Punitive Sanctions and the Transition Rate from Welfare to Work

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Abstract

In The Netherlands, the average exit rate out of welfare is dramatically low. Most welfare recipients have to comply with guidelines on job search effort that are imposed by the welfare agency. If they do not, then a sanction in the form of a temporarily benefit reduction can be imposed. This paper investigates the effect of such sanctions on the transition from welfare to work using a unique set of rich administrative data on welfare recipients in The Netherlands. We find that the imposition of sanctions substantially increases the individual transition rate from welfare to work. We also describe the other determinants of the transition from welfare to work.

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1 Introduction

In many OECD countries, the rate at which welfare recipients leave welfare is typically very low, even though welfare programs differ substantially between countries. In the U.S. it is typically used to support single-parent households, whereas in European countries it is also often used to support long-term unemployed workers.[1] Welfare (or "social assistance") then acts as a safety net for those unemployed workers who are not entitled (anymore) to any other social security benefits like unemployment insurance or disability benefits. Whatever their designs, the current welfare systems are subject to criticisms, and policy makers see a need to restructure it, in particular in order to stimulate the transition from welfare to work (see for example Gueron, 1990, and Moffitt, 1992). In principle, a large range of policy measures is available to prevent the unemployed from becoming dependent on welfare and to stimulate and assist the long-term unemployed in their search for jobs. Examples are subsidized employment for youth and long-term unemployed, training and schooling programs, special public employment services, and punitive benefit reductions.

In this paper we evaluate the effect of sanctions that are imposed if the welfare recipient does not comply with the minimum job search requirements and rules of registration laid out by the welfare agency. In particular, we evaluate the effect of a sanction on the duration until exit into work. A sanction consists of a temporary reduction of the welfare benefits level. The duration and size of the reduction depend on the nature of the infringement. Frequently used reasons for imposing sanctions are insufficient job search activity, fraud, unnecessary job loss and lack of willingness to participate in education or training programs (below we examine this in more detail).

Although income support has always been the primary aim of social security in general and welfare in particular, there is an increasing interest in stimulating welfare recipients to find a job. It is commonly felt that the Dutch social security system imposes a heavy burden on the economy and therefore a strong policy toward abuse is necessary to maintain support for at least the essential features of this system. The use of sanctions by welfare agencies to stimulate re-employment or prevent fraud is relatively new. Before 1992, sanctions were hardly ever used. By the mid-nineties, about 5% of the welfare recipients in a given year received a sanction. It should be noted that, in The Netherlands, the state guarantees

[1] European labor markets are characterized by a low inflow into unemployment and a high average duration of unemployment (see Bean, 1994, and Layard, Nickell and Jackman, 1991, for surveys).

the provision of a minimum income to each citizen in urgent financial need. This effectively restricts the magnitude of sanctions in welfare, and indeed it creates an upper bound on the harshness of a system with sanctions. As a result, the duration and size of the benefit reductions are relatively low in comparison to those for unemployment insurance (UI) recipients. Most welfare sanctions are only for one or two months and the maximum reduction of the welfare benefit is 20%.

It is important to stress that there is a difference between (1) the effect of actually imposing a sanction and (2) the effect of having a welfare system with sanctions as opposed to a welfare system without sanctions. The second effect, which is a preventive or ex-ante effect, is positive if the mere existence of a welfare system with sanctions stimulates the transitions from welfare to work. Our data are from a world with sanctions, so our reduced-form empirical analysis of micro duration data can not be used to evaluate the magnitude of this ex-ante effect. Concerning the ex-post effect, the benefit reduction that is involved probably makes the individual more prone to accept jobs and to search more intensively. However, a sanction is more than just a mechanic temporary reduction in benefits. The sanction is induced by a failure to oblige certain job search requirements, and the welfare agency will motivate its decision to the individual. Furthermore, individuals are closely monitored after a sanction, so they have an incentive to comply with the search requirements in order to prevent additional punishments. All this is likely to increase the search intensity of the individual from the moment at which the sanction is imposed onwards.[2] In sum, imposition of a sanction is expected to increase the exit rate out of welfare into work.

A substantial part of the literature on the effects of back-to-work policy programs focuses on the effect of program participation on future earnings (Heckman, Smith and Clements, 1997) or on cost-benefit analysis of such programs (Bell and Orr, 1994). Some studies focus on the effect on the transition rate from unemployment to employment, correcting for selection bias in a non-experimental context (see Bonnal, Fougère and Sérandon, 1997, and Gritz, 1993, who focus on training programs).[3] The empirical analysis in the present study closely follows the ap-

[2]There is evidence that an increase in search intensity increases the transition rate from unemployment to employment (see Devine and Kiefer, 1991, for a survey). Gorter and Kalb (1996) and Dolton and O'Neill (1996) estimate the effect of interviews that are supposed to provide advice and counseling to UI recipients. Both find a significant and lasting effect on the transition rate from unemployment to employment. Meyer (1995) finds significant effects of similar treatments in experiments across the U.S. ("search experiments").

[3]See also Cockx and Ridder (1996) who use a natural-experiment methodology to examine the effect of subsidized employment on the exit rate out of welfare in Belgium, and Ham and

proach developed in Abbring, Van den Berg and Van Ours (1997), who analyze the effect of sanctions in UI on the exit rate out of unemployment.[4] The main problem in any empirical analysis of sanctions concerns the endogenous selection involved in the imposition of sanctions. It is clear that sanctions are imposed by the welfare agency in response to the behavior of the welfare recipient. Welfare recipients who get a sanction are most likely different from other welfare recipients. Neglecting this gives a biased estimate of the sanction effect. Therefore, we model both process by which welfare recipients get a sanction and the process by which they leave unemployment. The two processes are allowed to be interdependent by way of their unobserved determinants and by way of a direct effect of a realized sanction on the transition rate to employment (this approach is similar to that used by Bonnal, Fougère and Sérandon, 1997, and Gritz, 1993). We allow the rate at which a sanction is imposed to depend on observed explanatory variables, on the elapsed unemployment duration, and on unobserved determinants (we use a Mixed Proportional Hazard (MPH) specification). For the duration dependence we take a flexible piecewise constant specification. The exit rate out of unemployment into employment is modeled in a similar way, with the qualification that one of its explanatory variables depends on the actual state of the sanction process. We identify the causal effect of a sanction on the transition rate from welfare to work by exploiting the information in the timing of the events we observe (imposition of a sanction and/or exit to work).

The outline of this paper is as follows. Section 2 gives a detailed description of the Dutch welfare system. We discuss the eligibility requirements for receiving a welfare benefit, the guidelines that a recipient must comply with, the sanctions for noncompliance, and the way sanctions are actually imposed. In Section 3 we discuss our model which is based on job search theory. Section 4 discusses the unique database we use to estimate the model. This database covers all unemployed individuals who started to collect welfare benefits in Rotterdam in 1994 and contains information about them until they left the welfare system or until October 1996, whichever was later. In Section 5 we present the estimation results, and we perform some sensitivity analyses. Section 6 concludes.

LaLonde (1996) who use experimental data to examine the effect of training on the transition rate to work for AFDC (Aid to Families with Dependent Children) claimants.

[4] Obviously, the populations of UI and welfare recipients are very different in terms of their background and their opportunities. UI recipients have better labor market prospects, since they necessarily have a substantial amount of recent work experience. This may make them more sensitive to financial stimuli. On the other hand, UI benefits are usually higher than welfare benefits, and this may make the UI recipients less sensitive to such stimuli. See also Section 2.

2 Welfare recipients in The Netherlands

2.1 Entitlement

In this section we describe some institutional aspects of the Dutch welfare (or "social assistance") system in the mid-nineties. It is not our intention to give an exhaustive description of the system. Instead, we explain the basic structure and highlight aspects that are relevant for our purposes. We rely on some publications in Dutch on welfare in The Netherlands (Angenent, Bommeljé and Schep, 1993, 1994; Angenent and Den Heeten, 1995).

The Netherlands has about 16 million inhabitants of which 6 million are employed workers. The aim of welfare is to support people without income who are not entitled to any other social security benefits. In addition, the individual must (*i*) be legally allowed to stay in The Netherlands, and (*ii*) be over 18 years. In 1994, 485.000 individuals without work received welfare benefits. Of these, 320,000 are counted as unemployed; these have a formal obligation to search for a job. The remaining 165,000 individuals received welfare benefits without an obligation to search for a job. Of the latter group, 55% belongs to a single parent household with children aged below 12 years (welfare for the latter type of individuals is similar to AFDC in the U.S.). In the sequel we ignore the recipients who do not have an obligation to search for a job, since both the rate of getting a sanction and the rate of finding a job are determined in a very different way than for the other recipients. For simplicity we will use the term "welfare recipient" to denote just those recipients who have an obligation to search for a job and who are counted as unemployed.

The welfare benefits level can be decomposed into a basic level and a series of bonuses. The basic level is fully determined by the household composition and by the extent to which other sources of income and assets are available. Thus, welfare benefits are means-tested. If the applicant has a partner with a sufficiently high income out of labor, or if the applicant has a sufficiently high amount of assets (like a house), then in general he does not qualify for welfare. Concerning the level of benefits, one may distinguish between four household categories.[5] In 1995, the net benefits level for a two-parent family (i.e., a married couple with or without children) was about 1800 Dutch guilders per month. For a single-parent family, this was about 1600 guilders. Finally, for a single individual aged over 23 it was about 1250 guilders, whereas for a single individual aged below 23 it was about 900 guilders.

[5]There are a few other cases that are less common; see e.g. Van Andel and Bommeljé (1996).

Municipalities have power to provide bonuses on top of the basic benefits level. For example, some municipalities pay bonuses for the use of sports facilities and public transport, or for health-related expenses like glasses. The types of bonuses, the rules on entitlement to a bonus and the levels of the bonuses vary considerably across different municipalities.[6]

In 1994, about 35% of the welfare recipients had been collecting welfare benefits for an uninterrupted duration of more than 3 years. Of the welfare recipients, 68% is single, 25% is married and only 7% belongs to a single parent family. Welfare recipients often have low skills. The fraction of individuals with primary education is 15% for the whole labor force but 35% for the welfare recipients. The age structure of the population of welfare recipients is about the same in the labor force.

One may distinguish between two types of individuals among the welfare recipients. The first are workers who enter unemployment after leaving full-time education. The second are job losers, i.e. workers with a history of labor force attachment. The workers in this group have either run out of eligibility for UI benefits or never collected UI benefits because they did not meet eligibility criteria at the start of their unemployment spell. The maximum duration of UI depends on the employment history of the individual and ranges from 6 months to 4.5 years. Note that the individuals entering welfare from UI are a selective sample of the inflow into UI. On average, the more disadvantaged individuals eventually move to welfare. In the inflow into welfare, the group of school leavers is much smaller than the group of job losers (10% versus 90%). There is also a large difference between the exit rates of the two groups (65% and 35% within a year, respectively; it should be noted that most welfare recipients under 21 years participate in youth job guarantee programs after having been on welfare for 6 months). In this study we restrict attention to the second group of welfare recipients, the job losers. These are more important both from a quantitative point of view (a large fraction of the inflow into welfare) and from a qualitative point of view (a small exit rate out of welfare), and it is obvious that the behavior in the two groups cannot be captured in a single model. In the remainder of the paper we refer to this second group as *the* welfare recipients, thereby omitting the qualification of them being job losers.

[6]This provides an incentive for welfare recipients to move to other municipalities. However, the housing market for lower income households is highly regulated, and in many municipalities the average waiting time for a house is very long, in particular if there are no household members with a job in that municipality. Since we only have data on recipients in the municipality of Rotterdam, we cannot exploit this feature to define a natural experiment.

A welfare recipient has several obligations in order to remain eligible for a benefit: he has to (*i*) prevent unnecessary job loss, (*ii*) take actions to prevent him from staying unemployed, so he has to search for a job and accept appropriate job offers, register at the public employment office, participate in education and training, etc., and (*iii*) keep the welfare agency informed about everything that is relevant to the payment of welfare benefits.

Although welfare recipients are obliged to search for a job, not all of them seem to be willing to work. A survey on attitudes of welfare recipients reveals that about 10% of the welfare recipients state that they do not want to have a job. The 90% that does want to work is not always eager to find a job (Angenent, Bommeljé and Schep, 1994). Only 55% indicate that they are actively searching for a job. Furthermore, a lot of workers only accept a job if the net wage is at least 250 Dutch guilders per month above the welfare benefits level. Finally, half of the welfare recipients indicate that they do not want to move if that is required for a new job.

2.2 Sanctions

Sometimes sanctions are imposed to punish welfare recipients because of administrative reasons like returning late from holiday, filling in forms incorrectly, etcetera. Nevertheless, the main reason to impose sanctions is noncompliance with job search guidelines. Previous studies on the Dutch welfare benefit system argued that sanctions can not affect the transition rate from welfare to work (Angenent, Bommeljé and Schep, 1993). Their argument runs as follows. Since welfare recipients have a weak labor market position, sanctions only induce apparent changes in behavior. Sanctioned welfare recipients or recipients at risk signal an increased search intensity while in reality they do not make an additional effort. For example, a welfare recipient may show the welfare officer fake application letters. Or, the welfare recipient may perform "negative" applications, by acting during an application interview in such a way as to minimize the probability of being hired. The worker transmits signals of lack of interest to the employer while at the same time it appears that he complies with job search guidelines.[7]

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

[7] A strategy in which individuals take a job upon imposition of a sanction, and quit immediately in order to make a "fresh start" in UI, would not be optimal: UI would be reduced immediately after quitting because of "lack of action to prevent job loss" (see Abbring, Van den Berg and Van Ours, 1997).

the welfare agency. Information on possible offenses can come from the monthly form a welfare recipient has to fill in, or from a conversation between an employee 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. About 90 percent of the cases of noncompliance is established in the so-called "re-investigation", which is a standard procedure that usually takes place 8 months after the start of collecting the welfare benefits. If a sanction is imposed because of insufficient job search activity, then the welfare agency is obliged to re-examine the job search activities of the welfare recipient within 3 months after the imposition. (If a sanction is imposed for other reasons then the welfare agency is not obliged to do so, although it often does.) Based on the outcome of the renewed examination, the welfare agency may decide to renew the sanction or punish the welfare recipient with a higher sanction. Our data show that in practice sanctions are almost never renewed.

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. 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. Welfare employees 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 like attitude, appearance and motivation (Angenent, Bommeljé and Schep, 1993). These are all characteristics that are unobserved by the researcher trying to investigate the effect of sanctions. A 1992 investigation of 3500 personal files shows that, even though noncompliance was established for approximately 10% of the welfare recipients, only about 5% did get a sanction. Noncompliance is more common among young welfare recipients. And, conditional on noncompliance, younger welfare recipients are more likely to get a sanction imposed. This may be because of the better labor market position of younger workers. Lower educated workers more often do not obey the search rules, but conditional on this they are less likely than higher educated workers to get a sanction imposed.

Again, the bad labor market position of lower educated workers may explain the difference. In Section 5 we examine whether these results are confirmed by our multivariate analysis.

The period between the establishment of noncompliance and the imposition of a sanction is usually 1 to 2 months. In some cases it may take years before noncompliance is established. This may happen if there are only postal investigations with respect to the behavior of the welfare recipient.

Although sanctions did exist before 1992, they were hardly ever imposed. By instruction of the Ministry of Social Affairs and Employment, the welfare agencies started to use sanctions as an instrument to stimulate re-employment of welfare recipients and as an instrument against fraud[8] at the end of 1992.

Now let us examine the two most important features of a sanction: its magnitude and its duration. These depend first of all on the nature of the infringement and the extent to which a welfare recipient can be held responsible for the infringement. There are general guidelines for the imposition of sanctions, but again the welfare agency may take individual circumstances of the welfare recipient into account. As explained in Section 1, the magnitude and the duration of the benefits reductions are limited. The reduction is either 5%, 10% or 20% of the benefits level. The duration of the reduction can be up to 6 months, but is usually only 1 or 2 months. According to the official guidelines there are four categories of sanctions: (1) If a welfare recipient does not register or renew his registration at a public employment office, a benefit reduction of 5% during 1 month is recommended. (2) A sanction of 10% during 1 month is recommended if a welfare recipient insufficiently searches for a job, neglects appointments at the welfare agency and does not cooperate in the search for appropriate training programs. (3) If the welfare recipient's behavior interferes with searching for a job or if he refuses training, a sanction can be imposed with a reduction of 20% during 1 month. (4) A benefit reduction of 20% during 2 months is recommended if the welfare recipient refuses an appropriate job offer or did not prevent unnecessary job loss prior to entering welfare.

[8]If a welfare recipient withholds information e.g. in order to get higher welfare benefits, then this is considered as fraud. Depending on the size of the fraud the welfare agency decides whether a sanction should be applied or whether legal prosecution is necessary.

3 The model

3.1 A theoretical framework

In this section we present the model that we estimate in the empirical analysis. The empirical model specification is motivated by a theoretical job search model framework in which punitive sanctions are incorporated. The latter model framework has been developed by Abbring, Van den Berg and Van Ours (1997). Here we merely sketch it (taking into account the modifications due to the fact that we consider welfare rather than UI), and we list its most important properties. Subsequently, we discuss the empirical model, and at the end of the section we discuss the parameterization of our model.

The point of departure is the basic job search model with endogenous search effort s as presented by e.g. Mortensen (1986). In this framework, sanctions can be incorporated as temporary benefit reductions, which are imposed at a certain rate if the job search intensity s is under some threshold value. It is useful to distinguish between sanctions as an institutional aspect of the environment of the individual, and the actual imposition of a sanction for an individual. Concerning the former, one may argue that the mere threat of a sanction should suffice to prevent it from ever being enforced. It is clear that the data contradict such a view. Alternatively, one may argue that the moment of occurrence of a sanction is perfectly foreseen by the benefits recipient and is taken into account in determining his choices. The data and the results of this paper as well as the institutional aspects of the welfare system (see the previous section) contradict this view as well. There is variation across individuals in the strictness with which the rules are applied, and presumably there is a certain degree of randomness in this (this is confirmed by field research; see the references in the previous section). We conjecture that the individual does not exactly know the rules that he has to comply with, and that he does not exactly know what type of behavior will generate a sanction, and even if he knows he is under risk then he does not know when a sanction will be imposed. It is however plausible that the individual does know the relation between his behavior and the probability that a sanction will be imposed. Some individuals will be more willing to take the risk of being given a sanction than others (e.g. because they have a higher non-pecuniary utility of being unemployed). We therefore assume that, for an unemployed individual who has not yet been punished, there is a rate $p(s)$ at which a sanction is imposed, with $p(s)$ decreasing in s. The individual does know the shape of $p(s)$ but he does not know in advance when a sanction is imposed. It may actually be realistic to have $p(s) = 0$ for all s exceeding a certain threshold value. This is because a

sanction policy is backed up by explicit minimum-requirement rules.

Abbring, Van den Berg and Van Ours (1997) derive the following three main results for this model. First, at the moment at which a sanction is imposed, the transition rate from unemployment to employment jumps upward. Thus the actual imposition of a sanction has no effect on this transition rate before the moment at which a sanction is imposed. However, once a sanction is imposed, it has a positive effect on this transition rate. The latter is for two underlying reasons. First, the benefits reduction generates a reduction in the reservation wage and an increase of the search intensity. Secondly, punished individuals are closely monitored, so they have an incentive to comply with the search requirements in order to prevent additional punishments. This also increases the search intensity. (Our data show that recidivism is rare; see Section 4.) Note that if the individual search intensity is close to a physical maximum and the probability of job acceptance by the individual is almost equal to one, then the transition rate from unemployment to employment is mostly determined by the selection and job offer behavior of employers and by the technology of the matching process. In that case, the effect of a sanction on this transition rate is small.

The second main result is that the transition rate from unemployment to employment is smaller in a system without sanctions than it is in the system with sanctions, in the time interval before the sanction is actually imposed. This holds for all individuals who have a positive probability of getting a sanction in the latter system (including those who by chance did not have sanctions imposed during unemployment). As noted in the introduction, our empirical analysis cannot be used to evaluate the effect on unemployment durations of having a welfare system with sanctions vis-à-vis a welfare system without sanctions.

The third result is that both the transition rate from unemployment to employment and the rate at which a sanction arrives depend on the same set of personal characteristics. This is because both depend on the individual's search intensity, which in turn depends on all determinants of the individual's decision problem. This has an obvious implication for the empirical analysis in case of unobserved heterogeneity amongst individuals, since it creates a spurious relation between the duration until a sanction arrives and the duration of unemployment (this is the selectivity problem discussed in Section 1). Note that a similar spurious relation is created if the policy parameters of the sanction rate itself differ across individuals in a way that is not observed by the researcher.

3.2 The empirical model

It is useful to start this subsection with a brief outline of the type of data we have. Our database consists of all individuals who started collecting welfare benefits in 1994 in Rotterdam. For each individual we know the precise duration of welfare, unless there was right-censoring at the end of the observation period, which is October 1996. We also observe the exit destination, which is usually employment. Other possibilities are: leaving the city, getting married or stopping to apply for welfare benefits for unknown reasons. Exit to such destinations is treated as independent right-censoring of the duration until exit to work. We do not have any information about what happens afterwards. For each individual we know whether or not he was punished with a sanction, and, if so, we know the exact moment of imposition. We also observe the length of the period during which the benefits were reduced and in most cases we also know the magnitude of benefit reduction. We will only use this additional information in the sensitivity analyses of Subsection 5.2.

The empirical model we use is similar to that of Abbring, Van den Berg and Van Ours (1997). Consider individuals receiving welfare benefits for t units of time. We assume that differences in transition rates from welfare to work can be characterized by the observed characteristics x, the unobserved characteristics v_u, the elapsed welfare duration itself, and a variable indicating whether a sanction has already been imposed during the spell. We assume x to be constant and v_u to be independent of x. Let t_s be the moment at which a sanction is imposed on the individual and $\mathrm{I}(t_s < t)$ the variable indicating whether a sanction has been imposed ($\mathrm{I}(\cdot)$ is the indicator function).

After imposition, a sanction is assumed to have a permanent multiplicative effect on the transition rate, equal for all types of individuals. In addition, we abstract from multiple sanctions in a single welfare spell. These assumptions are obviously rather strong. We will relax some of them in the sensitivity analysis of Subsection 5.2, although, as we will see, the data impose restrictions on what can be identified.

The transition rate from welfare to work at t conditional on x, v_u and t_s is denoted by $\theta_u(t|x, v_u, t_s)$ and is assumed to have the familiar Mixed Proportional Hazard (MPH) specification

$$\theta_u(t|x, v_u, t_s) = \lambda_u(t) \exp(x'\beta_u + \delta \cdot \mathrm{I}(t_s < t) + v_u)$$

in which $\lambda_u(t)$ represents the individual duration dependence. Let t_u be the realized duration when leaving to employment. The conditional density function

of $t_u|x, v_u, t_s$ can be written as

$$f_u(t_u|x, v_u, t_s) = \theta_u(t_u|x, v_u, t_s) \exp\left(-\int_0^{t_u} \theta_u(z|x, v_u, t_s)\, dz\right)$$

For an individual who has received welfare benefits for t units of time and on whom no sanction has been imposed, the sanction rate at t conditional on observed and unobserved characteristics x and v_s is denoted by $\theta_s(t|x, v_s)$ and is also assumed to have the MPH specification

$$\theta_s(t|x, v_s) = \lambda_s(t) \exp(x'\beta_s + v_s)$$

where x is assumed to be constant over time and independent of v_s. If t_s denotes the moment of imposing the first sanction, the conditional sanction duration density function of $t_s|x, v_s$ is

$$f_s(t_s|x, v_s) = \theta_s(t_s|x, v_s) \exp\left(-\int_0^{t_s} \theta_s(z|x, v_s)\, dz\right)$$

Now consider the joint distribution of t_u and t_s. Conditional on x, v_u and v_s, the only possible relation between the variables t_u and t_s is the relation by way of the direct effect of a sanction on the transition rate from welfare to work. This means that if $\delta = 0$ then, conditional on x, the variables t_u and t_s are only dependent if v_u and v_s are dependent. In case of independence of v_u and v_s, we would have a standard duration model for t_u in which $\mathrm{I}(t_s < t)$ can be treated as a time-varying regressor that is orthogonal to the unobserved heterogeneity term v_u. However, if v_u and v_s are not independent, inference on $t_u|x, t_s$ has to be based on $t_u, t_s|x$. Let $G(v_u, v_s)$ be the joint distribution function of the unobserved characteristics v_u, v_s. The joint density function of t_u, t_s conditional on x equals

$$f_{u,s}(t_u, t_s|x) = \int_{v_u} \int_{v_s} f_u(t_u|x, v_u, t_s) f_s(t_s|x, v_s)\, dG(v_u, v_s)$$

It is straightforward to derive the individual contributions to the likelihood function from this joint density function (note the recursive nature of the expression in the integral above). The use of a flow sample of welfare spells implies that we do not have any initial conditions problems. The right-censoring in the data is exogenous and is therefore solved in a straightforward manner within the hazard rate framework.

The intuition of the identification of this model is as follows. The data can be broken into two parts: (i) a competing risk part for the duration until a welfare recipient either finds a job or gets a sanction imposed, whichever comes first, and (ii) the residual duration from the moment of imposition of a sanction until exit to work. From Heckman and Honoré (1989) it follows that under general conditions the whole model except for δ is identified from the data corresponding to the competing risk part. Subsequently, δ is identified from the data corresponding to part (ii) of the model. Basically, the timing of the consecutive events of imposition of a sanction and exit into work is informative on the presence of the causal effect of a sanction. The nonparametric identification of treatment effects in duration models like this is discussed at length in Abbring and Van den Berg (1997).

3.3 Parameterization

For the duration dependence functions and the bivariate unobserved heterogeneity distribution we take the most flexible specifications used to date. We take both $\lambda_u(t)$ and $\lambda_s(t)$ to have a piecewise constant specification,

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

where j is a subscript for time intervals and $I_j(t)$ are time-varying dummy variables that are one in consecutive time intervals. Note that with an increasing number of time intervals any duration dependence pattern can be approximated arbitrarily closely. By now it is well known that duration dependence specifications with only one parameter (like a Weibull specification) are overly restrictive (see e.g. Lancaster, 1990).

We take the joint distribution of the unobserved heterogeneity terms v_u and v_s to be multivariate discrete with two unrestricted mass-point locations for each term. Let v_u^a, v_u^b, v_s^a and v_s^b denote the points of support of v_u and v_s, respectively. The associated probabilities are denoted as follows:

$$\begin{array}{ll} \Pr(v_u = v_u^a, v_s = v_s^a) = p_1 & \Pr(v_u = v_u^b, v_s = v_s^a) = p_3 \\ \Pr(v_u = v_u^a, v_s = v_s^b) = p_2 & \Pr(v_u = v_u^b, v_s = v_s^b) = p_4 \end{array}$$

with $0 \leq p_i \leq 1$ for $i = 1, \ldots, 4$, and $p_4 = 1 - p_1 - p_2 - p_3$.

The covariance of v_u and v_s equals

$$\operatorname{cov}(v_u, v_s) = (p_1 p_4 - p_2 p_3) \cdot (v_u^a - v_u^b) \cdot (v_s^a - v_s^b)$$

It is easy to show that v_u and v_s are independent if and only if $\text{cov}(v_u, v_s) = 0$. Furthermore, the variables v_u and v_s are perfectly correlated if $p_1 = p_4 = 0$ or $p_2 = p_3 = 0$.

4 The data

Our database concerns welfare recipients in Rotterdam, which is the second largest city of The Netherlands. At the end of 1995 Rotterdam had almost 600,000 inhabitants of which approximately 260,000 were employed workers. About 40% of the Rotterdam population consists of immigrants or their children. There were around 35,000 unemployed workers, which is 15% of the labor force. About 61,000 individuals were receiving some kind of social security benefit. Of these, 78% had received this benefit already for more than one year.

The database contains administrative information on all unemployed individuals who started to collect welfare benefits in Rotterdam in 1994 and who were obliged to search for a job. The full database consists of 11350 individuals. As explained in Section 2, we exclude school leavers from the data. This reduces the size of the database with 10%. In addition, we exclude individuals who became eligible for welfare before 1994 but did not start to collect benefits until 1994, individuals for which moment of inflow into welfare is equal to the moment of outflow, individuals for which the location of the neighborhood is missing, and individuals for which a sanction was imposed before the moment of inflow (this can be a UI sanction that has not yet expired at the moment of transition from UI to welfare). Finally, we exclude individuals who had a sanction imposed immediately at the start of their welfare spell. The reason is that it is not possible to identify the selectivity involved in the imposition of sanctions at the start of a spell. Also, sanctions at the start are given for reasons related to behavior before receiving any welfare benefits, which are very different from reasons for sanctions during the spell. As a result, the final dataset consists of 7978 individuals.

All information on events is daily. In 25% of the cases, information on the magnitude of the sanction is missing, but we do not omit these cases. Unfortunately, we not observe multiple welfare spells per recipient. About 2% of the recipients have been given more than one sanction within a given welfare spell.

In the analysis we use the values of the explanatory variables x at the moment of inflow. In addition to standard personal characteristics, we include in x a variable indicating whether the individual has ever received welfare benefits before. The dummy variable "married" equals one in case of marriage or concubinage. The dummy variable "married or kids" will be used below to allow for

interaction between the effects of marriage and children. It should be stressed that variables that are not relevant for the welfare agency are not included in the database. This means that we do not have information on the profession and the level of education of the welfare recipients. Finally, Rotterdam is divided into 12 districts, for which we include dummy variables. Note that an advantage of using an administrative database is that the data do not suffer from selective nonresponse or attrition from the database.

Table 1 provides some statistics of the data set of 7978 individuals. Before October 1996, 39% of our sample has left welfare in order to work. Since some of the welfare recipients were "exposed to the risk" of leaving the welfare system since January 1994, while others entered in December 1994, it is difficult to draw conclusions from this number. Nevertheless, we can get a first impression of differences between individuals by comparing such probabilities for different groups. About 41% of our sample is younger than 25 years when entering the welfare system. Of these, 47% exited from the welfare system before October 1996. About 8% of the workers in the sample is older than 45 years. Of this group only 21% left the welfare system before October 1996. The exit probabilities of males and females and of unmarried and married welfare recipients are about the same. The exit probabilities of non-Dutch recipients, recurrent recipients, single recipients and recipients with children are lower than those of their counterparts. About 14% of the individuals in the sample had a sanction imposed on them. This seems high in comparison to the nation-wide annual average of about 5%, but both figures are hard to compare. First, some of the individuals in our sample have been in the welfare system for almost three years. Second, our data are collected by sampling from an inflow, while the 5% figure relates to the stock of welfare recipients. A large part of the stock has been in the welfare system for several years and their sanction rate may be lower than for the welfare recipients with a short duration. As mentioned in Section 2, welfare agencies are more tolerant towards long-term welfare recipients. We thus expect the duration dependence of the sanction rate to become negative after a while.

5 Estimation results

5.1 Parameter estimates

In this section we discuss the results of our empirical analysis. In the current subsection we present the parameter estimates, while in Subsection 5.2 we perform sensitivity analyses.

We estimate the parameters of our model using the method of Maximum Likelihood. We take the unit of time to be a month. Furthermore, we specify the piecewise constant duration dependence in terms of quarters. Thus, we estimate the parameters v_u^a, v_u^b, v_s^a, v_s^b, δ, $\lambda_{u,t}$ and $\lambda_{s,t}$ $(t = 1, \ldots, 11)$, p_1, p_2, p_3, β_u and β_s, where both β_u and β_s are vectors of 21 parameters. We normalize by taking $\lambda_{u,1} = \lambda_{s,1} = 0$. Because we do not observe transitions to work with an elapsed duration in welfare in its 11th quarter, we do not estimate $\lambda_{u,11}$.

Table 2 presents the parameter estimates. The parameter estimates of p_2 and p_3 are on the boundary of the parameter space, which implies that the unobserved heterogeneity components of θ_u and θ_s are perfectly correlated ($p_2 = p_3 = 0$). The computed standard errors of all other parameters are conditional on this. Note that $v_u^a > v_u^b$ whereas $v_s^a < v_s^b$. The perfect negative correlation between v_u and v_s implies that neglecting the endogenous selectivity in the imposition of the sanctions would produce a downward bias in the estimate of δ. We return to this in the next subsection. The estimates of p_1 and p_4 indicate that (ignoring differences in observed characteristics) there are two groups of welfare recipients which differ substantially in terms of job finding rate and sanction rate. The group which represents 68% of the welfare recipients find a job rather quickly and face a small sanction rate. The other group has a job finding rate that is only 14% of that in the first group, while the sanction rate is 6 times higher than that in the first group.

The main parameter of interest is δ, which represents the effect of a sanction on the exit rate from welfare to work. The estimated value of δ is 0.89 and is significantly different from 0. A sanction thus raises the transition rate from welfare to work with about 140%, so this transition rate more than doubles. Perhaps surprisingly, our estimate is very close to the estimates in Abbring, Van den Berg and Van Ours (1997) on sanction effects for UI recipients.[9] Now one may argue that a doubling of a small transition rate still gives a small transition rate. However, our estimates do imply that a sanction imposed at a relatively early stage in a welfare spell has a large effect on the probability of becoming long-term dependent on welfare. Consider for example a 25-year old single-living Dutch man who lives downtown and experiences his first welfare spell. Suppose that his unobserved characteristics equal the mean values of v_u and v_s in the inflow, and suppose that exit to destinations other than work are ruled out. If no sanctions are applied then his probability of leaving welfare within 2 years after inflow is equal to 0.66. However, if the same individual would have had a

[9]For example, their δ estimates are 0.57 for UI recipients in the metal industry and 0.81 for UI recipients in the banking sector; both are significant.

sanction imposed after 6 months of welfare then the probability of leaving within 2 years increases to 0.88. Now consider a 50-year old individual who is otherwise equal. If no sanctions are applied then his probability of leaving welfare within 2 years after inflow is equal to 0.29. If he would have been given a sanction after 6 months then this probability increases to 0.50.

It is thus clear that welfare recipients are sensitive to financial stimuli. Now recall that the decrease in benefits associated with a sanction is often not very large. To see why such a small change in benefits can have a large effect, note that welfare recipients have a very low income level. Most of their benefits are spent on the most elementary needs like housing, clothing and food. Moreover, given the welfare system, there are no strong incentives for precautionary savings, and given the length of an average welfare spell, there is no scope for consumption smoothing to deal with the shock in income. In sum, the marginal utility levels of the welfare recipients may be very high, and this may explain a large change in behavior upon imposition of a sanction.

Now let us turn to the covariate effects on θ_u. These are all significantly different from 0. Age, marital status and nationality seem to be the most important covariates in the transition rate from welfare to work. This rate is lower for older, unmarried and non-Dutch welfare recipients. It is interesting to pay some attention to the household characteristics, as they are closely related to the welfare benefits level. Recall that a household with married members and no children receives benefits that are much lower per person than what a single individual receives, so one may expect someone in the former household to have a higher θ_u (note that someone who is married to a full-time employed person is in general not entitled to welfare, so he would not be in our data). To check on this, note that the estimated empirical effect of "married" depends on whether there are children in the household. However, it turns out that in both cases the individual in the "married" household does have a higher θ_u. Now consider the effect of children. Having children increases the benefits level of unmarried recipients, so one may expect this to decrease θ_u (of course, having children may also increase the non-pecuniary utility of being unemployed, and this is an additional reason to expect a lower θ_u). It turns out that children do have a negative effect on θ_u, whether one is married or not. Note that if the individual is a single parent and one of the children is below 12 years then he is not obliged to search for a job, so then he is not in our data.

The duration dependence of θ_u is shown in Figure 1. Overall, the individual transition rate from welfare to work decreases as the duration increases. (There are slight increases after 3 months and after 18 months.) Apparently, stigmati-

zation and discouraged worker effects play a significant role.

The sanction rate rises during the first year of welfare, which is consistent with the fact that the welfare agency needs some time to gather information on the behavior of the welfare recipient. As indicated before most welfare recipients have a first thorough investigation of their files after 8 months. If there is evidence of noncompliance with job search guidelines then a sanction will imposed 1-2 months later. This time pattern is reflected in Figure 2, where the sanction rate has a peak at 10-12 months. After the first year the sanction rate has a downward trend. This suggests that welfare agencies are more tolerant towards long-term unemployed individuals (who have lower exit rates), or at least towards individuals who are categorized in groups with a high expected unemployment duration.[10] The estimates of the covariate effects β_u and β_s provide other evidence of this suggestion concerning the attitude of the welfare agencies. Age and gender have significant effects on θ_s as well as on θ_u, and the signs of the two effects are the same. This may of course be due to a systematic relation in behavior across the two dimensions we consider, along the lines we discussed in Subsection 3.1. However, it may also indicate that whenever the decision has to be made whether to impose a sanction or not, the agency takes the expected remaining unemployment duration (or the exit rate) of the individual into account. If an individual has a high expected remaining duration (e.g. if the individual is old and/or female and/or long-term unemployed) then the agency may regard a sanction to be morally less acceptable. In such a case, it may be expected that it will be very difficult anyway for the individual to find a job soon, so that the individual would have to bear the full weight of the sanction. Recall from Section 2 that field research has provided evidence for this attitude of the welfare agencies. Abbring, Van den Berg and Van Ours (1997) find similar results for UI. From an econometric point of view, this is selectivity from the side of the agency imposing the sanctions.

It should be stressed, though, that this is not the whole story concerning the behavior of welfare agencies. There are two other personal characteristics that have a significant effect on the sanction rate, but these have an opposite effect on the exit rate to work. In particular, married individuals and new welfare recipients have a low sanction rate but a high exit rate to work. These may simply be individuals who have a high search intensity because of certain values of their structural parameters. In addition, new welfare recipients may have lower

[10]The latter explanation suggests a more complicated interaction between unobserved heterogeneity v_s and duration dependence λ_s in the sanction rate θ_s, but such a model would not be identified.

sanction rates because they do not have a history record yet.

5.2 Sensitivity analysis

In this subsection we examine the sensitivity of the parameter estimates with respect to the model specification.[11] First of all, we test whether the unobserved heterogeneity terms v_u and v_s are independent. Under the maintained assumption that both terms are dispersed, the Likelihood Ratio (LR) test has a chi-square distribution with one degree of freedom under the null hypothesis of independence. Table 3 reports the estimation results for the model with independent unobserved heterogeneity. The LR test statistic is equal to 14.7, indicating that, indeed, selectivity in the imposition of sanctions is non-ignorable. Note that in the restricted model we do not find any unobserved heterogeneity in the sanction rate or the job finding rate. Also note that neglecting the (negative) relation between the unobservable components leads to underestimation of the effect of a sanction. Indeed, the sanction effect in Table 3 is insignificant.

We also perform sensitivity analyses with respect to the model specification of the sanction effect. First, we allow the effect δ of a sanction to vary over the population, by specifying $\delta = \delta(x) = x'\gamma$. The vector x includes an intercept and all explanatory variables used before, except for the district indicators, as the number of sanctions per district is rather low. Table 4 gives the parameter estimates for γ. The LR test statistic on joint significance of all elements of γ is equal to 20.4. Since we have 10 additional parameters, we reject the null hypothesis that the sanction effect is independent of individual characteristics, at the 5% level. This is actually in agreement to the theoretical model framework (see Subsection 3.1), which predicts that the magnitude of the sanction effect depends on the structural determinants like the discount rate, which may vary over individuals. Also, evaluations of training programs often find that the effect depends on individual characteristics (Bonnal, Fougère and Sérandon, 1997; Gritz, 1993). Note that the only characteristic with a significant coefficient is whether one is a new client. The sanction effect is larger for a new client than for a recurrent welfare recipient.

So far we have assumed that once a sanction is imposed, it has a permanent effect on the transition rate to work. We relax this assumption by allowing the effect after expiration of the benefits reduction to differ from the effect during the period of benefits reduction. Letting t_s be the moment at which the sanction is

[11] We focus on the estimates of the effect of a sanction. The other parameter estimates do not change much from those reported in Subsection 5.1.

imposed and t_e the moment at which the benefits reduction ends, we specify δ as $\delta = \delta_1 \cdot \mathrm{I}(t_s < t \leq t_e) + \delta_2 \cdot \mathrm{I}(t_e < t)$. The duration $t_e - t_s$ of the benefits reduction differs across sanctions (see Section 2), but in most cases it equals 1 month (602 cases) or 2 months (541 cases). It exceeds 2 months in only 13 cases.[12] Table 5 gives the parameter estimates of δ_1 and δ_2. Using a LR test, we reject the null hypothesis that $\delta_1 = \delta_2$. In fact, the effect after expiration of the benefits reduction is somewhat larger than during the period of the reduction. From a theoretical point of view this is puzzling. The benefits level increases upon expiration, and the search intensity is not expected to increase at that moment (see Subsection 3.1). A possible explanation is that in reality it takes some time to adjust one's behavior upon imposition of a sanction. Since the mean duration $t_e - t_s$ of benefits reduction is relatively short, this may imply that most of the adjustment occurs after expiration.[13]

The empirical model of Subsection 3.2 does not take into account that the amount of benefits reduction differs across sanctions. We observe 669 sanctions with a 5% reduction, 207 with 10%, 133 with 20% and 147 sanctions where information on the magnitude is missing. The magnitude depends on the reason for imposition of the sanction, so it is plausible that it is related to v_u. We ignore this additional selection problem, for the simple reason that we cannot correct for it. Basically, we use all available information to deal with the selectivity in the moment at which a sanction is imposed, and there is no additional information to deal with the selectivity in the magnitude. We therefore estimate a model that differs from the basic model merely because δ now depends on the magnitude of the benefits reduction. Specifically, let δ_x be the effect in case of a reduction of

[12] It should be stressed that we neglect any selectivity involved in the choice of a particular duration of the sanction. In reality, this duration depends on the reason for imposition of the sanction. Welfare recipients who are confronted with a long duration $t_e - t_s$ may be different from those with a short duration.

[13] We also estimated a model extension in which δ is specified as a flexible piecewise-constant function of the elapsed duration $t - t_s$ since imposition of the sanction. This specification does not take account of the expiration time, but it does allow the sanction effect to diminish slowly as time proceeds. The estimation results (not presented here) are as follows. The estimated unobserved heterogeneity distribution is basically such that *either* one never gets a sanction and has a reasonably high transition rate from welfare to work, *or* one has a high rate of getting a sanction and the transition rate from welfare to work is almost zero. The estimated direct sanction effect is estimated to be extremely high for all values of $t - t_s$. As a result, the second subgroup of individuals only leave unemployment after imposition of a sanction. These estimation results are very implausible. It can be argued that this specification is so flexible that it asks too much from the data. Note that this in turn suggests some caution concerning the results in Table 5.

$x\%$, $x = 5, 10, 20$, and δ_0 the effect of a sanction where the magnitude is missing in the database. Table 6 shows the parameter estimates of δ_5, δ_{10}, δ_{20} and δ_0 for this extended model. The LR test does not reject the null hypothesis that $\delta = \delta_x$ for all $x = 5, 10, 20, 0$.

The sensitivity analyses above seem to indicate that the effect of a sanction is not restricted to the period of benefits reduction. Furthermore, the amount of benefits reduction seems to be unimportant for the sanction effect. This could be taken as evidence that any pecuniary incentive of a sanction is dominated by non-pecuniary factors[14], setting aside for the moment the objection that the results above can be affected by selectivity. However, a permanent effect works by way of an increased search intensity, and this is most likely due to the combination of increased monitoring and the threat of a severe punishment upon detection of recidivism. Because of the latter, it could be argued that a permanent effect is at least partly due to financial incentives. Moreover, insensitivity of the transition rate to work with respect to the exact amount of benefits reduction can also be explained if even a small decrease in welfare benefits causes the individual to increase his search effort up to a physical maximum.

Now let us turn to sensitivity analyses concerning the labor market states before and after welfare. First of all, recall from Section 2 that we restrict attention to welfare recipients who once lost a job, excluding school leavers on welfare. However, estimation of the model with the joint data on both types of welfare recipients does not affect the major conclusions. In particular, the estimate of δ is 0.63 (standard error 0.25), so it is significantly positive and only marginally smaller than in Table 2. The number of observed sanctions for school leavers is too small to estimate the full model separately for that group (some parameters could not be estimated).

Concerning the destination states, recall that we treat exit to other destinations than work as independent right-censoring of the duration until exit to work. Relaxing this (e.g. by postulating a competing-risks model with potentially related unobserved heterogeneity terms for each destination) would result in estimates that are very sensitive to functional-form assumptions. We therefore estimate a model extension in which we impose independence of the unobserved heterogeneity terms. In particular, each transition rate to a destination state is modeled by way of a MPH specification, where we allow each rate to depend on whether a sanction has been imposed or not, but we do not allow for related unobserved heterogeneity terms. It turns out that sanctions do not have a sig-

[14]This would be in line with Fortin and Lacroix (1997), who find for Canada that the level of welfare has a negative but small effect on the individual transition rate from welfare to work.

nificant effect on exit to the other destinations, except for leaving the city. This suggests that some welfare recipients leave the city upon imposition of a sanction, possibly to try to collect benefits in another municipality.

6 Conclusions

In The Netherlands, welfare recipients often stay unemployed for a long period of time, even though they are obliged to comply with guidelines by the welfare agency on search effort. Recipients who do not comply with these or with other rules set by the agency may have a sanction imposed, i.e. their benefits may be temporarily reduced. We find that the imposition of a sanction has a significant positive effect on the transition rate from welfare to work. Indeed, this transition rate is about twice as large after a sanction than before. This estimate (obtained while correcting for selectivity) turns out to be very close to estimates reported elsewhere on sanction effects for UI recipients. A sanction that is imposed at a relatively early stage in a welfare spell thus has a substantial negative effect on the probability of becoming long-term dependent on welfare.

This result establishes that welfare recipients are sensitive to financial stimuli. Apparently, marginal utility levels of welfare recipients are so high, and consumption smoothing is so difficult, that a relatively small sanction (and the threat of an additional severe punishment in case of recidivism) can cause a large change in search behavior. We also found some evidence that the effect of a sanction varies with individual characteristics.

From the theoretical analysis it follows that individuals are expected to have a higher transition rate to work in a system with sanctions even though they have not (yet) been given a sanction. The estimated sanction effect is basically a lower bound of the over-all effect of a welfare system with sanctions vis-à-vis a system without sanctions. To quantify the "ex ante" effect of a system with sanctions we would need to have additional data from a period with a welfare system without sanctions. Alternatively, we would need sufficient information to estimate a structural job search model.

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Exit	observed		unobserved		Total
Sanction	no	yes	no	yes	
Individual characteristics					
Age 18–25	43%	4%	41%	12%	3249
Age 26–35	35%	3%	50%	11%	2879
Age 36–45	26%	3%	60%	11%	1210
Age 46–55	22%	2%	67%	10%	533
Age 56–65	8%	1%	86%	5%	107
Male	37%	4%	46%	13%	5206
Female	34%	2%	56%	8%	2772
Not married	36%	3%	49%	11%	6542
Married	36%	3%	50%	11%	1436
Dutch	39%	3%	47%	10%	6034
Non-Dutch	26%	3%	57%	14%	1944
No children	39%	3%	46%	11%	6241
Children	26%	3%	60%	11%	1737
Collected welfare before	34%	4%	49%	14%	4399
New client	39%	3%	50%	8%	3579
Not married, no kids	38%	4%	47%	11%	5735
Married or kids	30%	3%	56%	10%	2243
Districts					
Centrum	35%	3%	49%	13%	444
Delfshaven	34%	3%	51%	12%	1695
Kralingen/Crooswijk	43%	3%	43%	10%	879
Noord	40%	3%	47%	9%	805
Prins Alexander	45%	2%	46%	8%	437
Overschie	30%	3%	48%	13%	160
Hillegersberg/Schiebroek	44%	3%	45%	7%	203
Hoek van Holland	53%	9%	29%	9%	34
Charlois	36%	4%	51%	11%	1065
Feijenoord	28%	4%	53%	14%	1353
IJsselmonde	38%	1%	53%	7%	493
Hoogvliet	37%	3%	49%	11%	410
Total	36%	3%	49%	11%	7978

Explanatory note: The table shows how individuals with a certain characteristic are distributed over the four groups defined by whether a transition from welfare to work is observed and whether a sanction is imposed within the observed welfare spell. The last column gives the total number of individuals in the sample with a certain characteristic.

Table 1: Some characteristics of the data set.

	Exit hazard θ_u		Sanction hazard θ_s	
Effect of a sanction				
δ	0.89	(0.34)		
Unobserved heterogeneity				
v^a	−2.75	(0.14)	−5.00	(0.52)
v^b	−4.73	(0.32)	−3.21	(0.27)
p_1	0.68	(0.24)		
p_4	0.32	(0.11)		
Duration dependence				
λ_1	0		0	
λ_2	0.11	(0.057)	0.25	(0.10)
λ_3	−0.041	(0.067)	0.41	(0.10)
λ_4	−0.23	(0.079)	0.45	(0.11)
λ_5	−0.28	(0.090)	0.021	(0.13)
λ_6	−0.34	(0.10)	−0.038	(0.14)
λ_7	−0.28	(0.11)	0.041	(0.15)
λ_8	−0.71	(0.13)	−0.32	(0.18)
λ_9	−0.75	(0.15)	−0.43	(0.22)
λ_{10}	−1.19	(0.21)	0.061	(0.23)
λ_{11}	−		−1.05	(0.59)
Individual chararcteristics				
Age 26–35	−0.35	(0.049)	−0.28	(0.076)
Age 36–45	−0.77	(0.074)	−0.41	(0.10)
Age 46–55	−1.14	(0.11)	−0.63	(0.15)
Age 56–65	−2.25	(0.34)	−1.47	(0.43)
Female	−0.091	(0.049)	−0.69	(0.090)
Married	0.67	(0.11)	−0.43	(0.17)
Non-Dutch	−0.64	(0.059)	0.034	(0.085)
Children	−0.31	(0.10)	0.039	(0.17)
New client	0.17	(0.043)	−0.58	(0.080)
Married or kids	−0.36	(0.13)	0.10	(0.21)
Districts				
Delfshaven	−0.11	(0.099)	0.0079	(0.15)
Kralingen/C.	0.25	(0.11)	−0.17	(0.17)
Noord	0.14	(0.11)	−0.24	(0.18)
Pr. Alexander	0.32	(0.12)	−0.48	(0.21)
Overschie	−0.16	(0.18)	0.021	(0.27)
Hillegersb./S.	0.30	(0.15)	−0.39	(0.27)
Hoek v.H.	0.65	(0.34)	0.48	(0.53)
Charlois	−0.0072	(0.11)	−0.046	(0.16)
Feijenoord	−0.22	(0.10)	0.15	(0.15)
IJsselmonde	0.10	(0.12)	−0.64	(0.21)
Hoogvliet	−0.015	(0.13)	−0.11	(0.20)
$\log \mathcal{L}$	−20690.07			
N	7978			

Explanatory note: Standard errors in parentheses.

Table 2: Estimation results of the basic model.

	Exit hazard θ_u		Sanction hazard θ_s	
Effect of a sanction				
δ	0.00095	(0.066)		
Intercept				
	−3.07	(0.090)	−4.02	(0.14)
Duration dependence				
λ_1	0		0	
λ_2	0.078	(0.054)	0.28	(0.10)
λ_3	−0.10	(0.060)	0.46	(0.10)
λ_4	−0.31	(0.067)	0.50	(0.11)
λ_5	−0.37	(0.072)	0.075	(0.12)
λ_6	−0.45	(0.077)	0.024	(0.13)
λ_7	−0.42	(0.080)	0.11	(0.13)
λ_8	−0.87	(0.10)	−0.25	(0.16)
λ_9	−0.91	(0.12)	−0.35	(0.20)
λ_{10}	−1.36	(0.19)	0.14	(0.21)
λ_{11}	−		−1.00	(0.598)
Individual chararcteristics				
Age 26–35	−0.30	(0.041)	−0.33	(0.068)
Age 36–45	−0.69	(0.063)	−0.50	(0.093)
Age 46–55	−1.04	(0.095)	−0.74	(0.14)
Age 56–65	−2.12	(0.32)	−1.58	(0.42)
Female	−0.11	(0.042)	−0.64	(0.078)
Married	0.60	(0.10)	−0.33	(0.15)
Non-Dutch	−0.57	(0.050)	0.043	(0.071)
Children	−0.27	(0.087)	0.037	(0.16)
New client	0.14	(0.037)	−0.51	(0.064)
Married or kids	−0.34	(0.12)	0.090	(0.19)
Districts				
Delfshaven	−0.092	(0.088)	−0.025	(0.14)
Kralingen/C.	0.21	(0.093)	−0.12	(0.15)
Noord	0.11	(0.095)	−0.20	(0.16)
Pr. Alexander	0.27	(0.10)	−0.40	(0.20)
Overschie	−0.16	(0.16)	0.0056	(0.24)
Hillegersb./S.	0.23	(0.13)	−0.31	(0.24)
Hoek v.H.	0.67	(0.25)	0.56	(0.45)
Charlois	−0.0080	(0.093)	−0.062	(0.15)
Feijenoord	−0.19	(0.092)	0.088	(0.14)
IJsselmonde	0.071	(0.11)	−0.60	(0.20)
Hoogvliet	−0.028	(0.11)	−0.080	(0.18)
$\log \mathcal{L}$	−20697.44			
N	7978			

Explanatory note: Standard errors in parentheses.

Table 3: Estimation results of the model where the selectivity in the process of imposing sanctions in ignored.

	Exit hazard θ_u		Sanction hazard θ_s		Effect of a sanction δ	
Unobserved heterogeneity						
v^a	−2.75	(0.16)	−4.93	(0.63)		
v^b	−4.49	(0.44)	−3.21	(0.30)		
p_1	0.69	(0.35)				
p_4	0.31	(0.16)				
Duration dependence						
λ_1	0		0			
λ_2	0.11	(0.057)	0.25	(0.10)		
λ_3	−0.048	(0.067)	0.42	(0.11)		
λ_4	−0.25	(0.080)	0.46	(0.11)		
λ_5	−0.29	(0.090)	0.033	(0.14)		
λ_6	−0.36	(0.10)	−0.025	(0.15)		
λ_7	−0.31	(0.11)	0.057	(0.15)		
λ_8	−0.74	(0.13)	−0.31	(0.18)		
λ_9	−0.78	(0.15)	−0.41	(0.22)		
λ_{10}	−1.23	(0.21)	0.082	(0.23)		
λ_{11}	−		−1.05	(0.59)		
Individual charactеristics						
Intercept					0.44	(0.36)
Age 26–35	−0.35	(0.050)	−0.30	(0.076)	0.12	(0.17)
Age 36–45	−0.78	(0.076)	−0.42	(0.10)	0.44	(0.24)
Age 46–55	−1.14	(0.11)	−0.65	(0.15)	0.27	(0.40)
Age 56–65	−2.30	(0.35)	−1.48	(0.43)	1.64	(1.42)
Female	−0.090	(0.049)	−0.68	(0.091)	−0.10	(0.20)
Married	0.71	(0.11)	−0.42	(0.17)	−0.73	(0.38)
Non-Dutch	−0.64	(0.060)	0.026	(0.085)	0.19	(0.20)
Children	−0.33	(0.10)	0.034	(0.17)	0.38	(0.43)
New client	0.14	(0.044)	−0.57	(0.081)	0.39	(0.17)
Married or kids	−0.39	(0.14)	0.10	(0.21)	0.46	(0.52)
Districts						
Delfshaven	−0.11	(0.097)	0.0078	(0.15)		
Kralingen/C.	0.23	(0.10)	−0.16	(0.17)		
Noord	0.13	(0.11)	−0.23	(0.18)		
Pr. Alexander	0.30	(0.12)	−0.47	(0.21)		
Overschie	−0.16	(0.18)	0.020	(0.27)		
Hillegersb./S.	0.28	(0.14)	−0.38	(0.27)		
Hoek v.H.	0.65	(0.33)	0.50	(0.54)		
Charlois	−0.018	(0.10)	−0.044	(0.16)		
Feijenoord	−0.22	(0.10)	0.15	(0.15)		
IJsselmonde	0.091	(0.12)	−0.64	(0.22)		
Hoogvliet	−0.022	(0.12)	−0.11	(0.20)		
$\log \mathcal{L}$	−20679.89					
N	7978					

Explanatory note: Standard errors in parentheses.

Table 4: Estimation results of the model where the effect of a sanction is allowed to depend on the observed individual characteristics.

	Exit hazard θ_u		Sanction hazard θ_s	
Effect of a sanction				
δ_1	0.94	(0.35)		
δ_2	1.39	(0.37)		
Unobserved heterogeneity				
v^a	−2.72	(0.12)	−5.34	(0.43)
v^b	−5.08	(0.29)	−3.15	(0.23)
p_1	0.68	(0.14)		
p_4	0.32	(0.066)		
Duration dependence				
λ_1	0		0	
λ_2	0.12	(0.056)	0.25	(0.10)
λ_3	−0.027	(0.066)	0.40	(0.10)
λ_4	−0.22	(0.077)	0.45	(0.11)
λ_5	−0.26	(0.087)	0.018	(0.13)
λ_6	−0.32	(0.098)	−0.041	(0.14)
λ_7	−0.27	(0.10)	0.038	(0.15)
λ_8	−0.69	(0.13)	−0.33	(0.18)
λ_9	−0.72	(0.15)	−0.43	(0.22)
λ_{10}	−1.17	(0.21)	0.059	(0.23)
λ_{11}	−		−1.08	(0.59)
Individual chararcteristics				
Age 26–35	−0.36	(0.050)	−0.26	(0.079)
Age 36–45	−0.78	(0.075)	−0.38	(0.11)
Age 46–55	−1.16	(0.11)	−0.61	(0.15)
Age 56–65	−2.28	(0.34)	−1.44	(0.44)
Female	−0.085	(0.050)	−0.70	(0.091)
Married	0.69	(0.11)	−0.45	(0.17)
Non-Dutch	−0.65	(0.059)	0.051	(0.086)
Children	−0.32	(0.11)	0.058	(0.18)
New client	0.17	(0.044)	−0.60	(0.080)
Married or kids	−0.36	(0.14)	0.11	(0.22)
Districts				
Delfshaven	−0.11	(0.10)	0.022	(0.16)
Kralingen/C.	0.26	(0.11)	−0.17	(0.18)
Noord	0.15	(0.11)	−0.25	(0.18)
Pr. Alexander	0.33	(0.13)	−0.48	(0.22)
Overschie	−0.15	(0.19)	0.023	(0.28)
Hillegersb./S.	0.33	(0.15)	−0.42	(0.27)
Hoek v.H.	0.65	(0.36)	0.50	(0.57)
Charlois	−0.0054	(0.11)	−0.033	(0.17)
Feijenoord	−0.22	(0.11)	0.18	(0.16)
IJsselmonde	0.11	(0.12)	−0.64	(0.21)
Hoogvliet	−0.0015	(0.13)	−0.12	(0.21)
$\log \mathcal{L}$	−20687.63			
N	7978			

Explanatory note: Standard errors in parentheses.

Table 5: Estimation results of the model where the effect of a sanction is split into an effect during the sanction and an effect afterwards.

	Exit hazard θ_u		Sanction hazard θ_s	
Effect of a sanction				
δ_5	0.95	(0.35)		
δ_{10}	0.67	(0.39)		
δ_{20}	0.91	(0.41)		
δ_0	0.99	(0.39)		
Unobserved heterogeneity				
v^a	−2.75	(0.14)	−5.02	(0.53)
v^b	−4.43	(0.31)	−3.21	(0.27)
p_1	0.68	(0.23)		
p_4	0.32	(0.11)		
Duration dependence				
λ_1	0		0	
λ_2	0.11	(0.057)	0.25	(0.10)
λ_3	−0.041	(0.067)	0.41	(0.10)
λ_4	−0.24	(0.079)	0.45	(0.11)
λ_5	−0.28	(0.090)	0.021	(0.13)
λ_6	−0.34	(0.10)	−0.038	(0.14)
λ_7	−0.28	(0.11)	0.041	(0.15)
λ_8	−0.71	(0.13)	−0.32	(0.18)
λ_9	−0.74	(0.15)	−0.43	(0.22)
λ_{10}	−1.18	(0.21)	0.062	(0.23)
λ_{11}	−		−1.05	(0.59)
Individual chararcteristics				
Age 26–35	−0.35	(0.049)	−0.28	(0.076)
Age 36–45	−0.77	(0.074)	−0.41	(0.10)
Age 46–55	−1.14	(0.11)	−0.63	(0.15)
Age 56–65	−2.25	(0.34)	−1.47	(0.43)
Female	−0.091	(0.049)	−0.69	(0.090)
Married	0.67	(0.11)	−0.43	(0.17)
Non-Dutch	−0.64	(0.059)	0.034	(0.085)
Children	−0.31	(0.10)	0.039	(0.17)
New client	0.17	(0.044)	−0.58	(0.080)
Married or kids	−0.36	(0.13)	0.10	(0.21)
Districts				
Delfshaven	−0.10	(0.099)	0.0093	(0.15)
Kralingen/C.	0.25	(0.11)	−0.17	(0.17)
Noord	0.14	(0.11)	−0.24	(0.18)
Pr. Alexander	0.32	(0.12)	−0.47	(0.22)
Overschie	−0.16	(0.18)	0.021	(0.27)
Hillegersb./S.	0.31	(0.15)	−0.39	(0.27)
Hoek v.H.	0.65	(0.34)	0.49	(0.54)
Charlois	−0.0050	(0.11)	−0.045	(0.16)
Feijenoord	−0.22	(0.10)	0.15	(0.16)
IJsselmonde	0.10	(0.12)	−0.64	(0.22)
Hoogvliet	−0.011	(0.13)	−0.11	(0.21)
$\log \mathcal{L}$	−20689.12			
N	7978			

Explanatory note: Standard errors in parentheses.

Table 6: Estimation results of the model where the effect of a sanction is allowed to depend on the magnitude of the sanction.

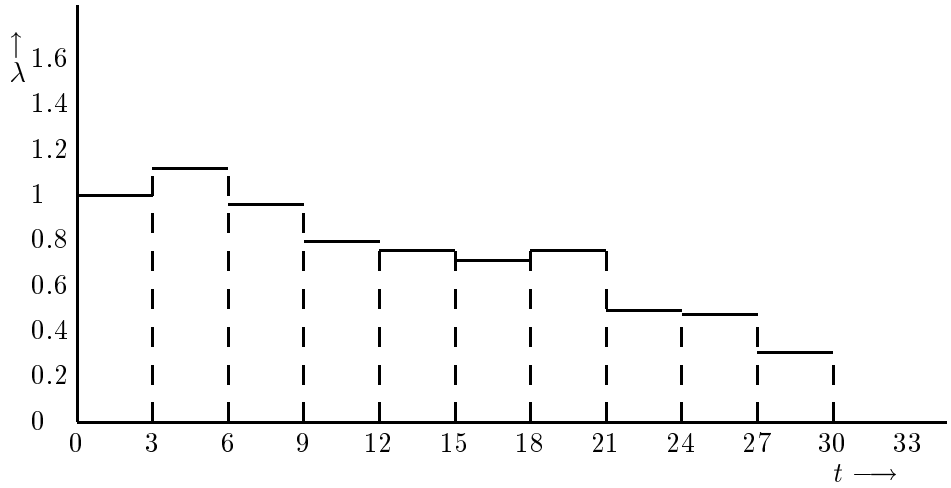

Figure 1: Duration dependence of the transition rate from welfare to work.

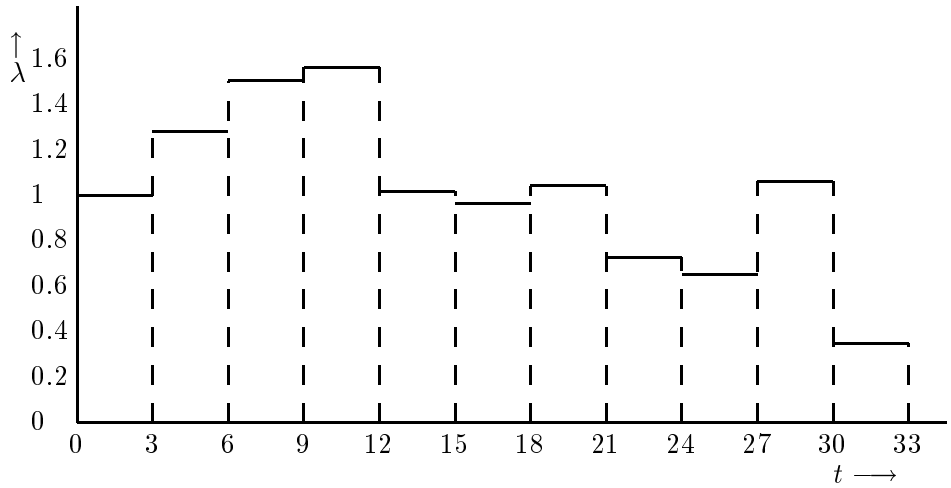

Figure 2: Duration dependence of the sanction rate.