Job Search Periods for Welfare Applicants: Evidence from a Social Experiment

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Abstract

This paper investigates the effect of a mandatory job-search period for welfare applicants. During this four-week period the application is put on hold and the applicant is obliged to make job applications. We exploit a field experiment combined with detailed administrative data to investigate the effects of imposing this job-search period. We find strong and persistent effects on the probability to collect welfare benefits. In the six months after applying for welfare, total benefits payments are reduced by, on average, 25 percent. We do not find any spillover to other benefits schemes, and the reduced welfare benefits are fully (112 percent) compensated by increased earnings from work. The heterogeneous treatment effects do not show evidence for adverse consequences of a job-search period for the most vulnerable applicants. Our results therefore suggest that a search period is an effective method for targeting welfare benefits applicants.

JEL-codes: C21, C93, I38, J64, J08

Keywords: job search, welfare-to-work, active labor market policies, randomized experiment

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1 Introduction

Many countries have some welfare system which provides benefits to low-income families. Welfare then acts as a safety net guaranteeing a minimum level of income to households. In the United States, welfare is mostly used to support single-parents households. In European countries, it also supports long-term unemployed workers, who are no longer entitled to social-insurance benefits. Whereas welfare aims at households with a low capability of generating sufficient income, governments often have imperfect information about the income-generating potential of a given household. This may induce moral hazard, which causes excess spendings on welfare benefits. In most countries, there is a tendency among policy makers to restrict access to benefit schemes and to be more strict on job-search requirements. Causal evidence on the effect on take up of more stringent entry requirements is, however, scarce (Currie, 2006).

This paper evaluates mandatory job-search periods for welfare applicants. During such a four-week period the application is put on hold, and applicants are supposed to very actively search for work. The application for welfare benefits will only be activated if the applicant returns to the agency after the job-search period. Benefits will then be paid from the moment of initial application. Therefore, the mandatory job-search period does not change eligibility or the amount of benefits, but only postpones the first payment. To evaluate such job-search periods we ran a field experiment between April 2012 and March 2013, incorporating the full population of welfare applicants with a potential to work in the city of Amsterdam. Combining various sources we construct a very detailed administrative dataset describing the participants in the experiment. This allows us to not only look at the implications of a job-search period on welfare benefits receipt, but also study the effect on alternative sources of income. In addition, we can establish whether a job-search period increases the likelihood to engage in criminal activities.

A search period can affect labor-market outcomes in several ways. First, the job-search requirement can increase the likelihood to find a job and thereby reduce the likelihood to receive welfare benefits. The obligation to very actively search for a job and and the message that all work is considered suitable, may intensify the job-search efforts of the applicant or lower his standard of what is considered an acceptable job. Second, a search period makes

the application process for welfare benefits more complex and increases the costs of applying. After the 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 an effect on job finding. Both mechanisms can serve as a self-selection or self-screening device (Parsons, 1991), but possibly effect 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.[1]

Several studies document a decrease in take up of means-tested welfare benefits as a result of increased application costs or complexity of the application process (e.g. Bhargava and Manoli (2015), Currie and Grogger (2001), Krueger (1990), Bitler et al. (2003) and Brien and Swann (1999)). These increased application costs include requiring more frequent visits to the welfare office, reduced re-certification intervals or requiring extensive income documentation. Kleven and Kopczuk (2011) model complexity as an instrument used by program administrators to extract a better signal of true eligibility, which is chosen jointly with benefit levels and eligibility rules in the design of a program. In all these cases the question remains whether (non-financial) barriers target the desired population. The mentioned studies report a decrease in take up, but do not observe the source of income of the non-participants. Without this it cannot be established whether the intended goal is accomplished. We exploit the availability of extensive administrative data in the Netherlands to follow participants in the experiment. This allows us to combine the advantages of field experiments and administrative data.

The job-search requirement in the search period relates this paper to the literature on the effectiveness of active labor-market policies. The majority of this literature concentrates on programs aimed at recipients of unemployment insurance benefits with relatively good employment prospects (for an overview, see Card et al. (2010)). Welfare recipients are often

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[1]A third mechanism could run via time-inconsistent preferences (O'Donoghue and Rabin, 1999). Many of the costs of applying for welfare are borne immediately, whereas benefits are borne later. Hence, a search period might discourage a person who puts more weight on the presence than on the future to carry through the application for benefits, even though it would be utility maximizing to receive benefits at a later date. It is, however, not clear that the two hypotheses (nonparticipants in social programs are 'irrational' in the sense that they have time-inconsistent preferences or that nonparticipants face high costs of enrollment) have different policy implications (Currie, 2006).

more disadvantaged and at risk of leaving the labor force permanently, so the potential gains of effective programs for welfare applicants can be large. Furthermore, for credit constrained welfare applicants small financial incentives can matter a lot. For example, Van der Klaauw and Van Ours (2013) find that imposing benefit sanctions substantially increases the individual transition rate from welfare to work. Card and Robins (1998) show that the financial incentives in the Canadian self-sufficiency program induced welfare recipients to work more. However, randomized field experiments with welfare applicants are scarce. Given the vulnerability of the population, authorities are not very likely to agree with a field experiment. This is particularly true when evaluating existing policies (in contrast to the evaluation of additional policies or resources).

In our field experiment we use a so-called encouragement design (Duflo et al., 2007), in which treatment is encouraged instead of imposed on a randomly selected group of subjects. We randomize treatment over caseworkers, who receive the instruction to apply one particular treatment, a default option, to all their new clients. In case the default option is really not appropriate, caseworkers are allowed to deviate. This increases support for the experiment among caseworkers. The design exploits the random assignment of applicants to caseworkers within a local welfare office.[2] Our empirical strategy is similar to Maestas et al. (2013), who exploit variation in examiners' allowance rates as an instrument for disability benefit receipt, using that applicants are randomly assigned to disability examiners. Also Behaghel et al. (2013) used an encouragement design to evaluate an active labor market program in France. Compared to these two studies, in our experiment the exogenous variation in the treatment probability is higher.

To preview our results, we find a strong and persistent negative effect of a search period on the likelihood to receive benefits. A search period reduces the likelihood to receive 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 (112 percent) by higher earnings. A search period does not increase the likelihood to engage

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

in criminal activities. The fact that the reduced income from benefits is fully compensated by higher earnings suggests that a search period targets the desired population of applicants, which is confirmed by a subgroup analysis. The effect of a search period increases with education; For applicants with at least a bachelor degree the likelihood to receive benefits decreases with 50 percent. We find no evidence of negative side effects of a search period for the most vulnerable applicants. Only for higher educated applicants the search period increases the probability to have a very low income.

The remainder of the paper is structured as follows. The next section provides details about the benefit system in the Netherlands, explains the experimental design and provides evidence on compliance rates. Section 3 describes the data used in this paper and provides evidence on the random assignment. In section 4 we discuss the empirical strategy and the identification. Section 5 presents the main results, while section 6 assesses the heterogeneity of treatment effects along the dimensions gender, age, education and position in the earnings distribution. Section 7 discusses the generalizability of our results by interpreting the estimated local average treatment effect. Section 8 concludes.

2 Setting and experimental design

Welfare in the Netherlands

In the Netherlands, welfare serves as a safety net and provides households that have no or not enough means of living with a minimum income level. Welfare benefits are means tested (on both income and wealth) and the benefit level only depends on the composition of the household. The benefits range from 802 euro per month for a single person household to 1336 euro per month for a couple with children.[3] In addition to welfare benefits, households can receive monthly housing subsidies, health insurance subsidies and child subsidies.[4]

If a welfare recipient finds part-time employment or has part-time employment with earnings below the welfare level, earnings have a marginal tax rate of 100 percent. For the health insurance and housing subsidies other marginal tax rates apply. At the time of our

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[3]The exact amounts for 2012 are given in Table A1 in the Appendix.

[4]The maximum monthly amounts of these subsidies are €309 (for housing subsidies), €70 (health insurance subsidies) and per child €84 (child subsidies).

experiment, the net minimum wage was approximately €1200 per month in case of full-time employment, such that the replacement rate for applicants with a low-earnings potential can range from 67 percent to almost 100 percent (depending on household composition). Unlike in the US, there is no maximum to the time period that a household can receive welfare benefits. 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.

Rules about eligibility and the level of benefits are determined at the national level, but the responsibility for the implementation is at the municipality level. Individuals apply for benefits at the local welfare office in their city district. The applicant will be invited for an intake meeting with a caseworker, during which the rights and obligations of receiving welfare benefits are explained. Applicants have to bring extensive proof of their (past) income, bank accounts, housing etc. to this meeting. Based on this information, supplemented with information from administrative sources, it will be determined whether an individual is entitled to benefits.

Municipalities receive a fixed annual budget for managing their welfare system, of which any unused excess may be kept.[5] How welfare recipients are guided to work and how the number of welfare recipients is kept under control is left to the municipalities' discretion. The level of the annual budget is determined with an allocation model based on average population characteristics of the municipality such as home ownership, age and education. These characteristics are not likely to depend on the effectiveness of the municipalities' labor market policy.

Setting of experiment

The sample for the experiment consists of individuals that applied for welfare in Amsterdam between April 2012 and March 2013 and who should be able to find regular employment within six months. The latter 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. Based on this profile the applicant is classified in one of four classes. The type

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[5]In 2012, the municipality of Amsterdam spent 103 percent of its budget for welfare which was also the average for all municipalities in the Netherlands.

and intensity of guidance given to the applicant and what is expected of him in terms of job-search effort varies over these classes.

Search periods are only applied to the class of applicants that should be able to find regular employment within six months, so this is the group that we will focus on. In addition, we restrict the experiment to individuals that are aged 27 and above, as different rules apply to welfare recipients under age 27. These selection criteria apply to approximately 25 percent of the total inflow. The applicants are divided over the five welfare offices of the different districts of Amsterdam.

A relatively large share of the population of Amsterdam receives welfare benefits. In January 2012, 6.4 percent (34,550 individuals) of the population between 20 and 65 years old received benefits in Amsterdam compared to 3.1 percent in the Netherlands. In 2012, the inflow in welfare in Amsterdam consisted of 11,706 individuals while in the same year 8,944 individuals stopped receiving benefits. The inflow exceeding the outflow is a direct result of the economic conditions. Figure B1 in the appendix shows GDP growth for the Netherlands and in- and outflow into welfare benefits in the city of Amsterdam from 2008 until 2014. At the time of the experiment, in 2012 and the first half of 2013, the Dutch economy was suffering from a second downturn as a consequence of the financial crisis. Worsening economic conditions have both a direct and a lagged effect on inflow into welfare benefits. If unemployment increases it will directly increase the inflow into welfare benefits for individuals that have no or limited entitlement to unemployment insurance benefits. From individuals that can first deplete their unemployment insurance benefits there will be a delayed inflow into welfare benefits.

During the experiment we asked caseworkers to fill in a form with information about the applicant in each intake meeting. Table A2 in the appendix gives a description of the population that took part in the experiment, based on these forms. The caseworkers report that 26 percent of the applicants have a bad financial situation. The applicants are considered relatively independent, which is probably related to the fact that they are classified as being able to find employment within six months. 24 percent of the applicants applied for benefits directly after losing (self)employment, while the remainder either depleted their UI benefits or applied for another reason. Other reasons to apply for welfare include exhaustion of savings, divorce, less hours at an existing job such that the total wage drops below the

welfare level, etc. Finally, 56 percent of the applicants are non-western immigrants and 47 percent of the applicants have received welfare benefits before.

Intervention: Search period

Our experiment focuses on the search period, which was introduced by the welfare agency in 2011. A search period postpones the application for welfare benefits for at most four weeks, during which the individual has to actively search for employment. The application for welfare will only be activated if the applicant returns to the agency after the 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 search period thus only delays the first payment of benefits; it does not reduce the amount of benefits that an individual is entitled to. Applicants that find employment during a search period can file a request for welfare for the period between the date of registration and the starting date of the new employment. This is quite some administrative hassle and also not actively promoted by the welfare agency, so not all individuals that find a job during the search period use this possibility. Figure 1 gives a schematic representation of the application process for welfare in Amsterdam, including the search period. Irrespective of the search period, the welfare application needs to be processed within eight weeks after the day of registration.

The decision to apply a search period is made during the intake meeting and, before the experiment, was left to the caseworkers' discretion. However, 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. An applicant cannot refuse a search period. When applying a search period the caseworker will specify a minimum number of job applications that the applicant should make within the search period. The caseworker also stresses that during the 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 any job. If the applicant returns from a 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 generally a 30% reduction in benefits for the duration of one month. In practice, these sanctions are almost never applied.

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Figure 1: Welfare application procedure
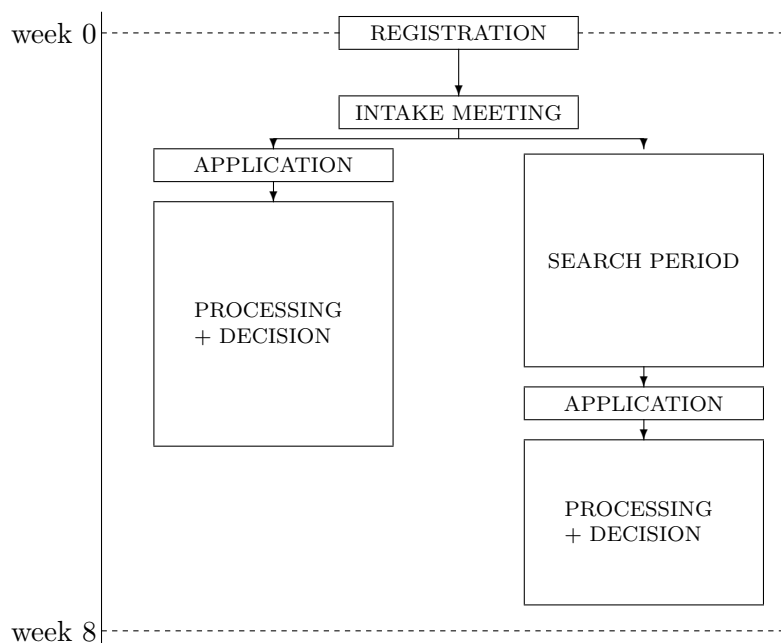
Experimental design

In order to isolate the causal effect of a search period we ran a field experiment in which we manipulated the assignment of a search period.[6] Applicants are not informed of the experiment, to prevent that this knowledge would influence their behavior. Instead of randomizing the treatment over individuals, we randomized the treatment over caseworkers. Caseworkers received the instruction to apply one particular treatment to all their new clients during a three-month period. This particular treatment we call their *default option*, which makes our design similar to an encouragement design (Duflo et al., 2007). We instructed caseworkers to deviate from the default option only in cases where the default option is really not appropriate. The possibility to deviate in special cases helped to make the experiment more acceptable for caseworkers and in getting their commitment to the experiment. Our design exploits that within local offices applicants are randomly allocated to caseworkers. The matching of applicants to caseworkers is an administrative process in which there is

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[6]The original research design, including a power analysis, can be found at `http://personal.vu.nl/b.vander.klaauw/OnderzoeksOpzetDWI.pdf` (in Dutch).

no regard for applicant characteristics. Generally, applicants are matched to the caseworker with the lowest case load. There were three different default options:

- Never: never apply a search period

- Always: always apply a search period if the financial situation of the individual allows for it

- Normal policy: the decision is left to the discretion of the caseworker

In the remainder we refer to these default options as 'never', 'always' and 'normal'. The default option 'normal' shows what the caseworkers would do with the applicant in absence of an experiment, which allows us to study targeting by caseworkers. The experimental period was divided into four periods of three months. Each period the caseworkers received a new default option which they had to apply to all new applicants assigned to them. 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.

For the success of the experiment it was crucial that compliance to the default options was sufficiently high. Before the start of the experiment we had 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, such that they were constantly reminded of their current default option. We kept track of the inflow and regularly tried to visit caseworkers if they had not filled in the forms for new applicants or deviated substantially from their given default option. During the experiment, we visited the welfare offices almost weekly to answer questions from caseworkers, pick up forms and to keep an eye on the implementation of the experiment.[7]

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[7]The forms were filled in for 72 percent of the observations. Given that all information is also available through the administrative records (for the full sample), we will not use the information from the forms in our analysis. Initially, we introduced the forms because we were not sure if the administrative system of the welfare office would also include applicants that applied for benefits but never returned after a search period. This turned out to be the case. However, 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 upon the caseworkers.
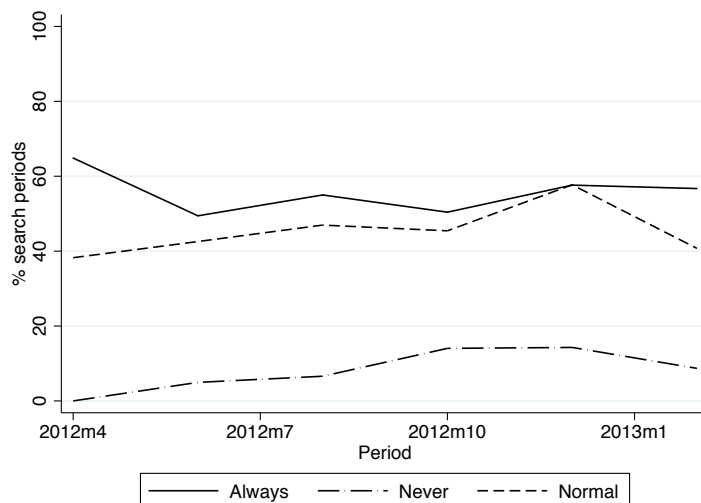
Figure 2: Percentage of applicants that was given a search period by default option, over time

Compliance rates

Figure 2 shows the fraction of search periods applied per default option over the experimental period. The distinction between the three default options is most pronounced at the start of the experiment. The percentage of search periods given under the default 'always' remained relatively stable over time, while it increased for the other two default options. During the experiment the policy of the welfare agency changed towards increased giving of search periods, which can explain the increase under default option 'normal'. Furthermore, given the amount of caseworkers that we had to communicate with (112 caseworkers in total) the increase in the number of search periods under the default option 'never' can be explained by new caseworkers entering the organization or replacing other caseworkers without being briefed by us. On average, over the experimental period, caseworkers with default option 'never' gave a search period to nine percent of applicants, caseworkers with default option 'always' gave a search period to 55 percent of applicants, and caseworkers with default option 'normal' gave a search period to 46 percent of applicants.

3 Data

Data sources

Our analysis employs data from three different sources, that are linked using unique identifiers for each individual. The welfare agency of Amsterdam provided 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 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 security administration to observe monthly information for each individual on the amount of income from employment, hours worked and income from other benefit schemes.[8] We have this information for all participants in the experiment from 2008 up to October 2013. The retrospective nature of the data allows us to construct a labor market history for all individuals, which can be used as a control variable. However, data on income from self-employment is missing. Third, we link the data to individual records of all Dutch citizens kept by Statistics Netherlands. Using these records we can determine whether an individual was suspect of a crime in a given year. The data from both Statistics Netherlands and from the national social security administration cover the full population of the Netherlands such that the experiment sample can be matched without attrition.

Sample

Based on inflow in previous years we expected 2500 individuals to participate in the experiment. This is also the inflow we based the initial power calculations on. Our final sample consists of 2860 welfare applications (2709 unique individuals).[9] Worsening economic circumstances are probably the explanation for this increase. 38 applicants have an incorrect personal identifier, such that we can not match them to their outcomes. For eight applicants information on their caseworker is missing, so we can not determine under which default

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[8]The other benefit schemes include among others unemployment insurance benefits and disability insurance benefits. We also observe whether someone receives welfare benefits in another municipality.

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

Table 1: Characteristics of applicants under different default options

	Full Sample	Default option			P-value difference		
		Normal	Always	Never	Normal vs Always	Normal vs Never	Always vs Never
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Background Characteristics							
Female	39%	39%	41%	36%	0.45	0.44	0.07*
Partner	11%	12%	9%	8%	0.79	0.68	0.57
Children	14%	14%	15%	13%	0.47	0.72	0.58
Age under 30	25%	24%	25%	27%	0.56	0.13	0.30
Age 31 - 36	25%	23%	28%	27%	0.07*	0.12	0.82
Age 37 - 45	26%	27%	24%	24%	0.17	0.22	0.96
Age above 45	24%	25%	23%	22%	0.31	0.07*	0.37
Bachelor/Master	28%	27%	28%	31%	0.66	0.59	0.52
Vocational	23%	24%	24%	21%	1.00	0.53	0.47
High school	13%	11%	16%	13%	0.06*	0.85	0.08*
Prep. vocational	20%	21%	17%	20%	0.04**	0.68	0.19
Primary education	10%	10%	11%	10%	0.35	0.29	0.94
Education missing	2%	3%	2%	1%	0.37	0.30	0.79
Yearly income in 2 yrs before (x1000 €)	13.6	13.5	13.4	14.2	0.31	0.95	0.34
Treatment							
Received search period	40%	46%	55%	9%	0.00***	0.00***	0.00***
Number of obs	2788	1657	571	560			

Note: The p-values in the last three columns are weighted by the office of registration, as randomization took place within welfare office.

*** =significant at 1% level, ** =at 5% level, * =at 10% level

option they were treated. Furthermore, for 24 applicants information on the search period is incomplete, and for seven applicants information on essential controls is missing (gender, age or household situation). This means that in total we exclude 72 observations from the analysis, which leaves us with 2788 observations (2640 unique individuals). For 64 of the 72 excluded observations we have information on the default option. The excluded observations are evenly distributed among the three default options (joint p-value of 0.70).

The first column of Table 1 provides information about background characteristics. The majority of the applicants (over 60 percent) are male. Applicants are relatively young: the

average age in the sample is only 38.4 years, with the median at 36 years. Recall that our sample includes only individuals older than 27 years. Young people have had less time to build up work history and, therefore, have in general shorter maximum unemployment insurance entitlement. Average yearly income in the two years before applying is approximately 13,600 euro.[10] Given that welfare is means tested and the income of the partner is also taken into account in the means test, couples are less likely to qualify for welfare. In our sample only 11 percent of the applicants have a partner, and 14 percent have children. The singles living with children are almost exclusively women. Finally, 28% of the applicants has at least a bachelor's degree (compared to 35% for the total population aged 25-55 in the Netherlands).

Random assignment

Our design hinges on the fact that within local welfare offices applicants are randomly allocated to caseworkers, and are, therefore, also randomly allocated to default options. The second to fourth column of Table 1 show the mean characteristics of applicants under the default options 'normal', 'always' and 'never'. Columns five to seven show the p-value of the difference between two groups, for all of the different combinations. The characteristics seem to be well balanced over the three treatment groups. There are no systematic differences and for only five out of 42 reported characteristics the difference is significantly different from zero. The lower panel in table 1 shows the treatment probability for applicants in each of the three treatment groups 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.

Table 2 shows the characteristics of only the applicants under the default option 'normal'. In this treatment group the decision to apply a search period was left to the caseworker. The first column represents the applicants that did not get a search period and the second column those that did get a search period. The third column gives the p-value of the difference. This table provides insight on how caseworkers target the search period in a non-experimental set-

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[10]For comparison: the yearly income of a full time worker at the minimum wage amounted 18,870 euro in 2012. At the median wage the yearly full time income amounted around 40,000 euro in 2012 (Statistics Netherlands, OECD).

Table 2: Targeting of caseworkers under default 'normal'

| | Search period | | p-value diff |
| | No | Yes | |
	(1)	(2)	(3)
Female	40%	37%	0.32
Partner	13%	11%	0.01**
Children	16%	11%	0.00***
Age under 30	20%	30%	0.00***
Age 31 - 36	21%	26%	0.02**
Age 37 - 45	30%	23%	0.00***
Age above 45	29%	21%	0.00***
Bachelor/Master	26%	27%	0.32
Vocational	24%	24%	0.90
High school	13%	9%	0.02**
Prep. vocational	22%	20%	0.36
Primary education	10%	10%	0.73
Education missing	1%	5%	0.00***
Yearly income in 2 yrs before (x1000 €)	13.7	13.3	0.32
Number of observations	899	758	

Note: The p-values in the last three columns are weighted by the office of registration, as randomization took place within welfare office.

*** =significant at 1% level, ** =at 5% level, * =at 10% level

ting. The results in this table confirm that without experimental manipulation caseworkers target search periods. For example, young applicants and those without children are more likely to get a search period.

Descriptive statistics of the outcome variables

Table 3 displays information on the outcome variables. We will mainly look at outcomes up to six months after registration. Table 3 shows that at that moment 50 percent of the applicants are still receiving welfare benefits. Average welfare payments per individual are almost 3,000 euro within six months and average earnings are 2,500 euro. Income from other benefits (mainly unemployment insurance benefits and disability insurance benefits) is 511 euro. Total income is the sum of these three income sources (welfare benefits, wage earnings and other benefits).

Table 3: Descriptive statistics of the outcome variables, full sample

	Mean	SD
Labor market outcomes		
Fraction of applicants on welfare after 6 months	0.51	0.50
Cum. welfare benefits received over 6 months (in €)	2953	2413
Cum. earnings over 6 months (in €)	2494	3766
Cum. other benefits over 6 months (in €)	511	1715
Cum. total income over 6 months (in €)	5957	3653
Nr of weeks I[benefits>0] over 6 months	16.2	10.8
Cum. hours worked over 6 months	190	266
Mean hourly wage over 6 months (in €, if I[wage>0])	13.3	6.3
Crime outcomes		
Suspect of a crime in 2012 or 2013	0.09	0.28
Suspect of a property crime in 2012 or 2013	0.04	0.20
Number of applicants	2788	

A search period might not only have an effect on labor market outcomes. We will, therefore, also look at the impact of a search period on crime. For the crime outcomes we do not know the exact date of the crime, just whether an individual was the suspect of a crime in a given year. We will look at the effect of a search period on crime outcomes in 2012 and 2013. For some applicants this might imply that the crime took place before the search period was issued. However, given the randomized design there is no reason to suspect a difference in crime rates before the start of the experiment. Table 3 shows that about nine percent of the applicants were suspect of a crime in 2012 or 2013.

4 Empirical strategy and graphical evidence

Empirical strategy

To estimate the effect of a search period on welfare receipt and other income variables we assume a linear relationship between these outcomes of individual i at time t who applied for welfare at time τ at welfare office w ($Y_{it\tau w}$). $SP_{i\tau}$ is an indicator for whether the individual

received a search period at the moment of registration.

$$Y_{it\tau w} = \alpha_{t-\tau} + \gamma_\tau + \delta_{t-\tau} SP_{i\tau} + X_i \beta_{t-\tau} + \omega_{w,t-\tau} + u_{it\tau} \tag{1}$$

In this equation $t - \tau$ indicates the number of weeks elapsed between the day of registration and the week in which the outcome is observed. X_i is a set of covariates including age at registration, gender, partner status, an indicator for children, cumulative income in the 24 months before registration and dummies for five education categories. $\alpha_{t-\tau}$ and γ_τ are fixed effects for the number of weeks elapsed since the registration and the quarter of registration. The latter are included to take business cycle effects into account. Finally, $\omega_{w,t-\tau}$ are local welfare office fixed effects, which also control for possible differences between the local labor market in the city districts that they serve. The parameters of interest are $\delta_{t-\tau}$ which describe the effect of a search period $t - \tau$ weeks after registration. We estimate equation (1) separately for each week since registration $(t - \tau)$. Standard errors are clustered at the level of the applicant, to account for multiple applications per individual.

If caseworkers are more likely to give a search period to applicants with better labor market prospects the OLS estimator of $\delta_{t-\tau}$ will be biased. We exploit our experimental design to estimate the causal effect of the search period using two strategies. First, we replace $SP_{i\tau}$ by the default option of the caseworker that conducted the intake meeting:

$$Y_{it\tau w} = \alpha_{t-\tau} + \gamma_\tau + \delta_{1,t-\tau} Normal_{i\tau} + \delta_{2,t-\tau} Always_{i\tau} + X_i \beta_{t-\tau} + \omega_{w,t-\tau} + u_{it\tau} \tag{2}$$

Because compliance was 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 search periods to the current policy (δ_1), or to a more strict 'always' policy (δ_2). However, since the parameters average over all applicants (including those that did not receive a search period), they do not reflect the effect of actually imposing a search period. Therefore, we employ a second strategy, where we instrument $SP_{i\tau}$ with the default option of the caseworker that conducted the intake. We estimate a first-stage

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equation of the form:

$$SP_{i\tau} = \eta_{t-\tau} + \kappa_\tau + \lambda_{1,t-\tau}Normal_{i\tau} + \lambda_{2,t-\tau}Always_{i\tau} + X_i\theta_{w,t-\tau} + \phi_{t-\tau} + v_{it\tau} \qquad (3)$$

In equation (3) λ_1 and λ_2 reflect the difference in the probability to receive a 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 36 percentage point (45 percentage point) more search periods than caseworkers with the default option 'never'.

Three key assumptions underlie our empirical strategy. First, for the default options to be valid instruments, applicants' assignment to a caseworker must be uncorrelated with unobserved characteristics that could influence labor market outcomes (conditional on observed characteristics). As discussed before, applicants are assigned to caseworkers in a process that is unrelated to applicant characteristics. Because applicants are assigned to caseworkers within a welfare office it is crucial to control for welfare office fixed effects in our analysis. Otherwise differences in the number of search periods applied under the default options could reflect differences in populations of applicants between locations, for example arising from differences in local labor market conditions.

A second assumption that is needed for the causal interpretation of the instrumental variables estimates is instrument monotonicity: No individual would have received a search period from a caseworker with default option 'never' and would not have received a search period from a caseworker with default option 'always'. This is very likely to hold for the same caseworker. Formally, we see that within a local welfare office some caseworkers have a higher search period rate under 'never' than (other caseworkers) under 'normal' or 'always'. However, in our experiment some caseworkers only have a small number of applicants per default option and observed differences in the fraction of applied search periods can also reflect differences in the average characteristics of the applicants. Furthermore, recall that only very few search periods are applied under the default option 'never'. Therefore, it is likely that an applicant who would receive a search period under 'never' would also get this under any of the other options in which case the monotonicity assumption holds.

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

Using instrumental variables, we estimate a Local Average Treatment Effect (LATE) (Angrist et al., 1996). The effect of a search period is only identified for the group applicants for which the caseworkers complied to their default options. In section 7 we elaborate further on the definition of compliers in the setting of the experiment and the interpretation of our estimated effect of a search period.

Graphical evidence

We start with a graphical description of how labor market outcomes are related to the three default options, before turning to a more detailed regression analysis. Figure 3 presents the evolution of the fraction of applicants that receive welfare benefits by default option. Two issues are important before interpreting the graph. First, an individual is counted as receiving benefits in a certain week if payments of benefits were made that are assigned to that week. For example, if an applicant returns from a search period and receives benefits retrospectively from the moment of applying, we count that person as being on benefits from the registration onwards (even though the first actual payment took place after eight weeks). This implies that a mechanical effect of the search period, the delayed payment of the first benefits, can not be a driver of possible effects. Second, from the figure it is clear that take-up of welfare benefits is less than 100 percent for all three default options. This arises because eligibility for welfare benefits is only determined if the application for benefits is activated, so after the intake meeting and a possible search period. If an individual does not return after a search period it will not be observable whether that individual would have
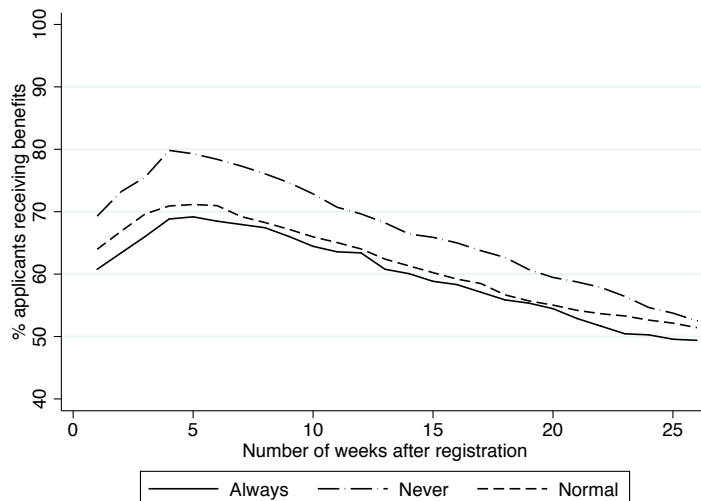
Figure 3: Fraction of applicants receiving welfare benefits by default option

been eligible for benefits. Therefore, our sample is determined based on all the applicants that have an intake meeting; conditioning on eligibility can lead to confounding effects as it is a possible outcome variable.[11]

Figure 3 shows that under the default option 'never' the fraction of people receiving welfare is higher than under the default 'always'. The fraction receiving welfare under the default option 'normal' is in between the two, but closer to the default option 'always'. This suggests that a search period has a substantial effect on the probability to be on welfare. Over time the difference between the three default options decreases, but 26 weeks after registration applicants under the default option 'never' are still more likely to receive benefits.

Figure 4 displays information on all possible sources of income that an individual can have, split by default option and for 10, 20 and 30 weeks after registration. We distinguish the income sources as being on welfare, receiving a combination of wages and welfare, receiving only wage, being dependent on other benefits and finally to have no (observed) source of income. The latter group can include self-employed individuals, individuals that move in with a partner or parents or have unreported income. Again it is clear that both after 10 and 20 weeks applicants in the default group 'never' are more likely to receive welfare

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[11]Third, we also see that during the first five weeks the fraction increases for all three default options. The increase is due to people who register at the welfare office before the date of exhaustion of UI benefits. This is advised by the unemployment office to prevent financial problems due to the processing time in which no welfare benefits are received).
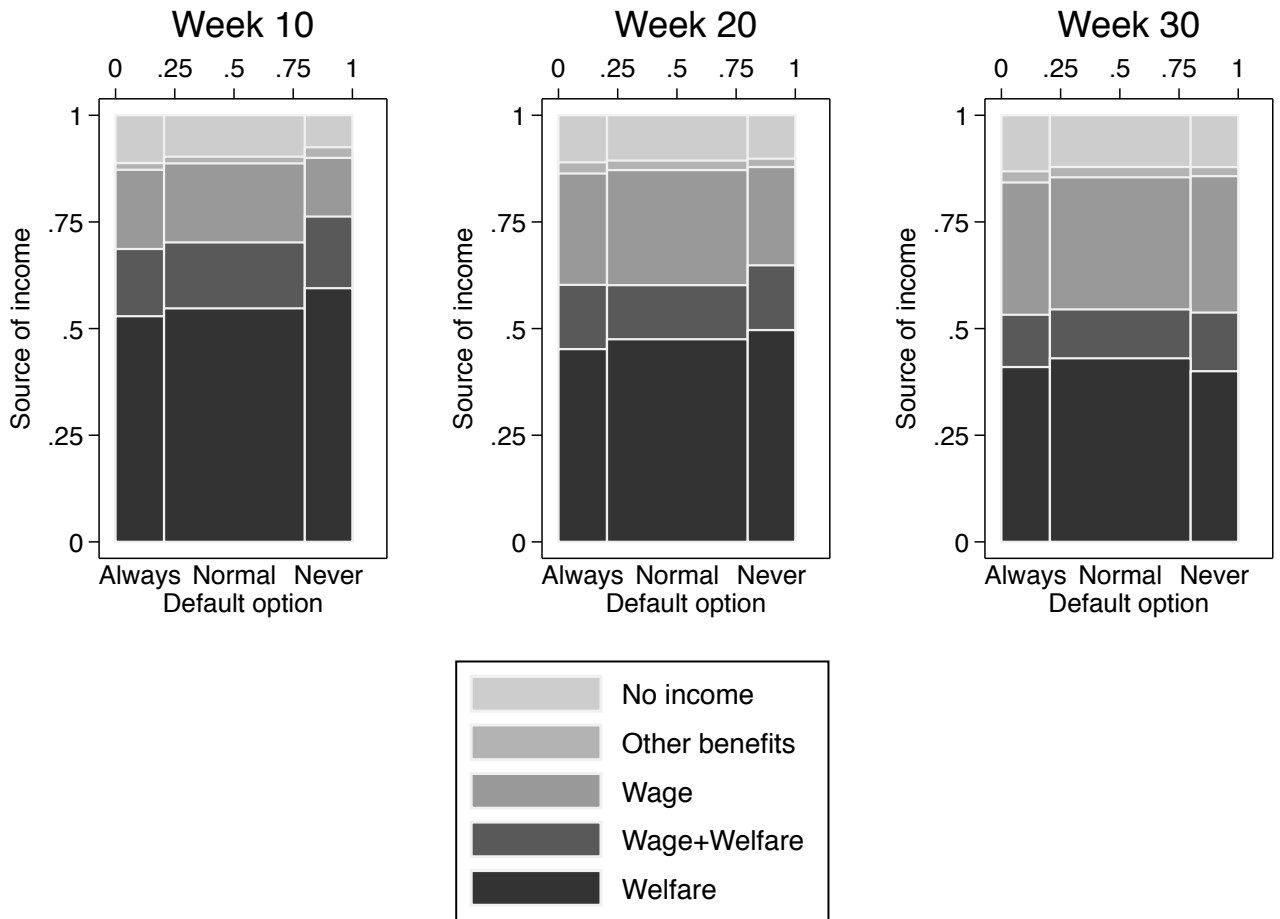
Figure 4: Source of income by default option

benefits or a combination of wage and welfare benefits than applicants in the other two default groups. In Figure 4 we can see which other sources of income they depend upon. Ten weeks after registration applicants in the default groups 'normal' and 'always' are more likely to have no (observed) source of income. After 20 weeks the proportion of applicants with no observed income is fairly equal again for the three default options, but applicants from the defaults 'normal' and 'always' are more likely to receive wages. Finally, after 30 weeks the proportions of the different sources of income are very similar for the three default groups. These results suggest that a search period reduces the probability to be on welfare, which during the first weeks leads to a higher probability to have no income but later the loss in income is compensated by an increased probability to have a job.

5   Results

The presentation of the main results is divided into five parts. We first present and discuss estimates of the effect of a search period on the likelihood to receive welfare benefits. We continue with estimates of the effect of a search period on earnings and other benefits, followed by a discussion of the impact on crime. Finally, we look at the long-term effects of a search period.

Welfare benefits

In Figure 5a we plot the point estimates and 90 percent confidence intervals of the intention-to-treat effect on the probability to receive benefits (0/1) for each week after registration (following equation (2)). In this figure, default options 'always' and 'normal' are compared to the default option 'never'. We see that individuals with a caseworker with default option 'always' have a ten percentage point lower probability to receive welfare. Over time the effect becomes slightly smaller. Individuals with a caseworker with the default 'normal' have a six percentage point lower probability to receive welfare. More than 20 weeks after registration both effects are still significantly different from zero. The results are summarized in Table 4, that reports the effect of the default options on cumulative outcomes half a year after registration. The first and second column provide estimates that only include controls for calendar time and welfare office, the third and fourth column also control for background characteristics of the applicant. The estimates hardly change when including control variables, which is expected since the default options were randomly assigned. Table 4 shows that compared to the default option 'never', applicants in the default group 'normal' receive benefits on average 1.72 weeks shorter. For applicants in the group 'always' the effect is larger and amounts to 2.18 weeks less. Recall that this is not a mechanical effect of the search period. The entitlement to benefits starts at the date of registration and independent of a search period applicants receive benefits from this day.

To get an idea about the size of the effect of a search period, we next estimate the effect using an instrumental variables strategy. The first-stage estimates of the default options on the probability to receive a search period are respectively 0.34 (s.e. 0.02) for the default 'normal' and 0.46 (s.e. 0.02) for the default 'never', with an F-statistic of the instruments

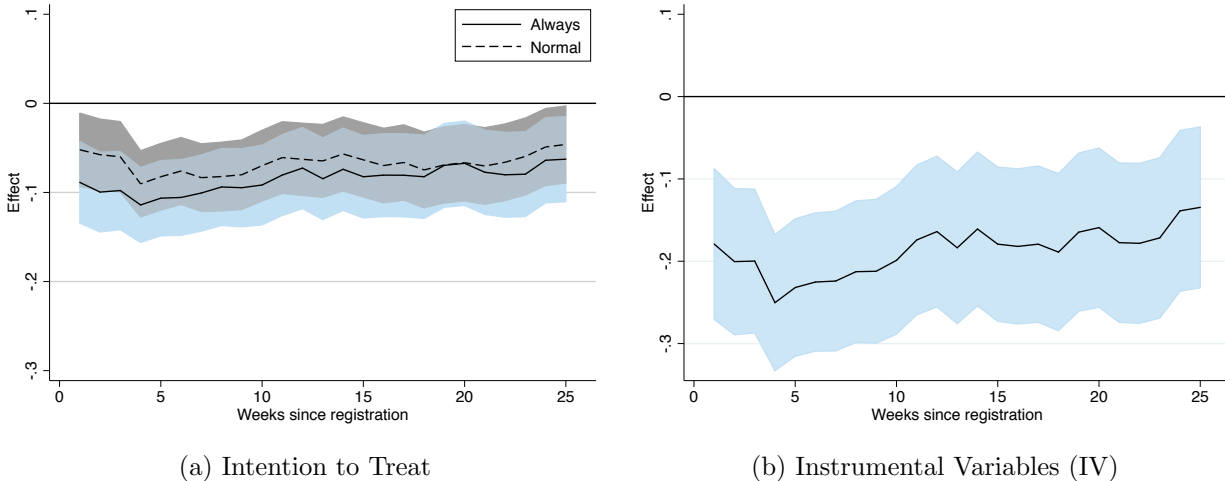(a) Intention to Treat (b) Instrumental Variables (IV)

Figure 5: Probability to receive benefits (colored areas are 90 percent confidence intervals)

of 237. Figure 5b plots the point estimates of the instrumental variable estimates of the probability to receive welfare. We see that a search period lowers the probability to receive welfare with around 20 percentage points in the first ten weeks. Given that in the group with the default option 'never' total take up of welfare benefits is around 80 percent this implies a reduction of about 25 percent. After the tenth week the effect somewhat decreases to minus 11 percentage points in week 26. The effect of a search period is still significantly different from zero 26 weeks after registration. This implies that the search period does not only have a short-run effect, but also has a more profound and longer lasting effect on welfare uptake. In the first row of Table 4 we see that a search period reduces the period an individual receives welfare by 4.8 weeks.

Figure 5b uses a binary variable for receiving welfare. Welfare recipients are, however, obliged to also accept part-time jobs and, therefore, also partial outflow can take place. In order to look at the total impact of the search period on welfare receipt Figure 6a looks at the effect of a search period on the amount of welfare benefits an individual receives. The pattern is quite similar to the pattern in Figure 5b with the binary welfare variable. The search period has a strong effect on welfare receipt that is long lasting and only becomes insignificant after 24 weeks. On average, about 30 euro per week is saved on welfare benefits if a search period is imposed. Table 4 shows that total welfare benefit payments decrease with 814 euro, which amounts to a 25 percent reduction of the mean cumulative amount of

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Table 4: Effect of 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)
Nr of weeks I[benefits>0]	-2.05***	-1.64***	-2.18***	-1.72***	-4.78***
	(0.63)	(0.56)	(0.62)	(0.56)	(1.25)
Benefits received (in €)	-372***	-260**	-389***	-261**	-814***
	(142)	(127)	(141)	(126)	(287)
Earnings (in €)	348	291	407*	342*	909**
	(225)	(197)	(220)	(195)	(449)
Other benefits (in €)	-50	-51	-42	-68	-122
	(98)	(88)	(95)	(87)	(199)
Total income (in €)	-74	19	-25	13	-27
	(218)	(193)	(213)	(189)	(438)
Nr of weeks I[Earnings>0]	0.71	0.35	0.75	0.40	1.48
	(0.35)	(0.54)	(0.60)	(0.54)	(1.23)
Cum. hours worked	26*	18	30*	21	64**
	(16)	(14)	(16)	(14)	(31)
Mean hourly wage (in €)	0.71	0.30	0.72	0.26	1.78
(conditional on work)	(0.60)	(0.43)	(0.58)	(0.43)	(1.33)
Observations	2788		2788		2788
Included controls:					
Calendar time fixed effects	Yes		Yes		Yes
Local office fixed effects	Yes		Yes		Yes
Applicant characteristics	No		Yes		Yes

Note: Columns (1) and (2) and columns (3) and (4) in each row represent one equation. The fifth column represents a separate equation. The controls for applicant characteristics include age at registration, gender, household composition, cumulative income in 24 months before registration and dummies for five education categories. Standard errors are clustered at the level of the applicant
*** =significant at 1% level, ** =at 5% level, * =at 10% level

welfare over the first 26 weeks.

**Income from wages and other benefits**

Not receiving welfare does not necessarily imply that someone is employed. A search period could discourage applicants to apply for welfare because of the increased application costs and the complexity of the application process. The remainder of Figure 6 shows the effect of a search period on other sources of income. Figure 6b shows the effect of the search period on weekly income from employment. A search period has a positive effect on earnings of about
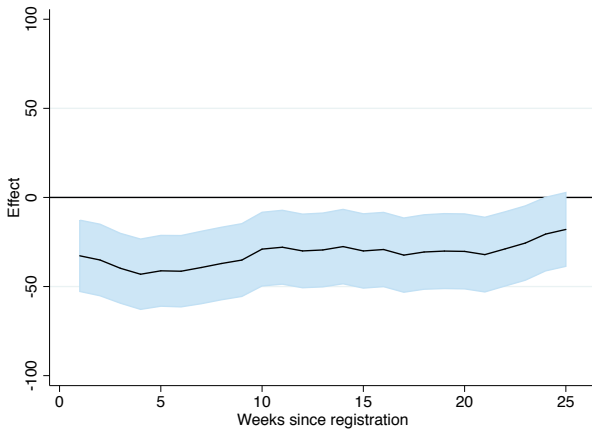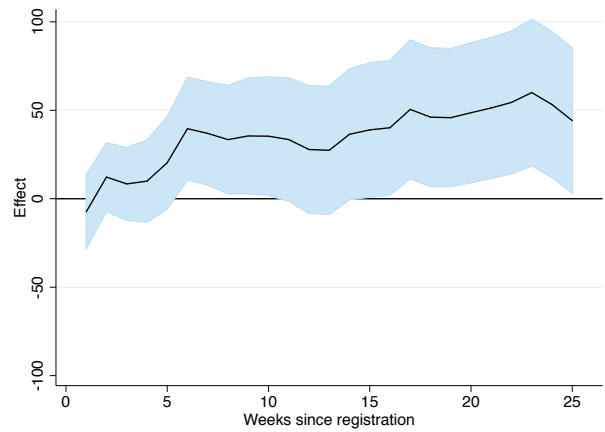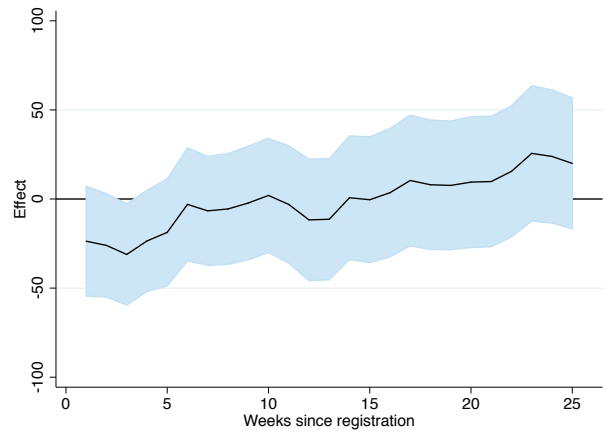
(a) Welfare benefits   (b) Earnings
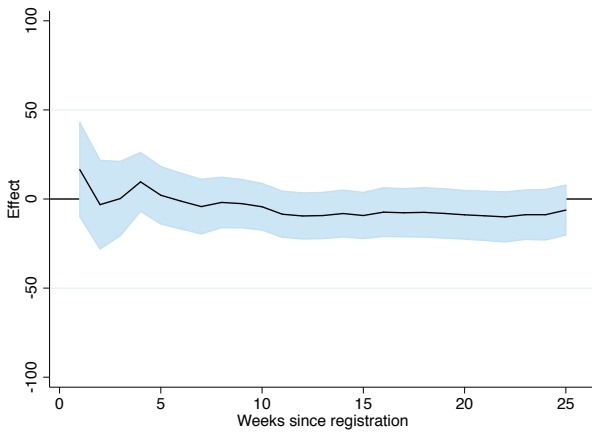
(c) Other benefits   (d) Total income

Figure 6: Instrumental variable estimates of the effects of a search period on labor market outcomes $t - \tau$ weeks after registration (colored areas are 90 percent confidence intervals)

30 euro per week that becomes significant after five weeks. After 14 weeks the effect steadily increases to 50 euro a week. Table 4 shows that during the first half year after registration individuals with a search period earn in total 909 euro more than individuals without a search period. This implies that, on average, individuals with a search period completely compensate (112 percent) their forgone welfare benefits by income from employment.[12]

A spillover effect of a search period could be that individuals try to get income from

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[12]Given that our experiment took place during an economic downturn (see Figure B1 in the appendix), this effect might be different when labor market conditions change. During the second half of the experimental period, there was a sharp increase in the inflow into welfare benefits. However, we find that the estimated effect of a search period is the same for applicants in both time periods. Within this limited time frame, we, therefore, do not find strong evidence that the effect of a search period varies with labor market conditions.

another benefit scheme or apply for welfare benefits in a different municipality. We do not expect large effects here, as welfare should be the safety net and people can only apply if they are not entitled to any other benefit scheme. Second, people would need to move in order to apply for welfare benefits in a different municipality. Figure 6c shows the estimates of the effect on income from other benefit schemes and these are indeed small and insignificant. This is confirmed by the insignificant effect on cumulative other benefits in Table 4. Finally, the effect of a search period on total income (the sum of income from welfare, wages and other benefits) is shown in Figure 6d. During the first four weeks the effect on total income is negative and (almost) significant. After that, the effect is close to zero and insignificant. The effect on cumulative total income is small and not significantly different from zero (Table 4). The negative effect during the first four weeks might be caused by individuals that find employment within the search period, and do not apply for benefits for the weeks they spent searching before they found a job.

The increase in earnings can be driven by three different channels: First, an increased probability to have a job, second, an increase in the amount of hours worked or third, an increase in the hourly wage. In the remainder of Table 4 we look at several outcomes to distinguish these channels. We see that a search period does not have a significant effect on the number of weeks that an applicant has a non-zero wage, although the point estimate is positive. The number of hours worked is, however, significantly increased by the search period. Applicants that receive a search period work 64 hours more, an increase of 36 percent. This signals that a search period mainly increases the likelihood to find a full-time job compared to finding a part-time job in which the applicant does not earn enough to leave benefits. Finally, we estimate the effect on mean hourly wage (conditional on having a job).[13] A search period could reduce the quality of the job that individuals are willing to accept, in case they are liquidity constraint and quickly accept a job to have an income. On the other hand, receiving benefits can have a stigma effect on future employers by giving a bad signal about the quality of the employee. In that case the fact that a search period

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[13]We condition on having a job in order to abstract from the possible employment effect of a search period. If a search period has a positive effect on the employment probability including the zeros could wrongly lead to the conclusion that a search period leads to a better paid job. Conditioning on having a job does imply that there is a possible composition effect: If more people work with a search period also those with a lower wage potential work. This will, however, only bias the results for mean hourly wage downwards.

reduces the likelihood to receive benefits can translate into a positive effect on the mean hourly wage. The positive point estimate in Table 4 points more in the direction of the second explanation, although the effect is not significant.

Crime

A search period increases the time that an individual has to bridge before receiving the first welfare benefits payment. Applicants of welfare benefits are unlikely to have access to savings or credit that can cover such temporary cash shortfalls. Individuals with low earnings prospects in regular economic activities might, therefore, turn to crime to supplement their income during this period. For example, Foley (2011) finds that in the US crime rates increase in the amount of time that has passed since welfare payments occurred. Table 3 showed that of our sample nine percent was the suspect of a crime in 2012 and/or 2013 (compared to approximately two percent of the total population aged between 27 and 65) which indicates that for our population crime is not an irrelevant outcome.

Before turning to the results a few things need to be mentioned. First, by the nature of the data, we only consider registered crime, which is likely to be an underestimation of actual crime. There is, however, not a clear reason to suspect a difference in the likelihood to get caught for the different treatment groups. Second, we only know whether an individual was registered as a suspect of a crime, not whether she was actually convicted. However, in the Netherlands, on average, 90 percent of the registered suspects are declared guilty (CBS et al., 2013), such that this is a very strong indicator for actually having committed a crime. Third, we do not know the exact date of the crime, just the year in which the crime was registered. This means we cannot consider the exact time elapsed between the application for benefits and the crime. We take as an outcome whether an individual was suspected of a crime in the year 2012 and/or 2013 (remember that the experiment started in April 2012 and ended in March 2013). This means that the crime could have taken place before the search period was issued. Given the randomized design there is no reason to suspect a difference in crime rates before the start of the experiment.

We find no support for the claim that a search period increases crime rates. With a point estimate of -0.02 (s.e. 0.04), the 2SLS estimate of a search period on total crime is both

not significantly different from zero and has the wrong sign. Following Foley (2011), we also look only at property crimes, to separate out crimes that have a financial motivation. For these crimes the point estimate is -0.01 (s.e. 0.03). To check whether the effects are sensitive to the definition of our outcome measure we repeat the analysis by period of registration, and only for crimes committed in 2013. Persistently, we find no evidence of an effect of the search period on crime.

Long-run effects

So far we have mainly looked at outcomes up to half a year after registration. In Figure 5b we saw that 26 weeks after registration the effect of a search period became insignificant. A search period can still have long-term effects though. If a search period causes individuals to accept short-term low-wage jobs and individuals without a search period find more stable employment, then the long-term effect of a search period can potentially be negative. Table 5 presents the instrumental variable results for 26, 52 and 78 weeks after registration. For 78 weeks the sample is smaller because not everyone is observed for such a long time yet.

Table 5 shows that, although precision decreases due to larger standard errors, the point estimates of the effects are fairly stable over time. The main effect of a search period thus takes place during the first 26 weeks, and after that not much changes, neither in a positive nor in a negative way. The reductions in welfare benefits payments are, therefore, permanent savings, that are not offset by a later increase in benefit dependency.[14] One thing that sticks out is the positive and significant effect on mean hourly wage one year after registration, an increase of 21 percent compared to individuals without a search period. These results clearly oppose the idea that a search period induces applicants to accept lower quality jobs. This can be explained by a stigma effect of receiving welfare benefits on future employers. Another possible explanation is that if individuals can decide themselves about job applications they find a better match than when the welfare benefits agency assists in the job search process.

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[14]In order to know whether this also holds for the really long term we would have to repeat this analysis in the future. As our research sample is matched to the database of Statistics Netherlands this can be realized later.

Table 5: Instrumental variable effects search period on long term outcomes

	Nr of weeks since registration		
	26 weeks	52 weeks	78 weeks
	(1)	(2)	(3)
Nr of weeks I[benefits>0]	-4.78***	-5.87**	-8.40*
	(1.25)	(2.47)	(4.98)
Benefits received (in €)	-814***	-824	-971
	(287)	(536)	(1063)
Earnings (in €)	909**	965	765
	(449)	(971)	(2069)
Other benefits (in €)	-122	-7	542
	(199)	(390)	(856)
Total income (in €)	-27	133	337
	(438)	(938)	(2020)
Nr of weeks I[earnings>0]	1.48	0.42	0.10
	(1.23)	(2.39)	(4.82)
Cum. hours worked	64**	62	40
	(31)	(68)	(144)
Mean hourly wage (in €)	1.38	1.76**	1.72
(conditional on work)	(1.17)	(0.79)	(1.26)
Observations	2788	2788	1399
Included controls:			
Calendar time fixed effects	Yes	Yes	Yes
Local office fixed effects	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes

Note: Each cell represents one equation. The controls for applicant characteristics include age at registration, gender, household composition, cumulative income in 24 months before registration and dummies for five education categories. Standard errors are clustered at the level of the applicant.
*** =significant at 1% level, ** =at 5% level, * =at 10% level

6   Heterogeneous treatment effects

In this section we explore heterogeneity in the effect of a search period. First, we consider three important determinants of labor market outcomes: age, gender and education. Second, we study variation in the effect of a search period by looking at the income distribution. This is motivated by the concern that although a search period might, on average, have a positive effect on labor market outcomes, there can still be a group that is seriously harmed by a search period.
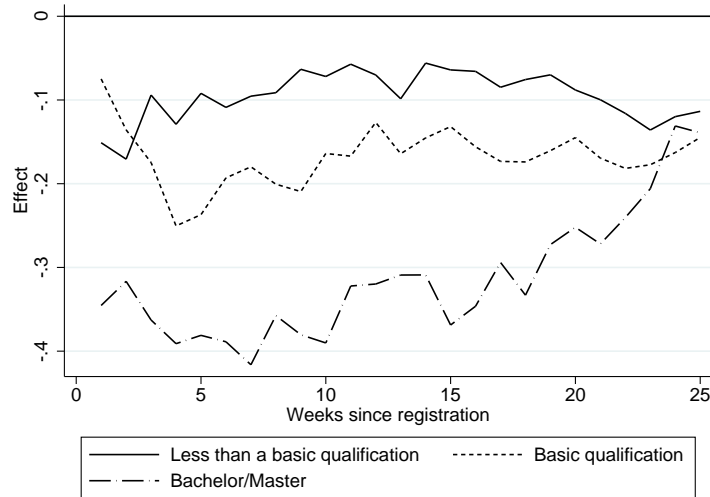
Figure 7: Effect on receiving welfare benefits by level of education

Gender, age and education

Figure 7 shows the effects on receiving welfare benefits for three different education groups. We distinguish between at least a bachelor degree, a basic qualification and less than a basic qualification.[15] The effects shown in the figure are estimated using a joint model for the three educational groups including interactions. For ease of presentation, the confidence intervals are not shown in the figure. The effects are significant for almost all weeks for applicants with at least a bachelor degree and those with a basic qualification. For the group with less than a basic qualification the point estimates are negative but the effect is not significant.

There are clear differences in the effectiveness of search periods between subgroups. The effect is largest for applicants with at least a bachelor degree. For this group a search period reduces the probability to receive welfare benefits by almost 40 percentage points, translating into a 50% reduction in the uptake of welfare benefits. The effect of a search period on the probability to receive welfare benefits monotonically declines with the level of education. The effect for the group with at least a bachelor degree is significantly different from the effect for applicants with less than a basic qualification. Table A3 in the appendix reports the effects on the other (cumulative) outcomes for the different education groups. For most outcomes the effect sizes monotonically increase with the level of education, and have the

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[15]A basic qualification is the government definition for the minimum level of education needed to be self-sufficient on the labor market. Such a qualification requires at least senior general secondary education, pre-university education, or level-2 of senior secondary vocational education.

same sign for all three groups. For the number of weeks on welfare benefits and total welfare benefits received the estimates for the group with at least a bachelor degree are significantly different from those for applicants with less than a basic qualification. Overall the results by education level suggest that a search period is most effective for individuals that have better prospects on the labor market, as reflected by their level of education.

Figure B2 in the appendix shows the effect of a search period on the probability to receive benefits split by gender (left) and above/below median age (right). In both cases the estimated coefficients are very similar. The lack of differential effects on these dimensions can be a result of the characteristics of the target population. Compared to the general population, welfare applicants are quite young (older workers have longer entitlement to UI benefits) and are less likely to have an (income-generating) partner. Furthermore, search periods are only applied to individuals for whom there are no direct restrictions to work. So, for example, single mothers with young dependent children are excluded from the target population.

Distributional impacts

In section 5 we found that the loss in benefits is completely (112 percent) compensated by an increase in earnings. However, given that the minimum wage for a full-time job (approximately 1200 euro per month) is substantially higher than the level of welfare benefits the earnings gain can be unequally distributed along the income distribution. If that is the case, a search period can still be harmful for part of the applicants. To inquire this further we estimate the marginal distribution of the outcome under different treatments for the subpopulation of compliers, following Imbens and Rubin (1997).[16] In our case the compliers are not the applicants but the caseworkers, that comply with their default option or not. This implies that we estimate the distribution of income for applicants that received a search period because their caseworker complied to the default 'normal' and 'always' and for applicants that did not receive a search period because their caseworker complied to the default 'never'. For sake of representation we take the default options 'always' and 'normal' together, thereby reducing the instrument to a binary instrument. The analysis

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[16]This method is briefly explained in appendix C.

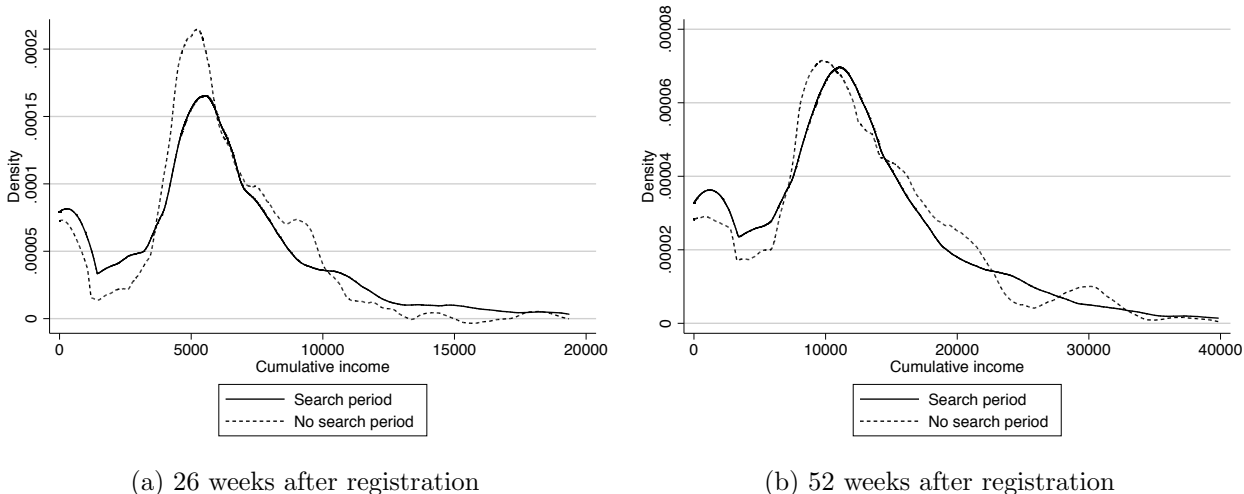(a) 26 weeks after registration   (b) 52 weeks after registration

Figure 8: Income distribution of compliers to the default options

could, however, be extended to consider both instruments separately.

Figure 8 plots the estimated distributions of cumulative income for applicants that did or did not receive a search period because their caseworker complied with the default options. Both 26 and 52 weeks after registration we see that the income distribution of the 'treated' compliers shifts slightly to the right. The treated compliers are, however, more likely to have a very low (close to zero) cumulative income. This suggests that for the majority of the applicants a search period has a positive effect on income because they find a job in which they earn more than the benefits level. However, for a fraction of the applicants a search period leads to a higher probability of having a very low income. Regression results (not reported) confirm that during the first ten weeks after registration a search period leads to a higher probability to have an income below 150 euro per week. This is below the welfare benefits level so after a search period some individuals neither have earnings nor receive benefits. After ten weeks this effect is no longer significant. This indicates that some individuals are harmed because of the search period as they have very little income for some weeks, up to two months after registration.

In the previous subsection we reported that the effect of a search period is particularly large for highly educated applicants. Figure 9, therefore, estimates the complier distributions split by level of education. We see that the higher probability of having very low income is mainly driven by the highly educated applicants (with a bachelor and/or master degree).

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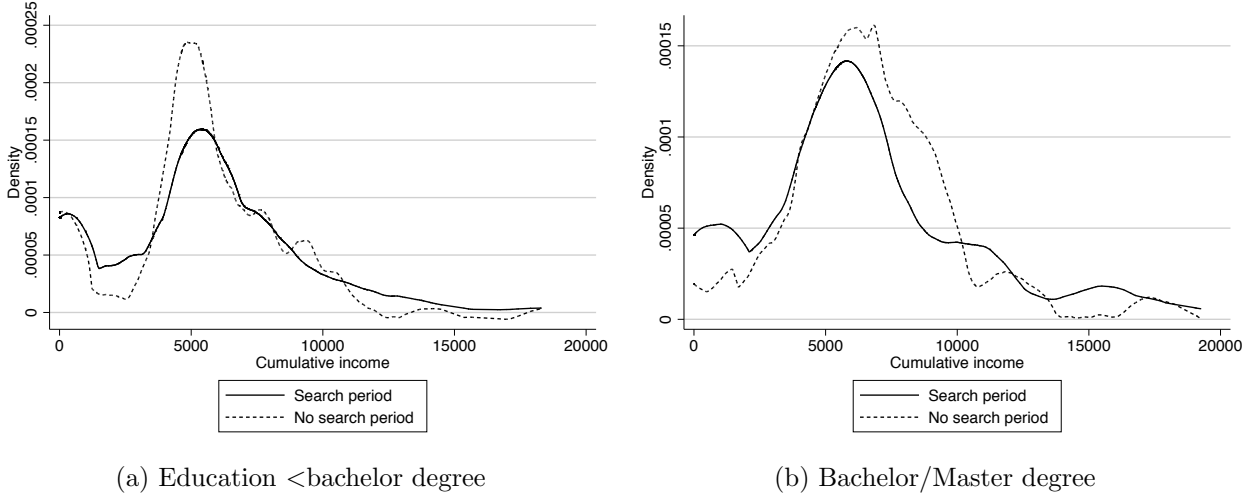(a) Education <bachelor degree   (b) Bachelor/Master degree

Figure 9: Income distribution (after 26 weeks) of compliers to the default options

It could be that there are two effective elements in the signal the search period sends out to applicants. The first element is that they are forced to actively look for employment during the search period to get welfare. The second element in the signal is that all jobs should be accepted, even if this is work far below the educational and experience level of the applicant. For highly educated individuals the second element could have more impact than for individuals with less education and even cause them to choose no income over welfare benefits.

7 Interpretation

Our estimated effect of a search period should be interpreted as a local average treatment effect. Recall that the caseworker decides about giving a search period to an applicant and that this decision depends on the default option assigned to the caseworker. In this section we provide some interpretation of the compliers for which we estimate the average treatment effect and test if the estimated effect changes when we consider other groups of compliers. The latter is informative about how well caseworkers can target search periods.

Suppose it is possible to rank applicants according to an (unobserved) index which we refer to as the propensity *not* to receive a search period. This propensity can be based on characteristics observed by the caseworker, but not by us. Applicants that are always given a search period have propensity zero and applicants with a propensity of one never

receive a search period. For ease of presentation we assume that these propensities are uniformly distributed. And for the moment we assume that there is no heterogeneity among caseworkers when executing the three default options. Then these default options can be translated into thresholds. Under the default option 'never' 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 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 the compliers. The default option 'normal' splits the compliers in two groups. First, applicants with a propensity between 0.09 and 0.46 who comply to both the default option 'normal' and 'always'. And second, applicants with a propensity between 0.46 and 0.55, who only comply to the default option 'always'.

If the effect of a search period is the same for all applicants, or if it only depends on applicant's characteristics that do not affect the propensity, the average treatment effects are the same for the compliers to the default options 'normal' and 'always' (Heckman and Vytlacil, 2001). However, there may be applicant-level heterogeneity in the response to the search period. For example, caseworkers may target search periods mainly to applicants for whom they expect the largest effects. In that case the effect is decreasing in the applicant's propensity and the estimated effect depends on the thresholds chosen by the caseworker. This relates to Heckman and Vytlacil (2001), who develop a framework in which they express treatment parameters as different weighted averages of the marginal treatment effects (MTE). In our case this means that the average treatment effect is smaller for the compliers to the default option 'always' than for the compliers to the default option 'normal'.

Nonparametric identification of the full set of MTEs requires an instrument that generates variation on the full support of the probability of treatment assignment. Our default options do not have this property.[17] However, we consider two alternative approaches to investigate if the effect of a search period declines in the applicant's propensity to receive a search period. First, we exploit that we have two default options deviating from 'never' giving a search period ('normal' and 'always'), which generate different groups of compliers. And second,

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[17] We could exploit differences in rates at which caseworkers assign search periods under the different default options (e.g. Maestas et al. (2013)). This yields much more variation, but in our case the average number of applicants is low so this analysis would incorporate a lot of noise.

Table 6: Comparing the two instruments, outcomes 26 weeks after registration

	Total sample (1)	P-value over-id (2)	Normal vs Never (3)	Always vs Normal (4)
Nr of weeks I[benefits>0]	-4.78***	0.88	-5.17***	-4.02
	(1.25)		(1.63)	(4.59)
Benefits received (in €)	-814***	0.75	-723*	-1063
	(287)		(371)	(1024)
Earnings (in €)	909**	0.83	979*	733
	(449)		(582)	(1650)
Other benefits (in €)	-122	0.61	-246	125
	(199)		(258)	(689)
Total income (in €)	-27	0.84	10	-204
	(438)		(562)	(1526)
Nr of weeks I[earnings>0]	1.48	0.72	1.10	3.19
	(1.23)		(1.60)	(4.51)
Cum. hours worked	64**	0.91	64	87
	(31)		(41)	(121)
Mean hourly wage (in €)	1.38	0.49	0.55	3.30
(conditional on work)	(1.17)		(1.27)	(3.30)
First stage coefficient default normal	0.34***		0.34***	
	(0.02)		(0.02)	
First stage coefficient default always	0.46***			0.12***
	(0.02)			(0.03)
Observations	2788		2217	2228
Included controls:				
Calendar time fixed effects	Yes		Yes	Yes
Local office fixed effects	Yes		Yes	Yes
Applicant characteristics	Yes		Yes	Yes

Note: Each cell represents one equation. The controls for applicant characteristics include age at registration, gender, household composition, cumulative income in 24 months before registration and dummies for five education categories. Standard errors are clustered at the level of the applicant.
*** =significant at 1% level, ** =at 5% level, * =at 10% level

we increase the group of compliers by only considering caseworkers with high compliance rates to the default options.

For the first approach we perform overidentification tests. In the LATE-framework with two valid instruments, rejection of the overidentification test indicates that treatment effects are heterogeneous (Angrist and Fernandez-Val, 2013). The intuition is that both instruments define different groups of compliers which may have different average treatment effects. The

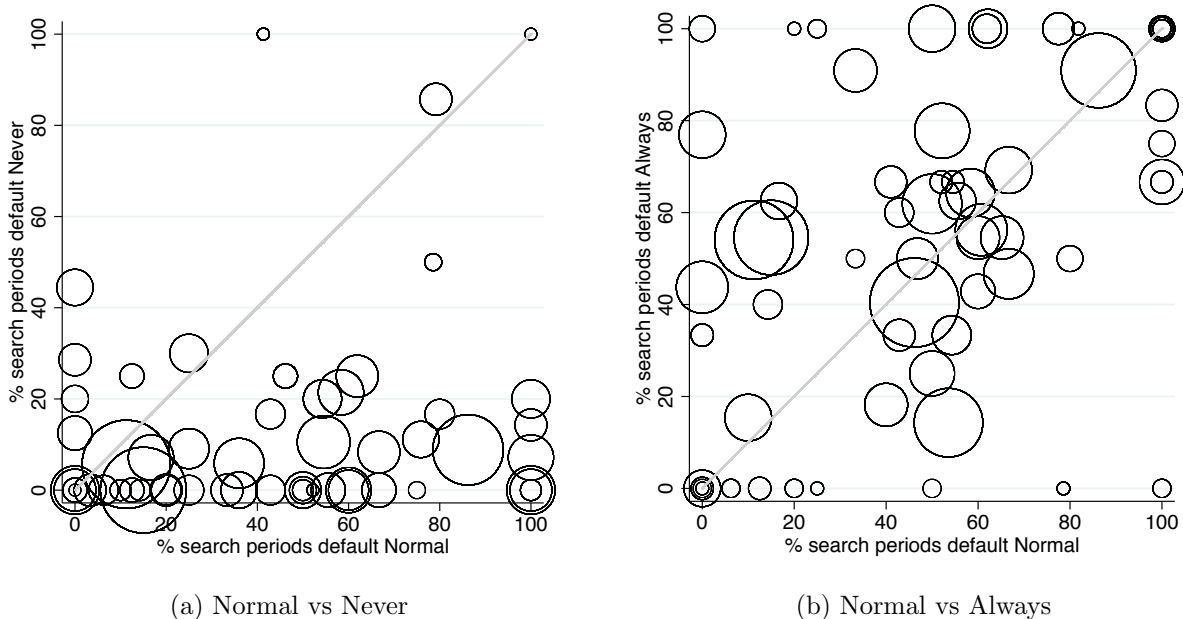(a) Normal vs Never   (b) Normal vs Always

Figure 10: Percentage of search periods applied by default option and caseworker

second column of Table 6 shows that the p-values for this overidentification test are for none of the outcomes below 0.49. This suggests either that both complier populations are very similar or that there is not much heterogeneity in the MTEs of search periods. Given that the default options 'always' and 'normal' yield complier populations which largely overlap, the first explanation is not unlikely.

Next, we estimate the effect of a search period using the following two strategies:

1. Instrument the search period with the default option 'normal' with as a reference the default option 'never'

2. Instrument the search period with the default option 'always' with as a reference the default option 'normal'

The first strategy captures the average treatment effect for applicants with a propensity between 0.09 and 0.46, while the second strategy captures the average treatment effect for applicants with a propensity between 0.46 and 0.55. For the first strategy, we exclude all applicants under the default option 'always', and for the second strategy we exclude all applicants under the default option 'never'. If caseworkers, indeed, mainly target search periods to applicants for which effects are highest, the estimated effects are smaller when

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estimated under the second strategy. Table 6 presents the results for the total sample (column (1)) and for the two strategies (columns (3) and (4)). The estimates in the third column are not very precise, due to less power in the first stage, but the point estimates are very similar to those in the second column. Again, there is no evidence that MTEs are decreasing in the propensity not to receive a search period.

This first approach exploits the difference in the rate at which search periods are applied under the default option 'normal' and 'always'. Recall that the rates are not very different, which affects the power of the analysis. Furthermore, it can be that MTEs are very different for those applicants with much higher propensities. Therefore, we consider the individual compliance of each caseworker. Figure 10a compares for each caseworkers the rate at which they give search periods 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 had under the default option 'never'.

Three things come to the attention. First, there is substantial variation in the rate at which caseworkers assign search periods under the default option 'normal'. Also in that case there are caseworkers who almost never apply a search period. Second, caseworkers who normally assign many search periods, substantially reduce this if they are assigned the default option 'never'. And third, there are a few caseworkers who do not change behavior when being assigned the default option 'never', i.e. under this default option they give as often search periods as under the default option 'normal'.

Figure 10b shows the same graph 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 apply search periods more often than usually. There are also some caseworkers who normally already apply search periods so often that this can hardly be increased when they are assigned the default option 'always'. Finally, there are some caseworkers who more or less refuse to give search periods. Even under the default option 'always' they (almost) never apply search periods.

Because within local offices applicants are randomly assigned to caseworkers, we can restrict our sample to caseworkers with substantial compliance rates to the default options without harming the randomization. This excludes caseworkers from the analysis that clearly

did not comply with the assigned default options.[18] The higher compliance rates increase the set of compliers, but it may also have some economic meaning when it is the case that caseworkers are more likely to listen to their managers than to us. In Table 7 we proceed in three steps and remove applicants of caseworkers who do not comply with the following rules:

1. Fraction 'never' <20% & fraction 'always' >40%

2. Fraction 'never' <10% & fraction 'always' >40%

3. Fraction 'never' <10% & fraction 'always' >60%

We only remove observations if a caseworker had more than five applicants in the relevant default options, such that we do 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 on applicant characteristics shows that there are no observable differences between applicants in the different groups (see Table A4 in the appendix). Only the indicator for the local welfare office is significant. This confirms that compliance to te experiment differed between local offices. Table 7 presents the estimated effects for the search periods for the different subsamples. At the bottom of the table we see that by removing non-complying caseworkers, the first-stage coefficients increase. In the most strictly defined sample (column 4) the estimates now cover propensities from 0.06 to 0.71 percent. Looking at the results, there is no evidence that the effect of a search period decreases with the propensity not to receive a search period. The effect on earnings even becomes slightly larger.

Overall both strategies do not provide evidence that the effect of a search period decreases in the propensity not to receive a search period. It also shows that the local average treatment effect are not sensitive to changes in the sample and the group of compliers. This shows that caseworkers do not manage to target search period to those applicants for which the effects are largest.

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[18]Restricting the sample implies that we only consider applicants assigned to a restricted number of caseworkers.

Table 7: Outcomes for different groups of compliers

	Total sample (1)	Nev.<20% Alw.>40% (2)	Nev.<10% Alw.>40% (3)	Nev.<10% Alw.>60% (4)
Nr of weeks I[benefits>0]	-4.78***	-4.28***	-4.66***	-5.72***
	(1.25)	(1.21)	(1.26)	(1.55)
Benefits received (in €)	-814***	-647**	-749***	-813**
	(287)	(275)	(285)	(326)
Earnings (in €)	909**	-832*	912**	1386***
	(449)	(429)	(440)	(518)
Other benefits (in €)	-122	-127	-86	-83
	(199)	(184)	(199)	(239)
Total income (in €)	-27	58	77	490
	(438)	(422)	(434)	(511)
Nr of weeks I[wage>0]	1.48	1.60	1.92	2.99**
	(1.23)	(1.17)	(1.23)	(1.39)
Cum. hours worked	64**	54*	58*	83**
	(31)	(30)	(31)	(37)
Mean hourly wage (in €)	1.38	1.63	0.81	1.05
(conditional on work)	(1.17)	(1.19)	(0.97)	(1.08)
First stage default normal	0.34***	0.37***	0.37***	0.39***
	(0.02)	(0.02)	(0.02)	(0.03)
First stage default always	0.46***	0.55***	0.56***	0.63***
	(0.02)	(0.03)	(0.03)	(0.03)
Observations	2788	2373	2207	1603
Included controls:				
Calendar time fixed effects	Yes	Yes	Yes	Yes
Local office fixed effects	Yes	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes	Yes

Note: Each cell represents one equation. The controls for applicant characteristics include age at registration, gender, household composition, cumulative income in 24 months before registration and dummies for five education categories. Standard errors are clustered at the level of the applicant.
*** =significant at 1% level, ** =at 5% level, * =at 10% level

8   Conclusion

This paper studies mandatory search periods for new applicants for welfare benefits. During such a four-week search period applicants have to very actively search for a job, and their first payment of benefits is postponed. We designed a field experiment based on an encouragement design to investigate both the short and long-run effects of applying these search periods.

For the empirical evaluation we supplement data from the experiment with various sources of administrative data, to determine the applicants' possible alternative sources of income.

Our empirical results provide evidence for a strong and persistent effect of a 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 lower income from welfare benefits is fully compensated (112 percent) by increased income from employment and there are no spillovers to other benefit schemes. A search period does not increase the likelihood to engage in crime. The effect of a search period increases with education, for applicants with at least a bachelor degree the likelihood to collect welfare benefits decreases with 50 percent. A search period increases the probability that high-educated applicants have a very low income. We do not find evidence for negative side effects for the most vulnerable applicants. Finally, we show that the estimated effect is not very sensitive to changes in the group of compliers. Taken together, our results suggest that a search period is an effective way to target welfare benefits to those people who need it most.

This study shows that a combination of stricter job search requirements and increased complexity of the application process reduces the take up of welfare benefits. Since the administrative costs of imposing a search period are minimal, it is also a very cost effective policy instrument. Furthermore, we show that with our encouragement design which allows for opting out in special cases, it is possible to evaluate (existing) policies using a randomized experiment for recipients of welfare benefits. The opt-out possibility has been important to obtain support of caseworkers, which ensured sufficient compliance to our randomization.

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A Additional tables

	Housing costs	
	Full	Shared
Single without children	935.80	802.12
Single with children	1203.19	1069.50
Couple without children	1336.87	1203.19
Couple with children	1336.87	1203.19

Note: 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 the target population

	Mean
Financial situation	
Good	26%
Average	48%
Bad	26%
Estimated level of independence (by the caseworker) of the applicant	
Very independent	40%
Somewhat independent	54%
Completely not independent	6%
Estimated time (by the caseworker) till exit to employment	
within 1 month	13%
2nd or 3rd month	39%
4th or 5th month	31%
6th month or later	17%
Reason application for welfare	
Lost job	16%
End self-employment	8%
End UI benefits	34%
Other reason	42%
Other characteristics	
Non-western immigrant	56%
Ever received welfare before start of experiment	47%

Note: The information in the first four panels of this table is taken from the forms that caseworkers filled in for the applicants that were part of the experiment. These forms were completed for 72 percent of the sample. The information in the lowest panel is for the complete sample and was derived by linking the experimental sample to administrative data of Statistics Netherlands. The degree of independence indicates whether the applicant is self-reliant and is able to independently search for work.

Table A3: Treatment effects by level of education 26 weeks after registration

	Education level		
	Lower	Middle	High
	(1)	(2)	(3)
Nr of weeks I[benefits>0]	-2.49	-4.32**	-7.94***
	(2.15)	(1.98)	(2.16)
Benefits received (in €)	-277	-838*	-1442***
	(483)	(470)	(488)
Earnings (in €)	562	1113	1060
	(633)	(687)	(953)
Other benefits (in €)	-283	-24	-66
	(366)	(298)	(358)
Cum. total income (in €)	1	251	-447
	(617)	(673)	(943)
Nr of weeks I[earnings>0]	0.57	2.19	1.61
	(2.12)	(1.79)	(2.10)
Cum. hours worked	36	101**	46
	(53)	(46)	(58)
Mean hourly wage (in €)	-0.69	1.79	3.03
(conditional on work)	(1.80)	(1.50)	(2.14)
Observations	1011	1007	770
Included controls:			
Calendar time fixed effects	Yes	Yes	Yes
Local office fixed effects	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes

Note: Each row in this table represents one regression including interactions for the different subgroups. The controls for applicant characteristics include age at registration, gender, household composition, cumulative income 24 months before registration and dummies for five education categories. Standard errors are clustered at the level of the applicant.

*** =significant at 1% level, ** =at 5% level, * =at 10% level

Table A4: Characteristics of different groups of compliers

	Nev.<20% Alw.>40% (1)	Nev.<10% Alw.>40% (2)	Nev.<10% Alw.>60% (3)
Applicant characteristics			
Female	-0.01	-0.01	-0.00
	(0.01)	(0.02)	(0.02)
Partner	0.02	-0.01	-0.02
	(0.02)	(0.03)	(0.03)
Children	0.02	0.02	0.01
	(0.02)	(0.02)	(0.03)
Age 31 - 36	-0.02	-0.03	-0.01
	(0.02)	(0.02)	(0.03)
Age 37 - 45	-0.01	-0.00	0.02
	(0.02)	(0.02)	(0.03)
Age above 45	0.03	0.04**	0.05*
	(0.02)	(0.02)	(0.03)
Bachelor/Master	-0.04*	-0.01	-0.01
	(0.02)	(0.03)	(0.03)
Vocational	-0.03	-0.00	-0.01
	(0.02)	(0.03)	(0.03)
High school	0.02	0.04	-0.01
	(0.02)	(0.03)	(0.04)
Prep. vocational	0.00	0.03	0.00
	(0.02)	(0.03)	(0.03)
Education missing	-0.01	-0.01	-0.00
	(0.01)	(0.02)	(0.02)
Income 2 yrs before (x1000E)	0.00*	0.00	0.00
	(0.00)	(0.00)	(0.00)
Location of registration			
Southeast	0.08***	0.25***	-0.32***
	(0.02)	(0.02)	(0.03)
North	0.03	0.12***	0.19***
	(0.02)	(0.03)	(0.03)
Centrum/East	0.02	0.19***	0.11***
	(0.02)	(0.02)	(0.03)
New West	-0.01	0.17***	0.02
	(0.02)	(0.03)	(0.03)
Quarter of registration			
Quarter 2	0.02	-0.00	-0.03
	(0.02)	(0.02)	(0.03)
Quarter 3	-0.01	-0.02	-0.05**
	(0.02)	(0.02)	(0.03)
Quarter 4	-0.00	0.01	0.02
	(0.02)	(0.02)	(0.03)

Note: *** =significant at 1% level, ** =at 5% level, * =at 10% level
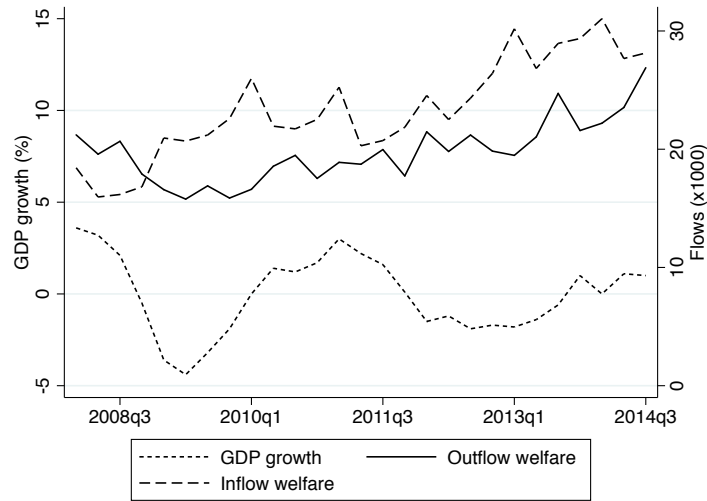
B   Additional figures

Figure B1: In- and outflow of welfare from 2011-2014 (source: Statistics Netherlands)
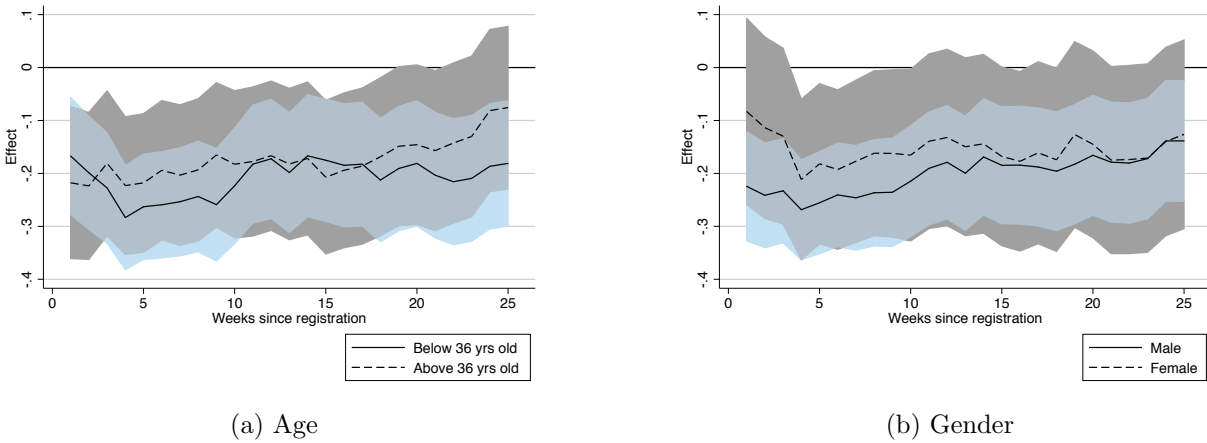

(a) Age

(b) Gender

Figure B2: IV estimates of the effects of a search period by age and gender $t - \tau$ weeks after registration (colored areas are 90 percent confidence intervals)

C Estimation of outcome distributions for compliers

Imbens and Rubin (1997) show how to estimate the marginal distribution of the outcome under different treatments for the subpopulation of compliers. In this section we briefly review their approach.[19] $Y_i(1)$ and $Y_i(0)$ will denote the applicant's total income with and without a search period, respectively. D_i denotes the default option of the caseworker to whom the applicant is matched, where $D_i = 1$ indicates the defaults 'always' or 'normal' and $D_i = 0$ the default 'never'. SP_i indicates whether the applicant had a search period (0/1).

For each observation we observe the triple (D_i, SP_i, Y_i). From the data we cannot directly identify the compliers, but we can identify some always takers ($D_i = 0$ and $SP_i = 1$) and some never takers ($D_i = 1$ and $SP_i = 0$). Because of the randomization, the value of the default option will be independent of the applicant's type, so in a large sample we can infer the distribution of $Y_i(0)$ for never takers and $Y_i(1)$ for always takers. These distributions are denoted by $g_a(y)$ and $g_n(y)$. The population proportions of compliers (ϕ_c), always takers (ϕ_a) and never takers (ϕ_n) can be identified from the data.

The distributions of interest are the distributions of $Y_i(0)$ and $Y_i(1)$ for compliers, described as $g_{c0}(y)$ and $g_{c1}(y)$. These cannot be directly observed from the data because the outcome distribution of applicants for whom $D_i = 0$ and $SP_i = 0$ consists both of never takers and compliers. Correspondingly, in the outcome distribution of applicants with $D_i = 1$ and $SP_i = 1$ there will be both always takers and compliers.

We denote the directly estimable distributions of Y_i for the subsample defined by $D_i = d$ and $SP_i = sp$ as $f_{d,sp}(y)$. This implies that $g_a(y) = f_{01}(y)$ and $g_n(y) = f_{10}(y)$. Imbens and Rubin (1997) show that the distributions for applicants that did or did not receive a search period because their caseworker complied to the default options can be expressed in terms of the directly estimable distributions in the following way:

$$g_{c0}(y) = \frac{\phi_n + \phi_c}{\phi_c} f_{00}(y) - \frac{\phi_n}{\phi_c} f_{10}(y), \tag{4}$$

and,

$$g_{c0}(y) = \frac{\phi_a + \phi_c}{\phi_c} f_{11}(y) - \frac{\phi_a}{\phi_c} f_{01}(y). \tag{5}$$

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[19]The notation in this section is partly taken from Ketel et al. (2015).