Center of Business and Economics (WWZ), University of Basel

October 2013

Do Long-term Unemployed Workers Benefit from Targeted Wage Subsidies?

WWZ Discussion Paper 2013/14

Benjamin Schünemann, Michael Lechner, Conny Wunsch

Corresponding author:

Prof. Conny Wunsch, Ph.D.

University of Basel
Faculty of Business and Economics
Peter Merian-Weg 6
CH-4002 Basel

Phone: +41(0)61 267 33 74
conny.wunsch@unibas.ch

A publication of the Center of Business and Economics (WWZ), University of Basel.

Do Long-term Unemployed Workers Benefit from Targeted Wage Subsidies?

Benjamin Schünemann, Michael Lechner, Conny Wunsch[1]

This version: March 29, 2013

Abstract

We evaluate a wage subsidy program that is targeted at long-term unemployed workers in Germany. We use an alternative identification procedure compared to empirical studies conducted so far. Exploiting the particular program regulations and large administrative data we estimate the impact of program availability using a regression discontinuity framework. Our results suggest no significant impact of the availability of the subsidy on labor market outcomes of the target group. Even though our analysis lacks some statistical power, our findings do not support the substantial positive effects obtained from matching studies. As our approach does not require observability of all drivers of selection, previous empirical studies justifying government expenditures on wage subsidies based on matching methods should be reconsidered.

Keywords: Wage subsidy, long-term unemployment, regression discontinuity

JEL classification: J08, J23

Addresses for correspondence: Benjamin Schünemann, Michael Lechner: Swiss Institute for Empirical Economic Research (SEW), University of Gallen, Varnbüelstrasse 14, CH-9000 St. Gallen, Switzerland, Benjamin.Schuenemann@unisg.ch. Conny Wunsch: University of Basel, Faculty of Business and Economics, Peter Merian-Weg 6, 4002 Basel, Switzerland, conny.wunsch@unibas.ch.

---

[1] Michael Lechner is also affiliated with CEPR and PSI, London, CES-Ifo, Munich, IZA, Bonn, and IAB, Nuremberg. Conny Wunsch is also affiliated with CES-Ifo, Munich, IZA, Bonn, and Tinbergen Institute, Amsterdam. This project received financial support from the Institut für Arbeitsmarkt und Berufsforschung, IAB, Nuremberg (contract 8104). We would like to thank Patrycja Scioch (IAB), and Darjusch Tafreschi (SEW, St. Gallen) for their help in the early stages of data preparation.

I. Introduction

For many years, wage subsidies have been and still are advocated as an effective measure of active labor market policy (ALMP) to reduce unemployment. They became particularly popular in many OECD countries in the late 1980s and 1990s when the subsidies were designed to influence employment of specific target groups with potential disadvantages in the labor market (e.g. welfare recipients, youths, veterans etc.). Subsidy programs targeting long-term unemployed workers are particularly prevalent in European countries, especially in Germany, due to persistently high long-term unemployment rates since the late 1990s.

The favorable view on wage subsidies can be motivated using a simple labor supply and demand framework: lower labor costs (employer-side subsidy) or higher take-home earnings (employee-side subsidy) should increase employment.[2] Katz (1998) argues that in markets with transaction costs or imperfect information the subsidy scheme should differ if policy makers intend to influence the search behavior of individuals (employee-side subsidy) or the recruitment decision of firms (employer-side subsidy).[3] Moreover, the actual impact depends largely on demand and supply elasticities, making the assessment of the effectiveness of such a policy foremost an empirical issue.

A large part of the empirical literature on wage subsidies consists of studies examining the effect of employee-side payments implemented mainly as tax credits for individual workers.[4] In this study, we analyze an employer-side wage subsidy program that is targeted at long-term unemployed workers in Germany and existed until 2002. We investigate the

[2] Theoretical contributions supporting the positive implications on the wage subsidy effects date back to the work by Kaldor (1936). More recent articles are e.g. Phelps (1994) or Snower (1994).

[3] In his survey on the development of wage subsidy programs in the US, Katz (1998) describes in more detail that e.g. in markets with wage rigidities or minimum wages employee-side subsidies are more effective in increasing income while payments to employers rather result in more employment.

[4] Prominent examples of the evaluation of such financial incentives are e.g. Meyer (1995, 1996). Liebman and Eissa (1996) or Eissa and Hoynes (2004) investigate the impact of the Earned Income Tax Credit in the US on labor supply. The Working Families Tax Credit in the UK is analyzed e.g. in Blundell and Hoynes (2001) or Duncan, Shepard, Brewer and Suárez (2006). Card and Hyslop (2005) assess the effectiveness of a wage subsidy experiment in Canada.

effect of being eligible for the subsidy on labor market outcomes of the target group. Using eligibility instead of actual take-up has two major advantages. Firstly, we argue that this so-called intention-to-treat effect is the more relevant policy parameter. Wage subsidies cannot be mandated because they require the willingness of an employer to hire a subsidized worker. Hence, policy makers can only provide the *option* of granting a subsidy. Secondly, it may be easier to empirically find its causal effect as we can exploit that eligibility is based on being unemployed for at least 12 months in a sharp regression discontinuity design (RDD).

Carling and Richardson (2004), Sianesi (2008), Jaenichen and Stephan (2011), Bernhard, Gartner, and Stephan (2008) or Neubäumer (2010) investigate the effect of the actual receipt of a wage subsidy. Besides estimating a parameter that is only under partial control of the policy maker, these studies face the problem that receiving the subsidy is conditional on being employed. Separating the effect of being employed from the effect of the subsidy is rather difficult. Using unemployed individuals as control group combined with selection correction based on observables in a matching approach, as in the abovementioned studies, is likely to overestimate the effect. Sianesi (2008) and Bernhard et al. (2008), for example, report, respectively, 20-35 and 40 percentage points increases in employment rates which they attribute to the subsidy. However, the estimates are likely to mainly capture the effect of gaining or having a job relative to a control group that leaves unemployment only with some potentially low probability, instead of the incremental effect of receiving the subsidy. The reason is that matching on observables is unlikely to be sufficient to correct for selection into employment.

As eligibility does not require being employed, this selection problem is absent in our approach. Moreover, our identification strategy does not require observability of the drivers of selection. We exploit the eligibility threshold that requires jobseekers to be unemployed for at least 12 month and use almost eligible individuals as control group in an RDD frame-

work. We use large administrative data that allow us to estimate the effect locally at the threshold. We additionally correct for potential biases that result from the fact that other incentives may also change at the eligibility threshold by exploiting the abolishment of the program in 2003. In a combined RDD and difference-in-differences (DiD) approach, we correct the RDD estimate from the period where the program was still in place by subtracting the RDD estimate from the period where the subsidy was no longer available.

In contrast to the large positive and significant effects of subsidy receipt obtained from matching approaches that use unemployed workers as control group, we find no impact of subsidy eligibility on exit rates to employment or employment rates up to three years after reaching eligibility with our approach. Although our analysis lacks some statistical power due to a small number of actual subsidy recipients in our estimation sample, our results do not support the very optimistic picture about the effectiveness of wage subsidies obtained from the matching studies. Applying the same matching approach as used by those studies to our data we can replicate the very large positive and statistically significant employment effects of 20-40 percentage points, even with our small sample of subsidy recipients. However, if these where purely caused by the subsidy, effects of that magnitude should lead to detectable effects of eligibility based on our approach. The fact that we do not find any effects suggest that justifying the government expenditures on wage subsidies based on matching studies should be reconsidered.

Our work is related to Boockmann et al. (2007) and Huttunen, Pirttilä, and Uusitalo (2013) who assess the effect of eligibility for an employer-side wage subsidy in Germany and in Finland, respectively. They exploit regulation changes to estimate the effects using a DiD approach.[5] Both programs are however, targeted at the elderly aged 50 or older[6] and they

[5] Huttunen et al. (2013) even apply a difference-in-difference-in-differences method to identify the effect given their specific program regulations.

[6] In case of the EGZ the unemployed have to be 50-55 or older and the Finish subsidy scheme requires workers to be older than 54 to be eligible.

do not consider long-term employment outcomes to assess job stability. Other studies on employer-side wage subsidies mainly during the 1990s[7] evaluate programs from the US and the UK that target unemployed youths aged 18 to 24.[8] Their findings point to mainly positive employment effects but none of these studies allows for conclusions about the effects on the much broader group of long-term unemployed jobseekers which comprise workers from all age groups. Our approach also has similarities to the one used by Hamersma (2008). She evaluates the effect of eligibility on employment rates for two US tax credit programs using quarterly data. The control group consists of "almost eligible" individuals which is similar to our approach. However, the target group of US welfare recipients is not comparable to the long-term unemployed we consider. In addition, our larger data with exact spell durations allows obtaining estimates much closer to the eligibility threshold.

The remainder of the paper proceeds as follows. The next section provides an overview of the institutional setting and the economic environment in the years under consideration. In Section 0 we describe the data and our evaluation sample, before we explain the identification strategy and the estimation procedure in Section IV. Section V presents the results of our analysis and Section VI concludes. An Internet Appendix provides supplementary information and is available on request.

[7] Previous attempts to analyze wage subsidies are e.g. Perloff and Wachter (1979) or Burtless (1985). Those are less relevant for our evaluation as they investigate a general tax credit for firms for employment increases of more than 2% and a wage voucher which leads to negative employment effects for welfare recipients due to stigmatization, respectively.

[8] Lorenz (1988), Hollenbeck and Willke (1991) and Katz (1998) are examples of the evaluation of the Targeted Jobs Tax Credit (TJTC) that exists from 1978 to 1994 in the US. Bell, Blundell, and Van Reenen (1999) and Van Reenen (2004) investigate the New Deal reforms starting in 1998 in the UK.

II. Institutional details and economic background

A. The wage subsidy program

Our empirical analysis is based on a wage subsidy program that specifically targets long-term unemployed workers.[9] It was introduced in 1989 as supplement to the standard mix of active labor market programs and existed until 2002.[10] The subsidy is paid to employers if they hire a person who has been registered as unemployed for at least one year directly before employment.[11] The contract for the worker has to be open-ended and must require at least 18 hours of work per week. The subsidy payment is limited to a maximum of 12 months. In order to incentivize permanent employment and to prevent repeated use of subsidized workers for the same job, employers have to pay back the subsidy or part of it if they lay off the worker within a period of the same length as the subsidy period after the subsidy ended.[12] The subsidy is paid as a percentage of standardized labor costs, i.e. the gross wage determined in collective agreements or the wage usually paid for that occupation in the respective region. For individuals who are unemployed for at least one year but no more than two years, the subsidy for the first six months is up to 60%, and for the following six months up to 40%.[13]

The specific objective of the subsidy is to induce employers to hire long-term unemployed jobseekers and to permanently reintegrate them into the labor market. From a theoretical point of view the subsidy is supposed to cover the gap between the worker's assumed

---

[9] The official German name is *Beschäftigungshilfen für Langzeitarbeitslose (BHI)*.

[10] The program was prolonged several times - in 1996 until the end of 1998 and in 1998 until the end of 2002. In 2003 there were no new entries but only previously accepted cases.

[11] The employer has to file an application for the subsidy at the appropriate employment agency before the start of the employment relationship.

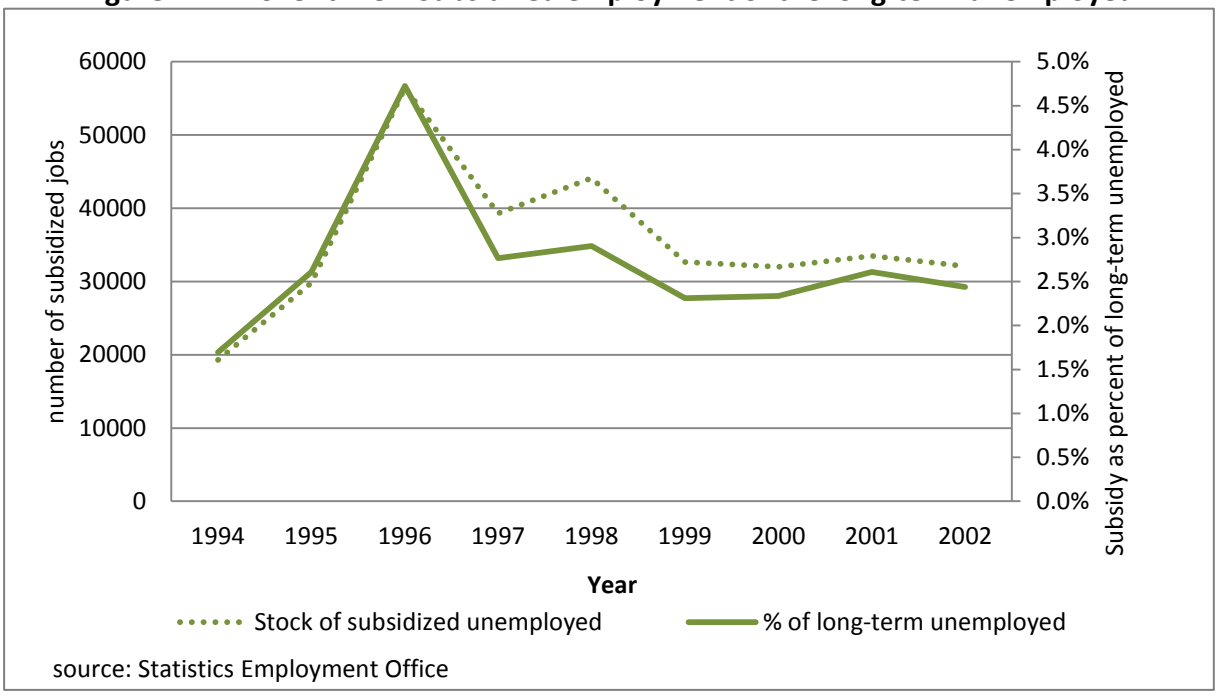
[12] This does not apply if the layoff is due to inappropriate worker behavior or economic problems of that specific firm.

[13] There exist two more thresholds for the subsidy. For individuals with unemployment durations of at least 2 but no more than 3 years, the subsidy for the first half year is up to 70% and for the second half up to 50%. For those with unemployment durations of at least 3 years the subsidy for the first half year is up to 80% and for the second half up to 60%. We only use the first threshold because in the data we observe only 9 individuals above the second threshold who received a subsidy, and there is nobody who reached the 36-month threshold before the program was abolished.

low productivity and either collectively agreed wages or their reservation wage (which are both higher than the worker's productivity). During the subsidized period, the worker's productivity is expected to increase due to on-the-job training such that unsubsidized wage payments are justified and stable employment is established.

Due to the supplementary character of the program, there was a fixed budget available for the subsidies each year which limited the number of potential beneficiaries. Usually, only a fraction of applications by employers could be accommodated,[14] and granting of a subsidy was at the discretion of the caseworker. Figure 1 shows the number of subsidy recipients over time. It increased significantly until 1996 when it reached almost 60,000 cases or about 4.5% of long-term unemployed workers. In the following year, the number of beneficiaries fell to around 30,000 or about 2.5% of long-term unemployed workers, remaining at that level until the program ended in 2003. In 2002, expenditures on the program amounted to about 290 million EUR or 1.3% of total spending on ALMP measures in 2002 (Bundesanstalt für Arbeit, 2003).

Figure 1: BHI over time – Subsidized employment of the long-term unemployed



Note: The stocks of long-term unemployed and program participants are obtained from statistics of the German Federal employment office. Based on these numbers we calculate the share.

[14] See e.g. http://doku.iab.de/chronik/31/1997_11_01_31_besc.pdf.

B. The unemployment insurance system

As the subsidy is targeted at unemployed workers, there are potential interactions with the unemployment insurance (UI) system. In the period under consideration (2000-2006), workers became eligible for UI benefits once they had worked for a minimum of 12 out of the past 36 months before filing the claim. To receive benefits, they also had to register with the public employment service (PES). The replacement rate for jobseekers with at least one dependent child was 67% of their previous average net earnings from insured employment, and 60% without dependent children. If eligible, workers qualified for a minimum of six months of UI payments. The maximum benefit period increased in two-month steps as a function of months contributed in the past seven years and age, to a maximum of 32 months for workers aged 57 or above. For a claim of c = 6, 8, 10,... , 32 months, jobseekers had to contribute for at least c * 2 = 12, 16, 20,... , 64 months in the past seven years. Two features of UI are important for analyzing the wage subsidy program. Firstly, jobseekers with UI claims of exactly 12 months exhaust UI at the same moment as they become eligible for the subsidy. Secondly, subsidized employment counts as contribution period to UI. Hence, employment in a subsidized job for at least 12 months allows the worker to acquire a new UI claim of at least six months. We will come back to these issues when we discuss identification in Section IV.B.

C. General trends in the German labor market

The overall economic environment is a crucial factor influencing the employment chances of (long-term) unemployed workers and the effectiveness of the subsidy program. Table 1 provides an overview of the economic development over our period of investigation (2000-2006) based on two broad indicators. At the start of our sampling period, real GDP growth is 3.2% in 2000, which is relatively high. However, at the end of 2000 the economy began to slow down and beginning September 11, 2001 marking the world economic downturn, a

period of stagnation started. Growth rates dropped to 1.2% in 2001 and even -0.2% in 2003. In 2004, due to the slow recovery of the world economy, Germany's growth rates increased again to 1.2% in 2004 and 0.8% in 2005. The unemployment rate declined from 10.5% in 1999 to 9.4% in 2001, as usual lagging behind the economy's growth rate. From 2002 onwards unemployment increased slowly to 11.7% in 2005 before it declined again to 10.8% in 2006. Our empirical analysis will be based on entries into unemployment 2000-2002. During that period, the unemployment rate was stable around a level of 10%, despite the economic downturn and subsequent recovery between 2000 and 2006.

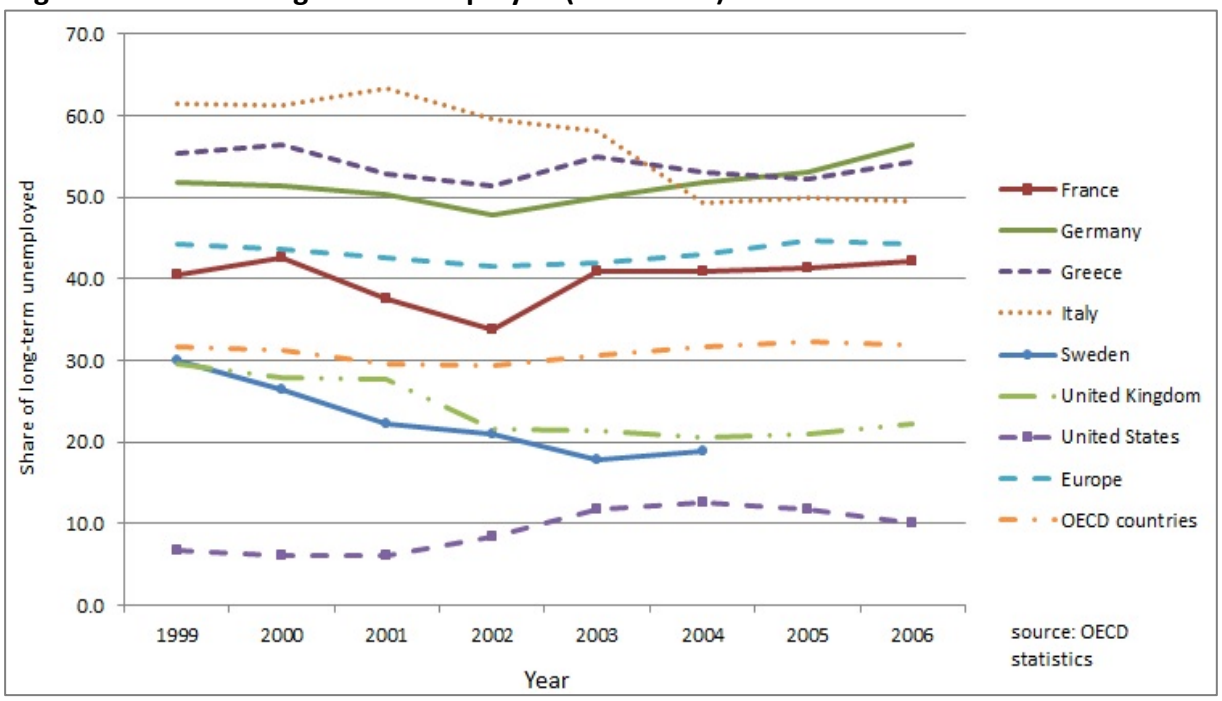
Table 1: GDP growth and unemployment rates for Germany 1999-2006

	1999	2000	2001	2002	2003	2004	2005	2006
Real GDP growth[a]	2.0	3.2	1.2	0.0	-0.2	1.2	0.8	3.2
Unemployment rate[b]	10.5	9.6	9.4	9.8	10.5	10.5	11.7	10.8

Note: All numbers are in %. a) GDP at constant 2000 prices. b) Registered unemployment as percentage of civilian labor force. Source: Statistics Employment Office, Statistisches Bundesamt.

In contrast to the US, UK, or e.g. Sweden, Germany has a large share of long-term unemployed jobseekers (see Figure 2). The rate is much higher than the OECD average and even higher than the European rate over the entire time horizon. It is comparable to the one of Greece. It is very persistent over time, ranging between 50-55% in the period 1999 to 2006. This explains the desire to foster employment of these individuals using various policy measures such as the wage subsidy program we analyze.

Figure 2: Share of long-term unemployed (12 months) in seven OECD countries 1999-2006



Note: Share of long-term unemployed in %. It represents the long-term unemployed as a fraction of all unemployed. Source: OECD statistics.

III. Data and sample definition

A. The Data

The empirical analysis is based upon a 2% random sample of all individuals that have been employed subject to social insurance at least once between 1990 and 2006. It is an administrative data set that combines social insurance records, program participation records, benefit payment files and job seeker registers. It contains about 1.4 million individuals. The available spell information allows the exact calculation of the duration of different labor market states such as employment and unemployment spells as well as program participation periods. The database contains many socio-demographic characteristics and allows computing detailed labor market histories for at least 10 years. Additionally, we match regional information to this data.

B. The evaluation sample

From the database described above we select all individuals entering unemployment[15] between April 2000 and December 2002. We start our analysis in April 2000 (and not earlier) due to missing information and potential underreporting of job search and program participation spells in the first months of 2000. We do not consider entries after December 2002 in order to observe outcomes for at least three years after reaching eligibility, i.e. 12 months after entering unemployment.

In order to obtain a homogeneous sample that describes typical unemployed workers we impose further conditions: We exclude individuals who participated in a program or who were unemployed for two weeks or more during the last year before UE entry to ensure that this entry is not just a short interruption of a previous unemployment spell. Furthermore, we only consider unemployed individuals who are eligible for UI at the beginning of unemployment and who were employment for at least six months during the last year before entry. The latter condition ensures that any person with 12 months unemployment duration in our sample is eligible for the subsidy.[16]

IV. Empirical strategy

A. The empirical evaluation problem

We are interested in investigating whether temporary employer-side wage subsidies improve the employment chances of long-term unemployed workers and induce stable em-

[15] We select individuals that enter from unsubsidized employment or general non-employment e.g. out of labor force.

[16] To calculate the unemployment duration that determines eligibility the rules of the subsidy program allow short disruptions of the unemployment spell within 5 years prior to eligibility e.g. 6 months of employment. To avoid complex structures of unemployment-employment spells resulting in unreasonable cases in our sample we require at least 6 months of employment before entry.

ployment after the subsidy period ends. However, establishing a convincing causal relationship between the subsidy and employment outcomes faces two main challenges.

Firstly, as all subsidy recipients are necessarily employed, the standard approach of using actual receipt of the subsidy as treatment creates the problem of separating the effect of gaining a job from the effect of gaining the subsidy. Ideally, one would like to compare subsidy recipients with other unemployed workers who have the same probability of getting a job without the subsidy. Thus, credible identification requires solving the so-called double selection problem (see Lee, 2009, or Lechner and Melly, 2010) of selection into employment and selection into the treatment. Using a control group of unemployed workers who do not receive a subsidy - as, for example, in Bernhard et al. (2008) - is likely to result in largely overestimated employment effects of the subsidy if selection into employment is not appropriately accounted for. The reason is that employed subsidy recipients are being compared to unemployed workers who get a job only with some - potentially low - probability.[17]

The second challenge arises even if selection into employment could be solved. As in most non-experimental program evaluation studies receipt of the subsidy is not random. However, in contrast to other measures such as training programs or job search assistance, the assignment process for wage subsidies involves not only the caseworker and the job seeker, but also the employer. To eliminate selection bias, one would ideally like to control for all factors that drive the decisions made by all three parties and affect the employment outcomes of the subsidy recipients. Our data, however, do not provide sufficient information on subsidy assignment. For instance, it is not known who initiated the process: Did the caseworker and/or the unemployed contact the firm, or did the firm and the unemployed agree and contact the caseworker afterwards? Furthermore, the extent of discretion of the

[17] Note that the problem of selection into employment also has to be solved if the control group is chosen as unemployed workers who find employment without the subsidy. The reason is that both treated and controls need to have the same probability of getting a job without the subsidy, but those who actually do find employment without the subsidy are likely to be a positively selected group of all job seekers.

caseworker or the particular rules the caseworker based her decision on is not observable. In the following section, we therefore propose an identification strategy that neither requires solving the problem of selection into employment, nor relies on the observability of all factors that drive receipt of the subsidy.

B. Parameter of interest and identification strategy

A difference of this program to training or job search assistance programs is that wage subsidies cannot be mandated: They require the willingness of an employer to hire a subsidized worker. Consequently, policy makers can only provide the *option* of granting a subsidy while actual take-up is the outcome of decisions that can only be influenced to some degree. The main policy parameter of interest is therefore the intention-to-treat (ITT) effect, i.e. the effect of eligibility for the subsidy, rather than the effect of actual receipt.

Using eligibility for the subsidy as treatment also has two methodological advantages. Firstly, as being eligible for the subsidy does not require finding employment, the double selection problem into both employment and subsidy receipt is absent when the treatment is defined as subsidy eligibility. Secondly, for solving the selection problem into this treatment we can exploit that eligibility is based on being unemployed for at least 12 months using a sharp regression discontinuity design (RDD) as introduced by Thistlethwaite and Campbell (1960). We can compare the employment outcomes of (ineligible) workers who have been unemployed for slightly less than 12 months to those of (eligible) workers who have been unemployed for 12 or slightly more months.

Specifically, we are interested in the effect of eligibility for the subsidy around the threshold in the period, in which the subsidy was available. Denote the potential outcomes above the threshold by Y^1 (eligible for subsidy) and under below the threshold by Y^0 (not eligible for the subsidy). Let x denote unemployment duration, let $\overline{x}$ be the 12-month eligibility threshold, denote by D the treatment (eligibility for the subsidy) which is defined

as $D = 1(x \geq \overline{x})$, and let $T = 0$ be the period after the abolishment of the subsidy and $T = 1$ the period in which the subsidy was available. Formally, the average effect of the treatment at the cut-off is given by:

$$E\left(Y^1 - Y^0 \mid X = \overline{x}, T = 1\right).$$

As usual, Y^1 and Y^0 are unobservable and must be somehow extracted from the observable outcome, *Y*, which is given by $Y = DY^1 - (1-D)Y^0$, giving raise to the identification problem. In a sharp RDD design identification is based on the assumption that the potential outcomes are continuous functions of unemployment duration locally around the 12-month threshold. If this condition is satisfied, any discontinuous jump in observed outcomes at the threshold must be due to the treatment, i.e. due to being eligible for the subsidy.[18] Validity of this assumption requires that there are no other institutional rules for unemployed workers that change at the 12-month threshold. Unfortunately, this is not satisfied in our evaluation period in Germany because there are three other programs that use the same threshold. Firstly, there is the so-called integration supplement (*Eingliederungszuschuss*). This program is targeted at various groups of hard-to-place unemployed workers such as disabled, elderly and long-term unemployed workers, where the latter is defined as workers who have been unemployed for at least 12 month. Secondly, there is an early retirement program for long-term unemployed workers aged 60 or older. Thirdly, a substantial fraction of unemployed workers has UI claims of exactly 12 months. Hence, these workers exhaust their UI claim in the same moment as they become eligible for the wage subsidy.

We solve these problems as follows. Firstly, we exclude all workers aged 60 or older and jobseekers with UI claims of exactly 12 months at the beginning of unemployment. In the vicinity of the 12-month threshold, bunching of UI claims then only occurs at 10 and, for workers aged 45 or older, at 14 months of unemployment. However, because of the discrete

[18] For a more detailed overview of this method and its assumptions see Hahn, Todd, and Van der Klaauw (2001), Imbens and Lemieux (2008), or Van der Klaauw (2008).

jump in the exhaustion probability due to this bunching, local continuity in potential outcomes may still be violated. Therefore, we additionally narrow the window around the 12-month threshold. The large sample size even very close to the threshold allows us to only use workers who have been unemployed for at least 11 or 12 months. Thus, all workers in the estimation sample with initial UI claims at bunching points are at least one month away from exhaustion of UI which reduces potential biases. Lastly and most importantly, we exploit the fact that the wage subsidy was no longer available to workers who satisfied eligibility in 2003, while the integration supplement and the rules for UI where still in place and remained unchanged.

Specifically, we combine the RDD with a difference-in-differences (DiD) approach. We first estimate the effect of eligibility based on RDD using workers who became eligible for the subsidy in 2001 or 2002 when the program was still available. We then repeat the estimation using workers who satisfy the eligibility criterion in 2003 when the subsidy was no longer available. Under certain conditions, this provides an estimate of the bias which is due to violations of the local discontinuity assumption, e.g. due to discrete jumps in UI claims and the change in eligibility for the integration supplement at the 12-month threshold. The difference between the two RDD estimates gives an estimate of the causal effect of eligibility for the subsidy under two assumptions that are local versions of the classical DiD assumptions.[19] Firstly, conditional on treatment status, satisfying the eligibility criterion should not have any effects locally at the threshold *after* the abolishment of the subsidy in $T = 0$:

$$E\left(Y^1 - Y^0 \mid X = \overline{x}, T = 0\right) = 0.$$

[19] See e.g. Angrist and Krueger (1999), Angrist and Pischke (2009), Blundell and Costa Dias (2009) or Imbens and Wooldridge (2009).

Secondly, the bias due to any determinants of employment outcomes other than eligibility for the wage subsidy that also change discretely at the 12-month threshold must be constant in the period 2001-2003 locally at the threshold:

$$\lim_{x\downarrow\bar{x}} E\left(Y \mid X = x, T = 1, D = 1\right) - \lim_{x\uparrow\bar{x}} E\left(Y \mid X = x, T = 1, D = 0\right) - E\left(Y^1 - Y^0 \mid X = \bar{x}, T = 1\right) =$$
$$= \lim_{x\downarrow\bar{x}} E\left(Y \mid X = x, T = 0, D = 1\right) - \lim_{x\uparrow\bar{x}} E\left(Y \mid X = x, T = 0, D = 0\right) - E\left(Y^1 - Y^0 \mid X = \bar{x}, T = 0\right).$$

Using $E\left(Y^1 - Y^0 \mid X = \bar{x}, T = 0\right) = 0$ and rearranging terms directly shows that the effect of eligibility at the 12-month threshold in the period in which the subsidy was available can be consistently estimated as the difference of the RDD estimates in the pre-abolishment period, $T = 1$, and in the post-abolishment period, $T = 0$:

$$E\left(Y^1 - Y^0 \mid X = \bar{x}, T = 1\right) = \lim_{x\downarrow\bar{x}} E\left(Y \mid X = \bar{x}, T = 1, D = 1\right) - \lim_{x\uparrow\bar{x}} E\left(Y \mid X = \bar{x}, T = 1, D = 0\right)$$
$$- \left[\lim_{x\downarrow\bar{x}} E\left(Y \mid X = \bar{x}, T = 0, D = 1\right) - \lim_{x\uparrow\bar{x}} E\left(Y \mid X = \bar{x}, T = 0, D = 0\right)\right].$$

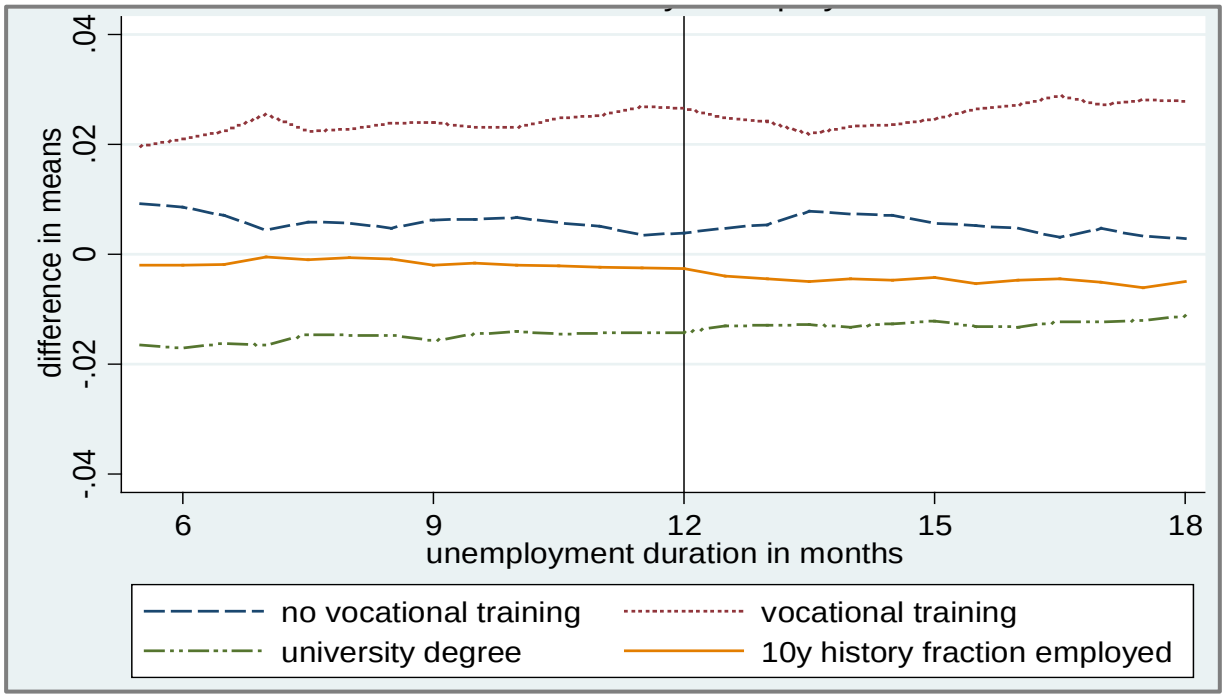
C. Validity of the identifying assumptions

Identification requires that there are no observable or unobservable determinants of employment outcomes whose influence at the 12-month threshold changes in the period 2001-2003. We perform a number of checks to investigate whether this is likely to be satisfied. Firstly, we check whether potential differences in observable determinants of employment outcomes change over time. Figure 3 plots the differences in means between workers who became unemployed in 2000-2001 (eligible for the subsidy 2001-2002, $T = 1$) and those who became unemployed in 2002 (eligible for the subsidy 2003, $T = 0$) by elapsed unemployment duration for three different levels of education and the fraction of months employed in the 10 years before unemployment.[20] A jump in the mean differences of these confounders at the cut-off point would raise concerns about identification as estimated outcome differ-

[20] The separate means for 2000-2001 and 2002 (i.e. the underlying levels) are shown in Figure IA.1 in Internet Appendix A.

ences may be due to those characteristics rather than the subsidy program. However, none of the differences are significantly different from zero and they appear to be continuous in unemployment duration especially at the threshold.[21] Computing the time difference of other available observable characteristics just below and above the threshold results in similar conclusions (see Table IA.1 in Internet Appendix A). Although there are a few significant differences in means over time, those differences are similar above and below the threshold which supports our identification strategy.

Figure 3: Differences in means between 2000-2001 and 2002 by unemployment duration for some selected characteristics



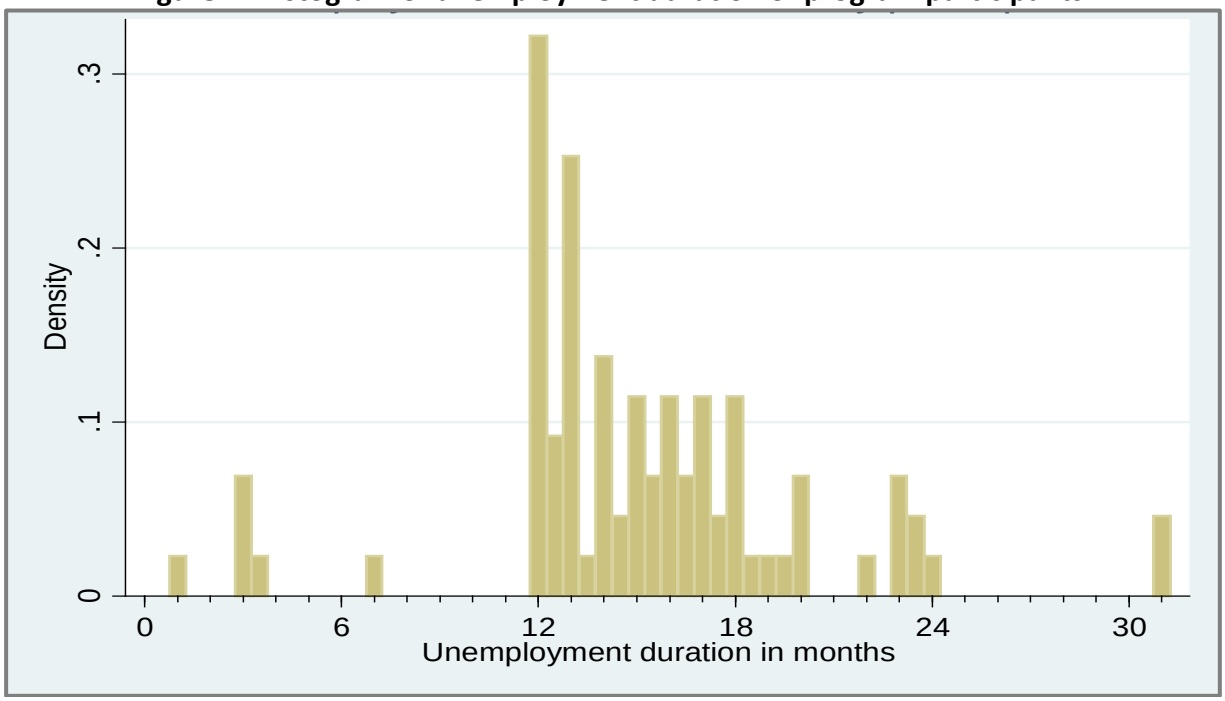
Note: The graphs are obtained using samples defined by unemployment duration, i.e. we select individuals with at least e.g. 9 months of unemployment separately for entries in 2000-2001 (T=1) and 2002 (T=0). For each of these samples we difference the means of the covariates over time.

Another potential concern regarding our identification strategy might be effects of the treatment before reaching the 12-month threshold. Such effects may on the one hand occur due to earlier receipt of the subsidy. Figure 4 shows that there are very few cases in which subsidies start before unemployment durations of 12 months. These are in line with

[21] We do not depict the confidence bands (95% level) here for the sake of clarity but all of them include zero over the entire range of unemployment durations in the graph.

special rules for people with repeated unemployment spells that are interrupted by very short employment spells. However, none of these cases is part of our final estimation sample. On the other hand, jobseekers and employers may have an incentive to wait for eligibility once they are close enough in order to get a job and to save wage costs, respectively. Caseworkers have ambiguous incentives. They may want the jobseeker to exit before she becomes eligible for a costly program, or they may try to remove her from their unemployment statistics via a subsidized job. In Figure 5, we therefore investigate whether there are any unusual drops or increases in exit rates from unemployment to unsubsidized employment just before the 12-month threshold. The evolution of the exit rate for T = 1, i.e. when the subsidy program was existent, does not show any unusual shifts just before the cut-off point. The exit rate declines with unemployment duration but the slope at 11 months is not much different from the overall trend. The fact that we do not find evidence for anticipatory effects just before reaching eligibility also makes it very unlikely that there are anticipation effects with respect to the abolishment of the program. Moreover, the exit rates for T=0 (when the program was not available) also do not show any distinct patterns at the threshold relative to the overall evolution of the exit rate.

Figure 4: Histogram of unemployment duration of program participants



Note: The histogram is based on our entire sample. We identify the actual participants and depict their unemployment duration until they (their employer, respectively) receive the subsidy, i.e. until they are employed.

Figure 5: Exit into unsubsidized employment by unemployment duration

Note: Exit rate in %. The lines represent the exit probability conditional on previous unemployment duration. An individual with e.g. 6 months unemployment has a 4.5% chance to exit in the next month (based on the exit rate for T=1).

With respect to potentially different responses to changes in the economic environment we benefit from the fact that we impose the constant bias assumption only very locally

– in a one-month window – around the 12-month threshold. As shown in Section II.C labor market conditions worsened somewhat in the period 2000-2003 although the unemployment rate and the share of long-term unemployed jobseekers did not change very much. It is unlikely that a one-month difference in elapsed unemployment duration implies different behavior in response to the relatively small changes in economic conditions, especially given the fact that we do not find any compositional effects in terms of observed characteristics around the threshold. Moreover, as we will show in the results section, there are no time effects with respect to employment outcomes.

D. Estimation

In Section IV.B we have shown that the parameter of interest is identified at the 12-month threshold from four conditional expectations. Given the large sample sizes in our data we can estimate these conditional expectations within a very small interval around the discontinuity point. We only use observations within a one-month window around the cutoff, i.e. with elapsed unemployment durations of at least 11 or 12 months. Van der Klaauw (2008) argues that in this case comparing mean outcomes just below and above the cut-off is sufficient to obtain a consistent estimate of the effect without having to control for the forcing variable unemployment duration or other covariates. The latter is confirmed by the absence of compositional effects in terms of observed characteristics of the workers around the threshold (see Figure 3 and Table IA.1 in Internet Appendix A). Consequently, in this close neighborhood around the 12-month cutoff we use a fully saturated OLS regression on a constant, the period dummy, *T*, the treatment dummy, *D*, and an interaction term between those two dummies which captures all relevant variation of the outcome of interest:

$$Y_i = \alpha + \tau T + \delta D + \theta DT + \varepsilon.$$

Hence, it yields non-parametric estimates of the relevant means and differences in means. The constant α represents the mean no-treatment outcome in period $T = 0$, $E[Y_i|x <$

$\bar{x}, T = 0, D = 0]$; τ is the period effect for the non-treated, $E[Y_i|x < \bar{x}, T = 1, D = 0] - E[Y_i|x < \bar{x}, T = 0, D = 0]$; δ is the bias in the abolishment period $T = 0$, $E[Y_i|x \geq \bar{x}, T = 0, D = 1] - E[Y_i|x < \bar{x}, T = 0, D = 0]$; and θ is the effect of interest: the difference in mean outcomes between eligible and ineligibles at the threshold in the period where the subsidy was available minus the same difference in the abolishment period.

As a sensitivity check, we estimate two extended regression models, one which includes elapsed unemployment duration, and another one including controls for further covariates. The results from all models are similar.

V. Results

A. The average effects of eligibility

In line with the program's objectives we measure the effects on exit rates out of unemployment into employment and investigate employment stability over time. For the workers who are unemployed at a given duration (11 or 12 months in our estimation sample) we investigate exit from unemployment in the next month. We distinguish between exit to unsubsidized employment and to so-called regular employment which includes subsidized employment. Table 2 presents the results. The effects of eligibility are very close to zero and not statistically significant.[22] This result is robust to inclusion of elapsed unemployment duration and other covariates as control variables (see Table IA.5 in Internet Appendix C). Interestingly, the estimate of the bias in the abolishment period is also very close to zero and not statistically significant. Hence, the sample selection criteria seem to have already removed any relevant biases. There is also no significant time trend. Both results support our identification strategy.

[22] Standard errors are obtained as in the usual least squares framework. The concerns addressed in Bertrand, Duflo, and Mullainathan (2004) or Donald and Lang (2007) about downward biased standard errors due to combined group and time specific random effects are not important here. The effect is not significant, so any correction resulting in larger confidence intervals would not change the results.

Table 2: Baseline results for exit rates

Dependent Variable:	Exit to unsubsidized employment		Exit to regular employment	
	Coeff.	S.E.	Coeff.	S.E.
Constant	0.029	0.0020	0.032	0.0021
Bias in T = 0	-0.002	0.0028	-0.001	0.0031
Time effect	-0.001	0.0026	-0.001	0.0028
Effect of eligibility (ITT)	-0.001	0.0037	0.003	0.0040
"Average treatment effect on the treated (ATET)"*	-0.084	0.4100	0.307	0.4391
Observations	31049		31049	

Note: The baseline regression defines the eligibles as those with elapsed unemployment duration of 12 months, ineligibles have previous unemployment duration of 11 months. Exit rates measure exit in the month after determining eligibility, i.e. in month 12 (ineligibles) and 13 (eligibles) after becoming unemployed. *The ATET is calculated as the ITT divided by the share of subsidy recipients. Standard errors (S.E.) are obtained from 4999 bootstrap replications. According to MacKinnon (2006) this avoids ties when computing the quantiles.

Conclusions about employment stability are based on the effects on the monthly share of workers in unsubsidized employment over a time horizon of three years after determining eligibility status. Figure 6 shows the rates of unsubsidized employment one year after determining eligibility status around the 12-month unemployment duration threshold for both the subsidy and the abolishment period. As expected, future employment rates fall with previous unemployment duration but there is no visible jump at the threshold. No effects on employment stability are confirmed in Figure 7, where we display the effects on monthly employment rates. All effects are very close to zero and not statistically significant. The same is true if we look at the effects of eligibility on the number of months in unsubsidized employment that have been accumulated over the three years after determining eligibility status.

Figure 6: Effect on unsubsidized employment 1 year after eligibility

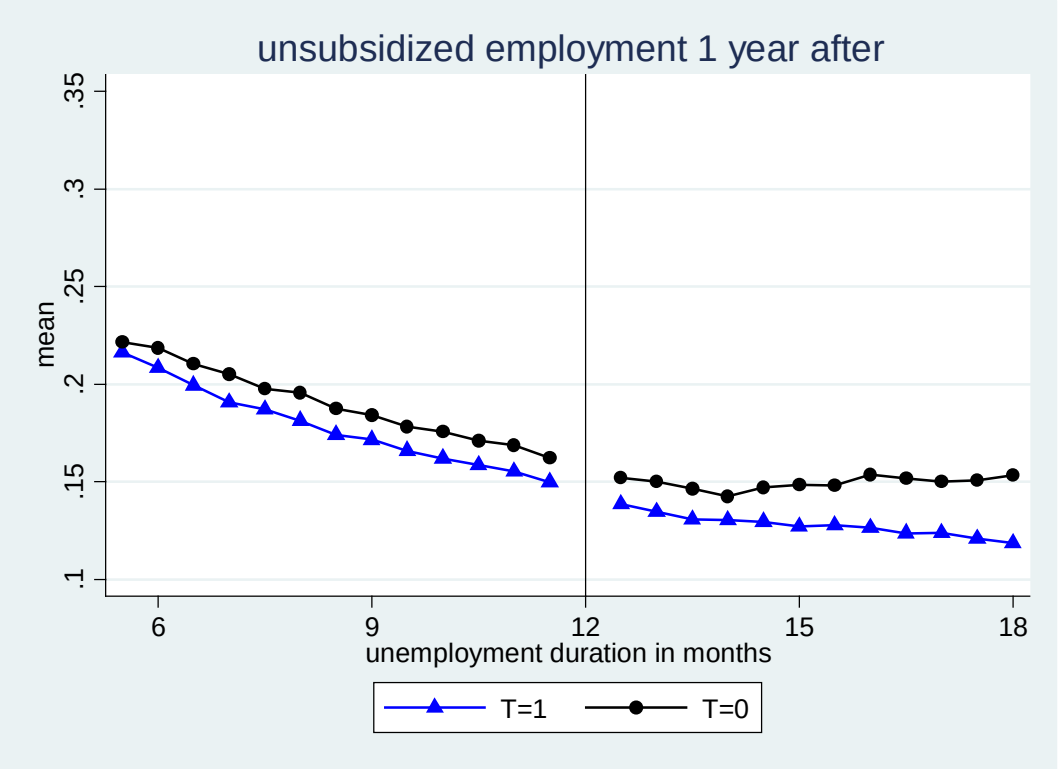
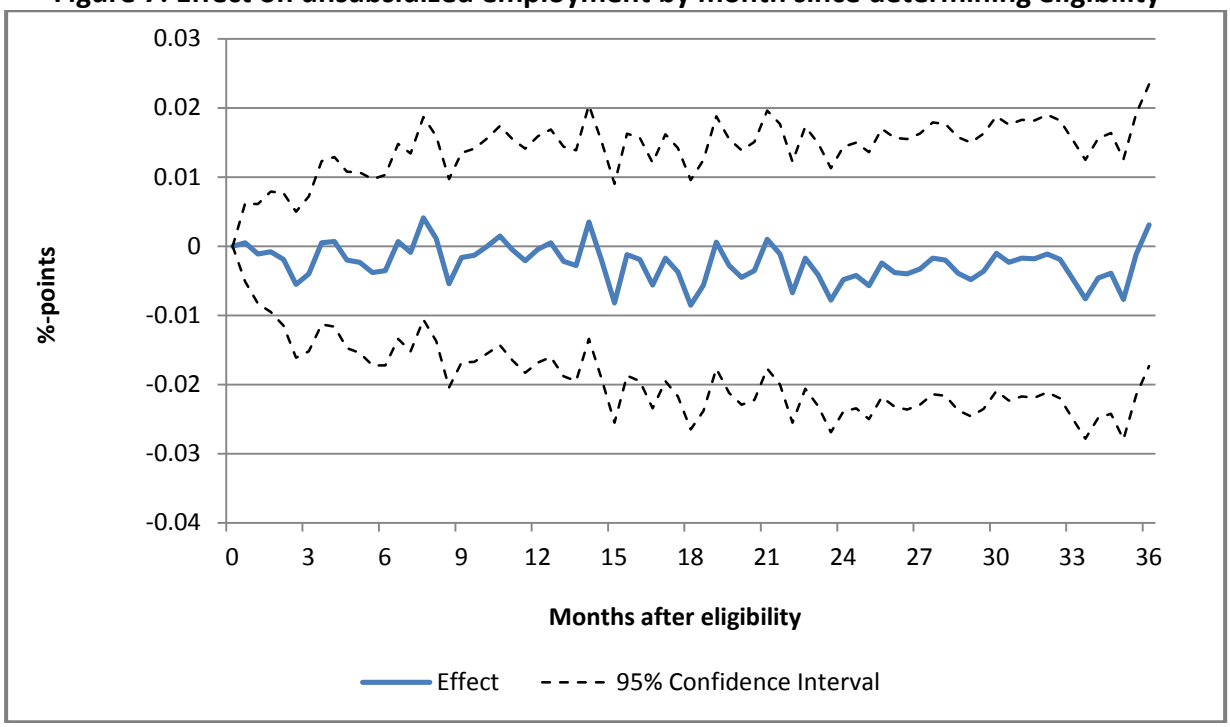


Figure 7: Effect on unsubsidized employment by month since determining eligibility



Note: The effect is obtained based on the proposed estimation strategy. Eligibles are those with elapsed unemployment duration of 12 months, ineligibles have previous unemployment duration of 11 months.

The absence of any significant effects in combination with small standard errors seems to suggest that the wage subsidy program was ineffective in raising exit rates to unsubsidized employment or improving employment stability. However, it is important to note that despite the large number of observations in our estimation sample, our analysis lacks some statistical power due to the low take-up rate of the subsidy in the sample. We observe only 81 actual subsidy recipients or about 0.9% of all eligibles from the years when the program existed (eligible in 2001-2002). The implications of this are illustrated by calculating a rough guess of the average effect for the actual participants (ATET). Assuming random selection into employment (a rather implausible assumption), it can be obtained from the average effect of eligibility for the subsidy, i.e. the intention-to-treat (ITT) effect, by dividing the ITT by the fraction of actual subsidy recipients. For exit to unsubsidized employment we obtain a large negative effect of 8.4 percentage points. The negative sign is in line with the fact that beneficiaries enter subsidized employment. For exit to regular employment we obtain a large positive effect which reflects entrants to subsidized employment. However, both effects are estimated with a very large standard error due to the small share of actual subsidy recipients (see Table 2).

To further assess the issue of statistical power, we extend the estimation sample by including those jobseekers with UI claims of exactly 12 months. In terms of identification, we then rely on the DiD-step to take out the effect of exhausting UI when reaching eligibility for the subsidy. We estimate this effect in the period when the subsidy was no longer available and subtract this estimate from the RDD estimate in the subsidy period. Including these observations more than doubles the number of actual recipients of the subsidy to 188 and should therefore increase statistical power. The effect of the exit rate to regular employment, which includes subsidized employment, shows that this is indeed the case. We obtain a statistically significant positive effect of 0.8 percentage points (see Table IA.6 in Internet

Appendix C) which reflects the entrants to subsidized employment. However, the effects on the exit rate to unsubsidized employment (see Table IA.6 in Internet Appendix C) and on employment stability (see Table IA.4 in Internet Appendix C) are still very close to zero and not statistically significant. Therefore, our results are not entirely driven by lack of statistical power. Although we still cannot rule out small effects of the subsidy with sufficient statistical confidence, the least we can say is that we do not find evidence for any substantial effects that are comparable to those obtained in recent studies like Sianesi (2008) and Bernhard et al. (2008) that are based on matching approaches. In the next section we investigate the differences between these approaches and our approach further.

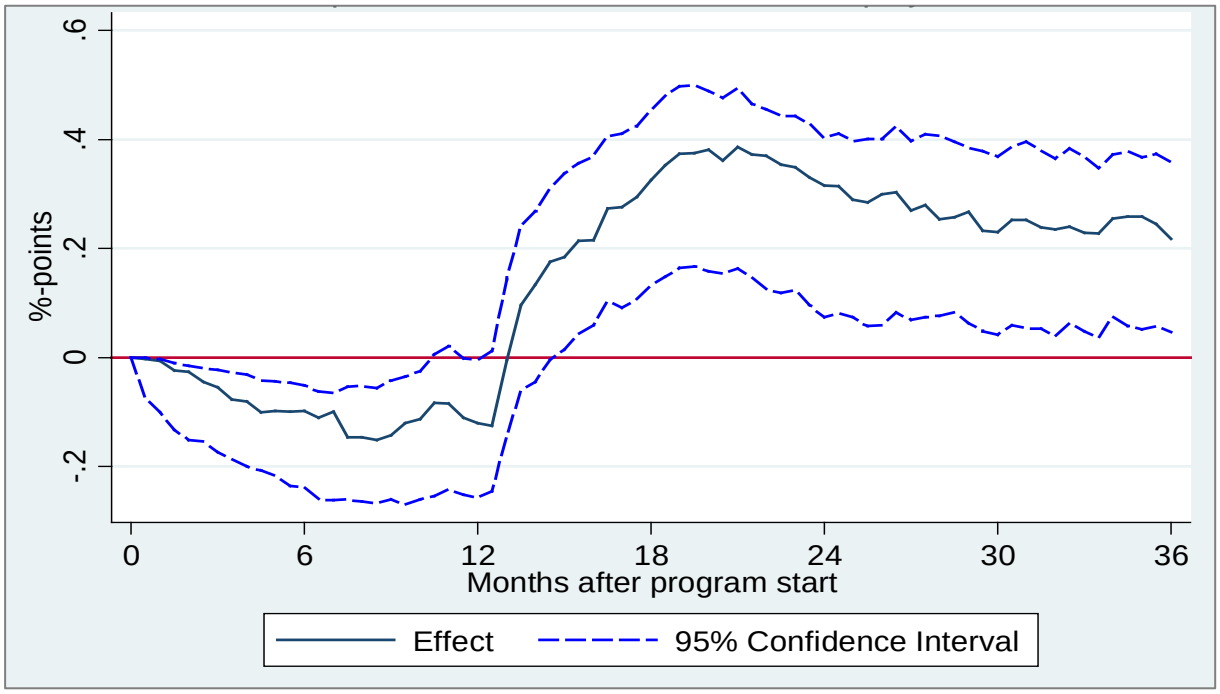
B. Results from a matching approach

To compare these results to those obtained with standard matching, we follow Sianesi (2008) and Bernhard et al. (2008). We use all individuals entering unemployment between April 2000 and December 2001 and impose the same sample selection criteria as before (to ensure that the results are based on comparable samples). As in the two benchmark studies, we define treated individuals as actual subsidy recipients, and the control group contains all long-term unemployed who do not receive the subsidy. Identification is based on the assumption that taking up a subsidized job is random conditional on observable characteristics.[23] Following Sianesi (2008) and Bernhard et al. (2008), we control for socio-demographic information such as age, education, nationality, number of children, marital and health status. Moreover, we use many variables that describe each individual's labor market and earnings history in order to capture future employment prospects as well as motivation and preferences. We also control for characteristics of the last employer (e.g. industry, firm size), amount of UI benefits and time to exhaustion of UI payments, region indicators and other regional information (e.g. long-term unemployment rate, GDP per capita) which are related

[23] For further details on this approach and its assumptions see e.g. Imbens (2004) and the references therein.

to the local labor market as well as to the financial situation of the employment office (for the full list of control variables see Table IA.2 in Internet Appendix B). We use the propensity score radius matching with bias adjustment proposed by Lechner, Miquel, and Wunsch (2011) which has been shown to perform well by Huber, Lechner and Wunsch (2013). The propensity score is estimated using a probit model, the results of which are displayed in Table IA.2 in Internet Appendix B. As outcome variable we use monthly rates of employment in unsubsidized jobs from the beginning of the subsidized job up to 3 years thereafter. Figure 8 shows the results.

Figure 8: Effect of program participation on unsubsidized employment



Note: To obtain the results we use the matching estimator proposed by Lechner et al. (2011) for which Huber, Lechner, and Wunsch (2013) provide further details on its performance compared to other propensity score-based estimators. Our sample includes 8661 observations and we obtain the confidence interval as the 2.5th and 97.5th percentile of the distribution of the effect based on 4999 bootstrap replications.

For unsubsidized employment the effect is negative for the typical duration of the wage subsidy of 12 months (lock-in). Thereafter, just as in previous studies based on matching, our findings suggest a large and significant effect for subsidy recipients. Three years after starting the job, subsidy recipients are about 20%-points more likely to be in unsubsidized employment than the control group. The results are striking because we find large

positive and statistically significant effects with the matching approach despite the small number of participants in our sample. Our main objection to the matching approach is that conditioning on unemployment duration and observable worker characteristics at the beginning of unemployment is not sufficient to solve the selection problem of taking up a subsidized job relative to a comparison group of long-term unemployed workers. The estimated effects are likely to reflect the impact of gaining a job rather than the incremental impact of the wage subsidy itself. The approach we presented in the last section does not suffer from this problem.

C. Further sensitivity checks

Although we have many observations close to the threshold, a natural sensitivity check is to expand the sample around the discontinuity point. This increases statistical power because of both, the larger sample size, and the increased number of actual subsidy recipients. We re-estimate our model in three additional samples where we include, respectively, observations with elapsed unemployment durations of two, three and four months (instead of one month) below and above the cut-off point of 12 months. Table IA.3 in Internet Appendix C presents the regression results for the exit rates into unsubsidized and regular employment. Figures IA.2-4 in Internet Appendix C show the results on employment stability for the three samples. All effects are very close to zero and insignificant. Including the forcing variable unemployment duration in the regression to better extrapolate the exit rates further away from the threshold reduces the effects even more. Thus, our results are again confirmed.

VI. Conclusion

In this study, we propose an alternative strategy to evaluate wage subsidy programs. We investigate an employer-side wage subsidy targeted at long-term unemployed workers in Germany. The program's objective is to increase exit rates from unemployment to employ-

ment and to induce stable employment relationships for the economically disadvantaged target group. Using a large administrative dataset we assess the effectiveness of the program by estimating the impact of subsidy eligibility on employment outcomes. We exploit the eligibility criterion of the program using a combined regression discontinuity and difference-in-differences approach. The large sample size of our data allows us to estimate the effect locally at the eligibility threshold.

We do not find any significant impact of the program on exit rates to unsubsidized employment or employment stability. Due to low take-up of the program in our sample it would be premature, though, to conclude from this that the program was completely ineffective. However, we find no evidence for substantial positive employment effects that are comparable to those obtained e.g. by Bernhard et al. (2008) from a matching approach that uses other long-term unemployed workers as control group. Matching methods are not well suited for separating the effect of finding employment from the incremental effect of the wage subsidy. We demonstrate this by applying this approach to our program and sample as well. Despite the small number of subsidy recipients in our data and no evidence for large effects from our approach, we find very large employment effects that are similar in magnitude to those obtained by Bernhard et al. (2008) from matching. These estimated effects are likely to mainly reflect the impact of gaining a job rather than the impact of the wage subsidy program.

To improve the usefulness of matching approaches it is necessary to better understand how employer-side wage subsidies work. In particular, it would be helpful to know the actual assignment process in more detail. If, for instance, the reasons of the caseworker to allocate individuals to the program or of firms to hire long-term unemployed with the subsidy were observable, one could systematically correct for selection bias by a selection-on-observable strategy. Nevertheless, even in this case disentangling employment and subsidy

effects remains the major challenge. The approach we propose does not require observability of all drivers of selection and uses a different comparison group. The estimated policy parameter (eligibility for the benefit) is different from the one estimated with matching approaches (effect of actual receipt of benefit). We argue that the parameter we estimate is the more relevant policy parameter. Wage subsidies cannot be mandated because they require the willingness of an employer to hire a subsidized worker. Hence, policy makers can only provide the *option* of granting a subsidy.

References

Angrist, J. D., and Krueger, A. B. (1999). Empirical strategies in labor economics. *Handbook of Labor Economics*, *3*, 1277-1366.

Angrist, J. D., and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. New York: Princeton University Press.

Bell, B., Blundell, R., and Van Reenen, J. (1999). Getting the Unemployed Back to Work : The Role of Targeted Wage Subsidies. *International Tax and Public Finance*, *6*, 339-360.

Bernhard, S., Gartner, H., and Stephan, G. (2008). Wage subsidies for needy job-seekers and their effect on individual labour market outcomes after the German reforms. *IZA Discussion Paper,* 3772.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics*, 119, 249-275.

Blundell, R., and Costa Dias, M. (2009). Alternative Approaches to Evaluation in Empirical Microeconomics. *The Journal of Human Resources*, *44, 565-640*.

Blundell, R., and Hoynes, H. (2001). Has "In-Work" Benefit Reform Helped the Labor Market? *NBER Working Paper Series,* 8546.

Boockmann, B., Zwick, T., Ammermüller, A., and Maier, M. (2007). Do hiring subsidies reduce unemployment among the elderly? Evidence from two natural experiments. *ZEW Working paper,* 07-001.

Bundesanstalt für Arbeit (2003). Arbeitsmarkt 2002. Amtliche Nachrichten der Bundesanstalt für Arbeit, 51. Jahrgang, Sondernummer, 18. Juni 2003.

Burtless, G. (1985). Are Targeted Wage Subsidies Harmful? Evidence from a Wage Voucher Experiment. *Industrial and Labor Relations Review*, *39*, 105-114.

Card, D., and Hyslop, D. R. (2005). Estimating the effects of a time-limited earnings subsidy for welfare-leavers. *Econometrica*, *73*, 1723-1770.

Carling, K., and Richardson, K. (2004). The relative efficiency of labor market programs: Swedish experience from the 1990s. *Labour Economics*, *11*, 335-354.

Donald, S. G., and Lang, K. (2007). Inference with Difference-in-Differences and other Panel Data. *The Review of Economics and Statistics*, *89*, 221-233.

Duncan, A., Shephard, A., Brewer, M., and Suárez, M. J. (2006). Did working families' tax credit work? The impact of in-work support on labour supply in Great Britain. *Labour Economics*, *13*, 699-720.

Eissa, N., and Hoynes, H. W. (2004). Taxes and the labor market participation of married couples: the earned income tax credit. *Journal of Public Economics*, *88*, 1931-1958.

Hahn, J., Todd, P., and van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, *69*, 201-209.

Hamersma, S. (2008). The Effects of an Employer Subsidy on Employment Outcomes: A Study of the Work Opportunity and Welfare-to-Work Tax Credits Abstract. *Journal of Policy Analysis and Management*, *27*, 498-520.

Hollenbeck, K. M., and Willke, R. J. (1991). The Employment and Earnings Impacts of the Targeted Jobs Tax Credit. *W.E. Upjohn Institute for Employment Research,* Kalamazoo Mich.

Huber, M., Lechner, M., and Wunsch, C. (2013). The Performance of Estimators Based on the Propensity Score, *Journal of Econometrics*, 175, 1-21.

Huttunen, K., Pirttilä, J., and Uusitalo, R. (2013). The Employment Effects of Low-Wage Subsidies. *Journal of Public Economics*, 97, 49-60.

Imbens, G. W., and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, *142*, 615-635.

Imbens, G.W. (2004). Nonparametric Estimation of Average Treatment Effects under Exogeneity: A Review. *The Review of Economics and Statistics*, 86, 4-29.

Imbens, G. W., and Wooldridge, J. M. (2009). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature*, *47*, 5-86.

Jaenichen, U., and Stephan, G. (2011). The Effectiveness of Targeted Wage Subsidies for Hard-to-place Workers. *Applied Economics*, 43, 1209-1225.

Kaldor, N. (1936). Wage Subsidies as a Remedy for Unemployment. *The Journal of Political Economy*, 44, 721-742.

Katz, L. F. (1998). Wage Subsidies for the Disadvantaged. In R. B. Freeman and P. Gottschalk (Eds.), *Generating Jobs: How to increase demand for less-skilled workers*. The Russell Sage Foundation, New York, pp. 21-53.

Lechner, M., and Melly, B. (2010). Partial Identification of wage effects of training programs. *Working Paper Brown University,* 2010-8.

Lechner, M., Miquel, R., and Wunsch, C. (2011). Long-Run Effects of Public Sector Sponsored Training in West Germany. *Journal of the European Economic Association*, *9*, 742-784.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *The Review of Economic Studies*, *76*, 1071-1102.

Liebman, J. B., and Eissa, N. (1996). Labor Supply Response to the Earned Income Tax Credit. *Quarterly Journal of Economics*, *111*, 605-637.

Lorenz, E. C. (1988). The Targeted Jobs Tax Credit in Maryland and Missouri 1982-1987. *National Commission for Employment Policy*.

MacKinnon James G. (2006): Bootstrap Methods in Econometrics, *The Economic Record*, 82, S2–S18.

Meyer, B. D. (1995). Lessons from the U.S. Unemployment Insurance Experiments. *Journal of Economic Literature*, *33*, 91-131.

Meyer, B. D. (1996). What Have We Learned from the Illinois Reemployment Bonus Experiment? *Journal of Labor Economics*, *14*, 26-51.

Neubäumer, R. (2010). Can Training Programs or Rather Wage Subsidies Bring the Unemployed Back to Work? A Theoretical and Empirical Investigation for Germany. *IZA Discussion Paper,* 4864.

Perloff, J. M., and Wachter, M. L. (1979). The new jobs tax credit: An evaluation of the 1977-78 wage subsidy program. *American Economic Review*, *69*, 173-179.

Phelps, E. S. (1994). Low-Wage Employment Subsidies versus the Welfare State. *The American Economic Review*, *84*, 54-58.

Sianesi, B. (2008). Differential effects of active labour market programs for the unemployed. *Labour Economics*, *15*, 370-399.

Snower, D. J. (1994). Converting Unemployment Benefits into Employment Subsidies. *The American Economic Review*, *84*, 65-70.

Thistlethwaite, D. L., and Campbell, D. T. (1960). Regression-Discontinuity Analysis: An Alternative to the Ex Post Facto Experiment. *The Journal of Educational Psychology*, 51, 309-317.

Van Reenen, J. (2004). Active labour market policies and the British New Deal for the young unemployed in context. In D. Card, R. Blundell, and R. B. Freeman (Eds.), *Seeking a Premier Economy: The Economic Effects of British Economic Reforms, 1980-2000*. University of Chicago Press for NBER, London, pp. 461-496.

Van der Klaauw, W. (2008). Regression–Discontinuity Analysis: A Survey of Recent Developments in Economics. *Labour*, *22*, 219-245.