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ABSTRACT

Subsidizing Domestic Services as a Tool to Fight Unemployment: Effectiveness and Hidden Costs*

European countries have increasingly adopted wage subsidies for the sector of domestic services to reduce low-skilled unemployment. Yet, empirical evidence on their effectiveness is scarce. In this paper, we use Belgian administrative data to estimate how participation in the subsidized domestic services sector impacts the labour market outcomes of program participants. Our identification strategy rests on a dynamic event study difference-in-differences model combined with coarsened exact matching. Our findings indicate that such subsidies can be effective in reducing unemployment and inactivity, but only by increasing employment within the subsidized domestic services sector. We also find that program participation deteriorates physical health, thus increasing the worker's probability of claiming disability insurance benefits.

JEL Classification: J08, J24, J28, J38

Keywords: wage subsidies, low-skilled workers, unemployment, disability, domestic services, personal and household services, female employment

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1 Introduction

In recent years, policymakers in the United States and across Europe have increasingly adopted active labour market policies for jobseekers and welfare recipients to curb unemployment (Card et al., 2018; Kluve, 2010; Sianesi, 2008). One of these policies that has received growing attention lately relates to the subsidization of so-called Personal and Household Services[1] (PHS). Such subsidies are intended to: (i) create a large number of relatively low-skilled jobs, (ii) reduce undeclared work in domestic services and (iii) help working families achieve a better work-life balance (Manoudi et al., 2018). In this paper, we focus on the first of these goals and assess the effectiveness of PHS subsidies as a tool to fight unemployment.

Intuitively, PHS subsidies aim at tackling the growing incidence of unemployment among workers with lower levels of (perceived) productivity by fostering the creation of low-skilled jobs in a sector that is labour-intensive and not outsourceable. This is done by decreasing the cost of labour for the employer (e.g., by offering direct wage subsidies, exemptions from social contributions or more flexible work regulations) and/or decreasing the cost of the service for the consumer (e.g., through tax credits). Unless demand for PHS is perfectly inelastic, such subsidies should increase subsidized employment in the PHS sector through either the creation of new activities or the formalisation of pre-existing undeclared activities.

Another mechanism through which PHS subsidies could tackle unemployment is by moving jobseekers into subsidized jobs that serve as steppingstones towards non-subsidized activities (Manoudi et al., 2018). Program participants might indeed be more likely to enter non-subsidized employment after their subsidized working experience if program participation allows them to increase their (perceived) productivity and/or to acquire new skills (Bördos et al., 2015; Heckman et al., 2002). Working as a subsidized domestic worker could, for example, increase the individual's perceived productivity with employers if it signals a higher capacity to work as compared to not possessing a (declared) job for many years (Eriksson and Rooth, 2014; Kroft et al., 2013). It could also help the subsidized workers to acquire new skills and increase their productivity through professional experience and on-the-job training

[1] According to the European Commission's definition, "Personal and household services (PHS) cover jobs and services carried out to support households. 63% are care activities [including] childcare, assistance to the elderly, dependent or disabled, excluding healthcare, and 37% are non-care activities [including] cleaning, laundry, meal preparation, gardening, small house repairs and private lessons." This paper focuses on PHS subsidies for non-care activities.

(Konings and Vanormelingen, 2015).[2] However, targeted wage subsidies have also been shown to have stigmatizing effects that can decrease the probability of program participants of finding a job (Burtless, 1985; Baert, 2016; Liechti et al., 2017). Given the social stigma associated with domestic work (Ashforth and Kreiner, 1999), participating in the subsidized PHS sector might therefore lower the probability that participants find a non-subsidized job. Moreover, PHS subsidies tend to be permanent – they are offered as long as the individual is employed in the subsidized sector – and subsidy duration has been shown to negatively influence job-finding rates (Caliendo et al., 2008; van Ours, 2004). All things considered, it is thus unclear whether PHS subsidies might improve the employment prospects of program participants in non-subsidized activities as well as in the subsidized PHS sector.

Furthermore, PHS subsidies might affect the health of program participants. Domestic work is indeed physically demanding and solitary, is often associated with little social gratification, and is characterized by frequent contacts with chemicals, all of which could negatively affect the domestic helpers' physical and/or psychological well-being[3] (Goffin et al., 2018; Brun, 2009). Therefore, encouraging employment in the PHS sector might deteriorate the health of program participants. If this were the case, PHS subsidies would entail indirect costs – both for the workers and the Government – that would need to be included in their evaluation as a policy tool.

In this context, our paper aims at evaluating the effects of PHS subsidies on the labour market outcomes of program participants. This is a relevant research question given the growing number of countries that have introduced measures specifically designed to increase employment in domestic services.[4] One country that has vigorously supported its PHS sector is Belgium, through its implementation of the Service Voucher Scheme in 2004. Indeed, by 2013, over 150,000 workers were employed under the scheme and its total budgetary cost amounted to nearly €2 billion (approximately 0.4 % of GDP), making it the most important active labour market policy in the country. Exploring the case of Belgium therefore offers a unique opportunity to evaluate the potential of PHS subsidies as a policy tool to fight unemployment among low-skilled workers.

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[2] For example, for foreign-born workers who do not speak the national language, working as a domestic helper could help them to learn the language through contacts with clients, thus enhancing their human capital.

[3] High disability rates are indeed common in the cleaning sector, even for non-subsidized cleaning activities (Woods and Buckle, 2006). For example, in Belgium, the long-term disability rate in the non-subsidized cleaning sector was above 10% in 2015.

[4] Countries that have implemented policies to support PHS include Austria, Belgium, Denmark, Finland, France, Germany, Italy, Sweden, the Netherlands and the UK.

Specifically, using quarterly administrative data for the years 2003-2015, we evaluate the effect of working as a subsidized domestic helper on a wide variety of labour market outcomes, including employment, unemployment, inactivity and short- and long-term disability. Identifying causal effects is challenging because participation in the scheme is correlated with observable and unobservable factors that may simultaneously affect future labour market outcomes. We tackle this issue in two ways. First, we focus our analysis on women[5] who participate in the program and exploit the timing of their entry as a means to construct a treatment and control group. Concretely, those who enter the scheme in its first years are part of the treatment group while those who enter it in its latter years constitute the control group. This first step allows us to control for constant unobserved factors affecting participation in the program. Second, we build on the empirical strategy developed by Iacus et al. (2012) and use coarsened exact matching to select individuals in the treatment and control groups who are comparable in terms of observable characteristics. These two preliminary steps ensure that we perform our analysis on a homogeneous group (both in terms of observable and unobservable characteristics), allowing us to estimate a dynamic event study difference-in-differences model (Rellstab et al., 2020; Jeon and Pohl, 2017; Jacobson et al., 1993). In short, we employ a quasi-experimental research design that constructs counterfactuals for subsidized PHS workers by using similar individuals who entered the program a few years in the future. Identification rests on a parallel trend assumption, which we substantiate through a series of validity checks.

Our main findings show that, one year after entering the program, participants are 22.6 percentage points (pp) more likely to be formally employed (which corresponds to a 56% effect). The effect decreases slightly over the years and reaches a size of 16.8 pp (42%) five years after program entry. This effect is only driven by an increase in employment probability within the subsidized PHS sector (38.0 pp increase after 5 years), suggesting that the program does not encourage a transition towards a non-subsidized job. Moreover, our results indicate the existence of a lock-in effect whereby, once the individual starts to work as a subsidized domestic helper, her probability of being employed in a job outside of the scheme decreases both in the short- and the long-run (21.2 pp decrease after five years). Our results thus indicate that subsidized PHS jobs should not be considered as temporary jobs that lead to non-subsidized

[5] We restrict our analysis to women because, given the activities targeted by Belgian PHS subsidies (mainly cleaning and ironing), it is essentially a wage subsidy targeted at low-skilled women (who compose 98% of subsidized workers).

employment, but rather as long-term employment opportunities for workers with few job prospects on the non-subsidized labour market.

Next, we find that the increased employment probability in the subsidized PHS sector is accompanied by a lasting reduction in the unemployment and inactivity probabilities of program participants. We find that the probability of program participants of being unemployed or inactive is respectively reduced by 12.9 and 8.7 pp (47% and 46%) one year after entry. After five years, individuals who participated in the program are still respectively 10.8 pp (39%) and 10.0 pp (53%) less likely to be unemployed or inactive. Such findings are encouraging, not only with regard to the scheme's objective of reducing unemployment among the low-skilled, but also with regard to the goal of increasing female labour market participation and reducing undeclared employment in domestic services.

Finally, we find that participating in the subsidized PHS sector entails important health costs for the workers, with sharply increased odds of claiming short- or long-term Disability Insurance (DI) benefits.[6] Participation indeed nearly triples the worker's probability of claiming short-term DI benefits one year after entering the scheme (3.7 pp increase). This effect then decreases over time to reach a size of 2.2 pp (105% increase) five years after entry as some short-term disabilities become long-term disabilities. At the same time, the effect on the probability of claiming long-term DI benefits gradually increases to reach a size of 4.0 pp[7] five years after program entry. In our heterogeneity analysis, we show that this effect on disability is driven by an adverse effect on the health of program participants, and not by the novelty of social security eligibility for some previously ineligible workers. We also differentiate the effects on disability according to the type of illness and find that the effect is mainly driven by a strong increase in the risk of suffering from osteoarticular illnesses.[8] These findings indicate the existence of spillover effects of PHS subsidies on disability which raise concerns for two reasons. First, adverse effects on the

[6] In Belgium, any employed or unemployed worker satisfying some minimum amount of seniority and prior earnings is insured against health shocks that affect her ability to work through the payment of disability benefits whose amount are a fraction of the last monthly earnings. During the first 14 days of sickness absence, blue-collar workers remain fully paid by their employer. This first stage is called the period of "guaranteed salary". After the period of guaranteed salary, they become supported by their health insurance fund (the mutuality) and are considered short-term disabled. If the illness lasts more than a year, the individuals enter the long-term disability program and are considered long-term disabled.

[7] This corresponds to a 20-fold proportional increase. This large proportional effect stems from the division by a constant that is very close to zero due to the very small number of individuals who were on long-term disability in the period preceding their program entry.

[8] According to the WHO, musculoskeletal conditions are the leading contributor to disability worldwide. They are also commonly linked with depression and increase the risk of developing other health conditions.

health of program participants imply that PHS subsidies might contribute to the further labour market exclusion of an already vulnerable target group.[9] Second, from a public finance perspective, these spillover effects lead to indirect costs of PHS subsidies in the form of additional DI benefits payments.

These findings contribute to the literature in three ways. First, we complement the literature on active labour market policies by evaluating a broadly targeted, particularly generous and permanent wage subsidy that is aimed at low-skilled female workers. Existing research on active labour market policies usually examines wage subsidies that are conditioned on earnings, age and/or labour market status of program participants (e.g. Albanese and Cockx, 2019; Sjögren and Vikström, 2015; Huttunen et al., 2013; Jaenichen and Stephan, 2011; Blundell et al., 2004). In this paper, we study a wage subsidy that is free of any eligibility criteria, whereby anybody wishing to participate in the subsidized PHS sector can benefit from it. Moreover, although existing research has shown that women tend to be particularly responsive to wage subsidies in general (Card et al., 2018), ours is, to our knowledge, the first paper studying a wage subsidy that is *de facto* aimed at female workers. Finally, despite the vast literature on wage subsidies and their effects on employment (Card et al., 2018), very few studies have focused on employer-side subsidies targeting low-wage or low-skilled workers. One exception is Huttunen et al. (2013), which we distinguish ourselves from in two ways. First, the subsidy studied in our paper does not impose any eligibility criteria (although it does implicitly target low-skilled female workers), while Huttunen et al. (2013) study a subsidy for which only older, full-time, low-wage workers are eligible. Second, our research differs from theirs in terms of the duration and size of the subsidy that is studied. Indeed, the tax cut examined in Huttunen et al. (2013) amounts to 16% of gross monthly earnings maximum and is temporary, while the wage subsidy studied in this paper is permanent and amounts to a minimum of 70% of the labour costs. We therefore provide new evidence on the effects of wage subsidies aimed specifically at low-skilled workers and discuss how design characteristics such as targeting, duration and size can impact their effectiveness.

Second, we contribute to the existing literature on domestic services (in the area of labour economics more specifically) by evaluating the potential of PHS subsidies as a means to tackle unemployment among low-skilled (female) workers. Until now, this strand of the literature has mainly focused on (i)

[9] In 2018, low-skilled workers (with a below upper secondary education) had an unemployment rate of 9.8% on average in the OECD, more than double the unemployment rate among high skilled workers (with a tertiary education) (OECD, 2020).

job-quality in domestic services (e.g. Mousaid et al., 2017; Bailly et al., 2013; Defourny et al., 2010) and (ii) the effect of price changes in domestic services (including child- or elderly care) on the employment outcomes of high-skilled women (e.g. Cortes and Pan, 2013, 2019; Peri et al., 2015; Lefebvre and Merrigan, 2008). Few have, however, focused on the effect of subsidizing such activities as a means to reduce unemployment among low-skilled female workers. To our knowledge, three papers study the domestic services sector from the standpoint of employment of low-skilled workers and are therefore related to our research question. Brück et al. (2006) use an ordered probit model to estimate demand price-elasticity for domestic services and infer corresponding effects in terms of job creation. Carbonnier (2015) also considers the question of PHS subsidies from the perspective of demand price-elasticity and uses a difference-in-differences estimator to establish the causal effect of PHS subsidies on demand. Both these papers argue for a positive effect of wage subsidies on demand for domestic services – and thus on employment in the PHS sector – but neither paper considers a counterfactual for what would have happened to the subsidized workers in the absence of the subsidy. Lastly, Raz-Yurovich and Marx (2018) use a difference-in-differences estimator with macro-level data on employment rates of women and find that the introduction of PHS subsidies has both short-term and long-term positive effects on the employment of low-skilled women. Although their study represents the first attempt to estimate the effect of PHS subsidies on low-skilled workers, the authors acknowledge the need to use micro-level data to identify the program's effects more precisely. We heed their acknowledgement and use micro-level data to evaluate the effect of PHS subsidies on employment – distinguishing between subsidized and non-subsidized work – and further examine how these effects translate in terms of unemployment and inactivity for program participants.

Finally, we contribute to the literature on spillovers between social security programs by studying the effect of PHS subsidies, not only on employment outcomes of program participants, but also on disability. A growing body of literature has shown that reforms affecting one area of social security can have spillover effects on other branches of social security (e.g., Petrongolo, 2009; Lammers et al., 2013; Staubli and Zweimüller, 2013; De Brouwer et al., 2019). However, although the effects of wage subsidies on employment outcomes have been widely studied (Card et al., 2010, 2018; Kluve, 2010), their effects on the probability that program participants enter disability programs are often disregarded. In this paper, we provide the first evidence of spillover effects that wage subsidies can have on the probability of claiming DI benefits. Moreover, we investigate whether these spillovers occur through an effect on health or through the process of becoming eligible for social security after a sufficiently long (subsidized) working experience. Through this contribution, we highlight the importance of adopting a broader perspective – that includes potential health and other spillover effects, on top of usual employment effects – in evaluations of active labour market programs.

2 Institutional Setting: The Belgian Service Voucher Scheme (SVS)

Belgium implemented the Service Voucher Scheme (SVS) in 2004. In a nutshell, this program subsidizes the purchase of vouchers that households can use to pay for certain domestic services.[10] Subsidized services are restricted to cleaning, ironing, washing, small mending jobs, food preparation, transport of people with reduced mobility and doing groceries. Any other activity, such as childcare or elderly care, is forbidden.

The scheme is organized as follows (see Figure 1).[11] The voucher issuer is a private firm whose role is essentially to produce the vouchers, sell them to the users, and then collect them from the licensed SVS firms in exchange for payment. It receives 13.04€/voucher from the Government and 9€/voucher from the user, and gives the total value of 22.04€ to the licensed firms for each voucher they send back.[12] The users buy the vouchers from the Voucher issuer for 9€/piece but get back 30% of this value (2.7€) in the shape of a tax refund from the Government. They can then use the vouchers to pay for every hour of domestic service provided by a subsidized worker, who collects the vouchers and gives them to her

[10] Households are the only users allowed in the SVS and self-employed individuals or small firms are not allowed to hire SVS workers for professional purposes.

[11] The amounts presented in Figure 1 are those in place at the end of our observation window – i.e.in 2015 – but they vary only slightly over the period. The direct subsidy from the Government to the Voucher Issuer gradually decreased from 14.30€/voucher in 2004 to 13.04€/voucher in 2015. The price of the voucher, on the other hand, progressively increased from 6.70€/voucher in 2004 to 9€/voucher in 2015. The total value of the voucher perceived by the licensed SVS firms varied between 20.28€ and 22.04€ per voucher over the period. The tax deduction rate stayed constant at 30% over the period. In 2014, the SVS was regionalised and it is now regulated at the regional level. The fiscal refunds have, as a result, been adapted in some regions (it is now of 10% in Wallonia and 15% in Brussels, whilst it stayed at 30% in Flanders). However, these changes in fiscal deduction rates only started to occur in 2016, after our observation period.

[12] Usually, SVS firms function entirely on the payments made by the Voucher Issuer. However, they can also get access to additional financial resources if they charge administrative fees to the users or receive extra wage subsidies when they hire eligible workers (e.g., reduced social security contributions for workers who are particularly distanced from the labour market).

employer – the licensed SVS firm[13] – in exchange for an hourly wage that is pre-determined in an employment contract.[14] There are no eligibility conditions for the workers to be subsidized by the scheme, except that they must be employed by a licensed SVS firm and can only perform the authorized domestic services.

[Figure 1]

The wage subsidy offered by the Government is thus composed of two parts: (i) a direct wage subsidy paid to the SVS firms employing the workers and (ii) a tax refund for the users of domestic services. Ultimately, each voucher has a value of 22.04€, of which 6.30€ is paid by the user and 15.74€ (or over 70%) is paid by the Government. The size of the intervention was designed in a way to suppress any financial incentive for the users and workers to prefer the shadow economy. For the workers, the main advantage of working in the SVS compared to being employed in a similar job but in the shadow economy is that they receive an employment contract and therefore build rights to social security, including a pension, unemployment benefits and disability benefits. For the users, the main advantage of the SVS over the black market is that their domestic helper is legally employed.

As illustrated in Figure 2, the program, which employed just above 15,000 workers in 2004, has experienced a very rapid growth over the years and now employs more than 150,000 people in Belgium, or 6% of the female working population. In 2015, one in five households used vouchers to pay for domestic services. This massive (and, to some extent, unexpected) growth has led the SVS to become the largest employment program in Belgium, reaching a total gross expenditure of nearly €2 billion per year.[15] In recent years, on top of the scheme's important budgetary cost, a growing concern within the sector and Government has been the high incidence of disability among SVS workers. Between 2003 and 2015, the

[13] There exist different types of SVS firms: commercial firms (private companies, temporary work agencies, private individuals), non-commercial firms (non-profit companies, local employment agencies, integration agencies) and public entities (public social welfare centres and municipalities).
[14] The legal minimum wage in the SVS is 11.04€/hour (gross). However, very few workers – approximately 10% – actually do have a full-time contract. In our dataset, the average quarterly income of people registered as having worked in the SVS during the quarter is approximately 2.500€ (net of tax), or a monthly wage just above 800€. However, because our dataset counts all those who "have worked" in the SVS during a quarter as SVS workers, it is probable that 800€/month net of tax is a lower-bound estimation of their monthly salary as some do not work the entire period.
[15] This represented approximately 0.4% of Belgium's GDP in 2013. As a comparison, public expenditure on training programs represented 0.16% of Belgium's GDP in the same year (OECD, 2020).

Joint Committee[16] 322 (that covers workers of the SVS) was indeed the one that experienced the single highest growth of disability rates (see Appendix A). Such trends echo recent findings that, in Belgium, disability has increased dramatically among blue-collar women (De Brouwer and Tojerow, 2018).

[Figure 2]

3 Data

This paper uses administrative data from the Belgian Labour Market Data Warehouse (LMDW) of the Crossroad Bank for Social Security (CBSS) covering the entire Belgian population. The database aggregates administrative data from multiple governmental and social security institutions in Belgium. It contains extensive individual information, for the years 2003 through 2015, on more than 10 million individuals. More specifically, our dataset includes yearly information from the national registers and quarterly information from the National Social Security Office, the National Unemployment Agency, the National Institute for Health and Disability Insurance and the National Intermutualist Board. Given our research question and the fact that 98% of SVS workers are female, we decide to select only observations of women who are of working age (i.e., between 18 and 65 years old) in our dataset.

The Belgian LMDW classifies individuals' position on the labour market at the end of each quarter of observation on the basis of all the datasets it collects. We use these positions at the end of the quarter to construct dummy variables for each outcome of interest, namely being employed, unemployed, inactive and claiming short- or long-term DI benefits. Specifically, we define being "employed" as having a salaried contract or being self-employed, being "unemployed" as receiving unemployment benefits, being "inactive" as being outside of the labour force, and "claiming short- or long-term DI benefits" as receiving such benefits at the end of the quarter of observation. In our heterogeneity analysis, we also distinguish between being employed in the SVS and being employed outside of the SVS on the basis of whether or not the individual has worked within the SVS at least once during a given quarter. Specifically, if the individual is employed at the end of a quarter and has, at some point during that quarter, worked in the SVS, she is registered as being employed in the SVS; if the individual is employed at the end of a

[16] In Belgium, Joint Committees are permanent bodies that are created (in different branches of activity) and that serve as a negotiation platform between workers and employers who perform similar activities to determine general working conditions in the sector.

quarter but has not once worked in the SVS during that quarter, she is registered as being employed outside of the SVS.

[Figure 3]

Figure 3 illustrates the evolution of the labour market position of all women (on the left) and of women who participated at least once in the SVS (on the right), compared to their respective base-level in 2003. The graph on the left shows that, although inactivity has tended to slightly decrease over the years and disability has grown, the position of women in the labour market has been relatively stable on average between 2003 and 2015. On the other hand, the graph on the right shows a much stronger evolution among program participants, with sharp increases in the share of women working or receiving DI benefits, and a decrease in the share outside of the labour force or receiving unemployment benefits. Such trends suggest that labour market participation, employment and disability have grown – and unemployment has decreased – more strongly among women who participated in the program than among women in general. These graphs, although descriptive, suggest that the SVS might improve labour market opportunities for program participants, but might also cause them to have a work-related disability. This paper aims at causally estimating this hypothesis.

But before doing so, it is useful to better understand who the subsidized workers are and how they differ from the rest of the population. Table 1 presents descriptive statistics at the end of 2003 for two populations: the SVS workers – i.e., women who will enter the SVS at least once between 2004 and 2015 – and all other women aged between 18 and 65 – i.e., those who will never enter the scheme during our observation window.[17] It shows that women who participate in the program between 2004 and 2015 are eight years younger on average than women who do not ever participate. They are also more likely to be mothers, and more specifically single or unmarried mothers. In terms of geographical location, SVS workers are slightly more likely to live in the Northern (Flemish speaking) part of the country, although geographical differences within Belgium are relatively negligible. In fact, the strongest difference in terms of geographical location between SVS workers and other women is that 25% of SVS workers were not yet living in Belgium at the end of 2003, compared with 12% of other women. Relatedly, Table 1 shows that women who decide to participate in the SVS are nearly twice as likely to have a foreign nationality

[17] For computational reasons, the descriptive statistics presented in Table 1 refer to a random sample of 10% of the working age female population.

(particularly eastern European, African, or South and Central American) than other women in our sample. Finally, in terms of labour market position, Table 1 indicates that SVS workers are on average as likely to participate in the labour market than other women in 2003, but are nearly three times more likely to be unemployed and less likely to be employed. Similarly, women who participate in the SVS are less likely to be inactive or to claim long-term DI benefits in 2003.

[Table 1]

Given that SVS workers are so different from the rest of the population, we decide to restrict our sample to the population of all women who enter the SVS at least once between 2004 and 2015. Our empirical analysis is performed on this sub-population, which includes 305,908 individuals. The next section describes how we recover treatment effects using this sample.

4 Empirical Approach

4.1 Selection of the Treatment and Control Group

In our empirical analysis, treatment is defined as starting to work (for the first time) in the SVS. Finding a credible control group in this context is challenging because, as shown in the previous section, SVS workers and other women differ significantly – both in terms of observed characteristics and their endogenous decision to participate in the scheme. Simply comparing individuals who enter the scheme with individuals who do not would therefore likely lead to biased estimates. For this reason, we first restrict our dataset to the population of all women who enter the SVS at least once between the start of the scheme (2004) and the end of our observation period (2015). Differences in the timing of entry are then used to recover treatment effects. As shown in Figure 4, we consider women who enter the SVS between 2004 and 2007 – the "early treated" – as the treated group. We consider women who enter the SVS between 2013 and 2015 – the "future treated" – as the control group. This allows us to compare the outcomes of the "early treated" to those of the "future treated" for at least five years, after which some of the controls might become treated.[18]

[18] We perform a robustness check in Section 6 in which we reduce the number of years between the treatment and control pools. This allows us to check that the estimated short-term effects do not change when we match individuals who enter at closer dates, although this comes at the cost of not being able to estimate longer-term effects.

[Figure 4]

4.2 Coarsened Exact Matching (CEM)

Although we perform our analysis only on women who participate in the SVS, the treated group still differs from the control group, as shown in Table 2.[19] The treated are indeed older on average than the comparison group – which is reflected in the household positions – and are much less likely to be of foreign (and more specifically eastern European) origin.[20] For this reason, we make the treatment and control groups more comparable on observables using coarsened exact matching (Iacus et al., 2012) – CEM henceforth –, an exact matching algorithm that creates a set of strata with the same coarsened values of matching variables. Individuals from the treatment group are thus matched, in the quarter before they enter the program, with individuals from the control group who are identical in terms of coarsened observed characteristics.[21] We choose CEM over other matching methods as it was shown that it dominates commonly used existing matching techniques in its ability to reduce imbalance, model dependence, estimation error, bias, variance and mean square error (Iacus et al., 2009, 2011 and 2012).[22]

[Table 2]

The main trade-off with CEM relates to internal and external validity. On the one hand, the more strata, the more accurate the match will be and the higher the internal validity. On the other hand, a greater number of strata decreases the probability of finding a match for the treated, thus lowering external validity. We attempt to balance these two considerations by choosing the following matching algorithm. First, we set the matching period so that treated and control individuals are paired on the basis of their

[19] Characteristics in Table 2 are evaluated in the last quarter of 2003, before any individual becomes treated.

[20] These differences are likely due to the way we define treatment and control samples as, by construction, some of the women in the control group were not of working age or living in Belgium at the time that the treated entered the scheme.

[21] In addition, for each stratum, the CEM algorithm returns weights that can be used to reweight observations in the matched control sample and balance the empirical distributions of the matching variables between the treated and control groups. These weights are also used in the regression analysis described in Section 5. Control individuals receive a weight $w_j = {n_t^j}/{n_c^j} * {N_c}/{N_t}$ where N_c and N_t are the total number of control and treated individuals in the sample, and n_t^j and n_c^j are the number of treated and control individuals in the stratum j. All matched treated individuals receive a weight equal to 1.

[22] For example, one key advantage of CEM compared to other matching methods is that the maximum acceptable level of imbalance between the treated and their matched controls is decided ex-ante and doesn't need to be checked ex-post (as is often the case with other matching techniques). Moreover, reducing maximum imbalance on one variable doesn't have any effect on the maximum imbalance specified for any of the other variables. CEM also restricts the matched data to areas of common empirical support by trimming unmatched observations from both the treated and control samples.

characteristics for the same quarter-year before the treated enter the scheme.[23] Next, we match on age (cut-offs at 25, 30, 35, 40, 45, 50 and 55) and position in the household (single, married with children, married without children, unmarried with children, unmarried without children, single parent, dependent child and other) because, as shown in Table 2, these two characteristics constitute the key differences between the treated and control samples. We match on the individual's nationality (Belgium, Northern Europe, eastern Europe, Southern Europe, Other EU, Middle East, Northern Africa, Africa, Asia, North America, South and Central America, Oceania and unknown) because, as shown in Table 2, individuals from the comparison group are more likely to be of foreign origin. We match on the socio-economic position before entry (employed, self-employed, unemployed, social aid, career interruption, exempted jobseeker, pension, dependent child, short- or long-term disability, professional illness or accident, handicap, inactive and unknown status[24]) to capture differences in individual labour market attachment before program entry. Finally, we match on labour market histories to control for potentially unobserved characteristics that might affect treatment and future labour market outcomes. Specifically, we condition on the number of periods spent employed, unemployed, inactive, ST disabled, or LT disabled in the year preceding the quarter of entry in the scheme. As demonstrated by Lechner and Wunsch (2013), using information on labour market histories in the matching procedure should allow us to remove most of the selection bias due to potentially unobserved characteristics (also recently supported by Caliendo et al., 2017). This matching procedure leads us to drop 38.8% of our treated individuals for whom no match could be found.[25] While our strict matching algorithm (and the trimming it implies) might diminish the

[23] In the matching procedure, only pre-treatment observations of the treated group are kept so that we obtain only matches on the basis of their characteristics in the quarter before treatment. For computational reasons, we cannot keep all control observations for the matching procedure and we therefore draw a random sample of 20% of all the control observations to which treated observations are matched.

[24] These categories are described in more detail in Appendix B.

[25] There are 93,542 women in our treated pool, 57,264 (36,278) of whom (do not) find an appropriate match. The main risk we identify with our trimming is that we lose some observations of foreign-nationality workers. Comparing Table 2 and the first column of Table 3 indeed shows that, after matching, the share of Belgian individuals increases among the treated sample, which indicates that the matching procedure leads to trim more observations of foreign-nationality workers. This might be due to the number of matching requirements imposed on the data which, combined with the matching requirement on nationality, leads to too little control observations for foreign treated workers. As a robustness check (in Section 6), we present our results without matching on nationality, which leads to trim significantly fewer observations of foreign-born workers. Although the estimated effects are similar when nationality is removed from matching covariates, we prefer to keep it in our benchmark analysis as this is a key difference between our treated and control samples (as shown in Table 2) and migrant status has been shown to affect labour market outcomes (e.g., Clark and Drinkwater, 2008).

external validity of our results, we select it instead of other, less strict, matching algorithms because it guarantees that our identifying assumptions hold.[26]

The effect of CEM and its weighting algorithm on the pre-treatment characteristics of the treated and control groups can be seen in Table 3.[27] The first column shows the distribution after the matching procedure, but without any reweighting of observations, while the second column shows the distribution of covariates after matching and reweighting.

[Table 3]

As shown in Table 3, matching in itself already allows us to somewhat improve the balancing of covariates by removing from the sample individuals who do not find a similar counterpart in the opposite group. However, even after matching but without reweighting observations, some standardized differences are still quite high.[28] The reweighting procedure of our coarsened exact matching, on the other hand, makes the distribution of covariates in the control and treatment groups almost identical, thus ensuring that the linear regression methods used are not sensitive to the model specification. CEM (and its built-in reweighting) applied to the sub-population of individuals who participate in the program thus allows us to study individuals who are reasonably comparable.

4.3 Event Study Difference-in-Differences

After applying CEM, we obtain a homogeneous sample of treated and control individuals whom we can compare around their date of entry in the scheme using a dynamic event study difference-in-differences model that allows the treatment effect to vary over time (Rellstab et al., 2020; Jeon and Pohl, 2017; Jacobson et al., 1993). In order to do so, we define an indicator of how many quarters an individual is away from entry in the scheme, $Dist^k_{it}$ with $k \in [-4, 20]$, zero indicating the quarter at which the individual enters for the first time. For the control group, this variable is coded according to the corresponding treated individuals in the attached treatment cohort. We also create a dummy variable indicating assignment to the treatment group, $Early_i$. We then estimate the following model with interactions

[26] In Section 6, we perform a robustness check where we remove the matching requirement on labour market history to verify that our results are not sensitive to this relatively high trimming rate and show that our findings remain largely unaffected.
[27] Descriptive statistics are shown for the last quarter of 2003, before any individual becomes treated.
[28] Imbens and Wooldridge (2009) suggest as a rule of thumb that a standardised difference should be below 0.25 to ensure that the linear regression methods are not sensitive to the model specification.

between the treatment cohort dummy ($Early_i$) and distance to entry dummies ($Dist^k_{it}$) and applying CEM weights:

$$Y_{it} = \alpha + \alpha_t + \gamma Early_i + \sum_{k=-4}^{20} \theta^k Dist^k_{it} + \sum_{k=-4}^{20} \beta^k Dist^k_{it} Early_i + \delta X_{it} + \epsilon_{it} \quad (1)$$

Y_{it} is the labour market outcome of interest, i.e., being employed, unemployed, inactive, and claiming short- or long-term DI benefits. The parameters of interest in this equation are the coefficients on the interactions between treatment status and event time, β^k. These coefficients identify the effect of working in the subsidized PHS sector on the outcome, k periods relative to the quarter of entry. Given that CEM sets the matching weight to one in the treatment group, our estimates are to be interpreted as an average treatment effect on the treated (ATT). In addition, quarterly time-fixed effects α_t, time-varying controls X_{it} (including age, nationality, country of origin, position in the household and district of residence) and the error term ε_{it} are included in the model.

Our approach rests on the assumption that the outcomes of the treated and controls would have run parallel in the absence of treatment. The procedure described in the previous paragraphs – whereby we select only those who enter the SVS at least once and match treated and control individuals on the basis of their characteristics in the period before the treated enter the scheme – aims at reinforcing the plausibility of this parallel trend assumption. This procedure enables us to compare program participants who, in the pre-treatment period, are roughly the same age, have the same family structure and occupy the same position in their family, are of the same nationality and have a similar level of attachment to the labour market (indicated by current labour market status and labour market history).[29] Ultimately, the only difference between the two groups is that some enter the SVS before others do. This means that the remaining condition for the parallel trend assumption to hold in our framework is that there should not be any time-varying unobserved factors that affect both the timing at which individuals enter the program and their labour market outcomes. In this respect, the fact that we use labour market histories in our matching procedure should remove most of the selection bias due to potentially unobserved characteristics (Lechner and Wunsch, 2013; Caliendo et al., 2017). We formally test the

[29] As a matter of fact, these strong similarities between the treatment and comparison groups are reflected in their very close (if not identical) pre-treatment outcomes (see Figure 5, Figure 6 and Figure 7), which has been shown to be an important factor in satisfying the identifying assumptions of a difference-in-differences estimator (Kahn-Lang and Lang, 2019).

validity of this assumption in Table 4 by including interactions between the treatment dummy and time periods before the quarter of entry in our regressions. Pre-treatment coefficients are all statistically insignificant for all of our outcomes of interest, which supports the validity of the parallel trend assumption over the four quarters preceding program entry.

[Table 4]

From a theoretical point of view, survey data on the motivations for joining the scheme (Peeters et al., 2007) also support the validity of the parallel trend assumption. Although there appears to be several drivers of program participation that could, in theory, threaten the validity of our results, we arguably limit such threats in our estimation strategy. These factors are linked to: (i) family characteristics, (ii) social security eligibility and (iii) labour market attachment. First, changes in family composition and parenthood appear to be important drivers of program participation, namely through the attractiveness of job characteristics such as proximity and flexibility. Family composition and parenthood could influence labour market outcomes as well as participation, which would threaten the validity of our findings if they were not accounted for in our estimation strategy. We are, however, able to shield ourselves from such a risk by conditioning our CEM algorithm on position in the household (which contains information on partnership and parenthood characteristics) in the pre-treatment period and by controlling for changes in household position in our analysis. Second, the opportunity to build rights to social security appears to be another important driver of participation, which could threaten the validity of our findings if changes in the attractiveness of social security eligibility and labour market outcomes are linked in some way (e.g., if treated individuals tend to have a worsening health condition and join the program to be eligible for social security in case of health issues). If this were the case, however, we should observe an increase in the probability of receiving social security benefits among participants who were not previously eligible, but not among those who were already eligible. Indeed, the attractiveness of social security eligibility is only a motivation to join the program for those who are not already eligible (i.e., those who were inactive) and this threat therefore only concerns this subgroup. In our heterogeneity analysis we show that previously eligible and ineligible workers are similarly affected by participation in terms of their probability of claiming social security (UI or DI) benefits, which indicates that the parallel trend assumption is not threatened by the fact that some workers join the program to build rights to social security. Third, the satisfaction from working and earning a living appears to be another important driver of program participation, which could bias estimates as these motivations likely affect labour market outcomes as well as treatment. By matching on pre-treatment labour market histories, we control for labour market attachment and thus shield ourselves from this risk.

All things considered, we thus argue that it is plausible to assume that the labour market trends of treated and control individuals would have run parallel in the absence of program participation. Nevertheless, this might still be considered quite a strong assumption given that there are at least five years between the dates at which the treatment and control groups enter the program in our setting. We therefore perform a robustness check in Section 6 by which we match treatment and control individuals who enter the scheme at much shorter time intervals.

5 Findings

5.1 Benchmark Findings

Table 5 shows the estimated effects of participating in the subsidized PHS sector on the probability of being employed, unemployed, inactive, or claiming short- or long-term DI benefits. Estimated treatment effects are presented for each year following the date of entry in the scheme (i.e., at event time k=4, 8, 12, 16 and 20) and illustrated graphically in Figure 5, Figure 6 and Figure 7. In the following paragraphs, we discuss the effects one and five years following the date of entry in the scheme in order to provide evidence on both short- and long-term impacts of program participation.

[Table 5]

First, we start by evaluating whether PHS subsidies increase the employment probability of program participants in the first column of Table 5. Our estimates show that starting to work as a subsidized domestic helper increases the probability of the program participant of being (formally[30]) employed by 22.6 percentage points (pp) after one year (which corresponds to a 56% effect[31]) and by 16.8 pp (42%) after five years. In their meta-analysis, Card et al. (2018) find that, on average, private sector subsidies

[30] Note that when we speak of employment effects, we are referring to *formal* employment. It is indeed likely that part of the women who joined the SVS were already employed as domestic helpers in the shadow economy before their entry in the scheme, but we cannot distinguish between effects on employment formalisation and employment itself as we do not observe undeclared work.

[31] Proportional effects are obtained by dividing the size of the treatment effect by the constant.

increase employment probability of program participants by 6.2% after 1-2 years and by 21.1% after more than 2 years. Focusing on wage subsidies for low-skilled workers more specifically, Huttunen et al. (2013) find no effect of a Finnish payroll tax subsidy scheme on the employment rates of older, full-time, low-wage workers. Our estimated employment effects are thus large compared to what can be found in the existing literature on wage subsidies, which could be due to three factors. First, the program targets female workers, who have been shown to be particularly responsive to wage subsidies (Card and al., 2018; Boockman et al., 2012). Second, due to the specific sector being subsidized, part of the employment effects could stem from the formalisation of pre-existing but non-declared activities, which could contribute to inflate employment effects (Carbonnier, 2015). Finally, and most importantly, these substantial employment effects could be explained by the large size of the subsidy[32] and its permanent nature. Notably in this vein, Sjögren and Vikström (2015) show that the effects of wage subsidies on the job-finding rate are stronger for larger subsidies and proportional to the length of the subsidy. All in all, these findings are encouraging as they show that PHS subsidies can have important and long-lasting effects on employment of program participants.

[Figure 5]

Next, we evaluate how the creation of new employment opportunities translates in terms of unemployment and labour force participation. In this respect, our estimates in the second and third columns of Table 5 indicate that working in the subsidized PHS sector decreases both unemployment and inactivity probability by 12.9 pp (47%) and 8.7 pp (46%), respectively, one year after program entry. The effect on unemployment only slightly decreases over time while the effect on inactivity increases. Subsidized workers are indeed still 10.8 pp (39%) and 10.0 pp (53%), respectively, less likely to be unemployed or inactive five years after their program entry. Such findings are encouraging not only with regards to the scheme's objective of reducing unemployment, but also in terms of increased female labour market participation and the formalisation of previously undeclared activities. The decrease in unemployment probability indeed suggests that PHS subsidies create new employment opportunities for individuals who would otherwise have been unemployed. Likewise, the decrease in inactivity suggests

[32] 70% of the worker's wage is subsidized in the SVS. As a comparison, in Belgium, the average subsidy for older workers studied in Albanese and Cockx (2019) amounts to about 4% of the median wage cost, including all payroll taxes and is at maximum 14% of the wage costs for minimum wage workers. In Finland, the tax cut studied in Huttunen et al. (2013) that is targeted at older, full-time, low-wage workers, amounts to maximum 16% of gross monthly earnings

that PHS subsidies create new types of employment opportunities for women who would not have worked in other kinds of jobs (e.g., single mothers who could not have found other jobs with schedules that fit their parental responsibilities). Finally, both the decreased unemployment and inactivity probability indicate that PHS subsidies can serve as a means to formalize some pre-existing undeclared activities. We cannot determine to what extent this formalisation took place as, by construction, undeclared employment is not observed in administrative data. However, given the large prevalence with which undeclared domestic services were performed in Belgium in the 90s[33], it is likely that at least part of the reduction in inactivity (and unemployment) was due to a formalisation of formerly undeclared jobs.

[Figure 6]

Finally, we evaluate the effect of PHS subsidies on the program participants' probability of claiming DI benefits. Our estimates in the fourth column of Table 5 show that entering the subsidized PHS sector nearly triples the program participant's probability of claiming short-term DI benefits one year after entry in the scheme (3.7 pp increase). This effect then decreases over time and reaches a size of 2.2 pp (105%) five years after program entry. This does not mean, however, that the effect on disability decreases over time. Individuals who spend more than one year on short-term disability indeed automatically shift to the long-term DI scheme. This is reflected in the fifth column of Table 5 in which it can be seen that, although participating in the SVS has no effect on the probability of claiming long-term DI benefits after one year (individuals simply do not have the time to enter the long-term DI in one year), it increases the probability of claiming long-term DI benefits by 4.0 pp after five years. In other words, five years after starting to work as a subsidized domestic helper, program participants are 20 times more likely to claim long-term DI benefits. These findings suggest that PHS subsidies have important spillover effects on disability and echo previous research showing that job-search monitoring and job-search assistance programs – that also aim at reducing unemployment – can have spillover effects on DI (De Brouwer et al., 2019; Lammers et al. 2013; Petrongolo, 2009). This implies that PHS subsidies

[33] Employment in the shadow economy is, by definition, complex to estimate, but survey data suggests that approximately 10% of users of the Service Voucher Scheme use to employ an undeclared domestic helper before the implementation of the scheme, while nearly 25% state that they would hire an undeclared domestic helper if the SVS did not exist. On the other hand, only between 1% and 5% (depending on the year of the survey) of the workers admit to having worked on the black market before the SVS, but 67% of them state that escaping employment in the shadow economy is an important advantage of working as a subsidized domestic helper (Peeters et al., 2007). These numbers are likely to underestimate the extent of black-market employment as they come from survey data and respondents might have had incentives to hide having illegally employed someone or having illegally been employed.

have an indirect cost for the Government in the form of additional social security spending. Such findings also indicate that program participation might deteriorate the workers' health, although the increased disability probability could also stem from the fact that some workers become eligible for social security through their subsidized working experience. We untangle these health and eligibility effects on disability in the next section.

[Figure 7]

5.2 Heterogeneity Analysis and Mechanisms

5.2.1 Effects on Employment Inside versus Outside of the Subsidized PHS Sector

In the previous section, we showed that program participation leads to an increase in the probability of being employed. There are two potential mechanisms through which this could occur. First, unless demand for PHS is perfectly inelastic, PHS subsidies create new employment opportunities in the subsidized PHS sector through a decreased cost for the employers and service users. Second, PHS subsidies might improve employment opportunities of program participants if it allows them to increase their (perceived) productivity and/or acquire new skills. In this section, we attempt to disentangle these two mechanisms by estimating effects on employment within and outside of the subsidized PHS sector separately.

[Table 6]

As shown in Table 6, we find that, one year after entering the subsidized PHS sector, participants are 51.9 pp more likely to be employed within the scheme, but 29.4 pp (73%) less likely to be employed outside of the subsidized PHS sector. These effects decrease over time and, after five years, subsidized workers are 38.0 pp more likely to be employed within the scheme, while they are 21.2 pp (53%) less likely to have a non-subsidized job. Our findings suggest that PHS subsidies can create long-term subsidized employment opportunities, but few job prospects on the regular non-subsidized labour market. The failure of the program to foster non-subsidized employment opportunities is consistent with some previous research on wage subsidies (Groh et al., 2016) and might lie in its insufficient emphasis on general human capital accumulation, a key factor of successful employment policies (Card et al., 2018; Sianesi, 2008). The few trainings that appear to be followed by SVS workers are, indeed, focused on domestic services activities that would not necessarily be transferable to most other non-subsidized jobs.[34] Furthermore, our results indicate the existence of a lock-in effect, whereby non-subsidized employment prospects lastingly decrease following program participation. This could be due to the potential stigma associated with domestic work that might decrease (rather than increase) the workers' perceived productivity (Burtless, 1985; Ashforth and Kreiner, 1999; Baert, 2016; Liechti et al., 2017). Alternatively, and in line with the well-documented lock-in effects of training programs (e.g., van Ours, 2004; Ham and Lalonde, 1996), the lock-in effect of PHS subsidies could also be due to their duration (Caliendo et al., 2008; van Ours, 2004). Because there is no obligation to ever leave the program, employment as a subsidized domestic helper can indeed be (and often is) a long-term employment solution[35], which mechanically lowers the participants' probability of working in another, non-subsidized, job. All in all, these findings therefore indicate that subsidized PHS jobs should not be considered as means to help program participants transition towards non-subsidized employment, but rather as long-term employment opportunities for workers with few job prospects on the non-subsidized labour market.

5.2.2 Socio-economic Status before Entry

PHS subsidies differ from most wage subsidies in the sense that they are very broadly targeted. Indeed, they only require participants to be employed in the subsidized PHS sector to benefit from the subsidy and, unlike most other wage subsidies, they do not impose eligibility criteria based on labour-market position (e.g., being unemployed), earnings or age. This implies that there are many different types of subsidized workers and these different types of participants might experience very different effects as a result of program participation. Therefore, in this section, we check for heterogeneous effects among individuals entering the scheme from different labour market positions in order to identify the profiles that benefit the most (as well as least) from a subsidized working experience in the PHS sector. This will also help us better understand the mechanisms through which PHS subsidies influence labour market outcomes of program participants. Specifically, in Table 7, we estimate the effect of participating in the SVS on future labour market outcomes separately for individuals who, the period before treatment, were (i) employed, (ii) unemployed, (iii) inactive, (iv) disabled or (v) receiving social aid.

[34] The main trainings followed by SVS workers in 2011 were those on "health and security at work" and "knowledge of products and material" (Idea Consult, 2012).

[35] This observation is supported by survey evidence showing SVS workers often consider their subsidized job as stable employment, particularly those who are older (Schooreel and Valsamis, 2017).

[Table 7]

The first column of Table 7 shows that all types of participants experience an increase in employment probability in the long-term, with particularly large effects among previously inactive or unemployed participants. This is also true in the short-term, except for previously employed participants who actually experience a slight decrease in employment probability one year after entering the program. This decrease in the probability to be employed for this particular group is accompanied by an increased probability of being inactive or short-term disabled, as shown in columns (3) and (4), suggesting that program participation leads some previously employed individuals to become inactive or disabled in the short-term.

The second column of Table 7 shows that participants who experience the largest decrease in unemployment in the short- and long-run are those who were unemployed before they participated. Previously employed participants also experience a decreased unemployment probability at all time horizons, although effects are much smaller. Individuals who were receiving DI benefits or social aid before they entered the SVS also experience a decrease in unemployment, but only in the long-term. Conversely, previously inactive participants only experience a small decrease in unemployment in the short-term. Interestingly, unemployment probability does not increase among those previously inactive at any time horizon, thus disproving the existence of an eligibility effect of participation – i.e., becoming eligible for social security and thus having a higher probability of receiving social security benefits after having worked as a subsidized domestic helper.[36]

The third column of Table 7 shows that, in the short-term, inactivity is only reduced for previously inactive participants while it increases among previously employed and unemployed workers. This suggests that the subsidized experience in the PHS sector pushes some individuals out of the labour market.[37] On the other hand, in the long-term, all participants (except those previously receiving social

[36] In Belgium, workers are entitled to Unemployment Benefits in two instances: (i) after graduation and a waiting period of 12 months or (ii) after involuntary dismissal from a sufficiently long-lasting job (12 months). Inactive individuals are therefore not eligible to Unemployment Insurance.

[37] For jobseekers, a possible explanation for this phenomenon is that participation in the scheme was encouraged by Employment Agencies during monitoring interviews although the workers themselves did not always wish to work as domestic helpers (Peeters et al., 2007). This might have led some previously unemployed workers to leave the labour market after having tried to work in the subsidized PHS sector.

aid) experience a decreased probability of being inactive, although the effect is by far the strongest for those previously inactive.

Finally, the fourth and fifth columns of Table 7 show that working as a subsidized domestic helper increases the odds of claiming DI benefits for all participants, irrespective of their previous labour market status. One exception relates to previously disabled individuals, who actually experience no long-term effects and a short-term decrease in the probability of receiving long-term DI benefits. This might be explained by the fact that program participants who already had health issues before entering the scheme would have been disabled either way and program participation does not, therefore, impact them in the same way as other workers. Interestingly, the workers who appear to experience the largest increase in disability are those previously unemployed – a group that was already eligible for social security before program participation. In other words, those who were already eligible for DI (i.e., previously unemployed) have a slightly higher tendency to become disabled following their participation than those who were not eligible (i.e., previously inactive). Such findings are consistent with the existence of a strong health effect attached to working as a subsidized domestic helper and the absence of an eligibility effect.[38]

5.2.3 Disaggregating Disability Claims by Medical Conditions

In the previous section, we show that the increased probability of claiming DI benefits following program participation is due to an adverse effect on health, and not to the novel eligibility for social security. In this section, we estimate treatment effects separately for four types of disabilities that are prevalent among workers in the cleaning sector, namely osteoarticular, psychological, respiratory and skin conditions.[39]

[Table 8]

As shown in Table 8, the adverse health effect of working in the subsidized PHS sector is mainly driven by a strong increase – 1.7 pp after five years, or a massive 35-fold increase – in the probability of suffering from osteoarticular diseases. Table 8 also shows that working as a subsidized domestic helper increases

[38] This finding is further supported by the absence of a positive effect of treatment on the probability of being unemployed for previously inactive (and therefore ineligible) participants.

[39] We focus on long-term disabilities because these are the only ones for which we have detailed information on the type of illness causing the disability.

the odds of suffering from other types of illnesses, although to a lesser extent. The probability of suffering from psychological conditions increases by 1.2 pp after five years – an 11-fold increase – while the probability of having respiratory or skin diseases respectively increases by 0.06 and 0.03 pp five years after entry in the scheme.[40] These findings are consistent with previous research studying the health hazards associated with working in the cleaning sector. Cleaners have repeatedly been shown to be particularly subject to musculoskeletal illnesses because of job-related factors such as movement repetition, frequent awkward body posture, excessive force required to handle equipment, inadequate rest breaks and a rapid working pace (Woods and Buckle, 2006; Kumar and Kumar, 2008; Brun, 2009). Existing research has also highlighted the strong exposure of cleaners to chemical and biological hazards which, through dermal contact and inhalation, can lead them to suffer from respiratory and skin-related illnesses (Brun, 2009). Finally, previous research has highlighted a number of factors that can cause psychological difficulties for cleaners such as job insecurity, lack of control on the organization of their work, difficult working schedules, poor social recognition, lack of social relations at work and limited learning and career development opportunities (Brun, 2009; Goffin et al., 2018).

Altogether, these findings suggest that PHS subsidies might further exclude part of the (already vulnerable) target group from the labour market by increasing their probability of disability. Policymakers considering PHS subsidies as a means to curb unemployment should therefore pay particular attention to ensuring the physical and psychological well-being of program participants. Strategies that can mitigate health risks for cleaners include, but are not limited to, ensuring better equipment and its regular maintenance, offering training programs that promote healthy work practises, implementing procedures for risk assessment and reporting systems for ill health, and fostering communication, social support networks among cleaners and, possibly, teamwork (Woods and Buckle, 2006).

6 Robustness Checks

We check the robustness of our findings in Table 9, with the first column showing our benchmark results for ease of comparison. Our first robustness test, in the second column of Table 9, is intended to check

[40] Proportional effects cannot be presented here because the constant is not estimated precisely enough, which results in a zero denominator.

that our findings are not sensitive to the long time-lapses between program entry of the treated and control groups. The main threat to our estimation strategy lies in the fact that we match treated individuals to controls who enter the scheme at least 5 years later. One might argue that, because controls enter the program many years after the treated do, they are in fact not valid counterfactuals. In order to attenuate this concern, we perform a robustness check in which we impose an additional matching condition that ensures treated individuals are matched with a control counterpart who enters exactly two years later.[41] This allows us to verify the sensitivity of our short-term estimates but comes at the cost of not being able to estimate longer-term effects. As shown in Table 9, our findings remain qualitatively the same. Estimates on employment, unemployment and inactivity are slightly larger when we condition on closer dates of entry. As in our benchmark, effects on long-term DI are estimated to not be statistically different from zero after one year. Short-term disability effects, on the other hand, are estimated to be slightly smaller than in the benchmark analysis.

A second potential concern with our estimation strategy is that, because we impose quite a strict matching rule that includes nationality, foreign workers (especially those with a less represented nationality) are less likely to find an appropriate counterfactual. This leads us to trim relatively more foreign workers from the sample in our benchmark analysis, which might affect our estimates if foreign participants react differently from Belgians. The third column of Table 9, therefore, shows estimated effects when the condition on nationality is dropped from our matching algorithm. Doing this slightly increases the matching rate (and therefore the external validity of results).[42] Table 9 shows that estimates are either slightly larger or not statistically different from our benchmark results when nationality is dropped from the set of matching covariates, which supports our main conclusions.

Finally, our last robustness check aims at verifying the external validity of our findings. In our benchmark analysis, we trim nearly 40% of individuals from our treated group, which might affect external validity as it means our estimates only hold for 60% of the treated. Therefore, in the fourth column of Table 9, we remove the matching condition on labour market histories in order to increase the matching rate from 61.2% to 89.5%. Again, the findings remain qualitatively the same, although we find strong

[41] We decide to match treated and control individuals who enter the program at a two-year interval (instead of only one year) in order to be able to compare labour market outcomes one year after the treated have received treatment and without risking to pick-up any potential changes in labour market trends of the controls just before they participate themselves.

[42] However, this threatens the validity of the parallel trend assumption as we find a significant pre-trend for long-term disability.

indications that the parallel trend may not hold when labour market history is removed from the matching covariates.

[Table 9]

Altogether, the robustness checks presented in Table 9 thus support our findings, with remarkably little variation in the size of the estimates.

7 Conclusion and Discussion

In spite of their growing popularity, there appears to be very little empirical evidence on the effectiveness of PHS subsidies as a policy tool to fight unemployment among low-skilled workers. In this paper, we attempt to address this hole in the literature by studying the effects of the Service Voucher Scheme – a generous PHS subsidy in Belgium – on various labour market outcomes of program participants. We do so by estimating a dynamic event study difference-in-differences model combined with coarsened exact matching and using administrative data on the entire population of subsidized PHS workers. Thereby, we produce the first econometric estimations of the effects of PHS subsidies on labour market outcomes of participants using detailed micro-level data.

Our main findings show that participating in the subsidized PHS sector considerably increases the worker's probability of being employed both in the short- and long-term, which translates into a lasting decrease in the probability of being unemployed or inactive. However, this only occurs through an increased probability of being employed in the subsidized PHS sector, while the probability of working in a non-subsidized job decreases, thus suggesting the existence of a lasting lock-in effect. These results add to the existing literature on active labour market policies by evaluating a new type of wage subsidy that is aimed at low-skilled female workers, is broadly targeted and has the particularity of being permanent and very generous compared with subsidies studied so far. Our findings also contribute to the existing research on domestic services by studying the effects of subsidizing PHS on labour market outcomes of workers, distinguishing between employment within and outside of the subsidized PHS sector, and showing how employment effects translate in terms of unemployment and inactivity.

Furthermore, our findings show strong evidence demonstrating a negative effect of program participation on health (particularly on musculoskeletal illnesses), alongside the increased odds of claiming short- or long-term DI benefits. These findings echo the growing body of literature on spillover effects between different branches of social security (Petrongolo, 2009; Lammers et al., 2013; De Brouwer et al., 2019). We contribute to this strand of the literature by showing that, just like reforms pertaining to unemployment insurance, programs aimed at reducing unemployment like wage subsidies can also have effects on DI.

Overall, our findings indicate that PHS subsidies can be an effective policy tool to reduce unemployment and increase (formal) labour market participation among low-skilled workers. However, this policy should essentially be considered as a means to create long-term subsidized employment opportunities for workers with few job prospects on the non-subsidized labour market, and not as a means to offer a path towards non-subsidized employment. These results imply that putting an end to PHS subsidies, for budgetary reasons for example, could potentially generate large flows of entry into the Unemployment Insurance and the shadow economy. Moreover, our findings highlight the fact that subsidizing employment in a sector that is associated with high physical and psychosocial workloads can lead to indirect costs for both the subsidized workers (in terms of health) and the Government (in terms of additional DI spending). Policymakers should keep these indirect effects in mind when considering PHS subsidies as a tool to fight unemployment and pay attention to preventive measures that could reduce the health toll on workers. More generally, these results highlight that employment programs may have much broader effects than those initially anticipated, and program evaluations should pay more attention to health and other potential spillover effects of active labour market policies.

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Tables

Table 1: Characteristics of Subsidized Workers Compared to the Rest of the Female Population

	SVS workers	Other women		SVS workers	Other women
Age	32.442	40.426	Nationality		
	(9.510)	(13.330)	Africa	0.041	0.011
				(0.199)	(0.106)
Position in the household			Asia	0.022	0.012
Child	0.111	0.125		(0.145)	(0.110)
	(0.314)	(0.331)	Belgium	0.667	0.825
Single Parent	0.124	0.074		(0.471)	(0.380)
	(0.329)	(0.261)	Eastern Europe	0.117	0.024
Married w/ children	0.260	0.316		(0.321)	(0.151)
	(0.439)	(0.465)	Middle East	0.012	0.011
Married w/o children	0.049	0.160		(0.107)	(0.104)
	(0.216)	(0.367)	North America	0.000	0.004
Other	0.038	0.029		(0.018)	(0.065)
	(0.191)	(0.167)	Northern Europe	0.042	0.055
Single	0.059	0.095		(0.200)	(0.228)
	(0.235)	(0.293)	Northern Africa	0.032	0.018
Unmarried w/ children	0.071	0.040		(0.176)	(0.133)
	(0.256)	(0.196)	Oceania	0.000	0.000
Unmarried w/o children	0.045	0.046		(0.006)	(0.017)
	(0.208)	(0.209)	Other EU	0.003	0.002
Unknown	0.243	0.116		(0.057)	(0.039)
	(0.429)	(0.321)	South and Central America	0.022	0.004
				(0.145)	(0.064)
Province			Southern Europe	0.042	0.033
Antwerp	0.118	0.141		(0.201)	(0.180)
	(0.322)	(0.348)	Unknown	0.001	0.000
Flemish Brabant	0.051	0.091		(0.022)	(0.018)
	(0.220)	(0.287)			
Walloon Brabant	0.019	0.032	Labour market position		
	(0.136)	(0.175)	Employment	0.329	0.466
Brussels	0.051	0.083		(0.470)	(0.499)
	(0.221)	(0.275)	Unemployment	0.160	0.057
West Flanders	0.104	0.094		(0.367)	(0.231)
	(0.305)	(0.292)	Inactivity	0.130	0.157
East Flanders	0.102	0.118		(0.337)	(0.364)
	(0.303)	(0.323)	Short-term DI	0.027	0.018
Hainaut	0.085	0.109		(0.163)	(0.134)
	(0.279)	(0.312)	Long-term DI	0.007	0.026
Liège	0.070	0.079		(0.080)	(0.160)
	(0.256)	(0.270)	Other	0.098	0.160
Limburg	0.083	0.069		(0.297)	(0.367)
	(0.275)	(0.253)	Unknown	0.249	0.116
Luxemburg	0.016	0.019		(0.433)	(0.321)
	(0.123)	(0.137)			
Namur	0.035	0.039			
	(0.184)	(0.192)			
Unknown	0.266	0.127			
	(0.442)	(0.333)			

Note: Random sample of 10% of the population of women aged between 18 and 65. Characteristics are evaluated in the last quarter of 2003, before any individual becomes treated. "Other" position in the household includes individuals living in a collective household, individuals living within another household, and those who do not belong to any other category. "Unknown" position in the household and labour market position refers to individuals who were not in the Belgian national registers in the last quarter of 2003 (i.e., who were not living in Belgium).

Table 2: Summary Statistics – Pre-matched Samples

	Treated	Control	StdDiff
Age	33.444	24.812	0.579
<25	0.229	0.538	-0.474
25-30	0.150	0.124	0.055
30-35	0.166	0.116	0.101
35-40	0.162	0.101	0.129
40-45	0.139	0.074	0.151
45-50	0.094	0.034	0.174
50-55	0.042	0.010	0.142
>=55	0.017	0.003	0.101
Position in the household			
Child	0.137	0.291	-0.270
Other	0.044	0.033	0.040
Single parent	0.152	0.052	0.236
Married w/ children	0.303	0.130	0.303
Married w/o children	0.059	0.026	0.116
Single	0.074	0.036	0.118
Unmarried w/ children	0.086	0.033	0.158
Unmarried w/o children	0.049	0.026	0.084
Unknown	0.096	0.373	-0.488
Nationality			
Unknown	0.000	0.001	-0.030
Africa	0.034	0.044	-0.038
Eastern Europe	0.045	0.179	-0.309
Belgium	0.795	0.566	0.357
Southern Europe	0.030	0.059	-0.102
Northern Europe	0.036	0.044	-0.028
Other EU	0.004	0.004	-0.001
Northern Africa	0.023	0.038	-0.061
Middle East	0.006	0,017	-0.072
North America	0.000	0.000	-0.004
Oceania	0.000	0.000	0.000
South and Central America	0.015	0.022	-0.038
Asia	0.013	0.025	-0.062
Labour market position			
Employed	0.357	0.183	0.283
Self-employed	0.031	0.023	0.033
Unemployed	0.208	0.072	0.284
Career interruption	0.005	0.002	0.027
Exempted unemployed	0.020	0.007	0.080
Social aid	0.032	0.017	0.071
Pension	0.006	0.002	0.053
Dependent child	0.064	0.238	-0.353
Disability	0.019	0.006	0.082
Professional illness	0.000	0.000	0.014
Handicap	0.000	0.000	0.008
Inactive	0.155	0.074	0.182
Unknown	0.102	0.377	-0.480
Observations	96,087	57,674	

Note: This table shows summary statistics of the treated and control samples in the last quarter of 2003, before any individual becomes treated. The treated group is composed of the overall population of women entering the SVS between 2004 and 2007, while the control group is composed of those entering the scheme between 2013 and 2015. “Other” position in the household includes individuals living in a collective household, individuals living within another household, and those who do not belong to any other category. “Unknown” position in the household and labour market position refers to individuals who were not in the Belgian national registers in the last quarter of 2003 (i.e., who were not living in Belgium). Standardised difference $\text{StdDiff} = \frac{\overline{X_C}-\overline{X_T}}{(\hat{\sigma}_C^2-\hat{\sigma}_T^2)^{0.5}}$ where $\bar{X}_c$ corresponds to the mean of variable X of the control group in the last quarter of 2003 and $\hat{\sigma}_C^2$ the estimated variance.

Table 3: Summary Statistics – Matched Samples

	(1) Unweighted Matched Sample			(2) Weighted Matched Sample		
	Treated	Control	StdDiff	Treated	Control	StdDiff
Age	35.239	32.231	0.221	35.239	35.147	0.007
<25	0.170	0.283	-0.192	0.170	0.170	0.000
25-30	0.142	0.145	-0.005	0.142	0.142	0.000
30-35	0.160	0.150	0.020	0.160	0.160	0.000
35-40	0.177	0.157	0.038	0.177	0.177	0.000
40-45	0.166	0.151	0.029	0.166	0.166	0.000
45-50	0.118	0.083	0.082	0.118	0.118	0.000
50-55	0.048	0.025	0.089	0.048	0.048	0.000
>=55	0.019	0.007	0.072	0.019	0.019	0.000
Position in the household						
Child	0.098	0.200	-0.205	0.098	0.098	0.000
Other	0.028	0.040	-0.047	0.028	0.028	0.000
Single parent	0.187	0.135	0.100	0.187	0.187	0.000
Married w/ children	0.382	0.338	0.065	0.382	0.382	0.000
Married w/o children	0.068	0.060	0.024	0.068	0.068	0.000
Single	0.079	0.071	0.022	0.079	0.079	0.000
Unmarried w/ children	0.104	0.091	0.032	0.104	0.104	0.000
Unmarried w/o children	0.055	0.066	-0.034	0.055	0.055	0.000
Unknown	0.000	0.000	-	0.000	0.000	-
Nationality						
Unknown	0.000	0.000	-	0.000	0.000	-
Africa	0.016	0.017	-0.002	0.016	0.016	0.000
Eastern Europe	0.036	0.014	0.097	0.036	0.036	0.000
Belgium	0.898	0.920	-0.056	0.898	0.898	0.000
Southern Europe	0.016	0.015	0.002	0.016	0.016	0.000
Northern Europe	0.014	0.012	0.013	0.014	0.014	0.000
Other EU	0.001	0.001	0.000	0.001	0.001	0.000
Northern Africa	0.011	0.012	-0.008	0.011	0.011	0.000
Middle East	0.002	0.003	-0.014	0.002	0.002	0.000
North America	0.000	0.000	-	0.000	0.000	-
Oceania	0.000	0.000	-	0.000	0.000	-
South and Central America	0.003	0.002	0.015	0.003	0.003	0.000
Asia	0.004	0.004	0.002	0.004	0.004	0.000
Labour market position						
Employed	0.388	0.481	-0.134	0.388	0.388	0.000
Self-employed	0.024	0.044	-0.080	0.024	0.024	0.000
Unemployed	0.304	0.158	0.250	0.304	0.304	0.000
Career interruption	0.002	0.002	-0.001	0.002	0.002	0.000
Exempted unemployed	0.011	0.009	0.014	0.011	0.011	0.000
Social aid	0.018	0.024	-0.028	0.018	0.018	0.000
Pension	0.005	0.003	0.028	0.005	0.005	0.000
Dependent child	0.028	0.102	-0.217	0.028	0.028	0.000
Disability	0.005	0.004	0.001	0.005	0.005	0.000
Professional illness	0.000	0.000	-	0.000	0.000	-
Handicap	0.000	0.000	0.000	0.000	0.000	0.000
Inactive	0.212	0.162	0.089	0.212	0.212	0.000
Unknown	0.004	0.009	-0.051	0.004	0.004	0.000
Observations	57,264	61,355		57,264	61,355	

Note: This table shows summary statistics of the treated and control samples in the pre-treatment quarter *k=-1,* after matching is performed, without (1st column) and with (2nd column) application of the reweighting procedure to balance the distribution of matching covariates. The treated group is composed of the overall population of women entering the SVS between 2004 and 2007, while the control group is composed of those entering the scheme between 2013 and 2015. "Other" position in the household includes individuals living in a collective household, individuals living within another household, and those who do not belong to any other category. "Unknown" position in the household and labour market position refers to individuals who were not in the Belgian national registers in the pre-treatment quarter *k=-1* (i.e., who were not living in Belgium). Standardised difference $\text{StdDiff} = \frac{\overline{X_C}-\overline{X_T}}{(\hat{\sigma}_C^2-\hat{\sigma}_T^2)^{0.5}}$ where $\bar{X}_c$ corresponds to the mean of variable X of the control group in the last quarter of 2003 and $\hat{\sigma}_C^2$ the estimated variance.

Table 4: Pre-trend Analysis

	(1)	(2)	(3)	(4)	(5)
	Employment	Unemployment	Inactivity	ST Disability	LT Disability
β: Effect of program participation k quarters relative to entry – Eq. (1)					
k=-3	0.000	0.002	0.001	-0.002	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)
k=-2	-0.003	0.001	0.001	0.002	-0.000
	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)
k=-1	-0.002	0.000	0.001	0.001	-0.000
	(0.003)	(0.003)	(0.002)	(0.001)	(0.000)
Constant	0.403***	0.277***	0.190***	0.021***	0.002***
	(0.004)	(0.005)	(0.004)	(0.001)	(0.000)
Observations	2,965,475	2,965,475	2,965,475	2,965,475	2,965,475
R-squared	0.124	0.152	0.144	0.018	0.022

Note: Results of estimation of Equation (1) for the overall population of treated and control individuals. “Employment” is defined as being salaried or self-employed at the end of a quarter. “Unemployment” is defined as receiving Unemployment Insurance benefits at the end of a quarter. “Inactivity” is defined as being outside of the labour force at the end of a quarter. “ST Disability” is defined as receiving short-term Disability Insurance benefits at the end of a quarter and “LT Disability” as receiving long-term Disability Insurance benefits at the end of a quarter. Treatment effects β^k are shown at k=-3, -2 and -1. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

Table 5: Benchmark Findings

	(1) Employment	(2) Unemployment	(3) Inactivity	(4) ST Disability	(5) LT Disability
β: Effect of program participation k quarters relative to entry – Eq. (1)					
k=4	0.226***	-0.129***	-0.087***	0.037***	-0.000
	(0.004)	(0.004)	(0.003)	(0.002)	(0.000)
k=8	0.192***	-0.107***	-0.093***	0.033***	0.011***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.001)
k=12	0.179***	-0.095***	-0.102***	0.028***	0.021***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.001)
k=16	0.167***	-0.096***	-0.102***	0.027***	0.031***
	(0.005)	(0.005)	(0.004)	(0.002)	(0.001)
k=20	0.168***	-0.108***	-0.100***	0.022***	0.040***
	(0.005)	(0.005)	(0.004)	(0.002)	(0.002)
Constant	0.403***	0.277***	0.190***	0.021***	0.002***
	(0.004)	(0.005)	(0.004)	(0.001)	(0.000)
Observations	2,965,475	2,965,475	2,965,475	2,965,475	2,965,475
R-squared	0.124	0.152	0.144	0.018	0.022

Note: Results of estimation of Equation (1) for the overall population of treated and control individuals. "Employment" is defined as being salaried or self-employed at the end of a quarter. "Unemployment" is defined as receiving Unemployment Insurance benefits at the end of a quarter. "Inactivity" is defined as being outside of the labour force at the end of a quarter. "ST Disability" is defined as receiving short-term Disability Insurance benefits at the end of a quarter and "LT Disability" as receiving long-term Disability Insurance benefits at the end of a quarter. Treatment effects β^k are shown at k=0, 4, 8, 12, 16 and 20. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

Table 6: Heterogeneous Results – Employment Inside versus Outside of the SVS

	(1) Employment inside SVS	(2) Employment outside SVS
β: Effect of program participation k quarters relative to entry – Eq. (1)		
k=4	0.519***	-0.294***
	(0.002)	(0.004)
k=8	0.459***	-0.267***
	(0.002)	(0.004)
k=12	0.431***	-0.253***
	(0.002)	(0.005)
k=16	0.405***	-0.237***
	(0.002)	(0.005)
k=20	0.380***	-0.212***
	(0.002)	(0.005)
Constant	0.000	0.403***
		(0.004)
Observations	2,965,475	2,965,475
R-squared	0.391	0.149

Note: Results of estimation of Equation (1) for the overall population of treated and control individuals, distinguishing employment effects between work in the subsidized PHS sector and work outside of the subsidized PHS sector. "Employment inside SVS" is defined as being employed at the end of a quarter and having worked in the SVS during that quarter. "Employment outside SVS" is defined as being employed at the end of a quarter and not having worked in the SVS during that quarter. Treatment effects β^k are shown at k=0, 4, 8, 12, 16 and 20. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

Table 7: Heterogeneous Effects by Status before Entry

	(1) Employment	(2) Unemployment	(3) Inactivity	(4) ST Disability	(5) LT Disability
β: Effect of program participation k quarters relative to entry – Eq. (1)					
Benchmark					
k=4	0.226***	-0.129***	-0.087***	0.037***	-0.000
	(0.004)	(0.004)	(0.003)	(0.002)	(0.000)
k=20	0.168***	-0.108***	-0.100***	0.022***	0.040***
	(0.005)	(0.005)	(0.004)	(0.002)	(0.002)
Previously working					
k=4	-0.032***	-0.009**	0.023***	0.025***	-0.002**
	(0.005)	(0.004)	(0.003)	(0.003)	(0.001)
k=20	0.027***	-0.051***	-0.010***	0.008**	0.032***
	(0.006)	(0.004)	(0.003)	(0.003)	(0.002)
Previously unemployed					
k=4	0.366***	-0.400***	0.029***	0.051***	0.003**
	(0.009)	(0.010)	(0.005)	(0.005)	(0.001)
k=20	0.236***	-0.261***	-0.021***	0.031***	0.053***
	(0.010)	(0.011)	(0.006)	(0.005)	(0.004)
Previously inactive					
k=4	0.490***	-0.013***	-0.493***	0.042***	0.000
	(0.009)	(0.003)	(0.009)	(0.002)	(0.000)
k=20	0.333***	-0.004	-0.401***	0.036***	0.043***
	(0.010)	(0.005)	(0.010)	(0.003)	(0.002)
Previously disabled					
k=4	0.305***	-0.087	-0.032	0.017	-0.111***
	(0.045)	(0.053)	(0.032)	(0.039)	(0.035)
k=20	0.167***	-0.141**	-0.062**	0.005	0.060
	(0.050)	(0.058)	(0.028)	(0.034)	(0.049)
Previously on social aid					
k=4	0.324***	-0.009	-0.004	0.036***	0.001
	(0.021)	(0.000)	(0.015)	(0.007)	(0.001)
k=20	0.190***	-0.099***	-0.010	0.023**	0.017***
	(0.022)	(0.022)	(0.016)	(0.009)	(0.005)

Note: Results of estimation of Equation (1) for the overall population of treated and control individuals by previous status before entry. “Previously working” is defined as being employed at the end of the quarter preceding entry in the program. “Previously unemployed” is defined as receiving unemployment benefits at the end of the quarter preceding entry in the program. “Previously inactive” is defined as being outside of the labour force at the end of the quarter preceding entry in the program. “Previously disabled” is defined as receiving short- or long-term Disability Insurance benefits at the end of the quarter preceding entry in the program. “Previously on social aid” is defined as receiving social aid or social integration benefits at the end of the quarter preceding entry in the program. Treatment effects β^k are shown at k=4 and 20. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

Table 8: Heterogeneous Effects on Different Types of Medical Conditions

	Osteoarticular	Psychological	Respiratory	Skin
β: Effect of program participation k quarters relative to entry – Eq. (1)				
k=4	0.0004*	-0.0004	0.0000	0.0000
	(0.0002)	(0.0003)	(0.0000)	(0.0000)
k=8	0.0044***	0.0036***	0.0003***	0.0001*
	(0.0004)	(0.0005)	(0.0001)	(0.0001)
k=12	0.0085***	0.0063***	0.0003***	0.0000
	(0.0006)	(0.0007)	(0.0001)	(0.0001)
k=16	0.0126***	0.0088***	0.0005***	0.0002**
	(0.0008)	(0.0010)	(0.0001)	(0.0001)
k=20	0.0168***	0.0116***	0.0006***	0.0003***
	(0.0009)	(0.0010)	(0.0001)	(0.0001)
Constant	0.0005***	0.0010***	0.0000	0.0000
	(0.0001)	(0.0002)	(0.0000)	(0.0000)

Note: Results of estimation of Equation (1) for the overall population of treated and control individuals, for different types of long-term disabilities that are associated with domestic work. Osteoarticular illnesses comprise conditions like arthrosis, joint inflammation and related musculoskeletal issues. Psychological illnesses cover all mental health issues. Respiratory illnesses cover all disorders that affect an individual's breathing. Skin disabilities encompass all dermatological disorders that are serious enough to cause disability. Treatment effects β^k are shown at k=0, 4, 8, 12, 16 and 20. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

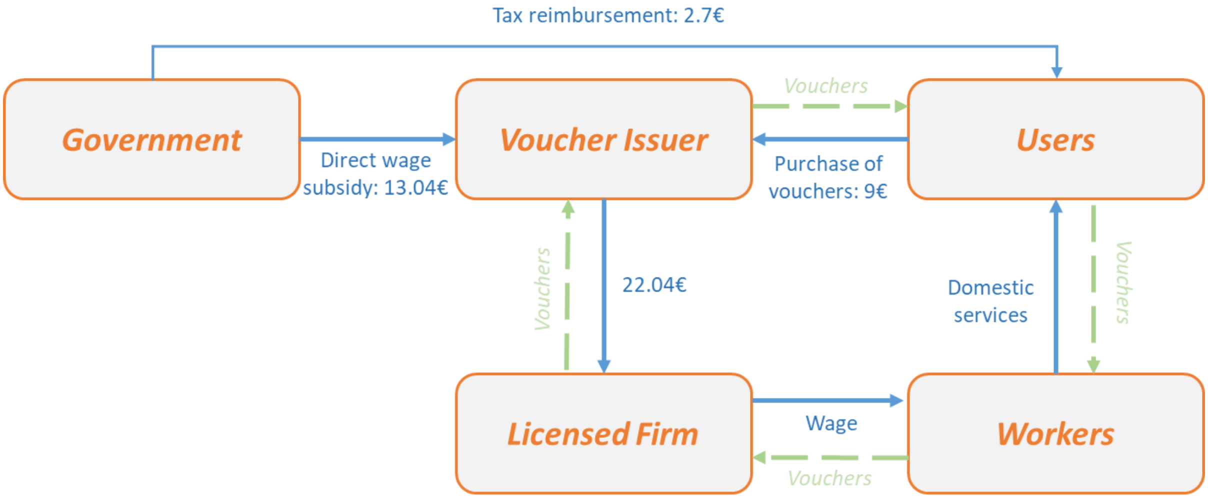
Table 9: Robustness Tests

		(1) Benchmark	(2) Shorter time laps between program entry dates	(3) CEM w/o nationality	(4) CEM w/o labour market history
β: Effect of program participation k quarters relative to entry – Eq. (1)					
Employment	*k=4*	0.226***	0.296***	0.238***	0.221***
		(0.004)	(0.004)	(0.004)	(0.004)
	k=20	0.168***		0.177***	0.157***
		(0.005)		(0.005)	(0.005)
Unemployment	*k=4*	-0.129***	-0.193***	-0.123***	-0.129***
		(0.004)	(0.003)	(0.003)	(0.003)
	k=20	-0.108***		-0.105***	-0.108***
		(0.005)		(0.004)	(0.004)
Inactivity	*k=4*	-0.087***	-0.094***	-0.105***	-0.072***
		(0.003)	(0.002)	(0.003)	(0.003)
	k=20	-0.100***		-0.111***	-0.085***
		(0.004)		(0.004)	(0.003)
ST Disability	*k=4*	0.037***	0.031***	0.038***	0.035***
		(0.002)	(0.002)	(0.002)	(0.002)
	k=20	0.022***		0.022***	0.015***
		(0.002)		(0.002)	(0.002)
LT Disability	*k=4*	-0.000	0.001	0.000	-0.002***
		(0.001)	(0.000)	(0.000)	(0.001)
	k=20	0.040***		0.038***	0.043***
		(0.002)		(0.001)	(0.001)
Matching rate		61.2%	67.0%	70.0%	89.5%

Note: Sensitivity of benchmark results (column (1)) to a series of robustness checks. Column (2) shows estimation results when treated individuals are matched with a comparable control who enters exactly two years after the treated does in order to reduce time lapses between entry dates of matched individuals. Column (3) shows estimation results when nationality isn't used as a matching covariate, reducing the number of trimmed foreign SVS workers in our analysis. Column (4) shows estimation results when labour market history isn't used as a matching covariate, which increases the matching rate and, therefore, external validity. All regressions are weighted by CEM weights and include time fixed effects. Standard errors in parentheses are clustered at the individual level. *** p<0.01 ** p<0.05 * p<0.1

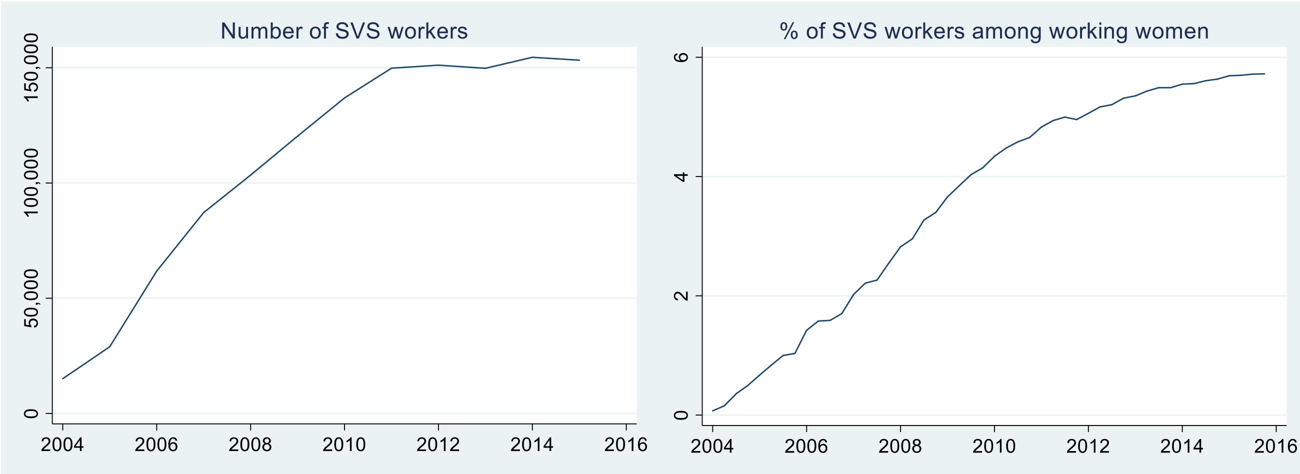
Figures

Figure 1 : Organisation of the Service Voucher Scheme



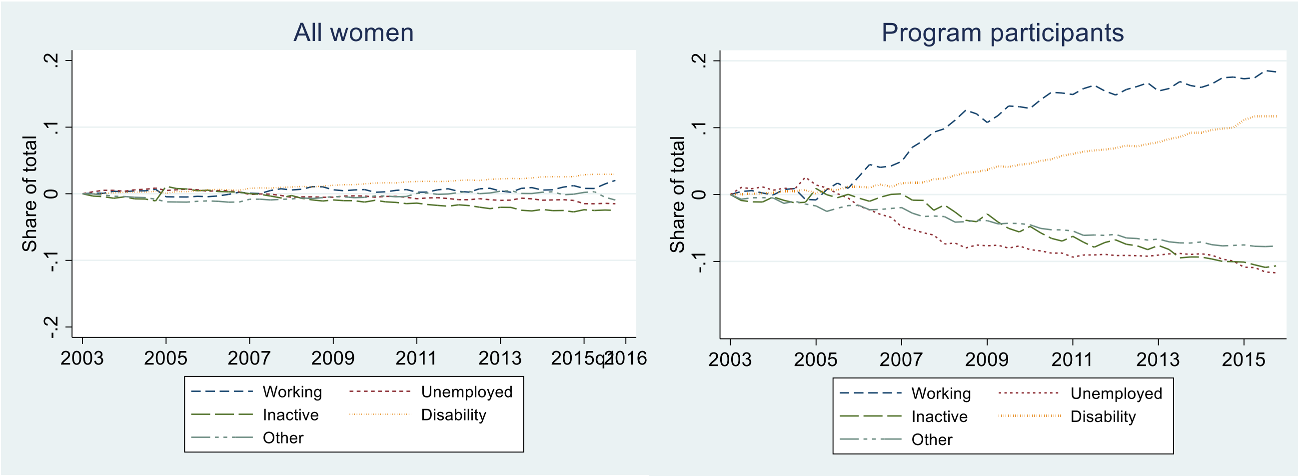
Note: The Belgian Service Voucher Scheme is composed of 5 actors. The voucher issuer is a private voucher company (under a procurement contract) that produces the service vouchers. It sells them to consumers for 9€/piece, who can use the vouchers to pay for domestic services. Users are also entitled to a fiscal deduction of 2.7€/piece, so the net cost of each voucher for the user is 6.3€/piece. For each hour of domestic service performed, the workers receive one voucher from the users. They then give them to their employer – a licensed SVS firm – in exchange for their monthly wage. The licensed firms can then give all the vouchers from its workers to the Voucher issuer in exchange for 22.04€/piece which serve – at least in part – to pay for the workers' wage. The difference between what the users pay for each voucher – 9€ – and what the SVS firms are reimbursed for each voucher – 22.04€ – is financed through a direct wage subsidy of 13.04€/voucher.

Figure 2: Growth of the Service Voucher Scheme



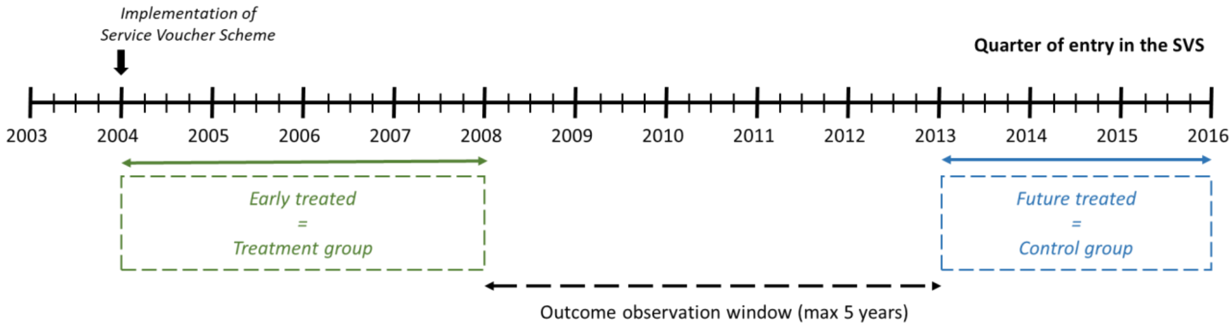
Note: These graphs show how the Service Voucher Scheme gradually became an important employer of female workers in Belgium. The number of SVS workers grew ten-fold in less than ten years, from about 15,000 in 2004 to more than 150,000 as of 2011. In 2015, nearly 6% of all working women in Belgium were employed under the Service Voucher Scheme.

Figure 3: Labour Market Participation – All Women versus Program Participants



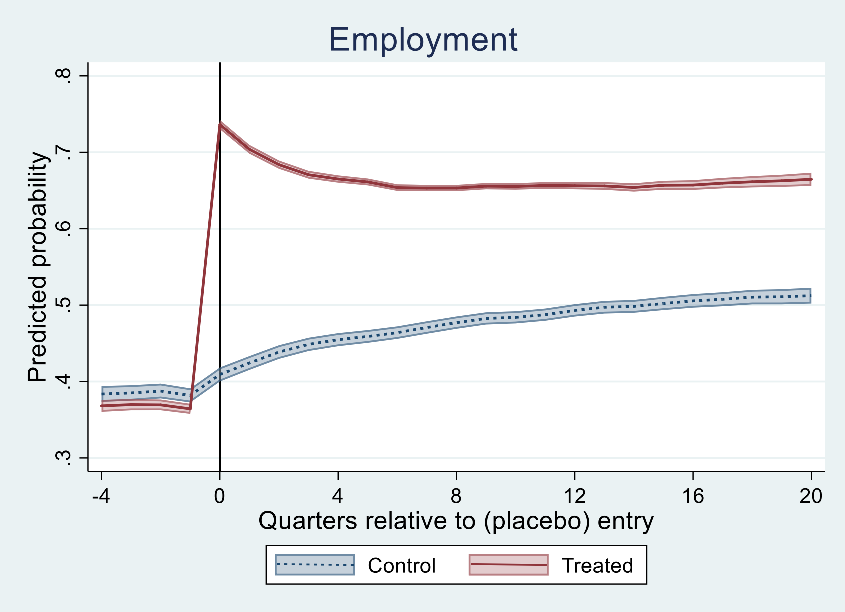
Note: Random sample of 10% of the population of women aged between 18 and 65. These graphs illustrate the evolution of women's (left panel) and program participants' (right panel) position on the labour market relative their base level in the first quarter of 2003. "Working" is defined as being salaried or self-employed at the end of a quarter. Being "unemployed" is defined as receiving Unemployment Insurance benefits at the end of a quarter. Being "inactive" is defined as being outside of the labour force at the end of a quarter. "Disability" is defined as receiving short- or long-term Disability Insurance benefits at the end of a quarter. "Other" is defined as being neither working, unemployed, inactive or in disability.

Figure 4: Treatment Assignment Mechanism



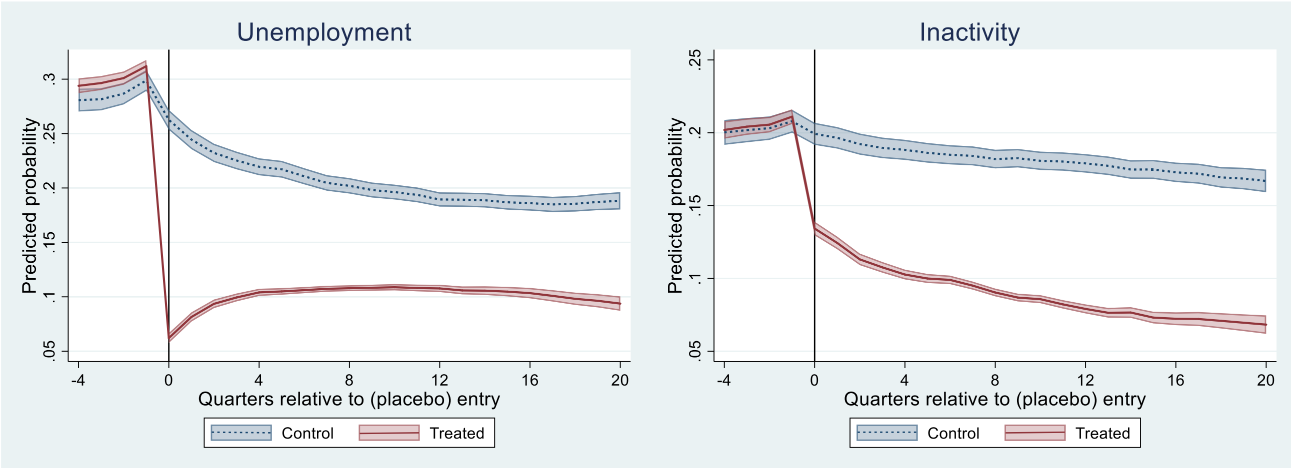
Note: Diagram illustrating the assignment mechanism to the treatment and control samples. The SVS was implemented in 2004. All women who enter the SVS between its implementation – in the first quarter of 2004 – and the last quarter of 2007 are part of the treatment sample. All those who enter the SVS between the first quarter of 2013 and the last quarter of 2015 are part of the control sample. This allows a comparison for a period of five years, after which some of the controls might start entering the scheme (e.g., in the case where an individual entering in 2007q4 is matched with an individual entering in 2013q1).

Figure 5: Graphical Evidence – Employment



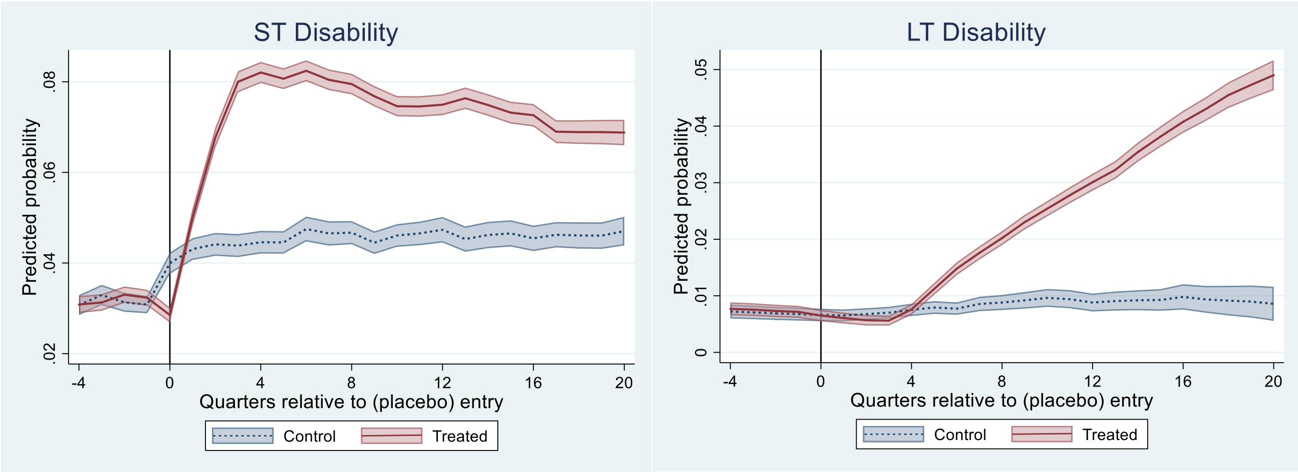
Note: Graphical representation of the estimated probability (95% confidence intervals) of being employed using Equation (1) for the matched treated and control groups around the (placebo) date of entry in the scheme. “Employment” is defined as being salaried or self-employed at the end of a quarter.

Figure 6: Graphical Evidence – Unemployment and Inactivity



Note: Graphical representation of the estimated probability (95% confidence intervals) of being unemployed (left) or inactive (right) using Equation (1) for the matched treated and control groups around the (placebo) date of entry in the scheme. "Unemployment" is defined as receiving Unemployment Insurance benefits at the end of a quarter. "Inactivity" is defined as being outside of the labour force at the end of a quarter.

Figure 7: Graphical Evidence – Disability



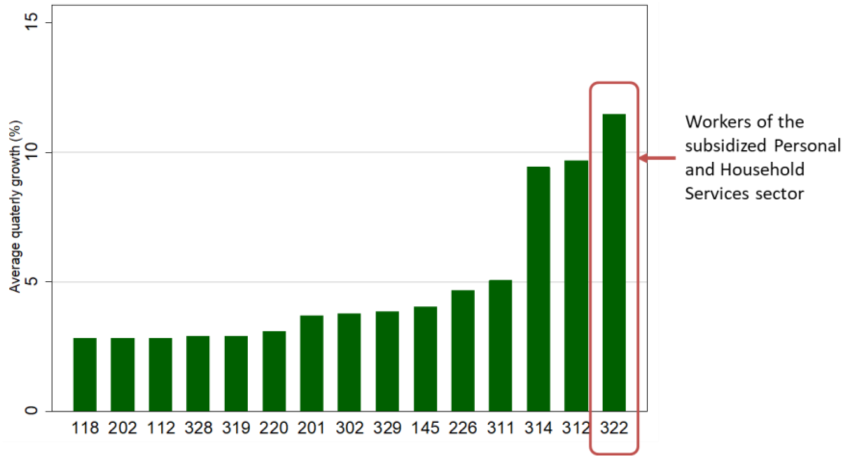
Note: Graphical representation of the estimated probability (95% confidence intervals) of claiming short- or long-term Disability Insurance benefits using Equation (1) for the matched treated and control groups around the (placebo) date of entry in the scheme. "ST Disability" is defined as receiving short-term Disability Insurance benefits at the end of a quarter and "LT Disability" as receiving long-term Disability Insurance benefits at the end of a quarter.

Appendix

Appendix A: The Service Voucher Scheme and the Rise in Disability

The Joint Committee[43] number 322, which includes workers of the SVS, is the one that has experienced the single highest growth of disability rate between 2003 and 2014 in Belgium. This further supports the importance of exploring the effects of subsidizing Personal and Household Services on program participants' probability of claiming DI benefits (as participation in the scheme may *cause* more worker to become disabled).

Joint Committees with the Highest Growth of Disability Rates (2003-2014)



[43] In Belgium, Joint Committees are permanent bodies that are created (in different branches of activity) and that serve as a negotiation platform between workers and employers who perform similar activities to determine general working conditions in the sector.

Appendix B: Previous Labour Market Status Used in Matching Procedure

Coarsened value	**Detailed socio-economic position**
Employed	Occupied in a single salaried job Occupied in several salaried jobs Occupied in a salaried job and as a self-employed or support of a self-employed – the main job is in a salaried job
Self-employed	Occupied as a self-employed (main job) Occupied as a self-employed (complementary job) Occupied as support of a self-employed (main job) Occupied as support of a self-employed (complementary job) Occupied in a salaried job and as a self-employed or support of a self-employed – the main job is as a self-employed Occupied in a salaried job and as a self-employed or support of a self-employed – the main job is as support of a self-employed
Unemployed	Jobseeker after full-time work with UI benefits Jobseeker after voluntary part-time work with UI benefits Jobseeker after studies with intermediary allowance or transition allowance Jobseeker with support allowance
Career interruption	Full-time career interruption / « crédit-temps »
Exempted jobseeker	Exemption from registration as a jobseeker
Social aid	Integration income Financial aid
Pension	Beneficiary of a pension w/o work
Dependent child	Child beneficiary of family allowances
ST or LT Disability	Primary Incapacity (<1 year) Long-term disability (>1 year)
Professional illness or accident	Work incapacity because of an occupational disease Work incapacity because of a work-related accident
Handicap	Beneficiary of an allocation for handicapped people
Inactive	Other
Unknown	Not observed in official national register