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evidence from an institutional change

by Alfonso Rosolia and Paolo Sestito

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THE EFFECTS OF UNEMPLOYMENT BENEFITS IN ITALY: EVIDENCE FROM AN INSTITUTIONAL CHANGE

by Alfonso Rosolia* and Paolo Sestito*

Abstract

We document the effects of a change in the Italian Ordinary Unemployment Benefits Scheme on the job search process. As of January 2001, the replacement rate was raised from 30% to 40% and benefits' duration extended from 6 to 9 months for workers aged 50 or more. Our results show that (a) the average duration of benefits' collection increased by around one month for individuals entitled to 3 additional months, while it did not change significantly for those only exposed to higher replacement rates; (b) the pace of re-employment is never found to be statistically different across regimes, although point estimates for those exposed to a longer duration consistently indicate a 2 to 4 percentage points lower probability of re-employment at several horizons. Graphical evidence suggests that job-separation rates did not change with the new regime, while take-up apparently did, although the clear cyclical pattern could bias the picture. We conclude that, if any, the behavioural response induced by the change, must have been modest in economic terms. We discuss the reasons why the response may have been so subdued.

JEL Classification: J64, J65.

Keywords: unemployment insurance, unemployment duration, replacement rate.

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1 Introduction¹

According to the 2007 Italian Labor Force Survey, about 10 percent of active persons aged 20-60 with prior work experience had been receiving some kind of unemployment welfare assistance in the month prior to the survey. Anastasia, Mancini and Trivellato (2008) report a rough estimate of the number of beneficiaries of passive labor market policies in 2007 of about 1.5 millions, corresponding roughly to a total expenditure of about 0.6 percent of GDP.

The elasticity of unemployment duration to unemployment benefits is a key ingredient in the design of optimal unemployment schemes (Baily (1978), Chetty (2006), Chetty (2008)). The primary goal of unemployment insurance (UI) is to reduce the welfare costs caused by job loss, workers reallocation and people flows in and out of the labor market. It achieves this goal by providing insurance¹ to job losers. Importantly, such insurance provision also generates disincentives to work as it alters the relative prices of consumption and leisure. This means that the estimated elasticity of unemployment duration to unemployment benefits is a mixture of two effects: a socially suboptimal “substitution” effect, triggered by the distortion to relative prices, and a socially optimal “liquidity” effect, caused by the availability of cash that fixes financial markets failures and eases the pressure on liquidity constrained beneficiaries to find quickly a new job. Both effects imply that more generous benefits schemes increase unemployment duration and such positive association has been widely documented (for example, the reviews in Atkinson and Micklewright (1990) and Krueger and Meyer (2002)). However, Chetty (2008) shows that in the US a large portion of the response of search behavior to unemployment benefits can be traced to the liquidity effect rather than to the moral hazard effect induced by

¹The views expressed here are our own and do not necessarily reflect those of the Bank of Italy. We thank Federico Cingano, Francesco D’Amuri, Francesca Lotti, and Roberto Torrini for their comments. We also received useful comments from seminar participants at the Bank of Italy and Ministero dell’Economia e delle Finanze. Email: alfonso.rosolia@bancaditalia.it, paolo.sestito@bancaditalia.it

UI.

This paper documents the effects of replacement rates and benefits duration on the job search process in Italy. Specifically, it only attempts to quantify the overall elasticity without addressing the issue of its determinants and, as a consequence, its (sub) optimality. We pursue our goal exploiting a change in the Italian ordinary unemployment benefits scheme, the major scheme providing unemployment insurance, that raised replacement rates from 30 to 40 and duration from 6 to 9 months for eligibles older than 50. We focus on the effects of such change on the length of time on benefits, on separation rates, and on the duration of job-to-job transitions exploiting a variety of empirical strategies. To our knowledge this is the first paper that looks into the relationship between unemployment insurance and duration in Italy.

We find that time spent on benefits increases by about one month for those entitled to a longer duration, while no effect can be detected for those only entitled to the higher replacement rate. When we explore the behavioral responses of such changes, we only find mild and generally non robust effects on re-employment probabilities. This result has to be interpreted with caution, however. On the one hand, our data do not allow to control for important determinants of search behavior such as asset holdings, marital status and other relevant socio-demographics, or the availability of other welfare schemes at benefits expiration. On the other, our sample covers only a couple of years after the specific change we examine was implemented raising the possibility that it was not entirely perceived and exploited by potential beneficiaries. Finally, the limited variation in the amount of resources provided by the change, especially as concerns the increase in replacement rates, together with the small sample size, yield very imprecise estimates that might lead us to overlook the behavioral responses of interest if these are not sufficiently strong.

The paper is organised as follows. In the next section we briefly illustrate the main unemployment insurance schemes available in Italy. We then move to the data and describe its main features. Next, we present the empirical analysis. We then conclude.

2 Institutional setting

The Italian unemployment insurance system is highly fragmented into several schemes that are extremely heterogeneous as concerns eligibility criteria and benefits levels; relevant segments of the labor force are eligible only for residual schemes and large portions are not covered; the system is broadly unmonitored. These features makes it unsuitable to soften the private burden of job loss and to help smooth the social costs of workers re-allocation while preserving the correct private incentives to work.

The standard unemployment insurance scheme for Italian private non-agricultural employees is the Ordinary Unemployment Benefits (OUB) which constitutes the object of our investigation. Eligible individuals are private non-agricultural employees who either have been dismissed or whose temporary contract has expired, who have contributed to the scheme for at least 12 of the previous 24 months and are actively searching for a job. Of this set of criteria, the dismissal and the active search ones have never been seriously enforced. In principle, to claim benefits from the Social security institute (INPS) a dismissed worker should first register with the Public Employment Service (PES). However, the two institutions are only weakly connected and the PES itself does not really monitors the search behavior of individuals registered as unemployed². Eventually, the only really relevant eligibility condition is having a sufficiently long

² Pirrone and Sestito (2006) discuss in depth the institutional details of the relationship between the two institutions and the more recent proposals to implement the activation criterion.

employment span prior to unemployment. In 2006, about 360,000 beneficiaries relied on this scheme, with an average payment for each spell of about 3,000 Euro (Anastasia et al. (2008)).

Figure (1) shows the evolution of the replacement rate since 1955. Initially, the coverage was a fixed nominal daily amount, only occasionally revised to account for inflation. Between 1972 and 1987 prices increased at an average of 15 percent a year, bringing the replacement rate to a low 1 percent by 1987. In 1988 the replacement rate was set as a percentage of previous wages and fixed at 7.5 percent; in the subsequent years several interventions rose it bringing it up to 60 percent since 2005³. In particular, in this paper we focus on the increase occurred in 2001, from 30 to 40 percent, passed with the 2001 budget law, voted in December 2000. Together with such increase in the replacement rate, potential benefit duration was also selectively extended from 6 to 9 months for workers aged 50 or more⁴. We provide an evaluation of the effects of these changes on unemployment duration, on benefits spell duration, and on job separation rates⁵.

³Benefits are capped; in 2010 the cap was 892.96 euro if the unemployed monthly wage was not larger than 1931.86 euro and 1073.25 if above. In practice, in 2010 the average full time worker earned roughly 2300 euro per month so that the actual replacement rate for the average worker would fall short of 50 percent.

⁴Currently, potential duration is 8 months for unemployed younger than 50 and 12 months for older ones. Replacement rates are set at 60 percent in the first 6 months, 50 percent for the 7th and 8th month and, for workers older than 50, 40 percent from the 9th to 12th months.

⁵ The two other largest unemployment benefits schemes are the Mobility Indemnity and the Reduced Unemployment Benefits. The Mobility Indemnity (MI) is accessible by those employees who have worked in industrial firms with at least 15 employees and have been involved in a collective dismissal procedure. MI benefits amount to 80% of the previous wage with a cap such that the average worker gets about 2/3 of his wage; the scheme lasts for a maximum of 12 months for workers below age 40, 24 months for those in between 40 and 50, and 36 months for unemployed older than 50; residence in the South earns an additional 12 months of duration. MI typically follows the expiration of Cassa Integrazione Guadagni, a scheme financing temporary layoffs at about 80% of the previous wage. In 2006 about 100,000 persons have claimed and obtained MI. The Reduced Unemployment Benefits (RUB) scheme is targeted to private non-agricultural employees not covered by either OUB or MI. It provides a lump sum payment in the calendar year following the one when the non-employment spell was experienced provided at least 3 month of employment were filed in that year. Beneficiaries are not required to be actively searching for a job nor to be effectively unemployed when cashing in the payment. In 2006 RUB was claimed by about 470,000 persons with an average payment of 1,400 Euro. All three schemes entitle the beneficiaries also to fictional pension contribution. This turns out to be especially relevant for RUB beneficiaries, who typically are seasonal workers.

3 Empirical analysis

In this section we document whether and to what extent individual behaviors have been affected by the institutional change described above. The change tilted two relevant margins of UI: benefits' generosity and benefits' duration. Since potential duration only changed for job losers beyond age 50, appropriate comparisons allow to isolate the effects of the two margins on the outcomes of interest. Before turning to the empirical analysis, we briefly illustrate the data sources and motivate the choice of the outcome variables studied in the paper.

3.1 Data and outcome variables

We use Social security records collected for a large representative sample of Italian private non-agricultural employees over the period 1985-2002 by the Ministry of Labor⁶(CLAP). The final sample is obtained combining two sources: the registry of employment relationships and the registry of benefits collection. Each provides information on a limited set of socio-demographic characteristics except education and, respectively, on wages, weeks worked, initial and final dates of each employment relationship and on the type, amount and period over which ordinary benefits are collected. The combination of the two archives to recover a longitudinal dataset of transitions in and out of employment and benefits collection involves some choices and assumptions. First, we restrict the analysis of non-employment spells to those initiated in the period 1997-2001. This is motivated both by the fact that a consistent classification of the type of benefits collected is available only since 1997 and by the need to leave a sufficiently long time window to observe (possibly censored) spells duration both before and after the schedule change.

⁶The sample corresponds to 1/90 of the relevant population. It is constructed by extracting from the relevant registries all individuals born in 4 specific days of the year. See <http://www.lavoro.gov.it/Lavoro/Strumenti/StudiStatistiche/CLAP> for further details.

Second, because our focus is on the ordinary unemployment benefits scheme, we exclude from the sample the entire histories of individuals who are observed in self-employment, on some special benefits scheme, in early retirement and people collecting some kind of disability benefit. Third, we assume that overlapping employment relationships represent a unique employment spell; a similar assumption is made for a few observations for whom we observe overlapping ordinary benefits collection spells. Fourth, we assume that subsequent employment spells with at most one week of non-employment represent direct job-to-job transitions⁷. Applications of these rules yield a sample of 136,884 non-employment spells, of which 10,185 with benefits collection, initiated over the period 1997-2001, corresponding to 84,909 different individuals of age 20-65 at the beginning of the spell.

The data allow to study two different outcome variables. More specifically, we observe with a high degree of precision the length of the spell over which benefits are collected (time on benefits, henceforth) and, subject to the above assumptions, a noisy measure of the length of the spell between two subsequent employment relationships (non-employment spell, henceforth). Needless to say, time on benefits is observed only for those who collect benefits. For them, time on benefits is a censored version of the actual duration of the non-employment spells, the censoring threshold being the potential duration of benefits. While from the behavioral point of view the relevant outcome is the length of a non-employment spell, studying the reaction of time on benefits to the institutional change allows to quantify the increased cost associated with the change borne by the Social Security administration. Moreover, because it provides a cleaner, although censored, measure of non-employment duration it provides a useful benchmark to compare the results obtained for the more relevant non-employment duration.

⁷This is consistent with the formal requirement that a benefits claim can be filed not earlier than one week after separation.

3.2 UI generosity and time on benefits

Figure (2) displays the distribution of weeks on benefits by year when the spell began; the top panel refers to persons aged 20-48 at job termination, exposed from 2001 only to a higher replacement rate; the bottom one to those 51 and older, for whom also potential duration was increased. Every year around 40% of spells reaches the expiration limit and the pattern turns out to be similar across age groups. Visual inspection suggests the proportion of spells expiring at the limit is highly cyclical. For the population aged 20-48, for whom the institutional change only implied an increase in the replacement rate, there is no clear pattern before and after the relevant date (2000-2001). On the other hand, the extension of benefits duration to 9 months for the population older than 50 clearly shifted the spike to the right and decreased it by a half.

To quantify the effects of the two changes in the unemployment scheme of time on benefits we resort to simple linear models of the type:

$$d_{it} = \theta E_{it} + X_{it}\beta + \epsilon_{it} \quad (1)$$

where d_{it} is completed time on benefits of individual i for the spell beginning in year t , E_{it} is a dummy for the exposure to the change, X_{it} are individual and aggregate controls, ϵ_{it} a residual. We report results for θ obtained from a variety of specifications of the control set (X_{it}) and of the underlying time window. Because the outcome under analysis is time on benefits, the samples obviously only includes beneficiaries.

Longer potential duration

The extension of the potential duration of benefits from 6 to 9 months was age-specific and applied only to beneficiaries older than 50 when filing for benefits. This provides two main margins to assess its effects, age and time. Columns (1) to (7) in table (1) report estimates of θ from equation (1) in which $E_{it} = 1$ if $t = 2001$ and age is equal or above 51 and zero otherwise.

Estimates thus include both the effect of the longer potential duration and that of the higher replacement rate. In columns (1) to (3) we only use persons aged 51-65, so that identification relies exclusively on comparisons over time of individuals of similar age. In column (1) we simply compare duration on benefits for beneficiaries in 2000 and in 2001 and find a significantly longer duration for the latter of about 32 days; the result is broadly confirmed when we add individual controls for residence, sex, age and characteristics of the previous job (col. 2). In column (3) we extend the sample to the period 1997-2001 and augment the control set with a cubic in time, quarterly unemployment and GDP growth rates to capture cyclical conditions. We find again a significantly longer benefit spell for beneficiaries entitled to the extended limit of about one month. These exercises do not allow to take fully into account common differences between periods. Therefore, in columns (4) to (7) we also exploit the age margin to estimate the effect of interest. This allows to control for time effects under the assumption that these are common to persons of different ages. Perhaps more relevantly, conditional on the above assumption, this specification allows to quantify only the effects of a longer potential duration as both groups are exposed to a higher replacement rate⁸. Specifically, we compare time spent on benefits of people aged 51-55 to people aged 45-48 (cols. (4)-(6)) and of people aged 51-65 to 20-48 (col. (7))⁹. In column (4) we only focus on spells begun in 2001, the first year benefits were extended, and simply compare time on benefits of people aged 45-48 to those of people aged 51-55. The results are remarkably consistent with those presented in the previous exercises: we find a statistically significant longer duration on benefits of about 26 days for beneficiaries

⁸ This interpretation also requires that the effect of a higher replacement rate is the same across the age threshold.

⁹We drop from the sample persons aged 49-50 because of potential misclassification at the threshold. Specifically, we compute age as the difference between year when the spell was initiated and birth year. This implies that people with computed age of 50 could also be either 49 or 51; those with computed age of 49 could as well be either 48 or 50. On the contrary, those with computed age of 51 are certainly entitled to the extended duration.

entitled also to the extended benefits. In column (5) we estimate equation (1) on the sample of 45-48 and 51-55 year old including also spells begun in 2000, the year prior to the change, and controlling for a sex-specific linear age term, year dummies and dummies for the initial week of the spell. While the precision is lower, because the control for age is highly collinear with the dummy for the treatment (that identifies persons older than 50 in 2001), we still obtain a 10 percent significance level and a stable point estimate of about 22 days. In columns (6) and (7) we extend the time window to the whole 1997-2001 period and, in column (7), the age span to include all persons aged 20-65, so as to weaken the correlation between age and the relevant dummy E_{it} . We also augment the control set with characteristics of the previous job spell, dummies for area of residence, quarterly unemployment and GDP growth rates when the spell begun. In both cases, we find a significant increase of about 20-30 days, consistent with the earlier results based only on comparisons over time. Incidentally, the stability of the estimates across specifications that do and do not control for time-varying common unobserved determinants of time on benefits, suggests these to be of little relevance at the juncture under analysis.

In columns (8) and (9) we perform two robustness checks. First, we pretend the reform was anticipated to year 2000; therefore, we estimate (1) on the period 1997-2000 on persons aged 20-65 and set $E_{it} = 1$ if older than 51 and $t = 2000$ and zero otherwise¹⁰. Second, we pretend it was persons aged 45-48 to be affected by the extension of benefits duration in 2001. We thus estimate (1) on the sample of 20-48 year old over the period 1997-2001 setting $E_{it} = 1$ if $t = 2001$ and age between 45 and 48. In both cases, we do not detect any statistically significant difference, suggesting that results in column (1) to (7) reflect the consequences of exposure to

¹⁰For consistency, we still drop the specific ages 49 and 50.

a longer potential duration of benefits.

Higher replacement rate

The increase in the replacement rate affected all beneficiaries, independently of their age, starting from 2001. Comparing the point estimates in columns (1)-(3) with those reported in columns (4)-(7) of table (1) suggests that the increased replacement rate may have had some, perhaps tiny, effect. In fact, estimates in columns (1)-(3), that include both the effects of a longer duration and a higher replacement rate, appear to be consistently larger than those of subsequent columns (4)-(7) that in principle do not include the effects of a higher replacement rate. However, to get a more precise assessment of the higher replacement rate we estimate (1) on the sample of individuals aged 20-48 at the beginning of their spell, only exposed to the higher replacement rate. The dummy for exposure to treatment thus equals unity for spells begun in 2001. Importantly, this exercise can only be based on comparisons over time of younger individuals. In column (1) of table (2) we estimate (1) with no controls on the sample of spells begun in 2000 and in 2001¹¹. We find that duration on benefits is only about 5 days longer for spells beginning in the first semester of 2001 with respect to those begun one year earlier, but the estimate is not statistically significant at conventional levels. Furthermore, when in column (2) we control for area of residence, age, sex, a sex-specific linear age term, characteristics of the previous job spell, and a set of dummies for the initial week of the spell the point estimate becomes basically nil, with similar standard errors. Because identification of the effects of a higher replacement rate hinges on comparisons of persons in unemployment in different moments in time, estimates may reflect pre-trends or differences in cyclical conditions.

¹¹We only focus on spells begun in the first semester of each year to avoid the problems related to the possibility that ongoing spells at January 1st 2001 were entitled to the new scheme for the remaining duration. These observations are however too few to exploit the unexpected change while on benefits to study its effects on subsequent duration.

Thus, in column (3) we augment the control set with a cubic in calendar time and extend the sample to all spells begun in the first half of years 1997-2001; in column (4) we also include the quarterly unemployment rate and GDP growth in the initial quarter of the benefits spell. In both cases, the effect remains small and statistically non significant. Finally, in column (5) we consider all spells except those begun in the second half of 2000, because they could be partially exposed to the regime change, and those begun in the second half of 2002, because of potential censoring. We find again no evidence of longer duration on benefits; beyond being not statistically different from zero, the point estimate turns out to be also economically small, about 8 days.

Overall, the results presented in this section suggest that after the change in the benefits scheme, average time on benefits increased primarily because of the extension granted to older beneficiaries, while there does not seem to be reliable evidence of longer duration traceable to the increase in the replacement rate.

3.3 UI generosity and non-employment

We now turn to the analysis of the duration of non-employment spells. As discussed above, from a behavioral point of view, this is the relevant variable as it reflects individual search effort. In this respect, the increase in the average length of time on benefits documented above does not necessarily imply longer non-employment spells or, alternatively, lower re-employment probabilities. The increase could reflect the higher expiration limit faced by older people so that a larger fraction of the non-employment spell is spent on benefits. Alternatively, absence of an effect on time on benefits among younger job losers could hide effects on exit rates not strong enough to significantly increase average time on benefits within the (unchanged) maximum

benefits window.

A first hint on the effects of the reform on the length of non-employment spells can be obtained from visual inspection of figure (3). We plot non-parametric estimates of the survival rate in non-employment by age-class and initial year of the spell both for UI beneficiaries and non beneficiaries. Among younger job losers, only affected by the higher replacement rate, beneficiaries in 2000 and 2001 do not display different patterns of re-employment, while among non-beneficiaries there appears to be some mild evidence of a slight slow down in re-employment patterns. Among older beneficiaries, also entitled to a longer benefits duration, re-employment is slower in 2001, with a lower proportion of reemployed individuals after 26 weeks of non-employment, corresponding to the pre-reform expiration limit; however, a slowdown of reemployment shows up also among older non beneficiaries.

A more precise assessment of the effects of the change in the benefits scheme on the probability of re-employment is reported in tables (3) and (4). We estimate a set of linear models for the probability of being again in employment at several horizons after job termination. Specifically, we estimate:

$$e_{it} = \alpha + X_{it}\beta + \delta E_{it} + \epsilon_{it} \tag{2}$$

where e_{it} is a dichotomous variable equal to unity if after a separation at time t , i is observed again in employment within the time window reported in the column headings; X_{it} is a set of controls and E_{it} is the exposure dummy¹².

Table (3) focuses on the effects of the increase in the replacement rate. In this setting, the exposure dummy $E_{it} = 1$ if termination occurs in 2001 or 2002. As before, we only consider

¹²An alternative strategy would involve estimation of (semi-)parametric duration or hazard models. Indeed, such models are more efficient with respect to a set of multivariate analyses as performed here at the cost of specific functional form assumptions. As to our linearity assumption, results are substantially unaffected if estimating non-linear bivariate models such as logit or probit. We therefore keep the linearity assumption.

persons aged 20-48 not entitled to the longer duration whose non-employment spell begins in the first half of each year. Recall also that the only source of identification for the effects of a higher replacement rate is over time, as the increase was granted to all beneficiaries from 2001 onwards. In the top panel we focus only on benefits beneficiaries. The first row reports results from estimates of (2) on terminations from 2000-2001 and no additional controls; the second row augments the specification with dummies for the initial week of non-employment, sex and macroarea, a sex-specific age effect and (log of) tenure, last and average wages at the previous job. In the third row, we extend the sample to the whole 1997-2001 period and augment the control set with a cubic in calendar time to control for the possibility of pre-existing trends. In none of the exercise we detect significant differences in the probabilities of reemployment. In the above exercises inference is based on comparisons of non-employment spells over time. Therefore, we must assume that no aggregate shock is driving the (common) pattern of re-employment and possibly compensating the behavioral effects of a higher replacement rate. To address this issue in the bottom panel we include in the sample also non-beneficiaries. This allows to control explicitly for time effects under the assumption that these are common to beneficiaries and non-beneficiaries. We thus replicate the exercises reported in the top panel with the addition of a full set of area-year dummies and a claimant dummy (C) capturing systematic differences between the two groups. Notice that benefits collectors display a systematically lower probability of reemployment at all horizons. The effect of the higher replacement rate on the probability of reemployment (E) turns out to be sizeable and significant only at longer horizons, beyond benefits expiration: after nine months, that is three months after expiration, benefit collectors exposed to a higher replacement rate are around 10 percentage points less likely to be back in employment than comparable collectors in earlier years. Moreover, the dif-

ference appears to be too large: to put it in context, it amounts to half the systematic difference in reemployment probabilities between beneficiaries and non-beneficiaries while the change in replacement rate was quite modest, from 30 to 40 percent. Moreover, it is hardly compatible with the lack of sizeable effects detected in the analysis of time spent on benefits displayed in table (2).

Table (4) focuses on the effects of the longer potential duration. In this case, the exposure dummy $E_{it} = 1$ if termination occurs in 2001 and age at termination is above 50. Because the change in benefits duration is age specific, the exposure dummy varies within year so that we can account for aggregate time effects while focussing on benefits beneficiaries only. Specifically, we focus on beneficiaries aged 40-48, not exposed to an increased duration, and those aged 51-60, from 2001 entitled to the longer benefits duration¹³. The first row reports the estimated effect of a longer duration on re-employment probabilities at the various horizons obtained from simple comparisons of re-employment patterns of benefits beneficiaries in 2001. We only detect some significant difference at longer durations. In the second row we control for log tenure, last wage and average wage at the previous job, area, initial week and sex dummies. We still find a significantly lower reemployment probability at longer horizons. However, identification of this effect relies heavily on the assumption that, absent the change, beneficiaries aged 40-48 and 51-60 would display the same re-employment patterns. This could fail if, for example, older non-employed have less incentives to search for a new job as retirement is closer. To address the role of this assumption in the third row we extend the sample to include also pre-2001 non-employment spells. This allows to introduce an explicit control for age, which we let vary with sex¹⁴. Under this specification the point estimate of the effect of exposure to a longer potential

¹³Results are broadly unchanged if we restrict the age-comparisons to 44-48 vs 51-55.

¹⁴An explicit control for age could have been introduced also in the exercise using only 2001 non-employment spells. However, it would have resulted highly collinear with the entitlement to longer benefits thus weakening

duration shrinks considerably, in the range of 2-4 percentage points, becoming statistically non significant.

3.4 Potentially confounding effects of the increased generosity and potential duration

The institutional change under assessment involves an increase in benefits generosity common to all beneficiaries and a lengthening of potential duration for job losers beyond age 50. Such changes are likely to affect not only the job search behavior of beneficiaries and thus their duration in non-employment, but also the behavior of previously non-beneficiaries. If such effects are in place, the increased generosity would generate a change in the pool of beneficiaries and thus potentially invalidate comparisons of relevant outcomes across UI regimes.

More generous benefits can induce some marginal job losers to start collecting benefits. In fact, those who actually collect benefits are a non-random subset of the non-employed. This can be clearly seen from figure (4) where we report predicted probabilities of collecting benefits by beneficiary status. Predicted probabilities are obtained with a probit model estimated for year 2000, before the change occurred, and considering sex, a cubic in age, dummies for the three main areas of residence and (log) last wage and tenure. Ideally, if beneficiary status were randomly assigned to the non-employed we would have observed two overlapping boxes; on the contrary, the figure clearly shows that beneficiaries are more likely to be such because of their observable characteristics¹⁵. Therefore, even absent changes in the pool of non-employed, if the new benefits scheme alters the pool of beneficiaries, comparisons of non-employment spells

considerably the inference. On the contrary, a longer time dimension provides enough degrees of freedom to identify both coefficients.

¹⁵Of course, observing the same predicted probability would only say that beneficiary status is independent of the set of regressors considered in the specific probit model.

under different regimes would not provide valid inference on the effects of UI. This may occur, for example, if the relative utility of collecting benefits changes with the benefits scheme so that some otherwise non-claimant individuals may now find it preferable to cash in UI. Figure (5) plots the share of jobless collecting benefits by month-year of job loss for individuals aged 20-48, only affected by the increase in the replacement rate, and for those aged 51-65, also affected by the extension of benefits duration. We define takeup as an employment spell followed by a spell on benefits, while non-beneficiaries are those who are observed again in employment without an intervening spell on benefits¹⁶. The picture shows a downward trend until 2000 and then some evidence of an increase in 2001. Between 2000 and 2001 the take-up rate increased by about 1 percentage point for both groups. Although this pattern may reflect macroeconomic conditions rather than a behavioral response to a change in unemployment insurance, it suggests that the pools of beneficiaries before and after the change may not be fully comparable.

To investigate further this aspect, we ask whether beneficiaries' characteristics are associated with exposure to the relevant treatment. First, in table (5) we describe a set of relevant characteristics of beneficiaries in 2000 and 2001 for those aged 20-48, only entitled to the higher replacement rate, and those aged 51-65, also entitled to a longer duration. The results show that differences between the two groups of beneficiaries are not statistically significant so that, at least as concerns the available observable characteristics, beneficiaries before and after the change appear to be broadly comparable. This is largely confirmed from a second, slightly more elaborate, exercise. Specifically, we estimate a probit model for the probability that a beneficiary in years 2000-2001 is also exposed to treatment, which in our setting amounts to collecting benefits in year 2001, on a cubic in age, dummies for sex and area of residence,

¹⁶Specifically, we retain in the sample only individuals that after an employment spell are observed again, either on benefits or in employment. We drop some observations for beneficiaries for whom benefits spells are observed much later than job termination date.

and log wage and tenure at last job. The models are estimated separately for beneficiaries only exposed to a higher replacement rate (age 20-48) and for those also exposed to a longer potential duration (51-65). Figure (6) plots the predicted probability of being in the treatment group (i.e. claiming benefits in 2001) for beneficiaries aged 20-48 (top panel) and 51-65 (bottom panel), against the actual year when benefits are cashed. The exercise confirms that younger beneficiaries in 2001 are not different from those in 2000, while some difference emerges for older ones, who represent also a much smaller sample.

A second potentially harmful effect could come from the separation behavior of current employees in reaction to the more generous system. Changes to the unemployment benefits schemes may affect employment and job turnover through their effect on the outside option of workers who may then find optimal under the new regime to terminate certain jobs, collect benefits and search for a new more suitable one. While a fully-fledged analysis of such margin is outside the scope of the paper, in figure (7) we report the jobs terminated each month as a share of those existing at the beginning of each month. The top panel reports separation rates for the entire observed population; the middle and bottom panels display separation rates for individuals aged 20-48 at job termination, potentially affected by the increase in replacement rates from 2001 onwards, and for those aged 51-65, potentially affected by both the higher replacement rate and the extended benefits duration. The figures show a remarkably stable pattern over time. There is no clean evidence of an increase in separation rates across the relevant discontinuities, suggesting that the institutional change had no major effect on the composition of the pool of potential beneficiaries.

Finally, absence of any sizeable effect of the higher replacement rate may be due to the fact that caps to benefits payments were binding for a sizeable portion of potential claimants before

the policy change. In 2000 benefits were capped to about 745 euros per month if the relevant monthly wage was below 1611 euros and to 895 if it was above; in 2001 such thresholds were updated, as happens annually, to account for inflation and brought to, respectively, 760 and 913 euros if the wage was below or above 1645 euros per month. Figure (8) displays the distribution of monthly wages of job losers in 2000 and 2001 against the actual benefits payments according to the relevant schedules in 2000 and 2001. It is clear that binding caps are not a major concern.

Summing up, the above evidence suggests that, over the observation window and conditional on the limited available information on observable characteristics, the increased UI generosity did not cause significant changes along the observable characteristics in the composition of the pool of actual beneficiaries nor in the (larger) pool of job losers, thus justifying the comparisons discussed above.

4 Conclusion and further questions

In this paper we have documented how time on benefits and time to next job responded to a change in unemployment benefits scheme in Italy. The change implied an increase in replacement rate from 30 to 40% of the last wage and, exclusively for beneficiaries aged at least 50, an increase in the maximum duration of benefits from 6 to 9 months. The change was passed at the end of 2000 and phased in January 1st 2001.

We exploit a representative sample of administrative records that allows to recover the precise dates at which job relationships start and terminate, whether termination is followed by a spell on benefits and its duration. The data allow to recover wages and tenure of the workers along with a limited set of socio-demographic characteristics (sex, age, birthplace, residence); importantly, we have no information on household composition, education and other important

determinants of the labor supply choice.

Not surprisingly, we find that *recorded* time on benefits increased for individuals entitled to the extended benefits duration. A set of exercises based on comparisons of benefits duration of entitled and non-entitled beneficiaries that control for a number of individual characteristics and time effects suggest that the average spell on benefits increased by 20-30 days for people entitled to the extension. On the contrary, we do not find significantly longer duration on benefits among younger beneficiaries only entitled to the higher replacement rate.

We then study how re-employment probabilities were affected by the more generous UI schedule. Our exercises do not detect lower re-employment probabilities for beneficiaries only entitled to a higher replacement rate. When we limit the analysis to before/after comparisons within beneficiaries we fail to find differences comparing beneficiaries in 2000 (before) with beneficiaries after (2001) the change; extending the sample to include earlier claims (pre-2000) in the control group even yields a surprising higher employment likelihood for beneficiaries paid a higher replacement rate. We are able to detect a lower re-employment probability only at longer horizons (beyond 180 days) when the sample includes also non-beneficiaries, which allows to control for common time effects. In such exercises, the probability of re-employment at horizons longer than 6 months turns out to be lower by about 10 percentage points for higher replacement rate beneficiaries, an effect implausibly large considered the limited amount of additional resources provided by the higher replacement rate. As regards the extension of the duration limit, we are able to detect some effect on the likelihood of re-employment, but these results become much smaller, around of 2-4 percentage points lower probability of reemployment at all horizons, and statistically non significant when we account for differences in the age profile of re-employment patterns.

As most of our exercises are based on comparisons of the relevant outcome variables over time for observationally similar individuals, results are potentially tainted by changes in the composition of benefits collectors, perhaps induced by the more generous system itself. We thus provide some descriptive evidence on the evolution of job-separation and take-up rates across the relevant discontinuities. While we find no evidence that the change in the benefits scheme was associated with changes in the separation rates, we unveil some mild evidence that takeup rates slightly increased. However, the underlying time series shows a marked cyclical pattern and the available data do not allow for a clearcut conclusion on the nature of such empirical association. Yet, it raises concerns that pre- and post-reform beneficiaries may be different. We provide evidence that, as concerns observable characteristics, this seems not to be the case, however.

Our reading of the results is that the changes in the main features of the ordinary unemployment benefits scheme had at best mild effects on the probability of re-employment. It is worth keeping in mind that: (a) our findings are local, in the sense that they refer to the specific sample and institutional change investigated; there is no guarantee that larger increases in replacement rates would not significantly affect labor supply. (b) The assumptions under which our estimates reflect causal effects are strong, especially as concerns the response to a higher replacement rate. (c) We only look at the first year the change was phased in; however, anecdotal evidence suggests that little information was available on the new regime so that the choices we observe could have been made assuming an unchanged benefits scheme; more data are thus needed for a broader assessment. as the only source of identification is time variation in re-employment patterns. (d) The limited sample size often leads to very imprecise estimates.

The aim of this preliminary investigation was to simply document, and possibly quantify,

the responses to a change to the unemployment benefits scheme. While we did not find much evidence of a behavioral response, it is worth emphasizing that in our framework detecting a response in line with theoretical predictions would have still left open an important question about the sources of such reaction. The main purpose of unemployment insurance schemes is to provide insurance against the event of job loss. Insurance is beneficial because it allows to smooth consumption across states and to improve welfare. However, because agents can manipulate their search behavior, providing insurance also distorts the relative price of leisure and consumption and reduces the incentives to work¹⁷. This substitution effect is generally claimed to be the reason why unemployment insurance programs tend to increase unemployment duration. This view neglects another important effect of social insurance programs, namely the fact that it relaxes liquidity constraints by providing income. This additional effect also leads to an increase of unemployment duration but only for individuals who cannot smooth consumption. The two channels through which unemployment insurance affects unemployment duration, the substitution and the liquidity effects, have opposite welfare effects. The first response is socially suboptimal as it is an individually optimal response to a distortion in relative prices; the second is a socially optimal response to failures in financial markets. Therefore, an assessment of the relative importance of these channels is crucial for the design of an efficient unemployment benefits scheme. In particular, from a public policy point of view, while such a system should attempt to minimize the distortions entailed by the substitution effect, it should carefully consider the beneficial effects it produces in the presence of imperfect credit

¹⁷This moral hazard is at the heart of a vast literature studying optimal insurance schemes (Hopenhayn and Nicolini (1997), Shavell and Weiss (1979)), that is how to balance the trade-off between insurance and incentives to search. The main conclusion is that an optimal unemployment scheme requires benefits to decay with unemployment duration. Pavoni and Violante (2007) study a more complex framework in which different policies (unemployment insurance, monitoring of job search, social assistance, wage subsidies) can be combined. Also in their setting UI decreases over time, and is eventually substituted by a combination of social assistance and no labor market participation. For Italy, Bobbio (2011) shows that the payment schedule of ordinary unemployment benefits is rather close to the constrained efficient one with a minimum utility bound.

markets by relaxing liquidity constraints and moving the individual decision towards the optimal unconstrained one.

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Figure 1: The evolution of replacement rates in Italy: ordinary unemployment benefits.

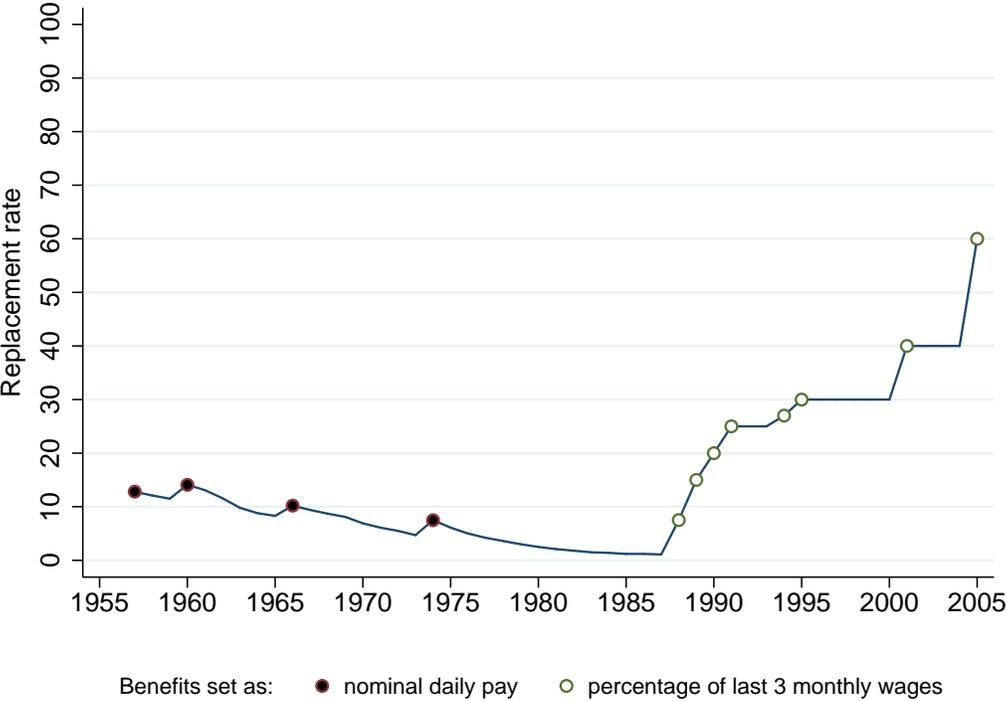


Figure 2: The distribution of time on benefits.

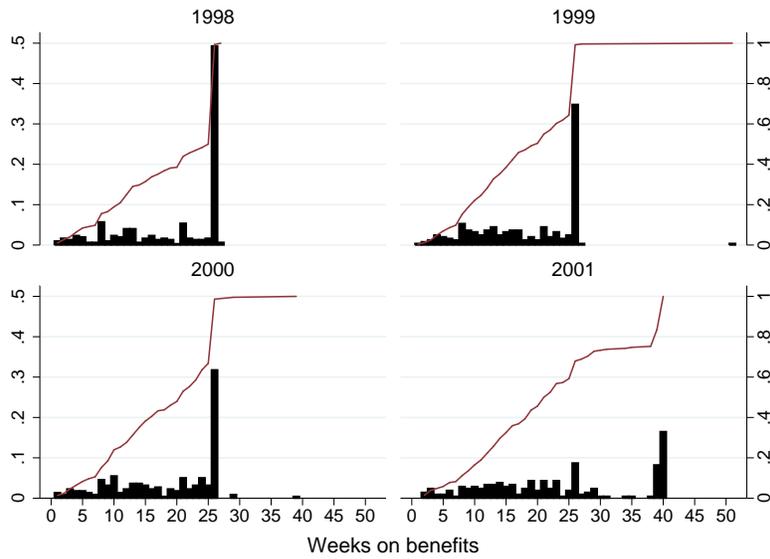
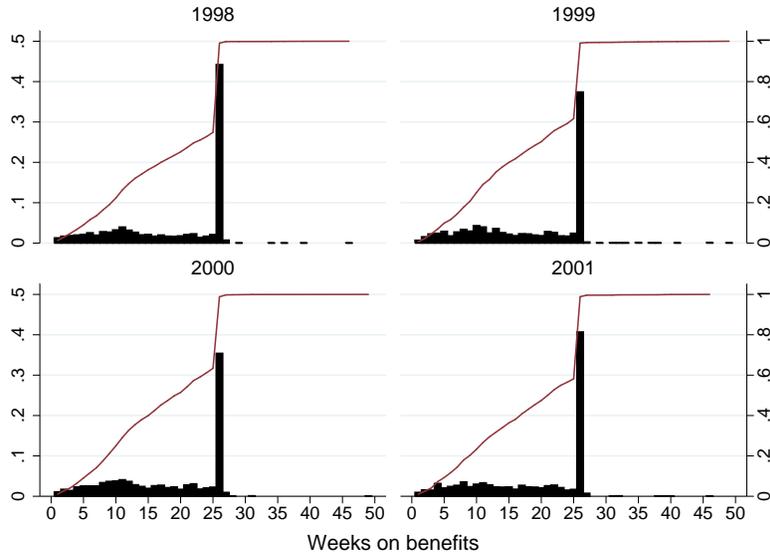


Figure 3: Survival rates into non-employment.

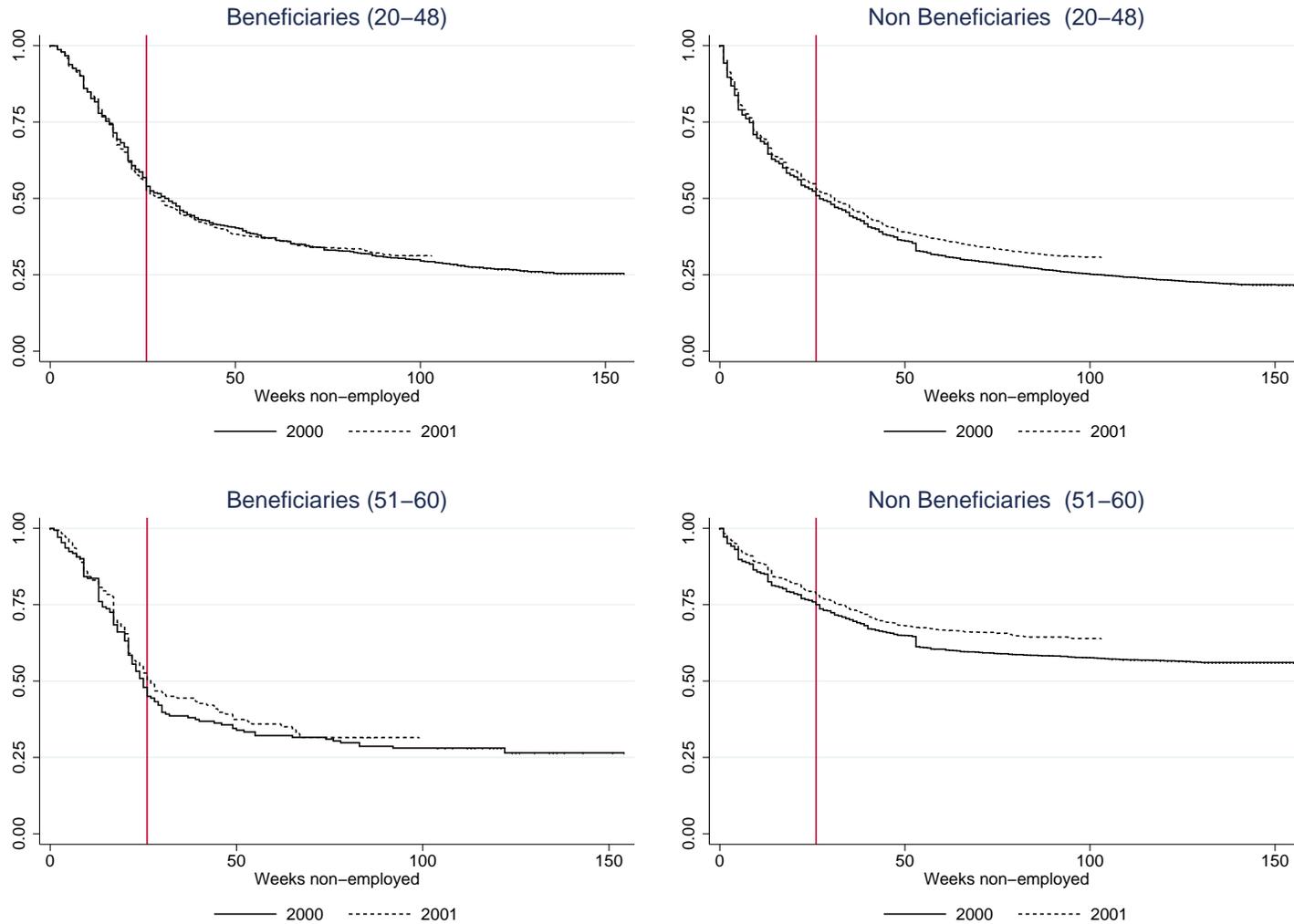
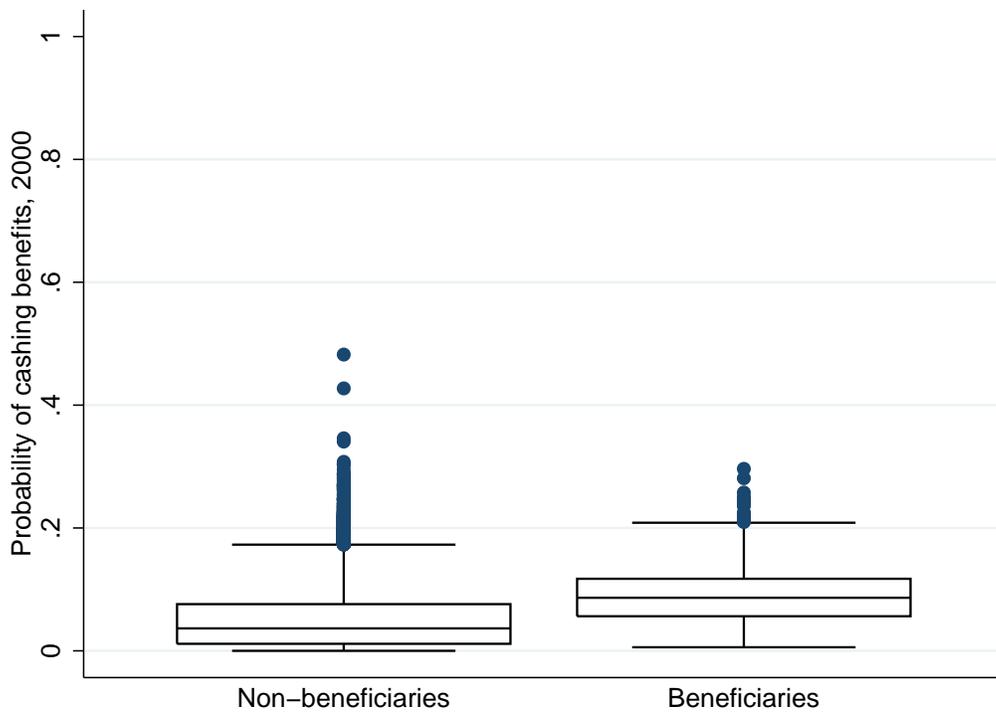
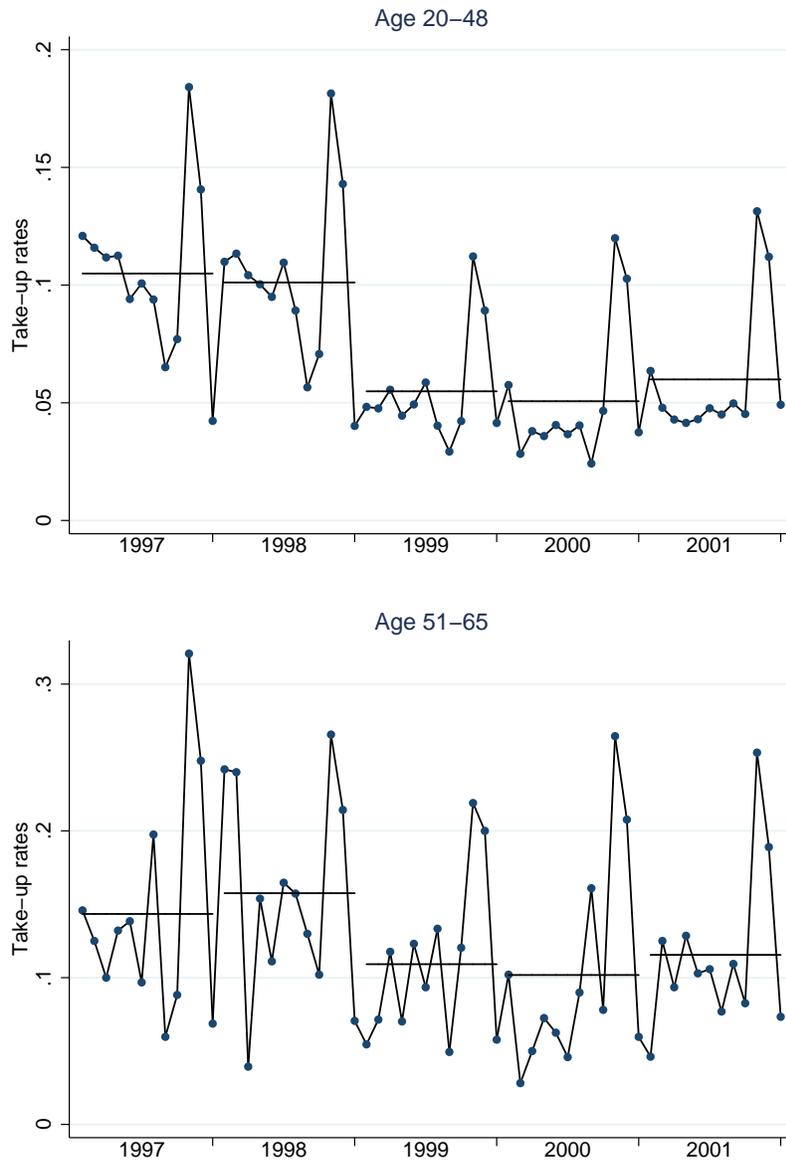


Figure 4: Probability of receiving benefits.



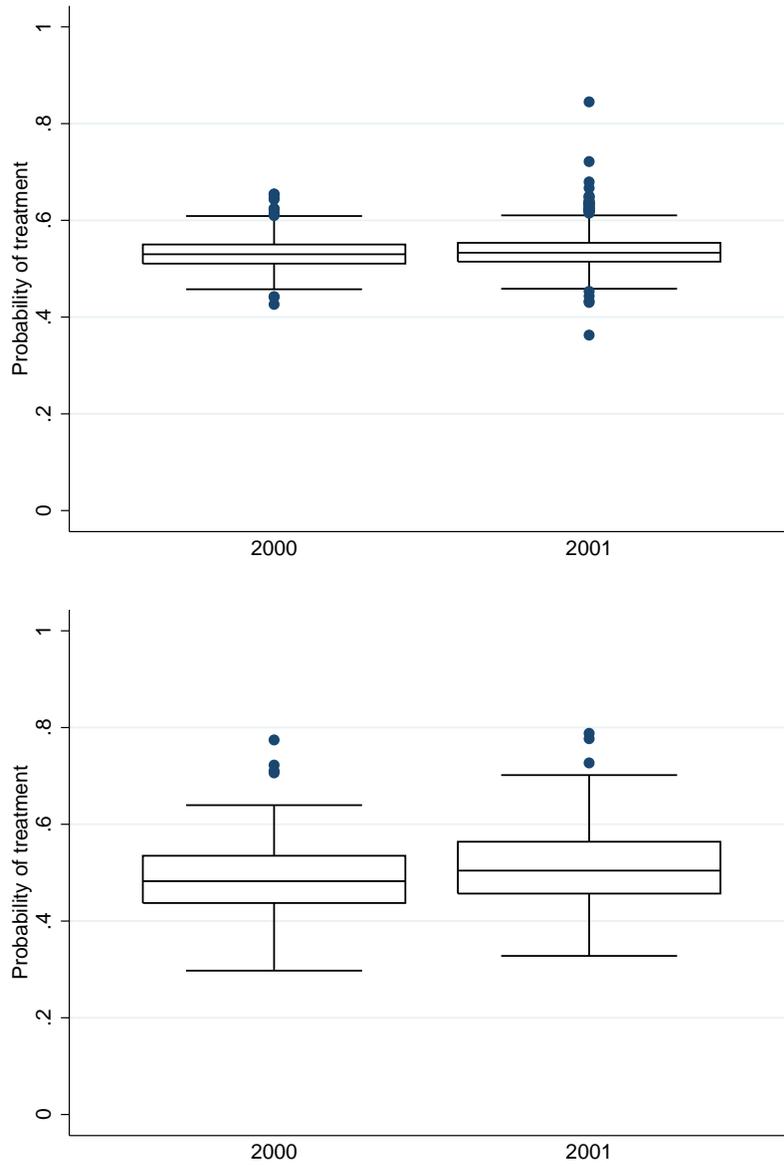
Predicted probabilities are obtained from a probit regression of beneficiary status on the sample of individuals whose non-employment spell begins in 2000 and who enter benefits (beneficiaries) or are observed again in employment (non-beneficiaries). The conditioning set includes log of tenure, alst wage and average wage at the previous job, a cubic in age, dummies for sex and three macroareas of residence.

Figure 5: Take-up rates.



Take-up is defined as an employment spell followed by a spell on benefits; the underlying population is employment spells followed by either spell on benefits (takeup) or in employment (no takeup). Figures report monthly and average yearly take-up rates.

Figure 6: Probability of treatment.



Predicted probabilities are obtained from a probit regression of year being 2001 on the sample of beneficiaries 2000-01. The conditioning set includes log tenure, last wage and average wage at the previous job, a cubic in age, dummies sex and for three macroareas of residence.

Figure 7: Monthly job separation rates.

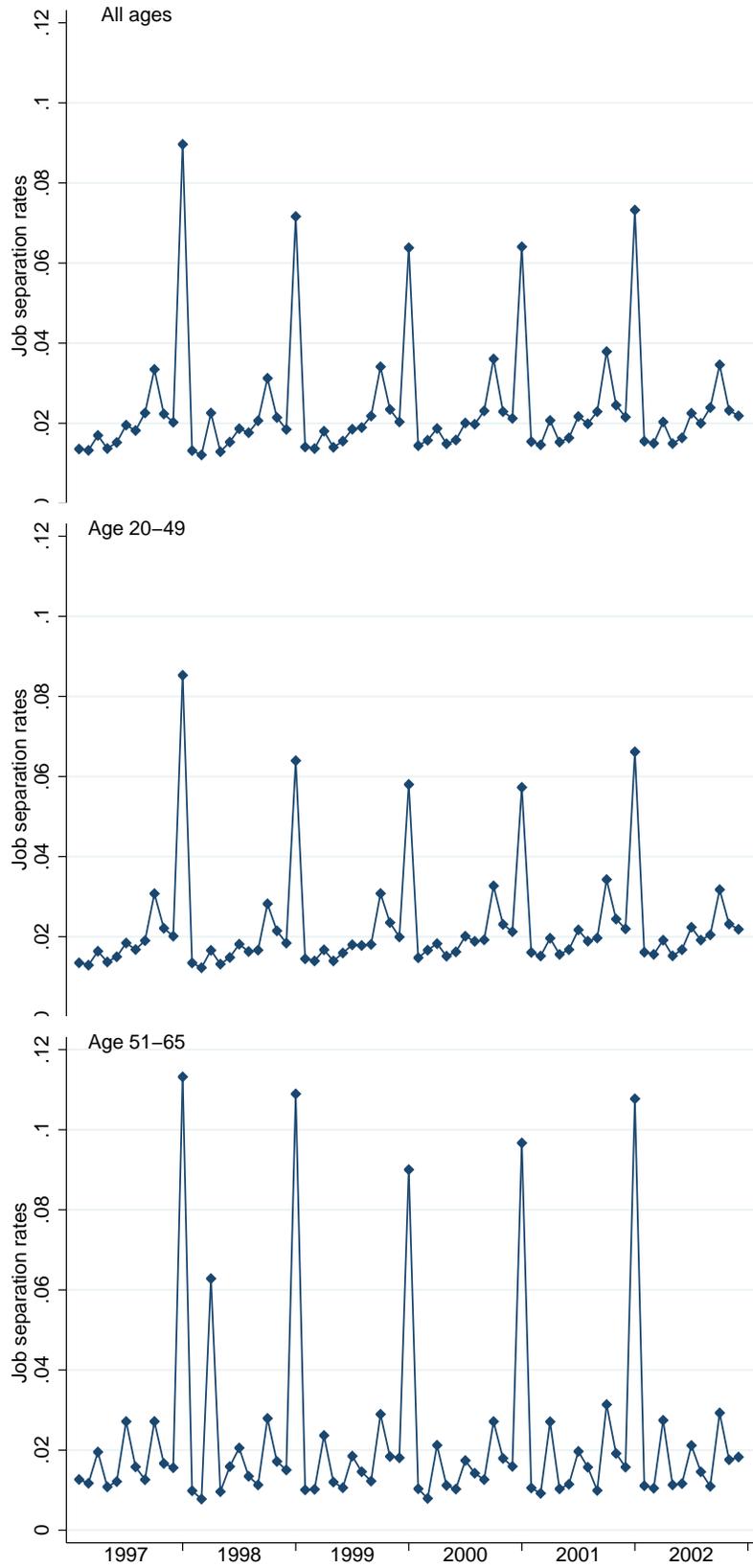


Figure 8: Wage distribution at separation and UB schedules.

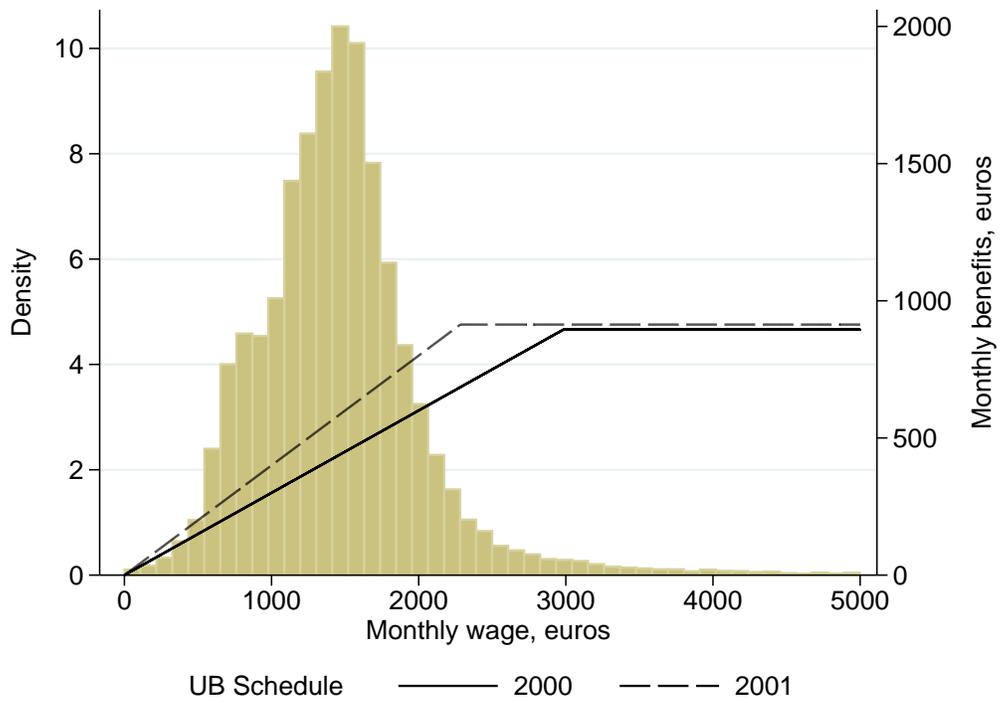


Table 1: The effects of benefits extension.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
								Placebo	
E	31.9** (6.7)	29.0** (7.0)	34.0** (12.4)	25.7** (9.9)	21.6 [†] (12.2)	21.7* (9.2)	29.5** (4.5)	2.4 (2.8)	5.3 (6.3)
Years	2000/2001	2000/2001	1997/2001	2001	2000/2001	1997/2001	1997/2001	1997/2000	1997/2001
Age	51-65	51-65	51-65	45-48/51-55	45-48/51-55	45-48/51-55	20-48/51-65	20-48/51-65	20-44/45-48
Obs.	473	389	1124	225	440	1079	10195	8507	9071
R^2	0.05	0.38	0.23	0.03	0.28	0.20	0.11	0.11	0.10

Standard errors in parentheses. ([†]) significant at 10%; (*) significant at 5%; (**) significant at 1%

Dependent variable: days on ordinary benefits. Table entries are estimates for a dummy for treatment exposure. Treatment exposure: $E = 1$ if $\text{age} \geq 51$ and $t = 2001$ in cols. (1)-(7); $E = 1$ if $\text{age} \geq 51$ and $t = 2000$ in col. (8); $E = 1$ if $\text{age} \in [45 - 48]$ and $t = 2000$ in col. (9).

Controls - col. (1): none; col. (2): area, initial week and sex dummies, sex-specific linear age term, log of tenure, average and last wage at the previous job; col. (3): area, initial week and sex dummies, sex-specific linear age term, log of tenure, average and last wage at the previous job, cubic in time (quarters), quarterly unemployment and GDP growth rates; col. (4): none; col. (5) year, initial week and sex dummies, sex-specific linear age term; col. (6)-(7): area, year, initial week and sex dummies, sex-specific linear age term, log of tenure, average and last wage at the previous job, quarterly unemployment and GDP growth rates; cols. (8)-(9): area, year, initial week and sex dummies, sex-specific linear age term, log of tenure, average and last wage at the previous job, quarterly unemployment and GDP growth rates.

Table 2: The effects of replacement rate increase.

	(1)	(2)	(3)	(4)	(5)
E	5.8 (3.5)	0.6 (3.6)	4.2 (5.3)	2.1 (7.9)	8.7 (5.3)
Years	2000/2001	2000/2001	1997/2002	1997/2002	1997/2002
Semester	1st	1st	1st	1st	No 2:00, 2:02
Age	20-48	20-48	20-48	20-48	20-48
Obs.	1264	1060	4263	4263	8871
R^2	0.00	0.14	0.12	0.12	0.11

Standard errors in parentheses. (†) significant at 10%; (*) significant at 5%; (**) significant at 1%

Dependent variable: days on ordinary benefits.

Table entries are estimates for a dummy for treatment exposure. Treatment exposure: $E = 1$ if spell begins in 2001-2002. Controls - col. (1): none; col. (2): area, initial week and sex dummies, sex-specific linear age term, log tenure, average and last wage at the previous job; col. (3): area, initial week and sex dummies, sex-specific linear age term, log tenure, average and last wage at the previous job, cubic in time (quarters); cols. (4)-(5): area, initial week and sex dummies, sex-specific linear age term, log tenure, average and last wage at the previous job, cubic in time (quarters), quarterly unemployment and GDP growth rates.

Cols. (1)-(4) only use observations from first semester of each year; col. (5) uses all observations except spells beginning in the second semester of 2000 and of 2002.

Table 3: Replacement rate and probabilities of reemployment.

	Days to next job \leq :				
	30	60	180	270	330
Only beneficiaries					
Specification 1					
E	0.015 (0.020)	-0.005 (0.030)	-0.03 (0.040)	-0.05 (0.041)	-0.05 (0.040)
Specification 2					
E	0.02 (0.021)	-0.001 (0.030)	-0.04 (0.041)	-0.05 (0.042)	-0.05 (0.041)
Specification 3					
E	0.07 (0.053)	0.05 (0.086)	0.17 (0.13)	0.15 (0.13)	0.13 (0.13)
All non-employed					
Specification 4					
E	0.015 (0.036)	-0.005 (0.040)	-0.03 (0.039)	-0.05 (0.035)	-0.05 (0.031)
C	-0.22** (0.027)	-0.25** (0.030)	-0.24** (0.029)	-0.21** (0.026)	-0.23** (0.023)
Specification 5					
E	0.03 (0.036)	-0.015 (0.040)	-0.07 [†] (0.039)	-0.10** (0.035)	-0.11** (0.031)
C	-0.26** (0.027)	-0.25** (0.029)	-0.21** (0.029)	-0.19** (0.026)	-0.21** (0.023)
Specification 6					
E	-0.01 (0.025)	-0.04 (0.028)	-0.10** (0.030)	-0.10** (0.028)	-0.11** (0.026)
C	-0.21** (0.010)	-0.23** (0.012)	-0.18** (0.012)	-0.18** (0.012)	-0.20** (0.011)

([†]) significant at 10%; (*) significant at 5%; (**) significant at 1%
Standard errors in parentheses. Table entries are estimates of the coefficient for a dummy E indicating exposure to the regime change (i.e. beneficiary in 2001) and one indicating being a beneficiary (C). All samples only include persons aged 20-48 at the beginning of the non-employment spell.

Specs. 1: non-employment spells beginning in 1st semester of 2000-2001; no controls. Spec. 2: non-employment spells beginning in 1st semester of 2000-2001; area, initial week and sex dummies, sex-specific age effect, log of tenure, last wage and average wage at previous job. Spec. 3: non-employment spells beginning in 1st semester of 1997-2001; area, initial week and sex dummies, sex-specific age effect, log of tenure, last wage and average wage at previous job, a cubic in calendar time (quarters). Specs. 4: non-employment spells beginning in 1st semester of 2000-2001; claimant dummy. Spec. 5: non-employment spells beginning in 1st semester of 2000-2001; year, area, initial week sex and claimant dummies, sex-specific age effect, log of tenure, last wage and average wage at previous job. Spec. 6: non-employment spells beginning in 1st semester of 1997-2001; year, area, initial week sex and claimant dummies, sex-specific age effect, log of tenure, last wage and average wage at previous job.

Table 4: Benefits duration and probabilities of reemployment.

	Days to next job \leq :				
	30	60	180	270	330
Only beneficiaries					
Specification 1					
E	-0.023 (0.020)	-0.003 (0.035)	-0.072 (0.052)	-0.119* (0.049)	-0.138** (0.046)
Baseline	0.04	0.13	0.56	0.68	0.72
Specification 2					
E	-0.025 (0.021)	0.005 (0.037)	-0.09 [†] (0.050)	-0.121* (0.047)	-0.146** (0.045)
Baseline	0.04	0.13	0.56	0.68	0.72
Specification 3					
E	-0.024 (0.020)	0.030 (0.035)	-0.015 (0.051)	-0.026 (0.050)	-0.044 (0.049)
Baseline	0.03	0.11	0.50	0.63	0.66

([†]) significant at 10%; (*) significant at 5%; (**) significant at 1%
Standard errors in parentheses. Table entries are estimates of the coefficient for a dummy E indicating exposure to the regime change (i.e. beneficiary in 2001 and 50 or older).

Spec. 1: non-employment spells beginning in 2001, persons age 40-48 and 51-60; no controls. Spec. 2: non-employment spells beginning in 2001, persons age 40-48 and 51-60; initial week, area and sex dummies, log of tenure, last wage and average wage at previous job. Spec. 3: non-employment spells beginning in 1997-2001, persons age 40-48 and 51-60; year, initial week, area and sex dummies, sex-specific age effect, log of tenure, last wage and average wage at previous job.

Table 5: Differences in beneficiaries' characteristics, 2000 vs. 2001.

	20-48			51-65		
	Paid in:		Diff	Paid in:		Diff
	2000	2001		2000	2001	
Age	34.0	34.4	0.4 (0.29)	55.3	55.3	0.06 (0.4)
Sex	0.48	0.50	0.015 (0.022)	0.47	0.45	-0.02 (0.056)
South	0.37	0.40	0.03 (0.02)	0.36	0.36	-0.01 (0.054)
Tenure at previous job (log)	5.83	5.89	0.06 (0.054)	5.85	5.93	0.075 (0.15)
Last wage (log)	3.98	4.02	0.04 [†] (0.022)	3.98	3.99	0.01 (0.053)
Average wage at last job (log)	3.88	3.89	0.007 (0.017)	3.93	3.89	-0.032 (0.054)

Standard errors in parentheses. (†) significant at 10%; (*) significant at 5%; (**) significant at 1%

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