

**THE IMPACT OF THE ITALIAN “MOBILITY LISTS” ON EMPLOYMENT CHANCES:
NEW EVIDENCE FROM LINKED ADMINISTRATIVE ARCHIVES**

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Abstract: The “Mobility Lists” is an Italian labour market policy targeted to dismissed employees combining a wage subsidy to an income support. Benefits varies according to the size of the dismissing firm and with the worker age at dismissal. We exploit the variability of these provisions to evaluate the impact of the programme. We use linked administrative data from two sources for two Italian provinces to evaluate the 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 the income support is negative except among young males.

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JEL code: J38, J65, J68

Very preliminary, for comments only. Please, do not quote.

1. Introduction

The ‘Mobility Lists’ (ML) is an Italian labour market policy introduced in the early ‘90s combining 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 than 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 at the time of dismissal, while it lasts two years for those old 40 to 49 at the time of dismissal. There is also a third category of workers maintaining their eligibility status over three years made up of workers older than 49. We will not consider them in the analysis because the policy 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 field (among the others, see Borzaga and Brunello, 1997, Brunello and Miniaci, 1997, Paggiaro and Trivellato, 2002) have exploited administrative data resulting from the management of the policy which provide poor information on employment spells during enrolment in the Lists precluding a detailed analysis of the effects of enrolment.. On the other hand, such studies have relied 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 particular 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:

- (i) 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 important to control for selection bias when evaluating the impact of the programme.
- (ii) To identify the differential impact of the various packages of benefit 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 finally presents some concluding remarks.

2. Mobility Lists: provisions and questions about their impact

2.1. The policy

The policy, regulated mainly by laws 233/91 and 236/93, 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 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, 40 to 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 losing 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 his/her eligibility period.

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”). There are some other minor exceptions which are not relevant to our case study.

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 period of eligibility 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 one 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 other 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 policy 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 priori* uncertain whether enrolled workers 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 benefit. In particular, in this paper we focus on the identification of the impact of

- (a) eligibility for the two years of benefits as opposed to just one year
- (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 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 from.

As the age group over 50 years very often take the “long mobility” option and flow directly to retirement, in the following we focus on workers younger than 49.

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 obviously conditioned by the availability of good information. As a result of the management of the policy regional administrative archives are available registering enrolments in the Lists, exits to employment and exits due to expiration of the eligibility period. This is the information on which all previous studies on the ML impact relied on.

Unfortunately, data from these archives lack crucial information. Firstly, only poor information on employment spells during the eligibility period are available; in particular, the archive lacks information on temporary jobs, which are crucial to an adequate evaluation of the policy. Secondly, as 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 them to the Netlabor archive 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, linking the two archives turns out useful to checking the coherence, and indirectly the quality, of data coming from the two sources and to enriching information on socio-demographic characteristics of workers in the Lists

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

3. The design of the impact evaluation

Since there is no sensible comparison group made up of unemployed workers ineligible for the benefits issued by the policy the only room left for evaluation is to look at the differential impact of alternative packages of benefits. Specifically, we evaluate i) the impact of being eligible for two years of benefits rather than just one and ii) the impact of being eligible to receive both the passive and the active components of the policy rather than just the active one.

The econometric problem we need to solve is that 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 firm they have been dismissed from the possible impact of the benefits might be obscured by the differential composition of the groups receiving alternative packages of benefits. We shall focus on unemployed workers younger than 50 since most of them are allowed to use the period of eligibility as a route to retirement.

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 old 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 the 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 subject would experience being exposed to, and denied, the treatment, respectively. The mean impact of the treatment on the treatment group is

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

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

$$E[Y^T | I = 1] - E[Y^{NT} | I = 0] = E[\alpha | I = 1] + (E[Y^{NT} | I = 1] - E[Y^{NT} | I = 0]). \quad (2)$$

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 specific case, by design of the policy treatments are older than controls implying that the observed difference between the two groups in the probability to be 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 so called Regression Discontinuity Design (see Hahn, Todd and Van der Klaauw, 2001). The design 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:

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

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 this design is that if the program 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 of the design we consider matching estimators. That is we compare treatments to controls conditioning 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

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

Considering that in our problem 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 directly observable on the controls, namely all those younger than 40. Then, to test the hypothesis

$$H_0 : Y^{NT} \perp et\grave{a} \mid X. \quad (4)$$

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

By the same token, we could compare old treatments to young ones after balancing the two subgroups 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, on rejecting the null hypothesis

$$H_0 : Y^T \perp et\grave{a} \mid X$$

one cannot say whether it is Y^{NT} to depend on age, or the impact $(Y^T - Y^{NT})$, or both. On the other hand, on accepting the null 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, as usual in this literature to ease calculations we match treatments to controls on the so called propensity score

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

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

Both logical and practical problems preclude using the Regression Discontinuity Design to evaluate the impact of the income support. As for the practical problem, we cannot observe the firm size in our data set. But even if we could, the contrast treatments/controls at the firm size threshold is unlikely to identify the mean impact of the income support since there are other discontinuities in the Italian labour market institutions taking place exactly at the same threshold (the main one is in the legislation providing protection to employees against unjust dismissals). On finding a discontinuity in the probability to be at work at the firm size threshold one could not say whether it is caused by the income support or it is due to others institutional discontinuities.

The route we take in this paper is again based on matching. We match one control to each treatment on basic socio-economic characteristics (education, age, gender,) as well as recent labour

market history (labour force status in the 24 months previous to enrolment in the Lists, characteristic of the last job).

As compared to the case discussed in the previous section, here we cannot exploit the knowledge of the selection process to specify 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 one

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

In words, controlling for 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} -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. On observing significant differences we would 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 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. Socio-demographic characteristics (gender, age, education, province), industry of the dismissing firm and qualification of the worker, date of enrolment in the Lists, entitlement to income support are recorded on 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 more 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 provide more reliable information on socio-demographics and on each employment spell experienced by the worker whether prior to, or during, or after the eligibility period. In particular, (i) the nature of the employment contract (permanent vs temporary, part-time vs full-time, apprenticeship); (ii) the

worker qualification; (iii) industry of the hiring firm; (iv) beginning and end of each employment spell (more details in Bassi, Gambuzza and Rasera, 2001, and Trivellato, 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. Secondly, the occurrence of spell 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 during the eligibility period, 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* ending up with 4,230 sample units. Main descriptive evidence follows. 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.

Age group	Men with income support		Women with income support		Men without income support		Women without income support		Total	
	N	%	N	%	N	%	N	%	N	%
<30	169	23.4	425	32.9	150	30.7	775	44.8	1519	35.9
30-39	276	38.3	497	38.5	197	40.4	712	41.2	1682	39.8
<40	445	61.7	922	71.4	347	71.1	1487	86.0	3201	75.7
40-49	276	38.3	369	28.6	141	28.9	243	14.0	1029	24.3
Total	721	17.1	1291	30.5	488	11.5	1730	40.9	4230	100.0

In the following, we use as a summary of the worker labour market history the month by month proportion of days spent at work no matter for the nature of the contract between the employee and his/her employer. 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.

The crude contrast of workers eligible for two years of benefits (workers 40 to 49 years old) to workers eligible for one years of benefits (workers less than 40 years old) is in Fig. 1. Their employment rates 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 are the following:

- Employment rates after enrolment sharply differ in the two age groups, in particular among workers eligible for the income support. Whether it is a genuine causal effect of the policy or rather due to selection bias is the issue we deal with here below.
- 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 the older group is no longer on the income support.

* 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.

- As for workers without income support, there are no relevant differences across age groups among women, while among men the old 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 employment rates 12 and 36 months after enrolment by age at enrolment. In the logic of the regression discontinuity design, 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 employments rate 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 on eligible workers. The comparison group is obtained by matching to each worker experiencing the treatment a worker eligible for one year in the ML. We use one-to-one matching on the propensity score allowing for at most a .01 difference in the propensity scores. The propensity scores are estimated by logistic regressions, using as covariates a province dummy, professional qualification, education, industry of the dismissing firm, month by month labour force status over the two years prior to enrolment. The analysis is carried out separately by gender and entitlement to income support. Table 2 shows the performance of matching. On the whole results are satisfactory, as 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.

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

	<i>Men with income support</i>		<i>Women with income support</i>		<i>Men without income support</i>		<i>Women without income support</i>		Total	
	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>
Treatments (age 40-49)	276		369		141		243		1029	
Controls (age <40)	445		922		347		1487		3201	
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

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 in 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. The performance of matching using only workers 30 to 39 years old as the comparison group are in Table 3. 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

	<i>Men with income support</i>		<i>Women with income support</i>		<i>Men without income support</i>		<i>Women without income support</i>		Total	
	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>
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 the age effect among not exposed workers is much weaker. Among exposed workers evidence is less clear, and among women employment rates 36 months after enrolment seem to be lower for older workers.

Figures 7 and 8 confirm this evidence, as there is non sign of an age effect among workers younger than 40[†]. On the contrary, among women undergoing the treatment there is some evidence of an age effect in the third year after enrolment. As we clarify 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 a little cautious.

Figures 6, 7 and 8 about here

[†] 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 matching set. In practice due to the small sample size we had problems in the specification of propensity scores.

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 their enrolment. 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 are much smaller and very 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. Nevertheless, keep in mind that this results are under the threat of a potential selection bias, in particular for women with income support. Figure 8 shows employment rates for women 40 to 49 decrease with age. If this pattern were driven by the impact of age on 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 i) bears a positive impact on the employment rates of workers dismissed by firms below the 15-employee 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 components 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 to conclude that the impact of the passive component is negative, as the composition of the pools of workers from firms below and above, respectively, the 15-employee threshold could sharply differ. This problem is discussed in Section 4.3.

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 of time 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 time waiting for the first job, no matter for the nature of the associated 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 results presented in Figure 10. For the other groups survival functions of treatments and controls are statistically indistinguishable (this result is confirmed by a 5% log-rank tests on the whole distributions), but even in these cases the sign of the effects are consistent with those in Figure 10.

Figure 11 about here

A complementary evidence comes from Figure 12, showing estimates of the hazard functions (smoothed by kernel methods) associated to the survival functions in 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 exhibit a large peak by 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 time spent on temporary employment contribute the same way to the definition of the spell (note that these are typical durations analysed in previous studies of the ML impact; see among others Paggiaro and Trivellato, 2002).

The impact of the additional year of eligibility on the survival probability is positive for men with income support, specially starting from the second year, while it is statistically not significant for the other three groups. The hazard functions represented 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 experience a spell of temporary employment during the first year converted into a permanent position after one year.

Figures 13 e 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 those we use in the previous analysis and a polynomial on age, in order to take

into account the different age composition of the pool of workers dismissed by firms with more and less than 15 employees, respectively.

Preliminarily, we conduct the specification test discussed at the end of Section 3.2, in order 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 turn out to 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 we have available does not seem enough rich to compensate for the different composition of the pool of workers dismissed by firms below and above, respectively, the 15-employee threshold.

Nonetheless, we go on with the evaluation exercise including in the set of matching variables the labour force status in months -24, -23 and -22 in the hope that this way the bias turns 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 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. Which confirms from another point of view that workers dismissed from small firms are systematically different from those dismissed from firms above the 15-employee threshold.

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

		<i>Men</i> 30-39		<i>Women</i> 30-39		<i>Men</i> 40-49		<i>Women</i> 40-49		Total	
		<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>
Treatments	(with income support)	276		497		276		369		1418	
Controls	(without income support)	197		712		141		243		1293	
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 the entitlement period.

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

In sharp contrast, men 30 to 39 feature a positive and increasing over time impact of entitlement to income support. Note that this feature is mainly due to the fact that the employment rates for workers without income after one year level out at .7 while the corresponding rates for workers with income support steadily increase over the 36 months. A possible explanation for the pattern observed on workers without income support is that those who do not find a new job in the months immediately after dismissal turn to other kinds of employment not covered by the Netlabor archive 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 *a priori* uncertain the sign of its expected impact. The active component amounts to a generous wage subsidy to any firm recruiting workers from the Lists, while the passive component is made up of a generous income support to the workers enrolled in the Lists. Duration of the eligibility period depends only on the worker 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 which we show provides a much richer description of the labour market history than that used in all the previous studies on the impact of this policy. Moreover, as compared to all the previous study which made use of parametric or semi-parametric models, we use a fully non-parametric methodology to identify the mean impact coupled to a set of specification tests designed to check the identifying restrictions on which the identification strategy relies on. This results in a clear identification of the sub-populations of workers to which the identified 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: (a) the impact of a longer eligibility for the benefits, e.g. two years instead of one; (b) 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 from firms with up to 15 employees – exhibits a composition apparently different from that of the treatment group – workers dismissed from 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 subgroup 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 this, 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 which feature a positive impact increasing over the 36 months of our observation window, an evidence which calls for further investigation.

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Figure 1: Employment rates 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).

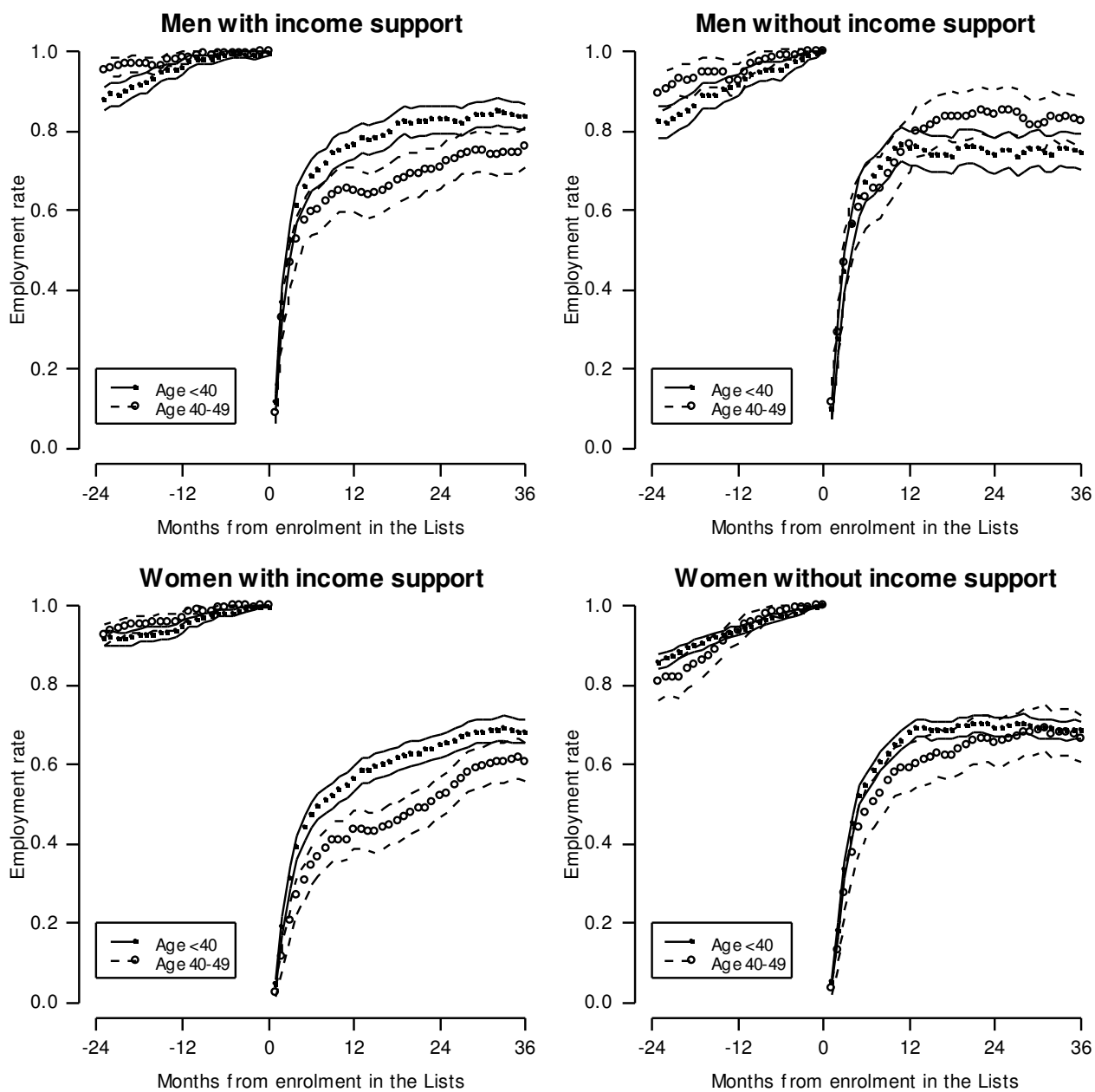


Figure 2: Employment rates 12 and 36 months after enrolment in the Lists by age, controlling for gender and entitlement to income support (point estimates and polynomial splines). Provinces of Treviso and Vicenza, 1997 and 1998.

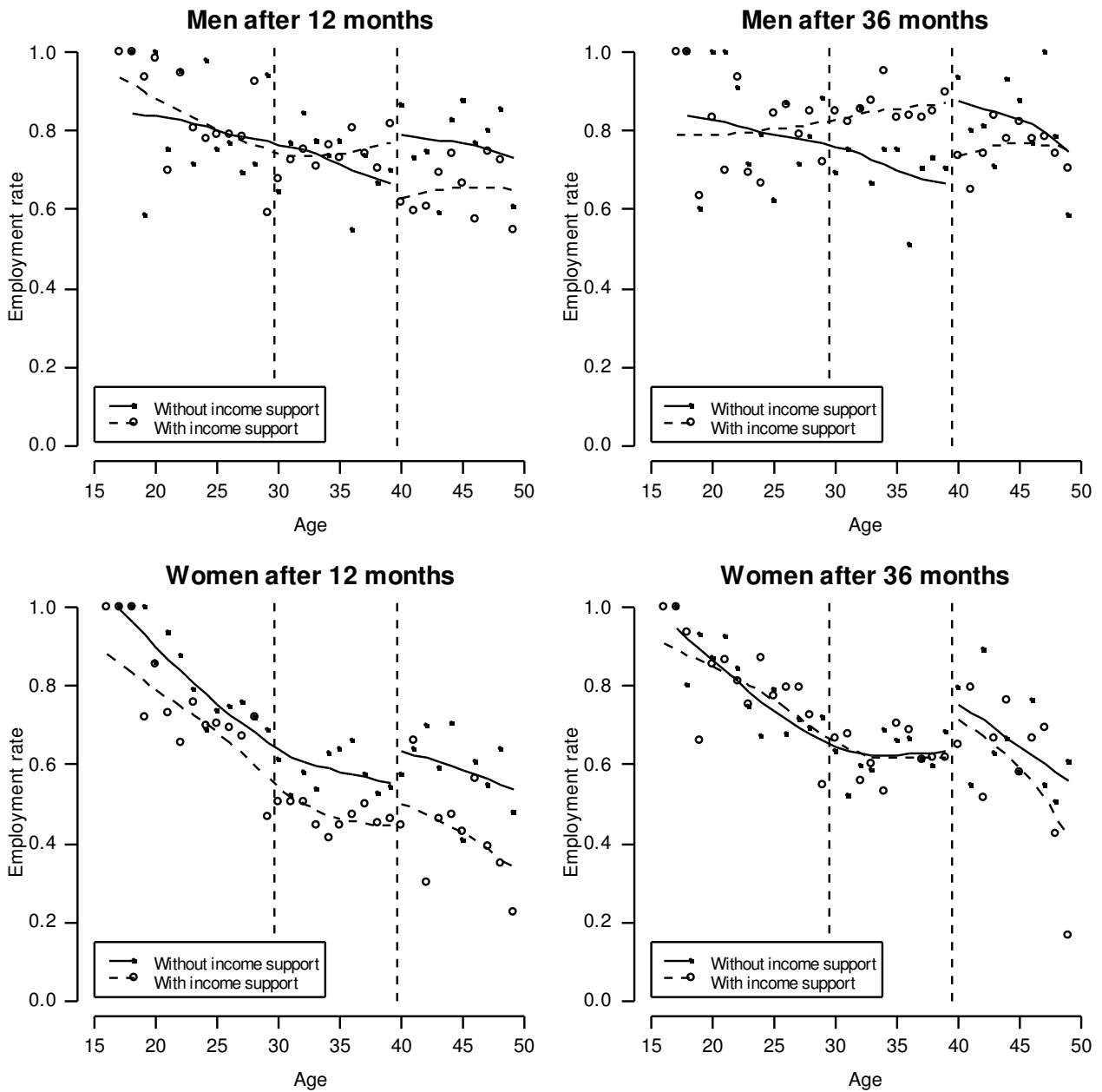


Figure 3: Employment rates 12 and 36 months after enrolment in the Lists by age, controlling for gender and entitlement to income support (point estimates and polynomial splines). Matching on p-score workers with two years of eligibility (40-49 years) to workers with one year of eligibility (<40 years).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

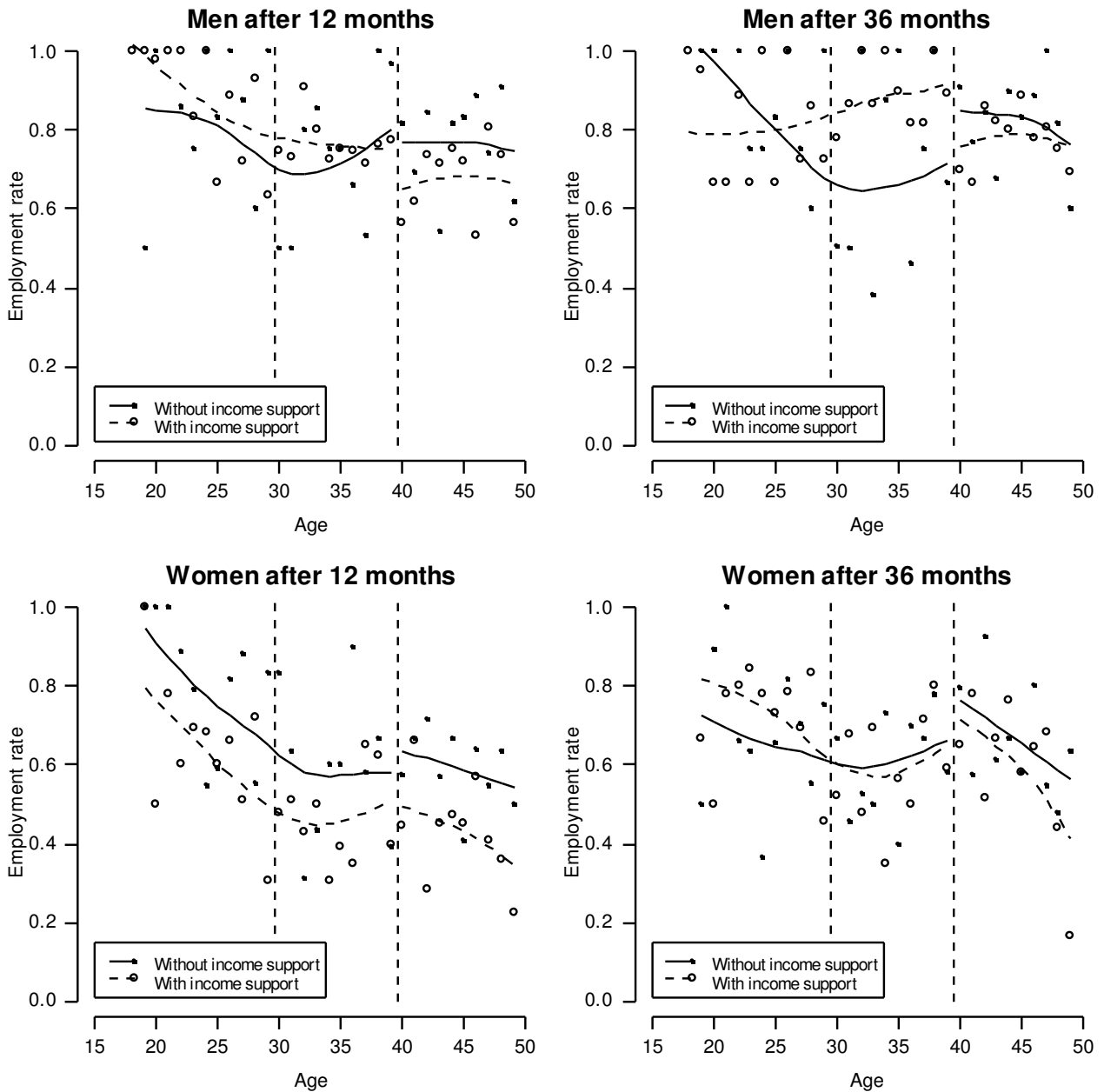


Figure 4: Testing for selection bias. Differences between employment rates of workers 30 to 39 and of matched workers under 30, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998

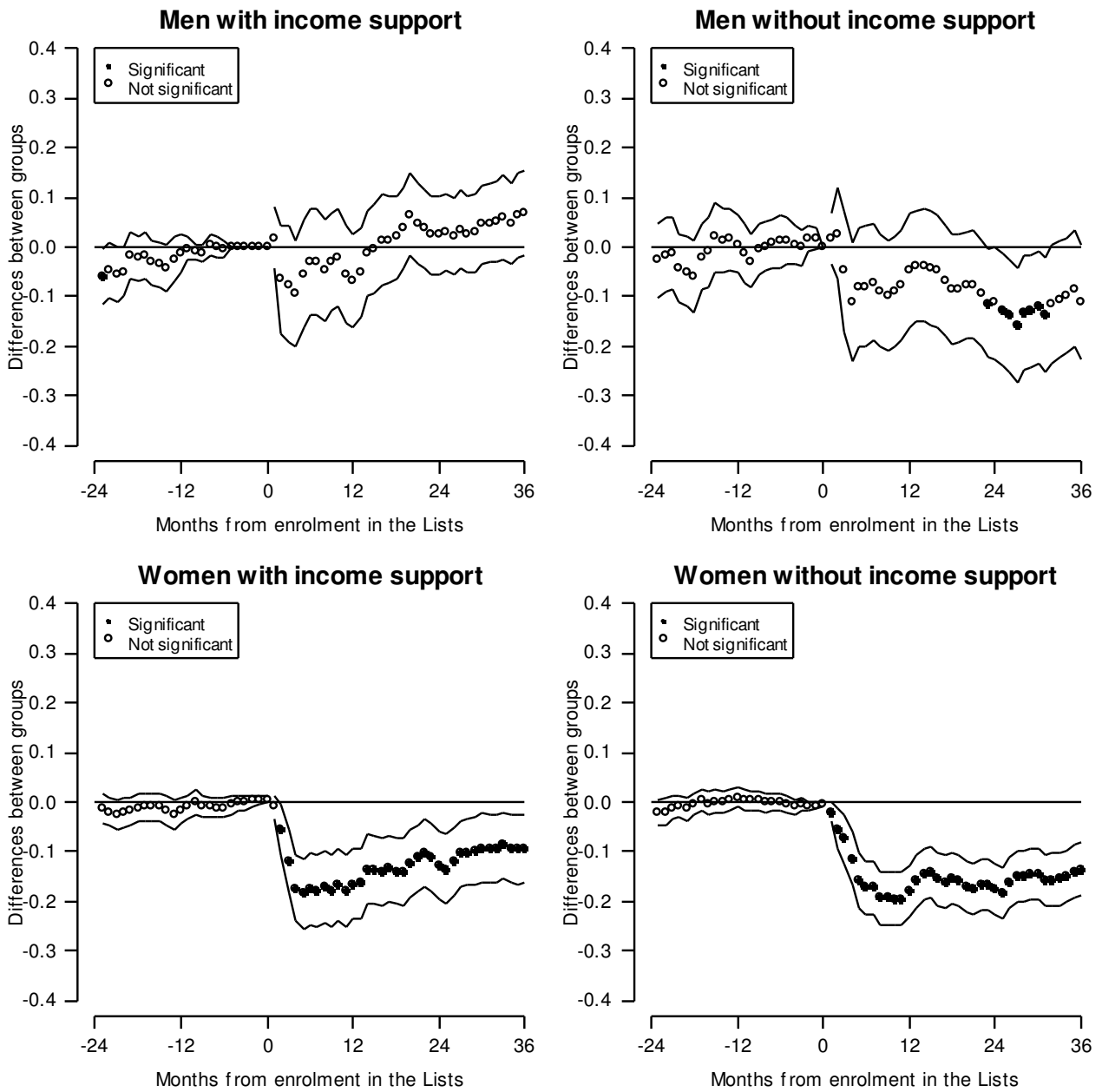


Figure 5: Estimating the impact of the additional year of eligibility. Distribution of p-score for treatments (workers 40 to 49) and controls (workers 30 to 39), by gender and entitlement to income support. Provinces of Treviso and Vicenza, 1997 and 1998.

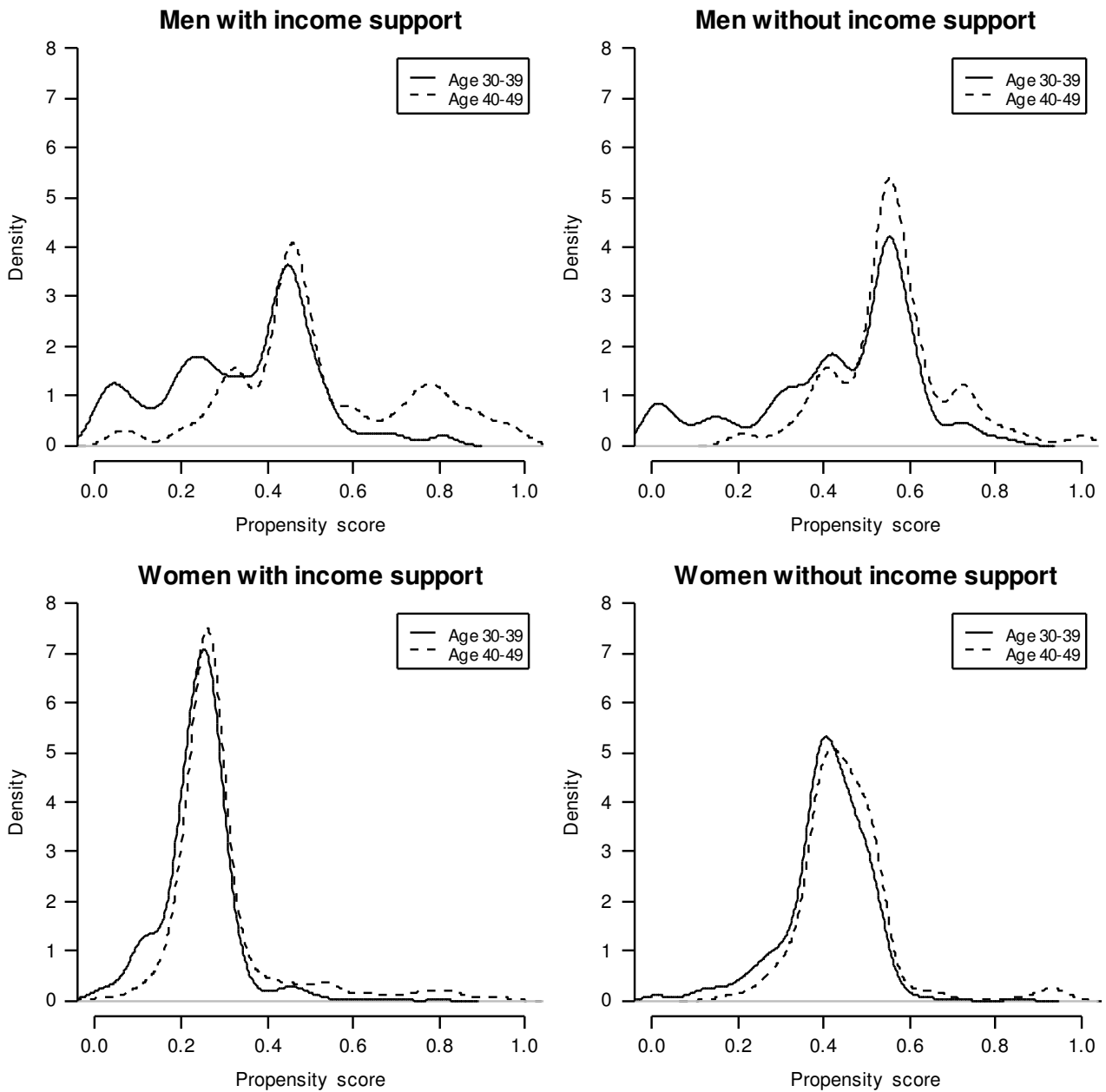


Figure 6: Employment rates 12 and 36 months after enrolment in the Lists, by gender, age and entitlement to income support. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility (30 to 39). Provinces of Treviso and Vicenza, 1997 and 1998.

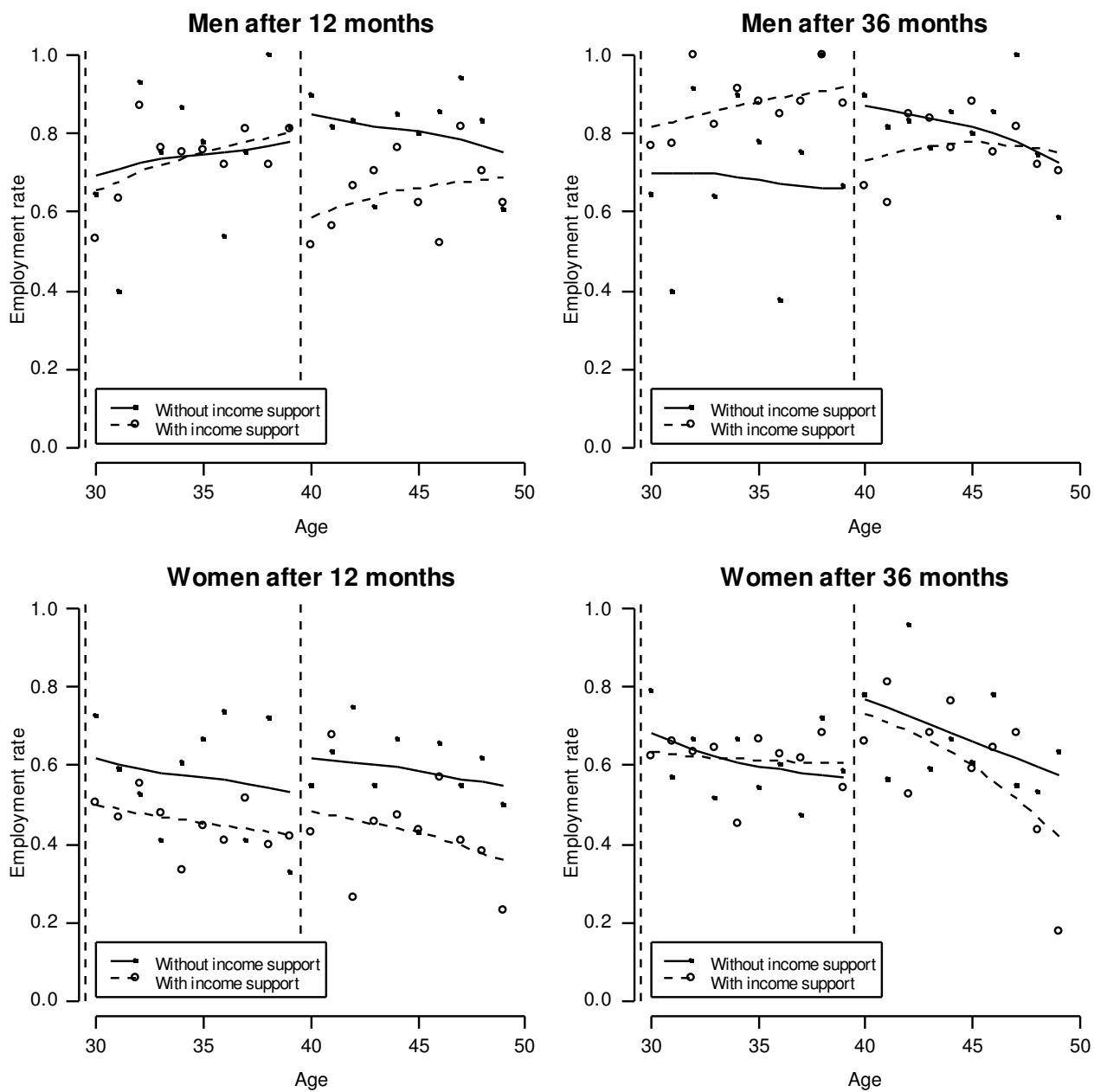


Figure 7: Testing for selection bias. Differences between employment rates of workers 35 to 39 and of matched workers 30 to 34, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.

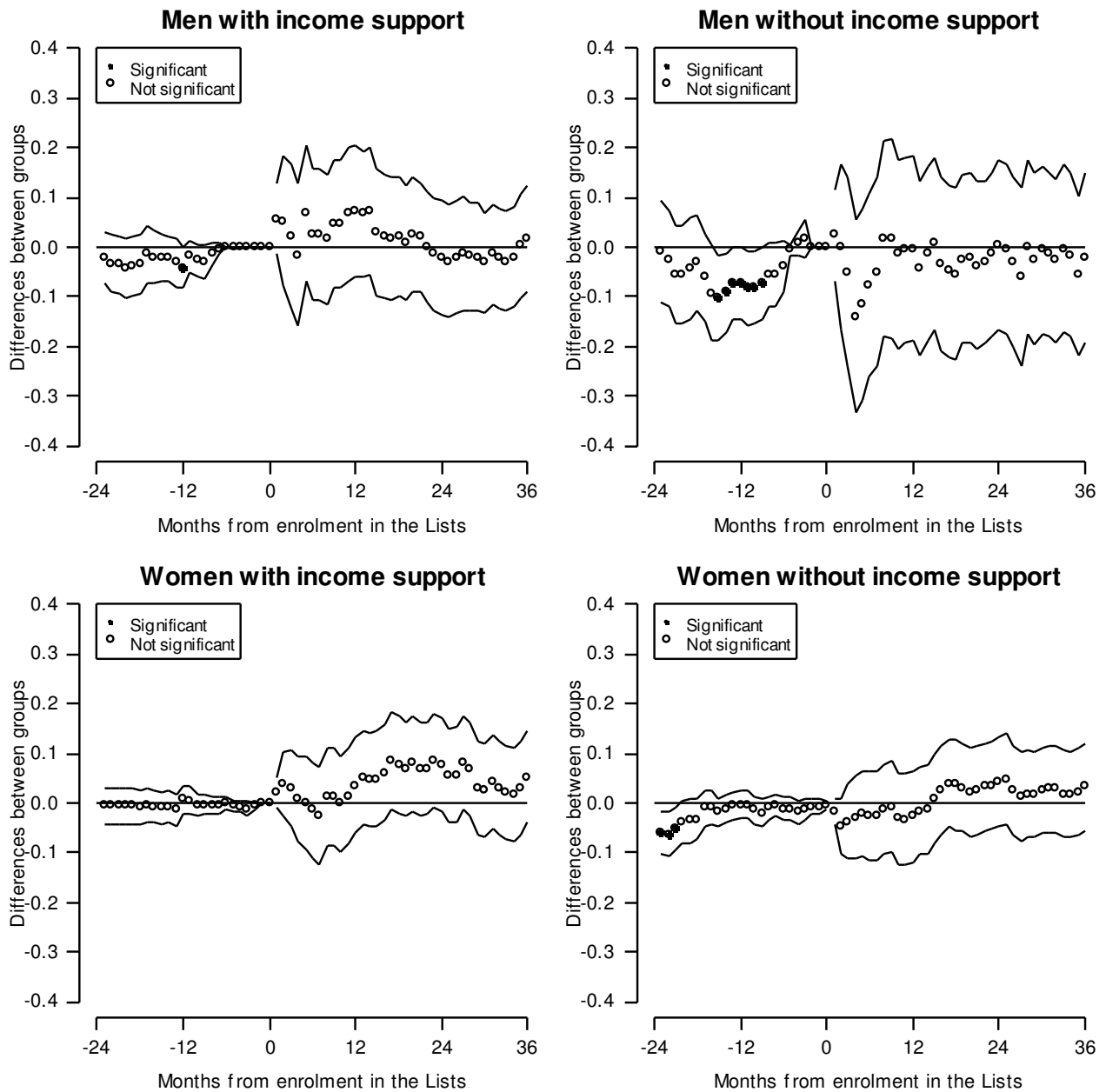


Figure 8: Testing for selection bias. Differences between employment rates of workers 45 to 49 age and of matched workers 40 to 44, by gender and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.

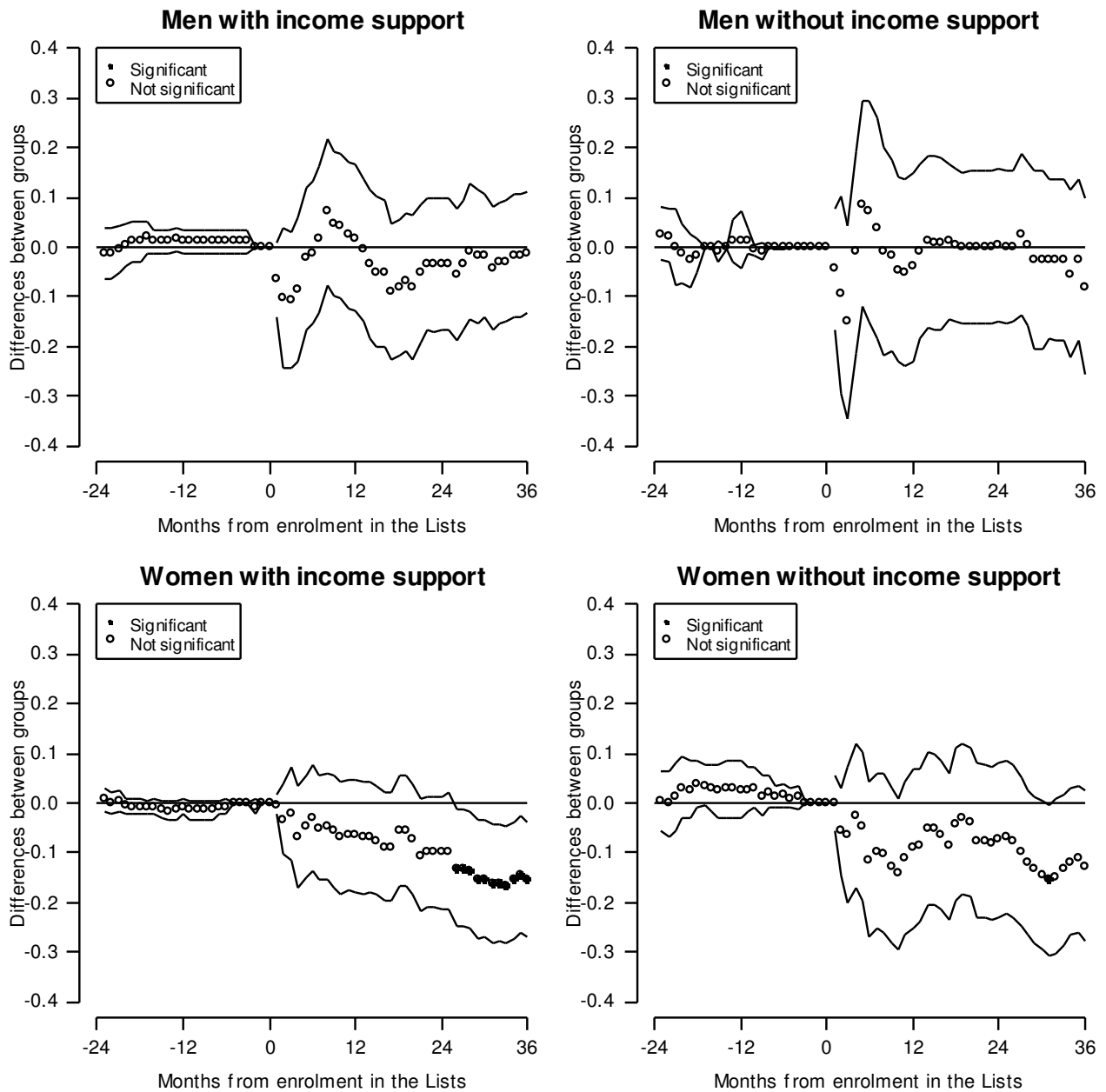


Figure 9: Estimates of the impact of the additional year of eligibility. Employment rates 24 months before to 36 months after enrolment in the Lists, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of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

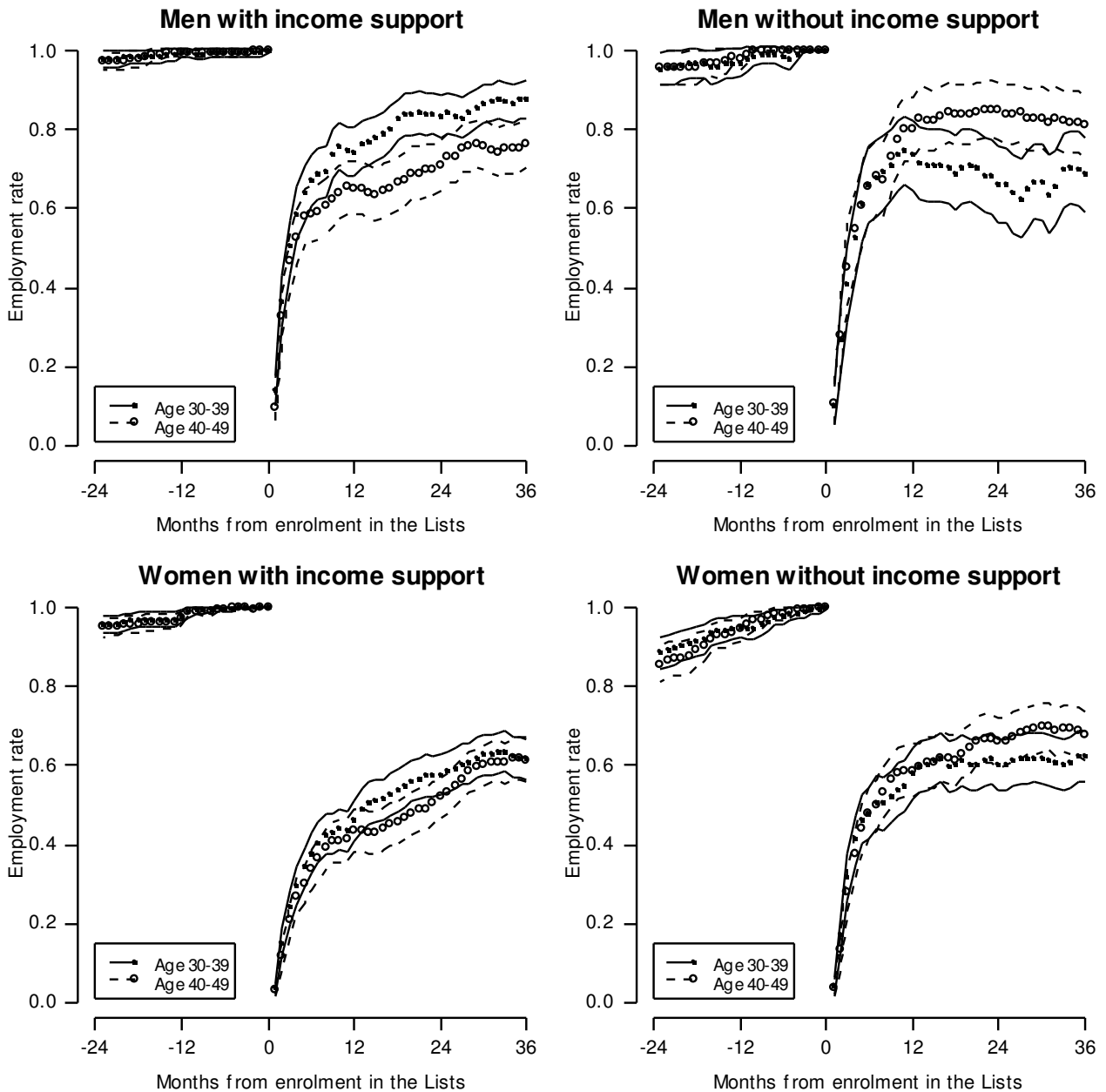


Figure 10: Estimates of the impact of the additional year of eligibility. Differences between employment rates of exposed and of not exposed workers, by gender and entitlement to income support. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility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

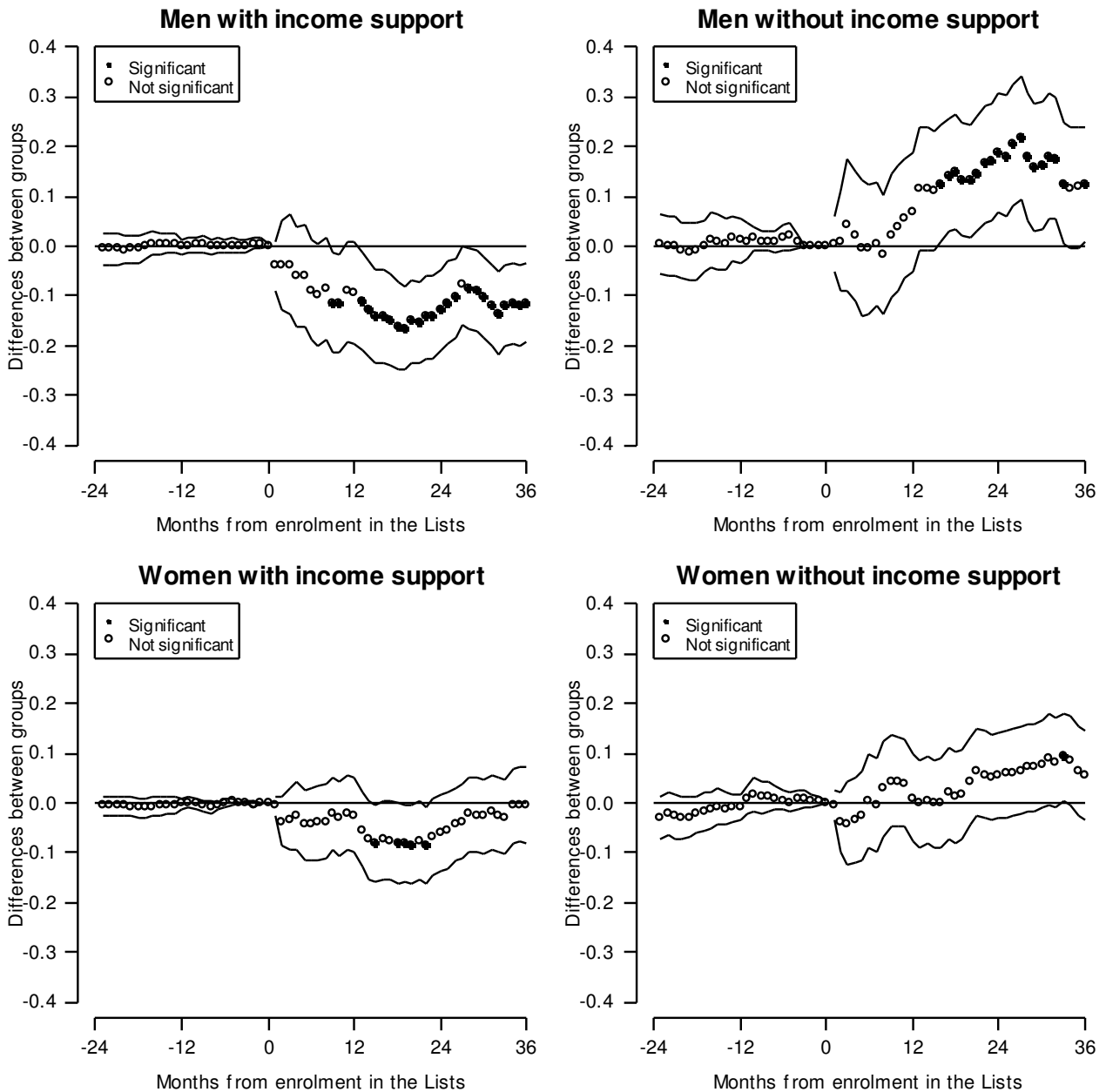


Figure 11: Estimates of the impact of the additional year of eligibility. Kaplan-Meier estimates of survival functions for transitions to employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of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

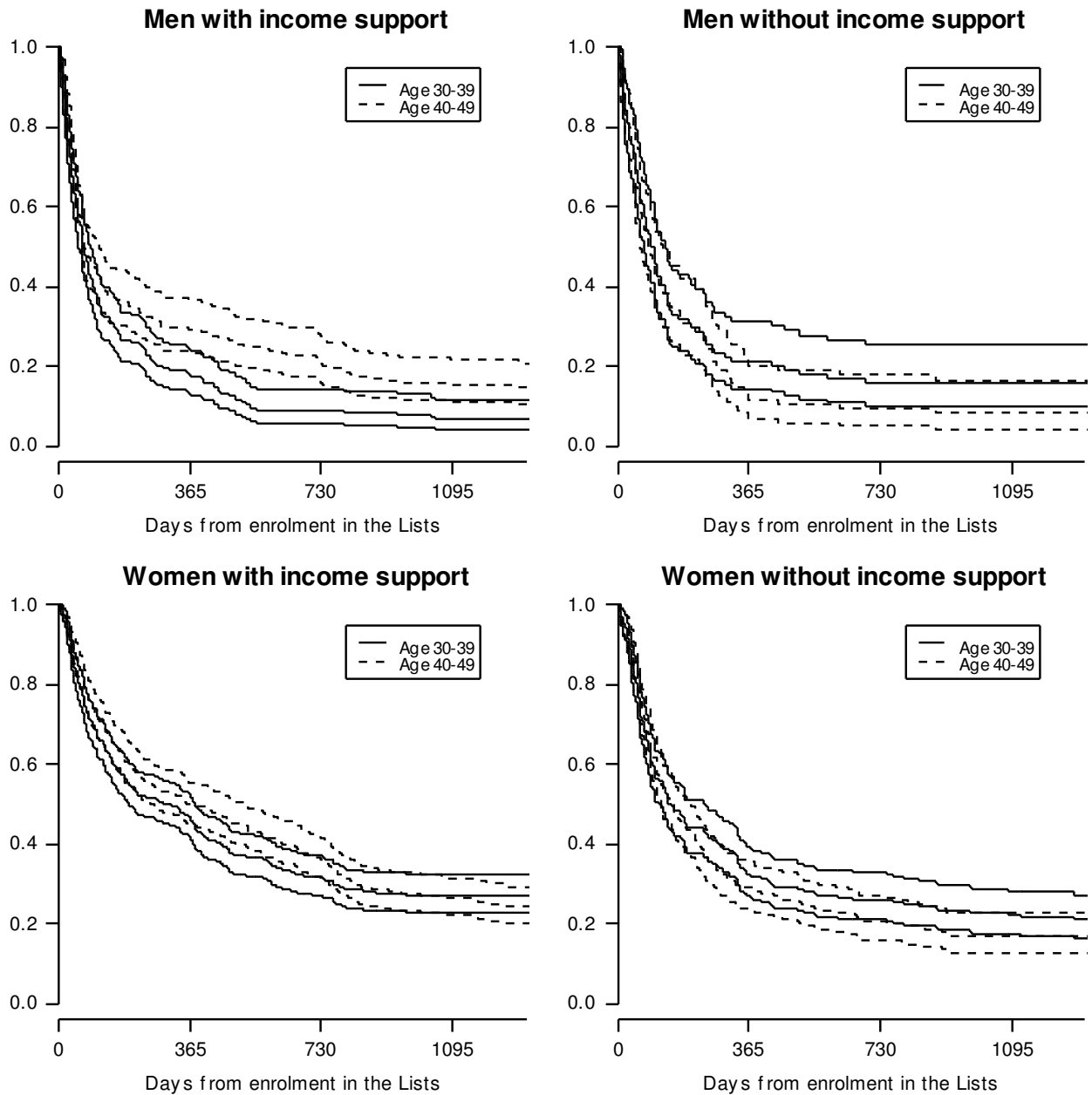


Figure 12: Estimates of the impact of the additional year of eligibility. Smoothed risk functions for transitions to employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility 30 to 39.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

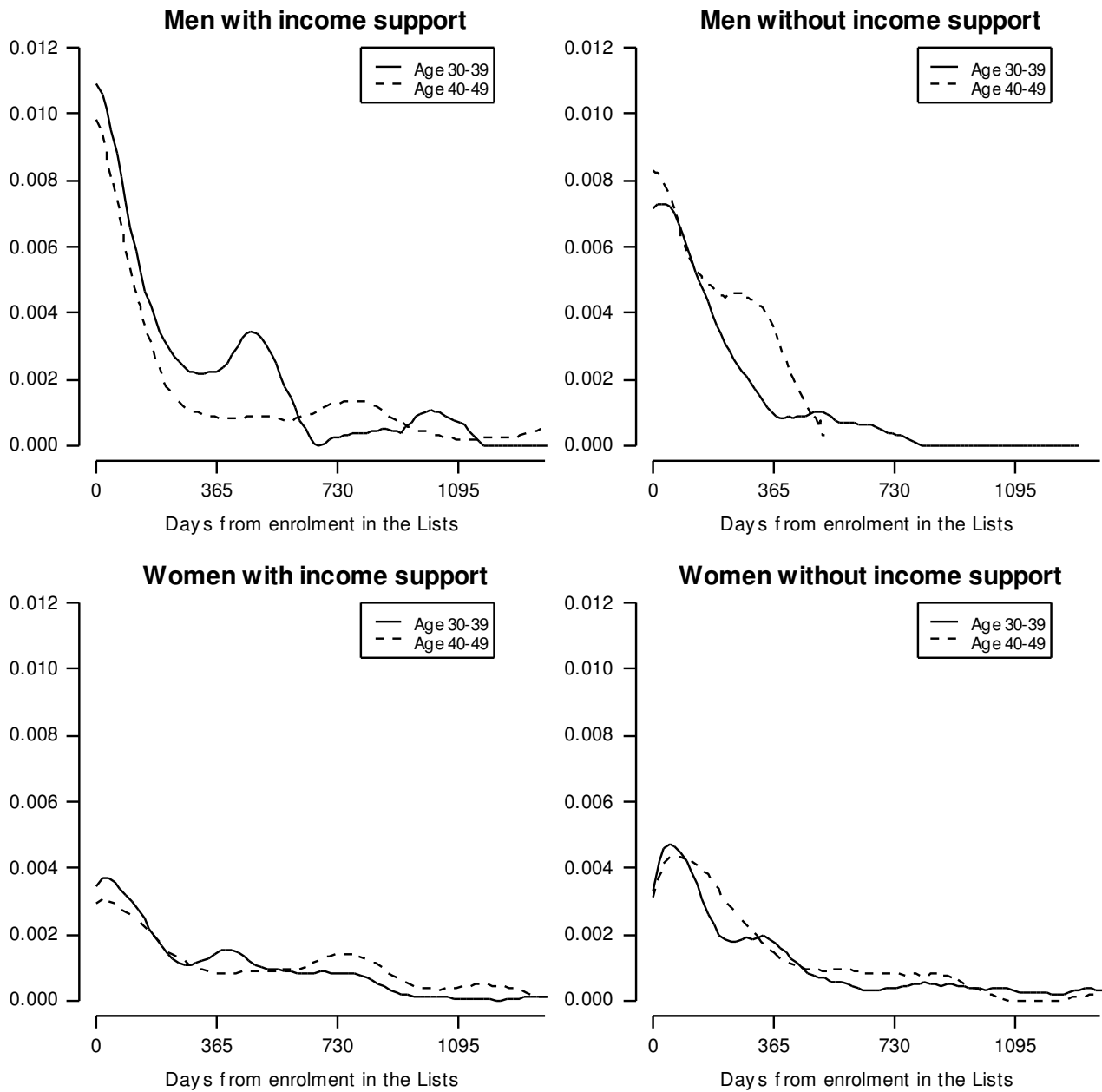


Figure 13: Estimates of the impact of the additional year of eligibility. Kaplan-Meier estimates of survival functions for transitions to permanent employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with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

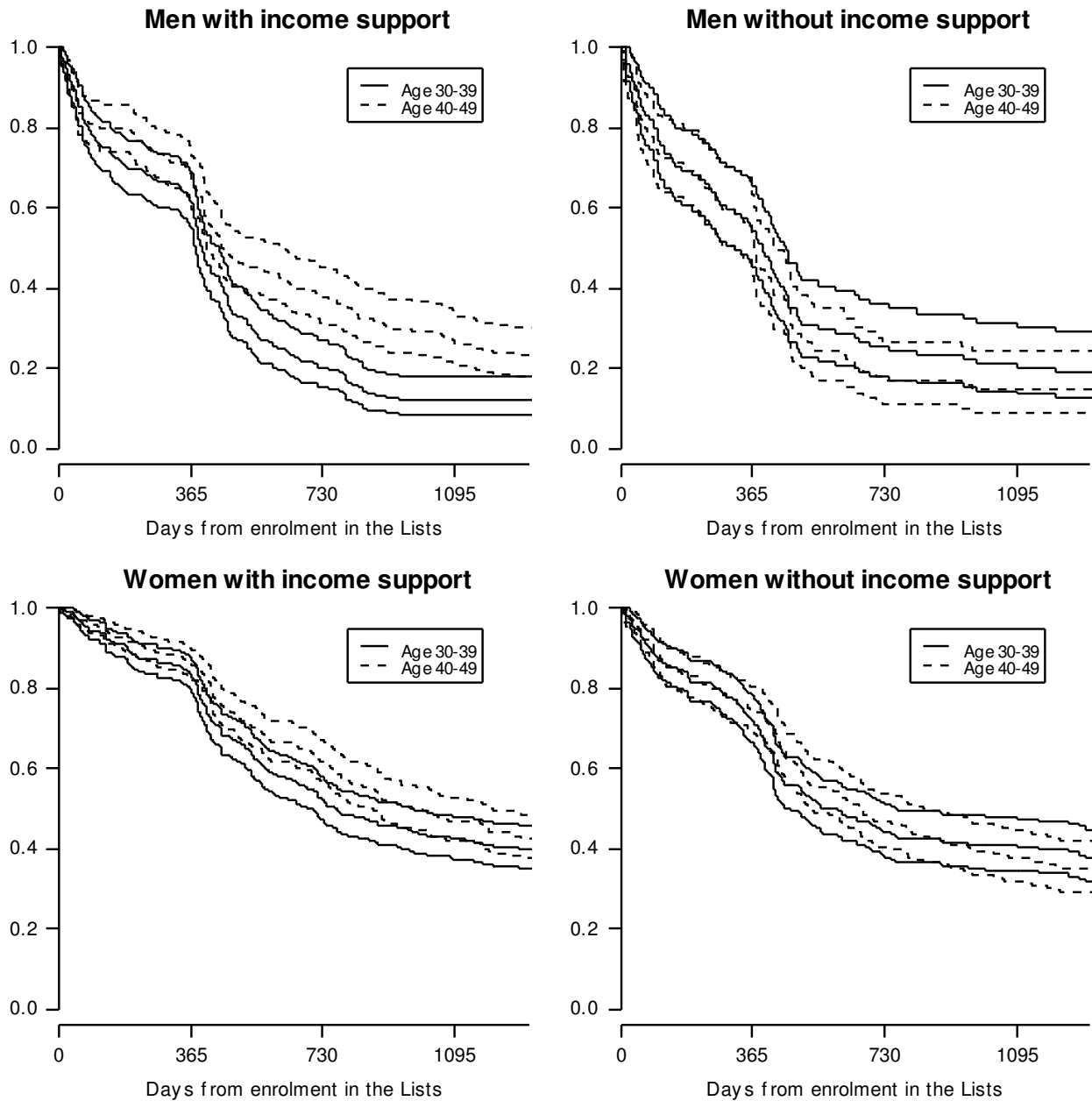


Figure 14: Estimates of the impact of the additional year of eligibility. Smoothed risk functions for transitions to permanent employment, by gender, entitlement to income support and age group. Matching on p-score workers with two years of eligibility (40 to 49) to workers with one year of eligibility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

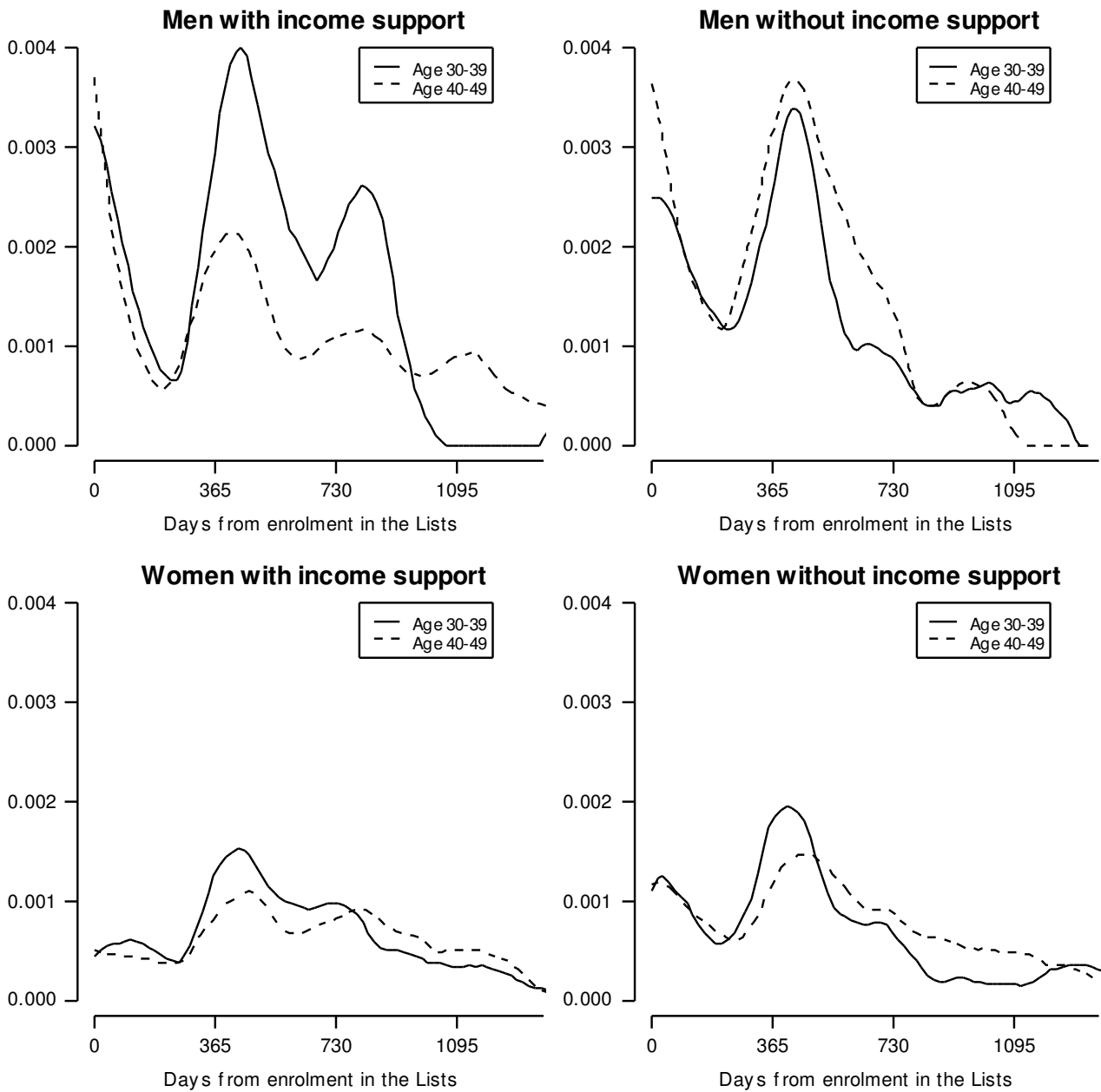


Figure 15: Estimating the impact of income support. Distribution of p-score for treatments (workers with income support) and controls (workers without income support), by gender and age group. Provinces of Treviso and Vicenza, 1997 and 1998.

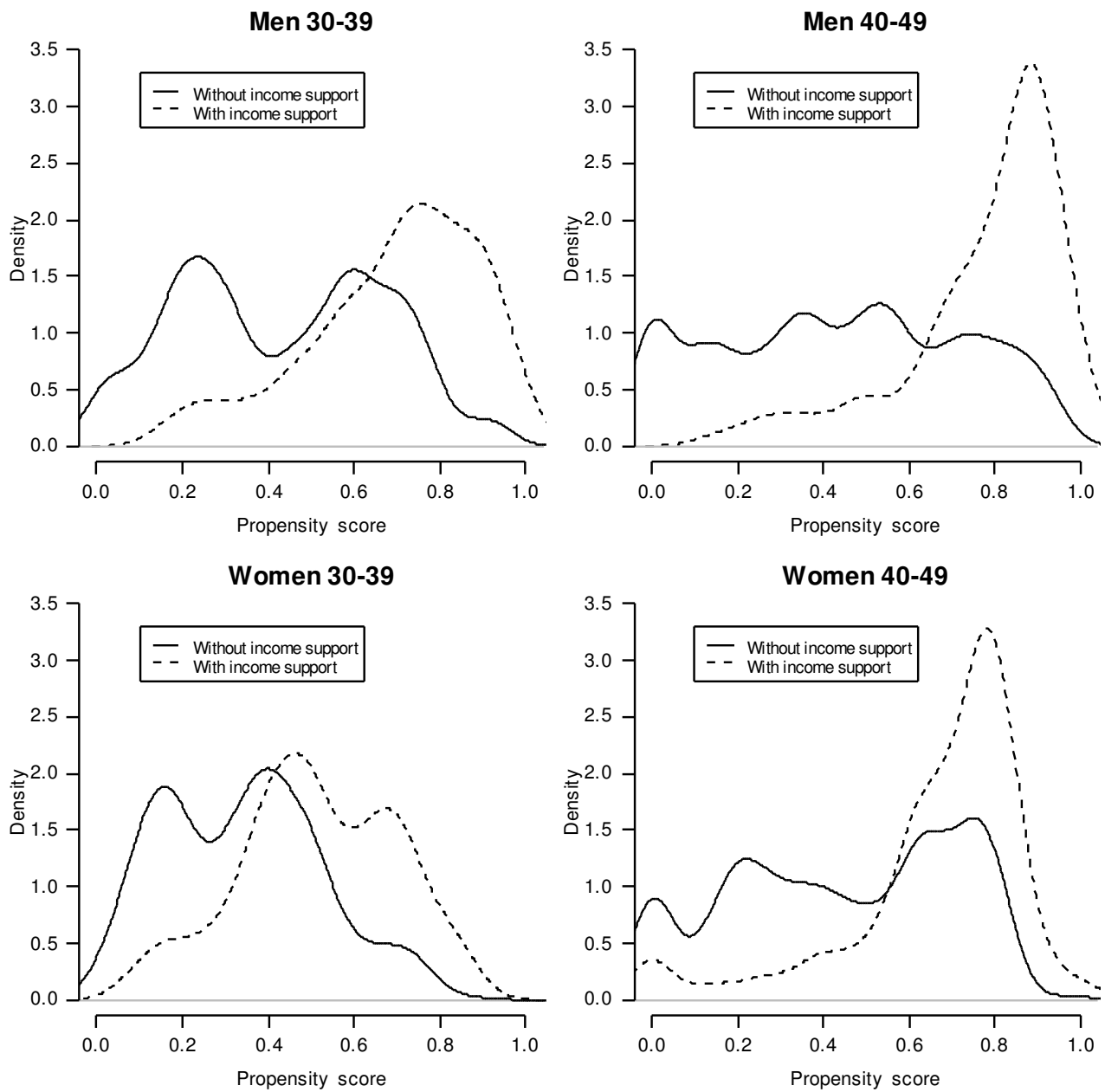


Figure 16: Estimates of the impact of income support. Employment rates 24 months before to 36 months after enrolment in the Lists, by gender, age group and entitlement to income support, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.

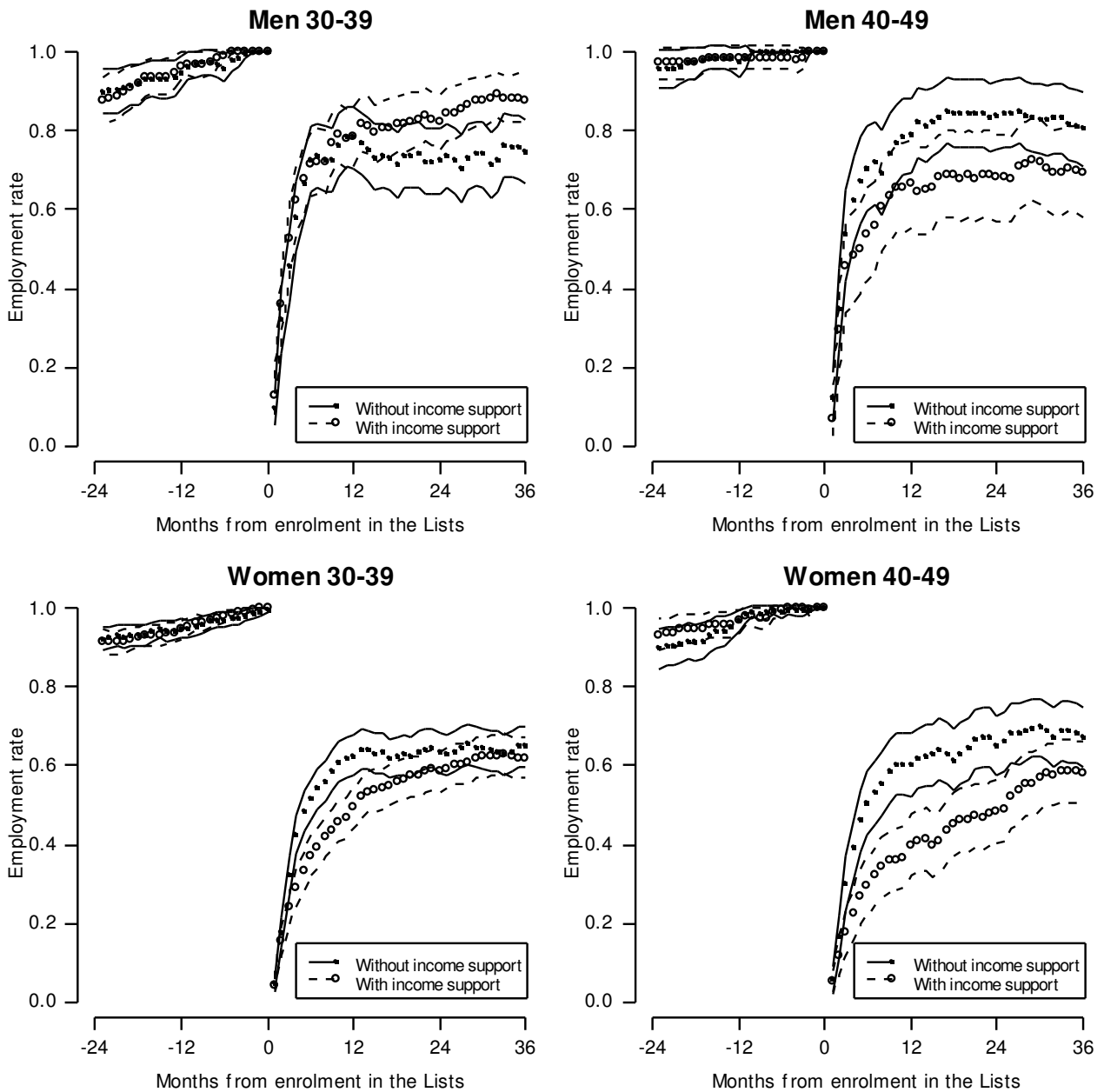


Figure 17: Estimates of the impact of income support. Differences between employment rates of treatments and controls, by gender and age group, matching on p-score (95% confidence intervals are reported). Provinces of Treviso and Vicenza, 1997 and 1998.

