

# CARROT AND STICK: HOW RE-EMPLOYMENT BONUSES AND BENEFIT SANCTIONS AFFECT EXIT RATES FROM WELFARE

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## SUMMARY

To increase the exit from welfare, benefit recipients in the municipality of Rotterdam were exposed to various financial incentives. Once their benefit spell exceeded one year, welfare recipients were entitled to a re-employment bonus if they found a job that lasted at least 6 months. However, they could also be punished for noncompliance with eligibility requirements and face a sanction, i.e. a temporary reduction of their benefits. We find that the financial sticks in the form of benefit sanctions were effective in stimulating the exit from welfare, while the financial carrots in the form of re-employment bonuses were not. Copyright © 2011 John Wiley & Sons, Ltd.

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## 1. INTRODUCTION

With the emerging financial crisis and rising unemployment governments consider various policy instruments to bring unemployed workers back to work. This is not easy. Traditional active labor market policies (ALMP) are often not very successful. Kluge (2010) presents a meta-analysis of 137 ALMP evaluation studies in Europe. He finds that traditional training programs appear to have at most a modest effect on transitions from unemployment to work, while direct employment programs in the public sector are rarely effective and frequently detrimental for the employment prospects of participants. Simple non-expensive programs with clear incentives for unemployed workers seem to work best.<sup>1</sup>

In this paper we investigate how positive and negative financial incentives affect the outflow from welfare. On the basis of outcomes from four US cash bonus experiments and six job search experiments Meyer (1995) concludes that financial incentives affect the speed by which unemployed workers leave unemployment insurance (UI). Some of these job search experiments included enforcement of job search rules and administrative measures of enforcement such as denials of benefits. Previous studies on financial incentives focus either on financial carrots in the form of re-employment bonuses or financial sticks in the form of benefit sanctions. We use data from the municipality of Rotterdam, which exposed welfare recipients to both financial carrots as well as financial sticks. Benefits could be reduced temporarily as punishment for noncompliance with eligibility requirements. However, once their benefit spell exceeded 1 year, welfare recipients were entitled to a bonus if they found a job that lasted at least 6 months.

So far, studies on the effects of re-employment bonuses have been mostly based on US experiments.<sup>2</sup> Woodbury and Spiegelman (1987) present an analysis of the first re-employment bonus experiments,

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<sup>1</sup>Card *et al.* (2010) present a meta-analysis of ALMP evaluations with similar findings but emphasizing that longer-term evaluations generally tend to be more favorable than short-term evaluations.

<sup>2</sup>For a more detailed description see Van der Klaauw and Van Ours (2009).

conducted in Illinois in the 1980s. They find that a re-employment bonus provided to the worker is more effective than a bonus provided to the employer.<sup>3</sup> Anderson (1992) studies a New Jersey re-employment bonus experiment, conducted later in the 1980s, in which the bonus declines over the duration of unemployment. The effect of a bonus on the job-finding rate is significantly positive early in the offer period, when the bonus was largest. Decker and O'Leary (1995), who analyze the results from re-employment bonus experiments conducted in the late 1980s in Pennsylvania and Washington, find that the impact of bonus offers increases in the generosity, but the overall effects are relatively modest. Both experiments reduced UI receipt by an average of about a week, while the impact on the post-unemployment earning was not statistically significant. Meyer (1996) re-examines the results from the part of the Illinois re-employment bonus experiment and concludes that a permanent bonus program might encourage unemployed workers to file for UI benefits. O'Leary *et al.* (2005) re-examine the Pennsylvania and Washington experiments and find some evidence that targeting of the bonus incentives to potential long-term unemployed may be beneficial. This is also the case when instead of re-employment bonuses a temporary subsidy on earnings is provided to low-income workers through in-work benefits. An example of this is the Canadian Self Sufficiency Project (SSP), which was introduced in the early 1990s and was intended to help welfare recipients to make a permanent break from welfare dependency. Card and Hyslop (2005) show that, indeed, even for long-term welfare recipients earnings subsidies have a positive effect on employment rates. However, the effect was not permanent. As soon as the subsidies stopped, the difference between treatment group and control group vanished.

Studies on the effects of benefit sanctions are mostly European. Abbring *et al.* (2005) analyze how benefit sanctions in the Netherlands affected the job-finding rates of UI recipients. They find that re-employment rates are significantly and substantially raised by imposition of a benefit sanction. Van den Berg *et al.* (2004) analyze the effects of sanctions on the behavior of welfare (benefits) recipients in Rotterdam. It turns out that when a benefit sanction is imposed the job-finding rate more than doubles and the effect is long lasting. From an analysis of Swiss data on benefit sanctions Lalive *et al.* (2005) conclude that by imposing a benefit sanction the job-finding rate goes up by 45%, causing the unemployment duration to decrease by roughly 3 weeks, from 33 to 30 weeks. Jensen *et al.* (2003) find a small effect of the sanctions that are part of the Danish youth unemployment program. However, also using Danish data Svarer (2011) finds that the unemployment exit rate increases by more than 50% following enforcement of a sanction. Boockmann *et al.* (2009) investigate the effectiveness of benefit sanctions applied to welfare recipients in Germany. They find that a benefit sanction increases the probability of leaving the welfare system within 8 months after the benefit cut by about 70% points. Therefore, from previous studies, it is clear that benefit sanctions speed up job finding, i.e. they have a positive 'treatment' effect—regardless of the size of the penalty or the duration of the punishment.<sup>4</sup>

Our contribution to the literature is twofold. First, we analyze the effect of re-employment bonuses in a European labor market with relatively high benefits and long benefit durations. So far, knowledge concerning the effect of re-employment bonuses is limited to a few 1980s pilot studies in the USA, where benefits are relatively low and short lasting. Second, we analyze financial incentives in a setting where the same group of unemployed workers are exposed to both positive and negative stimuli. This allows us to examine the difference between both types of financial incentives without having to worry about differences between groups of workers, differences in labor market environment, etc. As far as we know, there are no other empirical studies that analyze the effects of both positive and negative financial incentives within the same setting.

<sup>3</sup> The take-up rate among workers was 54%, while among employers it was only 12%. Bijwaard and Ridder (2005) report that in the Illinois experiment 15% of the eligible workers and 35% of the eligible employers refused participation. When selectivity of this noncompliance is taken into account the effects of the re-employment bonus are larger.

<sup>4</sup> However, the positive effect on exit rates from the benefits scheme seems to come at a cost in terms of a reduced quality of post-unemployment jobs; see Arni *et al.* (2009) and Van den Berg and Vikstrom (2009).

In our empirical analyses we use hazard rate models to identify the effects of both financial stimuli. Identification of the effect of benefit sanctions is based on the idea that welfare recipients cannot anticipate the exact moment of getting punished. We follow Abbring and Van den Berg (2003) and use multivariate mixed proportional hazard models, in which the potential selectivity in the use of benefit sanctions is accounted for by simultaneous modeling of the sanction rate and the exit rate from welfare. Identification of the effect of re-employment bonuses is based on the variations in the bonus policy that occurred during our period of analysis. In particular, during our observation periods first cash benefits were replaced by a tax rebate and next the tax rebate was abolished. Since only a sub-sample of the welfare recipients are exposed to the bonus policy, there is a well-defined control group to separate calendar time effects from the effects of the re-employment bonuses.

We use administrative data from the welfare agency in the municipality of Rotterdam to estimate the effects of the benefit sanctions and the re-employment bonus scheme.<sup>5</sup> We find that benefit sanctions are very effective, while re-employment bonuses are not. We argue that this may have to do with the present bias of some welfare recipients. An incentive scheme that requires immediate effort in exchange for delayed rewards may be less effective than an incentive scheme that imposes immediate costs to lack of search effort.

The setup of our paper is as follows. Section 2 discusses financial incentives to which welfare recipients are exposed in Rotterdam. This section also provides a description of our data. In Section 3 we present our statistical model, and in Section 4 we discuss our estimation results, from which we conclude that benefit sanctions speed up the transition from welfare to work, while re-employment bonuses have no effect. In Section 5 we provide an explanation of our main findings. Section 6 concludes.

## 2. FINANCIAL INCENTIVES FOR WELFARE RECIPIENTS IN ROTTERDAM

In the Netherlands, welfare benefits are a safety net and provide support to unemployed workers who are not entitled to any other social insurance benefits such as UI or disability insurance benefits. Welfare benefits are means tested and related to the family situation, but not limited in duration. Benefit replacement rates in the Netherlands are usually somewhat higher than in most other European countries and are much higher than in the USA (see Van den Berg *et al.*, 2004, for further details).

Welfare benefits recipients are often long-term unemployed with poor labor market prospects.<sup>6</sup> Even though in our data we only consider welfare recipients with an obligation to actively search for work, there are substantial compositional differences with UI benefits recipients. Among welfare recipients there is a much higher share of individuals under age 25 than among UI recipients. These are school leavers and individuals with only a short employment history which does not entitle them to UI benefits. Also, welfare recipients are most often single, while the majority of the UI recipients are married. This might, of course, be the consequence of the difference in age structure, but might also be a result of the fact that welfare benefits are means tested. Finally, a very substantial share of women receiving welfare benefits are single mothers, whereas these are only a small fraction in the population receiving UI benefits.

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<sup>5</sup>In the Netherlands municipalities are responsible for providing welfare benefits. Comparing the way municipalities operate De Koning *et al.* (2006) find that to stimulate welfare recipients to find a job quickly Rotterdam uses benefit sanctions and re-employment bonuses more often than other municipalities.

<sup>6</sup>In 2002 in the Netherlands there were about 180,000 individuals receiving unemployment insurance benefits and about 320,000 welfare recipients. Within the municipality of Rotterdam the median UI benefit duration was slightly over 100 days, while more than 60% of the welfare benefits lasted more than 600 days.

## 2.1. Re-employment Bonuses

In the period from 1997 until 2002 welfare recipients in Rotterdam were exposed to re-employment bonus schemes. During our observation period (2000–2003), the main eligibility condition was that someone had to be on welfare benefits for at least 1 year. Furthermore, after leaving welfare the worker had to remain employed for at least 6 months.<sup>7</sup> Even though the calendar period we consider is rather short, the re-employment bonus rules changed frequently. Since our inflow sample starts on the first day of January 2000, the rules related to accepting regular jobs in the years 2001, 2002 and 2003 are important. In 2001 and 2002 the eligibility conditions remained unchanged, but the payments changed. In particular, in 2001 the bonuses were cash payments, whereas in 2002 these consisted of a tax rebate. In 2003 the re-employment bonus schemes were abolished.

In 2001 workers were entitled to a maximum of €1800, to be paid in four equal amounts of €450, every 6 months after the start of the job, provided the worker remained employed. Workers had to apply themselves for the cash payments. The amounts are substantial as in 2001 welfare benefits were €1015 per month for married or cohabiting couples and €508 for singles. The minimum wage in 2001 was €1155, implying that for married or cohabiting welfare recipients the replacement rate for a minimum wage job was 91%. In 2002 workers were entitled to a re-employment tax rebate of a maximum €2269 over a period of 3 years: 1361 in the first year and 454 in the second and third years. In 2003 no re-employment bonuses were paid for employment that started after 1 January 2003.

From the perspective of the welfare recipients the changes in rules imply the following. Workers starting to receive welfare benefits in 2000 who, after at least 12 months of unemployment, found a job—of at least 6 months—in the calendar year 2001 were entitled to a re-employment bonus of a maximum of €1800. If these workers found a job in 2002 they were entitled to a tax rebate of a maximum of €2269. If they found a job in 2003 they were not entitled to a re-employment bonus. Workers starting to receive welfare benefits in 2001 who were unemployed for at least 12 months and found a job in 2002 were entitled to a tax rebate of a maximum of €2269. If they found a job in 2003 they were not entitled to a re-employment bonus. Finally, workers starting to receive welfare benefits in 2002 or 2003 were not entitled to a re-employment bonus. The changes in the entitlement rules across inflow cohorts and within cohorts over calendar time allow us to identify whether or not the re-employment bonuses affected the exit rates from unemployment.

There is a fundamental difference between the US experiments and the setup of the Rotterdam bonus system. Whereas in the US experiments a bonus was paid to workers who found a job quickly, in Rotterdam welfare recipients became entitled to a re-employment bonus after being unemployed for at least 1 year. The main reason for this was fear of deadweight loss, i.e. workers taking up bonuses who would have found a job anyway. This setup creates disincentives for workers who are close to being unemployed for 1 year: why accept a job shortly before becoming entitled to a re-employment bonus? In our empirical analysis we investigate whether such an anticipation effect exists. In our analysis there are three issues we have to deal with. First, there is a potential ‘anticipation’ effect: welfare recipients who were close to being unemployed for a year—and thus not yet entitled to a bonus—may have postponed starting on the job until they were unemployed for more than a year and would be entitled to a bonus. Second, people who were entitled may not have applied for a bonus. Third, if a job was lost within 6 months individuals who thought at the start of their job that they might be entitled to a bonus were not entitled after all. This means that job search behavior by welfare recipients may have been driven more by entitlement than by actual receipt of the bonus. Therefore, and because of data limitations discussed below, we analyze the effect of being entitled to a bonus. In our analyses we identify the potential effect of a re-employment bonus by comparing the pattern of duration dependence of workers who were entitled

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<sup>7</sup>Note that this could be in the same job or several consecutive jobs.

to a re-employment bonus with the pattern of those who were not entitled. We distinguish between an ‘anticipation’ effect in month 12 and a ‘treatment’ effect beyond month 12.

## 2.2. Benefit Sanctions

The main reason to impose benefit sanctions is noncompliance with job search guidelines. Other reasons relate to administrative infringements such as returning late from holiday or filling in forms incorrectly. The procedure of imposing a sanction consists of two steps. In the first step it is established that a welfare recipient does not comply with the guidelines of the welfare agency. Information on possible offenses can come from the monthly form a welfare recipient has to fill in, or from conversations between the employees of the welfare agency and the welfare recipient.

It is also possible that the public employment office informs the welfare agency about a lack of job search activity. In the second step of the sanction procedure, it is decided whether or not the noncompliance will be punished. Noncompliance does not always lead to a sanction. However, in case of severe fraud, the welfare agency may decide to pursue legal prosecution. Local or district governments are responsible for the payment of welfare benefits, but the national government has set binding rules and procedures concerning the imposition of sanctions. However, these rules do not imply that the sanctions are imposed automatically or fully objectively. The caseworkers of the welfare agency have some discretion to interpret the rules. According to the procedures, the decision to impose a sanction on a particular welfare recipient is taken by the local welfare employee after consulting a so-called ‘decision maker’. The decision maker checks the proposal to make sure that all the right legal steps in the procedure have been taken. The employee of the welfare agency takes the state of the local labor market into account when deciding whether or not a sanction should be imposed. Furthermore, conditional on noncompliance with the guidelines, the decision to impose a sanction also depends on characteristics of the welfare recipients such as attitude, appearance and motivation. These are all characteristics that are unobserved by the researcher trying to investigate the effect of sanctions.<sup>8</sup>

The period between the establishment of noncompliance by the caseworker of the welfare agency and the imposition of a sanction is usually 1–2 months. Many sanctions are treated in an administrative way, for example when welfare recipients do not provide information about their search efforts. The municipality has the possibility of sending a written warning to a welfare recipient, especially for failing to meet administrative obligations.<sup>9</sup>

## 2.3. Data

Our data are from the administrative records of the welfare agency and contain 28,039 individuals aged 16–60 who started collecting welfare benefits in Rotterdam between 1 January 2000 and 1 August 2003. In total, the data include 30,527 welfare spells. There are therefore some individuals with multiple spells, but the vast majority of individuals only experienced a single period of receiving welfare benefits within the observation period, which ended 1 September 2003. For each individual we observe the day of inflow and the reason for inflow into welfare. For individuals leaving welfare before the end of the observation period, we observe the day of leaving welfare and the reason for leaving welfare. If a sanction is given during the welfare period, we observe the exact day at which a sanction is imposed. The welfare agency does not collect information about the job welfare recipients find after leaving

<sup>8</sup>In the empirical analysis we allow these unobserved characteristics to affect both the sanction rate and the unemployment exit rate, thus correcting for potential selectivity in the imposition of benefit sanctions.

<sup>9</sup>Note that such written warnings do not specify a potential date of sanction implementation. As far as we know, apart from the written warnings welfare recipients are not informed about an investigation.

Table I. Summary statistics

	Men	Women
Number of individuals	15,350	12,689
Number of spells	17,055	13,472
Outflow from welfare	49.2%	40.4%
to study	1.2%	1.5%
because of marriage	0.3%	1.9%
to work	20.1%	15.3%
to other benefit	1.0%	0.6%
because of moving	5.1%	8.6%
for unknown reason	21.5%	12.5%
Entitlement to a re-employment bonus (number of spells) <sup>a</sup>	187	125
Re-employment bonus payments (number of spells)	61	57
Spells with sanction	12.5%	7.7%
Sanction types		
5% reduction	38.5%	48.2%
10% reduction	26.3%	21.2%
20% reduction	8.5%	5.4%
other	26.7%	25.2%
Age (<25)	28.3%	37.7%
Age (25–35)	35.0%	31.0%
Age (35–45)	23.4%	18.9%
Age (45–55)	10.7%	10.0%
Age (≥55)	2.6%	2.5%
Children	14.1%	56.5%
Non-Dutch	15.0%	9.9%
Married	13.1%	4.7%
Reason for inflow		
Leaving school	6.6%	10.3%
Divorce	1.6%	17.0%
End work	19.8%	12.9%
End UI benefits	14.0%	8.4%
Asylum	6.1%	2.7%
Other reasons or unknown	51.8%	48.5%

<sup>a</sup>Entitlement to a re-employment bonus implies outflow in 2001 after at least 1 year of welfare.

welfare. Therefore, we cannot analyze how benefit sanctions and re-employment bonuses affect the duration of the post-unemployment job.<sup>10</sup>

About 44% of the observed welfare spells describe women. Table I shows that about 40% of the women stop collecting welfare benefits during the observation period, and this is around 49% for men. The main reasons to stop collecting welfare benefits are job finding and moving to another municipality. Since it is very difficult to move and start collecting welfare benefits in another municipality, moving is often considered to be related to finding work or moving in with a partner. There is also a very substantial ‘unknown’ category. Often welfare recipients end their benefits without informing the welfare agency of the reason. The welfare agency believes that the majority of these individuals find work.

In Figure 1 we show the empirical survivor functions for men and women stratified by year of start of the welfare benefits. There are two striking features. First, women are more likely stay in welfare for a longer period; and second, the exit rate is lower for the cohort entering welfare in 2002 compared to 2000 and 2001. The latter is probably related to business cycle effects. Figure 2 shows that our observation period was characterized by a (nationwide) slowdown of gross domestic product (GDP) growth in 2000 and 2001, and zero growth in 2002 and 2003. Furthermore, in Rotterdam the number of

<sup>10</sup>In the data we observe some individuals experiencing multiple welfare spells, which would potentially allow us to analyze time out of welfare. However, in practice the number of individuals with multiple spells is too low to perform such an analysis. As we describe in more detail below, the availability of multiple spells does help us in the identification of our empirical model.

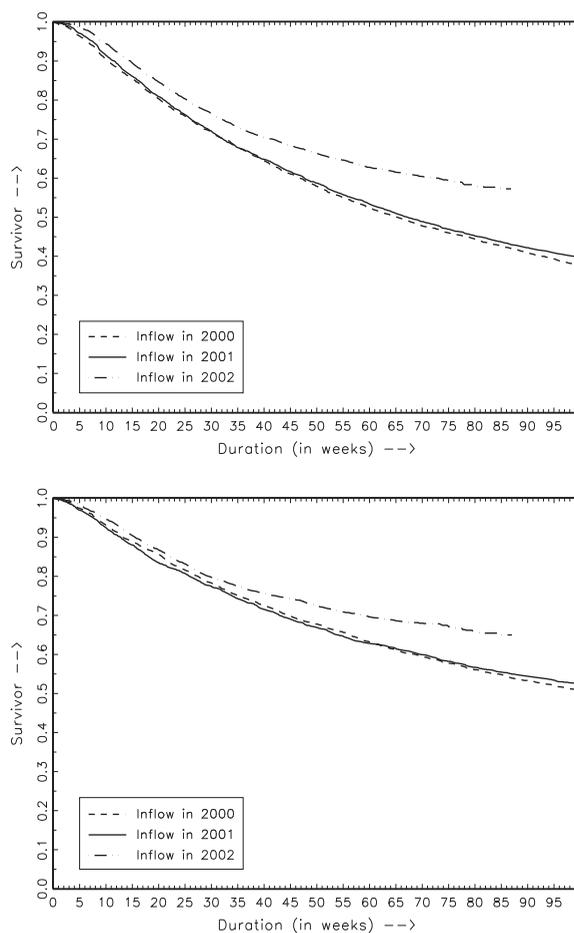


Figure 1. Kaplan–Meier estimate for men (top) and women (bottom) for different inflow cohorts

vacancies over the number of non-employed job seekers decreased. Thus individuals who entered welfare in 2002 were exposed to less favorable labor market conditions than individuals who entered welfare in earlier years.

The empirical survivor functions show that after 1 year about 60% of males and almost 70% of females were still collecting welfare benefits. These long-term welfare recipients are potentially entitled to a re-employment bonus when finding work. In Figure 3 we show empirical hazard rates for the three inflow cohorts. The exit rates for men are higher than for women, and exit rates for the 2001 and 2002 cohorts are higher than for the 2003 cohort. For both groups the exit rates peak after 10 weeks of collecting welfare benefits and decrease afterwards. The hazard rates around 1 year of benefit collection do not show any strong changes that could potentially be assigned to the existence of the re-employment bonuses.

Table I shows that even though men, on average, have shorter welfare spells than women, they are more likely to be punished with a sanction. The data contain some information on the size of the sanctions. Both for men and women the most common sanction is a 5% reduction, but also 10% reductions occur frequently. There are, however, also many sanctions that do not fit within one of the standard categories. This might, for example, be because there have been multiple, repeated or very severe violations of the guidelines.

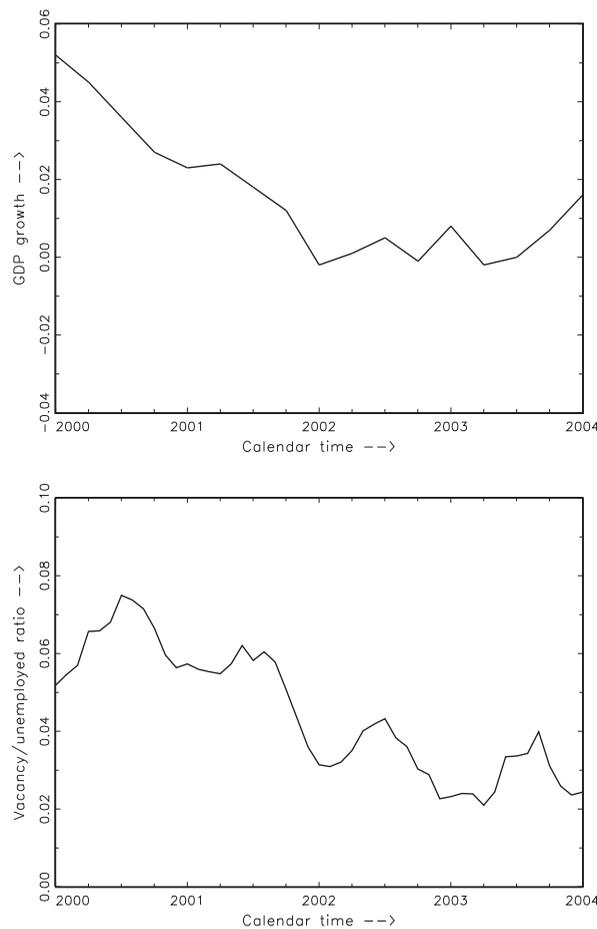


Figure 2. Business cycle indicators, nationwide GDP growth (top) and local vacancy over unemployment ratio (bottom)

Until 2001 the re-employment bonuses consisted of cash payments. In 2002 the bonuses were replaced with a tax rebate. Recall that individuals had to apply both for receiving the bonus and the tax rebate. Since we do not have any information about tax records we cannot observe which individuals requested the tax rebate. Our data include information on actual payments of re-employment bonuses. However, the data contain only an indicator to which individual the bonus was paid, but not to which spell the bonus was tight. We matched the bonuses to specific spells and also determined eligibility for a bonus (based on a minimum 1 year of collecting welfare, and not returning to welfare within 6 months). Table I shows that in 2001 there were 187 males and 125 females who found work after collecting welfare for 1 year and who were eligible for a bonus payment. Of this group, only 61 males and 57 females received a re-employment bonus payment, which are 32.6% and 45.6% of the eligible population.<sup>11</sup> The low take-up rate cannot be attributed to lack of information among welfare recipients. The caseworkers made the welfare recipients aware of the existence and nature of the re-employment bonuses, and the welfare recipients also

<sup>11</sup>This is the take-up rate for the first bonus payment. Conditional on remaining employed, individuals could receive multiple bonus payments.

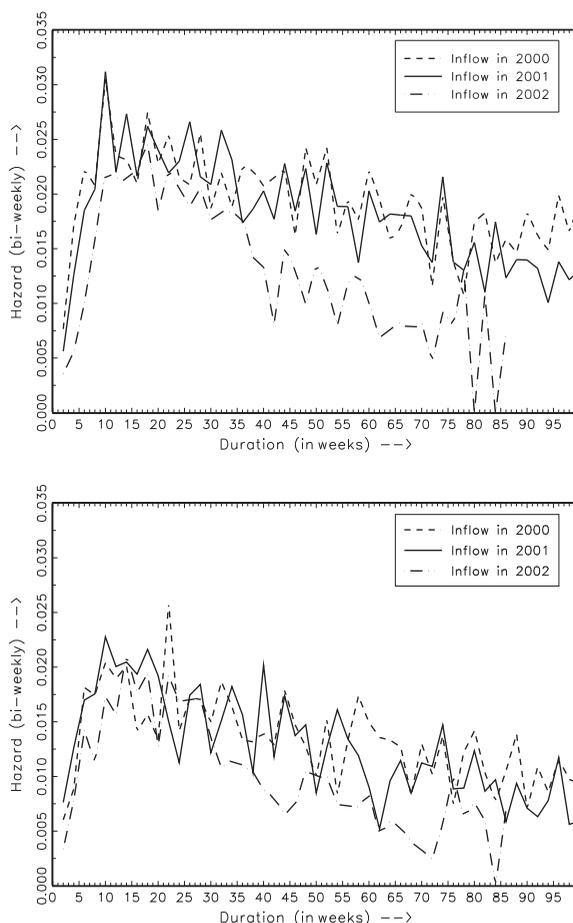


Figure 3. Empirical hazard rates (bi-weekly) for men (top) and women (bottom) for different inflow cohorts

received a letter at their home addresses about their eligibility for a re-employment bonus. Furthermore, each welfare recipient had regular meetings with caseworkers, during which the re-employment bonuses were announced and discussed.

Male and female welfare recipients differ substantially in individual characteristics. The age profile is similar, but women are much more likely to have children and are less likely to be married. Indeed, a substantial share of the women are single mothers. Also, the reason for entering welfare differs somewhat. Women enter welfare more after leaving school or getting divorced, while men more often enter after they lose work, their unemployment insurance benefit expires or they have been granted asylum.<sup>12</sup>

### 3. MODEL

In this section we provide the empirical model for estimating the effects of benefit sanctions and re-employment bonuses on the exit rate from welfare of benefit recipients. For this purpose, we use a continuous time duration model, which describes the transition out of welfare.

<sup>12</sup>Note that for both men and women the ‘other’ category is the largest. Most likely, caseworkers are not very precise when completing this information in the data.

In the identification of the model we exploit the fact that the exact timing of a sanction is unanticipated by welfare recipients. No anticipation implies that, before the actual moment of imposition of a sanction, the sanction does not have an effect on the exit rate. From the moment a sanction is imposed, the exit rates to work jumps. This is the so-called *ex post* sanction effect. No anticipation does not mean that sanctions are given randomly (conditional on observed characteristics). Welfare recipients may know if and when they are exposed to high risks of getting a sanction.<sup>13</sup> This approach has been used before by Abbring *et al.* (2005), Lalive *et al.* (2005) and Van den Berg *et al.* (2004). The details of the identification are discussed at length in Abbring and Van den Berg (2003).

We cannot use a similar approach for estimating the effects of re-employment bonuses. Welfare recipients are informed if and when they are entitled to receiving a bonus upon finding work. However, the rules change over time, and only part of the welfare recipients are affected by the re-employment bonuses. This allows us to distinguish between the effect of the re-employment bonus and calendar time effects. As in difference-in-difference estimation, the identification of the effect of the re-employment bonuses hinges on the assumption of a common trend for affected and non-affected welfare recipients.

Recall that the data describe individuals who started collecting welfare benefits between 1 January 2000 and 1 August 2003. Consider an individual entering welfare at date  $\tau_0$ , who has been unemployed for  $t$  days. The exit rate from welfare depends not only on calendar time  $\tau_0 + t$  and the elapsed duration of welfare  $t$ , but also on observed individual characteristics  $x$  and unobserved characteristics  $v_u$ . Furthermore, the exit rate from welfare can be affected by whether or not a sanction has been imposed on the welfare recipient. Let  $t_s$  denote the elapsed welfare duration of imposing a sanction, and  $I(t > t_s)$  is an indicator of whether or not after  $t$  days of welfare the individual has been punished by a sanction. For the welfare recipient the exit rate is described by the mixed proportional hazard rate:

$$\theta_u(t|x, \tau_0, t_s, v_u) = \lambda_u(t)\psi_u(\tau_0 + t) \exp(x\beta_u + \delta I(t > t_s) + \gamma(t|\tau_0) + v_u)$$

where  $\lambda_u(t)$  is true (piecewise constant) duration dependence and  $\psi_u(\tau_0 + t)$  are genuine calendar time effects modeled by dummies for each quarter.

The parameter  $\delta$  is the *ex post* effect of imposing a sanction. This parameter describes how the hazard rate shifts at the moment a sanction has been imposed on the welfare recipients. The other parameter of interest,  $\gamma(t|\tau_0)$ , is the effect of promising a re-employment bonus. Recall from Section 2.1 that individuals are entitled to receive a re-employment bonus if they find work after collecting welfare benefits for more than 1 year (and do not return to welfare within half a year). However, the implementation of the bonus schemes has been changed over the years. Changes were always implemented on 1 January. Furthermore, on 1 January 2003 the bonus schemes were stopped. This implies that  $\gamma(t|\tau_0)$  is zero if  $\tau_0 + t$  exceeds 1 January 2003.

We allow re-employment bonuses to have two effects. First, there can be a true incentive effect for those who are entitled to collecting the bonus when finding work. These are the individuals collecting welfare for at least 1 year. Second, there can be an anticipation effect. This effect arises if individuals close to 1 year of welfare reduce job search to avoid finding work, or perhaps delay starting to work until becoming entitled to the re-employment bonus. The latter would imply an increased exit rate in the 13th month of collecting welfare. We therefore specify the effect of the re-employment bonus as

<sup>13</sup>To the extent that welfare recipients want to avoid a benefit sanction, they may change their behavior before such a sanction is imposed. This is the so-called *ex ante* sanction effect. The size of the *ex ante* effect depends on the size of the sanction and intensity of monitoring. Using our data we cannot say anything about the size of the *ex ante* effect without making additional assumptions.

$$\gamma(t|\tau_0) = \begin{cases} 0 & \text{if } t \leq 11 \text{ months or } \tau_0 + t \geq 1 \text{ January 2003} \\ \gamma_{\text{anticipation}, \tau_0+t} & \text{if } 11 \text{ months} < t \leq 12 \text{ months and } \tau_0 + t < 1 \text{ January 2003} \\ \gamma_{\text{delayed}, \tau_0+t} & \text{if } 12 \text{ months} < t \leq 13 \text{ months and } \tau_0 + t < 1 \text{ January 2003} \\ \gamma_{\text{incentive}, \tau_0+t} & \text{if } t > 13 \text{ months and } \tau_0 + t < 1 \text{ January 2003} \end{cases}$$

The parameter  $\gamma_{\text{anticipation}, \tau_0+t}$  measures the anticipation effect of the bonuses. This implies that between 11 and 12 months of unemployment the model allows individuals to anticipate that they might get a bonus when finding work after 12 months. In Section 4.2 we investigate the importance of choosing 11 months as the start of the anticipation effect. If anticipation is important,  $\gamma_{\text{anticipation}, \tau_0+t}$  will be negative.

The parameter  $\gamma_{\text{delayed}, \tau_0+t}$  describes the effect in the first month of entitlement to the bonus. If some welfare recipients delay starting to work until after becoming entitled to the re-employment bonus, this parameter will be larger than the incentive effect of being eligible for the bonus measured by the parameter  $\gamma_{\text{incentive}, \tau_0+t}$ . We allow the parameters  $\gamma_{\text{anticipation}, \tau_0+t}$ ,  $\gamma_{\text{delayed}, \tau_0+t}$  and  $\gamma_{\text{incentive}, \tau_0+t}$  to be different for each (calendar) year. The reason is that in 2001 the re-employment bonuses consisted of cash payments, while in 2002 these were replaced by tax rebates.

In the model the effects of the re-employment bonuses do not depend on actually receiving bonus payments. In the previous section we already indicated that the take-up rates are low. The parameters describing the effects of the re-employment bonus scheme should thus be interpreted as intention-to-treat parameters.

In the specification above, the effect of imposing a sanction ( $\delta$ ) and the effects of the bonus scheme ( $\gamma_{\text{anticipation}, \tau_0+t}$ ,  $\gamma_{\text{delayed}, \tau_0+t}$  and  $\gamma_{\text{incentive}, \tau_0+t}$ ) are modeled as homogeneous effects. We estimate the model separately for men and women, thus allowing for gender-specific effects. In Section 4.3 we allow for additional heterogeneity in both effects. Abbring and Van den Berg (2003) show that within our model framework it is possible to allow the effects of the sanction to depend on the moment that the sanction was imposed, on the elapsed duration since the sanction was imposed and on individual characteristics.

The key identification problem is that we have to distinguish both between true duration dependence  $\lambda_u(t)$  and the bonus effects, and between calendar time effects  $\psi_u(\tau_0 + t)$  and the bonus effects. First, we exploit that from 1 January 2003 welfare recipients were no longer entitled to receiving re-employment bonuses. This implies that  $\gamma_{\text{anticipation}, \tau_0+t}$ ,  $\gamma_{\text{delayed}, \tau_0+t}$  and  $\gamma_{\text{incentive}, \tau_0+t}$  should be zero if  $\tau_0 + t$  exceeds 1 January 2003. Changes in exit rates over the duration of unemployment in 2003 are thus only the consequences of true duration dependence.

Second, to separate true calendar time effects from the effects of re-employment bonuses, we assume that anticipation effects do not start immediately after entering welfare. Changes in exit rates over calendar time shortly after entering welfare are thus only the consequence of true calendar time effects, while such changes for longer durations are the consequence of both calendar time effects and changes in bonus effects. Our identifying assumption is the proportionality of duration dependence and calendar time effects in the exit rate from welfare, which is equivalent to the common trend assumption in difference-in-difference estimation. A usual difference-in-difference approach is, however, not feasible in our setting. Owing to dynamic selection the composition of individuals collecting welfare for a particular time period changes over the business cycle. Furthermore, if there are strong anticipation effects the re-employment bonus scheme changes the composition of individuals on welfare for more than 1 year. Such compositional changes cannot be dealt with easily in the usual difference-in-difference framework. In duration models the process of dynamic selection is taken into account in a natural way.

An alternative identification strategy would be to focus on exact dates at which the rules for the re-employment bonuses changed. By considering exit rates just before and just after the policy change, we could identify the effect of the policy change (e.g. Van den Berg *et al.*, 2010). Such an approach

is equivalent to regression discontinuity and would require smoothness in calendar time effects around the moment of the policy change. Applying this approach is not very attractive in our case: first, because outflow rates from welfare are low, and we therefore observe few exits from welfare in a small interval around the thresholds (in particular when focusing on welfare recipients with spells exceeding 1 year); furthermore, and maybe more important, we lack information about the exact moment at which welfare recipients were informed.

Sanctions are not given randomly (not even conditional on observed characteristics). This suggests a correlation between the timing of a sanction  $t_s$  and unobserved characteristics  $v_u$ . To take account of this endogeneity problem, we jointly model the timing of imposing a sanction with exit from welfare. The rate at which sanctions are imposed on individuals is given by

$$\theta_s(t|x, \tau_0, v_s) = \lambda_s(t)\psi_s(\tau_0 + t) \exp(x\beta_s + v_s)$$

Again  $\lambda_s(t)$  is genuine duration dependence, and  $\psi_s(\tau_0 + t)$  are calendar time effects. The calendar time effects are modeled using dummy variables for each quarter. To account for selectivity in imposing sanctions the unobserved components of the exit rate from welfare and the sanction rate— $v_u$  and  $v_s$ —are allowed to be correlated to each other.

We take the joint distribution of the unobserved heterogeneity terms  $v_u$  and  $v_s$  to be bivariate discrete with unrestricted mass point locations for each term. When having  $K$  possible mass points the associated probabilities are denoted as follows:

$$p_k = \Pr(v_u = v_u^k, v_s = v_s^k) \quad \text{for } k = 1, \dots, K$$

with  $0 \leq p_k \leq 1$ , and  $p_K = 1 - p_1 - p_2 - \dots - p_{K-1}$ . This choice for the distribution of the unobserved heterogeneity terms in the multivariate case most closely resembles the suggestion by Heckman and Singer (1984).<sup>14</sup>

Finally, we parameterize the pattern of duration dependence. We take both  $\lambda_u(t)$  and  $\lambda_s(t)$  to have a piecewise constant specification:

$$\lambda_i(t) = \exp\left(\sum_{j=1,2,\dots} \lambda_{ij} I_j(t)\right) \quad i = u, s$$

where  $j$  is a subscript for time intervals and  $I_j(t)$  are time-varying dummy variables that are equal to one in consecutive time intervals. Note that with an increasing number of time intervals any duration dependence pattern can be approximated arbitrarily closely.

To estimate the parameters in our model, we use maximum likelihood estimation. Our data contain some multiple spells of collecting welfare of the same individuals, which we exploit by assuming that within different spells the unobserved heterogeneity term of an individual remains the same. It is well known that information on multiple spells improves the identification of the distribution of unobserved heterogeneity.

Specifying the log-likelihood function requires that we first introduce some additional notation. Consider a dataset including  $N$  individuals, and individual  $i = 1, \dots, N$  experiences  $J_i$  spells of collecting welfare benefits. Of course, not during all spells is a sanction imposed. The variable  $d_{ij}^s$  describes whether a sanction was imposed during spell  $j$  of individual  $i$ . If no sanction was imposed, the duration until imposing a sanction  $t_{s,ij}$  equals the observed duration of the welfare

<sup>14</sup> Allowing for probability mass for more interactions between mass points does not affect our empirical results.

spell  $t_{ij}$ . Finally, the indicator  $d_{ij}^u$  denotes whether we observe an exit from welfare. The log-likelihood function equals

$$\mathcal{L} = \sum_{i=1}^N \log \left\{ \sum_{k=1}^K P^k \prod_{j=1}^{J_i} \theta_u(t_{ij} | x_{ij}, \tau_{0,ij}, t_{s,ij}, v_u^k)^{d_{ij}^u} \exp \left( - \int_0^{t_{ij}} \theta_u(t | x_{ij}, \tau_{0,ij}, t_{s,ij}, v_u^k) dt \right) \theta_s(t_{s,ij} | x_{ij}, \tau_{0,ij}, v_s^k)^{d_{ij}^s} \exp \left( - \int_0^{t_{s,ij}} \theta_s(t | x_{ij}, \tau_{0,ij}, v_s^k) dt \right) \right\}$$

Note that this log-likelihood function indeed imposes that all spells of one individual share the same unobserved heterogeneity term. We optimize this log-likelihood function over all unknown parameters.

## 4. ESTIMATION RESULTS

### 4.1. Parameter Estimates

Welfare benefits are provided if the household has insufficient income. For a household with a single adult, it is clear who receives the benefits. In the case of a two-adult household, most often the man receives benefits. This generates compositional differences between men and women collecting welfare benefits, which was also shown in Section 2.3. We therefore estimate the model separately for men and women. Table II provides the parameter estimates of the baseline model.

For men we find only two points of support in the distribution of the unobserved heterogeneity, and for women three points of support. This implies that for men conditional on observed characteristics there is perfect negative correlation between the unobserved heterogeneity in the exit rate to work and the sanction rate. Individuals with a higher welfare exit rate have a lower sanction rate. However, the difference in sanction rate between welfare recipients with a high and low rate is very small. About 5.5% of the women have a zero exit rate from welfare. One-third of the women have a relatively high exit rate and a low sanction rate. The remaining women have a relatively high sanction rate, but a lower exit rate from welfare. Thus for both men and women most probability mass (about 61%) is located to welfare recipients with a relatively high sanction rate.

The main parameters of interest are the effects of the re-employment bonus schemes and the benefit sanctions. First, sanctions do have a positive and significant effect on the exit from welfare. The parameter estimate is twice as large for women as for men. Second, the parameters describing the effects of the bonuses are almost all insignificant. For women the parameters have the expected sign, which is not the case for men. For women we find that only for 2002 is the effect of delayed exit from welfare substantial and significant. Recall that the delayed exit effect is the result of individuals anticipating the start of eligibility of the re-employment bonus scheme. The parameter describing the anticipation effect is, however, much smaller and insignificant. This might imply that the period of anticipation is longer than 1 month, which we investigate in the Section 4.2.

A sanction increased the exit rate of men by about 21% ( $= \exp(0.194) - 1$ ). For women this is about 47% ( $= \exp(0.385) - 1$ ). Our findings for women are well in the range of previous studies, such as Abbring *et al.* (2005) finding an increase within 35–100%, Lalive *et al.* (2005) finding an increase of 45% and Svarer (2011) finding an increase of 50%. Our findings for men are relatively small compared to these previous studies. Sanctions are, however, more often imposed on men than on women. A man who did not find work within 6 months after entering welfare has a 6.3% probability of having a sanction imposed in this period, whereas this is only 2.4% for women. In recent years benefit sanctions were used more often than in the 1990s, which might reduce the effects and explain why we find a

Table II. Estimation results of the baseline model

	Males				Females			
	Re-employment rate		Sanction rate		Re-employment rate		Sanction rate	
<i>Unobserved heterogeneity</i>								
v1	-6.283	(0.381)	-8.687	(0.451)	-6.622	(0.334)	-9.583	(3.576)
v2	-6.964	(0.337)	-8.475	(0.371)	-∞	-8.523	(1.403)	
v3					-7.409	(0.365)	-7.479	(0.437)
p1	0.386	(0.467)			0.334	(0.304)		
p2	0.614	(0.467)			0.055	(0.031)		
p3					0.611	(0.288)		
<i>Sanction effect</i>								
Sanction	0.194	(0.085)			0.385	(0.138)		
<i>Bonus effects</i>								
2000 anticipation	-				-			
2000 delayed	-				-			
2000 incentive	-				-			
2001 anticipation	0.082	(0.095)			-0.159	(0.130)		
2001 delayed	-0.103	(0.120)			0.041	(0.145)		
2001 incentive	-0.107	(0.079)			0.061	(0.097)		
2002 anticipation	0.092	(0.101)			-0.081	(0.138)		
2002 delayed	0.071	(0.115)			0.353	(0.137)		
2002 incentive	-0.010	(0.063)			0.059	(0.080)		
2003 anticipation	0				0			
2003 delayed	0				0			
2003 incentive	0				0			
<i>Duration dependence</i>								
λ1-3	0		0		0		0	
λ4-6	0.325	(0.036)	-0.203	(0.070)	0.250	(0.044)	-0.325	(0.104)
λ7-9	0.290	(0.042)	-0.172	(0.078)	0.208	(0.052)	-0.512	(0.119)
λ10-12	0.194	(0.054)	-0.184	(0.089)	0.124	(0.066)	-0.421	(0.126)
λ13-18	0.145	(0.075)	-0.097	(0.086)	-0.006	(0.090)	-0.544	(0.119)
λ19-24	0.157	(0.083)	-0.029	(0.106)	-0.035	(0.097)	-0.524	(0.139)
λ25-36	-0.167	(0.095)	-0.185	(0.119)	-0.253	(0.105)	-0.524	(0.149)
λ37+	-0.248	(0.169)	-0.397	(0.246)	-0.339	(0.195)	-0.379	(0.247)
<i>Covariate effects</i>								
Age (< 25)	0		0		0		0	
Age (25-35)	-0.064	(0.030)	-0.121	(0.054)	0.013	(0.038)	-0.276	(0.079)
Age (35-45)	-0.173	(0.034)	-0.401	(0.064)	-0.087	(0.045)	-0.554	(0.101)
Age (45-55)	-0.459	(0.048)	-0.870	(0.094)	-0.505	(0.058)	-0.850	(0.137)
Age (≥ 55)	-0.674	(0.092)	-1.131	(0.189)	-1.173	(0.129)	-1.478	(0.301)
Children	-0.326	(0.050)	-0.358	(0.095)	-0.676	(0.036)	-0.426	(0.073)
Married	0.007	(0.050)	-0.081	(0.097)	0.312	(0.073)	0.233	(0.145)
Non-Dutch	-0.173	(0.036)	-0.140	(0.064)	-0.288	(0.055)	-0.331	(0.122)
School-leaver	0.128	(0.043)	-0.160	(0.096)	0.245	(0.049)	0.098	(0.106)
Divorce					0.173	(0.045)	-0.428	(0.115)
Lost work	0.076	(0.031)	0.316	(0.054)	0.268	(0.047)	0.374	(0.092)
UI expired	-0.118	(0.037)	-0.019	(0.068)	0.099	(0.057)	0.016	(0.121)
Asylum	-0.475	(0.062)	-0.980	(0.133)				
Joint household	0.282	(0.026)	0.213	(0.048)	0.712	(0.037)	0.013	(0.078)
<i>Calendar time effects</i>								
2000.I	0		0		0		0	
2000.II	0.165	(0.132)	0.833	(0.348)	0.410	(0.181)	-0.606	(0.407)
2000.III	0.051	(0.129)	0.626	(0.346)	0.305	(0.177)	-0.429	(0.370)
2000.IV	-0.136	(0.128)	0.464	(0.343)	0.193	(0.175)	-0.447	(0.357)
2001.I	0.008	(0.125)	0.945	(0.333)	0.330	(0.174)	-0.162	(0.342)
2001.II	0.124	(0.124)	0.776	(0.334)	0.299	(0.174)	-0.220	(0.342)
2001.III	0.067	(0.125)	0.796	(0.333)	0.360	(0.174)	-0.119	(0.336)
2001.IV	0.014	(0.125)	0.537	(0.335)	0.320	(0.174)	-0.363	(0.342)
2002.I	-0.013	(0.124)	0.790	(0.332)	0.258	(0.174)	0.132	(0.330)
2002.II	-0.129	(0.125)	0.845	(0.331)	-0.023	(0.175)	0.173	(0.329)
2002.III	0.071	(0.124)	0.985	(0.332)	0.317	(0.172)	0.158	(0.328)
2002.IV	-0.158	(0.124)	1.002	(0.329)	0.099	(0.173)	0.003	(0.327)
2003.I	-0.206	(0.125)	1.023	(0.329)	0.065	(0.175)	0.210	(0.325)
2003.II	-0.358	(0.126)	0.849	(0.330)	-0.188	(0.176)	0.202	(0.327)
2003.III	-0.834	(0.132)	1.273	(0.329)	-0.403	(0.180)	0.541	(0.327)

Note: standard errors in parentheses.

smaller effect compared to Van den Berg *et al.* (2004). If the welfare agency intensifies the sanction policy, the population of sanctioned individuals changes towards more minor noncompliances. Therefore, most likely the effect of a sanction reduces as punished welfare recipients have to make smaller changes in their (job search) behavior to avoid future sanctions. Furthermore, men are more likely to leave welfare than women. If no sanction would have been imposed, around 24.7% of the men and 18.8% of the women leave welfare within 6 months. If after 3 months a sanction is imposed, the probability that a man leaves welfare within 6 months increases from 24.7% to 27.4%. For women such a sanction would increase the re-employment from 18.8% to 22.7%. In Section 4.3 we discuss in more detail possible mechanisms underlying the different findings for men and women.

To gain some insight into the effects of financial incentives, we use our parameter estimates to perform some model simulations that mimic policy experiments. The probability that an individual with characteristics  $x$ , who enters unemployment at calendar time  $\tau_0$  and who gets a sanction after collecting welfare benefits for  $t_s$  periods, leaves welfare within  $t$  periods equals

$$1 - \sum_{k=1}^K p_k \exp\left(-\int_0^t \theta_u(s|x, \tau_0, t_s, v_u^k) ds\right)$$

We consider an individual entering welfare on 1 January 2001 and focus on exit from welfare within 2 years. We use the empirical distribution for  $x$  from our data to compute average exit probabilities for the population. The Delta method is used to compute standard errors. In the first policy experiment we assume that there is no bonus scheme by setting all parameters associated with the re-employment bonuses equal to zero. Furthermore, in this first experiment there are no sanctions imposed ( $t_s > t$ ). In the second policy experiment we introduce the bonus scheme by setting the parameters associated with the re-employment bonuses to their estimated values. In the third policy experiment we remove the bonus incentives, but assume that a sanction is imposed after 1 year (i.e.  $t_s$  equals 1 year). In the fourth experiment we consider both the sanction after 1 year and the bonus incentives.

Table III shows the results for these simulations. Without the re-employment bonuses scheme and without benefit sanctions, 49.2% of the women and 62.5% of the men exit from welfare within 2 years. The re-employment bonus scheme increases this slightly to 51.7% and 63.0% for women and men, respectively. For women the increase in exit from welfare due to the re-employment bonus scheme is significant; for men it is small and insignificant. The small effect is in line with previous re-employment bonus studies. Imposing a sanction after 1 year of welfare causes a much more substantial increase in the exit from welfare, i.e. the exit rates increases to 54.3% and 65.5% for women and men, respectively, and for both groups the increases are significant. Finally, the two financial incentives combined increase exit from welfare from 49.2% to 57.4% for women and from 62.5% to 66.2% for men. For both men and

Table III. Simulation experiments showing job-finding probability within 2 years after entering welfare on 1 January 2001

	Males		Females	
Sanction within 1 year	10.6%	(0.5)	5.0%	(0.4)
No bonuses, and no sanction	62.5%	(1.1)	49.2%	(1.1)
Bonus, but no sanction	63.0%	(1.0)	51.7%	(1.1)
No bonus, but sanction after 1 year	65.5%	(1.6)	54.3%	(2.2)
Bonus and sanction after 1 year	66.2%	(1.5)	57.4%	(2.3)
Effect of bonus	0.6	(1.2)	2.5	(1.2)
Effect of sanction	3.1	(1.4)	5.1	(2.0)
Effect of bonus and sanction	3.7	(1.9)	8.2	(2.6)

*Note:* Standard errors in parentheses. The simulation experiments use the estimated baseline model and simulate the effects of the re-employment bonus scheme and imposing a sanction. First we report exit rates, and second the effects (compared to no re-employment bonus scheme and no sanction).

women the increase is significant, but it seems that female welfare recipients are more responsive to financial incentives than their male counterparts.

For both men and for women the exit rate from welfare is highest between 4 and 6 months after entering welfare. After being on welfare for 6 months the exit rate decreases monotonically. For both men and women the sanction rate is highest during the first 3 months on collecting welfare benefits. After this period there is no clear pattern in the duration dependence.

The covariate effects of the observed individual characteristics are very often similar for men and women. Individuals with more favorable characteristics are more likely to be punished with a sanction. Older individuals, welfare recipients with children and non-Dutch individuals are both less likely to leave welfare and to get a sanction. For men, those who enter welfare after leaving school and those who enter immediately after losing work are more likely to exit welfare, while individuals who had a period of collecting UI benefits and those who were granted asylum are less likely to leave welfare. For women, entering welfare after asylum is rare, but entering welfare after a divorce is much more common. Therefore, we include the latter as a separate group and add asylum to the other category. For women the ranking of exit rates by reason of inflow is similar to that of men.

The calendar time effects show that at the end of 2002 exit rates started to drop. Recall that our observation period is characterized by a declining GDP growth rate since the end of 1999.

## 4.2. Sensitivity Analysis

In this subsection we present a few sensitivity analyses to investigate the robustness of our estimation results with respect to the model specification. We focus on the importance of controlling for selection in imposing sanctions, the specification of business cycle effects and anticipation effects for re-employment bonuses. Table IV shows the results of these sensitivity analyses for the effects of the re-employment bonus scheme and imposing sanctions.

In the first sensitivity analysis, we ignore that sanctions are imposed selectively. This implies that there is no correlation in the unobserved heterogeneity terms in the exit rate from welfare and the sanction rate. The exit rate can thus be estimated without jointly estimating the sanction rate. It does not mean that unobserved heterogeneity is absent. Panel A of Table IV presents the estimated sanction and bonus effects. The only substantial difference is that the size of the sanction effect for women reduces to about half the value. Compared to the results for men, the baseline model for women had much more dispersion in unobserved heterogeneity. This explains why we only find a substantial change in the effect for women.

The identification of the bonus effects relies on accounting correctly for calendar time effects. In the baseline model, we included dummy variables for each quarter. We replace these by business cycle indicators. In particular, we use nationwide GDP growth and as local business cycle indicator the number of job vacancies over the number of non-employed job seekers. Job vacancies registered at the local public employment office are estimated to be about 25% of all vacancies, but they are mainly the vacancies for low-skilled jobs, most relevant for welfare recipients. The number of non-employed job seekers are those registered at the public employment office, thus including recipients of both UI and welfare benefits. Additionally, we include year dummies and seasonal dummies. Panel B of Table IV shows that the estimated effects of sanctions and re-employment bonuses are very similar to the baseline model.

In the next sensitivity analysis we consider the length of the anticipation period prior to becoming eligible for the re-employment bonus. In the baseline model, we imposed that 1 month prior to becoming eligible welfare recipients can change their behavior. The estimation results did not show any significant anticipation effects, but in the sensitivity analysis we extend the anticipation period to 2 months. As can be seen in panel C of Table IV, both for men and women, the estimated anticipations

Table IV. Sensitivity analyses

	Males		Females	
<b>(A) No selectivity</b>				
Sanction on exit	0.159	(0.039)	0.201	(0.061)
<i>Bonus effects</i>				
2001 anticipation	0.014	(0.079)	-0.173	(0.129)
2001 delayed	-0.106	(0.121)	0.063	(0.143)
2001 incentive	-0.110	(0.079)	0.060	(0.094)
2002 anticipation	0.054	(0.083)	-0.095	(0.137)
2002 delayed	0.072	(0.115)	0.376	(0.136)
2002 incentive	-0.010	(0.064)	0.061	(0.078)
<b>(B) Polynomial in calendar time</b>				
Sanction on exit	0.184	(0.080)	0.373	(0.137)
<i>Bonus effects</i>				
2001 anticipation	0.111	(0.095)	-0.108	(0.129)
2001 delayed	-0.085	(0.120)	0.068	(0.145)
2001 incentive	-0.050	(0.078)	0.122	(0.096)
2002 anticipation	0.092	(0.100)	-0.074	(0.138)
2002 delayed	0.083	(0.115)	0.369	(0.137)
2002 incentive	-0.003	(0.063)	0.067	(0.078)
<b>(C) Anticipation during two months</b>				
Sanction on exit	0.195	(0.085)	0.386	(0.138)
<i>Bonus effects</i>				
2001 anticipation	0.014	(0.079)	-0.048	(0.100)
2001 delayed	-0.106	(0.121)	0.044	(0.145)
2001 incentive	-0.110	(0.079)	0.065	(0.098)
2002 anticipation	0.054	(0.083)	-0.033	(0.110)
2002 delayed	0.073	(0.115)	0.354	(0.137)
2002 incentive	-0.010	(0.064)	0.060	(0.080)

Note: Standard errors in parentheses. All three sensitivity analyses estimate separate models including the same covariates and the same specification of duration dependence and unobserved heterogeneity as in the baseline model.

effects become smaller compared to the baseline model. This shows that anticipation more than 1 month before becoming eligible for the re-employment bonus is even less strong than in the month before becoming eligible.

### 4.3. Heterogeneous Effects

Our parameter estimates show that, on average, sanctions affect the exit from welfare, while there is no strong effect of the re-employment bonus schemes. However, the model specifications used so far are somewhat restrictive as they assume that sanctions and bonus schemes have a homogeneous and permanent effect on the exit from welfare. By estimating the model separately for men and women we only allowed for gender differences in the effects. In this subsection we provide estimation results for models with heterogeneous effects of both policies. This should provide more insight into the underlying mechanism of negative financial incentives having a larger effect than positive financial incentives.

There are a few reasons why bonuses might have not been very effective in stimulating re-employment. First, recall from Section 2.3 that the take-up rate of the first bonus payment is less than 50%. This is not because welfare recipients are unaware of the re-employment bonus scheme. Caseworkers are instructed to inform welfare recipients about the existence of the scheme. Furthermore, at the moment of a change in the scheme, all welfare recipients received a letter. Changes in the re-employment bonus scheme always take place in January and this is the moment at which the welfare recipients receive written information. It might be that there is more emphasis on the re-employment bonus scheme at the start of each calendar year, while welfare recipients forget about the bonus later on in the year. Therefore,

we allow the effect of the re-employment bonus scheme to be different during the first half and the second half of each calendar year. Panel A of Table V shows the estimation results for this extension of the baseline model. The estimation results do not provide any evidence that the effects of the re-employment bonus scheme are smaller during the second half of the year than during the first half of the year.

The re-employment bonus only provides a positive financial incentive for individuals who collected welfare benefits for at least 1 year. Sanctions can be imposed immediately after entering welfare. In the next extension of the baseline model we allow the effect of the sanctions to depend on the moment they were imposed. In particular, we allow the effect to be different during the first year of collecting welfare benefits and in the later period. The effect of the sanction is thus specified as

$$\delta = \delta_1 I(t_s \leq 1 \text{ year}) + \delta_2 I(t_s > 1 \text{ year})$$

Panel B of Table V shows the parameter estimates for  $\delta_1$  and  $\delta_2$ . For men who collected welfare benefits for more than a year, the effect of a benefit sanction is larger than during the first year of welfare. For women the opposite is the case. Thus there is no strong evidence that the absence of incentive effects of the re-employment bonus scheme is due to the fact that long-term welfare recipients do not respond to financial incentives at all.

The effect of a sanction is larger for women than for men. In Section 2.3 we showed that women get different types of sanctions. Therefore, we investigate to what extent this might explain why the effect of a sanction is different for men and women. If welfare recipients face liquidity constraints, one might expect larger sanctions to have a bigger effect.<sup>15</sup> Panel C of Table V does not show a strong association between the size of the sanction and its effect.

Another way to consider liquidity constraints is to let the effect of the sanction depend on the elapsed duration since the sanction was imposed. Sanctions usually only reduce benefits for 1 or 2 months. After this period the welfare recipient again receives the usual welfare benefits level. Thus, during a short period after the sanction has been imposed, the welfare recipient is more liquidity constrained. Therefore, we allow the effect of the sanction to depend on the moment since the sanction has been imposed, so

$$\delta = \delta_1 I(t - t_s \leq 3 \text{ months}) + \delta_2 I(t - t_s > 3 \text{ months})$$

Panel D of Table V shows that for men the effect of a sanction decreases 3 months after the sanction has been imposed. The opposite is the case for women. However, both for men and for women the change in the sanction effect is not significant.

Finally, there are compositional differences between male and female welfare recipients. By allowing the effects of the financial incentives to depend on observed individual characteristics, we can investigate to what extent different types of welfare recipients respond differently to financial incentives. Panel E of Table V shows the relevant parameter estimates. For men the effect of the sanction seems to depend on the household situation. For married men the effect of a sanction is larger, while it is smaller for men with children. For women the effect a sanction increases with age (except for women above age 55). For the effects of the re-employment bonus scheme, it is difficult to see any pattern in how the demographic characteristics of a welfare recipient affect the exit rate.

All in all, the results discussed in this subsection do not provide strong evidence for heterogeneous treatment effects once gender is accounted for. We think that the evidence on heterogeneous treatment effects is too thin to draw strong policy conclusions on the basis of our research. The main heterogeneity in treatment effect that remains is our finding that women respond more strongly to sanctions than

<sup>15</sup> Note that one should be careful in interpreting the results since the size of the sanction is not exogenous.

Table V. Heterogeneous effects

	Males		Females	
<b>(A) Re-employment bonus effects different during the year</b>				
Sanction on exit	0.194	(0.085)	0.382	(0.138)
<i>Bonus effects</i>				
2001.I anticipation	-0.067	(0.108)	-0.374	(0.203)
2001.II anticipation	0.081	(0.099)	-0.005	(0.161)
2001.I delayed	-0.086	(0.158)	-0.213	(0.224)
2001.II delayed	-0.139	(0.162)	0.224	(0.172)
2001.I incentive	-0.284	(0.126)	0.135	(0.151)
2001.II incentive	-0.048	(0.085)	0.058	(0.105)
2002.I anticipation	-0.105	(0.117)	-0.090	(0.200)
2002.II anticipation	0.181	(0.102)	-0.077	(0.182)
2002.I delayed	0.125	(0.150)	0.389	(0.186)
2002.II delayed	0.005	(0.156)	0.336	(0.174)
2002.I incentive	-0.034	(0.078)	0.011	(0.089)
2002.II incentive	0.012	(0.073)	0.382	(0.138)
<b>(B) Sanction effect dependent on welfare duration</b>				
During first year	0.139	(0.088)	0.417	(0.141)
After first year	0.321	(0.107)	0.270	(0.182)
<b>(C) Sanction effect dependent on size of sanction</b>				
5% reduction	0.214	(0.098)	0.345	(0.157)
10% reduction	0.223	(0.109)	0.297	(0.183)
10% reduction	0.228	(0.137)	0.361	(0.268)
other	0.136	(0.100)	0.500	(0.162)
<b>(D) Sanction effect dependent on elapsed duration</b>				
First 3 months	0.264	(0.096)	0.315	(0.160)
After 3 months	0.157	(0.090)	0.414	(0.149)
<b>(E) Effects dependent on observed characteristics</b>				
Sanction on exit	0.178	(0.099)	0.274	(0.162)
Age (< 25)	0		0	
Age (25-35)	-0.016	(0.088)	0.103	(0.152)
Age (35-45)	0.036	(0.109)	0.274	(0.181)
Age (45-55)	0.040	(0.183)	0.457	(0.255)
Age (≥ 55)	0.080	(0.393)	0.057	(0.748)
Children	-0.463	(0.188)	0.061	(0.137)
Married	0.514	(0.183)	-0.127	(0.291)
<i>Bonus effects</i>				
2001 intercept	0.169	(0.129)	-0.181	(0.185)
2002 intercept	0.178	(0.137)	-0.102	(0.190)
Age (< 25)	0		0	
Age (25-35)	-0.181	(0.158)	0.254	(0.216)
Age (35-45)	-0.176	(0.182)	0.276	(0.251)
Age (45-55)	-0.026	(0.240)	-0.504	(0.412)
Age (≥ 55)	-0.310	(0.522)	-∞	
Children	0.127	(0.286)	-0.150	(0.190)
Married	0.052	(0.278)	0.074	(0.404)
2001 delayed	-0.072	(0.158)	0.134	(0.199)
2002 delayed	0.088	(0.162)	0.445	(0.181)
Age (< 25)	0		0	
Age (25-35)	-0.119	(0.180)	-0.158	(0.199)
Age (35-45)	-0.030	(0.201)	-0.273	(0.250)
Age (45-55)	0.368	(0.242)	-0.324	(0.336)
Age (≥ 55)	-0.198	(0.626)	0.155	(0.660)
Children	0.402	(0.373)	0.042	(0.175)
Married	-0.515	(0.414)	0.167	(0.365)
2002 incentive	-0.034	(0.092)	0.150	(0.117)
2002 incentive	0.062	(0.078)	0.145	(0.103)
Age (< 25)	0		0	
Age (25-35)	-0.114	(0.074)	-0.200	(0.087)
Age (35-45)	-0.134	(0.084)	-0.104	(0.101)

(Continues)

Table V. (Continued)

	Males		Females	
Age (45–55)	0.038	(0.110)	–0.522	(0.151)
Age ( $\geq 55$ )	–0.550	(0.253)	–0.348	(0.308)
Children	0.266	(0.119)	0.088	(0.080)
Married	–0.221	(0.121)	–0.043	(0.178)

Note: Standard errors in parentheses. All panels estimate separate models including the same covariates and the same specification of duration dependence and unobserved heterogeneity as in the baseline model.

men. Somewhat speculatively, one could conclude that this may have to do with men already having a higher exit rate from welfare so there is less scope for a change in behavior.

## 5. WHAT EXPLAINS OUR FINDINGS?

Our main findings are that the re-employment bonuses do not seem to have worked, while benefit sanctions increased the exit rate from welfare significantly. The latter finding is in line with previous empirical studies and is also in line with a search theoretical framework in which benefit sanctions increase the costs of remaining unemployed, thus stimulating welfare recipients to find a job. However, re-employment bonuses should also increase job finding because the value of accepting a job increases. What explains the differences in our findings concerning the effects of benefit sanctions and re-employment bonuses? It could be that welfare recipients are ‘impatient’, i.e. they assign a lower value to future benefits of job search and therefore exert less effort to find a job. In addition to this, welfare recipients may have so-called hyperbolic time preferences, i.e. they are ‘present biased’ in the sense that in the short run they discount highly whereas in the long run they discount less.

Della Vigna and Paserman (2005) investigate the relevance of impatience and hyperbolic discounting in job search decisions.<sup>16</sup> They argue that workers who are more impatient search less intensively and set lower reservation wages. With normal time preferences the effect of impatience on job-finding rates is unclear as the decrease in search intensity lowers the job-finding rate, while the lower reservation wages increase the job-finding rate. With hyperbolic time preferences the search effect dominates, and increases in impatience lead to lower job-finding rates. Analyzing NLSY and Panel Study of Income Dynamics (PSID) data, Della Vigna and Paserman (2005) conclude that the effect of impatience on search effort is negative and sizable; the effect of impatience on reservation wages and re-employment wages is essentially zero.

With this in mind we explain our findings as follows. The low take-up rate and the ineffectiveness of the re-employment bonuses are not very surprising. Whereas in the USA re-employment bonuses were provided at the start of unemployment spells, in Rotterdam only long-term welfare recipients were eligible for a re-employment bonus. The delay in payments might well limit the incentives from a re-employment bonus.<sup>17</sup> In 2001, for example, the maximum bonus of €1800 (1.6 × monthly minimum wage) is substantial, but paid in four equal amounts of €450, each 6 months after the start of the job. Thus the last payment would occur 2 years after the start of the job. The balance between short-run

<sup>16</sup>There are only a few studies empirically investigating the existence of hyperbolic discounting in labor market decisions. A rare example is Fang and Silverman (2009), who investigate the relationship between time discounting and welfare program participation decisions using National Longitudinal Survey of Youth (NLSY) data. They find evidence of a present bias and conclude that limiting the duration of welfare benefits may substantially increase employment. Under certain conditions also workfare may be useful, not only to increase employment but also increase lifetime utility of welfare recipients.

<sup>17</sup>Restricting re-employment bonuses to the long-term unemployed also implies that only workers with a bad labor market position qualified. Through a process of dynamic selection the most suitable workers have probably left the welfare system before becoming eligible for re-employment bonuses.

costs in job search and long-run benefits of the re-employment bonus may not be positive; thus welfare recipients had no incentive to increase their search intensity.

The effectiveness of benefit sanctions can be explained in a traditional job search model. Nevertheless, hyperbolic discounting has interesting welfare implications. Paserman (2008) argues that with hyperbolic discounting benefit sanctions might even be welfare improving for individual unemployed workers.<sup>18</sup> Because some workers are present-biased, an optimal unemployment benefit policy takes the form of monitoring search effort and imposing sanctions on workers who do not meet an effort threshold. This policy has a positive effect on the hyperbolic worker's long-run utility. Time-inconsistent workers may benefit particularly from policies that commit them to a higher search intensity.

## 6. CONCLUSIONS

It is not easy to bring welfare recipients back to work. Expensive active labor market programs are often not very effective. Low-cost programs focusing on monitoring and providing financial incentives seem to do a better job. We study the exit from welfare in the municipality of Rotterdam, where benefit recipients were exposed to various financial incentives. Their benefits could be reduced temporarily as punishment for noncompliance with eligibility requirements. However, they could also be rewarded for 'good' behavior. Once their unemployment spell lasted more than 1 year, they were entitled to a re-employment bonus if they found a job. In this paper we investigate how financial sticks in the form of benefit sanctions and financial carrots in the form of re-employment bonuses affected exit rates from welfare.

In line with previous studies we find substantial effects of benefit sanctions and no effects of re-employment bonuses. Previous US-based studies find positive effects of re-employment bonuses but these effects are often rather small. When comparing the incentive structure in the US bonus experiment with the re-employment bonus in Rotterdam, it is not surprising that we find very small effects. Whereas in the USA re-employment bonuses are paid at the start of the unemployment spell to unemployed with low and short-lasting benefits, in Rotterdam re-employment bonuses were offered to unemployed workers with relatively high and long-lasting benefits, the earliest after 1 year of unemployment when the job-finding rate was small anyway. In addition to this, there-employment bonus policy in Rotterdam changed frequently, such that every year a new system was in place. For researchers such variation is very helpful in identifying treatment effects, but for welfare recipients these changes may have been confusing.

Apart from institutional reasons why re-employment bonuses were not as effective as benefit sanctions, there may also be behavioral reasons related to the timing of effort and reward for that effort. Our findings that a carrot does not work whereas a stick does may be related to present bias of some workers. To the extent that some welfare recipients are present biased, an incentive scheme that requires immediate search effort in exchange for delayed rewards in terms of a future bonus may not be an effective scheme. The implication for labor market policy is that direct assistance that forces unemployed people to overcome their procrastination problem and to go through the most unpleasant steps of the search process is likely to be beneficial. The government's threat to cut benefits if the welfare recipient does not exert sufficient search effort acts as a commitment device. Benefit sanctions breaking the present bias by imposing immediate costs to lack of search effort might indeed be an effective and welfare-improving scheme.

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<sup>18</sup>Paserman (2008) compares different types of labor market policies: cutting the level of unemployment benefits, shortening the duration of benefits, changing the time profile of unemployment benefits, job search assistance, monitoring search intensity, monitoring job acceptance and re-employment bonus. The different policies are evaluated from the perspective of an individual who is not currently unemployed when deciding whether to implement a change to the UI system.

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