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Incentive-based active labor market programs: Insights from policy experimentation in Italy ☆

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ABSTRACT

Two programs providing financial incentives for reemployment of workers at risk of long-term unemployment are evaluated vis-à-vis intensive job-search assistance through policy experimentation involving about 10,000 job seekers in Italy: (i) a reemployment *voucher* that incentivizes specialized providers; (ii) a reemployment *bonus* that incentivizes job seekers directly. Results indicate that: the voucher is effective for men while the bonus works for women; each policy is no less effective than job-search assistance, but only the voucher is clearly cost effective; there are no side effects on post-treatment earnings or job duration. A one-sided job search model with endogenous search effort rationalizes these empirical findings.

1. Introduction

Finding cost-effective methods to expedite reemployment compared to traditional job-search assistance (JSA) is a question that arouses academic and policy interest. This interest stems from the contrast between the substantial public resources devoted to conventional active labor market policies (ALMPs) in OECD countries and their demonstrated effectiveness.[3] While Card, Kluve, and Weber (2018), in a meta analysis of 207 studies, conclude that the impact of JSA on reemployment is positive, Crépon and Van Den Berg (2016) note that these positive effects appear quite modest when compared to the resources deployed to generate them. According to these authors "the general outlook for ALMPs is rather grim. On the whole, evaluations have not shown these programs to be particularly effective" (p. 541). Motivated by these considerations, several authors have recently investigated innovative, low-cost forms of active labor market policy, reaching more optimistic conclusions. Prominent among these recent studies, Altmann et al. (2018) show that a simple brochure containing information about the labor market's state and basic job search advice helps the long-term unemployed in Germany returning to a job; Belot et al. (2019) implement a low-cost intervention in the UK that provides job seekers with targeted occupation advice via a search platform linked to a job vacancies database, finding a positive impact on job search breadth and job interviews; Briscese et al. (2022) demonstrate the effectiveness of online resources aimed at tailoring resumes and cover letters, which supplement the assistance traditionally provided by public employment service staff in Australia; and Carranza et al. (2022) find that providing job seekers with information on their assessed skills improves reemployment outcomes in South Africa.

We contribute to this research program with a new evaluation of other unconventional policies based on financial incentives, rather than information, to accelerate the exit from unemployment for workers

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[3] According to OECD public social expenditure statistics, expenditure in ALMPs (including program administration costs) during 2019, expressed as a percentage of GDP, was 0.10 in the United States, 0.41 in Canada, 0.15 in the United Kingdom, 1.86 in Australia, 0.27 in Italy, 0.59 in Germany, 0.72 in France, and 1.02 in Sweden.

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at risk of entering a state of long term joblessness. In doing so, this paper advances our understanding of the potential effectiveness of ALMPs. Our evaluation is based on two new policy experiments designed and implemented in collaboration with the Public Employment Service (PES) of Trento Province, Italy, during 2017–2018, to test the effectiveness of incentive-based activation schemes vis-à-vis JSA. These experiments involved about 10,000 job seekers in their fifth month of unemployment. The first is a RCT in which about 1300 job seekers were assigned to one of two treatment arms: (i) intensive JSA provided by an external agency; and (ii) a reemployment *voucher* consisting of a lump-sum payment of up to €5000 to these same external service providers if a job seeker is reemployed. While for JSA the provider is chosen by the PES, job seekers may choose the provider when using the voucher. Moreover, participation was not mandatory, which allows us to study the demand for ALMPs. A control group of similar size could not receive JSA or a voucher. The second experiment, involving about 7300 individuals, tests a reemployment *bonus*, i.e., a lump-sum payment of up to €3000 offered directly to job seekers who find a new job. This bonus could not be randomized, but it is subject to date-of-birth cutoffs that provide local randomization in a sharp regression discontinuity design. Therefore, strictly speaking, the bonus is evaluated in a quasi-experimental way; however, for convenience, we will refer to this evaluation as an "experiment" (like for the voucher) throughout the paper.

Vouchers for reemployment and training services provided by a specialized third party were adopted in Germany in the early 2000s (Ayaita et al., 2022), and a voucher-like system is in place in Australia since the early 1990s (Davidson and Whiteford, 2012). However, none of these were evaluated experimentally. The existing experimental evaluations concern voucher programs for small groups of economically disadvantaged, displaced, or disabled job seekers in the US (Barnow, 2009; Barnow and Smith, 2003). As for the reemployment bonus, experiments were undertaken in Illinois, New Jersey, Pennsylvania and Washington during the 1980s (Meyer, 1995; Decker and O'Leary, 1995; O'Leary et al., 2005) and in the Netherlands in the 1990s (Van der Klaauw and Van Ours, 2013). In the US, effects were generally positive but not large enough to make RB a cost-effective policy—except for job seekers more likely to exhaust UI benefits and when the eligibility period was longer. In the Netherlands, the bonus was targeted to workers unemployed for more than one year, who had exhausted UI benefits, and who depended on welfare. The financial incentive did not reduce welfare dependency for these job seekers with poor labor market prospects.

Our paper's novel contribution is twofold. First, a joint experimental evaluation of reemployment vouchers and bonuses for the broad population of job seekers who are collecting UI benefits in a European labor market. Second, an assessment of the relative effectiveness of JSA and incentive-based schemes like the voucher and the bonus in the same labor market, in contrast to existing evaluations in different contexts or labor markets that are hardly comparable. Although our two experiments were implemented one year apart (and so there was no interaction or interference between the two programs), the samples were drawn from the same population and the recruiting strategy was also the same. Thus, estimates are fully comparable, except that while the voucher and JSA treatment effects are averaged across all age groups, the bonus effects can only be identified locally at the date-of-birth cutoffs (corresponding to 40 or 50 years of age) where its value increases discontinuously.

We use administrative records from the PES internal data base that are directly linked to the experimental data. Individual outcomes are tracked at the monthly frequency until the end of January 2020, i.e., until labor market disruption caused by the Covid-19 pandemic. The primary outcome in our study is the probability of remaining in the PES ranks past a certain month (i.e., survival in unemployment until the first exit), which is most precisely measured in PES data and which is of primary interest to the PES because enrolled job seekers receive transfers and absorb caseworkers' resources. Additionally, since voucher and bonus schemes provide high-powered incentives to speed up exit from unemployment and so may distort effort towards this goal at the expense of match quality, we also consider two observable secondary outcomes as (imperfect) quality indicators: earnings in the new job, and the hazard of employment in a job that will last for at least six months. In consideration of the importance of gender heterogeneity in the labor market, we conduct the empirical analysis for the full sample and separately for men and women.

Our results can be summarized as follows. First, we confirm that passive labor market policies interfere with ALMP take-up, in line with results from well-identified studies of moral hazard in UI programs (e.g., Black et al., 2003). The take-up rate for voucher and JSA in our experiment is only 8.4%, but increases by up to 20 percentage points (pp) as UI exhaustion approaches. In other words, the demand for ALMPs is affected by the supply of UI.

Second, male job seekers assigned to voucher or JSA become about 5 pp less likely to remain in the PES ranks, an effect that is still detected after 34 months. This is larger than the 2 pp effect reported by Card et al. (2018) for JSA in the medium term. We estimate a corresponding effect for women that is smaller and statistically insignificant. These are intention-to-treat (ITT) effects. Given one-sided noncompliance, using assignment as an instrument for take-up identifies a larger average treatment effect on the treated (ATET).

Third, bonus-eligible women become about 10 pp less likely – at the age-40 cutoff – to remain in unemployment. This effect is insignificant for men and originates during eligibility, which echoes Card and Hyslop's (2005) finding that the impact of the Self-Sufficiency Project – a temporary subsidy for full-time work – vanishes at the end of eligibility. The magnitude of bonus treatment effect is larger than previously found in the US experiments, while gender heterogeneity is similar. The transitory effect of the bonus relative to more persistent JSA and voucher effects confirms that programs with a training component are more effective in the long term, which is one of the central conclusions of Card et al. (2018).

Fourth, conditional on reemployment, there are no differences in post-experimental earnings across treatment groups. Similarly, there are no important differences across JSA and voucher treatments in the likelihood of securing new jobs that last at least six months. Thus, the financial incentives embedded in voucher and bonus programs did not lead to visibly worse match quality. Finally, simple cost-benefit calculations show that the voucher was cost-effective relative to JSA, essentially because these two programs generated similar treatment effects but while the PES incurred the cost of JSA regardless of the outcome, the voucher was paid out to successful providers only. Not so for the bonus, which was a windfall gain for many job seekers, confirming the negative assessment of Decker and O'Leary (1995).

These results are interpreted through the lens of a search model where voucher and bonus programs are represented in their interaction with UI. The gender heterogeneity revealed by the experiments, in particular, has an interpretation in the model that is consistent with evidence of gender differences in search behavior (e.g., Le Barbanchon et al., 2020; Cortés et al., 2023). In the model, such differences reflect work disutility, a parameter that embeds market work disamenities like family care costs and commuting time. Due to gender roles in the family (Bergemann and Van Den Berg, 2008), work disutility is higher for women (Kaplan and Schulhofer-Wohl, 2018; Jacob et al., 2019; Le Barbanchon et al., 2020). In this case: (i) the bonus reduces women's likelihood of remaining unemployed more than men's—because the bonus counteracts the opportunity cost of work directly, thus generally reducing the reservation wage and increasing search effort more for women than for men; (ii) the voucher, instead, increases the job-finding rate more for men than for women if job seeker's and provider's search efforts are complements—because in this case the marginal productivity of provider's search effort is lower when the job seeker exerts less effort, so a form of statistical gender discrimination against women arises.[4]

The rest of the paper proceeds as follows. Section 2 presents the institutional setting and the experimental or quasi-experimental designs, Section 3 illustrates the data, Section 4 reports results for the reemployment voucher vis-à-vis intensive JSA, Section 5 reports results for the reemployment bonus, and Section 6 proposes a search framework to interpret the experimental findings in a disciplined way. Section 7 concludes.

2. The policy experiments

The experiments were designed and implemented during 2017–2018 in collaboration with the PES of Trento Province, an autonomous region in northern Italy with a population of about 540k and whose GDP per capita is about 40% above the national average. The unemployment rate in 2018 was 4.8%, less than half the national average of 10.8%; the employment rate was 68.3% (62% for women), vis-à-vis 58.5% (49.6% for women) in Italy.

2.1. Institutional context

The PES delivers employment services through 12 local job centers distributed across the province. A worker who becomes involuntarily unemployed and who has paid social security contributions for at least 13 weeks during the previous 4 years is entitled to benefits from the national UI program (NASpI). This provides a monthly payment (up to about €1300) with a replacement rate of at least 75% of the average gross monthly income during the 4 years preceding the job loss, for a number of weeks equal to half the weeks of social security contribution during those 4 years (up to a maximum of 24 months across multiple unemployment spells), with a monthly reduction of 3% commencing after three months.

In order to qualify for these benefits, job seekers must enroll at the local job center, where they are assigned to a caseworker. Enrollment implies a commitment to participate in specific programs, a requirement whose satisfaction is easily verifiable and hence enforceable by the threat of benefits suspension. Job seekers must also commit to looking for work and to accepting a suitable job offer; these behaviors are more difficult to enforce but are subject, in principle, to the same benefits suspension rule. Upon enrolling with the PES, job seekers are profiled by the caseworker using a mix of statistical and soft profiling, receive basic job-search orientation, and are assigned to an assistance or training program (or to no program) that the caseworker regards as most beneficial to that particular profile. Such program may be outsourced to external service providers. At the end of the program, individuals who are still unemployed may receive additional reemployment services. Job seekers remain enrolled with the PES until they find a job or lose UI eligibility for non-compliance with program requirements, although of course their records remain in the PES internal data base.

Given the Province's autonomy from the national government, the Trento PES has considerable leeway in the design and provision of ALMP. The experimentation studied in this paper was motivated by the PES's desire to test the effectiveness of new tools for the reemployment of job seekers who are at risk of becoming unemployed long term.

[4] This explanation does not require different responses to incentives arising from gender heterogeneity in "deep" preference parameters, like in Bandiera et al. (2021). Adding these differences would provide an alternative (less parsimonious, in our opinion) rationalization. For example, if women were more risk averse than men then they would respond to the financial incentives that come with the bonus by searching more intensely (and accepting job offers) earlier than men (Cortés et al., 2023).

2.2. The reemployment voucher (RV) experiment

The first experiment is a RCT that involves 2816 job seekers randomly selected in the Spring of 2017 from the universe of workers in their fifth month of unemployment and who are receiving UI benefits. At the time of selection, these job seekers were not benefiting from any ALMP, although they may have been involved during previous months in a program different from those being evaluated. Subjects were randomly assigned to one of three treatment conditions. First, 661 of them were assigned to an intensive job-search assistance program that amounts to 10–40 h (in total) of personalized coaching in writing an effective resume, searching job openings databases, managing job interviews, self-assessment, and motivation. The provision of these services was outsourced by the PES to accredited human resources agencies. Provider's compensation, about €35 per hour, was *not* conditional on reemployment outcomes. This treatment serves as our benchmark for a conventional ALMP.

Second, 751 job seekers were assigned to receive a voucher (RV, henceforth) that they could spend to hire these same private providers and obtain personalized reemployment services. If selected, a provider cannot turn down the job seeker or refer her/him to another provider, but it is free to choose the specific services or job-search training to achieve reemployment. So, while outright cream-skimming was not possible, providers could tailor their own effort to job seekers' characteristics. A provider that places a job seeker within six months (with a possible extension up to one year) in a job lasting at least six months redeems the value of the voucher. The RV value ranges between about €500 and €5000, depending on an individual profiling score that measures the reemployment difficulty, and the type of job—the value is lower the easier it is to reemploy the focal worker and for reemployment in a temporary position. Since the providers involved in JSA and RV are the same, the specific actions that they may undertake (and which we do not observe) are presumably also similar. The point is that provider's incentives are quite different under the two schemes.

The remaining 1404 job seekers were assigned to a control group that, for six months, could not be offered JSA or RV. Ethical considerations prevented our exclusion of the control group from these programs for a longer period. The restriction was implemented by flagging these job seekers in the PES information system, so that caseworkers would not offer them JSA or RV. There was full caseworker's compliance. In retrospect, only 15 individuals in the control group benefited from these programs in the 28 months that followed the six-month exclusion. Thus, the difference between JSA treatment and control is that while every job seeker in the sample (including the control group) received basic orientation from PES caseworkers, only those assigned to JSA received the intensive reemployment services described above and that were provided by external agencies selected by the PES.

The size imbalance across the three groups reflects that the RCT involved two different RV's that were simultaneously randomized by drawing letters to be matched with the last character of a job seeker's social security code (SSC). The first was a *national* RV (*Assegno di Ricollocazione*) introduced by the Italian government in 2015, whose value ranges between about €500 and €5000, and randomly assigned to an experimental group with the purpose of evaluating a possible roll-out at the national level. The national government assigned to the national RV job seekers whose SSC ended with either E or Y (two randomly drawn letters),[5] resulting in 444 individuals located in Trento Province. The final size of the national sample that could be used in our RCT was $N = 377$, after excluding those currently enrolled in one of the local programs involved in the experiment. The second was a *local* RV (*Inserimento Lavorativo*) in use at the Trento PES since 2014, whose value ranges between about €700 and €1800. The PES assigned

[5] The Italian SSC (*Codice Fiscale*) is a 16-character alphanumeric string, with its last character being a random letter.

to this program job seeker's whose SSC ended with either J or K (also randomly drawn), resulting in $N = 374$. Since, apart from the voucher's different values, the two schemes are equivalent in terms of incentive structure, we aggregate these two groups into a single RV treatment group ($N = 751$) to gain statistical precision. The PES aimed at a JSA group of similar size by randomly drawing another four letters (F, S, T, and Z) and by assigning to this group job seeker's whose SSC ended with one those letters, but obtained a slightly smaller sample ($N = 661$) due to some imbalance in the distribution of final SSC character in the Province population.[6] The Trento PES synchronized the randomization of the local RV to the randomization enacted at the national level for the national RV.[7]

Ex-post, power analysis indicates that given an empirical exit rate from unemployment of 50% (SD = 0.5) by the second year of losing one's job, a test power of 90%, and the institutional constraint that produced treatment groups that are half the size of the control group, a $N = 1600$ control group and a $N = 800$ treatment group would have been required to detect a treatment effect of 7 pp. Card et al. (2018) compute average treatment effects of 5.4 pp after 1–2 years and 8.7 pp from the third year onward. Despite this lack of power that characterizes the RV experiment and that we fully acknowledge, we believe that our RV evaluation is easily interpretable and thereby informative for public policy.

Job seekers assigned to a treatment arm (except for 29 individuals whose mailing address in PES records was invalid) received an identical paper letter that informed them of eligibility for a program aimed at helping them find a job. This letter invited recipients to apply at the local job center to receive more information and to enroll in the program. A key feature of the RCT is that take up was *not* mandatory. Refusal to take up the proposed program was not regarded as non-compliance with PES requirements and had no consequences for future UI benefits receipt or future reemployment services. This critical feature of the experimental design enables us to investigate program take-up decisions, on top of overcoming the common problem that experimental evaluation of ALMP are typically unable to separate the effect of the take up from the effect of enforcement (Ashenfelter et al., 2005).

2.3. The reemployment bonus (RB) experiment

The second experiment is generated by date of birth cutoffs that determine eligibility for a reemployment bonus (RB, henceforth). As illustrated in Fig. 1, job seekers born on or after 1/1/1978 are not eligible for any bonus. Since the RB experiment took place in 2018, this first date-of-birth cutoff is labeled "age-40 cutoff". Those born before 1/1/1978 but on or after 1/1/1968 are eligible for a lump-sum bonus of €2000, and those born before 1/1/1968 are eligible for a lump-sum bonus of €3000. Thus, this second date-of-birth cutoff is labeled "age-50 cutoff". Similar to RV, eligibility for RB starts at the end of the fifth month of unemployment. In order to qualify for any RB payment, a job seeker must find a new job within six months, keep it for at least three months, and work for at least 20 h per week for a monthly after-tax wage of no more than €2000 (a wage level that was about 130% of the national average). This makes the Trento RB more generous that its US counterparts.[8] Finally, the bonus can be obtained only once every three years, a provision that mitigates the moral hazard issues – that Meyer (1996) discusses in the Illinois context – related to making a reemployment bonus permanent.

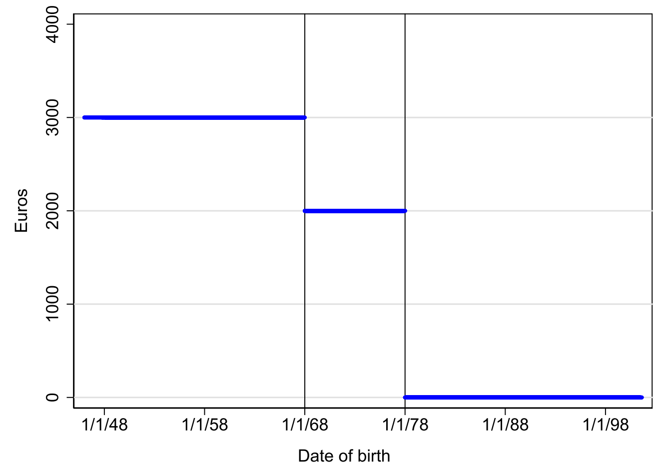

Fig. 1. The Trento reemployment bonus.
Notes: The figure illustrates the Trento reemployment bonus as a function of a job seeker's date of birth. Job seekers born on or after 1/1/1978 are not eligible for any bonus. Those born on or before 1/1/1978 but on or after 1/1/1968 are eligible for a lump-sum bonus of €2000, and those born before 1/1/1968 are eligible for a lump-sum bonus of €3000.

An institutional constraint prevented us from randomizing RB eligibility, and so credible identification of its causal effects is possible only locally at the two cutoffs in a sharp RDD. On the other hand, the RB experiment involves the universe of newly unemployed workers in cohorts spanning one year. Thus, while the RV experiment uses a sample of job seekers and so is subject to the problem of general equilibrium effects following a hypothetical broader roll-out (Crépon et al., 2013), the RB experiment presumably nets out such effects. A second, more subtle difference is that RB eligibility did not require to be recipients of UI benefits, which as shown in Section 3 results in a slighter younger study sample than in the RCT.

The recruiting strategy mimics the RV experiment: job seekers who entered their fifth month of unemployment and born no later than 1/1/1978 received a letter from the PES that informed them of eligibility for a payment of €2000 or €3000 (depending on recipient's age) and the conditions to receive it, inviting them to apply at their respective local job centers to claim the bonus upon qualification. This letter was sent starting in January 2018 to a total of 3876 workers in 12 different cohorts who had lost their jobs and who started an unemployment spell between September 2017 and August 2018, on a rolling basis (308 individuals who became unemployed in September 2017 received the letter in January 2018; 552 individuals whose spell started in October 2017 received the letter in February 2018; and so on). Another 4135 job seekers born after 1/1/1978 (and thus ineligible for RB) and who belong to the same unemployment cohorts as the eligible individuals, were set apart as a control group at the 1/1/1978 cutoff. Of these 8011 job seekers, only 7275 turned out to be actually unemployed at the time the letters were sent. These job seekers from the unemployment state constitute our final sample for the RB evaluation. Notice that the overlap between the RV and RB experimental samples is negligible: only 24 individuals who were part of the RCT (10 from treatment groups and 14 from the control group) were later recruited into the RB experiment because they started a new unemployment spell.

3. Data and measurement

Our data come from the Trento PES's administrative data base, the *Sistema Provinciale Informativo del Lavoro* (SPIL), which is fed by two sources: first, job centers register workers at the outset of an unemployment spell and keep track of any active labor market program these

[6] All letters were extracted from the set of 26 letters in the English alphabet, with replacement. If a letter had already been drawn, a new one was drawn after replacement.

[7] A first evaluation of the latter has not produced significant results (OECD, 2019).

[8] For example, eligibility for the Illinois bonus lasted 11 weeks and its value was $500, or about $1200 in 2018 dollars. Moreover, the requirement was that the job should have been held for at least 4 months.

workers go through; second, a national information system maintained by Italian Ministry of Labor, the *Sistema Informatico per le Comunicazioni Obbligatorie* (SICO), that records in a standardized way all hirings and dismissals of employees, with detailed information on industry and job. Thus, for each individual who ever started an unemployment spell and who enrolled with the PES to collect unemployment benefits, the PES administrative records contain basic demographic information and longitudinal data on programs attended, job offers accepted, and subsequent separations from employers (if any).

This data source has two limitations. The first is that the information flow from SICO into SPIL stops if a job seeker moves outside the province. Thus, the labor market status of these movers (who cannot be identified in SPIL) is not observed. The consequences of these facts are likely unimportant if one considers that in 2018 only 0.2% of the Trento Province population relocated outside the province. The second limitation is that SPIL is not linked to social security records, which, in turn, has two implications. First, we do not know earnings from accepted job offers. We overcome this problem by exploiting information on industry (3-digit ATECO 2007 classification),[9] occupation, and work hours in a new job to predict full-time equivalent earnings based on wages from collective agreements (which in Italy are binding for most employers) for the respective 3-digit ATECO 2007 industry and blue collar vs white collar jobs.[10] Second, transitions from unemployment into self-employment are not observed. Job seekers who make this transition stop collecting unemployment benefits but are not classified as employed in SPIL because no employer hired them according to SICO. These job seekers end up in a residual category of individuals who, from the Trento PES viewpoint, are neither employed nor unemployed, like those who moved outside the province or who left the workforce. Among the cohort of those who started an unemployment spell in December 2017, about 5% were in this residual pool at the end of 2018.

The experimental subjects are directly linked to the respective administrative records in SPIL, and are followed until the end of January 2020. The subsequent, prolonged disruption of the Italian labor market in the wake of the Covid-19 pandemic makes the investigation of the effects of RV and RB after that date of little use. This means that reemployment outcomes can be tracked for 34 months in the RV experiment (because the RCT started in March 2017) and for 12 months in the RB experiment when pooling all cohorts (because the last cohort became eligible for the bonus in January 2019).

The outcome of primary interest is survival in PES enrollment until the first exit, which is approximated by whether in a certain month a job seeker has ever left the PES ranks. This is our best proxy for the unemployment survival function. This outcome is measured without error from the administrative records, and is of interest to the PES because enrolled job seekers receive transfers (except those who exhausted benefits) and absorb scarce PES resources like caseworker time and attention. We also consider two additional outcomes that, despite being measured with error, are proxies for match quality: whether a job seeker left unemployment for a job that, retrospectively, lasted at least 6 months (this outcome is measured from the SICO flow with some error because this flow misses self-employed workers and employees who find a job outside the province); and average, full-time equivalent gross monthly earnings since assignment (this outcome is measured with error from SICO using wages that are imputed as described in footnote 10). Estimating the effects of RV and RB on measures of job quality is important because these programs provide high-powered, financial incentives to improve one particular outcome – short-run reemployment – but no incentives to improve other outcomes like job quality or long-term employment and so may distort effort towards the short-run goal at the expense of other desirable goals that were not incentivized.

After removing from the RCT sample the 29 observations without a valid mailing address and 85 individuals without any information to compute residual national UI benefits – a crucial variable in the analysis of take up – we are left with a final sample of 2690 job seekers in the RV experiment. Table 1 reports, in columns [1]–[3], summary statistics computed from this sample by treatment status, and the *p*-values from a balancing test.

Consistent with random assignment, the means of covariates are quite similar across treatment and control groups. The table also reports, in column [4], the corresponding statistics for the 7275 job seekers that constitute the final sample for the analysis of the RB experiment, alongside the *p*-value from a test of equality of means across the RV treatment group and the RB sample. The most notable difference across the two study samples pertains to the age distribution. As mentioned in Section 2, while the RCT sample was extracted from a population of national UI recipients, RB-eligibility did not require an individual to also be eligible for UI benefits from the national government.[11] This implies that there is a larger proportion of young job seekers in the RDD sample, as these job seekers are less likely to have contributed to the social security system for at least 12 weeks in the past. Given that most of these UI-ineligible job seekers are far from the age-40 cutoff in our RDD, such difference is of little concern for the comparability of RV and RB estimates. We report in Appendix Table A.1 the gender breakdown of Table 1.

4. Results: reemployment voucher vis-à-vis JSA

Program take up. A first result of interest in the RV experiment concerns the response rate (i.e., the fraction that responded to the invitation letter by visiting a local job center) and the program take-up rate. These rates are reported in Table 2 for the entire sample and by gender. The overall response rate is slightly below 40% for both JSA and RV treatments. However, just slightly more than a fifth of the respondents decided to take up the proposed policy. These figures imply a take-up rate among the assigned of 8.4% for either program, a total of 111 individuals. Response and take-up rates are generally smaller for women than men, although the small sample size allows to detect with sufficient statistical confidence only the large gender difference in take up among the assigned.

In order to understand some of the correlates of response and take-up behavior, we regress the respective dummies on the following covariates: age dummies (defined by the quartiles of the age distribution), nationality (Italian vs foreign-born), whether a job seeker is in her/his first unemployment spell, whether the job center is located in the two largest urban areas of the province (as opposed to the remaining job centers, which are smaller and are located in more rural areas) and dummies that capture the distance from UI benefits exhaustion (12 to 9 months; 8 to 5 months; 4 to 0 months; being entitled to more than 12 months of residual UI is the omitted category). The results are presented in Fig. 2. The propensity to respond or to take up the proposed policy conditional on responding are increasing in age, particularly among men. Note also that job seekers in their first unemployment spell are less likely to visit a local job center to

[9] ATECO 2007 is the Italian version of NACE (Nomenclature of Economic Activities), the European statistical classification of economic activities. The 3-digit level results in about 270 categories.

[10] Specifically, we match information on industry, occupation, and full/part-time status in the new job with the contractual wage minima in collective agreements that were in force at the time of the experiment. These minima provide a reasonable estimate of the wage at which a job seeker was reemployed, as most wage offers in the private sector in Italy are tied to them.

[11] This difference could not be resolved during the design stage. As explained in Section 2.2, the RV experiment included a voucher (the *Assegno di Ricollocazione*) that the Italian government was testing on NASpI recipients *only*, so the entire RV experiment inherited this constraint. When designing the RB quasi-experiment, instead, the PES did not wish to impose this requirement.

Table 1
Sample statistics.

Study:	[1] RCT	[2]	[3]	*p*-val	*p*-val	*p*-val	[4] RDD	*p*-val
Treatment:	JSA	RV	Controls	[1]–[2]	[1]–[3]	[2]–[3]	RB	[2]–[4]
Female	0.61	0.60	0.58	0.80	0.09	0.14	0.59	0.47
	(0.49)	(0.49)	(0.50)				(0.49)	
Age	45.9	46.4	45.0	0.43	0.12	0.01	40.8	0.00
	(11.8)	(11.8)	(11.8)				(13.3)	
Italian	0.74	0.75	0.70	0.68	0.07	0.02	0.73	0.32
	(0.44)	(0.43)	(0.46)				(0.44)	
1st unemp. spell	0.34	0.37	0.37	0.22	0.27	0.76	0.37	0.96
	(0.47)	(0.48)	(0.48)				(0.48)	
Job center 1	0.32	0.32	0.30	0.99	0.44	0.43	0.29	0.20
	(0.47)	(0.46)	(0.46)				(0.45)	
Job center 2	0.16	0.18	0.17	0.50	0.74	0.65	0.17	0.77
	(0.37)	(0.38)	(0.38)				(0.38)	
Job center 3	0.10	0.10	0.11	0.38	0.32	0.99	0.11	0.80
	(0.29)	(0.31)	(0.31)				(0.31)	
Job center 4	0.09	0.10	0.11	0.46	0.10	0.38	0.16	0.00
	(0.28)	(0.30)	(0.31)				(0.36)	
Residual UI months	8.1	8.0	8.2	0.58	0.85	0.40	–	–
	(4.9)	(5.1)	(5.1)				–	
N	621	701	1,368				7,275	

Notes: Columns [1]–[3] report the mean and (in parentheses) SD of pre-experimental variables for job seekers assigned to job-search assistance (JSA), reemployment voucher (RV), and the control group in the RV experiment. Column [4] reports the corresponding covariates in the reemployment bonus (RB) experiment. Job centers 1–4 are in the four largest urban centers. The *p*-values are from a test of the hypothesis that the means of a covariate are equal in the groups indicated by column number.

Table 2
Response and program take-up rates in the RCT.

	All job seekers		Men		Women	
	JSA	RV	JSA	RV	JSA	RV
Response rate (job center visit)	0.383	0.389	0.416	0.387	0.362	0.391
	[0.020]	[0.018]	[0.032]	[0.029]	[0.025]	[0.024]
Take-up rate among respondents	0.218	0.216	0.267	0.250	0.182	0.194
	[0.027]	[0.025]	[0.044]	[0.042]	[0.033]	[0.031]
Take-up rate among assigned	0.084	0.084	0.111	0.097	0.066**	0.076
	[0.011]	[0.010]	[0.020]	[0.018]	[0.013]	[0.013]
N :						
assigned	621	701	243	279	378	422
respondents	238	273	101	108	137	165

Notes: The table reports response and take-up rates [s.e. in brackets], for job seekers assigned to job-search assistance (JSA) or the reemployment voucher (RV), by gender.
** Means that the gender difference is significant at the 95% confidence level.

receive more information about the proposed policy and to take it up. This result may reflect unfamiliarity with the PES. Job seekers living in the largest urban areas are more likely not only to show up at the employment center (in response to the invitation letter) but also to take up the proposed program. This outcome may reflect the different characteristics of urban workers as well as the different skills of job advisors at the largest job centers. Most importantly, we find that the take-up propensity increases as the exhaustion of unemployment benefits approaches. So relative to UI recipients with more than 12 months of benefits still to collect, the likelihood of taking up the proposed program is larger (by up to ≈20 pp for men) and increases as UI benefits exhaustion approaches.

These results indicate that the passive policy (UI) interferes with ALMP, as already demonstrated quasi-experimentally by Black et al. (2003). Moreover, heterogeneity along gender and other dimensions points to selection on gains: only those who expect to benefit from the assigned program take it up; if these expectations are correct, then mandatory take up may give the false impression that a program is ineffective. Such an interpretation is consistent with the small ITT effects of ALMP typically reported in the literature.

Survival in unemployment. Next, we plot in Fig. 3 the share of job seekers who never left the PES ranks – i.e., the unemployment survival function – during the 34 months following the start of the RCT, by month, treatment status, and gender. During the first four months, this function is identical in the treatment and control groups. Afterwards, a gap opens up in favor of job seekers assigned to treatment that is entirely driven by men. The central panel of the figure shows that for men the gap is about 5 pp and that it persists well into the third year from assignment to treatment; for them, the survival functions in the intensive JSA and RV groups are almost overlapped, indicating that the RV program's effectiveness is similar to the conventional JSA benchmark.

We estimate the ITT effects of JSA and RV (relative to the control group) on these survival functions parametrically, by means of the following linear probability model,

$$Y_{i,m} = \alpha_m + \beta_{\mathrm{JSA},m}\mathrm{JSA}_i + \beta_{\mathrm{RV},m}\mathrm{RV}_i + \gamma X_i + \varepsilon_{i,m}, \tag{1}$$

where $Y_{i,m}$ is a dummy indicating whether job seeker i never left the PES ranks after m months from assignment to treatment (for $m = 1, \dots, 34$), JSA_i and RV_i are dummies for whether i was assigned to (respectively) JSA or RV, and X_i is a vector that contains the individual covariates used in the participation analysis illustrated in Fig. 2. In order to mitigate the consequences of multiple testing, we stack the data in an individual-level panel at the monthly frequency and we estimate equation (1) using a single regression, interacting treatment and month

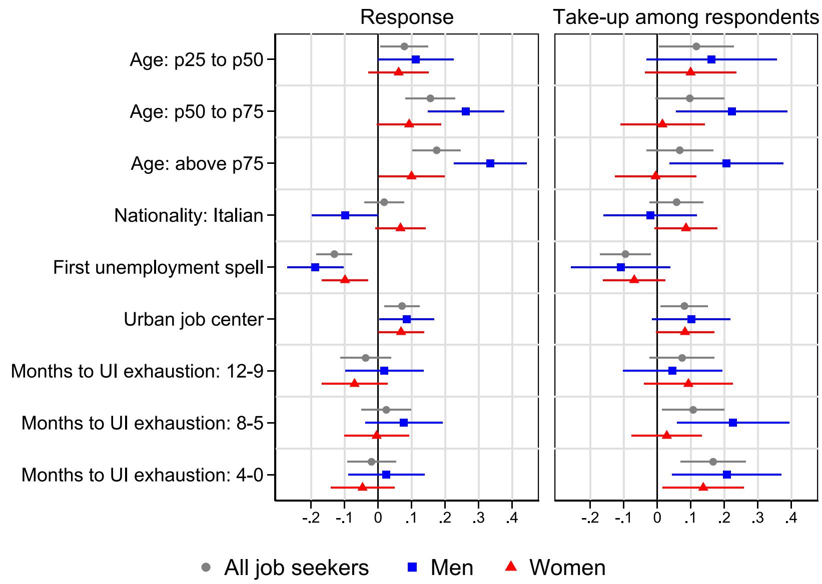

Fig. 2. Effect of covariates on response and program takeup.
Notes: Coefficients and 95% confidence intervals (computed from robust s.e.) from a linear regression of response (whether a job seeker responded to the invitation letter by showing up at a local job center) or take-up dummies (conditional on responding) on covariates. The 25th, 50th, and 75th percentiles of the age distribution are computed in the full sample and are 35.4, 44.8 and 55 years, respectively. Sample: 1322 invited job seekers described in columns [1]–[2] of Table 1; sample sizes are in Table 2.

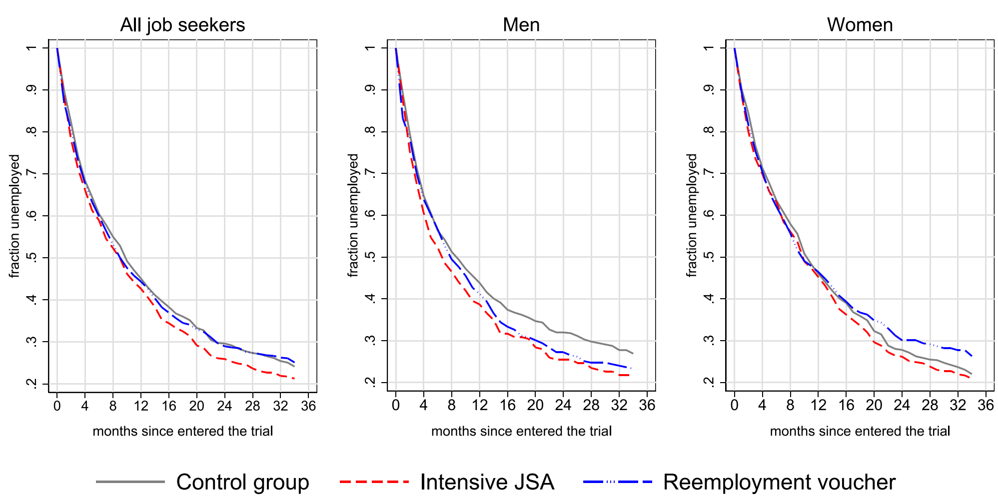

Fig. 3. Unemployment survival function by treatment status and by gender.
Notes: The figure shows the fraction of job seekers in the RCT sample who have not left the unemployment state since assignment to treatment. We censor the outcome at the end of the 34th month, i.e., the end of January 2020 (shortly before labor market disruption due to the Covid-19 pandemic). Sample: 2690 job seekers described in columns [1]–[3] of Table 1.

dummies and clustering standard errors at the job seeker level.[12]

Fig. 4 illustrates parameter estimates and 90% CI, for all job seekers (left column) and by gender (middle and right). Consistent with the nonparametric estimates of the survival functions in Fig. 3, RV (top row) and JSA (bottom row) have a persistent, similar negative impact on men's likelihood of being unemployed of about 5 pp. For women, if anything, the RV shifts the survival function upward from the 20th month onward.[13]

Appendix Fig. A-1 reports estimates of the ATET—noncompliance is essentially one-sided because job seekers not assigned to JSA or RV could not be offered these programs for 6 months and only 15 individuals in the control group eventually benefited from them. Despite large standard errors, intensive JSA and RV were quite effective for men who took them up. For example, the raw data show that among the 27 men who took up RV (out of 243 who were offered it), the gap, relative to the control group, between the respective unemployment survival functions is four times as large, after 6 months from assignment, as the corresponding gap for those who did not take up the RV offer (−11.9% vs −3.1%), and still twice as large after 18 months (−10.3% vs −4.7%).

Job match quality. As for our measures of job match quality, Table 3 shows, in columns [1]–[3] the effect of RV or JSA on the average (log) earnings of job seekers who left the unemployment state for at least one month during the 34 months following assignment to treatment.

[12] This procedure yields estimates of parameters $\beta_{\text{JSA},m}$ and $\beta_{\text{RV},m}$ that are identical to those produced by 34 distinct regressions (except that we impose a common γ) but avoids shrinking standard errors.

[13] The hypothesis that the ITT of JSA and RV are equal for a given group is never rejected by a formal test (not reported in the interest of space).

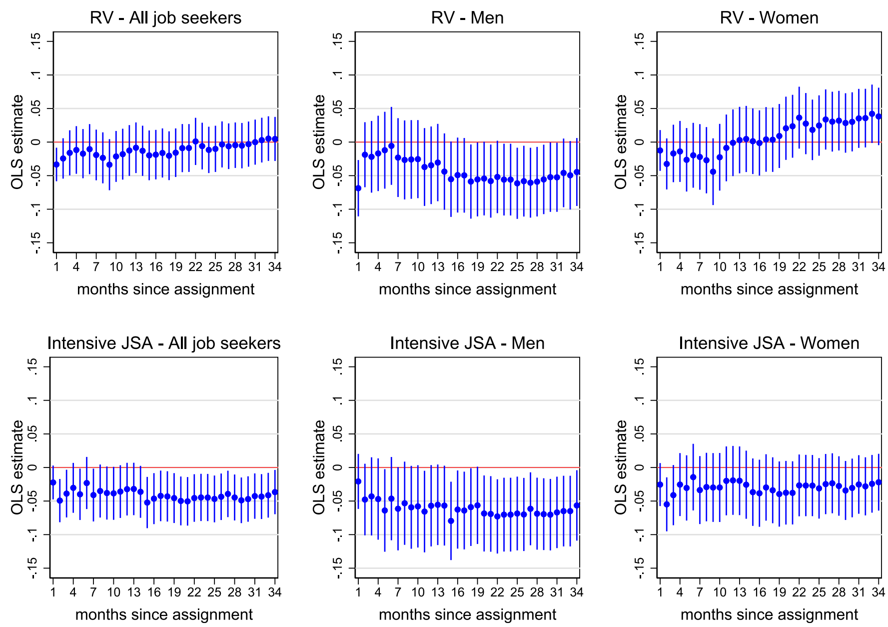

Fig. 4. ITT effects of RV and intensive JSA on the unemployment survival function.
Notes: Estimates of ITT parameters $\beta_{\text{JSA},m}$ and $\beta_{\text{RV},m}$ and 95% confidence intervals, from OLS estimation of Eq. (1). The dependent variable is a dummy for whether a job seeker is still unemployed m months after assignment ($m = 1, \dots, 34$). Standard errors are clustered at the job seeker level. Sample: 2690 job seekers described in columns [1]–[3] of Table 1.

Table 3
ITT effect on average post-treatment earnings.

	[1]	[2]	[3]	[4]	[5]	[6]
	Reemployed job seekers			All job seekers		
	All	Men	Women	All	Men	Women
RV	−0.004	0.002	−0.008	0.038	0.209	−0.068
	(0.006)	(0.009)	(0.008)	(0.214)	(0.334)	(0.276)
Intensive JSA	0.000	0.006	0.003	0.348	0.417	0.293
	(0.006)	(0.010)	(0.008)	(0.217)	(0.337)	(0.282)
Annual earnings	22,930	23,029	22,861	16,144	15,932	16,294
N	1,894	770	1,124	2,690	1,113	1,577

Notes: The table reports the coefficients from a OLS regression of average, monthly earnings in the 34 months following assignment to treatment on a constant and treatment dummies (Eq. (2)). In columns [4]–[6], job seekers who never leave unemployment are assigned zero log earnings. Covariates are included as conditioning variables. Annual earnings refer to average, gross annual earnings in the sample, expressed in current euros. Job seekers who never left the PES ranks are imputed a wage of zero Samples: in columns [1]–[3], 1,894 job seekers who worked for at least one month during the 34 months following assignment to treatment; in columns [4]–[6], 2,690 job seekers described in columns [1]–[3] of Table 1.

Columns [4]–[6] show the corresponding effect for all job seekers, assigning zero log earnings to those who never left unemployment. These coefficients are produced via OLS estimation of the following linear regression,

$$\ln w_i = \alpha + \beta_{\text{RV}}\text{RV}_i + \beta_{\text{JSA}}\text{JSA}_i + \delta X_i + \varepsilon_i, \tag{2}$$

where w_i measures average gross earnings in the post-treatment period. Thus, relative to workers in the control group, those who left the unemployment state after being assigned to RV or JSA experienced the same earnings, on average; the faster reemployment rate induced by these programs did not come at the expense of job quality, as measured by earnings.

Similarly, Fig. 5 shows that the faster reemployment rate of treated men did not come through short-term jobs: the positive effect of RV or JSA on the probability of holding a job that will last at least six months mirrors the negative effect of these programs on the unemployment survival function that is shown in Fig. 4.

Cost-benefit calculation. Cost effectiveness of RV in the experiment is established by noting that providers are paid the value of the voucher only if they successfully reemploy the job seeker, while in the case of intensive JSA the PES bears the cost of reemployment services regardless of the outcome. In the experiment, the average value of RV was little more than €1000 for the local voucher and somewhat higher for the national one. Given that RV turned out to be no less effective than JSA for men and that the cost of intensive JSA to the PES was about €35 per hour for between 10 and 40 h regardless of the reemployment outcome, RV is a rather inexpensive policy relative to benchmark JSA.

5. Results: reemployment bonus

Identification. A sharp RD design identifies RB's average treatment effect locally at the two cutoffs where the value of the bonus changes discontinuously. The identifying assumption is that the potential reemployment rates – both in the absence and in the presence of a uniform RB of either €2000 or €3000 – are continuous at the age-40 or age-50 cutoffs, respectively. We corroborate this assumption by means of the Canay and Kamat (2017) continuity test, which is more general than the McCrary test. The null hypothesis is that the distribution of each observable covariate is *anywhere* continuous at these cutoffs. The p-values from this test are reported in Table 4. Except for nationality in the female subsample at the age-50 cutoff, the null is never rejected.

Survival in unemployment. We next provide in Fig. 6 graphical evidence of RB's effects on the share of job seekers who never left the PES ranks

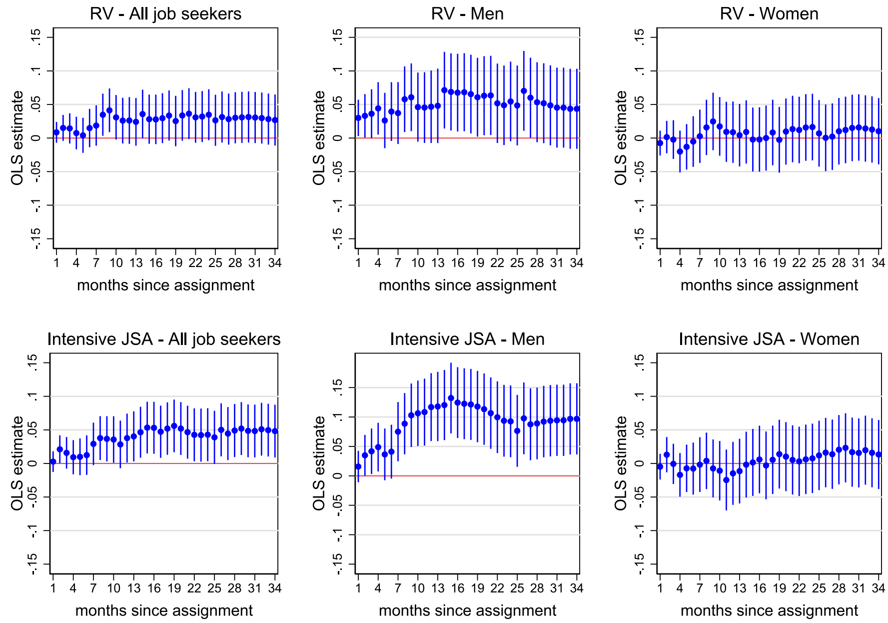

Fig. 5. ITT effects of RV and intensive JSA on employment hazard in 6+ months jobs.
Notes: Estimates of ITT parameters $\beta_{\mathrm{JSA},m}$ and $\beta_{\mathrm{RV},m}$ and 95% confidence intervals, from OLS estimation of Eq. (1). The dependent variable is a dummy for whether a job seeker is employed, m months after assignment ($m = 1, \dots, 34$), in a job that will last at least 6 months, retrospectively. Standard errors are clustered at the job seeker level. Sample: 2690 job seekers in columns [1]–[3] of Table 1.

Table 4
Continuity of the distribution of covariates at the RB cutoffs, *p*-values.

	All job seekers		Men		Women	
	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff
Disjoint tests:						
Female	0.59	0.36	–	–	–	–
Age	0.69	0.87	0.80	0.66	0.57	0.18
Nationality: Italian	0.99	0.99	0.99	0.18	0.30	0.03
First unemployment spell	0.57	0.43	0.99	0.61	0.39	0.66
Urban job center	0.27	0.20	0.62	0.10	0.99	0.28
Joint test	0.20	0.61	0.65	0.31	0.17	0.58
N	5,204	3,640	2,044	1,574	3,160	2,066

Notes: The table reports the *p*-values from the Canay and Kamat (2017) test of the null hypothesis that the distribution of each observable covariate is continuous at the "age-40" (date of birth 1/1/1978) and "age-50" (date of birth 1/1/1968) cutoffs, where the value of the reemployment bonus changes discontinuously from €0 to €2,000 and from €2,000 to €3,000, respectively (see Fig. 1). The *p*-values are from disjoint tests on each covariate or from a joint test. The tests at the age-40 cutoff use only observations born on or after 1/1/1968, while the tests at the age-50 cutoff use only observations born before 1/1/1978. Sample: 7,275 job seekers who started an unemployment spell between September 2017 and January 2018, column [4] of Table 1.

after one or twelve months from the start of bonus eligibility. For this purpose we use Local Linear Regression on both sides of each cutoff, with a triangular kernel and optimal bandwidth selection from Calonico et al. (2014). Such optimal bandwidth ranges between 2128 and 3603 days. The figure shows the estimated probability as a function of job seekers' date of birth (lines) as well as the average fraction unemployed in bins defined by semester of birth (circles and triangles). Each bin contains, on average, 78.8 individuals (33 men and 47.9 women, on average, when splitting the sample by gender).

Consider the outcome after one month (continuous line). Moving from right (younger job seekers) to left (older job seekers) along the horizontal axis, there is a sizable discontinuity at the age-40 cutoff – job seekers who barely qualified for a strictly positive bonus are less likely unemployed than those who barely did not (left panel) – that is entirely driven by women (right panel). For them, the discontinuity at this first cutoff is about 10 pp, while it is nil for men (middle panel). There is also a smaller discontinuity at the age-50 cutoff that is instead driven by men. After twelve months, the schedule representing the fraction who never left the PES ranks shifts downward as more workers leave the unemployment state independently of RB, yet the early discontinuity observed for women is reduced. Some discontinuity (statistically insignificant, as shown below) persists for men at the age-50 cutoff. This pattern suggests that RB induces a quick activation, particularly of women, at the outset of eligibility, but this advantage is transitory.

To obtain parametric estimates of these discontinuities (and a standard error) on a month-by-month basis, we again stack the data in an individual-level panel at the monthly frequency and we estimate the following equation, separately at each cutoff $c \in \{1/1/1978, 1/1/1968\}$:

$$Y_{i,m} = \alpha_{m,c} + \beta_{\mathrm{RB},m,c}\mathrm{RB}_{i,c} + \gamma_{m,c}X_i + f_{m,c}(D_i) + \epsilon_{i,m}. \tag{3}$$

Here $Y_{i,m}$ again indicates whether job seeker i never left the PES ranks after m months from the start of RB eligibility, $\mathrm{RB}_{i,c}$ indicates whether i was born before date-of-birth cutoff c, D_i is i's date of birth (the running variable), and $f_{m,c}(D_i)$ is a second-order polynominal in the running variable. Thus, parameter $\beta_{\mathrm{RB},m,c}$ represents the average treatment effect of RB on the unemployment survival function after m

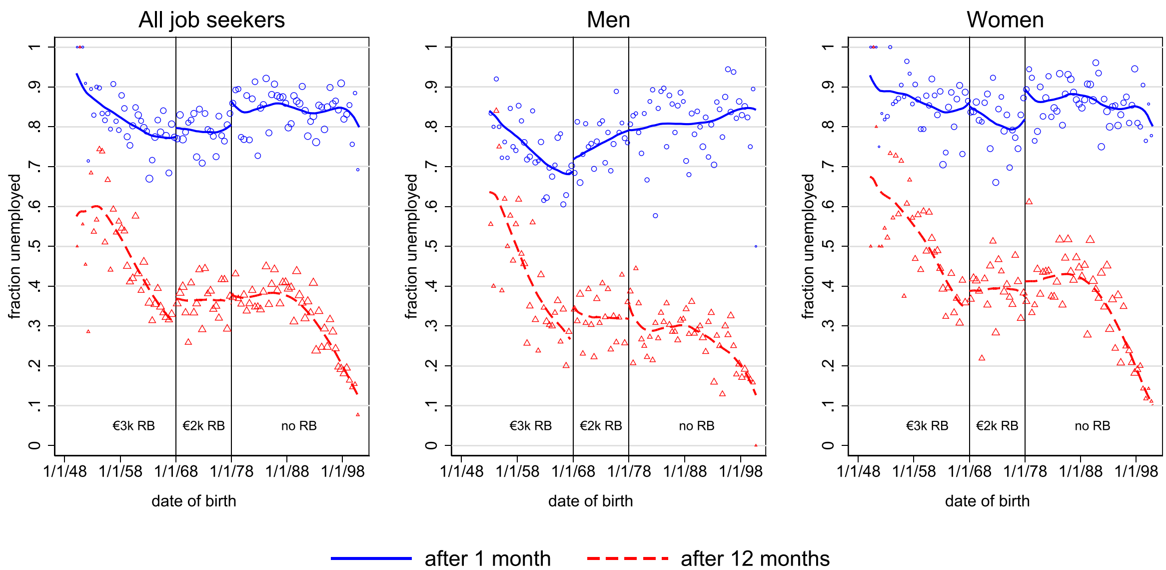

Fig. 6. Fraction continuously enrolled with the PES by date of birth and by gender.
Notes: The figure illustrates the fraction of job seekers who, after 1 month or 12 months from the start or the RB experiment never left the PES ranks, as a function of date of birth. The lines are produced by Local Linear Regression on the underlying individual observations, separately on each side of the cutoffs, with a triangular kernel and optimal bandwidth selection from Calonico et al. (2014). The optimal bandwidths in this figure at the age-40 and age-50 cutoffs are: in the left panel, 2277 and 2899 days, respectively, after 1 month, and 2887 and 2481 days, respectively, after 12 months; in the middle panel, 3603 and 2535 days, respectively, after 1 month, and 2299 and 2545 days, respectively, after 12 months; in the right panel, 2129 and 2663 days, respectively, after 1 month, and 2855 and 2419 days, respectively, after 12 months. The circles and triangles represent the average fraction of individuals who are unemployed, in bins defined by semester of birth (the size of a marker is proportional to the number of job seekers in a bin). Sample: 7275 job seekers who started an unemployment spell between September 2017 and January 2018, described in column [4] of Table 1.

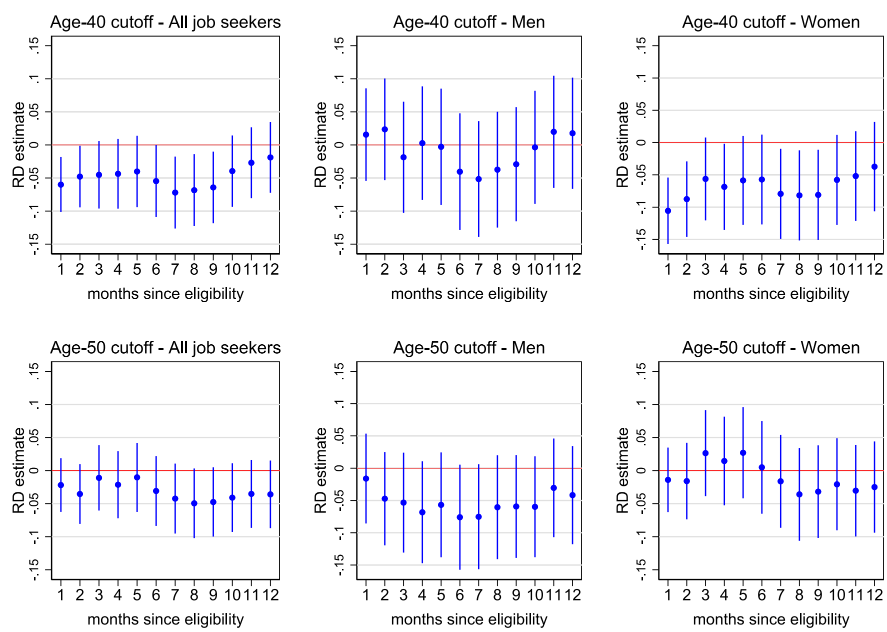

Fig. 7. Effects of RB on the unemployment survival function at date-of-birth cutoffs.
Notes: The figure reports RD estimates of parameter $\beta_{RB,m,c}$ and 95% confidence intervals, from OLS estimation of Eq. (3). The dependent variable is a dummy for whether a job seeker is still unemployed m months after the start of RB eligibility ($m = 1, \dots, 12$). Standard errors are clustered at the job seeker level. Sample: 7275 job seekers who started an unemployment spell between September 2017 and January 2018, described in column [4] of Table 1.

months from eligibility, locally at cutoff c.[14]

Estimates of this parameter and 90% confidence intervals are presented in Fig. 7, for all job seekers (left column) and by gender (middle and right column) at the age-40 cutoff (top row) and at the age-50 cutoff (bottom row).

In line with Fig. 6, we estimate a significant negative discontinuity at the age-40 cutoff that is driven by women, originates at the outset of RB eligibility, and that tends to vanish. Thus, for these job seekers, RB speeds up reemployment by at least nine months; afterwards, the gap with ineligible job seekers fades away. No significant effect is detected for men at this first cutoff or for either men or women at the age-50 cutoff. Also observe that the discontinuities in women's survival

[14] In accord with the construction of Fig. 6, our estimation of this parameter at the age-40 cutoff uses only job seekers born after 1/1/1968 while the corresponding estimation at the age-50 cutoff uses only job seekers born on or before 1/1/1978.

Table 5
Effect of RB on average post-eligibility earnings of reemployed job seekers.

	Reemployed job seekers					
	All		Men		Women	
	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff
RB	−0.001	−0.001	0.001	−0.020	0.001	0.015
	(0.008)	(0.007)	(0.011)	(0.010)	(0.011)	(0.009)
N	4,758	3,193	1,884	1,382	2,874	1,811

	All job seekers					
	All		Men		Women	
	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff	age-40 cutoff	age-50 cutoff
RB	0.048	0.467	0.229	0.287	−0.119	0.561
	(0.195)	(0.206)	(0.301)	(0.315)	(0.268)	(0.287)
N	5,204	3,640	2,044	1,574	3,160	2,066

Notes: The table reports the coefficients from a OLS regression of average, monthly earnings in the 12 months following the start of RB eligibility (or average wages in the same period for RB-ineligible job seekers), a constant, and a second-order polynomial in DOB (Eq. (4)). Samples: in the top panel, 6,571 job seekers who started an unemployment spell between September 2017 and January 2018 and who worked for at least one month during the following 12 months, out of the 7,275 job seekers described in column [4] of Table 1. In the bottom panel, all of the 7,275 job seekers described in column [4] of Table 1. Here, log wages are set to zero for job seekers who never left unemployment. Estimation at the age-40 cutoff uses only job seekers born on or after 1/1/1968 while the corresponding estimation at the age-50 cutoff uses only job seekers born before 1/1/1978. For this reason, the sum of sample sizes in the first two columns exceeds the total number of job seekers in the sample.

function at the age-40 cutoff are somewhat larger than RV's ITT effect for men reported in Fig. 4. This is the proper comparison because the average treatment effect of RB at the eligibility cutoff averages the responses of both job seekers who increase search effort in response to eligibility (a group akin to the compliers) and of those who do not increase effort (a group akin to the always or never takers). Yet we cannot determine (as we could in the RV experiment) the share of "compliers" among those workers eligible for RB.

The Trento RB was more effective than its US counterparts. Among these, the Illinois reemployment bonus produced the largest drop in mean weeks of unemployment: 1.37 (Meyer, 1996). When using this measure, which in our data is censored after 12 months, the corresponding drop in our experiment – pooling men and women – is 2.37 (p-value = 0.08) at the age-40 cutoff and 1.82 (p-value = 0.17) at the age-50 cutoff. The gender heterogeneity pattern is also similar: Meyer (1996) reports that the hazard rate of exit from unemployment was 16% higher for women who qualified for the Illinois RB than for men who qualified, although that difference was statistically insignificant.

Job match quality. We leverage again our measures of job quality to establish that, like in the RV experiment, the financial incentives generated by RB, while inducing faster exit from unemployment for women, do not translate into lower earnings or an increased propensity to accept short-term jobs in this group. The first fact is evident from Table 5, which in analogy to Table 3 presents estimates of coefficients $\beta_{\mathrm{RB},c}$ from the following RD model:

$$\ln w_i = \alpha + \beta_{\mathrm{RB},c}\mathrm{RB}_{i,c} + \delta_c X_i + f_c(D_i) + \epsilon_i, \tag{4}$$

where w_i are individual i's average gross earnings following the start of RB eligibility (or average earnings in the same period for RB-ineligible job seekers). In the top panel, which refers to reemployed job seekers, coefficients are zero. However, for men a marginally significant discontinuity of −2% is detected at the age-50 cutoff, so we cannot exclude that for these older male job seekers RB speeds up reemployment in somewhat lower-paying jobs. In the bottom panel, which refers to all job seekers, log wages are again set to zero for job seekers who never left unemployment.

The second claim is supported by Fig. 8, which shows that the drop in the unemployment survival function estimated for women at the age 40-cutoff at the outset of RB eligibility is mirrored by an increase in the probability of employment in a job that will last six months or more. Interestingly, this effect is detected also at the age-50 cutoff. So actually RB speeds up reemployment towards job that last longer than those found by ineligible job seekers, for both younger and older women. This outcome is again unsurprising given that one of the conditions to claim the bonus is that the new job is kept for at least three months.

Cost-benefit calculation. Cost-effectiveness of RB in the experiment is harder to establish than for RV, because of the many eligible job seekers for whom RB is a windfall gain (i.e., individuals who cash in the bonus even if they would have found a job anyway). Given no significant effects for men at either cutoff or for women at the age-50 cutoff, for these groups RB was clearly a loss for the PES. As for women at the age-40 cutoff, notice that the fraction who are no longer enrolled with the PES in the *absence* of RB at the end of the eligibility period (six months) is about 45%. Considering an average effect of about 7 pp that lasts for nine months (see Fig. 7), for every 100 women in this group the cost of the policy is bonus payment to $45+7 = 52$ of them (i.e., €104k), and the benefit to the public sector is that 7 of them no longer collect UI and pay taxes on earnings nine months earlier than in the absence of the bonus. To break even, these benefits should be €104k$/(7 \times 9)$ ≈€1650 per month, per job seeker. This is an implausible figure given that the maximum UI monthly allowance is €1300 and that income and payroll taxes levied in Italy on employees with annual earnings of €22k (the average wage of reemployed women in the RB sample) are about 33%, or about €600 per month. Thus, if it is on top of conventional policies, RB is a loss even for women at the age-40 cutoff.

6. Interpreting the experimental findings

These empirical effects of RV and RB can be interpreted using a one-sided, partial equilibrium search framework. Although the mechanisms described in the model are well-known, it is useful to expose them in the context under investigation because an important aspect of these programs is the presence of time limits that induce, in different directions and to a different extent, discontinuities in the reservation wage and in the job-offer arrival rate. Thus, the model provides theoretical predictions that can be contrasted with the empirical findings in order to speculate in a disciplined way about the driving mechanisms.

In the model, the job-offer arrival rate is endogenous and reflects job seeker's search effort and provider's search effort for RV-eligible job seekers who hire the provider. These efforts may be strategic

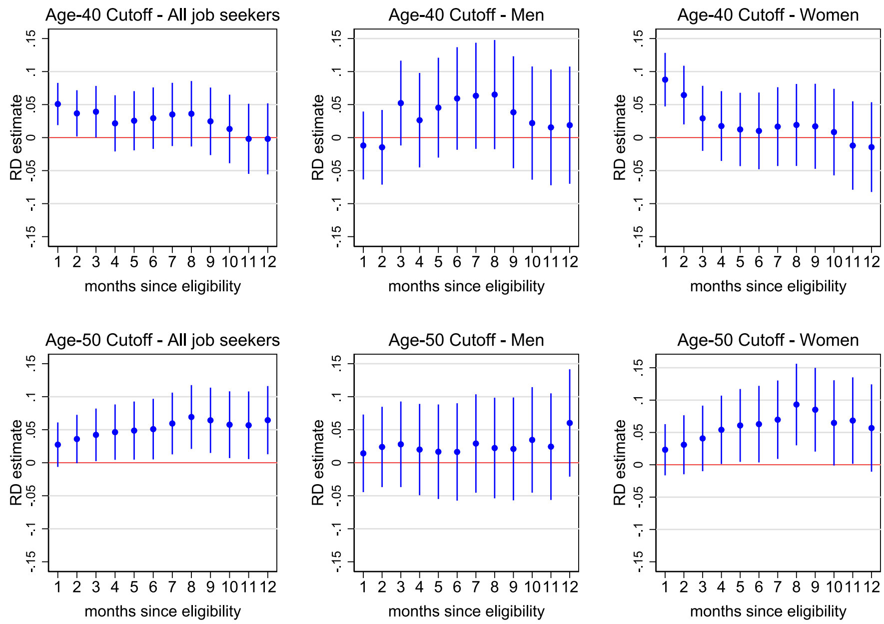

Fig. 8. Effects of RB on the probability of employment in a job lasting 6+ months.
Notes: RD estimates of parameter $\beta_{\mathrm{RB},m,c}$ and 95% confidence intervals, from OLS estimation of Eq. (3). The dependent variable is a dummy for whether a job seeker is employed, m months after the start of RB eligibility ($m = 1, \ldots, 12$), in a job that will last at least 6 months, retrospectively. Standard errors are clustered at the job seeker level. Sample: 7275 job seekers who started an unemployment spell between September 2017 and January 2018, described in column [4] of Table 1.

complements or substitutes. Reemployment services catered for by the provider may also persistently improve the job-offer arrival rate because of their training content in terms of search skills. The model predicts that RB, by reducing the reservation wage and by increasing job seeker's search effort and thereby the job-offer arrival rate, has an unambiguous yet transitory negative effect on the probability of remaining in the unemployment state. RV, instead, has an ambiguous effect on this probability because it increases both the reservation wage and the job-offer arrival rate. Yet, any effect may be persistent. Moreover, the model predicts gender heterogeneity if men and women value work disamenities (such as child care costs and commuting time) differently. If some women value them more than men, they may be registered as unemployed but exert low search effort and set a high reservation wage, *ceteris paribus* (Bergemann and Van Den Berg, 2008). In this case, RB reduces women's likelihood of being unemployed more than men's and RV increases the job-finding rate more for men than for women if job seeker's and provider's search efforts are complements. Although observational and experimental studies indicate that women actually set *lower* reservation wages than men (Brown et al., 2011; Cortés et al., 2023; McGee and McGee, 2023; Bonaccolto-Töpfer and Satlukal, 2024; Basbug and Fernandez, 2024), women may have a higher opportunity cost of employment due to family arrangements. For example, Le Barbanchon et al. (2020) find that women in France set a lower reservation wage than men but have a *higher* reservation commute. In our model there is no commute and so the reservation wage also captures the propensity to accept job offers that, in reality, are associated with different disamenities (and different evaluation of such disamenities by women) like commuting time.

6.1. Model setup

Time is denoted by t, is continuous, and the horizon is infinite. The timeline is the horizontal axis of Fig. 9. At $t = 0$, a risk-neutral worker who maximizes the discounted present value (DPV) of earnings joins the workforce as unemployed and starts receiving unemployment benefits, $u(t)$. These benefits decline at a constant rate and jump to 0 at exhaustion date $t = u_{\mathrm{off}}$. The job seeker draws, without recall, independent and identically distributed job offers at rate $\lambda(\cdot) \geq 0$ from a given, stationary wage distribution $F(w)$ with support $[0, \infty)$. The arrival rate depends on search effort. The job seeker exerts search effort $e(t)$ directly. Using subscripts to denote partial derivatives, it is reasonable to assume that $\lambda_e(\cdot) > 0$ and $\lambda_{ee}(\cdot) < 0$. The cost of a job seeker's search effort is given by $c(e)$, an increasing and convex function such that $c(0) = 0$. Jobs last forever.

In this setting we introduce two alternative policies that capture the essence of RB and RV in interaction with UI in our experiments. First, at $t = b_{\mathrm{on}} > 0$, the job seeker becomes, unexpectedly, eligible for a one-off payment $b > 0$ (a **bonus**) if a job offer is received and accepted before $t = b_{\mathrm{off}} > b_{\mathrm{on}}$. Thus, $b(t) = b$ if $t \in [b_{\mathrm{on}}, b_{\mathrm{off}}]$ and $b(t) = 0$ otherwise. Second, at $t = v_{\mathrm{on}} > b_{\mathrm{off}}$ the job seeker is endowed, again unexpectedly, with a sum $v > 0$ (a **voucher**) that can be used to hire a service provider. If hired, the provider exerts search effort E on behalf of the job seeker, thus bearing the cost $C(E)$, a function that is also assumed to be increasing, convex, and such that $C(0) = 0$, and collects v if the job seeker receives and accepts a job offer before $t = v_{\mathrm{off}} > v_{\mathrm{on}}$. Thus, $v(t) = v$ if $t \in [v_{\mathrm{on}}, v_{\mathrm{off}}]$ and $v(t) = 0$ otherwise.[15] The combination of job seeker and provider search efforts results in an offer arrival rate of $\lambda(e, E)$. We also assume that $\lambda_E(.) > 0$ and $\lambda_{EE}(.) < 0$. Function $\lambda(\cdot)$ may depend on time, although we do not keep track of this fact in the notation.

The job seeker's problem consists of (i) whether (or not) to accept a job offer; (ii) deciding how much search effort to exert; and (iii) whether (or not) to hire the provider in case of eligibility for RV. The provider's problem is how much search effort to exert on behalf of the job seeker, if hired. Let $\rho \in (0, 1)$ denote the discount rate, $V(w, j)$ the value of being employed in a job obtained at $t = j$ and paying wage w, d work disutility (e.g., child care costs, commuting, foregone leisure), and $U(t)$ the value of being unemployed (i.e., the value of search). Then, part (i) of job seeker's problem is characterized

[15] In Fig. 9, it is $v_{\mathrm{on}} > b_{\mathrm{off}}$ for mere expositional convenience. The RV experiment and the RB quasi-experiment took place one year apart and involved different cohorts of job seekers in their fifth month of unemployment, without any interaction. Thus, when representing RV and RB in the model, it is convenient to assume that one program starts after the eligibility for the other runs out. The order is irrelevant.

by Bellman's equations:

$$\rho V(w,j) = w - d + \rho b(j), \tag{5}$$

$$\rho U(t) = u(t) + \lambda(e,E)\int_0^\infty \max\{V(w,t) - U(t), 0\}dF(w) - c(e). \tag{6}$$

The reservation wage, $w^*(t)$, is time-dependent due to the time limits in UI, RB, and RV, and is such that the indifference condition $V(w,t) = U(t)$ holds. This property leads to:

$$w^*(t) - d + \rho b(t) - u(t) = \frac{\lambda(e,E)}{\rho}\int_{w^*(t)}^\infty [1 - F(w)]dw - c(e). \tag{7}$$

As for part (ii) of the problem, since the job seeker maximizes the DPV of earnings, search effort is chosen so as to maximize $U(t)$. It follows (considering Eq. (7) and the envelope condition $\frac{\partial w^*(t)}{\partial e} = 0$) that job seeker's optimal effort, $e^*(t)$, is also time-varying and is such that the following first-order necessary condition holds:

$$\frac{\lambda_e(e^*(t),E)}{\rho}\int_{w^*(t)}^\infty [1 - F(w)]dw - c_e(e^*(t)) = 0. \tag{8}$$

Eqs. (7) and (8) form a system of two equations that define implicitly the reservation wage and job seeker's search effort, for given provider's effort. We take into account the endogeneity of provider's effort below.

6.2. Predictions

The job seeker's response to RB is easy to characterize:

Proposition 1. *A larger reemployment bonus value at t decreases $w^*(t)$ and increases $e^*(t)$. These effects are transitory and are concentrated during the eligibility period.*

This claim is established in two steps: first, deriving equation (8) with respect to $b(t)$ and using the second-order sufficient condition for optimal search effort indicates that $\frac{\partial w^*(t)}{\partial b(t)}$ and $\frac{\partial e^*(t)}{\partial b(t)}$ have opposite sign; second, deriving equation (7) with respect to $b(t)$ and using equation (8) leads to $\frac{\partial w^*(t)}{\partial b(t)} < 0$, and so $\frac{\partial e^*(t)}{\partial b(t)} > 0$. It is intuitive that RB decreases the reservation wage and increases search effort, as this policy simply counterbalances the search disincentive that comes with unemployment benefits. Thus, denoting by $\lambda^*(t)$ the job-offer arrival rate at the optimum, the next result follows directly.

Proposition 2. *A larger reemployment bonus value at t increases $\lambda^*(t)$. This effect is transitory and is concentrated during the eligibility period.*

Taking the job seeker's reservation wage and optimal effort as given, the provider chooses effort to maximize expected profits $\lambda(e^*(t),E)[1 - F(w^*(t))]v(t) - C(E)$, and so its optimal effort, $E^*(t)$, in case it is hired,[16] is implicitly defined by

$$\lambda_E(e^*(t),E^*(t))(1 - F(w^*(t)))v(t) - C_E(E^*(t)) = 0. \tag{9}$$

Note that, contrary to the job seeker, the provider's problem is static. At the interior optimum, the implicit function theorem applied to Eq. (9) establishes the following:

Proposition 3. *Provider's optimal search effort, $E^*(t)$, increases with the reemployment voucher value. This effect may persist beyond the eligibility period.*

[16] In the absence of eligibility for RV or if an eligible job seeker chooses not to hire the provider, it is $v(t) = 0$ and we have the corner solution $E^*(t) = 0$. An eligible job seeker hires the provider at $\tau \in [v_{\text{on}}, v_{\text{off}}]$ iff $U^*(\tau; v > 0) > U^*(\tau; v = 0) + \kappa$, for κ the opportunity cost of job seeker's engagement. Note that, in this condition, the value of searching is computed at the optimum (reservation wage, job seeker's search effort, and provider's search effort), as indicated by a *.

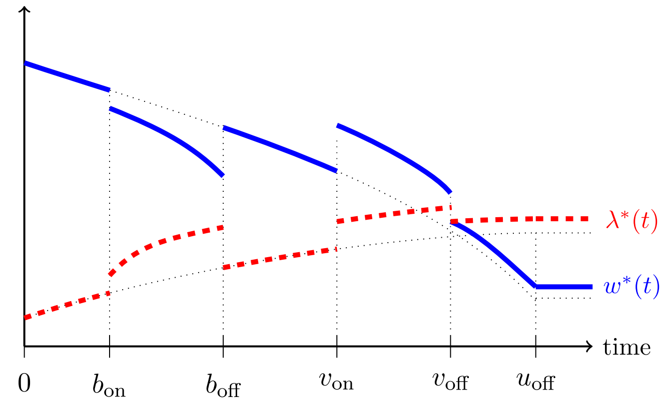

Fig. 9. Reservation wage and job-offer arrival rate dynamics.
Notes: The figure illustrates the path followed (qualitatively) by the reservation wage $w^*(t)$ and the job-offer arrival rate $\lambda^*(t)$ at the optimum. The job seeker is eligible for UI in $[0, u_{\text{off}}]$, for RB in $[b_{\text{off}}, b_{\text{off}}]$, and for RV in $[v_{\text{off}}, v_{\text{off}}]$.

The second part of this statement (and of Proposition 4) derives from the fact that if provider's services persistently improve one's search skills (i.e., have a training component), then $\lambda_E(e^*(t), E^*(t))$ may include a direct positive impact of E on $\lambda(\cdot)$, a persistent shift.

The response of job seeker's own effort to provider's effort depends on whether the two are strategic complement or substitutes, i.e., on the sign of λ_{eE}. Yet it follows from basic results in producer theory that the job-offer arrival rate cannot decline. To see this, note that function $\lambda(e,E)$, from the job seeker's perspective, is a job-offer production function with a costly input e and a free input E that is determined exogenously. A larger RV value induces a windfall increase in E. While, as a result, e^* may increase or decrease depending on the sign of λ_{eE}, output cannot decline at the new optimum and so RV also increases the option value of searching. These observations lead to the following:

Proposition 4. *A larger reemployment voucher value at t weakly increases $\lambda^*(t)$ and $w^*(t)$. This effect may persist beyond the eligibility period.*

We are now ready to characterize the reservation wage and job-offer arrival rate in this nonstationary search environment. Since $b(t)$, $u(t)$, and $v(t)$ are functions of time, Van Den Berg's (1990) results apply directly and lead to reservation wage and job-offer arrival rate dynamics that, qualitatively, follow the pattern represented in Fig. 9. Absent RB and RV, $w^*(t)$ and $\lambda^*(t)$ would, respectively, decrease and increase continuously due to time-limited UI. At the end of UI eligibility, these functions would become flat. When RB and RV are introduced, $w^*(t)$ and $\lambda^*(t)$ jump discontinuously in the direction indicated by Propositions 1, 2, and 4. During RB eligibility, $w^*(t)$ and $\lambda^*(t)$ decrease and increase continuously, respectively, while during RV eligibility, $\lambda^*(t)$ increases and $w^*(t)$ decreases. In the RB case, the reservation wage and job-offer arrival rate revert discontinuously to the respective initial paths at the end of eligibility. In the RV case, the reservation wage and job-offer arrival rate may be both above the respective initial paths.

We are also interested in how $w^*(t)$ and $\lambda^*(t)$ vary with work disutility, d. This parameter captures child care costs, commuting, etc., and so in light of the evidence that it is higher for women than men (e.g., Kaplan and Schulhofer-Wohl, 2018; Jacob et al., 2019; Le Barbanchon et al., 2020), such relationship has a bearing on gender heterogeneity in the effects of RB and RV programs. It is immediate that in our model higher work disutility, by decreasing the value of being employed, increases the reservation wage and decreases job seeker's search effort. Applying the implicit function theorem to Eq. (9) shows that provider's optimal effort, E^*, responds instead in an ambiguous way to the work disutility of RV-eligible job seekers who hire it. However, if $\lambda_{eE} > 0$ (i.e., if job seeker's and provider's search efforts are complements) then E^* decreases with d. Intuitively, in the presence of such complementarity, the provider exerts less costly effort for high-d job seekers because they also exert relatively low search effort—a form of statistical gender discrimination. Similarly, applying the implicit function theorem to Eq. (7) shows that RB boosts job seeker's effort more for high-d individuals. RB also decreases the reservation wage more when d is larger, generally.

The hazard from unemployment at time t is

$$h(t) = \lambda^*(t)(1 - F(w^*(t))), \tag{10}$$

which decreases in $w^*(t)$ and increases in $\lambda^*(t)$, while the unemployment survival function is

$$S(t) = 1 - \int_t^\infty h(\tau)d\tau = 1 - \int_0^t \lambda^*(\tau)(1 - F(w^*(\tau)))d\tau, \tag{11}$$

theoretical counterpart of the main outcome in our empirical analysis. The dynamics illustrated in Fig. 9 imply that: (i) RB has a negative effect on $S(t)$ that arises during eligibility; (ii) RV's corresponding effect is ambiguous (negative if the reservation wage effect is dominated) yet possibly persistent.[17] Moreover, the concavity of $\lambda(e, E)$ and the responses of $w^*(t)$ and $\lambda^*(t)$ to d imply that if women have higher work disutility than men then: (iii) RB reduces $S(t)$ more for women than for men; (iv) RV reduces $S(t)$ more for men than women if job seeker's and provider's efforts are complements.

6.3. Contrast with the experimental findings

Our empirical findings align well with these theoretical predictions. Starting with the response and take-up rates in the RV experiment, we have shown that these rates are generally smaller for women than for men, as implied by the model if female job seekers have a larger opportunity cost of engagement time with providers of reemployment services.

Gender heterogeneity in RV and RB impacts on the unemployment survival function $S(t)$ is one of our central findings. For the RV, such heterogeneity is consistent with the theory if women have higher work disutility than men *and* job seeker's and provider's search efforts are complements. In this case, in light of Proposition 4, the persistent negative impact on men's unemployment survival function implies that the reservation wage effect is dominated and that providers' reemployment effort has a training component—i.e., providers build search skills that confer treated male job seekers a persistent advantage over the control group in terms of job-offer arrival rate. The nil and possibly detrimental effect of RV for women would then reflect statistical gender discrimination.

As for the RB, gender heterogeneity is again consistent with the theory if women have higher work disutility than men. In this case, the bonus reduces women's likelihood of remaining unemployed more than men's because by counteracting women's higher opportunity cost of work directly, it reduces the reservation wage and increases search effort more for them than for men. The fact that a significant effect is detected for women at the age-40 cutoff but not at the age-50 cutoff, can be explained in the model if women's work disutility is higher at 40, when children are still young, than at 50, when childcare requirements are no longer pressing. Moreover, the result that RB affects women's reemployment at the start of eligibility implies that also in this case the effort effect dominates the reservation wage effect. Yet this quick activation induced by the RB confers women only a transitory advantage. These results are consistent with Propositions 1 and 2.

[17] The ambiguity is resolved if $F(\cdot)$ belongs to particular distribution families (Van Den Berg, 1994).

7. Conclusions

We have assessed, via policy experimentation, programs aimed at speeding up reemployment of job seekers at risk of long-term joblessness via financial incentives to either third party providers—the reemployment voucher—or job seekers themselves—the reemployment bonus. The voucher was effective for men, and we conjecture that its ineffectiveness for women may reflect statistical gender discrimination by profit-maximizing providers. While this is a mere conjecture suggested by the particular job search theory that we use to interpret the experimental findings, this possibility suggests the need for close monitoring of private providers' activities. The voucher is also cost effective in our experiment, owing to the conditional (on reemployment) nature of providers' compensation. On the contrary, while the bonus was effective for women, it was not cost effective, due to the large share of job seekers who would have found a job anyway and for whom the bonus is therefore a windfall income gain. We conclude that the voucher is a viable alternative to traditional job-search assistance when the Public Employment Service is subject to a tight resource constraint. However, we have also demonstrated that when job seekers are allowed to take up the voucher or decline it, their demand is dampened by the supply of UI benefits. This finding is suggestive of the need to strengthen conditionality in UI provision. Overall, our evaluation confirms that broadening the scope of active labor market policy makes it overly pessimistic to view this class of public policy as largely ineffective relative to its cost.

CRediT authorship contribution statement

Giulio Zanella: Writing – review & editing, Writing – original draft, Software, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Riccardo Salomone:** Project administration, Conceptualization.

Declaration of competing interest

Giulio Zanella is a member of the Trento PES Scientific Committee, and Riccardo Salomone is the President of the Trento PES and the Chair of its Scientific Committee. The Trento PES Scientific Committee was also the IRB for the policy experiments described in the article. Ethical aspects were carefully evaluated by the Committee during the design stage. Although the Trento PES is an interested party, no one had the right to review this article prior to its circulation. No specific financial support was received for this research, other than general research funding from the authors' respective universities. However, the Trento PES provided in-kind support in the form of access to administrative data and sending letters (bearing the paper and mailing costs) to the job seekers involved in the policy experiments described in the article.

Appendix

Fig. A-1 shows estimates of the ATET of RV (top row) and intensive JSA (bottom row) programs. These are obtained by estimating by 2SLS the analog of Eq. (1), where the RHS, explanatory variables are program take-up dummies that are instrumented by random program assignment. That is, the second- and first-stage equations are:

$$Y_{i,m} = \kappa_m + \mu_{\mathrm{RV},m}\pi_{\mathrm{RV,i}} + \mu_{\mathrm{JSA},m}\pi_{\mathrm{JSA},i} + \eta X_i + \epsilon_{i,m}, \tag{A.1}$$

$$\pi_{\mathrm{RV},i} = \hat{\chi}_{\mathrm{RV}} + \hat{\theta}_{\mathrm{RV}}\mathrm{RV}_i + \hat{\phi}_{\mathrm{RV}}\mathrm{JSA}_i + \hat{\psi}_{\mathrm{RV}}X_i, \tag{A.2}$$

$$\pi_{\mathrm{JSA},i} = \hat{\chi}_{\mathrm{JSA}} + \hat{\theta}_{\mathrm{JSA}}\mathrm{JSA}_i + \hat{\phi}_{\mathrm{JSA}}\mathrm{JSA}_i + \hat{\psi}_{\mathrm{JSA}}X_i, \tag{A.3}$$

where $\pi_{\mathrm{RV,i}}$ and $\pi_{\mathrm{JSA,i}}$ denote the predicted probabilities that individual i takes up RV and JSA, respectively, a "hat" denotes a prediction coefficient from a linear probability model (first-stage equations), and $\mu_{\mathrm{RV,m}}$ and $\mu_{\mathrm{JSA,m}}$ are ATET parameters because of one-sided noncompliance (see the paper for details).

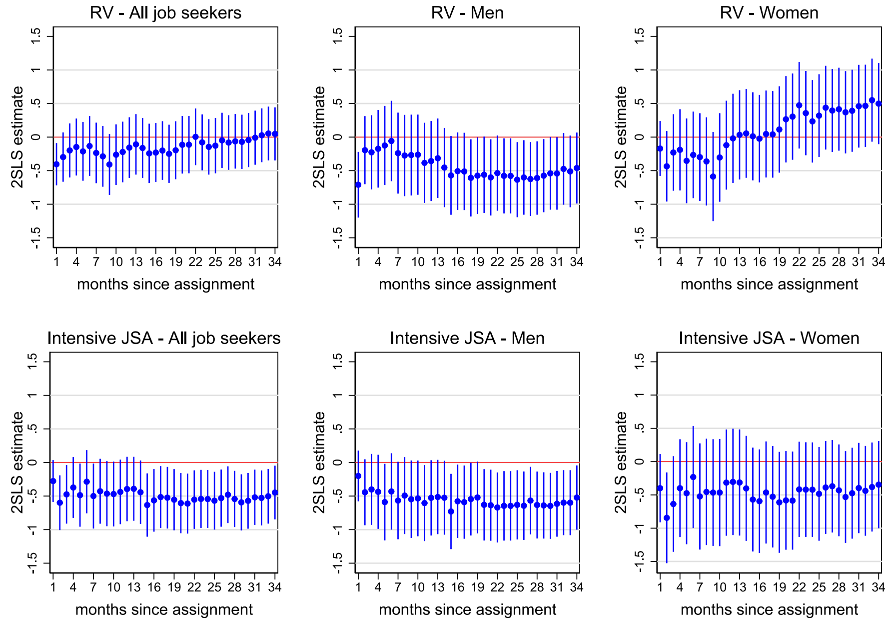

Fig. A-1. ATET of RV and JSA by month from assignment and by gender.
Notes: The figure reports estimates of ATET parameters $\mu_{RV,m}$ and $\mu_{JSA,m}$ and the associated 90% confidence intervals, from 2SLS estimation of Eq. (A.1). Standard errors are clustered at job seeker level. Sample: 2690 job seekers in columns [1]–[3] of Table 1.

Table A.1
Sample statistics by gender.

	[1] Men	[2]	[3]	[4]	[5] Women	[6]	[7]	[8]
	RCT study			RDD study	RCT study			RDD study
	JSA	RV	Controls	RB	JSA	RV	Controls	RB
Age	47.0	48.1	45.8	41.3	45.2	45.3	44.5	40.4
	(11.5)	(11.9)	(11.9)	(13.9)	(11.9)	(11.7)	(11.8)	(12.9)
Italian	0.76	0.78	0.72	0.75	0.72	0.73	0.68	0.72
	(0.43)	(0.41)	(0.45)	(0.43)	(0.45)	(0.45)	(0.47)	(0.45)
First unemployment spell	0.30	0.32	0.28	0.33	0.37	0.41	0.43	0.41
	(0.46)	(0.47)	(0.45)	(0.47)	(0.48)	(0.49)	(0.50)	(0.49)
Job center 1	0.34	0.29	0.30	0.31	0.30	0.33	0.29	0.28
	(0.47)	(0.46)	(0.46)	(0.46)	(0.46)	(0.47)	(0.46)	(0.45)
Job center 2	0.16	0.18	0.18	0.18	0.17	0.18	0.16	0.17
	(0.37)	(0.38)	(0.38)	(0.38)	(0.37)	(0.38)	(0.37)	(0.38)
Job center 3	0.09	0.11	0.12	0.10	0.10	0.11	0.11	0.11
	(0.29)	(0.31)	(0.32)	(0.31)	(0.30)	(0.31)	(0.31)	(0.31)
Job center 4	0.08	0.11	0.10	0.15	0.10	0.09	0.12	0.16
	(0.28)	(0.31)	(0.30)	(0.36)	(0.28)	(0.29)	(0.32)	(0.37)
Residual UI (months)	7.9	8.4	8.3	–	8.3	7.7	8.1	–
	(4.9)	(5.1)	(5.1)		(5.0)	(5.1)	(5.1)	
N	243	279	591	2,997	378	422	777	4,278

Notes: The table reports, in columns [1]–[3], the mean and (in parentheses) SD of pre-experimental variables for job seekers assigned to job-search assistance (JSA), the reemployment voucher (RV), and the control group in the RV experiment. Column [4] reports the corresponding covariates in the reemployment bonus (RB) experiment. Job centers 1–4 are in the four largest urban centers.

Table A.1 reports summary statistics by gender. Like in Table 1, averages are similar across treatment and control groups for both men and women, and also across the RCT and RDD study samples—except for age, for the reasons explained in the main text.

Data availability

Data will be made available on request.

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