0.06)			(0.05)		

Note: ATT_{NN} is estimated by means of Nearest Neighbor Propensity Score Matching, while ATT_K is estimated by means of Kernel Propensity Score Matching. In both estimators, the ATT for Tuscany is obtained as the weighted average of the province-specific ATTs, in order to control for geographical stratification. Standard errors are calculated as: $SE = (\sum \frac{N_i^2}{N^2} SE_i^2)^{1/2}$, where $i =$ pi, lu, li, gr, ms. The province-specific ATTs are obtained by using the regional estimates of the Odd of the Propensity Score, in order to control for choice-based sampling. Standard errors are reported in brackets. As a reference, note that in Tuscany the observed probability of finding a permanent job for controls is 17%, while the observed probability for the treated is 31%.

Table 7: Effect of a TWA assignment on the probability to find a permanent job in Sicily - *Nearest Neighbor and Kernel Propensity Score Matching*

	ATT_{NN}	N.treated	N.controls	ATT_K	N.treated	N.controls
Catania	-0.02	112	51	0.01	112	137
	(0.09)			(0.08)		
Messina	0.15	27	18	0.11	27	176
	(0.12)			(0.13)		
Palermo	0.09	76	49	0.08	76	255
	(0.07)			(0.05)		
Trapani	0.26	15	10	0.27	15	175
	(0.17)			(0.15)		
SICILY	0.11	230	128	0.10	230	743
	(0.06)			(0.05)		

Note: ATT_{NN} is estimated by means of Nearest Neighbor Propensity Score Matching, while ATT_K is estimated by means of Kernel Propensity Score Matching. In both estimators, the ATT for Sicily is obtained as the weighted average of the province-specific ATTs, in order to control for geographical stratification. Standard errors are calculated as: $SE = (\sum \frac{N_i^2}{N^2} SE_i^2)^{1/2}$, where i = ct, pa, me, tp. The province-specific ATTs are obtained by using the regional estimates of the Odd of the Propensity Score, in order to control for choice-based sampling. Standard errors are reported in brackets. As a reference, note that in Sicily the observed probability of finding a permanent job for controls is 13%, while the observed probability for the treated is 23%.

Table 8: Heterogeneity of the treatment effect - *Nearest Neighbor Propensity Score Matching*

	TUSCANY			SICILY		
	ATT_{NN}	Treated	Controls	ATT_{NN}	Treated	Controls
Only atypical	0.14	281	228	-0.33	230	224
	(0.16)			(0.18)		
Under 30	0.12	199	326	0.00	170	410
	(0.11)			(0.06)		
Over 30	0.37	82	302	-0.23	60	481
	(0.12)			(0.13)		
University	0.34	35	113	0.35	17	112
	(0.08)			(0.12)		
High school	0.20	174	332	-0.09	149	460
	(0.09)			(0.08)		
No high school	0.24	72	183	0.14	64	319
	(0.16)			(0.11)		
Manufacturing	0.04	169	740	0.02	123	998
	(0.06)			(0.06)		
Services	0.17	100	809	-0.01	96	1025
	(0.08)			(0.06)		

Note: All ATTs are estimated by means of Nearest Neighbor Propensity Score Matching. They are estimated at the regional level by using appropriate weights, in order to control for both geographical stratification and choice-based sampling. Analytical standard errors are reported in brackets. The first-row ATT is estimated by dropping the unemployed from the control group. The ATTs for individuals under 30, over 30, with university degree, with or without high school degree, are computed separately in these sub-samples (*treatment-effect heterogeneity*). The ATTs in manufacturing or service sectors are computed by interacting the TWA experience with the sector of the using firm (*treatment heterogeneity*). The number of controls refers to all available controls and not only to matched controls.

Fig.1) Pre-treatment "gap" in Tuscany: controls vs. matched controls

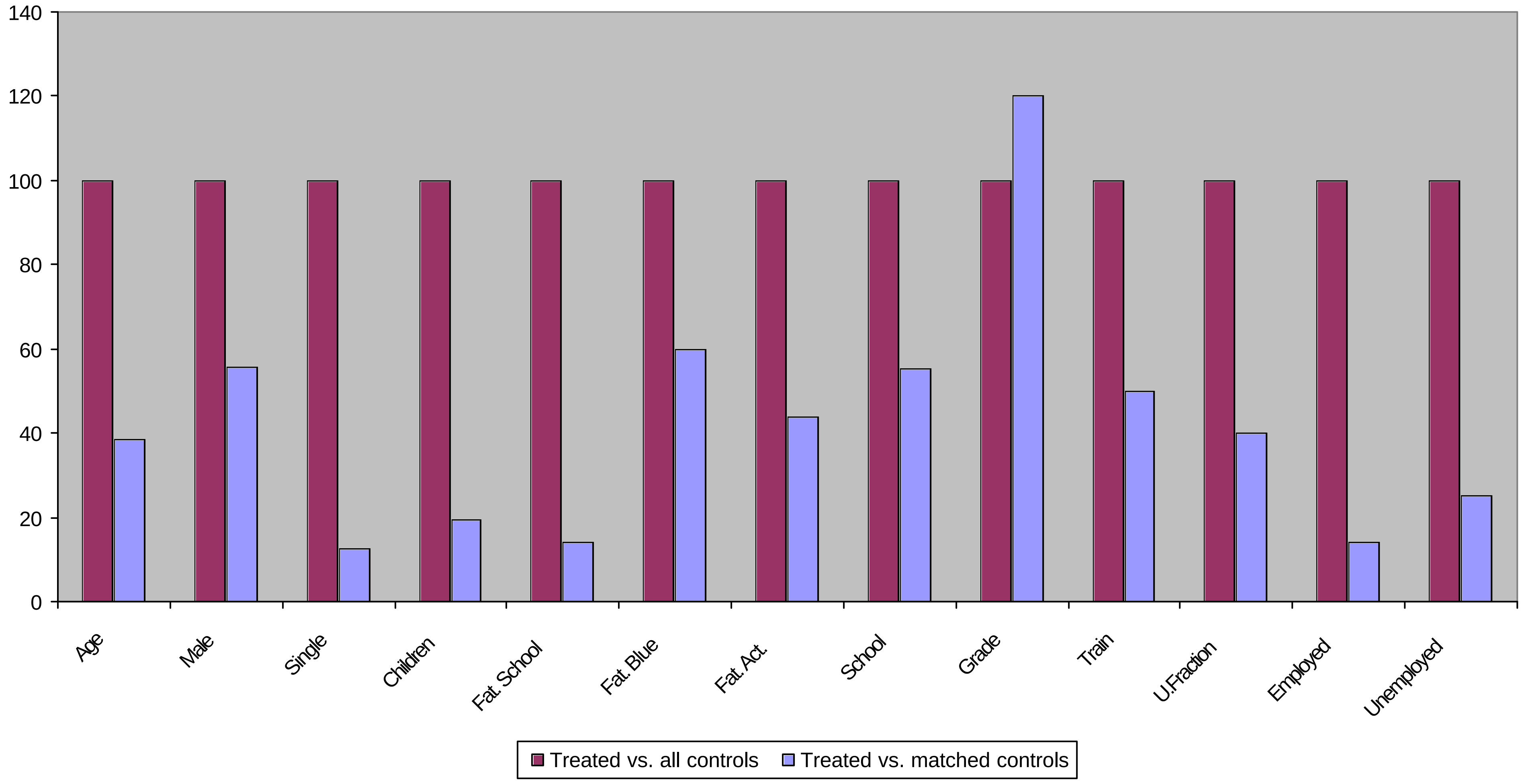

Fig.2) Pre-treatment "gap" in Tuscany: employed controls vs. matched employed controls

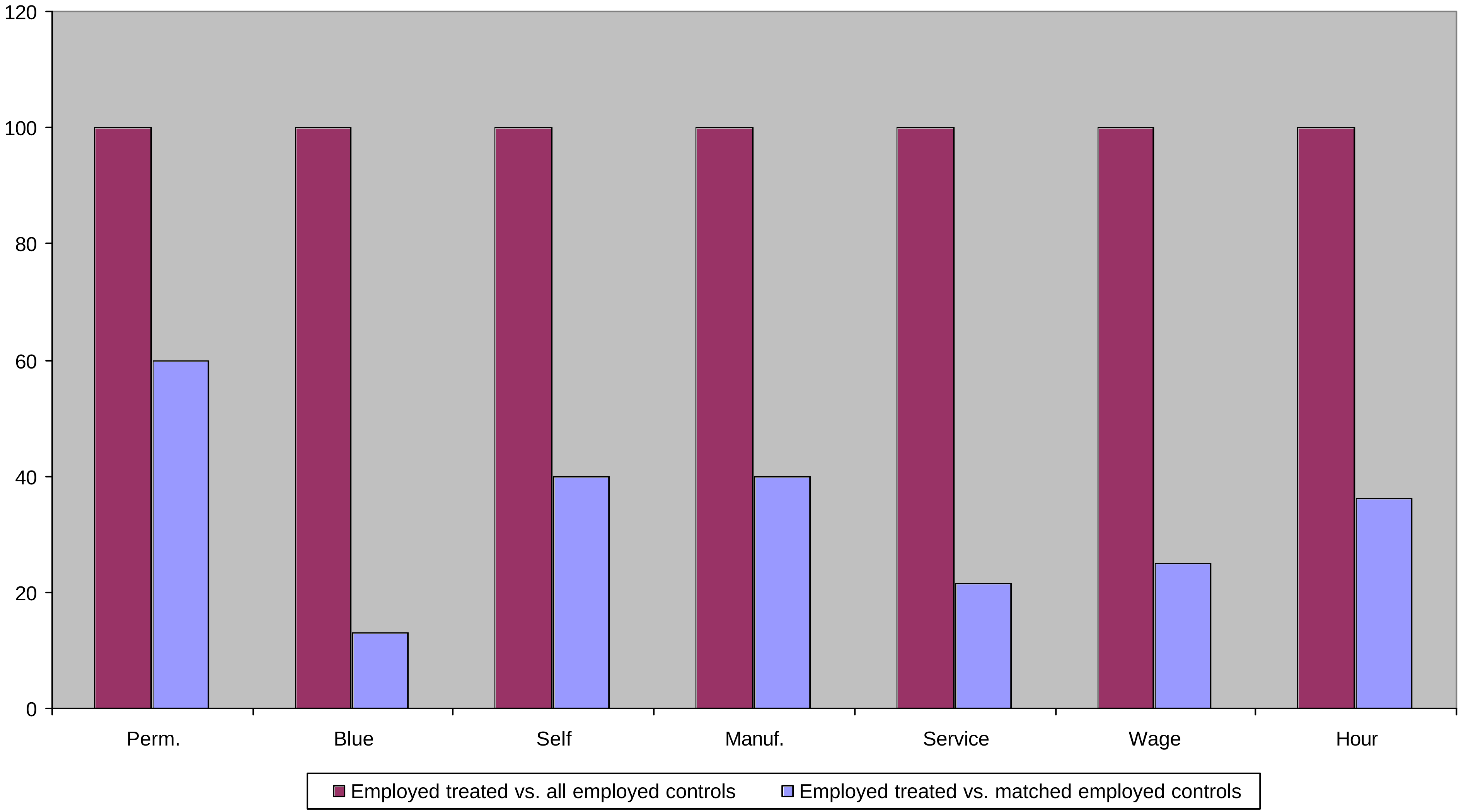

Fig.3) Pre-treatment "gap" in Sicily: controls vs. matched controls

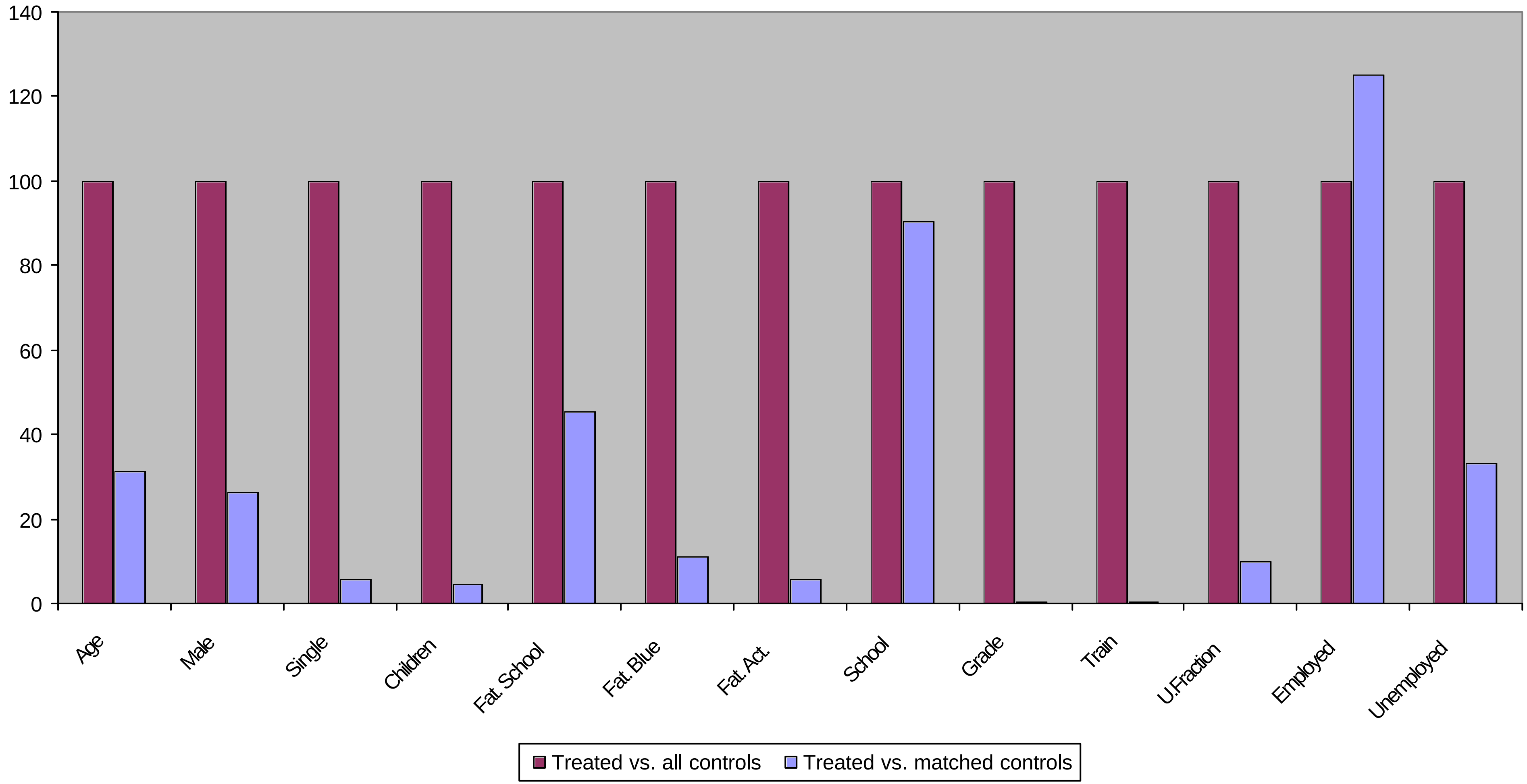

Fig.4) Pre-treatment "gap" in Sicily: employed controls vs. matched employed controls