Punitive Sanctions and the Transition Rate from Welfare to Work

Gerard J. van den Berg *
Bas van der Klaauw †
Jan C. van Ours ‡

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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 temporary benefit reduction can be imposed. This paper investigates the effect of such sanctions on the transition rate from welfare to work using a unique set of rich administrative data on welfare recipients. We find that the imposition of sanctions substantially increases the individual transition rate from welfare to work.

*Free University Amsterdam, Tinbergen Institute and CEPR.
†Free University Amsterdam and Scholar.
Address: Department of Economics, Free University Amsterdam, De Boelelaan 1105, NL–1081 HV Amsterdam, The Netherlands.
‡Tilburg University, CentER and CEPR.

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1 Introduction

In the U.S., welfare is typically used to support single-parent households, whereas in European countries it is also often used to support long-term unemployed workers. Welfare (or "social assistance") then acts as a safety net for those unemployed workers who are not entitled to any other social security benefits like unemployment insurance or disability benefits. Whatever their designs, exit rates out of welfare are low, and the current welfare systems are subject to criticism (see for example Gueron, 1990, and Moffitt, 1992).

In principle, a large range of policy measures is available to prevent unemployed individuals from becoming dependent on welfare and to stimulate and assist the long-term unemployed workers in their search for jobs. Examples are subsidized employment for youth and long-term unemployed workers, 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. Reasons for imposition of sanctions can be: 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). In The Netherlands, before 1992, sanctions were hardly ever used. However, 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 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 (*i*) the duration effect of the actual imposition of a sanction and (*ii*) the duration 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 threat of 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. The benefit reduction involved in the ex-post effect 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.[1] In sum, the imposition of a sanction is expected to increase the transition rate from welfare to work.

The empirical analysis in the present study resembles the approach developed in Abbring, Van den Berg and Van Ours (1997) to analyze the effect of sanctions in UI on the transition rate to work.[2] The main problem 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 the 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, in their studies on training effects). 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. For the duration dependence we take a flexible piecewise constant specification. The transition rate from welfare to work is modeled in a similar way, with the qualification that one

[1] 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 positive 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"). On the other hand, Ashenfelter, Ashmore and Deschênes (1999) do not find any effect of closer monitoring. However, it should be noted that they study UI recipients with relatively good labor market prospects.

[2] 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. UI benefits are usually higher than welfare benefits. See also Subsection 2.1.

of its explanatory variables depends on whether a sanction has been imposed. 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 welfare, the guidelines that a recipient must comply with, the sanctions for non-compliance, and the way sanctions are actually imposed. In Section 3 we discuss the empirical model which is inspired by 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 first. In Section 5 we present the estimation results, and we perform some sensitivity analyses. Section 6 concludes.

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 old. 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 the old AFDC program in the U.S.[3]). In the sequel we

[3]AFDC has currently been replaced by Temporary Aid to Needy Families (TANF). The main difference is that in the new system claimants only receive benefits for a fixed period. Another difference is that TANF is decentralized, while AFDC was organized on the federal

ignore the recipients who do not have an obligation to search for a job, since both the rate of getting a sanction and the transition rate to work 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.[4] In 1995, the net benefits level was about 1800 Dutch guilders per month for a two-parent family (i.e., a married couple with or without children), about 1600 for a single-parent family, about 1250 for a single individual aged over 23, and about 900 for a single individual aged below 23. The net replacement rates on welfare in 1995 were 62% for a single individual, 74% for a married couple without children, 80% for a married couple with 2 children and 72% for a single individual with two children.[5] These are slightly higher than the replacement rates on welfare in most other European countries and much higher than those in the U.S. (see for a more extensive discussion on these replacement rates OECD, 1997).

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 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

level. Under TANF, states can more or less construct their own welfare program.

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

[5]These replacement rates are relative to the net earnings of the average productivity worker, which is a worker with average earnings (OECD, 1997).

[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.

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 unemployed 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 latter group have either run out of eligibility for UI benefits or have 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.[7]

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. The exact guidelines may be determined by the municipalities. However, most municipalities have not formulated strict rules and leave some room for the discretion of the case worker. Therefore, the rules to which the welfare recipients must comply do not only vary between municipalities, but also between case workers of the welfare agency.

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

[7] Three quarters of the group of job losers did not collect unemployment insurance benefits before entering welfare, one quarter did.

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, etc. 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.[8]

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. Many cases of noncompliance are established in the so-called "re-investigation", which is a standard procedure of evaluating the welfare recipients behavior that usually takes place around 8 months after the start of collecting the welfare

[8] 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: The individual would probably not be entitled to UI benefits, because the recent history of labor force attachment was too short. If, however, the individual would meet the requirements for UI, 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).

benefits.

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.[9] 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 case workers 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 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.

Once a sanction has been imposed, the welfare agency provides the welfare recipient with some assistance on how to improve his behavior to avoid future sanction and on how to search for jobs more effectively. At the same time the behavior of the welfare recipient is more closely monitored. 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 renewal examination, the welfare agency may decide to renew the sanction

[9] However, in case of severe fraud, the welfare agency may decide to pursue legal prosecution.

or punish the welfare recipient with a higher sanction. Our data show that in practice sanctions are almost never renewed.

The period between the establishment of noncompliance by the case worker of the welfare agency and the imposition of a sanction is usually 1 to 2 months. In rare 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.

Now let us examine the two most important features of a sanction: its magnitude and its duration. These depend 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: (*i*) 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. (*ii*) 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. (*iii*) 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. (*iv*) 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.

3 The model

3.1 A theoretical framework

Our empirical model specification is motivated by a theoretical job search model framework in which punitive sanctions are incorporated. Abbring, Van den Berg and Van Ours (1997) formally develop a basic version of such a model. Here we describe a more general framework, taking into account that we consider welfare rather than UI. 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). Consider an unemployed individual who searches sequentially for a job. Given a particular search intensity s, job offers arrive according to the rate $\lambda(s)$, with $\lambda(s)$ increasing in s. The individual is able to choose s. Offers are random drawings from a wage offer distribution $F(w)$. Every time an offer arrives the decision has to be made whether to accept it or to reject it and search further. Once a job is accepted it will be held forever at the same wage. During unemployment, a flow of benefits b is received and a flow of search costs $c(s)$ has to be paid, with $c(s)$ increasing in s, and with b possibly including a non-pecuniary utility of being unemployed. The individual aims at maximization of the expected present value of income over an infinite horizon, with discount rate ρ. It is well known that in this model the optimal strategy of unemployed individuals can be characterized by a reservation wage ϕ (giving the minimal acceptable wage offer) and an optimal search intensity s.

We now introduce sanctions in this framework. We start by assuming that sanctions are equivalent to temporary benefit reductions imposed at a certain rate depending on s. We then increase the degree of realism of the model by incorporating other features of the sanctions system.

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, from the point of view of an unemployed individual there is a rate $p(s)$ at which a sanction is imposed, with $p(s)$ decreasing in s. We assume that the individual knows the relationship

between s and $p(s)$, but he does not know in advance when a sanction will be imposed. It may actually be realistic to have $p(s) = 0$ for all s exceeding a certain threshold value. This threshold value can be interpreted as the explicit minimum-requirements on job search by the welfare agency. Suppose that the function $p(s)$ and all other structural parameters are constant over the welfare duration. Then the optimal reservation wage ϕ and search intensity s^* of an individual who is not currently punished can be expressed as functions,

$$\phi = \phi(b, c(.), \lambda(.), p(.), F(.), \rho, \mathcal{P})$$

$$s^* = s^*(b, c(.), \lambda(.), p(.), F(.), \rho, \mathcal{P})$$

where $\mathcal{P}$ denotes the structural parameters that prevail after imposition of a sanction (notably, the lower level of b). The corresponding transition rate from welfare to work θ_u equals $\lambda(s^*)(1 - F(\phi))$, and the sanction rate θ_s equals $p(s^*)$.

Since the welfare recipient does not anticipate the actual moment of imposition of a sanction, the sanction causes a downward shift in the reservation wage of the welfare recipient at the moment of imposition. At the same time, the welfare recipient increases his search effort since the lower benefit level makes it less attractive to be unemployed. Therefore, at the moment at which a sanction is imposed, the transition rate from welfare to work jumps upward. These effects are temporary. The welfare recipient knows the duration of the sanction and anticipates on the moment at which the sanction period expires, so, as the moment of expiration of the sanction approaches, his reservation wage increases and his search effort decreases. At the moment the period of benefit reduction ends, both the reservation wage and the search effort are at the level that would prevail if no sanction had been imposed.

A comparison of a system without sanctions to a system with sanctions entails a comparison of the model outcomes with $p(s) \equiv 0$ to the outcomes above. It is not difficult to show that θ_u is smaller in the former system than in the latter system before a sanction is actually imposed. This holds for all individuals who have a positive probability of getting a sanction in the latter system.

Now consider the more realistic kind of sanction that includes additional job search assistance and closer monitoring. In most cases the welfare agency re-examines the job search activities of the welfare recipient 3 months after imposition of the sanction. Because of the closer monitoring, the punished individual has an incentive to comply with the search requirements in order to prevent future and additional punishments. This increases the search intensity and reduces the reservation wage beyond the period of benefits reduction. (Our data show that recidivism is rare; see Section 4.) The search assistance may also consist

of the provision of additional information on the search requirements. It could be that welfare recipients had an imprecise idea of the relationship between s and $p(s)$. In terms of a threshold value, this would mean that welfare recipients knew there was a threshold in s above which $p(s) = 0$, but they did not know its exact position. After getting a sanction they may adjust their estimate of the threshold value in an upward direction. As a result, sanctioned welfare recipients maintain a higher search effort after expiration of the benefits reduction. Furthermore, because the additional search effort generates extra search costs, being unemployed is less preferable, i.e. the welfare recipient decreases his reservation wage. Both have a lasting positive effect on θ_u. A similar effect could occur if individuals over-estimate the extent of monitoring after the imposition of the sanction. These effects may be more relevant for individuals who experience their first welfare spell.

So far we assumed that $p(s)$ is constant over the welfare duration. In reality, monitoring is more intense at around 8 months of welfare duration than before that. If individuals only realize this when they are in welfare for 8 months then this generates a sudden increase in the transition rate from welfare to work. If individuals anticipate the increased monitoring then θ_u increases more gradually during the first 8 months of welfare. In both cases, θ_u may decrease when monitoring becomes less intense. Of course, there are various other reasons for why θ_u may display positive or negative duration dependence during the first 8 months (and thereafter).

We should emphasize that all these effects concern individuals who are actually at risk of getting a sanction. These may constitute a large or small fraction of a given data set. Moreover, for the individuals at risk, the quantitative effect depends on the structural parameters. If the individual search intensity is close to a physical maximum and the job acceptance probability is almost equal to one, then θ_u 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 is small.

We should also emphasize that in practice, the above effects may work with a certain lag because of institutional constraints. Notably, there is a time interval between the submission of a successful application letter and the moment at which the individual accepts the job. Also, a job usually starts at the first day of the first or second month after the individual has accepted the job. Finally, as we have seen in the previous section, there may be some time between the detection of a violation and the starting date of the benefits reduction.

For the empirical model specification it is important to note that θ_u and θ_s

depend on the same set of personal characteristics. This is because both depend on the individual's optimal search intensity s^*, which in turn depends on all determinants of the individual's decision problem (see the equations above). 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 $p(.)$ 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 municipality, 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.

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 may be assumed to have a permanent multiplicative effect on the transition rate, equal for all types of individuals. In addition, one may abstract from multiple sanctions in a single welfare spell. These assumptions are obviously rather strong, and we relax them in the empirical analysis,

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. In case of independence of v_u and v_s, we would have a standard duration model for $t_u|x, t_s$ 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 identification of the model framework is proven and discussed at length in Abbring and Van den Berg (2000). They also demonstrate that the model where some welfare recipients are never punished with a sanction (i.e., the sanction duration distribution is defective) is also identified. To provide some intuition, first note that 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.

To further clarify what drives the identification of δ, consider individuals who get a sanction at time t. The natural control group consists of individuals who are in welfare for the same period of time at t but who have not yet received a sanction. A necessary condition for a meaningful comparison of these groups is that there is some randomization in the sanction assignment at t. The duration model framework allows for such a randomization because it specifies assignment by way of the *rate* of imposition of a sanction. In addition, we have to deal with the selection issue that the unobserved heterogeneity distribution is different between the treatment and control groups at t. This is handled by exploiting the information in the data on what happened to individuals who received a sanction and/or left welfare before t.

Another way to look at this is to note that 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. If sanctions are often followed very quickly by exit to work then this indicates a positive treatment effect. The selection effect does not give rise to the same type of quick succession of events.

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.

We take the joint distribution of the unobserved heterogeneity terms v_u and v_s to be bivariate discrete with two unrestricted mass-point locations for each term.[10] 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

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

It is easy to show that v_u and v_s are independent if and only if $\mathrm{cov}(v_u, v_s) = 0$. 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

[10] In the empirical analyses we allowed for more mass points. However, during the optimization of the log likelihood function the locations of the additional mass points converged to each other an no improvement in the optimal value of the log likelihood function was found. A more general issue is whether the results are sensitive to the choice of a discrete distribution for unobserved heterogeneity as opposed to a continuous or mixed discrete/continuous distribution. The methodological and empirical literature provides evidence that discrete distributions are sufficiently flexible to capture "random effects" unobserved heterogeneity (see Van den Berg, 2001).

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 became eligible for welfare, started to collect welfare benefits in Rotterdam in 1994 and who were obliged to search for a job. After excluding school leavers our data set consisted of 8411 job losers. In addition, we exclude 58 individuals for which the location of the neighborhood is missing. 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 econometrically 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. It should be stressed that if an unemployed worker who receives UI benefits gets a sanction imposed towards the end of his eligibility period, the sanction is not continued by the welfare agency, not even if the duration of the sanction is not yet expired. The final data set 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 can not identify multiple welfare spells of a single welfare recipient. If a worker experiences more than one spell of welfare starting in 1994, the data do not reveal that these spells are experienced by the same worker. About 2% of the welfare recipients have been given more than one sanction within a given welfare spell. Figure 1 shows the rate at which welfare recipients move to employment. This rate is highest at the beginning of the welfare period. During the first 6 months, the monthly re-employment probability is approximately 3.5%. After this period, the transition rate to work decreases over the welfare duration. The sanction rate is shown in Figure 2. The sanction rate is highest in the period from 5 months until 1 year, which coincides with the period at which the first thorough investigation takes place (see Subsection 2.2). During this period the sanction rate is around 1.5%.

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. An individual is considered as being married in case of marriage or cohabitation. The dummy variables "UI received" and "extended UI received" indicate if the unemployed worker entered welfare after his eligibility period for unemployment insurance (UI) expired. The dummy variable "UI received" equals one if the wel-

fare recipient did not receive the extended UI benefits. In this case the individual received UI benefits for 6 months. Extended UI benefits are only provided to unemployed workers who worked 3 of the 5 years previous to unemployment. For those unemployed workers the total UI period always exceeds 6 months (see Abbring, Van den Berg and Van Ours (1997) for a more extensive discussion of the UI system in The Netherlands). We do not have information on profession and level of education, because variables that are not relevant for the welfare agency are not included in the database. We include dummy variables for the 12 districts of Rotterdam. The administrative data do not suffer from selective nonresponse or attrition.

Table 1 provides some statistics of the data set. In 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. The last column of the table shows χ^2-test statistics for equality in re-employment and sanction probabilities between the subgroups of individuals (all based on one characteristic). The tests basically show that all characteristics are relevant, as we never accept equality. The exit probabilities of youths, females, non-natives, recurrent recipients, singles, and parents are lower than those of their counterparts. Individuals who collected UI benefits, but no extended UI benefits, previous to welfare, have higher re-employment rates than individuals who either did not collect UI benefits or also were eligible for extended UI benefits. 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.

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 of our baseline model, 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 of our baseline model. The parameter estimates of p_2 and p_3 are on the boundary of the parameter space $(p_2 = p_3 = 0)$, which implies that the unobserved heterogeneity components of θ_u and θ_s are perfectly correlated. The computed standard errors of all other parameters are conditional on this. Note that $v_u^a > v_u^b$ whereas $v_s^a < v_s^b$, so 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. Taken literally, the estimates of p_1 and p_4 imply that for given observed characteristics there are two groups of welfare recipients, which differ substantially in terms of re-employment rate and sanction rate. The group that represents 68% of the welfare recipients find a job rather quickly and face a small sanction rate. The other group has a re-employment rate that is only 13% of that in the first group, while the sanction rate is almost 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 transition rate from welfare to work. The estimated value of δ is 0.91 and is significantly different from 0. A sanction thus raises the transition rate from welfare to work by more than 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.[11] 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 Dutch man who lives downtown, experiences his first welfare spell and did not collect any UI benefits previous to welfare. Suppose that his unobserved characteristics equal the mean values of v_u and v_s in the inflow, and suppose that exits 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

[11] 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.

individual would have had a sanction imposed after 6 months of welfare then the probability of leaving within 2 years increases to 0.91. 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 0.54.

It is thus clear that welfare recipients are sensitive to benefit sanctions. 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 level of the welfare recipients may be very high, and this may explain a large change in behavior upon imposition of a sanction. Additionally, after the imposition of the sanction, the welfare recipient is more closely monitored. At the same time, the welfare agency will provide more information on how to search more effectively for a job. Part of the effect of the sanctions may therefore be caused by the counseling and increased monitoring of the welfare agency.

Now let us turn to the covariate effects on θ_u. These are all significantly different from 0. The rate is lower for older, non-Dutch and single individuals and for parents. Those who received UI benefits before have a higher re-employment rate than those who did not receive these benefits or who received extended UI benefits. However, the differences are small. It is interesting to pay close 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 much lower per-person benefits than a single individual, so one may actually 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). 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). 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 3. 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, stigmatiza-

tion 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, many welfare recipients have a 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 4, where the sanction rate has a peak at 10-12 months. The level is 60% higher than in the first quarter. After the first year the sanction rate has a downward trend. This suggests that the relevant individuals changed their search behavior. Alternatively, 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. The estimates of the covariate effects β_u and β_s provide other evidence of such an 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. Again, this may 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 re-employment 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 Subsection 2.2 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 re-employment rate. In particular, married individuals and new welfare recipients have a low sanction rate and a high re-employment rate. 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 sanction rates because they do not have a record of past sanctions.

Finally, there are significant differences in sanction rates between the dis-

tricts. The Likelihood Ratio (LR) test statistic for the sanction rates being the same across districts equals 37.0. This is consistent with the policy of most municipalities that gives the welfare agencies discretionary power in the process of imposing sanctions (see Subsection 2.2). However, the result may also be due to compositional differences of the population of welfare recipients between the districts.

5.2 Sensitivity analysis

In this subsection we examine the sensitivity of the parameter estimates with respect to the model specification. 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. Table 3 reports the parameter estimates for the effect of a sanction for the different model specifications investigated in this subsection.

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 LR test has a chi-square distribution with one degree of freedom under the null hypothesis of independence. The LR test statistic is equal to 19.8, 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 re-employment rate. More importantly, neglecting the (negative) relation between the unobservable components leads to underestimation of the effect of a sanction. Indeed, the sanction effect becomes insignificant. This again shows the importance of taking selectivity into account.

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. The LR test statistic on joint significance of all elements of γ is equal to 20.2. Since we have 12 additional parameters, we can not reject the null hypothesis that the sanction effect is independent of individual characteristics, at the 5% level. Taken in isolation, some individual characteristics have a significant effect on the effect of sanctions, which is actually in agreement to the theoretical model framework (see Subsection 3.1). In particular, the dummy variable for "new client" and the household composition variables have significant coefficients. The sanction effect is larger for new clients and single welfare recipients with children. It might be that new clients have less information about the seriousness of the sanction regime and the actual job search

requirement of the welfare agency than recurrent welfare recipients and therefore increase their job search effort more due to the closer monitoring and additional counseling of the welfare agency after imposition of a sanction. Single welfare recipients with children seem to be a special group as these individuals have relatively low re-employment rates, the highest sanction rates, and are obviously also very sensitive to sanctions. It seems that these individuals display lower job search effort. Therefore they have a large risk of getting a sanction imposed and once a sanction has been imposed they have to increase their search effort much more than other welfare recipients to comply to the welfare agency's guidelines.

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 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 Subsection 2.2), but in most cases it equals 1 month (602 cases) or 2 months (541 cases). It exceeds 2 months in only 13 cases.[12] 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. At first sight this seems puzzling from a theoretical point of view. After all, the benefits level increases upon expiration, and the search intensity is not expected to increase at that moment (see Subsection 3.1). An explanation is that in reality it takes some time before the adjusted job search behavior becomes effective after imposition of a sanction. In general, the process of hiring an individual takes time due to job interviews and administrative obligations of firms. This implies that there is some time lag between the moment at which the unemployed worker increases his job search effort and the moment his re-employment rate increases. It is not exceptional that this lag is one or two months, which is also the duration of benefits reduction $t_e - t_s$. This may imply that most of the increase in transition rate to work occurs after expiration. In addition, the additional job search assistance only starts after the sanction has been imposed, and this may also cause a lag. Finally, increased monitoring often only starts some months after the imposition of the sanction.[13]

[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

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_m be the effect in case of a reduction of $m\%$, $m = 5, 10, 20$, and δ_0 the effect of a sanction where the magnitude is missing in the database. The LR test does not reject the null hypothesis that $\delta = \delta_m$ for all $m = 5, 10, 20, 0$.

The sensitivity analyses above 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

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 asks too much from the data. Note that this in turn suggests some caution interpreting the results when allowing for different sanction effects during and after imposition.

[14]This would be in line with Fortin and Lacroix (1997), who find for Canada that the level of welfare benefits has a negative but small effect on the individual transition rate from welfare to work. A similar result for the U.S. is found by Hoynes and MaCurdy (1994), who use variation in the welfare benefits level over calendar time to estimate the impact of the benefits level on the welfare duration.

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 Subsection 2.1 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. Therefore, we estimate the same model focusing on all exits from welfare, i.e. we include exits because of marriage, leaving the municipality and stopping to apply for welfare benefits for unknown reasons in the transitions out of welfare. The estimated effect of a sanction is slightly lower but still significant, 0.73 (with a standard error of 0.23).[15]

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. In the empirical analysis on the effects of benefit sanctions

[15] In addition, we have modeled each transition rate to a destination state by way of a MPH specification, where we allow each rate to depend on whether a sanction has been imposed or not. We impose independence of the unobserved heterogeneity terms, i.e. the unobserved heterogeneity terms are not related to the unobserved heterogeneity terms in the sanction process. It turns out that sanctions do not have a significant effect on exit to the other destinations, except for stopping to apply for welfare benefits for unknown reasons. This suggests that some welfare recipients stop collecting welfare benefits after imposition of a sanction, possibly because they have alternative sources of income and are afraid of becoming accused of fraud. Alternatively, some welfare recipients may leave the municipality upon imposition of a sanction, possibly to try to collect benefits in another municipality.

one has to allow for selectivity in the imposition of benefit sanctions. We find that there is indeed such selectivity. Conditional on observed characteristics and elapsed duration, welfare recipients with a lower re-employment rate experience a higher sanction rate. After correcting for this selectivity, 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 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 the system of benefit sanctions affects the behavior of welfare recipients. The benefit sanction itself is temporary, but the effects are long lasting. Even after the sanction period expires the transition rate from welfare to work is higher than before the sanction was imposed. Apparently, marginal utility levels of welfare recipients are so high, and/or consumption smoothing is so difficult, that a relatively small sanction (and the threat of an additional severe punishment in case of recidivism) can have a large effect on search behavior. Closer monitoring and counseling by the welfare agency, which accompanies the imposition of a sanction, may also have a positive effect on the job search behavior of welfare recipients.

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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Sanction	Unemployment spell exits to work: no	Unemployment spell exits to work: yes	Unemployment spell is right-censored: no	Unemployment spell is right-censored: yes	Total number of individuals	χ^2-test for similarity (df)
Individual characteristics						
Age 18–25	43%	4%	41%	12%	3249	298.6
Age 26–35	35%	3%	50%	11%	2879	(15)
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	93.9
Female	34%	2%	56%	8%	2772	(6)
Dutch	39%	3%	47%	10%	6034	112.5
Non-Dutch	26%	3%	57%	14%	1944	(6)
Collected welfare before	34%	4%	49%	14%	4399	85.1
New client	39%	3%	50%	8%	3579	(6)
Single, no kids	38%	4%	47%	11%	5735	151.7
Single, with kids	20%	3%	67%	10%	807	(12)
Married, no kids	44%	3%	43%	10%	506	
Married, with kids	31%	4%	54%	11%	930	
No UI received	35%	3%	50%	11%	6064	20.0
UI received	42%	4%	43%	11%	929	(9)
Extended UI received	36%	3%	49%	12%	985	
Districts						
Centrum	35%	3%	49%	13%	444	131.2
Delfshaven	34%	3%	51%	12%	1695	(36)
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 "Total" column gives the total number of individuals in the sample with a certain characteristic. The last column gives the χ^2-test statistic for equality of the re-employment and sanction fraction across the values of the characteristic. The degrees of freedom of the test statistic are in parentheses.

Table 1: Some characteristics of the data set.

	Exit hazard θ_u		Sanction hazard θ_s	
Effect of a sanction				
δ	0.91	(0.34)		
Unobserved heterogeneity				
v^a	−2.76	(0.14)	−4.96	(0.49)
v^b	−4.80	(0.32)	−3.22	(0.26)
p_1	0.68	(0.22)		
p_4	0.32	(0.10)		
Duration dependence				
λ_1	0		0	
λ_2	0.12	(0.057)	0.25	(0.10)
λ_3	−0.035	(0.067)	0.41	(0.10)
λ_4	−0.23	(0.080)	0.45	(0.11)
λ_5	−0.27	(0.090)	0.016	(0.13)
λ_6	−0.33	(0.10)	−0.044	(0.14)
λ_7	−0.27	(0.11)	0.033	(0.15)
λ_8	−0.69	(0.13)	−0.33	(0.18)
λ_9	−0.73	(0.15)	−0.44	(0.21)
λ_{10}	−1.17	(0.21)	0.051	(0.23)
λ_{11}	−		−1.07	(0.59)
Individual characteristics				
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	−1.15	(0.11)	−0.63	(0.15)
Age 56–65	−2.26	(0.34)	−1.47	(0.43)
Female	−0.089	(0.049)	−0.69	(0.089)
Non-Dutch	−0.64	(0.059)	0.029	(0.084)
New client	0.17	(0.043)	−0.58	(0.079)
Single, with kids	−0.66	(0.091)	0.13	(0.13)
Married, no kids	0.32	(0.085)	−0.32	(0.15)
Married, with kids	0.0069	(0.073)	−0.28	(0.11)
UI received	0.13	(0.063)	−0.13	(0.11)
Extended UI received	−0.091	(0.067)	0.038	(0.10)
Districts				
Delfshaven	−0.093	(0.10)	0.0061	(0.15)
Kralingen/Crooswijk	0.26	(0.11)	−0.17	(0.17)
Noord	0.16	(0.11)	−0.25	(0.18)
Prins Alexander	0.32	(0.12)	−0.46	(0.21)
Overschie	−0.16	(0.18)	0.028	(0.27)
Hillegersberg/Schiebroek	0.32	(0.15)	−0.39	(0.27)
Hoek van Holland	0.67	(0.35)	0.50	(0.53)
Charlois	−0.011	(0.11)	−0.035	(0.16)
Feijenoord	−0.22	(0.10)	0.15	(0.15)
IJsselmonde	0.14	(0.12)	−0.66	(0.22)
Hoogvliet	−0.036	(0.13)	−0.079	(0.20)
log likelihood	-20685.89			
N	7978			

Explanatory note: Standard errors in parentheses.

Table 2: Estimation results of the baseline model.

	Sanction effect δ		log likelihood	df
Independent unobserved heterogeneity				
	0.0013	(0.066)	-20694.35	-1
Effect dependent of individual characteristics				
Intercept	0.38	(0.36)	-20675.80	+12
Age 26–35	0.10	(0.18)		
Age 36–45	0.44	(0.24)		
Age 46–55	0.26	(0.40)		
Age 56–65	1.52	(1.55)		
Female	−0.097	(0.21)		
Non-Dutch	0.18	(0.20)		
New client	0.41	(0.17)		
Single, with kids	0.84	(0.31)		
Married, no kids	−0.27	(0.39)		
Married, with kids	0.11	(0.26)		
UI received	0.11	(0.23)		
Extended UI received	0.056	(0.24)		
Effect different during and after sanction				
During	0.88	(0.34)	-20683.43	+1
After	1.32	(0.35)		
Effect dependent on the magnitude				
5%	0.89	(0.34)	-20684.97	+3
10%	0.62	(0.38)		
20%	0.86	(0.40)		
Unknown	0.95	(0.38)		

Explanatory note: Standard errors in parentheses; df is the change in the degrees of freedom relative to the basic model.

Table 3: Parameter estimates of the effect of a sanction for different specifications of the model.

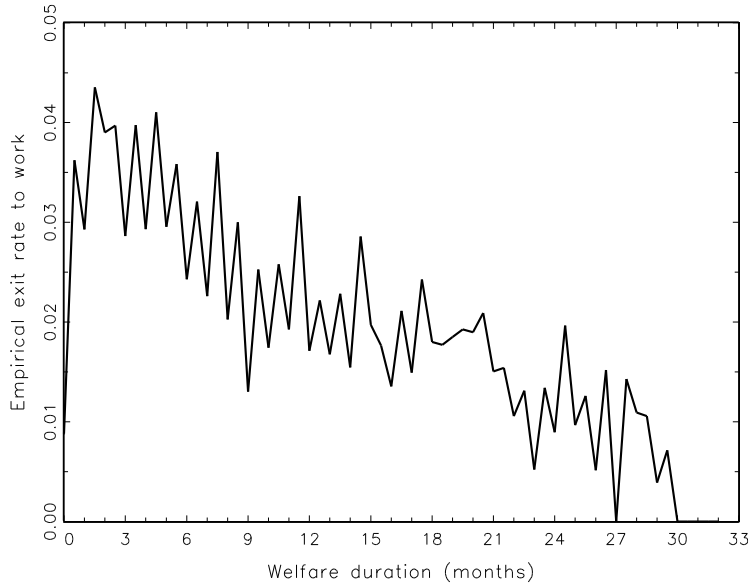


Figure 1: Empirical transition rate from welfare to work.

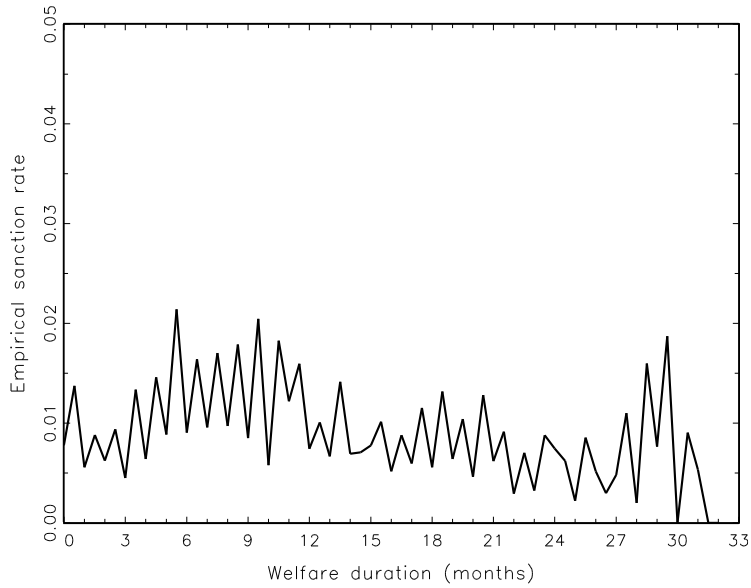
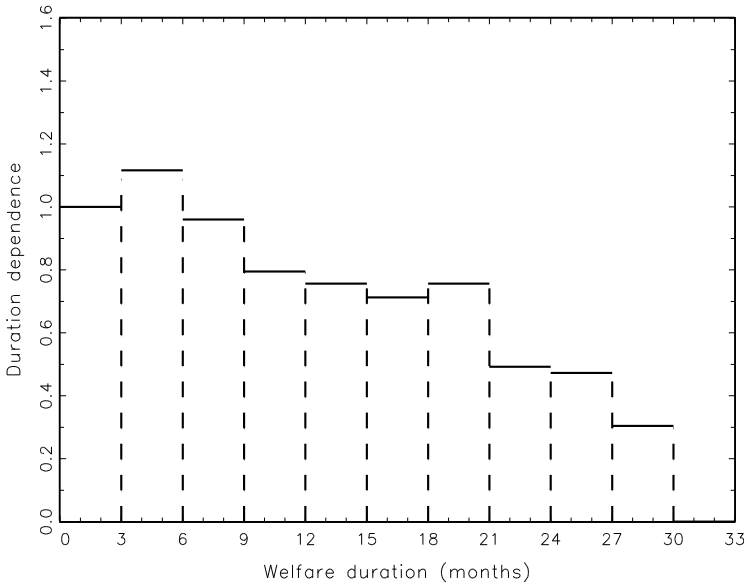
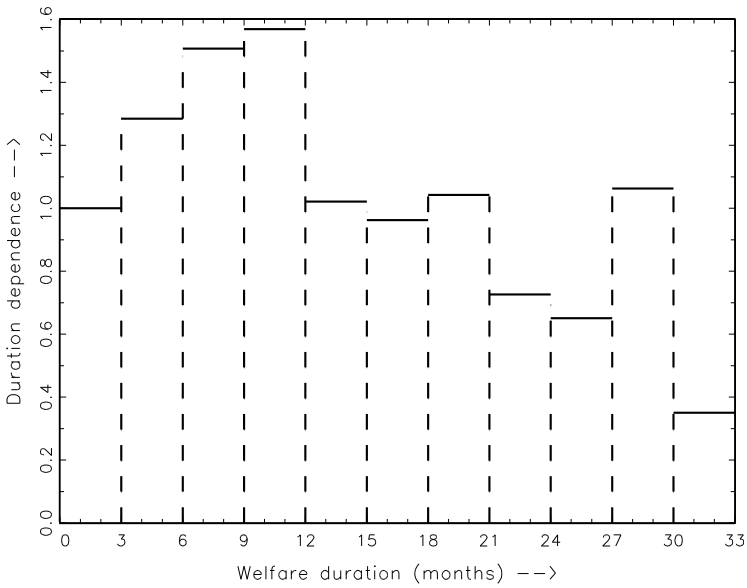


Figure 2: Empirical sanction rate.



Explanatory note: The first duration interval is normalized to 1.

Figure 3: Duration dependence of the transition rate from welfare to work.



Explanatory note: The first duration interval is normalized to 1.

Figure 4: Duration dependence of the sanction rate.