Per la presente pubblicazione sono stati adempiuti gli obblighi previsti dalle norme per la consegna obbligatoria di esemplari degli stampati e delle pubblicazioni di cui alla legge del 2 febbraio 1939 n.374 e successive modificazioni, presso la Procura della Repubblica e la Prefettura di Padova.

1. Introduction[♦]

The "Mobility Lists" (ML) is an Italian labour market programme introduced in the early '90s. It combines a passive component – an income support to workers dismissed by firms with at least 15 employees – to an active one – a wage subsidy to employers who hire a worker from the Lists. The basic question we deal with in the paper regards the effects of these benefits on the probability of transition to a new job.

The exact content of the package of benefits the worker is entitled to depends on his/her age at the time of dismissal and on the size of the dismissing firm. Workers dismissed by firms with less then 15 employees are eligible only for the active component of the policy, while those dismissed by firms with at least 15 employees are eligible also for the income support. On the other hand, the spell of time over which they are eligible for the benefits lasts one year for those younger than 40 years at the time of dismissal, while it lasts two years for workers aged 40 to 49 at the time of dismissal. There is also a third category of workers who maintain their eligibility status over three years, made up of workers older than 49. We will not consider them in the analysis because the programme offers them also early retirement as an option (see below).

Evaluating the whole impact of the programme is unfeasible due to the lack of a comparison group made up of workers ineligible for the benefits. As a result, only the differential impact of alternative packages of benefits is in principle identifiable.

So far studies on the ML impact (see Borzaga and Brunello, 1997, Brunello and Miniaci, 1997, and Paggiaro and Trivellato, 2002, among others) have exploited administrative data resulting from the management of the policy. These data provide poor information on employment spells during enrolment in the Lists, thus precluding a detailed analysis of the effects of the programme. On the other hand, the studies just mentioned rely upon parametric specifications of models for transitions in a two-state space: enrolled in the Lists or permanently hired. Eventually, one is left wondering how much results depend on the parametric specification assumptions and to which particular sub-population results refer to.

To overcome these limitations, we use a much more informative information set in a fully non-parametric framework. In short:

(a) We link administrative data resulting from the managing of the policy to the Netlabor files, an archive resulting from the field operations of public labour exchanges. The linkage provides richer information on socio-demographic characteristics of enrolled workers; detailed information on workers' labour market histories while they are in the Lists; follow-up information on their employment spells after they exit the Lists; finally, better information on working histories before they enter the Lists, which turns out to be quite important to control for selection bias when evaluating the impact of the programme.

(b) To identify the differential impact of the various packages of benefits we use propensity score matching. We point out that, due to the design of the policy, the key identifying restriction on which our strategy relies on has testable implications, which we exploit to validate our matching estimator.

The paper proceeds as follows. Section 2 presents the main features of the programme and the relevant questions about the effects of benefits. Section 3 outlines the methodological choices to evaluate differential effects of the benefits, based on matching methods. Section 4 shows the improvements for evaluation coming from the use of an integrated dataset, and presents the results of the analysis. Section 5 offers some concluding remarks.

[♦] ADDRESS FOR CORRESPONDENCE: rettore@stat.unipd.it

2. Mobility Lists: provisions and questions about their impact

2.1. The policy

The policy, regulated mainly by laws 233/1991 and 236/1993, combines income support for eligible dismissed employees with substantial benefits to employers who hire them. The policy is characterised with respect to three basic components: the length of the period over which dismissed workers are eligible for the benefits, the direct benefits to enrolled workers and the benefits to hiring firms. The exact content of the package of benefits varies according to the size of the dismissing firm and to the worker age at dismissal.

Workers younger than 40 year, aged 40-49 and older than 49 are entitled to one, two and three years of eligibility for the benefits, respectively. During the eligibility period workers are allowed to experience temporary employment spells without loosing their eligibility status. The clock measuring time since enrolment stops as they start a temporary employment spell and restarts as they complete it re-entering unemployment. The duration of a single temporary employment spell cannot be longer than one year and the total duration of temporary employment spells experienced by a worker during his/her eligibility period cannot be longer than the eligibility period itself.

In addition, workers over 49 meeting some additional conditions with respect to retirement rules are entitled to extended income support up to retirement age: this is the so-called "long mobility"[1].

As for the passive component of the policy, enrolled workers dismissed by a firm with more than 15 employees receive an income support over the eligibility period they are entitled to. The income support is equal to 80% of the worker wage at the time of dismissal over the first year of eligibility. During the second year of eligibility the replacement ratio drops to 64%.

As for the active component of the programme, firms hiring workers from the Lists on a permanent basis enjoy an 18-month long massive cut in social security contributions: they drop from the standard rate to the rate due for apprentices, which is about 2.5% of the standard one. Firms can also hire workers from the Lists on a temporary (up to) one-year basis, and obtain an (up to) one-year cut in social security contributions, the same size as before. Lastly, firms can largely cumulate these cuts by hiring workers on a temporary one-year contract and then switching to a permanent one as it expires: this way, the cut in social security contributions they enjoy lasts two years.

In addition, firms hiring workers from the Lists on a permanent basis receive a bonus equal to 50% of the residual benefits the hired worker would have received had s/he remained in the Lists. This feature of the programme is close to the benefit transfer scheme proposed by Snower (1994). However, its practical relevance is doubtful when compared to the benefits for the hiring firm coming from the cut in social security contributions (see Paggiaro and Trivellato, 2002).

2.2. Issues in designing the evaluation of the policy impact

The crucial issue concerns the impact of the policy on the chances for participants to move into employment. The mixture of active and passive components is such that it is *a*

[1] There are some other minor exceptions, which are not relevant to our case study.

priori uncertain whether enrolled workers will flow into employment at an higher or lower rate than in the counterfactual scenario. This motivates the empirical evaluation we, and many others before us, run.

Limitations to the evaluation exercise are severe. The identification of the impact of introducing/withdrawing the whole policy is precluded, since a comparison group suitably approximating the counterfactual is hard to find. So one is eventually led to point to identification of the differential impact of alternative packages of benefits. In particular, in this paper we focus on the identification of the impact of:

(a) eligibility for two years of benefits as opposed to just one year, and
(b) eligibility for both components of the policy as opposed to eligibility just for the active component.

Setting the evaluation problem this way, the crucial econometric problem becomes how to deal with the selection bias arising from the fact that, by the very same design of the policy, alternative packages of benefits are issued to workers differing with respect to their age and/or to the size of the firm they have been dismissed by.

As workers from the age group over 49 years very often take the "long mobility" option and flow directly to retirement, in the following we focus on workers up to 49 year old.

2.3. Implications for the data needs

The feasibility of a credible evaluation of the differential impact of the various provisions of the policy is conditional on the availability of adequate data. As a result of the management of the policy, regional administrative archives are available. They register enrolments in the Lists, exits to employment and exits due to expiration of the eligibility period. This is the kind of data used by all previous studies on the ML impact.

Unfortunately, data from these archives lack some essential information. Firstly, only poor information on employment spells during the eligibility period is available; specifically, the administrative archives lack information on temporary jobs, which is fundamental to an appropriate evaluation of the policy. Secondly, when the worker leaves the Lists – either because s/he finds a permanent job or because s/he completed the eligibility period – no additional information on the subsequent labour market history is collected, thus the analysis of medium-term effects is precluded. Finally, there is essentially no information on the pre-enrolment labour market history, which is crucial to control for across workers heterogeneity.

In this paper we fill the lack of information by linking the ML archive to the Netlabor archive for some provinces of the Veneto region, in the North-East of Italy. Netlabor is the archive resulting from the operations of the public labour exchange (see below).

In addition, the resulting linked dataset turns out useful for checking the consistency, and indirectly the quality, of data coming from the two sources and for enriching information on socio-demographic characteristics of workers in the Lists.

A major limitation of the linked dataset is the lack of information on wages.

3. The design of the impact evaluation

There is no sensible comparison group made up of unemployed workers not eligible for the benefits of the policy. Thus, the only room left for evaluation is to look at the differential impact of alternative packages of benefits. As already pointed out, we will evaluate (i) the impact of being eligible for two years of benefits rather than just one, and (ii) the impact of being eligible for receiving both the passive and the active component of the policy rather than just the active one.

Since the content of the package of benefits varies across workers depending on their age at the time of dismissal and on the size of the dismissing firm, the possible impact of the benefits might be obscured by the differential composition of the groups receiving alternative packages of benefits. This is precisely the econometric problem we need to solve.

3.1. Evaluating the impact of the second year of eligibility

Unemployed workers younger than 40 at the time of firing are eligible for one year of benefits, while those aged 40 to 49 are eligible for one additional year (with or without the income support, depending on whether the dismissing firm is above or below the 15 employees threshold). The treatment whose impact we seek to identify here is exactly the eligibility for the additional year. The treatment status is a deterministic function of age at the time of firing according to the rule

$$I = \begin{cases} 1 & \text{age} \geq 40 \\ 0 & \text{otherwise} \end{cases},$$

where I=1 denotes eligibility for the additional year of benefits.

The outcome we look at is the fraction of days a worker has been working in each of the 36 months subsequent to the enrolment in the Lists. Let Y^T and Y^{NT} be the outcomes a specific worker would experience being exposed to, and denied, the treatment, respectively. The mean impact of the treatment on the treatment group is

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

The last term in equation (1) is unobservable by construction, since the outcome Y^{NT} is never observed on the treatments. We do observe the mean value of Y^{NT}, but on the comparison group only. By contrasting it to the mean outcome experienced by the treatments, we obtain the following identity:

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

which clarifies that the observed difference between treatments and controls includes the so-called selection bias, namely the difference between treatments and controls we would have observed had the treatments been denied the treatment.

In our case, by the design of the policy treatments are older than controls, which implies that the observed difference between the two groups in the probability of being at work in the months after enrolment includes the likely effect of age.

A popular strategy to solve the selection bias problem in the presence of a selection process deterministically depending on an observable characteristic of the subjects is the Regression Discontinuity Design (RDD; see Hahn, Todd and Van der Klaauw, 2001). RDD exploits the near independence between the treatment status I and the potential outcomes (Y^T, Y^{NT}) holding in a neighbourhood of the threshold relevant for selection:

$$(Y^T, Y^{NT}) \perp I \mid \text{age} = 40 .$$

The straightforward intuition is that treatments close to the threshold in the absence of the treatment would experience the same outcome as the controls close to the threshold, since they are approximately the same with respect to age, which is the *only* individual characteristic relevant for the selection process.

The drawback of RDD is that, if the programme impact is heterogeneous across subjects – as it is likely to be, then it only allows to identify the mean impact in the neighbourhood of the threshold for selection.

As an alternative identification strategy, to overcome this drawback we use matching estimators. That is, we compare treatments to controls conditional on a suitable set of observables *X.* The unbiasedness of the resulting estimator for the mean impact on the treatments crucially rests on the so-called ignorability condition:

$$Y^{NT} \perp I \mid X . \tag{3}$$

Taking into account that in our case *I* is a deterministic function of age, condition (3) as applied to our problem asserts that the matching estimator works if conditioning on *X* removes the dependence between Y^{NT} and age. This condition has testable implications since Y^{NT} is observable on the controls, namely all workers younger than 40. To test the hypothesis

$$H_0 : Y^{NT} \perp age \mid X , \tag{4}$$

we split the controls in two sub-groups, young and old; then, we match them on *X*; finally, we check whether the mean outcomes in the resulting sub-groups differ.

By the same token, we could compare old treatments to young ones after balancing the two sub-groups with respect to *X*, to check whether age matters for their outcome. Note, however, that the outcome observable on treatments is $Y^T=Y^{NT}+(Y^T-Y^{NT})$. As an implication, when rejecting the null hypothesis

$$H_0 : Y^T \perp età \mid X ,$$

one cannot say whether it is Y^{NT} that depends on age or the impact (Y^T-Y^{NT}) or both. On the other hand, when accepting the null hypothesis one can confidently conclude that neither Y^{NT} nor (Y^T-Y^{NT}) depend on age (unless one is ready to believe that both variables depend on age in a way such that their sum does not).

As for the computational aspects, to ease calculations we match treatments to controls on the propensity score:

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

the usual device for constructing matched sets (Rosenbaum and Rubin, 1983).

3.2. Evaluating the impact of the income support

Workers fired by firms with up to 15 employees are eligible for the active component of the policy (for one or two years, depending on their age), while workers fired by firms with more than 15 employees are eligible also for the income support component (again for one or two years, depending on their age). We move from the crude contrast ‘above/up to the 15-employee threshold’ to identify the impact of the income support on the probability of being at work in each of the 36 months subsequent to the enrolment in the Lists.

In contrast with the previous case, however, both logical and practical problems preclude using RDD to evaluate the impact of the income support. As for the practical problem, our dataset does not include information on firm size. But even if we could observe that variable, likely the contrast treatments/controls at the firm size threshold would not identify the mean impact of the income support, because there are other discontinuities in the Italian labour market regulations taking place exactly at the same threshold (the main one is in the employment protection legislation against unjust dismissals: see Schivardi and Torrini, 2004). Faced with a discontinuity in the probability of being at work at the firm size threshold, one could not say whether it is caused by the income support or is due to others institutional discontinuities.

The route we take is again based on matching. We match one control to each treatment on basic socio-demographic characteristics (gender, age, education) as well as recent labour market history (labour force status in the 24 months previous to enrolment in the Lists, characteristics of the last job).

As compared to the case just discussed in Section 3.1, here we cannot exploit the knowledge of the selection process to obtain a specification test. To seek for evidence supporting the validity of the identifying restriction on which the matching estimator relies, we use the following test. Let u be the unobservables relevant for the selection process as well as for the labour market outcome. The identifying restriction on which the validity of the matching estimator relies is the following:

$$H_0 : Y^{NT} \perp u \mid X .$$

In words, conditional on X, the unobservable u is irrelevant for the outcome Y^{NT}. If this condition is met, then controlling for X should produce two groups exhibiting the same Y^{NT} – the same labour market history both after *and* before enrolment in the Lists. Apparently, this implication is not testable with reference to the labour market history *after* enrolment in the Lists, since Y^{NT} is not observed on treatments. Instead, it is testable with reference to the pre-enrolment history, that is when we observe Y^{NT} both on treatments and controls.

Operationally, we do not include among the matching variables X the first three months of labour market history preceding enrolment in the Lists, and we use them to check whether treatments and controls matched on all other variables differ. Evidence of significant differences would lead us to conclude that controlling for the variables on which matching actually takes place is not enough to solve the selection bias problem.

4. The case study

4.1. Data and descriptive evidence

The data we use in this study are from the following sources.

(a) The administrative archive resulting from the management of ML in the Veneto region, up to April 1999. Due to data quality problems (see Paggiaro and Trivellato, 2002), we restrict our attention to enrolments over the period January 1995-April 1999. Worker's demographics (gender, age, education, province of residence), industry of the dismissing firm and qualification of the worker, date of enrolment in the Lists, entitlement to income support are recorded for each worker. In principle, the worker is then followed during his/her eligibility period up to either (i) hiring on a permanent basis or (ii) exhaustion of the eligibility period. If none of these events is observed, then (iii) the worker is still in the Lists. Thus, in principle time spent in the Lists and current state are observed for each worker. In practice, the problem is that much of this information is unreliable. The only information one can confidently rely on is the occurrence of an enrolment, its timing and the eligibility for the income support (for details, see Gobitti, 1997, and Paggiaro and Trivellato, 2002).

(b) The Netlabor archive from public labour exchanges in Veneto, up to 2001. The quality of the information in this archive varies a lot over time and across provinces, so we restrict our attention to the provinces of Treviso and Vicenza. The Netlabor archive provides reliable information on worker's demographics and on each employment spell experienced by the worker, whether prior to or during or after the eligibility period. In particular, (i) the type of the labour contract (permanent *vs*. temporary, part-time *vs*. full-time, apprenticeship), (ii) worker's qualification, (iii) industry of the hiring firm, and (iv) beginning and end of each employment spell are documented (more details in Bassi, Gambuzza and Rasera, 2001).

The linkage procedure is described in Paggiaro and Trivellato (2001) and Paggiaro (2002). The linked dataset improves a lot on the ML archive, which has been the typical source of information previous studies relied on to assess the impact of the policy. Firstly, the linkage to Netlabor allows us to observe a much higher number of transitions to permanent employment as compared to what emerges from the ML archive. This is partly due to a wider observation window, but also to a large number of transitions unrecorded in the ML dataset. It is worth noting that the occurrence of spells of temporary employment during the eligibility period is far from negligible: on the whole, in our sample time spent on temporary employment amounts to 38% of the observed time in the Lists; 72% of the workers enrolled in the Lists experience at least one spell of temporary employment during the eligibility period; 60% of those eventually transiting to permanent employment experience a spell of temporary employment, which in most cases is immediately switched into a permanent position as it expires.

In the following we focus on workers less than 50 years old, enrolled in the Lists in the years 1997 and 1998. We end up with 4,230 sample units[2]. The breakdown by gender, age group and eligibility to income support is in Table 1.

Table 1. *Workers enrolled in the ML by gender, age group, eligibility to income support. Provinces of Treviso and Vicenza, 1997 and 1998.*

	Men with income support		*Women with income support*		*Men without income support*		*Women without income support*		Total	
Age group	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
<30	169	*23.4*	425	*32.9*	150	*30.7*	775	*44.8*	1,519	*35.9*
30-39	276	*38.3*	497	*38.5*	197	*40.4*	712	*41.2*	1,682	*39.8*
<40	445	*61.7*	922	*71.4*	347	*71.1*	1,487	*86.0*	3,201	*75.7*
40-49	276	*38.3*	369	*28.6*	141	*28.9*	243	*14.0*	1,029	*24.3*
Total	721	*17.1*	1,291	*30.5*	488	*11.5*	1,730	*40.9*	4,230	*100.0*

As a summary of the worker's labour market history, we use the month by month proportion of days spent at work, no matter for the type of labour contract; in the sequel we will refer to it as the 'employment rate'. We also look at the length of time spent in the Lists waiting for the first job and the first job on a permanent basis, respectively.

We present most of our empirical analyses and results by means of graphs.

The crude contrast of workers eligible for two years of benefits (40 to 49 years old) to workers eligible for one years of benefits (less than 40 years old) is in Figure 1. Their employment rates from 24 months before to 36 months after enrolment in the Lists are represented separately by gender and by entitlement to income support. The main evidence can be summarized as follows:

- Employment rates after enrolment sharply differ in the two age groups, in particular among workers eligible for the income support. Whether this is a genuine causal effect of the policy or is rather due to selection bias, is precisely the issue we deal with.

[2] We dropped from the sample a small number of workers for whom there is evidence of frauds in the use of the policy provisions and the few ones for whom information is missing on variables included in the matching set.

- In fact, the pattern of employment rates *before* enrolment in the Lists suggests that selection bias is non negligible, at least for men.
- As for workers eligible for the income support, workers younger than 40 exhibit much higher employment rates than workers 40 to 49, with the difference tapering off after the second year, that is when also the older group is no longer on the income support.
- As for workers without income support, there are no relevant differences across age groups among women, while among men the 40-49 years group shows higher employment rates than the younger one.

Figure 1 about here

Evidence on the relative role of age-driven selection bias and of the ML impact comes from Figure 2, which reports the employment rates 12 and 36 months after enrolment by age at enrolment. In the logic of RDD, Figure 2 highlights the discontinuity of the employment rates at the 40-year threshold, which identifies the impact of the eligibility for an additional year on 40 years old workers. It also highlights the pattern of employment rates by age. The main evidence is the following:

- There is a large variability of employment rates over the observed range of age, particularly for women and for workers less than 30 years old, with older workers exhibiting lower rates.
- As for the discontinuity at the 40-years threshold, the clearest evidence is for men: a negative impact of the additional year on employment rates emerges for workers entitled to income support, while the impact is positive for workers without income support. Among women, there is always a positive impact of the additional year, but the size of the impact is comparatively smaller.

Figure 2 about here

4.2. Estimating the impact of the additional year of eligibility

In this Section we apply the techniques presented in Section 3.1 to estimate the mean impact of the additional year of eligibility for the ML. The comparison group is obtained by matching a worker eligible for one year in the ML to each worker experiencing the treatment. We use one-to-one matching on the propensity score, allowing for at most a .01 difference in the propensity scores. The analysis is carried out separately by gender and entitlement to income support. The propensity scores are estimated by logistic regressions, using as covariates a province dummy, workers' qualification and education, industry of the dismissing firm, month by month labour force status over the two years prior to enrolment. Table 2 shows the performance of matching.

Table 2. *Matching on p-score workers aged less than 40 to workers aged 40 to 49*

		Men with income support		*Women with income support*		*Men without income support*		*Women without income support*		,	
		N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Treatments	(age 40-49)	276		369		141		243		1,029	
Controls	(age <40)	445		922		347		1,487		3,201	
Matched treatments		236	*85.5*	345	*93.5*	111	*78.7*	233	*95.9*	925	*89.9*
% matched controls			*53.0*		*37.4*		*32.0*		*15.7*		*28.9*

On the whole results are satisfactory: in the worst case – men without income support – 79% of the treatments got their match. Note that differences in the composition of the two groups with respect to *X*, which is the main reason for the missing matches, is less severe among women.

As we outlined in Section 3.1, the full knowledge of the selection process allows us to test whether matching succeeds in solving the selection bias problem. Figure 3 clearly shows that the age effect has not been eliminated by conditioning on *X*, in particular for women. This evidence is strengthened by Figure 4, which shows the differences in the employment rates between workers in the age groups <30 and 30-39, respectively, matched on the same propensity scores as before.

Figures 3 and 4 about here

The age effect on employment rates, as it results from Figure 3, is weaker among workers older than 30. This evidence suggests to select the matched comparison group looking only at the oldest among the workers excluded from the treatment. Table 3 documents the performance of matching using only workers 30 to 39 years old as the comparison group. The proportion of treatments getting their match is lower than in the previous case, in particular among men, but it turns out still satisfactory (in the worst case – men without income support – it is 67%). Figure 5 shows the distributions of propensity scores for treatments and controls, by gender and entitlement to income support. Apparently, the distributions are quite well overlapping, thus easing the search for suitable matching.

Table 3. *Matching on p-score workers aged 30 to 39 to workers aged 40 to 49*

		Men with income support		*Women with income support*		*Men without income support*		*Women without income support*		**Total**	
		N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Treatments	(age 40-49)	276		369		141		243		1029	
Controls	(age 30-39)	276		497		197		712		1682	
Matched treatments		188	*68.1*	337	*91.3*	94	*66.7*	221	*90.1*	840	*81.6*
% matched controls			*68.1*		*67.8*		*47.7*		*31.0*		*49.9*

Figure 5 about here

Figure 6 shows that, by restricting the comparison group to the oldest among workers eligible for one year in the Lists, the age effect among not treated workers is much weaker.

Among treated workers evidence is less clear, and among women employment rates 36 months after enrolment seem to be lower for the older ones.

Figures 7 and 8 confirm this evidence, as there is no sign of an age effect among workers younger than 40[3]. On the contrary, among women undergoing the treatment there is some evidence of an age effect in the third year after enrolment. As we pointed out in Section 3.1, whether this is an effect of age on the counterfactual outcome or on the impact of the additional year it is not possible to say. Thus, in the interpretation of results about this group one should be cautious.

Figures 6, 7 and 8 about here

Figures 9 and 10 present the estimates of the impact of the additional year of eligibility on the treatments (who got a match: see Table 3). The overall evidence is fully consistent with the impacts at the 40-year threshold identified in Figure 6. The additional year of eligibility enjoyed by male workers in the 40-49 age group *with income support* results in a 10 to 20 percentage points negative impact on their employment rates during the second and third year after enrolment in the Lists. This is a clear evidence that the effect of the passive component of the policy prevails on the effect of the active one.

As for men *without income support*, the sign of the impact is reversed: starting from the beginning of the second year, employment rates of treatments are higher than those of matched controls by 10 to 20 percentage points. Even if with some caution related to the small sample size of this group (94 enrolled workers: see Table 3), this evidence points to a positive effect of the active component of the policy.

As for women, the overall pattern of impacts is very close to the one observed for men, but their size is much smaller and often statistically not significant. A negative impact emerges for women with income support during the second year of eligibility, and a positive one by the end of the third year for those without support. However, one should keep in mind that these results are under the threat of a potential selection bias, in particular for women with income support. Figure 8 shows that the employment rates for women 40 to 49 decrease with age. If this pattern were driven by the impact of age on the counterfactual outcome, the mean impact in Figure 10 would be systematically underestimated (see the discussion at the end of Section 3.1).

Summing up, the evidence so far is that an additional year of eligibility for the ML bears (i) a positive impact on the employment rates of workers dismissed by firms up to the 15-employees threshold, who are eligible only for the active component of the policy, while it bears (ii) a negative impact for workers dismissed by firms above the threshold, who are eligible for both the active and the passive component of the policy. Impacts are large and statistically significant for men, while they are much smaller and only occasionally significant for women.

Carefully note that this evidence does not allow us to conclude that the impact of the passive component is negative, as the composition of the pools of workers from firms up to and above the 15-employees threshold, respectively, could sharply differ. This problem is discussed in Section 4.3.

[3] In the pre-enrolment period some statistically significant differences appear among workers without income support. In principle this should not happen, since pre-enrolment labour market history is included in the matching set. In practice, due to the small sample size we had problems in the specification of propensity scores.

Figures 9 and 10 about here

By using the same treatments and matched controls, we turn to the analysis of the impact of the additional year of eligibility on the duration of the spell in the Lists waiting for the first job and the first permanent job, respectively.

Figure 11 presents Kaplan-Meier estimates of survival functions for the waiting time for the *first* job, no matter for the type of the associated labour contract. The main evidence is that a significant impact emerges only for men with income support, for whom the additional year bears longer waiting times, consistently with the results presented in Figure 10. For the other three groups survival functions of treatments and controls are statistically indistinguishable (this evidence is confirmed by the results of a 5% log-rank test on the whole distributions), but even in these cases the sign of the effects is consistent with those in Figure 10.

Figure 11 about here

A complementary evidence comes from Figure 12, which shows the estimates of the hazard functions (smoothed by kernel methods) associated to the survival functions of Figure 11. The main evidence is the decline of the hazard, particularly sharp for men. The only noticeable exception to this pattern is for male workers eligible for one year of ML with income support: their hazard exhibits a large peak at the beginning of the second year, that is when their eligibility period is expired. Thus, among males the probability of transiting to employment by the end of the first year of eligibility in the presence of income support is negatively affected by the eligibility for an additional year in the ML.

Figure 12 about here

Figures 13 and 14 present survival and hazard functions, respectively, for the waiting time for a *permanent* employment. That is, time spent in unemployment and in temporary employment contribute the same way to the definition of the spell (note that these are the typical durations analysed in previous studies of the ML impact; see Paggiaro and Trivellato, 2002, among others).

The impact of the additional year of eligibility on the survival probability is positive for men with income support, especially starting from the second year, while it is statistically not significant for the other three groups. The hazard functions in Figure 14 highlight that transition to permanent employment is particularly high by the end of the first year, and partly after two years. This is essentially because, one year after enrolment, transitions to permanent employment for some of those who have continuously been unemployed cumulate with transitions for some of those who experienced a spell of one-year temporary employment converted into a permanent position.

Figures 13 and 14 about here

Summing up, the impact of the additional year of eligibility is particularly strong for men with income support: employment rates during the 36 months after enrolment are substantially lower (Figures 9 and 10), while time both to the first job after enrolment (Figures 11 and 12) and to the first permanent job (Figures 13 and 14) is longer. As regards the other groups – women with income support and all workers without it, evidence shows an impact of the additional year on employment rates but not on time to the first job.

4.3. Estimating the impact of income support

In this Section we present the estimates of the mean impact of income support on workers
entitled to it. Entitlement to income support depends on the size of the dismissing, firm while its duration depends on the worker age at the time of dismissal. To begin with, a natural comparison group is made up of workers in the same age dismissed by firms with at most 15 employees.

Evaluation is carried out separately by gender and age group, by matching on the propensity score a comparison worker to each worker in the treatment group. Covariates included in the propensity score are the same as in the previous analyses plus a polynomial on age, in order to take into account the different age composition of the pool of workers dismissed by firms with more than and up to 15 employees, respectively.

Preliminarily, we carry out the specification test discussed at the end of Section 3.2, to test whether conditioning on X is enough to solve the selection bias problem. We exclude the first three months in the observation window (months -24, -23 and -22 with respect to the enrolment date) from the labour market history included in the propensity score. Then we look at employment rates in those months for treatments and matched controls: as discussed in Section 3.2, if the matching variables were enough to compensate for the selection bias the two groups should not exhibit differences in months -24 to -22.

In our sample differences result highly significant even if not large for women, less significant for men in the 40-49 age group, not significant for younger men. On the whole, the set of matching variables available to us does not seem rich enough to compensate for the different composition of the pool of workers dismissed by firms below and up to the 15-employees threshold, respectively.

Nonetheless, we go on with the evaluation exercise and include in the set of matching variables the labour force status in months -24, -23 and -22, in the hope that this way the bias will turn out at least attenuated. Results on the performance of matching are in Table 4. The first thing to note is that, in sharp contrast to the performance of matching in Section 4.3, the proportion of treatments for whom the match turns out successful is small. The only exception are women aged 30 to 39, which is the only group for whom the size of the pool of controls exceeds that of treatments.

Figure 15 highlights the severity of the common support problem. Apparently, the distribution of p-scores for treatments is quite a lot to the left of the corresponding distribution for controls. This evidence confirms, from a different perspective, that workers dismissed by small firms are systematically different from those dismissed by firms above the 15-employees threshold.

Table 4. *Matching on p-score workers without income support to workers with it*

		Men 30-39		*Women 30-39*		*Men 40- 49*		*Women 40- 49*		Total	
		N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Treatments	(with income support)	276		497		276		369		1,418	
Controls	(without income support)	197		712		141		243		1,293	
Matched treatments		112	*40.6*	346	*69.6*	71	*25.7*	147	*39.8*	676	*47.7*
% matched controls			*56.9*		*48.6*		*50.4*		*60.5*		*52.3*

Figure 15 about here

Figure 16 and 17 present the estimated impact on the employment rates. With the exception of young men, the main evidence is a negative impact of entitlement to income support during the entitlement period, which tends to fade out more or less rapidly by the end of it.

Women 30 to 40, who are entitled to one-year income support, exhibit negative statistically significant differences during the first year of enrolment. Similar evidence holds for men and women 40 to 49, with the impact lasting more than two years, thus closely reflecting the length of the entitlement period to income support.

In sharp contrast, men 30 to 39 feature a positive impact of entitlement to income support; besides, the impact increases over time. Note that this feature is mainly due to the fact that the employment rates for workers without income support after one year level out at .7, while the corresponding rates for workers with income support steadily increase over the 36 months. A tentative explanation for the pattern observed for workers without income support is that an appreciable fraction of those who do not find a new job in the months immediately after dismissal moves to other kinds of employment not covered by the Netlabor archive, such as self-employment or maybe non-regular jobs.

Figures 16 and 17 about here
----

5. Concluding remarks

In this paper we provide new evidence on the impact of the benefits issued by the Italian 'Mobility Lists' policy to workers dismissed from their previous job on their subsequent labour market outcomes. The policy features both an active and a passive component, which makes the sign of its expected impact *a priori* uncertain. The active component amounts to a generous wage subsidy to any firm recruiting workers from the Lists, while the passive component – when present – is made up of an appreciable income support to the workers enrolled in the Lists. Duration of the eligibility period depends only on the worker's age at dismissal, while the size of the dismissing firm determines whether the worker is or is not eligible for the income support.

We use a new dataset resulting from linking information on workers from two different administrative archives, and show that it provides a much richer description of the labour market history than that used in all previous studies on the impact of this policy. Moreover, as compared to those studies, which made use of parametric or semi-parametric models, we take on a fully non-parametric methodology to identify the mean impact, complemented by a set of specification tests designed to check the identifying restrictions on which the evaluation strategy relies on. This results also in a clear identification of the sub-populations of workers to which the mean impact refers to.

Focusing on a sample of workers from two provinces of the Veneto region entering the Lists in 1997 or 1998, we separately evaluate: (i) the impact of a longer eligibility for the benefits, that is two years instead of one; (ii) the impact of entitlement to income support.

The impact of the additional year of eligibility turns out apparent for men with income support: employment rates during the 36 months after enrolment in the Lists are substantially lower for workers eligible for the additional year, while waiting times to employment are significantly longer. As for men eligible only for the active component of the policy, the impact of the additional year is significantly positive on employment rates, while it is statistically not significant on waiting times to the first job. Finally, the additional year of eligibility turns out irrelevant in the case of women.

Identification of the impact of income support turned out much less simple. The problem is that the natural comparison group – workers dismissed by firms with up to 15 employees – exhibits a composition apparently different from that of the treatment group – workers dismissed by firms above the 15-employee threshold. The main implications are that (i) we could find a match in the comparison group only for a relatively small sub-group of the treatments, and (ii) we have some evidence that the set of matching variables we use does not fully compensate for the selection bias.

Keeping in mind all that, the main evidence we get is that the impact of income support on employment rates is large and negative over the entitlement period. A notable exception are young men, who feature a positive impact increasing over the 36 months of our observation window: an evidence which calls for further investigation.

References

Atkinson A.B. and J. Micklewright (1991), "Unemployment compensation and labor market transitions: a critical review", *Journal of Economic Literatur*e, 29: 1679-1727.

Bassi F., M. Gambuzza and M. Rasera (2001), "Il sistema informatizzato Netlabor: struttura, caratteristiche e qualità", in U. Trivellato (2001), *cit*..

Bell B., R. Blundell and J. Van Reenen (1999), "Getting the unemployed back to work: an evaluation of the New Deal proposal", *International Tax and Public Finance*, 6: 339-360.

Borzaga C. and G. Brunello (eds.) (1997), *L'impatto delle politiche attive del lavoro in Italia*, Firenze: Edizioni Scientifiche Italiane.

Brunello G. and R. Miniaci (1997), "Benefit transfers in Italy: an empirical study of Mobility Lists in the Milan area", *Oxford Bulletin of Economics and Statistics*, 59: 329-347.

Calmfors L. (1994), "Active labour market policy and unemployment. A framework for the analysis of crucial design features", *OECD Economic Studies*, 22 (1): 7-47.

Gobitti G. (1997), "I lavoratori in mobilità", in Agenzia per l'impiego del Veneto, *Il mercato del lavoro Veneto. Tendenze e politiche. Rapporto 1997*, Milano: Franco Angeli.

Hahn J., P. Todd and W. Van der Klaauw (2001), "Identification and estimation of treatment effects with a regression-discontinuity design', *Econometrica*, 47: 201-209.

Heckman J.J., R.J.Lalonde and J.A. Smith (1999), "The economics and econometrics of active labor market programs", in O. Ashenfelter e D. Card (eds.), *Handbook of Labor Economics, Vol. 3A*, Amsterdam: Elsevier.

Lancaster T. (1990), *The econometric analysis of transition data*, Cambridge (MA): Cambridge University Press.

Meyer B. (1990) “Unemployment insurance and unemployment spells”, *Econometrica,* 58: 757-782.

Paggiaro A. (2002), “I lavoratori in mobilità: informazioni dagli archivi dei Centri per l’impiego”, in Veneto Lavoro (a cura di), *Il mercato del lavoro nel Veneto. Tendenze e politiche. Rapporto 2002*, Milano: Franco Angeli.

Paggiaro A. and U. Trivellato (2001), “Il monitoraggio e la valutazione delle ‘Liste di Mobilità’: prime evidenze dalla integrazione con gli archivi Netlabor”, in U. Trivellato (2001), *cit.*.

Paggiaro A. and U. Trivellato (2002), “Assessing the effects of the ‘Mobility Lists’ programme by flexible duration models”, *Labour*, 16 (2): 235-266.

Rosenbaum P. and D. Rubin (1983), “The central role of the propensity score in observational studies for causal effects”, *Biometrika*, 70: 41-55.

Schivardi F. and R. Torrini (2004), *Firm Size Distribution and Employment Protection Legislation in Italy*, Temi di discussione n. 504, Roma: Banca d’Italia.

Snower D. (1994), “Converting unemployment benefits into employment subsidies”, *American Economic Review*, 84 (2): 65-70.

Trivellato U. (ed.) (2001), *Servizi per l’impiego e ricerche sul lavoro. L’esperienza del Veneto*, Quaderni di economia del lavoro n. 72, Milano: Franco Angeli.

Figure 1: *Employment rates from 24 months before to 36 months after enrolment in the Lists by gender, entitlement to income support and age group. Provinces of Treviso and Vicenza, 1997 and 1998 (95% confidence intervals are reported).*

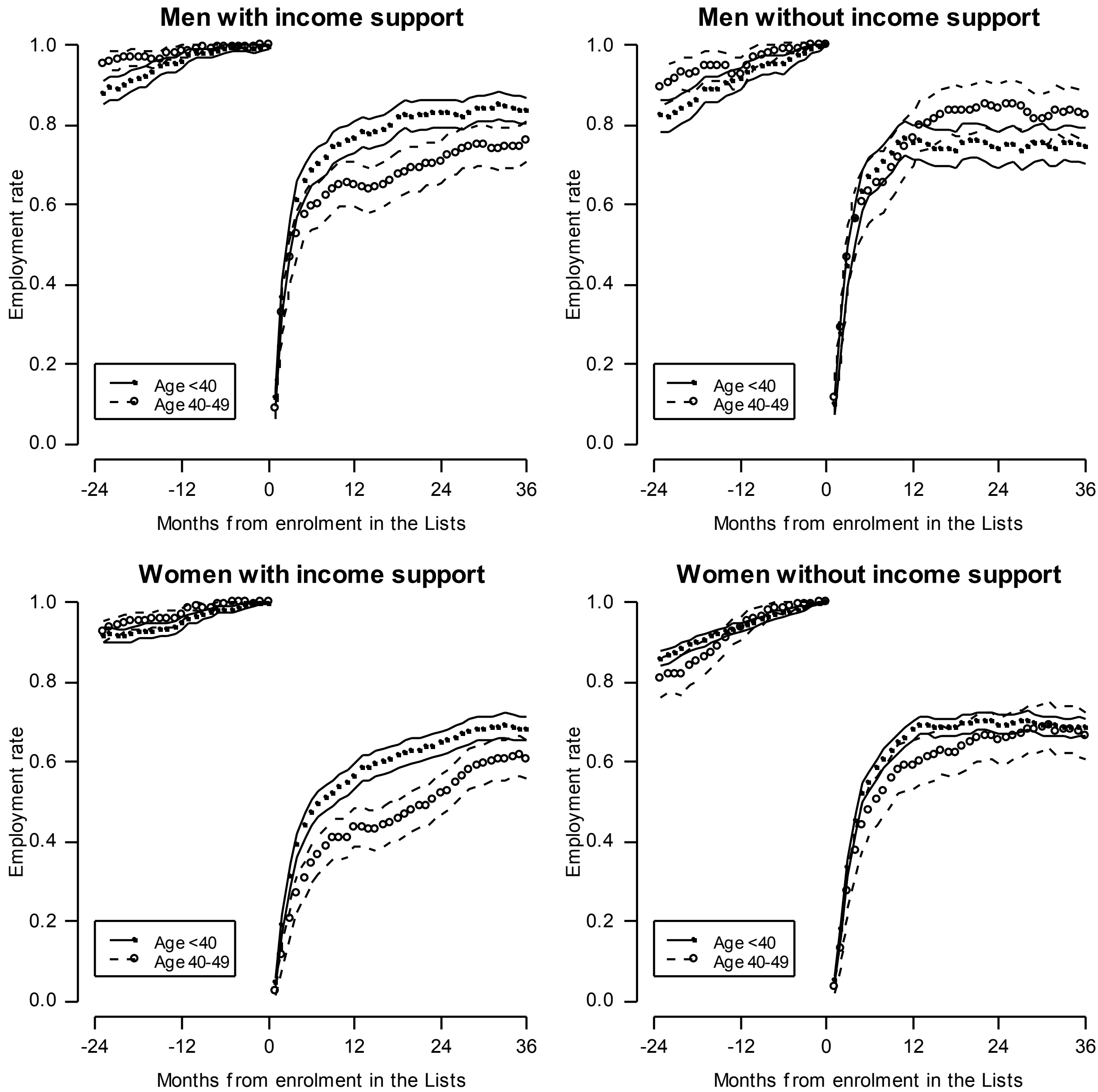


Figure 2: *Employment rates 12 and 36 months after enrolment in the Lists by age, controlling for gender and entitlement to income support (point estimates and polynomial splines). Provinces of Treviso and Vicenza, 1997 and 1998.*

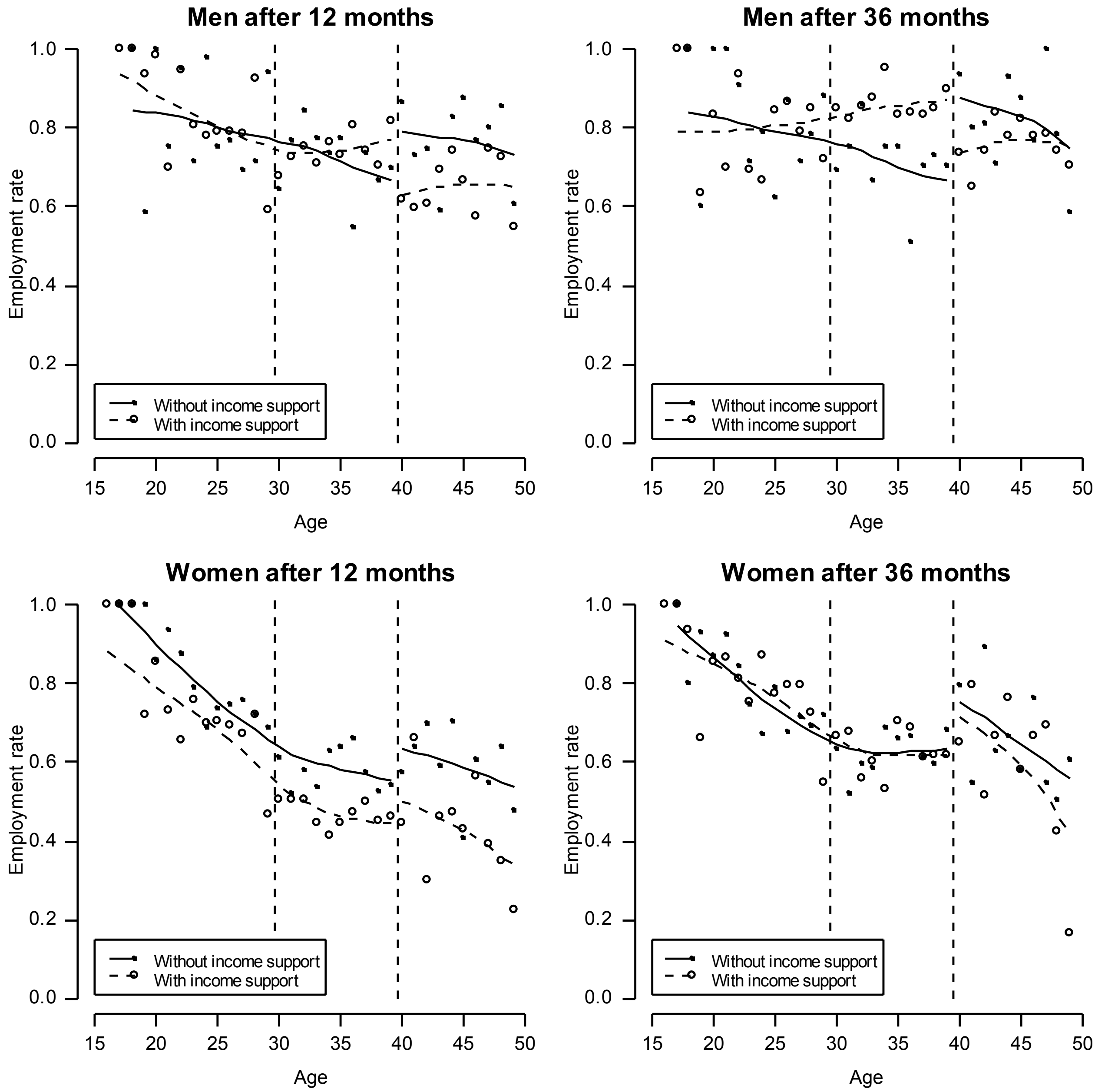


Figure 3: *Employment rates 12 and 36 months after enrolment in the Lists by age, controlling for gender and entitlement to income support (point estimates and polynomial splines). Matching on p-score workers with two years of eligibility (40-49 years) to workers with one year of eligibility (<40 years). Provinces of Treviso and Vicenza, 1997 and 1998.*

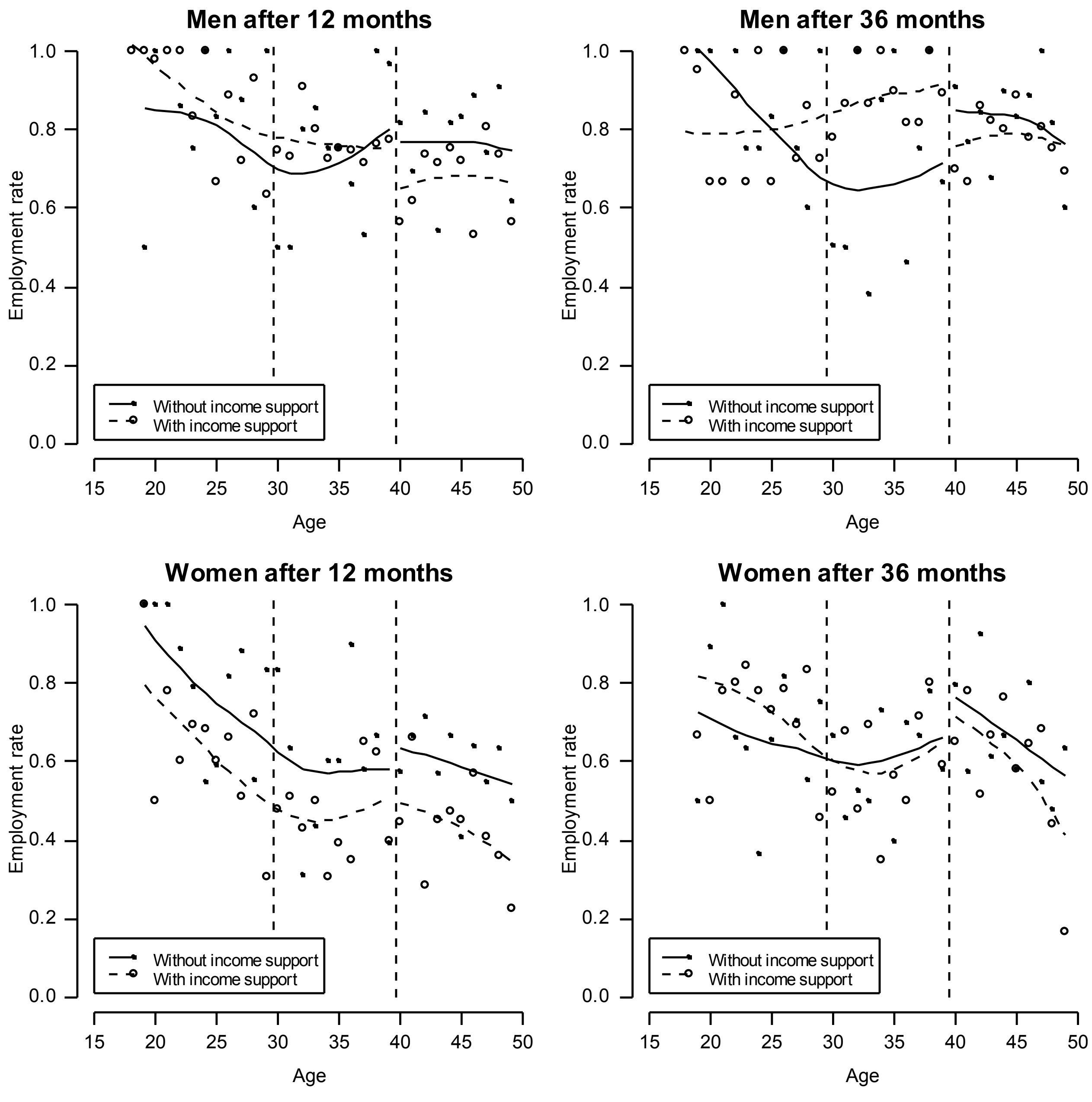


Figure 4: *Testing for selection bias. Differences between employment rates of workers 30 to 39 and of matched workers under 30, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998*

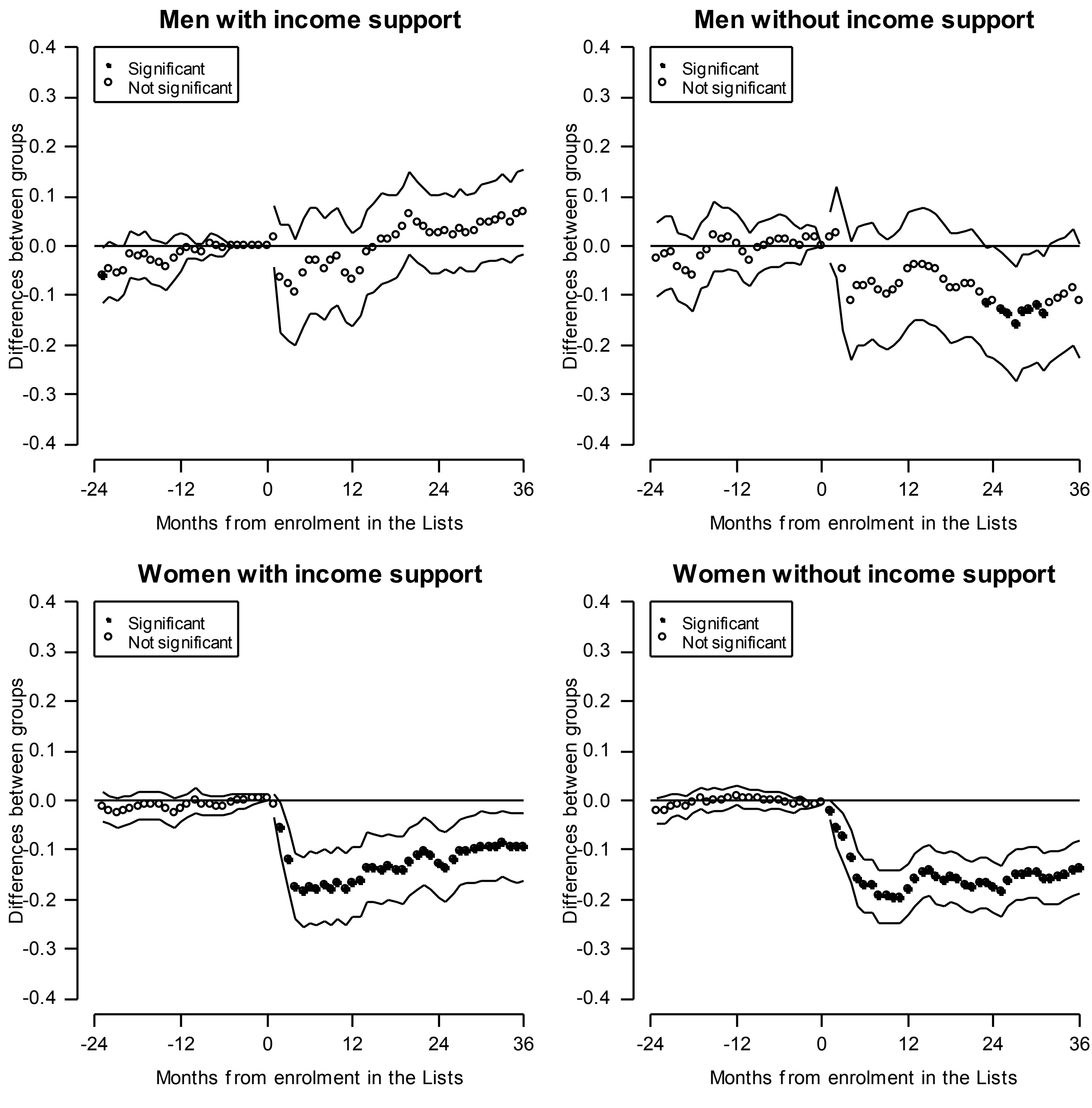


Figure 5: *Estimating the impact of the additional year of eligibility. Distribution of p-score for treatments (workers 40 to 49) and controls (workers 30 to 39), by gender and entitlement to income support. Provinces of Treviso and Vicenza, 1997 and 1998.*

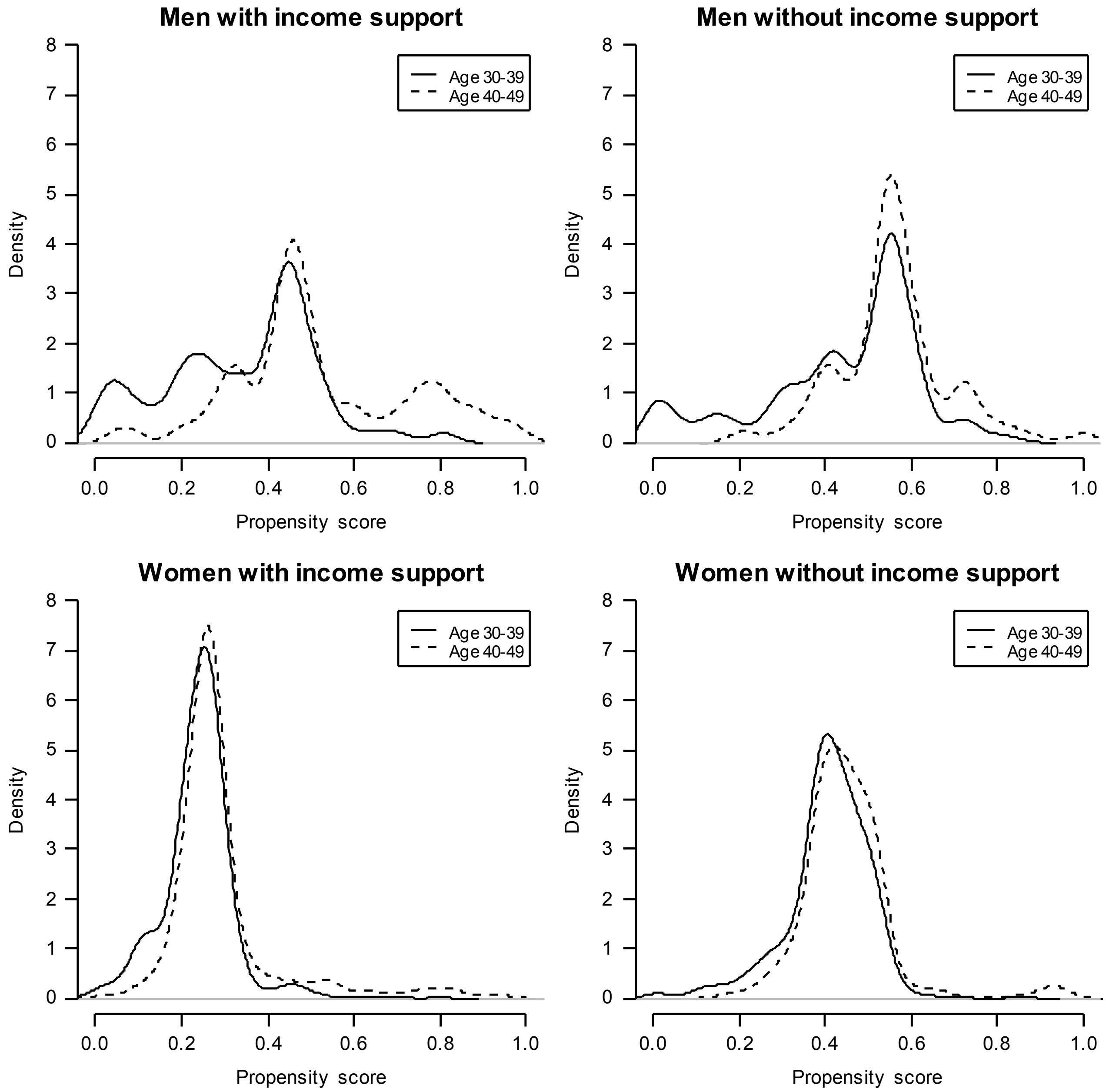


Figure 6: *Employment rates 12 and 36 months after enrolment in the Lists, by gender, age and entitlement to income support. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility (30 to 39). Provinces of Treviso and Vicenza, 1997 and 1998.*

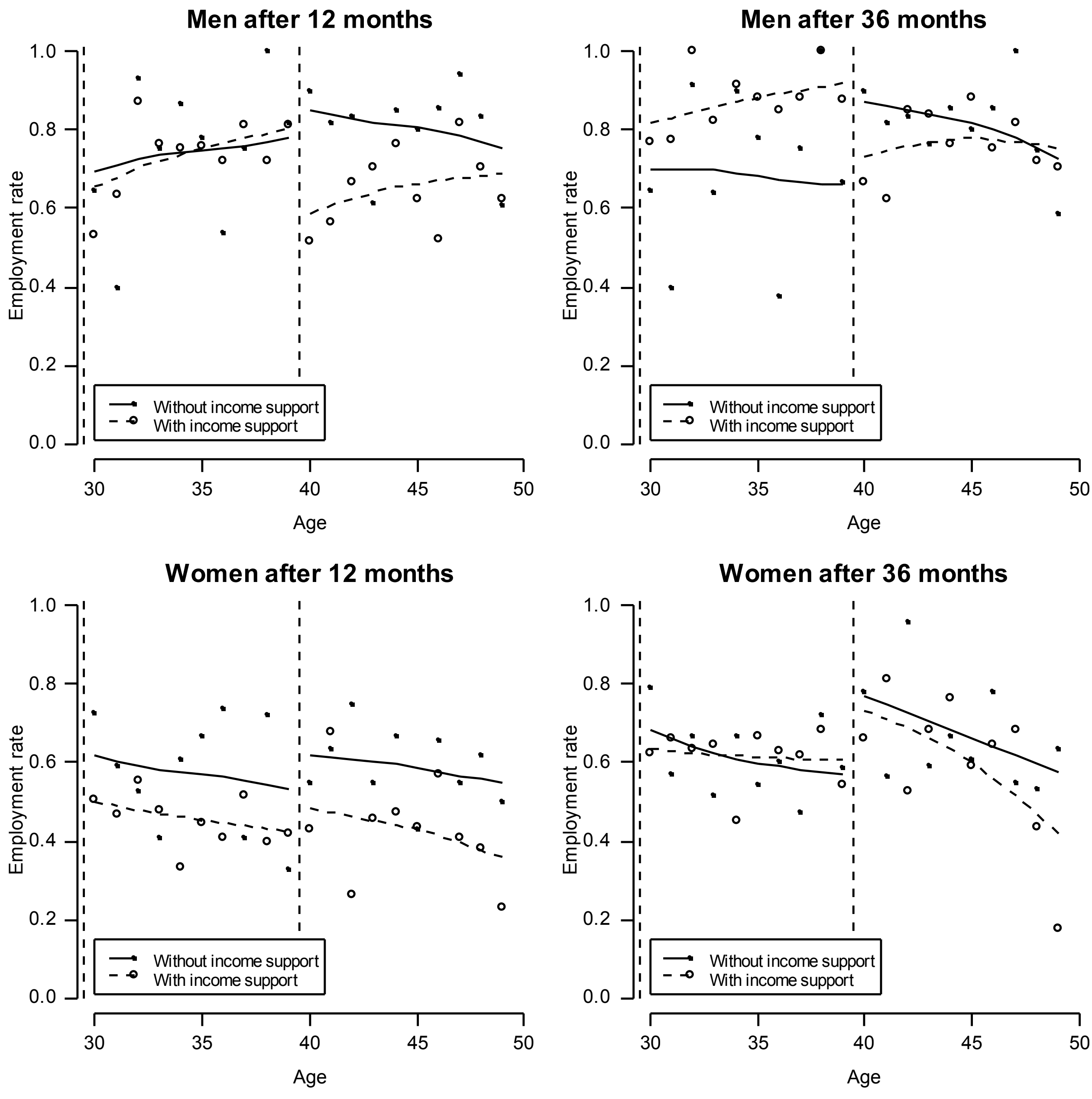


Figure 7: *Testing for selection bias. Differences between employment rates of workers 35 to 39 and of matched workers 30 to 34, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

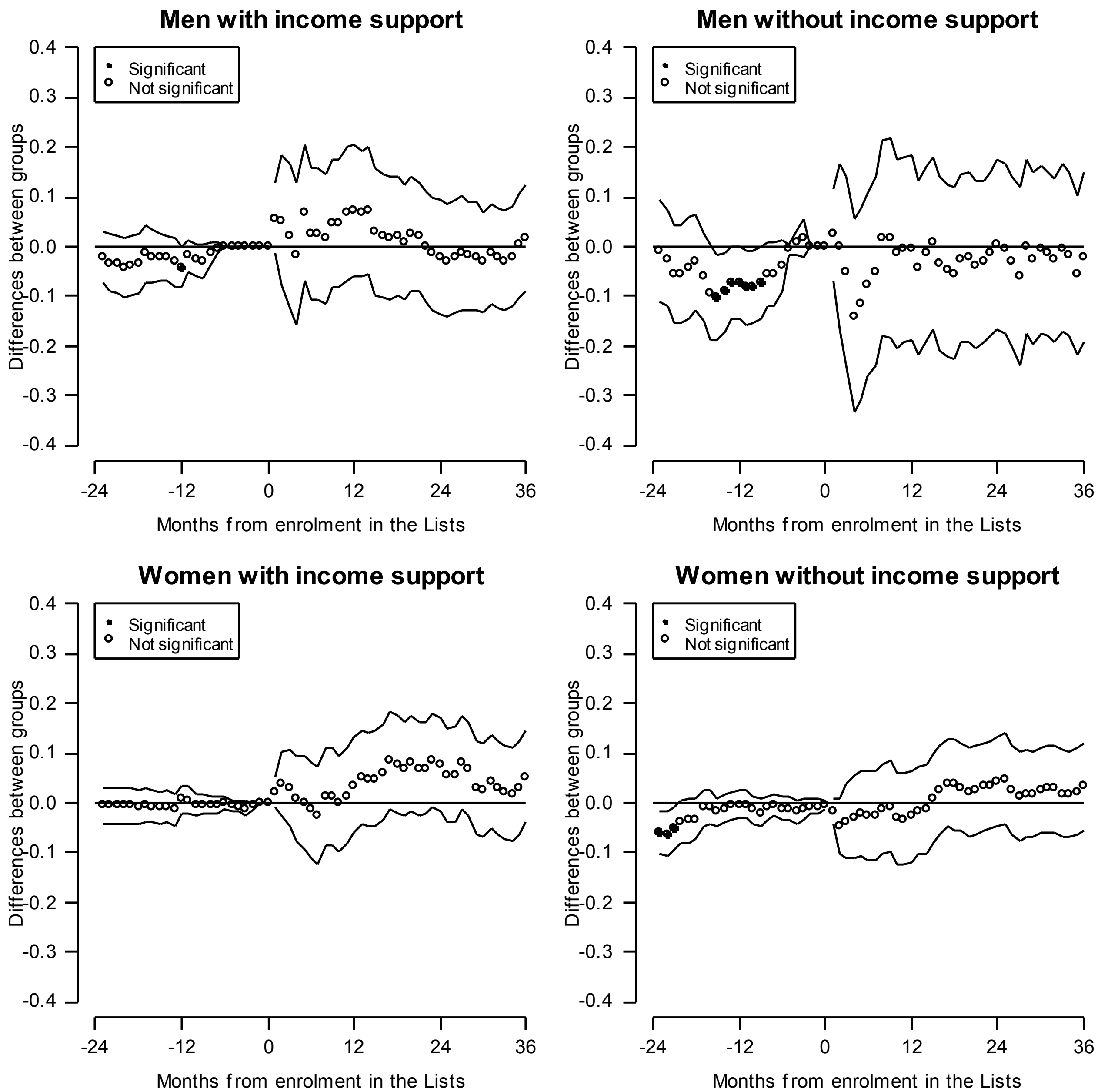


Figure 8: *Testing for selection bias. Differences between employment rates of workers 45 to 49 age and of matched workers 40 to 44, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

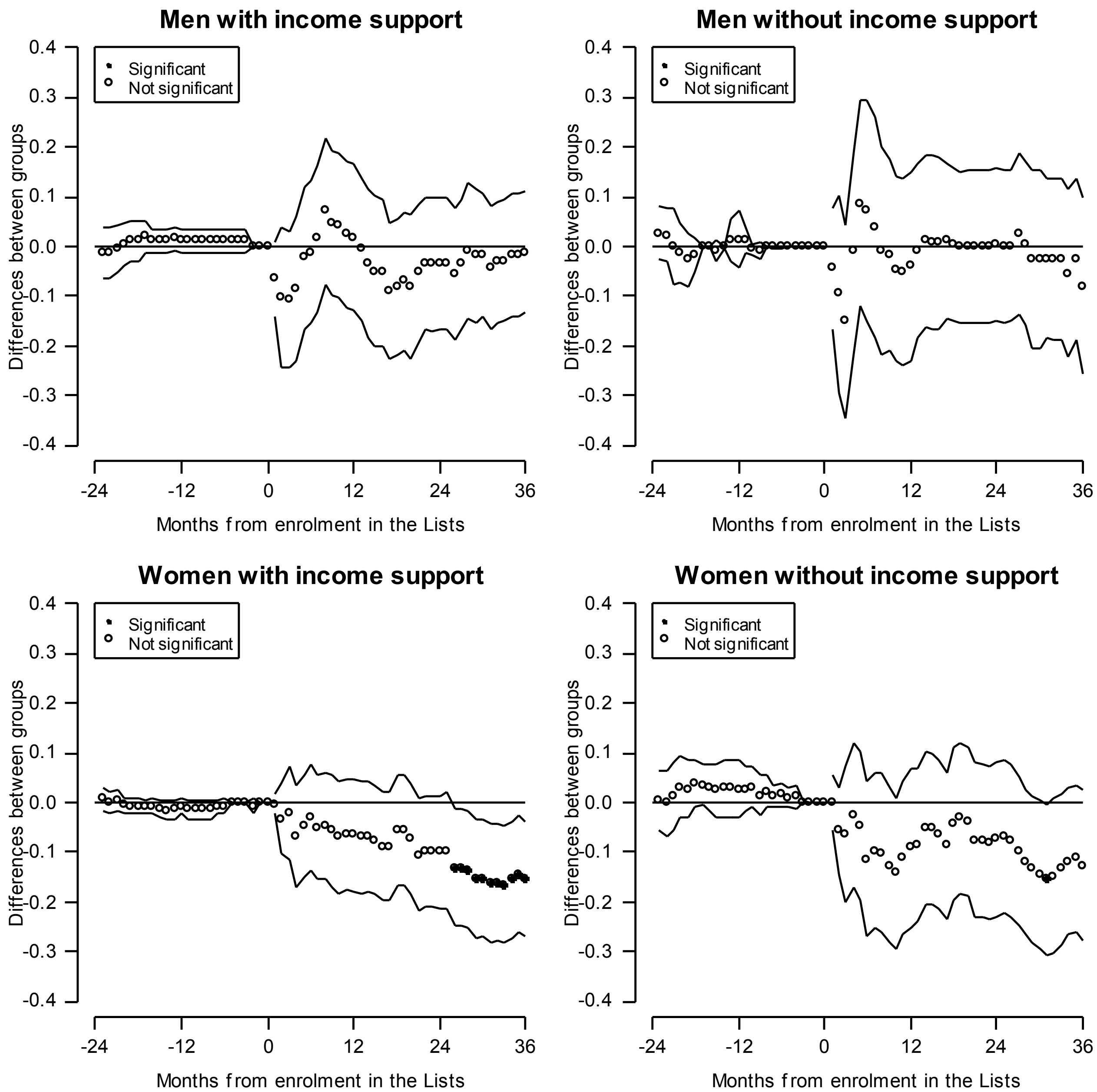


Figure 9: *Estimates of the impact of the additional year of eligibility. Employment rates from 24 months before to 36 months after enrolment in the Lists, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39 (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

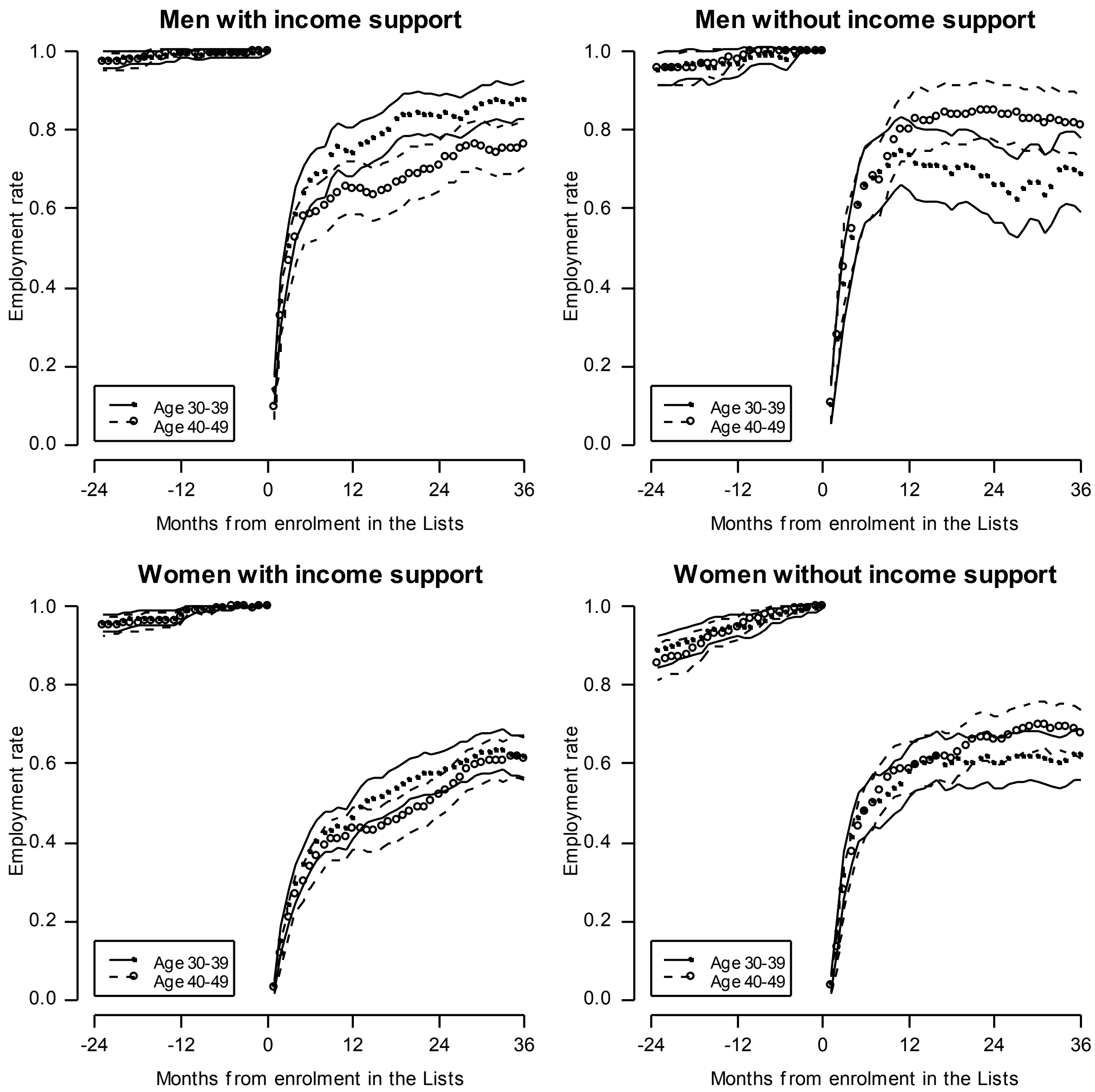


Figure 10: *Estimates of the impact of the additional year of eligibility. Differences between employment rates of treated and not treated workers, by gender and entitlement to income support. Matching on p-score workers with two years of eligibility (40 to 49) to workers withone year of eligibility 30 to 39 (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

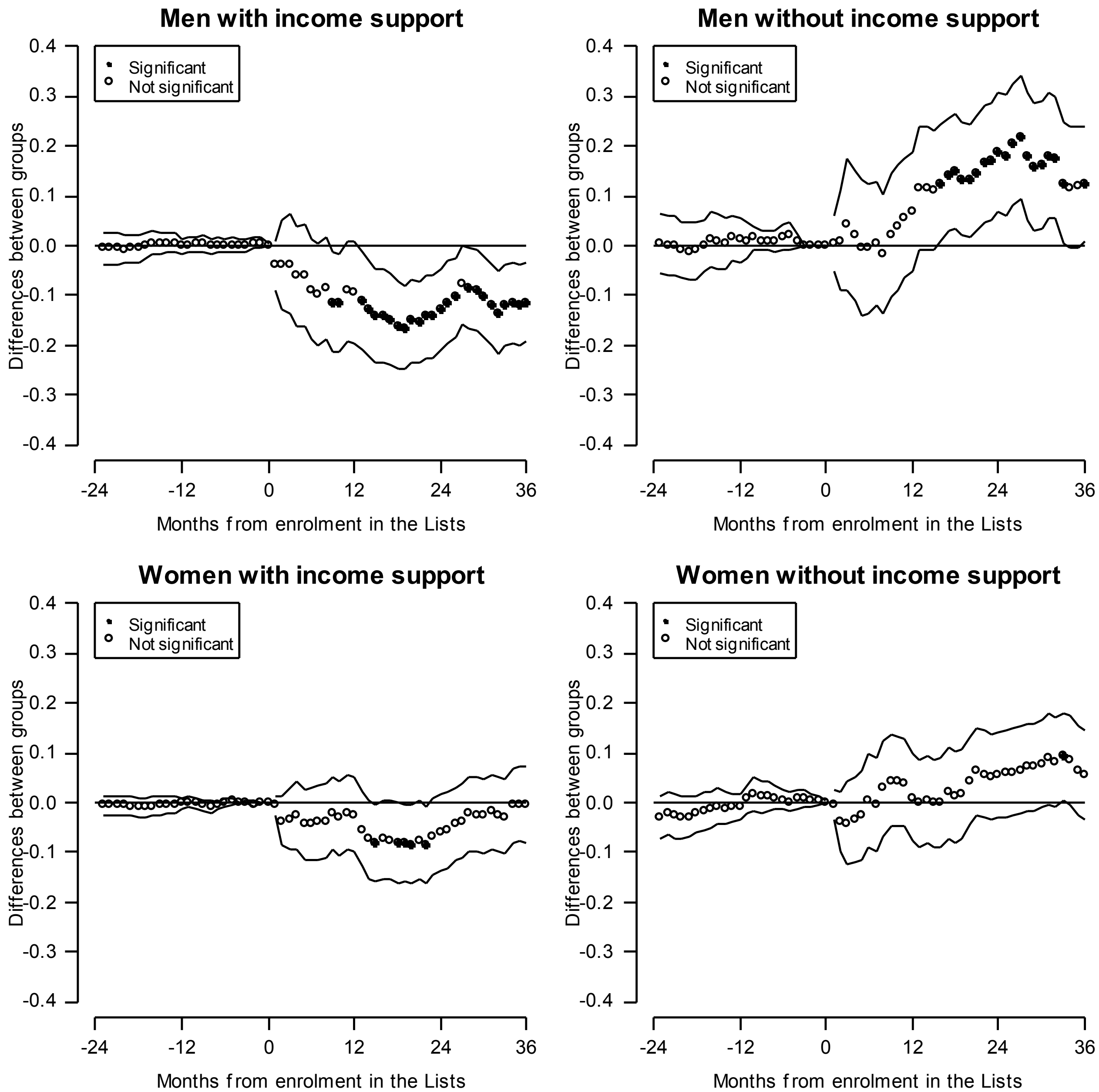


Figure 11: *Estimates of the impact of the additional year of eligibility. Kaplan-Meier estimates of survival functions for transitions to employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39 (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

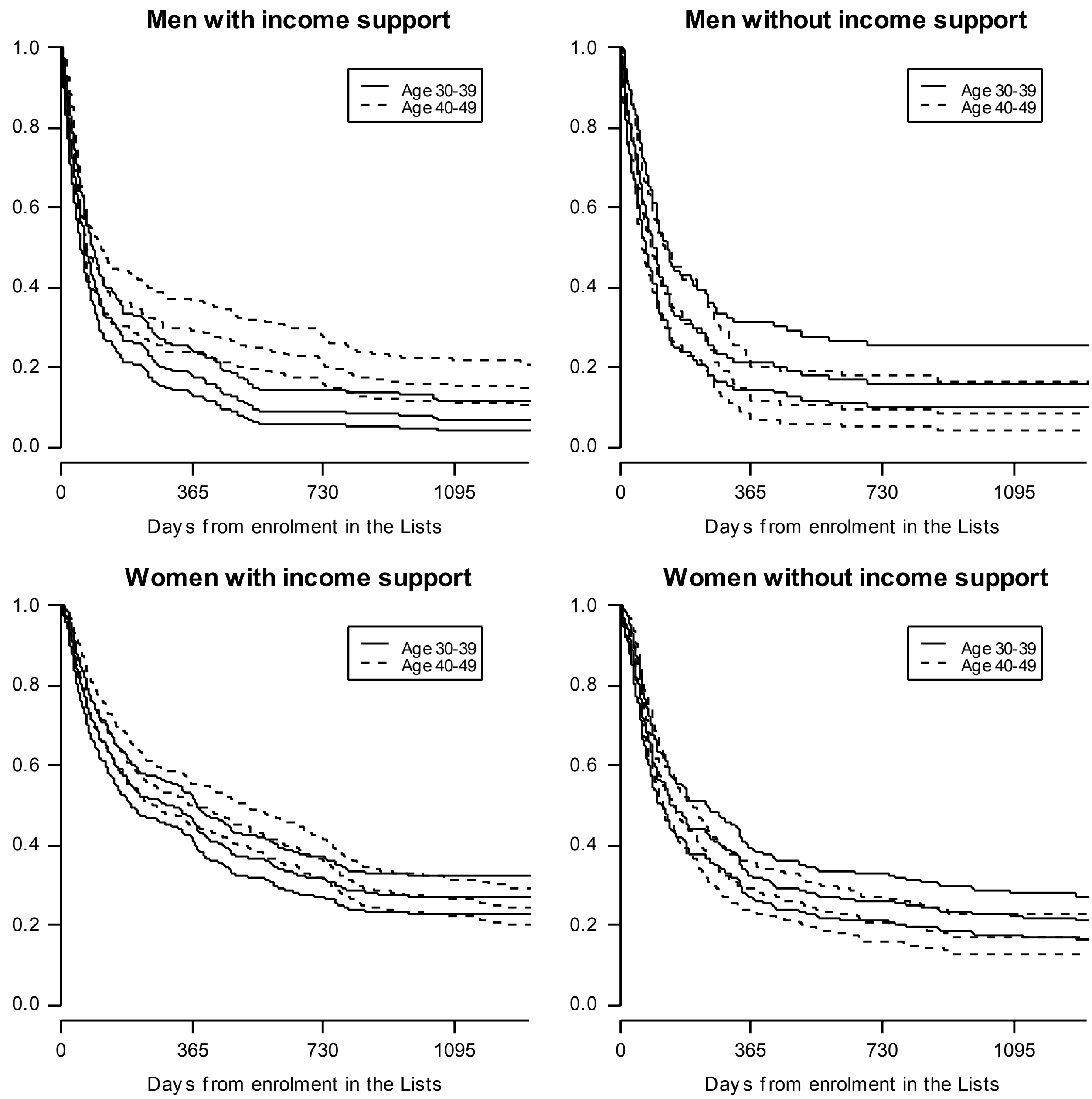


Figure 12: *Estimates of the impact of the additional year of eligibility. Smoothed risk functions for transitions to employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39. Provinces of Treviso and Vicenza, 1997 and 1998.*

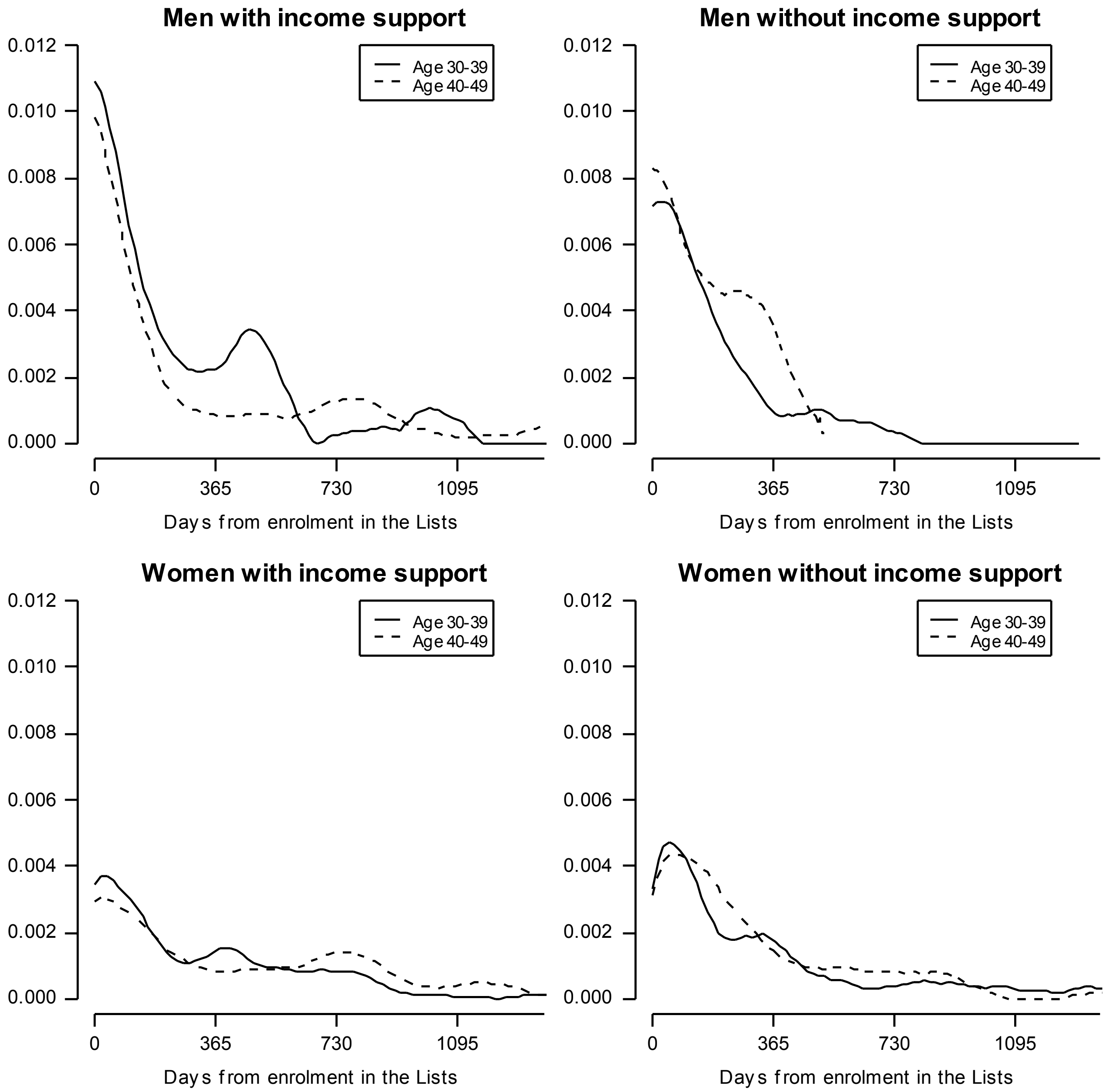


Figure 13: *Estimates of the impact of the additional year of eligibility. Kaplan-Meier estimates of survival functions for transitions to permanent employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39 (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

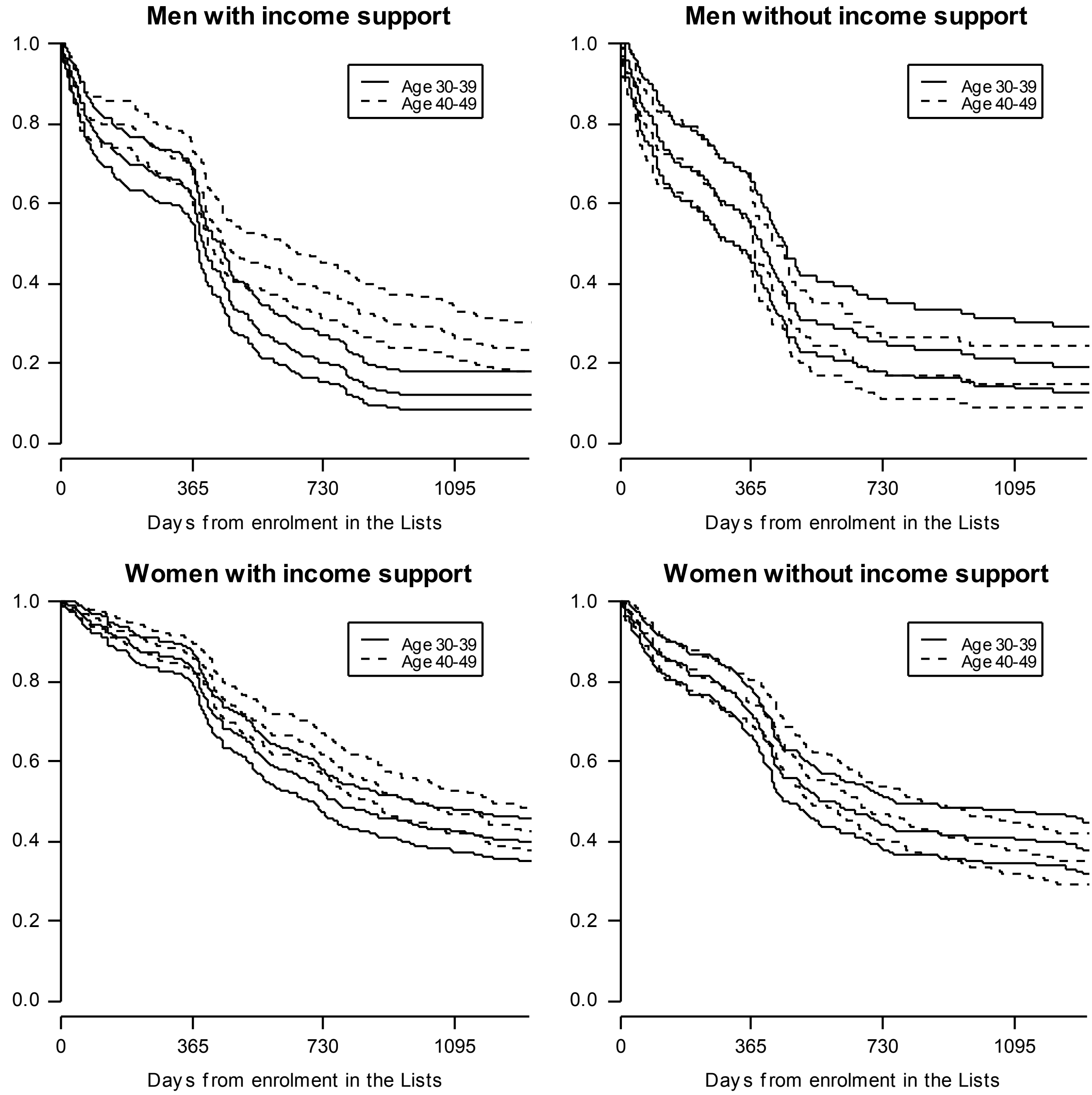


Figure 14: *Estimates of the impact of the additional year of eligibility. Smoothed risk functions for transitions to permanent employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39. Provinces of Treviso and Vicenza, 1997 and 1998.*

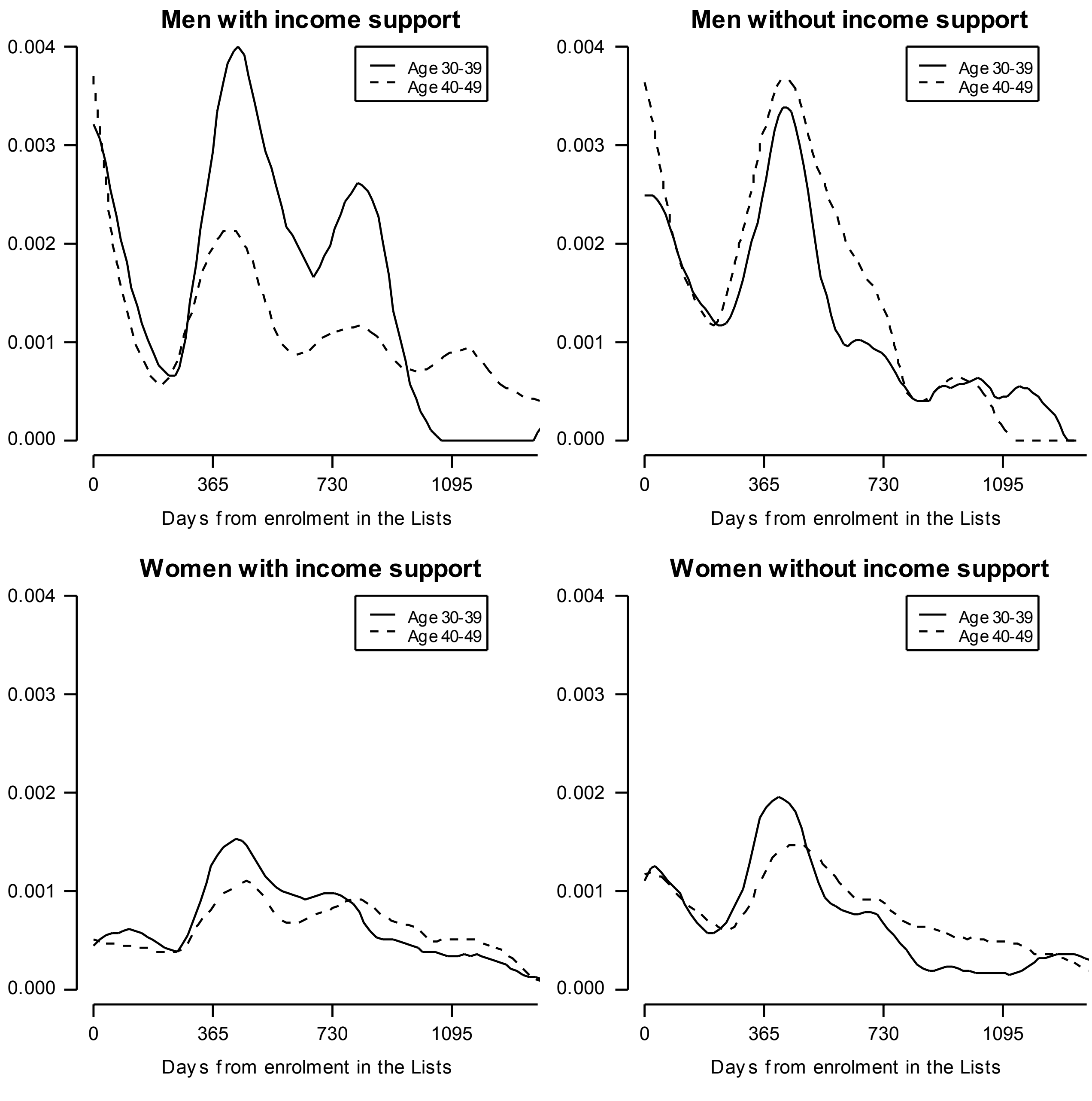


Figure 15: *Estimating the impact of income support. Distribution of p-score for treatments (workers with income support) and controls (workers without income support), by gender and age group. Provinces of Treviso and Vicenza, 1997 and 1998.*

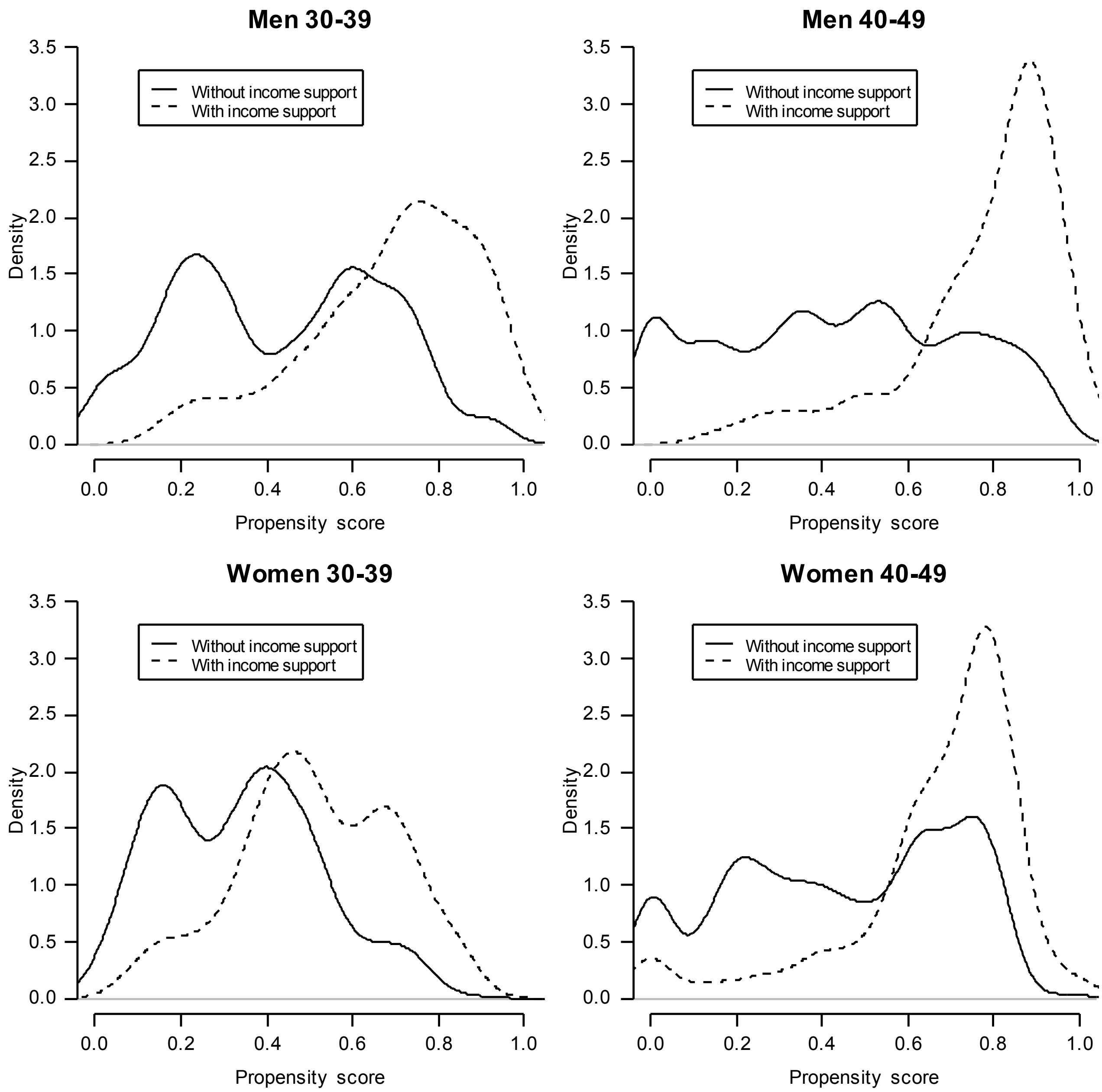


Figure 16: *Estimates of the impact of income support. Employment rates from 24 months before to 36 months after enrolment in the Lists, by gender, age group and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*

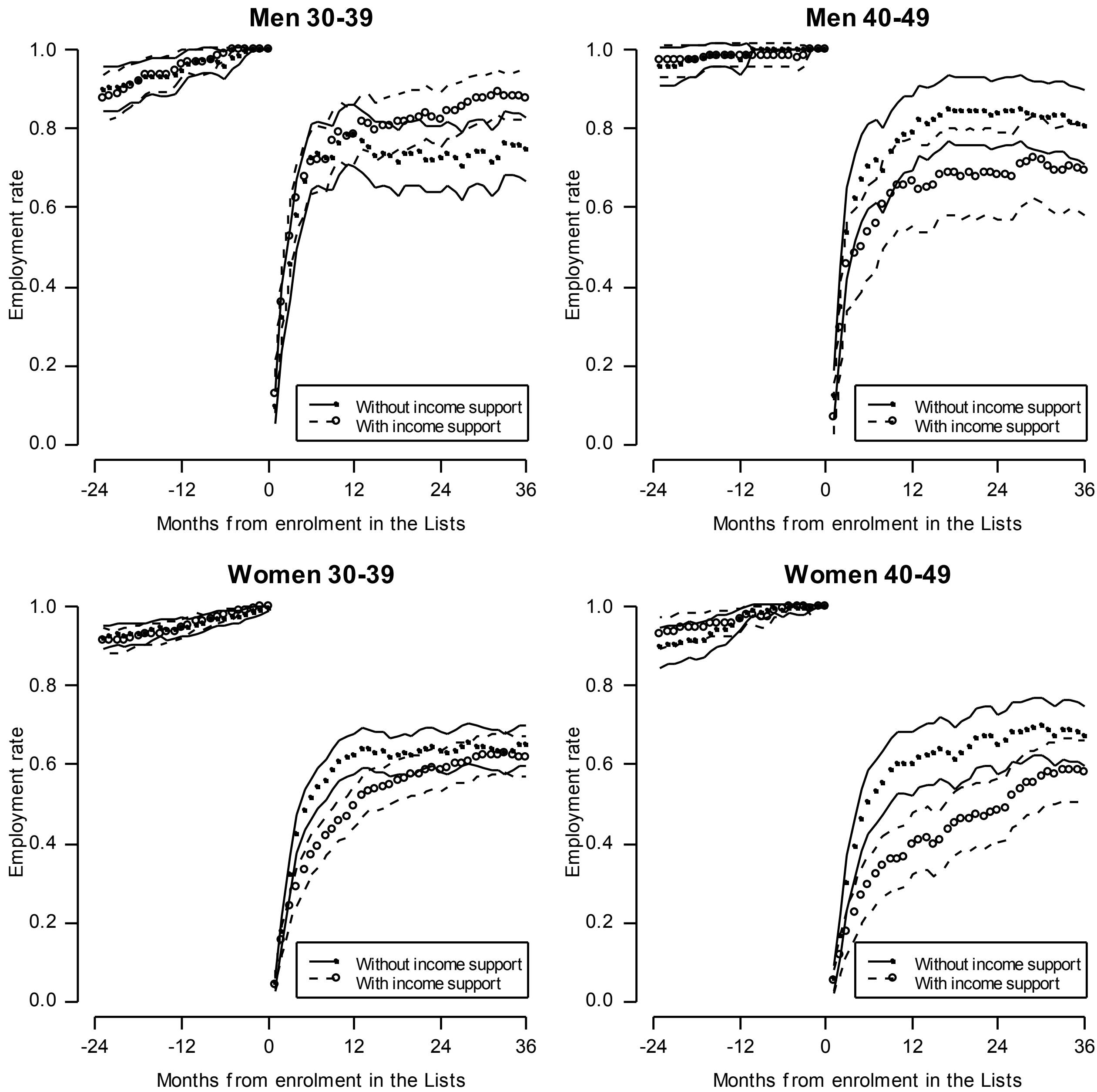
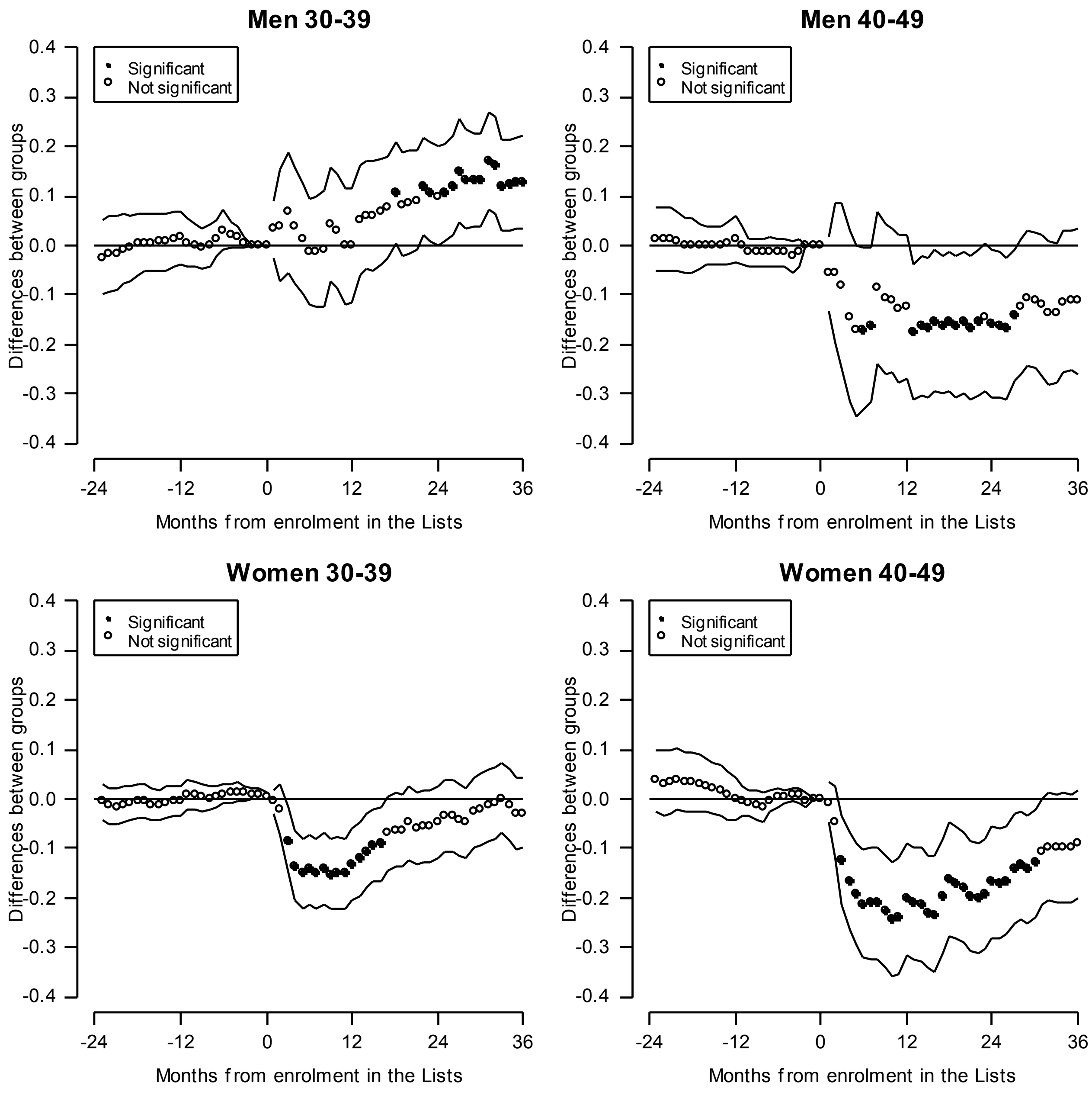


Figure 17: *Estimates of the impact of income support. Differences between employment rates of treatments and controls, by gender and age group, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.*



The impact of the Italian "Mobility Lists" on employment chances: new evidence from linked administrative archives

Summary
The "Mobility Lists" is an Italian labour market policy targeted to dismissed employees, which combines a wage subsidy to an income support. Benefits varies according to the size of the dismissing firm and the worker age at dismissal. We exploit the variability of these provisions to evaluate the differential impact of alternative packages of benefits of the programme. We use linked administrative data from two sources for two Italian provinces, to evaluate the differential impact of the policy on the probability to work over the 36 months subsequent to enrolment in the Lists. The impact of being eligible for an additional year in the Lists is negative for male workers receiving the income support, while it is positive for those eligible only for the active component. The impact is overall negligible among females. The impact of receiving income support is negative, except for young males.

Keywords: Income Support, Wage Subsidy, Regression Discontinuity Design, Matching Estimator, Linked data.

JEL code: J38, J65, J68.

Working Papers già pubblicati

65. A. Paggiaro, E. Rettore, U. Trivellato, The impact of the Italian "Mobility Lists" on employment chances: new evidence from linked administrative archives, Novembre 2005

64. D. Bondonio, R.T. Greenbaum, Do Tax Incentives Affect Local Economic Growth? What Mean Impacts Miss in the Analysis of Enterprise Zone Policies, Novembre 2005

63. D. Bondonio, The employment impact of business incentive programs in declining areas. Mean impacts versus impacts by degrees of economic distress, Novembre 2005

62. U. Trivellato, F. Bassi, A. Discenza, A. Giraldo, Transizioni e mobilità nel mercato del lavoro italiano, 1979-2003, Giugno 2005.

61. A. De Angelini, A. Giraldo, La mobilità dei lavoratori nel Veneto.Confronto fra misure su dati RTFL e su dati NETLABOR, Settembre 2003.

60. F. Bassi, E. Salvan, Dinamiche di ricollocamento per lavoratori che perdono un'occupazione stabile, Settembre 2003.

59. M. Romano, Abbinamento tra gli archivi INPS e Netlabor: il caso degli iscritti alle liste di mobilità, Settembre 2003.

58. P. Baretta, U. Trivellato, La mobilità dei lavoratori da fonti amministrative e da surveys sulle famiglie: un'analisi comparata, Settembre 2003.

57. D. Bondonio, R.T. Greebaum, A comparative evaluation of spatially targeted economic revitalization programs in the European Union and the United States, Settembre 2003.

56. F. Berton, L. Pacelli, G. Segre, Tra lavoro dipendente e lavoro parasubordinato: chi sono, da dove vengono e dove vanno i lavoratori parasubordinati, Settembre 2003.

55. G. Betti, V. Verma, M. Natilli, F. Ballini, Statistical imputation in conjunction with micro-simulation of income data, Settembre 2003.

54. V. Verma, G. Betti, F. Ballini, M. Natilli, S. Galgani, Personal income in the gross and net forms: applications of the Siena micro-simulation model (SM2), Settembre 2003.

53. R. Canu, G. Tattara, Quando le farfalle mettono le ali. Osservazioni sull'ingresso delle donne nel lavoro dipendente, Settembre 2003.

52. D. Favaro, S. Magrini, Gender wage differentials among young workers: methodological aspects and empirical results, Settembre 2003.

51. F. Devicienti, Downward nominal wage rigidity in Italy: evidence and consequences, Novembre 2002.

50. V. Verma, G. Betti, Longitudinal measures of income poverty and life-style deprivation, Novembre 2002.

49. A. Borgarello, F. Devicienti, Trends in the Italian earnings distribution, 1985-1996, Novembre 2002.

48. S. Bragato, F. Occari, M. Valentini, I problemi di contabilizzazione dei lavoratori extracomunitari. Una verifica nelle province di Treviso e Vicenza, Novembre 2002.

47. B. Anastasia, D. Maurizio, Misure dell'occupazione temporanea: consistenza, dinamica e caratteristiche di uno stock eterogeneo, Novembre 2002.

46. E. Rettore, U. Trivellato, A. Martini, La valutazione delle politiche del lavoro in presenza di selezione: migliorare la teoria, i metodi o i dati?, Novembre 2002.

45. U. Trivellato, A. Giraldo, Assessing the ‘choosiness’ of the job seekers. An exploratory approach and evidence for Italy, Novembre 2002.

44. E. Battistin, E. Rettore, Another look at the regression discontinuity design, Novembre 2002.

43. B. Contini, F. Cornaglia, C. Malpede, E. Rettore, Measuring the impact of the Italian CFL programme on the job opportunities for the youths, Novembre 2002.

42. F. Devicienti, Estimating poverty persistence in Britain, Novembre 2001.

41. A. Giraldo, E. Rettore, U. Trivellato, Attrition bias in the bank of Italy’s survey on household income and wealth, Novembre 2001.

40. A. Giraldo, E. Rettore, U. Trivellato, The persistence of poverty: true state dependence or unobserved heterogeneity? Some evidence form the Italian survey on household income and wealth, Novembre 2001.

39. G. Barbieri, P. Gennari, P. Sestito, Do public employment services help people in finding a job? An evaluation of the italian case, Novembre 2001.

38. N. Torelli, A. Paggiaro, Estimating transition models with misclassification, Novembre 2001.

37. B. Anastasia, M. Gambuzza, M. Rasera, Le sorti dei flussi: dimensioni della domanda di lavoro, modalità di ingresso e rischio disoccupazione dei lavoratori extracomunitari in Veneto, Novembre 2001.

36. L. Pacelli, Fixed term contracts, social security rebates and labour demand in Italy, Novembre 2001.

35. A. Borgarello, F. Deficienti, C. Villosio, Mobilità retributiva in Italia 1985-1996, Novembre 2001.

34. B. Contini, C. Villosio, Job changes and wage dynamics, Novembre 2001.

33. B. Contini, Earnings mobility and labor market segmentation in Europe and USA: preliminary explorations, Novembre 2001.

32. A. Borgarello, F. Devicienti, Trend nella distribuzione dei salari italiani 1985-1996, Novembre 2001.

31. F. Bassi, U. Trivellato, Gross flows from the French labour force survey: a reanalysis, Novembre 2001.

30. F. Bassi, M. Gambuzza, M. Rasera, Imprese e contratti di assunzione: prime analisi da Netlabor, Novembre 2001.

29. N. Rosati, Further results on inequality in Italy in the 1980s and the 1990s, Aprile 2001.

28. A. Ichino, M. Polo, E. Rettore, Are judges biased by labor market conditions?, Novembre 2000.

27. E. Battistin, E. Rettore, Testing for the presence of a programme effect in a regression discontinuity design with non compliance, Novembre 2000.

26. E. Battistin, E. Rettore, U. Trivellato, Measuring participation at work in the presence of fallible indicators of labour force state, Giugno 2000.

25. A. D’Agostino, G. Ghellini, L. Neri, Percorsi di ingresso dei giovani nel mercato del lavoro, Giugno 2000.

24. F. Bassi, M. Gambuzza, M. Rasera, E. Rettore, L’ingresso dei giovani nel mercato del lavoro: prime esplorazioni dall’archivio Netlabor, Giugno 2000.

23. A. Paggiaro, U. Trivellato, Assessing the effects of the “Mobility List” programme in an Italian region: do (slightly) better data and more flexible models matter?, Marzo 2000.

22. A. D'Agostino, G. Ghellini, L. Neri, Modelli statistici per l'analisi dei comportamenti di transizione scuola lavoro, Marzo 2000.

21. G. Betti, B. Cheli, A. Lemmi, Analisi delle dinamiche di povertà e disoccupazione su uno pseudo panel italiano, Marzo 2000.

20. N. Rosati, Permanent and Temporary Inequality in Italy in the 1980s and 1990s, Marzo 2000.

19. F. Bassi, M. Gambizza, M. Rasera, Struttura e qualità delle informazioni del sistema NETLABOR. Una verifica sui dati delle Scica delle province di Belluno e Treviso, Marzo 2000.

18. B. Anastasia, M. Gambuzza, M. Rasera, La durata dei rapporti di lavoro: evidenze da alcuni mercati locali del lavoro veneti, Marzo 2000.

17. G. Betti, B. Cheli, A. Lemmi, Occupazione e condizioni di vita su uno pseudo panel italiano: primi risultati, avanzamenti e proposte metodologiche, Ottobre 1999.

16. A. D'Agostino, G. Ghellini, L. Neri, A Multiple Imputation Method for School to Work Panel Data, Ottobre 1999.

15. A. Paggiaro, N. Torelli, Una procedura per l'abbinamento di record nella rilevazione trimestrale delle forze di lavoro, Ottobre 1999.

14. S. Campostrini, A. Giraldo, N. Parise, U. Trivellato, La misura della partecipazione al lavoro in Italia: presupposti e problemi metodologici di un approccio "time use", Ottobre 1999.

13. T. Di Fonzo, P. Gennari, Le serie storiche delle forze di lavoro per il periodo 1984.1-92.3: prospettive e problemi di ricostruzione, Giugno 1999.

12. A. Paggiaro, Un modello di mistura per l'analisi della disoccupazione di lunga durata, Maggio 1999.

11. M. Lalla, F. Pattarin, Alcuni modelli per l'analisi delle durate complete e incomplete della disoccupazione: il caso Emilia Romagna, Maggio 1999.

10. F. Bassi, M. Gambuzza, M. Rasera, Il sistema informatizzato NETLABOR. Caratteristiche di una nuova fonte sul mercato del lavoro, Maggio 1999.

9. B. Contini, M. Filippi, L. Pacelli, C. Villosio, Working careers of skilled vs. unskilled workers, Gennaio 1999

8. B. Contini, L. Pacelli, C. Villosio, Short employment spell in Italy, Germany and Great Britain: testing the "Port-of-entry" hypothesis, Gennaio 1999

7. B. Contini, Wage structures in Europe and in the USA: are they rigid, are they flexible?, Gennaio 1999.

6. M. Forni, S. Paba, Industrial districts, social environment and local growth. Evidence from Italy, Novembre 1998.

5. Ginzburg, M. Scaltriti, G. Solinas, R. Zoboli, Un nuovo autunno caldo nel Mezzogiorno? Note in margine al dibattito sui differenziali salariali territoriali, Ottobre 1998.

4. F. Bassi, Un modello per la stima di flussi nel mercato del lavoro affetti da errori di classificazione in rilevazioni retrospettive, Ottobre 1998.

3. U. Trivellato, Il monitoraggio della povertà e della sua dinamica: questioni di misura e evidenze empiriche, Settembre 1998.

2. N. Rosati, E. Rettore, G. Masarotto, A lower bound on asymptotic variance of repeated cross-sections estimators in fixed-effects models, Agosto 1998.

1. E. Battistin, A. Gavosto, E. Rettore, Why do subsidized firms survive longer? An evaluation of a program promoting youth entrepreneurship in Italy, Agosto 1998.