ASSESSING THE EFFECTS OF THE "MOBILITY LISTS" PROGRAMME BY FLEXIBLE DURATION MODELS

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Abstract: The "Mobility Lists" programme handles collective redundancies, and combines income support to eligible dismissed employees with benefits to employers who hire them. Benefits vary according to dismissing firm size and are greater for older workers. We focus on the differential effects of programme treatments on the probability to move from unemployment into permanent jobs. We specify flexible duration models in order to estimate the profile of differential effects over time. Older workers, enjoying longer packages of benefits, have significantly lower chances of moving to employment. Differential effects vary with time and are higher when younger workers approach the exhaustion of benefits.

Keywords: Benefit transfers, Labour market policies, Programme evaluation, Regression discontinuity design, Survival analysis.

1. Introduction

During the last three decades, labour market programmes, targeted at specific groups of individuals, have become more and more important. Typically, these programmes are classified either as passive policies, aimed at providing income support to the unemployed, or active policies, focusing on training, employment subsidies, and direct job creation (see, among others, OECD, 1990, and Calmfors & Lang, 1995).

Several labour market programmes combine both passive and active features in different ways and degrees. A well-known example is the benefit transfer scheme proposed by Snower (1994). In essence, it stipulates that the unemployed receive income support (passive element) and may voluntarily transfer the benefit as a voucher to the employers who hire them (active element).

Some of these features are shared by a programme introduced in Italy in 1991. The programme is designed to handle collective redundancies in the labour market, and is known as "Mobility Lists" (*Liste di mobilità*), because eligible workers have to enrol in *ad hoc* lists managed by regional employment agencies. It combines income support for eligible dismissed employees with substantial benefits to the employers who hire them. Employers hiring a worker from the lists are entitled both to a temporary social security rebate and to a bonus equal to part of the unemployment benefit still to be paid to the worker. Benefits, for both workers and firms, vary mainly according to dismissing firm size and age at dismissal and, *ceteris paribus,* are greater for older workers.

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A crucial policy issue concerns the effects of the programme. Broadly speaking, the question is whether the programme increases the chances for participants to move into employment[1]. If we restrict attention to the differential effects within the programme itself – as is the case because of limitations in available information – the issue may be reformulated as follows: *ceteris paribus*, does allowing older workers to stay in the lists longer, and draw larger benefits, increase their hazard rate to employment relative to younger workers? Answers to these questions are far from being clearcut. With the standard job search model as a background (Mortensen & Pissarides, 1999), it is apparent that two contrasting forces are at work. On one hand, income support to eligible dismissed employees is likely to increase their reservation wage, then to extend the spell of unemployment. On the other hand, benefits to the employers who hire them are likely to increase the flow of job offers these workers receive, and to shorten the spell of unemployment. If we look at the issue in comparative terms, both these forces tend to be higher for older workers, because they are entitled to better treatment in the lists and carry a larger bonus to the hiring firm, with respect to younger workers. From a theoretical point of view, the sign of the net effect is *a priori* uncertain, and depends on which of the two effects prevails. It will also be inextricably mixed with the effect of the provisions of the programme about cuts in social security contributions. Thus, issues about the impact of the programme must be addressed empirically.

Previous analyses of the Mobility Lists programme include Belluardo (1997), Borzaga & Carpita (1997), Brunello & Miniaci (1997a, 1997b), Caroleo *et al.* (1997), Franceschini & Trivellato (1998), Caruso (2001). These studies refer to various Italian regions, and also differ somewhat in the models and methods used – albeit always within a duration analysis set-up. Thus, not surprisingly, they provide partly diversified evidence about the effects of the programme. Generally speaking, the effect of the passive element seems to prevail over the active one, especially in the South and for older workers.

This paper presents results for the Veneto region, for the period January 1995-March 1999. We use data from the administrative database handled by the Veneto Regional Employment Agency. While poor available information severely limited the evaluation exercise, by restricting our analyses to the period starting from January 1995 we were able to avoid some of the data deficiencies.

The paper focuses on the differential effects of various programme treatments – chiefly, the more generous packages of benefits for workers aged 40-49 years with respect to those for workers under 40 – on the probability for registered workers to move into a permanent job. Compared with previous empirical studies, our evaluation exercise presents some distinct features.

(a) We consider the entire pool of workers registered in the lists, which comprises both those entitled to income support and those not entitled to it, as they were dismissed by small firms. At the same time, we carry out analyses separately for the two groups of workers, in order to avoid potential selection bias problems.
(b) We pay very careful attention to the specification of flexible duration models, with the purpose of taking into account the dynamics of the effects of some essential features of the programme and of controlling for observed and unobserved heterogeneity. To this end, we carry out extensive specification searches.
(c) We estimate the profile of differential effects over time.

[1] It must be stressed that we focus here on evaluation of the impact of the programme on participants in it, and do not look at its broader role in terms of substitution effects, deadweight losses, or displacement costs. For a recent discussion of the likely general equilibrium effects of targeted wage subsidies, see Bell, Blundell & Van Reenen (1999).

This allows us to draw reasonably robust conclusions, which add to, qualify, and partly rectify evidence from previous studies.

The paper proceeds as follows. Section 2 provides essential information on the Mobility Lists programme. Section 3 presents the data and strategy for empirical analyses. Section 4 examines the distribution of duration of stay in the lists for registered workers. Section 5 outlines a preliminary, non-parametric survival analysis within a two-state setting: moving to permanent employment, or staying out of it. Section 6 proceeds in the same vein and presents the core results from a semi-parametric proportional hazards model. Concluding remarks are offered in Section 7.

2. The Mobility Lists programme: basic features

The Mobility Lists programme was introduced in August 1991 by law no. 223. It then underwent significant modifications and extensions, chiefly by laws nos. 236/1993 and 451/1994 (a clear presentation is made in Brunello & Miniaci, 1997a: 331-333, to which work we refer for details).

According to the programme, firms with more than 15 employees may dismiss redundant workers and place them, at a cost, in a local Mobility List. Workers dismissed by small firms – up to 15 employees – may also enter the lists, but for them registration is voluntary. The basic features of the programme may be summarised under three points: eligibility and maximum allowed period in the lists; benefits for enrolled workers; benefits to hiring firms.

Eligibility depends on tenure in the last job, which must be at least one year, and on the type of contract in the last job, which must be permanent. Enrolled workers may stay in the lists for periods which vary according to their age, measured at the time of dismissal. Maximum duration is: (i) one year for workers under 40; (ii) two years for workers between 40 and 49; (iii) 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.

Enrolled workers dismissed by a firm employing more than 15 workers are entitled to income support (*indennità di mobilità*). 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. Income support is equal to 80% of the previous pay during the first year, and is reduced to 80% of this first-year benefit during the second and third years[2]. 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”). It should be stressed that dismissing firm size is the key criterion for workers eligible for income support, and that this benefit is significant, at least within the Italian welfare system[3].

Remarkable benefits are also given to employers who hire enrolled workers.

[2] Note, however, that after the first year the benefit is not taxed, so that the reduction in take-home pay is much smaller.

[3] For the period covered by our analyses, recipients of unemployment benefits, other than those enrolled in the Mobility Lists, are unemployed individuals with previous work experience: they have a replacement ratio which is substantially lower, close to 30% of their previous pay. Unemployed individuals looking for their first job draw no benefits at all.

Firms which hire 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 – and in addition receive bonuses equal to 50% of the residual benefits that workers would have received had they remained in the lists. 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.

How do we expect such a programme to work? It is apparent that results crucially depend on the way the programme is designed. Some of the operational features outlined above make the picture rather blurred, in that they act in potentially conflicting directions. Let us consider the role of the bonus for the hiring firm. Taken *per se*, this bonus is a benefit transfer from the worker to the employer which is granted whenever a new permanent match is formed, basically as proposed by the benefit transfer scheme advocated by Snower (1994). But this is just one piece of the programme. In order to assess its role, we must take into account other elements: (i) the importance of the other benefit for the hiring firm, *i.e.*, reduction in social security contributions; (ii) the convenience for the potential employer to combine a sequence of contracts, first temporary and then permanent; (iii) the contrasting effect of provisions for "long mobility". One might speculate that the overall result of these additional features of the programme would be to attenuate notably, and possibly to overcome, the active policy element implied by the benefit transfer scheme.

Some examples given in Table 1 help to illustrate the point. The first column refers to the annual cost of a new permanent hire from the market. To this benchmark, we compare the costs of hiring from the lists: (i) a worker entitled to income support and aged 39 or 40 years respectively; (ii) under four different hiring strategies, which result from the combination of two criteria: type of contract, a permanent hire contrasted with a first temporary-then permanent contract; time of hiring, whether immediately after registration of the worker in the lists (*i.e.*, enjoying the entire potential bonus) or the last day the worker is still enrolled (*i.e.*, no bonus).

Table 1 about here

The best strategy for a potential employer is to hire a worker aged 40 the first day s/he is enrolled in the lists, on a temporary one-year contract which is then switched to a permanent one. Overall, the employer will save 14,183 Euros over two years - on average, 42.6% of the total labour cost. Note that the dominant part of the savings – 9,264 Euros (i.e., 65%) is made up of reductions in social security contributions.

In relative terms, permanently hiring a worker entitled to income support immediately after s/he enters the lists has definite, but not dramatic advantages over the strategy of hiring a worker with long seniority in the lists and almost no bonus left (or a worker not entitled to income support) with a first temporary-then permanent contract. The additional saving for the potential employer is 28% (2,602 Euros) if s/he hires a worker aged 40, and falls to a negligible 4.5% (416 Euros) if the worker to be hired is aged 39. On the other hand, it must be considered that hiring on a first temporary-then permanent basis may be attractive for the firm, because it allows more flexibility as well as the opportunity to look for a good match.

To sum up, in the Mobility Lists programme, reductions in social security contributions are the dominant part of the advantages for potential employers, much higher than the bonus carried by some workers – a point overlooked in many of the previous studies. Moreover, they are not conditional upon dismissing firm size, and for the hiring firm they do not vary with worker's age. In this respect, a sound strategy for the firm might consist of hiring workers from the lists on a first temporary-then permanent basis, irrespective of their age and possibly of the fact that they carry a bonus[4].

Let us now consider the other component of the programme (for workers entitled to it): income support. This gives a relative advantage to older workers over younger ones, because they qualify for a longer period of benefits and because potential employers can enjoy larger benefit transfers. Whether this advantage ends up by improving their chances of moving to permanent job, however, remains dubious, because of its already-mentioned contrasting effects on the reservation wage (which should increase, especially if dismissed workers have easy access to the underground labour market) and on the flow of job offers. Thus, the sign of the net effect of a longer period of income support is unknown, and this source of uncertainty is added to the previous ones.

3. Data and strategy for evaluation

The Mobility Lists programme is managed by regional employment agencies[5], which are also responsible for data collection. There is no common format for collecting individual data on the programme across the country; therefore, there is no consistent national database available in Italy. We use data from the administrative records of the Veneto Regional Employment Agency.

The Veneto region is a large, relatively well-developed region of North-Eastern Italy. With more than 4.4 million inhabitants, it makes up 7.7% of the Italian population. The Veneto has an employment rate close to 42%, an unemployment rate around 5.2%, and a *per capita* GNP some 15-20% higher than the national average. These traits of comparatively low unemployment and high economic activity characterise the Veneto as similar to the rest of Northern Italy, but far from representative of the much less developed South.

The regional administrative database extends from the enactment of the Mobility Lists law up to now. However, we restrict our analysis to the period starting from January 1995. Indeed, the quality of the administrative data improved substantially after 1995, for three reasons: appreciable improvements in the process of data handling itself; changes in legislation[6]; careful revision of the database recently carried out by the Regional

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[4] Note, however, that the picture becomes less clear if we consider that older workers have an advantage in terms of the maximum total period of temporary hires with social security rebates, which may also be exploited by firms. *Ceteris paribus*, this feature of the programme may reduce the chance for old workers to move to permanent job, with respect to younger workers. Both workers and firms may be interested in extending the period of temporary hires, via a sequence of contracts with different firms/workers respectively (the former in order to look for a good match; the latter for the very same reason and, additionally, in strict terms of savings).

[5] By legislative decree no. 469/1997, the functions of the State with respect to the programme (and several other labour market domains) now devolve upon the Regions. However, in general – and specifically in the Veneto region – no appreciable novelties were introduced into the design and management of the programme.

[6] One problem with the first years of operation of the programme was a phenomenon that Brunello & Miniaci (1997: 334) called "collective hires", that is to say "*groups of workers who have been dismissed by a firm*

Employment Agency. Thus, it is reasonable to argue that our data-set is appreciably better than those used in previous studies. It has information on all workers who registered in the regional lists from January 1 1995 to March 31 1999.

We follow each worker over that time interval and may observe one of the following events: (i) exit into a permanent job; (ii) cancellation from the lists because the allotted time has expired. If we do not observe either of these two events, this means that (iii) the worker is still enrolled in the lists. Thus, we can distinguish registered workers by their current status: (permanently) hired, cancelled, and (still) enrolled.

For each worker, we have data on the following variables: gender, age, industry (of dismissing firm), occupation (in dismissing firm), education, province of residence, entitlement to income support, day of enrolment and length of stay in the lists, current status.

Limitations on the evaluation exercise[7] arising from data availability are quite evident. Mobility Lists is a universal programme, offered to all workers who meet eligibility requirements. This makes identification of a suitable comparison group not only problematic but even operationally unfeasible, because the data-set refers only to enrolled workers.

As a consequence, programme evaluation must be restricted to differential effects among enrolled workers. However, even within this narrower context, things are far from easy. First, in principle a threat could come from self-selection into the programme, because our data do not include workers dismissed by small firms who decide not to register. Fortunately, in practice this problem turns out to be irrelevant: informed evidence by officials of the Veneto Regional Employment Agency indicates that, basically, all workers dismissed by small firms do register in the lists. However, it must be taken into account that firm size, type of contract and job tenure are probably correlated with each other and with individual unobserved characteristics, and they jointly affect entitlement to or exclusion from the programme and allocation to different programme treatments. A sensible way to deal with this issue will be to carry out analyses separately for workers dismissed by firms with more than 15 employees and up to 15 employees respectively.

Second, information on the individual characteristics for enrolled workers is rather poor, and we have no information on local labour demand conditions. What is more, we do not have enough information in the data-set to identify spells in temporary jobs: we only know that some of these spells did exist, from the fact that the length of enrolment in the lists was longer than that allotted according to age. This prevents us from discriminating between periods spent in the lists as temporarily employed and as unemployed (drawing income support, if so entitled). Those periods are perforce collapsed into a spell in the initial, common state – being enrolled in the lists.

For all these reasons, we focus here on differential effects among registered workers by exploiting the variability of benefits according to age, conditionally upon entitlement or lack of entitlement to income support. The differential effects of the programme must also be assessed with respect to only one outcome dimension: transition to a permanent job. Lastly, we must look at the differential impact of the treatments taken as a whole, *i.e.*, the various packages of benefits, without trying to disentangle their

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and hired as a group *by another firm on the same day*", largely as a form of fraud. These collective hires were prohibited by law no. 451/1994.

[7] On methodological issues of evaluation of labour market programmes, see the thorough review by Heckman, Lalonde & Smith (1999). Specifically on duration modelling in this context, see Meyer (1990).

components – benefit transfers, social security rebates, and extension periods due to temporary contracts.

Starting from January 1995, we have a total of 43,734 workers registered in the regional lists. However, for some of them, data were missing for variables such as education and occupation. Descriptive analyses, not reported here for the sake of brevity, show that this lack of information is either irrelevant or essentially distributed at random. Thus, we decided to drop the variable education[8] and to confine our analyses to the sub-set presenting complete data on all the remaining variables, consisting of 42,061 workers.

Their distribution by gender, entitlement to income support and age group is given in Table 2. Women prevail (59%); a large proportion of them (64%) come from firms with less than 15 employees, while the opposite holds for men (56% have income support). Looking at the age groups, as defined by the age limits which mark differences in allotted duration in the lists, we see a remarkable concentration of young workers (66% are under 40). Polarisation in the age group under 40 is more pronounced among women, and for both men and women among workers coming from small firms.

Tables 2-3 about here

Turning to workers' current status in the lists, Table 3 shows that permanent hires are about 26% of the whole sample. Compared with previous studies, this is slightly higher than the average performance of Mobility Lists in Northern Italy and at least four times higher than in the South. Women have both less permanent hires than men (21 *vs.* 32%) and more cancellations due to expiry of the entitlement period in the lists (44 *vs.* 34%). As regards age, the relative frequency of permanent hires shows small differences among the first two groups, whereas it declines sharply for the group over 49 years. However, for meaningful interpretation of this evidence, we should consider that older workers are allowed to stay longer in the lists: in other words, the fact that a remarkable proportion of workers older than 49 are still enrolled is partly due to the very provisions of the programme.

4. Duration of stay in the lists

Figure 1a presents the distribution of the length of stay in the lists, for both the whole pool of workers and the three age groups to which the maximum allotted duration in the lists is related. The most remarkable features are the spike at the one-year duration and other spikes at multiples of the year. Specifically:

(a) the first age group shows a tall spike at one year and a smaller but still important spike at two years;
(b) in the second age group, the spike at two years prevails, and a smaller spike corresponds to the three-year duration;
(c) the third age group has its tallest spike at three years, and does not exhibit any spike around four years (as one would have expected); instead, it has a non-negligible spike at two years, probably due to errors[9].

[8] Education is highly correlated with industry and occupation and, after controlling for these characteristics, it turns out not to have any significant effect on the hazard to employment.

[9] As will be seen in section 6, the age group over 49 years is excluded from the semi-parametric survival analysis. Thus, the poor quality of these data do not affect the main results.

Figure 1 about here

There are two main reasons for these spikes: exhaustion of benefits, both for workers (chiefly income support, if they are entitled to it) and potential employers (cuts in social security contributions, which are conditional upon workers still being registered); opportunity for employers to hire workers immediately with a temporary one-year contract. Although available information does not allow us to distinguish between these two factors, indirect evidence comes from Figure 1b, which presents the distribution of durations for hired workers. It clearly shows that spikes for hired workers are considerably lower, and that the most noticeable spike is systematically at one year for all age groups. In addition, comparison of Figures 1a and 1b suggests that, apart from the spikes, the distribution of durations for the entire pool of registered workers and for hired workers is largely similar. Therefore, a good proportion of the spikes observed in Figure 1a should be associated with cancelled workers - which is in fact neatly confirmed by their distributions of duration of stay in the lists (Figure 1c).

Some broad conclusions may be drawn. (i) Most of the spells ending with cancellation from the lists reach their allotted maximum length, which means that they are not interrupted by temporary jobs. Instead, temporary contracts appear to be mostly concentrated over a one-year period. (ii) The distribution of hired workers has appreciably higher frequencies at durations around the beginning of the stay and just after the one-year peak. This suggests that a fairly large proportion of hires take place quite soon after workers have registered in the lists, either on a permanent or first temporary-then permanent basis: this is *prima facie* evidence that the programme is working. This evidence is strengthened by noting (Figure 1b) that these features of the distribution are largely found across all the age groups of hired workers, with only small peaks remaining at the two and three-year durations.

5. Preliminary evidence from non-parametric survival analysis

We now analyse transitions to permanent jobs by workers registered in the lists. The set-up for our analyses is given by single-spell one-state transitions from enrolment in the lists to permanent employment, and we treat: (i) permanent hires as completed spells; (ii) cancelled workers and enrolled workers as censored spells.

We focus on the hazard function $h(t)=g(t)/S(t)$ or, equivalently, on the survival function $S(t)=1-G(t)$, where $g(t)$ and $G(t)$ denote the density function and the distribution function of duration in the unemployment state, respectively.

We start with non-parametric survival analysis, comparing the Kaplan-Meier estimates of the survival functions for various subsets of workers. Revealing evidence comes from Figure 2, where we control for gender, entitlement to income support, and age.

Figure 2 about here

Some features are clearly discernible:

(a) The declining shape of the functions has a clear step pattern, with steps at durations corresponding exactly to one, two and three years. This is consistent with the evidence on heaping documented by the preceding analysis, and supports the opinion that the programme provisions mentioned above do matter. Moreover, the step pattern consistently affects all groups.

(b) Survival functions by age group are different, with a systematically smaller hazard to employment for the older age group[10]. Indeed, for workers over 49, particularly for those with income support, the survival functions are remarkably flat, which suggests that most of them are simply transiting from employment to retirement.

(c) Two effects begin to become visible: a true age effect, and the treatment effect. The pattern is somewhat different between men and women. Men move more rapidly to permanent jobs than women, under both programme treatments defined by dismissing firm size. While the survival functions for women do not vary remarkably by entitlement to income support, this is not the case for men: for male workers without income support, the survival functions of the first two age groups are quite close to one another and intersect at around one year[11]. Focusing on workers with income support, both younger men and women transit more rapidly to employment than their colleagues aged 40-49, thus indicating the possible negative effect of the longer benefits enjoyed by older workers.

These statements, however, should be regarded as hypotheses rather than conclusions. At least, better control of heterogeneity is necessary in order to ascertain net effects.

6. Assessment of differential effects of the programme by means of flexible proportional hazards modelling

Entering the core of the evaluation exercise, we decided to focus attention on the first two age groups, excluding workers aged 50 years or over from further analyses. The main reason is that this age group largely comprises workers in “long mobility”, who tend to use the provisions of the programme as a bridge to retirement. Our final sample is thus reduced to 36,405 workers.

Our purpose was to estimate the differential effects of the more generous programme packages for workers aged 40-49 years with respect to those for workers under 40, separately for workers entitled or not entitled to income support. In the light of the dynamic features of the programme treatments (maximum allotted period in the lists; fading out of income support and thus of bonuses to potential employers, with time; extension periods for temporary contracts), we must look at the profile of differential effects over time. Thus, other things being equal, the information of interest is the differences in the hazard functions to permanent employment between the relevant pairs of age groups. We refer to these differences as the “differential treatment effect”.

Clearly, the *ceteris paribus* condition is crucial. Note first that, conditionally upon dismissing firm size, the allocation of workers to one of the two programme treatments only depends on age at dismissal. Thus, the assignment process fits the sharp regression discontinuity design, in which compliance with assignment is perfect (Trochim, 1984)[12]. In

[10] For workers under 40, the observed non-zero values of the survival function immediately after 730 days are clearly due to errors or delays in cancellation from the lists, because spells lasting more than two years are not permitted for such workers. Note that this problem is irrelevant in the following semi-parametric analyses, as spells are censored after two years.

[11] This is the only case in which, based on the log-rank test, we do not reject the hypothesis of equality of survival functions, at the 1% level.

[12] Allocation to one of the two programme packages being based on age at dismissal, it is very probable that

principle, one could exploit this feature in order to identify and estimate, entirely non-parametrically, the differential treatment effect of the programme package for older workers, the intuition being that, for each worker in the neighbourhood of 40 years, the programme treatment to which s/he is assigned and the potential outcome are conditionally independent. Unfortunately, the sample size of registered workers in the neighbourhood of 40 years is rather small, and the percentage of transitions to permanent employment is quite modest[13]. As a consequence, the results are very unstable and hardly interpretable.

We therefore have to use the whole age group samples. In this case, however, we need to control appropriately for heterogeneity[14]. Indeed, by extending the age bandwidth, workers assigned to the various programme treatments may differ for a variety of factors – individual characteristics, working history, and so on, which probably affect their chances of being enrolled in the lists. In this context, we pay attention to a quite flexible semi-parametric model specification, within a proportional hazards set-up, as regards both baseline hazard and controlling for observed and unobserved heterogeneity. Specifically:

(a) Previous analysis on the distribution of durations strongly suggests a non-monotonic shape for the baseline hazard function, flexible enough to take into account spikes around years. A piecewise exponential, with bandwidths designed to allow for that pattern, turned out to be appropriate.
(b) As regards observed heterogeneity, we used the entire set of variables at our disposal. These are listed in the Appendix, together with their distribution in the sample, and are self-explanatory[15]. The only comment is perhaps on items ‘Province’ of residence and ‘Year of enrolment’ in the lists, which are intended to capture, admittedly rudimentarily, local labour demand conditions and the cycle, respectively.
(c) Unobserved heterogeneity is dealt with by a non-parametric mass point specification, according to Heckman & Singer (1984)[16].

We started our analyses with some specification searches, aimed at several interconnected purposes: (i) to identify groups that demand to be analysed separately,

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actual status and status determined on the basis of age coincide. For a general discussion on conditions for identification of programme impact in a regression discontinuity design, see Hahn, Todd & van der Klaaw (1999).

[13] Taking a bandwidth of one year, *i.e.*, considering workers aged 39 and 40 years respectively, the sample size varies from 188 (men aged 39 without income support) to a maximum of 327 (women aged 40 without income support). The proportion of transitions to permanent employment is around 30% for men and 20% for women. Instead, with larger bandwidths, age effects are clearly present and regression discontinuity design assumptions become untenable.

[14] In principle, the regression discontinuity design requires us to control only for age at dismissal, in order to estimate correctly the differential treatment effect in the neighbourhood of 40 years. Note, however, that what we want to estimate is the profile of differential effects over time. Thus, we should consider the risk set at each duration of stay in the lists and estimate the difference in the conditional probability of transition to permanent employment. But here some difficulties arise. On one hand, the natural choice for the time unit would be in days, but the number of observed transitions is too small – or indeed nil – for most durations. On the other hand, by choosing broader duration intervals we obtain censored spells within those intervals, and consequently missing values for the binary indicator function of transitions. Because of these problems, we abandoned the regression discontinuity design set-up in favour of the flexible duration modelling approach presented in the main text.

[15] All explanatory or stratification variables we consider are dummy variables, with the only exception of age (on which, see further in this section).

[16] Identification is ensured by conventional normalisations on baseline hazard and observed heterogeneity, respectively, and by the assumption of finite values for the parameters of the discrete distribution of unobservables (Heckman & Taber, 1994).

because they are characterised by different parametric structures; (ii) to find an appropriate representation for unobserved heterogeneity; (iii) to find a flexible, yet parsimonious way of distinguishing the differential treatment effect from a true age effect. We only outline the conclusions here[17].

(a) As pointed out above, analysis must be carried out separately for the two groups of workers with/without income support. Extensive testing on nested models provided convincing evidence that the analysis should also be stratified according to gender[18]. Lastly, as a starting-point for each of the four sub-populations identified by entitlement to income support and gender, we specified entirely different models for the two age groups, < 40 and 40-49 years respectively, *i.e.*, for the two programme treatments.

(b) As regards the appropriate number of mass points for characterising unobserved heterogeneity, the models are non-nested and direct use of likelihood ratio tests is not possible. However, if different selection criteria are applied (including the Akaike criterion), binomial heterogeneity is clearly adequate for all groups[19].

(c) The last step deserves more attention, because it is directly related to our main interest. As we are dealing with the whole age group samples, and as the differential treatment effect depends on age, controlling for the age effect is more difficult than for the other individual characteristics. At the same time, such controlling is essential in order to disentangle the differential treatment effect from the true age effect. After some searches, we found it convenient to treat 'Age' (at dismissal) as a continuous variable and represent it by a polynomial of suitable order in age, with order varying across the various groups.

Moving on from these specifications, we proceeded to test more restricted models, essentially with the aim of ascertaining if, within each of the four sub-populations, we would end up with a single model for the pool of workers in the two programme treatments - apart from a differential treatment effect on the baseline hazard function. Table 4 presents the main results for the sub-population of women without income support, taken as an illustrative example. Model A is the more general one, allowing for entirely different parametric structures for the two age groups considered. A sequence of nested models, from B to F, characterised by growing restrictions, were then tested by means of conventional likelihood ratio tests (LRT). Model C, which amounts to assuming equality restrictions on all the heterogeneity parameters (*i.e.*, a single parametric model for the workers in the two programme treatments, apart from different baseline hazard functions), was not rejected. This was also the case for model D, which restricts the order of the polynomial in age to two, and further for model E, which describes the differential treatment effect over time parsimoniously, by means of four parameters.

Table 4 about here

Thus, our final model takes on the following mixed proportional hazards specification (Lancaster, 1990):

[17] Results on the full set of specification searches are available from the authors upon request. Some of them are given in Paggiaro & Trivellato (2000).

[18] No similar evidence emerged for worker's 'Occupation', which may reasonably be captured by a dummy variable within a proportional hazards specification for each of the final models.

[19] Neglecting unobserved heterogeneity produced the typical effect of a shift towards (spurious) negative duration dependence, whereas the use of a greater number of mass points did not improve model fitting.

$$h(t,x,z;\alpha,\beta) = h_0(t;\alpha,z)\exp(x'\beta)\varphi(\vartheta,p),$$

where: parameter vector α defines the shape of the piecewise exponential baseline hazard function, and depends on z, a dummy variable indexing workers according to age – up to 39, or 40-49; β is the vector of parameters associated with explanatory variables x; $\varphi(\vartheta,p)$ denotes binomial unobserved heterogeneity[20].

The differential treatment effect was captured by the interactions between indexing variable z and the shape of the baseline function. More specifically, for each class of duration, we estimated the difference between (the log of) the hazard of workers aged 40-49 years and that of workers aged <40, taken as the reference group[21].

Detailed evidence on the differential treatment effect parameters is given in Table 5, for specifications from D to F. Clearly, model D is quite flexible, in that it provides a separate effect for each class of duration. Model E restricts the pattern of the differential treatment effect by specifying only four constant effects: for the first and second years – excluding the last week of each year – and for the last week of each of the two years, respectively. Based on the LRT, these restrictions turned out to be quite plausible (see Table. 4, row E). Model F is the most restricted: it assumes no variation of the differential treatment effect over time, and thus collapses it to an average effect over the entire spell in the lists; however, it is clearly rejected (see Table 4, row F). In short, model E was our final model.

Table 5 about here

Note that, for women without income support, model F would have driven us to the wrong conclusion of a negative average differential effect of the more generous programme treatment for workers aged 40-49. Instead, model E clearly showed that the differential effect is significant only during the second year. It is worth adding that the specifications used in previous works (see chiefly Brunello & Miniaci, 1997, and Paggiaro & Trivellato, 2000) closely resemble model F. Therefore, they miss the time profile of the differential treatment effect, possibly with undesirable consequences on interpretation and policy redesign prescriptions.

Similar results were also found for the other three sub-populations, apart from a different order of the polynomials with age. The estimates of the final models for the four groups are listed in Table 6 and graphs of the estimated baseline hazard functions in Figure 3. Some findings emerge quite clearly.

Table 6 and Figure 3 about here

Broadly speaking, the shape of the baseline hazard functions shows negative duration dependence within both the first and second years (the only exception is the first year for women without income support, in which the function is flat), whereas the hazard rates to employment go up considerably during the last few days of each year. The spikes

[20] In the context of labour market evaluation studies, a similar model, with different baselines for treatment and control group, was used by Jensen, Svarer Nielsen & Rosholm (1999).

[21] Note that, due to the very provisions of the programme, it is not possible to observe young workers with duration in the lists longer than two years. We thus decided to restrict our analysis to the first two years by censoring longer periods.

at 360-366 and 725-732 days vary somewhat across sub-populations and programme treatments, but they are in any case pronounced, hinting at the crucial role of the maximum period enrolled workers can stay in the lists, which is the deadline for hires with social security rebate.

Next, let us consider the effect of the explanatory variables. Two pieces of evidence deserve attention. First, the white/blue-collar distinction is significant for male workers only: for them, being a white-collar worker systematically reduces the hazard to employment. Second, the parameters associated with the other variables ('Industry', 'Province' and 'Year of enrolment') are almost always significant, thus highlighting the fact that industry-specific, local demand and cyclical factors do matter.

Focusing on our topic of paramount interest, differential treatment effects, we look first at effects among workers entitled to income support. In this case, the most favourable programme treatment for older workers comprises – and, indeed, largely consists of – the higher benefit transfer component (the fact that they can draw income support longer and transfer a larger bonus to potential employers). It appears to have a significant negative effect on the hazard to move to permanent employment, with a duration-varying profile and distinct gender differences.

(a) For men, the differential effect is negligible during the first year but has a strong negative peak just at the end of the year, at the 360-366-day band, when the basic entitlement period for younger workers is close to expiry. Within the second year, the effect stays negative but is less pronounced, possibly because, *inter alia*, the reference group of workers under 40 is somehow selected, as it consists only of workers who obtained temporary contracts[22]. A peak also emerges around the two-year duration, but with a positive sign, the interpretation of which is far from straightforward[23].

(b) The pattern of the differential entitlement effect is quite different for women. It is negative from the start of the spell, stays basically constant throughout the first year, including the last week, and then increases during the second year as well as at the end of it.

Similar differential treatment effects, but less marked, are also found for registered workers dismissed by small firms, for whom benefit packages do not include any benefit transfer component. Within this category, the best programme treatment for older workers simply consists of the longer period they are allowed to stay in the lists (with cuts in social security contributions for employers hiring them from there), and consequently of extending their stay *via* temporary contracts. For both men and women, the differential treatment parameter turns to be around zero during the first year, as expected. Then, however, the differential effect parameters become negative: fairly high in absolute value, although not significant, for the last week of the first year; decidedly negative during the second year, and for women also at the end of it. On the whole, this evidence indicates that (positive) differences only in the allotted period in the lists, within which hires enjoying the rebate are allowed[24], do have a (negative) impact on the conditional probability of transiting to a permanent job.

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[22] For workers under 40, this is the only way to extend their stay in the lists beyond one year.

[23] It may be conjectured that the reversal of the sign of the parameter at the 725-732-day duration, with respect to the negative pattern of the overall differential effect profile, is due to the fact that two years is the maximum allotted period in the lists for workers aged 40-49 years. They themselves now perceive the dominant pressure of approaching the expiry date of the period for social security rebate. We are reluctant to go this far for two reasons: the quite low number of cases and transitions within that band of duration; and the possibly confounding effect of censoring longer durations at two years.

[24] Note that, in addition to being equal across programme treatments, the size of the rebate stays equal over

7. Concluding remarks

The first lesson to be learned from our analyses of the Mobility Lists programme concerns the severe limitations on the inferences which may be drawn, about the differential impact of programme treatments, because of deficiencies in available information. Although it is true that we were able to use slightly better data than those used in previous studies, they are still exceedingly crude. An especially severe deficiency is the impossibility of identifying spells in temporary jobs, within the periods registered workers spent enrolled in the lists. Indeed, in their authoritative review of the econometrics of labour market programmes, the comment by Heckman, Lalonde & Smith (1999: 1867) sounds particularly appropriate: "*Too little [emphasis has been] given to the quality of the underlying data. Although it is expensive, obtaining better data is the only way to solve the evaluation problem in a convincing way*".

With this *caveat* as a background, three main substantive aspects deserve attention. Given the flexible model specification adopted, it is sensible to argue that they are reasonably robust. First, older workers with income support, who can enjoy substantial benefits longer, have a significantly lower chance of moving to employment than their younger colleagues. The differential treatment effect varies appreciably with the time spent in the lists: it is (almost) consistently negative, but much higher at the end of the first year, when younger workers approach the exhaustion of (some) benefits. Besides, the profile of the differential effect varies according to gender: it is negative from the very beginning for women; it is delayed, but at the same time stronger, for men.

Interestingly enough, differential effects are also found for registered workers dismissed by small firms, for whom benefit packages do not include the benefit transfer component. For older workers, they are negative from the end of the first year and significant during the second year. Thus, there is evidence that the longer period allotted for hires with social security rebate induces a decrease in the hazard rate to employment.

There are indirect but unequivocal indications that the possibility for hiring firms to accumulate social security reductions, by hiring on temporary contracts and then switching them to permanent ones, is used. This evidence, coupled with the latter, suggests that the proper benefit transfer provision included in the programme might not play a dominant role, when compared with the effect of reduction in social security contributions. It is this latter component of benefit packages that seems to prevail: a component – note – the amount of which is fixed, with no modulation at all according to the time spent in the lists.

Altogether, our results suggest that, if the programme aims at increasing the hazard of dismissed workers from unemployment to permanent job, and particularly to favour the transition to employment for workers aged between 40 to 49, it must be reconsidered and substantially redesigned. The absence of any phasing-out of cuts in social security contributions, combined with the longer opportunity to stay in the lists for older workers, appears to be especially questionable.

time (obviously, within the entitlement period).

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Table 1: *Benefits for hiring firms according to different hiring strategies: some typical cases* (Euros; relative benefits in brackets)

	From the market (one year)*	From the lists (two years)					
		39 years old				40 years old	
		Immediately		Late (no bonus)		Immediately **	
		Permanent	Temporary + permanent	Permanent	Temporary + permanent	Permanent	Temporary + Permanent
Pay, before tax	11,880	23,760	23,760	23,760	23,760	23,760	23,760
Social security contributions	4,751	2,555	238	2,555	238	2,555	238
Bonus		-2,733	-2,733			-4,919	-4,919
Total labour cost	16,631	23,582	21,265	26,315	23,998	21,396	19,079
Total savings		**9,680**	**11,997**	**6,947**	**9,264**	**11,866**	**14,183**
		(29.10)	***(36.07)***	***(20.89)***	***(27.85)***	***(35.67)***	***(42.64)***
1st year		7,365	4,632	4,632	4,632	7,365	4,632
		(44.28)	*(27.85)*	*(27.85)*	*(27.85)*	*(44.28)*	*(27.85)*
2nd year		2,315	7,365	2,315	4,632	4,501	9,551
		(13.92)	*(44.28)*	*(13.92)*	*(27.85)*	*(27.06)*	*(57.43)*

* Source: Brunello and Miniaci (1997), Table 1. Additional working assumptions for hiring from the lists (taken from same authors) are the following: yearly social security contributions reduced from 4,751 to 119; income support for first year 5,466.

** For a worker aged 40 hired late (no bonus), benefits are the same as for a 39-year-old.

Table 2: *Workers in the lists by gender, income support and age group*

Gender	**Income support**	**Age group**			
		<40	*40-49*	*>49*	*Total*
Men	*Yes*	4,209	2,734	2,705	9,648
		(43.63)	*(28.34)*	*(28.04)*	*(55.79)*
	No	4,652	1,747	1,247	7,646
		(60.84)	*(22.85)*	*(16.31)*	*(44.21)*
	Total	8,861	4,481	3,952	17,294
		(51.24)	*(25.91)*	*(22.85)*	*(41.12)*
Women	*Yes*	5,924	2,008	966	8,898
		(66.58)	*(22.57)*	*(10.86)*	*(35.93)*
	No	12,903	2,228	738	15,869
		(81.31)	*(14.04)*	*(4.65)*	*(64.07)*
	Total	18,827	4,236	1,704	24,767
		(76.02)	*(17.10)*	*(6.88)*	*(58.88)*
Total		27,688	8,717	5,656	42,061
		(65.83)	*(20.72)*	*(13.45)*	*(100.00)*

Table 3: *Workers' status in the lists by gender and age group*

Gender	**Age group**	**Current status**			
		Still enrolled	*Hired*	*Cancelled*	*Total*
Men	*<40*	1,850	3,187	3,824	8,861
		(20.88)	*(35.97)*	*(43.16)*	*(51.24)*
	40-49	1,644	1,678	1,159	4,481
		(36.69)	*(37.45)*	*(25.86)*	*(25.91)*
	>49	2,333	661	958	3,952
		(59.03)	*(16.73)*	*(24.24)*	*(22.85)*
	Total	5,827	5,526	5.941	17,294
		(33.69)	*(31.95)*	*(34.35)*	*(41.12)*
Women	*<40*	5,205	4,283	9,339	18,827
		(27.65)	*(22.75)*	*(49.60)*	*(76.02)*
	40-49	2,157	821	1,258	4,236
		(50.92)	*(19.38)*	*(29.70)*	*(17.10)*
	>49	1,222	183	299	1,704
		(71.71)	*(10.74)*	*(17.55)*	*(6.88)*
	Total	8,584	5,287	10,896	24,767
		(34.66)	*(21.35)*	*(43.99)*	*(58.88)*
Total		14,411	10,813	16,837	42,061
		(34.26)	*(25.71)*	*(40.03)*	*(100.00)*

Table 4: *Specification searches on a sequence of nested models (basic specification: piecewise exponential proportional hazards model with binomial unobserved heterogeneity): women without income support (N=15,131)*

	Model	Log-likelihood	LRT statistic	Degrees of freedom	p-value
A.	*Separate models for two age groups <40 and 40-49*	-26,997.09			
B.	*Equality restrictions on heterogeneity parameters, age excluded*	-27,010.80	27.42	20	.124
C.	*B + equality restrictions on age parameters (4^{th} - order polynomial)*	-27,011.10	.60	4	.963
D.	*C + restrictions on degree of polynomial in age (2^{nd} - order)*	-27,012.92	3.64	2	.162
E.	*D + restrictions on differential effects parameters (No. of parameters=4)*	-27,013.68	1.52	4	.823
F.	*E + restriction of mean differential effect (No. of parameters=1)*	-27,019.38	11.40	3	.009

Table 5: *Three specifications of differential treatment effect of programme package for workers aged 40-49: ML estimates of piecewise exponential proportional hazards models with binomial unobserved heterogeneity; women without income support (N=15,131)*

Variable	Differential effects		
	D. Full specification	*E. Four time classes*	*F. Age dummy*
Constant (1-29)	-7.93 (0.27)**	-7.93 (0.27)**	-7.91 (0.27)**
Duration: 30-89	0.23 (0.08)**	0.22 (0.07)**	0.23 (0.07)**
90-179	-0.06 (0.11)	-0.08 (0.11)	-0.06 (0.11)
180-359	0.10 (0.12)	0.11 (0.11)	0.13 (0.11)
360-366	1.66 (0.16)**	1.66 (0.16)**	1.63 (0.16)**
367-549	1.05 (0.12)**	1.04 (0.12)**	1.02 (0.12)**
550-724	-0.05 (0.15)	-0.04 (0.14)	-0.07 (0.14)
725-732	3.39 (0.17)**	3.39 (0.17)**	3.28 (0.16)**
≥40 years:			-0.35 (0.14)**
1-359		-0.15 (0.16)	
1-29	-0.19 (0.19)		
30-89	-0.22 (0.21)		
90-179	-0.27 (0.23)		
180-359	-0.05 (0.18)		
360-366	-0.58 (0.42)	-0.58 (0.42)	
367-724		-0.51 (0.16)**	
367-549	-0.52 (0.17)**		
550-724	-0.44 (0.26)*		
725-732	-0.86 (0.33)**	-0.86 (0.33)**	

Asymptotic standard errors in brackets: * significant at 5% level; ** significant at 1% level.

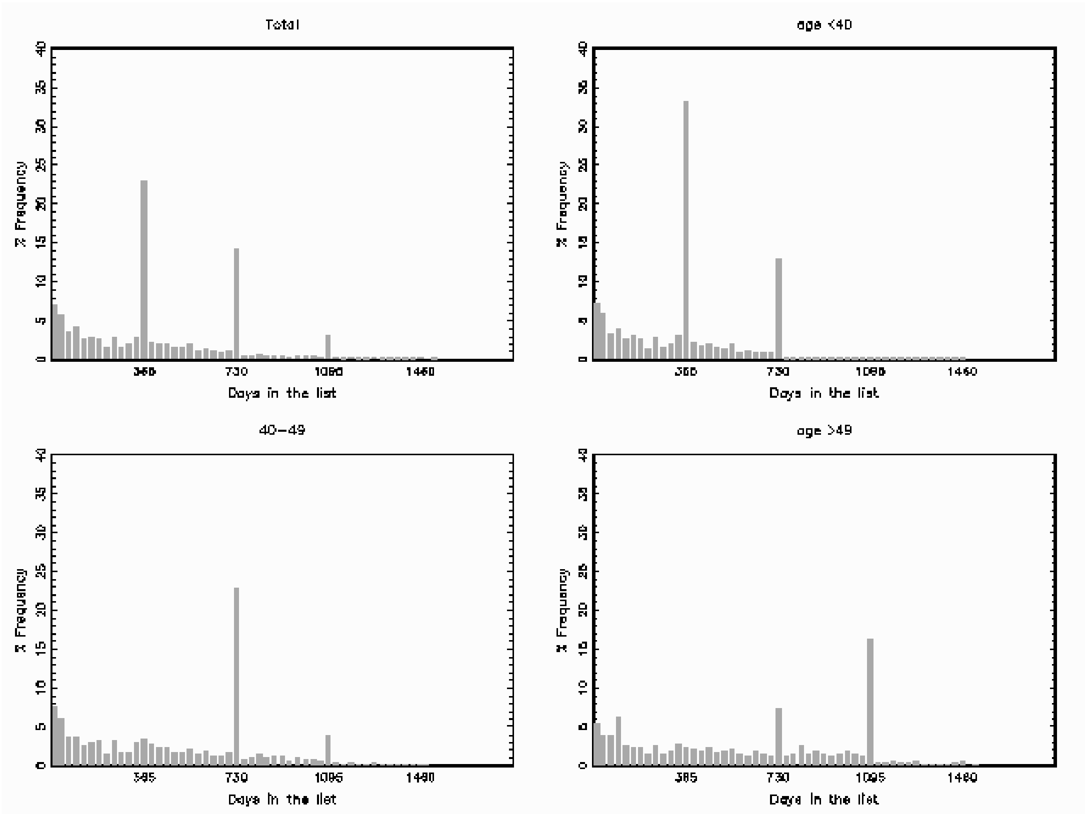
Table 6: *ML estimates of piecewise exponential proportional hazards models with binomial unobserved heterogeneity; four groups identified by entitlement to income support and gender*

Variable	**With income support**		**No income support**	
	Men	***Women***	***Men***	***Women***
Constant (1-29)	-4.70 (0.18)**	-6.69 (0.32)**	-5.47 (0.16)**	-7.93 (0.27)**
Duration: 30-89	-0.67 (0.08)**	-0.79 (0.11)**	-0.54 (0.09)**	0.22 (0.07)**
90-179	-1.09 (0.11)**	-1.34 (0.14)**	-1.23 (0.12)**	-0.08 (0.11)
180-359	-0.98 (0.11)**	-1.17 (0.15)**	-1.25 (0.13)**	0.11 (0.11)
360-366	1.42 (0.15)**	0.73 (0.21)**	-0.23 (0.25)	1.66 (0.16)**
367-549	-0.47 (0.12)**	-0.19 (0.16)	-0.61 (0.14)**	1.04 (0.12)**
550-724	-1.25 (0.15)**	-0.99 (0.18)**	-1.82 (0.20)**	-0.04 (0.14)
725-732	1.07 (0.36)**	2.02 (0.25)**	0.35 (0.47)	3.39 (0.17)**
≥40 years 1-359	-0.05 (0.12)	-0.46 (0.19)**	0.01 (0.07)	-0.15 (0.16)
360-366	-1.48 (0.29)**	-0.62 (0.36)*	-0.58 (0.50)	-0.58 (0.42)
367-724	-0.53 (0.14)**	-1.06 (0.20)**	-0.29 (0.12)**	-0.51 (0.16)**
725-732	1.06 (0.38)**	-2.71 (0.64)**	-0.87 (0.74)	-0.86 (0.33)**
Age (order of polyn.)	3	4	0	2
White-collar workers	-0.26 (0.06)**	-0.02 (0.07)	-0.17 (0.07)**	0.01 (0.06)
Year of enrolment	Dummies	Dummies	Dummies	Dummies
Province	Dummies	Dummies	Dummies	Dummies
Industry	Dummies	Dummies	Dummies	Dummies
Unobserved het.: θ_{1-p}	3.62 (0.10)**	3.87 (0.19)**	3.18 (0.14)**	4.58 (0.10)**
p	0.87	0.96	0.90	0.92
Log L	-20,162.40	-13,467.27	-15,140.42	-27,013.68
No. observations	6,943	7,932	6,399	15,131

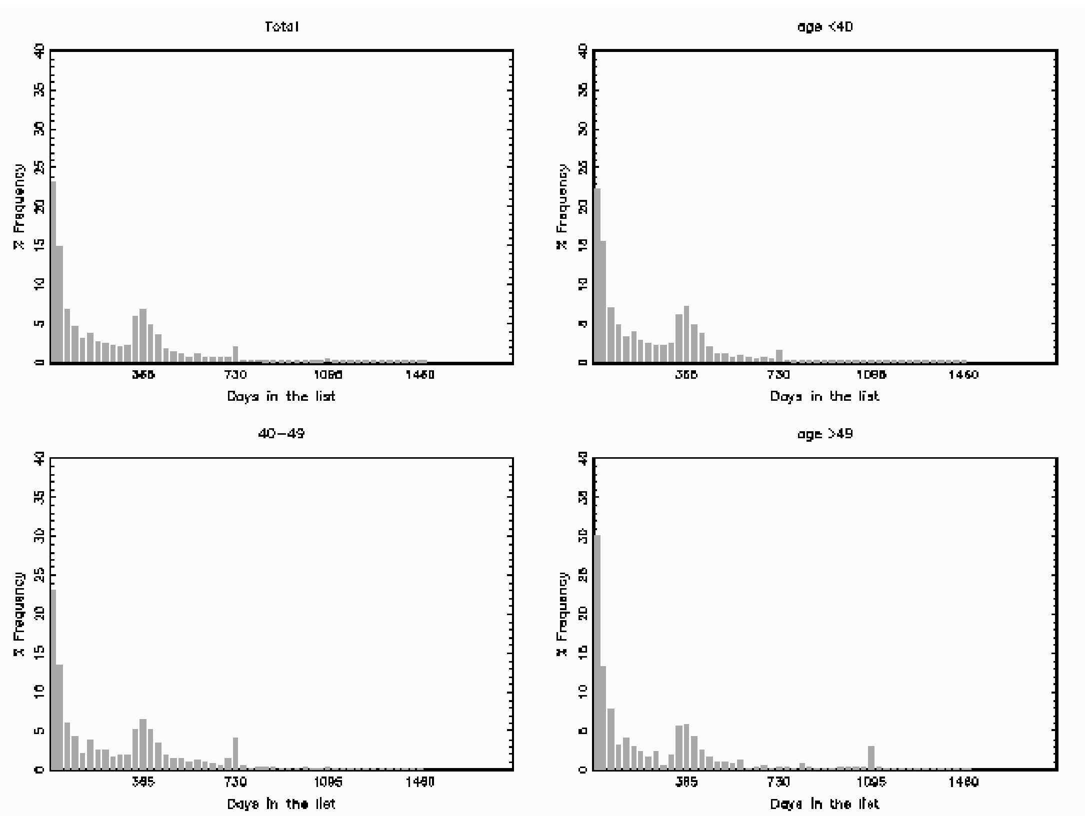
Asymptotic standard errors in brackets: * significant at 5% level; ** significant at 1% level.

Figure 1: *Duration of stay in the lists: whole sample, hired workers and cancelled workers by age group*

a. Whole sample



b. Hired workers



c. Cancelled workers

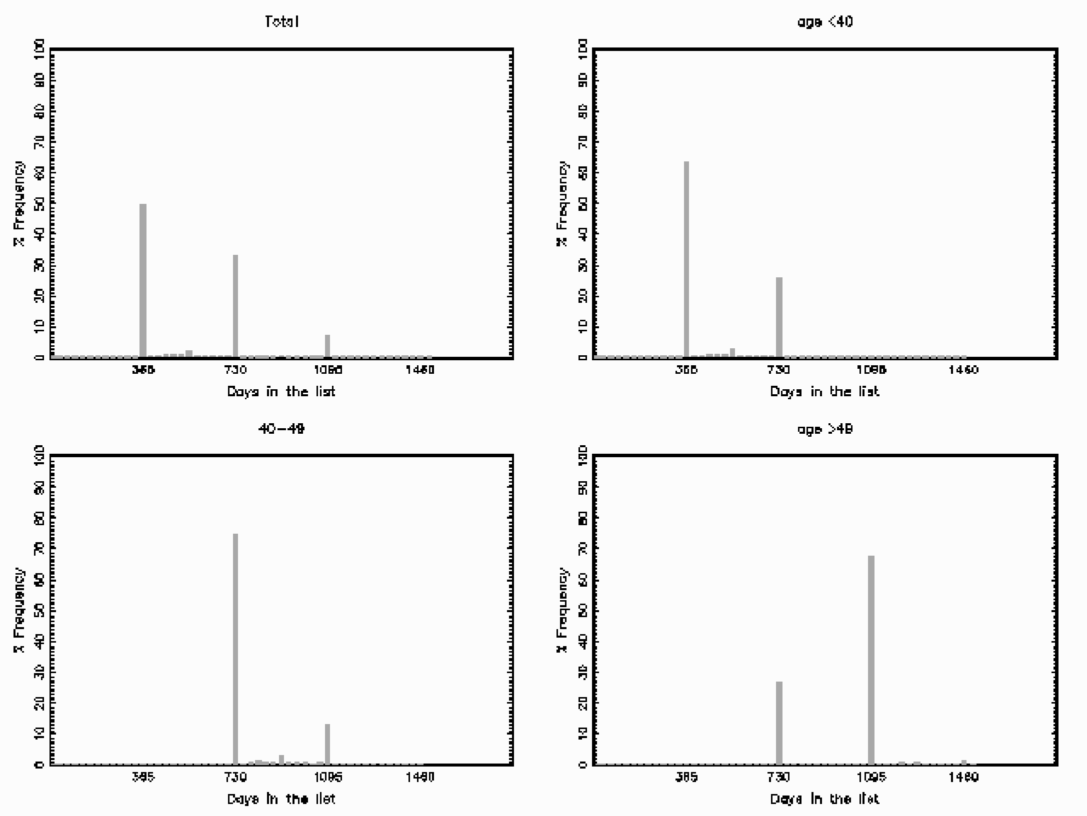


Figure 2: *Kaplan-Meier survival functions by entitlement to income support, gender and age group*

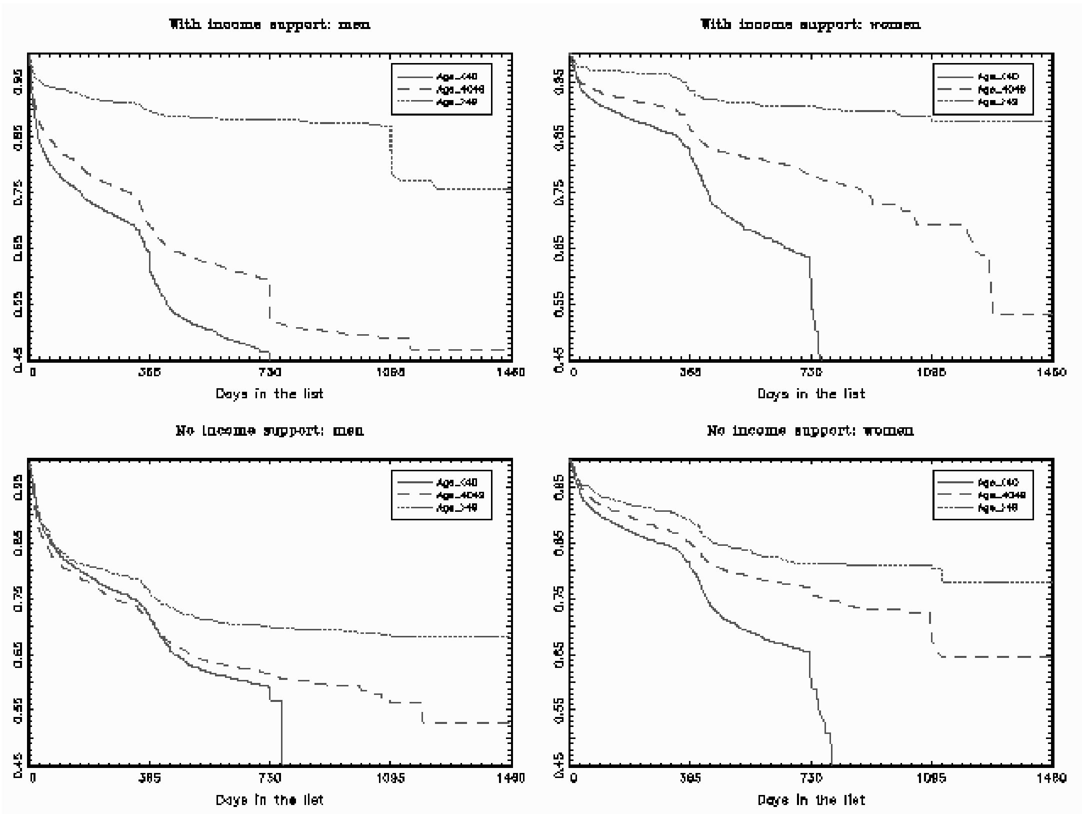
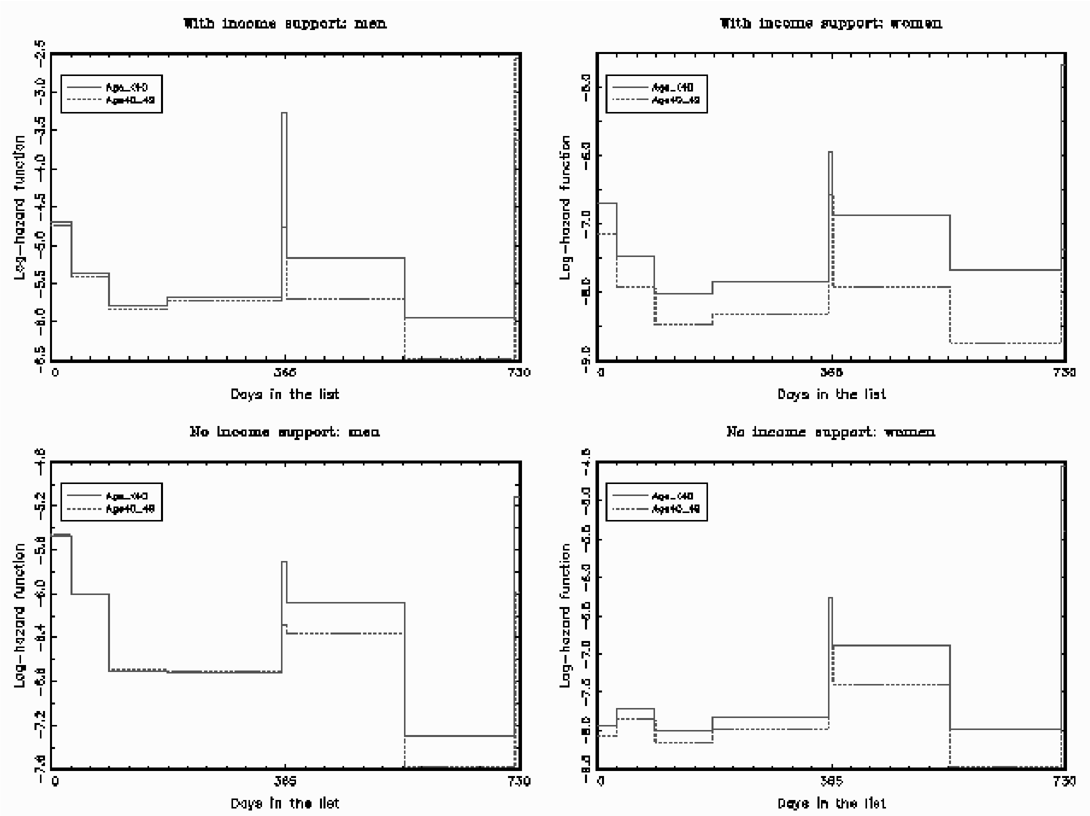


Figure 3: *Graphs of estimated baseline hazards from final models (see Table 5)*



Appendix: *Summary statistics for piecewise exponential proportional hazards models (% frequencies in four groups, plus mean age)*

Variable		With income support		No income support	
		Men	***Women***	***Men***	***Women***
Days in the lists	*1-30*	14.8	7.0	12.4	6.8
	30-90	9.1	5.6	10.2	8.8
	90-180	8.5	8.4	8.1	8.7
	180-360	13.9	14.4	12.2	13.6
	360-367	16.4	27.9	21.6	27.6
	367-559	8.6	9.7	10.6	10.2
	550-725	6.9	7.4	6.8	7.0
	725-732	21.9	19.7	18.3	17.2
Status in the lists	*Permanently hired*	40.7	21.4	31.8	22.5
Age	*< 40*	60.6	74.7	72.7	85.3
	Mean	*36.3*	*33.3*	*33.9*	*30.7*
Year of enrolment	*1995*	28.5	21.6	23.0	20.4
	1996	28.8	25.8	23.3	24.4
	1997	22.3	24.9	26.9	25.2
	1998-99	20.4	27.7	26.8	30.0
Occupation	*Blue-collar workers*	75.3	74.1	79.0	73.2
Industry	*Agriculture*	11.0	6.9	3.8	1.8
	Textiles	17.5	62.1	10.6	49.5
	Mechanical	37.7	10.1	21.8	6.7
	Chemical	7.2	4.9	4.0	2.4
	Building	7.2	1.2	17.2	2.2
	Paper and publishing	3.0	1.4	3.0	1.6
	Trade	7.8	10.4	27.6	31.4
	Services	3.3	0.7	4.4	0.9
	Other	5.3	2.2	7.6	3.5
Province	*Belluno*	5.2	4.2	4.7	2.9
	Padova	21.4	26.1	20.9	21.3
	Rovigo	7.5	5.5	13.5	11.0
	Treviso	19.0	19.2	14.5	17.8
	Venezia	16.8	10.9	18.6	16.5
	Verona	17.7	15.1	19.6	19.9
	Vicenza	12.5	19.0	8.2	10.5
No. observations		6,943	7,932	6,399	15,131