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## Health Effects of Labor Market Policies: Evidence from Drug Prescriptions

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# Health Effects of Labor Market Policies: Evidence from Drug Prescriptions

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Comprehensive evidence has shown that unemployment can have adverse effects on an individuals' mental and physical health condition. However little is known about the side effects of governmental policies that aim to promote the unemployed's reintegration into the labor market on the health status of participants. On the one hand, these interventions could have positive health effects if they increase the chances of finding new employment, however, on the other hand, they might also have unintended consequences as participation per se might be stressful. We analyze the consequences of two policies: 1) training programs that represent a group of supportive measures (carrots) and 2) sanctions as a very restrictive intervention (sticks) for on an inflow sample into unemployment in 2006/2007 and utilize a combination of Swedish administrative data on labor market and prescription drug records. We apply a dynamic difference-in-difference approach accounting for detailed information on socio-demographics, employment biographies and the individual prescription drug history to estimate the impact on three common types of health problems that can be assumed to show up and to be diagnosed quickly after a potential intervention: cardiovascular problems, mental health issues and infections. Our results show positive health effects for training programs, but no effect for sanctions.

**Keywords:** Unemployment, Active labor market policy, Health Effects

**JEL codes:** J68, I12, I18, H51

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# 1 Introduction

The existing literature on labor market policies for unemployed workers typically distinguish between policies with a supportive nature (“carrots”), such as training programs and job search assistance, and policies that constrain individual behavior (“sticks”), such as benefit sanctions and workfare (Arni et al., 2017). Previous studies show that training programs (carrots) tend to have favorable effects on earnings and other employment outcomes in the long-run, possibly due to human capital accumulation (see e.g. Lechner et al., 2011; Card et al., 2017), while activation in the form of monitoring and sanctions (sticks) increase re-employment rates (see e.g. van den Berg and Van der Klaauw, 2006; van den Berg et al., 2004; Lalive et al., 2005), but also lead to lower wages and reduced job stability (Arni et al., 2013; van den Berg and Vikström, 2014). These and related employment and earnings effects have been extensively evaluated in the past (see Card et al., 2010, for an overview), but very little is known about the side effects of carrots and sticks policies on the participants health status. This paper fills this gap in the literature by providing first comprehensive evidence on the health effects of these different labor market policies.

There are several reasons why labor market policies can affect health outcomes. First of all, evidence from the medical, psychological and economic literature, indicates that unemployment has adverse effects on individuals’ mental and physical health (see e.g., discussions in Dooley et al., 1996; Catalano et al., 2000; McKee-Ryan et al., 2005). As discussed by Bartley (1994), potential explanations for the persistent relationship between individual unemployment and the personal health status are related to the reduced financial means that individuals have during unemployment, social isolation and loss of self-esteem, health-related behavior, as well as reduced subsequent reemployment prospects. Therefore, both carrots and sticks policies may have favorable health effects if they increase the chances of finding employment. Moreover, supportive policies that emphasize human capital accumulation may have additional positive effects on the individual health status if they affect the participants’ perception of their own situation or change their daily routines. This is related to a small strand of literature that has analyzed the relationship between labor market policies and subjective well-being. The findings indicate

that participation in training (see Creed et al., 1998; Machin and Creed, 2003; Andersen, 2008) or work practice (see Oddy et al., 1984; Strandh, 2001; Crost, 2016) programs leads to higher levels of life satisfaction (relative to being unemployed). However, despite concerns about the causal interpretation of these findings, it remains unclear whether this indeed translates into an improvement of participants health status. On the other hand, benefit sanctions and intensified monitoring may lead to increased social and economic stress, possibly associated with negative health effects.<sup>1</sup> Lower wages and reduced job stability may also have negative health consequences.

In this paper, we study health effects of three different types of labor market policies: 1) a training program that represents a supportive policy and 2) benefit sanctions as an restrictive activation policy. In the empirical analyses, we use rich administrative data from Sweden. In particular, we use detailed information from prescription drug records to construct various health outcomes. Based on the Anatomical Therapeutic Chemical (ATC) Classification System, we identify three common types of health problems that can be assumed to show up and to be diagnosed quickly after a potential intervention: cardiovascular problems, mental health issues and infections. All three are also expected to be affected by any increased stress due to program participation, and allows us to measure health effects both shortly after the policy as well as more long-run health responses.

The main empirical problem is that the propensity to participate in training/work practice and to experience a benefit sanction may be correlated with individual health. We therefore adjust for a rich set of characteristics that are typically used when evaluating labor market policies, i.e. like socio-demographics and employment histories (see Lechner and Wunsch, 2013). We also use the panel dimension of our data and control for the prescription drug history of each individual, utilizing the fact that drug prescriptions reflect a wide range of severe and less severe health problems. We also explicitly acknowledge that the treatment may start after any elapsed unemployment duration. In order to address this dynamic treatment assignment, we apply a dynamic approach, estimating the effect of starting a treatment in month  $t$  versus remaining unemployed (see Sianesi, 2004).

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<sup>1</sup>This is supported by findings from Ochsen and Welsch (2012) showing that more generous unemployment benefit schemes are associated with higher levels of subjective well-being.

Our main results are that participating in a training program reduces the probability to have a prescription for drugs that are related to cardiovascular and mental health problems between 0.4 and 0.6 percentage points within a year after the program start. For benefit sanctions we find no evidence of any health effects. From this we conclude that supportive measures (carrots) in the form of training have positive side effects on the participants health status, while purely restrictive interventions (sticks) in the form of benefit sanctions have no health effects on average.

To understand the mechanisms behind these effects, we relate our findings to estimates of the effect on employment outcomes and perform a comprehensive subgroup analysis. The results show that the positive health effects of training programs are realized even before participants leave the program and reenter the labor market, which implies that the treatment itself has a direct effect on the health status, while the indirect effect through improved employment prospects seems to be negligible in the short-run. This interpretation is supported by the fact that the positive health effects are driven by individuals with a low level of education who might be particularly at risk of lacking daily routines during unemployment, which could be compensated by the treatment. Moreover, we observe a substantial increase in drug prescriptions in the month prior to the program start, which seem to reflect adjustments of the individual behavior when preparing for the treatment rather than real health effects.

The remainder of the paper is organized as follows. Section 2 describes the Swedish administrative data, Section 3 discusses the identification strategy. Section 4 shows the main estimation results, while Section 5 discusses the relevance of different mechanisms and Section 6 concludes.

## **2 Institutional Background, Data and Descriptives**

### **2.1 Active Labor Market Policies in Sweden**

Sweden has a long tradition of ALMP programs geared towards helping unemployed individuals and encouraging them to start new employment. For our analysis, we focus on three types of active labor market policies that generally represent different reintegration

tion strategies. First, training programs aim to improve the skills of the unemployed and thereby enhance their reemployment prospects. For the purpose of our study we focus on vocational training courses, which are provided by education companies, universities, and municipal consultancy operations. The local employment office or the county employment board pay these organizations for the provision of courses. The contents of the courses should be directed towards the upgrading of skills or the acquisition of skills that are in short supply or that are expected to be in short supply. The most common courses involve manufacturing (11.6% of participants), machine operators (9.8%), office/warehouse work (15.1%), health care (6.1%) and computer skills (15.1%). Training programs typically last for around six months, but can continue upon request of the training provider. During the treatment, participants receive a training grant. Individuals who are entitled to unemployment insurance (UI) receive a grant equal to their UI benefits level, and for those not entitled to UI the grant is lower fixed at a certain amount. In all cases, training is free of charge. As shown by Richardson and van den Berg (2013) participants experience a large, positive and significant employment effect shortly after the treatment.

Second, sanctions are benefits reduction for a limited period of time that are imposed if the unemployed's search behavior is not in accordance with the UI guidelines, i.e. rejects suitable employment offers.<sup>2</sup> The monitoring of an unemployed individual is carried out by the caseworker of the PES office. The caseworker is the only person who can take the initiative to give a sanction. For first-time offenders, a sanction is a 25% benefits reduction for a period of 40 days. For second-time offenders, the sanction is 50% for 40 days. A third violation during the same UI entitlement period entails a full loss of benefits until new employment has been found. The caseworker is also supposed to verify during the course of an unemployment spell that the unemployed individual does not violate the UI entitlement conditions in the first place. This concerns, for example, unreported employment. If the individual is deemed non-eligible then he is not registered anymore as being unemployed. Moreover, his UI benefits payment is terminated for an indefinite

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<sup>2</sup>In addition to this, UI benefits can be reduced upon inflow into unemployment, if the individual has not registered physically at the employment office. We do not analyze this type of benefit reduction (imposed within the first 14 days of the unemployment spell) since this might be a direct consequence of previous health issues.

period of time.<sup>3</sup> Previous results have shown that sanctions in Sweden encourage job seekers to accept lower wages and work on lower occupational level (see van den Berg and Vikström, 2014). We only consider the first sanction an individual receives during the unemployment spell since subsequent sanctions can be considered as an outcome of the first sanction.

## 2.2 Data

Our study is based Swedish administrative records combining different data sources. First, the population register (Louise) provides yearly information on the entire Swedish population in age 16-64. It contains a rich set of socio-economic and background variables (e.g. age, sex, income, immigration status, marital status, employment status and social insurance benefits). The second register, called Händel (received from the employment offices), covers all registered unemployed persons. It contains day-by-day information on the unemployment status. This includes UI eligibility, entries into and exits from active labor market programs, and the reason for the unemployment spell to end (as a rule, this is re-employment, but occasionally it is a transition into education, social assistance, or other insurance schemes). Händel also includes a number of personal characteristics recorded at the beginning of the unemployment spell. Third, ASTAT (provided by the unemployment insurance fund), collects information on sanctions, including the timing, the main reason, and the size of the benefit reduction. Finally, we also exploit administrative data on the universe of individual prescription drug usage including the medication and the date of the prescription.

For the empirical analysis, we consider all inflows into unemployment between January 2006 and December 2007. We randomly draw one entry if an individual enters unemployment several times during the observation period. Moreover, exclude all individuals who have been registered at the PES within the last six months. The latter ensures that we only consider fresh entries into unemployment (no returnees from ALMP or periods of sickness, etc.) making the assumption that the selected individuals indeed search for em-

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<sup>3</sup>In addition, eligibility is terminated if the individual sabotages cooperation with the employment office, for example by refusing to follow: contributing an individualized pathway back to work or participation in active labor market programs.

ployment plausible. Finally, we also exclude individuals who have been treated within the first 14 days of the unemployment spell as it is likely that the treatment assignment has been justified by a previous unemployment spell. Our final estimation sample includes 8,083 individuals participated in a training program, 3,088 that have been sanctioned and 9,929 participated in a work practice program within the first year of their unemployment spell.

## 2.3 Classification of Prescriptions and Health Outcomes

The prescription data are based on the Anatomical Therapeutic Chemical (ATC) Classification System that is used for the classification of active ingredients of drugs according to the organ or system on which they act and their therapeutic, pharmacological and chemical properties. It is controlled by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOC), and was first published in 1976. This pharmaceutical coding system divides drugs into different groups according to the organ or system on which they act and/or their therapeutic and chemical characteristics. Each bottom-level ATC code stands for a pharmaceutically used substance, or a combination of substances, in a single indication (or use).<sup>4</sup>

To test the health effects of labor market programs, we consider three types of health outcomes that are assumed to show up and to be diagnosed quickly after a potential treatment to reduce the impact of reverse causality. Moreover, the health problem should be relatively common among typical unemployed individuals in order to have some statistical power, but it should not be overwhelmingly common due to other causes that affect treated and controls alike. Therefore, we define three health outcomes of interest. First, problems related to *cardiovascular system* (ATC Code C) include, e.g., high blood pressure or heart attacks. Among the outcomes of interest, these problems are least common. Only about 4% of the sample had a prescription related to the cardiovascular system within the last 6 months before the entry into unemployment, but somatic effects of unemployment are

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<sup>4</sup>This means that one drug can have more than one code: acetylsalicylic acid (aspirin), for example, has A01AD05 as a drug for local oral treatment, B01AC06 as a platelet inhibitor, and N02BA01 as an analgesic and antipyretic. On the other hand, several different brands share the same code if they have the same active substance and indications.



often assumed to be manifested in the cardiovascular system (see e.g. Mattiasson et al., 1990; Janlert et al., 1992; Bartley, 1994). Second, we also consider *mental health problems* (ATC Code N05 and N06) that might be related to stress or depressions. The literature has widely investigated the relationship between unemployment and mental health issues. While, for instance, Iversen and Sabroe (1988), Clark and Oswald (1994) and Maier et al. (2006) find a negative impact of unemployment on psychological well-being, respectively mental health, Browning et al. (2006) and Salm (2009) find no evidence for an effect of job loss on hospitalization for stress-related diseases, respectively mental health problems. About 9.5% of all individuals had a prescription related to mental health issues in the last 6 months. Finally, prescriptions against *infections* (ATC Code J01) are the most common type of medication, about 13% had a prescription within six months before becoming unemployed. Antibiotics (abbreviated as AB) are they key medication against bacterial infections. Other types of infections are due to viruses (but such infections are extremely common and they are usually not treated at all) or funghi and so on (but those are very uncommon). It has been shown that unemployment (see e.g. Cohen, 1999) and job insecurity (see e.g. Mohren et al., 2003) are positively related to the higher risk for a wide range of infectious diseases.

## 2.4 Differences in Observed Characteristics

Table 1 presents descriptive statistics for the observed characteristics with respect to socio-demographics, labor market histories and prior prescriptions before entering unemployment. It can be seen that there substantial significant differences for a variety of variables. For instance participants in training programs are more often men, are more likely to be between 25 and 44 years, are less often Swedish citizens and are less likely to hold an university degree compared to the control group. Moreover, they tend do have more often young children, more unemployment experience and had higher earnings in the past. Finally, they also had fewer prescriptions (with respect to the most of top-level ATC categories) before entering unemployment. Individuals who receiving sanctions are slightly more often male, are substantially older, less likely to be Swedish citizens and are more often married relative to those not participating in any program. Regarding their la-

Table 1: Selected Differences in Baseline Characteristics and Prescription Histories

	Non-treated	Treated			
		Training	<i>P</i> -value	Sanctions	<i>P</i> -value
No. of observations	357,864	7725		2898	
<b>Baseline characteristics</b>					
Female	0.5258	0.3366	0.0000	0.4752	0.0000
Age					
20-24 years	0.2471	0.2703	0.0000	0.2098	0.0000
25-34 years	0.3424	0.3506	0.1346	0.3161	0.0030
35-44 years	0.2307	0.2359	0.2913	0.2315	0.9194
45-54 years	0.1314	0.1177	0.0004	0.1718	0.0000
55-60 years	0.0484	0.0256	0.0000	0.0707	0.0000
Married	0.3142	0.3028	0.0326	0.2995	0.0902
Swedish citizen	0.3743	0.3208	0.0000	0.3385	0.0001
Educational level					
Compulsory school	0.2186	0.2230	0.3503	0.2067	0.1223
Upper secondary school	0.4648	0.5441	0.0000	0.5290	0.0000
Higher education	0.3166	0.2329	0.0000	0.2643	0.0000
Children age 0-6					
One child	0.1562	0.1566	0.9127	0.1429	0.0491
Two or more children	0.0867	0.0980	0.0005	0.0859	0.8749
Local unemployment rate	0.0568	0.0601	0.0000	0.0560	0.0006
Days in unemployment in year					
t-1	31.0409	32.3935	0.0824	35.4258	0.0005
t-2	45.5469	52.1526	0.0000	54.5649	0.0000
t-3	40.4851	44.7361	0.0000	47.5717	0.0000
Eligible for UI	0.7197	0.7336	0.0073	0.9917	0.0000
Wider job search	0.2406	0.3420	0.0000	0.3778	0.0000
Registered as disabled	0.0838	0.1012	0.0000	0.0683	0.0027
Yearly labor income in SEK in year					
t-1	83,824	85,733	0.1401	126,115	0.0000
t-2	79,644	79,512	0.9156	116,053	0.0000
t-3	77,702	78,010	0.8014	106,795	0.0000
<b>Prescription history within six months before entry into unemployment</b>					
<i>Pre-treatment outcomes</i>					
Cardiovascular system	0.0402	0.0353	0.0303	0.0400	0.9565
Mental health problems	0.1037	0.0955	0.0195	0.1063	0.6524
Infections	0.1296	0.1192	0.0073	0.1391	0.1304
<i>Other prescriptions<sup>(a)</sup></i>					
Alimentary tract and metabolism	0.0755	0.0660	0.0017	0.0732	0.6307
Blood and blood forming organs	0.0233	0.0180	0.0021	0.0235	0.9556
Genito-urinary system and sex hormones	0.0981	0.0778	0.0000	0.0897	0.1289
Systematic hormonal preparations	0.0275	0.0220	0.0032	0.0252	0.4420
Antiinfectives for systematic use	0.0188	0.0150	0.0142	0.0176	0.6237
Antineoplastic and immunomodulating agents	0.0054	0.0045	0.2937	0.0031	0.0910
Antiparasitic products, insecticides and repellents	0.0140	0.0102	0.0049	0.0121	0.3749
Respiratory system	0.1040	0.0928	0.0014	0.1008	0.5649
Sensory organs	0.0342	0.0321	0.3062	0.0259	0.0136
Number of prescriptions in total	2.4417	2.0601	0.0000	2.2605	0.1108

*Note:* Shares unless otherwise indicated, *p*-values refer to two-tailed t-tests based on equal means.

*Additional covariates included in analysis:* Children age 7-17; Month of entry into unemployment; Region or origins; UI/welfare benefits in year t-1, t-2 and t-3.

<sup>(a)</sup> Categorization of other prescriptions refers to 16 main ATC groups including also dermatologicals, musculo-skeletal system, nervous system and various.

bor market histories, they have more unemployment experience and earned substantially higher income during the last three years, while they are similar to the control group with respect to the number of prescriptions before the unemployment spell.

## 3 Empirical Strategy

### 3.1 Dynamic Difference-in-Difference Approach

The aim of our empirical analysis is to identify the causal effect of training programs and sanctions on the probability to have a prescription with respect to one of the three health issues discussed in Section 2.3 (cardiovascular issues, mental health problems and infections) and to call in sick in the unemployment insurance system. Therefore we apply a dynamic difference-in-difference strategy controlling socio-demographic characteristics, labor market histories and previous prescriptions between non-treated and treated individuals. Following Sianesi (2004), we estimate the effect of  $D_{it}$  indicating the start of the treatment in month  $t$  versus not starting the treatment (and remaining unemployed) for each month of the elapsed unemployment duration  $t = 1, \dots, 12$ . We use this dynamic setting in order to avoid a bias that might arise from a static approach that compares outcomes of treated and non-treated without taking into account the timing of the treatment (see Fredriksson and Johansson, 2008). This is particularly important in the context of health outcomes, since previous studies have shown that individuals with health issues are less likely to leave unemployment (see e.g. Lindholm et al., 2001; Stewart, 2001; García-Gómez et al., 2010; Rosholm and Andersen, 2010) and therefore the relationship between the selection into the treatment and the health status of an individual might vary substantially with respect to the elapsed unemployment duration.

For each  $t$ , the main outcome variable of interest  $Y_{it}$  is an indicator whether the individual has valid prescription or called in sick within the first three, six or twelve months following the potential treatment in month  $t$ . Column 1 and 2 of Table 2 show the distribution of treated and non-treated individuals for each month  $t$  separated for the two labor market policies. However, since there might be differences between treated and non-treated with respect to the probability of already having a prescription before the be-

ginning of the treatment, we combine the dynamic selection-on-observables strategy with a difference-in-difference (DID) approach, using the difference between our outcome variables of interest and their pre-treatment levels in the reference period before the potential treatment in month  $t$ . Since potential participants in training programs are typically informed about the program participation several weeks in advance and individuals who face the risk of being sanctioned receive a warning about one month before the imposition of the sanction, we decided to use the time period six ( $t - 6$ ) to two ( $t - 2$ ) months before the treatment as our reference period.<sup>5</sup> The month immediately prior to the treatment is considered as an additional outcome variable, since we expect that individuals might already adjust their behavior in reaction to the information about the upcoming treatment. RM: Mention some papers on anticipation effects of labor market policies? Applying this DID approach allows us to explicitly account for time-constant unobserved heterogeneity by directly comparing individual prescriptions between the pre- and post-treatment period. It should be noted that this strategy particularly increases the validity of our results compared to typical evaluation studies focusing on labor market outcomes.<sup>6</sup>

Assuming that there is only selection into the treatment based on observed characteristics, we can identify the causal effect of a participation for treated individuals in month  $t$ :

$$\delta_t = E[\Delta Y_{it}^1 | D_{it} = 1, X_i = x] - E[\Delta Y_{it}^0 | D_{it} = 1, X_i = x],$$

where  $E[\Delta Y_{it}^1 | D_{it} = 1]$  describes the difference with respect to the outcome variable between the pre- and post-treatment period and can be observed in the data, while the expected counterfactual outcome  $E[\Delta Y_{it}^0 | D_{it} = 1]$  for treated individuals without participating can be only inferred if the conditional independence assumption (CIA), respectively in our case its dynamic counterpart, holds. We have several arguments supporting the plausibility of the CIA. First of all, the selection-on-observable identification strategy is motivated by the large set of background characteristics provided by the Swedish administrative records, including socio-demographics and Labor market histories that have

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<sup>5</sup>Note that it is not possible to increase the pre-treatment reference period since prescription drug records are only available from July 2005 onwards.

<sup>6</sup>Note that when estimating treatment effects on labor market outcomes considering an inflow sample with newly unemployed workers would not allow a comparison of the pre- and post-treatment period since employment and earnings in the pre-treatment period would be pre-determined.

Table 2: Descriptive Statistics of the Propensity Score

A. Training	No. of observations		Propensity score			Pseudo- $R^2$		Mean standardized bias <sup>(a)</sup>	
	Non-treated	Treated	Mean	Standard deviation	Minimum	Maximum		before matching	after matching
Elapsed unemployment duration									
1 month	365,281	308	0.0008	0.0008	$6.9 \times 10^{-10}$	0.0287	0.0445	9.4893	4.1600
2 months	341,116	895	0.0026	0.0024	$1.9 \times 10^{-05}$	0.2010	0.0485	9.2060	2.2567
3 months	294,585	1,109	0.0038	0.0033	$9.3 \times 10^{-05}$	0.0913	0.0453	8.8788	2.3090
4 months	244,489	1,096	0.0045	0.0033	$7.7 \times 10^{-10}$	0.1168	0.0383	8.9874	1.8501
5 months	209,956	808	0.0038	0.0034	$2.7 \times 10^{-09}$	0.0509	0.0478	9.2679	2.1639
6 months	186,494	721	0.0039	0.0032	$1.4 \times 10^{-05}$	0.0524	0.0441	8.9469	3.0644
7 months	167,691	625	0.0037	0.0031	$6.6 \times 10^{-09}$	0.1857	0.0451	8.5191	2.4353
8 months	152,413	568	0.0037	0.0028	$1.7 \times 10^{-09}$	0.0768	0.0371	7.4457	3.0957
9 months	138,187	467	0.0034	0.0031	$1.4 \times 10^{-09}$	0.0495	0.0501	9.5297	2.9427
10 months	127,087	416	0.0033	0.0028	$2.4 \times 10^{-12}$	0.0778	0.0450	8.2850	3.9400
11 months	117,302	410	0.0035	0.0034	$1.5 \times 10^{-09}$	0.1064	0.0554	10.0166	3.4857
12 months	108,699	302	0.0028	0.0027	$5.3 \times 10^{-09}$	0.0444	0.0535	9.4637	4.6057
B. Sanctions									
	No. of observations		Propensity score			Pseudo- $R^2$		Mean standardized bias <sup>(a)</sup>	
	Non-treated	Treated	Mean	Standard deviation	Minimum	Maximum		before matching	after matching
Elapsed unemployment duration									
1 month	360,541	221	0.0006	0.0008	$1.5 \times 10^{-12}$	0.1712	0.0653	12.8827	4.3513
2 months	336,819	452	0.0013	0.0015	$4.1 \times 10^{-13}$	0.1233	0.0710	13.1583	3.2860
3 months	291,004	393	0.0013	0.0016	$1.8 \times 10^{-16}$	0.3325	0.0737	14.0590	3.8785
4 months	241,669	335	0.0014	0.0015	$5.7 \times 10^{-11}$	0.1632	0.0701	12.4269	3.9740
5 months	207,700	244	0.0012	0.0014	$2.6 \times 10^{-17}$	0.2673	0.0754	12.5852	5.0130
6 months	184,713	246	0.0013	0.0013	$3.4 \times 10^{-16}$	0.2097	0.0659	11.7574	4.5508
7 months	166,331	204	0.0012	0.0015	$2.2 \times 10^{-16}$	0.1794	0.0773	13.1718	4.9735
8 months	151,436	185	0.0012	0.0016	$2.5 \times 10^{-21}$	0.1173	0.0926	13.6713	5.5980
9 months	137,504	173	0.0013	0.0017	$1.1 \times 10^{-15}$	0.2485	0.0787	12.2771	4.6195
10 months	126,657	163	0.0013	0.0014	$7.1 \times 10^{-16}$	0.0469	0.0688	12.1056	5.2821
11 months	117,123	159	0.0014	0.0019	$2.4 \times 10^{-17}$	0.1782	0.0917	15.4822	6.0734
12 months	108,699	123	0.0011	0.0016	$2.1 \times 10^{-14}$	0.0387	0.0981	15.7171	5.3112

Note: Depicted are summary statistics for the estimated propensity score based on logit models separated for each month of the elapsed unemployment duration.

<sup>(a)</sup>Standardized bias for variable  $x$  is defined as:  $SB(x) = 100(\bar{x}_c - \bar{x}_t) / \sqrt{\frac{1}{2}(s_{xc}^2 + s_{xt}^2)}$ , with  $\bar{x}_c$  being the mean of the control group,  $\bar{x}_t$  the mean of the treatment group,  $s_{xc}^2$  the variance of the control group and  $s_{xt}^2$  the variance of the treatment group.

consistently shown to be key drivers of the selection into labor market programs (see Dolton and Smith, 2011; Lechner and Wunsch, 2013). As discussed by Eriksson (1997) and Carling and Richardson (2004) caseworkers have a large influence and a large degree of discretionary power over enrollment decisions, in particular for training and work practice, suggesting that individual self-selection into the program is less of a problem. Second, as discussed above, we explicitly account for the elapsed unemployment duration, which has also shown to be one of the most important determinants of the selection process (Biewen et al., 2014). Moreover, it should be noted that our main interest is in estimating treatment effects on health outcomes and therefore selection into the program based on unobserved characteristics would be only a threat to our identification strategy if it is particularly correlated with these health outcomes. To address this concern explicitly, we additionally control for the prescription history before entering unemployment in a very detailed way. Finally, as we are using  $\Delta Y_i$  (instead of  $Y_i$ ) as our outcome variable, only unobserved factors that are directly connected to the beginning of the treatment would be a threat to our identification strategy.

To estimate the treatment effect  $\delta$  based on our comprehensive control variables, we use inverse probability weighting (IPW) with weights obtained from logit estimations. The distribution of the propensity score separated by the elapsed unemployment duration, as well as corresponding Pseudo- $R^2$ 's are summarized in column 3 to 7 in Table 2, while column 8 and 9 show the mean standardized bias (Rosenbaum and Rubin, 1983), which assesses the distance of the covariates before and after matching.<sup>7</sup>

### 3.2 Balancing Tests

To assess the validity of our empirical approach, Table 3 shows balancing tests with respect to a several pre-treatment outcome that are exploited for the empirical analysis, including prescriptions related to the three health issues of interest (see Panel A) and sickness absence from the UI system (see Panel B) within the reference period separated by the month relative to the program start. It can be seen that differences between treated and

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<sup>7</sup>For each covariate  $X$ , it is defined as:  $SB(x) = 100(\bar{x}_c - \bar{x}_t) / \sqrt{\frac{1}{2}(s_{xc}^2 + s_{xt}^2)}$  with  $\bar{x}_c$  being the mean of the control group,  $\bar{x}_t$  the mean of the treatment group,  $s_{xc}^2$  the variance of the control group and  $s_{xt}^2$  the variance of the treatment group.

matched controls are generally small, not statistically significant (except for a few cases) and, most importantly, there is no evidence that there treated and controls would be on different trajectories, which would indicate a violation of the common trend assumption. Moreover, Panel C also depicts balancing test for the days spent in unemployment within year  $t - 3$ ,  $t - 2$  and  $t - 1$  before the entry unemployment. Again, differences are negligible and the trends are very similar for treated and controls.

Table 3: Balancing Test on Pre-Treatment Outcomes

	Training				Sanctions			
	Non-treated	Treated	SB <sup>(a)</sup>	P-value	Non-treated	Treated	SB <sup>(a)</sup>	P-value
<i>A. Prescription related to ... within month</i>								
Cardiovascular system								
$t - 2$	0.0040	0.0032	1.0	0.422	0.0069	0.0066	0.4	0.872
$t - 3$	0.0048	0.0063	-2.1	0.194	0.0076	0.0062	1.6	0.526
$t - 4$	0.0045	0.0065	-2.7	0.103	0.0073	0.0055	2.1	0.410
$t - 5$	0.0040	0.0044	-0.6	0.709	0.0045	0.0052	-1.0	0.705
$t - 6$	0.0035	0.0043	-1.2	0.438	0.0041	0.0048	-1.1	0.694
Mental health problems								
$t - 2$	0.0107	0.0128	-1.7	0.233	0.0155	0.0155	0.0	1.000
$t - 3$	0.0155	0.0145	0.8	0.597	0.0128	0.0128	0.0	1.000
$t - 4$	0.0120	0.0144	-2.0	0.205	0.0152	0.0117	2.8	0.254
$t - 5$	0.0098	0.0110	-1.1	0.476	0.0121	0.0131	-0.9	0.724
$t - 6$	0.0092	0.0105	-1.3	0.415	0.0100	0.0090	1.0	0.684
Infections								
$t - 2$	0.0214	0.0163	3.4	0.021	0.0204	0.0197	0.5	0.851
$t - 3$	0.0194	0.0170	1.8	0.253	0.0238	0.0169	4.7	0.063
$t - 4$	0.0167	0.0174	-0.5	0.756	0.0173	0.0166	0.5	0.839
$t - 5$	0.0129	0.0166	-3.1	0.062	0.0124	0.0135	-0.9	0.727
$t - 6$	0.0097	0.0100	-0.2	0.871	0.0117	0.0152	-3.2	0.254
<i>B. Sickness absence from UI within month</i>								
$t - 2$	0.0186	0.0192	-0.4	0.813	0.0290	0.0179	7.2	0.005
$t - 3$	0.0157	0.0176	-1.5	0.345	0.0252	0.0204	3.3	0.218
$t - 4$	0.0166	0.0190	-1.9	0.248	0.0231	0.0259	-1.9	0.497
$t - 5$	0.0167	0.0164	0.2	0.900	0.0190	0.0200	-0.8	0.776
$t - 6$	0.0177	0.0177	0.0	1.000	0.0221	0.0211	0.7	0.786
<i>C. Days in unemployment in year</i>								
$t - 1$	32.39	32.61	-0.3	0.853	35.43	35.74	-0.4	0.872
$t - 2$	52.15	52.16	-0.0	0.997	54.57	53.09	1.5	0.563
$t - 3$	44.74	45.40	-0.7	0.655	47.57	46.80	0.8	0.752

*Note:* Depicted are weighted averages of pre-treatment outcomes for treated and matched controls A) referring to an indicator whether the individual had a valid prescription in the corresponding month prior to treatment start, B) referring to an indicator whether the individual called in sick in the UI system and C) referring to number of days spent in unemployment in a given year relative to the entry into unemployment. *P*-values refer to two-tailed t-tests based on equal means.

<sup>(a)</sup>Standardized bias for variable  $x$  is defined as:  $SB(x) = 100(\bar{x}_c - \bar{x}_t) / \sqrt{\frac{1}{2}(s_{xc}^2 + s_{xt}^2)}$ , with  $\bar{x}_c$  being the mean of the control group,  $\bar{x}_t$  the mean of the treatment group,  $s_{xc}^2$  the variance of the control group and  $s_{xt}^2$  the variance of the treatment group.

### 3.3 Differences in Health and Labor Market Outcomes

Table 4 shows weighted averages of our outcome variables for treated and non-treated, where weights are obtained based on the number of treated and non-treated for each month of the elapsed unemployment duration as depicted in Table 2. It can be seen that participants in training programs, generally have significantly less prescriptions. For instance, over the whole time period  $t-1$  to  $t+12$ , participants face a two percentage points lower probability to have prescription related to the cardiovascular system or infections and even a four percentage points lower likelihood for prescriptions related to mental health problems (see Panel A). In line with this, participants also call in sick less often, while the difference within the outcome period is about three percentage points (see Panel B). When considering the probability of leaving unemployment in Panel C, it can be seen that there is locking-in effect since participants face a lower reemployment probability three months after the program start, while a substantially higher employment probability of about 16 percentage is realized one year after the program start.

For recipients of sanctions, the overall picture looks less clear. Although, they have a slightly lower probability to have a prescription the cardiovascular system (0.8 percentage points) and mental health problems (1.8 percentage points), there is substantially higher likelihood that they called in sick of about five percentage points. Finally, receiving a sanction is associated with a significantly higher employment rate both in the short- and medium-run.

## 4 Baseline Results

Figure 1 presents the baseline estimation results using the dynamic DID approach for the three types of drug prescriptions and the probability of calling in sick. All outcome variables refer to differences between the indicator whether the event took place in the corresponding outcome and the reference period ( $t-6$  to  $t-2$ ) prior to the potential program start. Moreover, we distinguish between five different outcome periods. First,  $t-1$  indicates the month before the treatment start, which allows us to capture anticipation effects as a reaction to the information about the upcoming treatment. Moreover, we



Table 4: Observed Differences in Outcome Variables

	Training			Sanctions		
	Non-treated	Treated	$P$ -value	Non-treated	Treated	$P$ -value
<i>A. Prescription related to</i>						
Cardiovascular system						
within month $t - 1$	0.0112	0.0075	0.0002	0.0112	0.0059	0.0002
up to month $t + 3$	0.0405	0.0263	0.0000	0.0411	0.0407	0.9180
up to month $t + 6$	0.0483	0.0308	0.0000	0.0490	0.0476	0.7234
up to month $t + 12$	0.0592	0.0375	0.0000	0.0601	0.0545	0.1875
Total ( $t - 1$ to $t + 12$ )	0.0643	0.0405	0.0000	0.0651	0.0573	0.0722
Mental health problems						
within month $t - 1$	0.0238	0.0110	0.0000	0.0238	0.0245	0.8150
up to month $t + 3$	0.0893	0.0607	0.0000	0.0905	0.0776	0.0100
up to month $t + 6$	0.1002	0.0687	0.0000	0.1015	0.0859	0.0029
up to month $t + 12$	0.1151	0.0795	0.0000	0.1167	0.0970	0.0004
Total ( $t - 1$ to $t + 12$ )	0.1251	0.0848	0.0000	0.1265	0.1087	0.0022
Infections						
within month $t - 1$	0.0256	0.0232	0.1591	0.0256	0.0173	0.0005
up to month $t + 3$	0.0749	0.0678	0.0150	0.0759	0.0745	0.7833
up to month $t + 6$	0.0961	0.0844	0.0003	0.0974	0.0973	0.9933
up to month $t + 12$	0.1288	0.1106	0.0000	0.1305	0.1249	0.3648
Total ( $t - 1$ to $t + 12$ )	0.1425	0.1227	0.0000	0.1441	0.1342	0.1204
<i>B. Sickness absence from UI</i>						
within month $t - 1$	0.0215	0.0114	0.0000	0.0215	0.0366	0.0000
up to month $t + 3$	0.0362	0.0140	0.0000	0.0365	0.0576	0.0000
up to month $t + 6$	0.0582	0.0309	0.0000	0.0586	0.0897	0.0000
up to month $t + 12$	0.0951	0.0687	0.0000	0.0957	0.1377	0.0000
Total ( $t - 1$ to $t + 12$ )	0.1105	0.0775	0.0000	0.1111	0.1622	0.0000
<i>C. Exit from unemployment to regular work</i>						
up to month $t + 3$	0.1504	0.1191	0.0000	0.1518	0.2191	0.0000
up to month $t + 6$	0.2329	0.2885	0.0000	0.2345	0.3527	0.0000
up to month $t + 12$	0.3157	0.4757	0.0000	0.3159	0.4638	0.0000

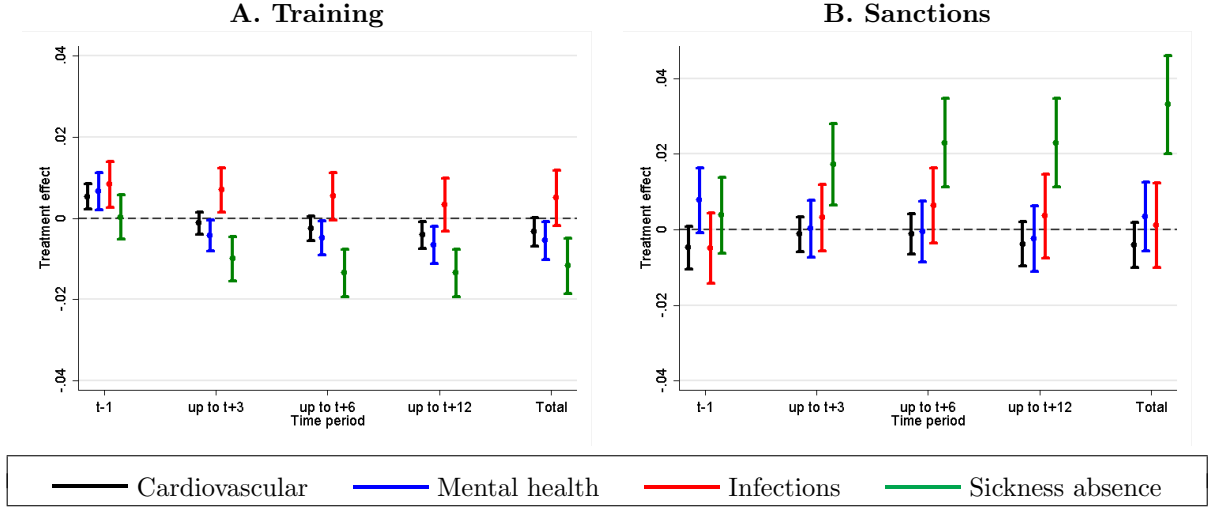
*Note:* Depicted are weighted averages of the outcome variables referring to an indicator whether the corresponding event took place within the corresponding time period relative to the treatment start. Weighted averages are calculated for the dynamic estimation sample where, weights are obtained based on the number of treated and non-treated for each month of the elapsed unemployment duration depicted in Table 2.  $P$ -values refer to two-tailed t-tests based on equal means.

consider three different post-treatment periods  $t + 3$ ,  $t + 6$  and  $t + 12$ , as well as the total effect within  $t - 1$  to  $t + 12$ .

## 4.1 Anticipation Effects

The first set of results presented in Figure 1 shows that participants in training program compared to non-participants face an increase with respect to all three types of prescriptions in the month prior to the program start relative to the reference period, while there are no statistically significant anticipation effects for sanctions. The anticipation effects

Figure 1: Baseline Results: ATTs on Health Outcomes



*Note:* Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes refer to the difference between the indicator whether the individual has a valid prescription related to one of three health problems (black, blue, red), respectively has been registered as sick (green) in the corresponding time period relative to the beginning of the treatment and the reference period.

No. of observations: A.Training  $N = 365,589$ ; B.Sanction  $N = 360,762$ .

for training programs range from 0.53 percentage points for cardiovascular diseases up to 0.83 percentage points for infections and are statistically significant at least the 5%-level. There are several possible explanations for the presence of these relatively strong effects in the pre-treatment period. First, the announcement of the treatment might have real health effects, as the prospect of participating in a program might create stress that has negative implications for the health status.

However, there might be other behavioral effects that could explain the increase in drug prescriptions before the program start. For instance, individuals who are informed about the upcoming treatment and do not want to participate could try to avoid the treatment by visiting their physicians more often in order to get a medical certificate that might allow them to abandon the participation, while the larger number of prescriptions would be a by-product of the more frequent visits to the doctor. To test the presence of such a mechanism, we consider episodes of sickness absence registered in the unemployment insurance system as an additional outcome variable. Interestingly, our results show no anticipation effect with respect to the probability to call in sick, which contradicts the

hypothesis since individuals who want to avoid the treatment would call in sick more often as this would be requirement to avoid the treatment due to sickness.

Finally, a third possible explanation would imply that individuals who expect to participate in a time-consuming training program soon move their doctor visits to the pre-treatment period as they anticipate that they will face time constraints in the future. Assuming that these visits are associated with drug prescriptions in many cases, this could potentially explain the relatively strong anticipation effects in the month before the start of the training program. We further explore this interpretation in our subgroup analysis presented in Section 5.2.

## 4.2 Post-Treatment Effects

The second set of results presented in Figure 1 shows the treatment effects for the period after the enrollment in the training program, respectively the imposition of the benefit sanction. For training programs, we observe an increase in drug prescriptions related to infections, which is statistically significant at the 5%-level only within the first three months after the program start and becomes insignificant afterwards. On the contrary, being enrolled in a training program reduces the probability for prescriptions related to the cardiovascular or mental health problems. Over the course of 12 months, the participants have a lower probability of receiving a prescription related to the cardiovascular system of 0.42 percentage points, while the effect on mental health issues is even larger (0.66 percentage points). Both effects are statistically significant at the 5%-level. Moreover, we also consider the total effect as the sum of the anticipation effect in  $t - 1$  and the post-treatment effect over the course of twelve months after the program start as an additional outcome variable. This is particularly interesting since both effects work into opposite directions for two of the outcome variables. In total, we find a significant (at the 10%-level) reduction of the probability to receive a prescription related to mental health problems of about 0.55 percentage points, while the effect on cardiovascular diseases is also negative, but not statistically significant. In summary, the baseline results suggest that training programs have some favorable effects on the participants' health status, while drug prescriptions also seem to capture some adjustments of the individual behavior

induced by the announcement of the treatment, e.g. a temporal shift of doctor visits, which are not necessarily associated with an actual change of the health status. The notion that training programs generally improve participants' health status is supported by the fact that the treatment also reduces the probability to call in sick. We find a reduction of about one percentage point that is statistically significant at the 1%-level.<sup>8</sup>

For sanctions, we do not find any sizable or significant effects on the likelihood of receiving drug prescriptions, but there is a strong positive effect on the probability to call in sick in the time period after the sanction was imposed. However, since this is not associated with an increase in drug prescriptions, the findings suggest that this is not a real health effect, but rather a behavioral response in order to avoid future sanctions since job seekers only face a minimum of search requirements when reporting sick (see also van den Berg et al., 2016). When comparing the effects for the two types of labor market policies, the results indicate that the change of daily routines and the potential acquisition of skills through training programs has on average much stronger implications for the health status of participants than the reduction of financial means through the imposition of a sanction, which might only has indirect effects on the individual behavior.

## 5 Effect Mechanisms

### 5.1 Relationship between Employment and Health Effects

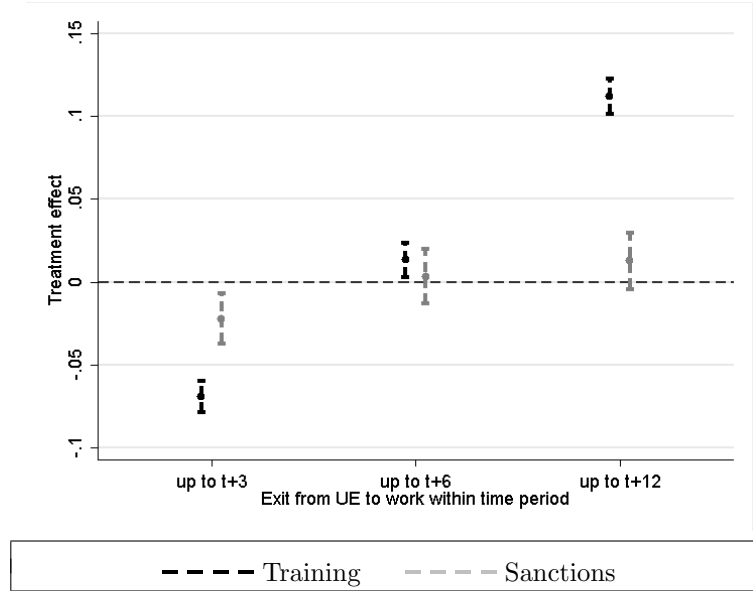
As a first step to shed light on the underlying effect mechanisms, we now compare the health effects of training programs and sanctions with their impact on subsequent employment prospects. This is assumed to provide evidence whether the positive (negative) health effects are caused by corresponding improvements (deteriorations) of the participants employment probabilities. Therefore, Figure 2 shows the employment effects for the time periods up to  $t + 3$ ,  $t + 6$  and  $t + 12$  after the treatment (corresponding to the presentation of the post-treatment health effects in Figure 1). The reference level for the conditional DID estimator is given by the average probability the the individual

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<sup>8</sup>It should be noted that the outcome variable only captures sickness absence registered in the unemployment insurance system. Therefore, the effect could be also generated by a positive treatment effect on the exit rate from unemployment. We will shed further light on this in Section 5.1.

had received any labor income in the years  $t - 1$ ,  $t - 2$  and  $t - 3$  before the entry into unemployment in order to explicitly account for time-constant unobserved heterogeneity.

Figure 2: ATTs on Exit Rates to Employment



*Note:* Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes refer to an indicator whether the individual has left unemployment to work within the corresponding time period relative to the beginning of the treatment.

No. of observations: Training  $N = 365,589$ ; Sanctions  $N = 360,762$ .

The estimation results show that there is a substantial locking-in effect for participants in training programs of about 6.9 percentage points within the first three months after the enrollment, which turns into a strong positive effect one year after the program start. When considering sanctions, there is small negative effect of 2.2 percentage points on the reemployment probability within the first three months, which also turns into a positive but insignificant effect in later periods. When comparing the estimated treatment effects on health and employment outcomes, we do not find evidence for a connection between the two types of outcomes, neither for training programs nor for sanctions. In particular, the reduction of drug prescriptions for participants in training programs seem to start when individuals are still enrolled in the program and before the positive effect on reemployment prospects occurs. This implies that the direct implications of the treatment, such as the change of daily routines, are more relevant for the participants health status than the indirect effect through improved employment outcomes.

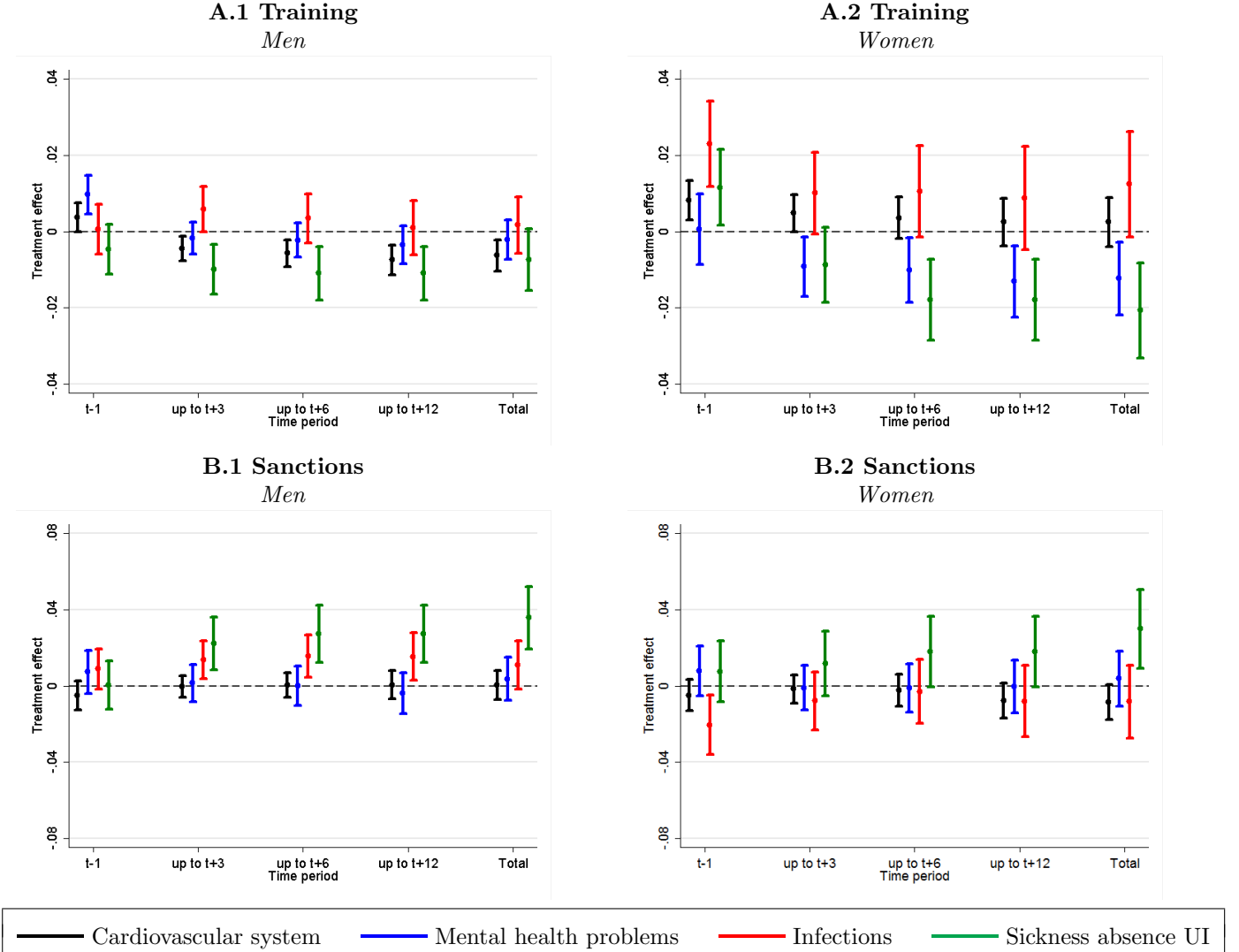
## 5.2 Subgroup Analysis

In the following section, we provide additional evidence with respect to the underlying effect mechanisms by analyzing heterogeneous treatment effects of the two policies with respect to gender, the level of education and the initial health status.

**Gender:** It is well known that there are substantial gender differences with respect to health help-seeking behavior. In particular, men are often characterized as being less likely to seek help from health professionals (see Galdas et al., 2005, for an overview). Therefore, it seems natural to look into heterogeneous treatment effects with respect to participants' gender as receiving drug prescriptions typically requires doctor appointments. As shown in Panel A of Figure 3, the favorable effects of training programs with respect to number of prescriptions related to cardiovascular diseases are driven entirely by men, which seems to be reasonable given that cardiovascular diseases typically arise much earlier in life for men than for women (Rossouw, 2002). Moreover, the program reduces the likelihood of prescriptions related to mental health problems for women but not for men. This seems to be surprising in the first place as men are typically more distressed by unemployment than women (see Paul and Moser, 2009), which would suggest that labor market interventions have a higher potential to improve the mental health status of men. However, the finding might reflect the fact that men want to rely on problem-focuses activities such as job search, while women seek social support when becoming unemployed (see Leana and Feldman, 1991). Given that participating in a training program might reduce job search capacities, while it increases social interactions, this could explain the gender differences with respect to the effects on prescriptions related to mental health problems. Moreover, there is no clear pattern with respect to the anticipation effects of training programs.

Finally, when focusing on the effects of benefit sanctions there are some gender difference with respect to the likelihood of receiving prescriptions for infections, but not for the other outcome variables (see Panel B of Figure 3). It seems to be plausible that benefit sanctions could potentially trigger infections as they might create stress which might affect immunity and infectious diseases (see Cohen, 1996) or they force individuals reduce their expenses which could lead to poor nutrition, lower levels of personal hygiene or a

Figure 3: Subgroup Analysis on Health Outcomes by Gender



*Note:* Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes refer to the difference between the indicator whether the individual has a valid prescription related to one of three health problems (black, blue, red), respectively has been registered as sick (green) in the corresponding time period relative to the beginning of the treatment and the reference period.

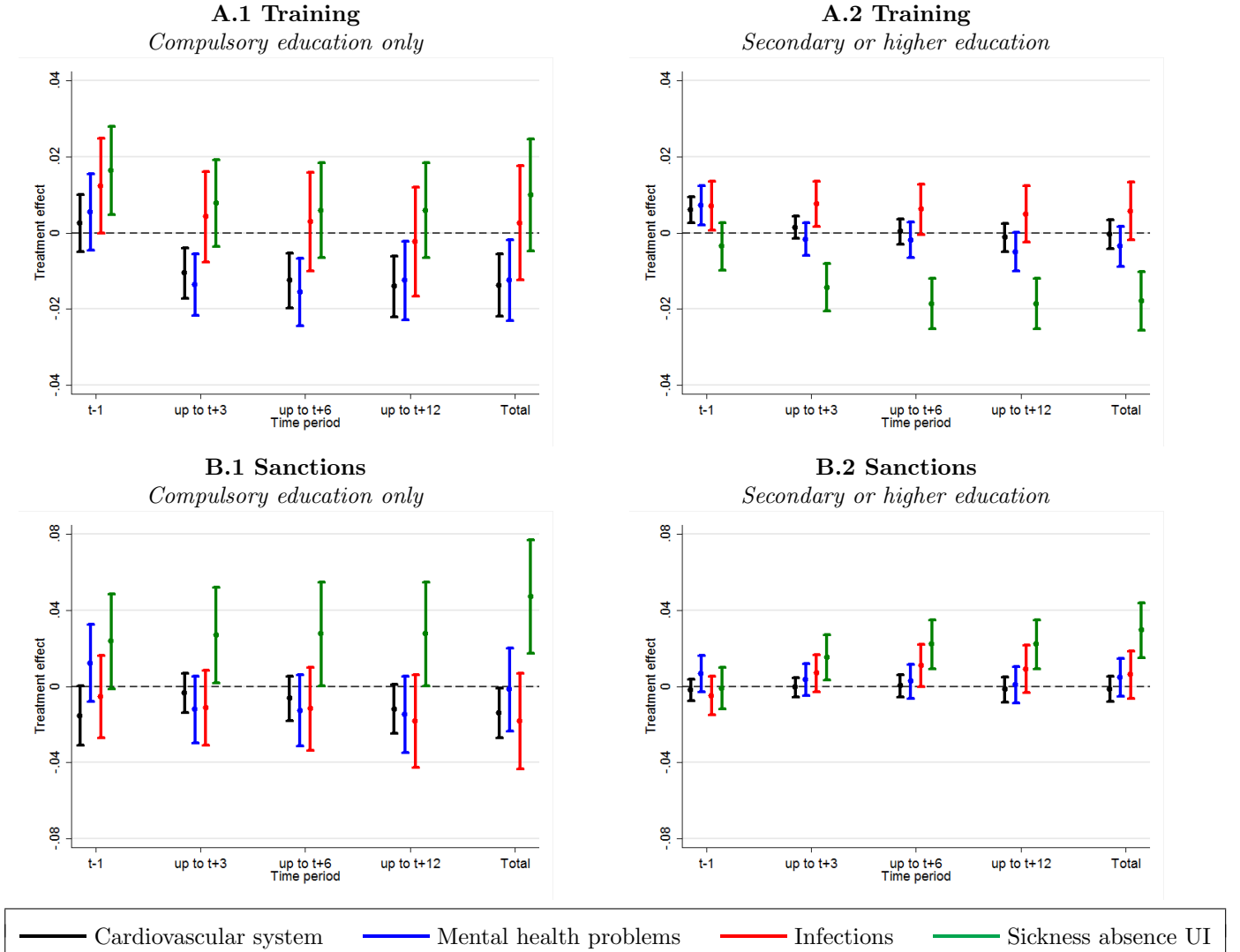
No. of observations: Training (men)  $N = 174,835$ ; Training (women)  $N = 190,754$ ; Sanctions (men)  $N = 171,231$ ; Sanctions (women)  $N = 189,531$ .

reduction of physical activities and therefore increase the of infections (see Cohen, 1999).

Our findings suggest that such factors might play an important role for men, since there is a positive and statistically significant effect on the likelihood of having prescriptions related to infections after the imposition of a sanction, but not for women.

**Education:** In a second step, we split our estimation sample with respect to the education status. Therefore, we distinguish between individuals who only have compulsory

Figure 4: Subgroup Analysis on Health Outcomes by Education



*Note:* Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes refer to the difference between the indicator whether the individual has a valid prescription related to one of three health problems (black, blue, red), respectively has been registered as sick (green) in the corresponding time period relative to the beginning of the treatment and the reference period.

No. of observations: Training (compulsory education only)  $N = 79,953$ ; Training (secondary or higher education)  $N = 285,636$ ; Sanctions (compulsory education only)  $N = 78,829$ ; Sanctions (secondary or higher education)  $N = 281,933$ .

education, which ends after nine years of schooling, and those who a secondary degree or higher. Interestingly, the results for training programs, as shown in Panel A of Figure 4, reveal that the favorable post-treatment effects on the health status, as expressed by a reduction in prescriptions related to cardiovascular and mental health problems, are driven by unemployed with a low level of education. Since those individuals seem to face the highest risk of lacking daily routines when being unemployed, this can be interpreted

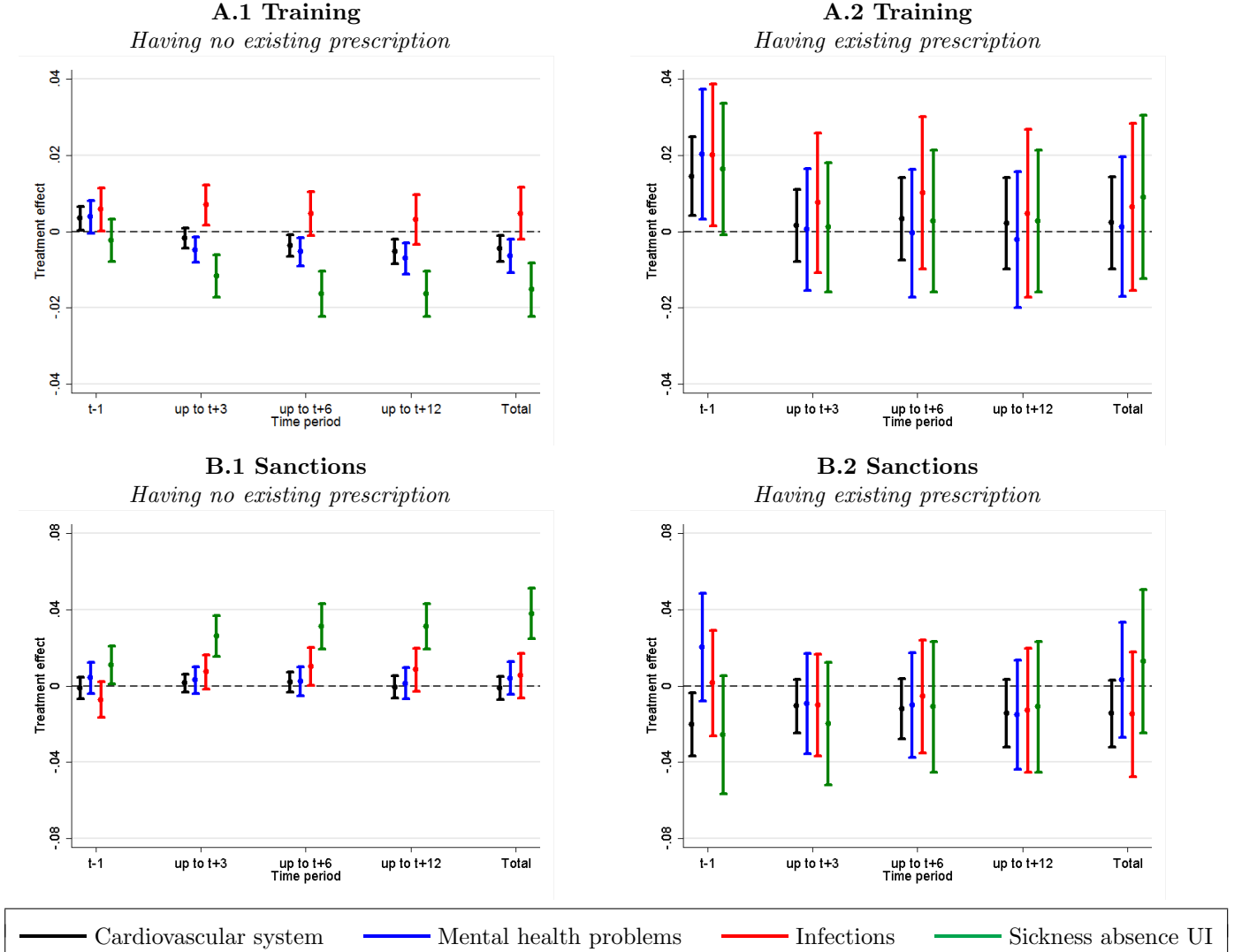


as evidence that training programs improve the physical and mental health status by encouraging the development of daily routines.

**Initial Health Status:** Finally, it can be assumed that the relevance of the behavioral adjustments discussed before that might explain the anticipation effects depend on the initial health status of the potential participants. In particular, a temporal shift of doctor visits that would lead to more prescriptions in the month before the start of a training program would be only relevant for individuals with a concrete health issue. Moreover, it is also possible that the reduction of redeemed prescriptions in the post-treatment is induced by participants who stop to collect their medication or reduce doctor visits due to time constraints during the treatment. Again, this mechanism can be assumed to be much more important for individuals who already have existing health problems.

To test the relevance of these effect mechanisms, we divide the estimation sample with respect to their initial health status approximated by having a prescription related to one of three outcomes of interest within the six months before the entry into unemployment. As shown in Panel A of Figure 5, the anticipation effect for training programs is substantially stronger for the sample with an existing prescription in the period before the entry into unemployment, while the post-treatment effect is driven only by individuals without an existing prescription. The findings are particularly interesting as they show that the anticipation effects in the month before the program start are a behavioral reaction of participants with an existing health issue, which indicates that they might move doctor appointments in the pre-treatment period, while there seems to be no real health effect. On the other hand, the reduction of prescription is not unlikely to be the consequence of less frequent visits at the doctor or the pharmacy as they occur mainly for people without an existing prescription and they are accompanied by a lower probability to report sick. This provides further evidence that the post-treatment effect reflects a real improvement of the participants' health status.

Figure 5: Subgroup Analysis on Health Outcomes by Initial Health Status



*Note:* Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes refer to the difference between the indicator whether the individual has a valid prescription related to one of three health problems (black, blue, red), respectively has been registered as sick (green) in the corresponding time period relative to the beginning of the treatment and the reference period.

No. of observations: Training (compulsory education only)  $N = 301,345$ ; Training (secondary or higher education)  $N = 64,244$ ; Sanctions (compulsory education only)  $N = 297,244$ ; Sanctions (secondary or higher education)  $N = 63,518$ .

## 6 Conclusion

We combine Swedish administrative data on the universe of individual drug prescriptions with detailed labor market records and provide first and comprehensive evidence on the health effects two labor market policies representing different reintegration strategies. Training programs are seen as supportive measures (carrots) that aim to help participants

acquiring new skills, while sanctions are restrictive interventions (sticks) that financially punish non-compliance with UI guidelines.

In order to identify the causal health effects of these interventions, we adopt a dynamic selection-on-observables identification strategy in combination with a difference-in-difference approach. For each month of the elapsed unemployment duration, we estimate the effect of receiving a treatment (for the first time) versus waiting out in unemployment and explicitly compare differences with respect to the probability of having a valid prescription between the pre- and post-treatment period for treated and non-treated. In particular, we consider three types of health issues that are assumed to show and diagnosed quickly after a potential treatment, i.e. problems related to the cardiovascular system, mental health problems and infections. Moreover, we take into account observed differences with respect to socio-demographic characteristics, labor market histories and the past usage of prescription drugs.

While we find no clear evidence that sanctions, representing the stick, have any impact on the health status of recipients, our estimation results show that training programs, representing the carrot, have on average favorable effects on participants health status once they are enrolled. This effect is not directly related to improvements of the employment prospects of participants, but seems to be a direct consequence of the enrollment in the program. For instance, the treatment might affect participants daily routines, which improves their health status. This interpretation is supported by the fact that the treatment effect is driven by individuals with a low educational background who might face a higher risk of lacking daily routines when they are unemployed. Moreover, we also find evidence for an increase of drug prescriptions just before the start of the program, which is likely to reflect behavioral adjustments, like a temporal shift of doctor appointments to the pre-treatment period, but do not imply negative health effects.

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