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Abstract

To evaluate search effort monitoring of unemployed workers, it is important to take account of post-unemployment wages and job-to-job mobility. We structurally estimate a job search model with endogenous job search effort by the unemployed along various search channels that deals with this. The data are from an experiment in the Netherlands in which the extent of monitoring is randomized. They include registers of post-unemployment outcomes like wages and job mobility, and survey data on measures of search behavior. As such we are the first to study monitoring effects on post-unemployment outcomes. Once employed, individuals have the opportunity to further improve their position by moving to better-paid jobs, and we find that this reduces the extent to which monitoring induces substitution towards formal search channels in unemployment. In general, job mobility compensates for adverse long-run effects of monitoring on wages. We use the structural estimates to compare monitoring to counterfactual policies against moral hazard, like re-employment bonuses and changes in the unemployment benefits path. Replacing monitoring by an over-all benefits reduction in a way that is neutral to the worker results in slightly smaller effects with lower administrative costs.

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1 Introduction

Generous unemployment benefits schemes are potentially subject to moral hazard: unemployed workers reduce search effort and increase their reservation wage. This may reduce their transition rate to work. Policy makers have become interested in approaches to counteract this by using alternative policy measures (e.g., OECD, 2007). The monitoring of job search behavior (including the threat of punitive benefit reductions) and the payment of re-employment bonuses are examples of such policy measures. These measures, as well as the simple policy device of lowering unemployment benefits, have the disadvantage that they tend to reduce the quality of the post-unemployment jobs (e.g. Acemoglu and Shimer, 2000). The relative merits of the various policy measures can only be assessed empirically. In the absence of a large range of randomized controlled trials, such an assessment involves counterfactual evaluations.

This paper provides a structural analysis based on a randomized social experiment of a monitoring scheme for unemployed workers. Structural analysis aims at uncovering policy-invariant parameters characterizing individual preferences and boundary conditions resulting from labor market imperfections and policy constraints. Our data include post-unemployment outcomes like wages and job durations. The observation of such outcomes is important because it enables us to address the extent to which policies against moral hazard have detrimental effects on them. Moreover, it enables us to address the importance of two tools that the individual has at his disposal to mitigate the reduction of his expected present value that these policies cause. First, the individual may substitute search effort away from the search channels that are not monitored to the channel that is monitored. Channel substitution may be beneficial from the individual point of view and it may help to prevent a low-quality job match, but it tends to reinforce the moral hazard problem. The second tool is on-the-job search. Job-to-job transitions reduce the importance of the first job accepted after unemployment. A low starting wage can be mitigated by subsequent wage gains. On-the-job search therefore reduces adverse post-unemployment effects of policies aimed at fighting moral hazard, while at the same time it does not stimulate moral hazard during unemployment. The structural model we develop is consistent with the differences in behavior and labor market outcomes between the treatment and control group observed in the data.

By structurally estimating a model that distinguishes between different search channels and that allows for job-to-job mobility, we can quantify the relative importance of the two above-mentioned mechanisms. Moreover, we may study their interaction. We show that the extent of channel substitution depends on job mobility. If it is easy to move to better jobs while in employment then channel substitution is less strong than otherwise. This is because with a high job mobility, any job with a low wage can be exchanged quickly

for a better job, so the attractiveness of finding work using all available channels is high. On the other hand, a large option value of job mobility may decrease the incentive to spend large amounts of search costs by the unemployed. If a high job mobility goes along with a high search effort by the unemployed, then, with high mobility, the imposition of a minimum threshold for the monitored search effort level will more often not be binding. All this suggests more in general that it is relevant to take post-unemployment choices into account when evaluating the policy measures of the employment office. In the policy evaluation literature, the role of subsequent job mobility has typically been ignored.

The data concern a sample of relatively skilled individuals from the Netherlands. A subset of the variables in the data has been analyzed in Van den Berg and Van der Klaauw (2006). That study consists of a reduced-form analysis of the average treatment effect of the monitoring program on unemployment durations. It did address the issue of channel substitution, but the study did not have access to post-unemployment outcomes. The reduced-form results do not provide evidence for a strong effect of monitoring on the unemployment duration, but they do indicate that channel substitution takes place. Reduced-form studies cannot extrapolate such results to individuals in different circumstances. Structural analysis is more amenable to this. In our present paper, we use the estimation results to address the size of the effects under less favorable conditions than in the sampling period. In addition, we exploit the advantage of structural analysis that it enables counterfactual policy analysis (see e.g. Eckstein and Van den Berg, 2007, for a more general discussion of the advantages of structural analysis, with a focus on unemployment outcomes).

Our study complements the empirical literature on the effectiveness of monitoring unemployed workers (see Johnson and Klepinger, 1994; Dolton and O'Neill, 1996; Gorter and Kalb, 1996; Klepinger, Johnson and Joesch, 2002; Van den Berg and Van der Klaauw, 2006; Ashenfelter, Ashmore and Deschênes, 2005; McVicar, 2008; Manning, 2009; and Micklewright and Nagy, 2010). All these studies provide reduced-form analyses, and many are based on randomized social experiments. The evidence is surveyed in Van den Berg and Van der Klaauw (2006). In general, the effect of monitoring is stronger if labor market conditions and job prospects are worse.

Evaluation studies on post-unemployment effects of "treatments" during unemployment include Ham and LaLonde (1996), who examine training programs, Dolton and O'Neill (2002), who examine a counseling and monitoring program, and Van den Berg and Vikström (2014) who examine punitive sanctions for individuals who do not comply with monitoring guidelines. The latter study finds that sanctions on average lead to significantly lower accepted daily wages. Finally, there is an expanding branch of literature that uses structural models to evaluate active labor market programs (Adda, Costa Dias, Meghir and Sianesi, 2009; Cockx, Dejemeppe, Launov and Van den Linden, 2011; Fougère, Pradel and Roger, 2009; Gautier, Muller, Van der Klaauw, Rosholm and Svarer, 2012; Lise,

Seitz and Smith, 2005; Wunsch, 2013).

Section 2 provides institutional details for the policy that is evaluated in the social experiment. It also describes the experiment itself. Section 3 summarizes our data. These are from a range of registers as well as from a survey among the participants in the experiment. In Section 4 we develop and analyze the theoretical job search model with multiple job search channels and job-to-job mobility. We discuss identification of the structural model and we derive the likelihood function. Section 5 presents the parameter estimates, the evaluation of counterfactual policies, and the effects in different labor market settings. We describe a range of sensitivity analyses in Section 6. Section 7 concludes.

2 Unemployment insurance and monitoring policies and the randomized social experiment

2.1 Unemployment insurance

In this subsection we briefly describe the Dutch unemployment insurance (UI) system in the late 1990s, which includes our observation period. If a worker younger than 65 years becomes unemployed, she is entitled to UI benefits (provided that some conditions of her employment history are fulfilled). The entitlement period and level of benefits are determined by the worker's labor market history. Usually, the initial level of benefits equals 70 percent of the wage in the job previous to unemployment with a maximum of 138.84 euro gross per working day.[1] The minimum wage equals 49.12 euro gross per working day. The exact duration of the entitlement period for initial benefits lies between six months and five years (depending on the worker's employment history). After the initial entitlement to benefits expires, the unemployed worker receives extended benefits which cannot exceed 70 percent of the minimum wage.

According to the Unemployment Law, an unemployed worker has the following obligations in order to be entitled to UI benefits: (*i*) prevent unnecessary job loss, (*ii*) take actions to prevent from staying unemployed, so she has to search for a job and accept appropriate job offers, register as a job searcher at the public employment office, participate in education and training, etc., and (*iii*) keep the local UI agency informed about everything that is relevant to the payment of the UI benefits. If an unemployed worker does not comply to these rules, the local UI agency is authorized (not obliged) to apply a sanction to that worker. In general, although the local UI agencies are mainly responsible

[1]Some individuals are only entitled to "short-term" benefits, which is at most 70 percent of the minimum wage for a six months period, but these are not subject to the monitoring regimes we consider and are not in our data.

for paying UI benefits, they also provide training and schooling. The public employment offices act as matching agents, not only to UI recipients, but also to welfare recipients and employed workers searching for (new) jobs.

2.2 Job search monitoring

The monitoring program that we consider is a nationwide policy since April 1998. The program was targeted towards UI recipients with low expected unemployment durations, during their first six months of unemployment.

At the *intake meeting* of UI, any individual is classified ("profiled") into one of four "types", based on individual characteristics such as work experience, age and education, and on some subjective measures such as expected job search behavior, flexibility, language skills and presentation skills. Only those who are expected to have sufficient skills to find a job (Type I) are exposed to the monitoring scheme we consider, with the additional restriction that the UI eligibility period exceeds six months. In the inflow of unemployed workers into UI, 80 percent is classified as Type I, whereas in the stock of UI recipients, about 60 percent is classified as Type I. Excluded from the policy are individuals who know at the date of UI registration that they will start a new job within three weeks and Type I unemployed workers collecting short-period benefits. Exposure to the monitoring scheme lasts half a year. During this period the unemployed workers have a meeting at the local UI agency every four weeks.

The *intake meeting* takes place within three days after the start of the payment of the UI benefits. The quality of application letters and the resume are examined, the different channels through which work can be found are discussed and a plan is made about what the individual should do until the next meeting. Although the local UI agency can inform the unemployed worker about possible job entries, it is not allowed to act as an intermediary between unemployed workers and firms. Offering or pointing out specific vacancies to unemployed workers is the task of the public employment offices. During the intake meeting it is emphasized that a positive and active attitude toward job search is warranted.

The *follow up meetings* focus on efforts to apply to specific job vacancies and employers. During these meetings the plans of the previous meeting are evaluated and a planning for the next period is made. If the unemployed worker did not comply with the plan, she may be punished with a sanction in the form of a reduction of the UI benefits. The average sanction for insufficient job search is a ten percent reduction of the UI benefits for a period of two months. In addition, reports on search activities have to be sent in every week. Below, when we refer to "the" monitoring scheme or policy, we refer to the monitoring that goes beyond the processing of these handwritten weekly reports. This is because in the experiment, both treated and controls were obliged to submit these reports.

The monitoring scheme is inexpensive. The Dutch National Institute for Social Security pays the local UI agencies on average 152.46 euro per individual for monitoring. This is paid at the beginning of UI entitlement period and does not depend on the realized unemployment duration. Each meeting includes a check on whether the unemployed worker is still eligible for UI benefits. Performing this check would otherwise cost on average 17.52 euro. So those costs should be excluded from the costs of the scheme, for each month that an individual collects UI benefits.[2]

2.3 The experiment

The experiment concerns all Type I unemployed workers who started collecting UI benefits between August 24 and December 2, 1998 at the local agencies in two of the largest cities of one nationwide UI agency. In the remainder we refer to these as City 1 and City 2. The inflow into UI at these local agencies is relatively large, and the agencies have a high reputation for carrying out monitoring activities in a highly orderly fashion. Both facts have played a role in the selection of these local agencies as venues for the experiment. All individuals who satisfy the policy eligibility criteria as listed in the previous subsection are included in the experiment.

During the UI intake meeting, the employee of the local UI agency establishes whether or not a UI recipient is eligible for monitoring. In case of eligibility, the unique ID-number of the unemployed individual is instantly electronically transmitted to an independent computer center. Th is center does not know anything about the unemployed individual on top of the ID-number. The center then decides based on a series of random numbers, which were realized by a random generator in SPSS before the start of the experiment, whether the unemployed individual is assigned to the treatment group or the control group. Notice that this is equivalent to a procedure where randomization takes place within the local agencies in both cities. Here, "treatment" refers to the exposure to the actually implemented monitoring policy, whereas "control" refers to the absence of this exposure. Individuals selected in the treatment group have to show up at a monitoring intake meeting within three days, and subsequently in the follow-up meetings which take place every four weeks. The unemployed workers in the control group only communicate with the local UI agency by way of sending in written forms stating the current status of their job search activities.

After the first six months of collecting UI benefits, the monitoring ends for individuals in the treatment group. All individuals who are still unemployed after six months thus end up in the same regime which may involve alternative active labor market policies.

[2]The figures mentioned here are average realized amounts. The amounts may vary between individuals and local UI agencies.

The participants in the experiment are not informed about the occurrence of the experiment. In the absence of the experiment, all of them would be subject to the monitoring treatment. None of the individuals in the control group in the experiment complained about a lack of monitoring. The setup ensures that the data do not suffer from initial nonrandom nonparticipation/noncompliance in the experiment and participants can not leave the experiment for any reason other than stopping collecting UI benefits.

3 Data

3.1 Merged data registers and survey data

We collected information on the 393 individuals who participated in the experiment, i.e. who started to collect UI benefits between August 24 and December 2, 1998 in the two experiment cities. During the observation period, unemployment rates were low and the labor market was tight.

Our data set merges three different sources of information on the sample members. The first source is the register of UI recipients for the period 1998–2004. The second source is the wage and job duration register, for the same period. The third source is a survey on search effort and search behavior, held in March 1999 among the sample members.

All information on events is daily, i.e. we observe the exact day of inflow into and outflow out of UI.[3] We right-censor all observations after 26 weeks of collecting UI benefits, because an individual enters a new regime of active labor market programs after being unemployed for 26 weeks (see discussion above). Among the treated, 38.5 percent of the unemployment spells are censored, while among the controls this is 39.4 percent. Censoring can also occur if the exit destination differs from employment (illness, prison, not accepting suitable work, leaving the country). There is no systematic difference in how often these other exits occur in the treatment and in the control group. With register data, the empirical analyses do not suffer from selective nonresponse or follow-up attrition from the database.

If an individual finds work within 26 weeks, we record a number of subsequent labor market outcomes: the gross wage in the first job, the length of the first job spell, the destination state after the first job spell, and the gross wage in the second job (if the destination state after the first job spell was work). Wages are deflated to obtain real weekly wages (measured in January 1999 euros). The gross minimum weekly wage is 245 euros. In our data about 5.8 percent of the observed wages are below the minimum wage

[3]As mentioned in Subsection 2.1, UI recipients are not all full-time unemployed; e.g. they may have lost only part of their working hours and still work for the remaining hours. We simply refer to the period from the start of collecting UI benefits until the end of collecting UI benefits as unemployment, and to UI recipients as unemployed workers.

and 44 percent below the observed benefits level. Among individuals with more than one observed wage, the second wage is below the first wage in 32 percent of the cases.

The response rate to the survey questionnaire was 33 percent. We manually match survey respondents to individuals in the administrative database. To match records, we use information on the month of birth, the city of residence, gender, treatment status, having collected UI benefits before, current labor market status, and day of starting collecting UI benefits. Due to item nonresponse on these variables, we only succeeded in matching 49 individuals in the treatment group and 55 individuals in the control group. Van den Berg and Van der Klaauw (2006) investigate if there was selective nonresponse and concluded that this was not the case. In the estimation of the structural model all 393 individuals in the administrative data are used.

The survey includes questions on how unemployed workers evaluate monitoring and on which job search channels they have used, in addition to subjective evaluations of satisfaction with aspects of the benefits and re-employment system. Van den Berg and Van der Klaauw (2006), using the answers to the survey, demonstrate that the unemployed workers experience monitoring as controlling rather than advisory. This is important, as the occurrence of follow-up meetings in the treatment arm suggests that treatment may include counseling as well.

The main information we take from the survey concerns the use of job search channels. Individuals were asked to report from a list of possible job search methods which methods they had actually used during their spell of collecting UI benefits. We define the *formal* search channel as the channel in which personnel advertisements at the public employment offices / local UI agency or (commercial) employment agencies are used as methods. The *informal* search channel uses open application letters, referrals by employed workers, and search through friends and relatives, as search methods. The formal channel is monitored and the informal channel is not. We keep job advertisements in newspapers out of the analyses as in our opinion this can be formal as well as informal search – it is not clear how the unemployed worker became acquainted to the advertisement. In fact, almost all unemployed workers indicate that they examined job advertisements in newspapers.

3.2 Summary statistics

In Table 1 we provide summary statistics for the variables we use in our empirical analyses. There are slightly more individuals in the treatment group than in the control group, 205 individuals received monitoring while 188 were excluded from monitoring. Because the registers only contain variables that are needed by the UI agency, the number of variables in this database is limited. For example we do not have any information on occupation and level of education. The differences in gender, the city in which they live, age, level

Table 1: Summary statistics.

	Treatment group		Control group		p-value for equality
Number of individuals	205		188		
Percentage female	38.5%		41.0%		0.625
Percentage living in City 2	62.0%		59.0%		0.557
Average age	35.8		36.7		0.312
Percentage collected UI before	22.4%		26.6%		0.341
Average UI benefits	384		381		0.819
Measure of formal search channels	0.79	(0.12)	0.52	(0.12)	0.114
Measure of informal search channels	0.79	(0.15)	1.00	(0.14)	0.322
Hazard to first job	0.0431	(0.0038)	0.0418	(0.0038)	0.814
Wage in first job	413	(14)	424	(17)	0.621
Job separation hazard	0.0023	(0.0004)	0.0024	(0.0004)	0.864
Job-to-job hazard	0.0088	(0.0006)	0.0068	(0.0006)	0.026
Wage in second job	460	(25)	434	(23)	0.440

Explanatory notes: Wages and benefits are before taxes and measured in euros on January 1, 1999. Time unit is one week. Hazards are estimated transition rates for exponential duration distributions without covariates. Standard errors of the averages and hazards are in parentheses.

of benefits and whether the collected UI benefits before are relatively small between both groups and never significant. Van den Berg and Van der Klaauw (2006) provide some more extensive checks for the random assignment of treatment and conclude that randomization holds. Finally, we observe if the local UI agency imposed a sanction on the UI recipient. We do not have any information on the reason why the sanction was imposed or the size and the duration of the benefit reduction. In the database, the percentage of individuals who had a sanction imposed was less than three percent among the treated as well as among the controls.[4]

Monitored individuals use on average more formal and fewer informal search methods than the unemployed workers in the control group. They also have a slightly higher reemployment rate, but the wage in their first job is on average slightly lower. These differences are not significant in the raw data. The job separation fractions in the first job are the same for both groups, but individuals in the treatment group are significantly more likely

[4]Sanctions can also be imposed for other reasons than lack of job search effort, for example because of not complying to administrative obligations regarding the provision of information on eligibility status.

Table 2: OLS regression for logarithm of number of applications in the past month.

Intercept	1.303	(0.164)
Measure of formal search channels	0.218	(0.120)
Measure of informal search channels	0.157	(0.107)

Explanatory note: standard errors in parentheses.

to make a job-to-job transition. Furthermore, their wage in the second job is higher. The correlation between the wage in the first and second job is 0.79 and is similar in the treatment and control group. Even though the differences between the treatment and control group are often not significant, the direction of the difference is always consistent with the search model that will be presented in the next section.

Search channel usage will be our measure for search intensity in the empirical analyses. One could argue that the number of search channels used is too crude a measure for search intensity. We look into this by examining the total number of job applications made in the past month as reported by those who were still unemployed at the interview date. It should be noted that this question was only answered by 46 individuals (excluding one person claiming to have made 400 applications in the past month). On average the individuals in our sample make 6.6 applications per months with a maximum of 40. In Table 2 we regress the logarithm of the number of applications on the number of formal and informal search methods used by the unemployed worker. Use of an additional formal search method is associated with an increase in the number of applications by 22 percent, with a p-value of about 0.07, while monthly job applications are 16 percent higher if the unemployed worker uses an additional informal search method. Jointly, the channels have a significant impact at the 10 percent level.

4 Structural analysis

4.1 Job search model with search channels, search effort, monitoring, and job mobility

Our structural job search model generalizes the model used in Van den Berg and Van der Klaauw (2006). Their model is used to interpret the estimation results from their reduced-form analysis. The model is a sequential job search model with endogenous search effort (e.g. Mortensen, 1986), extended by allowing job offers to arrive through formal as well as informal search channels, and by allowing for job search monitoring. In the present paper, we further explicitly incorporate job-to-job mobility. Our model shares some features with

the model framework of Pavoni and Violante (2007). We consider a general class of search cost functions.

Consider an unemployed worker searching for a job. This individual can search along the formal and the informal channel, which are denoted by subscripts 1 and 2, respectively. An amount of search effort $s_i \geq 0$ is devoted to search along channel i. This variable s_i, which is also called the search intensity for channel i, is to be chosen optimally by the unemployed worker. Job offers along search channel i arrive at the individual according to a Poisson process with rate $\lambda_i s_i$.

A job offer is characterized by a random draw from the wage offer distribution F. Arrival times and wage offers are independent across channels, and given the channel they are independent across time. We assume that F is continuous with a connected support stretching to infinity, on which the density is positive. If a job offer arrives, the individual has to decide immediately whether to accept it or to reject it and continue searching.

The costs of search are expressed by the function $c(s_1, s_2)$. We require c to be increasing and convex in its arguments, with $c(0,0) = 0$. Moreover, we require $\partial^2 c/(\partial s_1 \partial s_2) \geq 0$ for $s_1, s_2 > 0$, to capture that efforts along the two channels are relatively similar activities compared to most other ways to spend time and money, and to capture that a certain fraction of vacancies may be found along either channel. In the literature on search models with endogenous search effort s and a single search channel, the arrival rate and the search costs are generally taken to be proportional to s and s^2, respectively (see the survey by Mortensen and Pissarides, 1999). We require that our specification for c reduces to such a quadratic specification in case only one channel is used, or in case both channels are equivalent. So, our function c has to be such that $c(s,0), c(0,s)$ and $c(s,s)$ are quadratic in s. We take the following specification,

$$c(s_1, s_2) = \left(c_1 s_1^\gamma + c_2 s_2^\gamma\right)^{2/\gamma}. \tag{1}$$

This satisfies the above requirements if $c_i > 0$ and $1 \leq \gamma \leq 2$. There are two interesting special cases of this cost function. First, if $\gamma = 1$, there is perfect substitution. In this case the cost function simplifies to $c(s_1, s_2) = (c_1 s_1 + c_2 s_2)^2$, which means that to keep costs constant, one unit of formal search effort can always be replaced by $\frac{c_1}{c_2}$ units of informal search effort. Second, if $\gamma = 2$, the costs function equals $c(s_1, s_2) = c_1 s_1^2 + c_2 s_2^2$ so formal and informal search effort contribute additively to total costs.

The instantaneous utility of income is given by $u(w) = w$. While being unemployed a worker received benefits b, the instantaneous utility of unemployment equals $u(b) = \kappa b$. A value for κ smaller than 1 implies that individuals dislike being unemployed relative to working at a wage equal to the benefits level. Individuals maximize their expected discounted income over an infinite time horizon. The expected discounted income (or "value of search") and the discount rate are denoted by R_u and ρ, respectively.

While being employed, individuals can search on the job. Employed workers receive job offers with rate λ and lose their job with rate δ. A job offer is drawn from the wage offer distribution F. We do not model endogenous search effort while employed, and accordingly we do not distinguish between search channels while employed. The main reason for this is that our data do not provide observations of indicators of search effort or the use of search channels during employment. This data limitation implies that it is problematic to identify the search cost function for employed individuals and the effect of search efforts on the job offer arrival rate in employment. Of course, our primary interest is in the behavior of unemployed workers rather than employed workers. Modeling the job-to-job mobility by way of a fixed job offer arrival rate in employment should be sufficiently accurate to capture the beneficial effect of mobility on the expected present value of a job for an unemployed individual. An alternative approach is to simply assume that the search cost function and the effect of search efforts on the offer arrival rate are the same in employment as in unemployment. However, it is by no means clear whether this assumption is realistic. Moreover, this approach would be computationally more demanding than our approach, because of the complicated expressions for the expected present value of employment (see below).

An individual losing her job believes that the state of unemployment is equivalent to the state of unemployment before the employment spell she just completed. This implies that workers who received monitoring prior to employment believe that they will receive this again, and similarly for workers who did not receive monitoring. The optimal behavior of an employed worker is to accept all job offers with a wage higher than the current wage. This implies that the Bellman equation for an employed worker with wage w equals

$$\rho R(w) = w + \delta(R_u - R(w)) + \lambda \int_w^\infty (R(x) - R(w)) dF(x)$$

where $R(w)$ is the present discounted value of working in a job with wage w. For an unemployed person the Bellman equation is

$$\rho R_u = \max_{s_1, s_2 \geq 0} \kappa b - (c_1 s_1^\gamma + c_2 s_2^\gamma)^{2/\gamma} + (\lambda_1 s_1 + \lambda_2 s_2) \int_\phi^\infty (R(w) - R_u) dF(w) \tag{2}$$

The optimal strategy of an unemployed individual has a reservation wage property in the sense that she accepts a job offer if and only if the wage exceeds a reservation wage ϕ^{opt}. The reservation wage follows from $R(\phi^{\text{opt}}) = R_u$. It can be shown that in the generic case of $\gamma \neq 1$ the optimal search efforts have a unique interior solution and satisfies

$$s_i^{\text{opt}} = \frac{\lambda_i}{2c_i} \left(c_i + c_j \left(\frac{c_i}{\lambda_i} \frac{\lambda_j}{c_j} \right)^{\frac{\gamma}{\gamma - 1}} \right)^{1-2/\gamma} \int_{\phi^{\text{opt}}}^\infty (R(w) - R_u) dF(w) \qquad i \neq j \tag{3}$$

We assume that job search monitoring concerns only the formal search effort s_1 and not the informal search effort s_2. The local UI agency can check the number of times the UI recipient responds on a job advertisement, the number of application letters written, subscription at public employment offices, etc. It is for the local UI agency much more difficult to measure how often an individual asks friends and relatives about job openings. The monitoring effort of the local UI agency therefore focuses on search along the formal channel. Specifically, the agency imposes a minimum search effort (or threshold value) devoted to formal job search denoted by s_1^*.

Full compliance can be achieved by imposing severe punishment for noncompliance to the search requirements. In practice, the most common punishment in case of noncompliance is a sanction, which is a temporary benefit reduction. At the time of the experiment, the Netherlands had a strict sanction regime compared to many other countries (Abbring, Van den Berg and Van Ours, 2005). The data show that sanctions are virtually absent among monitored individuals, confirming that punishments are indeed regarded as severe. We therefore simply assume that there is no noncompliance. We return to this issue in Subsection 5.3 when we compute how large punitive benefit reductions should be to ensure compliance.

It is clear that if optimal formal job search effort s_1^{opt} in the unrestricted case lies above the threshold value s_1^*, then the individual will not change her behavior, so monitoring does not have any effect. We focus on the more interesting case in which the required effort is higher than the effort in the absence of monitoring. The individual will comply to the formal search requirement by choosing $s_1 = s_1^*$. In this case, the optimal strategy of the unemployed worker can be summarized by ϕ^* and s_2^*.

Given the reservation wage and given s_1^*, the optimal search effort along the informal channel satisfies the first-order condition

$$2c_2(c_1 s_1^{*\gamma} + c_2 s_2^{*\gamma})^{(2/\gamma)-1} s_2^{*\gamma-1} = \lambda_2 \int_{\phi^*}^{\infty} (R(w) - R_u) dF(w)$$

This equation does not have a closed-form solution. There are however two interesting cases to consider. First, in case of perfect substitution ($\gamma = 1$),

$$s_2^* = -\frac{c_1}{c_2} s_1^* + \frac{\lambda_2}{2c_2} \int_{\phi^*}^{\infty} (R(w) - R_u) dF(w)$$

Here, increased monitoring is relatively ineffective due to effort substitution.[5] Second, if $\gamma = 2$, then

$$s_2^* = \frac{\lambda_2}{2c_2} \int_{\phi^*}^{\infty} (R(w) - R_u) dF(w)$$

[5]Keeley and Robins (1985) also mention the possibility of substitution of search effort in response to monitoring of the formal search channel. They do not provide a formal theoretical analysis.

Now there is no direct substitution effect. There is an "income" effect via the decrease in the reservation wage, which increases the optimal s_2^*. Notice that $s_1^{\text{opt}}, s_2^{\text{opt}}$ and s_2^* all depend on the job offer arrival rate λ in employment.[6]

The optimal reservation wage ϕ^* follows from equation (2), where the right-hand side is now maximized over s_2 while s_1 is fixed at s_1^*. Note that the marginal returns to formal job search effort are now lower than the marginal costs. The optimal reservation wage is decreasing in the binding minimum required formal search effort level. Unemployed workers are forced to behave sub-optimally, so being unemployed becomes less attractive, and therefore they are willing to accept jobs with lower wages. For essentially the same reason, unemployed workers would not participate voluntarily in a monitoring scheme with a binding minimum search effort. Of course, the advantages of monitoring are outside of the individual's decision problem. The agency may want to reduce the total payment of UI (i.e., to increase θ by way of monitoring) because it believes that the advantages of this outweigh the reduction of the unemployed worker's present value. We return to this issue when performing policy simulations using the estimated model.

The reemployment rate θ is given by

$$\theta = (\lambda_1 s_1 + \lambda_2 s_2)\, \overline{F}(\phi) \tag{4}$$

with $\overline{F} := 1 - F$. Monitoring causes an increase in formal search s_1, which positively affects θ. Next, depending on the value of γ, there may be a substitution effect on s_2, which reduces θ. Furthermore, monitoring causes a reduction in the reservation wage ϕ, which positively affects $\overline{F}(\phi)$. The reduction in ϕ transmits the "income" effect on s_2, which entails an increase of s_2, which has a positive effect on θ. If $\gamma = 2$, the negative substitution effect is absent and monitoring only has positive effects on the reemployment rate. For lower values of γ the over-all effect is theoretically ambiguous, and it depends on the relative effectiveness of the search channels.

Monitoring has adverse effects on post-unemployment wages. At the individual level, the distribution function of the first accepted wage w equals $F(w)/\overline{F}(\phi)$. The lower ϕ, the lower the average accepted wage. The mean of the subsequent job duration is decreasing

[6]It is difficult to derive comparative statics results on the sign of the effects of λ on search effort for general values of the search cost parameter γ, and even more difficult to analytically analyze the interaction effects between λ and the presence of monitoring on search effort and other model outcomes. In a simpler model with one search channel in unemployment and no monitoring, an increase of λ has two effects on search effort in unemployment. A substitution effect reflects that it becomes more attractive to be employed to search further for an even better job. This increases the incentive to replace unemployment by employment, i.e. to increase search effort. An income effect reflects that the expected present value of unemployment increases by the improved attraction of being employed. This is a reason to cut down the spending on search costs in unemployment, i.e. to decrease search effort. In numerical analyses we find that the first effect typically dominates.

in the wage w. In our standard modeling of on-the-job search, these effects linger on in subsequent jobs. In sum, individuals who were subject to a binding job search monitoring scheme have, on average, lower income and shorter job spells after leaving unemployment.

4.2 Connecting search effort to the use of search channels

In this subsection we discuss the connection between the theoretical search effort variable s_i and the observed use of search channel i. Specifically, we assume proportionality. Let n_1 denote the number of formal search methods and n_2 the number of informal search methods, with formal and informal as defined in Subsection 3.1, and with superscripts $*$ and opt denoting the treatment regime. Then

$$\alpha_1 = \frac{s_1^{\text{opt}}}{n_1^{\text{opt}}} = \frac{s_1^*}{n_1^*}$$

and

$$\alpha_2 = \frac{s_2^{\text{opt}}}{n_2^{\text{opt}}} = \frac{s_2^*}{n_2^*}$$

where α_1 and α_2 are two unknown scale parameters.

It can be shown that the values of the parameters $c_1, c_2, \lambda_1, \lambda_2, s_*, \alpha_1, \alpha_2$ can be modified in particular ways without affecting the reservation wage, the re-employment rate, and the observed numbers of formal and informal search channels, in either regime. This means that these parameters are not identified. Intuitively, this is because we have not specified the term "effort" in search effort. We deal with this by imposing the innocuous normalization $\alpha_1 = \alpha_2 = 1$, implying that effort is measured as the number of channels used. This delivers identification of $c_1, c_2, \lambda_1, \lambda_2, s_*$. For example, notice that the reemployment rate θ in equation (4) can be expressed as $\theta = (\lambda_1 n_1 + \lambda_2 n_2)\,\overline{F}(\phi)$ so that if the job offer acceptance probability $\overline{F}(\phi)$ is identified then λ_1, λ_2 are identified from the association between n_1 and n_2 on the one hand and the unemployment duration on the other. In the next subsection we discuss the functional form of F and its identification.[7]

[7]Details available upon request. Not surprisingly, many interesting model features and outcomes are identified regardless of the above normalization. For example, s_1^{opt}/s_1^* and s_2^{opt}/s_2^* are identified from $s_i^{\text{opt}}/s_i^* = n_i^{\text{opt}}/n_i^*$, which does not depend on α_1. Other quantities that are identified without normalizations are θ and ϕ in both regimes and total search costs $c(s_1, s_2)$ in both regimes. The substitution parameter γ is identified from the degree to which individuals decrease their informal search as result of the increased formal search due to monitoring. We can also calculate the value of search R_u with and without monitoring. The latter is important for policy simulations. The results of the policy simulations below do not depend on the normalization of α_1 and α_2.

4.3 Parameterization

The structural model has a number of unknown parameters. We allow some of these structural parameters to vary with observed covariates using parametric specifications. Let x denote a vector of individual characteristics. The job search efficiency parameters while being unemployed λ_1 and λ_2 are specified as $\lambda_i = \exp(\lambda_{0i} + x'\lambda_x)$. The search efficiency parameters have different intercepts, but they share covariate effects.

The shape and mass of the wage offer distribution F below the reservation wage are not non-parametrically identified without additional assumptions or data. This is the recoverability problem discussed by Flinn and Heckman (1982). We assume that wage offers always exceed the mandatory minimum wage $w_{\min}$ and that the wage offer distribution is exponential,

$$F(w) = 1 - \exp\left(-\mu(w - w_{\min})\right) \qquad w \geq w_{\min} \tag{5}$$

This functional form assumption suffices for full identification of F. Since many observed wages are close to the mandatory minimum wage, the lowest reservation wage is at or below the mandatory minimum wage, so we are able to assess whether the assumption is valid. In the results sections of the paper we examine the fit of the functional form and the sensitivity of the results with respect to it. Notice that without a parametric assumption on F and its support, counterfactual analyses where the reservation wage drops below the lowest observed wage are not identified. Clearly, this problem is avoided if wage offers cannot drop below a binding mandatory minimum wage.

The expected wage offer is $w_{\min} + \frac{1}{\mu}$. We allow μ to depend on individual characteristics. Specifically, $\mu = \exp(\mu_0 + x'\mu_1)$.

For unemployed workers exposed to the monitoring policy their formal job search effort should exceed s_1^*. We allow this minimum search requirement to depend on individual characteristics. This captures, for example, that the UI agencies in both cities might apply guidelines differently or that caseworkers might take individual labor market prospects into account. The job search monitoring can thus have a different impact on different workers. We parameterize $s_1^* = \delta_0 + x'\delta_1$.

The remaining unknown structural parameters in the model are the job offer arrival rate λ of employed workers, the job separation rate δ, the parameter κ in the utility of unemployment, and the discount rate ρ. We fix the discount rate ρ to 5% annually. We estimate the other parameters, but restrict them to be the same for all workers.

4.4 Measurement errors and likelihood function

In this subsection we only briefly discuss the derivation of the likelihood function, as it closely resembles the derivations in the literature on structural estimation of search models

(e.g. Eckstein and Van den Berg, 2007). When specifying the log likelihood function we allow for measurement errors both in observed wages and search effort. The introduction of measurement errors serves two purposes. First, the estimation results become less sensitive to outliers. Secondly, the estimated variances of the measurement errors are informative on the extent to which the observed variation in an outcome can be explained by the model, i.e. they are informative on the fit of the model. For ease of presentation we suppress covariates x and individual indicators in the remainder of this subsection.

Recall from the subsection on identification that we impose a normalization that effectively delivers that the number of used search methods per channel equals search effort per channel. We now assume that formal and informal search effort are observed with individual and channel-specific measurement errors, according to $\widetilde{s}_i = s_i + \varepsilon_i$ (with ε_i normally distributed with mean zero and variance σ_i^2). This implies the following likelihood contribution of search effort

$$\frac{1}{\sqrt{2\pi}\sigma_i} \exp\left(-\frac{1}{2}\frac{(\widetilde{s}_i - s_i)^2}{\sigma_i^2}\right) \qquad (i = 1, 2)$$

Obviously, only those individuals who answered the questions on search channels in the survey contribute to this part of the likelihood function. But when estimating the model all 393 individuals in the administrative data are taken into account since they contribute to the other parts of the likelihood function discussed below.

Note that the quantification of a likelihood contribution for given parameter values always requires the calculation of the individual's optimal strategy. The latter is formulated in terms of optimal search effort and reservation wage. As the model does not have closed-form solution for the optimal strategy, it must be computed numerically. Of course, when computing the optimal strategy, we take into account whether or not the unemployed worker is exposed to job search monitoring.

We proceed by presenting the other building blocks of the total likelihood contribution provided by a randomly chosen individual in the sample. The full contribution involves integration over the distribution of wage measurement errors.

For unemployed workers devoting effort s_1 and s_2 to formal and informal job search respectively and who have a reservation wage ϕ, the reemployment hazard equals

$$\theta = (\lambda_1 s_1 + \lambda_2 s_2) \exp\left(-\mu(\phi - w_{\min})\right)$$

which implies the following likelihood contribution of an unemployment spell of length t_1, given the true s_1, s_2,

$$\theta^{d_1} \exp(-\theta t_1)$$

where d_1 indicates whether exit to work is observed.

If the individual is observed to find work ($d_1 = 1$), we observe the accepted wage $\widetilde{w}_1$. We allow for measurement errors in these observed wages. The observed wage is related to the actual wage in the first job w_1 by $\widetilde{w}_1 = w_1 + \epsilon$, with ϵ normally distributed with mean zero and variance σ^2. This implies that $\widetilde{w}_1$ is drawn from

$$\Phi\left(\frac{\widetilde{w}_1 - \phi - \mu\sigma^2}{\sigma}\right) \mu \exp\left(-\mu(\widetilde{w}_1 - \phi) + \frac{\mu^2\sigma^2}{2}\right)$$

Throughout the analysis, we assume that all measurement errors at the individual level $(\varepsilon_1, \varepsilon_2, \epsilon)$ are jointly independent.

Within the first job, the transition rate to the next job is

$$\theta_w = \lambda \exp\left(-\mu(w_1 - w_{\min})\right)$$

while the lay-off rate is δ. This implies that the likelihood contribution provided by the first job spell given the true w_1 is

$$\theta_w^{d_w} \delta^{d_u} \exp\left(-(\theta_w + \delta)t_2\right)$$

where d_w indicates a job-to-job transition and d_u indicates a lay-off. If the individual moves to a new job, the new wage w_2 must exceed w_1. Notice that because of measurement errors, the observed new wage $\widetilde{w}_2$ may actually be lower than $\widetilde{w}_1$. We take the wage measurement errors to be i.i.d. across jobs for a given individual. The likelihood contribution provided by the observed wage $\widetilde{w}_2$ given w_1 is

$$\Phi\left(\frac{\widetilde{w}_2 - w_1 - \mu\sigma^2}{\sigma}\right) \mu \exp\left(-\mu(\widetilde{w}_2 - w_1) + \frac{\mu^2\sigma^2}{2}\right)$$

The specification of the likelihood function allows for concentrating out the estimates of the variances of the measurement errors of the search effort. The maximum likelihood estimates for these parameters are the sample variances of the difference between observed efforts $\widetilde{s}_1$ and $\widetilde{s}_2$ and optimal efforts s_1 and s_2.

5 Estimation results

5.1 Parameter estimates

Table 3 provides the parameter estimates of the structural model. The results show that it is important to allow for heterogeneity across covariates. Ignoring this reduces the number of parameters by nine, while the log likelihood value increases by 34.2 points. A likelihood ratio test thus indicates joint significance.

Table 3: Parameter estimates for the structural model.

	estimate	(s.e.)
Formal-search channel: $\log \lambda_1$: intercept	-3.061	(0.392)
Informal-search channel: $\log \lambda_2$: intercept	-3.302	(0.532)
Female	0.183	(0.124)
Age (divided by 10)	-0.103	(0.048)
City 2	-0.058	(0.103)
Wage offers: $\log \mu$: intercept	-4.440	(0.142)
Female	0.695	(0.080)
Age (divided by 10)	-0.206	(0.033)
City 2	-0.163	(0.074)
σ_ε (measurement error wages)	88.235	(5.446)
Minimum formal-search requirement s^*: intercept	1.659	(0.555)
Female	-0.546	(0.304)
Age (divided by 10)	-0.180	(0.118)
City 2	-0.030	(0.278)
Costs formal search $\log c_1$	3.850	(0.474)
Costs informal search $\log c_2$	3.447	(0.573)
γ	1.310	(0.103)
$\log \lambda$ (offer arrival rate on the job)	-3.963	(0.076)
$\log \delta$ (job destruction rate)	-6.065	(0.132)
κ (non-pecuniary utility of unemployment)	0.434	(0.587)
Number of individuals	393	
Log likelihood	-4675.0	

The search efficiency parameter of formal search is slightly higher ($\lambda_1 > \lambda_2$). This suggests that one additional formal search method is more likely to generate a job offer than one additional informal search method. The difference is, however, not significant. Age has a significant impact on the efficiency of job search effort. Both λ_1 and λ_2 are higher for younger workers than for older workers. There are no significant differences between males and females or between residence in either city. All covariate effects in the wage offer distribution are significant. Women, younger workers and individuals living in City 1 receive lower wage offers than their counterparts.

The main policy parameters are those associated to the minimum requirements on formal job search effort. The highest values of s^* are attained for young men (about 1.25). However, none of the covariate effects is significant and also jointly the effects are insignificant (p-value for joint significance is 0.23). The minimum search requirements are binding for 82% of the individuals in the treatment group. For 18%, the unrestricted optimal formal search effort s_1^{opt} is above the minimum requirements s_1^*.

The formal search channel is slightly more costly than the informal search channel. The difference is not significant. Since also λ_1 and λ_2 are close to each other, search along both channels is about equally efficient. The estimate of the parameter γ equals 1.31 and significantly different from both one and two. Therefore, the search channels neither act additively nor are they perfect substitutes. The model predicts that without the job search monitoring 43% of all first jobs are found through the formal search channel. Applying job search monitoring increases this to 57%.

While being employed, each week a worker has a probability of 0.019 ($= \exp(-3.963)$) of receiving a job offer. On average a job offer arrives every 53 weeks. The weekly job separation rate is only 0.0023. So the workers lose their job on average every 431 weeks. The latter reflects that the individuals in our data are the least disadvantaged among the inflow into unemployment.

5.2 Fit of the model

Table 4 shows observed values for the main outcome variables and the corresponding predictions from the estimated model. In all dimensions the model predictions are quite close to the observed average values. Only the difference in the re-employment hazard between the treated and controls is somewhat larger for the model predictions than observed. The opposite holds for the first wage, where the observed difference is slightly larger than the predicted difference between treated and controls.

We can also use the estimated variances of the measurement errors as measure for the goodness-of-fit of the model. Comparing the variance of the measurement error in wages with the variance in observed wages shows that only 24.7% of the variance in observed wages

Table 4: Fit of the model.

	Data		Predictions	
	Treatment	Control	Treatment	Control
Formal search channels	0.79	0.52	0.75	0.55
Informal search channels	0.79	1.00	0.79	0.93
Re-employment hazard	0.043	0.042	0.044	0.038
First (weekly) wage	413	424	425	427

is due to measurement error. Since the individuals in our sample are quite homogeneous in terms of current labor market status and prospects and we only allow for a limited set of individual characteristics, measurement errors are quite small.

Finally, we compare the observed distribution of first wages after unemployment with the model predictions. Figure 1 shows a kernel estimate of the density of observed first wages and the predicted density of observed wages. The model assigns slightly more probability mass to the left tail of the distribution, but the densities are close.

5.3 Counterfactual policy evaluations

We use the estimated structural model for counterfactual policy simulations. Recall from Subsection 4.3 that effects of policy changes that reduce reservation wages are identified. However, assuming that the wage offer distribution F is invariant to policy changes means that we abstract from equilibrium effects in the counterfactual analyses.

Monitoring schemes. In Table 5 we provide the results of some policy simulations. In the first column, we describe the situation in which the UI agency does not impose any job search requirements (or implements any other policy measures). Of course, the numbers in this column coincide with the predictions from the estimated model for what we call the control group. We mainly focus on the first 26 weeks after starting collecting UI. Without job search requirements about 62.9% of the individuals start working within 26 weeks. The expected costs of the eligibility checks over the first six months are about 61.80 euro.

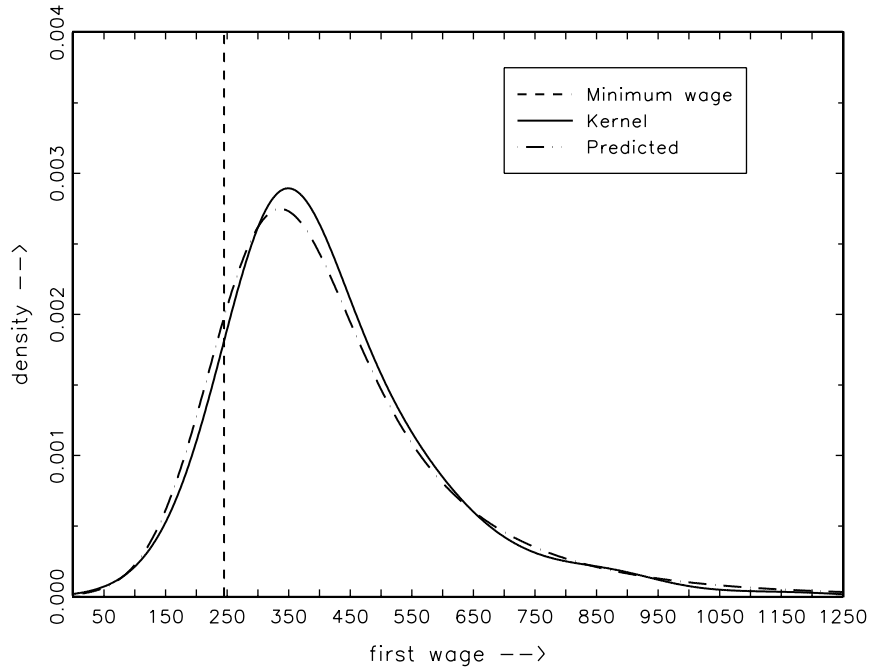
In column (2) we consider the case that all workers are subject to job search monitoring. These numbers coincide with the predictions from the estimated model for the treatment group. Recall that the monitoring is binding only for about 81% of the individuals in our sample. The remaining 19% workers have unrestricted formal search effort above the minimum requirement. Compared to the situation without job search monitoring, formal search effort is much higher and informal search effort is substantially lower. The reemployment

Table 5: Policy simulations.

	(1)	(2)	(3)	(4)	(5)	(6)
Formal search channel	0.543	0.808	2	0.546*	0.549*	0.559
Informal search channel	0.918	0.770	0.530	0.924*	0.929*	0.945
Job founds informally	57.1%	43.9%	17.2%	57.1%	57.1%	57.1%
Re-employment hazard	0.0381	0.0441	0.0815	0.0385*	0.0390*	0.0406
First (weekly) wage	430.46	423.03	406.91	429.63	429.55	423.03
Max{reservation wage,$w_{\min}$}	268.55	261.13	245.00	267.30*	266.14*	261.13
Present value of unemployment	593,602	591,554	567,758	593,731	593,724	591.554
Monitoring binding		81%	100%			
Agency costs	62.80	152.46	152.46	62.40	62.27	60.81
Bonus payment				94.06	99.15	
Prob. work within 26 weeks	0.6290	0.6820	0.8865	0.6343	0.6340	0.6537
Present value benefits	11020	9421	4806	10922	10914	8834
benefits (26 weeks)	6518	6105	4230	6483	6470	5417
Sanctions (10% detection)		1052	4761			
Benefit reduction						16.2%
"Welfare"	582,520	581,980	562,800	582,652	582,648	582,659

Explanatory notes:
"welfare" equals the present value of unemployment minus the agency costs and the present value of benefits.
(1) No monitoring.
(2) Monitoring.
(3) Intense monitoring $s_1^* = 2$.
(4) Reemployment bonus 150 euros (effort, hazard and reservation wage in first week reported).
(5) Decreasing reemployment bonus (start 260 euros, 10 euros decrease per week).
(6) Reduction of benefits such that workers are indifferent between benefits reduction and monitoring.
The reported reservation wage is the maximum of the quantity defined in Subsection 4.1 and the mandatory minimum wage.
* Due to bonuses the model is non-stationary, these are outcomes in first week of unemployment.

Figure 1: Observed and predicted first wages.



hazard slightly increases, which also means that after 26 weeks the fraction of individuals finding work increases with 6.3 percentage points from 62.9% to 68.2%. Therefore, the present value of expected benefit payments decreases.

Because the reservation wage[8] decreases somewhat when going from column (1) to (2), the expected first wage is slightly lower in column (2). The costs of providing job search monitoring are 152.46 euro per individual entering unemployment. The monthly monitoring meetings include the eligibility checks. So providing monitoring increases the policy costs by about 90 euro. Unemployed workers dislike being subject to job search monitoring as it restricts their search behavior. The value of being unemployed decreases from 593,602 to 591,554.

We obtain a "welfare measure" by subtracting the present value of benefits and policy costs from the value of unemployment. Of course, this measure of welfare is very crude as it, for example, ignores that employed workers are productive to firms. The monitoring reduces our measure of welfare from 582,520 to 581,980. We should point out that the absolute levels of the expected present value of unemployment and the welfare measure are

[8]In the remainder of the paper, we simply call the reservation wage to be the maximum of *(i)* the reservation wage as defined in the model and *(ii)* the mandatory minimum wage. Clearly, both reservation wage definitions are behaviorally and observationally equivalent.

less informative than their changes due to the monitoring. With the subjective discount rate of 0.05 per year, the levels are dominated by the earned income in the decades after the current unemployment spell.

Column (3) provides the results of more intense monitoring where every individual is subject to the formal search effort requirement $s_1^* = 2$. Individuals then reduce informal search effort almost to 0. The change in search effort causes the total search effort and the reemployment hazard to increase substantially. Due to this extreme monitoring, about 91.2% of individuals find work within 26 weeks of unemployment.

Because monitoring forces individuals to behave suboptimally, unemployed workers prefer to deviate from the minimum search requirements. To ensure that unemployed workers nevertheless comply with the requirements, the unemployment insurance agency can impose financial punishments. We can use our estimated model to compute the required size of such sanctions to be deterring. Since individuals are heterogeneous in characteristics and in their benefits level, the required sanction level differs between unemployed workers. Furthermore, the detection rate of non-compliance is important. In our computations we assume that if an unemployed worker does not comply, each time unit of one week the probability of getting a sanction equals 0.1. It turns out that in order to force all unemployed workers to comply to the high formal search requirement $s_1^* = 2$, the financial punishment should be at least 4761 euro. Furthermore, if the sanction is below 3103 euro, no unemployed worker complies to the formal search requirements. These are large numbers in the light of the sanction magnitudes in the regime that was prevalent at the time (see Subsection 2.2). This suggests that enforcement of such a regime requires a higher detection probability, either through a higher sampling rate of individuals by the monitoring agency or through a higher probability of observing a violation conditional on having sampled a non-compliant individual.

The sanctions above apply to the counterfactual strict monitoring regime. We can, however, also compute the size of the sanctions for the actual monitoring policy. Recall that the minimum search requirements under this monitoring scheme are not binding for 19% individuals, so for these unemployed workers the threat of a sanction is not necessary. To accomplish that all unemployed workers comply to the minimum formal search requirements, the size of sanctions should be at least 1052 euro. This magnitude is in line with the sanction magnitudes in the regime that was prevalent at the time. And indeed, as we have seen, sanctions are virtually absent in that regime, according to our data. This constitutes an external validation of our structural estimation results.

Reemployment bonuses. Next, we consider policies that combat moral hazard without monitoring. In particular, we focus on reemployment bonuses and unemployment benefits reductions. We compare these policies to monitoring, invoking cost neutrality on the part

of the UI agency or neutrality in terms of the expected present value of unemployment.

For reemployment bonuses[9] we consider two alternative schemes. First, we show simulation results where an unemployed worker receives a one-time bonus when finding work within 26 weeks after becoming unemployed. The model thus becomes non-stationary as bonuses are tied to a specific period of being unemployed. The level of the bonus is chosen such that the expected bonus payment (and other policy costs) roughly equal the costs of providing job search monitoring. This implies that in column (4) we focus on a reemployment bonus of 150 euro. The reemployment bonus slightly increases both formal and informal search effort, and it reduces the reservation wage somewhat right at the start of unemployment. As unemployment proceeds, individuals modestly increase their search effort and lower their reservation wage, until 26 weeks. The increase in search effort is proportional, such that the fraction of jobs found informally remains unaffected. Overall, the exit rate within 26 weeks is only slightly higher due to the reemployment bonus. The reemployment bonus slightly increases welfare. Not only do the unemployed workers benefit from a higher value of search, the total costs (benefits, checking, bonus payments) to the UI agency are slightly lower than in the no-policy case. It should be noted that we abstract from direct costs of the bonus system. This includes costs made to prevent abuse, i.e. to verify that the accepted job is held for a certain amount of time.

In the second bonus scheme, individuals receive 260 euros for finding work within one week, while every subsequent week this bonus decreases with 10 euros, so that the bonus is zero after 26 weeks of unemployment. This bonus scheme shares some features of the optimal unemployment insurance scheme as discussed by Hopenhayn and Nicolini (1997). In particular, accepting a job becomes less attractive each period. So individuals should be encouraged to devote more effort to search, and to accept more job offers. However, our simulation results show that after 26 weeks reemployment is about the same as in the previous bonus scheme, while the expected bonus payments are slightly higher. It should be noted that in the first few weeks reemployment rates are higher than in (4) while towards the end of the first 26 weeks of unemployment they are lower. However, in terms of value of search, costs for the UI agency and welfare, the two bonus schemes have very similar outcomes.

We may summarize the contrast between monitoring and reemployment bonuses as follows. Monitoring generates a decrease of unemployment durations. In this process it causes effort substitution and utility loss for the unemployed worker. However, due to job mobility, the relative size of this loss and the long-run effects on wages are small. Monitoring is cheap for the UI agency. Replacing such a cheap monitoring system with an

[9]Experimental studies on the effect of re-employment bonuses are surveyed in Meyer (1995). See also Card and Hyslop (2005) for a more recent study. Usually the effects on re-employment are found to be positive.

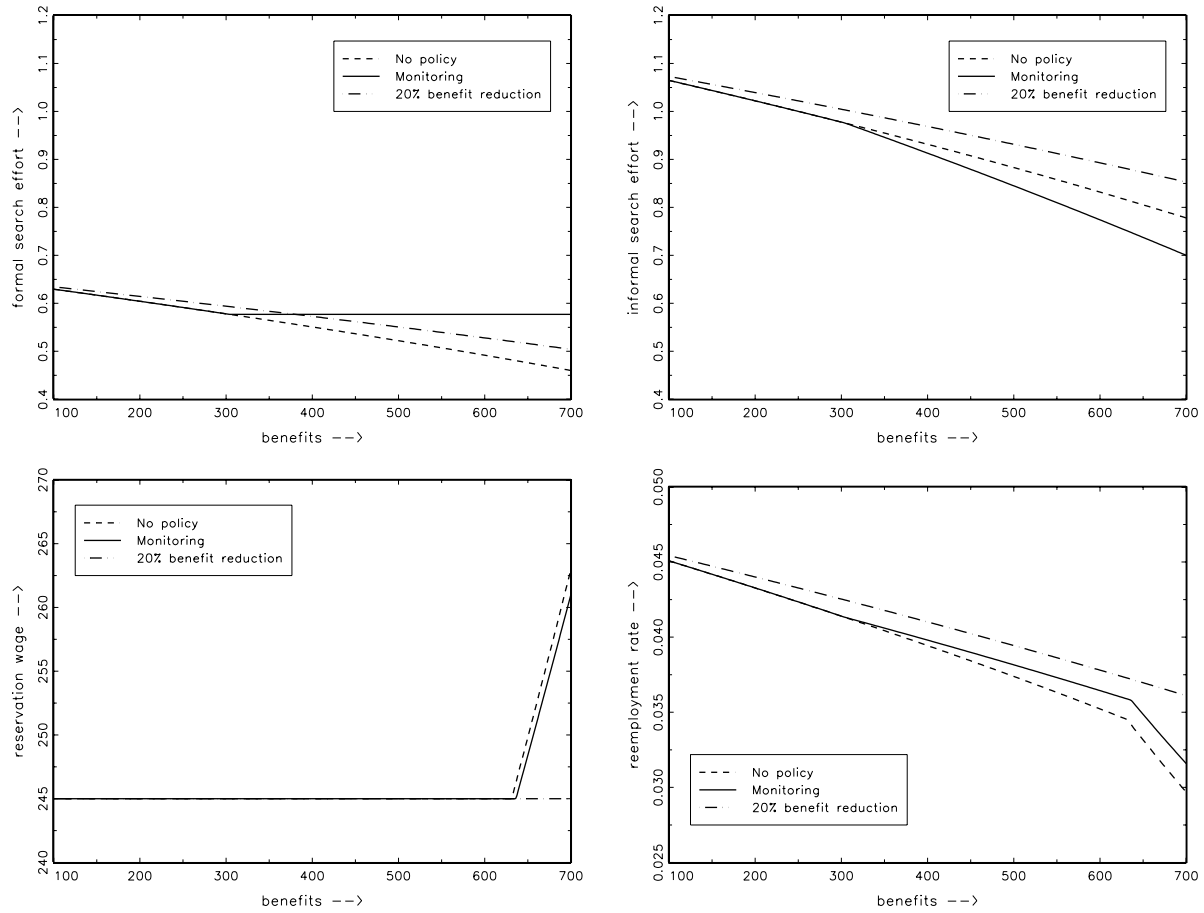
equally cheap reemployment bonus system is not useful: the latter exerts almost no effects whatsoever.

Unemployment benefits reductions. In the final counterfactual scenario, presented in column (6), we reduce the UI benefits such that individuals are indifferent between *(i)* high benefits with receiving monitoring, and *(ii)* low benefits while not being subject to monitoring. The workers for whom the monitoring is not binding are not willing to give up any benefits to avoid the monitoring. However, on average the unemployed workers are willing to have their benefits reduced by 16.2% to avoid job search monitoring. Of course, monitoring and the benefit reduction yield the same reservation wage and thus expected post-unemployment wage. Monitoring does cause a change in search behavior, which increases reemployment rates. Therefore, after 26 weeks more unemployed workers find work. The monitoring policy is much more expensive to the unemployment insurance agency. The reduced-benefits scenario yields the highest welfare of all policies investigated so far in this subsection.

We finish this subsection by examining how the effects of monitoring and a benefits reduction depend on the benefits level. In Figure 2 we show the effects of monitoring and a 20% benefit reduction on search behavior and reemployment of a representative individual, as a function of the UI benefits levels. First, without any policy intervention, both formal and informal job search effort are decreasing in the benefits level. Only for unemployed workers with high benefits does the reservation wage exceed the minimum wage. The reemployment rate is decreasing in the benefits level, and this decline accelerates if the reservation wage is higher. Job search monitoring is not binding for individuals with low benefits levels, as their formal search effort already is above the minimum search requirements. The impact of job search monitoring on formal search efforts becomes larger as the benefits level increases. This is because monitoring mainly affects individuals with low formal search effort, and these are individuals with high benefits. Therefore the substitution away from informal search also increases in the benefits level. In comparison, the benefits reduction affects individuals at all levels of unemployment benefits. As with monitoring, the effect on search effort becomes more substantial as the benefit level increases. Both job search monitoring and benefits reductions tend to depress reservation wages, and therefore reemployment rates increase in either case. In sum, the main qualitative difference is that, at low benefits levels, monitoring does not exert an influence, whereas a benefits reduction does. Notice that a proportionate decrease of the benefits level is stronger in absolute terms for individuals with high benefits levels. If the benefits decrease involves a fixed amount, or if individuals use a logarithmic utility flow function, then the findings would be less dramatic.

We may summarize the contrast between monitoring and benefits reductions as follows.

Figure 2: Simulated effect of job search monitoring and a 20% benefits reduction for a representative unemployed worker.



Initially (in (5)) we designed the policies such that workers are indifferent between them, implying a stronger benefits reduction for those with binding monitoring. For the UI agency the benefits reduction is the cheaper policy, even though the corresponding gain in reemployment rates is smaller. From a mean welfare point of view, the benefits reduction seems to be the most attractive. However, implementation of a benefits scheme that depends at the individual level on whether monitoring is binding or not seems unrealistically complex. A benefits reduction across the full population is unattractive among workers with adverse labor market conditions. Therefore a benefits reduction policy may be politically hard to sell.

5.4 Counterfactual labor market conditions

The effects of the monitoring scheme are modest. Since the unemployed workers in our data are considered to be less disadvantaged workers, and the experiment was conducted in a period of relatively favorable economic conditions, it is interesting to consider the outcomes in less favorable conditions.

Adverse market conditions in unemployment. First, we consider lower values of λ_1 and λ_2 (i.e., a setting with more frictions, e.g. due to less vacancies and more unemployment). We divide the search efficiency parameter estimates λ_1 and λ_2 by two, so that both the formal and the informal channel are affected. Columns (1) and (2) of Table 6 show the results. The effect of monitoring is roughly the same as in our baseline model (Table 5). Next, we examine a shift of the wage offer distribution towards lower wages, by doubling μ. Columns (3) and (4) show the effects. Notice that the reemployment rates are lower than in Table 5, because employment is now less attractive. The monitoring effect on the reemployment rate is now quite substantial. With employment being less attractive, search efforts are lower. Monitoring then induces more cross-channel substitution.

Alternative search and monitoring technology in unemployment. Next, we consider a setting in which access to the informal channel is restricted. One might argue that disadvantaged workers have limited access to informal search, or that their informal channel is less useful. Whereas formal job search remains always possible because new vacancies become available, the number of friend and relatives is finite. Most friends can only be asked a limited number of times for knowing about possible vacancies, so the informal search channel might dry up. It is interesting to see what the effects of job search monitoring would be if a worker would no longer have access to informal job search.

Therefore, in Table 7 we consider the case that the informal job search channel is absent. This implies that the unemployment insurance agency can observe all search effort of the unemployed worker. In the absence of informal search, unemployed workers devote much more effort to formal search. The regular monitoring is therefore only binding for a limited fraction of unemployed workers. However, the effects of very intense monitoring are still substantial, in particular on the reemployment rate. Post-unemployment wages are not affected much since for most individuals the reservation wages is at the mandatory minimum wage. However, the expected present value of unemployment reduces substantially as search costs increase.

An alternative to perfect monitoring would be to consider the case in which the unemployment insurance agency requires a minimum total effort. So the sum of formal and informal search effort should exceed some threshold. In particular, we consider $s_1 + s_2 \geq 2$.

Table 6: Counterfactual market conditions in unemployment.

	(1)	(2)	(3)	(4)
Formal search channels	0.505	0.801	0.403	0.790
Informal search channels	0.854	0.692	0.682	0.506
Job founds informally	57.1%	41.7%	57.1%	35.2%
Re-employment hazard	0.0196	0.0224	0.0315	0.0396
First (weekly) wage	406.91	406.91	325.99	325.95
Reservation wage	245.00	245.00	245.04	245.00
Present value of unemployment	537,585	533,290	407,189	403,017
Monitoring binding		82%		87%
Agency costs	79.48	152.46	68.16	152.46
Prob. work within 26 weeks	0.4013	0.4428	0.5610	0.6438
Present value benefits	19296	17034	12834	10062
benefits (26 weeks)	7807	7552	6923	6307
"Welfare"	518,210	516,104	394,287	392,808

Explanatory note:
(1) Low λ_1, λ_2, no monitoring.
(2) Low λ_1, λ_2, all monitoring.
(3) High μ, no monitoring.
(4) High μ, all monitoring.

Table 7: Counterfactual conditions: no informal search channel.

	(1)	(2)	(3)
Formal search channels	1.019	1.039	2
Informal search channels	0	0	0
Job founds informally	0%	0%	0%
Re-employment hazard	0.0338	0.0345	0.0676
First (weekly) wage	407.53	407.50	406.91
Reservation wage	245.63	245.59	245.00
Present value of unemployment	580,048	579,968	563,041
Monitoring binding		19%	100%
Agency costs	66.07	152.46	152.46
Prob. work within 26 weeks	0.5894	0.5966	0.8330
Present value benefits	11453	11251	5801
benefits (26 weeks)	6687	6638	4786
"Welfare"	568,530	568,564	557,087

Explanatory note:
(1) No monitoring.
(2) Monitoring.
(3) Intense monitoring $s_1^* = 2$.

Table 8: Counterfactual conditions: perfect monitoring.

	(1)	(2)
Formal search channels	0.543	0.666
Informal search channels	0.918	1.335
Job founds informally	57.1%	61.2%
Re-employment hazard	0.0381	0.0547
First (weekly) wage	430.46	422.92
Reservation wage	268.55	261.13
Present value of unemployment	593,602	590,255
Monitoring binding		100%
Agency costs	62.80	152.46
Prob. work within 26 weeks	0.6290	0.7573
Present value benefits	11020	7605
benefits (26 weeks)	6518	5521
"Welfare"	582,520	582,497

Explanatory note:
(1) No monitoring.
(2) Monitoring $s_1^* + s_2^* = 2$.

In Table 8 we show simulation results. The monitoring has a substantial effect on reemployment. The fraction of unemployed workers finding work within 26 weeks increases from 62.9% to 75.7%. Like in the previous case, this shows that monitoring can stimulate reemployment if there is perfect information to the unemployment insurance agency and monitoring affects all search effort. Of course, monitoring of informal search effort may be difficult and hence may be more expensive than monitoring of formal search. The results in Table 8 are based on the assumption that the costs of the former equal the costs of the latter which are observed. This probably underestimates of the real costs.

No job mobility. We now turn to what in our context is the most interesting counterfactual scenario, involving changes in post-unemployment conditions. The labor market in the Netherlands is characterized by a high degree of flexibility and job mobility (see e.g. Ridder and Van den Berg, 2003). Indeed, our data shows a substantial degree of mobility, often associated with relatively large wage increases. Recall from Table 5 that for most unemployed workers the reservation wage equals the minimum wage. In unemployment, the strategic rejection of low wage offers to wait for better offers is not optimal compared to using the first job as a stepping stone towards better paying jobs. Thus, virtually all

job offers are acceptable to the unemployed. Indeed, our results indicate that almost half of the unemployed workers even accept a wage below their benefits level.

It is therefore interesting to examine outcomes if job mobility (and job destruction) are not possible. In that case the effects of accepting lower wage offers are permanent. Table 9 gives the results. A comparison with Table 5 shows that poorer post-unemployment circumstances increase the reservation wage substantially, and reduce effort through both search channels. The consequence is thus that the re-employment hazard drops dramatically. Indeed, the possibility of job mobility is a major determinant of the well-being and the outcomes of the unemployed. As such it is much more important than monitoring or the other policy instruments that we considered, with the exception of the counterfactual intense monitoring regime.

Relatively speaking, monitoring is now more effective, as the weekly re-employment hazard increases by about 23%. However, the cumulative effect on the half-yearly reemployment probability is similar in magnitude to before. Without job mobility, the unemployed workers prefer a lower level of search effort in general, and therefore the substitution between formal and informal search induced by monitoring is somewhat higher. This can be seen e.g. by comparing which fraction of the jobs is found informally. The intense monitoring (Column (3)) is again efficient in stimulating reemployment.

To assess long-run effects on wages, it is useful to examine the present value of unemployment (i.e. the expected present value of all current and future income streams). This value decreases upon monitoring, but the decrease is smaller if job mobility is possible, even though with job mobility the short-run income stream is lower than without job mobility. We conclude from this that job mobility helps to counteract adverse long-run effects of monitoring on income in employment.

In general, we find in this subsection that the effectiveness of monitoring in reducing unemployment durations is higher if the labor market conditions are more adverse from the point of view of the unemployed worker. Not surprisingly, this goes at the expense of a larger reduction in their present values.

6 Sensitivity analyses

To assess the robustness of the results, we extend the model in a number of ways. First, we extend the specification of the instantaneous utility of being unemployed by taking $u(b) = \kappa_0 + \kappa_1 b$. It turns out that the parameter κ_0 is not significantly different from 0. This parameter is even estimated to be negative, implying that individuals dislike being unemployed for nonpecuniary reasons. It should be noted that κ_1 is estimated to be somewhat higher than in the baseline model.

The second extension is to allow for separate wage offer distributions for unemployed

Table 9: Counterfactual conditions: no job mobility.

	(1)	(2)	(3)
Formal search channels	0.469	0.795	2
Informal search channels	0.793	0.617	0.433
Job founds informally	57.1%	39.3%	14.5%
Re-employment hazard	0.0104	0.0128	0.0383
First (weakly) wage	619.12	610.90	546.80
Reservation wage	457.21	448.99	384.89
Present value of unemployment	487,060	478,302	410,023
Monitoring binding		84%	100%
Agency costs	89.92	152.46	152.46
Prob. work within 26 weeks	0.237	0.282	0.615
Present value benefits	37734	31215	11695
benefits (26 weeks)	8729	8489	6572
"Welfare"	449,236	446,934	398,177

Explanatory note:
(1) No monitoring.
(2) Monitoring.
(3) Intense monitoring $s_1^* = 2$.

and employed workers. We have also allowed the wage offer distribution of the formal and the informal channel to differ. These were estimated to be close to each other and the small difference was also not significant. Recall that we do not know through which channel unemployed workers found a job. Therefore, inference on wage offer distributions by channel seems problematic from the outset.

The difference between the wage offer distributions for unemployed workers and employed workers is significant. In particular, employed workers receive on average higher wage offer than unemployed workers. Because employed workers receive better wage offers, being employed becomes more attractive, and, therefore, for all workers the reservation wage drops to the minimum wage. Since the wage offer distributions only have support above the minimum wage, all job offers become acceptable. The predicted wage in the first job becomes, however, 374 euro which is substantially below what is observed in the data. The fit of the reemployment hazard improves somewhat, but the fit of the search effort becomes slightly worse. So, obviously while the difference in wage offer distributions is significant between employed and unemployed workers, the model predictions of our outcomes of interest do not improve.

Next, we have tried to allow the job offer arrival rate while being employed to depend on individual characteristics. However, none of the observed characteristics had a significant effect on the job offer arrival rate in employment, and jointly they were not significant either. The log likelihood value only decreased with 0.4 point, while three additional parameters were added.

Finally, we have tried to allow the cost function of job search effort to depend on unobserved characteristics. This is in line with Fougère, Pradel and Roger (2009), whose cost function contains such a term. We adopted a mass point specification allowing for two search cost levels. However, during the optimization of the log likelihood function, the mass points converged to each other. So allowing for unobserved heterogeneity in search costs does not yield a significant improvement of the model fit.

7 Conclusions

Job search monitoring, targeting the usage of formal job search methods, causes substitution away from informal search towards formal search. In the paper we consider effects on reemployment rates as well as on post-unemployment wages and other variables. Channel substitution mitigates effects of monitoring on these outcomes. Since monitoring is relatively cheap, the policy is cost effective for the unemployment insurance agency. However, unemployed workers suffer from it in terms of their expected present values. We estimate that, on average, unemployed workers are willing to lower their benefits level with 16% to avoid monitoring.

From the point of view of the UI agency, such a benefits reduction is a cheaper policy option than monitoring, even though the corresponding gain in reemployment rates is smaller. From an average welfare point of view, the benefits reduction is more attractive as well. However, benefits reductions are unattractive for the subset of workers with adverse labor market conditions. We also use the model to evaluate some other policy measures, notably reemployment bonuses. It turns out that replacing the monitoring regime with an equally cheap reemployment bonus system is not useful: the latter exerts almost no effects whatsoever. The introduction of a severe monitoring regime has a substantial effect, but such a policy would also require substantial punitive sanctions to induce unemployed workers to comply. Monitoring is also more useful if caseworkers are able to observe informal search behavior.

On the methodological side, this paper shows that a social experiment with data from a survey as well as from registers can be fruitfully used to estimate job search models that deal with non-trivial search technologies and post-unemployment outcomes. Indeed, the structural model captures all differences in labor market behavior and outcomes between the treatment and control group observed in the data. The structural estimation results allow for counterfactual analyses of a range of relevant alternative policy measures. This provides insights and results that cannot be obtained with reduced form evaluations. Because of the equipoise principle, social experiments are often deemed unethical if the expected treatment effect is large. Also, social experiments are often modest in size. This makes the combination of social experiments and structural inference, where restrictions from economic theory are used to complement the empirical evidence, particularly fruitful.

We find that post-unemployment outcomes are important determinants of the effectiveness of active labor market policies for the unemployed. This is not just a matter of active labor market policies affecting post-unemployment outcomes. Rather, the conditions after unemployment influence the extent to which a policy measure exerts short-run and long-run effects. Long-run effects may be unforeseen or may be deemed irrelevant by the UI agency, since the latter is primarily concerned about total UI payments. However, they may be important for the unemployed. In the paper we focus on the role of post-unemployment job mobility on the long-run effects of monitoring. The option of job mobility affects the unemployed individuals' behavior, but it also affects the extent to which undesirable long-run effects of monitoring on wages are mitigated. We find that a high job mobility slightly reduces the short-run impact of monitoring on reemployment, but perhaps more interesting, job mobility helps to counteract adverse long-run effects of monitoring on income in employment. To put it simple: with a high job mobility it does not matter so much that monitoring drives unemployed workers into a first job with a low wage. We view these results as important for policy makers. They may also serve as useful inputs for studies of optimal UI and optimal active labor market policy designs.

In addition to changes in job mobility, the paper examines a range of other counterfactual labor market conditions. In general, we find that the effectiveness of monitoring in reducing unemployment durations is higher if the labor market conditions are more adverse from the point of view of the unemployed worker. However, this goes at the expense of a larger reduction in their present values. Perhaps ironically, workers with adverse characteristics are sometimes subject to milder monitoring, because of fairness considerations by case workers (Van den Berg, Van der Klaauw and Van Ours, 2004).

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