Institute for Employment Research

The Research Institute of the Federal Employment Agency

IAB

IAB-Discussion Paper 5/2013

Articles on labour market issues

Subsidies for substitutes?

New evidence on deadweight loss and substitution effects of a wage subsidy for hard-to-place job-seekers

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ISSN 2195-2663

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

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Abstract

This paper estimates substitution effects of the German active labour market programme “JobPerspektive”', a wage subsidy for hard-to-place welfare recipients. Using a novel panel of the entire population of German establishments with full information on unsubsidized and subsidized employment, counterfactual employment levels of subsidized employers are modelled by way of matching on the propensity score of receiving the subsidy. Using the same method, the substitution of particular groups of workers, including those subsidized by other programme types, is investigated as well. Results provide little evidence for widespread substitution of regular workers due to receiving this particular subsidy; in fact, regular employment is somewhat increased in West Germany as a result of subsidization. Furthermore, JobPerspektive is being used by employers in lieu of wage-paying work opportunities, and receiving JobPerspektive causes establishments to employ more workers with regular hiring subsidies.

Zusammenfassung

Mit dem Lohnkostenzuschuss „JobPerspektive“ konnte von Oktober 2007 bis Ende 2011 die Beschäftigung von besonders schwer vermittelbaren Arbeitssuchenden gefördert werden. Stellte ein Arbeitgeber einen langzeitarbeitslosen erwerbsfähigen Leistungsberechtigten ein, dessen Vermittlung durch zwei in dessen Person liegende Vermittlungshemmnisse erschwert war, konnte er bis zu 75 Prozent der Lohnkosten vom Staat bezuschusst bekommen, dies zunächst für maximal zwei Jahre. War nach spätestens 24 Monaten Förderung abzusehen, dass die geförderte Person weiterhin nicht innerhalb der nächsten zwei Jahre auf dem ersten Arbeitsmarkt vermittelbar sein würde, konnte die Förderung sogar unbefristet gewährt werden. Die so Geförderten sollten dazu verwendet werden, die Geschäftstätigkeit in bislang nicht rentable Bereiche oder Umfänge auszuweiten.

Durch die großzügige Förderung war es jedoch denkbar, dass durch die Bezuschussung ungeförderte Beschäftigung abgebaut oder weniger stark aufgebaut wird; ist dies der Fall, so spricht man von Substitutionseffekten. In diesem Papier wird die Wirkung der Förderung eines Betriebes mit „JobPerspektive“ auf die ungeförderte Beschäftigung im selben Betrieb untersucht. Diese Wirkung wird identifiziert, indem mittels Propensity Score Matching kontrafaktische Beschäftigungsentwicklungen der Förderbetriebe modelliert werden. Die Ergebnisse bieten nur wenige Indizien für eine Substitution ungeförderter Beschäftigung; tatsächlich nimmt die ungeförderte Beschäftigung durch die großzügige Förderung sogar etwas zu. Durch die Förderung mit diesem Zuschuss werden allerdings weniger Teilnehmer an Arbeitsgelegenheiten der Entgeltvariante beschäftigt. Weiterhin geht mit einer JobPerspektive-Förderung auch eine erhöhte Förderung mit Eingliederungszuschüssen einher.

JEL classification: H25, M51, C31

Keywords: wage subsidies, disadvantaged workers, substitution effects

Acknowledgements

I would like to thank Joachim Wolff, Claus Schnabel and the participants of the 1st Potsdam PhD Workshop on Empirical Economics for their very helpful comments. Financial support by the Federal Ministry of Labour and Social Affairs under the project “Evaluation der Leistungen zur Beschäftigungsförderung nach § 16e Abs. 10 SGB II” is gratefully acknowledged. All errors are mine.

1 Introduction

A common instrument of many a country's active labour market policy (ALMP) regime are wage subsidies paid to employers for hiring disadvantaged job-seekers. Since the late 1980s, many evaluation studies on wage subsidies have attempted to quantify their deadweight losses, substitution and displacement effects. The consensus was that wage subsidies suffer from considerable deadweight loss and substitution effects, more so than any other programme type. For this reason, restrictive targeting and a more parsimonious use in general are often recommended (e. g. Martin/Grubb 2001; OECD 1993).

However, as Calmfors/Forslund/Hemström (2001) have pointed out, these results are highly sensitive to the evaluation methodology used, with macroeconometric impact analysis finding systematically higher estimates of deadweight loss and substitution effects than survey studies. In line with these findings, more recent studies using large establishment-level data sets to model counterfactual employment or wage levels of subsidized establishments (Hohendanner 2011; Rotger/Arendt 2010; Kangasharju 2007) find far lower, in some cases insignificant, deadweight losses and substitution effects. Furthermore, very few studies have ever considered which socio-demographic worker groups in particular are prone to being substituted by subsidized workers, usually because suitable data was not available.

This paper contributes to the existing literature on wage subsidies in four ways. First, the particular subsidy being analyzed uniquely combines the elements of very generous subsidization with very restrictive targeting. This gives rise to opposing expectations from economic theory, so that the effect on regular employment is a priori unclear. Second, given that more recent studies from Scandinavia found few negative effects of employment subsidies on regular employment, thereby contradicting much of the older literature, this paper provides further evidence through the modelling of counterfactual employment levels of subsidized establishments by matching on the propensity score of receiving the subsidy, making use of a population-size panel of German establishments based entirely on high-quality administrative data. Third, this paper not only considers the effect of subsidization on particular sub-groups of regular workers in the same establishment, such as older workers, but also estimates how the use of one subsidy affects the utilization of other programme types. Fourth, the effect of subsidization is estimated separately for several intensities of treatment, a first in establishment-level analyses of active labour market policy programmes.

The paper is laid out as follows: Section 2 describes the institutional background of the JobPerspektive subsidy. Section 3 derives hypotheses regarding the subsidy's substitution effects from theoretical considerations on employment subsidies. Section 4 discusses previous empirical work on deadweight loss, substitution and displacement effects of wage subsidies and explains why further research is needed. Section 5 explains the econometric strategy and describes the data set being used.

Section 6 presents the estimation results, which are interpreted in Section 7. Section 8 concludes.

2 Institutional background

In January 2005, Germany reformed its welfare regime with the introduction of a purely means-tested benefit, the unemployment benefit II. Implementing the paradigm of "activation", welfare benefit receipt is tied to a system of "enabling and demanding". Enabling means helping an unemployed welfare recipient to find a job through the use of improved placement services, training programmes and job creation schemes. Demanding means requiring their cooperation through the threat of benefit sanctions in the case of noncompliance. The goal is to reintegrate long-term unemployed persons who have long gotten used to not working and to receiving welfare benefits, and to prevent such a condition from forming in persons who only recently became unemployed. While particular policies have been the subject of criticism, these "Hartz" reforms – named after the chairman of the commission that recommended its policies – overall have been judged moderately successful (Fahr/Sunde 2009; Koch/Kupka/Steinke 2009; Jacobi/Kluve 2007).

One shortcoming demonstrated has been the lack of improvement of the situation of extremely difficult-to-place welfare recipients. These are long-term unemployed persons (unemployed for more than one year) who suffer from employment impediments such as low qualification, old age, disability or lack of German language skills. For job-seekers with such impediments, regular activation efforts to get them into unsubsidized employment have failed to achieve that goal (e. g. Wübbeke 2011, 2007; Koch/Kupka/Steinke 2009; Schels 2008; Deeke 2007). These results have met three responses. First, activation of these groups may simply need more time than the period available for study. In particular, person who have never worked, or have not worked in a long time, may require a sequence of activation measures that may well span several years. That sequence would start with work opportunities to learn to maintain a daily schedule and proper work habits, followed by training measures first providing general, then job-specific qualifications, concluded by subsidized employment that may lead to regular employment with the same or a similar employer (see e. g. Dengler/Hohmeyer 2010; Lechner/Wiehler 2007). Second, regular activation efforts may fail to provide the specific interventions that would be appropriate for particular disadvantaged groups. For example, as Rauch/Dornette (2010) point out, disabled welfare recipients are subject to contradictory regimes for activation (Social Code II) and "rehabilitation and participation"' (Social Code IX). Third, regular employment might not be the only outcome variable of interest. For some groups that have very low chances of regular employment, other outcomes such as participation in society should be considered more important.

For these groups, „JobPerspektive"' („Leistungen zur Beschäftigungsförderung'", short name: „Beschäftigungszuschuss", BEZ) was launched in October 2007. Assuming that there are some people who will never find unsubsidized employment and achieve high enough earnings to leave welfare, JobPerspektive is a subsidy

available to employers willing to employ such people at local wage (or union wage, if applicable), eliminating the need for regular welfare payments, and triggering payments to social security. Designed to compensate the employer for the lack of productivity of such workers, depending on how productive these workers are assumed (and later are found) to be, up to 75 percent of total wage costs are reimbursed.[1] The subsidy is granted initially for a maximum of 24 months, after which the subsidy may be extended permanently if the worker is judged to be unable to find a regular job within the next 24 months. For this reason, this programme is properly called a wage subsidy rather than a hiring subsidy, and consistent with Orszag/Snower (2003: 565, 569), it is designed for those whose productivity is unlikely to increase on-the-job ("dead-end workers").

Subsidized workers are not supposed to replace regular workers, instead, employers are expected to employ them for the purpose of expanding their business activities by a magnitude, or into economic fields, that would otherwise not be profitable or economical (Federal Employment Agency 2007). To reduce the likelihood of abuse of this programme given its generosity, two restrictive targeting methods are used. First, the programme is by statute only available to persons who are unemployed for more than one year and suffer from two additional employment impediments. Second, the subsidy is supposed to be only available for persons who after six months of intensified activation efforts are found to be unemployable within 24 months. This "activation phase" is not that much different from the regular activation efforts that apply to all German welfare recipients, and in practice, such regular efforts are often retroactively considered to fulfill the activation requirements once the decision to place a person using the JobPerspektive subsidy is made (ISG/IAB/RWI 2011). A third restriction was only in place from October 2007 until the end of March 2008: that the jobs subsidized with JobPerspektive had to be "additional"' and in the "public interest"' (Art. 71 Social Code II). The same requirement restricts Job Creation Schemes and One-Euro-Jobs, which are described below.

Among those regular activation efforts are several types of training programmes, job creation schemes and hiring subsidies. Of particular interest are those that like JobPerspektive involve employment with an obligation to pay social security contributions (henceforth labeled contributory employment).[2] The first are regular job creation schemes ("Arbeitsbeschaffungsmaßnahmen", ABM) that have been extensively described in many other studies (Caliendo/Hujer/Thomsen 2008; Thomsen 2007). The second are wage-paying work opportunities ("Arbeitsgelegenheiten der Entgeltvariante", AGH-E) that have existed with few participants since 2005 but since 2009 have come to replace the older job creation schemes. The third are hiring subsidies

[1] ISG/IAB/RWI (2011)'s implementation analysis of JobPerspektive subsidizations using process data from the Federal Employment Agency from October 2007 to March 2009 indicate that the full 75 percent reimbursement is granted in about 90 percent of all cases.

[2] Wage-paying employment amounting to less than 400 euros a month is not considered contributory employment and is referred to as minor employment.

("Eingliederungszuschüsse", EGZ) paid to employers hiring job-seekers meeting certain criteria of being disadvantaged such as old age or other employment impediments. Unlike the JobPerspektive wage subsidy, these hiring subsidies are paid only for a limited period (6 to 24 months), reflecting the assumption that after a certain time of on-the-job training, these workers' productivity will rise to a level that the employer no longer needs to be reimbursed.

The largest programme, in terms of the number of participants, of the activation regime is a workfare programme named "work opportunities with an allowance for additional expenses" ("Arbeitsgelegenheiten mit Mehraufwandsentschädigung", AGH-M), usually referred to by the simpler term One-Euro-Job. While these may take place in establishments as well, they only yield an additional payment of slightly above one euro per hour on top of continued welfare payments; as they do not involve social security contributions, the data used in this paper contains no information on them.

In 2012, Germany's conservative-liberal government retooled all ALMP programmes to improve their efficiency. The JobPerspective subsidy was merged with wage-paying work opportunities into "subsidized employment schemes". Subsidization is now strictly limited to 24 months within a five-year period, thus dropping the previously available option of permanent subsidization.[3]

Table A.1 shows some of the personal characteristics of those individuals who received the subsidy. Of particular interest is the last row, indicating the share of subsidizations whose total duration is longer than two years which, according to the subsidy design, is equivalent to an indefinite subsidization period. Since this permanent extension may only occur after two years of subsidization, is not surprising that the share is very low for subsidizations that started in 2010 or later.

3 Theoretical aspects of targeted wage subsidies

3.1 Taxonomy

Definitions of and the demarcation between deadweight loss and substitution effect vary throughout the ALMP literature, especially between individual-level, firm-level and macroeconomic points of view and whether the unit of analysis is individuals or jobs. Starting with the macroeconomic point of view of Calmfors (1994), deadweight loss is defined as "hirings from the target group that would have occurred also in the absence of the programme" (Calmfors 1994: 17), whereas substitution effects are defined "as the extent to which jobs created for a certain category of workers simply replace jobs for other categories, because relative wage costs are changed" (ibid.). Transferring these definitions to a micro level of establishments or job-seekers leads

[3] JobPerspektive was originally in Article 16a Section 10 of Social Code II, then moved to Article 16e Social Code II in January 2009, where it remained (under the different name "subsidized employment schemes") even after 2012's retooling.

to a somewhat inconsistent use of the terms: the situation in which a subsidized job would exist as an unsubsidized job, and the attributes of the worker are not controlled for, is sometimes considered deadweight loss[4], sometimes a substitution effect[5]. The latter understanding is based on the assumption that it is only deadweight loss if the job would have been given to exactly the same person, otherwise it is substitution. The former understanding on the other hand is based on the assumption that it's only substitution of the job would have been given to a person with identifyably different characteristics (different "categories"' of workers in Calmfors 1994; OECD 1993). Therefore, when not controlling for job holder characteristics, it is conceptually impossible to assign a loss of regular employment to either deadweight loss or substitution effect. Any estimated loss of regular employment caused by subsidization will therefore be a sum of deadweight loss and substitution effects.

3.2 Theory

3.2.1 Deadweight loss

The following theoretical considerations are based on that definition of deadweight loss under which given that subsidization occurs, the same person would have been hired without the subsidy. To explain deadweight loss according to this definition, our view can be restricted to only the employer and that particular job applicant; other applicants only need to be taken into account once substitution comes into the picture. With the applicant being hired with or without the subsidy and the subsidy therefore being irrelevant to the hiring decision, subsidy duration and amount therefore cannot explain the occurrence of deadweight loss. The employer's hiring decision will instead be based on criteria such as the applicant's assessed productivity. Deadweight loss therefore occurs if the applicant's productivity is already sufficient for the job. This means that deadweight loss will be more likely …

1. … the higher a worker's assessed productivity is,

2. … the lower a job's productivity requirement is.

Statement 1 provides the economic justification for targeting employment subsidies to disadvantaged job-seekers, as they are assumed to be so unproductive that makes it unlikely to be sufficient for most regular jobs. As for statement 2, a job requiring almost no qualifications can be filled even by some of the most unproductive applicants, hence, offering employment subsidies for those jobs is likely to lead to deadweight loss.

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4 e. g. in Martin/Grubb (2001: 20): "whether the subsidized jobs would have been created anyway in the absence of the subsidy"

5 e. g. in Layard/Nickell/Jackman (1991: 477 f.): "some of those recruited merely replace others whom the firm would have recruited instead"

This simple description assumes that every job-seeker's productivity will be assessed by an employer. In practice, this often will not be the case, as assessing applicants is costly. Welters/Muysken (2008, 2006) argue based on a sequential employer search model that firms will employ a screening device standard to screen out applicants that are obviously inadequate so that no costly assessment has to take place. One criterion for the screening device standard is likely to be a job-seeker's duration of unemployment. Thus, if a job-seeker has to be unemployed an amount of time to qualify for a hiring subsidy but an employer already screens out any applicants who are unemployed that long, no deadweight loss can occur. Thus predicting the screening device standard can only predict whether subsidized hirings will occur, not if deadweight loss will occur given that a subsidized hiring occurs.

3.2.2 Substitution

As Calmfors (1994) explains, substitution across worker categories occurs because of a change in relative wage costs. Hujer/Caliendo/Radic (2001) use a simple static model of labour demand with no adjustment costs and two groups of workers (high- and low-skilled) drawing on Hamermesh (1993): Subsidization of low-skilled workers increases demand for them (see Katz 1998) while ceteris paribus decreasing demand for high-skilled workers (substitution effect); lower production costs decrease output prices, increasing output demand and therefore creating a scale effect, potentially increasing the demand for high-skilled workers, with the total effect a priori unknown. The consideration of adjustment costs – with the assumption that adjustment takes longer and is more expensive for high-skilled workers – gives rise to the expectation that a reduction in high-skilled employment would manifest itself only after some time. Kettner/Rebien (2007)'s taxonomy of three types of substitution (direct, indirect and delayed substitution) in the context of wage subsidies and job creation schemes adds further nuance to the question to what extent and how quickly a substitution of regular workers should be expected:

Direct substitution occurs when a regular worker is fired and a subsidized worker is hired to fill the regular worker's place. Without the subsidy, the regular worker would continue to be employed. Such an exchange of workers induces considerable replacement costs, including severance pay, search, hiring and on-the-job training costs. The subsidy largely ends up being used by the employer to compensate the replacement costs, leaving only a small part left for its intended purpose, namely, to compensate a subsidized hire's lack of productivity. Therefore, direct substitution is more likely …

1. … the lower the replacement costs are,

2. … if the subsidized hires are good substitutes.

With regards to statement 1, replacement costs will be low if training costs are low, that is, for jobs that require little establishment-specific capital, and if there are few workers with long tenure that would trigger high severance payments. The higher

and the longer the subsidization, the more replacement costs can be compensated, and the more likely direct substitution becomes. As for statement 2, the higher and the longer the subsidization, the more of the subsidy remains, after making up for replacement costs, to be used for compensating a hire's lack of productivity, allowing the hiring of individuals that are not good substitutes, and the more likely direct substitution becomes.

Indirect substitution occurs when an existing vacancy is filled with a subsidized worker that, without the subsidy, would have been filled by a different applicant. Assuming that all applicants to a job vacancy are ranked by the employer according to assessed productivity, the prospect of subsidization increases an applicant's rank position among all applicants. However, a stigmatization effect may work in the opposite direction: the offer of a substantial subsidy amount may cause an employer to become wary about that applicant; his or her productivity might be assessed lower than it would be without the subsidy being available (see also Burtless 1985). Therefore, higher and longer subsidization should lead to more indirect substitution as the job-seeker's rank position among applicants increases up to the point where stigmatization dominates.

Delayed substitution occurs when subsidized hires are initially additional, but are trained by regular workers to perform their tasks, who in turn are made redundant. It can be seen as a variant of direct substitution with the regular workers being retained until the subsidized hires are ready to take up their work in order to reduce replacement costs. Subsidized hires need not be good substitutes right away, they only need to possess the potential to become good substitutes after training.

Targeting hard-to-place individuals can reduce substitution effects if the subsidized workers then will be too different from regular workers to serve as substitutes for them, even with the subsidy. Both direct and indirect substitution are not always considered a negative: direct substitution, or the threat of it, might serve to reduce insider behaviour (Möller 2005: 178). Furthermore, a redistribution of employment opportunities for the benefit of hard-to-place job-seekers as a result of substitution effects might be considered acceptable (OECD 1993), especially if those substituted manage to find a job elsewhere in the economy that otherwise would not exist at all or could not have been filled.[6]

3.2.3 Further additional employment

In addition to scale effects due to lower production costs mentioned before, a subsidized job can be additional in an establishment if a vacancy would remain unfilled without the subsidy, because an employer is unable to find a sufficiently productive

[6] Note that a substitution effect from an establishment-level point of view would appear as additional employment from an economy-wide point of view if the person being substituted finds a job elsewhere that otherwise would not have been created or filled.

applicant. This is more likely if unemployment is low and there are few job-seekers per vacancy, and for jobs with high productivity requirements.

3.2.4 Implications for the JobPerspektive subsidy

For the JobPerspektive subsidy, the following predictions can be made based on these considerations.

Given the restrictive targeting, deadweight loss should be low (but not zero), as it is unlikely that those hard-to-place individuals would have been hired in the absence of the subsidy. At least for establishments in sectors where employers routinely qualify for public works subsidization programmes, some deadweight loss should be expected however, as it is these sectors where unsubsidized jobs for low-skilled workers would be expected to exist. Another source of deadweight loss might be “creaming”, in which job centre workers deliberately try to subsidise easily-employable individuals whose subsidisation is most likely to be judged a “success”.

Also because of the rather unproductive persons being targeted, direct or indirect substitution of regular workers is unlikely because at the time that they are hired, they are unlikely to be good substitutes for regular workers, and the high subsidization amount is more likely to stigmatise them than turn them into good substitutes. Some amount of delayed substitution should be expected however given that implementation analyses show that it is often individuals with a vocational degree that are subsidized because of other employment impediments (in particular, age 50 years and older, see Table A.1 and Koch/Kupka/Steinke 2010); they can be assumed to possess a considerable amount of productivity potential.

Finally, adding another programme to the already complex landscape of activation programmes implies a considerable potential for replacing workers receiving one type of subsidy with another. In particular, direct or indirect substitution of job creation schemes and wage-paying work opportunities should be expected, as these, like the JobPerspektive, usually involve tasks with low productivity requirements.

4 Previous research

Macroeconometric estimates of programme impact using regional time-series data (e. g. Dahlberg/Forslund 2005; Hagen 2004; Pehkonen 1997) measure a composite effect on unsubsidized employment explained by, among other variables, the programme intensity (e. g. the number of participants), with a negative coefficient for programme intensity considered evidence for deadweight loss, substitution effect or displacement effect, with the contribution of each of these effect types to the composite employment effect unknown. Estimates of deadweight loss and substitution effects are prone to be biased upwards due to simultaneity bias (Calmfors/ Forslund/Hemström 2001), that is, that changes in the programme intensity explaining the level of regular employment may not only cause but may be caused by changes in regular employment. They furthermore require programmes of a medium to large scale with sufficient variation in time and across regions, making them un-

suitable for programmes with a small number of participants, such as the JobPerspektive subsidy.

Studies that employ direct interviewing (e. g. Welters/Muysken 2006) simply ask employers or participants if the subsidized job would have existed without the subsidy and if so, what kind of person would have been hired instead – the same (then considered deadweight loss) or another (then considered a substitution effect). Such assessments are often difficult to make especially for participants, while employers might not admit to accepting a subsidy for an employee they would have hired anyway, even in anonymous surveys, resulting in a downward bias of the measured deadweight loss or substitution effect. Reasons for employers for not answering honestly are fear of loss of anonymity as well as worrying that the programme might be abolished if too many employers admit to abusing it. Rarely discussed is the problem that actual hiring decisions are based on ex ante assumptions about a worker's productivity, which will strongly be influenced by easily observable employment impediments, whereas post-subsidization interviews are usually based either entirely or in part on ex post experiences of how well an employee has actually worked. Hence, ex-post assessments of deadweight loss and substitution are biased upwards if employers initially underestimate a job-seeker's actual productivity. Substitutions in the form of subsidized workers being additional while they are being trained on-the-job by regular workers who are subsequently fired later ("delayed substitution") are not covered by the way questions are usually framed either. Finally, direct interviewing studies will necessarily neglect the scale effect described in Section 3.2.2 while econometric studies measuring regular employment will invariably include both effects in their net effect estimate on regular employment.

A third type of studies uses data on individuals treated with a particular programme in treatment effects models, where the share of (matched) control group members for whom a positive employment outcome is observed is interpreted as a measure of deadweight loss "in a wider sense" (Jaenichen/Stephan 2011; Winterhager/Heinze/Spermann 2006). Such an approach cannot identify substitution effects however, as it is invariably limited to individuals having similar personal characteristics.

Establishment-level substitution effects (together with deadweight loss in a composite effect estimate) may be identified however by modelling counterfactual employment levels of establishments, as suggested in Maré (2005) and described in detail in section 5. Hohendanner (2011) uses the IAB Establishment Panel (Fischer et al. 2008), a large survey of German establishments, with about 1,000 treated establishments from 2004 to 2007 to estimate replacement of regular with subsidized jobs caused by the workfare programme "One-Euro-Jobs" that was introduced in 2005 by modelling counterfactual changes in regular employment using propensity score matching of establishments. He finds no such replacement provided that substitution of job creation schemes with the workfare programme is controlled for.

In a similar manner using difference-in-difference propensity score matching, Rotger/Arendt (2010) use monthly register data of 2,802 Danish firms subsidized with a wage subsidy during the year 2006 to study its impact on hirings and firings of regular workers in the subsidized firms. They find almost no deadweight loss or substitution effects during the time that the subsidy is paid, but some regular workers were fired after the fixed-duration subsidization period has ended. This could be an example of "delayed substitution", that is, subsidized workers are initially complementary, receiving on-the-job training by regular workers who become dispensable after the subsidy has ended and are therefore laid off. Kangasharju (2007) on the other hand uses Finnish tax data from 1995 to 2002 from about 30,000 firms by modelling counterfactual wage levels. Finding that the total effect on payroll is as high as would be expected in the absence of deadweight loss, substitution and displacement effects, he concludes that wage subsidies have the intended employment effect in magnitude. Finally, Hujer/Caliendo/Radic (2001) also use difference-in-difference propensity score matching with West German establishments subsidized with various programmes from 1995 to 1999. However, they do not attempt to measure deadweight loss or substitution effects themselves; instead their rationale is to use establishment-level data to measure overall employment gains as a result of subsidization (finding none), assuming that substitution effects will "net out" at the establishment level. The low number of subsidized establishments analysed (87 total, 77 used) demonstrates the problem of using existing surveys of establishments that do not oversample subsidized establishments.

Summarizing the findings of the literature on deadweight loss and substitution effects of employment subsidies and other ALMP programmes, the estimation method used seems to have a decisive impact on the estimates, with macroeconometric studies finding the highest estimates, followed by survey studies (a systematic distinction explicitly mentioned in Calmfors/Forslund/Hemström (2001), and microeconometric studies modelling counterfactual employment or wage levels of establishments finding the lowest estimates of deadweight loss and substitution effects. Comparing the various programme types, employment subsidies seem to have among the highest combined deadweight losses and substitution effects of all active labour market policy programme types (Maré 2005; Martin/Grubb 2001; Calmfors/Forslund/Hemström 2001; Dar/Tzannatos 1999; OECD 1993), with "tight targeting"' and "close employer monitoring of employer behaviour" (Martin/Grubb 2011: 24) listed as measures to increase net employment gains.

5 Method and data

5.1 Basic framework for causal identification

Both establishment-level deadweight loss and establishment-level substitution effects can be thought of as a relative net loss of unsubsidized employment in the subsidized establishment when comparing to a counterfactual scenario without sub-

sidization.[7] Let outcome variable y_{it} denote the level of unsubsidized employment in establishment i at time t with subsidization status D_i, and τ_{it} the effect of subsidization on unsubsidized employment. The notation convention used follows that of Caliendo/Kopeinig (2008).

In a Roy-Rubin model of potential outcomes (Roy 1951, Rubin 1974), the effect of a wage subsidy ("treatment") on a particular subsidized establishment ("unit") i is causally identified as the difference of the potential outcome in the case of subsidization $y_i(1)$ and the potential outcome without subsidization $y_i(0)$. $D_i \in \{0,1\}$ refers to whether the establishment actually does receive "treatment" in the form of subsidization:

$$y_{it} = D_i y_{it}(1) + (1 - D_i) y_{it}(0) \quad (1)$$

The effect at time t of subsidization on establishment i is therefore:

$$\tau_{it} = y_{it}(1) - y_{it}(0) \quad (2)$$

If a subsidized job is additional, the unsubsidized employment level will remain unchanged between the observed and counterfactual scenario, so $\tau_{it} = 0$. With one single worker subsidized, if the subsidized job would have existed as an unsubsidized job without the subsidy, the unsubsidized level will be higher in the nontreatment scenario by one unit, so $\tau_{it} = -1$, for two substitutive subsidized jobs, $\tau_{it} = -2$. All of this so far neglects the issue of scale effects; they would increase observed employment levels in the subsidized establishment, thereby raising τ_{it}, which therefore can be positive as well. This framework can be extended to substitution across specific categories of workers by choosing the employment level of the worker group being replaced as the outcome variable. Note that t only indicates a measurement point in time; the framework does not imply that subsidization continues throughout observed time.

Equation (2) indicates that both potential outcomes must be known for each establishment i at the same time t. The fact that it is impossible to observe the same unit at the same time both with and without treatment is commonly referred to as the fundamental evaluation problem. It is not solved by observing the same unit at different points in time (for example, before and after treatment), as the outcome would have changed in time even without treatment. For this reason, an individual treatment effect cannot be measured. However, an average treatment effect might be measured under certain conditions. This average treatment effect is defined as follows:

[7] For this basic framework, employment stock is chosen as outcome variable for simplicity's sake. The actual analysis in this paper will measure job flows instead of employment stock.

$$\tau_{ATE,t} = E(\tau_t) = E\big(y_t(1) - y_t(0)\big) \tag{3}$$

Only considering those units which actually do receive treatment produces the average treatment effect on the treated:

$$\tau_{ATT,t} = E(\tau_t|D = 1) = E(y_t(1)|D = 1) - E(y_t(0)|D = 1) \tag{4}$$

$E(y_t(0)|D = 1)$, the counterfactual outcome for treated units, is not observed. What is observed however is the actual outcome for non-treated ("control") units, $E(y_t(0)|D = 0)$. Identifying the average treatment effect on the treated is possible if both are the same, $E(y_t(0)|D = 1) = E(y_t(0)|D = 0)$. This will be true in the case of experimental studies with random assignment to treatment and nontreatment. For non-experimental studies, using $E(y_t(0)|D = 0)$ as a proxy for the counterfactual outcome produces selection bias of magnitude $E(y_t(0)|D = 1) - E(y_t(0)|D = 0)$ unless unconfoundedness holds, meaning that no systematic differences with regards to observable characteristics X in outcomes exist between treated and non-treated units other than treatment status:

$$Y(0), Y(1) \perp D|X \tag{5}$$

For the purpose of identifying the average treatment effect on the treated, a weak unconfoundedness assumption is sufficient:

$$Y(0) \perp D|X \tag{6}$$

Other terms for this unconfoundedness assumption are conditional independence assumption (Lechner 1999) and selection on observables (Heckman/Robb 1985, in Caliendo/Kopeinig 2008). However, conditioning on the exact values of all observable characteristics X (exact matching) introduces a severe problem of dimensionality. As Rosenbaum/Rubin (1983) show, balancing on a propensity score of receiving treatment $P(X) = P(D = 1|X)$ solves the dimensionality problem while maintaining (weak) unconfoundedness:

$$Y(0) \perp D|P(X) \tag{7}$$

Therefore, by matching each treated unit to a control unit with a similar propensity score, and using this matched control group as the counterfactual outcomes, the average treatment effect on the treated may be identified. In additional to the conditional independence assumption described, two more assumptions must be fulfilled: the outcome of one unit may only depend on that unit's treatment value and not on another unit's; this is known as the Stable Unit Treatment Value Assumption (SUTVA; Rubin 1986, 1980). Finally, effects are only identified for treated units for whom a matched control group observation having a similar propensity score actually exists (common support condition).

5.2 Data

Due to the comparatively low number of individuals subsidized with JobPerspektive (about 35,400 in June 2009), leading to an even lower number of subsidized establishments (about 12,500 in June 2009), achieving adequate statistical certainty necessitates the use of administrative data when analyzing establishment-level effects of the subsidy. The widely-employed IAB Establishment Panel survey data set (Fischer et al. 2008), for example, uses a disproportionately drawn one percent sample of all German establishments, resulting in a mere 331 subsidized establishments in June 2009. Moreover, no information on the personal characteristics of the subsidized individuals is available.

Therefore, the following analyses use a population-size data set that in this form has not been used for Germany. An extension of the Establishment History Panel from the Research Data Centre of the German Federal Employment Agency (Hethey-Maier/Seth 2010), it contains data on all German establishments for the years 2000 to 2010.[8] Aggregating individual employer reports to the German social security adminstration by the establishment identifier, the panel contains one observation per establishment and year with information valid at each June 30th. Establishments are characterized by size, sector, years since founding, employment structure (number of employees of various age groups, qualifications and nationalities) and wage distribution. This data set is extended for the analyses of this paper by the share of employees who receive welfare benefits while working and, most importantly, number of employees subsidized with different kinds of subsidization schemes that are paid to the employer. In addition to this stock data, both inflow and outflow numbers are available for most characteristics from year to year and from quarter to quarter for contributory employment. Using the NUTS level 3[9] district and sector identifiers, additional sector and district labour market data are added.

5.3 Implementation

As suggested in Maré (2005) and previously employed by Hohendanner (2011) with survey data and Rotger/Arendt (2010) with administrative data, this paper will identify a composite estimate of both deadweight loss and the substitution of regular workers by modelling counterfactual employment levels for subsidized establishments.

Subsidized establishments are matched to one or more non-subsidized establishments using the propensity score (Rosenbaum/Rubin 1983) of receiving a subsidy so that selection into subsidization (treatment) is independent conditional on ob-

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8 A firm may consist of several establishments. There is no publicly-available data set that would allow linking several establishments to one common firm.

9 The Nomenclature of Territorial Units for Statistics (NUTS) is a classification of territorial units (geocode) established by the European Union for its member states. NUTS level 3 is the lowest level describing the smallest regional units. The Federal Republic of Germany consists of 429 “Kreise” (districts) on NUTS level 3.

servable establishment characteristics (measured on June 30th 2007, before the subsidy was introduced on October 1st). Previous analyses of establishment utilization of wage subsidies in Germany have indicated the following:

Hartmann (2004) uses the IAB Establishment Panel to analyse determinants of various wage subsidy scheme utilization for German establishments in 1999. Wage subsidy use is more likely the larger the establishment, with rising output and in growing establishments. Bellman/Stephan (2012) also use the IAB Establishment Panel using data from the years 2003, 2005, 2007 and 2009 to estimate determinants specifically of hiring subsidies ("Eingliederungszuschüsse"). They find similar determinants as Hartmann (2004), but add the following: Hiring subsidy use is more likely in East Germany, in the sectors retail, construction, health and social work as well as business services. Establishments that receive hiring subsidies have a less skilled workforce, shorter average tenure and fewer women; they are less likely to be bound by collective wage agreements but are more likely to have an in-house union wage agreement (and therefore a works council), a finding mostly driven by establishment size (Bellmann/Stephan 2012: 8). Hiring subsidy use is not only more likely in growing establishments but also in those with high fluctuation. Lastly, establishments using hiring subsidies are more likely to make use of other labour market programmes as well.

Almost all of the variables that have proven relevant for explaining the use of wage subsidies in German establishments in survey data are found in the administrative data set used in this analysis. The only exception is the presence of works councils and firm-level wage agreements, whose effect is of some statistical but little economic significance (Bellmann/Stephan 2012). This is more than compensated for by the inclusion of employment dynamics variables that are far more detailed and accurate than is conceivably possible with employer survey data. Thus, the following information is included in the specification to estimate the propensity score of taking up the subsidy:

- establishment size (number of employees), years since founding;
- sector (five-digit economic sector classification from the Federal Statistical Office (2008), a German extension of the pan-European four-digit NACE[10] code);
- 25 percent, median and 75 percent quartiles of full time employees' wages;
- share of employees by age and tenure;
- share of minor employed[11], women, part-time, foreign, welfare-receiving, subsidized employees;

[10] "Nomenclature statistique des activités économiques dans la Communauté européenne", Statistical Classification of Economic Activities in the European Community

[11] earning 400 euros or less a month

- share of low-, medium- and high-skilled employees[12];
- job flow rate (Burgess et al. 2000) of total, contributory, minor and subsidized employment; fluctuation of contributory and minor employment; rates are calculated on one-year, three-year and five-year bases;
- NUTS level 3 district labour market data: unemployment rate, share of long-term unemployed (one year or longer), labour market tightness (number of vacancies relative to number of unemployed persons).

By matching on the propensity score of receiving the subsidy, every subsidized establishment is matched to one or several non-subsidized establishments with a similar propensity of receiving the subsidy. This matched control group will serve as the group of counterfactuals for measuring the subsidy's effect on an outcome variable.

The outcome variable is the job flow (change in employment stock, see Burgess et al. 2000) from 2007 to 2010 of the allegedly replaced group, i. e. of unsubsidized workers when estimating combined deadweight loss and the substitution of regular workers. While one could simply use employment stock as an outcome variable, job flows being the difference of the employment stock of two time periods, or alternatively, hirings minus firings between two time periods, is a better measure of employer behaviour and is more common in the literature (Burgess et al. 2000: 474).

The Conditional Independence Assumption should hold because of the high number of relevant covariates used to estimate the balancing score that either directly or latently convey information affecting the employer's subsidized hiring and the subsidy granting decision while also having a conceivable influence on the outcome variables. For example, although an establishment's labour adjustment costs - which will likely determine the potential for substituting regular workers - are not directly observable, an establishment's share of churning among worker flows (fluctuation, see Burgess et al. 2000) will reflect high or low adjustment costs. Even though the number of job-seekers applying for a particular position is not observable, the number of vacancies per unemployed person as a measure of labour market tightness is observable and included in the specification of the propensity score. And, as mentioned before, virtually all important variables that have been shown to be relevant in previous studies on wage subsidy use in Germany using survey data are included as well.

Nevertheless, it can never be ruled out that even after conditioning on all these covariates, unobserved systematic differences between the treatment group of subsidized establishments and the control group might remain. In particular, ALMP pro-

[12] Low-skilled employees have neither a vocational degree nor an Abitur schooling degree. Medium-skilled employees have either a vocational degree or Abitur. High-skilled employees have a university or technical college degree.

grammes that take place in firms but do not trigger payments to (and therefore no registration with) the social security administration are not included in the data set that is based on social security records; this applies in particular to the work opportunity programme "One-Euro-Jobs"' (see Hohmeyer/Wolff 2012) and in-firm training measures (see Kopf 2009). If such unobserved systematic differences have an effect on the outcome variable[13], some selection bias will remain. If this effect however is fixed over time, using the Difference-in-Difference method will eliminate it. Therefore, the variable being estimated is not just the post-treatment job flow, but the post-treatment job flow minus the pre-treatment job flow:

$$DiD_i = JF_{2007..2010,i} - JF_{2004..2007,i} \quad (8)$$

Subsidized hirings that are additional will leave the job flow in the allegedly replaced group unaffected, while subsidized hirings that replace others will reduce the job flow of the allegedly replaced group in the subsidized establishment compared to the establishment in the control group. For analyzing age-specific substitution, the job flow of regular workers from ages 18 to 49 is considered. Finally, to account for substitution across programme types, that is, employees subsidized by JobPerspektive substitute workers subsidized with other programme types, job flows of workers subsidized with hiring subsidies ("Eingliederungszuschuss"), job creation schemes ("Arbeitsbeschaffungsmaßnahme") and wage-paying work opportunities ("Arbeitsgelegenheiten der Entgeltvariante") are measured.

Using a probit model, the propensity score for receiving the JobPerspektive subsidy in June 2009 is estimated separately for West and East Germany using establishment attributes measured in June 2007. Variables are included in levels, squared and cubed forms; establishment size, establishment age, district labour market information and wages are included in logarithmic form.

Making use of the estimated propensity score of receiving the JobPerspektive subsidy, every treatment establishment is assigned to non-treated control establishments weighted by their proximity in the estimated propensity score; a treatment group establishment may be assigned to more than one control group establishment (radius caliper matching with replacement, see Caliendo/Kopeinig 2008). The caliper is chosen so that the five percent of treated/control pairs with the highest difference in propensity scores are discarded, making results representative for 95 percent of the sample.

Evidence for deadweight loss or substitution effects is found if treatment group establishments gain less or lose more unsubsidized/younger employees than matched control establishments. If subsidized jobs are additional instead, unsubsidized employment job flows should remain unaffected or even be higher. Analytical standard

[13] Hohendanner (2011) has analyzed the effect of One-Euro-Jobs on regular employment in the same establishment, finding none.

errors of effect sizes are calculated using Lechner (2001)'s variance approximation method. Even though the initial sample covers the entire population of subsidized establishments on June 30, 2009, calculating standard errors is permissible as they reflect the uncertainty from observing only one of the two potential outcomes necessary for reporting a causal effect (Imbens/Wooldridge 2009: 11), in addition to usual "population as a superpopulation sample" arguments.

The Stable Unit Treatment Assumption should hold since there is only a small number of subsidized establishments. This makes it unlikely that unsubsidized establishments are affected by displacement effects. Even if they were, such displacement effects would need time to make their way through the product market to adversely affect the employment in a competing unsubsidized establishment. In such a scenario, counterfactual employment would be incorrectly measured too low, biasing estimated deadweight loss and substitution effects downward. The presence of displacement effects can be tested by regressing the change in regular employment of unsubsidized establishments on the subsidization intensity in each establishment's sector and district (e. g. in Hohendanner 2011); a significantly negative coefficient of the sector/district subsidization intensity would be evidence for the presence of displacement effects. Such an analysis has been done for the JobPerspektive with an observation period until June 30th 2009, finding no displacement effects (ISG/IAB/RWI 2011).

5.4 Description of the sample

An establishment is a treatment establishment if it employs at least one worker subsidized with the JobPerspektive subsidy on June 30th 2009. A control establishment is an establishment which does not ever employ any workers with that subsidy up to June 30th 2009.[14]

The initial population of treated establishments is reduced from 12,499 to 7,336, and that of the control group from 2,810,630 to 1,219,672, for four main reasons (Table 1). First, given the specification of the difference-in-difference estimator, effects are only identified for establishments that exist continuously from 2004 to 2010. Second, some establishments are located in districts where welfare recipients are not administered by the Federal Employment Agency but instead just by the local community ("zugelassener kommunaler Träger"); as ALMP programme information

[14] This means that establishments receiving the JobPerspektive subsidy only on June 30th 2008 but not one year later are excluded. As the establishment-level data described in Section 5 only includes one observation per year, establishments with subsidizations that both begin and end between October 1st 2007 and June 29th 2008, or July 1st 2008 and June 29th 2009, would be misidentified as control group members. Using the establishment identifier in individual-level subsidization data from the Federal Employment Agency, such establishments are removed the sample completely, ensuring the composition of the control group being as described. The control group will however include establishments that hire workers with the subsidy at a later date after June 30th 2009. Excluding them would effectively condition the sample on future outcomes, biasing results, as discussed in Biewen et al. (2012: 47).

is incomplete for these districts, affected establishments must be excluded to preclude measuring subsidized as unsubsidized employment because the subsidization data is missing. Lastly, establishments with implausibly high job flows (realized by way of keeping the 1 to 99 percent quantiles of the job flow of regular employment) are excluded because they are taken as signs of otherwise unobserved changes in the company structure such as spin-offs.[15] Fourth, excluded from the control group are establishments in sectors or districts without any other subsidized establishments, as this is taken as a sign of unobserved district or sector characteristics that might invalidate the conditional independence assumption.

As seen in Table 2, treatment group establishments are larger, have higher median wages, fewer minor employed workers (earning less than 400 euros a month), employ far more subsidized (with programmes other than JobPerspektive) workers (especially in East Germany), and have higher fluctuation (churning among worker flows) than control group establishments. Treatment group establishments in West Germany on average have grown more and shrunk less than control group establishments; in East Germany, this trend is reversed. West German treatment establishments reside in districts with slightly less favourable labour market characteristics; no such distinction can be made for East Germany.

Table 3 shows the sector distribution of treatment and control group establishments, indicating strong selection of treatment establishments into administration/education/training, other medical services and churches/non-profit organizations/unions. This selection matches that of the German public employment schemes Job Creation Schemes and work opportunities.

6 Results

6.1 Predicting the propensity score

The propensity score is strongly driven by the share of otherwise subsidized employees in June 2007: establishments that have employed more workers subsidized with other programmes are more likely to receive JobPerspektive subsidization in June 2009; similarly, strong growth in subsidized employment in general leads to a higher probability of receiving JobPerspektive subsidization. High fluctuation leads to a higher propensity of receiving treatment, both in terms of the fluctuation rate of contributory employment as well as in terms of the share of workers with long tenure: the higher the share of long-term employed workers, the lower the probability of receiving treatment. Other significant factors explaining selection into treatment are establishment size (the higher, the more likely is treatment) as well as sector: manufacturing, construction and retail/hospitality make subsidization less probable compared to the reference category agriculture/forestry/mining, whereas medical ser-

[15] A more involved method of identifying spin-offs would be the method used by Hethey/ Schmieder (2010).

vices, administration/education, arts/entertainment and churches/non-profit organizations/unions make it more probable.

6.2 Matching quality and fulfilment of the common support condition

As seen in Table 4, even in the smaller sample (East Germany), the caliper is only 0.005, meaning that for the 95 percent of treated establishments for which effects are identified, the maximum distance between the treated establishment and matched control establishment in the propensity of receiving the subsidy is slightly less than 0.5 percentage points. Table 4 furthermore provides indicators for the quality of the matching procedure, that is, how much the groups of treated establishments and control establishments still differ after matching. The high pseudo R-square (over 0.3) before matching shows that the covariates explain the selection into treatment rather well. The likelihood ratio test after matching indicates that matching was successful in almost completely eliminating any difference between the treatment and control group with respect to the covariates (p-value exactly one). Not shown in any table, but available on request, are the t-tests for statistically significant differences in all the covariate means between treatment and matched control groups, which likewise speak for the comparability of the two groups.

6.3 Effect of subsidization on regular and other subsidized employment

Table 4 shows the change in job flows of unsubsidized and other workers caused by subsidization with JobPerspektive. The effect sizes (column ATT) show the average impact per establishment of treatment of this average intensity on the number of workers of several types. For example, the value 0.891 for regular employment in West Germany means that because of the JobPerspektive subsidy, subsidized establishments employ on average 0.891 regular workers more, a result that is statistically highly significantly different from zero, given the standard error of 0.113. Effect sizes are only identified for the average treatment intensity included in all results tables; for example, in West Germany, each treated establishment employs on average 1.769 workers subsidized with JobPerspektive. It would be invalid to divide the average treatment effects on the treated by the average treatment intensity and then to conclude that for every subsidized worker, regular employment rises by 0.891/1.769; this would imply a completely linear dose-response function, which is very unlikely.

In West Germany, the results indicate comparatively high positive effects on regular employment – with an average treatment intensity of 1.769 subsidized workers per establishment, the average effect on regular employment is about half that amount (0.891 workers). This increase occurs about evenly in the low- and mid-skilled regular worker groups, and mostly for workers below 50 years old. A slightly negative effect on workers subsidized both with the old Job Creation Scheme (Arbeitsbeschaffungsmaßnahmen, ABM) programme as well as the newer wage-paying

work opportunities (Arbeitsgelegenheiten der Entgeltvariante, AGH-E) programme can be seen as well. Lastly, a very robust positive effect of about 0.2 additional workers subsidized with hiring subsidies (Eingliederungszuschüsse, EGZ) is observed.

In East Germany, the effect on regular employment is small and barely significant (and not very robust, see below); however, the same positive effect on workers subsidized with hiring subsidies is found.

6.4 Varying the treatment intensity

For the purpose of the following analysis, "treatment intensity" is defined as the share of subsidized workers among all workers in an establishment. Because this share could only take on a very limited number of values in small establishments, causing the treatment intensity to become almost entirely a function of establishment size (see Table A.2), only establishments with a total of at least 20 employees are considered. Not meeting the distributional assumptions for a dose-response model with continuous treatments (Hirano/Imbens 2005) in any transformation, the sample is split into "high-intensity treatment" (treatment intensity above median, which is 7.7 % in West and 12.5 % in East Germany) and "low-intensity treatment" (treatment intensity below median but above zero) establishments, and the propensity of receiving high/low treatment versus no treatment is estimated for each subsample separately. While a multiple-treatment model could be used to estimate the effects of these two types of treatment, the sample reduction property described in Lechner (2001: 50) allows ignoring the existence of multiple treatments provided that low-intensity treatment establishments are removed from the sample when estimating the effect of high-intensity treatment (and vice-versa). As the subsamples become rather small especially with the establishment size at or above twenty restriction, this analysis is only done for the larger sample of West German establishments; the previous section's finding that the effects are so different between West and East Germany forbids combining both regions into one sample.

Table 5 shows the results of this analysis, with positive treatment effects of JobPerspektive subsidization on regular employment for low-intensity treatment establishments and no statistically significant effects for high-intensity treatment establishments. Assigning more workers subsidized with JobPerspektive therefore not only provides no additional positive employment effects, it even reduces the positive employment effects seen at lower treatment intensities. Table 5 also indicates that the substitution of wage-paying work opportunities (AGH-E) is mainly driven by the high intensity treatment establishments, and that the additional employment with hiring subsidies is more pronounced (albeit not statistically different) in low-intensity treatment establishments.

6.5 Robustness analyses

Replacing radius caliper matching with nearest-neighbour matching produces similar results provided that more than one neighbour is used. The large disparity between

the size of the treatment and control groups prompts the use of a complementary log-log model for predicting the propensity score. This provides little qualitative change in results, only the barely significant small positive effect on regular employment in East Germany shown in Table 4 becomes statistically insignificant. The same applies for a standard logit model.

In the current analysis, there are two years between 2007, when the covariates upon which the matching procedure is performed were measured, and 2009, when the treatment status is determined. This design was chosen to avoid the covariates being distorted by the anticipation of treatment, and to allow establishments into the sample that received treatment in both 2008 and 2009. However, there could be major differences in the employment growth between subsidized and non-subsidized establishments after 2007 and before 2009 that determine selection into treatment, thereby violating the Conditional Independence Assumption and thus biasing the results. Reestimating by determining treatment in 2009 as before, but matching upon the covariates in 2008, excluding all establishments that have received treatment by June 30, 2008 from the sample completely, yields qualitatively similar results.

7 Discussion

Positive effects of subsidization on regular employment are entirely within the scope of the theory presented in Section 3. JobPerspektive was explicitly designed to allow subsidized establishments to do business in fields where doing business was not profitable (or in the case of the public sector, cost-effective). To the extent that the low-productivity workers who qualify for the subsidy can be expected to require a considerable amount of training and supervision, they can be considered complements, rather than substitutes, to regular workers. Moreover, to the extent that subsidization lowers the cost of producing the goods or services that the subsidized establishment has always operated in, the resulting scale effect will further serve to increase regular employment; whether subsidization actually does produce a change in output prices can not be identified in the data set in use that is based on Social Security administration data.

Such scale effects could give the subsidized establishment a competitive advantage over competing non-subsidized establishments, possibly causing them to reduce more or build up less employment than they would have had in the hypothetical situation in which the subsidy had not been granted to the subsidized establishment. This is generally referred to as a displacement effect, and is not only undesirable from a policy perspective, but would bias the effect estimates in these establishment-level analyses, constituting a violation of the Stable Unit Treatment Value assumption. As mentioned in Section 5, this is however rather unlikely given the comparatively small number of subsidized establishments, the rather short observation period and the findings in ISG/IAB/RWI (2011).

This result is in line with the more recent studies of Hohendanner (2011), Rotger/ Arendt (2010) and Kangasharju (2007), but contrary to most of the older research summarized in Section 4. This need not necessarily be due to different econometric strategies being used in the analyses but possibly also the result of more recent programmes simply being run more efficiently following the earlier research's sometimes alarming findings.

That the positive effects of JobPerspektive subsidization on regular employment are small to non-existent in East Germany can be explained by the fact that subsidized employment, in particular Job Creation Schemes and work opportunities, have always played a larger role there due do the dearth of regular employment opportunities (Lechner/Wunsch 2009; Jacobi/Kluve 2007). Starting out from a higher initial level of subsidized employment, the introduction of this new subsidy provided little change from the previous regime.

Comparing the results for establishments with a high number of Job-Perspektive-subsidized workers and those with a low number indicated that positive effects only exist for low-intensity treatment establishments. Employing a high number (relative to the total number of workers) of JobPerspektive participants may indicate that those subsidized workers might be productive enough after all to substitute regular workers, with the scale effect preventing the overall effect from becoming significantly negative, whereas employing a low number of JobPerspektive participants indicates low productivity requiring a lot of supervision and assistance. The policy conclusion from this finding would be to limit the number of subsidized workers in each subsidized establishment to a certain percentage (about five to seven percent) of the total workforce.

Very apparent is the substitution of Job Creation Scheme and wage-paying work opportunity participants through the JobPerspektive subsidy in West Germany, mostly in establishments with a high number of JobPerspektive subsidized workers. One explanation for this is certainly the substitutability of these participants with JobPerspektive participants. That substitution of Job Creation Schemes is not observed in greater magnitude even though job creation schemes for welfare recipients were phased out at the end of 2008 is because that particular policy change applied to treatment group and matched control group establishments alike, so if JobPerspektive prompted an employer to replace Job Creation Scheme funding with JobPerspektive funding instead of wage-paying work opportunities funding, it will show up as a substitution of work opportunities in the results, not of Job Creation Schemes.

The most surprising result however is that subsidization with the JobPerspektive leads to a modest increase in employment subsidized with hiring subsidies (Eingliederungszuschüsse, EGZ). One explanation for this additional employment with hiring subsidies might be the low popularity of the JobPerspektive subsidy among private sector employers: when the bill to create the subsidy was passed, it

was estimated to have 100,000 participants at the end of the year 2009. Difficulty in finding employers willing to hire such hard-to-place workers meant that only about 41,000 people could be placed with the subsidy by December 2009 (ISG/IAB/RWI 2011). In order to place JobPerspektive participants, job centres might have to offer potential employers the option of hiring less hard-to-place workers from the unemployment pool with hiring subsidies. This can be seen as evidence for the often-mentioned tradeoff that designers of employment subsidies will face: either make a subsidy rather widely available while suffering high magnitudes of deadweight loss and substitution, or restrict it to hard-to-place job-seekers who would then, despite the subsidy, be difficult to place (Martin/Grubb 2011: 32).

8 Conclusion and further research

This paper estimated combined deadweight loss and substitution effects of German establishments receiving JobPerspektive, a long-term wage subsidy targeted at very hard-to-place job-seekers, by way of modelling counterfactual employment levels through propensity score matching of establishments, using a population-size establishment panel from administrative sources. The analysis involved establishments receiving the JobPerspektive subsidy in June 2009, observing them for one year until June 2010.

Subsidization with JobPerspektive led to an increase in regular employment in West German subsidized establishments, provided that not too many participants were hired into the establishment relative to its size. No robust effects on regular employment in East Germany were found. JobPerspektive also led to comparatively less employment subsidized with wage-paying work opportunities (AGH-E) in West Germany. Furthermore, an increase in the employment of workers with hiring subsidies can be found as a result of subsidization with JobPerspektive. A likely explanation for this finding is that employers will only accept JobPerspektive participants if they can receive subsidies for less hard-to-place hires from the unemployment pool, hinting at an unpopularity of the JobPerspektive subsidy as a consequence of the restrictive targeting.

The policy conclusion from these results is that wage subsidizes combining restrictive targeting and generous subsidization can indeed lead to positive employment outcomes in subsidized establishments. Job centres disbursing subsidies should be wary of employers willing to employ many participants compared to the total workforce size, as this may indicate that participants are good substitutes for existing workers. A low number of subsidized workers relative to the total workforce size should also provide a safeguard against distorting competition on the product market in a way that would lead to displacement effects between subsidized and unsubsidized establishments.

Two caveats are imposed upon this research. First, an observation period of just one year will not be sufficient to observe effects that manifest themselves only in the long run, especially since the subsidy often runs for two years or more. In particular,

delayed substitution might occur some time later, with regular workers being made redundant after having trained the low-productive subsidized workers to perform their jobs. Second, the fact that JobPerspektive subsidization leads to more hiring subsidies for more productive job-seekers may induce deadweight loss and substitution effects of its own and is not specifically accounted for in this analysis.

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Appendix

Table 1
Sample exclusions

	# of establishments	
	Treatment	Control
full population of establishments on June 30th 2007	12,499	2,810,630
excluded because …		
does not exist throughout 2004-2010	2,435	969,093
district with locally-administered welfare recipients any time in 2005-2010	1,127	508,944
no BEZ use in district on June 30th 2009	0	206
no BEZ use in sector on June 30th 2009	0	56,029
missing sector information	0	411
more subsidized than total workers on June 30th 2009	2	0
treatment any time before June 30th 2009	0	1,128
extreme employment stock changes	1,599	55,147
net sample	7,336	1,219,672

Source: own calculations based on extended Establishment History Panel.

Table 2
Characteristics of treatment and control group establishments

	West Germany		East Germany	
	Treatment	Control	Treatment	Control
establishment size (total # of workers) mean	56.6	11.2	36.5	10.8
establishment size (total # of workers) median	23	4	14	4
average…				
years since founding	21.2	15.7	12.5	11.2
median wage	2,124.28 €	1,465.38 €	1,599.02 €	1,331.44 €
share of minimally employed	27.3 %	40.1 %	19.2 %	24.4 %
share of women	57.2 %	60.4 %	55.7 %	56.6 %
share of part-time workers	49.4 %	49.3 %	40.9 %	35.0 %
share of foreign nationalities	4.6 %	6.9 %	1.3 %	2.5 %
share of welfare recipients	2.3 %	1.0 %	6.4 %	3.7 %
share of subsidized workers	3.9 %	0.6 %	11.8 %	1.6 %
share of low-skilled workers	15.3 %	10.4 %	6.0 %	5.1 %
share of medium-skilled workers	50.8 %	46.3 %	61.6 %	54.4 %
share of high-skilled workers	7.3 %	4.0 %	10.2 %	6.3 %
share of unknown skill workers	26.6 %	39.4 %	22.2 %	34.2 %
share of age <= 17	0.8 %	1.1 %	0.5 %	0.6 %
share of age 18-24	8.9 %	10.4 %	7.5 %	9.3 %
share of age 25-29	7.7 %	8.5 %	6.8 %	9.4 %
share of age 30-44	34.7 %	35.7 %	30.7 %	36.0 %
share of age 45-49	16.2 %	13.8 %	14.8 %	14.3 %
share of age 50-54	13.4 %	11.0 %	16.2 %	12.5 %
share of age 55-59	10.1 %	9.0 %	15.5 %	10.1 %
share of age >= 60	8.2 %	10.4 %	8.0 %	7.9 %
share of tenure < 6 months	10.8 %	8.4 %	13.0 %	8.4 %
share of tenure >=6 - < 12 months	9.1 %	7.0 %	8.8 %	6.5 %
share of tenure >=12 - < 24 months	10.9 %	10.6 %	12.2 %	10.3 %
share of tenure >=24 - < 60 months	23.2 %	29.2 %	23.4 %	27.5 %
share of tenure >= 60 months	45.9 %	44.9 %	42.6 %	47.3 %
job flow rate 2002-2007 (all)	10.5 %	6.5 %	-1.7 %	2.8 %
job flow rate 2002-2007 (SVP)	-0.4 %	-3.6 %	-10.2 %	-2.3 %
churning flow/worker flow ratio 2002-2007 (SVP)	87.2 %	76.1 %	86.3 %	79.0 %
district unemployment rate	8.3 %	7.2 %	14.3 %	14.6 %
share jobless > 1 year among all jobless	40.9 %	38.8 %	41.2 %	40.4 %
district vacancy/jobless ratio	0.209	0.242	0.124	0.132
total # of establishments	5,464	1,000,404	1,872	219,268

Source: own calculations based on extended Establishment History Panel.

Table 3
Sector distribution of treatment and control group establishments

sector		West Germany		East Germany	
	in %	Treatment	Control	Treatment	Control
agriculture/forestry/mining		1.6	2.1	4.2	2.9
manufacturing		4.1	8.8	5.3	7.7
energy/utilities/waste management		1.3	0.6	0.9	0.7
construction		3.3	9.5	4.8	11.2
retail/hospitality		12.8	29.3	13.5	29.5
IT/telecommunications		0.5	1.9	0.3	1.6
financial services/insurance/real estate		1.1	7.9	1.8	6.9
professionals/scientists/technicians		1.0	8.8	1.0	9.2
temporary employment agencies		0.2	0.2	0.3	0.2
other services		4.1	6.2	5.2	6.8
administration/education/training		28.2	4.6	19.6	4.5
hospitals/medical practices		2.1	9.0	1.9	11.1
other medical servces		25.0	1.8	22.9	2.1
arts/entertainment/sports		3.7	1.7	3.7	1.3
churches/non-profit organizations/unions		10.7	3.1	14.0	2.4
miscellaneous		0.4	4.6	0.8	1.8
total # of establishments		5,464	1,000,404	1,872	219,268

Source: own calculations based on extended Establishment History Panel.

Table 4
Estimated effects of JobPerspektive subsidization

	West Germany			East Germany		
Outcome	ATT		SE	ATT		SE
regular employment	0.891	***	0.113	0.333	*	0.192
regular employment (low-skilled)	0.424	***	0.067	0.092		0.124
regular employment (mid-skilled)	0.470	***	0.172	0.236		0.186
regular employment (high-skilled)	-0.029		0.040	-0.115		0.080
regular employment (age 49 and below)	0.715	***	0.111	0.334	*	0.187
regular employment (age 50 and above)	0.177	***	0.067	-0.001		0.135
job creation schemes (ABM)	-0.135	**	0.059	-0.491		0.339
wage-paying work opportunities (AGH-E)	-0.127	***	0.047	0.276		0.228
ABM + AGH-E	-0.262	***	0.069	-0.216		0.410
hiring subsidies (EGZ)	0.179	***	0.018	0.165	***	0.048
Statistics						
avg. number of BEZ subsidized workers	1.769			2.409		
number of treatment group observations	5,332			1,853		
pseudo R^2 before matching	0.329			0.297		
pseudo R^2 after matching	0.007			0.018		
LR ratio test p-value after matching	1.000			1.000		
caliper	0.001			0.005		

significance level: *** 1%, ** 5%, * 10%

Source: own calculations based on extended Establishment History Panel.

Table 5
Estim. effects of JobPerspektive subsidization by treatment intensity

relative treatment intensity, median = 7.7 %	above median			below median		
Outcome	ATT		SE	ATT		SE
regular employment	0.360		0.315	2.006	***	0.326
regular employment (low-skilled)	0.443	**	0.208	0.728	***	0.200
regular employment (mid-skilled)	-0.217		0.304	1.498	**	0.606
regular employment (high-skilled)	-0.249	**	0.119	-0.035		0.122
regular employment (age 49 and below)	0.315		0.302	1.600	***	0.324
regular employment (age 50 and above)	0.045		0.161	0.405	*	0.212
job creation schemes (ABM)	0.092		0.205	-0.087		0.103
wage-paying work opportunities (AGH-E)	-0.704	***	0.221	-0.017		0.076
ABM + AGH-E	-0.612	**	0.276	-0.104		0.104
hiring subsidies (EGZ)	0.153	***	0.053	0.175	***	0.041
Statistics						
avg. number of BEZ subsidized workers	3.244			1.359		
number of treatment group observations	1,459			1,441		
pseudo R^2 before matching	0.337			0.364		
pseudo R^2 after matching	0.037			0.015		
LR ratio test p-value after matching	1.000			1.000		
caliper	0.025			0.002		

West Germany only. Significance level: *** 1%, ** 5%, * 10%

Source: own calculations based on extended Establishment History Panel.

Table A.1
Characteristics of JobPerspektive subsidization in four time periods

	West Germany				East Germany			
	10/2007-3/2008	4/2008-12/2008	2009	2010-2011	10/2007-3/2008	4/2008-12/2008	2009	2010-2011
# BEZ subsidizations	1,547	13,595	14,452	3,372	926	7,749	7,044	1,414
of these (in %)								
male	70.7 %	69.3 %	65.0 %	65.3 %	48.6 %	58.7 %	59.3 %	66.2 %
age 18-24	1.9 %	1.7 %	1.4 %	1.3 %	0.5 %	1.8 %	1.3 %	2.3 %
age 25-29	3.6 %	4.9 %	4.5 %	2.8 %	5.5 %	4.4 %	4.2 %	6.9 %
age 30-49	49.1 %	52.8 %	51.4 %	49.6 %	44.6 %	40.1 %	40.1 %	42.4 %
age 50-57	40.0 %	34.9 %	34.8 %	35.6 %	43.1 %	42.7 %	41.6 %	35.2 %
age 58+	5.5 %	5.8 %	8.1 %	10.7 %	6.3 %	10.9 %	12.8 %	13.3 %
with vocational degree	53.4 %	61.8 %	59.5 %	57.8 %		37.3 %	31.0 %	28.0 %
foreigner	10.3 %	9.7 %	10.7 %	12.0 %	10.0 %	6.6 %	4.7 %	4.2 %
unempl. <= 1 year[1]	18.1 %	14.8 %	14.2 %	17.2 %	12.3 %	10.9 %	10.6 %	12.7 %
unempl. >1 <=2 years	23.3 %	20.1 %	20.2 %	22.1 %	18.4 %	19.3 %	19.3 %	19.2 %
unempl. >2 <=3 years	26.4 %	27.0 %	26.6 %	25.7 %	27.9 %	28.4 %	28.1 %	27.4 %
unempl. >3 <=4 years	22.2 %	25.8 %	25.8 %	24.7 %	29.6 %	28.6 %	28.1 %	28.7 %
unempl. >4 years	10.1 %	12.3 %	13.2 %	10.3 %	11.9 %	12.8 %	14.0 %	12.0 %
BEZ duration <= 1 year	11.7 %	20.4 %	26.5 %	31.9 %	14.5 %	14.8 %	22.9 %	27.2 %
BEZ duration >1 <=2 years	52.6 %	61.1 %	63.1 %	64.8 %	57.0 %	72.4 %	70.2 %	67.1 %
BEZ duration >2 years	35.8 %	18.5 %	10.4 %	3.3 %	28.5 %	12.8 %	6.9 %	5.7 %

[1] The unemployment duration is calculated according to the method required by § 18 Section 2 Social Code III. It is not increased during ALMP programme participation, and is reset to zero after regular employment of at least six months or an otherwise unaccounted-for unemployment interruption of at least one month.

Source: own calculations based on individual process data from the Federal Employment Agency.

Table A.2
Intensity of JobPerspektive subsidization by establishment size

	West Germany		East Germany	
	# subs. workers	share of total[2]	# subs. workers	share of total[2]
establ. size[1]	(avg. per establishment)		(avg. per establishment)	
1	1.14	67.2 %	1.14	76.5 %
2	1.27	61.8 %	1.18	52.1 %
3-5	1.20	43.1 %	1.67	37.8 %
6-10	1.32	25.8 %	1.96	23.3 %
11-20	1.58	14.9 %	2.33	16.4 %
21-30	1.92	10.0 %	2.36	11.5 %
31-50	1.92	6.5 %	3.24	8.5 %
51-100	2.33	4.3 %	3.28	5.5 %
101-200	2.62	2.6 %	4.54	3.8 %
201-500	3.81	1.7 %	4.03	1.4 %
>500	8.24	1.6 %	10.17	6.2 %

[1] in matching year (2007)

[2] in treatment year (2009)

Source: own calculations based on extended Establishment History Panel.

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IAB-Discussion Paper 5/2013

Editorial address
Institute for Employment Research
of the Federal Employment Agency
Regensburger Str. 104
D-90478 Nuremberg

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ISSN 2195-2663

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