ACTIVATION OF WELFARE RECIPIENTS:

IMPACTS OF SELECTED POLICIES ON RESERVATION WAGES, SEARCH EFFORT, RE-EMPLOYMENT AND HEALTH

INAUGURAL – DISSERTATION

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Contents

INTRODUCTION

When unemployment rose after the 1970s oil shocks, Europe's governments kept labor markets rigid and cut unemployment numbers by encouraging displaced workers to withdraw from the labor market. But over the next two decades, unemployment rates and the levels of structural unemployment in Europe continuously increased compared to the US and many economists blamed the policy responses of Europe's governments for this development (e.g. Nickell 2003).

Facing the lack of success of past policy responses in fighting unemployment, rising job insecurity due to globalization, rapid technological change and ageing labor forces, the OECD began in the 1990s to advocate the vision of an activating labor market policy (OECD 1997). The OECD recommended activating job-seekers by paying them unemployment benefits while at the same time fostering greater job mobility through requirements to look actively for work or participate in programs in order to promote their job prospects.

Activation of unemployed workers can be defined as all measures that provide incentives for the unemployed to increase their probability of finding a job. In turn, 'de-activating' policy measures inducing displaced workers to withdraw from the labor market, e.g. through the use of early retirement, disability benefits or unemployment benefits with lax job search requirements, should be avoided (Boeri and Van Ours 2008). Activation policies imply a mutual obligation system between the unemployed individual on the one side and the welfare state on the other side. To put it in a different way, activation policies try to provide a balanced mix of 'carrots' and 'sticks': financial benefits and job seeking support as desired 'carrots' which are linked to the obligation of the unemployed to be actively seeking for a job, enforced with benefit sanctions, the undesired 'sticks' (e.g. Martin 2000).

Like its European neighbor countries, Germany followed this recommendation and implemented between 2003 and 2005 one of the most ambitious and controversial reform projects in Germany's social insurance policies since World War II, called the *Hartz-Reforms* (Kemmerling and Bruttel 2005). One of the key objectives of the *Hartz-Reforms* was the activation of the unemployed in order to reduce unemployment, especially the high level of long-term unemployment. Still, four years after the implementation of the last of the four *Hartz-Reforms* the fight against unemployment remains being one of the most difficult challenges for Germany and its neighbor countries.

The most prominent part of the *Hartz-Reforms* was the *Hartz IV* Act. In 2005, *Hartz IV* merged former unemployment assistance and welfare benefits to the so-called 'Unemployment Benefit II' (UB II). This dissertation focuses on the activation of UB II recipients. Since UB II is a tax-financed and unlimited benefit designed to protect the recipients from poverty, I refer to them in the following as welfare recipients. All needy individuals who are classified as able-to-work are entitled to welfare benefits if they fulfill certain activation requirements by contributing efforts to end their benefit receipt. Activation requirements are enforced by strict benefit sanctions. Most welfare recipients are long-term unemployed and are less attached to the labor market than recipients of unemployment insurance (UI) benefits.

Although the key objective of the *Hartz-Reforms* was the activation of unemployed, until 2008, there was an exception regarding the group of unemployed individuals above 57 years, who before and after the reform were exempted from activation and rather 'de-activated' in order to fight registered unemployment ('*58er Regelung*', henceforth RULE58). According to RULE58, unemployed recipients of UI and welfare benefits above 57 years had the option to receive benefits and job search assistance, i.e. 'carrots', without being activated in the sense of having to prove their job search efforts, i.e. 'sticks'.

This dissertation contributes to the ongoing discussion among researchers and politicians about the effects and effectiveness of activating unemployed individuals. In particular, it analyzes the impact of selected activation policies on job search behavior (measured by reservation wages and search effort), on re-employment probabilities and on mental health of welfare recipients in Germany. The four empirical studies in this dissertation are among the first to study the effects of activation on behavioral and health outcomes in Germany.

Many earlier studies found positive effects of activating labor market policies on shorter durations of benefit receipt or higher re-employment probabilities of unemployed (for an overview consider Kluve et al. 2007). Yet, evidence on the impact of activation in general and of specific activation policies on the job search behavior of activated individuals – i.e. their reservation wages and search effort – and on the effectiveness of this job search behavior is missing.

Furthermore, 'sticks' in activation might also explain part of the negative impact of unemployment on mental health that is observed in the literature. Unemployed individuals might perceive 'sticks' in activation as non-pecuniary costs leading to a decrease in their utility. Thus, as an adverse side-effect that is usually not considered in the literature on activation, an increase in non-pecuniary disutility could lead to mental health problems of activated individuals. Finally, most studies on activating labor market policies analyze the population of unemployment insurance recipients; the population of welfare recipients might differ from unemployment insurance recipients since welfare recipients are typically less attached to the labor market.

In this dissertation, four questions are analyzed based on microeconometric ex-post evaluation studies: First, how do existing activation policies like benefit sanctions or customized 'individual action plans' affect the job search behavior of welfare recipients? Second, what are re-employment effects of these activation policies? Third, how does the job search behavior change if unemployed welfare recipients are not subject to 'sticks' in activation and get the opportunity to

receive benefits and support without job search requirements, monitoring and sanctions? Fourth, do we find that welfare recipients who are not treated with 'sticks' are in better mental health?

Microeconometric evaluation is concerned with the impact of selected policies on the individual. Depending on the timing of the evaluation one can differentiate between ex-post and ex-ante evaluation studies. Ex-post evaluation studies analyze the impact of policies after introducing them. Results of ex-post evaluation studies can be used to improve the design of policies. In addition, by assessing the benefits of a policy they can also be used for a cost-benefit analysis when related to the cost of the policy. A limitation of ex-post approaches is that in contrast to ex-ante evaluations, they do not provide ways of evaluating the effects of policies prior to introducing them.

Microeconometric evaluation can be regarded as the first step in an ideal evaluation process (Fay 1996). The second and third step would be a macroeconometric evaluation which answers the question if these impacts are large enough to yield net social gains and a cost-benefit analysis to see if this is the best outcome that could have been achieved for the money spent. This dissertation focuses on the first step.

Depending on the design of the policy that is to be evaluated, the selection process, and the available data, different evaluation strategies are in principle feasible (Caliendo and Hujer 2005). This dissertation assesses the impacts of activation on welfare recipients using variations of two main non-experimental evaluation approaches: statistical matching (for single and multiple treatments), and regression discontinuity design (fuzzy and sharp). The studies are based on a German cross-sectional survey "Life Situation and Social Security 2005". The rich survey entails a stock sample of more than 15,000 unemployed welfare recipients in January 2005 who were interviewed in winter 2005/2006.

The dissertation consists of seven chapters. Chapter 1 outlines the definition and goals of activating labor market policies and describes how the *Hartz-Reforms* strengthened the activation

principle in Germany's labor market policies. Chapter 2 describes the data. The first two studies are presented in Chapter 3 and 4 and assess the impact of key activation policies on the job search behavior of activated individuals and the question if potential changes in the job search behavior do indeed translate into larger probabilities for unemployed to find employment. The theoretical assumptions on the impact of these two policies job search behavior and employment are derived from a partial job search model.

Chapter 3 analyzes how benefit sanctions affect reservation wages, search effort and re-employment probabilities of welfare recipients who received a sanction using statistical matching for single treatments. Estimation results show that benefit sanctions have no effect on reservation wages or on search effort of welfare recipients but that they do increase the probability to find unsubsidized employment. This suggests that sanctions improve the job search effectiveness of welfare recipients.

Chapter 4 studies the effect of individual action plans as single treatment and of different types of individual action plans as multiple treatments on the same outcomes as in Chapter 3 – reservation wages, search effort and re-employment probabilities – using statistical matching for single and multiple treatments. Estimation results demonstrate that individual action plans significantly increase search effort and slightly reduce reservation wages of welfare recipients. A closer analysis of different types of individual action plans shows that only individual action plans with explicit search requirements yield these effects.

Yet, the effects on the search behavior had not translated into a higher probability for welfare recipients of being employed at the time of the interview, just into a higher probability of being enrolled in a work measure. Results therefore suggest substantial locking-in effects of individual action plans and lead to the question if these plans induce welfare recipients to substitute informal search effort with formal effort that can be less effective in finding employment.

In order to address the impact of the set of 'sticks' in activation on the job search behavior and on the mental health of activated individuals, Chapter 5 and 6 examine effects of the regulation RULE58 that lifts 'sticks' – job search monitoring, enforced by sanctions – in activation for welfare recipients older than 57 years.

Chapter 5 analyzes the impact of RULE58 on reservation wages using fuzzy regression discontinuity design. Estimation results show that participation in RULE58 increases reservation wages. This suggests that job search monitoring, enforced by sanctions, indeed can affect the job search behavior by reducing reservation wages.

Chapter 6 studies the effect of the mere option to enter RULE58 on mental health using sharp regression discontinuity design. Estimation results demonstrate that East Germans who have the option to enter RULE58 are in better mental health than non-eligibles while there is no similar effect found for West Germans. In West Germany, we have relatively low unemployment and a high sanction rate whereas in East Germany we have high unemployment and a low sanctions rate. This suggests that sanctions and other 'sticks' do not harm the unemployed in a situation when there is relatively low unemployment but that there are some potential benefits of lifting 'sticks' for unemployed in high unemployment regions.

Chapter 7 summarizes the findings and concludes. It also discusses potential shortcomings and problems of the analyses, provides the policy implications that can be drawn from the findings and offers an outlook for further research on activation policies.

CHAPTER 1

GERMANY'S SHIFT TOWARDS ACTIVATION

1 Activation in Labor Market Policy

Activation of unemployed workers can be defined as all requirements that provide incentives for the unemployed to increase their probability of finding a job (Boeri and Van Ours 2008), or, as Eichorst and Konle-Seidl (2008) define it, activation should bring the unemployed into work and should ensure sustainable independence from social benefits. Though activation is also supposed to alleviate the moral hazard problem of unemployment insurance by reducing the number of benefit recipients, activation is usually intended to increase the exit rates into regular employment (OECD 2005).

Activation policies imply a mutual obligation system between the employment office and the benefit recipient: benefits and support ('carrots') from the employment offices are linked to the obligation of the unemployed to take active steps to find a job, enforced with benefit sanctions ('sticks').[1] This links passive and active labor market policies, traditionally separate fields.

Activation of unemployed can include the general reduction of the level und duration of unemployment benefits, formerly a topic of passive labor market programs. Activation also involves direct requirements instructing recipients to supply their own efforts to end unemployment. Typical direct activation requirements involve participating in intensive interviews and the formulation of an individual action plan with case workers, applying frequently for job vacancies, undertaking verifiable independent search efforts, accepting offers of suitable work and attending work and qualification measures (OECD 2005). The work and qualification measures aim to improve or maintain the human capital of the unemployed.

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[1] The UK Restart program, which was introduced in 1987, can be viewed as a prototype for such strategies (Martin 2000).

Activation includes the monitoring of these requirements through the case workers in the employment offices. If unemployed workers do not meet them, they may lose their benefits entitlement temporarily through a benefit sanction or even permanently if they fail to meet requirements repeatedly. Thus, benefit sanctions belong to the set of activation instruments.

A practical rationale for activation is that activation is supposed to promote search effort and restrict benefits to the most needy. A priori it is not clear whether activation on the whole impose costs on the unemployed. The unemployed person perceives activation requirements like job search requirements as having costs and thus, disutility, but skill-increasing activation requirements can also be perceived as additional benefits and thus, utility. The theoretical impact of activation on the job search behavior is therefore ambiguous.

From the perspective of job search theory, activation has both ex-ante and ex-post effects on the job search behavior of job-seekers. Imposed activation requirements increase the job search intensity of benefit recipients, while the impact on their reservation wages and exit rates into employment remains ambiguous. Given the disutility involved in complying with activation requirements, activation requirements arguably have ex-ante effects. Some potential claimants might not want to initiate a benefit claim, or people on benefits drop their claim earlier than they would otherwise have done; in order to avoid complying with activation requirements, these individuals might increase their search efforts and reduce their reservation wages.[2]

As mentioned in the introductory section, there exist many studies on the effects of activating labor market policies in Europe finding that these policies can shorten the duration of benefit receipt or increase the re-employment probability, but there is no direct evidence of their behavioral impact of activation requirements on reservation wages, search effort or health of treated individuals.

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[2] Van den Berg et al. (2008) assess ex-ante effects of German activation requirements by estimating the impact of self-reported perceived treatment rates of active labor market measures on job search behavioral outcomes. Results suggest a negative ex-ante effect on the reservation wage and a positive effect on search effort.

Kluve (2006) performs a meta-analysis of the existing cross-country evidence on the effectiveness of European active labor market policies (ALMP). Traditional training programs modestly increase post-program employment rates. Relative to these programs, private sector incentive programs and 'services and sanctions' (a category comprising all measures aimed at increasing job search efficiency, such as counseling and monitoring, job search assistance, and corresponding sanctions in case of non-compliance) show a significantly better performance. Evaluations of these types of programs are 40-50% more likely to report a positive impact than traditional training programs.

By comparison, evaluations of ALMPs that are based on direct employment in the public sector are 30-40% less likely to show a positive impact on post-program employment outcomes. Kluve (2006) concludes that a well-balanced design of 'sticks' and 'carrots' in activation (consisting of job search assistance, counseling and monitoring, sanctions for noncompliance, and other active measures such as training and employment subsidies) seems to enhance job search effectiveness.

As indicated above, activation requirements might have adverse side effects. One potential adverse side effect of activation requirements is that needy job-seekers might not initiate a benefit claim. Further adverse side effects might be locking-in and substitution effects that reduce re-employment probabilities. On the one hand, workers might be locked in training or work measures and might reduce search effort because of their participation (Van Ours 2004). On the other hand, activation requirements might lead to a substitution of effort from informal to formal search, which might reduce the effectiveness of search (Van den Berg and Van der Klaauw 2006).

Further, unemployed who do not want to comply with activation requirements could instead choose to report sick and receive sickness or disability benefits in order not to have to comply but to avoid a benefit sanction (Hofmann 2009). Finally, complying with these requirements could impose

non-pecuniary costs on the unemployed. These costs might be detrimental to their well-being and health and ultimately lead to rising exits into disability and early retirement.

2 The Hartz-Reforms

The *Hartz-Reforms* consisted of a set of four labor market reforms that were implemented between 2003 and 2005, informally known as *Hartz I, II, III* and *IV*. These reforms rooted in the recommendations of a policy commission: the *Hartz-Kommission* named after its chairman Peter Hartz.[3]

Prior to the *Hartz-Reforms*, Germany had a three-tier system of income protection in case of unemployment: unemployment insurance benefits (UI, *Arbeitslosengeld*), unemployment assistance (UA, *Arbeitslosenhilfe*) and welfare (*Sozialhilfe* or *Hilfe zum Lebensunterhalt*). The unemployment insurance system in Germany built on an insurance principle according to which both employees and employers paid the same percentage (currently 3.3%) of the gross employees' salary into the unemployment insurance fund.

Employees who became unemployed received UI of 60% of their last net salary (67% if they had dependent children) for up to 32 months. The payment was not means-tested; its duration depended on the age of the unemployed and the length of time over which she or he had paid contributions in the seven years before becoming unemployed.

Once UI benefits had expired and the unemployed person passed a means test, she or he moved to means-tested UA, which was 53% of her or his last-earned net income (57% with dependent children). Whereas UI was contribution-based and limited in its duration, UA was

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[3] Facing high unemployment rates and high long-term unemployment, rising social security contributions, empty treasuries and a political scandal concerning exaggerated job placement numbers of the Federal Employment Office (FEO, now *Bundesagentur für Arbeit*), the government accelerated already initiated labor market reforms by implementing this *Hartz-Kommission* in 2002 and following its policy recommendations. Though former VW personnel chief Peter Hartz has lost his good reputation the reforms' name is settled. In January 2007, Peter Hartz was given a two-year suspended prison sentence and fined for awarding "special bonuses" to curry favor with the former head of the company's employee council.

unlimited and tax-financed. Both UI and UA were administered by the Federal Employment Office (FEO) which was also in charge of implementing active labor market policies (ALMP) and recipients of both benefits had access to the ALMP measures of the FEO.

In addition to UI and UA, there was former welfare which provided basic income protection on a flat-rate basis for all German inhabitants – with or without employment experience – who passed the means test and did not qualify for UI and UA. Welfare was paid for an unlimited period. Means-testing was stricter for welfare than it was for UA. If the individual level of UI or UA was below the threshold for welfare, additional welfare was paid until the minimum standard of living was secured. Able-to-work welfare recipients had access to a rudimentary labor market policy, the 'Help to Work' scheme (Article 18 to 20 BSHG) but not to ALMP measures of the FEO. The Help to Work scheme was operated by the municipalities with a considerable scope of discretion (Konle-Seidl et al. 2007).

The *Hartz-Reforms* made activation a key element of Germany's labor market policy. This already shows up in the principle of the reform, "*Fördern und Fordern*", i.e. supporting the unemployed on the one hand and demanding individual effort on the other. *Hartz I* (effective from January 1, 2003) established offices for temporary work (*Personal Service Agenturen*) and the implementation of training vouchers (*Bildungsgutscheine*). The criteria of suitable work (*Zumutbarkeitskriterien*) and the imposition of sanctions became tighter. *Hartz II* (effective from January 1, 2003) was supposed to activate the unemployed via tax/benefit incentives through "*Mini-Jobs*" and subsidized self-employment ("*Ich-AG*"). *Hartz III* (effective from January 1, 2004) aimed at the internal organizational reform of the FEO.

The final and most significant act, *Hartz IV*, reformed the German benefit system on January 1, 2005. *Hartz IV* merged former unemployment assistance and former welfare to a benefit for all needy and able-to work individuals, the so-called Unemployment Benefit II (UB II, *Arbeitslosengeld*

II). The new regulations were established in writing in the Second Book of Social Code (SC II, legally

absolute December 24, 2003).

The responsible body for the implementation of SC II, i.e. the payment of benefits and the

provision of employment services, are the FEO and the local authorities (Article 6 SC II). The FEO

is responsible for paying the basic welfare benefit and delivering the employment services. The local

authority is mainly responsible for housing and heating costs and delivering social services, such as

services for child care and drug- or debt-counseling services.

The maximum duration of earnings-related UI (now called Unemployment Benefit I,

Arbeitslosengeld I) is reduced from 32 months to 18 months.[4] After its expiry, needy jobseekers are

entitled to the so-called Unemployment Benefit II. Unemployment Benefit II is comparable to

welfare in other countries since it is an unlimited, means-tested benefit designed to protect the

recipient from poverty. Therefore in this dissertation UB II recipients are called welfare recipients

unless otherwise indicated.

Basically, every person is entitled to the new welfare benefit, who is able-to-work (defined as

being able to work at least three hours a day), who is between 15 and 64 years of age, who generally

lives in Germany and who is not fully able to cover her or his basic needs and the needs of the other

household members ("need unit", *"Bedarfsgemeinschaft"*)[5]. Benefit payments consist of the base

benefit, housing and heating allowances, and social security contributions. In the year 2005, the base

benefit was 345 Euros in Western Germany and 331 Euros in Eastern Germany.[6]

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[4] In the original *Hartz IV* Act, the maximum duration of UI was 12 months for unemployed below 55 years and 18 months for unemployed aged 55+. In 2009, the maximum duration of UI is 12 months for unemployed younger than 50 years, 15 months for unemployed aged 50+, 18 months for unemployed aged 55+, and 24 months for unemployed aged 58+.

[5] In the year 2005, a needy household (need unit) consisted in practice of the able-to-work, needy person, her or his partner and children (including single, able-to-work children below 18 years; now below 25 years). If a needy person is younger than 15 or older than 64 years, or not permanently able-to-work and member of a need unit, she or he is entitled to a benefit called "Sozialgeld" (Article 28 SC II).

[6] The base benefit is adjusted annually on July 1, in line with the current pension value of the statutory pension insurance system (Article 30,4 SC II). At the moment it is 359 Euros per month in East and West Germany.

The benefit is lower if the benefit recipient is member of a needy household: Adult partners receive 90%, children between 14 and 25 years 80% and children under 14 years 60%. The base benefit is higher for former UI benefit recipients, because in the two years after moving from UI benefit to UB II a declining bonus is paid.[7] Additional needs allowances for extra expenses not covered by the base benefit are paid for expectant mothers from the 13th week of pregnancy, for single parents depending on the age and number of children, for persons with disabilities and for expensive nutrition if demonstrably required for medical reasons.

3 The Shift towards Activation through Hartz IV

The central goal of activating labor market polices, the duty to actively overcome one's unemployment, is stated explicitly in the Social Code (Article 2 SC II). Already through the described reduction of the duration of UI benefits and the substitution of earnings-related UA benefits with strictly means-tested welfare benefits (UB II), the *Hartz IV* Act indirectly activated the group of unemployed entitled to UI and UA benefits. Also, *Hartz IV* directly activated the recipients of the new welfare benefit.[8]

Above all, the definition of suitable work is broader than for UI recipients and for former welfare recipients (Article 10 SC II). Basically, every job is suitable, independent of where the job is, of the needed qualification, the type of the contract and the wage. Only if a welfare recipient is strictly not capable of working on a certain job it is considered not suitable – in practice mainly, because of

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[7] Initially, the bonus is two thirds of the difference between UI benefits including housing allowance and UB II with an upper limit of 160 Euros for singles and 360 Euros for couples; each child raises the limit by 60 Euros. After one year, the bonus is cut in half.

[8] Activation has also increased for UI beneficiaries in the past years but to a considerably lesser degree. UI benefit recipients only have to accept and search for work that can be reasonably expected of them (Article 119(5), 121 SC III). In contrast to UB II, there is protection of an acquired earnings level and occupation. As mentioned above, the amount of UI benefit is regulated in Article 129 SC III and depends on family status, wage-tax bracket and weekly remuneration. Accordingly, insured persons with at least one child are entitled to 67% or, without children, to 60% of net remuneration fixed as a lump sum. UI claims do not take individual means or need into account. UI benefit recipients do not have to work in one-Euro jobs (see below).

temporary disability, because of parental leave with (one's own or one's partner's) children under three years, because of having children under 16 years and a lack of child care, because of having more than three children in school age and because of caring for relatives (when there is no other possibility). Furthermore, jobs are not suitable if they infringe upon employment protection or upon human dignity or personal rights of benefit recipients.

In contrast to former welfare, services for recipients of the new welfare benefit are largely identical to those for UI benefit recipients and involve counseling, job placement services, allowances for applications and travel costs as well as work and training measures (subject to Article 16 SC II). In contrast to the obligations of UI recipients, welfare recipients may be required to work in "one-Euro jobs", where participants are only allowed to work from 15 to 30 hours a week, and for no longer than three months to six months at a rate of around 1 to 3 Euros per hour as supplement to welfare.

Likewise, welfare recipients may be required to participate in qualification measures. These measures can be unacceptable only if they are not compatible with childcare or care for relatives and if these qualification measures clearly underchallenge the benefit recipient. It is the duty of the benefit recipients to prove that a job or a work or qualification measure is unacceptable.

Moreover, the SC II has introduced additional social services which have been designed specifically for welfare recipients and their particular barriers to employment like debt, abuse of alcohol or other drugs, psychological counseling and child care services stipulated in Article 16(2) SC II or a startup allowance pursuant to Article 29 SC II.[9]

It was mentioned already in the introductory section that individual action plans and strict benefit sanctions are key activation policies of Social Code II. Each case worker is supposed to conclude an individual action plan with the unemployed worker (Article 15 SC II) which regulates job search activities, verification of such activities, and all benefit payments and work or qualification

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[9] The Federal Employment Office is responsible for the base benefit of UB II and the employment services. The municipality is mainly responsible for housing and heating costs and social services.

measures the benefit recipient is entitled to. If welfare recipients do not meet their activation requirements, e.g. refuse a suitable job offer, a work or a qualification measure or do not sign an individual action plan, strict benefit sanctions can be imposed (Article 31 SC II). Chapter 3 and Chapter 4 describe benefit sanctions and individual action plans in detail.

Activating measures in SC II principally address all able-to-work welfare recipients. The medical definition of "able-to-work" – three hours a day in the foreseeable future under the usual conditions of the labor market – exceeds the target group of activation in many other European countries (Konle-Seidl et al. 2007). However, certain sub-groups of able-to-work persons are exempted from the availability criterion.

This holds for sick people and for persons who care for own children less than 3 years old or for family members. As mentioned in the introductory section, there was another noticeable exception to activation requirements: Subject to RULE58 (*58er Regelung*, Article 65 SC II), until 2008, older unemployed above 57 years – either receiving UI or welfare benefits – had the option to receive benefits and services of the employment office, i.e. 'carrots', without being activated in the sense of having to prove their job search efforts, i.e. 'sticks'. Chapter 5 offers a thorough description of RULE58.

CHAPTER 2

THE DATA

The four empirical studies in this dissertation are based on data from a German cross-sectional survey called "Life Situation and Social Security 2005" which was conducted on the behalf of the Institute for Employment Research (IAB) in winter 2005/2006. The aim of the survey was to analyze the effects of the implementation of *Hartz IV* in January 2005 (see Chapter 1). Thus, the survey is representative for two groups who were affected by the benefit reform: first, unemployed recipients of the new welfare benefit in January 2005,[1] and second, unemployed workers who were entitled to unemployment insurance benefits in December 2004 and ceased to receive benefits due to the reform in January 2005. The data is available as scientific use file. For further information on the survey consider infas (2006).

All of the four studies focus on the first group – a stock sample of 15,219 needy and able-to-work welfare recipients in January 2005.[2] The interviews took place about one year after the sample members entered welfare (in winter 2005/2006). This data set was matched with administrative data on the situation of the local labor market by district (e.g. unemployment duration or share of female unemployed) from December 2004 that stem from the Statistics Department of the Federal Employment Office.[3]

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[1] Defined as registered with the employment office as unemployed or job-seeking. Most welfare recipients are long-term unemployed but long-term unemployment is not a precondition for welfare receipt. Welfare recipients include all those who work and do not achieve sufficient earnings to cover the needs of their household and unemployed who did recently work, but not under social insurance or long enough to be entitled to UI benefit, when they lost their job. This sample, though, only includes unemployed welfare recipients.

[2] The survey is representative for the 266 German districts where in 2005 the employment offices and the municipality shared responsibility for the provision of services to the welfare recipients and where information on the able-to-work welfare recipients was available to the Federal Employment Office.

[3] The statistics are available at http://www.pub.arbeitsagentur.de/hst/services/statistik/detail/q.html (internet address from 22.11.2009).

The key advantage of using the survey for this dissertation is that it includes information on individual behavior and well-being of these welfare recipients as well as details on specific activation policies (e.g. on the type of individual action plans, see Chapter 4, or participation in RULE58, see Chapter 5) that cannot be obtained from administrative data. Thus, the survey enables us to address the question how selected activation policies (or the lack of certain activation policies) affect the individual search behavior and mental well-being of welfare recipients. An additional advantage is that the survey contains a huge variety of individual characteristics including extensive information on family background, education, and employment histories as well as benefit receipt.

A further advantage of this survey is that the information it collects stems from the implementation period (the year 2005) of the new benefit regime *Hartz IV*. The implementation of the new benefit system led to organizational restructuring, capacity constraints and software problems (see Koch et al. 2009). Therefore, there was less monitoring and thus more randomness in the assignment to activation policies than can be expected in normal times.

The new responsible institutions – the so-called *Job Centers* – had to be set up as one joint work unit of municipalities and employment offices who had not collaborated before. Apart from organizational and communication problems, there was by far not sufficient qualified personnel to deal with all cases. Nevertheless, in January 2005, case workers had to conduct a new means test for around 3.5 million potentially needy households. Moreover, by law case workers were meant to invite each of the 4.5 million able-to-work benefit recipients and carry out an individual profiling and individual action plans for them (statistics from the Federal Employment Office).[4]

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[4] 3.3 million households passed the means test in January 2005. These households consisted of 6.1 million individuals. Of these, 1.6 million individuals were not able-to-work, mainly children below 15 years (statistics from the Federal Employment Office).

Apart from the limited capacities, a further problem for intense monitoring and sanctioning was that the software was not reliably working the whole year (this is the reason why there exist no German administrative data on sanctions for 2005, see also Koch et al. 2009).

CHAPTER 3

EFFECTS OF BENEFIT SANCTIONS ON RESERVATION WAGES, SEARCH EFFORT, AND RE-EMPLOYMENT

1 Introduction

This study is concerned with a policy relevant question that is not answered yet: the impact of benefit sanctions on reservation wage and search effort of unemployed individuals. There exist several studies analyzing the effect of a benefit sanction on the duration of benefit receipt or the effect on re-employment probability, but there is no direct evidence of its impact on reservation wages or job search intensity of sanctioned benefit recipients.

Studies on optimal unemployment insurance show that a strict benefit sanctions policy might be more efficient than lower benefits to shorten unemployment duration (e.g. Boone et al. 2007). And indeed, most OECD countries have increased the use of strict benefit sanctions in the past years and continue to do so (OECD 2007). But a benefit sanction always leads to a cut in benefits that might cause economic hardship if benefit recipients are needy; hence, there should be evidence that sanctions show the effects supposed in theory.

In a job search theoretical framework, benefit sanctions reduce reservation wages and increase search intensity (Abbring et al. 2005). Since unemployed workers do not anticipate the actual moment of imposition of a sanction, the sanction reduces the reservation wage at the moment of imposition. At the same time, the unemployed worker increases search intensity since the lower benefit level makes it less attractive to stay unemployed. Both reduced reservation wages and increased search effort raise the exit rate to employment. These effects are temporary. The

unemployed worker knows the duration of the sanction and anticipates on the moment at which the sanction period expires.

However, it is reasonable to assume that sanctions are associated with more job search assistance on behalf of the employment office to avoid future sanctions and to improve job search effectiveness. At the same time the behavior of the unemployed worker is more closely monitored, and the magnitude of a subsequent sanction is typically larger. This suggests that unemployed workers are likely to comply with the requirements once they have received a sanction causing a permanent increase in re-employment rates through higher search intensity and lower reservation wages.

Note that already the possibility to receive a benefit sanction will affect reservation wages and search intensity (the so-called ex-ante effect of a sanction); the effect will be stronger if an unemployed worker actually received a sanction (the so-called ex-post effect).

There is no empirical research on how benefit sanctions affect reservation wages or job search intensity, either ex-ante or ex-post. But there are a couple of studies finding that both warnings and actually imposed benefit sanctions reduce the duration of benefit receipt and also increase exits into employment (Van den Berg et al. 2004, Abbring et al. 2005, Lalive et al. 2005, Svarer 2007; for Germany: Müller and Steiner 2008; Hofmann 2008).

Abbring et al. (2005), Lalive et al. (2004), and Svarer (2007) study exit rates out of unemployment benefit receipt, Van den Berg et al. (2004) study exit rates to employment. Abbring et al. (2005) found that exit rates of sanctioned benefit recipients increased by between 36% and 98% compared to non-sanctioned benefit recipients. This effect was stable across time and population. Lalive et al. (2004) could distinguish in their data the different impacts of warnings and real sanctions on exit rates out of unemployment. Warnings increased the exit rate by 25%, sanctions by 20%. The

effects did not differ over the population, but the warning effect fell to 16% after one month; the ex-post effect did not significantly differ over time.

Svarer (2007) found heterogeneous effects by gender: For males he noted significant evidence of ex-ante effects because the association between the risk of being sanctioned and the exit rate out of UI receipt was positive. After being sanctioned, exit rates increased by 98% for women and by 55% for men. Regarding welfare recipients, Van den Berg et al. (2004) find exit rates to employment to increase by more than 140% after a two-week reduction in benefits was imposed. The effect persisted beyond the sanction period. A harder sanction did not result in stronger effects. They did not find different impacts across the population.

For Germany, Müller and Steiner (2008) analyze ex-post effects of sanctions on unemployment-to-employment transitions of UI as well as unemployment assistance (UA) recipients in Germany. They find positive short- and long-term effects of benefit sanctions for men and women and in East and West Germany on the transition from unemployment to employment. The positive effects diminish with elapsed unemployment duration before the sanction is imposed. Hofmann (2008) studies the ex-post effect of UI sanctions in West Germany on two employment outcomes. For both men and women, the author reports evidence of an average ex-post effect of a UI sanction on the probability of being regularly employed These effects are mainly driven by young UI recipients. Regarding the outcome "other employment", the results are ambiguous: for women they are positive, but negative for men.

It is difficult to derive the direct effect of the impact of sanctions on the job search behavior from these studies. A priori both search and reservation wages can be affected by benefit sanctions. Moreover, these studies usually face the difficulty to separate the effect of benefit sanctions and the effect of the associated increased job search assistance on unemployment duration. Ashenfelter et al.

(2005) find no effect of pure monitoring associated with sanctions on the duration of unemployment in an experiment conducted in the US.[1]

Studies on the unemployment income elasticity of reservation wages find small and often insignificant results (e.g. Van den Berg 1990, Prasad 2003, Christensen 2005). Accordingly, the probability of accepting a job offer proves in the majority of cases to be close to one (Cahuc and Zylberberg 2004), suggesting that the reservation wage lies very close to the lower bound of the distribution of wages existing in the economy.

This study analyzes the ex-post effects of benefit sanctions on reservation wages and search effort using the cross-sectional survey of German unemployed welfare recipients in 2005 that is described in Chapter 2 in combination with statistical matching techniques. It completes the analysis by estimating the effect of sanctions on re-employment probabilities. Identification of the average treatment effect on the treated is achieved by means of the very rich dataset and additional variation generated by the implementation of the new welfare benefit system *Hartz IV* in 2005 (see Chapter 1).

The remainder of this chapter is organized as follows. Section two describes welfare sanctions in Germany. Section three describes the sample of analysis and treatment and outcome variables. Section four explains the identification and estimation method and presents the estimation results. Section five concludes.

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[1] Job-seekers were randomly separated in three groups, one control group and two treatment groups. The control group was faced with the usual conditions of eligibility for UI benefit. The two treatment groups were at their first visit notified of additional compulsory job search requirements. At their second visit, the two treatment groups were treated differently. For one, job search requirements were monitored, while this was not done for those in the other treatment group. The job-seekers who could not prove that they contacted an employer received a sanction. Ashenfelter et al. (2005) found that the rates of exit from unemployment for the individuals in the two treatment groups were not statistically different.

2 Welfare Sanctions in Germany

As mentioned in Chapter 1, welfare sanctions can be received for various non-compliances with activation requirements, above all for showing not enough search effort or refusing an integration measure (including work measures, so-called "one-Euro jobs", see Chapter 1) or a suitable job offer. Most non-compliances lead to sanctions that cut the monthly base benefit (without allowances for accommodation and heating) by 30% for three months.[2]

For example, the base benefit for a single living in the western part of Germany was reduced from 345 Euros to 242 Euros per month in 2005. Exceptions are sanctions for the failure to report to the *Job Center* or to meet appointments. These failures can reduce the base benefit by 10% (reduction to 311 Euros per month in the example). Benefits are always cut for three months, even if the requirements are met in the meantime.

If during the sanction period the sanctioned welfare recipient repeatedly does not comply, the monthly base benefit is supposed to be cut by another 30 percentage points (ten percentage points if the obligation to report is not met), and so on, up to a 100% reduction. The intensity of sanctions is higher for people younger than 25 years and lower for people older than 57 years. Young welfare recipients from 16 to 24 years of age can receive a 100% cut of the base benefit already for the first non-compliance (with allowances for housing and heating paid directly to the landlords).

Older welfare recipients who turned 58 years before January 1, 2008 have a lower risk of receiving a sanction since they can opt for RULE58 where they are allowed to receive welfare without being subject to activation requirements if they commit themselves to apply for a regular pension as soon as possible (see Chapters 1, 5 and 6).

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[2] The following description of welfare sanctions refer to the period from 2005 to 2007. Welfare sanctions were further tightened on July 25, 2006. A detailed description of welfare sanctions offer Bruhn Tripp and Tripp (2007).

3 Data

The empirical analysis is based on the survey described in Chapter 2, the cross-sectional survey called "Life Situation and Social Security 2005". The advantage of the dataset for this study are that it covers the implementation period of the new benefit regime and that it is very rich and contains information on reservation wages, active search effort and sanctions.

The survey measures two sets of wages. The first wage is the wage job seekers expect to earn in a post unemployment job. The second wage is measuring the minimum wage job seekers are willing to accept, i.e. the reservation wage.[3] Arguably, the stepwise collection of hourly reservation wages transmits the idea of the reservation wage better to the respondents than a one-step collection of monthly information as typically done in similar surveys (e.g. the GSOEP) leading to higher reliability of results.

Search effort is measured through the share of individuals reporting to have actively searched for a job in the four weeks prior to the interview. Both reservation wages and search effort are collected for all sample members, employed or not employed when interviewed.

3.1 Sample Description

The final sample contains all people aged 15 to 57 who entered the new welfare benefit (UB II) right from the start (January to March 2005), with complete information on sanctions, hourly reservation wages, current search effort and the control variables used in the analysis. Excluded are observations

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[3] The exact questions are: 1.) "What net wage do you expect to earn per month?" together with a question asking how many hours per week the person would expect to work for the reported amount. 2.a) Persons who did answer question one are asked in a second step if they would be willing to work for a monthly net wage lower than the first reported value. If so, they are asked how high this lower wage has to be at least so that they still would be willing to work. Again, persons are asked for the working hours per week they expect to work for this reported wage. 2.b). Persons who refuse to answer question one are in a second step asked: "What is the least net wage per month so that you still would be willing to work?" together with the working hours they would expect to work.

with reported reservation wages between one and 20 Euros per hour (excluding 73 observations).[4]

Excluded are also 27 observations that were sanctioned in a later welfare spell.[5] The final sample has

9,313 observations.

In the sample, 5.8% (541 observations) received sanctions in 2005, seven months after

entering welfare on average. Case workers increased the use of sanctions with the duration of the new

benefit system: the sanction rate per calendar month increased from less than 1% in spring 2005 to

2.4% in December 2005.

Sample members characteristics are presented in Table 1. Note that 50% in the sample are

women and 55% live in West Germany. 29% have no graduation or a graduation from a *Sonder-*

/Haupt- and Realschule and no vocational training; 60% have either *(Fach-) Abitur* and no vocational

training, or a graduation from a *Sonder-/Haupt- and Realschule* and an apprenticeship; 5% have *(Fach-*

) Abitur and an apprenticeship or are master craftsmen and 7% have a university degree. 33% of the

sample members have some type of migration background.

3.2 Descriptive Statistics

Table 2 shows the average labor market attachment at the time of the interview, approximately one

year after entering welfare. Only 11% are in unsubsidized employment.[6] Most sample members

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[4] Information on last net wages (i.e. wages resulting from an employment that started before our observation period 2005), and thus, on the ratio of reservation wage to last net wage (rwr), are only available for jobs lasting at least until January 2004; but three quarters of the sample members were already unemployed at this time. Thus, missings in last net wages are not a reason for exclusion. Due to our target group, reported net wages under one Euro per factual working hour (in 91 cases) and over 50 Euros (in two cases) are assumed to be implausible and put to missings.

[5] The focus here is on the first sanction people may receive within their first spell of UB II in 2005, since the used data stem from a survey of people who entered UB II for their first time in 2005 and are observed for the limited period of a year.

[6] A person is defined as currently in unsubsidized employment if she or he reported to be regular employed, self-employed, employed in a Personal Service Agency at the time of the interview, and not receiving welfare and/or being in work measures at the same time.

continue to receive welfare benefits (76%). 9% are in a work measure (a one-Euro job) and 13% are employed while receiving benefits.[7]

The mean reservation wage and the average share of active job-seekers (reporting to have been actively searching for a job during the last month) at the time of the interview are presented in Table 3. 63% of the sample report active search effort. The mean reservation wage of sample members is 6.15 Euros/hour (West Germans 6.67 Euros/hour, East Germans 5.50 Euros/hour).

Table 3 compares further the search effort outcomes and the employment states at interview by treatment. T-tests show that the sample means of search effort, reservation wages and employment are not statistically different at a significance level of 0.1% between the sample of the sanctioned and no-sanctioned individuals. A simple eyeball test of the histograms of the reservation wages by treatment status supports the resemblance of their distributions (see Figure 1).

Note that reservation wages of the sample lie at the lowest bound of the distribution of wages existing in the German economy, typical for the reservation wages of welfare recipients as suggested by Cahuc and Zylberberg (2004). Net wages at the bottom tenth percentile of the 2005 wage distribution were 6.05 Euros/hour in East Germany and 7.97 Euros/hour in West Germany (own calculations based on Gernandt and Pfeiffer 2006); and the upper threshold for low-wage jobs (defined as two-thirds of the median gross wage) was 10.20 Euros/hour in West Germany and 7.40 Euros in East Germany in 2004 (Rhein and Stamm 2006).

Even if imprecise questionnaires are accounted for, sanctions seem not to be well understood by the sanctioned individuals at least during the implementation period of the new benefit system. Only 36% of the sanctioned who are older than 24 years answer that they received a sanction because of refusing or quitting jobs, qualification or work measures, refusing to sign individual action plans or

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[7] Working individuals may still receive additional benefits if workers are below the socioeconomic poverty level (see Chapter 1).

not meeting duties of individual action plans, because of misinformation on income or assets or spending money in an uneconomical way ("justification one" in the following).

The picture of justifications for the sanctioned respondents who are younger than 25 years is a bit straighter: The majority (54%) report to have been sanctioned based on justification one; still, 17% are given "other reasons". The share of sanctioned individuals who report other justifications than given in law is surprisingly high. 46% answers they were given "other reasons" for their sanction. Also the sanction duration varies more than expected. Though by law, each sanction lasts three months, sanction duration is widely spread (mean 11.0 weeks, SD 9.9 weeks) and varied by justification.

4   Empirical Approach and Results

4.1   Identification and Estimation

In order to estimate the effect of benefit sanctions on reservation wages and search effort of sanctioned welfare recipients, we have to deal with a selection problem: even if monitoring was less intense than in normal times during the implementation period of the new benefit system, sanctioned individuals might differ in pre-treatment characteristics from the other unemployed welfare recipients.

The statistical matching approach is one possible solution to deal with this selection problem.[8] Its basic idea is to find non-sanctioned individuals who are similar to the sanctioned individuals in all relevant characteristics before the sanction was imposed. To identify the causal effect of benefit sanctions, the key assumption needed is the "Conditional Independence

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[8] The standard framework in evaluation analysis to formalize this problem is the "Roy-Rubin-model" (Roy 1951, Rubin 1974). The matching approach was originally developed in the statistical literature; see e.g. one benchmark study of Rosenbaum and Rubin (1983).

Assumption" (CIA): the data at hand include all relevant variables that affect both treatment

assignment (receiving a sanction) and outcome (reservation wage and search effort).[9]

In general terms, the better and more informative the data are, the easier it is to credibly

justify the CIA and the matching procedure. However, some randomness is needed that guarantees

that persons with identical observable characteristics X can be observed in both states (common

support or overlap condition, see e.g. Caliendo and Kopeinig 2008).

This study argues that the conditional independence between sanction status and the

outcomes reservation wages and search effort is plausible since first, the dataset is very rich in

variables and second, the data stem from 2005 which was the implementation period of the new

benefit system *Hartz IV* causing less monitoring and more randomness in treatment assignment than

one would expect (see Chapter 2). Hence, failure to comply with job search requirements did not

mechanically lead to a sanction in 2005. Therefore, the sanctioned individuals arguably are a less

selective sample than they would be in normal times.

In a case study with 90 *Job Centers* on behalf of the German Federal Audit Office, the *Job

Centers* did not follow up hints for facts that would lead to a sanction in 60% of all cases

(Bundesrechnungshof 2006). Though this number is only representative for the observed 90 *Job

Centers*, heterogeneity in the sanctioning behavior of the case workers is also reflected in our sample

(see above) and in former evaluations of UI sanctions (Müller and Oschmiansky 2006). But even

three years later, German case workers seem to be very heterogeneous in their sanctioning behavior.

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[9] See Caliendo and Kopeinig (2008) for a detailed description of the identifying assumptions The second assumption of no general equilibrium effects of imposed sanctions is plausible since the observed shares of sanctioned of below 3% per calendar month are so small that the actual imposition of these sanctions does not plausibly change reservation wages or search effort of non-sanctioned sample members. The third assumption of exogeneity of relevant control variables implies that all control variables are determined prior to the sanction. The fourth assumption implies that statistical matching is performed only on common support (see main text).

Some use sanctions as *ultima ratio*; some use them frequently as activation instrument (Baethge-Kinsky et al. 2007).

Given the additional variation in the sanctioning process that we have in our data due to the implementation period, this study argues that the actual selection process can be captured by the rich set of variables available in the dataset containing the information the case workers had about their unknown clients and a variety of good proxies for the welfare recipients' motivation and ability and the behavior of the case worker (see Meyers et al. 2006, Müller 2007).

Provided the CIA holds for some set of variables X, i.e. the treatment is independent of the treatment assignment conditional on X, Rosenbaum and Rubin 1983 showed that it is also independent conditional on the probability of being treated given observed characteristics X, the propensity score: $p(X)=P(D=1|X=x)$ ($D=1$ if treated, 0 otherwise). If the so-called balancing score property holds, then after matching on the propensity score, there will be no statistically significant differences in the covariate distributions between treated and controls for each distinct value of the estimated propensity score. In summary, the propensity score matching estimator is simply the mean difference in outcomes over the common support between treated and matched controls, appropriately weighted by the propensity score distribution of participants (Caliendo and Kopeinig 2008).

It is important to account for the duration of welfare receipt when estimating the probability to receive a sanction. First, all potential controls to a sanctioned person should receive welfare at least as long as the sanctioned person when the sanction took place. Second, as described above, case workers imposed more sanctions over the time of the implementation. Third, the behavior of a welfare recipient had to be watched by the case worker first, so that the probability of sanctions increased with welfare duration. Fourth, elapsed welfare duration is likely to capture to some extent a

person's unobserved ability and motivation to exit welfare. Ability and motivation, again, will have an impact on reservation wages and search effort.

Since the duration of welfare receipt after a sanction is endogenous, both the propensity score and the average treatment effects are estimated separately for each of the first four quarters of the first welfare spell (following Sianesi 2004 and Fitzenberger and Völter 2007). The treated in each estimation sample are those sample members where the start month of their sanction was in quarter $u=1,2,3,4$ of their first welfare spell; the controls are those sample members who were at risk of receiving a sanction in quarter u because they continued to receive welfare until the end of quarter u. Sample two, for example, contains all persons receiving at least six months welfare without being sanctioned during the observation period ("controls") and all persons receiving a first sanction in month four, five or six after entering welfare receipt ("treated"). Implicitly, the start of a sanction within one quarter is random conditional on X.

For the eligibles at u, treatment receipt is denoted by D_u, that is $D_u = 1$ for receiving a sanction that started in quarter u, and $D_u = 0$ for not receiving a sanction until this quarter, but welfare benefits. The estimated treatment parameter for each quarter u is then

$$ATT_u = E(r_1 - r_0 | X, D_u = 1) =$$

$$= E(r_1 | X, D_u = 1) - E_X\left[E(r_0 | X, D_u = 1) \middle| D_u = 1\right] =$$

$$= E(r_1 | X, D_u = 1) - E_X\left[E((r_0 | X, D_u = 0) \middle| D_u = 1\right],$$

where the first term can be estimated from the treatment group and the second term from the mean outcomes of the matched controls. The outer expectation is taken over the distribution of X in the treated population.

Note that a welfare sanction in our sample typically lasts around three months as described above. This indicates that at the time of the survey, sanctioned individuals in quarter one to quarter three had already seen their sanction removed and are not longer directly affected by the treatment.

Sanctioned individuals in quarter four are still directly affected by the treatment. Since reservation wages and active search effort refer to the time period when the interviews were conducted for the first three quarters, we estimate the effect of a past sanction on current reservation wages and search, and for the fourth quarter, of a current sanction on current reservation wages and search.

Hence, for the interpretation of the results of the first three quarters we have to assume that the behavioral impact of sanctions is not only temporary (as in the model of Abbring et al. 2005 and empirically backed by results of Lalive et al. 2005 and Van den Berg et al. 2004). Lifting this assumption, we can interpret the result in the fourth quarter as direct effect of a benefit sanction on reservation wages and the results in the other three quarters as indirect effects of a benefit sanction, e.g. by finding a job due to decreased reservation wages or increased search.

4.2 Estimation Results

To account for potential sources of unobserved heterogeneity and maintain the CIA plausible, a rich specification of the propensity score model was chosen. Demographic variables, skill level, employment history and former benefit receipt are assumed to capture the search behavior of the individual welfare recipient.[10] Variables for the level, structure, and dynamics of local unemployment in December 2004 are assumed to capture the behavior of case workers and the local labor market situation.[11]

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[10] Covariates used are age, gender, detailed household context with age and number of own children, existence and employment status of partner, marital state, nationality and nationality of parents, language skills, pre-observation period skill levels, employment history and benefit receipt (education and vocational training, ever employed, employed at 31st December 2004, duration of employment, unemployment and qualification in the five years before December 2004, last earnings, receipt of benefits in December 2004, driver license) as well as if the UB II recipient signed an individual action plan (including the timing).

[11] Variables are living in West or East Germany, information by district on unemployment rates, the average unemployment duration and proportions of statistically disadvantaged unemployed like long-term unemployed, foreigner, young people and women, the share of service jobs of all jobs subject to social insurance contributions per district as a proxy for structural change of a region, the ratio of offered jobs to all unemployed per district and the number of one-Euro jobs to all unemployed by district for 2005 (proxying the extent of active labour market policy).

Remember that the duration of welfare receipt is taken into account by estimating the effects for four subsequent quarters of welfare receipt. The chosen variables showed significant impact in previous research on sanctions (e.g. Meyers et al. 2006, Abbring et al. 2005, Müller 2007); moreover, they predict sample members' reservation wage and search well.[12] Details on the specification of the probit estimation and the estimation results for all four quarters are presented in Table 4. Sample averages of the covariates are displayed in Table 1.

Due to the medium sample size and the similarity of propensity scores between treated and controls, radius matching was used as matching procedure (see Caliendo and Kopeinig 2008). Radius matching aims to avoid bad matches by imposing a tolerance level on the maximum propensity score distance (a so-called caliper). Hence, radius matching is one form of imposing a common support condition. As mentioned above, the common support or overlap condition ensures that persons with similar values in observables have a positive probability of being both treated and controls. Only the subset of the controls whose treatment probability is comparable to the treated should be used in the analysis (Dehejia and Wahba 2002).

Smaller calipers increase the variance of the estimation but reduce the potential bias resulting from controls being too different to compare.[13] Thus, three different calipers are chosen to define the radius: (1) 1.00, (2) 0.10 and (3) 0.01 percentage points from the propensity score of the treated (caliper 0.01, 0.001 and 0.0001, respectively). The smaller the radius, where the propensity scores of the potential controls have to be in, the less controls are found for the treated, of course. Hence, an important step is to check the overlap and the region of common support between treated and controls.

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[12] Regressions are available upon request.

[13] For the analytical variances and hence the standard errors of these estimators see Becker and Ichino (2002).

4.3 Common Support and Matching Quality

Several ways are suggested in the literature, the most straightforward one is a visual analysis of the density distribution of the propensity score in both groups (Caliendo and Kopeinig 2008).

Figures 2 to 5 show the kernel density estimations of the propensity scores for each quarter of welfare receipt, for treated and controls respectively. The overlap in densities is satisfying. As a next check of common support, Table 5 presents the numbers of potential observations of treated and controls, of observations on common support, and of matched observations. For each of the three calipers and each quarter, only few observations of treated are lost through the matching procedure, and in all estimations, a few thousand controls are matched to the treated. Even with the smallest caliper and the last quarter of welfare spell, 1,479 controls remain (in this case for 147 treated).

Since matching is not performed on all covariates but on the propensity score, it has to be checked if the matching procedure is able to balance the distribution of covariates in both the control and treatment group. Indicators for the mean matching quality for each of the estimations are presented in Table 6. The matching quality is most satisfying for matching with the intermediate caliper 0.001 though the quality is also satisfactorily high for the other two calipers.

One suitable indicator to assess the distance in marginal distributions of the X-variables is the standardized bias suggested by Rosenbaum and Rubin (1985). For each covariate X, the standardized bias is defined as the absolute value of the difference of sample means in the treated and matched control subsamples as a percentage of the square root of the average of sample variances in both groups. One pitfall of this indicator often pointed out is the fact that it has no formal (statistical) threshold for assessing the success of the reduction in mean bias. Nonetheless, the obtained values seem to grant some success in the matching procedure, especially for the best-performing caliper 0.001, with mean absolute biases of 1.2 to 1.5 in the matched sample, while in the unmatched sample

the biases are between 9.4 and 15.2. These values are in line with previous empirical studies (Sianesi 2004, Caliendo and Kopeinig 2008).

Additionally, Sianesi (2004) suggests re-estimating the propensity score on the sample of matched treated and controls and comparing the Pseudo-R^2's before and after matching. After matching there should be no systematic differences in the distribution of covariates between both groups and therefore, the Pseudo-R^2 should be fairly low. As it can be seen in column 4 of Table 6, the Pseudo-R^2 in the propensity score estimation that used only the treated units and the matched control units falls to values close to zero.

Furthermore, one can also perform a likelihood ratio test on the joint insignificance of all regressors in the probit model. The test should be rejected before, and should not be rejected after matching. None of the estimations fails the test. As an ultimate check for the matching quality, Table 7 reports for each single covariate the p-values of the standard t-test for the equality of mean sample values and the standardized bias for the caliper 0.001.[14] In most cases, the standardized bias is reduced by between 80% and 90%. After matching, we fail to reject the null hypothesis of mean equality between the treatment and control groups for all variables.

The conclusion to be drawn from this exercise is that we achieve acceptable levels of covariate balancing as indicated by individual t-tests, the reduction in the absolute bias and the joint significance of covariates and pseudo-R^2 before and after matching.

4.4 Matching Estimates by Outcome

Having discussed the quality of our matching and the common support and overlap as a precondition of matching, we now turn to our estimation results of the effect of benefit sanctions on reservation

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[14] The remaining results are available from the authors upon request; qualitatively they are similar to the ones discussed herein.

wages, search effort and employment. The outcome variables are the log hourly reservation wage, search effort (having searched for a job in the last four months prior to the interview) and two types of employment at the time of the interview: being in unsubsidized employment without receiving welfare benefits (independently of the hours worked) and being employed but additionally receiving welfare. The category 'being employed but additionally receiving welfare' may include subsidized employment other than one-Euro jobs. We proceed by discussing first the results on reservation wages, then the results on search effort and finally, the results on employment outcomes.

4.4.1 Reservation Wages

The point estimates of the treatment effect on the treated in Table 8 indicate negligible impacts of the benefit sanctions on reservation wages. Remember that mean reservation wages are around 6.15 Euros in the sample. Sanctioned welfare recipients had reservation wages that were 1.4% lower in the first quarter and 2.6% and 4.5% higher in the second and third quarter than if they had not received a sanction. However, all estimates are not statistically different from zero. Likewise, the effect on reservation wages of sanctioned welfare recipients who still are subject to reduced benefits in the fourth quarter is very small and statistically insignificant (1.5%).

Regardless if we assume sanctions to exert only temporary or permanent effects, the impact of benefit sanctions on reservation wages of our sample of sanctioned welfare recipients stays both economically and statistically insignificant. Thus, in what is theoretically assumed is one of the behavioral effects of benefit sanctions, namely reducing the reservation wages of unemployed workers, the current assessment shows that the sanctions did not have the expected effects at least for our sample of welfare recipients.

4.4.2 Search Effort

Column 2 of Table 8 presents the behavioral effect of benefits sanctions on search effort. Again, results are statistically insignificant. Remember that 63% of the sample search actively for a job when interviewed. This time, point estimates indicate that the share of sanctioned individuals reporting to actively search for a job is between 0.6 to 4.2%points higher than of the non-sanctioned at the time of the interview in the first, third and fourth quarter and lower (-0.5%points) in the second one.

If we consider especially the point estimates of quarter four when sanctioned individuals are still subject to reduced benefits, the effect is again, like in the case with reservation wages, very small and not statistically different from zero. We can conclude that the impact of sanctions on the active search effort of our sample of sanctioned welfare recipients is small and statistically insignificant. Lifting the assumption of permanent effects of a sanction does not alter this conclusion.

4.4.3 Re-employment

We saw that the point estimates of a sanction on both reservation wages and search effort slightly differed over time though they were not statistically different from zero. Benefit sanctions are assumed to translate into increased employment probabilities (Abbring et al. 2005). Since for the first three quarters the results presented above show the effect of a past sanction on current reservation wages and search effort, reservation wages and search effort of those sanctioned at an earlier stage might be influenced from increasing exits into employment due to the imposed benefit sanction. To explore the bias this might have on our results on reservation wages and search effort, the effect of benefit sanctions on re-employment probabilities is analyzed. Apart from this methodological reason, the employment effects of benefit sanctions are of great policy relevance.

We start by exploring a traditional category of employment outcomes, namely, being in unsubsidized employment (independently of the hours worked). The results suggest that sanctions

increase the probability to be in unsubsidized employment: for the first quarter, the share of sanctioned individuals in unsubsidized employment is 8.6%points higher than if they had not been exposed to a sanction. Given that only 11% of the sample members are in unsubsidized employment when interviewed the size of this coefficient is very large and it is statistically significant. This effect is positive in the other three quarters and in the fourth quarter also statistically significant (+ 4.5%points).

This finding could be sensitive to the definition of controls observations since we expect them to remain in welfare receipt until the end of the fourth quarter. If control observations are allowed to leave welfare in the first month of the fourth quarter already, the point estimate in the fourth quarter changes to 3.6%points and lose their statistical significance. The first column of Table 9 reports the results.

Turning to the second column of Table 9 we see the results of benefit sanctions on current employment with additional welfare receipt. Note that this category includes subsidized employment other than one-Euro jobs. Contrary to the results on unsubsidized employment, the effect is negligible for all quarters: it is both economically and statistically insignificant.

If control observations are allowed to leave welfare in the first month of the fourth quarter already, coefficients stay close to zero and insignificant. That sanctions exert an effect only on employment without welfare receipt explain why Schneider (2008) in an earlier study on benefit sanctions did not find significant effects of sanctions on employment in general.

Hence, employment results suggest that benefit sanctions increase the probability for welfare recipients to find unsubsidized employment. This effect is larger and significant if the sanction is imposed early in the welfare spell. The positive effect on transitions into employment is in line with previous research on both UI and welfare sanctions (e.g. Abbring et al. 2005, Van den Berg et al.

2004, Müller and Steiner 2008). Sanctions do not seem to lead to take up (probably low-paid) employment with additional welfare receipt.

Under the assumption of homogenous responses of all treated independently of the quarter the sanction was imposed, we can interpret the effects in the quarters as short-term and long-term effects on outcomes. Under this assumption, sanctions increase employment already in the short run. In the longer run, the effect is stronger. However, for sanctioned German UI recipients, Müller and Steiner (2008) provide counter evidence for this assumption in showing that positive effects on unemployment-to-employment transition diminish with the elapsed unemployment duration until a sanction is imposed. This would suggest that those sanctions earlier in the welfare spell lead to stronger employment effects in the short and the long run.

4.5 Further Sensitivity Checks

Apart from matching with different calipers, estimation results are robust towards the specification of the number of months in the estimation samples, of different criteria for common support and matching algorithms (radius, kernel, nearest neighbor and stratification matching). Estimations were repeated using a six and 12-month stratum instead of four strata of welfare receipt. The main results do not change. Further, the results are not sensitive to the definition of control observation (minimum benefit receipt 1/3 of the quarter, 2/3 of the quarter or whole quarter).

5 Conclusion

This study analyzed the ex-post effects of benefit sanctions on reservation wages and search effort using a cross-sectional survey of German unemployed welfare recipients of 2005 and statistical matching techniques. Results suggest that benefit sanctions do not significantly reduce the reservation wages of welfare recipients. Neither do sanctions significantly increase the search effort measured as the share of active job-seekers at the time of the interview.

These results hold in all chosen specifications and independently of the timing of the sanction. Since search effort and reservation wages may only temporarily be influenced by a sanction it is important for the interpretation of our results that we do not find any effect on search effort and reservation wages even if the sanction was received shortly before the interview. The results for reservation wages are in line with previous research on the low and often insignificant elasticity of reservation wages with respect to unemployment income (e.g. Van den Berg 1990, Prasad 2003, Christensen 2005).

Findings on employment probabilities suggest that benefit sanctions substantially increase the probability to be in unsubsidized employment when interviewed (but not probably low-paid employment with additional welfare receipt). This result is in line with previous research on both UI and welfare sanctions. The positive employment effect is larger for those who received a sanction earlier in their welfare spell.

Hence, one should be cautious in saying that welfare sanctions have no effect on the job search behavior at all. If search effort and reservation wages react only temporarily after a sanction and if sanctioned in the first three quarters have shown a different behavior than sanctioned in the fourth quarter, they could have reduced their reservation wages and/or increased their search effort with the result of higher employment probabilities. This is a rather strong hypothesis, of course.

Another interpretation of the findings is that case workers increase counseling after a sanction and thereby enhance the search efficiency of job-seekers (i.e. a higher probability to receive a job offer for a given search effort). Therefore, results could be a sign that sanctions increase employment via an increase in the efficiency of job search and not via reduced reservation wages or whether or not someone searches at all. This hypothesis should be further investigated in future research with panel data and alternative measures for search intensity, for example the number and type of search channels unemployment use for their search.

One should further be cautious in applying the results on welfare sanctions to UI benefit sanctions since UI benefit recipients are typically better attached to the labor market which might lead to a different job search behavior. Mean reservation wages of our sample, for instance, are with 6.15 Euros per hour at the lowest bound of the distribution of wages existing in the German economy. Thus, most paid wages are above this level suggesting that the arrival rate of job offers is crucial for welfare recipients staying unemployed. Reservation wages, for instance, of UI recipients are generally higher than those of welfare recipients so UI recipients might show a larger decrease in their reservation wages due to receiving a benefit sanction.

Appendix: Tables and Figures

Table 1: Mean Characteristics of the Sample

	Mean	SD
Received a sanction?	0.058	0.234
Demographic and household variables		
Gender=woman	0.503	0.500
Married man	0.173	0.378
Married woman	0.164	0.370
Partner 2004 (reference: no partner)	0.566	0.496
Employed partner	0.144	0.352
Non-/unemployed partner	0.290	0.454
Age 2004 (reference:15 to 25 years old)	0.124	0.330
25 to 40 years old	0.409	0.492
40 to 50 years old	0.302	0.459
50 to 58 years old	0.165	0.371
More than 2 children in hh 2004	0.065	0.247
Child under 3 in household	0.092	0.288
OECD equivalent net monthly household income 12/2004 (Euros)	631.041	342.055
Other earners in household	0.203	0.402
Migrant status (reference: migrant: other nationality, parents and interview language)	0.108	0.310
Migration background: German/other nationality, parents or interview language	0.220	0.414
German: German nationality, parents and interview language	0.673	0.469
Qualification		
Qualification (reference: low)[a]	0.285	0.452
Intermediate	0.599	0.490
High	0.050	0.219
Very high:	0.066	0.248
Drivers' license?	0.670	0.470
(Very) good German language skills	0.877	0.328
Employment history		
Employed at 31 December 2004	0.100	0.300
Cumulated duration of employment[b]	19.609	18.220
Cumulated duration of unemployment	29.886	20.040
Cumulated duration of qualification	6.642	11.450
Hourly net wage (missing are zero)	1.958	3.751
Indicator for missing net wage	0.647	0.478
Benefit receipt 12/2004 (reference: no benefits)	0.132	0.339
Unemployment Benefits	0.633	0.482
Welfare benefits	0.175	0.380
Unemployment and welfare benefits	0.046	0.209
Individual action plan		
Individual action plan	0.292	0.455
Regional labor market		
Share of service jobs 12/2004 per district	0.645	0.083
Ratio of offered jobs to registered unemployed in 2005 per district	0.045	0.038
Ratio one-Euro jobs/stock of UB II-unemployed 2005	0.236	0.121
Unemployment duration 12/2004 in months per district	15.956	3.111
Share of long-term unemployed	0.403	0.066
Share of women unemployed	0.440	0.041
Share of foreigners unemployed	0.123	0.096
Share of under 25 unemployed	0.111	0.022
Unemployment rate per district 12/2004	15.595	5.670
Living in region qualified by sanction rate (reference: very high: Rheinland-Pfalz, Baden-Württemberg)	0.085	0.278
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.348	0.476
Intermediate: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.407	0.491
High: Thüringen, Bremen, Bayern	0.160	0.366
Living in West Germany	0.552	0.497

Source: Life Situation and Social Security 2005 and Statistics Department of the Federal Employment Office. **Notes:** The sample contains 9,313 observations. a. Low qualified means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified means *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, highly qualified means *(Fach-) Abitur* and apprenticeship or master craftsmen and very highly qualified means university degree. b. in months between 1 Jan 2000 and 31 Dec 2004. c. Average sanction rates were 2.9% for the low group, 3.9% for the intermediate group, 4.8% for the high group and 7.4% for the very high group.

Table 2: Labor Market Attachment at the Time of the Interview

	Mean	SD
Not employed, no welfare receipt	0.117	0.321
Welfare receipt (not employed, no work measure)	0.554	0.497
Work measure and welfare receipt	0.089	0.285
Employed and welfare receipt	0.126	0.332
Current unsubsidized (self)employment	0.114	0.318
Current unsubsidized (self)employment (< 16 hours/week)	0.055	0.277
Current unsubsidized (self)employment (>= 16 hours/week)	0.059	0.235

Source: Life Situation and Social Security 2005. **Notes:** The sample contains 9,313 observations.

Table 3: Sample Means of Search Effort, Reservation Wages and Employment by Treatment at the Time of the Interview

	Complete sample	Sanctioned	Non-sanctioned
Search effort	0.627	0.649	0.626
Reservation wage (Euros/hour)	6.148	6.176	6.147
Employed and welfare receipt	0.067	0.070	0.067
Current unsubsidized (self)employment	0.114	0.118	0.114

Source: Life Situation and Social Security 2005. **Notes:** The sample contains 9,313 observations including 541 treated observations.

Table 4: Estimation Results of the Probit Specification of the Probability to Receive a Sanction for all Four quarters of Permanent Welfare (UB II) Receipt

	Quarter 1: Month 01 to0 3 of permanent UB II receipt	Quarter 2: Month 04 to 06 of permanent UB II receipt	Quarter 3: Month 07 to 09 of permanent UB II receipt	Quarter 4: Month 10 to 12 of permanent UB II receipt
Gender=woman	-0.042	-0.173*	0.087	0.098
Married man	0.022	0.018	0.107	-0.293*
Married woman	0.133	0.292*	0.058	-0.232
Partner 2004 (reference: no partner)				
Employed partner	-0.023	0.142	0.072	-0.216
Non-/unemployed partner	-0.017	-0.030	-0.027	-0.129
Age 2004 (reference: 15 to 25 years old)				
25 to 40 years old	-0.356	-0.705***	-0.057	-0.077
40 to 50 years old	-0.428	-0.781***	-0.150	-0.312
50 to 58 years old	-0.635**	-1.120***	-0.339	-0.589**
More than 2 children in hh 2004	0.028	0.204	-0.093	-0.041
Child under 3 in household	-0.328*	-0.061	-0.054	-0.025
OECD equivalent net monthly household income 12/2004 (Euros)	0.000	0.000	0.000	0.000
Other earners in household	0.011	0.220*	0.105	0.237
Migrant status (reference: migrant: other nationality, parents and interview language)				
Migration background: German/other nationality, parents or interview language	-0.078	0.014	-0.055	0.237
German: German nationality, parents and interview language	-0.117	0.005	-0.070	0.363**

Table 4 ctd.

	Quarter 1: Month 01 to0 3 of permanent UB II receipt	Quarter 2: Month 04 to 06 of permanent UB II receipt	Quarter 3: Month 07 to 09 of permanent UB II receipt	Quarter 4: Month 10 to 12 of permanent UB II receipt
Qualification				
Qualification (reference: low)[a]				
Intermediate	-0.084	0.161*	-0.151*	-0.309***
High	0.207	0.061	-0.032	-0.276
Very high:	-0.330	0.050	-0.097	-0.379*
Drivers' license?	-0.008	0.084	-0.077	-0.063
(Very) good German language skills	0.342**	-0.113	0.234*	0.020
Employment history				
Employed at 31 December 2004	-0.013	-0.080	-0.053	-0.325
Cumulated duration of employment[b]	0.004	0.004	0.000	0.007***
Under 25*Cumulated duration of employment	-0.017	-0.004	-0.004	-0.012
Cumulated duration of unemployment	0.002	-0.002	-0.002	0.000
Under 25*Cumulated duration of unemployment	-0.001	0.002	0.002	0.008
Cumulated duration of qualification	0.006	-0.008	-0.002	0.006
Under 25*Cumulated duration of qualification	-0.003	0.000	0.009	0.010
Hourly net wage(missing are zero)	0.013	-0.051	-0.058	-0.054
Indicator for missing net wage	0.009	-0.549	-0.565	-0.945
Benefit receipt 12/2004 (reference: no benefits)				
Unemployment Benefits	-0.003	-0.085	0.150	-0.109
Welfare benefits	0.035	0.030	0.078	0.028
Unemployment and welfare benefits	0.306*	0.030	0.097	-0.045
Individual action plan				
Individual action plan	-0.114	-1.726	-0.405	-0.037
Individual action plan*month of individual action plan	0.000	0.003	0.001	0.000
Regional labor market				
Share of service jobs 12/2004 per district	0.052	-1.210**	-0.755	-1.475**
Ratio of offered jobs to registered unemployed in 2005 per district	0.068	-2.527*	-0.323	0.813
Ratio one-Euro jobs/stock of UB II-unemployed 2005	0.022	-0.589	-0.202	0.221
Unemployment duration 12/2004 in months per district	0.007	0.158***	-0.031	-0.008
Share of long-term unemployed	0.259	-8.124***	2.605	-1.646
Share of women unemployed	0.572	-0.716	1.599	-1.487
Share of foreigners unemployed	0.012	-1.038	-0.431	0.419
Share of under 25 unemployed	-0.584	-2.495	-0.280	-4.933*
Unemployment rate per district 12/2004	-0.003	-0.026	0.006	0.020
Living in region qualified by sanction rate (reference: very high: Rheinland-Pfalz, Baden-Württemberg)				
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	-0.155	-0.490***	-0.347**	-0.085
Intermediate: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	-0.094	-0.475***	-0.342**	-0.003
High: Thüringen, Bremen, Bayern	-0.025	-0.173	-0.183	0.066
Living in West Germany	-0.059	-0.357*	0.368**	-0.011
Constant	-2.449*	2.366*	-2.289*	1.765
N whole estimation sample	9,064	8,424	7,855	5,256
N potential treated	97	133	154	160
Log likelihood	-513.344	-624.074	-743.051	-707.051
Chi²	50.626	143.817	62.775	84.978
Pseudo *R²*	0.049	0.113	0.043	0.066

Source: Life Situation and Social Security 2005. **Notes:** Positive coefficients denote that the respective variable contributes positively to the probability to receive a first sanction within the quarter of permanent UB II receipt listed on top of each column. Therefore, the coefficients are estimated for four different samples. (*) significance on the 10%, (**) 5%, and (***) 1% level. a. Low qualified means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified means *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, highly qualified means *(Fach-) Abitur* and apprenticeship or master craftsmen and very highly qualified means university degree. b. in months between January1, 2000 and December 31, 2004.

Table 5: Common Support by Estimation

Number of potential treatment units	Number of potential control units	Number of treatment units on common support	Number of control units on common support	Number of treatment units that are matched	Number of control units that are matched	Caliper (radius)		Quarter of uninterrupted welfare spell
		93		93	4028	(1)	0.0001	
97	8967	97	8967	97	8846	(2)	0.001	1
		97		97	8963	(3)	0.01	
133	8291	131	8291	131	7813	(2)	0.001	2
		132		132	8284	(3)	0.01	
		146		146	3544	(1)	0.0001	
154	7701	154	7701	154	7447	(2)	0.001	3
		154		154	7695	(3)	0.01	
		147		147	1479	(1)	0.0001	
160	5096	157	5096	157	4730	(2)	0.001	4
		160		160	5089	(3)	0.01	

Source: Life Situation and Social Security 2005. **Notes:** Matching algorithm: radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls

Table 6: Mean Quality Indicators for the Matching by Sample and Caliper Before Matching (BM) and After Matching (AM)

(1) MSB		(2) Pseudo R^2		(3) P-value		Caliper (radius)		Quarter
(BM)	(AM)	(BM)	(AM)	(BM)	(AM)			
9.414	3.318	0.049	0.016	0.233	1.000	(1)	0.0001	
9.414	1.492	0.049	0.005	0.233	1.000	(2)	0.001	1
9.414	2.960	0.049	0.010	0.233	1.000	(3)	0.01	
15.238	3.185	0.113	0.019	0.000	1.000	(1)	0.0001	
15.238	1.481	0.113	0.004	0.000	1.000	(2)	0.001	2
15.238	1.863	0.113	0.006	0.000	1.000	(3)	0.01	
9.591	2.754	0.044	0.017	0.024	1.000	(1)	0.0001	
9.591	1.200	0.044	0.004	0.024	1.000	(2)	0.001	3
9.591	1.819	0.044	0.004	0.024	1.000	(3)	0.01	
11.262	3.715	0.065	0.019	0.000	1.000	(1)	0.0001	
11.262	1.482	0.065	0.004	0.000	1.000	(2)	0.001	4
11.262	1.522	0.065	0.002	0.000	1.000	(3)	0.01	

Source: Life Situation and Social Security 2005. **Notes:** Matching Algorithm: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls. The mean standardized bias (MSB) is the difference of the sample means of treated and controls as a percentage of the square root of the average of the sample variances in the treated and controls (Rosenbaum and Rubin 1985). The Pseudo R^2 from the probit model estimation of the propensity scores includes all variables, before and after the matching process (Sianesi 2004). P-values are from the likelihood-ratio test of the joint insignificance of all the regressors before and after matching (H0).

Table 7: Quality Indicators by Estimation Quarter for Caliper 0.001

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Demographic and household variables								
Gender=woman	0.952	74.6	0.855	51.0	0.838	82.2	0.817	82.7
Married man	0.916	47.6	0.86	70.1	0.991	98.8	0.726	85.7
Married woman	0.907	38.6	0.925	85.4	0.898	-109.7	0.806	57.3
Partner 2004 (reference: no partner)								
Employed partner	0.975	-252.1	0.97	84.2	0.925	88.0	0.925	86.2
Non-/unemployed partner	0.911	-16.1	0.945	90.7	0.813	-485.8	0.68	-8.9
Age 2004 (reference:15 to 25 years old)								
25 to 40 years old	0.896	77.5	0.781	31.2	0.879	81.6	0.922	93.7
40 to 50 years old	0.773	-35.1	0.927	89.2	0.921	85.2	0.932	93.7
50 to 58 years old	0.879	93.8	0.889	97.0	0.914	96.6	0.954	98.8
More than 2 children in hh 2004	0.92	-4.6	0.906	88.5	0.988	-6	0.871	21.6
Child under 3 in household	0.876	87.8	0.884	79.0	0.924	76.5	0.929	84.4
OECD equivalent net monthly household income 12/2004 (Euros)	0.776	17	0.897	79.6	0.886	74.7	0.912	69.8
Other earners in household	0.942	84.8	0.768	86.5	0.849	72.5	0.705	68.7
Migrant status (reference: migrant: other nationality, parents and interview language)								
Migration background: German/other nationality, parents or interview language	0.727	-160	0.987	95.8	0.95	50.1	0.689	42.3
German: German nationality, parents and interview language	0.784	-2626.3	0.924	80.4	0.943	-22.6	0.745	82.3
Qualification								
Qualification (reference: low)[a]								
Intermediate	0.793	39.6	0.911	87.2	0.928	93.8	0.934	94.9
High	0.89	87.1	0.981	95.7	0.927	29.7	0.757	24
Very high:	0.842	86.3	0.849	85.4	0.946	91.4	0.876	91.2
Drivers' license?	0.887	71.7	0.697	58.5	0.975	93.3	0.972	93.8
(Very) good German language skills	0.557	63.9	0.869	62.3	0.936	94.6	0.885	92.1
Employment history								
Employed at 31 December 2004	0.796	82.4	0.798	75.2	0.87	75.5	0.777	80.2
Cumulated duration of employment[b]	0.501	37.9	0.926	91.4	0.999	99.5	0.94	93.6
Under 25*Cumulated duration of employment	0.244	-2.2	0.439	66.2	0.857	77.8	0.883	87.2
Cumulated duration of unemployment	0.549	74.6	0.997	99.9	0.991	99.4	0.891	94.8
Under 25*Cumulated duration of unemployment	0.959	97.7	0.655	82.8	0.953	96.5	0.982	99.0
Cumulated duration of qualification	0.925	95.3	0.872	79.9	0.825	81.6	0.79	87.0
Under 25*Cumulated duration of qualification	0.585	83.1	0.874	92.8	0.92	94.6	0.512	75.8
Hourly net wage(missing are zero)	0.974	97.3	0.802	70.5	0.913	76.5	0.898	89.0
Indicator for missing net wage	0.836	87.6	0.765	73.7	0.855	76.7	0.771	81.8
Benefit receipt 12/2004 (reference: no benefits)								
Unemployment Benefits	0.455	-1421.9	0.861	-18.8	0.992	99.1	0.8	-379.2
Welfare benefits	0.502	30.1	0.889	91.2	0.989	99.0	0.762	71.6
Unemployment and welfare benefits	0.887	71.7	0.697	58.5	0.975	93.3	0.972	93.8
Individual action plan								
Individual action plan	0.877	75.5	0.864	-134.9	0.987	99.0	0.917	85.1
Individual action plan*month of individual action plan	0.868	77.6	0.867	65.0	0.975	98.4	0.949	92.3
Regional labor market								
Share of service jobs 12/2004 per district	0.934	93.7	0.794	92.8	0.991	99.6	0.924	93.3
Ratio of offered jobs to registered unemployed in 2005 per district	0.887	81.6	0.953	71.5	0.971	94.9	0.604	24.9
Ratio one-Euro jobs/stock of UB II-unemployed 2005	0.903	-15.3	0.853	79.7	0.998	99.7	0.925	17.2
Unemployment duration 12/2004 in months per district	0.794	68.3	0.889	93.7	0.948	84.5	0.850	79.1
Share of long-term unemployed	0.845	78.1	0.851	93.1	0.988	96.9	0.801	76.1
Share of women unemployed	0.942	61.3	0.864	85.8	0.993	99.1	0.846	4.4
Share of foreigners unemployed	0.331	-1475.4	0.852	86.2	0.926	-122.6	0.777	-327.3
Share of under 25 unemployed	0.788	-5997.1	0.81	91.8	0.849	75.5	0.718	-7032.2
Unemployment rate per district 12/2004	0.563	6.4	0.845	90.9	0.995	99.4	0.958	92.2
Living in region qualified by sanction rate (reference: very high: Rheinland-Pfalz, Baden-Württemberg)								
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.940	91.4	0.999	100	0.976	98.4	0.862	82.1
Intermediate: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.923	33.2	0.907	90.2	0.875	56.9	0.897	18.6
High: Thüringen, Bremen, Bayern	0.822	76.5	0.807	88.0	0.980	97.7	0.992	99.1
Living in West Germany	0.773	47.4	0.834	77.7	0.957	96.4	0.929	85.1

Source: Life Situation and Social Security 2005. **Notes:** Radius Matching with Caliper 0.001. (1) p(t-Test with H0: no difference in means). (2) Reduction of absolute mean bias (in %). The standardized bias (MSB) is the difference of the sample means of treated and controls as a percentage of the square root of the average of the sample variances in the treated and controls (Rosenbaum and Rubin 1985).

Table 8: Average Treatment Effect of Benefit Sanctions on the Treated: by Quarter of Welfare Receipt and Job Search Outcome

| ATT on reservation wages (in %/100)	ATT on search effort (in %points /100)	Quarter of welfare receipt
-0.014	0.029	1
(0.041)	(0.050)	
0.026	-0.005	2
(0.032)	(0.045)	
0.045	0.042	3
(0.029)	(0.039)	
0.015	0.006	4
(0.029)	(0.040)	

Source: Life Situation and Social Security 2005. **Notes:** The variables included in the estimation of each of the propensity scores upon which matching is performed are listed in Table 1. The results of the propensity score estimations and the balancing property are reported in Table 4 to 7. Standard errors (see Becker and Ichino 2002) are presented in parentheses. Matching algorithm for four subsequent quarters of uninterrupted welfare receipt: Radius matching with caliper 0.001 defining the maximum absolute propensity score distance between treated and controls. Dependent variables: Log reservation wages (Euros/hour) and a dummy for active search effort in the last month. The treatment is having received a benefit sanction in the respective quarter of welfare receipt vs. having continuously received welfare in the respective quarter. Radius Matching is performed with Stata package psmatch2 (Leuven and Sianesi 2003).

Table 9: Average Treatment Effect of Benefit Sanctions on the Treated: by Quarter of Welfare Receipt and Employment Outcome

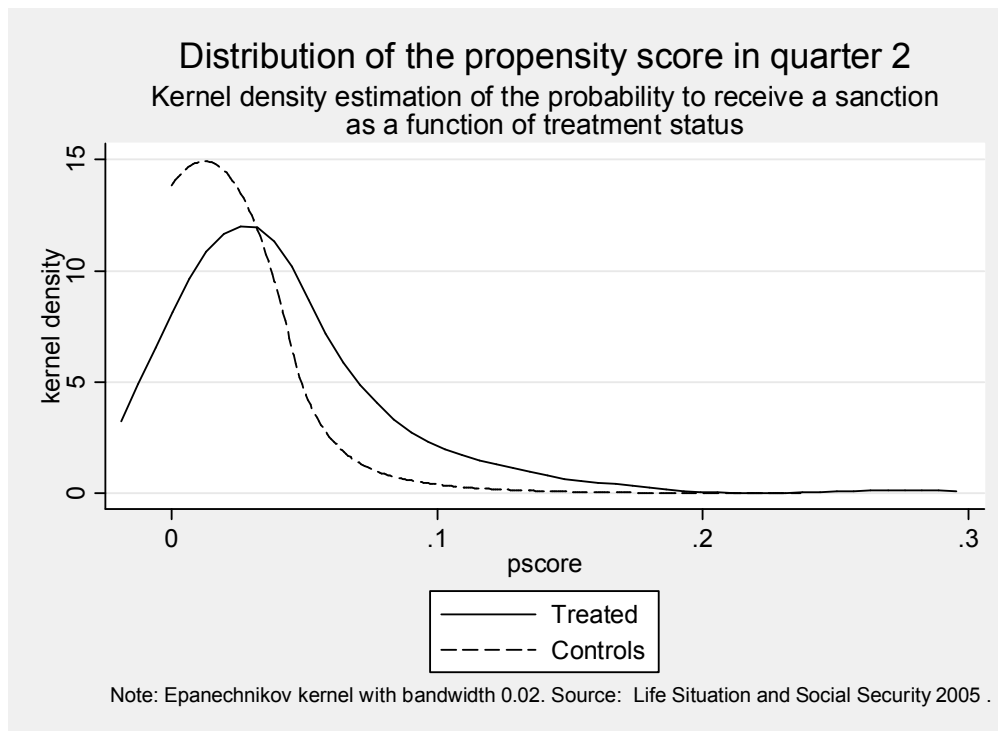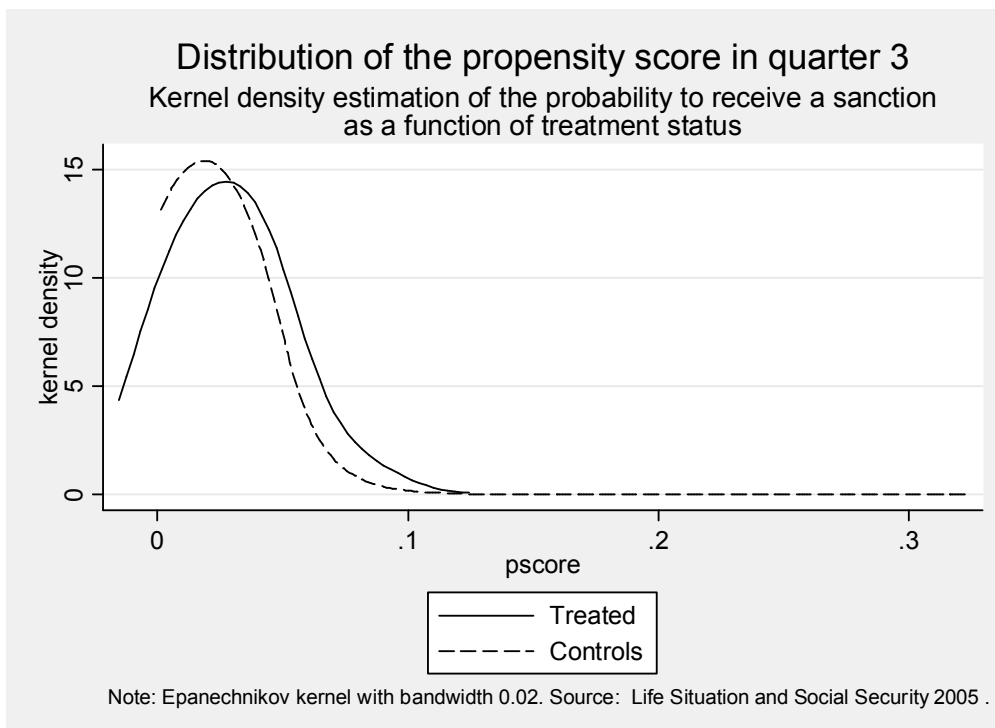| ATT on current unsubsidized employment (in %points /100)	ATT on current employment with additional welfare receipt (in %points /100)	Quarter of welfare receipt
0.086	-0.022	1
(0.042)	(0.023)	
0.060	-0.001	2
(0.032)	(0.024)	
0.019	-0.009	3
(0.021)	(0.020)	
0.045	-0.003	4
(0.022)	(0.022)	

Source: Life Situation and Social Security 2005. **Notes:** The variables included in the estimation of each of the propensity scores upon which matching is performed are listed in Table 1. The results of the propensity score estimations and the balancing property are reported in Table 4 to 7. Standard errors (see Becker and Ichino 2002) are presented in parentheses. Matching algorithm for four subsequent quarters of uninterrupted welfare receipt: Radius matching with caliper 0.001 defining the maximum absolute propensity score distance between treated and controls. Dependent variables: Dummies for current unsubsidized (self-) employment and current (self-) employment with additional welfare receipt. The treatment is having received a benefit sanction in the respective quarter of welfare receipt vs. having continuously received welfare in the respective quarter. If control observations are allowed to leave welfare in the first month of the fourth quarter already, coefficients change to 0.036 (0.022) for unsubsidized employment and to 0.005 (0.023) for current employment with additional welfare receipt. Radius Matching is performed with Stata package psmatch2 (Leuven and Sianesi 2003).
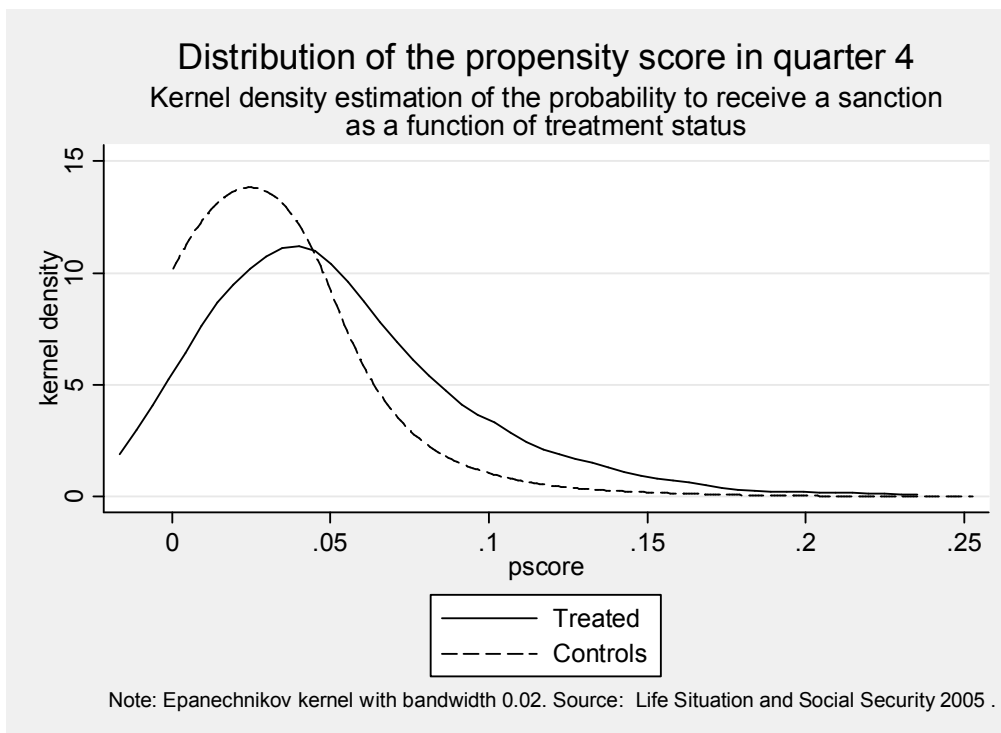
Figure 1: Density Histograms of Net Hourly Reservation Wages by Treatment Status in Euros
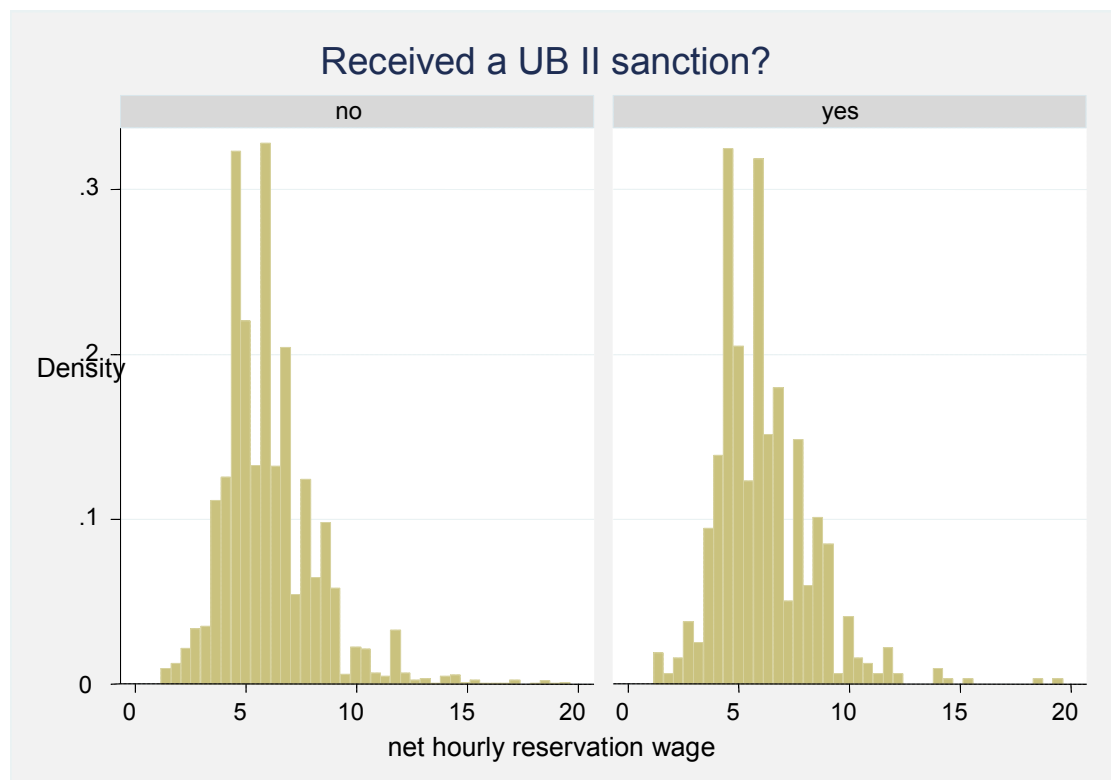
Figure 2: Distribution of the Propensity Score in Quarter 1
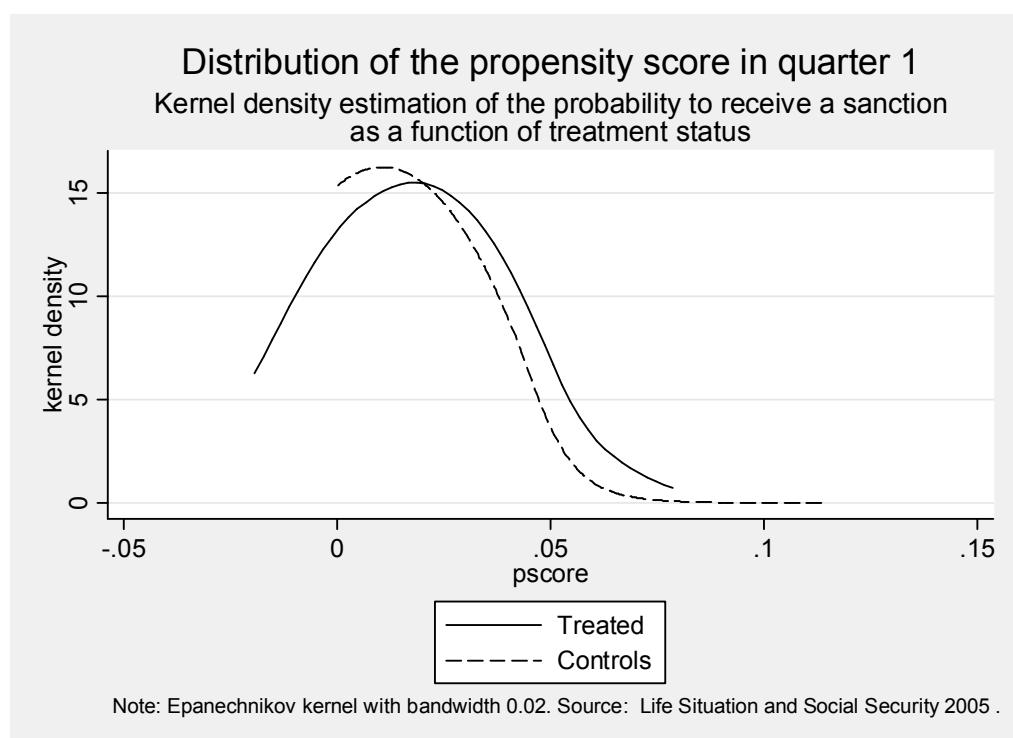
Figure 3: Distribution of the Propensity Score in Quarter 2

Distribution of the propensity score in quarter 2

Kernel density estimation of the probability to receive a sanction as a function of treatment status

Note: Epanechnikov kernel with bandwidth 0.02. Source: Life Situation and Social Security 2005 .

Figure 4: Distribution of the Propensity Score in Quarter 3

Distribution of the propensity score in quarter 3

Kernel density estimation of the probability to receive a sanction as a function of treatment status

Note: Epanechnikov kernel with bandwidth 0.02. Source: Life Situation and Social Security 2005 .

Figure 5: Distribution of the Propensity Score in Quarter 4

Distribution of the propensity score in quarter 4
Kernel density estimation of the probability to receive a sanction
as a function of treatment status

Note: Epanechnikov kernel with bandwidth 0.02. Source: Life Situation and Social Security 2005 .

CHAPTER 4

IMPACTS OF INDIVIDUAL ACTION PLANS ON RESERVATION WAGES, SEARCH EFFORT AND RE-EMPLOYMENT

1   Introduction

Setting up individual action plans with job-seeking benefit recipients has become an increasingly important element of activation strategies in OECD countries. Their names may vary (integration agreement, guidance plan, job-seeking agreement, activity agreement, etc.), but in each case an individual action plan (IAP) is a written document to be signed by both parties, the benefit providing employment service and the job-seeking benefit recipient, describing the job seeker's situation, laying down certain goals the job-seeker has to achieve and listing commitments by the employment service (OECD 2007).

The aim of these plans is to increase the 'success of job search' by raising the probability of the job-seekers to achieve and accept a job offer but research on their effectiveness is just starting. This study is the first to analyze the impacts of individual action plans on the reservation wages of job-seekers, their search effort and their re-employment probabilities.

Qualitative research on the design of IAP in various Western countries indicates that IAP are rarely based on preferences of the benefit recipient and seldom aim to improve substantive skills or address major employment barriers (Mosley and Sol 2005). Instead, IAP concentrate more on the duties of benefit recipients to raise their search efforts (Sol and Westerveld 2006). Since there is no research on the impacts of IAP on reservation wages, search effort, or re-employment, one might take results of previous quantitative research on effects of "services and sanctions" (a category comprising

all measures aimed at increasing job search efficiency, such as counseling and monitoring, job search assistance, and corresponding sanctions in case of noncompliance, see Kluve et al. 2007) since signing an IAP is assumed to increase the probability for benefit recipients to receive "services and sanctions".

Results on welfare sanctions based on the same dataset used in this study suggest no significant effect on reservation wages or search effort but on regular employment (see Chapter 3 and Schneider 2008). The large body of literature on the set of "services and sanctions" supports the view that increased counseling, monitoring, and a higher probability to receive a benefit sanction reduce the average duration of unemployment (e.g. Heckman et al. 1999 for overviews for US and Europe; Kluve et al. 2007 for a meta-analysis for Europe). From these previous results we might expect IAP to increase search effort and re-employment probabilities while it is not certain if we find a reaction of reservation wages in the data.

This study derives the theoretical predictions on the effects of IAP from a partial job search model. Accordingly, the search requirements of individual action plans raise search intensity and reduce reservation wages (the so-called compulsion effect). The qualification requirements actually raise search efficiency hence the probability to receive a job offer per given search intensity rises or the cost of job search goes down and search intensity as well as reservation wages go up (the so-called support effect).

Therefore, the net effect of IAP on search intensity is determined and positive while the net effect on reservation wages is ambiguous. The ambiguous net effect on reservation wages is responsible that also the net effect on exit rates to employment depends on the supportive and compulsory character of the IAP.

In the following empirical analysis, the general effects of individual action plans on reservation wages, search effort and the probability to find employment are estimated. The

estimation utilizes the cross-sectional survey of German unemployed welfare recipients of 2005 described in Chapter 2 and statistical matching techniques.

Based on the assumption, that signing an IAP with search requirements has a stronger compulsory effect, and signing an IAP with qualification elements has a stronger support effect, the (ambiguous) theoretical predictions of individual action plans on reservation wages, search effort and employment are further substantiated in a multiple treatment framework (e.g. Imbens 2000). Identification of the average treatment effect on the treated is achieved by means of the rich dataset and additional variation in the data generated by the implementation of the new benefit system *Hartz IV*.

The remainder of this chapter is organized as follows. Section two describes individual action plans for welfare recipients in Germany. Section three considers the theoretical relationship between individual action plans, reservation wages, search effort, and exits to employment in a partial job search model with endogenous job search. Section four describes the sample of analysis, as well as treatment and outcome variables. Section five explains the empirical approach and presents the estimation results. Finally, Section six concludes.

2   Individual Action Plans for Welfare Recipients in Germany

The German version of an individual action plan between a benefit provider and a job-seeking benefit recipient is called integration agreement (*Eingliederungsvereinbarung*). In principle, it is comparable to individual action plans in employer-employee-talks. IAP were invented as a management-technique to increase the productivity of employees by controlling their achievement of jointly

defined goals and not processes and inputs.[1] The agreement on objectives in IAP breaks down the goals of the company to the single employee, specifying until when she or he has to achieve the goals, the indicators if goals were achieved, and what happens if goals were achieved or not achieved.

In analogy, an individual action plan for German welfare recipients is a binding contract each welfare recipient and the welfare providing employment center (called *Job Center*, see Chapter 1) have to conclude (Art. 15 Social Code II).[2] The difference between IAP in business and IAP in benefit systems is of course that benefit recipients cannot change their benefit provider as employees can change their employer in case they dislike the offered IAP.

The idea of individual action plans is to increase the 'success of job search', the exits to employment. The German version of individual action plans called 'integration agreement' contains the goal of the agreement – integration into the labor market – already in its name. Individual action plans break down the abstract goal "fast integration into the labor market" into specific goals based on a customized integration strategy, specifying which goals have to be achieved, until when the job-seeker has to achieve the goals, the indicators if goals were achieved, and what happens if goals were not achieved.

The key feature of individual action plans is that they are binding for the contracting parties, in our case the welfare recipient and the *Job Center*. If the welfare recipients do not fulfill the goals, they can receive a benefit sanction[3]. Additionally, they have to pay compensation if they quit a training measure that is specified as a goal. Likewise, if the *Job Center* does not enable the welfare recipients to achieve a goal (by not offering the training measure specified in the IAP for instance) it

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[1] In their goal setting theory of motivation, Locke and Latham (1990) resumed that a specific challenging goal can lead to higher task performance than a vague goal. Sol and Westerveld 2006 resume a motivating effect of taking benefit recipients as contract partners.

[2] See BT-Drucksache, 15/1516: 54. For further details on the institutional setting, consult Chapter 1.

[3] These benefit sanctions cut the monthly base benefit (without allowances for accommodation and heating) by 30% for three months (see Chapter 3).

can be sued for specific performance. In order to work out the best integration strategy, the plan should be concluded jointly by the welfare recipient and the case worker after an assessment of the individual chances and problems of the job-seeker (called "profiling").

The law allows case workers to avoid concluding individual action plans with welfare recipients who either very probably can be integrated into the labor market soon (within the next eight weeks), or who do not want to sign an IAP in a time when employment or an ALMP measure is not suitable (including single parents, under 25-year-olds in full-time school or vocational training, welfare recipients with care responsibilities, not able-to-work, or not capable in terms of their life situation or personality to understand the consequences of signing an individual action plan). Older welfare recipients who turned 58 years before 1st January 2008 do not have to sign an IAP, if they enter RULE58 (see Chapter 5).

Individual action plans are not beyond dispute: jurisprudence discusses individual action plans as endangering the constitutional right of freedom of contract (Schleger et al. 2005) since the welfare recipient can receive a benefit sanction if she or he refuses to sign an IAP proposed by her or his case worker and the proposed integration strategy is nevertheless implemented by the *Job Center*. This inferior legal protection of citizens is supposed to be compensated through the increased binding force of individual action plans compared to administrative acts.

3   Individual Action Plans in a Job Search Model

As described above, individual action plans aim to increase the success of job search of job-seeking benefit recipients. To illustrate how IAP could possibly affect reservation wages and search effort and ultimately exit rates to employment a simplified partial job search model with endogenous search is used. We saw that the key feature of an IAP is its binding force compared to administrative acts. IAP

enable both the *Job Center* and the unemployed welfare recipient to sue the other contract party if it does not fulfill its contract obligations arising from the customized integration strategy.

Thus, on the one hand, the welfare recipients might experience more support in the job search process since they know how to follow their optimal integration strategy by achieving specific goals and since they can sue the *Job Center* if it does not offer the integration measures guaranteed in the IAP. This support effect of IAP will reduce the costs of job search and increase the arrival rate of job offers for a given job search intensity.

On the other hand, the welfare recipient might experience more compulsion compared to a situation without an IAP since the probability rises that an underperformance in the job search process is detected and punished by a benefit sanction. This compulsory effect of IAP will reduce their utility derived from the flow of unemployment benefits. Consequently, we would expect individual action plans to have both a support effect and a compulsory effect on the reservation wages and the search effort of welfare recipients.

Therefore the model is based on Van Ours (2007) whose basis assumption is that measures of active labor market policy in general may cause support and compulsory effects.[4] In the model, individuals are risk-neutral and maximize their utility (i.e. their expected discounted income) over an infinite time horizon. Unemployed workers receive a constant amount of unemployment benefit b and are looking for job offers (characterized by wage w) with search intensity $s{\geq}0$. The model does not allow for on-the-job search (although the opposite assumption would change the outcome very little, see Mortensen 1986).

The search for a job entails costs at every turn, summed up to a single scalar $\gamma(s)=1/2\gamma s^2$. Hence, marginal search costs increase with search intensity s. The unemployed job-seeker knows the

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[4] In contrast to Van Ours (2007) who calls the support effect "treatment effect", since every effect of any treatment can be considered as treatment effect.

time-constant cumulative distribution of the possible wages H, from that the wages of successively arriving job offers are independently drawn. Job offers follow a Poisson process with arrival rate μs (μ reflects the tightness of the labour market for the job-seeker).

At any moment the labor market status of the job-seekers may change with rate μs. If they do receive a job offer with wage w, they will accept the job if their discounted expected utility from unemployment (their reservation wage) is lower than w. Now we introduce individual action plans. When the job-seekers enter unemployment benefit, it is assumed that they sign an individual action plan that lasts from the beginning until the end of the unemployment spell. Individual action plans are modeled (1) as a penalty on unemployment benefits $\varphi \in [0,1]$ (the compulsory effect) and (2) as a subsidy on search costs $\sigma \in [0,1]$ (the support effect). Combining both effects yields the unemployed worker's discounted expected utility when having signed an IAP:

$$r = \rho V_u = [(1-\varphi)b - (1-\sigma)\gamma(s)] + \mu s \int_{r}^{+\infty} (V_e(w) - V_u)dH(w) \qquad (1)$$

where V_u and V_e are the values of being unemployed and employed respectively, ρ is the discount rate, and r the reservation wage.

Equation (1) defines implicitly the reservation wage of the unemployed worker with an IAP for a given search intensity s. The flow value of unemployment consists of (1) the flow of utility during unemployment (benefits less costs) and (2) the expected additional utility after a job is found.

Now the optimal value of search intensity s^* maximizes, by definition, the discounted expected utility of the unemployed job-seeker (equation (1)). Differentiating equation (1) gives

$(1-\sigma)\gamma'(s^*) = \mu \int_{r}^{+\infty} (V_e(w) - V_u)dH(w)$ from which it is easy to derive that

$$s^* = \frac{\mu}{(1-\sigma)\gamma} \int_{r}^{+\infty} (V_e(w) - V_u)dH(w) \qquad (2)$$

Equation (2) tells us that the optimal search intensity increases with the difference between the values of employment and unemployment – and thus with the size of the penalty on unemployment benefits φ, i.e. the compulsory effect – and with the size of the subsidy on search costs σ (i.e. the support effect). In addition, optimal search intensity increase with the tightness of the labor market and when search costs are lower.

The reader can verify that the hypotheses concerning the arrival rate of job offers and search costs guarantee that the amount of search intensity defined by this relation is indeed a maximum. Combining equations (1) and (2), we obtain:

$$r = (1 - \varphi)b - (1 - \sigma)[\gamma(s^*) - s^* \gamma'(s^*)] \tag{3}$$

Equation (3) shows that the effect of individual action plans on reservation wages is ambiguous. The reservation wage of the unemployed worker decreases with the size of the penalty on unemployment benefits φ, i.e. the compulsory effect, but it increases with the size of the subsidy on search costs σ, i.e. the support effect.

Since job-seekers become employed when a) they receive a wage offer (which occurs at rate μs) and b) the offer is at least equal to their reservation wage (which occurs with probability $1-H(r)$) the success of job search measured as the exit rate to employment takes the value $\mu s(1-H(r))$ at any moment.[5] When the number of job-seekers is large, this rate approaches the hazard rate.

Thus, the exit rate to employment is a decreasing function of the reservation wage and an increasing function of the search intensity and the arrival rate of wage offers. Since we do not know in which direction individual action plans change reservation wages we do not know if they increase the success of job search measured as the exit rate to employment though it is likely because of the

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[5] The underlying random variables, μs and $H(r)$, are not independent.

increased search intensity and the increased arrival rate of job offers.[6] In sum, the model implies that individual action plans increase search intensity but the net effects on reservation wages and exit rates to employment are not clear.

These theoretical predictions can be tested in the empirical analysis. If we further assume that signing an IAP with search requirements has a stronger compulsory effect, and signing an IAP with qualification elements has a stronger support effect, we can test with appropriate data the ambiguous theoretical predictions of the effect of support and compulsion in individual action plans: we can estimate the effect of signing different types of IAP in terms of qualification elements (support) and search requirements (compulsion) on reservation wages, search effort and re-employment.

4 Data

The empirical analysis is based on the data from the cross-sectional survey called "Life Situation and Social Security 2005" which is described in Chapter 2. Key for this study is that the dataset is very rich and includes detailed information on reservation wages, search effort, and individual action plans and that it covers the implementation period of the new benefit regime.

Regarding our treatment, interviewers asked all sample members if an individual action plan with specified goals and explicit duties and benefits for both sides has been signed. Individuals with IAP were asked further details on their plan.[7] As in Chapter 3, our outcome variables in this analysis are the log hourly reservation wage, search effort, and re-employment probabilities. As an extension compared to Chapter 3, re-employment is now divided in three, not two types of employment at the

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[6] This is different in the work of Van Ours (2007). Since by assumption workers accept every wage offer in his model, unemployment duration is reduced through both the compulsory and the support effect.

[7] If respondents denied, they were asked if they refused to sign an IAP, if it was in preparation or if they were not offered an IAP. If respondents agreed they were asked if the IAP was concluded after a personal counseling or after a group activity, when they signed it the first time and how long it was valid. Then, interviewers asked if the IAP contained the specific elements described in detail below.

time of the interview: unsubsidized employment without receiving welfare benefits, employment with additional welfare receipt, and employment in a work measure (so-called "one-Euro jobs", see Chapter 1).

4.1 Sample Selection

In the final sample remain all people aged 15 to 57 who entered the new welfare benefit (UB II) right from the start (January to March 2005), who are not retired when interviewed, and who report complete information on their first individual action plan under the new benefit, hourly reservation wages, and current search effort. Excluded are observations with reported reservation wages below one Euro and above 20 Euros per hour (excluding 33 observations below one Euro and 67 observations above 20 Euros) and observations with missings in control variables.[8] Excluded are also non-participants with shorter welfare spells than a simulated start date of an IAP from the sample (for the sensitivity of results to this assumption see Section 5). The final sample has 10,192 observations.

4.2 Descriptive Statistics

Sample members characteristics are presented in Table 1. According to the data but in contrast to the legal requirements, in the year 2005 the majority of the sample members did not receive a profiling and an individual action plan when they entered welfare. Instead, a constant rate of two to three percent of the welfare receiving sample members signed an IAP in each calendar month of 2005.

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[8] Information on last net wages (i.e. wages resulting from an employment that started before our observation period 2005), and thus, on the reservation wage ratio (rwr), are only available for jobs lasting at least until January 2004; but three quarters of the sample members were already unemployed at this time. Thus, missings in last net wages are not a reason for exclusion. Due to our target group, reported net wages under one Euro per factual working hour (in 91 cases) and over 50 Euros (in two cases) are assumed to be implausible and put to missings.

Only 57% of the sample members with IAP had received a profiling before signing the IAP (compared to 32% of non-participants).

Of the whole sample, 29% had signed an individual action plan between entering welfare benefit and the interview. Of the non-participants, only half a percent had refused to sign an IAP; for 2% it was in preparation. On average, participants signed the IAP six months after they entered welfare. The low rate of people with IAP in the sample corresponds to results of two previous studies on the implementation of individual action plans for recipients of UI and welfare (WZB and infas 2006, Bundesrechnungshof 2006).

Table 2 shows the contents of the signed individual action plans as reported in the interviews. As mentioned above, if there is enough variation in the content, we can define different types of IAP in terms of qualification elements (support) and search requirements (compulsion) and then estimate the effect of IAP as single treatment and as multiple treatments on reservation wages and search effort.

The vast majority of individual action plans (95%) contain search requirements, for example to show a certain number of applications or to increase the search activity by posting own job advertisements, asking employers for jobs, or searching internships. The share of individual action plans with qualification elements is lower (66%). Qualification elements involve the requirement to participate in any integration measure and to improve one's application documents. Note that by using this definition, integration measures (including work and qualification measures) are not assumed to be indirect compulsory instruments to increase the search intensity of benefit recipients.

Given the sample size, we can define three mutually exclusive types of individual action plans in terms of qualification elements (support) and search requirements (compulsion), leading to four possible treatments (see Table 3): (1) having signed an individual action plan with qualification elements and no search requirements ("IAB_qual"); (2) having signed an individual action plan with

no qualification elements and search requirements ("IAB_search"); (3) having signed an individual action plan with both qualification elements and search requirements ("IAB_both"); (4) not having signed an individual action plan ("no IAP").

Net reservation wages are on average 6.11 Euros/hour, a wage level in the low-wage sector (see Table 4). Having signed an IAP is associated with significantly lower reservation wages: Participants would accept a wage of 5.77 Euros/hour on average compared to 6.24 Euros/hour of non-participants. While reservation wages of participants with IAB_search and IAB_both are similar, reservation wages of participants with IAB_qual are significantly higher than those of the other participants.

Table 5 presents the sample means on active search effort. At the time of their interview, 63% reported to have searched for a job in the last four weeks. Again, search effort differs significantly with treatment. Search effort of participants is significantly higher than of non-participants, but within participation, participants with IAB_qual search significantly less than the other two groups.

Regarding the average labor market attachment at the time of the interview approximately one year after entering welfare, 8.6% are in unsubsidized employment (with average net wages of 5.96 Euros/hour).[9] Most sample members continue to receive welfare benefits (80%). 10% are in a work measure and 13% are employed while receiving benefits.[10]

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[9] A person is defined as currently in unsubsidized employment if she or he reported to be regular employed, self-employed, employed in a Personal Service Office at the time of the interview, and not receiving welfare and/or being in work measures at the same time.

[10] Working individuals may still receive additional benefits if workers are below the socio-economic poverty level.

5   Empirical Approach and Results

5.1   Identification and Estimation

The aim of this study is to estimate the average effect of signing an individual action plan on those welfare recipients who actually signed one. To deal with the selection problem described in Chapter 3, statistical matching is applied due to the rich dataset and the implementation period of the new benefit system. An additional advantage of statistical matching for our research question is that it allows for treatment heterogeneity which is crucial if we want to differentiate between the effects of different types of IAP and compare them to effects of IAP in general.

5.2   Conditional Independence

The CIA can be assumed to hold in this analysis for the following reasons: First, the data contain a rich set of variables that can proxy the information the case workers had about their new and unknown clients, the welfare recipients' motivation and ability to find a new job, the behavior of the case worker, and the situation on the local labor market.

In order to capture a person's labor market attachment and unobservable characteristics of job-seekers like motivation and ability, it is generally considered to be especially important to include indicators of an individual's previous (un)employment history in the set of matching variables (see e. g. Lechner 2002; Caliendo and Kopeinig 2008). Consequently, the histories of employment, unemployment and training in the last five years, labor market status in December 12/2004, last hourly net wages, the type of last employment, incidence and type of former benefit receipt are taken into account.

In addition, detailed, pre-observation-period information on sociodemographics and the household context (including if there are other earners in the household, the type of partnership, of

parenthood, and of available childcare), and qualification and skills (including language skills, driving license, disability, own e-mail account, and house ownership) proxy the information the case workers had about their unknown clients and the legal exceptions when *Job Centers* can avoid forming individual action plans.

Furthermore, potentially influential unobservable factors of welfare recipient and case worker are proxied by the type of motivation for entering welfare (getting a job, a work measure, further training, counseling, wait until pension or the next job starts, or receiving benefits), if the *Job Center* accepted the housing costs of the welfare recipient, if a profiling was carried out, the incidence and month of the first contact between *Job Center* and welfare recipient, the average rate of benefit sanctions by German state as indicator for the regional intensity of monitoring and the average percentage of sample members with IAP by region as indicator for regional differences in *Job Centers'* "philosophies" of offering IAP.[11]

Since it might be important to account for the duration of welfare receipt until the individual action plan was signed, for each non-participant, a hypothetical start date is drawn from the known sample distribution of start dates. Non-participants with shorter welfare spells than their simulated start date are deleted from the sample.[12] Finally, to account for the situation on the local labor market,

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[11] Counties were grouped according to the average percentage of sample members with IAP: (1) 15.1%, (2) 19.2%, (3) 21.7%, (4) 28.4%.

[12] The timing of IAP would be included for two reasons. First, all potential controls to a person with IAP should have at least received welfare as long as the person with the IAP when she or he signed it. Second, elapsed welfare receipt captures to some extent unobserved characteristics influencing selection into treatment and outcomes: unobserved characteristics of welfare recipients like ability and motivation to find a job as well as unobserved characteristics of case workers like their monitoring intensity, competence and decreases in their workload. Lechner (2002) contains a sensitivity analysis of this procedure by using start dates predicted by relevant characteristics instead of random start dates. Results appear robust. See also Section 6 for a sensitivity analysis for the estimation here. Moreover, the effect of simulating start dates for non-participants is minimized by using only pre-observation-period variables (December 2004) rather than at the hypothetical start date. Arguably, the timing of IAP is less important than the timing of activation measures which can be applied with considerable discretion of the case workers (like qualification measures or benefit sanctions); even more so, because IAP are supposed to be concluded repeatedly until the end of welfare receipt.

the average duration and rate of unemployment by district from the month before people entered the new welfare benefit (December 2004) are included in the estimation.

Second, our participants arguably are a less selective sample than one would assume when reading the law since our observation period was the implementation period of the new benefit system leading to considerably less monitoring than in normal times (see Chapter 2). Indeed our data reflects this low degree of monitoring: only a small share of those individuals that by law were to be treated actually received treatment, and if they signed an IAP it happened only after a couple of months on average and not everybody received a profiling beforehand (see Section 4).[13]

In fact, as will be shown below, the matching procedure is very successful in balancing the differences in observable characteristics between treated and control observations. Given the additional variation in the data stemming from this lack of monitoring together with the rich dataset, the CIA is assumed to hold in our case.

An additional question that has to be addressed is whether the effect of individual action plans on the reservation wages and search effort can be identified with the data at hand. Suppose the compulsory effect made people search more intensively for a job and they were more successful than the comparison groups and much more often found jobs. In that case their search intensity might have decreased due to having found a job already and might even be lower than for comparable job-seekers without an integration contract at the time of the interview. Hence, in the case of substantial increases in re-employment rates due to IAP, the estimators of the effect of IAP on reservation wages and search are biased downwards and could in extreme cases even reverse signs.

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[13] But even in 2008, German case workers are very heterogeneous in offering IAP to their clients. Some use IAP as *ultima ratio*; some use them frequently as activation instrument, and some use them until a certain percentage of their clients has signed one in order to satisfy required "numbers" (Baethge-Kinsky et al. 2007).

5.3 Estimation

Compared to Chapter 3 where we analyze the effect of benefit sanctions as one single treatment, the analysis of the effects of individual action plans is extended. First, the effect of IAP as single treatment is analyzed; second, the effects of different types of IAP as multiple treatments are analyzed.

Consider the case of one or more *(M+1)* mutually exclusive treatments where each individual can receive only one of the treatments. Hence, if we look at the effect of an IAP in general, *m=1* (IAP, no IAP), if we look at the effect of three types of IAP, m=3 (IAB_qual, IAB_search, IAB_both, no IAP). But in each case, for any individual, exactly one outcome of *{y0, y1, …, yM}* is observed. The remaining *M* outcomes are counterfactuals. Participation in a particular treatment *m* is indicated by the variable $D \in \{0,1,…,M\}$.

Lechner (2001) defines pairwise average treatment effects of treatments *m* and *l* for the participants in treatment *m*:

$$\theta_0^{m,l} = E(y^m - y^l \mid D = m) = E(y^m \mid D = m) - E(y^l \mid D = m) \tag{5}$$

$\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of participants in treatment m. If participants in treatments m and l differ in a way that is related to the distribution of confounding variables X, and if the treatment effects vary with X, then , $\theta_0^{m,l} \neq -\theta_0^{l,m}$, i.e. the treatment effects on the treated are not symmetric.

In the model with multiple treatments (Lechner 2001), the CIA can be formalized by

$$y^0, y^1,…y^M \perp D \mid X = x \ \ \forall x \in X \tag{6}$$

Lechner (2001) shows that the CIA identifies the effects defined in equation (5). For any pairwise comparisons the common support condition in the multiple treatment framework requires that for all values of *X* for which those treated have positive marginal probability there should be comparison observation as well.

Denote the marginal propensity score of treatment j conditional on X as $P(D=j|X)=Pj(X)$.

Lechner (2001) shows that the following result holds for the effect of treatment m compared with

treatment l on the participants in treatment m:

$$\theta_0^{m,l} = E(y^m \mid D=m) - \underset{P^{l|ml}(X)}{E}[E\{y^l \mid P^{l|ml}(X), D=l\} \mid D=m]$$

(7)

$$P^{l|ml}(X) = P^{l|ml}(D=l \mid D \in \{l,m\}, X) = \frac{P^l(X)}{P^l(X)+P^m(X)}$$

(8)

If $P^{l|ml}(X)$ is modeled directly, no information from subsamples other than those containing

participants in m and l is needed for the identification and estimation of $\theta_0^{m,l}$ and $\theta_0^{l,m}$, similar to the

binary treatment framework.[14] Given the estimated propensity scores for all treatments, the terms

appearing in equation (3) will be estimated by matching. The idea of matching on the propensity

score is to estimate $E(y^l|D=m)$ by forming a comparison group of selected participants in l that has

the same distribution for the propensity score $P^{l|ml}(X)$ as the group of participants in m. The

estimator of $E(y^l|D=m)$ is the mean outcome in that selected comparison group. The variances are

computed as the sum of empirical variances in the two groups.[15] Table 6 presents the results of all

probit estimations.

The matching procedure used is radius matching to allow for higher precision when many

similar comparison observations are available (e.g. Dehejia and Wahba 2002). The caliper chosen is

0.01 defining a radius of 1%points from the propensity score of the treated.[16] Given that any order-

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[14] See Lechner (2002) for the advantages of modeling the selection probabilities separately for each conditional binary choice equation for observations being in either treatment state compared to modeling them as complete choice problem in one model for the full sample.

[15] For the analytical variances and hence the standard errors of these estimators see Becker and Ichino (2002).

[16] Rosenbaum and Rubin (1983) propose the following formula for the caliper c:

$c = 0.25[s_t^2 + s_c^2)/2]^{1/2}$

(with s_i^2 being the point estimate of the variances of the estimated propensity score in the treated (t) and control (c) groups), yielded calipers that were almost double of size.

preserving transformation of the propensity score is sufficient to matching purposes, the predicted linear index rather than the predicted probability is used, as the former allows one to be more discriminating on individuals with predicted probabilities in the tails of the distribution (Sianesi 2004).

5.4 Estimation Results

Since valid matching requires sufficient common support and overlap, consider first Figure 1 showing the histograms of the propensity scores for each pairwise estimation, for treated and controls respectively. The overlap is satisfying in all estimations. The least overlap is reached in the estimations with the fewest observations (especially IAB_qual).

As a next check, Table 7 presents the numbers of potential observations of treated and controls and of matched observations. For each of the pairwise estimations, only few treated observations are lost through the matching procedure, and in most estimations, there are thousands of controls matched to the treated. Even for the smallest control group, 990 controls remain (in this case for 135 treated).

Since matching is not performed on all covariates but on the propensity score, it has to be checked if the matching procedure is able to balance the distribution of covariates in both the control and treatment group (see Chapter 3). Table 7 shows indicators for the mean matching quality for each of the estimations: the mean standardized bias, the Pseudo-R^2's of the propensity score estimations before and after matching and p-values of a likelihood ratio test on the joint insignificance of all regressors in the probit model. The test should be rejected before, and should not be rejected after matching.

The obtained values of the quality indicators seem to grant some success in the matching procedure, especially in the estimations with many matched controls available, but matching

performed reasonably well in the estimations with fewer observations, too. The values of the mean absolute biases are 0.8% in the single treatment estimation and between 1 and 2% in most multiple treatment estimations (the highest bias is 5.3% in the smallest estimation sample of IAB_qual vs. IAB_search). Common values in previous empirical studies are between 3% and 5% though there is no formal (statistical) threshold (Sianesi 2004, Caliendo and Kopeinig 2008).

The Pseudo-R^2 from the propensity score estimation on all the regressors before matching are clearly higher than after matching on the matched samples where they fall close to zero in most cases. Moreover, the p-values of the likelihood ratio test on the joint insignificance of all regressors reject the hypothesis prior to matching (0.000) but do not reject after matching (1.000). As an ultimate check for the matching quality, Table 8 reports for each single covariate the p-values of the standard t-test for the equality of mean sample values in the matched samples and the reduction of the mean standardized bias due to the matching. In most cases, the mean standardized bias is reduced by more than 80%. After matching, we fail to reject the null hypothesis of mean equality between the treatment and control groups for all variables.

The conclusion to be drawn from the assessment of the matching quality is that we achieve acceptable levels of covariate balancing in all estimations.

5.5 Effects of IAP as Single Treatment

The outcome variables in this analysis are the log hourly reservation wage, search effort, and three types of employment at the time of the interview: being in unsubsidized employment without receiving welfare benefits, being employed but additionally receiving welfare, and employed in a work measure (a one-Euro job). First, the estimation results for the overall effects of individual action plans on the reservation wages and search effort are presented (see Table 9, column (I)). Signing an IAP significantly increases search effort: the share of welfare recipients who had searched for a job in

the last four weeks prior to interview is 7.6%points higher when they had signed an IAP. Signing an IAP also reduces their reservation wages by 1.6%. Though the effect is statistically significant, the size of the effect is not of high economic significance, also reflecting that reservation wages of the population are already at the bottom of the German wage distribution.

Under the assumption that an increase in the share of active job-seekers is associated with an increase in the average job search intensity, we can interpret results as support for our theoretical prediction that IAP increase job search intensity. Regarding reservation wages, the empirical analysis enriches the theoretical analysis by showing that the compulsion effect seems to slightly dominate the support effect since on average IAP reduce reservation wages.

As shown in the model, increased search intensity and reduced reservation wages should both increase the exit rate to employment. Their employment implications, however, need not necessarily show up in a short time window after these changes occurred. Now consider column (II) of Table 9 presenting estimation results of IAP on different indicators for employment. At the time of the interview, increased search effort and reduced reservation wages had not translated into a higher probability to be in unsubsidized employment. Also the effect of IAP on employment with additional welfare receipt (due to ongoing neediness of workers) is small but it is positive and significant: the probability for participants to be employed but still needy is 1.3%points higher than for non-participants.

Finally, column (II) shows that the effect of IAP on work measures is larger and of high statistical significance: the probability to be in a work measure when interviewed is 4.1%points higher for participants than for non-participants. Results suggest that IAP in general increase the participation in work measures and, to a very small extent, increase employment with additional welfare receipt.

Work measures may take place over longer periods of time and so positive effects on regular employment outcomes might emerge only in the longer term (locking-in effect). This possibility can be further tested with the multiple treatment approach by looking if a higher enrolment in work measures is correlated with a lower employment probability; higher enrolment is to be expected for treated with IAB_qual in particular.

On the other hand, since IAP with only compulsory elements should increase regular employment outcomes within a few months and active search effort has indeed increased, the slow reaction of employment outcomes has also to be explained with our sample of mostly long-term unemployed welfare recipients (see the sample description in the Section "Data").

As a final remark, though increases in re-employment rates are not substantial but increases in the probability to be in a work measure are, the estimator of the effect of IAP on search effort is supposed to be biased downwards. Thus, the estimator can be interpreted as lower bound of the effects of IAP on the job search of participants.

5.6 Effects of IAP as Multiple Treatments

The results above describe the overall effect of signing an IAP. Now we turn to the results of IAP as multiple treatments which can tell us firstly, if different types of IAP in terms of support (qualification) and compulsion (search requirements) have different effects on search and reservation wages. Secondly, results can give us a hint if locking-in effects of IAP are responsible for the lack of substantial employment effects. The estimation results for the different treatment groups are presented in Table 10.

Consider again first column (I) of Table 10 that presents the effects of the three types of IAP on the reservation wages and search effort of the treated. Compared to the non-treated, search effort was substantially increased by IAB_search and IAB_both by +8 and +9%points. The effect of

IAB_qual on search is close to zero and statistically insignificant. The positive sign of the effect can result from the bias due to enrolment in work measures. The estimations within the treated confirm the result that treated with IAB_qual search significantly less than the other two groups.

Regarding reservation wages, the point estimates suggest that especially IAP with both qualification elements and search requirements (IAB_both) reduce reservation wages. The lack of statistical significance in reduced reservation wages of treatment groups IAB_qual and IAB_search might result from the smaller sample sizes. Again, even in group IAB_both, the size of the reduction is with 2.1% not of high economic significance.

Turning our attention to column (II) of Table 10, we find no evidence of any type of IAP to increase re-employment probabilities. The estimates are mostly insignificant, close to zero, and have a negative sign. Instead, results support our notion of locking-in effects: Participants with IAB_qual are much more likely to participate in a work measure when interviewed than non-participants (+19%points) and the other treatment groups. At the same time, their probability to be in unsubsidized employment and employment with additional welfare receipt is lower than of non-participants (-3 and -4%points respectively). The point estimates in the estimations within the treated confirm the result that treated with IAB_qual are less likely employed than the other two groups.

Though IAB_search and IAB_both seem to increase active search effort and reduce reservation wages, participants did not find employment more often than non-participants. They are also significantly more often enrolled in work measures than non-participants but to a less extent than participants with IAB_qual. However, the small and insignificant point estimates of the coefficients of IAB_search and IAB_both on employment probabilities suggest no substantial locking-in effects.

5.7 Sensitivity Analysis

To check the robustness of the results, various tests were carried out.[17] Results are robust towards an exclusion of the simulated start month of the IAP from the control variables as well as an exclusion of the self-reported motivation to enter welfare in the propensity score estimations. Further robustness tests were carried out with different sample restrictions (including 15-18-year-olds, and for the job search outcomes without employed individuals), different sizes of calipers, the original propensity score as balancing score, and various matching algorithms (Kernel, NN, Stratification Matching). Results stayed robust towards these changes.

6 Conclusion

This study is the first to analyze the effects of individual action plans on the reservation wages and search effort of job-seeking benefit recipients who conclude such a plan with their case worker. Theoretical predictions on the effects of IAP are derived from a partial job search model and imply that IAP increase the search intensity of job-seeking benefit recipients and yield ambiguous effects on their reservation wages and exit rates to employment, depending on the supportive and compulsory character of the IAP signed.

Estimation results suggest that individual action plans substantially increase search effort and slightly reduce reservation wages of welfare recipients who signed such a plan. Therefore we can conclude that on average the compulsory effect of individual action plans dominates their support effect. However, the increased search effort and reduced reservation wages do not translate into a higher probability of employment for participants at the time of the interview. Instead, participants with individual action plans are much more likely to be enrolled in a work measure when interviewed.

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[17] Results are available upon request.

Hence, in the short run, locking-in effects seem to delay exits to employment. But locking-in effects alone cannot explain the lack of reaction in employment outcomes. In addition, the lower labor market attachment of welfare recipients compared to unemployment insurance recipients might prevent fast exits to employment.

A further result is that the design of an IAP matters: the effects of IAP on search effort, reservation wages and employment outcomes differ with regard to the supportive and compulsory elements they contain. In line with predictions of search theory, IAP with search requirements significantly increase search effort and reduce reservation wages. From theory, we expected IAP with only qualification elements to reduce search costs, raise optimal search intensity but also raise reservation wages.

The empirical results, however, draw a different picture: IAP with just qualification elements have no significant positive effect on reservation wages or search effort – instead, the sign of the search coefficient is negative and they significantly reduce search effort compared to IAP with search requirements. The missing effect on search effort than might be due to the particular high enrolment in work measures of participants with IAP with just qualification elements.

This high enrolment in work measures is also reflected in a lower probability for them to be in employment, suggesting that a locking-in effect exists for these IAP. Hence, while the compulsory effects of IAP on the search behavior show up immediately in the data it is possible that with a longer observation window one could detect the support effect on the search behavior as well, i.e. that people with IAP with qualification measures increase their search effort and their reservation wages once they have left their work measures.

At least in the short run individual action plans do not improve the job search success measured as increased re-employment probabilities of job-seeking benefit recipients. Even welfare recipients with IAP with search requirements who had raised their search effort and reduced their

reservation wages compared to non-participants did not have a greater probability to be employed when interviewed.

If, however, the higher participation in work measures improved the labor market attachment of participants in the longer run, IAP could increase re-employment probabilities. This question needs to be addressed in future research, along with the question which design of IAP is especially suited to increase re-employment probabilities.

We saw that IAP with search requirements increase search effort but the increased search effort needs not automatically translate into better employment chances. If this increase in search effort measures a substitution of informal search effort with formal effort and informal job search is more effective than formal job search, IAP may easily have a perverse effect on re-employment probabilities, as Van den Berg and Van der Klaauw (2006) suggest for monitoring in general. Therefore, a useful extension of the presented results would be to test the effect of differently designed IAP on the job search channels people use.

Appendix: Tables and Figures

Table 1: Summary Statistics of the Sample

	Mean	SD
Sociodemographic Variables		
Gender: female	0.497	0.500
Age	38.393	10.332
Partner		
Married man	0.172	0.378
Married woman	0.168	0.374
Children		
More than 2 children in hh	0.064	0.245
Availability of child care (reference: no children of caring age in the household)	0.684	0.465
No child care available	0.038	0.191
Part-time child care available	0.110	0.313
Full-time child care available	0.167	0.373
Single parent	0.165	0.371
Other earners in the household?	0.222	0.416
German nationality	0.895	0.307
Qualification and skills		
Qualification[a] (reference: low qualification)	0.279	0.449
Intermediate qualification	0.604	0.489
High qualification	0.050	0.219
Very high qualification	0.066	0.249
(Very) good writing skills in German?	0.795	0.404
(Very) good language skills in German?	0.873	0.333
Driving License	0.675	0.468
Disabled	0.140	0.347
Own Email Account	0.366	0.482
House Owner	0.114	0.317
Motivation to enter welfare		
Job	0.650	0.477
Work Measure	0.387	0.487
Further Training	0.383	0.486
Counseling	0.678	0.467
Wait until pension	0.390	0.488
Wait until next job	0.395	0.489
Benefits	0.803	0.398
Labor market status in December 2004		
In vocational training or school	0.049	0.216
In unsubsidized employment	0.094	0.293
Employment history[b]		
Blue-collar worker in last employment	0.362	0.481
Years of employment[c]	1.611	1.510
Years of unemployment[c]	2.490	1.657
Years of training[c]	0.558	0.947
Last net wage (€/hour)	5.550	3.054
Last net wage missing?	0.647	0.478
Benefit Receipt in December 2004		
No benefits (reference)	0.146	0.353
UI	0.647	0.478
Welfare	0.161	0.368
UI+welfare	0.046	0.209

Table 1 ctd.

	Mean	SD
Information on behavior of Job Center		
Job Center accepted housing	0.884	0.321
Job Center did Profiling	0.386	0.487
Month of first contact to *Job Center*	3.786	3.060
Month of first contact after interview or missing	0.517	0.500
Sanction rate per *Bundesland* (monitoring indicator)[c]	1.885	0.489
Start month of IAP	5.833	3.454
Situation of regional labor market		
Unemployment duration in months by district	15.911	3.120
Unemployment rate by district	15.592	5.829
County group (reference: Group 4: SA, SA-AN, BRA, MVP, SAA)[d]	0.324	0.462
Group 1: HH, NRW, HE	0.247	0.431
Group 2: SCHL, BW, NS, BAY	0.243	0.429
Group 3: BLN, RP, TH, BR	0.186	0.389
Living in West Germany	0.545	0.498
Information on interview		
Telephone interview?	0.949	0.219

Source: Life Situation and Social Security 2005. Own calculations. **Notes:** Number of Observations: 10,192. If not indicated otherwise, all information refers to pre-treatment month 12/2004. a. Definition of qualification dummies: Low qualified means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified means *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, highly qualified means *(Fach-) Abitur* and apprenticeship or master craftsmen and very highly qualified means university degree. b. Histories refer to the five-year-period before treatment. c. Earliest available administrative data, from 10/2006. d. Counties were grouped according to the average percentage of sample members with IAP: (1) 15.1%, (2) 19.2%, (3) 21.7%, (4) 28.4%.

Table 2: Content of Individual Action Plans in the Sample (in % of Participants)

Search requirements (95.4%)	Show certain number of job applications in answer to advertisements	71.0%
	Ask employers for jobs	65.0%
	Inform about job possibilities	56.4%
	Show certain number of unsolicited job applications	55.1%
	Inform at center of occupational information of Job Center	35.6%
	Show certain number of applications for apprenticeships (for welfare recipients below 25 years)	29.5%
	Visit private employment office	24.1%
	Post own job advertisement	21.3%
	Search internship	13.3%
	Open answer: show own search efforts	3.2%
Qualification elements (66.3%)	Participate in integration measure	42.6%
	Improve application documents	31.0%
	Participate in application training	24.9%
	Participate in job market *("Arbeitsmarktbörse")*	24.6%
	Participate in start-up-workshop	3.4%
	Open answer: participate in one-Euro job	1.7%
	Open answer: participate in qualification measure	1.3%
	Open answer: participate in ALMP measure	1.0%

Source: Life Situation and Social Security. **Notes**. The total number of observations with IAP is 2,941

Table 3: Definition of the Four Treatment Groups

		Observations	In % of the sample	In % of participants
Individual action plan	(1) IAB_qual	135	1.3%	4.6%
	(2) IAB_search	990	9.7%	33.7%
	(3) IAB_both	1,816	17.8%	61.7%
(4) No Individual action plan		7,251	71.1%	
Total		10,192	100.00%	

Source: Life Situation and Social Security 2005. **Notes:** The total number of observations with IAP is 2,941.

Table 4: Summary Statistics of Reservation Wages by Treatment

	Mean	SD		Mean	SD
IAP	5.771	2.007	IAB_qual	6.152	2.066
			IAB_search	5.744	2.019
			IAB_both	5.757	1.994
No IAP	6.241	2.246			
Total	6.113	2.193			

Source: Life Situation and Social Security 2005.

Table 5: Summary Statistics of Search Effort by Treatment

	Mean	SD		Mean	SD
IAP	0.743	0.437	IAB_qual	0.601	0.491
			IAB_search	0.740	0.439
			IAB_both	0.755	0.430
No IAP	0.586	0.493			
Total	0.629	0.483			

Source: Life Situation and Social Security 2005.

Table 6: Propensity Score Estimations for the Different Treatments

	IAP vs. No IAP	IAB_qual vs. no IAP	Treatment IAB_search vs. no IAP	IAB_both vs. no IAP	IAB_qual vs. IAB_search	IAB_qual vs. IAB_both	IAB_search vs. IAB_both
Sociodemographic Variables							
Gender: female	-0.025	-0.134	-0.064	0.008	-0.049	-0.143	-0.113
Age (reference: 18 to 24 years)							
25 to 39 years	-0.019	-0.090	-0.063	0.022	-0.113	-0.234	-0.074
40 to 49 years	-0.179**	-0.181	-0.201*	-0.139	-0.108	-0.226	-0.067
50 to 57 years	-0.322***	-0.216	-0.252**	-0.325***	-0.124	-0.076	0.080
Partner							
Married man	-0.053	-0.174	-0.081	-0.019	-0.100	-0.197	-0.142
Married woman	0.073	0.083	0.167**	0.008	-0.141	0.039	0.183**
Children							
More than 2 children in hh	0.118*	-0.064	-0.011	0.181***	-0.116	-0.273	-0.169
Availability of child care (reference: no children of caring age in the household)							
No child care available	-0.577***	-0.765**	-0.613***	-0.441***	-0.374	-0.321	-0.268
Part-time child care available	-0.192***	-0.074	-0.171**	-0.204***	0.080	0.076	0.006
Full-time child care available	-0.173***	-0.014	-0.165***	-0.181***	0.193	0.187	0.040
Single parent	0.165***	0.279**	0.196***	0.118**	0.064	0.165	0.116
Other earners in the household?	-0.009	0.095	0.050	-0.048	0.119	0.177	0.113*
German nationality	0.161***	-0.052	0.398***	0.069	-0.539**	-0.211	0.392***
Qualification and skills							
Qualification[a] (reference: low qualification)							
Intermediate qualification	0.061*	-0.009	0.114**	0.024	-0.163	-0.030	0.065
High qualification	0.131*	0.147	0.249***	0.046	-0.144	0.066	0.197
Very high qualification	0.055	0.159	0.131	-0.007	0.063	0.241	0.101
(very) good writing skills in German?	0.200***	0.472**	0.157***	0.187***	0.386*	0.358*	-0.006
(very) good language skills in German?	0.227***	0.678**	0.226***	0.170***	0.734*	0.764**	0.128
Driving License	0.053*	0.057	0.150***	-0.006	-0.118	0.060	0.210***
Disabled	-0.085**	-0.170	-0.146**	-0.039	-0.124	-0.201	-0.088
Own Email Account	3.30E-02	0.043	0.090**	-0.002	-0.028	0.105	0.100*
House Owner	-0.04	-0.086	-0.038	-0.034	-0.105	-0.043	0.052
Motivation to enter welfare							
Job	0.121***	-0.095	0.245***	0.052	-0.382***	-0.149	0.224***
Work Measure	0.128***	-0.022	0.028	0.190***	-0.146	-0.248**	-0.170***
Further Training	0.115***	0.152*	0.031	0.150***	0.046	-0.023	-0.144***
Counseling	0.131***	-0.095	0.055	0.174***	-0.189	-0.371***	-0.152**
Wait until pension	0.038	-0.079	0.004	0.075**	-0.079	-0.147	-0.073
Wait until next job	0.026	0.003	-0.051	0.070*	0.089	-0.011	-0.134**
Benefits	0.128***	0.163	0.167***	0.068	0.019	0.238	0.120
Labor market status in December 2004							
In vocational training or school	-0.252***	-0.176	-0.228**	-0.249***	-0.100	-0.093	0.075
In unsubsidized employment	0.005	0.220	-0.072	0.025	0.332	0.197	-0.168
Employment history[b]							
Blue-collar worker in last employment	-0.033	-0.163*	-0.064	0.015	-0.214*	-0.177	-0.096*
Years of employment	-0.070*	0.031	-0.056	-0.092**	0.045	0.138	0.023
squared Years of employment	0.012	0.003	0.006	0.018*	0.018	-0.012	-0.007
Interaction: below 25 years*years of employment	0.074	-0.229	-0.176	0.268**	0.241	-0.299	-0.271

Table 6 ctd.

	IAP vs. No IAP	IAB_qual vs. no IAP	Treatment IAB_search vs. no IAP	IAB_both vs. no IAP	IAB_qual vs. IAB_search	IAB_qual vs. IAB_both	IAB_search vs. IAB_both
Interaction: squared below 25 years*years of employment	-0.013	0.085	0.056	-0.076*	-0.046	0.086	0.072
Years of unemployment	0.223***	0.017	0.168***	0.259***	-0.099	-0.354**	-0.103
squared Years of unemployment	-0.036***	0.010	-0.029**	-0.043***	0.036	0.074**	0.015
Interaction: below 25 years*years of unemployment	0.187	0.040	0.179	0.219*	-0.432	-0.122	0.018
Interaction: squared below 25 years*years of unemployment	-0.05	-0.010	-0.055	-0.052	0.137	0.027	-0.018
Years of training	0.146***	0.072	0.174***	0.134**	-0.089	0.075	0.050
squared Years of training	-0.041***	-0.008	-0.063***	-0.032**	0.050	-0.027	-0.036
Interaction: below 25 years*years of training	-0.043	0.141	0.005	-0.103	0.102	-0.028	0.048
Interaction: squared below 25 years*years of training	0.03	-0.023	0.007	0.046	-0.010	0.006	-0.029
Last net wage (€/hour)	-0.009	-0.005	-0.011	-0.008	-0.005	0.004	0.017
squared Last net wage (€/hour)	0.000	0.000	0.001	0.000	-0.001	-0.001	0.000
Last net wage missing?	0.000	0.000	-0.072	0.010	0.000	0.000	-0.041
Benefit Receipt in December 2004							
No benefits (reference)							
UI	6.80E-02	0.216	0.024	0.059	0.300	0.200	-0.064
Welfare	-0.024	0.355**	-0.008	-0.082	0.539**	0.517***	0.038
UI+welfare	0.123	0.036	0.041	0.148*	-0.085	-0.059	-0.110
Information on behavior of *Job Center*							
Job Center accepted housing	0.073	0.223	-0.028	0.108**	0.416**	0.176	-0.150*
Job Center did Profiling	0.424***	0.174**	0.290***	0.481***	-0.103	-0.250**	-0.198***
Month of first contact to *Job Center*	0.027***	0.039**	0.036***	0.017**	0.015	0.030	0.014
Month of first contact after interview or missing	-0.247***	-0.169	-0.161***	-0.266***	-0.031	0.038	0.080
Sanction rate per *Bundesland* (monitoring indicator)[c]	0.129**	0.037	0.153**	0.100	-0.085	-0.093	-0.010
Start month of IAP	0.011***	0.024**	0.017***	-0.001	-0.004	0.009	0.012
Situation of regional labor market							
Unemployment duration in months by district	0.025***	0.024	0.043***	0.012	-0.023	0.013	0.028**
Unemployment rate by district	-0.006	0.002	-0.009	-0.005	0.026	0.011	-0.003
County group (reference: Group 4: SA, SA-AN, BRA, MVP, SAA)[d]							
Group 1: HH, NRW, HE	-0.348***	-0.174	-0.298**	-0.328***	0.103	0.017	0.023
Group 2: SCHL, BW, NS, BAY	-0.218***	-0.003	-0.048	-0.287***	-0.022	0.199	0.281**
Group 3: BLN, RP, TH, BR	-0.115***	0.240**	-0.152**	-0.107**	0.412**	0.359**	-0.008
Living in West Germany	-0.103	0.164	-0.277**	-0.033	0.674*	0.344	-0.257
Information on interview							
Telephone interview?	0.222***	0.157	0.239**	0.232***	0.048	0.020	0.029
Constant	-2.517***	-4.672**	-3.381***	-2.420***	-2.294**	-2.772***	-0.962**

Source: Life Situation and Social Security 2005. Own calculations. **Notes:** */**/*** 90/95/99 CI. For Pseudo R^2 and number of sample and treated see Table 7. a. Definition of qualification dummies: Low qualified means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified means *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, highly qualified means *(Fach-) Abitur* and apprenticeship or master craftsmen and very highly qualified means university degree. b. Histories refer to the five-year-period before treatment. c. Earliest available administrative data, from 10/2006. d. Counties were grouped according to the average percentage of sample members with IAP: (1) 15.1%, (2) 19.2%, (3) 21.7%, (4) 28.4%.

Table 7: Common Support and Quality Indicators for the Matching Before Matching (BM) and After Matching (AM)

		Number of observations				Mean Standardized Bias (in %)		Pseudo R^2 (in %)		P-value	
		Potential treated	Potential controls	Matched treated	Matched controls	BM	AM	BM	AM	BM	AM
Single Treatment	IAP vs. no IAP	2941	7251	2906	6797	15.1	0.8	14.4	0.1	0.000	1.000
Treated vs. Non-treated	IAB_qual vs. no IAP	135	7251	130	4046	13.3	1.8	12.5	0.6	0.000	1.000
	IAB_search vs. no IAP	990	7251	986	6240	15.3	0.9	13.3	0.1	0.000	1.000
	IAB_both vs. no IAP	1816	7251	1783	6669	15.8	0.9	15.2	0.2	0.000	1.000
Treated vs. Treated	IAB_qual vs. IAB_search	135	990	123	564	12.7	5.3	13.7	4.7	0.000	1.000
	IAB_qual vs. IAB_both	135	1816	126	932	13.8	4.6	15.1	2.8	0.000	1.000
	IAB_search vs. IAB_both	990	1816	982	1750	7.9	1.3	6.1	0.3	0.000	1.000

Source: Life Situation and Social Security 2005. **Notes:** BM/AM: before/after matching. Radius Matching with caliper 0.01 is performed with Stata package psmatch2 (Leuven and Sianesi 2003). The mean standardized bias (MSB) is the difference of the sample means of treated and controls as a percentage of the square root of the average of the sample variances in the treated and controls (Rosenbaum and Rubin 1985). The Pseudo R^2 from the probit model estimation of the propensity scores includes all variables, before and after the matching process (Sianesi 2004). P-values are from the likelihood-ratio test of the joint insignificance of all the regressors before and after matching (H0).

Table 8: Quality Indicators for the Matching after Matching by Covariate

	p(t-Test with H0: no difference in means)	Reduction of MSB[a] (in %)
Sociodemographic Variables		
Gender: female	0.739	-53.2
Age (reference: 18 to 24 years)		
25 to 39 years	0.586	56.0
40 to 49 years	0.732	73.1
50 to 57 years	0.682	90.5
Partner		
Married man	0.529	88.5
Married woman	0.706	69.6
Children		
More than 2 children in hh	0.91	93.7
Availability of child care (reference: no children of caring age in the household)		
No child care available	0.269	91.3
Part-time child care available	0.96	98.2
Full-time child care available	0.871	86.7
Single parent	0.527	-69.8
Other earners in the household?	0.93	95.7
German nationality	0.785	97.5
Qualification and skills		
Qualification (reference: low qualification)		
Intermediate qualification	0.753	94.9
High qualification	0.895	51.0
Very high qualification	0.461	53.8
(very) good writing skills in German?	0.95	99.4
(Very) good language skills in German?	0.884	98.9
Driving License	0.661	83.2
Disabled	0.399	57.2
Own Email Account	0.477	53.5
House Owner	0.486	68.0
Motivation to enter welfare		
Job	0.58	96.8
Work Measure	0.578	96.1
Further Training	0.429	93.2
Counseling	0.377	94.9
Wait until pension	0.967	99.5
Wait until next job	0.759	97.1
Benefits	0.984	99.9
Labor market status in December 2004		
In vocational training or school	0.837	95.3
In unsubsidized employment	0.821	94.8

Table 8 ctd.

	p(t-Test with H0: no difference in means)	Reduction of MSB[a] (in %)
Employment history		
Blue-collar worker in last employment	0.688	87.4
Years of employment	0.981	99.3
squared Years of employment	0.99	99.7
Interaction: below 25 years*years of employment	0.767	89.1
Interaction: squared below 25 years*years of employment	0.686	67.4
Years of unemployment	0.921	98.3
squared Years of unemployment	0.99	99.7
Interaction: below 25 years*years of unemployment	0.937	98.7
Interaction: squared below 25 years*years of unemployment	0.935	98.2
Years of training	0.912	98.3
squared Years of training	0.982	99.5
Interaction: below 25 years*years of training	0.64	92.3
Interaction: squared below 25 years*years of training	0.57	89.3
Last net wage (€/hour)	0.971	87.0
squared Last net wage (€/hour)	0.565	86.6
Last net wage missing?	0.82	62.6
Benefit Receipt in December 2004		
No benefits (reference)		
UI	0.385	89.5
Welfare	0.5	92.3
UI+welfare	0.744	73.1
Information on behavior of *Job Center*		
Job Center accepted housing	0.545	76.4
Job Center did Profiling	0.612	97.1
Month of first contact to *Job Center*	0.78	97.4
Month of first contact after interview or missing	0.999	100.0
Sanction rate per Bundesland (monitoring indicator)	0.804	94.3
Start month of IAP	0.651	88.4
Situation of regional labor market		
Unemployment duration in months by district	0.969	99.2
Unemployment rate by district	0.946	99.3
County group (reference: Group 4: SA, SA-AN, BRA, MVP, SAA)		
Group 1: HH, NRW, HE	0.668	96.3
Group 2: SCHL, BW, NS, BAY	0.872	95.6
Group 3: BLN, RP, TH, BR	0.89	33.3
Living in West Germany	0.929	99.2
Information on interview		
Telephone interview?	0.558	89.1

Source: Life Situation and Social Security 2005. **Notes**: Radius Matching with Caliper 0.001. a. The standardized bias (MSB) is the difference of the sample means of treated and controls as a percentage of the square root of the average of the sample variances in the treated and controls (Rosenbaum and Rubin 1985).

Table 9: Estimation Results for the Effects of Individual Action Plans as Single Treatments

(I) Job Search Behavior		(II) Employment		
Search Effort	Log. Reservation Wage	Employed	Employed and Receiving Welfare	Work Measure
0.076***	-0.016*	-0.006	0.013*	0.041***
0.011	0.009	0.005	0.007	0.008

Source: Life Situation and Social Security 2005. **Notes:** */**/*** 90/95/99 CI. Standard Errors below coefficients. The respective outcome variables are the dummy "search effort in the last four weeks", the logarithm of the net hourly reservation wage and the dummies "currently in unsubsidized employment without welfare receipt", "currently in employment with welfare receipt (and not in work measure)" and "currently in a work measure". Radius Matching with caliper 0.01 is performed with Stata package psmatch2 (Leuven and Sianesi 2003).

Table 10: Estimation Results for the Effects of Individual Actions Plans as Multiple Treatments

		(I) Job Search Behavior		(II) Employment		
		Search	Reservation Wage	Employed	Employed and Receiving Welfare	Work Measure
Treated vs. Non-treated	IAB_qual vs. no IAP	-0.038	-0.008	-0.030*	-0.040	0.187***
		0.044	0.031	0.016	0.029	0.040
	IAB_search vs. no IAP	0.079***	-0.017	-0.011	-0.006	0.025**
		0.016	0.012	0.008	0.012	0.012
	IAB_both vs. no IAP	0.089***	-0.021**	-0.007	-0.001	0.042***
		0.013	0.010	0.007	0.010	0.010
Treated vs. Treated	IAB_qual vs. IAB_search	-0.119**	-0.013	-0.006	-0.051	0.167***
		0.051	0.039	0.021	0.037	0.048
	IAB_qual vs. IAB_both	-0.169***	-0.024	-0.008	-0.034	0.139***
		0.048	0.036	0.018	0.034	0.044
	IAB_search vs. IAB_both	-0.022	-0.016	0.000	0.004	-0.015
		0.018	0.015	0.009	0.014	0.015

Source: Life Situation and Social Security 2005. **Notes:** */**/*** 90/95/99 CI. Standard Errors below coefficients. The respective outcome variables are the dummy "search effort in the last four weeks", the logarithm of the net hourly reservation wage and the dummies "currently in unsubsidized employment without welfare receipt", "currently in employment with welfare receipt (and not in work measure)" and "currently in a work measure". Radius Matching with caliper 0.01 is performed with Stata package psmatch2 (Leuven and Sianesi 2003).

Figure 1: Matching Quality: Propensity Score Distributions for IAP (Type of Treated versus Type of Control Observations)
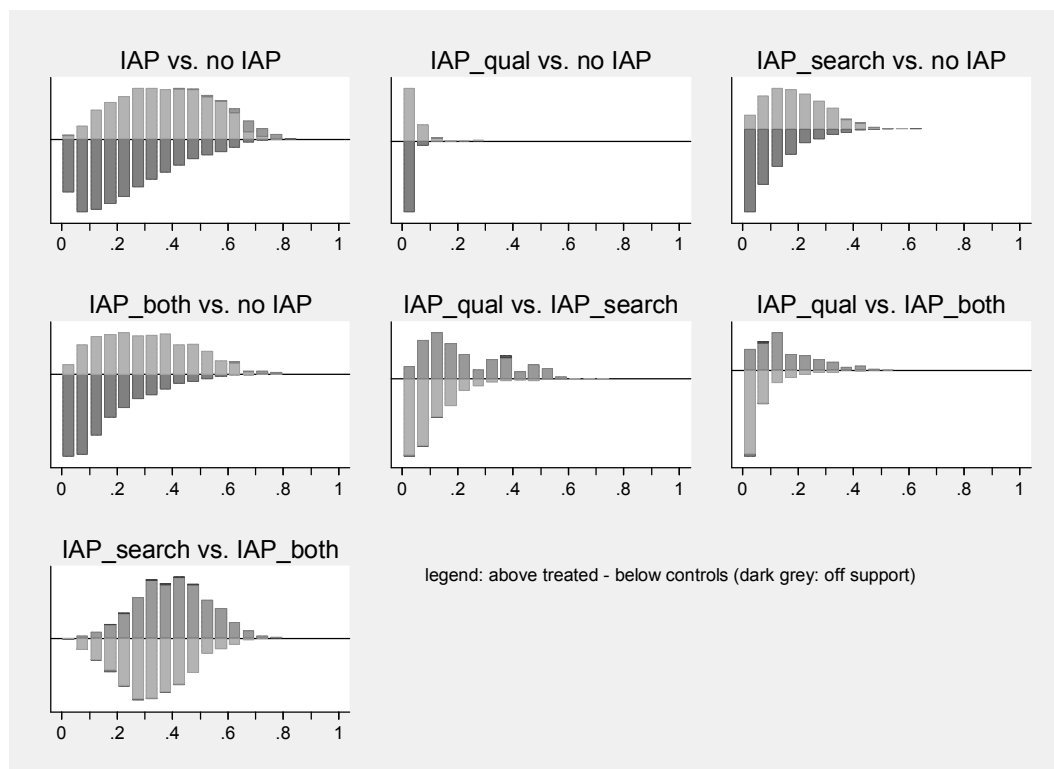
CHAPTER 5

JOB SEARCH MONITORING AND RESERVATION WAGES OF ELDERLY UNEMPLOYED[1]

1 Introduction

Although the key objective of the *Hartz-Reforms* was the activation of unemployed, until 2008, a regulation inconsistent with the activation principle (*'58er Regelung'*, henceforth RULE58) offered unemployed older than 57 years before and after the reform the possibility to receive benefits and job search support while being exempted from key activation policies as monitoring job search requirements and consequently benefit sanctions (see Chapter 1).[2] This study assesses the impact of monitoring job search requirements on the job search behavior by analyzing the impact of the regulation RULE58 on the reservation wages of elderly welfare recipients who opted for the regulation.

Regulations with a rather 'de-activating' character like the RULE58 stem from the 1970s and 1980s, when Western European welfare states including Germany established programs where older unemployed individuals were allowed to receive full benefits without the usual requirements to search for a job and end benefit receipt as soon as possible (Ebbinghaus 2000, Brussig and Wübbeke 2008).

These programs were introduced partly because of the concerns about non-pecuniary costs of usual search requirements to older unemployed with low job prospects. A further reason was presumably to help improving measured unemployment rates (Manow and Seils 2000). Programs

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[1] This chapter is based on joint work with Anton Nivorozhkin and Laura Romeu Gordo.
[2] Although RULE58 was terminated by the end of 2007, in the following the present verbal conjugation is used for simplicity reasons.

like the RULE58 are very popular but also impose immense costs to the unemployment insurance

systems (e.g. for Germany, see Schneider and Stuhler 2007), which is why their abolition is underway

or discussed in many European countries (Ebbinghaus 2000).

Monitoring, enforced with benefit sanctions, is often viewed as one of the major means to

activate unemployed individuals (e.g. Fredriksson and Holmlund 2006a). However there is scarce

literature on the effect of monitoring job search requirements on the different aspects of job search

behavior of the unemployed. Furthermore, the majority of studies on activating labor market policies

analyze the population of unemployment insurance recipients; the population of welfare recipients

might differ from unemployment insurance recipients since welfare recipients are typically less

attached to the labor market.

Johnson and Klepinger (1994), Boone et al. (2007) and Fredriksson and Holmlund (2006b)

present models in which job search monitoring reduces the duration of unemployment spells and

increases job entry rates.[3] Yet these results depend on the type of monitoring and the effectiveness of

the type of job search.

Van den Berg and Van der Klaauw (2006) distinguish between formal and informal job

search channels. In this model, an increase in job search monitoring may lead to a substitution

between the two channels of job searching. Unemployed workers will increase formal job search but

decrease informal job search. The overall impact of monitoring remains ambiguous and depends on

the effectiveness of each type of job search.

Manning (2005) concludes that unemployed workers may reduce search efforts and move to

unregistered unemployment if the search requirements are set too high. Menard (2006)

distinguishes between (a) monitoring search intensity and (b) monitoring rejections of suitable job

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[3] This holds provided that monitoring is not too costly.

offers. The author shows that monitoring the search intensity has a positive effect on the exit rate out of unemployment while monitoring job refusals leads to a decrease in search intensity.

Empirical evidence on the effect of monitoring provides a mixed picture since it is often hard to keep apart the effect of increased monitoring and accompanying measures, such as increased job search assistance (see Meyer 1995, Gorter and Kalb 1996, Dolton and O'Neill 1996, Blundell et al. 2004). Cockx and Dejemeppe (2007) find a positive effect of the threat of monitoring on the employment probability of highly skilled workers in Belgium. However, increased re-employment probabilities come at a cost. Highly skilled unemployed who were monitored accepted lower paid part time jobs.

If monitoring leads to lower accepted wages this also suggests that reservation wages decrease due to job search monitoring, a hypothesis that is in accordance with the results of Petrolongo (2008) and McVicar (2008) finding increased exits rates from unemployment.

Petrongolo (2008) provides evidence that stricter job search monitoring is a successful strategy to move individuals out of unemployment in Britain in the short run. The employment effect is, however, reversed in the long run; four years after the program finished the treated group had lower earnings and higher incidence of unemployment compared to the control group.

McVicar (2008) explores job search monitoring policy variations in Ireland. The author finds that periods of suspension in job search monitoring led to significantly lower exit rates from unemployment and an increased duration of the unemployment spell.[4]

The present study adds to this literature by providing first empirical evidence on the impacts of monitoring on reservation wages of welfare recipients. In addition, we apply a regression

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[4] The author reports similar findings using local administrative data (see McVicar 2009).

discontinuity design to address the problem of the possible endogeneity of monitoring job search requirements when analyzing reservation wages.

Regression discontinuity designs are based on the assumption that the treatment under evaluation depends on some observed variable according to a known, deterministic rule to identify the causal effect. Here, we exploit the age-discontinuity in the eligibility for RULE58 – the 58th birthday. As will be argued, while it is important to accommodate for non-linearities in the age-profiles, there is no reason to expect an abrupt chance in reservation wages with one's 58th birthday but eligibility changes discontinuously at this age.

Note that our analysis has some limitations rooting in the dataset we use: we are not able to extract the effect of the single policy measure monitoring – more specifically, the effect of merely eliminating monitoring while leaving everything else unchanged – on reservation wages.

On the one hand, the motivation of individuals when they decide to participate in RULE58 can be two-fold: some individuals may decide to keep looking for a job without the strict monitoring rules, and some individuals may decide to enroll in the program as a way of entering inactivity and stop job searching (Brussig and Wübbeke 2008). In the present study, we analyze the effect of entering the program without being able to distinguish between these different motivations. Therefore, we will be able to conclude how reservation wages react to the elimination of job search monitoring, but we are not able to observe whether this reduction is associated with the decision to leave the labor market or with the decision to dispense with institutional job search support.

On the other hand, we document the effect of the elimination of the monitoring job search requirements on reservation wages together with the elimination of benefit sanctions that enforce this monitoring.

A priori, it is not clear whether participation in RULE58 increases reservation wages. In a theoretical job search framework, the unemployed person perceives job search monitoring on the

one hand as having costs and thus, disutility, leading to reduced reservation wages. Furthermore, since for some attendants, participation in RULE58 is associated with a transition into inactivity, it is likely that for those participants a job offer has to be very attractive to be accepted. Thus, we can assume that the effect of transition into inactivity leads to an increase in reservation wages.

But monitoring job search might also increase the skills of unemployed workers to find work and therefore be perceived as additional utility, leading to increased reservation wages. However, taking into account the characteristics of our population (elderly long-term unemployed) and the fact that under RULE58 job search support is still available we expect this second effect to be small and hence the general effect of RULE58 on reservation wages to be positive.

The remainder of the study is organized as follows. The following section describes the institutional framework of the German RULE58. Section 3 describes the sample selection and the sample. Our identification strategy and results are presented in Section 4. Finally, Section 5 concludes.

2 Institutional Framework

In Germany, the regulation RULE58 for unemployed aged 58 and older was introduced in 1986. Until 2005, RULE58 was available only for persons receiving insurance-based, earnings-related unemployment benefits, i.e. unemployment insurance (UI) and unemployment assistance (UA) (Brussig and Wübbeke 2008). In 2005, the program was extended to recipients of the new welfare benefit 'Unemployment Benefit II' (see Chapter 1).

Under the RULE58, unemployed aged 58 years and older with an own social insurance pension[5] can choose to continue receiving full unemployment benefits and job search assistance ('carrots') but are exempted from monitoring job search requirements, and consequently sanctioning ('sticks'). If participants wish to get information about job offers despite of opting for RULE58, they are entitled to get job announcements and to register as 'job seeking', but they do not have to prove that they are searching (by visiting the employment office regularly and on short notice or signing an individual action plan), nor do they have to accept job offers or qualification or work measures they do not want.

Individuals are allowed to travel up to seventeen weeks per year as opposed to three weeks for individuals not affected by RULE58. In return, they have to declare in written form that they will apply for their pension as soon as possible. Until 2008, they had to apply for the pension as soon as they received a deduction-free pension which was in practice with 65 years,[6] so this was not a real constraint.

The program is popular and the number of persons opting for it has grown over time (Brussig and Wübbeke 2008). While in the 1990s, less than one third of unemployed persons of age 58 to 64 participated in the program, in 2007, around 60% of the UB I benefit recipients and 23% of the welfare recipients aged 58 to 65 enrolled in the program (statistics of the Federal Employment Office, Schneider and Stuhler 2007, Brussig and Wübbeke 2008).

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[5] About 90% of the workers in Germany are covered by the statutory pension insurance. Most of the remaining share is covered by the separate civil servants' pension scheme (Teipen and Kohli 2004). Old-age pensions are granted upon application and entail the standard social insurance pension as well as particular social insurance pensions for certain groups if they paid social insurance contributions during their working life: workers who paid social insurance contributions for 35 years, severely handicapped persons, unemployed, older people working part-time, and women. Three preconditions are necessary to receive a social insurance pension (Art. 34,1 SC VI): (1) personal preconditions (e.g. reaching a specific age), (2) certain preconditions in insurance law (e.g. a certain amount of obligatory contributions in a certain period), and (3) accomplishment of a qualifying period.

[6] As beginning a pension before the regular age of 65 is sanctioned by deductions (0.3% per month), a belated beginning is rewarded with a bonus (0.5% per month). Further details on early retirement in Germany can be found in Ebbinghaus (2000) and Brussig and Wübbeke (2008).

3   Data

The empirical analysis is based on the survey "Life Situation and Social Security 2005" described in Chapter 2. Key for this study is that the survey contains detailed information on reservation wages and participation in RULE58. Details on the collection of reservation wages can be found in Chapter 3. Regarding participation in RULE58, individuals aged 58 to 65 years in winter 2005 are asked if they entered RULE58 and the exact month and year of entry.

3.1   Sample Selection and Descriptive Analysis

Our dataset contains information on 15,219 unemployed who entered welfare receipt between January to March 2005. We restrict our sample to individuals born between December 1945 and December 1949 (14,170 observations are deleted) and who are eligible to an old age pension (178 observations are deleted). We further delete individuals with missing and implausible values of reservation wages.[7] The resulting sample consists of 670 observations.[8]

Table 1 presents descriptive statistics for individuals not eligible to participate, eligible non-participants and participants in the program. Note that a correlation of observed characteristics with eligibility is not problematic for our identification strategy as long as there is no discontinuity at age 58. We will provide sensitivity checks on this assumption below.

We do not observe remarkable differences in the variables presented between eligible and non-eligible individuals; this implies that these variables are not highly correlated with eligibility status. The only variables where we observe differences between both groups are gender and family

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[7] Missing values in reservation wage do not systematically differ between participants and non-participants (p(t-test on equal means) = 0.424). To delete outliers we trim the first upper and lower percentile of the reservation wage.

[8] When interviewed, most people are still unemployed; only 6% are in unsubsidized employment.

status. Non-eligible individuals are more likely to be women and to have a partner than eligible individuals.

Concerning differences between participants and eligible non-participants, we observe that living in East-Germany is positively associated with participation (almost 60% of all eligible non-participants but only 43% of all participants live in West-Germany). We find other differences between participants and non-participants in the level of household income and in debts. Non-participants are more often in the highest household income level considered (>900 Euros) and they have more often debts than participants.

The average hourly reservation wage in the total sample is slightly lower than six Euros. The low values of hourly reservation wage in the sample may be explained by the fact that we analyze the population of older welfare recipients.[9] Participants report a reservation wage of 6.22 Euros and eligible non-participants one of 5.86 Euros.

4 Empirical Approach and Results

Ideally, to make a correct inference on the effect of monitoring, we need to observe a reservation wage y_i and a binary treatment (participation) indicator t_i for each individual i in two states (monitoring and not monitoring) simultaneously. The evaluation problem arises because each individual is either monitored or not monitored and is never observed in two states simultaneously.[10]

Let $y_i(1)$ be the reservation wage given monitoring, and $y_i(0)$ the reservation wage in case of no monitoring. Then, the observed outcome can be defined as: $y_i = t_i y_i + (1 - t_i) y_i(0)$ and a common regression model for the observed outcome can be written as

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[9] Net wages of six Euros per hour correspond to an amount offered by low-wage jobs in Germany.

[10] This section draws heavily on Van der Klaauw (2008).

$$y_i = \beta + \alpha_i t_i + u_i \tag{1}$$

where $\alpha_i = y_i(1) - y_i(0)$ and $y_i(0) = E[y_i(0)] + u_i = \beta + u_i$. Non-random assignment between the monitoring and the non-monitoring state would generally not provide us with valid estimates of the treatment effect.

In this study, the source of identifying information of the effect comes from the fact that eligibility to the non-monitoring state changes discontinuously at age 58. This allows using a regression discontinuity (RD) design.[11]

Under a sharp RD design, individuals are assigned to treatment solely on the basis of a cut-off score of an observed continuous variable x, which in our case is age. Those individuals who fall above some distinct cut-off $\bar{x}$ (i.e. age of 58) are placed in the non-monitoring group $(t_i = 1)$, while those below the cut-off are placed in the monitoring group $(t_i = 0)$. Thus, assignment occurs through a known and measured determinist decision rule: $t_i = t(x_i) = 1\{x \geq \bar{x}\}$ where $1\{\cdot\}$ is the indicator function. As the assignment variable itself may be correlated with the outcome variable, the assignment mechanism is clearly not random. However, if it is reasonable to believe that individuals close to the threshold with very similar x are comparable, then we may view the design as almost experimental near the cut-off $\bar{x}$.

The idea underlining RD design is to compare individuals who are marginally above or below some known eligibility threshold where the probability of being placed into the non-monitoring group changes discontinuously. Such individuals should have similar characteristics except for participation in the program. In other words, inference made on the sample of individuals marginally

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[11] For contributions to the development of the RD models see Trochim (1984), Hahn et al. (2001) and a special issue of the Journal of Econometrics 2008, Volume 142, Issue 2. An up-to-date review of the application of the RD design in economics is presented in Lee and Lemieux (2009).

above and below the eligibility threshold can be as good as a randomized experiment (e.g., Lee 2008).

An important assumption used in the identification strategy is the so-called Local Continuity (LC) assumption. The Local Continuity assumption rules out the possibility that other programs, which use precisely the same cut-off, will influence the outcome. Moreover, the LC assumption prohibits a certain type of behavior on the part of participants and administrators.

It is assumed that potential treatment recipients as well as program administrators can neither manipulate the assignment variable nor the cut-off.[12] In our application we believe this assumption to hold since (1) the employment office has administrative data on the birthdates of potential treatment recipients and (2) there are no other changes for welfare recipients at their 58th birthday except eligibility for RULE58.

Application of the sharp RD design assumes that all individuals who reach the threshold would participate in the program. In practice, some individuals choose to participate in the program while others continue to search for a job and be subject of monitoring. In this case, where assignment to treatment often depends on x in a stochastic manner we have a so-called fuzzy RD design.

Fuzzy RD design implies a change in the treatment probability less than one. In the latter case, one may instrument participation status by the individual 'eligibility status' – non-eligibles below 58 years are assigned a 0, eligibles 58 years and over are assigned a 1. Note that it is important to account for nonlinearities in age-profiles especially if one considers larger age windows. Subsequently, the instrument eligibility status will only influence the decision to participate in the program, but not the outcome reservation wage.

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[12] Lee (2008) shows, in the context of a sharp RD, that the continuity assumption will be satisfied if individuals do not have perfect control over the position of the assignment variable relative to a cut-off.

More formally, assuming a homogeneous effect of participation in the program on reservation wage and one fix threshold we can write the fuzzy RD model (see Van der Klaauw 2002):

$$Rw = \gamma E(Treat \mid x) + a(x) + u \qquad (2)$$

$$E(Treat \mid x) = \eta 1(\{x \geq \bar{x}) + b(x) \qquad (3)$$

where Rw is the reservation wage and $Treat$ is the dummy of participation in RULE58. Finally $a(\cdot)$ and $b(\cdot)$ are flexible functions of age, and $E(u \mid x) = 0$.[13]

The parameter γ captures the causal effect on individuals whose treatment status – participation in RULE58 – changed as they crossed the eligibility threshold and turned 58 years. Thus, the causal effect corresponds to the Local Average Treatment Effect.

4.1 Results

Figure 1 shows the enrollment in RULE58 for our sample. We observe a sharp increase in the number of participants in RULE58 after crossing the eligibility threshold (i.e. age 58). At the same time, not everyone participates in the program. It follows that participation in the program is neither sharp nor fuzzy in the classical sense.

Battistin and Rettore (2008) describe such an intermediate case and label it 'partially fuzzy design' and provide its identification. The authors point out that the estimation of the 'partially fuzzy design' is similar to that of a fuzzy design. Yet, partially fuzzy design offers a greater degree of flexibility when it comes to testing the assumptions underlying RD design. We will describe the test suggested by Battistin and Rettore (2008) below.

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[13] For simplicity, we omit the individual subscripts.

Figure 2 shows reservation wage means 24 months before and after eligibility. We observe that although there is a high degree of dispersion, there is an increasing trend in reservation wages after eligibility.

We estimate the model equations (2) and (3) accounting explicitly for clustering of the regression errors at the age cell level to account for a possible specification error due to the fact that age is discrete in our data (see Lee and Card 2008). Enrollment in RULE58 is instrumented by the eligibility status.

We present three specifications based on linear, quadratic polynomials and linear spline to account for nonlinearities in age-profiles and restrict the observation window to 24 months before and after the threshold. Another approach would be to restrict the data to a narrower observation window to avoid the problem of having to rely on functional form assumptions about the control function in identifying the effect. This approach could, however, produce imprecise measures of the effect since the regression-discontinuity method is subject to a large degree of sampling variability.

The outcome reservation wage is transformed into the logarithmic form. Initially, in all specifications we control for gender, region (East/West Germany) and marital status (single). The main reason for the inclusion of the additional control variables is to enhance the efficiency of the estimates (e.g., Lee 2008). Moreover, given differences in observed covariates between eligibles and non-eligibles in our sample (see Section 2) and since our observation window is relatively wide, we may expect that baseline covariates may correlate with participation in the program and the outcome (see Lee and Lemieux 2009).

We start by presenting the results of the baseline specification in Table 2. The first stage regression predicting participation in RULE58 is instrumented by the eligibility status and yields satisfactory results (see Panel A). The coefficient of the dummy 'eligible' is above 0.38 and is highly statistically significant. The goodness of fit statistics R^2 exceeds 0.27 in all specifications.

Results of the estimation of equation (2) are close in absolute value, and correspond to a 22-23% increase in reservation wages.[14] All effects are statistically significant at the 5 or 10% level (see Panel B, Table 2).[15] We conclude that an elimination of monitoring indeed significantly increases the reservation wages of our group of older welfare recipients.

To test the validity of our results, we implement two specification tests as suggested by Battistin and Rettore (2008), and McCrary (2008) and Lee (2008). The test of Battistin and Rettore (2008) aims at comparing the outcomes of individuals who are not eligible to participate in the program and individuals who are eligible but choose not to participate.[16] In the absence of heterogeneous response to the program, we should not find significant differences in the outcomes for the two groups.

Presence of response heterogeneity would imply that also eligible non-participants will be affected by the treatment and therefore, would cast serious doubts on the validity of our identification strategy. We test for differences in outcomes between non-eligibles and eligible non-participants using linear regression.[17] Panel C, Table 2 presents the coefficient and standard error of γ. The results do not indicate significant differences in the outcomes; moreover the coefficients are close to zero in all specifications. We conclude that there is no heterogeneous response of reservation wages of eligible and non-eligible non-participants to RULE58 in this application.

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[14] Marginal effects are calculated according to the following formula: $(\exp(\gamma)-1)$.

[15] To get some guidance on the choice of polynomial functional form we implement the Lee and Card (2008) test based on the goodness of fit statistics. The test does not reject any of the specifications (results are available on request).

[16] Section 4.2 of Battistin and Rettore (2008) links this result to the earlier literature on testing non-experimental estimators (e.g. Heckman and Hotz 1989).

[17] We estimate the following specification: $Rw=\gamma 1(\{x\geq\bar{x}\})+b(x)+u$.

The second test follows McCrary (2008) and Lee (2008) and aims to examine whether the observed baseline covariates are locally balanced on either side of the eligibility threshold.[18] The test exploits outcomes that are on logical grounds not affected by the eligibility status and are likely to affect the reservation wage. Consider for instance the case of individual wealth. Wealth is known to affect the reservation wage (e.g. Bloemen and Stancanelli 2001). At the same time, wealth should not show a jump at the eligibility-age 58.

We apply the same procedure as described in Section 4 on a battery of outcomes (see Table 3). We select a number of covariates capturing individual socioeconomic and demographic variables which should satisfy the condition described above. The results in Table 3 indicate that the causal effect of eligibility on the selected variables is close to zero and not statistically significant. We conclude that potentially important baseline covariates are locally balanced on either side of the eligibility threshold.

We further examine potential effect heterogeneity by splitting the sample into men and women and East and West Germans. The results of the estimation are presented in Table 4-7. The results are broadly consistent with the previous findings with a stronger effect for females and for individuals living in West Germany. The effect of participation in RULE58 remains large and positive, although statistical significance is often affected due to the smaller sample sizes. We conclude that our principal findings do not change due to potential effect heterogeneity.

4.2 Robustness Check

In this section we implement further checks on the robustness of our baseline estimates. We narrow our observation window in order to restrict our sample to individuals who are closer to the age

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[18] In other words we test whether other potentially confounding factors are smooth functions in the proximity of the threshold.

threshold than in our baseline observation window of 24 months below and above age 58. We present results for the observation windows 18 and 12 months below and above age 58. Table 8 shows the results.

Restricting the observation window results in higher estimates of the effect of the program as compared to our baseline estimates (see Table 2). At the same time we do not observe considerable disparities in the estimated effect between the 18 and 12 months observation windows. As we move to a narrower observation window, the statistical significance of our results declines, yet our main result on the strong positive impact of participation in RULE58 on reservation wages holds.

In the final check, we restrict the sample to a narrower observation window and additionally exclude the explanatory variables gender, region and marital status (see Table 9).[19] Exclusion of explanatory variables should not affect the estimates since in the neighborhood of the threshold, control observations should be as good as randomly assigned conditional on $a(\cdot)$ and $b(\cdot)$. The estimates are close to our baseline estimates reported in Table 2.

5 Conclusion

Although job search monitoring increased in almost all OECD countries over the past two decades, there is an ongoing discussion in the literature on how exactly monitoring affects job search behavior (see OECD 2000). The present study adds to this literature by providing first empirical evidence on the impact of monitoring on reservation wages of welfare recipients.

To this end, the impact of a German regulation for elderly unemployed (RULE58) is analyzed. According to RULE58, unemployed welfare recipients turning 58 years had until the end of

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[19] The small number of observations prevents us from considering a "narrower" observation window.

2007 the option to receive benefits and job search support without having to prove their job search efforts.

A priori, it is not clear whether participation in this program increases reservation wages. However, assuming that monitoring has no real skill-enhancing effects for the analyzed population – unlike job search assistance and qualification measures, for instance – the general effect of RULE58 on reservation wages would be positive. In our empirical analysis we confirm this prediction.

In all specifications considered, enrollment in RULE58 implies a substantial increase in reservation wages of around 23 percentage points. The measured effect corresponds to the Local Average Treatment Effect (LATE). This result suggests that monitoring job search requirements indeed can affect the job search behavior by reducing reservation wages. Also, the result corresponds to previous findings in the literature that monitoring leads to lower accepted wages and increased exits rates from unemployment.

Yet one should be cautious in applying the size of the found LATE to the whole population of unemployed since one intention of regulations like RULE58 was the 'de-activation' of unemployed and participants were unemployed welfare recipients older than 57 years. First, reservation wages of unemployed with a closer labor market attachment might increase to a lower degree if job search monitoring was eliminated for them since some participants use the program as a way to inactivity.

Second, the analyzed population has a lower labor market attachment than the average unemployed benefit recipient. Therefore, their reservation wages might respond stronger to job search monitoring. One promising question for further research is whether the observed differences in reservation wages among welfare recipients subject or not subject to monitoring are replicated in studies on UI recipients. Further, it is of interest if these differences will ultimately lead to shorter unemployment spells of monitored welfare recipients. This research will need data with a longer observation period.

Appendix: Tables and Figures

Table 1: Descriptive Statistics

	Non-eligibles	Eligible non-participants	Eligible participants
Female	0.51	0.41	0.45
West	0.53	0.59	0.43
Child in household	0.10	0.07	0.05
Disabled	0.26	0.30	0.27
German	0.67	0.65	0.69
Partner in household			
Single	0.38	0.42	0.43
Non-employed partner	0.15	0.17	0.13
Employed partner	0.37	0.32	0.34
Equivalent net monthly household income (Euros)			
<= 600	0.20	0.19	0.14
601 - 900	0.62	0.56	0.67
> 900	0.18	0.25	0.19
Qualification			
Low qualified	0.25	0.27	0.19
Intermediate qualified	0.61	0.57	0.64
High qualified	0.04	0.04	0.07
Very high qualified	0.18	0.25	0.19
Personal wealth			
House Owner	0.19	0.18	0.12
Debts	0.33	0.41	0.31
Duration of the last Unemployment spell (in months)			
<= 30	0.40	0.39	0.36
30 - 90	0.43	0.43	0.44
> 90	0.17	0.19	0.20
Number of observations	426	154	90

Source: Life Situation and Social Security 2005. **Notes:** All information refers to the month before our observation period (December 2004). a. Definition of qualification dummies: Low qualified refers to no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified refers to *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, high qualified refers to *(Fach-) Abitur* and apprenticeship or master craftsmen; very high qualified refers to university degree.

Table 2: Effect of Participation in the Program RULE58 on Reservation Wages (24 Months Before and After Eligibility)

	(1)	(2)	(3)
A. Eligible	0.382***	0.396***	0.387***
	(0.0401)	(0.0512)	(0.0468)
N	670	670	670
Adj. R²	0.272	0.273	0.271
B. Treat	0.218*	0.230**	0.219*
	(0.115)	(0.112)	(0.112)
C. Test	0.0641	0.0636	0.0551
	(0.0529)	(0.0565)	(0.0549)

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Living in West Germany, Male and Single. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)). C: Test on differences between non-eligibles and eligible non-participants (Battistin and Rettore 2008).

Table 3: Test on Other Discontinuities around Age 58 (Lee 2008)

	(1)	(2)	(3)
Child in household	-0.691	-0.868	-0.934
	(0.499)	(0.633)	(0.672)
Disabled	-0.043	-0.192	-0.143
	(0.362)	(0.420)	(0.424)
German	-0.017	0.083	0.019
	(0.508)	(0.573)	(0.564)
Partner in household			
Non-employed partner	0.703	0.468	0.470
	(0.488)	(0.484)	(0.480)
Employed partner	-0.703	-0.468	-0.470
	(0.488)	(0.484)	(0.480)
Equivalent net monthly household income (Euros)			
<= 600	-0.432	-0.480	-0.521
	(0.426)	(0.507)	(0.504)
601 - 900	0.461	0.385	0.450
	(0.356)	(0.391)	(0.387)
> 900	-0.045	0.256	0.170
	(0.587)	(0.543)	(0.514)
Qualification			
Low qualified	0.540	0.484	0.517
	(0.340)	(0.336)	(0.325)
Intermediate qualified	-0.312	-0.333	-0.351
	(0.303)	(0.309)	(0.290)
High qualified	0.010	-0.655	-0.487
	(0.986)	(1.144)	(1.162)
Very high qualified	-0.309	0.145	-0.012
	(0.550)	(0.453)	(0.431)
Personal wealth			
House Owner	0.731*	0.574	0.621
	(0.378)	(0.409)	(0.394)
Debts	0.471	0.294	0.326
	(0.322)	(0.318)	(0.313)
Duration of the last Unemployment (in months)			
<= 30	0.279	0.217	0.207
	(0.406)	(0.439)	(0.431)
30 - 90	-0.182	0.084	0.054
	(0.425)	(0.404)	(0.398)
> 90	-0.158	-0.445	-0.406
	(0.425)	(0.380)	(0.403)

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, * $p<0.10$, ** $p<0.05$, *** $p<0.01$ (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. All information refers to the month before our observation period (December 2004). a. Definition of qualification dummies: Low qualified refers to no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualified refers to *(Fach-) Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, high qualified refers to *(Fach-) Abitur* and apprenticeship or master craftsmen; very high qualified refers to university degree.

## Table 4: Effect of Participation in the Program RULE58 on Reservation Wages (24 Months Before and After Eligibility, Men)

	(1)	(2)	(3)
A. Eligible	0.382***	0.394***	0.387***
	(0.0694)	(0.0784)	(0.0740)
Adj. R^2	0.248	0.248	0.247
B. Treat	0.113	0.127	0.115
	(0.164)	(0.161)	(0.163)
N	349	349	349

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Living in West Germany and Single. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Table 5: Effect of Participation in the Program RULE58 on Reservation Wages (24 Months Before and After Eligibility, Women)

	(1)	(2)	(3)
A. Eligible	0.387***	0.397***	0.386***
	(0.0558)	(0.0776)	(0.0710)
Adj. R^2	0.298	0.296	0.296
B. Treat	0.300*	0.300	0.292
	(0.176)	(0.202)	(0.200)
N	321	321	321

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Living in West Germany and Single. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Table 6: Effect of Participation in the Program RULE58 on Reservation Wages (24 Months Before and After Eligibility, East Germany)

	(1)	(2)	(3)
A. Eligible	0.396***	0.373***	0.380***
	(0.0707)	(0.0842)	(0.0730)
adj. R^2	0.334	0.335	0.337
B. Treat	0.160	0.235	0.194
	(0.168)	(0.176)	(0.164)
N	315	315	315

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Male and Single. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Table 7: Effect of Participation in the Program RULE58 on Reservation Wages (24 Months Before and After Eligibility, West Germany)

	(1)	(2)	(3)
A. Eligible	0.363***	0.391***	0.381***
	(0.0582)	(0.0694)	(0.0683)
adj. R^2	0.210	0.217	0.214
B. Treat	0.279	0.253	0.245
	(0.172)	(0.159)	(0.158)
N	355	355	355

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Male and Single. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Table 8: Effect of Participation in the Program RULE58 on Reservation Wages (18 and 12 Months Before and After Eligibility)

18 months	(1)	(2)	(3)
A. Eligible	0.355***	0.354***	0.354***
	(0.0481)	(0.0542)	(0.0483)
adj. R^2	0.266	0.264	0.265
B. Treat	0.347**	0.332**	0.342**
	(0.128)	(0.129)	(0.128)
N	514	514	514
12 months			
A. Eligible	0.253***	0.259***	0.268***
	(0.0482)	(0.0252)	(0.0225)
adj. R^2	0.269	0.276	0.276
	(1)	(2)	(3)
B. Treat	0.340	0.348*	0.355*
	(0.201)	(0.197)	(0.188)
N	363	363	363

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses, Additional controls are: Living in West Germany, Male and Single. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Table 9: Effect of Participation in the Program RULE58 on Reservation Wages (18 and 12 Months Before and After Eligibility, Excluding Covariates)

18 months	(1)	(2)	(3)
A. Eligible	0.366***	0.366***	0.365***
	(0.0537)	(0.0537)	(0.0482)
adj. R^2	0.264	0.264	0.264
B. Treat	0.224*	0.224*	0.239*
	(0.128)	(0.128)	(0.127)
N	514	514	514
12 months			
A. Eligible	0.277***	0.277***	0.285***
	(0.0237)	(0.0237)	(0.0213)
adj. R^2	0.271	0.271	0.272
B. Treat	0.208	0.208	0.230
	(0.197)	(0.197)	(0.191)
N	363	363	363

Source: Life Situation and Social Security 2005. **Notes:** Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. (1): Linear specification; (2): Quadratic polynomials; (3): Linear spline. A: First stage regression (prediction of program participation, equation (3)). B: Estimation of participation effect (equation (2)).

Figure 1: Discontinuity in RULE58 Participation
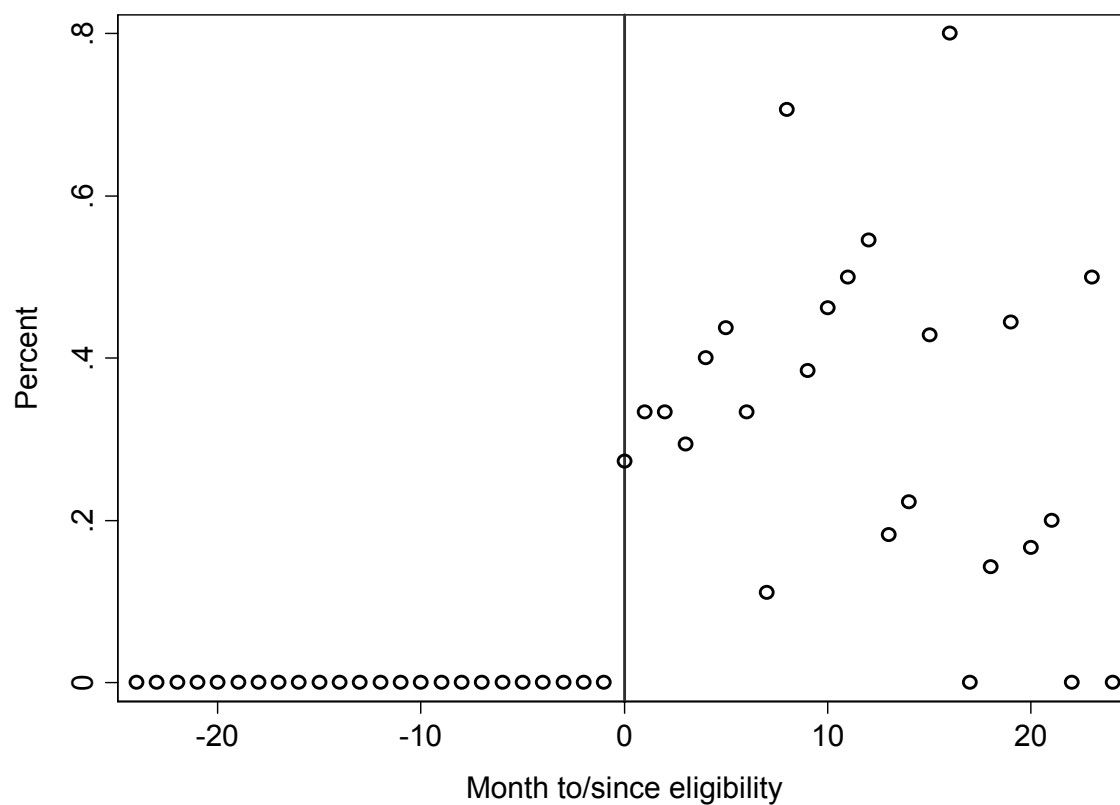
Figure 2: Discontinuity in Hourly Reservation Wages
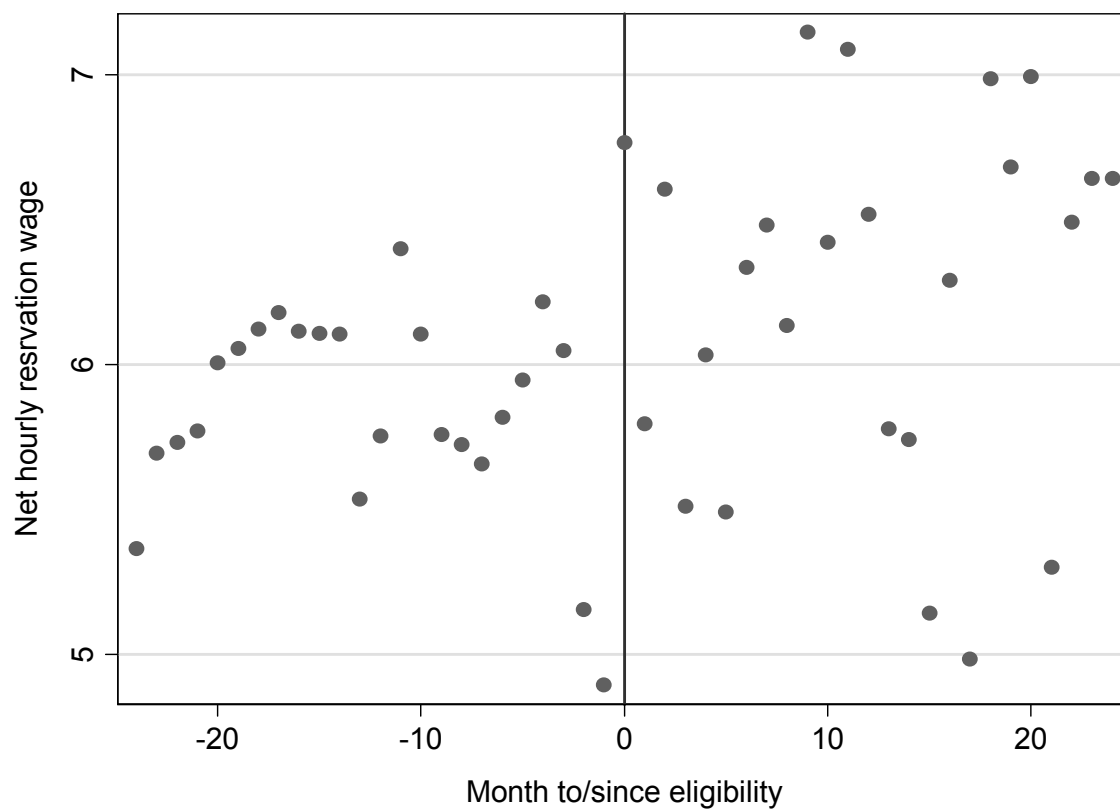
CHAPTER 6

JOB SEARCH MONITORING AND MENTAL HEALTH OF ELDERLY UNEMPLOYED[1]

1   Introduction

The design of activating unemployment benefit regimes typically involves 'carrots', i.e. supporting the unemployed in form of unemployment benefits, counseling and help in job search, and 'sticks', i.e. monitoring job search requirements of the unemployed, enforced by benefit sanctions (see Chapter 1). The 'sticks' are supposed to improve the efficiency of matching unemployed to vacancies, and thus reduce the costs of the unemployment insurance.

However, 'sticks' might also explain part of the negative impact of unemployment on mental health, especially if the probability to find employment is rather low for the unemployed who are treated with 'sticks'. In this study, we look at the *58er-Regelung* (RULE58) in Germany to address the question if the design of unemployment benefit regimes does affect the mental health of unemployed. Specifically we ask if a reduced exposure to 'sticks' improves the mental health of older unemployed.

Unemployed individuals consistently report lower levels of well-being measured either in terms of general life-satisfaction (for example Winkelmann and Winkelmann 1998) or in terms of worse mental or physical health (for example Bjorklund 1985 or Gerdtham and Johannesson 2003). This drop in well-being cannot be explained by the loss of income alone (Winkelmann and Winkelmann 1998), suggesting that non-pecuniary effects of unemployment may play a more important role than loss of income in explaining the adverse effects of unemployment.[2] One potential

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[1] The chapter is based on joint work with Steffen Reinhold.

[2] The literature on the relationship between labor force status and health is also summarized in Currie and Madrian (1999).

reason why unemployment exerts a negative influence on the job-seekers are conflicts with the employment office and the case workers.

In the field of public health, there exists already a small body of literature investigating the role of the particular design of social programs in mitigating the adverse effects of unemployment on mental health of the unemployed. Rodriguez et al. (2001), for instance, investigate the protective effects of different social programs on mental health for men and women, and find differential impact of government entitlement benefits versus other means-tested benefits in the United States.

Similar, Rodriguez (2001) argues that means-tested benefits do not seem to be high enough to prevent adverse effects of unemployment on health. Furthermore, Artazcoz et al. (2004) suggest that the size of the benefits plays a role in ameliorating any adverse effects of unemployment. However, this literature generally does not address the possibility that the particular regime for the unemployed may be endogenous. For instance, if only unemployed with previous long-term employment are eligible for benefits then comparing recipients with non-recipients may be problematic.

As described in Chapter 5, until 2008, German unemployed aged 58 or over were eligible for RULE58, an age-related opt-out where they could continue to receive full unemployment benefits and assistance ('carrots') without any of the usual 'sticks`.[3] Chapter 5 showed further that Western European welfare states including Germany introduced programs like RULE58 in the 1970s and 1980s, but that their abolition is underway in many European countries since these programs impose immense costs to the unemployment insurance systems (e.g. Schneider and Stuhler 2007, Ebbinghaus 2000).

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[3] The institutional framework is described in Chapter 5, Section 1.

However, if the costs of enforcing job search requirements are large there is the possibility that opting out of the usual regime improves the well-being of the participating individuals sufficiently to warrant those costs. The study presented in Chapter 5 finds that RULE58 raises reservation wages which can be seen as one metric of evaluating the welfare of the unemployed (Shimer and Werning 2007).

In this study we contribute to the literature of unemployment benefit regimes on health and well-being by considering the effect of RULE58 on mental health as an additional measure of the unemployed's well-being. The fundamental assumption in this study is that already the possibility to opt out of the regular activation scheme reduces the probability of job search monitoring and consequently sanctions – 'sticks' – for all eligible unemployed older than 57 years, and thus, the potential for conflicts with the case worker.

Both the unemployed and the case worker know that at any given moment, the eligible unemployed could circumvent the unwelcome 'sticks' trough their case worker by entering RULE58. We hypothesize that this arrangement reduces the adverse effects of unemployment and improves mental health outcomes for the unemployed. As we will show below, our data backs our argumentation in showing that unemployed aged 58 years and over receive less job search requirements and monitoring from their employment offices than unemployed under 58 years, even if they did not participate in RULE58.

A further contribution to the previous literature consists in using a regression discontinuity design to address the problem of the possible endogeneity of job search requirements, sanctions, and monitoring exploiting the fact that individuals aged 58 and older may opt-out of the strict monitoring regime. Our identification strategy relies on comparing mental health of individuals around the age cut-off to estimate the causal effect of 'sticks' in unemployment benefit regimes on the mental health of older unemployed.

The study is organized as follows. Section 1 explains the construction of our sample and describes the sample. Section 2 presents our empirical approach, and the results. We provide robustness checks and extensions to our core analysis in section 3. The final section 4 offers our conclusion.

2 Data

The data for the analysis stem from the "Life Situation and Social Security 2005" survey which is described in Chapter 2. Key for this analysis is the rich set of variables provided by the survey which also contains information on mental health. In Section 2.2 we will explain in detail how we construct our outcome variable 'good mental health'.

2.1 Sample Selection

For our analysis we use the survey's initial stock sample of 15,219 unemployed between 15 to 64 years who received welfare in January 2005. We restrict our attention to those aged around the discontinuity for program eligibility of 58 years and include in our core sample all individuals aged 56 to 60 at the time of the survey (deleting 14,160 observations). We further restrict our sample to all individuals who have complete information on health, age and relevant control variables including the information on pensions (deleting 90 observations),[4] and to all potentially eligible individuals who can claim an own pension (deleting 75 observations).

Since the East German labor market with its huge unemployment rates over the last two decades differs from the West German one in terms of job opportunities for unemployed, we

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[4] Missing values in both outcome variables differed randomly between participants and non-participants; a t-test on equal sample means in missing values could not be rejected (t= -0.97).

additionally divide our data in an East and a West German sample. The resulting core samples consist

of 422 individuals in the East German sample including 192 individuals aged 58 to 60, and of 472

individuals in the West German sample including 228 individuals aged 58 to 60.

2.2 Descriptive Statistics

Tables 1 and 2 show some key characteristics of both samples and offer t-values of a t-test for

differences in sample means between individuals under and above the age threshold 58. Apart from

German nationality in the West German sample, none of the characteristics (except, of course, age)

show statistically significant differences in sample means between the two groups.

Note, that compared to the West German sample, the unemployment rate per district shortly

before entering welfare was almost double as high for East Germans (21% vs. 11%), mirroring the

worse situation on the East German labor market in that period. This labor market situation is related

to a higher share of participants among the eligibles: In East Germany, 63% of the eligibles aged 58 to

60 had opted for RULE58 when they were interviewed; in West Germany, this share was smaller

(46%).

A look in our data reveals that indeed, both participants and non-participants of RULE58 are

less closely monitored than non-eligible individuals younger than 58 years (Table 3). The Table

shows by eligibility and participation status, how many sample members reported to have received

certain types of search requirements, monitoring and counseling during 2005 – for instance, if they

signed an individual action plan with search or qualification elements, or if a job or qualification

measure was offered to them by the employment office. The shares are given for non-eligible

individuals between 56 and 57 years and eligible non-participants and participants between 58 and

60 years. Due to the limited sample sizes, we combined the East- and the West German sample for this Table.

The data reflects that under RULE58, participants can still receive job search assistance: for example, 41% of them reported that they talked about their job chances with their employment office and 19% reported that they received job search tips. Note the low shares of individuals over 57 years who received job offers compared to the higher level of work measures offered. This reflects the low probability to find a job with the help of the employment office over the age of 57. Both eligible non-participants and participants received less job offers and profiling measures than the younger group and concluded less often individual action plans. Since eligible participants have no need to cooperate with the employment office, this share is still lower in their group than in the group of eligible non-participants.

Our interest in this analysis is the effect of eligibility for RULE58 on mental health. Our measure of mental health uses two questions in the survey. The first question asked all individuals in the sample how often they (i) felt distressed, (ii) felt depressed, (iii) felt relaxed, (iv) felt full of energy, (v) were exhausted, (vi) could not cope with their normal stresses of life, (vii) were unusually aggressive, and (viii) had severe physical problems in the last 12 months prior to the interview.

The respondents could choose on a 1-to-5-scale between "*always*" and "*never*". We construct a dummy for 'good mental health' that takes the value 1 for all respondents who said they never felt mentally or physically distressed (answer 5 to question (i), (ii), (v)-(viii), answer 1 to question (iii) and (iv)). But this applies only to 1.9% of the East Germans and 0.9% of the West Germans in the sample.

The second question asked the remaining majority of respondents (who answered in a different way) "*How often did it happen in the last 12 months that you did not manage to do as much as you wanted in your job, the housework or other daily duties due to mental or emotional problems?*" Again,

the respondents could choose on a 1-to-5-scale between "*always*" and "*never*". The respondents who answered the question with "*rarely*" or "*never*" also received the value 1 for the outcome variable 'good mental health'.

Table 4 shows the answers of respondents on the 1-5-scale and describes our outcome variable. East Germans reported less often than West Germans that they underperformed due to mental problems. Turning to the dummy 'good mental health', Table 4 reveals that 64.0% of the East Germans and 51.9% of the West Germans reported 'good mental health'. In both samples, the share of people with good mental health state is higher for those aged 58 years and over.

3 Empirical Approach and Results

Not every eligible person participates in the program RULE58. Brussig and Wübbeke (2008) find that the motivation to opt for RULE58 is twofold: for some participants, RULE58 serves as a form of early retirement, while for others it serves as an opportunity to escape from the 'sticks' while enjoying the 'carrots' and continue to search for jobs.

One could therefore argue that RULE58 should only have an effect on participants, especially if some participants mainly see it as way to get into early retirement. However, we take a different stance on this issue. Key for this study is that a program like RULE58 changes the 'rules of the game' for all unemployed who become eligible, no matter if participating or not. Both unemployed and case workers know that the unemployed is eligible as soon as he or she turns 58 years.

Our hypothesis is that the probability for the unemployed to receive 'sticks' through the case worker and resulting conflicts are reduced as soon as the unemployed becomes eligible, since the unemployed can circumvent the 'sticks' by simply opting for RULE58. This reduced probability of 'sticks' for all eligible unemployed aged 58 and over might have a positive effect on their mental

health if the design of unemployment benefit schemes matters for the well-being of unemployed at all.

In our analysis, we split the sample in East and West Germany. The reason for doing so is, that the survey was conducted in winter 2005/06 when the probability of finding a job in West Germany was considerably higher as in East Germany because of the lower unemployment rates (see Section 2).

In a regime with low unemployment, it is plausible that 'sticks' are seen as less threatening for the unemployed since it is easier to show adequate search efforts to the case worker. In addition, the unemployed will probably undertake the search efforts with a stronger motivation because the success probabilities are higher.

In East Germany, on the other hand, employment probabilities are low. In this environment there are only low returns to searching resulting in more motivational problems. In this environment, 'sticks' in activation play a more dominant role for the unemployed possibly resulting in more conflicts with the case workers. Holding 'sticks' in activation constant, we therefore expect a bigger effect of a lifting of job search requirements, monitoring and sanctions on the mental well-being of East German unemployed.

Then again, case workers know that there are less job offers in East Germany and so they are more reluctant to issue sanctions against the unemployed (which is consistent with reports of the Federal Employment Office showing lower sanction rates in East Germany). Therefore, we compare two regimes: low unemployment/high sanctions in West Germany and high unemployment/low sanctions in East Germany. Given these considerations, it is not clear whether we expect a larger or smaller effect in East Germany.

Our main empirical approach exploits the discontinuity in the eligibility for RULE58. Since

the regression discontinuity (RD) model that is applied in Chapter 5 is a fuzzy RD design, we will

explain the sharp RD design in the following explicitly.

Consider the regression model:

$$Y_{ia} = \beta_0 + \beta_1 ELIGIBLE_{ia} + \delta_{ia}(a) + \varepsilon_{ia} \qquad (1)$$

where Y_{ia} is an outcome variable for individual i of age a. The effect of age on the outcome

variable is captured by the function $\delta(a)$, while $ELIGIBLE_{ia}$ is a dummy that captures eligibility for

RULE58 at age 58. It is defined as

$$ELIGIBLE_{ia} = \begin{cases} 0...if...a < 58, \\ 1...if...a \geq 58. \end{cases} \qquad (2)$$

The evaluation problem consists of estimating the effect β_1 of eligibility on the outcome

variable good mental health. The key identification assumption that underlies the regression

discontinuity (RD) strategy is that $\delta(\cdot)$ is a continuous function of the regression variable (age).

Under this assumption, β_1 is the average causal effect for the threshold population of

individuals eligible for RULE58 (Hahn et al. 2001). How reasonable is this assumption? Variables of

interest like income exhibit well-known age profiles. For instance, log earnings are a concave function

of age, which is consistent with a standard model of investment in human capital (e.g. Mincer 1974).

So while it is important to let $\delta(\cdot)$ be flexible enough to accommodate non-linearities in the

age-profiles, there is no reason to expect an abrupt chance at age 58. In Section 3.2, we provide

evidence that important background characteristics such as marital status or previous unemployment

duration as well as regional labor market characteristics like the local rate and duration of

unemployment are balanced around age 58.

Note that age is a discrete variable in our data since it is measured in months. This introduces

a specification error in our model (1). Random specification error causes heteroskedasticity in the

variance-covariance matrix of the error term. We address this issue by clustering on the age cells (Lee and Card 2008).

In practice, the estimated treatment effect depends on how the smooth function $\delta(\cdot)$ is itself estimated. As in any non-parametric estimation problem, there is a difficult trade-off between precision and bias. We balance this trade-off between precision and bias by estimating a variety of polynomial specifications for the regression function $\delta(\cdot)$. We present estimates of the treatment effect using different specifications for the regression function. The specifications include standard linear and quadratic functions, as well as linear splines (separate regressions on both sides of the discontinuity).

In addition, we apply a semiparametric version of the linear spline model by estimating local linear regressions on both sides of the discontinuity. Intuitively, the local linear regression weighs data such that information close to the discontinuity gets more weight than information further away from the discontinuity. In the empirical core analysis, we set the bandwidth to 2 years at the age discontinuity because of the limited size of our two samples. We also investigate sensitivity of estimates to the bandwidth choice. Specifically, we also use the bandwidths 1 year and 3 years at the age discontinuity.

We first present some graphical evidence before showing the regression results. Figure 1 plots the average values of the outcome variable over a set of age bins for East and West Germany, respectively. There seems to be no jump in mean mental health in West Germany at age 58. In contrast, in East Germany mental health jumps abruptly once individuals become eligible for RULE58. However, the improvement in mental health that we see for East German individuals 58 to 59 years vanishes for individuals 59 years and older.

Turning to the regression results, Table 5 shows the estimated treatment effect of eligibility for RULE58 on mental health, based on Equation 1. Our results indicate that RULE58 improves

mental health outcomes for individuals in Eastern Germany but not in Western Germany. Eligible individuals in Eastern Germany experience an increase by more than 20 percentage points in their probability to report being in good mental health.

One appealing feature of the results is that all parametric and semiparametric specifications yield very similar coefficient estimates, only the standard errors are different in the semiparametric specifications due to the larger weight of observations closer to the discontinuity. As the graphical evidence suggested, the treatment effect differs between East and West Germany.

In the Eastern part of the country, eligibility for RULE58 increases the probability to report good mental health by over twenty percentage points. The health impact is statistically significant at the 5% level in all six specifications. In the West German sample, we do not find that eligibility for RULE58 has any significant effect on the probability to report good mental health. The point estimates of the treatment effect in West Germany show even a negative sign, but all are statistically insignificant.

Since our age window contains observations with age values not too close to the threshold, we re-estimate the linear spline regression by including observed characteristics, in order to improve the efficiency of the estimates and remove small sample biases, arising for example through the sample differences regarding the share of foreigners and women (Imbens and Lemieux 2008). Then, the estimate in the West German sample gets smaller and stays insignificant. The estimate in the East German sample increases somewhat and stays statistically significant.

4 Robustness Checks and Extensions

In order to further check the robustness of our results, we offer in this section a selection of robustness checks and extensions to our core analysis.

4.1 Falsification Tests

We first run a series of "falsification experiments" in Table 6 to present further evidence on the robustness of our findings. The approach used here consists of testing for a zero effect in settings where it is known that the effect would be zero. We follow Imbens and Lemieux (2008) and test for jumps at the median of the two subsamples on either side of the discontinuity age 58. The specification used for all three models is the linear spline.

Table 6 indeed indicates a sharp contrast between the health effect in the East German sample of the discontinuity at age 58 when individuals become eligible for RULE58 and the "false" discontinuities where health effects are never significant and close to zero in both the East and the West German sample. It is reassuring that we fail to reject the null of a zero jump at age values away from age 58 because there is no theoretical reason why one would expect other discontinuities.

4.2 Testing for the Absence of Other Discontinuities

Predetermined characteristics such as nationality, income or gender that should not be affected by the eligibility but are likely to affect mental health should be continuous around the cut-off age 58. Otherwise, these discontinuities may affect our outcome mental health, and these effects may be attributed erroneously to our treatment eligibility for RULE58.

We test the continuity of X around the cut-off by testing if all the coefficients in separate regressions of $ELIGIBLE$ on X are zero. This would be additional evidence that observations on both sides of the cut-off are exchangeable and that they can be treated as randomly assigned to the treatment (see Imbens and Lemieux 2008). The specification used for all regressions on X is the linear spline.

The predetermined characteristics we consider are gender, German nationality, marital status, number of children below 18 years in the household, qualification in three categories (low, intermediate, high), and equivalent net monthly household income.

Table 7 presents the results, indicating that the causal effect of eligibility for RULE58 on X is close to zero and not statistically significant. We interpret these results as evidence for the assumption underlying our RD approach that there are no discontinuities at age 58 in other important variables that may affect health.

4.3   Different Specification of the Outcome Variable

In the core specification, the outcome variable is a dummy for "good mental health" that takes the value 1 if sample members report to have had rarely or never mental problems in their daily life during the last year, and otherwise zero. This outcome variable has the attractive feature that we can interpret the estimation results easily. Nevertheless, we lose information by constructing the outcome that way since in the original data individuals answer on a 1-5-scale if they had mental problems during the last year (1=always, 5=never).

In the following, we will check the robustness of our results by using this additional information and take the ordinal variable as our dependent variable. Turning to the regression results, Table 8 shows the estimated treatment effect of eligibility for RULE58 on mental health on a 1-5-scale. As in the core analysis, we present linear and quadratic polynomials, linear spline and local linear regressions on both sides of the discontinuity with bandwidth 1, 2 and 3 years, for East and West Germany, respectively.

Again, all parametric and semiparametric specifications yield very similar coefficient estimates and the treatment effect differs between East and West Germany. In the East German sample, eligibility for RULE58 increases mental health between 0.411 to 0.424 units.

The health impact is statistically significant at least at the 10% level in all six specifications. In the West German sample, we do not find that eligibility for RULE58 has any significant effect on the probability to report good mental health. As in our core analysis, the point estimates of the treatment effect in West Germany show a negative sign, but all are statistically insignificant.

5 Conclusion

In this study, we assessed whether eligibility for RULE58 improves mental health outcomes for unemployed. Since this group is in general in worse health than the group of the employed it is an important question how to improve (mental) health outcomes of unemployed. The design of unemployment benefit systems may play a large role, but recent research in economics has mainly focused on the transitions in and out of unemployment. Our study is among the first for Germany studying the effect of `sticks' in activation – of monitoring job search requirements of the unemployed, enforced by benefit sanctions – on (mental) health outcomes.

We do not find that mental health improves in West Germany after becoming eligible. At the same time, we document that East German individuals eligible for RULE58 are in better health than non-eligibles. In West Germany, we have relatively low unemployment and a high sanctions rate whereas in East Germany we have high unemployment and low sanctions rate.

This suggests that sanctions and other 'sticks' do not harm the unemployed in a situation when there is relatively low unemployment. Individuals can react to the potential threat of 'sticks' by showing adequate search efforts and thus avoiding conflicts and sanctions which is easier and more effective in a low unemployment environment. On the other hand, when unemployment is high, this is not so easy resulting in adverse effects of 'sticks' on the welfare of unemployed in a high unemployment environment.

Our findings indicate that 'sticks' in activation might affect the mental health of affected individuals and point to the need for more research on how the particular design of unemployment support systems affects the well-being of the unemployed. It follows as policy implication that there are some potential benefits to lifting strict monitoring- and sanction regimes for certain groups of unemployed individuals, arguably those who have only slight chances of finding regular jobs on their own.

Appendix: Tables and Figures

Table 1: Sample Characteristics (East Germany)

	Total		56 -<58 years		58-<=60 years		t-test for differences in sample means (t-values)
	Mean	SD	Mean	SD	Mean	SD	
Age in years	57.817	[1.204]	56.860	[0.592]	58.962	[0.596]	-36.225
Woman	0.564	[0.496]	0.600	[0.491]	0.521	[0.501]	1.634
German	0.962	[0.191]	0.961	[0.194]	0.964	[0.188]	-0.143
Married and living together	0.547	[0.498]	0.548	[0.499]	0.547	[0.499]	0.020
Number of children below 18 in household	0.050	[0.228]	0.061	[0.257]	0.036	[0.188]	1.094
Qualification[a]							
low	0.192	[0.395]	0.197	[0.398]	0.188	[0.391]	0.233
intermediate	0.672	[0.470]	0.681	[0.467]	0.661	[0.474]	0.429
high	0.135	[0.343]	0.122	[0.328]	0.151	[0.359]	-0.858
Equivalent self-reported net monthly hh income (Euros)	601.360	[224.315]	609.802	[275.970]	591.452	[141.585]	0.828
Duration of last unemployment spell (months)	53.748	[47.243]	53.523	[45.654]	54.011	[49.148]	-0.104
Local unemployment rate	20.661	[3.210]	20.798	[3.245]	20.497	[3.168]	0.959
Local unemployment duration	16.999	[2.092]	16.972	[2.068]	17.032	[2.127]	-0.292
N	422		230		192		

Source: Life Situation and Social Security 2005. **Notes:** a. Low qualification means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualifications means *(Fach-)Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, and high qualification means *(Fach-)Abitur* and apprenticeship, master craftsmen or university degree. The local unemployment figures refer to the month before entering welfare (December 2004).

Table 2: Sample Characteristics (West Germany)

	Total		56 -<58 years		58-<=60 years		t-test for differences in sample means (t-values)
	Mean	SD	Mean	SD	Mean	SD	
Age in years	57.876	[1.158]	56.898	[0.560]	58.924	[0.564]	-39.161
Woman	0.411	[0.493]	0.443	[0.498]	0.377	[0.486]	1.444
German	0.875	[0.331]	0.910	[0.287]	0.838	[0.370]	2.377
Married and living together	0.449	[0.498]	0.430	[0.496]	0.469	[0.500]	-0.849
Number of children below 18 in household	0.108	[0.395]	0.119	[0.442]	0.096	[0.338]	0.614
Qualification[a]							
low	0.309	[0.463]	0.316	[0.466]	0.303	[0.460]	0.303
intermediate	0.549	[0.498]	0.553	[0.498]	0.544	[0.499]	0.205
high	0.142	[0.349]	0.131	[0.338]	0.154	[0.361]	-0.695
Equivalent self-reported net monthly hh income (Euros)	630.229	[225.524]	641.848	[266.493]	617.789	[170.866]	1.154
Duration of last unemployment spell (months)	60.012	[57.812]	58.286	[54.691]	61.913	[61.145]	-0.651
Local unemployment rate	11.154	[3.083]	11.005	[2.963]	11.313	[3.206]	-1.084
Local unemployment duration	15.182	[3.500]	14.985	[3.437]	15.393	[3.563]	-1.267
N	472		244		228		

Source: Life Situation and Social Security 2005. **Notes:** a. Low qualification means no graduation or graduation from *Sonder-/Haupt- and Realschule* and no vocational training, intermediate qualifications means *(Fach-)Abitur* and no vocational training, or graduation from *Sonder-/Haupt- and Realschule* and apprenticeship, and high qualification means *(Fach-)Abitur* and apprenticeship, master craftsmen or university degree. The local unemployment figures refer to the month before entering welfare (December 2004).

Table 3: Job Search Requirements, Monitoring and Assistance by Eligibility and Participation Status (Shares of Recipients in % of All Respondents)

	56 -<58 years	58-<=60 years	
		Non-participant	Participant
Profiling	47.7%	28.0%	23.2%
Individual action plan with search requirements	18.6%	7.3%	6.6%
Individual action plan with qualification requirements	9.9%	6.2%	4.4%
Job Center talked about job chances	63.8%	47.6%	41.1%
Job Center gave job search tips	36.2%	25.4%	18.6%
Job Center advised to move for a job	48.3%	28.0%	19.7%
Job Center informed about qualification measures	34.5%	12.7%	5.6%
Full-time job offer	7.7%	3.2%	0.9%
Part-time job offer	6.6%	1.6%	0.4%
Mini job offer	5.4%	2.6%	0.9%
Vocational training offer	0.6%	0.5%	0.0%
Work measure offer	30.6%	28.9%	17.6%
N	474	193	227

Source: Life Situation and Social Security 2005.

Table 4: Description of the Mental Health State of the Samples

	East Germany			West Germany		
	Total	56 -<58 years	58-<=60 years	Total	56 -<58 years	58-<=60 years
			Mental health			
		Underperformance due to mental problems in the last 12 months: 1-5-scale				
always	2.1%	2.0%	2.5%	4.0%	4.1%	3.8%
often	13.3%	14.0%	11.6%	15.9%	15.6%	17.0%
sometimes	20.6%	20.9%	19.8%	28.2%	29.0%	25.5%
rarely	33.4%	29.9%	42.2%	22.0%	20.8%	26.4%
never	30.6%	33.2%	24.0%	29.9%	30.6%	27.4%
Dummy "good mental health"	64.0%	63.1%	66.1%	51.9%	51.4%	53.8%
N	422	301	121	472	366	106

Source: Life Situation and Social Security 2005. **Note:** The dummy "good mental health" is 1 if people said they had "rarely" or "never" underperformed due to mental/physical problems in the last 12 months.

Table 5: Regression Discontinuity Estimates of the Effect of Eligibility for RULE58. Dep. Variable: Dummy for "Good Mental Health"

Specification for age	East Germany	West Germany
Mean of the dependent variable		
	0.640	0.519
Regression discontinuity estimates		
Linear spline	0.232**	-0.076
	[0.115]	[0.079]
Linear	0.232**	-0.077
	[0.117]	[0.078]
Quadratic	0.233**	-0.077
	[0.115]	[0.078]
Local linear (bw 2 years)	0.230**	-0.073
	[0.089]	[0.098]
Local linear (bw 1 years)	0.220**	-0.061
	[0.099]	[0.099]
Local linear (bw 3 years)	0.231**	-0.075
	[0.094]	[0.095]
With Covariates		
Linear spline	0.261**	-0.030
	[0.112]	[0.082]
N	422	472

Source: Life Situation and Social Security 2005. **Notes:** Results show RD estimates of eligibility for RULE58, i.e. 1(age>=58), on the dummy "good mental health". Age is measured in months. The age width around the threshold age 58 is +/- 24 months. Standard errors in parentheses. Standard errors of parametric regressions are clustered by age cells (Lee and Card 2008). Standard errors of local linear regressions are bootstrapped (reps. 100). The symbols *, **, and *** indicate statistical significance at the .1, .05, and .01 levels, respectively. With covariates: We control additionally for gender, German nationality, marital status, number of children below 18 years in the household, qualification in three categories (low, intermediate, high), equivalent net monthly household income, duration of last unemployment spell (months), mean unemployment rate and duration per district.

Table 6: Falsification Test: Linear Spline Regression Discontinuity Estimates with "False" Age Discontinuities

Discontinuity point	East Germany	West Germany
Eligibility for RULE58 (age=58)	0.232**	-0.076
	[0.115]	[0.079]
N	422	472
Median age of the lower subsample	-0.039	0.027
	[0.078]	[0.096]
N	504	540
Median age of the upper subsample	-0.061	-0.017
	[0.116]	[0.087]
N	333	423

Source: Life Situation and Social Security 2005. **Notes:** Results show RD estimates of different discontinuities on the dummy "good mental health". Age is measured in months. The age width around the threshold age 58 is +/- 24 months. Standard errors in parentheses. Standard errors of parametric regressions are clustered by age cells (Lee and Card 2008). The median in the lower subsample is 56.8 years and in the upper subsample 59.9 years.

Table 7: Test on Discontinuities: Linear Spline Regression Estimates of the Effect of Eligibility for RULE58 on Other Health-Related "Pseudo Outcomes" around Discontinuity Age 58

Pseudo Outcome	East Germany	West Germany
Gender	0.014	0.007
	[0.117]	[0.094]
German	-0.002	-0.111
	[0.040]	[0.057]
Married and living together	-0.103	0.054
	[0.107]	[0.097]
Number of children below 18	-0.029	0.019
	[0.027]	[0.070]
Qualification[a]		
Low qualified	0.044	0.031
	[0.059]	[0.075]
Intermediate qualified	-0.017	0.005
	[0.003]	[0.071]
High qualified	-0.027	-0.037
	[0.061]	[0.077]
Equivalent net monthly hh income (Euros)	-15.336	-2.073
	[21.751]	[28.564]
N	422	472

Source: Life Situation and Social Security 2005. **Notes:** Results show RD estimates of eligibility for RULE58, i.e. 1(age>=58), on the respective pseudo outcome. Age is measured in months. Standard errors in parentheses. Standard errors of parametric regressions are clustered by age cells (Lee and Card 2008). The symbols *, **, and *** indicate statistical significance at the .1, .05, and .01 levels, respectively.

Table 8: Regression Discontinuity Estimates of the Effect of Eligibility for RULE58. Dep. Variable: Mental Health on 1-5-Scale (1=Worst)

Specification for age	East Germany	West Germany
Mean of the dependent variable		
	3.740	3.570
Regression discontinuity estimates		
Linear spline	0.416*	-0.124
	[0.231]	[0.189]
Linear	0.416*	-0.124
	[0.239]	[0.186]
Quadratic	0.424*	-0.124
	[0.233]	[0.187]
Local linear (bw 2 years)	0.415**	-0.120
	[0.192]	[0.223]
Local linear (bw 1 years)	0.411*	-0.100
	[0.223]	[0.243]
Local linear (bw 3 years)	0.416**	-0.122
	[0.197]	[0.243]
With Covariates		
Linear spline	0.416*	-0.044
	[0.229]	[0.201]
N	422	472

Source: Life Situation and Social Security 2005. **Notes:** Results show RD estimates of eligibility for RULE58, i.e. 1(age>=58), on mental health on a 1-5-scale (1=worst). Age is measured in months. The age width around the threshold age 58 is +/- 24 months. Standard errors in parentheses. Standard errors of parametric regressions are clustered by age cells (Lee and Card 2008).Standard errors of local linear regressions are bootstrapped (reps. 100). The symbols *, **, and *** indicate statistical significance at the .1, .05, and .01 levels, respectively. With covariates: We control additionally for gender, German nationality, marital status, number of children below 18 years in the household, qualification in three categories (low, intermediate, high), equivalent net monthly household income, duration of last unemployment spell (months), mean unemployment rate and duration per district.

Figure 1: Mental Health by Age around the Discontinuity Age 58
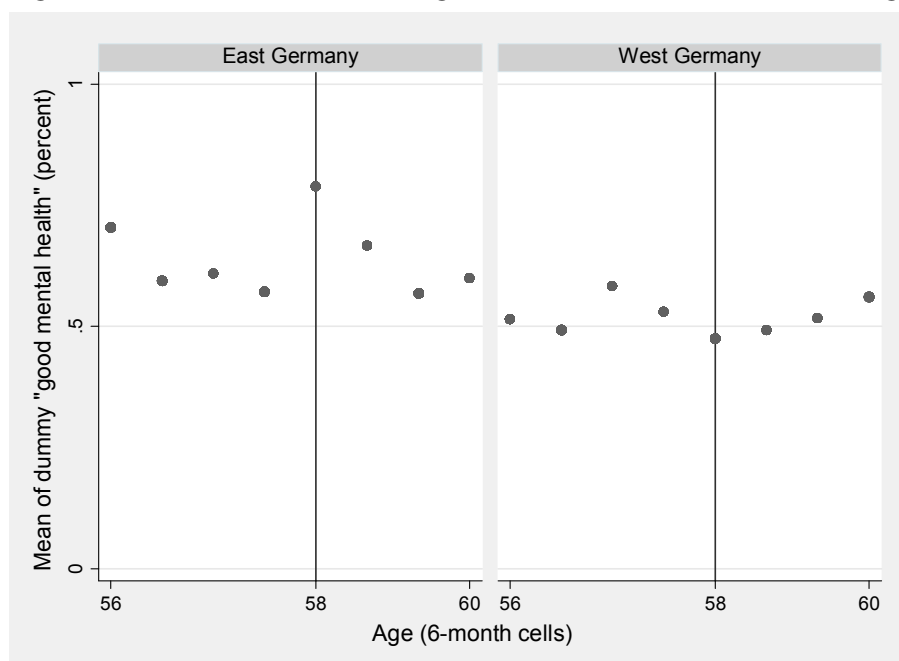
Source: Life Situation and Social Security 2005. **Notes:** The discontinuity point age 58 is marked with a vertical line. Each scatter plot shows the mean outcome by age cell. The width of the age cells has to be large enough to have a sufficient amount of precision, hence each age cell represents all individuals whose age lies within half a year due to the small number of cases in the monthly age cells; for example, the age cell "58" represents all individuals who are 58 to 58.5 years at the interview.

CHAPTER 7

CONCLUSION AND OUTLOOK

1 Conclusion

During the past two decades, European governments have been implementing activating labor market policies to reduce high and increasing unemployment rates and levels of structural unemployment. So did Germany: between 2003 and 2005, Germany strengthened the activation of unemployed and especially of long-term unemployed through the *Hartz-Reforms*. But the reduction of unemployment remains one of the major challenges of Germany and its neighbor countries.

This dissertation contributes to the ongoing discussion among researchers and politicians about the effects and effectiveness of activating unemployed individuals. In particular, it analyzes the impact of selected activation policies on job search behavior (measured by reservation wages and search effort), on re-employment probabilities and on mental health of welfare recipients in Germany.

The four empirical studies in this dissertation are among the first to study the effects of activation on behavioral and health outcomes in Germany. The studies are based on a German cross-sectional survey called "Life Situation and Social Security 2005" applying statistical matching and regression discontinuity models. In the following, the principal findings and conclusions of the four studies are presented.

Chapter 3 first analyzes the impact of benefit sanctions on job search behavior in a partial job search model. Accordingly, the benefits of remaining unemployed decrease if the costs of job search associated with benefit sanctions increase, thereby reducing reservation wages and increasing search intensity, and ultimately shortening the duration of unemployment. For the empirical analysis of

sanctions on reservation wages, search effort and re-employment probabilities, statistical matching for single treatments is applied. Estimation results show that benefit sanctions have no effect on reservation wages or on search effort even if the sanction was received shortly before the interview. Results for reservation wages are in line with previous research on the unemployment income elasticity of reservation wages. Results on the probability to find unsubsidized employment suggest that benefit sanctions substantially increase this probability. This effect is larger for those who received a sanction earlier in their welfare spell. This result is in line with previous research on both UI and welfare sanctions.

On the contrary, sanctions do not seem to increase (probably low-paid) employment with additional welfare receipt. Results therefore suggest that sanctions increase the probability to receive a regular job offer for a given search effort, maybe because case workers increase the counseling after a sanction. Yet, one should be careful in applying the results on welfare sanctions to UI benefit sanctions since UI benefit recipients are typically better attached to the labor market. Reservation wages, for instance, of UI recipients are generally higher than those of welfare recipients so UI recipients might show a larger decrease in their reservation wages due to receiving a benefit sanction.

The theoretical framework of Chapter 4 is also a partial job search model. Here, theoretical predictions on the effects of individual action plans (IAP) on reservation wages, search intensity and exit rates to employment were derived. Accordingly, the job search requirements of individual action plans should raise search intensity and reduce reservation wages (the so-called compulsion effect). The qualification elements should raise search efficiency hence the probability to receive a job offer per given search intensity rises or the cost of job search goes down and search intensity as well as reservation wages go up (the so-called support effect).

Therefore, the net effect of IAP on search intensity is determined and positive while the net effect on reservation wages is ambiguous. The ambiguous net effect on reservation wages is

responsible for the equally ambiguous net effect on exit rates to employment, depending on the dominance of supportive and compulsory elements in the IAP.

For the empirical analysis of individual action plans in general and of different types of individual action plans on reservation wages, search effort and re-employment probabilities statistical matching for single and multiple treatments is applied. Estimation results suggest that individual action plans substantially increase search effort and slightly reduce reservation wages of welfare recipients who signed such a plan. Therefore we can conclude that on average the compulsion effect of individual action plans dominates their support effect.

However, the increased search effort and reduced reservation wages do not translate into a higher probability of employment for participants at the time of the interview. Instead, participants with individual action plans are much more likely to be enrolled in a work measure when interviewed. Locking-in effects seem to delay exits to employment. A question that needs to be addressed in future research is if the higher participation in work measures that was found in this dissertation improves the re-employment probabilities in a longer observation period.

Apart from this question, increased search effort does not automatically translate into better employment chances. If this increase in search effort measures a substitution of informal search effort with formal effort and informal job search is more effective than formal job search, IAP may easily have a perverse effect on re-employment probabilities, as Van den Berg and Van der Klaauw (2006) suggest for general monitoring of UI recipients.

A further result is that the design of an IAP matters: IAP with search requirements significantly increase search effort and reduce reservation wages. In contrast, IAP with just qualification elements have no significant effect on reservation wages or search effort. This might be due to the particular high enrollment in work measures that we find for those participants with IAP with just qualification elements. This high enrollment in work measures is also reflected in a lower

probability for them to be in employment, suggesting that a locking-in effect exists especially for this type of IAP. But even welfare recipients with IAP with search requirements did not have a greater re-employment probability.

In order to address the impact of potentially unwelcome 'sticks' in activation – like monitoring and benefit sanctions – on the job search behavior and on mental well-being, Chapters 5 and 6 study effects of a 'de-activating' regulation called RULE58 that until 2008 offered unemployed recipients of UI and welfare benefits older than 57 years the possibility to receive benefits and support while being exempted from 'sticks' in activation.

Chapter 5 analyzes the effect of participation in RULE58 on reservation wages of participating welfare recipients using fuzzy regression discontinuity design. Assuming that 'sticks' in activation have no skill-enhancing effect (unlike job search assistance and qualification measures for instance) but do reduce reservation wages since they decrease the utility of staying unemployed, one would expect to see an increase in reservation wages for participants of RULE58. Estimation results show that participation in RULE58 significantly increases reservation wages. This result suggests that job search monitoring, enforced with benefit sanctions, ('sticks') can indeed affect the job search behavior by reducing reservation wages.

Yet, one should be cautious in applying the size of the found Local Average Treatment Effect (LATE) to the whole population of unemployed. Given that one intention of programs like RULE58 was the 'de-activation' of elderly unemployed and that participants are at least 58 years old, the motivation to participate can be two-fold: some individuals may decide to keep looking for a job without the strict monitoring rules, and some individuals may decide to enroll in the program as a way of entering inactivity and stop job searching. Thus, reservation wages of unemployed with a closer labor market attachment might increase to a lower degree if 'sticks' in activation were lifted for them.

The last empirical Chapter 6 analyzes how the option to circumvent 'sticks' and potential conflicts with the employment office by participating in RULE58 affects the mental health of elderly welfare recipients using a sharp regression discontinuity design. The underlying assumption in this study is that RULE58 reduces the probability to be treated with job search requirements, monitoring and benefit sanctions – 'sticks' – for *all* recipients of unemployment benefits who turn 58 years.

If 'sticks' can affect the mental well-being at all – for example via an increase in distress caused by strict monitoring or the threat of sanctions and potentially resulting conflicts with the employment office –, we would expect to find a positive effect of these reduced probabilities to be treated with 'sticks' on mental health once people turn 58 years. In the analysis, East and West Germans are analyzed separately since the chances to find a job are lower for elderly East German welfare recipients, at least for the period in question (2005). The assumption is that the lower labor market chances make 'sticks' less effective for East Germans for finding a new job, hence the effect of 'sticks' might be worse for their mental health in comparison to West Germans in the same age group.

Estimation results do not show that mental health improves in West Germany after becoming eligible. At the same time, results document that East German individuals eligible for RULE58 are in better health than non-eligibles. This suggests that 'sticks' do not harm the unemployed in a situation when there is relatively low unemployment. Individuals can react to the potential threat of 'sticks' by showing adequate search efforts which is easier in a low unemployment environment. On the other hand, when unemployment is high, this is not so easy resulting in adverse effects of 'sticks' on the mental well-being of unemployed in a high unemployment environment. This suggests that there are some potential benefits of lifting 'sticks' for unemployed in high unemployment regions.

2 Policy Implications

The policy implications which can be drawn from the results of these analyses are the following. The analysis of benefit sanctions showed that benefit sanctions indeed increase the probability to find unsubsidized employment. However, changes in reservation wages or search effort of welfare recipients due to benefit sanctions were not found, even if the benefit sanction was imposed shortly before the interview.

Regarding the level of reservation wages the welfare recipients wished to earn in their next job, most paid wages in Germany are above this level suggesting that the arrival rate of job offers is crucial for welfare recipients staying unemployed. This does not point to the need for stricter benefit sanctions in order to reduce excessive reservation wages but much more to an increase of the intensity of individualized counseling in order to raise the arrival rate of job offers.

On the other hand, according to results presented in this dissertation, individual action plans – for an observation period between one and eleven months after treatment was imposed – were not effective in increasing the probability of welfare recipients to find employment, even though participants had increased their search efforts and slightly reduced their reservation wages. Instead, participants had entered work measures to a considerably higher degree. This indicates that the job search efficiency of welfare recipients was not improved. A closer look at the data revealed that the concluded individual action plans were very general and standardized and not based on individualized counseling.

As policy implication, it seems desirable to individualize these plans to a higher degree and develop optimal individual search strategies in order to translate the measured increases in monitored search effort into more job offers and higher job search efficiency. This policy implication is substantiated by the result that the compulsion effect of individual action plans is stronger than the support effect.

Even if the found increase in search effort measures a substitution of informal search effort with formal effort, the policy implication would be to concentrate on individual counseling. In this case, one would want to decrease the monitoring of formal search effort in order to avoid the substitution of more effective with less effective search channels.

Results did not show an economically significant reaction of reservation wages to individual action plans and benefit sanctions. This suggests that reservation wages of welfare recipients are not very elastic towards specific activation instruments. The results on the effect of eliminating the whole set of 'sticks' in activation – job search requirements, monitoring and benefit sanctions – for older welfare recipients due to the regulation RULE58, however, reveal a large increase of reservation wages. This result tells us that without the 'sticks' in activation reservation wages of welfare recipients would be higher. Thus, job search monitoring, enforced with benefit sanctions, can be used as policy measures to decrease potentially excessive reservation wages.

Finally, 'sticks' in activation seem do not seem to be generally detrimental to the mental health of welfare recipients. East Germans, however, on average show better health as soon as they have a lower probability of being treated with 'sticks' due to turning 58 years. This is possibly due to their lower labor market chances. This result indicates that 'sticks' in activation might affect the mental health of affected individuals. A policy implication is that there are some potential benefits to lifting strict monitoring- and sanction regimes for at least some unemployed persons, arguably those who have only slight chances of finding regular jobs on their own.

3 Future Research

This dissertation showed for selected activation policies that activation can affect the job search behavior via increased job search efforts and via slightly reduced reservation wages. However, effects

vary, the design of these policies matters and changes in the job search behavior do not automatically translate into increased job search success.

Very important for future research is the question which design of activation policies is especially suited to affect the job search behavior and increase the success of job search. A further useful extension of the results presented in this dissertation would be to test the effect of activation policies on the job search channels that individuals use because job search channels might differ in their effectiveness.

Next, it seems probable that activation policies yield heterogeneous effects, depending for example on the personal arrival rate of job offers. This insight points to the need for studies that can use larger data sets to analyze potentially heterogeneous impacts of activation policies on different types of activated individuals, but also potentially heterogeneous impacts of different types and designs of activation policies.

Furthermore, until now there is only little literature on the effect of specific activation instruments on job search behavior or subjective well-being at all. It would therefore be important to dispose over studies of other countries to compare the results and in order to see whether a policy – like the individual action plan that exists in almost every European country – works differently under a different institutional framework or labor market situation.

Also, shortcomings rooting in the dataset should be addressed in future research. Since the data used is a cross-sectional survey, it was not possible to observe the outcomes before and immediately after welfare recipients were subject to a certain treatment. Thus, it is not possible to completely rule out the effect of changes in the search behavior due to people having taken up employment. Therefore, future research should explore panel data to assess the impact of activation in order to deal with (fixed) unobserved heterogeneity and time-varying variables.

In addition, the results presented here stem from a very specific period, characterized by a depressed labor market situation and the implementation period of *Hartz IV*. The implementation of the policies by the *Job Centers* may have been driven by lack of personnel and insufficient experience both with their clients and with the policies at hand. The latter implies that some trial-and-error-approach might have been going on during the implementation period.

Also, if the labor demand in an economy is higher activation policies might be more effective in re-integrating unemployed into the labor market. Hence, it is certainly worth studying the effects of activation policies on the behavior of welfare recipients again during another time period in which German *Job Centers* and its labor market were in a much better position than they were in the year 2005.

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DEUTSCHE ZUSAMMENFASSUNG DER DISSERTATION

Einführung und zentrale Ergebnisse

Während der letzten beiden Jahrzehnte setzten Regierungen in Europa verstärkt auf das Primat einer aktivierenden Arbeitsmarktpolitik, um die Problematik eskalierender Arbeitslosigkeitszahlen und leerer öffentlicher Kassen in den Griff zu bekommen. Deutschland bildet hier keine Ausnahme: zwischen 2003 und 2005 stärkte die deutsche Regierung die Aktivierung von Arbeitslosen, und speziell von Langzeitarbeitslosen, durch die Hartz-Reformen.

Aktivierung ist keine singuläre arbeitsmarktpolitische Maßnahme, sondern ein System wechselseitiger Verpflichtungen zwischen dem arbeitslosen Individuum auf der einen Seite und dem Wohlfahrtsstaat auf der anderen Seite, ein Prozess aus fördernden und fordernden Maßnahmen und Handlungen. Bildlich gesprochen, zielt Aktivierungspolitik auf eine ausgewogene Mischung aus 'Zuckerbrot' und 'Peitsche'. Finanzielle Leistungen und Hilfe bei der Arbeitssuche werden als 'Zuckerbrot' im Austausch gegen den mandatorischen und sanktionsbewehrten Nachweis aktiver Arbeitssuche ('Peitsche') gewährt. Dennoch bleibt auch im Jahr 2009, vier Jahre nach Einführung der letzten der vier Hartz-Reformen, die Bekämpfung von Arbeitslosigkeit eine der größten Herausforderungen für Deutschland und seine Nachbarstaaten.

Diese Dissertation leistet einen Beitrag zur aktuellen Diskussion innerhalb der Forschungsgemeinschaft und der Politik über die Effekte und die Effektivität der Aktivierung arbeitsloser Individuen. Welche Wirkungen insgesamt und durch Einzelkomponenten von Aktivierung ausgelöst werden, ist in der Forschung bislang nur unzureichend geklärt. Weitgehend unbeleuchtet sind besonders die Effekte von Aktivierung auf das Arbeitssuchverhalten aktivierter Arbeitsloser, also auf deren Anspruchs- oder Reservationslöhne sowie deren Suchanstrengungen,

und auf die Effektivität dieser Suche im Sinne der Anzahl erhaltener Arbeitsangebote bei gegebener Suchanstrengung. Vernachlässigt wurden in der Literatur auch bislang weitergehende Effekte von Aktivierung auf die Gesundheit und das Wohlbefinden aktivierter Individuen.

Konkret untersucht die vorliegende Dissertation die Effekte ausgesuchter Aktivierungskomponenten auf Empfänger/innen der 2005 eingeführten Leistung Arbeitslosengeld II (ALG II), die aufgrund der mehrheitlich vorliegenden Langzeitarbeitslosigkeit und Abhängigkeit von Transferleistungen durch die umfassendste der Hartz-Reformen, die Hartz IV-Reform, besonders stark aktiviert werden sollen.

Die Dissertation beschäftigt sich auf der Grundlage von vier mikroökonometrischen Ex-Post-Evaluationsstudien mit den folgenden Fragen. Zum ersten: Wie beeinflussen zentrale Aktivierungskomponenten wie leistungskürzende Sanktionen und maßgeschneiderte Eingliederungsvereinbarungen das Arbeitssuchverhalten von ALG II-Empfänger/innen? Zum zweiten: Welche Effekte auf deren Wiederbeschäftigungswahrscheinlichkeit zeitigen diese Aktivierungskomponenten? Zum dritten: wie verändert sich das Arbeitssuchverhalten, wenn ALG II-Empfänger/innen nicht mehr die sanktionsbewehrte Verpflichtung haben, ihre aktive Arbeitssuche unter Beweis zu stellen, sondern das ‚Zuckerbrot', das heißt, finanzielle Leistungen und Unterstützung bei der Arbeitssuche, ohne die sonst obligatorische ‚Peitsche' im Sinne der sanktionsbewehrten Kontrolle ihrer Suchanstrengungen erhalten? Viertens: Zeigen sich bei jenen ALG II-Empfänger/innen, die ‚Zuckerbrot' ohne ‚Peitsche' erhalten, positive Effekte auf ihr mentales Wohlbefinden?

Die Datengrundlage aller vier empirischen Studien ist eine Querschnittsbefragung mit dem Titel „Lebenssituation und soziale Sicherung 2005", die im Winter 2005/2006, ein Jahr nach Einführung von Hartz IV, vom Institut für Arbeitsmarkt- und Berufsforschung in Auftrag gegeben wurde. In dieser Befragung finden sich reichhaltige Informationen zu über 15.000 Personen, die im

Januar 2005 arbeitslose Empfänger/innen des neu eingeführten Arbeitslosengeldes II waren. Die interessierenden Effekte der betrachteten Aktivierungskomponenten wurden mittels statistischer ‚Matching' Modelle und ‚Regression Discontinuity' Modelle geschätzt.

Ziel dieser deutschen Zusammenfassung ist es, eine Gesamtschau über die Forschungsergebnisse zu geben, die in der Dissertation in den oben angerissenen Forschungsfragen erbracht wurden. Eine gründliche Darlegung der Definition und Zielsetzung von Aktivierung, der Umsetzung des Aktivierungsgrundsatzes durch die Hartz-Reformen und im speziellen durch Hartz IV, der verwendeten Datenbasis sowie der angewendeten empirischen Methoden findet sich im Langtext.

Die erste empirische Studie wird in Kapitel 3 der Dissertation dargestellt. Die Studie analysiert den Effekt von leistungskürzenden Sanktionen auf das Arbeitssuchverhalten sanktionierter ALG II-Empfänger/innen. In einem ersten Schritt werden auf der Grundlage eines theoretischen, partiellen Arbeitssuchmodells Hypothesen zu den erwarteten Wirkungen einer solchen Sanktion auf das Arbeitssuchverhalten arbeitsloser Individuen aufgestellt. Demnach sinkt der Nutzen eines Verbleibens in der Arbeitslosigkeit durch die sanktionsbedingt erhöhten Kosten der Arbeitssuche in Arbeitslosigkeit. Dadurch verringern sich die Reservationslöhne und steigt die Suchintensität, was schließlich zu einer Verkürzung der Arbeitslosigkeitsdauer führt.

In der empirischen Analyse werden mittels eines statistischen ‚Matching' Modells der Effekt von Sanktionen auf Reservationslöhne, Suchanstrengungen und die Wiederbeschäftigungswahrscheinlichkeit geschätzt. Die Schätzergebnisse zeigen, dass leistungskürzende Sanktionen keinen Effekt auf Reservationslöhne oder Suchanstrengungen bewirken, selbst, wenn die Sanktion kurz vor dem Interview, an dem Reservationslöhne und Suchanstrengungen abgefragt wurden, ausgesprochen wurde. Damit korrespondieren die Ergebnisse mit Resultaten aus vorherigen Studien zur geringen empirischen Elastizität von Reservationslöhnen

gegenüber der Höhe der gezahlten Leistungen in Arbeitslosigkeit. Des Weiteren zeigen die Schätzergebnisse der vorliegenden Studie, dass Sanktionen die Wahrscheinlichkeit, unbezuschusste, reguläre Arbeit zu finden, signifikant erhöhen.

Dieser Effekt ist besonders ausgeprägt für jene Gruppe von ALG II-Empfänger/innen, die frühzeitig in ihrem Leistungsbezug sanktioniert wurden. Auch dieses Ergebnis korrespondiert mit Resultaten früherer Studien zu den positiven Beschäftigungseffekten von Sanktionen besonders in einem frühzeitigen Stadium des Leistungsbezugs, und zwar sowohl für Sanktionen von arbeitslosen Empfänger/innen von Leistungen der Arbeitslosenversicherung als auch von jenen armutsvermeidender, steuerfinanzierter Sozialleistungen.

Im Gegenzug dokumentieren weitere Schätzergebnisse der vorliegenden Studie keinen Effekt der ausgesprochenen Sanktionen auf die Wahrscheinlichkeit, eine niedrig entlohnte Beschäftigung zu finden, bei der ein gleichzeitiger ALG II-Bezug notwendig bleibt ('Aufstockung'). Die Schätzergebnisse legen daher nahe, dass Sanktionen die Wahrscheinlichkeit erhöhen, ein reguläres Arbeitsangebot zu erhalten und zu akzeptieren bei gleicher Suchanstrengung und gleichem Reservationslohn, möglicherweise erzielt durch eine intensivierte Beratungsleistung der Sachbearbeiter/innen in den Job Centern und gesunkene nicht-monetäre Joberwartungen.

Es empfiehlt sich, bei der Verallgemeinerung dieser Ergebnisse von Effekten von ALG II-Sanktionen auf Effekte von Sanktionen für Empfänger/innen von Leistungen der Arbeitslosenversicherung (Arbeitslosengeld (ALG) I in Deutschland) Vorsicht walten zu lassen, da ALG I-Empfänger/innen im Allgemeinen eine größere Beschäftigungswahrscheinlichkeit als ALG II-Empfänger/innen aufweisen. So sind zum Beispiel auch die Reservationslöhne jener ersten Gruppe generell höher und damit vielleicht beweglicher nach unten, was dazu führen könnte, dass sanktionierte ALG I-Empfänger/innen im Gegensatz zu sanktionierten ALG II-Empfänger/innen ihre Reservationslöhne durchaus senken.

Die zweite empirische Studie findet sich in Kapitel 4 der vorliegenden Dissertation. Die Studie analysiert den Effekt von Eingliederungsvereinbarungen auf die Reservationslöhne, die Suchanstrengungen und die Wiederbeschäftigungswahrscheinlichkeit von ALG II-Empfänger/innen, die eine solche Vereinbarung mit ihren Sachbearbeiter/innen in den Job Centern eingegangen sind. Wie in Kapitel 3 werden die der empirischen Analyse zugrundeliegenden Hypothesen aus einem partiellen Arbeitssuchmodell abgeleitet.

Demzufolge sollten in Eingliederungsvereinbarungen (EGV) verbindlich festgehaltene Suchanforderungen die Suchintensität erhöhen und den Reservationslohn senken (der sogenannte ‚Zwang-Effekt'). Umgekehrt sollten vereinbarte Qualifizierungsangebote die Sucheffizienz, das heißt, die Wahrscheinlichkeit, bei gleicher Suchanstrengung ein Arbeitsangebot zu erhalten, steigern oder die Kosten der Arbeitssuche verringern, was einen Anstieg der Suchintensität und der Reservationslöhne zur Folge hat (der sogenannte ‚Unterstützungs-Effekt'). Daher ist der Nettoeffekt von EGV auf die Suchintensität klar positiv, während der Nettoeffekt von EGV auf die Reservationslöhne unbestimmt bleibt.

Letzteres führt zu einem ebenfalls theoretisch unbestimmten Effekt auf die Abgänge aus der Arbeitslosigkeit, der eben von der Ausprägung von Zwangs- und Unterstützungskomponenten in den betrachteten EGV abhängt. In der empirischen Analyse werden mittels statistischer ‚Matching' Modelle EGV im Allgemeinen und spezifische Typen von EGV (EGV mit reinen Zwangskomponenten, mit reinen Unterstützungskomponenten, und die Mischform) untersucht. Die Schätzergebnisse zeigen, dass EGV im Allgemeinen zu einem substanziellen Anstieg der Suchanstrengungen führen, und auch die Reservationslöhne leicht senken.

Daher können wir schlussfolgern, dass bei EGV im Durchschnitt der Zwang-Effekt überwiegt. Die erhöhten Suchanstrengungen und leicht verringerten Reservationslöhne führen jedoch zu keiner größeren Wiederbeschäftigungswahrscheinlichkeit. Stattdessen sind ALG II-

Empfänger/innen mit EGV sehr viel wahrscheinlicher in sogenannten Ein-Euro-Jobs beschäftigt, bei denen Teilnehmer/innen für drei bis sechs Monate zwischen 15 und 30 Stunden die Woche einer gemeinnützigen Tätigkeit nachgehen, für die sie eine sogenannte Mehraufwandsentschädigung zwischen einem und drei Euro jenseits ihres ALG II erhalten. Ein-Euro-Jobs sollen Teilnehmer/innen für eine Einstellung auf dem ersten Arbeitsmarkt qualifizieren.

Zumindest kurzfristig – ein bis elf Monate nach Unterzeichnung der EGV – scheinen Ein-Euro-Jobs allerdings bei den Teilnehmer/innen ‚Verharrungseffekte' (‚locking-in effects') auszulösen, die einen Einstieg in reguläre Beschäftigung verzögern. Ob dieser Einstieg langfristig über die durch die EGV gestiegene Teilnahme an Ein-Euro-Jobs besser gelingt, sollte in zukünftigen Studien untersucht werden. Abgesehen von dieser Forschungsfrage, müssen sich gestiegene Suchanstrengungen auch theoretisch nicht unbedingt in einer höheren Beschäftigungswahrscheinlichkeit niederschlagen.

Wenn nämlich diese gestiegenen Suchanstrengungen eine Substitution von informellen, nicht nachweisbaren Suchanstrengungen durch formelle, nachweisbare Suchanstrengungen abbilden, und informelle Suchanstrengungen aber effektiver für den Erfolg der Arbeitssuche sind, könnten EGV leicht zu einem perversen Effekt auf die Wiederbeschäftigungswahrscheinlichkeit führen. Van den Berg und Van der Klaauw (2006) verweisen auf diese Möglichkeit bei einer Erhöhung der Kontrolle der erbrachten Suchanstrengungen.

Ein weiteres Ergebnis der vorliegenden Studie ist, dass das Design der EGV von Bedeutung ist: EGV mit Suchkomponenten erhöhen signifikant die Suchanstrengungen und reduzieren die Reservationslöhne. Im Gegensatz dazu haben EGV mit reinen Qualifikationskomponenten keinen signifikanten Effekt auf das Arbeitssuchverhalten, weder auf Suchanstrengungen noch auf Reservationslöhne. Dies mag daran liegen, dass besonders EGV mit reinen Qualifikationskomponenten zur Teilnahme an Ein-Euro-Jobs führen, die ja als

Qualifikationskomponente gelten. ‚Verharrungseffekte' scheinen besonders bei diesem Typ EGV vorhanden zu sein. Aber auch ALG II-Empfänger/innen von EGV mit Suchkomponenten zeigten keine höhere Wiederbeschäftigungswahrscheinlichkeit als ALG II-Empfänger/innen ohne EGV.

Die dritte empirische Studie wird in Kapitel 5 präsentiert. Diese Studie beschäftigt sich mit der Frage, wie sich das Arbeitssuchverhalten verändert, wenn ALG II-Empfänger/innen nicht mehr die sanktionsbewehrte Verpflichtung haben, ihre aktive Arbeitssuche unter Beweis zu stellen, sondern das ‚Zuckerbrot', das heißt, finanzielle Leistungen und Unterstützung bei der Arbeitssuche, ohne die sonst obligatorische ‚Peitsche' im Sinne der sanktionsbewehrten Kontrolle ihrer Suchanstrengungen erhalten.

Diese Möglichkeit eröffnete sich noch bis 2008 ALG I- und ALG II-Empfänger/innen über 57 Jahren im Rahmen der sogenannten ‚58-Regelung', die ursprünglich aus dem Jahre 1986 stammte und eher auf eine Desaktivierung als eine Aktivierung von Arbeitslosen abzielte. Die vorliegende Studie untersucht den Effekt einer Teilnahme an der ‚58-Regelung' auf die Reservationslöhne von ALG II-Empfängerinnen mittels eines ‚Regression Discontinuity' Modells. Unter der Annahme, dass eine obligatorische sanktionsbewehrte Kontrolle der Suchanstrengungen keinen Qualifikationseffekt beinhaltet (anders als Unterstützung bei der Arbeitssuche und Qualifizierungsangebote zum Beispiel) aber die Reservationslöhne senkt, würde man bei einer Teilnahme an der ‚58-Regelung' einen Anstieg der Reservationslöhne von Teilnehmenden erwarten.

Tatsächlich zeigen die Schätzergebnisse, dass eine Teilnahme an der ‚58-Regelung' die Reservationslöhne substanziell steigert. Dieses Ergebnis impliziert umgekehrt, dass eine obligatorische sanktionsbewehrte Kontrolle der Suchanstrengungen in der Tat das Arbeitssuchverhalten von Arbeitslosen beeinflussen kann, indem sie ihre Reservationslöhne reduziert. Allerdings empfiehlt sich eine Relativierung der Größe des geschätzten Effektes. Schließlich war eine der Zielsetzungen der ‚58-Regelung' die Desaktivierung älterer Arbeitsloser und

bei der untersuchten Gruppe handelt es sich um zumeist langzeitarbeitslose ALG II-Empfänger/innen, die mindestens 58 Jahre alt sind.

Die Teilnahme dieser Gruppe an der Regelung kann aus zwei Motivationen herrühren. Manche Teilnehmer/innen möchten weiterhin ihrer Arbeitssuche nachgehen ohne eine sanktionsbewehrte Kontrolle ihrer Suchanstrengungen. Andere zielen mit ihrer Teilnahme auf einen Rückzug aus dem Arbeitsleben und beenden ihre Arbeitssuche. Daher ist es möglich, dass Reservationslöhne von Arbeitslosen mit einer höheren Beschäftigungswahrscheinlichkeit in geringerem Maße steigen würden, wenn die obligatorische sanktionsbewehrte Kontrolle ihrer Suchanstrengungen aufgehoben werden würde.

Die vierte und letzte empirische Studie wird in Kapitel 6 der Dissertation vorgestellt. Diese Studie widmet sich der Frage, ob sich bei jenen ALG II-Empfänger/innen, die ‚Zuckerbrot', das heißt, finanzielle Leistungen und Unterstützung bei der Arbeitssuche, ohne die sonst obligatorische ‚Peitsche' im Sinne der sanktionsbewehrten Kontrolle ihrer Suchanstrengungen erhalten, positive Effekte auf ihr mentales Wohlbefinden finden.

Die grundlegende Annahme in dieser Studie ist, dass allein schon die Option, an der ‚58-Regelung' teilzunehmen, dazu führt, dass die Suchanstrengungen aller arbeitslosen ALG II-Empfänger/innen über 57 Jahren weniger scharf kontrolliert werden und daher auch das Risiko einer Sanktion und eines Konfliktes mit dem Job Center sinkt, da diese Gruppe ja jeden Moment von der ‚58-Regelung' Gebrauch machen könnte und dann ohnehin von diesen Aktivierungsmaßnahmen befreit wäre. Wenn also eine sanktionsbewehrte Kontrolle der Suchanstrengungen überhaupt das mentale Wohlbefinden beeinträchtigen kann, beispielsweise über schädlichen Stress, die durch diese sanktionsbewehrte Kontrolle ausgelöst wird, oder über Konflikte mit dem Job Center, würden wir erwarten, dass das mentale Wohlbefinden von ALG II-Empfänger/innen über 57 Jahren steigt. Die empirische Schätzung des Effekts der Option, an der ‚58-Regelung' teilzunehmen, und damit

geringerer Kontroll- und Sanktionswahrscheinlichkeiten auf das mentale Wohlbefinden wird mittels eines ‚Regression Discontinuity' Modells durchgeführt.

In der Analyse werden Ost- und Westdeutsche getrennt betrachtet, da die Arbeitsmarktchancen für ältere Arbeitslose in Ostdeutschland in der betrachteten Periode (2005) deutlich schlechter waren als in Westdeutschland. Ausgehend von der Überzeugung, dass bei einer niedrigeren Arbeitsnachfrage die sanktionsbewehrte Kontrolle von Suchanstrengungen weniger effektiv für den Erfolg der Arbeitssuche ist als bei einer höheren Nachfrage, bildet die Studie die Hypothese, dass die sanktionsbewehrte Kontrolle von Suchanstrengungen nachteiligere Effekte auf das mentale Wohlbefinden ostdeutscher Arbeitsloser haben könnte.

Die Schätzergebnisse bestätigen nicht, dass sich das mentale Wohlbefinden von westdeutschen ALG II-Empfänger/innen verbessert, sobald sie 58 Jahre sind und die ‚58-Regelung' in Anspruch nehmen können. Gleichzeitig zeigen die Schätzungen positive Effekte der Option, an der ‚58-Regelung' teilzunehmen, und damit geringerer Kontroll- und Sanktionswahrscheinlichkeiten, auf das mentale Wohlbefinden der ostdeutschen ALG II-Empfänger/innen. Diese Ergebnisse implizieren, dass eine obligatorische sanktionsbewehrte Kontrolle der Suchanstrengungen mittels Arbeitsloser nicht deren mentales Wohlbefinden einschränkt, wenn es realistisch ist, einen neuen Arbeitsplatz zu finden. Dann können die Arbeitslosen dem Zwang, der durch diese sanktionsbewehrte Kontrolle ausgeübt wird, begegnen, indem sie adäquate Suchanstrengungen nachweisen.

Dies fällt in einem entspannten Arbeitsmarkt mit höherer Arbeitsnachfrage leichter, umso mehr, da diese Suchanstrengungen mit positiver Wahrscheinlichkeit zu Arbeitsangeboten führen können. Hingegen zeigen die Ergebnisse, dass der obligatorische, sanktionsbewehrte Nachweis der Suchanstrengungen bei niedrigen Arbeitsmarktchancen zu adversen Effekten auf das mentale Wohlbefinden von Arbeitslosen führen kann. Dies legt nahe, dass das mentale Wohlbefinden von

Arbeitslosen mit geringen Arbeitsmarktchancen durch weniger strikte Kontroll- und Sanktionsregimes verbessert werden könnte.

Politikempfehlungen

Im Folgenden werden die Politikempfehlungen dargelegt, die aus den oben dargestellten Analysen abgeleitet werden können. Die Analyse der leistungskürzenden Sanktionen ergab, dass Sanktionen die Wahrscheinlichkeit von sanktionierten ALG II-Empfänger/innen, reguläre Beschäftigung zu finden, in der Tat erhöhen. Es zeigten sich jedoch keine Wirkungen auf Reservationslöhne oder Suchanstrengungen.

Betrachtet man das Niveau der Reservationslöhne, das die untersuchten ALG II-Empfänger/innen bei ihrer nächsten Arbeitsstelle erwarten, wird deutlich, dass dieses Niveau mit etwa sechs Euro unterhalb des Gros' der 2005 in Deutschland erhaltenen Nettolöhne liegt. Dies impliziert, dass die Reservationslöhne von ALG II-Empfänger/innen erlaubten, fast jedes Arbeitsangebot anzunehmen, und dass daher wohl die tatsächlich eingehenden Arbeitsangebote von entscheidender Bedeutung für den Verbleib von ALG II-Empfänger/innen in der Arbeitslosigkeit ist. Die daraus abzuleitende Politikempfehlung ist daher nicht, striktere Sanktionen einzuführen, um unangebracht hohe Reservationslöhne von ALG II-Empfänger/innen zu senken, sondern, die Intensität der persönlichen Beratungs- und Betreuungsleistungen zu erhöhen, um deren Anzahl eingehender Arbeitsangebote zu steigern.

Weiterhin zeigten die Ergebnisse dieser Dissertation, dass Eingliederungsvereinbarungen zumindest in der kurzen Frist keine effektiven Instrumente darstellen, die Wiederbeschäftigungswahrscheinlichkeit von ALG II-Empfänger/innen zu steigern, trotzdem sie ihre Suchanstrengungen substanziell erhöht und ihre Reservationslöhne leicht gesenkt hatten. Dies indiziert, dass die Sucheffizienz von ALG II-Empfänger/innen nicht verbessert werden konnte. Ein

genauerer Blick auf die Daten verdeutlichte, dass die abgeschlossenen Eingliederungsvereinbarungen sehr allgemein und standardisiert gehalten waren. Die Politikempfehlung lautet daher, diese Vereinbarungen stärker zu individualisieren und auf der Grundlage eines individuell erstellten optimalen Suchprozesses abzuschließen, um die nachgewiesenermaßen gestiegenen Suchanstrengungen in einer verbesserten Sucheffizienz münden zu lassen.

Diese Politikempfehlung wird gestützt durch das Analyseergebnis, dass der Zwang-Effekt von Eingliederungsvereinbarungen deren Unterstützungs-Effekt dominiert. Sie hält selbst dann, wenn wir davon ausgehen, dass die durch die Vereinbarung gestiegenen Suchanstrengungen eine Substitution von informellen, nicht nachweisbaren Suchanstrengungen durch formelle, nachweisbare Suchanstrengungen abbilden, und informelle Suchanstrengungen effektiver für den Erfolg der Arbeitssuche sind. In diesem Falle wäre es zusätzlich ratsam, den obligatorischen, sanktionsbewehrten Nachweis formeller Suchanstrengungen abzumildern, um der Substitution effektiverer Suchmethoden durch weniger effektive Methoden entgegenzuwirken.

Die Ergebnisse der Dissertation zeigten keine ökonomisch signifikante Reaktion der Reservationslöhne von ALG II-Empfänger/innen auf Eingliederungsvereinbarungen oder Sanktionen. Daraus können wir folgern, dass die Reservationslöhne dieser Gruppe nicht besonders stark auf singuläre Aktivierungskomponenten reagieren. Die Analyse der Reaktion der Reservationslöhne auf ein ganzes Aktivierungspaket, nämlich das Wegfallen der sanktionsbewehrten Kontrolle von Suchanstrengungen, ergibt jedoch, dass die Reservationslöhne in diesem Fall deutlich steigen. Daraus können wir schließen, dass Reservationslöhne von ALG II-Empfänger/innen in einem Regime ohne eine obligatorische sanktionsbewehrte Kontrolle von Suchanstrengungen höher wären. Daher kann dieses Aktivierungspaket als Politikmaßnahme genutzt werden, um überhöhte Reservationslöhne zu senken.

Schließlich zeigten die Ergebnisse aus der vorliegenden Dissertation, dass eine obligatorische sanktionsbewehrte Kontrolle von Suchanstrengungen nicht generell schädlich für die mentale Gesundheit von ALG II-Empfänger/innen ist. Es wurde jedoch auch deutlich, dass ältere ostdeutsche Arbeitslose in besserer mentaler Verfassung sind, sobald sie die Option haben, an der ,58-Regelung' teilzunehmen, und damit geringeren Kontroll- und Sanktionswahrscheinlichkeiten ausgesetzt sind. Vermutlich liegt das an ihren niedrigeren Chancen, in ihren lokalen Arbeitsmärkten überhaupt noch Arbeit zu finden. Daher können weniger strikte Kontroll- und Sanktionsregimes als Politikmaßnahmen dienen, um die mentale Gesundheit von Arbeitslosen mit geringen Arbeitsmarktchancen zu verbessern.

Forschungsausblick

Diese Dissertation verdeutlichte am Beispiel spezifischer Aktivierungsmaßnahmen, dass Aktivierung in der Tat das Arbeitssuchverhalten von Arbeitslosen beeinflussen kann, indem sie vor allem deren Suchanstrengungen erhöhen kann und in geringerem Maße auch deren Reservationslöhne senken kann. Doch die Maßnahmeneffekte variieren, das Design der Maßnahmen spielt eine große Rolle, und ein verändertes Arbeitssuchverhalten zeitigt nicht automatisch einen erhöhten Erfolg der Arbeitssuche.

Eine zentrale Frage für zukünftige Forschungsprojekte ist daher, welches Design von Aktivierungsmaßnahmen besonders geeignet ist, das Arbeitssuchverhalten zu beeinflussen und den Erfolg der Arbeitssuche zu verbessern. Eine weitere sinnvolle Ergänzung der hier vorgestellten Ergebnisse wäre, den Effekt von Aktivierungsmaßnahmen auf die Suchmethoden von Arbeitslosen zu untersuchen, da die Art der Arbeitssuche unterschiedlich effektiv für den Sucherfolg sein könnte. Weiterhin scheinen Aktivierungsmaßnahmen heterogene Effekte auf aktivierte Arbeitslose zu

bewirken, die unter anderem davon abhängen, wie hoch die Anzahl der individuell erhaltenen Arbeitsangebote ist.

Daher sind zukünftige Analysen von Aktivierungsmaßnahmen auf der Grundlage größerer Datensätze erforderlich, um mögliche heterogene Effekte auf unterschiedliche Typen von Arbeitslosen wie auch heterogene Effekte unterschiedlicher Formen und Designs von Aktivierungsmaßnahmen zu untersuchen. Außerdem gibt es auch international bislang nur sehr begrenzte Literatur zu den Effekten spezifischer Aktivierungsmaßnahmen auf das Arbeitssuchverhalten oder die Gesundheit von Arbeitslosen überhaupt. Deswegen wäre es wichtig, über Studien aus anderen Ländern zu diesen Fragen zu verfügen, um die Forschungsergebnisse vergleichen zu können und um herauszufinden, ob eine bestimmte Aktivierungsmaßnahme – wie die Eingliederungsvereinbarung, die in ähnlicher Form in fast allen europäischen Ländern verwendet wird – unter unterschiedlichen institutionellen Bedingungen und Arbeitsmarktsituationen verschiedene Wirkungen hat.

Auch gewisse datenbasierte Einschränkungen der hier vorgenommen Analysen sollten in zukünftigen Forschungsprojekten adressiert werden. Da die hier verwendete Datenbasis eine Querschnittsbefragung ist, war es nicht möglich, die interessierenden, zu erklärenden Variablen vor und direkt nach einer Aktivierungsmaßnahme zu beobachten. Daher konnten Effekte einer Veränderung des Arbeitssuchverhaltens durch die Aufnahme einer Beschäftigung nicht komplett ausgeblendet bleiben. Dementsprechend sollte sich die zukünftige Forschung in diesem Bereich geeignete Panel-Datensätze erschließen, um für zeitlich variierende Variablen und fixe unbeobachtete Heterogenität kontrollieren zu können.

Schließlich stammen die hier präsentierten Ergebnisse aus einer sehr spezifischen Periode, gekennzeichnet durch eine sehr angespannte Arbeitsmarktsituation und die Implementierung von Hartz IV. Es ist anzunehmen, dass der zielorientierte und passgenaue Einsatz von

Aktivierungsmaßnahmen von Seiten der Job Center unter dem Fehlen qualifizierten Personals und ungenügenden Erfahrungswerten – sowohl mit ihren Klienten als auch mit den zu verwendenden Maßnahmen – litt. Auch kann eine größere Anzahl offener Stellen Aktivierungsmaßnahmen effektiver machen. Daher bietet es sicherlich einen zusätzlichen Erkenntnisgewinn, die Effekte spezifischer Aktivierungsmaßnahmen auf ALG II-Empfänger/innen in einer späteren Beobachtungsperiode zu untersuchen, in welcher sich die Job Center und der Arbeitsmarkt in einer besseren Situation befanden als im Jahr 2005.