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The effects of targeted labour market programs for job seekers with occupational disabilities

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WORKING PAPER 2014:27

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ISSN 1651-1166

The effects of targeted labour market programs for job seekers with occupational disabilities[a]

by

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October 29, 2014

Abstract

In this study, we estimate the effects of three targeted labour market programmes (LMPs) on the labour market outcomes of occupationally disabled job seekers. Using propensity score matching, we estimate the average treatment effect on the treated of wage subsidies, sheltered public employment, and employment at Samhall, a Swedish state-owned company whose aim is to provide employment for persons with disabilites. The control group consists of individuals who are eligible for the targeted LMPs, but have not (yet) received treatment. Using a rich panel data set, containing demographics as well as health and sickness absence measures, we are able to estimate short- to medium-term effects. Our results show large positive effects of all LMPs on labour income, disposable income and employment, and the effects are relatively persistent. However, consistant with the previous empirical literature, we find considerable locking-in effects, measured by a decrease in un-subsidized employment. Furthermore, the yearly amounts of disability insurance paid decrease as a result of program participation, and the decrease becomes more pronounced with time since treatment start. Finally, the effects on disability insurance prevalence are heterogenous, both with respect to the different LMPs and gender.

Keywords: occupational disability; wage subsidies; locking-in effects; treatment effects
JEL-codes: C21; J14; J23

[a]We are grateful for comments from Per Johansson, Pathric Hägglund, and seminar participants at IFAU and the Swedish Social Insurance Inspectorate (ISF).

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1 Introduction

People with disabilities face considerable difficulties on the labor market. Descriptive statistics from OECD member countries reveal lower education, lower employment rates and lower earnings for people with disabilities compared to those without (OECD, 2010).[1] At the individual level, these difficulties can have negative consequences, both in terms of poverty and social marginalization. Moreover, this group is far from insignificant seen at the macro level. Self-reported disability prevalence in OECD member countries ranges from over 20% (Estonia, Hungary and Denmark) to 6% (South Korea), with an OECD-average of about 14% of the working age population (OECD, 2010). Consequently, public expenditure on disability-related programmes is substantial in many cases. Norway's expenses of 5.6% of GDP are the highest among OECD member states, followed by Sweden and the Netherlands spending 4.7% and 4.6%, respectively (OECD, 2003). Because of these high costs, it is of great importance to evaluate the impact that the programmes have on labour-market outcomes.

The purpose of this study is to estimate the effects of Swedish labour market programmes (LMPs), targeted at job seekers with disabilities. The outcomes of interest include labour income, employment, un-subsidized employment, and disability insurance. Using a rich administrative data set with information not only on the usual background characteristics but also on current and previous labour market outcomes, hospital inpatient care, and sickness absence, we estimate the average treatment effect on the treated (ATT) using propensity-score matching. The ATT is estimated for 11 years surrounding programme participation. We follow Fredriksson and Johansson (2008) by defining a potential control group of not-yet treated whose observations are discarded as soon as they (potentially) participate in any targeted LMP.

A general concern when using matching methods in this setting is whether the po-

[1] Studies on the causal impact of acquiring a disability on labour market outcomes are few. Lechner and Vazquez-Alvarez (2003) are an exception and using matching methods, they provide evidence of substantial differentials between people with and without disabilities in Germany. Using a notion of legal disability and the exact timing of when individuals become legally disabled, Lechner and Vazquez-Alvarez find that the probability of working is almost 10% higher among those without disabilities, and that the earnings differential is about 16% on average.

tential controls are relevant for the treated population of disabled job seekers. As the programmes that we study are targeted, this is a highly relevant issue. In this study, we therefore restrict the group of controls to individuals who also are eligible for programme participation. Eligibility is defined by the occupational disability coding, assigned by case workers at the Public Employment Service (PES), and available to us in the administrative registers. Thus, we compare eligible individuals who participate in the LMPs, with matched not-yet treated eligibles. Using the various sources of data, we are able to attain good covariate balance on variables relevant for the likelihood of receiving treatment as well as for the outcome variables. To estimate the ATT, we assume that treatment assignment is independent of the potential non-treatment outcome, conditional on our chosen set of variables (i.e., the CIA). Although this assumption is untestable, we are at least able to investigate it informally by using the rich set of variables in the data.

The LMPs that we consider are various forms of targeted wage subsidies. Research on wage subsidies has a long history in economics, with an early contribution made by Kaldor (1936) who wrote a paper on the theoretical advantages of non-targeted wage subsidies, compared to other measures of reducing what would probably be called structural unemployment today. Nowadays, wage subsidies are targeted to specific disadvantaged individuals or groups, rather than to all workers. The main economic principle behind wage subsidies is to reduce the cost of hiring in order to stimulate the demand for those workers and thereby increase their probability of employment, as well as their earnings (Katz, 1996). The analogy to the specific case that we consider, that of job seekers with an occupational disability, is very straightforward: If lower productivity is the main reason for the difficulties on the labor market that persons with disability experience, wage subsidy rates that are tied to some measure of the level of the reduction of work capacity can be thought of as a direct replacement for lost production.[2]

Theoretically, the effect of wage subsidies could then only be positive for those workers who participate. What complicates matters is that targeted wage subsidies can also play a stigmatizing role: Assume for the sake of an example that the wage subsidy serves

[2]If the difficulties in getting and keeping a job or the low earnings are instead due to discrimination, the wage subsidy plays the role of a hiring incentive.

as a perfect replacement for the productivity differential due to a certain occupational disability, but that the employer finds this as a signal of a more severe disability. Then a wage subsidy might actually damage the chances of getting a job. In this study, we are not able to disentangle those two potential mechanisms. Furthermore, wage subsidies can have significant locking-in effects because of the job seekers' reduced effort to find an ordinary job.[3]

We are only aware of a few previous empirical studies on the effects of wage subsidies on labour market outcomes of job seekers with disabilities. Datta Gupta and Larsen (2010) estimate the intention-to-treat effect of eligibility for participation in the Danish Flexjob scheme, and find a substantial positive effect on employment, but no effect on disability insurance (DI)-prevalence. Since treatment is not defined in terms of actual programme participation, these results are not directly comparable to ours. Another study is Deuchert and Kauer (2013) who use a small-scale social field experiment performed in Switzerland to measure the effect of hiring subsidies on interview call-back rates. Deuchert and Kauer (2013) show that the subsidy is ineffective or even counterproductive in a group of adolescents who are at the end of their vocational training program, but may increase call-back rates in a group of clients of job coaching services.

The empirical evidence for the effect of wage subsidies targeted at other low skilled groups is mixed. Huttunen et al. (2013) estimate the employment effects of a Finnish payroll tax subsidy, targeted at older low-wage workers, and find no effect on the employment rate, but a slight positive effect on working hours. Somewhat similar results are reported in Schünemann et al. (2013), who use a regression discontinuity approach to estimate the intention-to-treat effect of being eligible for wage subsidies on long-term unemployed workers in Germany, and find no effect on the labour market outcomes of the target group. Jaenichen and Stephan (2009) on the other hand find positive effects of targeted wage subsidies on the employment rates of hard-to-place workers in Germany. Finally, also Forslund et al. (2004) report positive effects of a wage subsidy programme in Sweden, in terms of shorter unemployment duration.

[3] See for instance Calmfors et al. (2004) who discuss the mechanism of locking-in effects on the Swedish labour market, and van Ours (2004) for empirical estimates using data from the Slovak republic.

The paper proceeds with section 2, where we outline the institutional setting, including the PES' disability coding and the LMPs that we consider. Section 3 then outlines our estimation strategy, and section 4 introduces the data. We present the results in section 5, and conclude in section 6.

2 Institutional setting

We begin this section with a short description of the PES' disability coding system. We then describe the labour market programmes that are evaluated in this study.

2.1 The PES' disability coding

The PES' occupational disability coding system serves four purposes: to ensure that the job seeker as early as possible receives adequate support in the job search process; to make the person eligible to special measures and programs targeted to job seekers with disabilities; to facilitate planning and evaluation of the targeted measures; and to provide statistics for the estimation of resource needs (Arbetsförmedlingen, 2011).

The initiative for coding a job seeker as occupationally disabled is taken by the responsible caseworker. However, most cases require a medical report or a report from another specialist (e.g., a psychologist or speech therapist), describing the extent of impairment and its effect on work capacity. Although the job seeker has the right to refuse being coded as occupationally disabled, this rarely seems to happen (Garsten and Jacobsson, 2013).

The PES' occupational disability coding system contains 11 different occupational disability codes depending on the specific impairment: cardio, vascular, and/or lung disease (code 11); hearing impairment and deafness (code 20); visual impairment (code 30); motor disability (code 40); other somatically related disabilities (code 51); mental disability (code 61); learning disability (code 71); socio-medical disability (code 81); asthma, allergy, and hypersensitivity (code 91); dyslexia and specific learning difficulties (code

92); and acquired brain injury (code 93).[4] As all codes but the one for socio-medical disability are self-explanatory, no further description will be provided here.[5] [6]

2.2 The targeted LMPs under evaluation

We consider three different LMPs targeted to job seekers with an occupational disability: wage subsidies (WS), sheltered public employment (SPE), and sheltered employment at the government-owned company Samhall.[7] The three programmes are described below.

2.2.1 Wage subsidies

According to the PES, the purpose of the WS programme is to increase disabled job seekers' likelihood of employment, the main idea being that the wage subsidy should compensate for job-related or workplace-related adjustments.[8] The employer can be either public or private, but has to meet several legal requirements, such as being registered as an employer at the Tax Agency (Skatteverket); not having records of non-payment at the Swedish Enforcement Administration (Kronofogden); and having a collective agreement or comparable employee protection insurance.

To be eligible, the job seeker first has to be assigned an occupational disability code by the PES. The job seeker could be currently employed at the firm (e.g., an employee coming back to work after an accident or severe illness), employed at another firm, or unemployed. An employer can receive wage subsidies for a certain employee during at

[4] In July 2000, codes 20, 30, and 40 were replaced by two codes each (i.e., 21-22, 31-32, and 41-42) also categorizing the severity of the impairment. At the same time three new codes were introduced, 91, 92, and 93. In all analysis the codes 21-22, 31-32, and 41-42 will be collapsed to correspond to the old codes 20, 30, and 40, respectively.

[5] A more detailed description, including a discussion about the incentives of the PES to give a disability code, and of the job seekers to get one, is given in Angelov and Eliason (2014) and Johansson and Skedinger (2008).

[6] Socio-medical disability is a code that lacks an international equivalent. The impairments included in this code are caused by social difficulties that have led to long-lasting need of means-tested social benefits, or by a complex of relational problems, substance abuse, criminality, or difficult childhood and adolescence. For those with a socio-medical disability either an investigation by the PES' own social consultants, or by another government agency (e.g., the Social services) is necessary. Most other disability codes require a medical report or a report from another specialist (e.g., a psychologist or speech therapist), describing the extent of impairment and its effect on work capacity. For those with congenital deafness (code 20) and learning disability (code 71), documentation from a specialist school is sufficient.

[7] These LMPs correspond to the following codes in HÄNDEL: wage subsidies and sheltered public employment are coded as job search category 42 and 43, respectively, and employment at Samhall has deregistration code 4.

[8] The description in this section is based on Arbetsförmedlingen (2012a).

most four years, but there is no limit at the individual level.

The PES decides upon the wage subsidy amount, as a function of the wage of the employee and the level of work capacity. [9] Obviously, as the employer can turn down any offer, the PES cannot make the decision in isolation. Apart from the wage subsidy, the employer can get reimbursement for additional costs of at most SEK 70 per day (about EUR 7/day) for employees whose wage subsidy rate is at least 80%.

The initiative for participation in the WS programme can be taken by either the employer or the PES. The employer then has to apply formally, and during the whole duration of the programme, the employer has to claim the WS each month, the amount being paid each month in arrears. During the whole WS programme, the PES has a responsibility to follow up and, if necessary, provide support for the firm and the employee.

2.2.2 Sheltered public employment

SPE can only be provided by public employers, and the job itself cannot be within a competitive sector.[10] The first of its two target groups is job seekers with socio-medical impairments.[11] The second group is job seekers who are entitled to assistance according to the Act Concerning Support and Service for Persons with Certain Functional Impairments, abbreviated in Swedish as LSS. The LSS (Lagen om stöd och service till vissa funktionshindrade) is an entitlement law aimed at improving living conditions for people with extensive and permanent functional impairments (Government act 1993:387). The act applies to those with an intellectual developmental disorder or autism; those who have considerable and permanent intellectual impairment following brain damage; and those who have some other lasting physical or intellectual impairment that is not due to normal ageing, and are thus in need of an need for extensive support. Finally, SPE can be offered to persons who have been away from the labour market during a prolonged period due to severe mental illnesses.

To summarize, the target group for the SPE programme is more negatively selected with respect to labour market outcomes compared with the WS programme. The SPE

[9] The part of the wage exceeding SEK 13,700 (EUR 1,590) – increased to SEK 15 200 (EUR 1,590) and SEK 16 700 (EUR 1,750) in 2005 and 2006, respectively – is not subsidized.

[10] The description here is from Arbetsförmedlingen (2012b).

[11] See section 2.1.

programme differs on two additional aspects: It can last at most one year, and the employee does not have the same level of job protection. Apart from that, SPE involves wage subsidies at the same level as the WS programme.

2.2.3 Employment at Samhall

Samhall is a state-owned company with a government assignment to provide employment to people with disabilities. Sheltered employment in Sweden began in the 1960s, and was operated in a decentralized manner during the first couple of decades. The responsibility for organizing the various initiatives was spread out among various government and municipal bodies. The activities consisted of sheltered workshops, office work, or home-based work. In 1972, a state inquiry recommended that the different activities should be coordinated at the regional and national level (SOU 1972:54), and this issue was further stressed in still one inquiry three years later (SOU 1975:82). Consequently, in 1977, the Swedish parliament voted on creating a new foundation which would start operating on 1st January 1980. The purpose of the new foundation was to equalize the supply of sheltered jobs across the country and to provide meaningful jobs for persons with disabilities. By increasing the level of commercial activity, an additional idea was that the new organization would lead to cost savings (SOU 2003:56). Today's Samhall has its roots in the foundation Stiftelsen Samhällsföretag, founded in 1980, which consisted of a main body as well as 24 regional foundations who ran the actual activities. In 1992, the foundation was transformed into a state-owned limited liability mother company, which in turn owned the regional companies.

Samhall's current articles of association state that the company is to produce goods and services in general demand, and thereby provide meaningful and developing jobs for persons with disabilities. About half of the revenues come from sold goods and services, and the rest are received from the government (SOU 2003:56). To get an employment at Samhall, a job seeker has to have an occupational disability. In the long run, the purpose of a Samhall employment is to be able to get a job on the regular labor market, although there is no time limit for a Samhall employment. Wages and other employment benefits are set according to the collective agreement in the sector (Arbetsförmedlingen, 2013).

3 Estimation strategy

We estimate the average treatment effect on the treated (ATT) of participation in one or more LMPs for disabled job seekers. The outcome variables of interest include two measures of income, employment status, un-subsidized employment status, and disability insurance (DI). Treatment contains three types of LMPs, considered both together and separately. Both treatment and outcomes are measured on yearly basis, meaning that we estimate yearly ATTs following treatment start in a specific year. The group of potential controls consists of individuals classified as occupationally disabled by the PES and who were not treated in that year, but may get treatment later on. Thus, due to the disability coding, the controls are eligible for programme participation.

We use propensity-score matching to estimate the ATT.[12] Treatment is defined as participation start in a LMP during year t, and the alternative to treatment is non-participation at time t in any of the three LMPs that we consider. Later participation is possible, but when estimating the effect of treatment start in year t on an outcome measured during a future year $j > t$, we drop observations for potential controls that are treated in j, or that have been treated in year s with $t < s < j$. In other words, we compare treated individuals with matched not-yet-treated individuals. For inference, we use the estimator for the standard errors suggested by Abadie and Imbens (2006).

Each year $j = t, t+1, \cdots, t+5$, the ATT is estimated using one-to-one matching on the propensity score. Since the pool of controls is different each year (as some controls get later treatment), we estimate a new propensity score model each year and perform the matching separately for each $j = t, t+1, \cdots, t+5$. We estimate the propensity score using a logit model and perform 1-1 matching such that each treated is matched to the closest neighbour. If there are ties such that several of the closest neighbours have the same propensity score, then all matches are included.[13]. The matches are then weighted such that the number of treated individuals is equal to the weighted number of controls.

The key identification assumption that we make to identify the ATT is that the potential outcome under non-treatment is independent of treatment assignment, conditional on

[12] We use R for estimation; see Sekhon (2011).

[13] Two predicted propensity scores are regarded as equal if the difference between them is at most 0.00001.

the set of covariates that we match on. This is the conditional independence assumption (CIA), and formally, for treatment assignment during year t, we impose the following series of assumptions:

$$Y_{t+s}^{k}(0) \perp\!\!\!\perp W_t^j | \boldsymbol{X}_{t^-} \text{ for } j = 1,2,3,4;\ k = 1,2,\ldots,6;\ \text{and } s = 1,2,\ldots,5. \tag{1}$$

Here, $Y_{t+s}^{k}(0)$ is potential outcome k under non-treatment during year $t+s$; the index k denotes the type of outcome (to be defined in section 5.3); $W_t^j = 1$ for treatment type j during year t and 0 otherwise; j is an index denoting the particular treatment type (i.e., any targeted LMP, WS, SPE, or Samhall-employment); and $\boldsymbol{X}_{t^-}$ is a vector of observed covariates measured prior to t, as well as time invariant demographic variables. In words, we exclude the possibility that there are unobservables linked to both the potential outcome and the treatment. Although the CIA cannot be tested directly, in section 5.1 we provide an informal assessment.

Furthermore, we assume that for all possible values of $\boldsymbol{X}_{t^-}$ for the treated individuals, there are untreated individuals (overlap). We investigate overlap in section 5.2. Finally, we assume that the potential outcome $Y_{t+s}^{k}(0)$ for one individual should be unaffected by the treatment assignment of any other individual (SUTVA, see e.g., Rubin, 2005).

4 Data

In this section, we first describe the data sources, and then the sampling.

4.1 Data sources

The data are drawn from various universal administrative registers. First, the PES' own administrative register (HÄNDEL) is used to identify job seekers. HÄNDEL contains individual level data on unemployment spells with information about LMP participation, as well as occurrence and type of disability code. From HÄNDEL, we also construct annual individual days in unemployment. Our second source of information is the National Patient Register collected by the National Board of Health and Welfare (Socialstyrelsen) and contains information on all hospital in-patient care in Sweden. In addition

to the start and end date of each in-patient episode, we use information on the type of discharge diagnosis at the ICD-10 chapter level.[14] Third, we use data on annual sickness absence, calculated using sickness absence spells from the Social Insurance Agency (Försäkringskassan). Fourth, background characteristics, various income measures, and employment status in November each year are obtained from Statistic Sweden's longitudinal databases (LOUISE/LISA). Finally, in addition to individual-level data, we use municipality-level data from the PES on the share of the labour force who are in open unemployment, as well as the share in active LMPs.

4.2 The sample

The population of treated individuals for whom we estimate the ATT consists of job seekers with an occupational disability code who enter into any of the considered LMPs (WS, SPE, or Samhall employment) in 2004. The group of potential controls consists of job seekers who had an occupational disability code in 2004, but either enter a LMP in a later year, or not at all. The potential controls are therefore eligible for the LMPs, but are not-yet treated. For further reference, we will use the notation t to index the year of treatment, meaning that $t-1$ indexes one lagged value, and so forth.

We restrict the sample in several ways. First, because the set of covariates that we match on includes lagged variables, we discard both treated and controls who have received treatment during the period $t-5$ to $t-1$. The reason for this conditioning is that if we allow the treatment effect to be non-zero, previous treatment could have an effect on lagged values of variables that we match on. Matching on those variables could then introduce bias in the ATT estimator. Further, when we estimate the ATT after programme start, our second sample restriction is to exclude potential controls who (possibly) get treatment in $j > t$, starting from the year of treatment for the control individuals. Finally, when we consider the effect for a specific LMP, we apply both previous restrictions (i.e., we include previously non-treated, and include only not-yet treated) with respect to the LMP of interest as well as the other two LMPs. An alternative would have been to let the controls be un-treated in the specific LMP of interest, but potentially treated in any of the

[14] ICD-10 is the 10th revision of the International Statistical Classification of Diseases and Related Health Problems from the World Health Organization.

two other LMPs. However, we believe that our estimand is more well-defined under the applied sample restrictions. To summarize, the sample restrictions we impose allow us to estimate the compound ATT of participation in any LMP, and the ATT of participation in a specific LMP.

To start with, there are 368,226 job seekers with an occupational disability code in our data, once we have removed all observations with missing values for any covariate (to be listed in section 5) and all individuals who are older than 59 or younger than 21 in t. The latter restriction is imposed in order to avoid having retired and not-yet in the labour force in the data. Finally, we remove all individuals who had participated in any of the considered LMPs during the period $t-5$ to $t-1$. This leaves us with 275,094 individuals, of which 8,290 participated in a targeted LMP at t, and the rest serve as potential controls. The overwhelming part of the treated job seekers received wage subsidies (7,107 individuals), 670 had a sheltered public employment, and 464 got an employment at Samhall. Because a minor group of job seekers participated in more than one LMP in t, we have removed those individuals from the separate programmes, but not from the total group. Therefore, the participants in the individual LMPs sum up to 8,241 individuals, slightly less than those who participated in any of the three LMPs.

5 Empirical findings

We start this section with a short variable description and an analysis of the covariate balance achieved by the propensity score matching. This is followed by a check of the overlap in the propensity score distributions of treated and controls. We then report the estimation results, and discuss the possible mechanisms that drive the effects. The section ends with separate estimates for males and females.

5.1 Matching variables, match quality, and the plausibility of the CIA

As in all matching applications, the CIA is critical assumption that is not directly testable. An obvious objection to matching in the current setting is the following: Suppose that the PES case workers systematically assign treatment to job seekers based on some variable U which is observed by the case worker and/or by the individual, but unobserved by the

researcher. If U is positively partially correlated with both potential outcome and treatment assignment once we have conditioned on the observed variables, then we estimate the ATT with an upward bias. Given that we have access to past and present values of variables related to labour-market outcomes, hospital in-patient care, sickness absence, local labor market conditions, and demographics, we do not believe that it is plausible for such a U to confound our results in any practically significant way. It is hard to imagine that the case-worker would be able to gather information beyond the battery of covariates that we use, simply by meeting the unemployed. We do however attempt to substantiate this claim using data. Before explaining how, we introduce the types of variables used in the propensity score model.

We have a vast array of available covariates to potentially include in the propensity score model. To avoid small-sample bias resulting from having to estimate the propensity score model on finite data (especially for the case of the LPMs with fewer participants), we need to restrict the number of covariates. The general principle for covariate choice is to include varibles that are considered to be relevant for both treatment assignment and the outcomes. To start with, we have included the following variables: (1) the usual socio-demographic variables such as sex, age, immigrant status, and education; (2) health-related variables, such as previous length of hospital in-patient care and the corresponding hospital discharge diagnoses; (3) previous sickness absence, which is health-related, but can also be affected by economic incentives (see Johansson and Palme, 1996), and (4) a combination of duration from registration at the PES to programme start and to receiving a disability code, respectively[15].

Furthermore, we have included lagged values of several of the outcome variables. Lagged values of the outcome variables can be used to improve on the matching procedure, but in addition, as noted in Imbens and Wooldridge (2009), they can also be used to perform an informal assessment of the CIA. The main idea is to estimate the treatment effect on a lagged outcome value (e.g., measured one year prior to treatment), matching on, among others, more distant lagged values of the outcome variable. Since there should be no effect prior to treatment, a no-effect result would provide some evidence in support

[15] All possible combinations of the two durations in quarters (censored at 8 quarters) resulting in 36 categories.

for the CIA. Thanks to the high quality of our data, we believe that we can do slightly more, which is explained in the following: To investigate the plausibility of the CIA, we do not use lagged income or disposable income values in the propensity score model. The main idea is that if we achieve good balance on the lagged income measures by matching on the rest of the variables, than the CIA is appears more plausible. In other words, if we are able to achieve balance on several lagged variables, then it is more plausible that our matching procedure is able to successfully remove bias in the estimation of the treatment effect.

To this end, in Table 1 in Appendix A, we present means for the groups of treated (T) and controls (C), as well as standardized differences and t-tests for difference in means between T and C, prior to and after matching. The last column in the table indicates whether the variable has been included in the propensity score model (Logit).

We begin by discussing the means of some of the variables for the treated and the controls prior to matching. The yearly labour incomes of both groups decrease monotonically from $t-5$ to $t-1$. The labour income at $t-1$ is about 39,000 SEK and 43,000 SEK for treated and controls, respectively, and the group of treated has seen a higher labour income decrease after $t-5$. In other words, both groups consist of individuals whose labour income has decreased during the years prior to treatment, with the decrease being more pronounced for the treated group. The same is true when we consider the categorical variable employment in November. However, both groups' disposable incomes are fairly stable, and even increase slightly over time. Since disposable income consists of labour and net capital income as well as net transfers (sickness absence, disability insurance, parental benefits, welfare benefits, etc.), this suggests that the social security net is relatively good at providing income loss protection. Disposable income is also substantially higher than labour income, with an increasing factor of roughly 1.4 in $t-5$ and 3.4 in $t-1$.

Looking at disability insurance (increasing from roughly 4,000 SEK in $t-5$ to about 11,000 SEK in $t-1$ for the participants), gives an indication that the decrease in labour income is, at least partly, related to lost work capacity. Moreover, the share of treated individuals who are sick-absent during a whole year increases from about 4% in $t-5$ to

25% in $t-1$.[16] Thus, health-related work incapacity seems to be related to the decrease in labour income for both treated and potential controls. In terms of demographic variables and age, the treated individuals are slightly older; somewhat more likely to be born in Sweden; and the share of females is somewhat smaller among the treated.

Prior to matching, the standardized differences for most variables are relatively small.[17] The highest standardized differences are those for days in unemployment per year (ranging from 0.33 to 0.42), and for some of the registration-to-treatment and registration-to-coding groups. For the rest of the variables, the standardized differences are generally small prior to matching. For lagged labour income, the standardized differences are all below 0.12 prior to matching, and those for disposable income are all lower than 0.04. To summarize, even prior to matching, with some exceptions, we have a reasonably balanced sample. We believe that this is a strength with our data which comes as a result of the relevant set of controls, namely job seekers that have received a disability coding from the PES and are thus eligible to the same type of targeted LMPs as the treated are.[18]

It is clear from Table 1 that matching improves the balance significantly. The largest standardized difference is 0.02 after matching, a low value by any standard. Also, prior to matching, most of the t-tests result in a rejection of the null of equal means. After the matching is performed, there is no rejection of the null for any variable, and the p-values are generally high. As explained above, we started by not including some of the lagged outcome variables (labour income, disposable income, and a categorical variable for payment of any DI) in the propensity score model. Initially, the t-tests indicated a statistically significant difference with regards to disposable income one year prior to treatment, but no significant differences for any other variable. The difference was not substantial, but guided by the result from the t-test we decided to also match on disposable income during the year prior to programme entry, although this lacks any practical significance for the estimation results. Note however that we achieve good balance on the rest of the lagged

[16] Because of the non-standard distribution of days sickness absence per year, with large probability masses at 0 and 365, we use a spline specification rather than days in sickness absence.

[17] Rosenbaum and Rubin (1985) consider an absolute value of 0.2 as "substantial" and Normand et al. (2001) a value less than 0.1 as "small".

[18] For a detailed description of the coding of occupational disability, see Angelov and Eliason (2014).

disposable income values, as well as on all lagged labour income values, without including those in the propensity score model. Although this is no proper test, we interpret this result as an indication that the CIA is plausible in this setting.

5.2 Overlap in the propensity score distributions

Figure 1 illustrates the degree of common support that we achieve. The two topmost histograms show the distribution of the predicted propensity scores for non-treated and treated individuals. Both distributions are highly skewed, with most of the mass close to zero, but the skewness is more pronounced for the controls. Also, looking at the whole distribution gives the impression that there is no common support for predicted propensity scores above 0.2. This is in fact incorrect, as can be seen from the two histograms at the bottom, where we show the distributions above 0.2. There is clearly good coverage also above 0.2.

One way to further investigate the degree of overlap is to enforce common support in the matching procedure, i.e., to remove treated with a propensity score above the highest propensity score among the controls, and do the same for controls. In the current application, doing this does not exclude any observation, and thus, we have sufficient overlap with regards to outliers.

Another way to assess overlap is to regard whether there is good coverage in the interior of the distributions, or in other words, to look for inliers. A convenient way to do this is by means of caliper matching. As mentioned in section 3, we use one-to-one matching such that each treated is matched to the closest neighbour, or neighbours if several of the closest matches have the same predicted propensity score. With caliper matching on the other hand, not only the closest neighbour, but all neighbours within a certain distance ("caliper") are included (Cochran and Rubin, 1973). For instance, setting the caliper at 0.2 implies that all matches equal to or within 0.2 standard deviations of the propensity score are included, while the rest are discarded. This in turn implies that some treated observations are excluded from the analysis. We do not use caliper matching as a way of discarding bad matches, mainly because it is difficult to choose a caliper setting

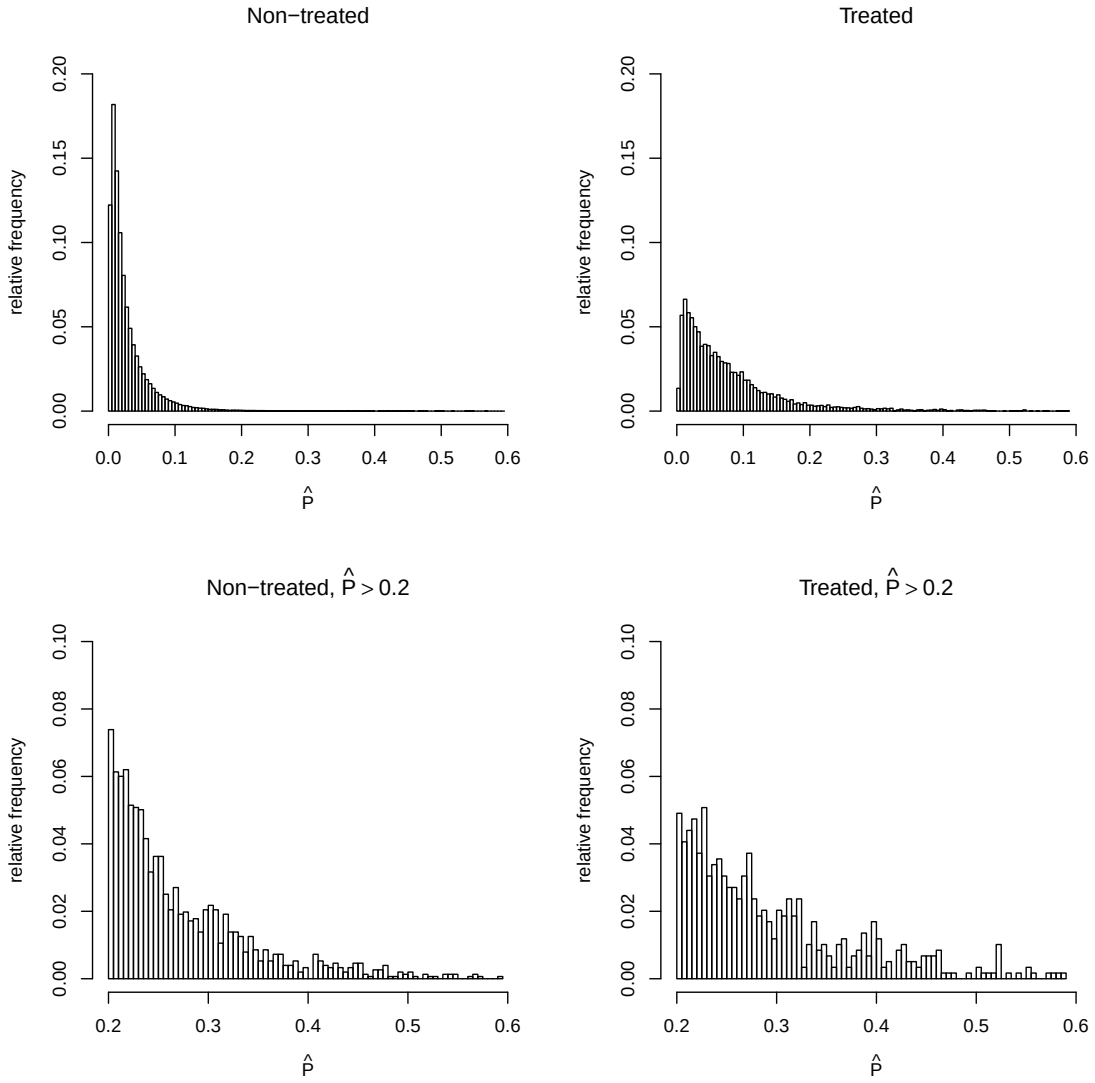

Figure 1: Histograms of the distributions of predicted propensity scores for treated and non-treated individuals

Note: The treatment variable is participation in any of the three LMPs, and the full list of covariates is given in Table 1.

a priori (Smith and Todd, 2005). However, in order to investigate the quality of our matches and whether there are any interior overlap problems, we experiment by setting the caliper at different values to see how many observations are dropped. The results are encouraging: Setting the caliper at 0.25 results in no dropped observations, and caliper values of 0.1 and 0.05 result in six and 18 dropped observations, respectively. As the total number of treated individuals in this particular sample is over 8,000, we feel reasonably assured that that we are able to find high-quality matches for most treated individuals.

5.3 Effect estimates

We estimate the compound ATT for all LMPs (i.e., participation in any of the three LMPs), and the ATT for each of the three LMPs separately. The outcome variables are labour income, employment status, un-subsidized employment status, disposable income, a categorical variable for received disability insurance (DI), and the amount of received DI. All measures are 12-month measures except for employment status which is measured in the month of November. We estimate annual effects for all these outcome variables from $t-5$ to $t+5$ (note that there should be no effect prior to treatment assignment at t). Due to the vast amount of estimates, the results are presented graphically in the main text. For completeness, the exact estimates and standard errors are collected in tables in Appendix B, but when discussed in the text, the numbers are rounded for the sake of simplicity. Although we present results estimated separately for each of the LMPs, we discuss those only when the results differ from the compound effects.

5.3.1 Labour income and employment status

We start with the compound effects of participation in any LMP on labour income and employment status, where we find large positive effects (see Figures 2 and 3). During the year of treatment start, there is an increase in labour income of 61,500 SEK. This is substantial by any standard, but even more so if compared to the mean labour income during the five-year pre-treatment period, which varies between 39,500 SEK and 87,200 SEK. The effect in $t+1$ is even higher (SEK 96,500). For employment in November, we see an effect of 47 percentage points during year t and 48 percentage points the year after. The likely reason for the higher effects during the year after treatment is that treatment start is spread out over the year, meaning that many individuals are not in treatment during a large portion of the start year.

In the short run, the large effects should be expected, since various forms of wage subsidies by definition imply an increase in labour income and employment. More importantly, however, the effects are persistent: For $t+5$, we estimate an effect of 57,100 SEK (labour income) and 26 percentage points (employment in November). Thus, the LMPs

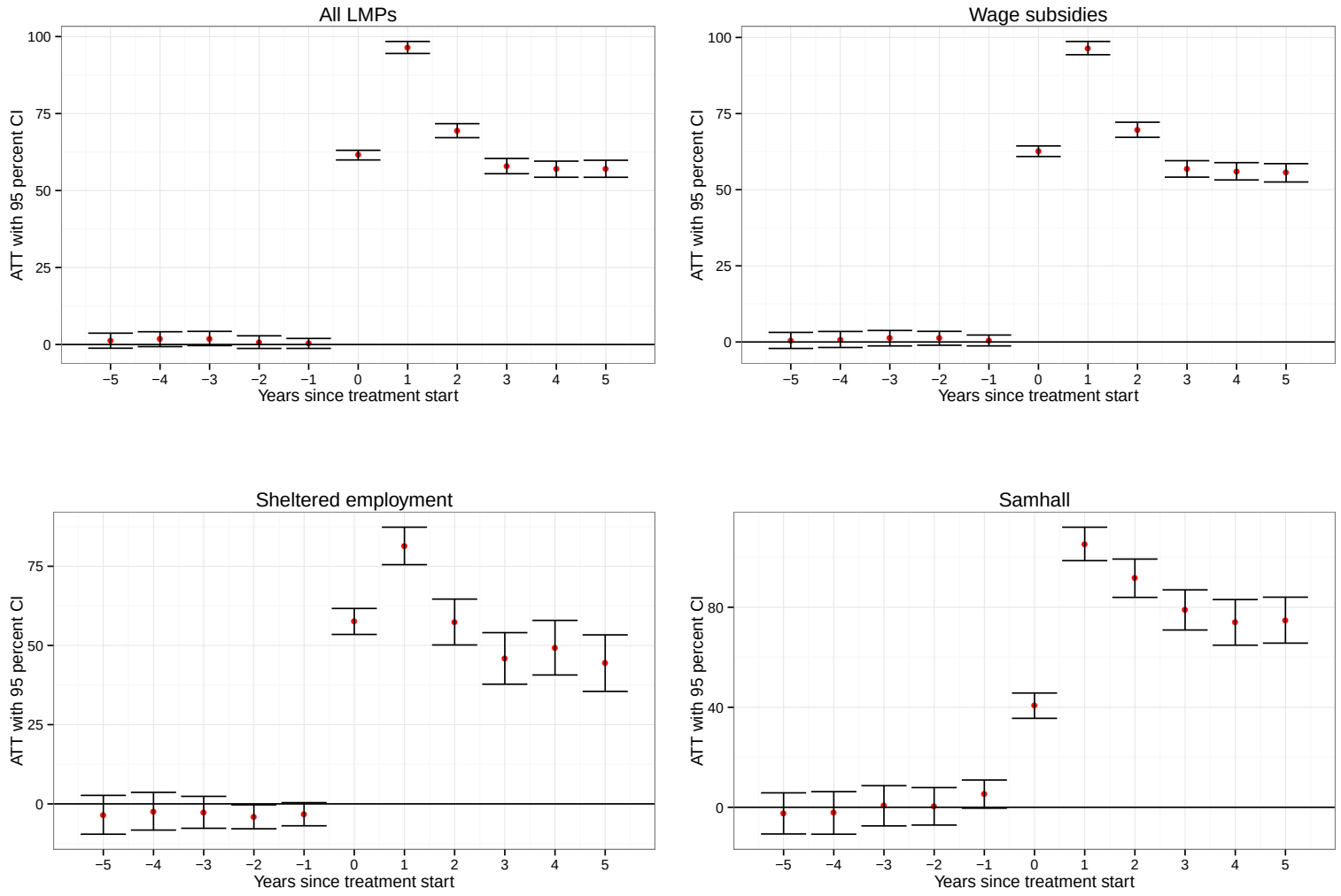

Figure 2: The effect of targeted LMPs on yearly labour income (1000s SEK)

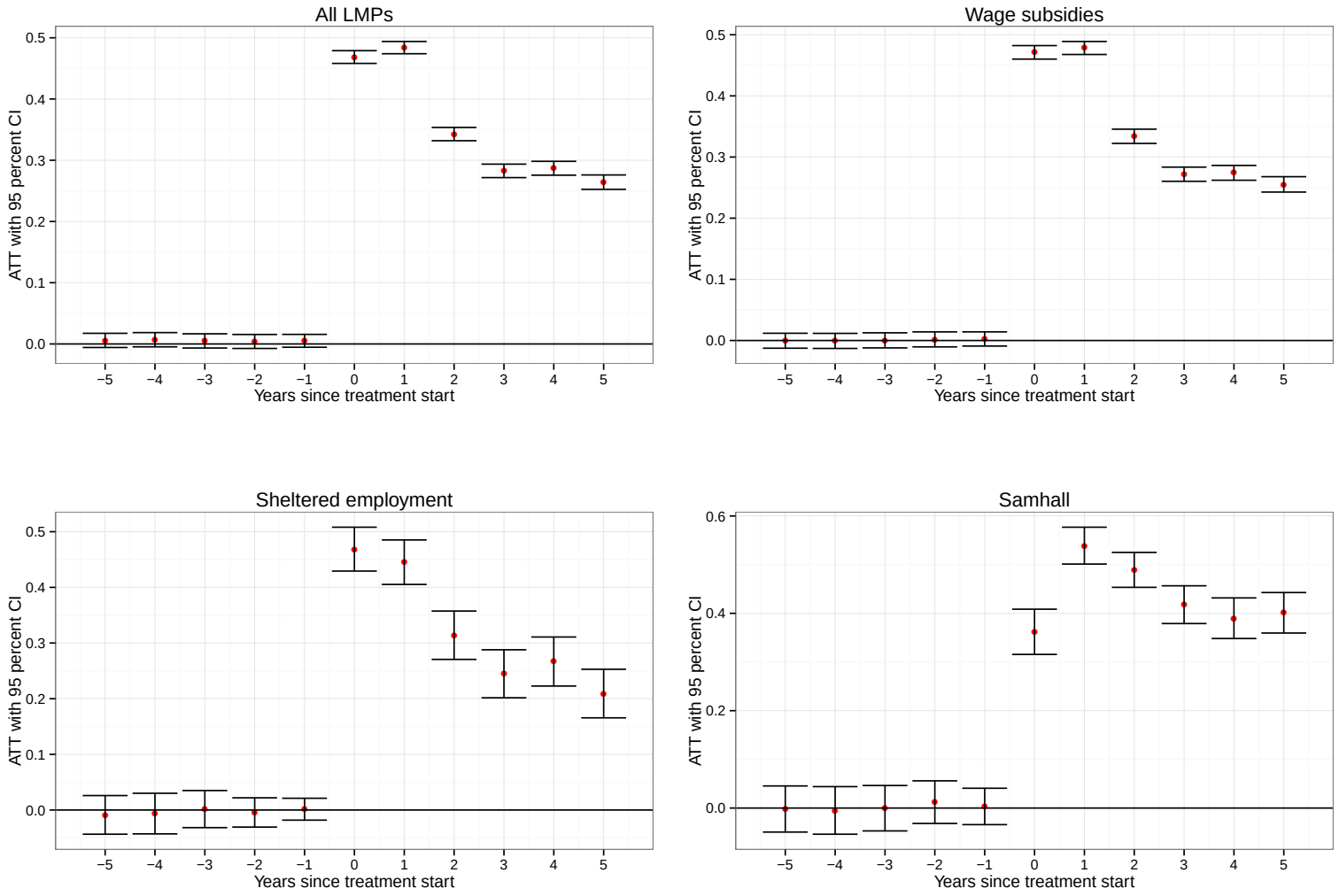

Figure 3: The effect of targeted LMPs on employment status (1 if employed in November)

that we consider seem to have a positive medium-term impact on both employment and earnings. In the next subsection, we investigate whether this is through other subsidized employment, either within the targeted LMPs or other LMPs involving wage subsidies, or through un-subsidized jobs on the regular labour market.

The effects estimated separately for the three LMPs are presented in Figures 2 and 3 for labour income and employment status, respectively. Qualitatively, the results for WS, SPE, and Samhall employment, are very similar; the effects are large, positive, and persistent. However, there are clear differences regarding the actual effect sizes, with the largest effects found for Samhall and the smallest for SPE. This is true for labour income as well as employment status, and the differences are perhaps more pronounced in terms of persistence (i.e., long-term effects), rather than during t or $t+1$. One possible explanation is that this is a more or less mechanic effect driven by differences in the maximum length of participation in the programmes. A sheltered employment at Samhall has no upper time limit, while approved wage subsidies or sheltered public employment should be reviewed after 1–3 years and 1 year, respectively. However, for both wage subsidies and sheltered public employment the time period can be prolonged and in case of a permanently reduced work capacity there is not necessarily an upper time limit. Another, and perhaps more probable, explanation is that the three programmes target quite different populations of job seeker with disabilities.

5.3.2 Un-subsidized employment

Since the targeted LMPs imply employment by definition, the positive effects of the LMPs on employment (see section 5.3.1) could be just a mechanical effect. To investigate this further, we estimate the effects on un-subsidized employment. Although not directly available in our data, an indicator for un-subsidized employment can be defined using a combination of the employment status variable (1 if employed in November) and participation in any LMP involving wage subsidies. Besides the targeted LMPs in the present study, there are ten additional LMPs involving wage subsidies.[19] Roughly three percent of the sample participated in these programmes during the pre-treatment period (see

[19]These correspond to search categories 45, 47, 48, 49, 50, 51, 56, 58, 77, and 78 in the administrative registers (see Arbetsmarknadsstyrelsen, 2007)

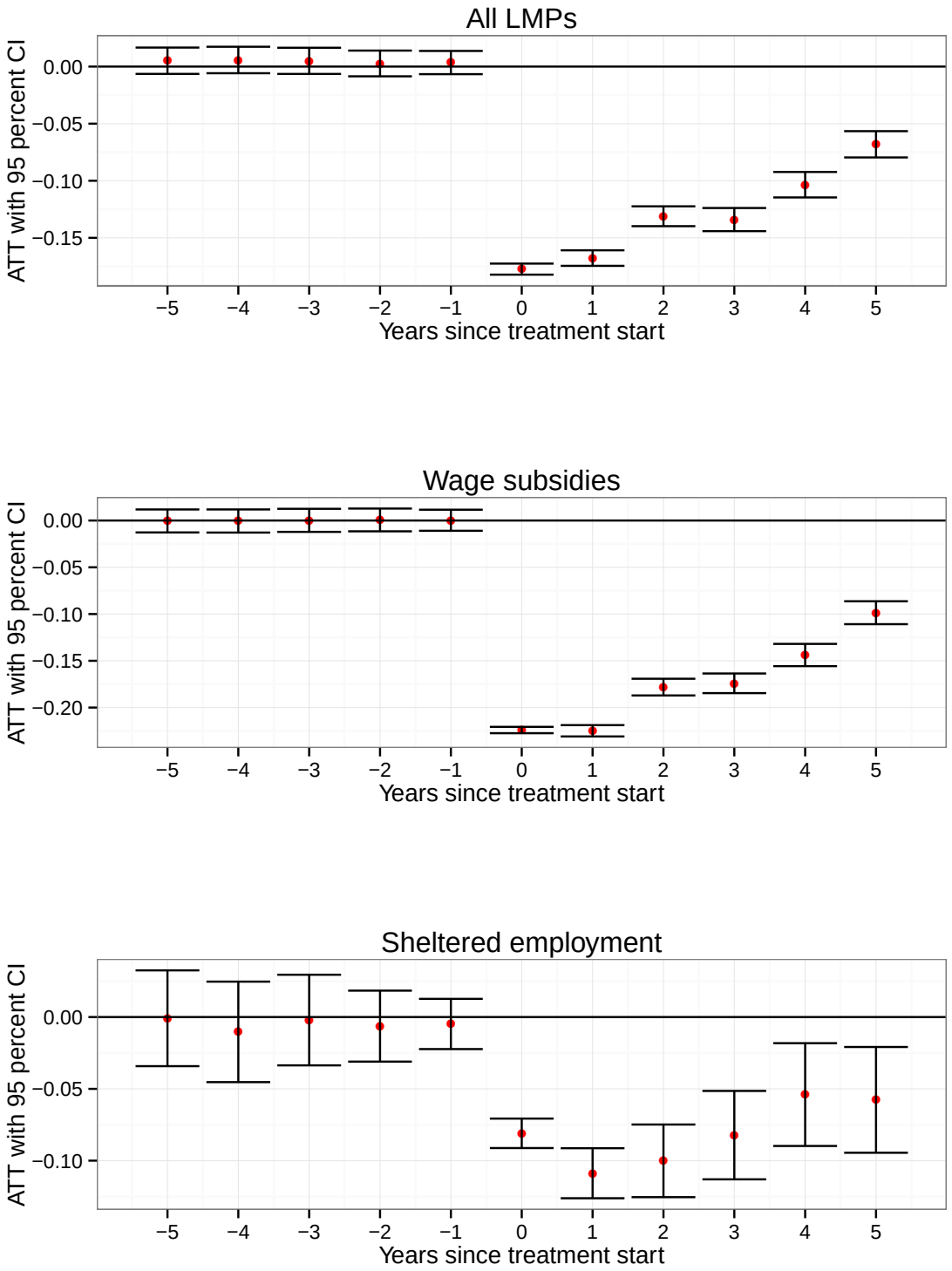

Figure 4: The effect of targeted LMPs on un-subsidized employment (1 if employed in November and non-participant in any LMPs involving wage subsidies during the year)

Table 1). Thus, an employment is deemed as un-subsidized if 1) the person is employed in November during a specific year, and 2) the person has not participated in any of the targeted LMPs, as well as any of the other ten LMPs involving wage subsidies, during the same year. Since we lack information on Samhall-employment other than the start date, the un-subsidized employment measure is not useful in that case, and we do not report any results for Samhall. When estimating the compound effect of program participation in any of the targeted LMPs, this implies that we overestimate the share of un-subsidized employments somewhat among the treated (but not among the matched controls). We see an advantage to estimating all effects on the same population, and have therefore not removed the Samhall-participants from the sample. For two reasons, this is not a problem of any significance. First, the share of Samhall-participants is quite small. Second, as will be seen below, excluding the Samhall-participants would simply strengthen the results.

The results are presented in Figure 4 and reveal clear negative effects of the LMPs on un-subsidized employment. The short-term effects are perhaps to be expected since the participants, by definition of the outcome variable, cannot have an un-subsidized employment during the year of program start. The share of employed in $t-1$ is 30 percent (see Table 1) and becomes zero by definition during t, even for participants who start receiving wage subsidies for an on-going employment. However, we see negative effects also in the longer run: The compound effect of receiving wage subsidies or sheltered employment is -7 percentage points five years after the program start. In other words, we find clear locking-in effects of program participation.

The effect magnitude is smaller for SPE than for wage subsidies, but the sign is negative and qualitatively, the results are very similar. One possible explanation for the difference in effect size is that the group with SPE is more negatively selected with respect to employment.

5.3.3 Disposable income

Disposable income is the sum of labour and net capital income as well as various transfers (net of taxes), the most significant being sickness absence, disability insurance, parental benefits, and welfare benefits. The results are presented in Figure 5 and we find a positive

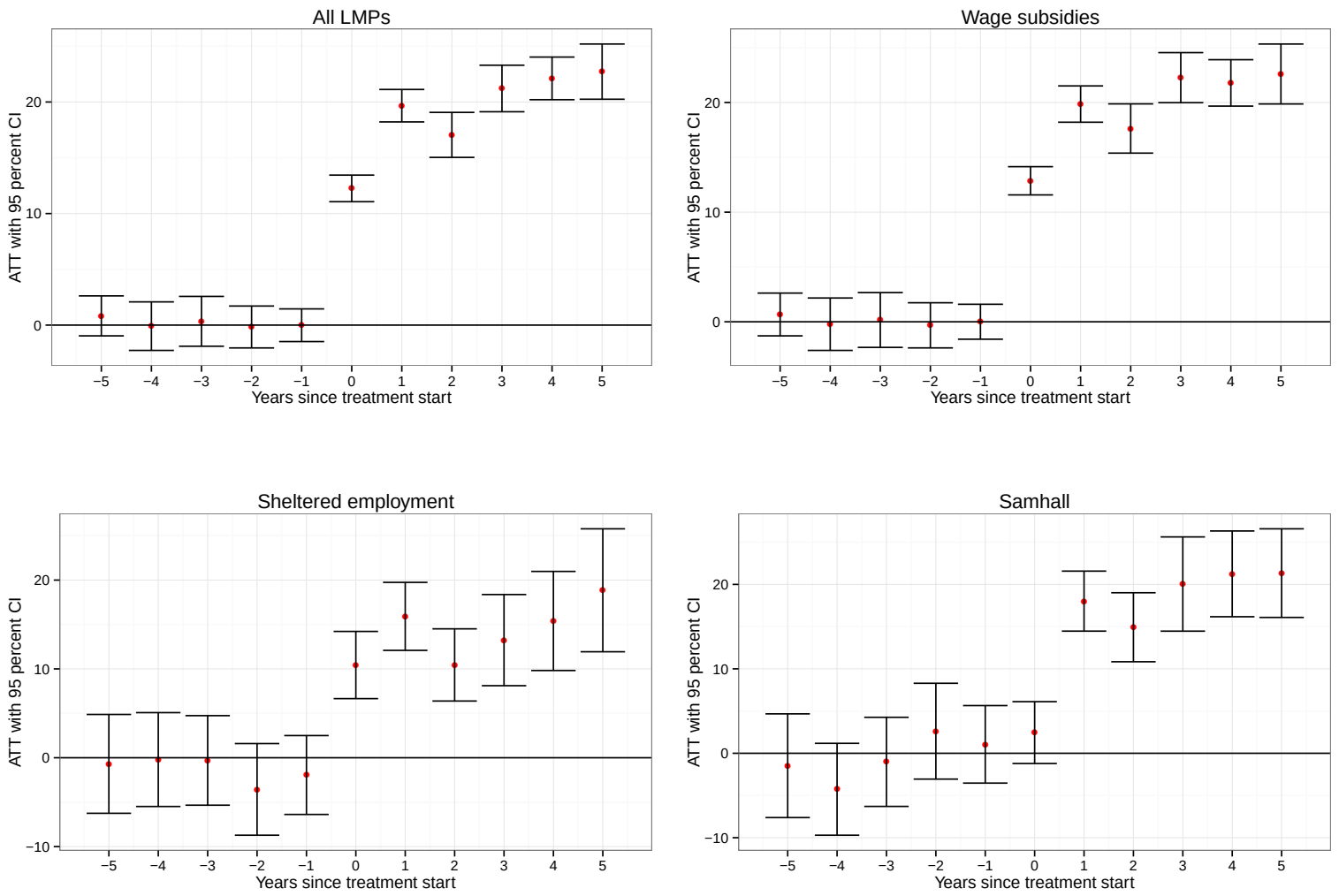

Figure 5: The effect of targeted LMPs on disposable income (1000s SEK/year)

effect of 12,300 SEK for participation in any LMP during the year of treatment. The effect is mostly driven by those receiving wage subsidies (12,900 SEK) and to some extent of the much fewer individuals in SPE, while there is no initial effect for Samhall. As with labour income and employment, the effects on disposable income are persistent, and even increasing, with a compound effect of 22,700 SEK five years after programme start. Starting from the year after programme start, there is also a positive effect of Samhall-employment on disposable income.

The effects on disposable income are lower than those on labour income. One reason is definitional, namely that disposable income is net of taxes, while labour income is expressed in gross terms. A second reason is that although labour income provides a substantial contribution to disposable income, an increase in labour income implies lower utilization of unemployment and social insurances.

5.3.4 Disability insurance

Disability insurance (DI) is administered by the Social Insurance Agency (SIA). A person can receive DI-benefits for a permanent work capacity reduction of at least 25% (Försäkringskassan, 2013). The amount of benefits received is at most 64% of the person's labour income during "some years" prior to the decision.[20] Depending on the extent of the loss of work capacity it can be paid in quarters of the full rate. We consider two alternative outcome measures of DI: prevalence of DI payment ($\mathbf{1}$[DI]), and the actual received amount.

The effects are presented in Figures 6 and 7. For the received amount of DI we find negative effects for all programmes, and they are increasing in magnitude over time. The magnitude of the effects when we consider all programmes is high relative to the mean DI value of about 10,900 SEK during $t-1$ (see Table 1). The highest effects are those following a Samhall-employment.

For DI-prevalence, the picture is somewhat more complex. Looking at the compound effect of all LMPs, there is an increase in the likelihood of receiving DI during the year of programme start and the following year. Then the estimates decrease in magnitude, and

[20]The information in Försäkringskassan (2013) is not clearer than that.

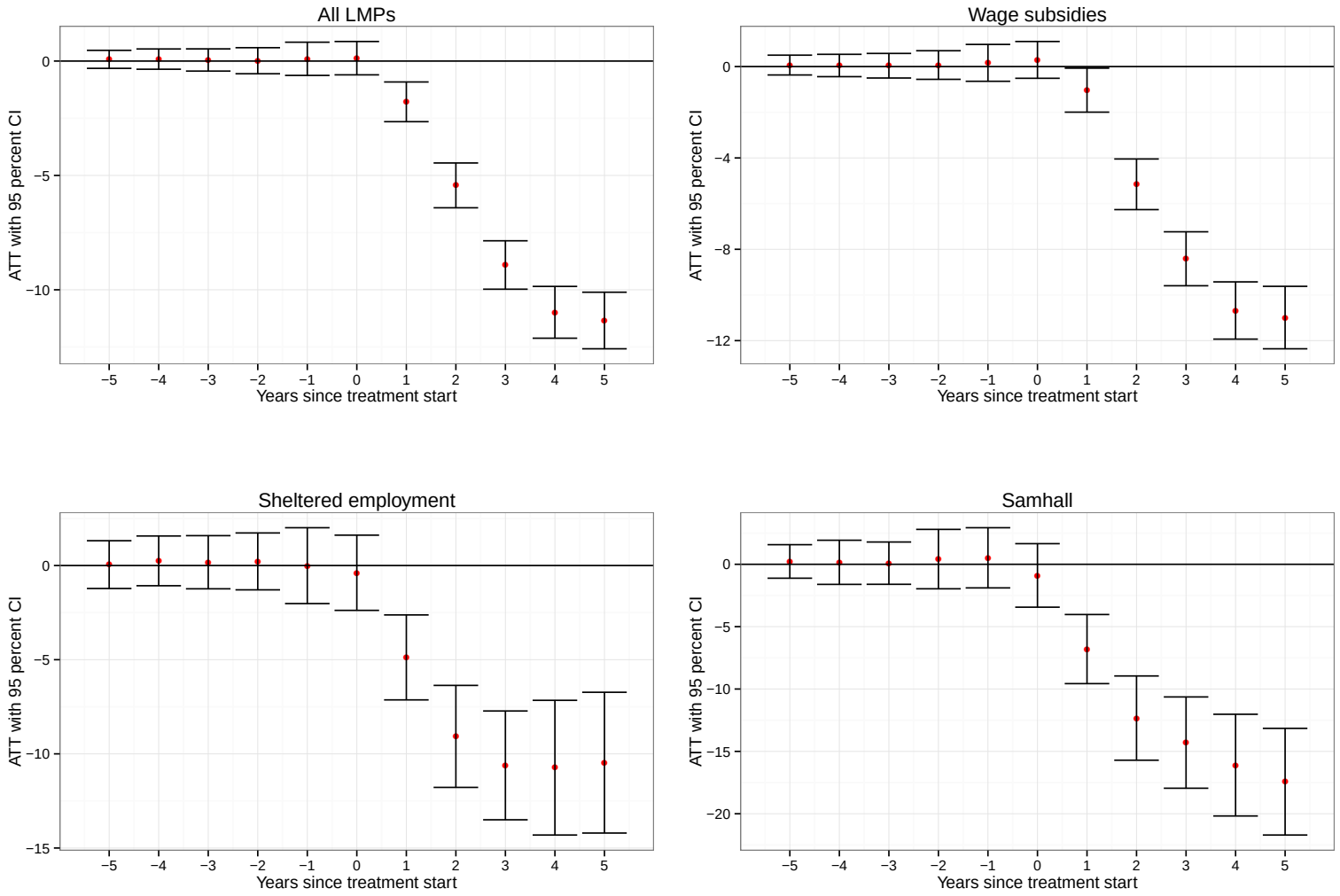

Figure 6: The effect of targeted LMPs on disability insurance payment (1000s SEK/year)

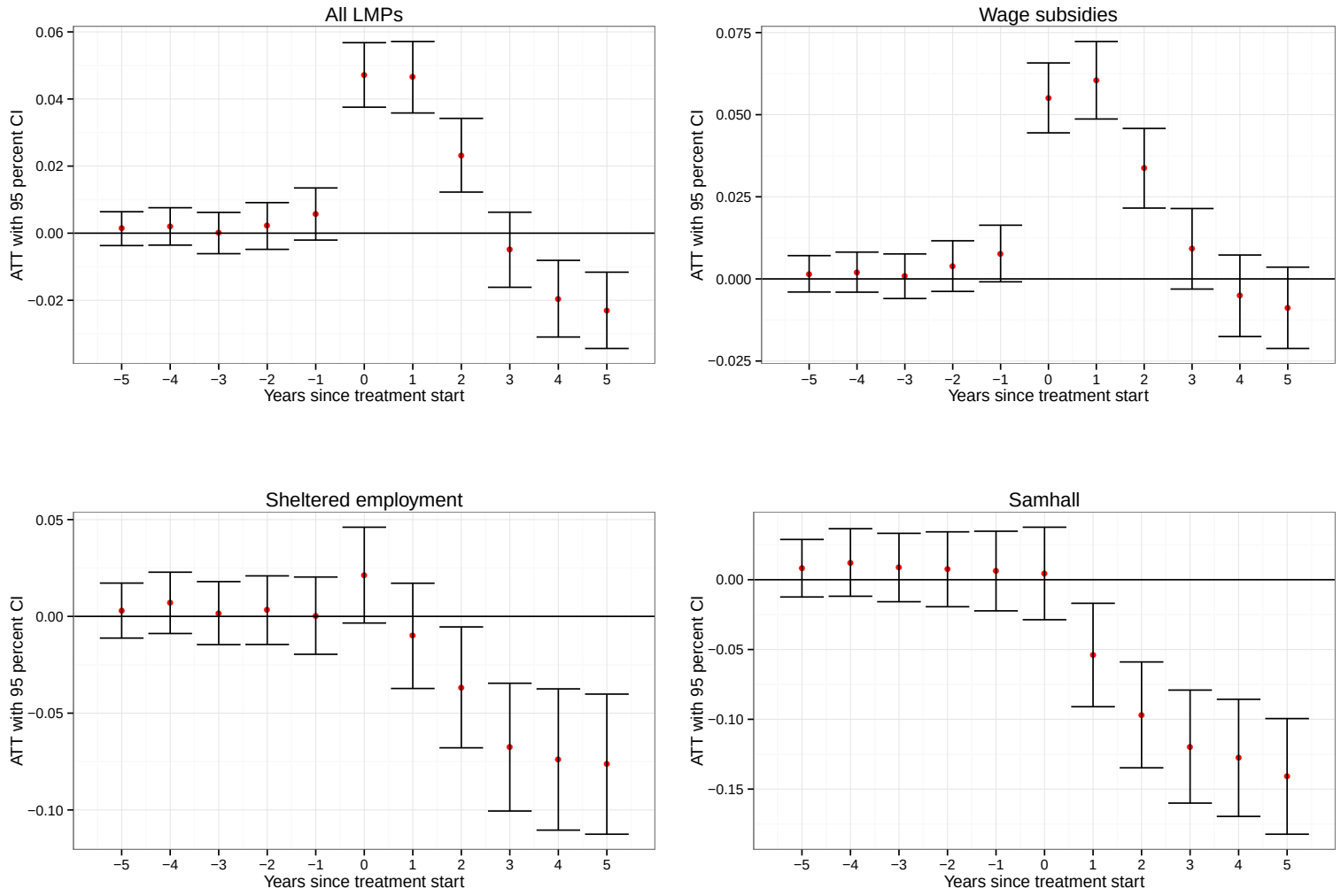

Figure 7: The effect of targeted LMPs on disability insurance prevalence (1 if yearly DI > 0)

eventually become negative and significant during years four and five after programme start. To study whether this is driven by the group of treated, or by the matched controls, we have looked at the prevalence of DI in both groups from $t-1$ to $t+5$. First, as can be seen in Table 1, the DI-prevalence is 0.13 in both groups one year prior to treatment start. There is an increase in the likelihood of receiving DI throughout the whole period for both groups: During the year of programme start, the prevalence of DI among the treated is 0.234 and the corresponding number among the controls is 0.186 (the difference being the effect estimate). Eventually, five years after treatment, the DI-prevalence increases to 0.33 among the treated and 0.36 among the controls. Thus, although the effect estimates are statistically significant four and five years after treatment start, they are relatively small compared to the DI-prevalence in both groups. The effects on the amount of DI paid are on the other hand significant; one of the impacts of the considered LMPs appears to have been a decrease in the extent of DI-payments along the 100%-75%-50%-25% levels of full payment, while the corresponding effects on DI-prevalence are of lower magnitude and less clear.

5.3.5 Separate results for males and females

In Appendix C, we report effects estimated separately for males and females. We estimate the effects using the same matching covariates and method as before, but separately for the two groups. Although the total number of observations is slightly higher for women (143,512 compared to 131,582), the number of treated males is higher (4,968 compared to 3,222). The only LMP with a relatively similar amount of female and male participants is employment at Samhall (226 females and 238 males). Below, we start by discussing qualitative (in terms of effect signs) gender differences in the compound effects of all LMPs, followed by qualitative gender differences in the effects of the various LMPs separately. We then discuss the relative magnitude of the effects.

In terms of the compound effects of all programmes, the results presented in Tables 8–13 are not qualitatively different for males and females with the exception of DI-prevalence: We see large positive effects on labour income, employment status, and disposable income for both groups during t to $t+5$, and negative effects on the amount

of DI received. However, the results for DI-prevalence once more turn out to be more complex. For males, there is a positive effect during the year of programme start, i.e., an increase in the likelihood of receiving DI. From two years after programme start and onwards, the effects are negative. For females on the other hand, the effects are positive during all years from t to $t+5$, and the effects close to programme start are much higher than for males. These differences between the effects for men and women are altogether driven by the larger group receiving wage subsidies. For Samhall-employment there seem to be no gender differences at all. Moreover, for SPE the effects for men are instead increasingly negative during the five years following programme start, while the effects for women are not significantly different from zero.

A similar gender difference for sheltered public employment is also found for the actual received amount of disability insurance (see Table 12). The estimates for females are not statistically significant, while those for males are both statistically and economically significant. Once more, the results for DI-prevalence point in different directions. First, for sheltered public employment, there are no significant effects for females. Second, the effects from wage subsidies for females close to programme start are much higher than those for males. These gender differences should not necessarily be interpreted as if the programmes were more effective for men, and might instead be due to the large differences between participating men and women regarding the type of impairment. For example, among males with sheltered public employment, 77 percent have a socio-medical disability, while the corresponding share among females is 36 percent.

If we instead turn to the magnitudes of the effects, apart from the already discussed cases where there are qualitative differences, the effect magnitudes in absolute terms are consistently larger for males than for females for all outcomes but employment status. This is to be expected for outcome variables expressed in levels since females earn less on average.

6 Conclusion

In this study, we estimate the effects of targeted LMPs on the labour market outcomes of job seekers with occupational disabilities using propensity score matching. We argue that is a particularly useful method in the setting at hand, for at least two reasons: First, the control group consists of individuals who are eligible for the targeted LMPs, but have not (yet) received treatment. Second, we have access to a rich data set, allowing us to estimate short- to medium-term effects, as well as to perform an informal check of the plausibility of the CIA.

Our results show large positive effects of all LMPs on labour income, disposable income, and employment. The largest effects are found for employment at Samhall, and the smallest for SPE. In the short run, the large effects should be expected almost by construction of the LMPs we investigate, but more importantly, the effects are persistent: Five years after programme start, we estimate an effect of 57,100 SEK (labour income), 26 percentage points (employment in November), and 22,700 SEK (disposable income). Thus, the LMPs appear to fulfil their aim of providing employment and earnings opportunities for persons with occupational disabilities. In addition, since the effects on disposable income also are positive, the increase in labour income is not netted out by a decrease in social security transfers, and so there are positive economic incentives at the individual level to participate in the programmes.

However, we also find clear locking-in effects, as has also been found in the previous literature (see e.g., van Ours, 2004). Five years after programme start, there is a negative effect on un-subsidized employment, which is more pronounced for wage subsidies (-10 percentage points) than for sheltered public employment (-6 percentage points). These effects are quite substantial as the share with un-subsidized employment among the treated ranges between 20 percent (for sheltered employment) and 31 percent (for wage subsidies). We see two possible explanations for the locking-in effects: either the participants tend to reduce their effort in search for jobs on the regular labour market, or the PES case workers reduce their matching effort. Of course, both of these mechanisms might be working at the same time, and we cannot disentangle their effects. Also, there is no

doubt that the heterogeneity among the job seekers in our sample is considerable: A large share of the occupationally disabled job seekers with severe disabilities probably have low chances of finding a job on the regular labour market. At the same time, the average locking-in effects that we estimate are quite substantial, which casts doubt on whether the PES caseworkers assign wage subsidies to the most needy.

Disability retirement, at least on part time, is likely to be a reality for many job seekers with disabilities. Therefore, to investigate if and how the programmes affect the utilization of disability insurance, we estimate the effects on the likelihood of receiving DI, and on the received amount of DI. While there are clear negative effects of programme participation on the received amounts (i.e., the participants have lower utilization rates), the effects on the likelihood of receiving DI are less clear-cut: The compound effects are changing from positive to negative over time, but are all small compared to the shares of disability insurance receivers. This suggests that programme participation affects the intensive, but not the extensive margin of utilization of disability insurance.

The largest differences between the effects of the three programmes as well as between males and females are found for DI-utilization. Compared to wage subsidies, sheltered employment, either at a public employer or at Samhall, seems to a lesser extent be combined with disability retirement. Among women, wage subsidies even seem to increase the share that utilize DI, while sheltered employment, especially at Samhall, seems to have the opposite effect. At least partly, these differences could be a consequence of the programmes' different target groups, and if men and women within the same programme having different impairments. This illustrates an important point: The findings in this study cannot be generalized to populations of job seekers other than those with occupational disabilities; nor can the findings for a particular programme be assumed to be valid for groups of job seekers with other types of disabilities.

Finally, it would be interesting to relate the total cost of the programmes to the benefits in terms of higher earnings and employment. This is important not least because of the substantial locking-in effects that we report. This type of cost-benefit analysis might be possible to perform if we had access to the actual amounts of wage subsidies paid, which are unfortunately unavailable to us.

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Appendix A: Data description and covariate balance

Table 1: Covariate balance

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
						Labour income in 1000s SEK							
$t-1$	39.4734	*69.9827*	43.3505	*71.6904*	39.1272	*69.577*	-0.0554	0	*	0.0049	0.7492		
$t-2$	59.0982	*88.9021*	58.8758	*85.0036*	58.3278	*87.3575*	0.0025	0.8223		0.0087	0.5727		
$t-3$	78.9876	*100.1109*	72.4164	*94.0174*	77.0364	*99.5791*	0.0656	0	*	0.0195	0.2066		
$t-4$	87.7781	*104.2641*	77.1191	*96.7233*	86.0528	*104.3795*	0.1022	0	*	0.0165	0.2844		
$t-5$	87.1854	*105.064*	74.6381	*96.7544*	85.9504	*105.1532*	0.1194	0	*	0.0118	0.4452		
						Employment in November (1/0)							
$t-1$	0.2994	*0.458*	0.3095	*0.4623*	0.2943	*0.4557*	-0.0221	0.0473	*	0.0112	0.4682		✓
$t-2$	0.4131	*0.4924*	0.4007	*0.4901*	0.4091	*0.4917*	0.0252	0.0239	*	0.0083	0.5927		✓
$t-3$	0.4545	*0.498*	0.4357	*0.4958*	0.4495	*0.4974*	0.0378	0.0007	*	0.01	0.519		✓
$t-4$	0.4996	*0.5*	0.4683	*0.499*	0.4927	*0.4999*	0.0626	0	*	0.0138	0.3722		✓
$t-5$	0.4906	*0.4999*	0.4404	*0.4964*	0.4848	*0.4998*	0.1004	0	*	0.0116	0.4509		✓
						Subsidized employment during the year (1[participation in *other* than the examined LMPs involving employment subsidies])							
$t-1$	0.0379	*0.1909*	0.0302	*0.1712*	0.0385	*0.1924*	0.04	0.0003	*	-0.0033	0.8272		✓
$t-2$	0.0256	*0.1579*	0.0265	*0.1606*	0.0257	*0.1581*	-0.0059	0.5962		-0.0006	0.9687		✓
$t-3$	0.0264	*0.1604*	0.0263	*0.1602*	0.0267	*0.1611*	0.0005	0.9678		-0.0015	0.9206		✓
$t-4$	0.0282	*0.1656*	0.027	*0.162*	0.0283	*0.1659*	0.0075	0.499		-0.0006	0.9714		✓
$t-5$	0.0288	*0.1673*	0.0264	*0.1604*	0.0283	*0.1657*	0.0143	0.1989		0.0034	0.8263		✓
						Un-subsidized employment (employment in November and no subsidized employment during the year)							
$t-1$	0.2789	*0.4485*	0.2928	*0.455*	0.2754	*0.4467*	-0.031	0.0055	*	0.0079	0.6103		
$t-2$	0.3957	*0.489*	0.3835	*0.4862*	0.3929	*0.4884*	0.0249	0.0256	*	0.0056	0.7161		
$t-3$	0.4372	*0.4961*	0.4183	*0.4933*	0.4321	*0.4954*	0.0379	0.0007	*	0.0101	0.5127		
$t-4$	0.4808	*0.4997*	0.4502	*0.4975*	0.4751	*0.4994*	0.0612	0	*	0.0115	0.4553		
$t-5$	0.4735	*0.4993*	0.425	*0.4943*	0.4683	*0.499*	0.0971	0	*	0.0103	0.5056		
						Disposable income in 1000s SEK							
$t-1$	134.8527	*64.2291*	137.1851	*68.0823*	134.864	*59.3138*	-0.0363	0.0012	*	-0.0002	0.9906		✓
$t-2$	137.8506	*82.7397*	138.3349	*68.676*	138.0187	*71.1152*	-0.0059	0.598		-0.002	0.8883		
$t-3$	135.3528	*100.2984*	134.721	*68.7818*	135.0139	*67.9921*	0.0063	0.5691		0.0034	0.7989		
$t-4$	132.879	*92.7341*	131.0591	*81.9787*	132.9725	*92.893*	0.0196	0.0775		-0.001	0.9483		
$t-5$	124.4366	*77.9548*	121.4538	*67.107*	123.6095	*70.7386*	0.0383	0.0006	*	0.0106	0.4732		
						Unemployment benefits in 1000s SEK							
$t-1$	191.9759	*374.9233*	260.3186	*416.2562*	194.1625	*377.746*	-0.1823	0	*	-0.0058	0.7024		✓
$t-2$	123.9092	*280.3008*	166.5996	*307.3982*	124.7546	*280.6483*	-0.1523	0	*	-0.003	0.8442		✓
$t-3$	117.3201	*258.8881*	160.1896	*293.7924*	120.0106	*262.1107*	-0.1656	0	*	-0.0104	0.5001		✓
$t-4$	146.9668	*280.7109*	190.1814	*310.7365*	152.331	*290.2964*	-0.1539	0	*	-0.0191	0.2175		✓
$t-5$	159.7405	*298.0101*	200.1934	*324.1486*	162.9341	*302.3004*	-0.1357	0	*	-0.0107	0.4876		✓

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Table 1 – Continued

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
						Disability insurance in 1000s SEK							
$t-1$	10.9139	*31.5543*	8.8751	*28.013*	10.816	*31.943*	0.0646	0	*	0.0031	0.8423		✓
$t-2$	7.4024	*24.7743*	6.0336	*21.8316*	7.388	*24.7965*	0.0552	0	*	0.0006	0.9701		✓
$t-3$	5.465	*21.0451*	4.626	*19.0057*	5.4182	*21.0312*	0.0399	0.0003	*	0.0022	0.8858		✓
$t-4$	4.5158	*19.3481*	3.8143	*17.5059*	4.4302	*19.1857*	0.0363	0.0011	*	0.0044	0.7746		✓
$t-5$	3.5573	*17.1104*	2.9448	*15.1907*	3.4822	*16.9676*	0.0358	0.0013	*	0.0044	0.7766		✓
						Disability insurance prevalence (1/0)							
$t-1$	0.1333	*0.3399*	0.1131	*0.3167*	0.1276	*0.3336*	0.0594	0	*	0.0168	0.2732		
$t-2$	0.1029	*0.3038*	0.0881	*0.2834*	0.1007	*0.301*	0.0487	0	*	0.0071	0.6465		
$t-3$	0.0774	*0.2673*	0.0687	*0.253*	0.0774	*0.2672*	0.0326	0.0034	*	0.0002	0.9907		
$t-4$	0.0628	*0.2427*	0.0545	*0.227*	0.0608	*0.239*	0.0345	0.0019	*	0.0083	0.5896		
$t-5$	0.0507	*0.2193*	0.0437	*0.2043*	0.0493	*0.2165*	0.0319	0.0041	*	0.0062	0.6883		
						Welfare benefits in 1000s SEK							
$t-1$	7.8266	*21.114*	6.3091	*18.2504*	7.9629	*21.8714*	0.0719	0	*	-0.0065	0.6811		✓
$t-2$	5.8209	*18.5522*	4.9245	*16.2095*	5.9327	*18.6683*	0.0483	0	*	-0.006	0.6983		✓
$t-3$	5.3678	*17.242*	4.9512	*15.88*	5.4764	*17.112*	0.0242	0.0299	*	-0.0063	0.6839		✓
$t-4$	5.8824	*17.3736*	5.6724	*16.5388*	5.9501	*17.3048*	0.0121	0.2779		-0.0039	0.8012		✓
$t-5$	6.077	*17.7188*	5.8761	*16.3776*	6.0712	*17.0885*	0.0113	0.3083		0.0003	0.9828		✓
						Days in unemployment/year							
$t-1$	117.4973	*147.6179*	176.7037	*153.5756*	119.3946	*145.643*	-0.4011	0	*	-0.0129	0.369		✓
$t-2$	88.6466	*135.1496*	145.9467	*150.8758*	89.7009	*133.2722*	-0.424	0	*	-0.0078	0.5914		✓
$t-3$	82.8404	*131.3221*	132.1871	*148.4929*	83.2536	*130.0005*	-0.3758	0	*	-0.0031	0.8305		✓
$t-4$	84.3106	*133.5434*	129.7543	*149.5088*	84.7866	*131.9466*	-0.3403	0	*	-0.0036	0.8095		✓
$t-5$	85.614	*134.8081*	129.9245	*149.8519*	85.9153	*132.8626*	-0.3287	0	*	-0.0022	0.8799		✓
						In-hospitalization days (total number of days $t-5$ to $t-1$)							
	1.7324	*22.778*	1.833	*23.0363*	1.7481	*17.9465*	-0.0044	0.6924		-0.0007	0.9609		✓
						In-hospitalization category based on ICD-10 code (any occurence in main or secondary diagnosis $t-5$ to $t-1$; see Appendix D for list of categories)							
1	0.0285	*0.1663*	0.0216	*0.1453*	0.0281	*0.1653*	0.0414	0.0002	*	0.0022	0.8865		✓
2	0.0181	*0.1333*	0.0199	*0.1397*	0.0177	*0.1317*	-0.0136	0.2217		0.0033	0.8324		✓
3	0.0066	*0.0812*	0.0074	*0.086*	0.0069	*0.0829*	-0.01	0.3702		-0.0036	0.8176		✓
4	0.0259	*0.159*	0.0279	*0.1646*	0.0259	*0.1589*	-0.0123	0.2717		0.0001	0.9952		✓
5	0.0922	*0.2893*	0.0763	*0.2655*	0.091	*0.2877*	0.0548	0	*	0.0039	0.802		✓
6	0.0263	*0.16*	0.0236	*0.1518*	0.0266	*0.1608*	0.0169	0.1283		-0.0017	0.9143		✓
7	0.0058	*0.0759*	0.0055	*0.0737*	0.0057	*0.0753*	0.0043	0.697		0.0011	0.9422		✓
8	0.006	*0.0774*	0.006	*0.0771*	0.0059	*0.0765*	0.0006	0.9578		0.0018	0.9056		✓
9	0.0441	*0.2054*	0.0374	*0.1898*	0.0442	*0.2055*	0.0327	0.0033	*	-0.0001	0.9939		✓
10	0.0303	*0.1714*	0.0298	*0.1701*	0.0293	*0.1687*	0.0027	0.8106		0.0057	0.7128		✓
11	0.054	*0.2261*	0.0566	*0.231*	0.0543	*0.2267*	-0.0112	0.3142		-0.0013	0.9338		✓
12	0.0113	*0.1059*	0.0095	*0.0971*	0.011	*0.1043*	0.0172	0.1211		0.0032	0.8335		✓
13	0.0597	*0.237*	0.0491	*0.2161*	0.0588	*0.2353*	0.0448	0.0001	*	0.0038	0.8069		✓

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Table 1 – Continued

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
14	0.0341	*0.1816*	0.0382	*0.1916*	0.0337	*0.1806*	-0.0221	0.0479	*	0.0022	0.8879		✓
15	0.0456	*0.2086*	0.0832	*0.2761*	0.0459	*0.2093*	-0.18	0	*	-0.0015	0.9202		✓
16	0.0002	*0.0155*	0.0003	*0.0184*	0.0002	*0.0158*	-0.0062	0.5814		-0.0005	0.9765		✓
17	0.0055	*0.0743*	0.006	*0.077*	0.0057	*0.075*	-0.0055	0.6206		-0.0014	0.9284		✓
18	0.0795	*0.2705*	0.078	*0.2682*	0.0792	*0.27*	0.0055	0.6208		0.0012	0.9402		✓
19	0.1075	*0.3097*	0.0911	*0.2878*	0.1057	*0.3075*	0.0528	0	*	0.0056	0.7164		✓
21	0.0583	*0.2343*	0.0588	*0.2353*	0.0579	*0.2336*	-0.0025	0.8227		0.0014	0.9298		✓
99	0.0006	*0.0246*	0.0005	*0.0229*	0.0005	*0.0234*	0.0032	0.7742		0.0022	0.8851		✓
Disability code at t (see Appendix D for code list)													
Young disabled	0.051	*0.2201*	0.0552	*0.2284*	0.0513	*0.2207*	-0.0191	0.0875		-0.0014	0.9302		✓
11	0.0317	*0.1753*	0.0258	*0.1586*	0.0323	*0.1767*	0.0336	0.0025	*	-0.0031	0.8439		✓
20–22	0.0333	*0.1794*	0.0446	*0.2065*	0.0332	*0.1792*	-0.0632	0	*	0.0004	0.9777		✓
30–32	0.0162	*0.1261*	0.0167	*0.128*	0.0156	*0.124*	-0.0039	0.7244		0.0042	0.7829		✓
40–42	0.4403	*0.4965*	0.4587	*0.4983*	0.4353	*0.4958*	-0.0371	0.0009	*	0.01	0.5174		✓
51	0.1345	*0.3412*	0.1579	*0.3647*	0.1346	*0.3413*	-0.0687	0	*	-0.0004	0.9812		✓
61	0.1925	*0.3943*	0.1754	*0.3803*	0.1937	*0.3952*	0.0435	0.0001	*	-0.0029	0.8512		✓
71	0.0616	*0.2405*	0.0406	*0.1973*	0.0654	*0.2473*	0.0876	0	*	-0.0158	0.3129		✓
81	0.1636	*0.3699*	0.1018	*0.3024*	0.1635	*0.3698*	0.167	0	*	0.0003	0.9841		✓
91	0.0333	*0.1794*	0.0556	*0.2292*	0.0343	*0.182*	-0.1245	0	*	-0.0056	0.717		✓
92	0.0615	*0.2403*	0.082	*0.2744*	0.0622	*0.2414*	-0.0854	0	*	-0.0026	0.8651		✓
93	0.0104	*0.1013*	0.007	*0.0832*	0.0114	*0.1061*	0.0335	0.0025	*	-0.01	0.5312		✓
Demographic variables at t													
Widow(er)	0.0076	*0.0868*	0.0084	*0.091*	0.0081	*0.0895*	-0.0087	0.4342		-0.0055	0.7273		✓
Married	0.3103	*0.4626*	0.2986	*0.4576*	0.3118	*0.4632*	0.0252	0.0239	*	-0.0032	0.8348		✓
Unmarried	0.5122	*0.4999*	0.5136	*0.4998*	0.5117	*0.4999*	-0.0027	0.8059		0.0009	0.9522		✓
Divorced	0.17	*0.3756*	0.1795	*0.3838*	0.1685	*0.3743*	-0.0254	0.0231	*	0.004	0.7946		✓
Immigrant	0.1846	*0.388*	0.2237	*0.4167*	0.1842	*0.3877*	-0.1009	0	*	0.0009	0.9538		✓
Female	0.4007	*0.4901*	0.5254	*0.4994*	0.4015	*0.4902*	-0.2545	0	*	-0.0016	0.9161		✓
Age group													
26–30	0.093	*0.2905*	0.1026	*0.3034*	0.0923	*0.2894*	-0.0329	0.0033	*	0.0024	0.8747		✓
31–35	0.1148	*0.3188*	0.1299	*0.3362*	0.1139	*0.3177*	-0.0472	0	*	0.003	0.8466		✓
36–40	0.1552	*0.3622*	0.1594	*0.366*	0.1555	*0.3624*	-0.0114	0.3052		-0.0008	0.9604		✓
41–45	0.1552	*0.3622*	0.1502	*0.3572*	0.157	*0.3638*	0.014	0.2093		-0.0048	0.7583		✓
46–50	0.1373	*0.3442*	0.1318	*0.3383*	0.1371	*0.3439*	0.0158	0.1563		0.0005	0.9736		✓
51–55	0.1366	*0.3434*	0.1199	*0.3248*	0.1369	*0.3438*	0.0485	0	*	-0.0011	0.9452		✓
56–	0.0899	*0.286*	0.0806	*0.2722*	0.09	*0.2862*	0.0323	0.0037	*	-0.0004	0.9785		✓

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Table 1 – Continued

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
Education group													
upper secondary	0.6097	*0.4879*	0.6152	*0.4865*	0.6092	*0.4879*	-0.0114	0.308		0.0009	0.9539		✓
university	0.1014	*0.3019*	0.1242	*0.3298*	0.1017	*0.3022*	-0.0754	0	*	-0.0008	0.9594		✓
unknown	0.0089	*0.0941*	0.0066	*0.0809*	0.0092	*0.0954*	0.0248	0.0253	*	-0.0028	0.8575		✓
Number of children in different age groups													
0–3	0.0788	*0.2999*	0.102	*0.3432*	0.078	*0.3069*	-0.0774	0	*	0.0025	0.8737		✓
11–15	0.2439	*0.5545*	0.2538	*0.5654*	0.2459	*0.5588*	-0.0179	0.1086		-0.0035	0.8223		✓
16–17	0.0953	*0.3081*	0.0905	*0.2998*	0.0947	*0.3082*	0.0157	0.16		0.0021	0.8934		✓
>17	0.2732	*0.5758*	0.2458	*0.5558*	0.271	*0.5829*	0.0476	0	*	0.0039	0.8053		✓
4–6	0.0793	*0.2949*	0.0991	*0.3283*	0.0801	*0.2988*	-0.0672	0	*	-0.0028	0.8591		✓
7–10	0.1438	*0.414*	0.1598	*0.4247*	0.1465	*0.4094*	-0.0387	0.0005	*	-0.0066	0.6708		✓
County													
Uppsala	0.0288	*0.1673*	0.0281	*0.1654*	0.0295	*0.1692*	0.0041	0.7147		-0.0039	0.8015		✓
Södermanland	0.0296	*0.1694*	0.0338	*0.1807*	0.0299	*0.1704*	-0.025	0.0254	*	-0.0022	0.8865		✓
Östergötland	0.044	*0.2052*	0.0473	*0.2122*	0.0444	*0.206*	-0.0158	0.1575		-0.0017	0.9109		✓
Jönköping	0.0373	*0.1894*	0.029	*0.1679*	0.037	*0.1887*	0.0434	0.0001	*	0.0016	0.9183		✓
Kronoberg	0.0172	*0.1302*	0.0133	*0.1147*	0.0178	*0.1322*	0.0301	0.0069	*	-0.0041	0.7933		✓
Kalmar	0.0263	*0.16*	0.0301	*0.1709*	0.0261	*0.1593*	-0.0238	0.0333	*	0.0014	0.927		✓
Gotland	0.0097	*0.0978*	0.0084	*0.0915*	0.0095	*0.0972*	0.0123	0.2694		0.0011	0.9443		✓
Blekinge	0.0183	*0.1342*	0.0146	*0.1198*	0.0177	*0.1319*	0.0281	0.0116	*	0.0047	0.761		✓
Skåne	0.1276	*0.3337*	0.1401	*0.3471*	0.1255	*0.3313*	-0.0374	0.0008	*	0.0064	0.6783		✓
Halland	0.0265	*0.1607*	0.0214	*0.1449*	0.0275	*0.1635*	0.0317	0.0044	*	-0.0058	0.7087		✓
Västra Götaland	0.1378	*0.3447*	0.1548	*0.3617*	0.1374	*0.3443*	-0.0494	0	*	0.001	0.949		✓
Värmland	0.0462	*0.2099*	0.0463	*0.2101*	0.0458	*0.2091*	-0.0003	0.975		0.0018	0.908		✓
Örebro	0.0372	*0.1891*	0.0285	*0.1663*	0.0374	*0.1897*	0.046	0	*	-0.0012	0.9407		✓
Västmanland	0.0346	*0.1828*	0.0378	*0.1906*	0.0347	*0.1831*	-0.0172	0.123		-0.0007	0.9651		✓
Dalarnas	0.0356	*0.1853*	0.0362	*0.1868*	0.0364	*0.1872*	-0.0034	0.7582		-0.0043	0.7857		✓
Gävleborg	0.0355	*0.185*	0.0321	*0.1763*	0.0359	*0.1862*	0.018	0.1054		-0.0026	0.8667		✓
Västernorrland	0.0375	*0.19*	0.0354	*0.1849*	0.0368	*0.1884*	0.0109	0.3267		0.0036	0.8176		✓
Jämtland	0.024	*0.1531*	0.0167	*0.1283*	0.023	*0.15*	0.0474	0	*	0.0064	0.6769		✓
Västerbotten	0.0416	*0.1997*	0.0329	*0.1783*	0.0427	*0.2021*	0.0438	0.0001	*	-0.0053	0.7322		✓
Norrbotten	0.0507	*0.2193*	0.056	*0.23*	0.0516	*0.2213*	-0.0244	0.0287	*	-0.0044	0.7756		✓
Sickness absence (days per year; Q_i denotes ith quartile calculated using values >0 and <365)													
$(0, Q_2]$, $t-1$	0.0434	*0.2038*	0.0638	*0.2444*	0.0445	*0.2061*	-0.0999	0	*	-0.0051	0.7416		✓
$(Q_2, Q_3]$, $t-1$	0.0448	*0.2068*	0.0633	*0.2435*	0.0443	*0.2057*	-0.0896	0	*	0.0024	0.8778		✓
$(Q_3, Q_4]$, $t-1$	0.0637	*0.2442*	0.0874	*0.2824*	0.0635	*0.2438*	-0.097	0	*	0.001	0.9491		✓
$(Q_4, 365)$, $t-1$	0.1099	*0.3128*	0.1171	*0.3216*	0.1078	*0.3101*	-0.0232	0.0379	*	0.0067	0.6666		✓
$=365$, $t-1$	0.2532	*0.4349*	0.1705	*0.376*	0.2506	*0.4334*	0.1903	0	*	0.006	0.6941		✓
$(0, Q_2]$, $t-2$	0.0514	*0.2208*	0.0682	*0.2521*	0.0511	*0.2202*	-0.0761	0	*	0.0013	0.9336		✓
$(Q_2, Q_3]$, $t-2$	0.0584	*0.2345*	0.0701	*0.2553*	0.0598	*0.237*	-0.05	0	*	-0.0058	0.7082		✓

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Table 1 – Continued

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
$(Q_3, Q_4]$, $t-2$	0.0847	*0.2784*	0.0979	*0.2971*	0.0842	*0.2777*	-0.0474	0	*	0.0017	0.9135		✓
$(Q_4, 365)$, $t-2$	0.1238	*0.3293*	0.125	*0.3307*	0.1232	*0.3287*	-0.0037	0.7407		0.0018	0.9099		✓
$= 365$, $t-2$	0.1924	*0.3942*	0.1331	*0.3397*	0.1893	*0.3918*	0.1503	0	*	0.0078	0.6082		✓
$(0, Q_2]$, $t-3$	0.0565	*0.2308*	0.0683	*0.2523*	0.0565	*0.2309*	-0.0514	0	*	-0.0002	0.9898		✓
$(Q_2, Q_3]$, $t-3$	0.065	*0.2466*	0.0735	*0.261*	0.0646	*0.2458*	-0.0345	0.002	*	0.0018	0.9057		✓
$(Q_3, Q_4]$, $t-3$	0.0867	*0.2815*	0.0911	*0.2877*	0.0852	*0.2791*	-0.0154	0.1682		0.0056	0.7197		✓
$(Q_4, 365)$, $t-3$	0.1211	*0.3263*	0.1139	*0.3177*	0.121	*0.3261*	0.0221	0.0474	*	0.0003	0.9848		✓
$= 365$, $t-3$	0.1186	*0.3233*	0.092	*0.289*	0.1168	*0.3212*	0.0823	0	*	0.0054	0.7269		✓
$(0, Q_2]$, $t-4$	0.0559	*0.2296*	0.0677	*0.2513*	0.0553	*0.2286*	-0.0517	0	*	0.0023	0.8807		✓
$(Q_2, Q_3]$, $t-4$	0.0589	*0.2354*	0.0665	*0.2492*	0.0572	*0.2322*	-0.0324	0.0037	*	0.0072	0.6425		✓
$(Q_3, Q_4]$, $t-4$	0.0774	*0.2673*	0.079	*0.2697*	0.0763	*0.2655*	-0.0058	0.6055		0.0042	0.7866		✓
$(Q_4, 365)$, $t-4$	0.1053	*0.307*	0.0966	*0.2954*	0.1045	*0.3059*	0.0285	0.0107	*	0.0027	0.8609		✓
$= 365$, $t-4$	0.0673	*0.2506*	0.057	*0.2319*	0.0668	*0.2497*	0.0411	0.0002	*	0.0019	0.9038		✓
$(0, Q_2]$, $t-5$	0.0491	*0.2161*	0.0598	*0.2371*	0.0501	*0.2181*	-0.0494	0	*	-0.0044	0.7756		✓
$(Q_2, Q_3]$, $t-5$	0.0538	*0.2256*	0.0585	*0.2348*	0.0543	*0.2266*	-0.021	0.0597		-0.0022	0.8883		✓
$(Q_3, Q_4]$, $t-5$	0.0621	*0.2414*	0.0638	*0.2444*	0.0604	*0.2382*	-0.0071	0.5263		0.0073	0.6361		✓
$(Q_4, 365)$, $t-5$	0.0829	*0.2757*	0.0784	*0.2688*	0.0822	*0.2747*	0.0163	0.1432		0.0024	0.8759		✓
$= 365$, $t-5$	0.0415	*0.1994*	0.0344	*0.1821*	0.0409	*0.198*	0.0358	0.0013	*	0.003	0.8453		✓
Registration-to-treatment and registration-to-coding groups (see Appendix D for an explanation)													
2	0.1293	*0.3356*	0.0864	*0.281*	0.1258	*0.3316*	0.1277	0	*	0.0106	0.4818		✓
3	0.0462	*0.2099*	0.0216	*0.1455*	0.0458	*0.2091*	0.1169	0	*	0.0018	0.9088		✓
4	0.0602	*0.2379*	0.0864	*0.281*	0.0581	*0.2339*	-0.1104	0	*	0.0089	0.557		✓
5	0.0317	*0.1753*	0.0216	*0.1455*	0.0309	*0.173*	0.0575	0	*	0.0048	0.756		✓
6	0.0277	*0.1642*	0.0114	*0.1062*	0.0288	*0.1672*	0.0994	0	*	-0.0063	0.6845		✓
7	0.0475	*0.2128*	0.0864	*0.281*	0.047	*0.2116*	-0.1829	0	*	0.0025	0.8666		✓
8	0.0158	*0.1247*	0.0216	*0.1455*	0.0153	*0.1226*	-0.0469	0	*	0.0042	0.7824		✓
9	0.0198	*0.1393*	0.0114	*0.1062*	0.0193	*0.1374*	0.0601	0	*	0.0038	0.8036		✓
10	0.0203	*0.1409*	0.0072	*0.0845*	0.0212	*0.1442*	0.0928	0	*	-0.007	0.6562		✓
11	0.0341	*0.1816*	0.0864	*0.281*	0.0355	*0.185*	-0.2881	0	*	-0.0075	0.6237		✓
12	0.0121	*0.1092*	0.0216	*0.1455*	0.012	*0.1087*	-0.0878	0	*	0.0009	0.954		✓
13	0.0101	*0.1002*	0.0114	*0.1062*	0.0097	*0.0981*	-0.0128	0.2513		0.0041	0.7873		✓
14	0.0116	*0.107*	0.0072	*0.0845*	0.0113	*0.1055*	0.041	0.0002	*	0.003	0.8467		✓
15	0.0144	*0.119*	0.0049	*0.0699*	0.0147	*0.1205*	0.0794	0	*	-0.0033	0.8338		✓
16	0.0277	*0.1642*	0.0864	*0.281*	0.0298	*0.1701*	-0.3574	0	*	-0.0128	0.3991		✓
17	0.0118	*0.1081*	0.0216	*0.1455*	0.0118	*0.108*	-0.0909	0	*	0.0001	0.9957		✓
18	0.0074	*0.0855*	0.0114	*0.1062*	0.0072	*0.0847*	-0.0475	0	*	0.0016	0.9194		✓
19	0.0064	*0.0797*	0.0072	*0.0845*	0.006	*0.0771*	-0.01	0.3695		0.0052	0.7351		✓
20	0.0081	*0.0895*	0.0049	*0.0699*	0.008	*0.0891*	0.0354	0.0014	*	0.0009	0.9536		✓
21	0.0098	*0.0984*	0.0037	*0.0609*	0.0096	*0.0976*	0.0615	0	*	0.0015	0.9248		✓
22	0.0204	*0.1413*	0.0864	*0.281*	0.023	*0.1499*	-0.4674	0	*	-0.0184	0.2194		✓

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Table 1 – Continued

			Prior to matching		*After matching*		*Prior to matching*			*After matching*			
	T	*st. dev.*	C	*st. dev.*	C	*st. dev.*	St. diff.	p-val.	signif. 5%	St. diff.	p-val.	signif. 5%	in PSM?
23	0.0074	*0.0855*	0.0216	*0.1455*	0.008	*0.0889*	-0.1672	0	*	-0.0071	0.6516		✓
24	0.0036	*0.0601*	0.0114	*0.1062*	0.0039	*0.0627*	-0.1299	0	*	-0.0055	0.7289		✓
25	0.0028	*0.0526*	0.0072	*0.0845*	0.0029	*0.054*	-0.084	0	*	-0.0029	0.8512		✓
26	0.0041	*0.0639*	0.0049	*0.0699*	0.0039	*0.0619*	-0.0127	0.258		0.0039	0.7989		✓
27	0.0053	*0.0727*	0.0037	*0.0609*	0.0053	*0.0723*	0.0219	0.0488	*	0.0008	0.9613		✓
28	0.0065	*0.0805*	0.003	*0.0543*	0.0062	*0.0785*	0.0442	0.0001	*	0.0038	0.8029		✓
29	0.0837	*0.277*	0.0864	*0.281*	0.0813	*0.2732*	-0.0099	0.377		0.0088	0.5621		✓
30	0.0223	*0.1477*	0.0216	*0.1455*	0.0215	*0.1452*	0.0045	0.6854		0.0052	0.7331		✓
31	0.0169	*0.1289*	0.0114	*0.1062*	0.0169	*0.1288*	0.0425	0.0001	*	0	0.9974		✓
32	0.0125	*0.1113*	0.0072	*0.0845*	0.0126	*0.1115*	0.0481	0	*	-0.0004	0.9808		✓
33	0.0121	*0.1092*	0.0049	*0.0699*	0.0131	*0.1136*	0.0655	0	*	-0.0093	0.5581		✓
34	0.0142	*0.1185*	0.0037	*0.0609*	0.0159	*0.1251*	0.0888	0	*	-0.0142	0.3715		✓
35	0.0175	*0.1311*	0.003	*0.0543*	0.0191	*0.1369*	0.1109	0	*	-0.0123	0.4235		✓
36	0.0894	*0.2853*	0.0157	*0.1244*	0.0928	*0.2902*	0.2582	0	*	-0.0121	0.3447		✓
	Subsidized employment rate (SE) and unemployment rate (UE) in municipality at t (%)												
SE	1.7256	*0.856*	1.7562	*0.9132*	1.7276	*0.8777*	-0.0357	0.0014	*	-0.0022	0.8863		✓
UE	4.3206	*1.0328*	4.4051	*1.0555*	4.3231	*1.0426*	-0.0818	0	*	-0.0025	0.8746		✓

Note: All monetary values are expressed in CPI-adjusted 1000s of SEK with base year 2008.

Appendix B: Main estimation results

Table 2: ATT of participation in various LMPs for disabled job seekers on yearly labour income (1000s SEK)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment	Employment at Samhall
-5	1.24	0.51	-3.45	-2.45
	(1.25)	(1.34)	(3.13)	(4.2)
-4	1.73	0.82	-2.33	-2.25
	(1.23)	(1.34)	(3.04)	(4.34)
-3	1.95	1.24	-2.67	0.6
	(1.18)	(1.29)	(2.57)	(4.1)
-2	0.77	1.2	-4.08*	0.39
	(1.05)	(1.16)	(1.93)	(3.83)
-1	0.35	0.49	-3.27	5.28
	(0.82)	(0.91)	(1.86)	(2.87)
0	61.5***	62.64***	57.58***	40.64***
	(0.8)	(0.89)	(2.09)	(2.58)
1	96.47***	96.51***	81.42***	105.33***
	(1)	(1.11)	(3.01)	(3.41)
2	69.45***	69.68***	57.39***	91.62***
	(1.16)	(1.26)	(3.69)	(3.91)
3	57.95***	56.83***	45.9***	78.93***
	(1.26)	(1.38)	(4.15)	(4.1)
4	56.93***	56.04***	49.29***	73.96***
	(1.33)	(1.45)	(4.4)	(4.65)
5	57.07***	55.55***	44.41***	74.85***
	(1.41)	(1.54)	(4.55)	(4.69)
Observations	275,094	273,911	267,474	267,268
Treated obs.	8,290	7,107	670	464

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 3: ATT of participation in various LMPs for disabled job seekers on employment status (1 if employed in November)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment	Employment at Samhall
-5	0.01	0	-0.01	0
	(0.01)	(0.01)	(0.02)	(0.02)
-4	0.01	0	-0.01	0
	(0.01)	(0.01)	(0.02)	(0.02)
-3	0	0	0	0
	(0.01)	(0.01)	(0.02)	(0.02)
-2	0	0	0	0.01
	(0.01)	(0.01)	(0.01)	(0.02)
-1	0.01	0	0	0
	(0.01)	(0.01)	(0.01)	(0.02)
0	0.47***	0.47***	0.47***	0.36***
	(0.01)	(0.01)	(0.02)	(0.02)
1	0.48***	0.48***	0.45***	0.54***
	(0.01)	(0.01)	(0.02)	(0.02)
2	0.34***	0.33***	0.31***	0.49***
	(0.01)	(0.01)	(0.02)	(0.02)
3	0.28***	0.27***	0.24***	0.42***
	(0.01)	(0.01)	(0.02)	(0.02)
4	0.29***	0.27***	0.27***	0.39***
	(0.01)	(0.01)	(0.02)	(0.02)
5	0.26***	0.26***	0.21***	0.4***
	(0.01)	(0.01)	(0.02)	(0.02)
Observations	275,094	273,911	267,474	267,268
Treated obs.	8,290	7,107	670	464

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 4: ATT of participation in various LMPs for disabled job seekers on un-subsidized employment (1 if employed in November and no participation in any LMPs involving wage subsidies)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment
-5	0.01	0	0
	(0.01)	(0.01)	(0.02)
-4	0.01	0	-0.01
	(0.01)	(0.01)	(0.02)
-3	0.01	0	0
	(0.01)	(0.01)	(0.02)
-2	0	0	-0.01
	(0.01)	(0.01)	(0.01)
-1	0	0	0
	(0.01)	(0.01)	(0.01)
0	-0.18***	-0.22***	-0.08***
	(0)	(0)	(0.01)
1	-0.17***	-0.22***	-0.11***
	(0)	(0)	(0.01)
2	-0.13***	-0.18***	-0.1***
	(0)	(0)	(0.01)
3	-0.13***	-0.17***	-0.08***
	(0.01)	(0.01)	(0.02)
4	-0.1***	-0.14***	-0.05**
	(0.01)	(0.01)	(0.02)
5	-0.07***	-0.1***	-0.06**
	(0.01)	(0.01)	(0.02)
Observations	275,094	273,911	267,474
Treated obs.	8,290	7,107	670

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 5: ATT of participation in various LMPs for disabled job seekers on yearly disposable income (1000s SEK)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment	Employment at Samhall
-5	0.83	0.67	-0.69	-1.47
	(0.91)	(1)	(2.84)	(3.13)
-4	-0.09	-0.22	-0.2	-4.26
	(1.11)	(1.22)	(2.7)	(2.78)
-3	0.34	0.16	-0.3	-1.02
	(1.14)	(1.27)	(2.57)	(2.69)
-2	-0.17	-0.32	-3.56	2.62
	(0.96)	(1.05)	(2.63)	(2.89)
-1	-0.01	0.01	-1.94	1.06
	(0.75)	(0.81)	(2.27)	(2.34)
0	12.25***	12.86***	10.44***	2.45
	(0.61)	(0.66)	(1.93)	(1.87)
1	19.67***	19.86***	15.91***	18.02***
	(0.74)	(0.84)	(1.95)	(1.81)
2	17.05***	17.63***	10.45***	14.93***
	(1.03)	(1.15)	(2.07)	(2.09)
3	21.21***	22.27***	13.24***	20.04***
	(1.06)	(1.16)	(2.62)	(2.85)
4	22.12***	21.79***	15.4***	21.25***
	(0.97)	(1.08)	(2.85)	(2.59)
5	22.72***	22.6***	18.85***	21.33***
	(1.26)	(1.39)	(3.53)	(2.68)
Observations	275,094	273,911	267,474	267,268
Treated obs.	8,290	7,107	670	464

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 6: ATT of participation in various LMPs for disabled job seekers on disability insurance (1000s SEK/year)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment	Employment at Samhall
-5	0.08	0.07	0.05	0.23
	(0.2)	(0.22)	(0.65)	(0.69)
-4	0.09	0.05	0.25	0.16
	(0.23)	(0.25)	(0.67)	(0.9)
-3	0.05	0.04	0.17	0.09
	(0.25)	(0.27)	(0.72)	(0.87)
-2	0.01	0.07	0.22	0.42
	(0.29)	(0.32)	(0.77)	(1.21)
-1	0.1	0.16	-0.01	0.52
	(0.37)	(0.41)	(1.03)	(1.23)
0	0.13	0.29	-0.38	-0.89
	(0.37)	(0.41)	(1.02)	(1.3)
1	-1.78***	-1.03*	-4.88***	-6.8***
	(0.44)	(0.49)	(1.15)	(1.41)
2	-5.44***	-5.15***	-9.07***	-12.33***
	(0.5)	(0.57)	(1.38)	(1.72)
3	-8.92***	-8.42***	-10.61***	-14.29***
	(0.54)	(0.6)	(1.47)	(1.87)
4	-10.98***	-10.68***	-10.73***	-16.1***
	(0.58)	(0.64)	(1.82)	(2.08)
5	-11.34***	-10.99***	-10.47***	-17.43***
	(0.63)	(0.7)	(1.91)	(2.18)
Observations	275,094	273,911	267,474	267,268
Treated obs.	8,290	7,107	670	464

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 7: ATT of participation in various LMPs for disabled job seekers on DI-prevalence (1 if yearly disability insurance > 0)

Years since treatment	All LMPs	Wage subsidy	Sheltered public employment	Employment at Samhall
-5	0	0	0	0.01
	(0)	(0)	(0.01)	(0.01)
-4	0	0	0.01	0.01
	(0)	(0)	(0.01)	(0.01)
-3	0	0	0	0.01
	(0)	(0)	(0.01)	(0.01)
-2	0	0	0	0.01
	(0)	(0)	(0.01)	(0.01)
-1	0.01	0.01	0	0.01
	(0)	(0)	(0.01)	(0.01)
0	0.05***	0.06***	0.02	0
	(0)	(0.01)	(0.01)	(0.02)
1	0.05***	0.06***	-0.01	-0.05**
	(0.01)	(0.01)	(0.01)	(0.02)
2	0.02***	0.03***	-0.04*	-0.1***
	(0.01)	(0.01)	(0.02)	(0.02)
3	0	0.01	-0.07***	-0.12***
	(0.01)	(0.01)	(0.02)	(0.02)
4	-0.02***	-0.01	-0.07***	-0.13***
	(0.01)	(0.01)	(0.02)	(0.02)
5	-0.02***	-0.01	-0.08***	-0.14***
	(0.01)	(0.01)	(0.02)	(0.02)
Observations	275,094	273,911	267,474	267,268
Treated obs.	8,290	7,107	670	464

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Appendix C: Separate estimates for males and females

Table 8: ATT of participation in various LMPs for disabled job seekers on yearly labour income (1000s SEK)

	Males				*Females*			
Years since treatment	All LMPs	Wage subsidy	Sheltered employment	Samhall	All LMPs	Wage subsidy	Sheltered employment	Samhall
-5	1.26	0.85	0.11	-5.88	-0.57	-1.63	-6.22	0.28
	(1.76)	(1.9)	(4.34)	(6.98)	(1.64)	(1.81)	(4.55)	(4.77)
-4	0.61	-0.09	-0.18	-5.34	1.29	-0.23	-5.91	3.5
	(1.73)	(1.9)	(3.98)	(6.86)	(1.61)	(1.78)	(5)	(4.99)
-3	1.79	1.18	-0.52	0.3	-0.77	-1.34	-6.17	3.33
	(1.67)	(1.85)	(3.54)	(6.46)	(1.51)	(1.65)	(4)	(4.81)
-2	0.21	0.58	-4.13	-0.6	-0.53	-0.64	-6.45	-2.08
	(1.49)	(1.67)	(2.5)	(6.07)	(1.3)	(1.46)	(3.45)	(4.23)
-1	0.35	-0.14	-2.26	8.2	-0.77	-0.79	-7.86***	0.98
	(1.18)	(1.32)	(1.72)	(4.47)	(1.01)	(1.12)	(2.35)	(3.7)
0	67.17***	68.8***	59.21***	42.83***	53.04***	52.99***	51.18***	39.2***
	(1.12)	(1.23)	(2.83)	(3.53)	(1.07)	(1.19)	(3.42)	(3.85)
1	104.69***	105.58***	82.46***	107.69***	84.01***	83.24***	79.15***	100.67***
	(1.38)	(1.53)	(4.15)	(5.22)	(1.34)	(1.45)	(5.21)	(4.95)
2	77.22***	77.67***	59.16***	96.76***	57.7***	55.59***	45.41***	87.28***
	(1.62)	(1.78)	(4.89)	(5.9)	(1.57)	(1.7)	(7.82)	(5.95)
3	64.54***	65.41***	51.2***	82.05***	46.27***	43.3***	30.03*	83.72***
	(1.79)	(1.97)	(5.54)	(6.67)	(1.69)	(1.83)	(13.14)	(5.64)
4	64.74***	65.13***	52.44***	77.95***	43.94***	42.72***	29.43**	77.12***
	(1.86)	(2.04)	(5.8)	(7.24)	(1.78)	(1.92)	(10.19)	(6.28)
5	64.3***	64.76***	48.98***	77.18***	44***	40.6***	10.85	77.88***
	(1.97)	(2.17)	(5.97)	(7.33)	(1.89)	(2.11)	(14.99)	(6.64)
♯ obs.	131,582	130,875	127,045	126,852	143,512	143,036	140,429	140,416
♯ treated	4,968	4,261	431	238	3,322	2,846	239	226

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 9: ATT of participation in various LMPs for disabled job seekers on employment status (1 if employed in November)

	Males				*Females*			
Years since treatment	All LMPs	Wage subsidy	Sheltered employment	Samhall	All LMPs	Wage subsidy	Sheltered employment	Samhall
-5	0	0	0.01	-0.01	0.01	0	-0.03	0.01
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
-4	0	0	0.01	0	0	0	-0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.04)	(0.01)	(0.01)	(0.04)	(0.03)
-3	0	0	0.01	0	0	0	-0.01	0
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
-2	0.01	0	0	-0.01	0	0	-0.02	0
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
-1	0	0	0	0	0	0	-0.03	-0.01
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
0	0.47***	0.47***	0.47***	0.37***	0.46***	0.47***	0.46***	0.36***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.04)
1	0.47***	0.46***	0.42***	0.55***	0.5***	0.5***	0.52***	0.55***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
2	0.34***	0.33***	0.31***	0.48***	0.34***	0.34***	0.28***	0.51***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
3	0.28***	0.28***	0.25***	0.43***	0.29***	0.27***	0.24***	0.45***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
4	0.29***	0.28***	0.26***	0.39***	0.27***	0.27***	0.21***	0.4***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
5	0.27***	0.26***	0.23***	0.41***	0.25***	0.24***	0.14**	0.42***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
♯ obs.	131,582	130,875	127,045	126,852	143,512	143,036	140,429	140,416
♯ treated	4,968	4,261	431	238	3,322	2,846	239	226

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 10: ATT of participation in various LMPs for disabled job seekers on un-subsidized employment (1 if employed in November and no participation in any LMPs involving wage subsidies)

	Males			*Females*		
Years since	All	Wage	Sheltered	All	Wage	Sheltered
treatment	LMPs	subsidy	employment	LMPs	subsidy	employment
-5	0	0	0.01	0.01	0	-0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)
-4	0	0	0	0.01	0	-0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)
-3	0	0	0	0	0	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)
-2	0.01	0	0	0	0	-0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
-1	0	0	0	0	0	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
0	-0.19***	-0.23***	-0.07***	-0.16***	-0.21***	-0.1***
	(0)	(0)	(0.01)	(0)	(0)	(0.01)
1	-0.17***	-0.23***	-0.1***	-0.16***	-0.22***	-0.1***
	(0)	(0)	(0.01)	(0.01)	(0)	(0.01)
2	-0.14***	-0.18***	-0.09***	-0.13***	-0.18***	-0.14***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)
3	-0.15***	-0.18***	-0.12***	-0.12***	-0.16***	-0.06
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)
4	-0.11***	-0.14***	-0.07**	-0.1***	-0.15***	-0.05
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)
5	-0.07***	-0.1***	-0.07**	-0.06***	-0.11***	-0.07
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)
ǂ obs.	131,582	130,875	127,045	143,512	143,036	140,429
ǂ treated	4,968	4,261	431	3,322	2,846	239

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 11: ATT of participation in various LMPs for disabled job seekers on disposable income (1000s SEK)

	Males				*Females*			
Years since treatment	All LMPs	Wage subsidy	Sheltered employment	Samhall	All LMPs	Wage subsidy	Sheltered employment	Samhall
-5	1.24	1.4	-0.86	0.08	-1.19	-1.99	3.89	-2.05
	(1.34)	(1.45)	(3.35)	(4.93)	(1.12)	(1.2)	(5.51)	(4.06)
-4	-0.76	-0.45	-1.98	-6.7	-0.27	-0.72	-2.98	-0.3
	(1.51)	(1.7)	(3.2)	(3.89)	(1.48)	(1.67)	(6.18)	(3.94)
-3	1.27	1.35	-0.46	-3.26	-2.53*	-2.91*	-1.57	1.74
	(1.76)	(2)	(3.04)	(3.76)	(1.09)	(1.15)	(4.95)	(3.94)
-2	-0.17	-0.65	-2.7	1.59	-1.63	-1.28	-4.29	1.58
	(1.39)	(1.53)	(3.08)	(4.95)	(1.18)	(1.29)	(4.73)	(3.74)
-1	0.01	-0.44	-1.39	0.34	-0.35	-0.14	-3.09	0.8
	(0.98)	(1.06)	(2.65)	(3.43)	(1.16)	(1.28)	(3.95)	(3.6)
0	14.26***	15.02***	10.81***	-0.87	8.77***	8.56***	5.34	4.52
	(0.84)	(0.91)	(2.28)	(2.7)	(0.87)	(0.98)	(3.62)	(3)
1	22.61***	23.19***	17.13***	16.51***	13.8***	14.52***	15.33***	19.54***
	(1.06)	(1.19)	(2.42)	(2.55)	(0.97)	(1.08)	(3.95)	(3.06)
2	19.4***	19.24***	12.12***	17.96***	13***	12.87***	3.99	14.87***
	(1.48)	(1.76)	(2.84)	(3.49)	(1.28)	(1.44)	(5.48)	(3.25)
3	21.71***	23.71***	11.86**	19.86***	17.2***	17.97***	5.3	23.23***
	(1.49)	(1.63)	(3.64)	(4.75)	(1.65)	(1.86)	(8.79)	(3.36)
4	24.18***	25.03***	15.74***	18.51***	18.27***	18.08***	8.52	24.06***
	(1.36)	(1.45)	(3.96)	(4.99)	(1.37)	(1.51)	(5.99)	(4.53)
5	24.86***	23.21***	18.73***	17.14***	18.59***	18.12***	3	18.18**
	(1.52)	(1.77)	(4.27)	(4.92)	(2.13)	(2.48)	(8.77)	(6.08)
♯ obs.	131,582	130,875	127,045	126,852	143,512	143,036	140,429	140,416
♯ treated	4,968	4,261	431	238	3,322	2,846	239	226

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 12: ATT of participation in various LMPs for disabled job seekers on disability insurance (1000s SEK/year)

	Males				*Females*			
Years since treatment	All LMPs	Wage subsidy	Sheltered employment	Samhall	All LMPs	Wage subsidy	Sheltered employment	Samhall
-5	-0.01	-0.05	0.25	-0.15	0.08	-0.13	0.49	-0.94
	(0.26)	(0.29)	(0.81)	(0.8)	(0.32)	(0.37)	(1.05)	(1.32)
-4	0.02	-0.07	0.15	0.24	0.03	-0.14	0.79	-0.61
	(0.29)	(0.33)	(0.81)	(1.05)	(0.37)	(0.41)	(1.24)	(1.59)
-3	0.05	-0.05	-0.32	0.15	0.06	-0.06	1.12	-0.67
	(0.31)	(0.34)	(0.92)	(1.01)	(0.42)	(0.47)	(1.3)	(1.56)
-2	0.08	-0.03	-0.08	0.56	0.08	-0.06	0.8	0.01
	(0.36)	(0.39)	(0.88)	(1.91)	(0.49)	(0.55)	(1.54)	(1.58)
-1	0.12	0.01	-0.07	0.16	0.16	0.02	-0.2	-0.42
	(0.44)	(0.49)	(1.04)	(1.58)	(0.65)	(0.73)	(2.29)	(2.13)
0	-0.15	-0.13	-0.09	-0.57	0.7	0.68	-1.27	-1.8
	(0.45)	(0.51)	(1.1)	(1.6)	(0.63)	(0.7)	(2.52)	(2.27)
1	-2.55***	-1.45*	-5.2***	-6.64***	0.17	1.3	-5.61*	-4.71*
	(0.58)	(0.64)	(1.18)	(1.9)	(0.7)	(0.77)	(2.55)	(2.2)
2	-6.99***	-5.9***	-13.1***	-13.68***	-3.66***	-2.52**	-6.35*	-11.45***
	(0.68)	(0.74)	(1.66)	(2.55)	(0.78)	(0.85)	(2.84)	(2.78)
3	-9.95***	-8.89***	-16.75***	-13.2***	-7.65***	-6.31***	-4.48	-17.03***
	(0.74)	(0.83)	(2.11)	(2.83)	(0.81)	(0.91)	(3.16)	(2.87)
4	-12.09***	-11.22***	-16.62***	-16.55***	-9.3***	-8.63***	-4.95	-18.49***
	(0.78)	(0.87)	(2.22)	(3.06)	(0.88)	(0.94)	(3.8)	(2.97)
5	-12.54***	-13.32***	-15.65***	-19.79***	-9.02***	-7.56***	0.82	-20.08***
	(0.86)	(1)	(2.16)	(3.68)	(0.95)	(1.04)	(3.72)	(3.27)
♯ obs.	131,582	130,875	127,045	126,852	143,512	143,036	140,429	140,416
♯ treated	4,968	4,261	431	238	3,322	2,846	239	226

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Table 13: ATT of participation in various LMPs for disabled job seekers on DI-prevalence (1 if yearly disability insurance > 0)

	Males				*Females*			
Years since treatment	All LMPs	Wage subsidy	Sheltered employment	Samhall	All LMPs	Wage subsidy	Sheltered employment	Samhall
-5	0	0	0	0	0	0	0.01	-0.01
	(0)	(0)	(0.01)	(0.01)	(0)	(0.01)	(0.01)	(0.02)
-4	0	0	0	0.01	0	0	0.01	-0.01
	(0)	(0)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
-3	0	0	0	0.01	0	0	0.01	-0.01
	(0)	(0)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
-2	0	0	0	0	0	0	0	0
	(0)	(0)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
-1	0	0	-0.01	0.01	0.01	0.01	0	-0.02
	(0)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.03)
0	0.03***	0.03***	0.01	0.02	0.07***	0.08***	0.03	-0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)
1	0.02*	0.03***	-0.03*	-0.05	0.09***	0.12***	-0.02	-0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)
2	-0.01	0	-0.1***	-0.12***	0.07***	0.09***	0	-0.11***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
3	-0.03***	-0.02**	-0.14***	-0.12***	0.04***	0.07***	0.02	-0.14***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
4	-0.05***	-0.03***	-0.13***	-0.14***	0.02*	0.05***	-0.01	-0.15***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
5	-0.05***	-0.04***	-0.14***	-0.14***	0.02*	0.05***	0.02	-0.16***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
♯ obs.	131,582	130,875	127,045	126,852	143,512	143,036	140,429	140,416
♯ treated	4,968	4,261	431	238	3,322	2,846	239	226

$^{*}\ p<0.05,\ ^{**}\ p<0.01,\ ^{***}\ p<0.001$

Note: The ATT is estimated using propensity-score matching and a logit specification for the propensity score model. Standard errors from Abadie and Imbens (2006) are within parentheses. The full list of matching covariates is given in Table 1.

Appendix D: Various groupings used in the analysis

Table 14: The hospital inpatient categories based on the ICD-10 chapters

Chapter	Block	Title
I	A00–B99	Certain infectious and parasitic diseases
II	C00–D48	Neoplasms
III	D50–D89	Diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism
IV	E00–E90	Endocrine, nutritional and metabolic diseases
V	F00–F99	Mental and behavioural disorders
VI	G00–G99	Diseases of the nervous system
VII	H00–H59	Diseases of the eye and adnexa
VIII	H60–H95	Diseases of the ear and mastoid process
IX	I00–I99	Diseases of the circulatory system
X	J00–J99	Diseases of the respiratory system
XI	K00–K93	Diseases of the digestive system
XII	L00–L99	Diseases of the skin and subcutaneous tissue
XIII	M00–M99	Diseases of the musculoskeletal system and connective tissue
XIV	N00–N99	Diseases of the genitourinary system
XV	O00–O99	Pregnancy, childbirth and the puerperium
XVI	P00–P96	Certain conditions originating in the perinatal period
XVII	Q00–Q99	Congenital malformations, deformations and chromosomal abnormalities
XVIII	R00–R99	Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified
XIX	S00–T98	Injury, poisoning and certain other consequences of external causes
XXI	Z00–Z99	Factors influencing health status and contact with health services
99		Not ICD-10 (incorrectly coded)

Table 15: The PES' occupational disability codes

Code	Description
11	Cardio, vascular, and/or lung disease
20–22	Hearing impairment and deafness
30–32	Visual impairment
40–42	Motor handicap
51	Other somatically related disabilities
61	Mental disability
71	Learning disability
81	Socio-medical disability
91	Asthma, allergy, and hypersensitivity
92	Dyslexia and specific learning difficulties
93	Acquired brain injury
YD	Occupational disability code for young disabled

Registration-to-treatment and registration-to-coding groups

We create these groups in order to be able to match on duration in unemployment (see Sianesi, 2004), as well as on duration until receiving a disability code. The groups shown at the end of Table 1 are defined using the following procedure:

1. Generate a variable (dur_T) denoting time in quarters from PES registration date to treatment start. Censored at 8 quarters.
2. Generate a variable (dur_D) denoting time in quarters from PES registration date until being classified as disabled. Censored at 8 quarters.
3. Construct 36 study groups, one for each combination of dur_T and dur_D. That is 1-1; 2-2,2-1; 3-3,3-2,3-1;...; 8-8,8-7,...8-1. Note that $dur_T \geq dur_D$ by the eligibility criteria.

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