Making Work Pay for the Indebted?
Assessing the Effects of Debt Services on Welfare Recipients

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ABSTRACT

This paper investigates the effects of an intervention that was targeted at a specific group of Dutch Social Assistance (SA) recipients with debt problems. Since a large share of the income gains of work resumption is earmarked for creditors, these individuals generally experienced a strong a priori disincentive to resume formal work. This intervention had three aims: restructuring personal debts, preventing the occurrence of new debt problems, and increasing the direct incentives to resume work. The paper uses the Timing-of-Events method to identify the effects of debt programs on SA spells. Our main finding is that the debt program substantially increased the exit out of the SA schemes, but this was mainly due to exits out of the labor force. With a large share of assigned individuals who did not participate in the scheme, it thus appears that individuals perceived or experienced the program as unpleasant and opted to exit without work. Our results suggest the presence of threat effects, particularly for individuals who were assigned by their caseworkers but did not participate in the debt program.

Key words: Debt services, program evaluation, threat effects

JEL codes: c21, d14, h75, i38, j18, k35.

1. INTRODUCTION

In many Western countries, an increasing share of unemployed individuals faces unmanageable personal debts. These debts can have substantial consequences for work incentives, particularly when bankruptcy systems are creditor-oriented and a large share of the income gains from work resumption is transferred to the creditors for an extensive period of time (for a survey, see e.g. White 2011).[1] Unemployment Insurance (UI) and Social Assistance (SA) benefit administrations thus have an interest in the settlement of their clients' claims. Empirical evidence on the use and usefulness of debt programs for unemployed workers is, to the best of our knowledge, nonexistent. Also scarce, in a broader perspective, are empirical analyses on how household debt restructuring affects labor supply.

This paper attempts to break new ground by studying the effectiveness of an intervention that was targeted at Dutch SA recipients with debt problems. Individuals in this program lived in Amsterdam, the largest city of the Netherlands (about 800,000 inhabitants). This intervention had three aims: restructuring personal debts, preventing the occurrence of new debt problems and increasing the direct incentives to resume work. Individuals were first assisted with the restructuring of their debts and alerted about their entitlement to income supplements other than SA benefits, so as stabilize or solve debt problems. If clients did not succeed in debt restructuring, they were prepared for their eligibility to a formal, judicial (and more time-consuming) debt-restructuring program. Second, individuals were offered training programs to improve their budgeting- and financial literacy skills. These programs aimed to teach individuals how to become financially well organized and to understand the necessity of paid work.

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[1] In contrast to this, the most common personal bankruptcy procedure in the US exempts all future earnings from the obligation to repay (this is referred to as the 'fresh start').

The key question in this paper is whether the debt program contributed to the exit rates of the targeted group of SA recipients, into both employment and non-employment. Of particular interest throughout the analysis were potential threat effects, as individuals had to provide a full overview of their financial situation and income components. Threat effects may be particularly relevant for the individuals who were assigned but chose not to participate in the debt program – these were indicated as ‘no-shows’. For this group, abstaining from participation in the debt program was not without consequences, as it increased the likelihood of sanctions and intensive monitoring activities by their caseworkers.

Our analysis uses administrative data on SA unemployment spells and the assignment and actual start of debt programs. The identification of program effects relies on the assumption that SA recipients could not anticipate the exact timing of a debt program assignment – one of the key elements in the ‘Timing-of-Events’ method (Abbring and van den Berg 2003). Using this approach, we start by estimating the overall effect of the program on exit rates into employment and non-employment, regardless of whether or not the individuals who were assigned to the program actually participated. The resulting estimates can thus be regarded as ‘Intention-to-Treat’ effects. This model becomes our ‘baseline model’, which is then extended by allowing for different effects for program participants and the no-shows. We argue that extending the model in this way requires a more careful interpretation than is needed in the baseline model, as program effects may be biased by anticipation effects in the short run. This particularly holds if assigned individuals expected to exit the SA soon and therefore decided not to participate in the program.

This paper connects and supplements various strands of literature. To start with, a continuing stream of papers addresses the effectiveness of active labor market policies (ALMPs; see Kluve 2010 and Card et al. 2010 for recent surveys). Typically, this literature addresses instruments that directly aim at improving the job opportunities of workers by offering job training, job mediation, wage subsidies or subsidized employment. The idea behind the debt program, however, was to remove the incentive barriers that prevented individuals from accepting jobs and (related to this) improving their 'soft skills'— in order to prevent future debts. Although a growing body of research stresses the importance of social capital and financial literacy (see Heckman and Kautz 2012 and Lusardi and Mitchell 2014 for recent survey studies), there is virtually no evidence on interventions that are targeted at unemployed workers with debt problems.[2]

The second way this paper contributes to the literature is by building upon work on the importance of threat effects of mandatory job programs (for a recent survey, see Andersen 2013).[3] Threat effects are typically defined as increases in re-employment rates *prior* to the actual start of programs, when workers are already informed as to the starting date (Graversen and van Ours 2008; Geerdsen 2006; Rosholm and Svarer 2008). The implicit assumption is that mandatory programs are enforced for *all* workers that receive benefits at the time the program starts. In this context, a common finding is that threat effects are substantial in the relevant time interval, particularly when compared to the effects after the start of job programs. Presumably, unemployed workers derive disutility from job programs due a loss of leisure time and more interference by their caseworkers. As a result, they try to avoid program participation by searching more actively for jobs.

[2] Related to this literature, Della Vigna and Paserman (2006) investigated the effects of hyperbolic discounting on the return-to-work rates of unemployed individuals. The idea is that impatient workers will search less intensively for work; this effect dominates the effect of lower reservation wages and higher job-acceptance rates that characterizes impatient unemployed workers.

[3] In a broader perspective, this literature complements studies on the effect of sanctions on return-to-work rates (van der Klaauw and van Ours 2011; Abbring et al. 2005; van den Berg et al. 2004; Lalive et al. 2005; Jensen et al. 2003; Boockman et al. 2009).

In the current analysis, it is likely that threat effects were important as well, but worked in different ways. First, threat effects started from the moment individuals were contacted and assigned to a debt program. Participants had to provide a full overview of their financial situation and their income components. This explains why some individuals were not eager to participate, as this would harm their privacy or – worse – would reveal income fraud. Second, it should be noted that threat effects were likely to lead to increased exits into non-employment (see also Hagglund 2006, Arni et al. 2013 and Frijters and van der Klaauw 2006), rather than increased work resumption. This particularly holds for individuals who were assigned to the program but did not participate and were registered as 'no-shows'. The latter bore the risk of increased monitoring and sanctioning by their caseworker. Some individuals may also have left the scheme voluntarily for this reason.

Finally, this paper adds to the empirical literature on personal bankruptcy effects. Typically, this literature focuses on how between-state variation in exemption levels for wealth in the US affects the behavior of debtors and creditors. Evidence on the post-bankruptcy behavior of labor supply and work effort is, however, limited. Although filing for bankruptcy in the US generally reduces the obligations to repay debt from earnings, Han and Li (2007) find no evidence that this increases the labor supply of individuals.

Our main finding is that the debt program increased the exit out of the SA scheme. On average, the assignment to a debt program increased the exit probability by about 8 percentage-points, measured two years after the start of an unemployment spell. More strikingly, however, the effect is almost fully due to increased inflow into non-employment. This suggests the presence of substantial threat effects. We also find evidence that most of these threat effects are confined to the group of 'no-shows' that were assigned to the program but did not participate.

The paper proceeds as follows. Section 2 explains the institutional settings of the SA benefit scheme in the Netherlands, as well as the design of the debts program of the city of Amsterdam. We also provide a description of the data in this section. Section 3 explains the empirical strategy we use to assess the impact of the debt program. Section 4 presents estimation results and Section 5 concludes.

2. INSTITUTIONAL SETTINGS AND DATA

2.1 Priority Care Debt Services

In the Netherlands, SA benefits form a safety net that is provided by municipalities to support unemployed workers who are not or are no longer entitled to any other social insurance benefits (such as Unemployment Insurance (UI) or Disability Insurance benefits). In 2014, 22% of all new SA recipients consisted of unemployed workers who exhausted their UI benefits (UWV 2014).[4] Thus, the vast majority of the inflow consisted of individuals with insufficient work history for UI entitlement. SA benefits are both means- and asset-tested; individuals should not own more than 5,765 euro net worth of assets (for households with more persons, this net worth was set at a maximum of 11,895 euro). SA benefits are about 1,000 euro per month for single households, which is somewhat higher than in most other European countries. In principle, the provision of SA benefits is not limited in duration.

In June 2008, the social benefit administration of Amsterdam (DWI) announced its plans to provide debt services for SA clients (DWI 2008). DWI labeled the program as ‘Priority Care Debt Services’ (PCDS), referring to the fact that it intended to speed up the intake and treatment

[4] Depending on the work history of individual workers, UI benefits may last no longer than 38 months.

process for SA recipients with unmanageable debts.[5] The argument was that for many clients personal debts were a major impediment for work resumption, with creditors that are entitled to claim 90% of all additional income arising from work resumption.[6] After restructuring their personal debts, clients would no longer be restricted by time constraints in the repayment of their debts. DWI contracted two private organizations between November 2008 and July 2012 (*Westerbeek* and *Plangroep*) to organize the PCDS program. The financial condition of all new SA recipients was assessed during their first meeting with the caseworker. Depending on this assessment, caseworkers were then authorized to assign the client to the debt program. In principle, clients were informed of this fact either in this meeting or the next. After the announcement, it could take two weeks at the most before the actual program started. Participation in the debt program was not optional for clients; they were expected to cooperate with their caseworkers and the private debt-service providers. In case of non-compliance, temporary benefit reductions or benefit suspensions could be used to sanction individuals.

The provision of PCDS involved two types of services. First, budgeting courses aimed at helping individuals become more financially literate and increasing their ability to manage their income. Second, individuals were assisted in the informal restructuring of their debts and – if necessary – alerted as to their entitlement to income supplements (such as tax deductions and income subsidies). Debt-service providers started the program by gathering all relevant information on creditors and the size of debts. Clients thus had to provide full access to their financial administration. Typically, personal debts of clients originated from unpaid bills including fines, electricity or gas bills, (local) taxes, telephone bills, rents, insurance premiums

[5] Until that time, these individuals only had access to debt services that were provided by quarters in the city of Amsterdam, and were only were eligible if they had very substantial debts.

[6] Related to this argument, employers may find SA clients with debts unattractive for administrative reasons. For these workers, they have to cooperate in transferring earnings to creditors.

and medical treatments. Debt-service providers assisted clients in contacting their creditors and requesting a reduction of their debts and/or a relaxation of payment conditions. This part of the program was the most time-consuming and also the most costly.

In total, the individual debt programs were meant to last one year at the most. Typically, the programs consisted of about five meetings with the administration of the debt-services organization – with eight meetings at the most. Prior to these meetings, participants had to prepare information and learn the course material. From the perspective of DWI, a program was registered as ‘successful’ if it ended in one of three possible outcomes. First, the program could result in ‘financial stabilization’, with the client having no further direct need of debt services. Second, the outcome could be the start of a formal, judicial bankruptcy procedure. From the perspective of both the client and the social benefit administration, the outcome of such a process would harm work incentives for a long time. Third, if remaining debts were not too substantial, the city of Amsterdam arranged the settlement of debts by taking over the claims of individual creditors.[7]

2.2 Data description

Our analysis used an administrative sample of 29,855 unemployed spells of individuals between 18 and 65 years of age who entered into the SA scheme between November 2008 and December 2011. Benefit durations that had not ended are right censored at the first of July in 2012. 23,769 SA benefit durations in our sample are of individuals that are observed once.[8] For all clients that

[7] Unfortunately, we were denied access to information on the size of personal debts that were taken over, as well as on the share of debts that were cleared by the creditors. Interviews with caseworkers from DWI Amsterdam indicate that about 70% of debts were cleared.

[8] In particular, 2,705 individuals had two SA spells; 212 had three SA spells; ten individuals had four SA spells; and one individual had five SA spells.

received SA benefits in the time period under consideration, we observe the age, gender, education level, household status and their profiling category. We also know whether a client participated in unpaid municipal work programs; these were relevant for younger workers below the age of 27.

< INSERT TABLE 1 AND FIGURES 1 AND 2 HERE >

The second column of Table 1 presents the statistics of our full sample. Generally, job prospects of the inflow cohorts were poor; most SA recipients had low educational levels and resided in single households. In addition, only about 40% of completed spells were registered as ending in regular employment. Reasons (other than work resumption) for ending SA benefit spells include the imposition of sanctions due to insufficient search activities or fraud (16%); moving outside the city (10%); and other reasons (30%), such as incarceration, retirement, cohabitation and death.[9] To shed more light on the dynamics of SA exit rates, Figures 1 and 2 display Kaplan-Meier estimates of, respectively, the survival and hazard rates of SA benefit durations. After one year, only about 25% of the durations had ended for the group of individuals who were not assigned a debt program. As expected, Figure 2a shows that the hazard rate from the SA scheme into employment work decreases with the elapsed duration. For the hazard rate into non-employment, however, the picture is less pronounced – with some spikes at moments when the eligibility of clients was re-examined (see Figure 2b).

[9] Note that these sample percentages for exit destinations into non-employment could only be obtained for SA benefit recipients that did not participate in work programs (83% of the sample); for the group of individuals that participated, exit destinations are missing for about half of the sample.

In addition to the SA benefit durations, we have 2,940 records of (unique) SA clients who were assigned to the debt program. For assigned clients who started the program, we observe a sequence of dates when they reported to the two debt-service organizations – with the first date starting two weeks (at the most) after date of contact. For each meeting date, the corresponding activities are registered, together with an assessment of whether or not the assigned activities were completed. All of this eventually yields a sample of unique individual debt programs that can be merged to the sample of SA benefit spells.

< INSERT FIGURES 3 AND 4 HERE >

Figure 3 shows the debt-program assignment rates, which are measured as a function of the SA benefit durations. As expected, almost all assignments occur in the first three months of the SA benefit spell; this mirrors the fact that the financial assessment and the assignment were mostly carried out shortly after the moment of SA intake. Figure 4 shows the distributions of the debt-program lengths for completed debt programs. The average length was about nine months; about one-third lasted longer than the norm of one year.

Returning to Table 1, we see that clients who were assigned to a debt program were relatively young, male, more likely to join work programs, lower educated, and more likely to be a single parent than the full sample of SA recipients. In line with this, individuals that are profiled as lacking job skills are overrepresented in the targeted group. Figure 1 shows that the group of debt-program participants exited the SA scheme at a speed that is comparable to the group that was not assigned to the debt program. For the group of no-shows, exit probabilities are clearly higher.[10] Furthermore, columns (iv) to (vi) of Table 1 also show that the fraction of no-shows in the targeted group (33%) is substantial. Women, single parents and older workers are more likely to accept the debts program, whereas the opposite holds for individuals in work programs. In line with expectations, we also observe a relatively high share of SA exits due to sanctions among the group of no-shows (26%).

< INSERT FIGURE 5 HERE >

In our sample, we only observe information on the reported debts for 1,710 of the 1,944 clients that started the debts program.[11] Figure 5 shows the distribution of reported debts that follows from these data. The majority of individuals that started a program had debts that were not very substantial, with a medium value of 12,500 euro. Still, the highest percentile had debts of about 42,000 euros or more.[12] Debts were lowest for individuals under the age of 25 (about 9,500 euros on average); they then increase up to the age of 35, and remain more or less stable at the (average) level of about 25,000 euro.

3. EMPIRICAL IMPLEMENTATION

3.1 Identification

[10] As will be discussed in greater depth in the following sections, the higher exit rate of no-shows may reflect either sorting effects (as individuals may have anticipated leaving the scheme and therefore did not show up) or the presence of increased sanctioning and monitoring effects.

[11] Some program participants dropped out shortly after the start of the program. For some of these individuals, there was insufficient time to register their debts.

[12] To gain more insight into the determinants of personal debts, we ran an OLS regression with individual characteristics of program participants as explanatories. We then found age to be the most important driver of differences in debts. The estimation results are available upon request.

In our analysis, the assignment of individuals to debt programs can be characterized as dynamic – that is, SA recipients were assigned by caseworkers to debt programs at different moments in time. In general, this setting complicates the construction of comparable control groups, particularly if individuals already have left unemployment. With these data, many studies therefore advocate the use of continuous time methods to model selection on observable and unobservable variables; this is commonly referred to as the 'Timing-of-Events' approach (Abbring and van den Berg 2003). The idea is to jointly model exit rates and the rate into a certain treatment in a multivariate proportional hazard model. We also follow this approach in the current analysis.[13]

The identification of the Timing-of-Events methods hinges upon two key elements. First, proportionality in the hazard rates is necessary in order to identify the joint distribution of unobservable, time-constant variables. This identification requirement imposes a restriction on the parametric specification by using a mixed proportional hazard structure (van den Berg 2000). Second, we assume that the timing of debt-program assignments is not anticipated by the SA recipients. Arguably, this assumption can be justified by the short timespan between the announcement and the start of the debt programs that clients were assigned to, with clients having a timeframe of two weeks at the most within which to change their behavior.[14] Conditional on the no-anticipation constraint, it is possible to define counterfactual individuals that have not

[13] As an alternative approach to evaluating the treatment effect of the debt program, the use of matching techniques or instrumental variable estimation is not feasible in the current context. This would require the use of clear-cut assignment rules – for instance, depending on the level of debts that were registered. In contrast, the assignment largely depended on the discretion of caseworkers, rendering the use of Timing-of-Events most suitable.

[14] Since only ten clients who were targeted left the SA scheme in this period after notification, anticipation effects are probably negligible. Note that similar arguments apply in studies that estimate program effects (Kastoryano and van der Klaauw 2011) and the effect of meetings with caseworkers (van den Berg et al. 2012).

experienced a debt-program offer so far. This allows us to separately identify duration dependence, unobserved effects and the ex-post effect of the program.

It should be stressed that the non-anticipation assumption does not mean that debt programs are assigned randomly (conditional on observed characteristics). SA recipients may have known they were exposed to a high risk of being assigned to the PCDS program, but the idea behind the assumption is that they were unaware of the exact timing of this event.

3.2 The baseline model

Our empirical analysis starts by specifying a trivariate model that explains both SA benefit durations and the duration of realized SA benefit durations until the assignment to the debt program. We refer to this model as the ‘baseline model’ that explains the impact of debt-program assignment – regardless of whether or not this is followed by program participation. We also assume that the exit rates into employment and the exit rates into non-employment can be modeled as competing risks that explain SA benefit durations and the destinations after exit.

To formalize matters, consider an individual entering SA at date τ_0, who has been unemployed for t days. The exit rate from SA depends not only on calendar time $\tau_0 + t$ and the elapsed duration of SA benefits t, but also on observed individual characteristics $\boldsymbol{x}$ and unobserved characteristics v_u. Furthermore, we denote t_p as the elapsed duration after having started the debt program. Using the familiar mixed-proportional hazard structure and indicating the corresponding destinations into employment and non-employment as ‘e’ and ‘ne’, respectively, the SA exit rates into employment and non-employment are specified as:

$$\theta_l(t \mid x, \tau_0, t_p, v_l) = \lambda_l(t)\, \psi_l(\tau_0 + t) \exp\{ x\, \beta_l + \delta_l I(t > t_p) + v_l \} \qquad [1]$$

with l = { e, ne }.

In this expression, $\lambda_l(t)$ is a piecewise constant function that represents genuine duration dependence, and $\psi_l(\tau_0 + t)$ are genuine calendar time effects that are modeled as yearly dummies. The indicator *I* is a dummy variable that is equal to one for the event between parentheses. υ_l denotes the effect of time constant, unobserved characteristics on the exit rates into employment and non-employment. Our parameter of interest is δ_l , which describes the effect of the debt program on the transition rates out of the SA scheme into employment (l = e) and non-employment (l = ne), respectively. This effect comes into force at $t = t_p$, which is the moment at which a debt program is assigned.

Similar to the exit rates out of SA, we specify the transition rate of a debt-program assignment at time t_p as follows:

$$\theta_p\left(t_p \middle| x, \tau_0, \upsilon_p\right) = \lambda_p(t_p)\, \psi_p(\tau_0 + t_p) \exp\{ \boldsymbol{x}\, \beta_p + \upsilon_p \} . \qquad [2]$$

This equation makes apparent that the debt-program hazard rate is also driven by genuine duration dependence λ_p, calendar time effects ψ_p, and observed and unobserved individual characteristics (x and υ_p, respectively).

Equations [1] and [2] can now be used to formulate the individual likelihood contributions of both SA benefit durations and the SA benefit durations until the assignment to a debt program, with the individual indexed by i (i = 1,..N). Moreover, we use the subscript j to index the successive numbers of the SA benefit spell of the individual (j = 1,.., J_i). We denote c_i as an indicator that is equal to one (zero) if the outcomes are completed SA benefit durations. Likewise, c_{pi} is an indicator that is equal to one if the individual is assigned to the debt program. This yields the following expression for the likelihood contribution of an observed SA benefit duration t_{ij} with destination l_{ij} of individual *i*.

$$\mathcal{L}_{ij}\left(t_{ij}, l_{ij}\right) = \left\{ \theta_e\left(t_{ij} \middle| x_{ij}, \tau_{0ij}, t_{pij}, v_e\right)^{I(l_{ij}=e)} \theta_{ne}\left(t_{ij} \middle| x_{ij}, \tau_{0ij}, t_{pij}, v_{ne}\right)^{I(l_{ij}=ne)} \right\}^{c_{ij}}$$
$$\times \exp - \int_0^{t_{ij}} \left(\theta_e\left(t_{ij} \middle| x_{ij}, \tau_{0ij}, t_{pij}, v_e\right) + \theta_{ne}\left(t_{ij} \middle| x_{ij}, \tau_{0ij}, t_{pij}, v_{ne}\right) \right) dt \qquad [3a]$$

The individual likelihood contribution of the duration until the assignment to a debt program, during SA benefit duration j, t_{pij}, is equal to

$$\mathcal{L}_{ij}\left(t_{pij}, c_{pij}\right) = \theta_p\left(t_{pij} \middle| x_{ij}, \tau_{0ij}, v_p\right)^{c_{pij}} \exp - \int_0^{t_{pi}} \theta_p\left(t_{pij} \middle| x_{ij}, \tau_{0ij}, v_p\right) dt \qquad [3b]$$

We next assume the distributions of v_e , v_{ne} and v_p to follow a nonparametric, multivariate discrete distribution with unrestricted mass point locations for each term. This means that the SA exit rates and the debt-program assignment rate are allowed to be correlated, so as to control for selectivity on unobservables. When having *K* possible mass points, the associated probabilities of are denoted as follows:

$$p_k = \Pr\left(v_e = v_e^k, v_{ne} = v_{ne}^k, v_p = v_p^k \right) \quad \text{for} \quad k = 1,..K \qquad [4]$$

with $0 \leq p_k \leq 1$ and $p_K = 1 - p_1 - .. - p_{K-1}$ as restrictions. The joint likelihood function, $\tilde{\mathcal{L}}$, can then be written as

$$\tilde{\mathcal{L}} = \sum_{i=1}^{N} \log\left\{ \sum_{k=1}^{K} p^k \prod_{j=1}^{J_i} \mathcal{L}_{ij}^k\left(t_{ij}, l_{ij} \mid v_e^k, v_{ne}^k\right) \mathcal{L}_{ij}^k\left(t_{pij} \mid v_p^k\right) \right\} \qquad [5]$$

We use Maximum Likelihood estimation to estimate all parameters in equations [1], [2] and [4], while deriving the optimal number of mass points, K, by performing likelihood ratio tests. As we show later on, this yields three combinations of mass points.

3.3 The extended model

We argued earlier that many targeted individuals did not effectively start the debt program and were thus registered as no-shows. In this respect, the treatment coefficient estimates that are obtained from the baseline model can be characterized as Intention-to-Treat (ITT) effects. In the literature, this typically implies that the 'true' treatment effect is underestimated, since the effect applies to a fraction of the group that was assigned. Within the current context, however, this interpretation would ignore the fact that caseworkers may have increased their monitoring and sanctioning activities for the no-shows.

To shed more light on the mechanisms that drive the debt-program effects, one obvious extension of the model would be to differentiate the effects of program participants and the targeted group of individuals that did not participate. Denoting the actual start of a debt program with indicator dummy s, we can adapt the specification of the SA exit rates as follows:

$$\theta_l(t \mid x, \tau_0, t_p, s, v_l) = \lambda_l(t)\, \psi_l(\tau_0 + t) \exp\left\{ x\, \beta_l + I\left(t > t_p\right).\left(\delta_l^{part} s + \delta_l^{noshow}(1-s)\right) + v_s \right\} \qquad [6]$$

with l = { e, ne }. In this specification, δ_l^{part} and δ_l^{noshow} denote the effect of the debt-program assignment on the exit rate l for the debt-program participants and the targeted individuals that did not start the program, respectively. Conditional on assignment to a debt program, the probability of starting the program, P_s, is specified as a Random Effects Logit model that allows for duration dependence (ψ_s) and includes both observed and unobserved characteristics (υ_s) as explanatory variables:

$$P_s(\, s = 1 \mid x, \tau_0, t, \upsilon_s) \;=\; \frac{\exp(\psi_s(\tau_0+t) + x\,\beta_s + \upsilon_s)}{1+\exp(\psi_s(\tau_0+t) + x\,\beta_s + \upsilon_s)} \qquad [7]$$

The individual likelihood contribution of individual i with SA benefit duration j can then be written as

$$\mathcal{L}_{ij}\left(s_{ij} \mid t_{pij} < t_{ij}\right) = \; P_d\left(\, s_{ij} \,\middle|\, x_{ij}, \tau_{0ij}, t_{ij}, \upsilon_s\right) \qquad [8]$$

and the joint likelihood $\tilde{\mathcal{L}}$ can be extended as follows:

$$\tilde{\mathcal{L}} = \sum_{i=1}^{N} \log\left\{ \sum_{k=1}^{K} p^k \prod_{j=1}^{J_i} \mathcal{L}_{ij}^k\left(t_{ij}, l_{ij}, c_{ij} \mid \upsilon_e^k, \upsilon_{ne}^k\right) \; \mathcal{L}_{ij}^k\left(t_{pij}, c_{pij} \mid \upsilon_p^k\right) \mathcal{L}_{ij}^k\left(s_{ij} \mid t_{pij} < t_{ij}\right) \right\} \qquad [9]$$

with

$$p_k \; = \Pr\left(\, \upsilon_e = \; \upsilon_e^k, \upsilon_{ne} = \; \upsilon_{ne}^k \,, \upsilon_p = \; \upsilon_p^k \,, \upsilon_s = \; \upsilon_s^k\right) \qquad \text{for} \;\; k = 1, \ldots K\,. \qquad [10]$$

Similar to the baseline model, our estimation strategy is to allow for time-constant random effects υ_s that may be correlated with the random effects that influence the SA exit rates, the debt-program assignment rates, and the conditional probability of starting the debt program. At this point, however, it should be stressed that the no-anticipation assumption is probably more restrictive than in the baseline model. In contrast to the occurrence of a debt-program assignment, the participation decision may well be driven by the short-term expectations that individuals had to find employment or to exit the SA scheme without employment. Specifically, one may expect that targeted individuals who expect to leave SA benefit programs will not (choose to) participate in the debt program. This means that the outcomes of the extended model should be treated with greater care than for the baseline model – and with a special interest in the short-term and long-term effects for participants and the no-shows. We return to this issue in Section 4.

4. ESTIMATION RESULTS

4.1 The baseline model

Table 2 presents the parameter estimates of our baseline model explaining the SA exit rates into employment and non-employment, and the debt-program assignment rate (see equations [1], [2] and [3], respectively). For the distribution of unobserved effects that connects these three processes, we find at most three mass points, with probability weights equal to 26.4%, 53.2% and 20.4%. Note that for the third point of support we find the exit rate into employment to be equal to zero, whereas the exit rate into non-employment is highest for this category.

< INSERT TABLE 2 HERE >

According to our estimates, debt-program effects are substantial and significant only for the exit rate into non-employment. For the exit rate into employment, the effect estimate amounts to about 3.3% (=exp(0.032)-1), whereas the effect estimate is 42.9% (exp(0.357)-1) for the exit rate into non-employment. When assuming that debt-program assignment occurs in the second month of the SA benefit duration (which also was the average timing that is observed in our data), the total exit rate after two years of benefit receipt increases by about 8 percentage points. This effect is sufficiently large to make the debt program cost-effective: that is, the average debt-program costs of about 1,100 euro per person are already compensated within two years of the start of individual programs.[15] At the same time, however, our findings suggest that the targeted group perceived or experienced program participation as a disutility. Individuals may either have opted to leave the scheme for this reason, or they experienced more intensive monitoring and sanctioning activities that resulted in higher exit rates into non-employment. We return to this issue in section 4.2.

Regarding the other covariates, age is the most important determinant in all aspects. Both the SA exit rates and the debt-program assignment rate decrease strongly with age. A similar pattern is found for individuals that participate in a work program that is unpaid (i.e. they have higher SA exit rates and are more likely to be assigned to a debt program). SA exit rates into employment and non-employment increased as from 2010, reflecting the increased sanctioning

[15] For program participants, the average costs per individual amounted to 1,650 euro. With 67% of the targeted group participating in the debt program, the average costs for all individuals thus amounted to about 1,100 euro. The average monthly benefit level in our sample is 1,000 euro. After two years, the benefit savings for the targeted group amount to 1,350 euro (which is equal to a reduction of benefit spells of about six weeks). Note that we control for the fact that 30% of former SA recipients that were assigned to a debt program re-enter the scheme after one year.

and monitoring activities that started in these years (DWI 2010). As to the remaining explanatories, we find similar outcomes as were discussed in the data description.

< INSERT TABLE 3 HERE >

Table 3 displays the estimated treatment effects that follow from some additional analyses of the baseline model. To start with, we restrict the unobservable effects to be uncorrelated with the debt-program rate. Consequently, the effect of the debt program can be estimated without jointly modelling the debt-program rate. As the table shows, this would lead to a substantial downward bias in the treatment effect estimate on the exit rate into non-employment, justifying the use of Timing-of-Events.

We also investigated the persistency of the debt-program effects by allowing the effect to be different between the first four months and the later period of the benefit spell. Particularly for the exit rate into employment, we then find strong evidence for lock-in effects. That is, SA exit rates into employment decrease in the first four months after the assignment, but the effect becomes positive after that. Perhaps surprisingly, we find the SA exit rate into non-employment to persist and even increase when four months have passed since the assignment to the debt program. Presumably, caseworkers had a continued interest in the group of targeted individuals, and adhered more strictly to the rules for them as well. This confirms the findings of van den Berg et al. (2004), who argue that monitoring and sanctioning efforts may persist over time, particularly for individuals who have been sanctioned once.

Finally, the lower panel of Table 3 shows differences in treatment estimates for different samples, stratifying on age (younger or older than 30), gender and the education level (more or less than 11 years of education). Most notably, younger individuals respond to the debt program by increased exit into non-employment only – with a relatively high effect estimate. This contrasts with individuals above 30 years of age, who show comparable increases in the exit rate into employment and non-employment. In addition, the effect estimate of the debt program on the SA exit rate of female individuals into employment is substantially lower than that for men.[16]

4.2 The extended model

With debt-program effects that seemingly stemmed from threat effects into non-employment, a pertinent question is how effects differed between the program participants and the no-shows. To shed more light on this issue, Table 4 reports the main estimation results that follow from the extended model. Additionally, Table A.1 in the appendix to this paper displays all parameter estimates that are estimated. This table reveals that only a few variables have a significant impact on the conditional probability of debt-program participation, with the age of individuals as an exception to this. In particular, the participation probability increases with the age of individuals, suggesting that younger workers disliked program participation the most.

< INSERT TABLE 4 HERE >

Table 4 suggests that the increased exits into non-employment mainly originate from assigned individuals who did not show up at the start of the program. This confirms the idea that monitoring and sanctioning activities were increased for this group. To a lesser extent, we find evidence for both increased exit rates into non-employment for program participants, as well as increased exits into employment for the no-shows. There is no evidence that participants

[16] Note that 51% of the women that were contacted were single parents, whereas only 1.5% of males were single parents. Thus, gender differences are strongly correlated with household status.

benefitted by increased employment opportunities – as was intended by the debt program. On the contrary, our extended model outcomes indicate that almost all effects can be attributed to threat effects.

As argued earlier, the extended model does not incorporate the possibility of anticipation effects of unemployed individuals who were assigned to the program. In this respect, one may expect that individuals will be less likely to participate in the program if they expect to leave the SA benefit scheme soon. Consequently, the effect estimates for participants and no-shows may be misspecified, as sorting effects are only allowed to stem from time-constant unobservables. Similar to the baseline model, we therefore re-estimated the extended model with time-varying debt-program effects for participants and no-shows, defining the first four months after assignment as short-term effects (see the lower panel of Table 4 for estimation outcomes). As expected, we observe a strong drop in the exit rate into employment and non-employment for program participants in the four months after debt-program assignment. This effect is mirrored by increased SA exit rates for the group of no-shows, measured directly after the debt program. Both these findings suggest that anticipation effects did occur. At the same time, however, for the no-shows we also observe a further increase in the SA exit rate after the first four months had passed. As we presume that anticipation effects diminish over time, this lends credence to the idea that the treatment effect for the no-shows can be interpreted as behavioral effects – at least in the long run.

5 CONCLUSIONS

This paper studied the effectiveness of an intervention targeted at a specific group of Dutch SA recipients with debt problems who lived in the city of Amsterdam. Individuals were helped with the restructuring of debts, alerted on their entitlement to income supplements, and given access to training programs to improve their budgeting skills and financial literacy. The idea was that debt restructuring would remove an important impediment for work resumption. Without any informal settlement of claims, additional earnings of the targeted group would largely be transferred to creditors for a long period of time.

According to our analysis, the PCDS program impacted the behavior of individuals in different ways than was intended. Our main finding is that the debt program increased the exit from SA, but only into non-employment. This indicates the presence of threat effects. In addition, increased exits into non-employment were mostly confined to individuals who were assigned but did not participate in the debt program. To a lesser extent, threat effects may also have occurred for program participants, as they show increased exits into non-employment as well.

What explains these findings? Obviously, it is likely that assigned individuals derived disutility from the debt programs, as participation implied a loss of leisure time and more interference by their caseworker. For some individuals, providing full transparency on financial conditions may even have involved the risk of fraud detection. As such, our analysis adds to a recent strand of literature on threat effects as increases in exit rates into non-employment. As another explanation, one should also consider that the targeted group of indebted individuals probably discounted the future income gains at a high rate. As a result, they may well have undervalued the potential gains of the debt program (Della Vigna and Paserman 2005). This suggests that labor supply elasticity rates are low for the targeted group of workers, as they are restrained by time constraints – even if an important part of their debts will be resolved eventually. This contrasts e.g. to the analysis of Kostøl and Mogstad (2014), who do find substantial incentive effects for disabled workers with residual income capacity. Debt constraints may thus remain an important impediment for work resumption.

With this in mind, the policy implications of this study are not so clear-cut. The debt program was cost-effective. Presumably, it worked as a screening device to detect income fraud. At the same time, the debt program caused many assigned individuals to leave the labor market without employment. This poses the risk of increased debt problems, a higher incidence of illegal work, and high return rates into the SA scheme. How – and at what cost – these vulnerable groups should be stimulated to resume formal employment therefore largely depends on the normative judgment of policymakers.

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