THE IMPACT OF THE "MOBILITY LISTS": NEW EVIDENCE BASED ON LINKING ADMINISTRATIVE ARCHIVES

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Abstract: The ”Mobility Lists” is an Italian labour market policy targeted to dismissed employees combining an active component – a wage subsidy - to a passive one – an income support. The kind of benefits issued varies according to the size of the dismissing firm, while the length of time workers are entitled to receive benefits varies with their age. We exploit the variability of these provisions to evaluate the impact of the programme.
We use linked administrative data from two sources – the dataset resulting from the administration of the policy and a dataset from labour exchanges – for two Italian provinces, years of enrolment in the Lists 1997-98. The linked dataset provides detailed information on workers’ labour market history before, during and after their period of enrolment in the Lists. This allows us to use (a) “past” information to control for individual heterogeneity, (b) “in Lists” history to identify subgroups of workers receiving alternative packages of benefits, (c) information “after” exiting the Lists to look at medium-term effects of the programme.
Evaluation is carried on by looking at the probability to work over the 36 months subsequent to enrolment in the Lists. Selection bias arising from selection on age and on firm size is dealt with via matching techniques. As for the impact of being eligible for two years of benefits instead of just one the main evidence we obtain is that among workers receiving the income support the probability to work is negatively affected, while among workers eligible only for the active component the probability to work is positively affected in particular among males. As for the impact of the income support it turns out in an apparent way that finding a proper comparison group among workers dismissed by small firms, e.g. those not receiving the income support, is a difficult task. Anyway, among those workers for which a match is found receiving the income support results in a lower probability to be at work except among young males which seem to experience a positive impact.

Keywords: Labour market policies, Regression discontinuity design, Matching methods, Linked data.

1. Introduction

The ‘Mobility Lists’ 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 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 15

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employees or less are eligible only for the active component of the policy while those dismissed by firms with more than 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 the ages between 40-49 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 impact of the Mobility Lists have exploited administrative data resulting from the management of the policy. Moreover, they analysed them by using parametric specifications of transitions in a two-state space: enrolled in the Lists or permanently hired. This approach has several drawbacks: on the one hand, information on labour market histories of enrolled workers are limited, and preclude a detailed analysis of the effects of enrolment; on the other hand, results from a parametric approach depend on specification assumptions, which are always somehow arbitrary, and are limited to the identification of differential effects in a neighbourhood of the discontinuities between different benefit regimes.

In order to overcome these limitations, the main goal of this paper is to use more informative data and more robust methods to evaluate the impact of the programme.

(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, a detailed reconstruction of workers' labour market histories while they are in the Lists, the extension of the observation window after they exit the Lists, and 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) We use more robust inferential methods to estimate the effects of benefits for different groups of enrolled workers, and to obtain evidence about different specifications of our basic question: differential effects of maximum allowed duration of stay in the Lists, related to age, and of entitlement to income support, related to firm size; their variability between men and women.

Pre-enrolment labour market histories, together with a better quality of individual information, allow us to evaluate the impact of the policy with an approach based on matching techniques, by comparing individuals who are similar with respect to previous working histories and observed characteristics, while they differ as to the treatment status. In this way, the observed differences may be entirely attributed to the treatment. Moreover, we present some specification tests to validate the estimators.

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 223/91 and 236/93, combines income support for eligible dismissed employees with substantial benefits to employers who hire them. The basic features of the programme may be summarised under three points: maximum allowed period in the Lists, benefits for enrolled workers, benefits to hiring firms. Benefits, for both workers and firms, vary mainly according to dismissing firm size and age at dismissal.

Maximum duration of stay in the Lists is one year for workers under 40, two years for workers between 40 and 49, three years for workers over 49. The main exception to this rule, relevant to our analyses, is that workers hired on temporary (up to) one-year or part-time contracts may extend their stay in the Lists for the duration of that/those contract/s up to a period equal to the one they were in principle allowed – *i.e.*, they may double their stay. In addition, workers over 49 meeting some criteria 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 exceptions, but they have no influence on our case study.

Enrolled workers dismissed by a firm employing more than 15 workers are entitled to income support. Benefits extend up to the maximum stay in the Lists and thus vary according to age at dismissal; they are interrupted while the enrolled worker is hired on a temporary or part-time contract. Other workers in the Lists are only taking benefits to firms hiring them.

The active component of the programme is related to remarkable benefits which are given to employers who hire enrolled workers. Firms hiring workers from the Lists on a permanent basis enjoy an 18-month cut in social security contributions, from the standard rate to the very low rate paid for apprentices, about 2.5% of the standard one. Firms can also hire workers in the Lists on a temporary (up to) one-year basis, and obtain an (up to) one-year cut in social security contributions, of the same size as before. Lastly, firms can largely cumulate these reductions by hiring workers on a temporary one-year contract and then switching to a permanent contract when the first expires: in this case, the cut in social security contributions lasts two years.

In addition, firms hiring workers from the Lists on a permanent basis receive bonuses equal to 50% of the residual benefits that workers would have received had they remained in the Lists. This feature of the programme is close to the benefit transfer scheme proposed by Snower (1994) and is potentially relevant. Nevertheless, its importance as an active policy 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. Consequences for evaluating the impact of the policy

A crucial policy issue concerns the effects of the programme: does it increase the chances for participants to move into employment? The mixture of active and passive policies in the programme produces uncertain prior effects and justifies the interest for an empirical evaluation of its impact.

Limitations on the evaluation exercise are nevertheless quite severe. On the one hand, identification of a suitable comparison group is not only problematic but even operationally unfeasible. On the other hand, there are potential selection bias problems, as workers with different "treatments", which are related to age at dismissal and dismissing firm size, have different characteristics even before entering the programme.

The lack of a control group restricts the possibility of evaluation to differential effects related to different provisions:

(a) maximum allowed duration of stay in the Lists, depending on workers' age at dismissal (conditional on dismissing firm size);
(b) entitlement to income support, depending on dismissing firm size (conditional on workers' age at dismissal).

Nevertheless, it is important to note that a comparison among workers dismissed by firms of different size faces relevant problems, as the 15-workers threshold is not only relevant for the Mobility Lists programme but also for a broad set of contributory and fiscal dispositions.

Thus, the analysis presented in this paper is mainly about differential effects of provisions related to age, separately for the two sets of workers with and without income support. Moreover, as the age group over 50 years often uses the "long mobility" as a transition to retirement, we focus our attention on differential effects between provisions related to less than 40 years old workers and workers in the age group 40-49.

2.3. Implications for methods and data

A feasible evaluation of differential effects of different provisions of the programme is clearly conditioned by available information. Mobility Lists are managed by regional administrative databases, data being collected for the ordinary management of the programme and mainly to register enrolments in the Lists. Thus, regional data are lacking in information for a monitoring of the programme, and even more for an evaluation of its impact.

First of all, there is not enough information about working histories of enrolled workers while they are in the Lists; mainly, information is missing about temporary contracts, which are crucial for an adequate evaluation of the programme. Moreover, available information stops when the worker exits the Lists, thus the analysis of medium-term effects is not possible. Finally, available data are poor about working histories before enrolment in the Lists, which are important in order to control for heterogeneity among workers.

One of the main goals of this paper is to overcome the lack of information in the administrative data by linking them to Netlabor datasets for some provinces of the Veneto region. The linkage may potentially be useful for many goals:

(a) checking the coherence, and indirectly the quality, of data coming from the two sources;
(b) enriching information on socio-demographic characteristics of workers in the Lists;
(c) reconstructing working histories of enrolled workers during their stay in the Lists, in order to adequately consider the chances the programme offers about temporary contracts while still enrolled in the Lists;
(d) extending the observation window after the exit from the Lists, with two distinct advantages: (d1) observing a higher number of complete spells of unemployment, thus eliminating a potential problem of non-random right censoring which is strictly related to the characteristics of the programme itself; (d2) getting information on working histories after the "treatment", in order to analyse medium-term effects of the programme;
(e) enriching information about working histories before entering the Lists, which is important in order to control for selection bias problems when evaluating the impact of the programme;
(f) obtaining information about firms which are involved in the whole working histories of enrolled workers, both the firm taking to the enrolment in the Lists and the ones possibly related to previous or following spells of employment.

In this framework, it is important to underline that information on working histories before entering the Lists is potentially useful to control for heterogeneity in assigning different provisions to enrolled workers. Previous histories, joined with a better quality of available individual characteristics, allow us to face the problem of evaluating the effects of the programme by using a methodological approach based on matching techniques (for a brief presentation and discussion, also referring to the Italian context, see Rettore, Trivellato and Martini, 2003).

In order to better explain the approach, without lack of generality we restrict our attention to the effect of the different allowed maximum stay in the Lists among workers dismissed by firms with more than 15 workers. In this case, the variable which determines different treatments is age, with a distinction between the age groups <40 and 40-49. It is clear that carrying out the evaluation by directly comparing these two age groups would take to a biased estimate of the differential effect of the programme, as age is likely to have an autonomous influence on how workers behave in the labour market.

So far studies in this field (among the others, see Borzaga and Brunello, 1997, Brunello and Miniaci, 1997, Paggiaro and Trivellato, 2002) used convenient parametric specifications of transitions in a two-state space: enrolled in the Lists or permanently hired. In this way, they could not take into account that spells in the Lists sharing the same duration may hide deeply different situations, having as extremes unemployment spells or temporary employment spells. In this paper, we overcome this limitation by using the linked dataset.

Moreover, the parametric approach has two limitations: (i) results depend on specification assumptions, always somehow arbitrary, which are needed in order to identify the effects of different explanatory factors, specifically to distinguish between a pure age effect and the differential effect of different treatments; (ii) conclusions are restricted to estimates of differential effects for workers who are around the 40-years threshold.

Matching techniques potentially represent an attractive and robust methodology which could overcome both these limitations. If successful, they may indeed allow to compare workers who are identical as regards previous working histories and observed characteristics, while they are exposed to different treatments in the programme. In this case, differences which may be observed are to be entirely imputed to the different treatment, namely the maximum admissible duration for the oldest group. Moreover, conclusions obtained by matching techniques are not only valid around the 40-years threshold, as they may be extended to the whole age groups.

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 firing and on the size of the firm they have been fired 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 older workers 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. 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, respectively, the treatment. The mean impact of the treatment on the treatment group is

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

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 value experienced by the treatment group we obtain the following identity

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

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 the specific case, as a result of the selection process treatments are older than controls, implying that the observed difference between the two groups in the probability to be at work 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 conditional 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.

The drawback of this design is that if the program impact is heterogeneous across subjects 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 limitation 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 rests on the condition

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

In practice, to ease calculations we match treatments to controls on the so called propensity score (see Rosenbaum and Rubin, 1983):

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

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

$$H_0 : Y^{NT} \perp age \mid e(X). \quad (4)$$

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

$$H_0 : Y^T \perp age \mid e(X)$$

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).

3.2. Evaluating the impact of income support

Workers fired by firms with 15 or less 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 transfer (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 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 income support. As for the practical problem, we cannot observe 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 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 income support or it is due to other 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 matching estimator 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 selected matching on all other variables differ. On observing significant differences we would conclude that controlling for variables on which matching actually takes place is not enough to solve the selection bias problem.

The rationale for this test is the following. Let u be the unobservables relevant i) for the selection process as well as ii) for the labour market outcome. If the identifying restriction on which the validity of the matching estimator relies

$$H_0 : Y^{NT} \perp u \mid e(X)$$

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 is untestable with reference to the labour market history *after* enrolment in the Lists, since Y^{NT} is not observed on treatments.

Instead, the test is feasible with reference to the pre-enrolment history, that is when we observe Y^{NT} both on treatments and controls.

4. The case study in the Veneto region

4.1. Available datasets

The available datasets for the study are the following:

(a) Administrative datasets for the management of Mobility Lists in the Veneto region, from the beginning of the programme to April 1999 (starting from this moment the dataset ceased to be updated due to changes in competence for the management of the programme). Nevertheless, due to problems in data quality (Paggiaro and Trivellato, 2002) we restrict our attention to records related to enrolments in the period January 1995 – April 1999.

(b) Netlabor datasets from labour exchange offices in Veneto, up to 2001. Due to different quality standards, data coming from some provinces not being enough reliable, we restrict our attention to the provinces of Treviso and Vicenza.

The main characteristics of the dataset from the management of the Lists are the following (for more details, see Gobitti, 1997, and Paggiaro and Trivellato, 2002). For each worker in the Lists, the dataset has information on some socio-demographic characteristics (gender, age, education, province), sector of the dismissing firm, kind of job in the same firm, day of enrolment in the Lists, entitlement to income support. The worker is then followed during the stay in the Lists, until some event happens between: (a) hiring on a permanent basis, (b) exit from the Lists after the maximum allowed period. If none of these events is observed, then (c) the worker is still in the Lists. Thus, we know the elapsed duration of stay in the Lists and the current state: hired on a permanent basis, expired or still in the Lists.

However, much information from these regional datasets is not reliable, mainly due to the different competence of offices involved in the management of the programme. The only reliable information is related to enrolments in the Lists, while the registration of events that follow is full of gaps. One of the main reasons is that new records in administrative files are inserted by the same committee deciding enrolments in the Lists, while updating the records has no substantial consequences for both workers and firms.

Linking the database from the Lists with Netlabor deeply enriches available information for our analyses. A detailed description of characteristics and potentiality of Netlabor is in Bassi, Gambuzza and Rasera (2001), while elements about the framework in which it may turn out to be useful are in Trivellato (2001).

First of all, in Netlabor information about socio-demographic characteristics as gender, date of birth, education is more complete and reliable. Moreover, Netlabor provides information of primary importance about working histories of workers in the Lists. Specifically, for each working spell we may know: (a) type of contract (permanent or temporary, part-time or full-time, apprenticeships, and so on); (b) professional qualification; (c) name and sector of the hiring firm; (d) dates of hiring and dismissing.

The linkage of the two sources is described in Paggiaro and Trivellato (2001) and Paggiaro (2002), to which we refer for details on numbers and characteristics of linked workers. These are defined as workers in the Lists for whom we could find in Netlabor a working spell which ends very close to the day of enrolment. On the whole, the linkage process was quite satisfactory, with a 86% rate of linked workers.

The integrated dataset provides a remarkable enrichment of available information about workers in the Lists, as regards durations of stay in the Lists and reasons of exits, events occurring during the stay, working histories before and after it. First of all, the linkage with Netlabor allows us to observe a much higher number of transitions to permanent contracts, if compared to what emerges from the regional dataset. This is not only due to the enlargement of the observation window, but also to a great number of transitions which are not registered in the Mobility Lists dataset. Moreover, during the stay in the Lists frequency and incidence of temporary contracts are not negligible: on the whole, they cover more than 38% of the total time of stay in the Lists. Interestingly, distributions of durations of stays in the Lists vary appreciably depending on the different types of working paths experienced by enrolled workers.

All things considered, by using integrated data quite different descriptive evidence emerges about transitions from the Lists to employment, when compared to the traditional picture coming from the use of one only dataset. The number of enrolled workers who experience at least one working spell is remarkably higher, both for permanent and temporary contracts. On the whole, workers in the Lists showing at least one spell of employment during their stay are 72%. Moreover, almost 60% of who transits to permanent employment turns out to have experienced a temporary contract before, which is in most cases converted on a permanent basis at its expire. Thus, there is an overall indication of an intense and diversified utilization of the different chances offered by the programme, for both firms and enrolled workers.

4.2 Data for the analysis and first evidence

Starting from the whole linked dataset, estimates of the impact of the programme we present hereafter refer only to a restricted subgroup. First of all, we consider workers less than 50 years old, in order to eliminate from the analysis workers who may use the Lists as a transition to retirement. Among these, we consider only who entered the Lists in the years 1997 and 1998, in order to have enough information both on previous and following working histories. We also eliminate some workers who are strongly suspect to be involved in frauds, and the few ones for whom information is missing about variables which are necessary for matching techniques, as education, qualification, sector (note that this small sample appears to be randomly selected).

After this selection, workers used for the analysis are 4230. In the following we describe their main characteristics which are important for our analysis. Table 1 shows the size of the subgroups of interest, which are defined according to some considerations:

- analysis is carried out separately for men and women, as very different evidence is observed by gender;
- the characteristics of the programme lead us to consider as distinct groups workers dismissed by big firms (with more than 15 workers) or small firms (up to 15 workers);
- in the definition of who is exposed or not exposed to the different treatments age has a key role.

Table 1. Characteristics of workers used for evaluation

	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*	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*

Other data useful for the analysis are day of enrolment, working history during the two years before enrolment and three years after it, province, education, professional qualification and sector of the dismissing firm in the last working spell before enrolment. In the following, we use as summaries of working histories the rates of employment for each month from enrolment in the

Lists, which are defined as the rate of days when the worker is employed with any type of contract. Thus, with the only exception of Section 4.4, we do not consider the differences between who works on a temporary basis (still enrolled in the Lists) and who has a permanent employment (cancelled from the Lists).

A first analysis of the condition of workers enrolled in the Lists looks at employment rates during two years before enrolment and three years after it (Figure 1). The analysis is carried out separately by gender and entitlement to income support and, inside each resulting group, by age. The main evidence is the following:

- The age groups <40 and 40-49 show different working histories before enrolment in the Lists, and this is specially true for men.
- Working histories after enrolment are clearly different between the two age groups. How much these differences are due to age and how much to the impact of the additional year of stay in the Lists is the main question for the following analysis.
- In the two groups with income support employment rates are significantly lower for older workers. Differences between age groups are higher during the second year after enrolment, while they seem to reduce during the third year.
- As regards groups without income support, there are no relevant differences between age groups among women, while among men the older group shows higher employment rates than the younger one, at least starting from the second year.

Figure 1 about here

A first evidence about the age effect on employment rates comes from Figure 2, showing employment rates 12 and 36 months after enrolment, by age at enrolment. Following the idea of regression discontinuity design, the figure highlights the differences between rates around the 40-years threshold, as these should in principle identify the impact of the additional year of enrolment on 40 years old workers. However, the small sample sizes suggest a prudent analysis of this approach, and even the use of third-grade polynomial splines does not improve much the precision of estimates. Keeping this drawbacks in mind, the main evidence is the following:

- There is a large variability of employment rates with age, specially for women and for less than 30 years old workers.
- Age acts in the direction of lower employment rates for older workers, more or less clearly depending on gender and age. This is specially true for women.
- Regarding the discontinuity at the 40-years threshold, the clearest evidence is for men, with a negative impact of the additional year on employment rates for who is entitled to income support, while the effect is positive for workers without support. Among women, there is always a positive impact of the additional year, but the size of the impact is small, specially if compared with the high variability of employment rates with age.

Figure 2 about here

4.3 Estimating the impact of the additional year by matching techniques

In this section we apply the techniques presented in section 3.1 in order to estimate the mean impact on exposed workers of the additional year of stay in the Lists. As control group we use workers enrolled in the Lists when they were less than 40 years old. The analysis is carried out separately by gender and entitlement to income support, and inside each group we match the closer not exposed subject to every exposed worker, so that the difference between the two propensity scores is at most .01. Propensity scores are estimated by logistic regressions, using as covariates *X* province, professional qualification, education, sector of the dismissing firm, working history during the two years before enrolment.

Table 2 shows the performance of matching in the four groups by gender and entitlement to income support. On the whole results are satisfactory, as in the worst case, men without income support, it is possible to find a not exposed worker which may be matched under the chosen criteria for 79% of exposed workers. Note that the different composition of the two groups with respect to *X*, which is the main reason for missing a match, is less accentuated among women.

Table 2. Matching by p-score workers aged 40-49 to workers aged <40

	Men with income support		*Women with income support*		*Men without income support*		*Women without income support*		**Total**	
	N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Exposed (age 40-49)	276		369		141		243		1029	
Not exposed (age <40)	445		922		347		1487		3201	
Matched exposed	236	*85.5*	345	*93.5*	111	*78.7*	233	*95.9*	925	*89.9*
% matched not exposed		*53.0*		*37.4*		*32.0*		*15.7*		*28.9*

As we outlined in section 3.1, the full knowledge of the selection process for exposed workers, which is only based on age, allows us to test whether matching succeeds in solving selection bias problems. Figure 3 clearly shows that the age effect has not been eliminated by conditioning on *X*, specially for women. Moreover, the age effect keeps being stronger among less than 30 years old workers. This evidence is strengthened by Figure 4, which shows the differences between employment rates of workers in the age groups <30 and 30-39, matched by using the same propensity scores as before. Thus, even after eliminating differences related to *X*, employment rates in the 36 months after enrolling in the Lists are different between the two groups of not exposed workers. Specifically, among women younger workers show much higher employment rates than the group 30-39. Among men the differences are weaker but still sometimes significant.

Figures 3 and 4 about here

In Figure 3 the age effect on employment rates is weaker if we limit our attention to more than 30 years old workers. This evidence suggests us to select the control group by including only the oldest among the not exposed workers. Table 3 shows the performance of the matching method applied only to not exposed workers in the age group 30-39. The rate of exposed workers for whom we could find a matched not exposed worker is lower than before, specially among men, but it turns out to be still satisfying (in the worst case, men without income support, we match 67% of exposed workers).

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

	Men with income support		*Women with income support*		*Men without income support*		*Women without income support*		**Total**	
	N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Exposed (age 40-49)	276		369		141		243		1029	
Not exposed (age 30-39)	276		497		197		712		1682	
Matched exposed	188	*68.1*	337	*91.3*	94	*66.7*	221	*90.1*	840	*81.6*
% matched not exposed		*68.1*		*67.8*		*47.7*		*31.0*		*49.9*

Moreover, Figure 5 shows the distribution of propensity scores in both exposed groups and control groups, by gender and entitlement to income support. It is clear that, specially for women, the distributions are quite well overlapping, thus easing the search for a suitable matching.

Figure 6 shows that by restricting the control group to the oldest not exposed workers 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 the age effect has been eliminated among not exposed workers of both genders and exposed men (in the period before enrolment there are some significant differences between 30-34 and 35-39 age groups for all men and women without income support; this is due to problems in the specification of propensity scores related to the small sample sizes).

On the contrary, among exposed women, specially among those entitled to income support, employment rates for the oldest ones are significantly lower in the third year after enrolment. As we discussed in Section 3.1, in this case it is not possible to know whether the age effect is related to the counterfactual result of not exposed workers, leading to selection bias problems, or to the impact of the additional year. Thus, in interpreting results about this group we have to take into account the possible bias in the estimation of the impact in the third year after enrolment.

Figures 5, 6, 7 and 8 about here

Figure 9 presents the mean employment rates during two years before enrolment in the Lists and three years after it, for both exposed workers and the control group, the two groups being equivalent with respect to the observable characteristics *X*. Figure 10 shows the mean impact on exposed workers (more precisely, the mean impact on the subgroup of exposed workers which could be matched to a not exposed worker in the age group 30-39).

As regards men, the evidence is clear and fully agreeing to what we could observe in Figure 6 around the 40-years threshold. Among men with income support employment rates are significantly higher for the control group, specially starting from the second year. Thus, the additional year in the Lists available for workers in the 40-49 age group has a negative impact, about 10-20 points, on their employment rates during the second and third year after their enrolment. There is some evidence that the effects of the passive component of the programme prevail on those of the active component.

For men without income support we observe the opposite effect: starting from the second year employment rates of exposed workers exceed those of not exposed workers by 10-20 points. This evidence, with some caution related to the small sample size of this group (94 enrolled workers, see Table 3), hints to a positive effect of the active component of the programme.

Regarding women, the direction of the effects is similar to what observed for men, but there are not significant differences between exposed workers and the control group, with the only exceptions of some negative effects during the second year for women with income support and a positive impact at the end of the third year for those without support. Nevertheless, we have to remember that these results, specially those regarding women with income support, could be biased by selection problems. Figure 8 showed that among women in the 40-49 age group employment rates decrease with age, and if this decrease was related to the counterfactual result the mean impact in Figure 10 would be underestimated (see the discussion at the end of Section 3.1).

Finally, the evidence we produced so far shows i) a positive impact of the active component for workers dismissed by firms under the 15-workers threshold and ii) a stronger impact of the passive component with respect to the active one for workers coming from bigger firms. It is important to note that this does not allow us to conclude that the impact of the passive component is negative, as groups defined with reference to the 15-workers threshold could be so different to preclude any direct comparison. This problem is discussed in Section 4.5.

Figures 9 and 10 about here

4.4 Estimating the impact of the additional year on waiting times for an employment

Starting from the same exposed and control groups we used in Section 4.3, it is possible to analyse the differential effect of the additional year in the Lists on different variables of interest. In this section we analyse the main evidence about the impact on the duration of spells occurring between enrolment in the Lists and employment. Details on econometric duration analysis are in Lancaster (1990), while for its application to the evaluation of Mobility Lists see Paggiaro and Trivellato (2002).

Figure 11 presents Kaplan-Meier estimates of survival functions related to waiting times for the first transition to any kind of employment. The main evidence is that the only workers showing significant differences are men with income support, for whom the additional year takes to longer waiting times for employment, the sign of the impact agreeing with results presented in Section 4.3. For the other groups survival functions of exposed and control groups are statistically indistinguishable (this result is confirmed by 5% log-rank tests on the whole distributions), but even in this case the signs of the effects are the same as in Figure 10.

Figure 11 about here

More evidence comes from Figure 12, showing estimates of risk functions (smoothed by kernel methods) associated to survival functions in Figure 11. Risk functions share the same information as survival functions, but they allow us to analyse which are the moments when workers have a higher instant probability to find an employment. The main evidence is that for all groups, but more clearly for men, the maximum probability is right after enrolment in the Lists, while there is a sharp decline after it.

Among workers of both genders who are entitled to income support, the 30-39 age group shows a high peak (but much lower than the risk estimated during the first months of enrolment) around 1 year after enrolment, while the 40-49 group shows a less pronounced peak after 2 years. As we are analysing the duration of spells without any kind of employment, these peaks take place exactly at the expiring time for both age groups. Thus, there is an increase in the probability of finding an employment when income support is ceasing, coherently with a delay in transitions caused by entitlement to income support. Finally, groups without income support show similar peaks too, but they are much less pronounced.

Figure 12 about here

A different definition of origin and destination states may provide more elements for the analysis. Figures 13 and 14 respectively present survival and risk functions for the waiting time for a permanent employment. Thus, this is the only part of our analysis where spells of unemployment and temporary employment contribute in the same way to define waiting times (note that these are the same states analysed in Paggiaro and Trivellato, 2002).

The impact of the additional year on waiting times for a permanent employment is positive among workers with income support, specially starting from the second year, while it is negative or negligible for workers without support. Nevertheless, even in this case the only significant differences are observed for men with income support: starting from the second year, the 30-39 age group has a much higher probability of transition to a permanent employment.

Turning to the analysis of the shapes of the curves, we observe substantial differences with respect to Figures 11 and 12. All survival curves in Figure 13 have a strongly decreasing pattern, besides just after enrolment, also in the months immediately after the first year and, less evidently, after the second year. This evidence is even clearer in Figure 14, where we may observe peaks in risks of transition after one year, and partly after two years, which are much more evident than the ones observed in Figure 12. Specifically, peaks at the beginning of the second year reach heights which are never lower, and sometimes much higher, than the risk of transition just after enrolment. This happens because, right after one year, we observe transitions to permanent contracts for both who during the first year was unemployed and who had a temporary contract in the first days of enrolment which after one year is transformed on a permanent basis. This also explains why, contrasting with what observed in transitions to any employment, men with income support in the 30-39 age group keep having a higher risk of transition to permanent employment up to the beginning of the third year of enrolment. As before, the other 3 groups show no substantial differences between exposed and not exposed workers.

Figures 13 e 14 about here

All things considered, the impact of the additional year is clearly strong for men with income support: employment rates during the 36 months after enrolment are substantially lower (Figures 9 and 10) while waiting times are longer, both for the first employment after enrolment (Figures 11 and 12) and for transitions to permanent employment (Figures 13 and 14). As regards other groups, women with income support and all workers without it, evidence shows an impact of the additional year on employment rates, which are lower with income support and higher without it, but not on waiting times for employment.

4.5 Estimating the impact of income support by matching techniques

In this Section we present the results of evaluation of the mean impact of income support on exposed workers. Entitlement to income support for the whole period of stay in the Lists is related to being dismissed by firms with more than 15 workers. Thus, the natural control group for the evaluation are workers of the same age dismissed by firms with at most 15 workers.

As before, evaluation is carried out by matching every exposed worker to a not exposed worker having almost the same propensity score. Among covariates for the estimation of propensity scores here we include a polynomial on age, in order to take into account the different age composition of workers respectively dismissed by firms with more or less than 15 workers.

Preliminarily, we conduct the specification test discussed at the end of Section 3.2, in order to test whether conditioning to X variables is enough to solve selection bias problems. We exclude the first three observed months (respectively 24, 23 and 22 months before enrolment in the Lists) from the working history used to estimate the propensity score. Then we look at employment rates in the three excluded months for matched workers: as discussed in Section 3.2, if matching variables were enough to eliminate all differences between exposed and not exposed workers the two groups would not present differences in the first months of observation.

In our sample, differences in the three months turn out to be highly significant for all women, less significant for men in the 40-49 age group, not significant for younger men. On the whole, matching variables do not seem to be enough to eliminate differences in composition between the two groups of workers defined by the 15-workers threshold.

In the following, we present some results about the matching carried out by using the whole previous working history, including the first three months, as matching variables. Clearly, in this case it is not possible to test the presence of selection bias, but previous results are strong enough to suggest prudence in interpreting estimates of the impact.

Results about the performance of matching are in Table 4. The first thing to note is that, if compared to what we did in Section 4.3, the small size of the not exposed group takes to serious problems in finding suitable subjects to match to exposed workers. Thus, the rate of exposed workers for whom we could find a match is much lower than before. The only exceptions are women in the 30-39 age group, which is the only group where not exposed workers are more than exposed ones.

Besides sample sizes, Figure 15 shows that problems in finding suitable matches are related also to a different distribution of p-scores in exposed and control groups, mainly for workers in 40-49 age groups. This confirms that there are relevant and systematic differences between workers dismissed by firms with more than 15 workers and those coming from smaller firms.

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

		Men 30-39		*Women 30-39*		*Men 40- 49*		*Women 40- 49*		**Total**	
		N	*%*	*N*	*%*	*N*	*%*	*N*	*%*	*N*	*%*
Exposed	(with income support)	276		497		276		369		1418	
Not exposed	(without income support)	197		712		141		243		1293	
Matched exposed		112	*40.6*	346	*69.6*	71	*25.7*	147	*39.8*	676	*47.7*
% matched not exposed			*56.9*		*48.6*		*50.4*		*60.5*		*52.3*

Figure 15 about here

Figure 16 shows mean employment rates of groups with and without income support during 24 months before enrolment in the Lists and 36 months after it, while Figure 17 shows their differences. If we exclude younger men, the main evidence is a negative impact of income support during the first year, which tends to fade out more or less rapidly starting from the second year.

Younger women, who are entitled for one-year income support in the space of no more than two years, show significant differences during the first year, while the curves get closer just after 12 months and are almost indistinguishable during the third year, thus after the expiry of income support. We observe similar evidence for men and women in the 40-49 age group, with the impact lasting more than two years, perfectly reflecting the longer allowed period of entitlement to income support for older workers.

Men in the 30-39 age group present instead a positive estimate of the impact during the third year, thus after the exit from the Lists. It has to be explicitly noted that this feature is mainly due to the fact that the employment rate of workers without income support tends to diminish after one year and then stays stably around a level of 70%. Thus, three years after losing their job many men in the core of their working life are still not employed. A possible explanation is that these subjects, who do not have income support during the time needed to find a new job, turn to some kind of employment not registered by Netlabor, as self-employment or non-regular jobs.

Figures 16 and 17 about here
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5. Conclusions and future developments

The goal of this paper is the joint use of matched datasets and matching techniques in order to solve some problems usually found in evaluating the impact of the Mobility Lists programme. Specifically, for two provinces of the Veneto region we separately evaluate two kinds of differential effects which are potentially estimable starting from the programme design: (a) the effect of a longer stay in the Lists, which is available for workers enrolled when they are 40 years old or more; (b) the effect of income support, to which only workers dismissed by firms with more than 15 workers are entitled.

The linkage between the administrative datasets for the management of the Lists and Netlabor allows us to use richer and more reliable information on enrolled workers, for both their socio-demographic characteristics and an accurate reconstruction of their working histories before, during and after their stay in the Lists.

The availability of previous history and more detailed individual information allows us to evaluate the differential effects eliminating problems of selection bias, by using a methodological approach centred on matching techniques. Nevertheless, available information turned out not to be enough to eliminate selection bias in evaluating all differential effects, and results are robust only in estimating the effect of the additional year in the Lists.

The impact of the additional year is specially clear for men with income support: employment rates during the 36 months after enrolment in the Lists are substantially lower for exposed workers, while waiting times to employment are longer, both considering any kind or contract or restricting to permanent employment. Regarding other groups, women with income support and workers without it, the impact of the additional year is significant on employment rates, which are lower with income support and higher without it, but is not significant on waiting times for employment.

On the contrary, identification of an effect of income support, related to the 15-workers threshold for the dismissing firm, has strong problems of selection bias, as workers dismissed by firms with more than 15 workers show observable and not observable characteristics which are too different when compared to the ones of workers coming from smaller firms. These differences, besides the relations between firm size and individual characteristics of its workers, are probably related also to the many provisions involving the 15-workers threshold. Keeping in mind the potential bias of the results, the main evidence is, if we exclude younger men, a negative impact of income support during the first year, which tends to zero, faster or slower, starting from the second year after enrolment.

More detailed and robust estimation of differential effects of the programme would need better data, and a possible perspective is the use of Inps databases. First of all, the knowledge of firm size could be useful to reduce selection bias problems in estimating the effect of income support, by selecting exposed and control groups close to the 15-workers threshold, in order to make them less heterogeneous in the same way we reduced the age groups in this paper. Moreover, Inps databases could provide us more information both for both matching variables and variables of interest. As an example, we could know working histories up to 20 years before enrolment in the Lists, or have information about wages before and after enrolment, which could give us indications on possible effects of the programme on the quality of employment.

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Figure 1: Employment rates during 24 months before enrolment in the Lists and 36 months after it, by gender, entitlement to income support and age group. Workers enrolled in the Lists in Treviso and Vicenza during years 1997 and 1998 (confidence intervals at 95% level).

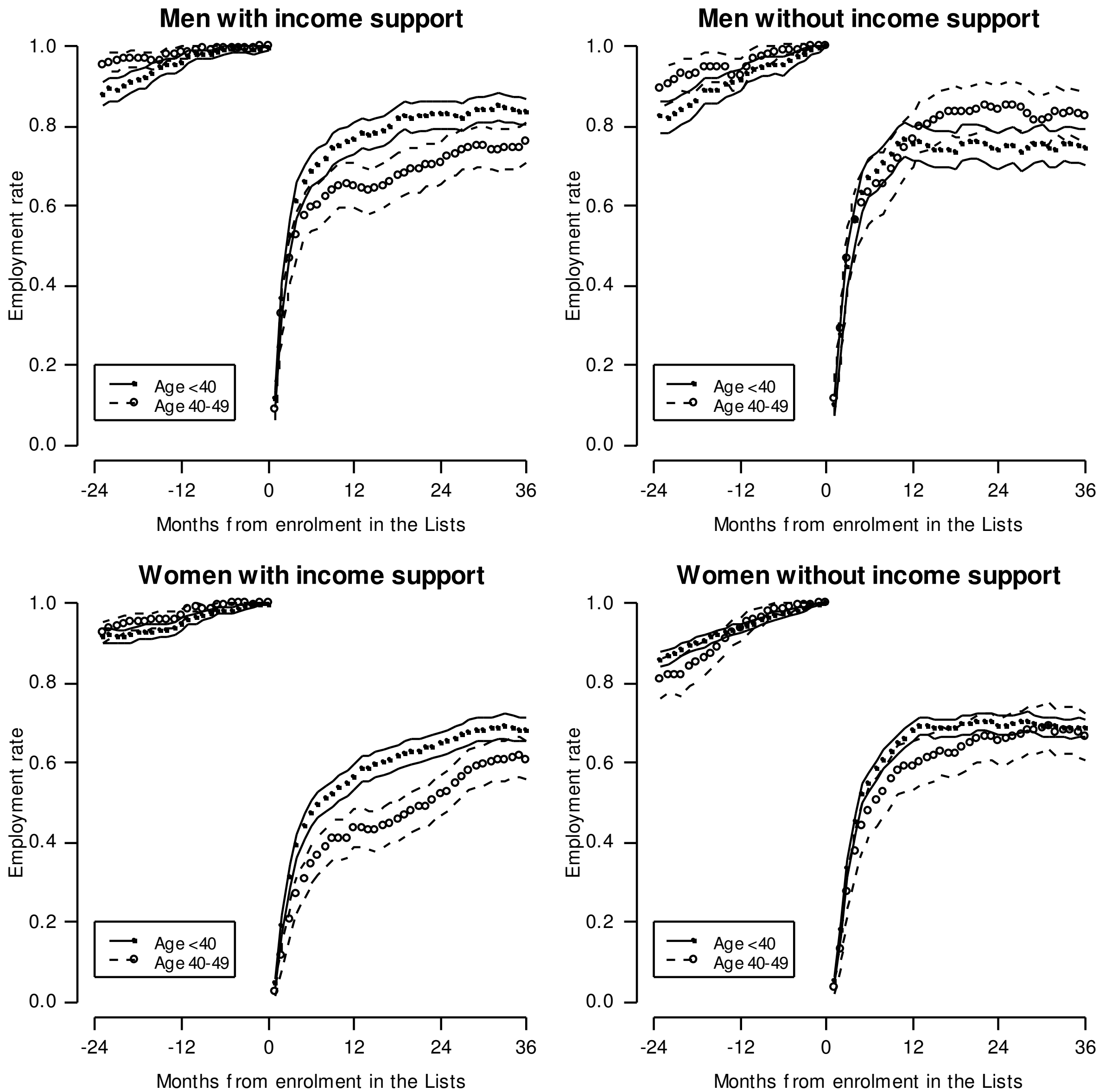

Figure 2: Employment rates 12 and 36 months after enrolment in the Lists, by gender, age and entitlement to income support (point estimates and polynomial splines). Workers enrolled in the Lists in Treviso and Vicenza during years 1997 and 1998.

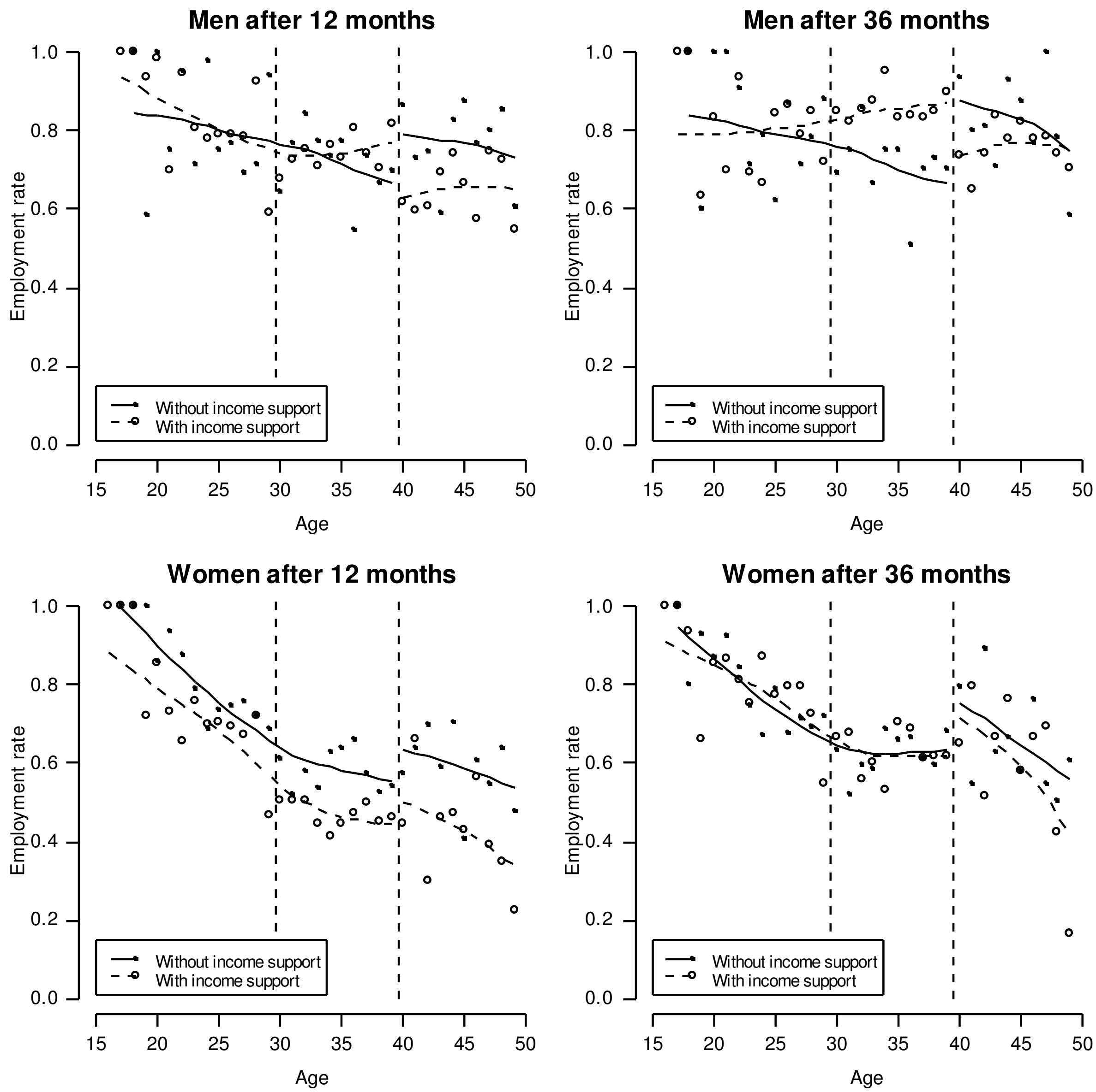

Figure 3: Employment rates 12 and 36 months after enrolment in the Lists, by gender, age and entitlement to income support (point estimates and polynomial splines). Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers (<40 years).

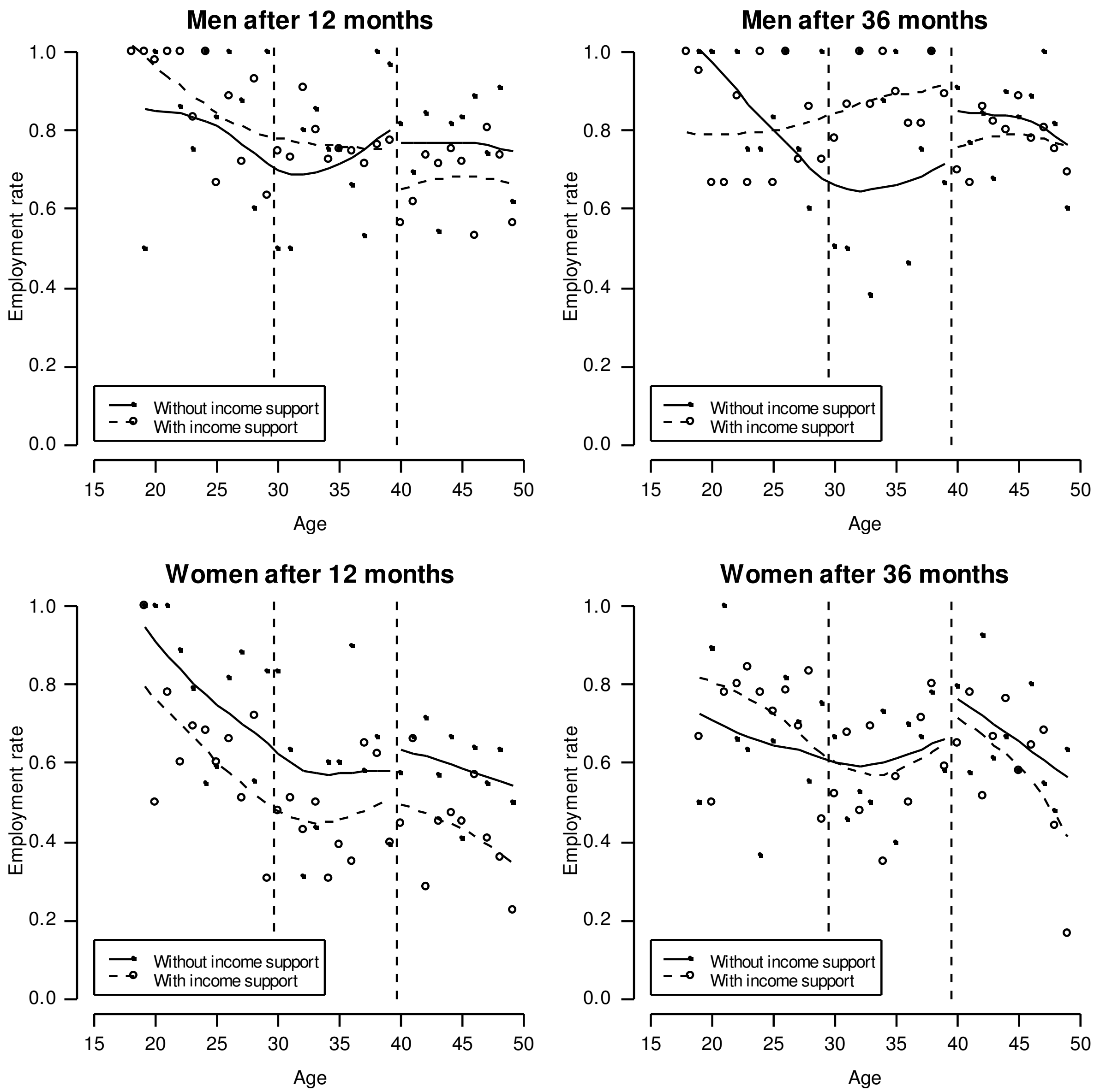

Figure 4: Test for selection bias. Differences between employment rates of not exposed workers in the 30-39 age group and not exposed workers under 30, by gender and entitlement to income support. Matching by p-score (confidence intervals at 95% level).

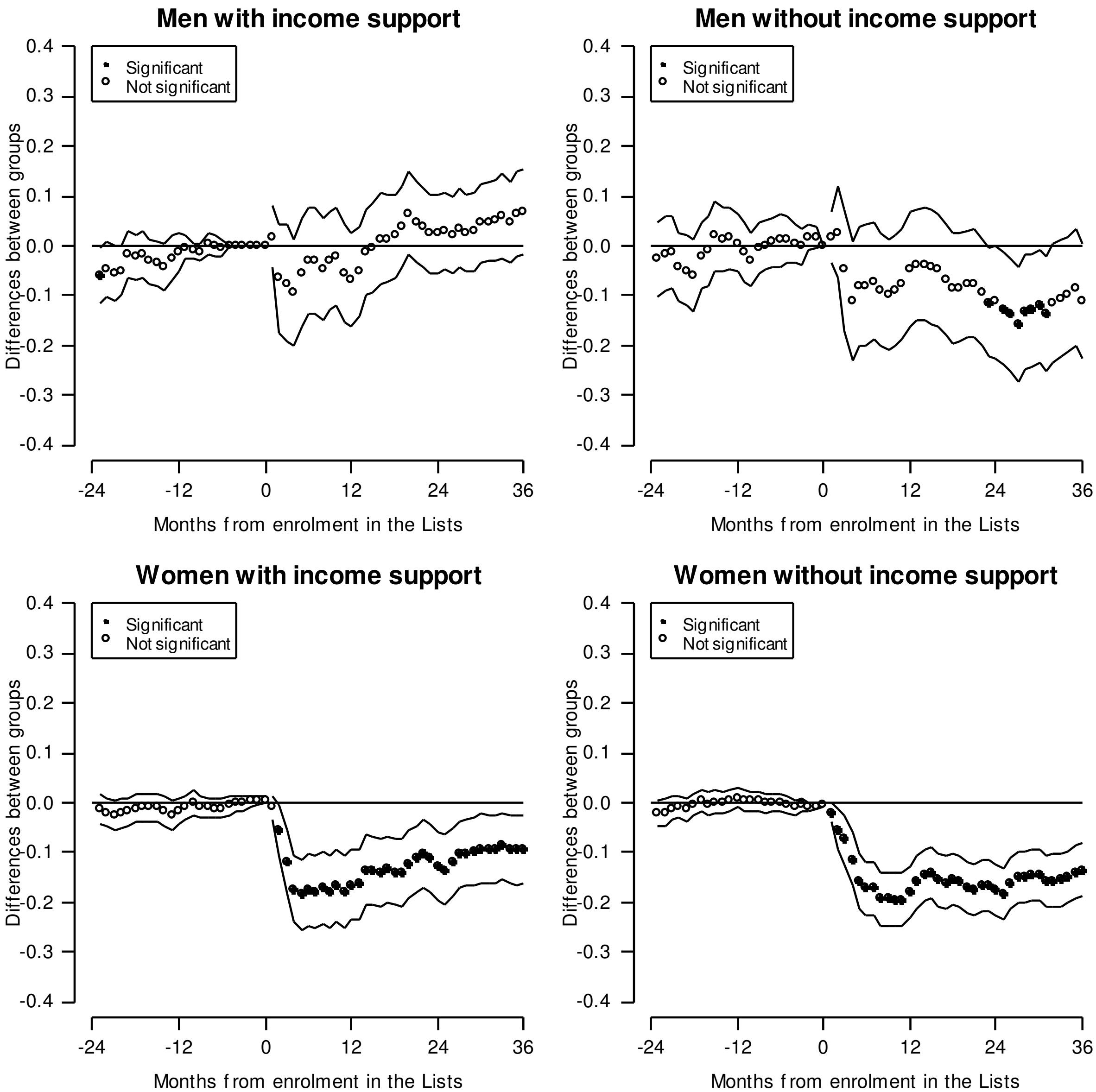

Figure 5: Estimating the impact of the additional year. Distribution of p-score in exposed (40-49 years) and not exposed (30-39 years) groups, by gender and entitlement to income support.

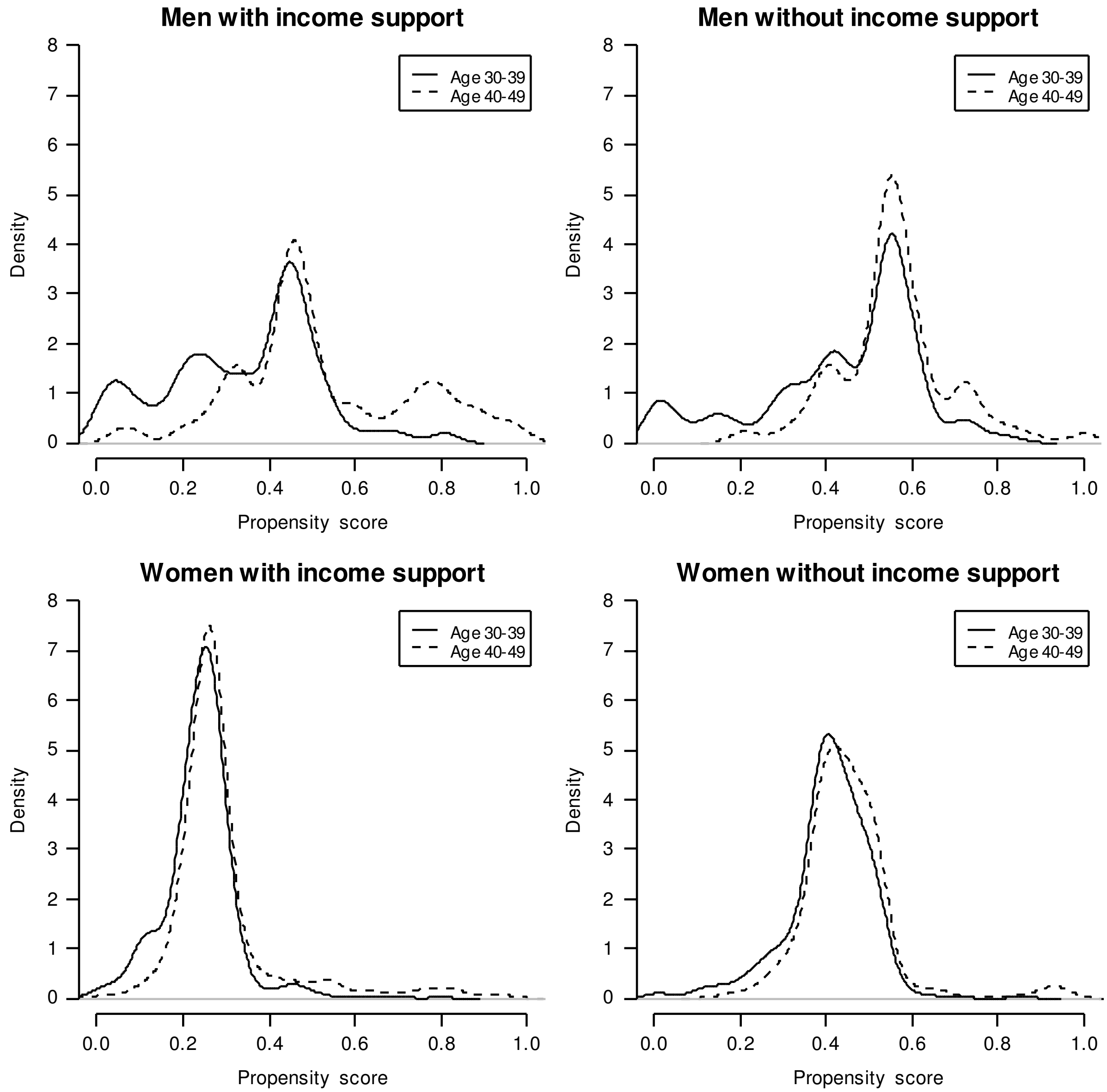

Figure 6: Employment rates 12 and 36 months after enrolment in the Lists, by gender, age and entitlement to income support. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers (30-39 years).

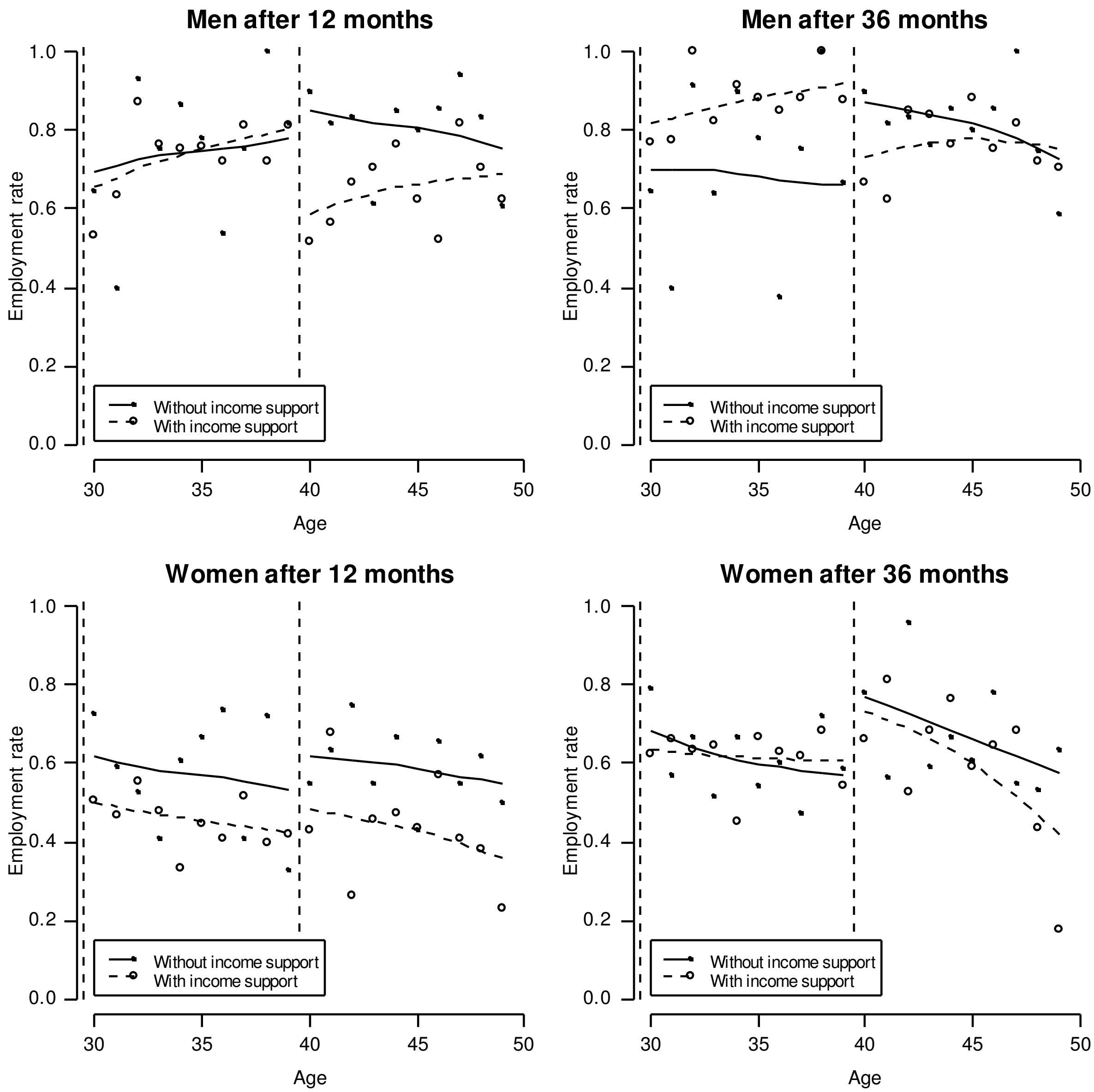

Figure 7: Test for selection bias. Differences between employment rates of not exposed workers in the 35-39 age group and not exposed workers aged 30-34, by gender and entitlement to income support. Matching by p-score (confidence intervals at 95% level).

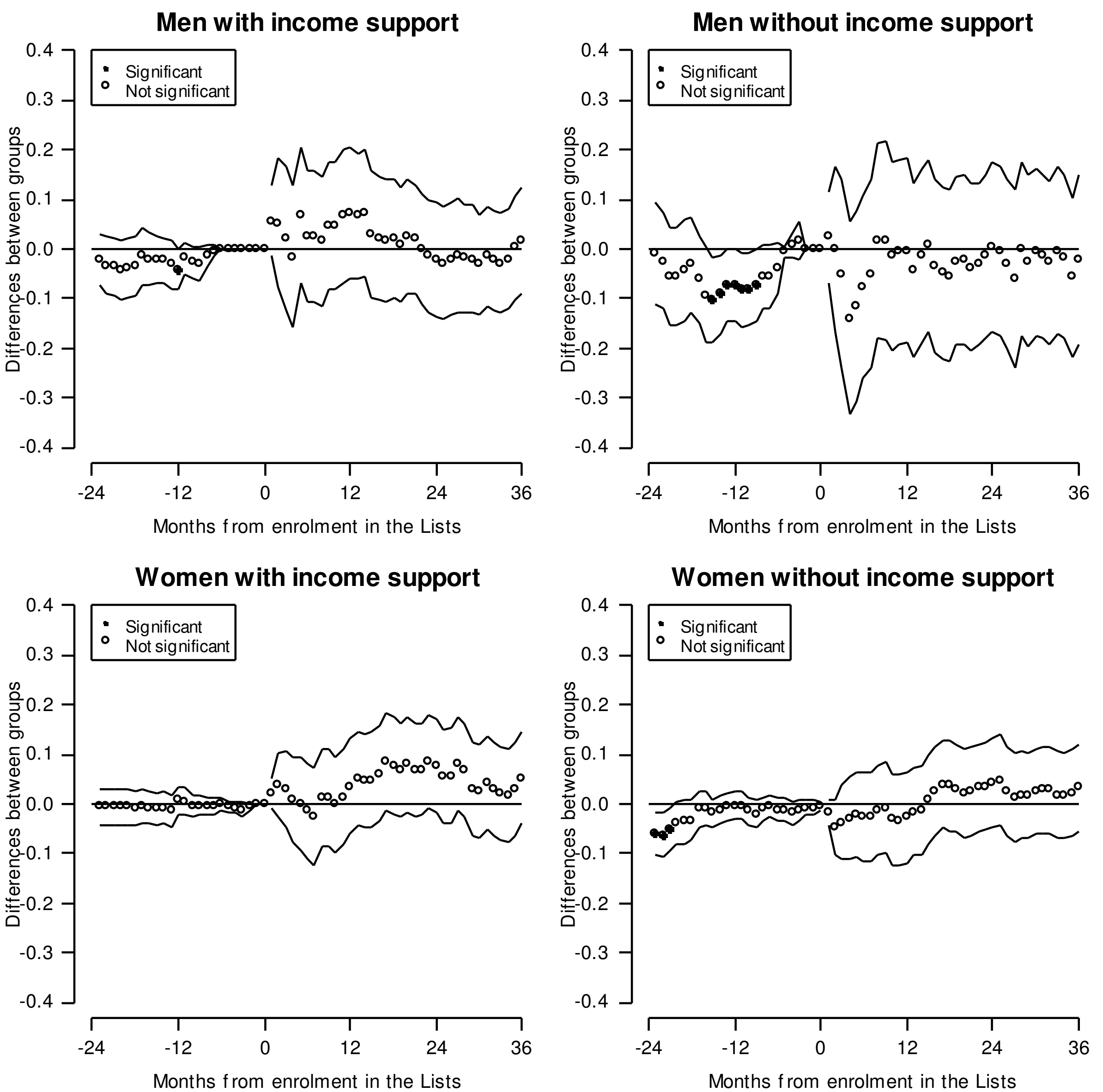

Figure 8: Test for selection bias. Differences between employment rates of exposed workers in the 45-49 age group and exposed workers aged 40-44, by gender and entitlement to income support. Matching by p-score (confidence intervals at 95% level).

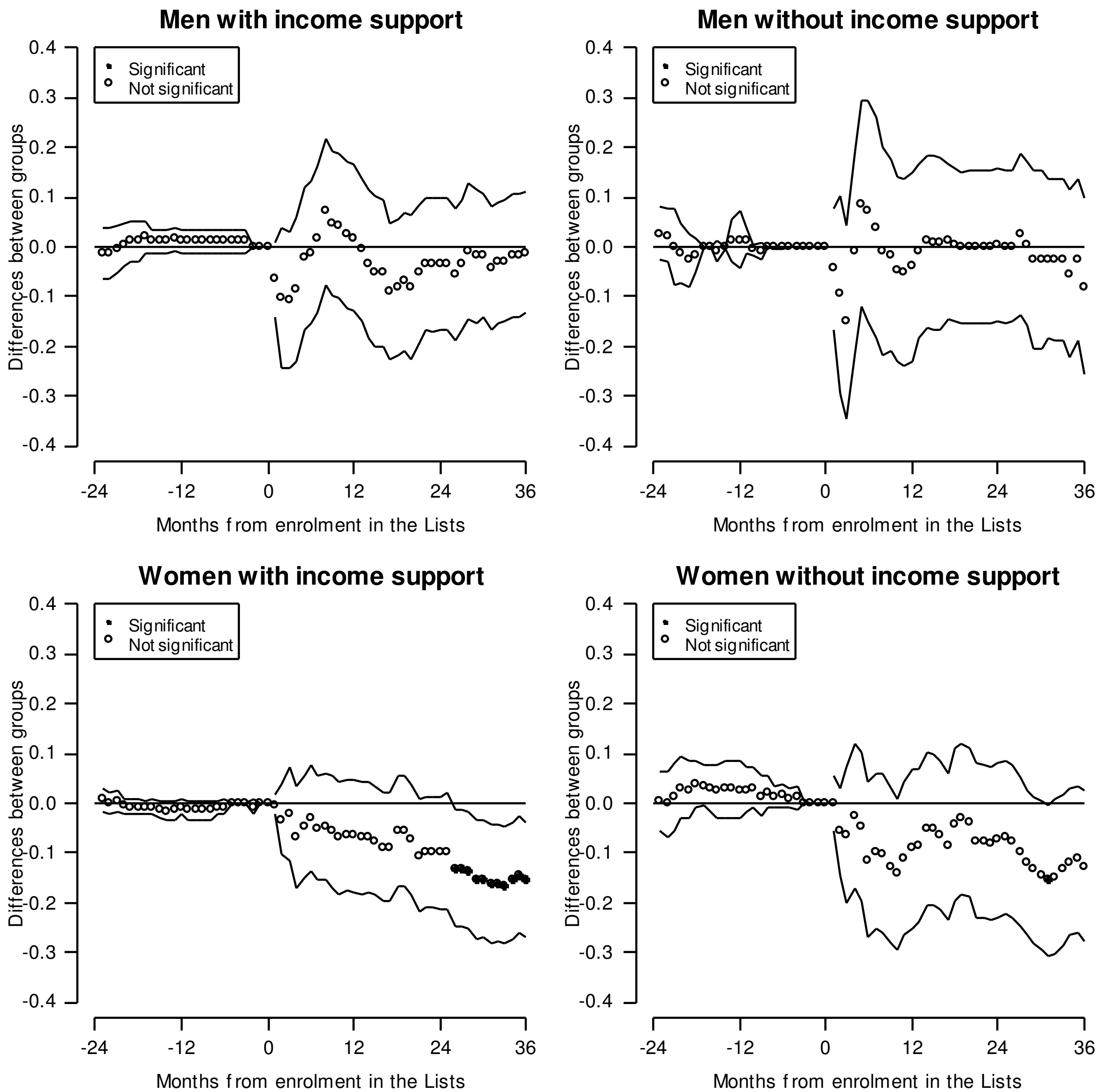

Figure 9: Estimates of the impact of the additional year. Employment rates during 24 months before enrolment in the Lists and 36 months after it, by gender, entitlement to income support and age group. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group (confidence intervals at 95% level).

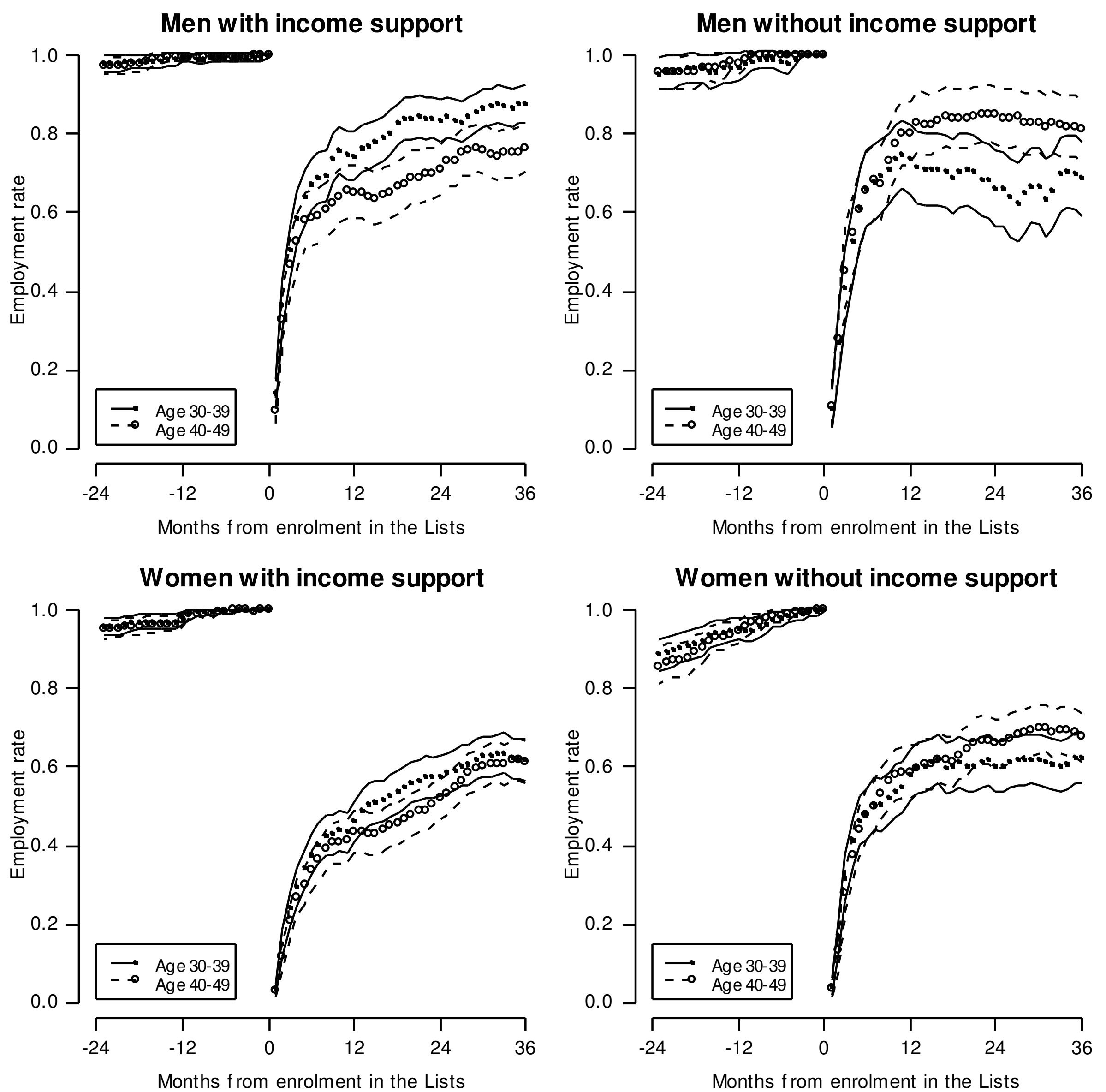

Figure 10: Estimates of the impact of the additional year. Differences between employment rates of exposed and not exposed workers, by gender and entitlement to income support. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group (confidence intervals at 95% level).

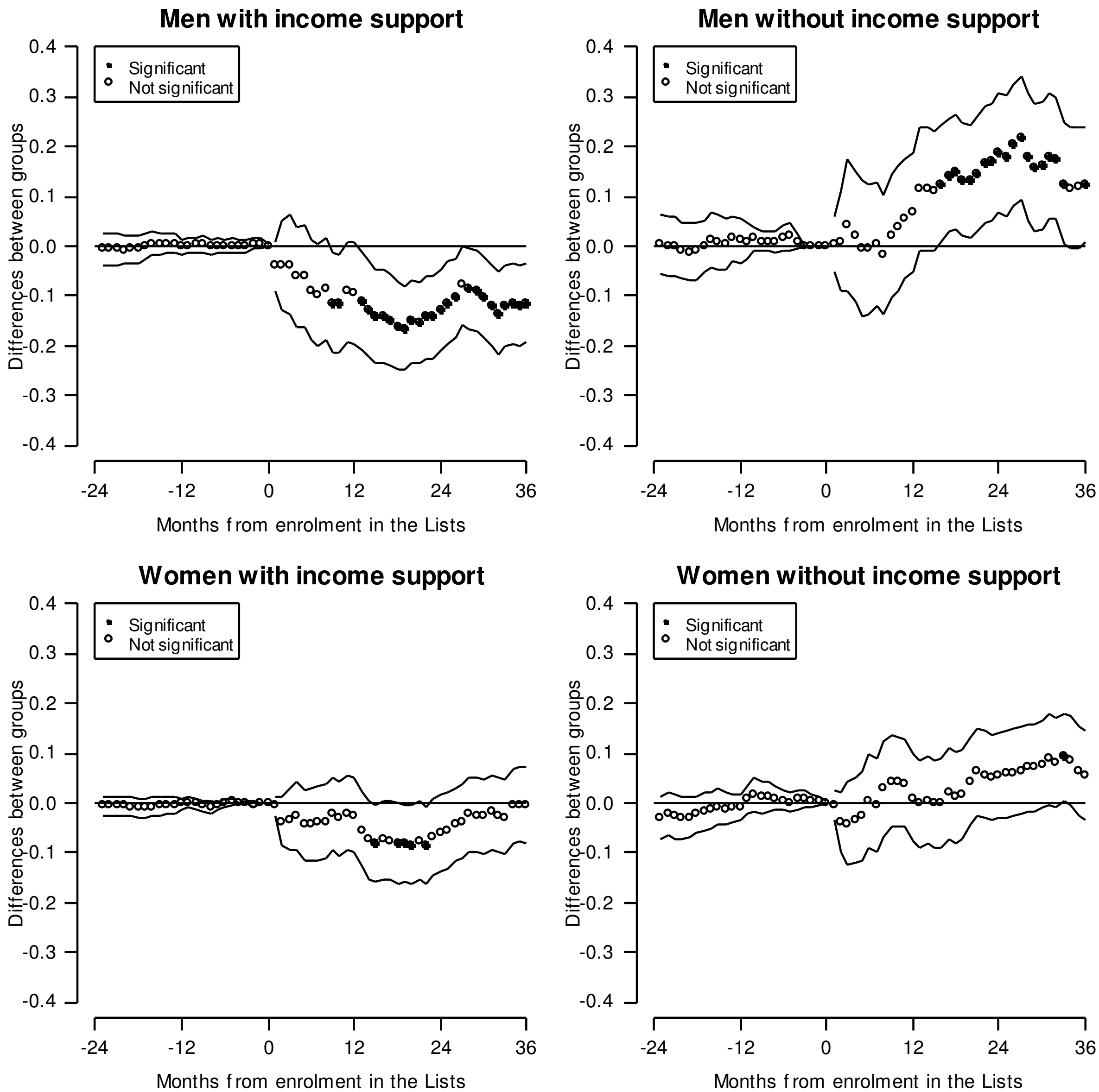

Figure 11: Estimates of the impact of the additional year. Kaplan-Meier estimates of survival functions related to transitions to employment, by gender, entitlement to income support and age group. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group (confidence intervals at 95% level).

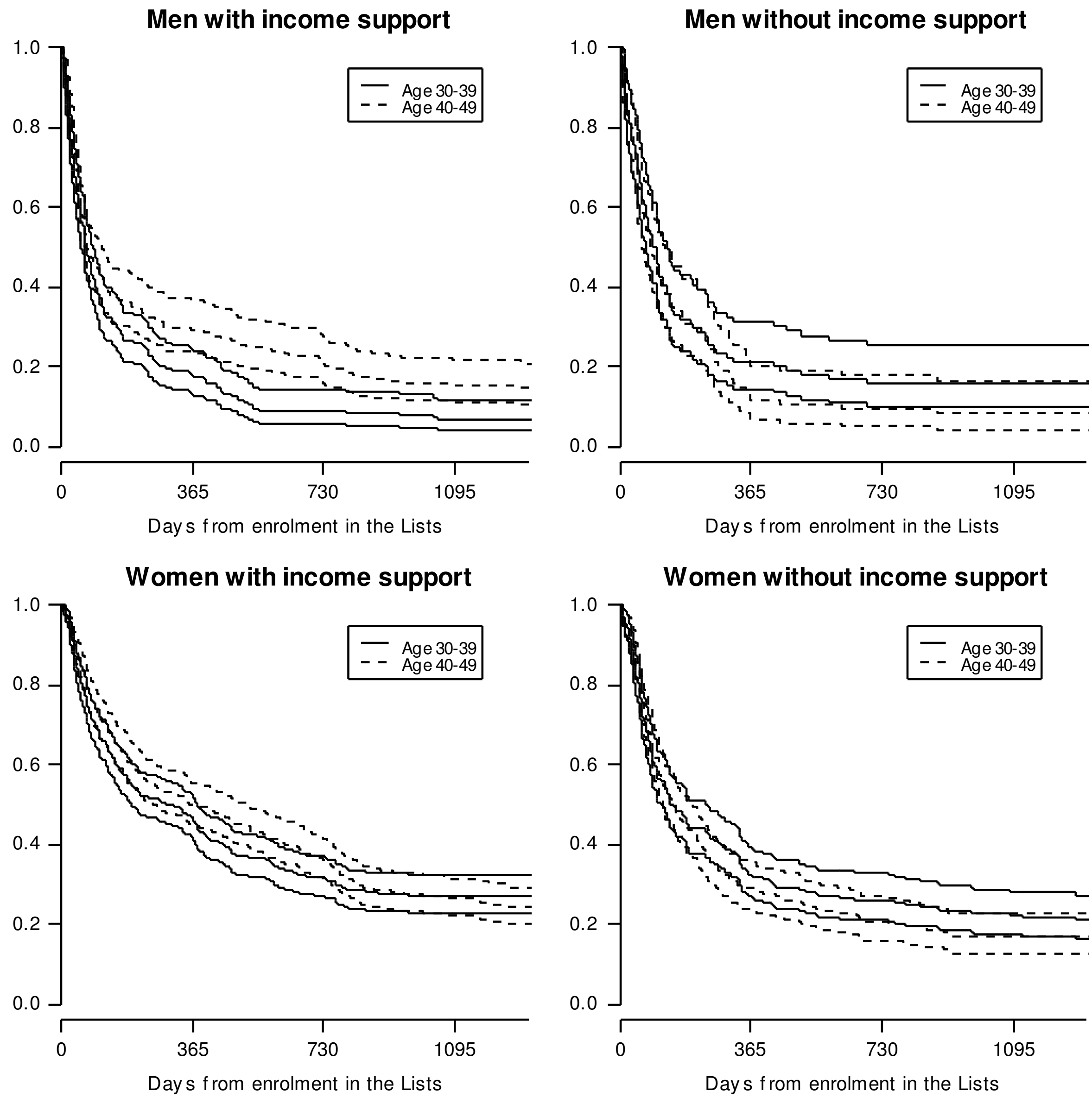

Figure 12: Estimates of the impact of the additional year. Smoothed risk functions related to transitions to employment, by gender, entitlement to income support and age group. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group.

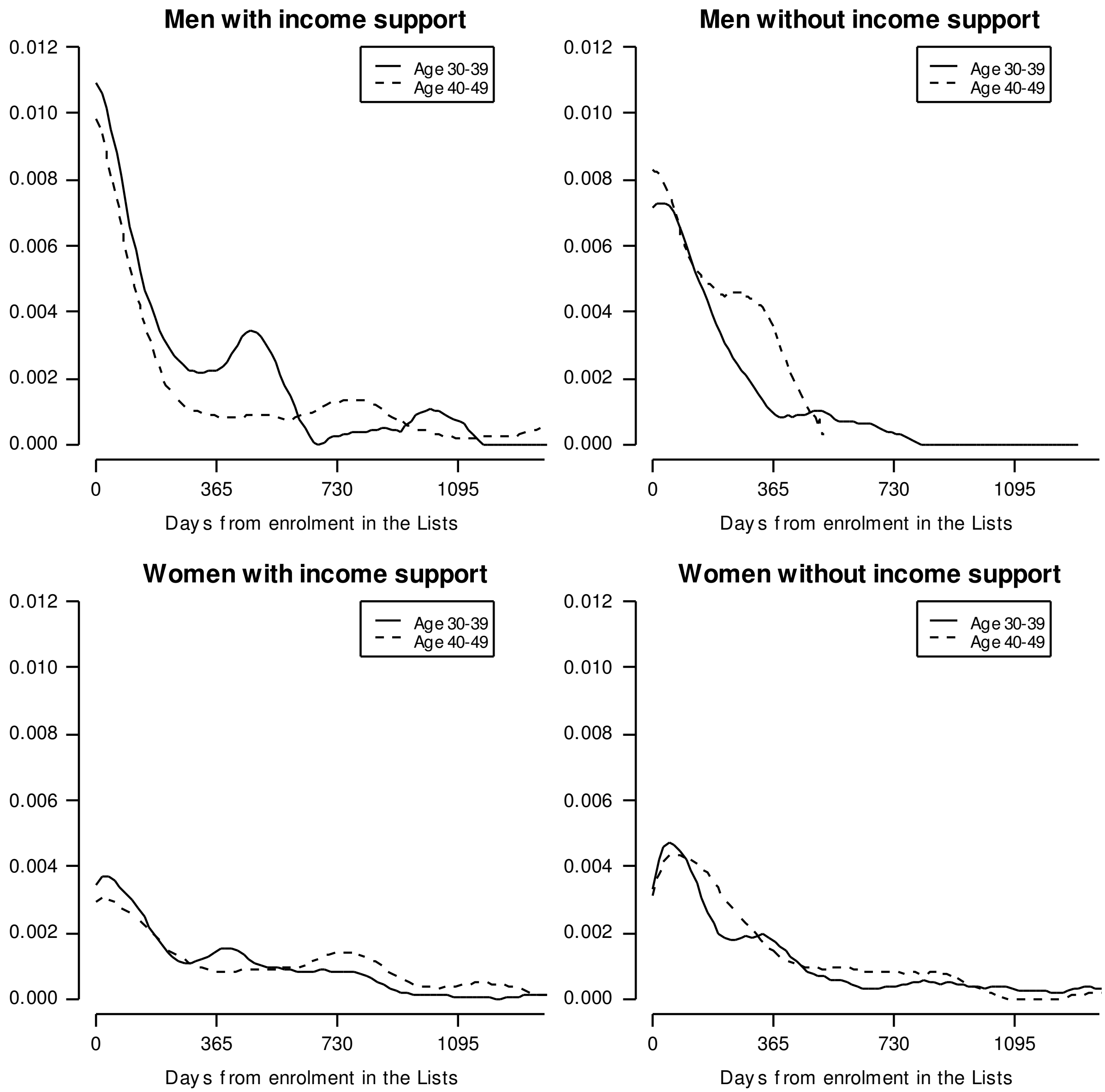

Figure 13: Estimates of the impact of the additional year. Kaplan-Meier estimates of survival functions related to transitions to permanent employment, by gender, entitlement to income support and age group. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group (confidence intervals at 95% level).

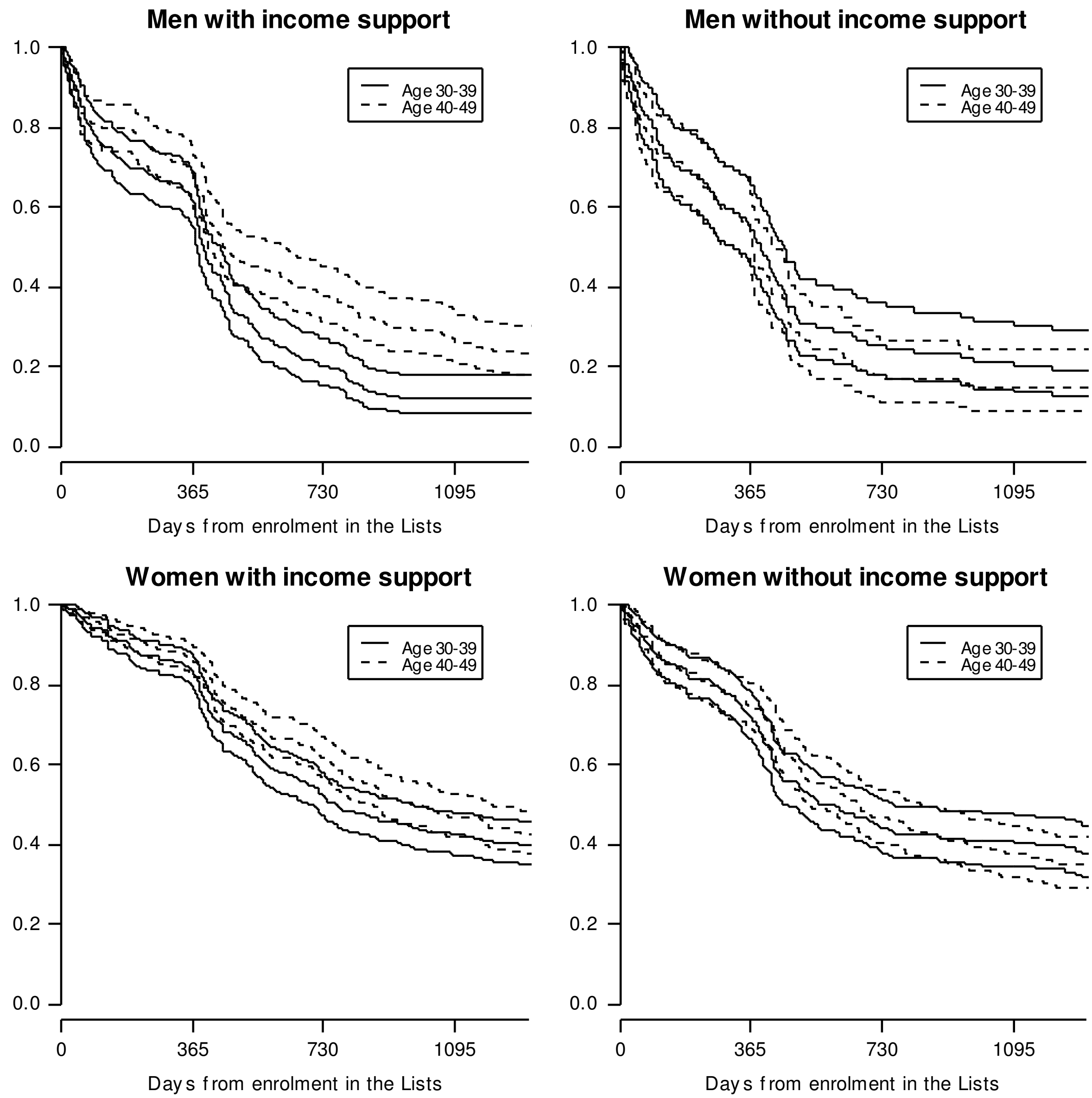

Figure 14: Estimates of the impact of the additional year. Smoothed risk functions related to transitions to permanent employment, by gender, entitlement to income support and age group. Matching by p-score workers exposed to the additional year (40-49 years) to not exposed workers in the 30-39 age group.

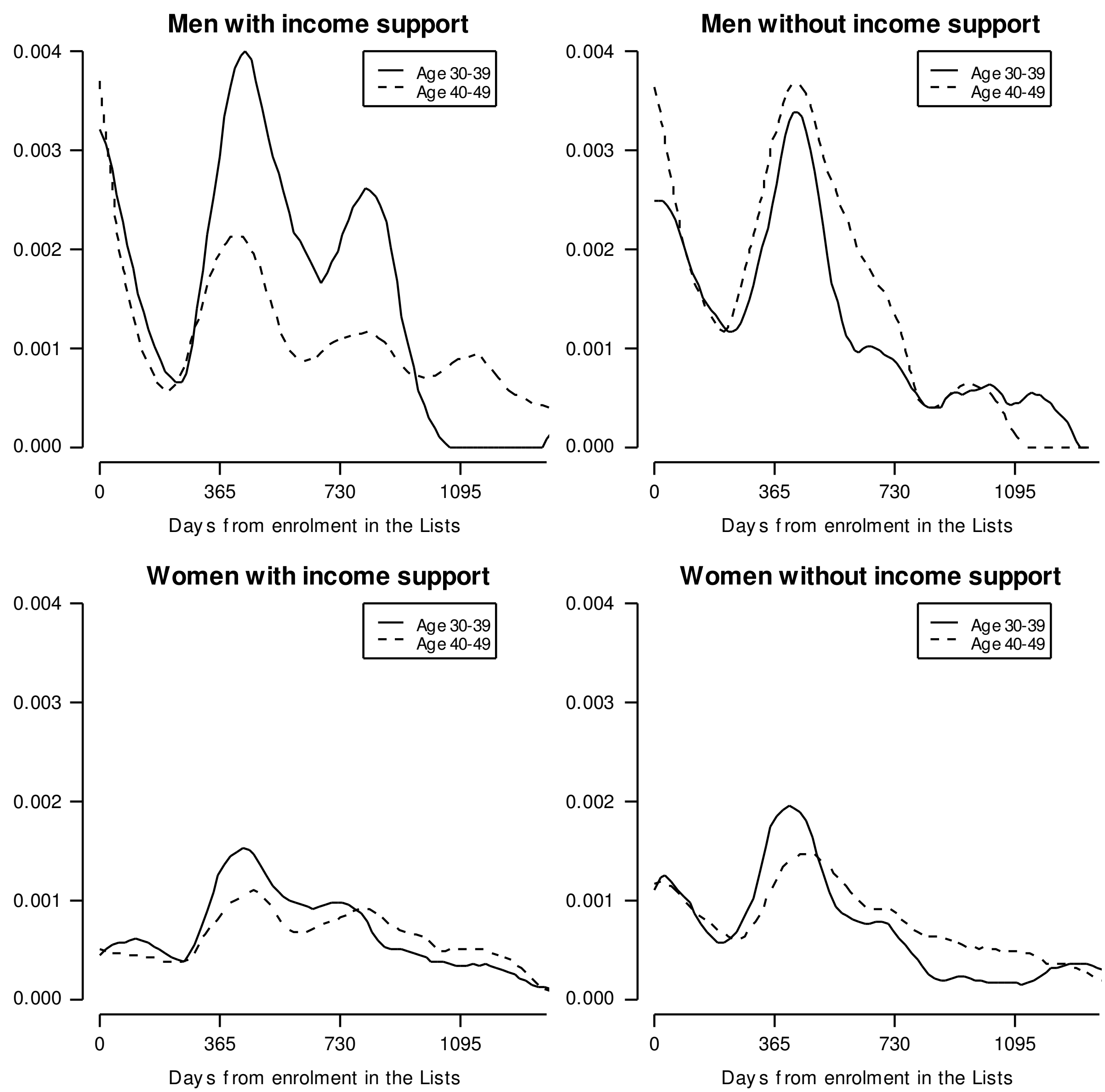

Figure 15: Estimating the impact of income support. Distribution of p-score in exposed (with income support) and not exposed (without income support) groups, by gender and age group.

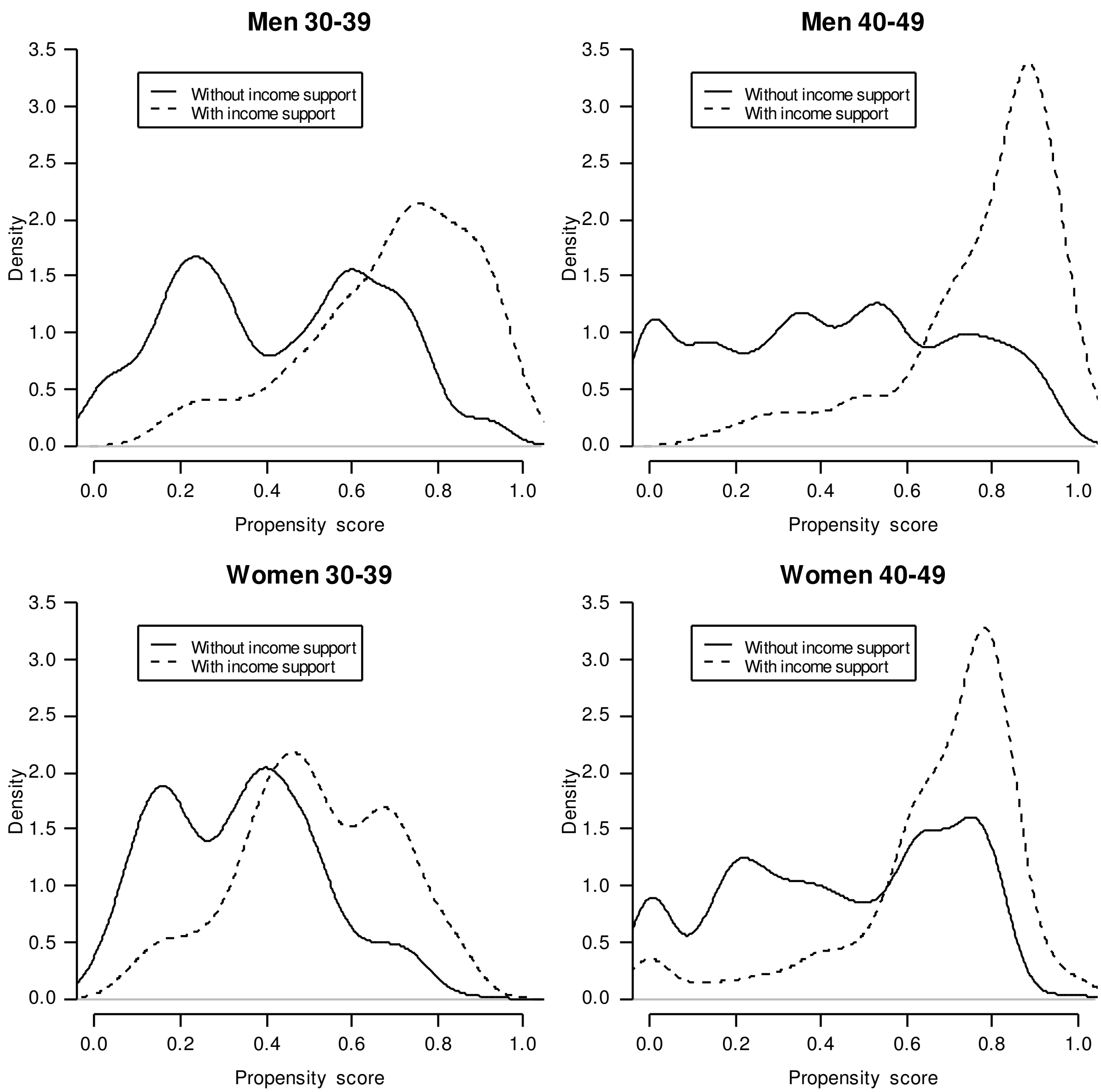

Figure 16: Estimates of the impact of income support. Employment rates during 24 months before enrolment in the Lists and 36 months after it, by gender, age group and entitlement to income support. Matching by p-score exposed and not exposed workers in the same age group (confidence intervals at 95% level).

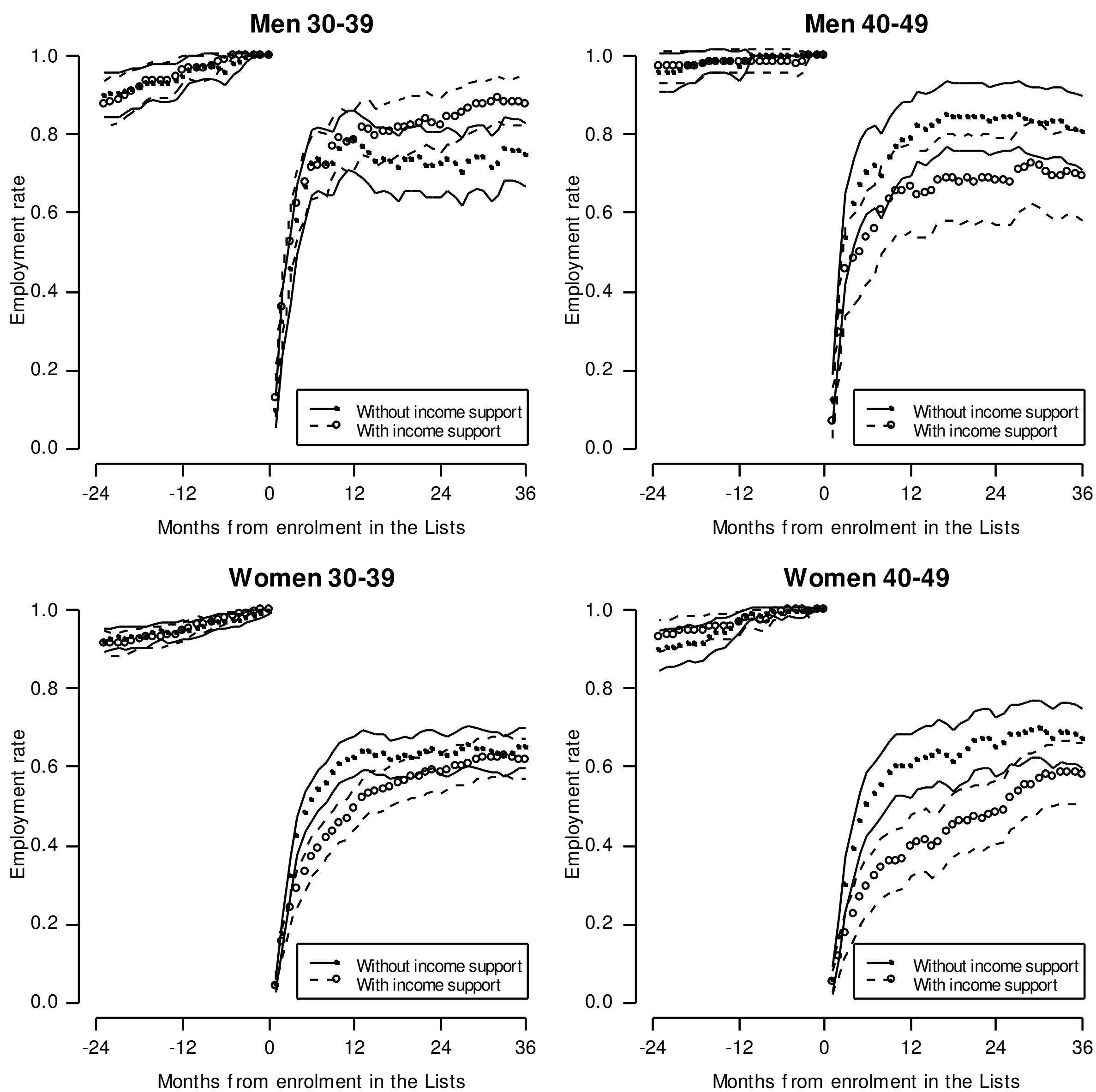

Figure 17: Estimates of the impact of income support. Differences between employment rates of exposed and not exposed workers, by gender and age group. Matching by p-score exposed and not exposed workers in the same age group (confidence intervals at 95% level).