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Job Search Periods for Welfare Applicants: Evidence from a Randomized Experiment[†]

By Jonneke Bolhaar, Nadine Ketel, and Bas van der Klaauw*

We combine a randomized experiment with administrative data to study the effects of mandatory job search periods in the Dutch welfare system. Job search periods postpone the first welfare benefits payment and encourage applicants to start searching for jobs actively. Job search periods substantially reduce benefits take up. The decline in benefits receipt is permanent, but fully compensated by increased earnings because of higher reemployment rates. We do not find detectable effects on health and crime outcomes, nor do we observe income declines for more vulnerable applicants. Our results suggest that job search periods are an effective instrument for targeting benefits to welfare applicants. (*JEL* C93, I38, J31, J64)*

This paper evaluates job search periods that aim to reduce moral hazard problems in the welfare benefits system. A job search period is a mandatory waiting period of at most four weeks in which an applicant is encouraged to actively search for work. During the job search period, the application is put on hold and eligibility for welfare benefits is checked after the job search period. When an applicant is considered eligible, benefits are paid retrospectively from the moment of initial application (so before the job search period was imposed).

A job search period can affect labor market outcomes in several ways. First, a job search period makes the application process for welfare benefits more complex and increases the costs of applying.[1] After the job search period, applicants have to pay a second visit to the welfare office to confirm their application for welfare benefits. This can decrease the likelihood to receive benefits even in absence of job finding. Second, the job search requirement can increase the likelihood of finding a job

and thereby reduce welfare benefits receipt. During the job search period workers can themselves decide which jobs to apply to, but when collecting welfare benefits workers should accept all jobs offered to them. Black et al. (2003) suggest that announcing obligations stimulates benefits recipients to find work. Both mechanisms serve as a self-selection or self-screening device (Parsons 1991), but affect a different part of the population of applicants. An increase in job finding reduces take up of applicants with relatively good labor market prospects, while increased complexity discourages applicants that do not find a job but cannot deal with the complexity of the application process.

There is a limited literature linking the take-up of benefits to the entry requirements of the program (Currie 2006).[2] For example, Parsons (1991) and de Jong, Lindeboom, and van der Klaauw (2011) show that more stringent entry requirements for disability insurance induce self-selection among applicants. Welfare differs substantially from disability insurance; it is a safety net covering individuals who have very low other income. In the United States, welfare is mostly used to support single-parent households. In European countries, it also supports unemployed workers, who are no longer entitled to unemployment insurance. The population of welfare benefits recipients contains workers with limitations, long-term unemployed workers, and workers with low skills. Therefore, it is important to consider heterogeneous effects and to focus on various sources of income, both for recipients and non-recipients of the benefits.[3]

To evaluate job search periods we conducted a randomized experiment from April 2012 to March 2013, incorporating the full population of welfare applicants with a potential to work in Amsterdam. We combine various data sources to construct a detailed administrative dataset describing the participants in the experiment. This allows us to look at the implications of a job search period on welfare benefits receipt and study the effect on alternative sources of income. In addition, we consider engagement in criminal activities and health status, which are associated to negative externalities but have to our knowledge not been studied before in the context of increased application costs for benefits programs.

The setup of our randomized experiment is similar to an encouragement design (Duflo, Glennerster, and Kremer 2008), but we impose a stronger encouragement than usually. We randomize treatments over caseworkers, who receive the instruction to apply one particular treatment, a default option, to all their new clients in a given period. In case the default option is really not appropriate, caseworkers are allowed to deviate but should provide a motivation. The possibility to deviate increases support for the experiment among caseworkers. The design exploits the random assignment of applicants to caseworkers within each local welfare office.[4] Our empirical

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[2] Bhargava and Manoli (2015) provides empirical evidence that the take-up of the earned income tax credit increases after explaining the application procedure, and Krueger (1990) argues that a waiting period reduces participation in a disability insurance program. Alatas et al. (2016) finds that adding a small application cost substantially improves targeting through self selection in Indonesia's Conditional Cash Transfer program.

[3] Observing income of non-recipients is important when studying take-up decisions of welfare benefits (Hotz, Mullin, and Scholz 2002; Bitler, Gelbach, and Hoynes 2006).

[4] Within a local office all caseworkers have the same target concerning exit to work. The random assignment of applicants to caseworkers allows the welfare agency to benchmark caseworkers and ensures that all caseworkers have the same fair chance to meet the target.

strategy is similar to Maestas, Mullen, and Strand (2013), who exploit variation in examiners' allowance rates as an instrument for disability benefit receipt, using that applicants are randomly assigned to disability examiners.[5] Crépon et al. (2013) use an encouragement design to evaluate active labor market programs in France, and Sherman, and Berk (1984) use a similar design to study the effects of police responses to domestic violence calls. Compared to these studies, the variation that we exploit is somewhat higher. Our study has a compliance rate to the random treatment assignment of 46 percent compared to 35 percent in Crépon et al. (2013), while Maestas, Mullen, and Strand (2013) exploit a natural variation of 23 percent.[6]

Imposing a job search period reduces the likelihood to receive welfare benefits by 20 percentage points. The effect is significant up to six months after registration, and during these months total welfare benefits payments are about 25 percent lower. There is no spillover to other benefits schemes and the lower income from welfare benefits is fully compensated (108 percent) by higher earnings.[7] A job search period does not increase the likelihood to engage in criminal activities and does not increase individual health-care expenditures. A job search period is most effective for the least vulnerable applicants, for this group benefits receipt decreases with 86 percent. The lack of negative side effects of a job search period also holds for the most vulnerable applicants. Our preferred conclusion is, therefore, that increased job finding is the main mechanism through which job search periods affect individual outcomes. Finally, we find that the estimated local average treatment effect is not sensitive to changes in the sample and the group of compliers. This implies that caseworkers do not succeed in targeting job search periods to those applicants for which they are most effective.

Currently, there is a tendency among policy makers in many countries to restrict access to benefits schemes and to be stricter on job search requirements. Lachowska, Meral, and Woodbury (2016) and McVicar (2008) show the importance of job search requirements for job finding. The costs of imposing job search periods are very low and the effects are larger than other policies targeted toward welfare recipients and disadvantaged unemployed workers. For example, the Canadian self-sufficiency project offers an earnings subsidy for three years to welfare recipients who find work within 12 months. Card and Hyslop (2005) find that after 18 months the effect on employment is 14 percentage points, which reduces afterward and fades away after the subsidy ends. Van der Klaauw and van Ours (2013) study financial incentives for welfare recipients in Rotterdam and estimate that a re-employment bonus of about 2,000 euros increases the job finding rate by about 2 percentage points, while a punitive benefits reduction has an effect of about 5 percentage points. Bitler, Gelbach, and Hoynes (2006) show that Connecticut's job first program, which reduces lifetime welfare benefits receipt to 21 months and substantially reduces the

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[5] We cannot exploit the natural variation per caseworker, such as Autor et al. (2017) or Maestas, Mullen, and Strand (2013), because caseworkers also give guidance to returning applicants after an unsuccessful job search period.

[6] The compliance rate in Sherman and Berk (1984) is unclear. Of the cases handed in by police officers, compliance is around 80 percent, but not all police officers handed in (all) cases.

[7] The full monetary compensation does not necessarily imply that individual welfare increases. When substituting earnings for benefits individuals lose leisure, but no longer have to make job search costs and adhere to requirements of the welfare agency.

marginal tax rate, has an earnings effect between −US\$300 and \$500 with a mean of \$82. Markussen and Røed (2016) consider an extensive activation program combined with income support for potential welfare recipients and find that after 4 years participation increases employment rates by 18 percentage points. Because the employment concerns small, poorly paid jobs, income effects are at most modest. Black et al. (2003) find that an activation program targeted toward disadvantaged unemployment insurance benefits recipients in Kentucky reduces benefits receipts by 2.2 weeks and increases earnings by about \$1,050. The threat effect is the main explanation for the effect on job finding. Our results emphasize the importance of factors that are not generally considered as the main incentives for the take-up of benefits. In particular, we show that the job search period, which entails no direct financial incentives, achieves an impact that is at least comparable to costly activation programs.

The remainder of the paper is structured as follows. Section I provides details about the Dutch benefit system, explains the experimental design and shows compliance rates. Section II describes the data and provides evidence on the random assignment. In Section III, we discuss the empirical strategy and identification. Section IV presents the main results, while Section V investigates whether a job search period has a negative effect on specific subgroups. Section VI discusses the generalizability of our results by interpreting the estimated local average treatment effect. Section VII concludes.

I. Setting and Experimental Design

A. *Welfare in the Netherlands*

In the Netherlands, welfare serves as a safety net and provides a minimum income to households with no or not enough means of living. Welfare benefits are means tested (on both income and wealth) and the benefits level depends on the composition of the household. Rules about eligibility and the level of benefits are determined nationwide, but the responsibility for the implementation is at the municipality level. The regular benefits range from 70 percent of the minimum wage for a single-person household to 100 percent for a couple with children.[8] During our observation period, the national net minimum wage was about 1,200 euros per month. Municipalities can give additional benefits on top of the regular benefits; Amsterdam pays a housing allowance of 133 euro per month. Furthermore, all Dutch households with low income are entitled to subsidies for housing, health insurance, and children.[9] Unlike in the United States, there is no maximum to the time period that a household can receive welfare benefits (or subsidies). If a welfare recipient finds part-time employment or has part-time employment with earnings below the welfare benefits level, earnings have a marginal tax rate of 100 percent. For the health insurance and housing subsidies other marginal tax rates apply (child subsidies are not income related).

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[8] Table A1 in the Appendix provides the exact benefits levels at the time of the experiment.
[9] The maximum monthly amounts of these subsidies are 309 euro (for housing subsidies), 70 euro (health insurance subsidies), and per child 84 euro (child subsidies).

Applicants for welfare benefits have to show extensive proof of their (past) income, bank accounts, housing etc. Welfare recipients have to comply with job search requirements, and are obliged to accept all jobs, irrespective of the match with their education or work experience. These obligations are set by the national government, but the municipality has discretion in deciding about how welfare recipients are supported in their job search.

B. *Job Search Period*

Our experiment focuses on the job search period, a policy that was introduced by the welfare agency in 2011. A job search period postpones the application for welfare benefits at most four weeks, during which the individual has to actively search for work. The application for benefits will only be activated if the applicant returns to the welfare agency after the job search period. If the welfare application is activated and processed, the applicant will (retrospectively) receive benefits starting at the date of the initial registration. A job search period thus only delays the first payment of benefits; it does not reduce the amount of benefits that an individual is entitled to.[10] Figure 1 gives a schematic representation of the application process for welfare benefits, including the job search period. Irrespective of the job search period, the welfare application needs to be processed within eight weeks after the day of registration.[11]

The decision to apply a job search period is made during the intake meeting and, before the experiment, was left to the caseworkers' discretion. However, job search periods should not be applied if an applicant has severe financial problems or can prove that she has been very active in applying for jobs prior to registration at the welfare agency. An applicant cannot refuse a job search period. When imposing a job search period, the caseworker specifies a minimum number of job applications that the applicant should make within the job search period. The caseworker stresses that during the job search period the applicant can still choose which jobs to apply to, but as soon as the applicant starts receiving welfare benefits it is mandatory to accept all jobs. If the applicant returns from a job search period, the caseworker generally checks whether the applicant has complied with the job search requirement, and can impose a sanction if this is not the case. This sanction is a 30 percent reduction in benefits for the duration of one month. In practice, these sanctions are almost never applied.

C. *Setting of the Experiment*

Our experiment took place in Amsterdam. In January 2012, 6.4 percent of the population between 20 and 65 years old received welfare benefits in Amsterdam

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[10] Applicants who find employment during a job search period can file a request for receiving welfare benefits for the period between the date of registration and the starting date of new employment. This requires completing the application process and is not actively promoted by the welfare agency. Most individuals that find a job during the job search period do not use this possibility.

[11] In our data, approved applicants without a job search period receive their first benefits payment, on average, 38 days after registration. A job search period delays this first payment to, on average, 54 days after registration.
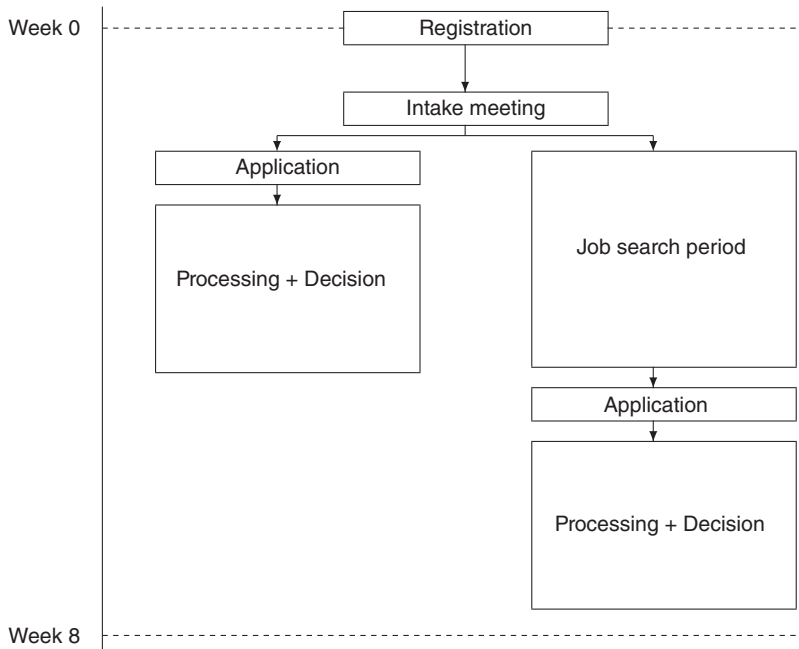
FIGURE 1. WELFARE APPLICATION PROCEDURE

(i.e., 34,550 individuals) compared to 3.1 percent in the Netherlands. During 2012, the inflow into welfare in Amsterdam consisted of 11,706 individuals, while in the same year 8,944 individuals stopped receiving benefits. The larger inflow is a direct result of the second economic downturn after the financial crisis.[12]

Welfare recipients in Amsterdam are divided over five welfare offices. Individuals have to apply for benefits at the local welfare office in their city district. Applicants are classified in one of four classes, depending on their labor market prospects. The type and intensity of guidance given to the applicant and the required job search effort varies over these classes. The class is determined through a computerized program that profiles all workers based on their characteristics. These characteristics include, among others, work history, age, education, language and computer skills, recent detention, and psychological problems.

Job search periods are only applied to applicants in the highest class (class four), which is the class of applicants that should be able to find regular employment within six months. The sample for our experiment consists of individuals in class four that applied for welfare benefits in Amsterdam between April 2012 and March 2013.[13] In addition, we restrict the experiment to individuals that are at least 27 years old, as

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[12] Figure B1 in the Appendix shows GDP growth for the Netherlands and inflow and outflow into welfare benefits in Amsterdam from 2008 until 2014.

[13] Figure B2 in the Appendix plots the inflow per class between 2012 and 2014, also indicating the start and end of the experimental period. The inflow per class is stable over time, and there are no significant changes at the beginning and the end of the experiment.

different rules apply to welfare recipients under age 27. Approximately 40 percent of the inflow are classified as class four.[14]

D. *Experimental Design*

We conducted a randomized experiment in which we manipulate the assignment of job search periods.[15] Applicants are not informed about the experiment to prevent that this knowledge would influence their behavior. Instead of randomizing the treatments over individuals, we randomize the treatments over caseworkers. Caseworkers receive the instruction to apply one particular treatment to all their new clients during a three-month period. We call this particular treatment their *default option*, which makes our design similar to an encouragement design (Duflo, Glennerster, and Kremer 2008). We instruct caseworkers to deviate from the default option only in cases where the default option is really not appropriate. This helped to make the experiment more acceptable for caseworkers. Our design exploits that within local offices welfare applicants are randomly allocated to caseworkers. Welfare applicants are matched to the caseworker with the lowest caseload.

There are three different default options:

- Never: never impose a job search period.
- Always: always impose a job search period if the financial situation of the individual allows for it.
- Normal: the decision to impose a job search period is left to the discretion of the caseworker.

The default option "normal" shows what the caseworkers decide in absence of the experiment, which allows us to study targeting by caseworkers. The experimental period is divided into four periods of three months. Each period the caseworkers receive a new default option that they have to apply to all new applicants assigned to them in this period. This allows us to control for business cycle effects. The randomization of default options over caseworkers took place at the level of the welfare office.

In two local offices (South-East and South-West) the application process is conducted by another team of caseworkers than the team that assists in job finding when actually collecting benefits. At these local offices, the default options were equally randomized over the periods and the caseworkers, with the restriction that each caseworker gets each default option at least once. At the other local offices (West, Center-East, North), the application process is done by the same caseworkers who also assist in job finding. Here, we restricted the randomization such that (i) a caseworker could have the default options "always" and "never" at most once, (ii) each period all default options were present at all local offices, and (iii) over

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[14] Table A2 in the Appendix presents the background characteristics of the applicants in the four different classes, based on the applications between January 2012 and end of April 2014. Applicants in class four have more favorable characteristics, they are slightly younger, higher educated, and less likely to have psychological or physical problems. However, the magnitude of these differences is not very large.
[15] The original research design, including a power analysis, can be found at http://personal.vu.nl/b.vander.klaauw/ResearchProposalDWI2012.pdf.
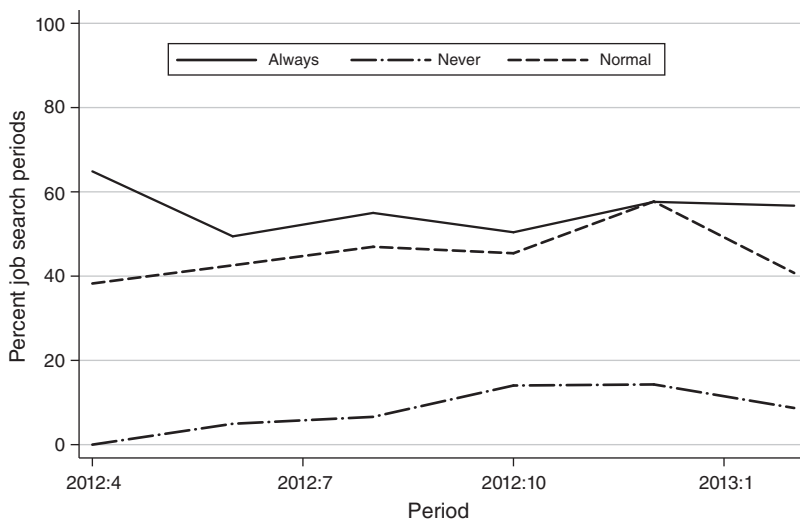
FIGURE 2. PERCENTAGE OF APPLICANTS THAT ARE GIVEN A JOB SEARCH PERIOD BY DEFAULT OPTION, OVER TIME

all local offices about 50 percent of all applicants were treated under "normal" and 25 percent under "always" and "never." Our data do not show evidence that the different randomization plans for the two groups of local offices affect the rate at which job search periods are applied under each default option.

Before the start of the experiment we organized meetings with all caseworkers to inform them about the experiment. At the start of every three-month period, each caseworker was instructed individually about her new default option. Caseworkers were asked to fill in a form about the applicant at each intake meeting. The forms were personalized for each caseworker and had the period-specific default option printed on the form. During the experiment, we visited the local welfare offices almost weekly to answer questions from caseworkers, pick up forms, and to visit caseworkers that had not filled in the forms for new applicants or deviated substantially from their given default option.[16]

E. *Compliance Rates*

Figure 2 shows the fraction of job search periods applied per default option during the experimental period.[17] The distinction between the three default options is most pronounced at the start of the experiment. The percentage of job search periods given under the default "always" remained relatively stable over time, while it increased for the other two default options. During the experiment, we communicated with 112 caseworkers. Some caseworkers left, and new caseworkers

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[16] The forms are filled in for 72 percent of the observations. Given that (almost) all information is also available through the administrative records (for the full sample), we do not use the forms for our main empirical analysis. For the experiment, the forms were very useful as the period-specific default option was printed on them and it gave us a reason to regularly check caseworkers.

[17] Figure 2 reports average compliance rates for all caseworkers. In Section VI, we discuss the variability of compliance across caseworkers.

entered the organization and took over their caseload. Furthermore a few times the caseload of a sick caseworker was taken over by a colleague. Such cases are associated with a larger noncompliance in both the default options "always" and "never." On average, during the experimental period, caseworkers with the default option "never" imposed a job search period to 9 percent of the applicants, caseworkers with default option "always" applied a job search period to 55 percent of the applicants, and caseworkers with default option "normal" gave a job search period to 46 percent of the applicants.

II. Data

A. *Data Sources*

Our analysis employs data from three different sources, that are linked using unique identifiers for each individual. The welfare agency of Amsterdam provides administrative information on the date of registration at the welfare office, date of application for welfare, start and end date of collecting welfare benefits, whether a job search period is applied and the identity of the caseworker that conducted the intake meeting. The individual characteristics of applicants that are registered in these data are date of birth, gender, household composition, and highest level of education. Furthermore, we observe the exact benefits payments. Second, we use data from the national social insurance administration to observe weekly information for each individual on the amount of income from employment, hours worked, and income from other benefit schemes.[18] We have this information for all participants in the experiment from January 2008 until October 2013. The retrospective nature of the data allows us to construct labor market histories for all individuals. Third, we link the data to individual records of all Dutch citizens kept by Statistics Netherlands. This contains individual-level information on criminal activities and health expenditures. In addition, it ensures access to labor market information (at a monthly level) up to December 2014, allowing us to look at longer term outcomes. The data from both Statistics Netherlands and from the national social insurance administration cover the full population of the Netherlands, such that the experiment sample is matched without attrition.

B. *Sample*

Based on inflow in previous years we expected 2,500 individuals to participate in the experiment. Our initial power calculation was based on this inflow number. Our final sample consists of 2,860 welfare applications (2,709 unique individuals).[19]

Thirty-eight welfare applicants have an incorrect personal identifier, such that we cannot match them to their outcomes. For eight applicants information on their

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[18] The other benefit schemes include unemployment insurance and disability insurance. We also observe welfare benefits receipt in another municipality. Data on income from self-employment are missing.

[19] An individual can have multiple applications if he/she applies for benefits multiple times within the experimental period. The average number of days elapsed between consecutive applications is 112.

TABLE 1—CHARACTERISTICS OF APPLICANTS UNDER DIFFERENT DEFAULT OPTIONS

| | | | | | *p*-value difference | | |
| | Full | Default option | | | Normal versus always | Normal versus never | Always versus never |
	Sample (1)	Normal (2)	Always (3)	Never (4)	(5)	(6)	(7)
Female	39%	39%	41%	36%	0.29	0.66	0.04
Partner	11%	12%	9%	8%	0.83	0.73	0.58
Children	14%	14%	15%	13%	0.55	0.66	0.83
Age under 30	25%	24%	25%	27%	0.56	0.12	0.36
Age 31–36	25%	23%	28%	27%	0.09	0.20	0.76
Age 37–45	26%	27%	24%	24%	0.19	0.24	0.94
Age above 45	24%	25%	23%	22%	0.35	0.10	0.57
Bachelor/master	28%	27%	28%	31%	0.39	0.65	0.46
Vocational	23%	24%	24%	21%	0.67	0.57	0.65
High school	13%	11%	16%	13%	0.04	0.82	0.02
Prep. vocational	20%	21%	17%	20%	0.02	0.46	0.18
Primary education or less	14%	14%	13%	14%	0.49	0.22	0.48
Education missing	2%	3%	2%	1%	0.22	0.34	0.70
Annual income in previous 2 years (×1,000 €)	13.6	13.5	13.4	14.2	0.67	0.95	0.39
p-value joint significance					0.14	0.52	0.32
Treatment							
Job search period applied	40%	46%	55%	9%	0.00	0.00	0.00
Observations	2,788	1,657	571	560			

Note: The *p*-values in the last three columns come from regressing each background characteristic on an indicator for getting a job search period, controlling for welfare office × three-month period.

caseworker is missing, so we cannot determine under which default option they were treated. Furthermore, for 24 applicants, information on the job search period is incomplete, and for seven applicants, information on essential controls is missing (gender, age, or household situation). In total, we exclude 72 observations from the analysis, which leaves us with 2,788 observations (2,640 unique individuals). For 64 of the 72 excluded observations, we have information on the default option. The excluded observations are evenly distributed over the three default options (joint *p*-value is 0.70).

The first column of Table 1 provides information about background characteristics. The majority of the applicants (over 60 percent) are male. The average age in the sample is 38.4 years, and the average annual income in the 2 years before the welfare application is approximately 13,600 euros. Couples are less likely to qualify for welfare, as the income of the partner is taken into account in the means test. In our sample, only 11 percent of the applicants have a partner, and 14 percent have children. Finally, 28 percent of the applicants have at least a bachelor's degree.

C. *Random Assignment*

Our design hinges on the fact that within local welfare offices applicants are randomly allocated to caseworkers, and are, therefore, also randomly assigned to default options. The second to fourth columns of Table 1 show the mean characteristics of applicants under the default options "normal", "always", and "never." Columns

5 to 7 show the p-value of the difference between two groups, for all different combinations. The characteristics are well balanced over the three treatment groups. There are no systematic differences and for only 6 out of 42 reported characteristics, the difference is significantly different from 0. The lower panel in Table 1 shows the treatment probability and the number of observations per treatment group. The treatment group with the default "normal" is the largest, as this was agreed upon with the welfare agency in the research design.

III. Empirical Strategy and Graphical Evidence

A. *Empirical Strategy*

To estimate the effect of a job search period on welfare receipt and other income variables, we assume a linear relationship. The outcomes of individual i observed t time periods after registering for welfare benefits at time τ at welfare office w are denoted by $Y_{i\tau tw}$, and $SP_{i\tau w}$ is an indicator for a job search period. Our regression model is specified as

$$(1) \qquad Y_{i\tau tw} = \alpha_{\tau tw} + \delta_t SP_{i\tau w} + X_i \beta_t + u_{i\tau tw}.$$

We estimate this model separately for different elapsed durations t since applying for welfare benefits. The vector X_i contains a set of covariates including age, gender, partner status, an indicator for children, cumulative income in the 24 months before registration, and dummies for 5 education categories. Each quarter we randomize treatments within local offices. Therefore, the regression specification includes $\alpha_{\tau tw}$, which are fixed effects for the quarter of registration interacted with the local welfare office.[20] The parameters of interest are δ_t, which describe the effect of a job search period t weeks after registration. Standard errors are clustered at the level of caseworker times the quarter of registration.

Job search periods are usually not applied randomly. Under the default option "normal" caseworkers target job search periods more to younger workers and those without children (see Table A3 in the Appendix). If caseworkers also take unobserved applicant characteristics into account when imposing a job search period, the OLS estimator of δ_t is biased. We exploit our experimental design using two strategies. First, we replace SP_i by the default option of the caseworker that conducted the intake meeting of individual i:

$$(2) \qquad Y_{i\tau tw} = \alpha_{\tau tw} + \delta_{1,t} Normal_i + \delta_{2,t} Always_i + X_i \beta_t + u_{i\tau tw}.$$

Because compliance is not perfect, δ_1 and δ_2 are the intention-to-treat effects (ITT). The advantage of the ITT parameters is that they reflect the change in outcomes if the welfare agency moves from abandoning job search periods to the current policy

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[20] Since within local offices applicants are randomly assigned to caseworkers, including local office fixed effects and quarter fixed effects without interaction would be sufficient. Indeed, estimation results in such a specification are very similar.

(δ_1), or to a stricter "always" policy (δ_2). However, since the parameters average over all applicants (including those that did not receive a job search period), they do not reflect the effect of actually imposing a job search period. Therefore, we employ a second strategy, where we instrument SP_i with the default option of the caseworker that conducted the intake. We estimate a first-stage equation of the form:

$$(3) \qquad SP_{i\tau w} \;=\; \kappa_{\tau w} + \lambda_1 Normal_i + \lambda_2 Always_i + X_i\theta + v_{i\tau w}.$$

In equation (3), λ_1 and λ_2 reflect the difference in the probability to receive a job search period for caseworkers with the default options "normal" and "always," compared to the default option "never". We saw before that caseworkers with the default option "normal" ("always") give 37 percentage points (46 percentage points) more job search periods than caseworkers with the default option "never".

Three key assumptions underlie our empirical strategy. First, within local offices, the assignment of applicants to default options is unrelated to unobserved characteristics of the applicant. This assumption is guaranteed by the random assignment of applicants to caseworkers in each local office and the random assignment of default options to caseworkers in each local office in each quarter. Controlling for local office fixed effects and quarter fixed effects is thus crucial. The balancing table presented earlier confirms the conditional random assignment.

Second, the probability that an individual finds employment (with or without a job search period) should not be related to whether other individuals receive a job search period (stable unit treatment value assumption (SUTVA)). Our design increased the probability for some applicants to receive a job search period, but decreased it for others, so approximately the same number of job search periods were given as before the experiment started. The treated population in the experiment is only a small fraction of the total population of unemployed workers in Amsterdam, which consisted of around 42,000 individuals in 2013. It is unlikely that the applied job search periods in our population have substantial spillover or general equilibrium effects.

These first two assumptions are required for interpreting the intention-to-treat effects. To interpret the instrumental variables estimates, our third assumption is monotonicity. No individual would have received a job search period from a caseworker with default option "never" and would not have received a job search period from a caseworker with default option "always." Under the monotonicity assumption, the instrumental variables estimates are Local Average Treatment Effects (LATE) (Angrist, Imbens, and Rubin 1996). The effect of a job search period is only identified for the group of applicants for which the caseworkers complied to their default options. Monotonicity likely holds for the same caseworker, but within a local welfare office some caseworkers have a higher job search period rate under "never" than (other caseworkers) under "normal" or "always." This may be explained by some caseworkers having only very few applicants under a default option. In Section VI, we elaborate further on the definition of compliers and the interpretation of our estimated effect of a job search period (also when monotonicity does not hold).

B. *Graphical Evidence*
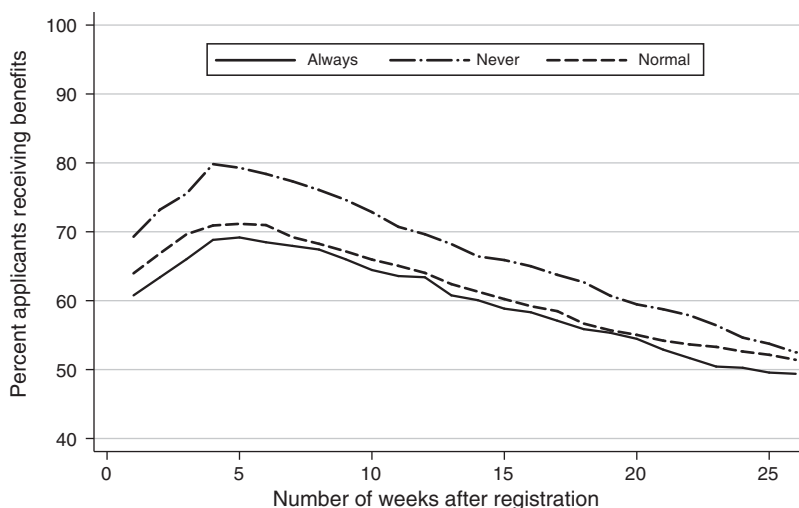
Figure 3 presents the fraction of applicants receiving welfare benefits by default option. Individuals are counted as receiving benefits in a certain week if payments of benefits were made that are assigned to that week. If an applicant returns from a job search period and receives benefits retrospectively from the moment of initial registration, this person is counted as a benefits recipient from registration onward. Take-up of welfare benefits is less than 100 percent for all three default options because eligibility for welfare benefits is only determined if the application for benefits is activated.[21]

Under the default option "never," the fraction of people receiving welfare benefits is higher than under the default "always."[22] Average benefits receipt under the default option "normal" lies in between, but is closer to the default option "always." This suggests that a job search period has a substantial effect on the probability to receive welfare benefits. Over time the differences between the three default options decrease, but after 26 weeks, applicants under the default option "never" are still more likely to receive benefits.

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[21] Conditioning on welfare benefits entitlement leads to possible confounding effects, as welfare benefits entitlement is not determined for individuals who do not reapply after a job search period.

[22] During the first five weeks the fraction of benefits recipients increases for all three default options. The increase is due to individuals that register at the welfare office before the date of exhaustion of UI benefits, leading to a small delay in the first day of eligibility for welfare benefits.

Panel A. Intention-to-treat

Panel B. Instrumental variables (IV)
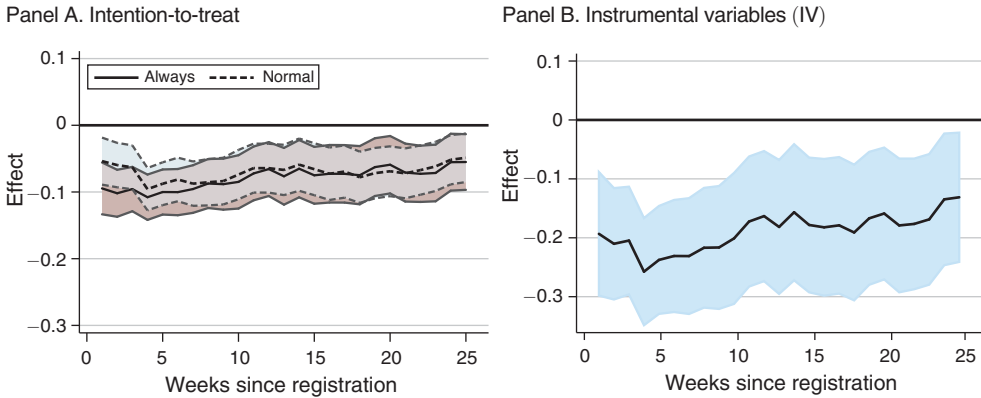
FIGURE 4. PROBABILITY TO RECEIVE WELFARE BENEFITS (*colored areas are 90 percent confidence intervals*)

IV. Results

A. *Welfare Benefits*

Figure 4, panel A plots the point estimates and 90 percent confidence intervals of the intention-to-treat effect on the probability to receive welfare benefits $(0/1)$ estimated separately for each week after registration (following equation (2)). Compared to the default option "never," the default "always" reduces the probability to collect some welfare benefits by 10 percentage points, while this is 6 percent for the default "normal." The effects decrease over time, but remain significant even later than 20 weeks after registration. Table 2 reports the effects of the default options on cumulative outcomes half a year after registration. The first two columns only include controls for calendar time and welfare office, the third and fourth column also control for applicant characteristics. As a consequence of the randomization, the estimates hardly change when including additional control variables. Applicants in the default group "normal" ("always") receive benefits, on average, 1.80 (2.02) weeks shorter than in the default group "never." This is not a mechanical effect of the job search period, as entitlement to benefits starts at the date of initial registration.

We next estimate the effect using our instrumental variables approach. The first-stage estimates of the default options on the probability to impose a job search period are, respectively, 0.35 (s.e. 0.02) for the default "normal" and 0.44 (s.e. 0.03) for the default "always." The *F*-statistic for joint significance of the instrumental variables is equal to 237. Figure 4, panel B plots the point estimates of the instrumental variables estimates of the probability to receive welfare. A job search period reduces welfare benefits receipt by around 20 percentage points in the first 10 weeks. In the group with the default option "never," total take-up of welfare benefits is around 80 percent, so this implies a reduction of about 25 percent. The effect decreases to 11 percentage points in week 26, but the effect remains significantly different from 0. The first row of Table 2 shows that a job search period reduces the period of receiving welfare benefits with 4.8 weeks.

A binary variable for receiving welfare benefits does not capture that individuals can receive lower benefits when having part-time work. Figure 5, panel A shows

TABLE 2—EFFECT OF JOB SEARCH PERIOD ON CUMULATIVE OUTCOMES 26 WEEKS AFTER REGISTRATION

	Intention-to-treat		Intention-to-treat		IV	
	Always	Normal	Always	Normal		
	(1)	(2)	(3)	(4)	(5)	(6)
Weeks on welfare	−1.89	−1.70	−2.02	−1.80	−4.60	−4.83
	(0.73)	(0.66)	(0.72)	(0.66)	(1.54)	(1.52)
Benefits received (in €)	−348	−272	−367	−279	−801	−825
	(159)	(141)	(158)	(141)	(340)	(334)
Earnings (in €)	341	244	392	303	760	887
	(219)	(203)	(223)	(199)	(473)	(469)
Other benefits (in €)	−39	−56	−34	−69	−117	−123
	(106)	(91)	(103)	(90)	(233)	(225)
Total income (in €)	−46	−84	−8	−45	−157	−61
	(196)	(192)	(198)	(182)	(436)	(423)
Weeks with earnings above benefits level	0.69	0.33	0.78	0.44	1.35	1.59
	(0.45)	(0.39)	(0.47)	(0.39)	(0.93)	(0.95)
Hours worked	25	15	28	19	52	61
	(15)	(14)	(16)	(14)	(32)	(32)
Hourly wage (in €) (conditional on work)	0.69	0.31	0.69	0.29	1.40	1.36
	(0.60)	(0.43)	(0.58)	(0.43)	(1.21)	(1.16)
First-stage coefficient default normal					0.34	0.35
					(0.02)	(0.02)
First-stage coefficient default always					0.43	0.44
					(0.03)	(0.03)
Observations	2,788		2,788		2,788	2,788
Included controls:						
Appl. characteristics	No		Yes		No	Yes
Local office × Calendar time fixed effects	Yes		Yes		Yes	Yes

Notes: Columns 1 and 2 and columns 3 and 4 in each row represent one regression; each cell in columns 5 and 6 represents a separate regression. The applicant characteristics are age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories. Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

the effect on the amount of welfare benefits received by an applicant. The pattern is very similar to Figure 4, panel B. A job search period has a strong effect on welfare receipt that is long lasting and only becomes insignificant after 24 weeks. Table 2 shows that total welfare benefits payments in the first 6 months decrease with 825 euros, which amounts to a 25 percent reduction of the mean cumulative amount of welfare during the first 26 weeks.

B. *Earnings, Total Income, and Other Benefits*

Not receiving welfare benefits does not necessarily imply employment. A job search period can discourage individuals to apply for welfare because of increased complexity and higher application costs. Figure 5, panel B shows the effect of the job search period on weekly income from employment. A job search period has a positive effect on weekly earnings of about 30 euros that becomes significant after 5 weeks. After 14 weeks, the effect steadily increases to 50 euros. Table 2 shows that

Panel A. Welfare benefits

Panel B. Earnings

Panel C. Other benefits

Panel D. Total income
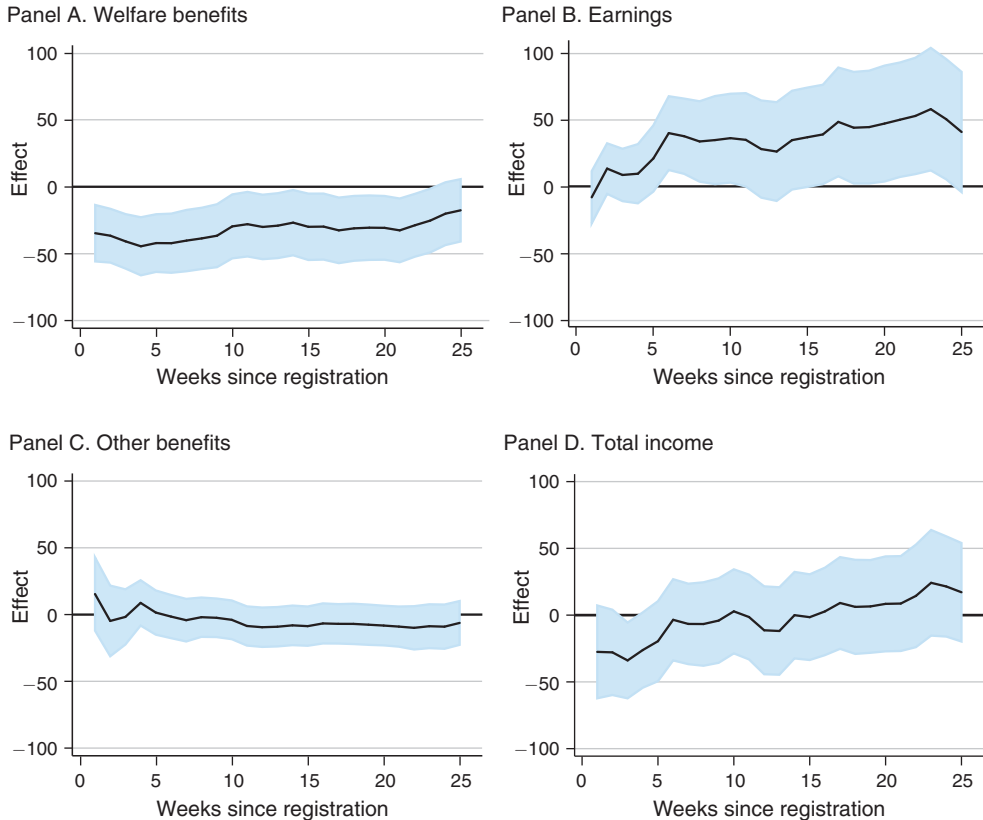
FIGURE 5. INSTRUMENTAL VARIABLE ESTIMATES OF THE EFFECTS OF A JOB SEARCH PERIOD ON LABOR MARKET OUTCOMES $t - \tau$ WEEKS AFTER REGISTRATION (*colored areas are 90 percent confidence intervals*)

during the first half year after registration a job search period induces individuals to earn, on average, 887 euros more. These additional earnings completely compensate (108 percent) the forgone welfare benefits of a job search period.[23]

Welfare is a safety net and applicants should not be entitled to other types of benefits. Figure 5, panel C shows that the estimated effect of a job search period on income from other benefit schemes (including welfare in other municipalities) is small and insignificant. The effect of a job search period on total income (the sum of income from welfare, wage, and other benefits) is shown in Figure 5, panel D. During the first four weeks the effect on total income is negative and (almost) significant. The negative effect during the first four weeks can be caused by individuals that find employment during the job search period, and, therefore, never collect any welfare benefits. After that, the effect is close to zero and insignificant. The effect on cumulative total income is small and not significantly different from zero. This

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[23] Our experiment took place during an economic downturn (see Figure B1 in the Appendix). During the second half of the experimental period, there was a sharp increase in the inflow into welfare benefits. The estimated effects of a job search period are the same for applicants in both time periods. Within this limited time frame, we, therefore, do not find evidence that effects vary with labor market conditions.

Table 3—Effect of a Job Search Period on Income Distribution at Different Weeks after Registration

	Weeks since registration			
	8 weeks (1)	16 weeks (2)	24 weeks (3)	32 weeks (4)
Income < 80% of benefits level	0.10 (0.05)	0.04 (0.06)	−0.00 (0.06)	0.05 (0.06)
Income < benefits level	−0.00 (0.05)	−0.00 (0.06)	−0.05 (0.06)	0.04 (0.06)
Income > minimum wage	0.01 (0.04)	0.02 (0.05)	0.05 (0.05)	0.01 (0.05)
Income > 125% of minimum wage	0.00 (0.04)	0.02 (0.04)	0.07 (0.05)	0.03 (0.05)
Observations	2,788	2,788	2,788	2,788

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level.

concurs with Markussen and Røed (2016), who find increased job finding of an activation program for welfare recipients without increased income.

The increased earnings can be driven by a higher likelihood to be employed, more working hours, and/or a higher hourly wage. Table 2 shows that after a job search period an individual, on average, experiences 1.59 additional weeks with earnings above the benefits level, and total working hours increase with, on average, 61 hours. These outcomes signal that a job search period mainly increases the likelihood to find a full-time job. Conditional on being employed, a job search period increases the hourly wage; the effect is quite substantial but insignificant.[24] Our estimates rule out that liquidity-constrained individuals have to accept lower quality jobs quickly. A positive wage effect arises if there is a negative stigma associated with actually receiving welfare benefits or if the welfare agency forces individuals to accept low-wage jobs.

The absence of an average effect on total income does not rule out that some individuals may suffer financially from a job search period. Table 3 shows the effects of a job search period on different parts of the total income distribution for 8, 16, 24, and 32 weeks since registration.[25] The estimates only show a significant effect 8 weeks after registration on the likelihood to have a very low income (below 80 percent of the benefits level). These results show that job search periods do not cause persistent income loss.

[24] Conditioning on having work can have compositional effects. If a job search period increases employment, the share of disadvantaged individuals that find work could increase. The estimated wage effect is then an underestimate.

[25] In a previous version of the paper, we estimated the marginal distribution of the outcome under different treatments for the subpopulation of compliers, following Imbens and Rubin (1997). See Bolhaar, Ketel, and van der Klaauw (2016) for details.

TABLE 4—EFFECT OF A JOB SEARCH PERIOD ON CUMULATIVE LONG-RUN OUTCOMES

	Weeks since registration		
	52 weeks	78 weeks	91 weeks
	(1)	(2)	(3)
Weeks on welfare	−4.14	−5.43	−6.38
	(2.69)	(3.74)	(4.32)
Benefits received (in €)	−956	−1219	−1,475
	(656)	(927)	(1,069)
Earnings (in €)	839	1214	1,424
	(1,027)	(1,666)	(2,027)
Other benefits (in €)	−415	−681	−558
	(361)	(493)	(559)
Total income (in €)	−607	−829	−792
	(813)	(1,353)	(1,658)
Weeks with earnings above benefits level	0.73	1.09	1.15
	(0.63)	(0.99)	(1.17)
Hourly wage (conditional on work)	1.84	NA	NA
	(0.81)		
Observations	2,788	2,788	2,788

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level. All regressions are based on information from Statistics Netherlands except for hourly wage, which is based on information from the social insurance administration. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 52 weeks after registration.

C. *Long-Run Effects*

The effect of a job search period on benefit receipt becomes insignificant 28 weeks after registration. Long-term effects can arise if the type of job is affected by the job search period. To investigate such effects we link our data to individual records of all Dutch citizens kept by Statistics Netherlands. The records are less detailed than the data used above, but they follow individuals until December 2014 (at least 91 weeks after registration).[26]

Table 4 presents the instrumental variable estimates for 52, 78, and 91 weeks after registration. The long-term effects are estimated less precisely than the effects after 26 weeks, but, if anything, the effect of the job search period increases rather than decreases over time. The reductions in welfare benefits payments are, therefore, permanent savings, that are not offset by a later increase in benefit dependency. One year after registration the hourly wage is 21 percent higher if a job search period is applied. Again, no empirical evidence is found that a job search period induces applicants to accept lower quality jobs.

[26] Statistics Netherlands provides monthly earnings information instead of weekly and the data do not contain working hours. Furthermore, the classification of benefits types is less detailed.

V. Heterogeneous Treatment Effects

A. *Degree of Vulnerability*

A job search period has, on average, positive effects on labor market outcomes, but there can be subgroups of the population suffering from a job search period. To capture such heterogeneity in the effect, we create a vulnerability index to rank applicants. We sum the presence of eight risk factors: no wage income in the two years before registration, positive income from welfare benefits in the two years before registration, older than 45 years, financial problems, children, low education, low self-reliance, and no favorable expectation of caseworker on job finding. We divide the sample in four groups with zero, one, two, or more than two risk factors present.[27]

Table 5 shows that the effect of a job search period is largest for the least vulnerable applicants and declines monotonically with the degree of vulnerability.[28] The point estimate of -11.02 implies a 86 percent decrease in the number of weeks on welfare for the least vulnerable applicants. For the two most vulnerable groups (columns 3 and 4), the effects are much smaller and not significant. There is no significant effect on total income for any of the groups. The lowest panel of Table 6 reports the first stages. For all groups the default option "normal" substantially increases the rate at which job search periods are applied with only limited variation between the groups. Under the default option "always," the rate of applying job search periods to the least vulnerable group only increases to 0.65. The heterogeneity of treatment effects is in agreement with the policy of the welfare agency to avoid applying job search periods to the most vulnerable applicants. We should also conclude that caseworkers are not targeting job search periods very effectively.

Above we reported that a job search period increases the likelihood to have very low income in the first few weeks after registration. This negative effect is present only for the least vulnerable group (see Table A6 in the Appendix). Again this effect is only short-lived, and 24 weeks after registration the least vulnerable group has an increased probability to earn more than 125 percent of the minimum wage.[29]

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[27] Self-reliance and the caseworker's expectation on job finding are taken from the forms that the caseworkers filled in (for 72 percent of the observations). Presence of the form is neither related to the default option nor to any of the outcome variables.

[28] Table A4 in the Appendix is based on an alternative vulnerability index that ranks individuals based on the first component of a Principal Component Analysis using a rich set of pre-experiment covariates. This approach gives very similar results. Table A5 reports effects by level of education, with results very similar to the results by level of vulnerability. In none of the approaches do we find evidence for negative effects of a job search period on a specific group.

[29] We test for heterogeneity of the effect of a job search period by duration of the search period and previous welfare receipt. A job search period can be at most four weeks, but caseworkers have some discretion in determining the duration. We do not find significantly different effects by duration of the job search period. The effect of a job search period is larger for first-time applicants, but it is not absent for returning applicants, implying that a job search period does not only deter first-time applicants.

TABLE 5—EFFECT OF JOB SEARCH PERIOD 26 WEEKS AFTER REGISTRATION, BY DEGREE OF VULNERABILITY

	Degree of vulnerability			
	Least vulnerable (1)	Below average (2)	Above average (3)	Most vulnerable (4)
Weeks on welfare	−11.02 (2.20)	−6.57 (2.24)	−2.43 (2.63)	0.46 (2.81)
Benefits received (in €)	−1,946 (468)	−1,277 (544)	−340 (626)	207 (687)
Earnings (in €)	2,652 (1,059)	993 (948)	256 (993)	75 (634)
Other benefits (in €)	279 (408)	−105 (348)	171 (476)	−471 (456)
Total income (in €)	985 (992)	−389 (918)	87 (930)	−188 (755)
Weeks with earnings above benefits level	4.60 (1.96)	2.39 (1.85)	0.69 (1.96)	−0.02 (1.35)
Hours worked	125 (68)	85 (63)	53 (74)	8 (48)
Hourly wage (in €) (conditional on work)	2.23 (2.26)	0.06 (1.81)	2.58 (4.30)	1.82 (1.90)
First-stage coefficient default normal	0.44 (0.06)	0.31 (0.05)	0.31 (0.04)	0.31 (0.04)
First-stage coefficient default always	0.52 (0.07)	0.47 (0.06)	0.40 (0.05)	0.37 (0.05)
Percent of job search periods under default never	0.13	0.14	0.07	0.05
Observations	462	680	679	967

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

B. *Crime and Health*

Even though we do not find negative effects on labor market outcomes, a job search period causes uncertainty and maybe (financial) stress. Health problems and crime can be the result of such stress. Statistics Netherlands provides administrative data on suspects of crime[30] and annual health-care costs covered by the (mandatory) basic health insurance. Crime and health are measured at a yearly basis, so we aggregate outcomes for the years 2012, 2013, and 2014.[31]

Crime and health outcomes need not necessarily worsen due to a job search period. A job search period increases job finding and work is usually associated with reduced crime and better health outcomes. One may expect that job finding

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[30] In the Netherlands, on average, 90 percent of the registered suspects are declared guilty (CBS, WODC, and Raad voor de Rechtspraak 2014)

[31] This means that for some individuals the crime/health expense can take place before the job search period. Given the randomized design, there is no reason to suspect a difference in crime and health outcomes before the start of the experiment. We repeat the analysis by period of registration, and only 2013 outcomes, which gives very similar results.

Table 6—Effect of a Job Search Period on Crime and Health by Degree of Vulnerability

	Degree of vulnerability			
	Least vulnerable (1)	Below average (2)	Above average (3)	Most vulnerable (4)
Total crime	0.07 (0.11)	0.14 (0.09)	0.03 (0.12)	−0.20 (0.13)
Property crime	0.08 (0.07)	−0.11 (0.06)	0.02 (0.07)	−0.06 (0.06)
Total health expenditures	−1,275 (1,730)	−2,362 (1,700)	−783 (2,061)	1,877 (1,756)
GP expenditures	−76 (44)	−36 (36)	31 (42)	−7 (36)
Physical health expenditures	−1075 (1,286)	449 (685)	−142 (963)	299 (1,478)
Mental health care expenditures	−224 (1,110)	−2,813 (1,214)	828 (1,224)	915 (954)
Observations	444	684	707	953

Notes: Each cell represents one equation. All regressions include local office $\times$ calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). The regressions on crime include a dummy for being suspect of a crime from 2008–2011 and the regressions for health include health expenditures in the relevant category for the years 2009–2011. Standard errors are clustered at the three-month period $\times$ caseworker level.

dominates for the least vulnerable groups and stress for the most vulnerable groups. Table 6 reports the estimated effects for the different groups.[32] We do not find any support for detrimental effects of a job search period on health or crime for the most vulnerable applicants. We also do not find very strong evidence that the increased job finding improves health and reduces crime. Only for the second group there is a negative effect on property crime and reduced mental health care expenditures.

A job search period is a bundle of treatments. First, it delays benefits payments, which may cause liquidity constraints and financial stress. Our results do not indicate that the delayed benefits payments have harmful effects. Labor market, health, and crime outcomes of the most vulnerable individuals remain unaffected, while it is not the case that the least vulnerable individuals have accepted very low-wage jobs. Second, applicants have to make many job applications, but can decide themselves where to apply. The most vulnerable individuals likely apply to the same (low-skilled) jobs during the job search period as when they are collecting benefits. For this group we do not find increased job finding, which indicates that for them the increased job search is not effective. For the least vulnerable workers, we find increased job finding and wages seem somewhat higher. This suggests that discretion in job applications is for the least vulnerable individuals an important element in the job search period.

[32] The health and crime outcomes for the full sample are reported in Table A7 in the Appendix.

VI. Interpretation

The estimated effect of a job search period should be interpreted as a local average treatment effect. Caseworkers decide about imposing a job search period and that decision depends on the randomly assigned default options. In this section, we provide some interpretation of the estimated local average treatment effect and test if the effect changes when we consider other groups of compliers. The latter is informative about how well caseworkers target job search periods.

Suppose it is possible to rank applicants according to an (unobserved) index that we refer to as the propensity *not* to receive a job search period. Applicants that are always given a job search period have propensity zero and applicants with a propensity of one never receive a job search period. If we assume that there is no heterogeneity among caseworkers when executing the three default options, the default options can be translated into thresholds. Under the default option "never," job search periods are only assigned to applicants with a propensity less than 0.09 and these applicants are the always takers. Under the default option "always," all applicants with a propensity less than 0.55 are assigned a job search period. Therefore, applicants with a propensity above 0.55 are the never takers and those with a propensity between 0.09 and 0.55 are compliers. The default option "normal" splits the compliers in two groups. First are applicants with a propensity between 0.09 and 0.46, who comply to both the default option "normal" and "always." And second are applicants with a propensity between 0.46 and 0.55, who only comply to the default option "always." Overidentification tests in the Appendix do not provide evidence that treatment effects differ between compliers to the default options "normal" and "always" (see Table A8). The power of this approach is not very high since the rates at which job search periods are imposed under the default option "normal" and "always" do not differ very much.

We next consider the individual compliance of each caseworker. Figure 6, panel A shows for each caseworker the rate of job search periods imposed under the default options "normal" and "never." Each circle represents a caseworker, and the size of the circle describes the number of applicants a caseworker has under the default option "normal."[33] There is substantial variation in the rate at which caseworkers assign job search periods under the default option "normal." Most caseworkers who normally assign many job search periods, substantially reduce this if they are assigned the default option "never." However, there are a few caseworkers who do not change behavior under the default option "never," and impose job search periods as often as under the default option "normal".[34]

Figure 6, panel B shows the same figure, but now comparing the default option "always" with the default option "normal." The figure is less pronounced than the previous figure. Obviously, many caseworkers find it difficult to impose job search

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[33] There are many caseworkers with only few applicants. Exploiting differences in rates at which caseworkers assign job search periods under the different default options (e.g., Maestas, Mullen, and Strand 2013) would incorporate too much noise.

[34] That compliance to a randomized treatment assignment differs between caseworkers is not uncommon. Sherman and Berk (1984) finds that about half of the police officers selected for participation in their field experiment never hand in cases.

Panel A. Normal versus never
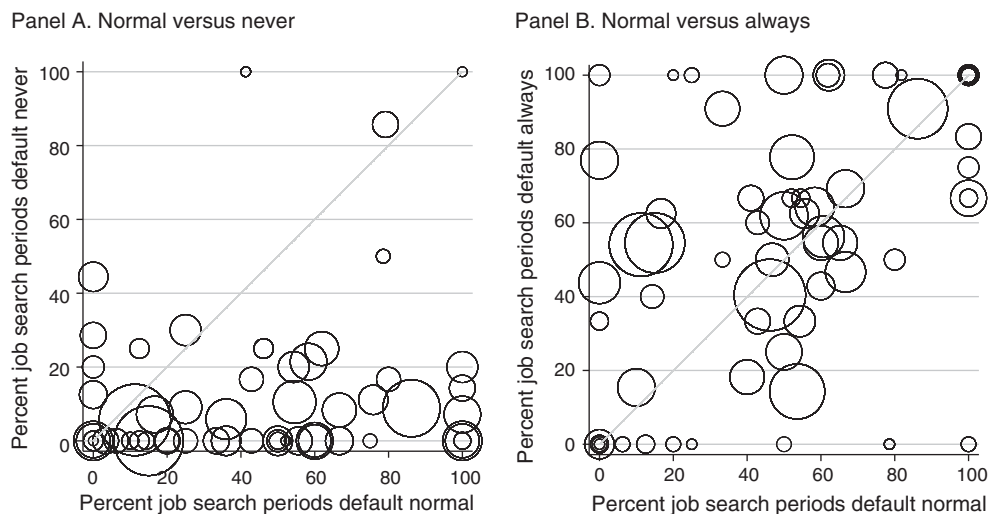
Panel B. Normal versus always

FIGURE 6. PERCENTAGE OF JOB SEARCH PERIODS APPLIED BY DEFAULT OPTION AND CASEWORKER

periods more often than usually. There are also some caseworkers who normally already impose job search periods so often that this can hardly be increased. Finally, there are some caseworkers who more or less refuse to give job search periods. Even under the default option "always" they (almost) never apply job search periods.[35]

Because within local offices applicants are randomly assigned to caseworkers, we can restrict our sample to caseworkers with substantial compliance rates without harming the randomization. In Table 7, we proceed in three steps and remove applicants of caseworkers who do not comply with the following rules:

(i) Fraction "never" < 20 percent and fraction "always" > 40 percent.

(ii) Fraction "never" < 10 percent and fraction "always" > 40 percent.

(iii) Fraction "never" < 10 percent and fraction "always" > 60 percent.

We only remove observations if a caseworker had more than five applicants in the relevant default options, to not run the risk of removing caseworkers that had a very peculiar draw of applicants. A regression of an indicator for the different samples that remain under these selection criteria shows that there are no observable differences between applicants in the different groups (see Table A9 in the Appendix). Table 7 presents the estimated effects of a job search period for the different subsamples. At the bottom of the table we see that by removing noncomplying caseworkers, the first-stage coefficients increase. In the most strictly defined sample (column 4)

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[35] In Figure 6, panels A and B, some caseworkers are actually defiers, applying more (less) job search periods under the default never (always) than under the default normal. Of the eight caseworkers that apply more job search periods under the default never than under normal, this difference is significant (at a 10 percent significance level) for one caseworker. Of the 23 caseworkers that apply less job search periods under always than under normal, the difference is significant for three caseworkers.

TABLE 7—OUTCOMES FOR DIFFERENT GROUPS OF COMPLIERS

	Total sample (1)	Never < 20% Always > 40% (2)	Never < 10% Always > 40% (3)	Never < 10% Always > 60% (4)
Weeks on welfare	−4.83 (1.52)	−4.24 (1.48)	−4.68 (1.55)	−5.22 (1.92)
Benefits received (in €)	−825 (334)	−666 (318)	−769 (336)	−754 (393)
Earnings (in €)	887 (469)	789 (449)	900 (469)	1498 (579)
Other benefits (in €)	−123 (225)	−106 (198)	−74 (211)	−142 (267)
Total income (in €)	−61 (423)	17 (404)	57 (401)	602 (484)
Weeks with earnings above benefits level	1.59 (0.95)	1.52 (0.92)	1.76 (0.96)	3.05 (1.17)
Hours worked	61 (32)	51 (31)	56 (33)	87 (42)
Hourly wage (in €) (conditional on work)	1.36 (1.16)	1.55 (1.16)	0.75 (0.97)	1.50 (1.16)
First-stage default normal	0.35 (0.03)	0.37 (0.03)	0.37 (0.04)	0.37 (0.04)
First-stage default always	0.44 (0.04)	0.51 (0.04)	0.53 (0.04)	0.59 (0.04)
Observations	2,788	2,373	2,207	1,603

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

the estimates now cover propensities from 0.06 to 0.71 percent. There is no evidence that the effect of a job search period decreases with the propensity not to apply a job search period. The effect on earnings even becomes slightly larger.[36]

The results in Table 7 also show that the local average treatment effects are not sensitive to changes in the sample and the group of compliers. Obviously, caseworkers do not succeed in targeting job search periods to those applicants for which the effects are largest. These findings justify the assumptions presented by de Chaisemartin (2017) to interpret instrumental variable estimates as causal effects in the presence of defiers.[37] The formal interpretation is that we estimate the average treatment effect for the surviving compliers. However, the robustness of the

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[36] This test is potentially not very powerful, as the sample that is excluded contributes little to the identification of the local average treatment effect.

[37] de Chaisemartin (2017) introduces the complier-defier assumption, which states that a group of compliers can be found which has the same size as the defiers and the same average treatment effect. He discusses how this assumption may hold in applications of judges and examiners. Our setting of caseworkers has strong similarities except that our experiment exogenously changes default options of caseworkers, which provides insight in which caseworkers are defiers. Our results are robust to excluding these caseworkers, which supports the complier-defier assumption and maybe even the stronger assumption that compliers and defiers have equal average treatment effects (e.g., Angrist, Imbens, and Rubin 1996).

estimated coefficients suggests that the estimated local average treatment is generalizable to a larger share of the sample.

VII. Conclusion

In this paper, we use a field experiment, in combination with detailed administrative data, to study mandatory job search periods for new applicants of welfare benefits. Our empirical results provide evidence for a strong and persistent effect of a job search period on receipt of welfare benefits. Six months after applying for welfare, the total benefits payments are reduced by, on average, 25 percent. The reduced welfare benefits are fully compensated (108 percent) by increased income from employment and there are no spillovers to other benefit schemes. A job search period is most effective for the least vulnerable applicants, for this group the likelihood to receive benefits decreases with 86 percent.

The job search period causes uncertainty concerning eligibility to welfare benefits and delays the first benefits payment by about 15 days. This may induce welfare benefits applicants to increase job search effort. The latter is also required by the job search requirements associated to the job search period. The ex post monitoring of compliance to the job search requirement is limited, but welfare benefits applicants are unaware of this. In addition, the job search period increases the complexity of the benefits application. Our results suggest that the increased complexity of the application process does not hurt the most vulnerable applicants. In particular, we do not find evidence of negative side effects, such as engagement in criminal behavior or an increase in health expenditures. Our preferred conclusion is that increased job finding is the main mechanism through which job search periods affect individual outcomes. There is no evidence that a job search period induces individuals to accept lower-wage jobs. Therefore, the job search period is an effective instrument for targeting welfare benefits to those people who need it most.

The job search requirement and the waiting period are easily transferable to other situations, which suggests that job search periods can be useful policy instruments for unemployment insurance and disability insurance. The administrative costs of imposing a job search period are small and it is an early intervention that can prevent more costly interventions later during the period of benefits dependency. However, the population in our experiment has two important features. First, they do not have serious limitations to work. Second, applicants have very limited financial resources, which is a condition for entitlement to welfare benefits. These aspects of the applicant population might be essential for a successful implementation of a job search period in other settings. Currently only a small share of all benefits recipients are exposed to a job search period at the same time. Expanding job search periods to other groups may reduce the effectiveness due to congestion effects in job search.

Randomized experiments with welfare applicants are still rare. We show that with an encouragement design, which allowed for opting out in special cases, it is possible to evaluate (existing) policies for welfare applicants. The opt-out possibility has been important to obtain support of caseworkers, which ensured

sufficient compliance to our randomization. Finally, the design of our experiment allowed us to also study whether caseworkers are able to target job search periods to those clients for whom they are most effective. We find that this is not the case; a job search period is effective for a larger share of the applicants than the population on which caseworkers normally impose a job search period. Our results suggest that the welfare administration should instruct the caseworkers to apply job search periods more frequently.

APPENDIX A: ADDITIONAL TABLES

TABLE A1—BENEFIT LEVELS (*net, in € per month*)

	Housing costs	
	Full	Shared
Single without children	935.80	802.12
Single with children	1,203.19	1,069.50
Couple without children	1,336.87	1,203.19
Couple with children	1,336.87	1,203.19

Notes: Benefit levels in period July 1 to December 31 in 2012, net including holiday allowance. Benefit levels outside this time frame differ only marginally. Shared housing costs apply if the costs are shared with an individual that is not the partner or child.

TABLE A2—CHARACTERISTICS OF APPLICANTS FOR WELFARE BENEFITS BY CLASS

	Class			
	Class 1	Class 2	Class 3	Class 4
Panel A. Personal characteristics				
Female	53%	46%	33%	40%
Age at application (in years)	39.6	38.6	36.6	35.6
Single	77%	74%	78%	82%
Single parent	8%	8%	9%	8%
Couple, no children	9%	8%	6%	5%
Couple, with children	7%	10%	7%	5%
Welfare in last 4 years	37%	41%	38%	29%
Panel B. Education				
Bachelor/master	13%	10%	11%	30%
Vocational	21%	18%	28%	32%
High school	9%	7%	9%	9%
Preparatory vocational	28%	26%	32%	19%
Primary or less	19%	27%	14%	8%
Education unknown	10%	12%	6%	3%
Panel C. Reason application for welfare				
Lost job	4%	4%	5%	13%
End of self-employment	4%	4%	4%	7%
End unemployment insurance benefits	6%	8%	18%	26%
End of other source of income	21%	23%	31%	41%
Split with partner	8%	6%	4%	1%
End of incarceration	2%	3%	2%	0%
End of study	3%	1%	2%	4%
Inflow for other reasons	59%	59%	52%	33%
Panel D. Barriers to entry on the labor market				
Physical problems	49%	30%	28%	25%
Psychological problems	51%	31%	20%	16%
Addiction	11%	13%	7%	3%
Language	17%	32%	18%	16%
Financial situation	31%	35%	44%	48%
Criminal history	11%	17%	20%	9%
Living situation	18%	27%	18%	10%
Observations	4,014	5,726	4,334	9,517

Notes: Statistics are based on applications for welfare benefits from January 1, 2012 until April 25, 2014. Information on barriers to entry in the labor market is only available for 14,534 of the observations.

TABLE A3—TARGETING JOB SEARCH PERIODS BY CASEWORKERS UNDER DEFAULT "NORMAL"

	Job search period		*p*-value differential
	No (1)	Yes (2)	(3)
Female	40%	37%	0.19
Partner	13%	11%	0.03
Children	16%	11%	0.00
Age under 30	20%	30%	0.00
Age 31–36	21%	26%	0.01
Age 37–45	30%	23%	0.00
Age above 45	29%	21%	0.00
Bachelor/master	26%	27%	0.08
Vocational	24%	24%	0.82
High school	13%	9%	0.01
Preparatory vocational	22%	20%	0.25
Primary education or less	13%	15%	0.84
Education missing	1%	5%	0.00
Annual income in previous two years ($\times 1{,}000$ €)	13.7	13.3	0.43
Observations	899	758	

Note: The *p*-values in the last three columns come from regressing each background characteristic on an indicator for getting a job search period, controlling for welfare office $\times$ three-month period.

TABLE A4—EFFECT OF JOB SEARCH PERIOD 26 WEEKS AFTER INITIAL REGISTRATION BY DEGREE OF VULNERABILITY USING PRINCIPAL COMPONENTS ANALYSIS

	Degree of vulnerability			
	Least vulnerable (1)	Below average (2)	Above average (3)	Most vulnerable (4)
Weeks on welfare	−9.38	−7.46	−1.97	2.18
	(2.74)	(2.11)	(2.03)	(3.23)
Benefits received (in €)	−1,950	−921	−833	528
	(572)	(505)	(450)	(764)
Earnings (in €)	3,003	796	574	−750
	(1,336)	(1,020)	(591)	(653)
Other benefits (in €)	−350	361	−469	−137
	(496)	(302)	(399)	(498)
Total income (in €)	704	236	−728	−360
	(1,222)	(937)	(609)	(809)
Weeks with earnings above benefits level	5.07	2.73	0.25	−1.88
	(2.47)	(1.81)	(1.33)	(1.43)
Hours worked	162	63	42	−23
	(81)	(60)	(48)	(52)
Hourly wage (in €) (conditional on work)	4.31	0.02	−0.09	−0.55
	(2.11)	(2.12)	(2.14)	(1.61)
First-stage coefficient default normal	0.30	0.40	0.29	0.34
	(0.06)	(0.05)	(0.05)	(0.05)
First-stage coefficient default always	0.35	0.49	0.49	0.39
	(0.07)	(0.06)	(0.05)	(0.06)
Percent of job search periods under default never	0.22	0.08	0.04	0.04
Observations	697	697	697	697

Notes: Each cell represents one equation. All regressions include local office $\times$ calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period $\times$ caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

TABLE A5—EFFECTS BY LEVEL OF EDUCATION 26 WEEKS AFTER REGISTRATION

	Education level		
	Low (1)	Middle (2)	High (3)
Weeks on welfare	−2.45 (2.52)	−4.31 (2.17)	−7.97 (2.26)
Benefits received (in €)	−264 (578)	−810 (499)	−1,486 (480)
Earnings (in €)	526 (720)	1,093 (690)	1032 (940)
Other benefits (in €)	−285 (375)	−39 (323)	−66 (355)
Total income (in €)	−23 (593)	244 (670)	−519 (895)
Weeks with earnings above benefits level	0.72 (1.52)	2.31 (1.43)	1.52 (1.63)
Hours worked	33 (58)	97 (47)	46 (58)
Hourly wage (in €) (conditional on work)	−0.66 (1.44)	1.72 (1.55)	2.92 (2.09)
Observations	1,011	1,007	770

Notes: Each row in this table represents one regression including interactions for the different subgroups. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, and cumulative income in 24 months before registration). Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

TABLE A6—EFFECT OF A JOB SEARCH PERIOD ON INCOME DISTRIBUTION AT DIFFERENT WEEKS AFTER
REGISTRATION BY DEGREE OF VULNERABILITY

	Degree of vulnerability			
	Least vulnerable (1)	Below average (2)	Above average (3)	Most vulnerable (4)
8 weeks after registration				
Income < 80% of benefits level	0.32 (0.09)	0.16 (0.11)	0.07 (0.11)	−0.13 (0.12)
Income < benefits level	0.09 (0.12)	0.04 (0.10)	−0.19 (0.13)	0.06 (0.10)
Income > minimum wage	0.04 (0.10)	0.11 (0.08)	0.04 (0.11)	−0.10 (0.08)
Income > 125% of minimum wage	0.06 (0.09)	0.05 (0.07)	0.05 (0.09)	−0.06 (0.06)
24 weeks after registration				
Income < 80% of benefits level	0.03 (0.12)	0.03 (0.11)	0.00 (0.13)	−0.12 (0.12)
Income < benefits level	−0.15 (0.13)	−0.04 (0.11)	−0.12 (0.14)	0.05 (0.10)
Income > minimum wage	0.29 (0.12)	0.16 (0.11)	−0.08 (0.13)	−0.07 (0.09)
Income > 125% of minimum wage	0.26 (0.11)	0.23 (0.10)	−0.05 (0.11)	−0.11 (0.07)
Observations	462	680	679	967

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

TABLE A7—EFFECT OF A JOB SEARCH PERIOD ON CRIME AND HEALTH OUTCOMES

	Included years of outcome variable	
	2012, 2013 (1)	2012, 2013, 2014 (2)
Total crime	0.00 (0.04)	−0.01 (0.05)
Property crime	−0.01 (0.02)	−0.03 (0.03)
Total health expenditures	−312 (796)	−366 (972)
GP expenditures	−17 (14)	26 (−18)
Physical health expenditures	167 (502)	175 (591)
Mental health care expenditures	−256 (419)	−344 (529)
Observations	2,788	2,788

Notes: Each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). The regressions on crime include a dummy for being suspect of a crime from 2008–2011 and the regressions for health include health expenditures in the relevant category for the years 2009–2011. Standard errors are clustered at the three-month period × caseworker level. All regressions are based on information from Statistics Netherlands.

TABLE A8—COMPARING THE TWO INSTRUMENTS, OUTCOMES 26 WEEKS AFTER REGISTRATION

	Total sample (1)	p-value over-id (2)	Normal versus never (3)	Always versus normal (4)
Weeks on welfare	−4.83 (1.52)	0.72	−5.50 (1.92)	−2.95 (8.02)
Benefits received (in €)	−825 (334)	0.89	−801 (420)	−1,099 (1,751)
Earnings (in €)	887 (469)	0.95	941 (587)	1,406 (2,596)
Other benefits (in €)	−123 (225)	0.59	−261 (271)	92 (1,019)
Total income (in €)	−61 (423)	0.82	−121 (554)	400 (2,309)
Weeks with earnings above benefits level	1.59 (0.95)	0.58	1.48 (1.12)	5.34 (5.92)
Hours worked	61 (32)	0.77	65 (40)	143 (196)
Hourly wage (in €) (conditional on work)	1.36 (1.16)	0.55	0.63 (1.27)	4.03 (4.43)
First-stage coefficient default normal	0.35 (0.02)		0.34 (0.02)	
First-stage coefficient default always	0.44 (0.03)			0.08 (0.03)
Observations	2,788		2,217	2,228

Notes: In columns 1, 3, and 4, each cell represents one equation. All regressions include local office × calendar time fixed effects and control for applicant characteristics (age at registration, gender, household composition, cumulative income in 24 months before registration, and dummies for 5 education categories). Standard errors are clustered at the three-month period × caseworker level. Hourly wage is not a cumulative outcome; it refers to the average hourly wage 26 weeks after registration.

TABLE A9—CHARACTERISTICS OF DIFFERENT GROUPS OF COMPLIERS

|	Never < 20% Always > 40% (1)	Never < 10% Always > 40% (2)	Never < 10% Always > 60% (3)
Female	−0.01 (0.01)	−0.01 (0.01)	−0.00 (0.02)
Partner	0.02 (0.02)	−0.01 (0.03)	−0.02 (0.04)
Children	0.02 (0.02)	0.02 (0.02)	0.01 (0.03)
Age 31–36	−0.02 (0.02)	−0.03 (0.02)	−0.01 (0.03)
Age 37–45	−0.01 (0.02)	−0.00 (0.02)	0.02 (0.03)
Age above 45	0.03 (0.02)	0.04 (0.02)	0.05 (0.02)
Bachelor/master	−0.04 (0.03)	−0.01 (0.03)	−0.01 (0.03)
Vocational	−0.03 (0.02)	−0.00 (0.03)	−0.01 (0.04)
High school	0.02 (0.03)	0.04 (0.03)	−0.01 (0.04)
Preparatory vocational	0.00 (0.02)	0.03 (0.03)	0.00 (0.03)
Education missing	0.02 (0.05)	−0.05 (0.08)	−0.05 (0.09)
Income 2 years before (× 1,000 €)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Location: Southeast	0.08 (0.06)	0.25 (0.07)	−0.32 (0.10)
Location: North	0.03 (0.07)	0.12 (0.09)	0.19 (0.09)
Location: Centrum/East	0.02 (0.06)	0.19 (0.07)	0.11 (0.08)
Location: New West	−0.01 (0.08)	0.17 (0.08)	0.02 (0.10)
Quarter 2	0.02 (0.07)	−0.00 (0.07)	−0.03 (0.09)
Quarter 3	−0.01 (0.07)	−0.02 (0.08)	−0.05 (0.09)
Quarter 4	−0.00 (0.07)	0.01 (0.07)	0.02 (0.09)

APPENDIX B: ADDITIONAL FIGURES
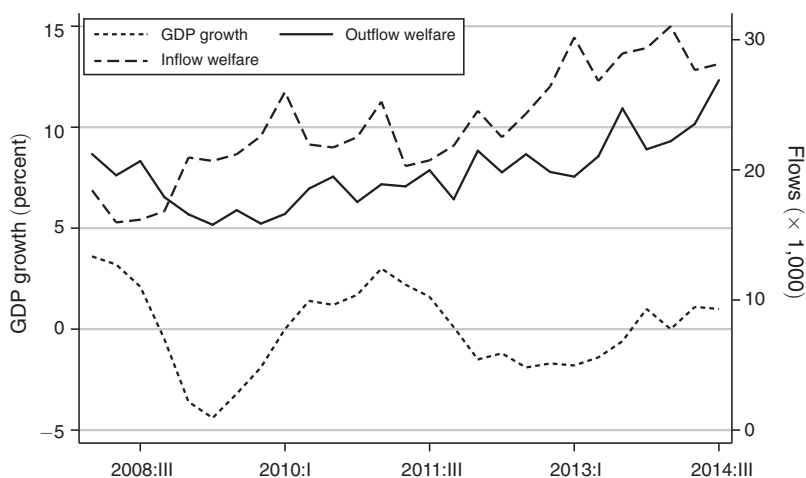
FIGURE B1. INFLOW AND OUTFLOW OF WELFARE BETWEEN 2008 AND 2014 AND MACROECONOMIC INDICATORS
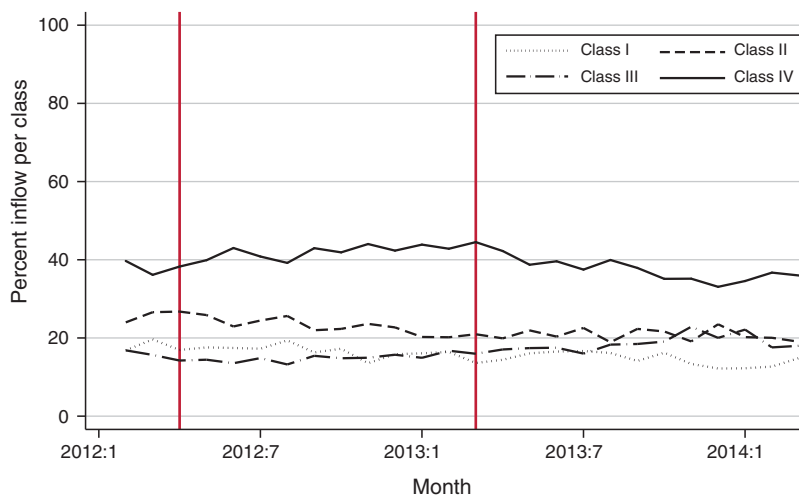
Source: Statistics Netherlands

FIGURE B2. INFLOW BY CLASS BETWEEN 2012 AND 2014, AS A PERCENTAGE OF TOTAL INFLOW

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