WORKING PAPER 2020:20

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ISSN 1651-1166

Side effects of labor market policies[a]

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October 2, 2020

Labor market policy tools such as training and sanctions are commonly used to help bring workers back to work. By analogy to medical treatments, the individual exposure to these tools may have side effects. We study effects on health using individual-level population registers on labor market events outcomes, drug prescriptions and sickness absence, comparing outcomes before and after exposure to training and sanctions. We find that training improves cardiovascular and mental health and lowers sickness absence. The results suggest that this is not due to improved employment prospects but rather to instantaneous features of participation such as, perhaps, the adoption of a more rigorous daily routine. Unemployment benefits sanctions cause a short-run deterioration of mental health, possibly due higher stress levels, but this tapers out quickly.

Keywords: Unemployment, health, sickness, prescriptions, mental health, drugs, training, depression, cardiovascular disease, sanctions.

JEL codes: J68, I12, I18, H51

[a] The authors thank Anders Forslund, Markus Gehrsitz, Aderonke Osikominu, Ulrika Vikman, and participants at conferences and workshops in Lyon, Nuremberg, Bath, Leipzig and Basel and seminars in Copenhagen, Potsdam, Hamburg and Uppsala for valuable comments. Marco Caliendo gratefully acknowledges funding from the German Research Foundation (Deutsche Forschungsgemeinschaft, DFG, 405629508) and Johan Vikström acknowledges support from FORTE (2015-00971).

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Contents

1 Introduction

The connection between individuals' employment situation and their health status has been widely investigated by the medical, psychological and economic literature. It is well-documented that unemployment is related to worse mental and physical health conditions[1], and that job displacement affects mortality rates, hospitalizations and harmful health-related attitudes.[2] However, despite this extensive literature on the connection between unemployment and health little is known how labor market policies that are primarily designed to improve re-employment prospects of unemployed individuals affect health-related outcomes. Key examples of commonly used policies are training programs for workers lacking certain skills and punitive sanctions for workers whose search effort is deemed insufficient, while other examples are job search assistance programs and job creation schemes (see Card et al., 2018, for a recent overview on their effectiveness).

By analogy to medical treatments, the individual exposure to the treatments prescribed by these policies may have side effects. Given the adverse connection between unemployment and health, it is particularly interesting to consider the effects of exposure to labor market policy measures on individual-level health outcomes. Such side effects could lead to a re-assessment of the costs and benefits of the policies.[3] Moreover, knowledge about side effects helps to understand why certain policies do or do not help to bring the unemployed back to work. For example, a health deterioration due to exposure to a certain policy measure may offset the positive effect of the measure on an individual's skill level, resulting in a zero net effect on the re-employment rate. As an opposite example, a positive effect on health may increase the individual's market value as perceived by employers, and this may be the only reason for why re-employment increases in certain contexts. Of course, health may further improve in response to re-employment (see e.g. Huber et al., 2011).

[1] Dooley et al. (1996) and McKee-Ryan et al. (2005) provide overviews documenting the adverse connection between unemployment and health, which can be attributed to several factors, such as the income loss involved (Ettner, 1996; Benzeval and Judge, 2001), a reduction in socio-economic status (Adler et al., 1994) and an increased exposure to unhealthy environments (Tertilt and Van den Berg, 2015).

[2] Job displacement is associated with harmful health-related attitudes such as more smoking and alcohol consumption (Eliason and Storrie, 2009; Black et al., 2015), an increased number of hospitalizations (Keefe et al., 2002; Browning and Heinesen, 2012), and higher mortality rates (Sullivan and von Wachter, 2009; Eliason and Storrie, 2009; Browning and Heinesen, 2012).

[3] Other related examples of (unintendend) side effects of public policies include the relationship between minimum wages and public health (Leigh et al., 2019), as well as the connection between tax credits and infant respectively maternal health (Hoynes et al., 2015; Evans and Garthwaite, 2014) or midlife mortality (Dow et al., 2019).

In this paper we examine effects of exposure to training and to sanctions on a range of health outcomes. Training and sanctions are the flagship representations of the commonly used dichotomy of carrot and sticks policies for the unemployed (see e.g. Arni et al., 2017). As such, their health effects may be representative for a range of other active labor market policies. As training aims to upgrade skills, one may expect more positive health effects than for sanctions which, after all, are punitive and involve temporary income cuts and hence may increase stress.[4]

It is a key feature of our study that we analyze *objective* health measures, using unique individual-level longitudinal population registers on drug prescriptions and sickness absence. The drug prescription data provide daily records of all redeemed prescriptions using the Anatomical Therapeutic Chemical (ATC) Classification System (see WHO, 2020). We focus on medication for cardiovascular diseases and mental health problems, which are ideal to study the effects of labor market policies. Both types of health problems are relatively common and can be identified and diagnosed quickly after they begin to develop. Moreover, they are known to respond to stressful events in the life of an individual and are often treated with medication. In addition to drug prescriptions, we consider episodes of sickness absence as a supplementary objective health outcome capturing health effects that do not necessarily lead to a prescription. We observe the total number of days that people receive sickness benefits as well as an indicator for spells lasting more than seven days that require a doctor's certificate. Note that one may expect a positive correlation between the various health outcomes. We merge the register data on drug prescriptions and sickness absence to other administrative register data containing the unemployment status and participation in labor market policies. We sample the full population of new entries into unemployment in Sweden in 2006 and 2007 and we use the various registers to trace individuals before, during and after these two years.

In our view, the fact that we do not need to rely on self-reported subjective health measures is particularly important when it comes to effects of labor market policy measures, since the latter are known to affect self-assessed measures of well-being in the short run for reasons that may be unrelated to objective health. In particular, training programs require a high level of participants' commitment. This can cause individuals to dislike participation and thus to report low well-being, but this does not necessarily translate into low objective health. Similarly, Ochsen

[4]While any policy that involves interactions with bureaucratic and unloved institutions may lead to health reductions, policies providing a net boost to re-employment may lead to subsequent health improvements. Previous studies show that training programs tend to have favorable effects on earnings and other employment outcomes in the long-run (see, e.g., Lechner et al., 2011), while sanctions also increase re-employment rates (see, e.g., Van den Berg et al., 2004; Lalive et al., 2005), but lead to lower wages and reduced job stability as well (Arni et al., 2013; Van den Berg and Vikström, 2014).

and Welsch (2012) find that a less generous unemployment insurance system is associated with lower levels of life satisfaction, but this in itself does not necessarily imply that such a system or other restrictive policy interventions lead to lower objective health. An additional advantage of health register data is that findings do not depend on the design of specific survey items. As shown by Bond and Lang (2019), analyses based on specific ordinal scales on subjective well-being in survey data are difficult to generalize to other settings. Finally, in contrast to survey data, prescription register data do not suffer from underreporting due to the stigma of mental illness (Bharadwaj et al., 2017), which is important in settings as ours where mental illness may be prevalent.

Our study aims to identify the *causal* effects on individual-level health outcomes. The main challenge here is that exposure to a labor market policy measure may be correlated with individual health. To proceed, we exploit the longitudinal nature of the data and apply conditional difference-in-differences methods (Heckman et al., 1998). Specifically, we use drug prescription outcomes before treatment, respectively before unemployment. This allows us to account for existing health differences between treated and non-treated individuals and for changes of individual health during the unemployment spell. We acknowledge that the treatment may start after any elapsed time in unemployment, by addressing that the selection into the treatment changes over the course of time.

The data also allow us to examine effects of treatment exposures on the transition rate into work. We use this to study the importance of *indirect effects* of the treatment on health by way of a transition into employment – to be distinguished from the *direct health effects* of participating in training or receiving a sanction as such. Such an analysis is necessarily somewhat descriptive as it does not allow for selection due to unobserved confounders, unlike the main analyses in the paper. However, as we shall see, data patterns of how events unfold over time provides insights into what are the most likely causal pathways.

A small number of existing studies examine the relation between labor market policy exposure of unemployed and health mainly originating from the psychological literature. In contrast to our study, they typically rely on subjective measures of well-being or health, and they are descriptive in the sense that they focus on associations (see Coutts et al., 2014; Puig-Barrachina et al., 2019, for overviews). For our purposes, perhaps the most relevant studies are Creed et al. (1998) and Machin and Creed (2003) who find a negative association between training and self-reported psychological symptoms related to mental health. Tefft (2011) reports that a more generous unemployment insurance system is associated with a lower susceptibility to anxiety and depression, which may suggest that exposure to benefits sanctions could lead to increased stress and thus to worse health.

We find that participation in a training program reduces the probability to have a prescription for drugs for cardiovascular and mental health problems within one year by about 6-8%. This is accompanied by a reduction of sickness absence by about 20%. The favorable effect on the participants' health status shows up before the treatment is realistically able to create a positive impact on the employment prospects. Thus, the findings reflect health improvements due to the instantaneous effect of training. Other possible explanations, such as a reduction of doctor or pharmacy visits due to time constraints during the treatment are less likely. A subgroup analysis shows that the effect is particularly pronounced for vulnerable groups like the low-educated, who may more often lack a regular daily routine in unemployment before training. For sanctions we do not find evidence of effects on drug prescriptions after the benefit reduction, but we do find an increase in sickness absence. Moreover, we observe a strong increase in prescriptions related to mental health problems in the month before the sanction was imposed, when the individual must have already received a warning. This may reflect higher levels of stress.

Our paper thus provides evidence that common tools of labor market policy have side effects on participants' health status. These should be taken into account when assessing the costs and benefits of an intervention. This is particularly important as better health conditions may help individuals to avoid or reduce future unemployment (e.g. Lindholm et al., 2001; Stewart, 2001; García-Gómez et al., 2010; Rosholm and Andersen, 2010). The study provides a first step towards a more holistic evaluation for the most relevant reintegration tools for unemployed workers in many industrialized countries since any type of unintended effects should be taken into account when assessing the overall welfare effects of a policy.

The remainder of the paper is organized as follows. Section 2 describes the institutional background and discusses the potential health effects of labor market policies. Section 3 presents the data and discusses the empirical strategy, while Section 4 shows the estimation results and examines the relevance of different mechanisms. Finally, Section 5 concludes.

2 Institutional Setting and Hypotheses

Sweden has a long tradition of active labor market programs geared towards helping unemployed individuals and encouraging them to start new employment. Historically, Sweden has had a relatively low unemployment rate and extensive usage of active labor market programs, but over the last two decades the unemployment rate as well as the program participation rate have moved towards more average European levels. In the beginning of the period studied in this paper (January 2006 to December 2008[5] the unemployment rate was 8.3%, and in July 2007 it was down to 5.4%. While it increased as a result of the Great Recession in the months thereafter, it was still not higher than 6.4% by December 2008. In the mid 1990s, around 1.2% of the Swedish workforce at any point in time participated in labor market training. By 2007, that number was down to 0.4%, in line with a general reduction of active labor market policy measures.

2.1 Health and Unemployment Insurance in Sweden

Health insurance: Health care in Sweden is managed and financed by the public sector. All health care activities, as regulated by the Swedish Health Services Act (1982:763), are organized by the 21 Swedish regions and financed by direct taxes raised from the residents in each region. The regions are obliged to provide its residents with equal access to health services and quality of care.[6]

As most other countries, Sweden separates between prescription drugs that legally requires a medical prescription to be dispensed and over-the-counter drugs. For instance, non-opiod painkillers are sold as over-the-counter drugs, whereas medicines for heart diseases and mental health problems are prescription drugs. Only physicians can prescribe prescription drugs, and new prescriptions are obtained after a health care visit. For the period studied in this paper, all prescription and over-the-counter drugs were only sold at government-owned pharmacies. In 2009, there were around 900 pharmacies all over Sweden for a population of 9 million. Typically, patients are allowed to buy medication for a maximum of 3 months at a time.

To ensure universal access to high-quality and effective treatments, the prescription drugs are tax-subsidized in Sweden. The subsidies are regulated by the Dental and Pharmaceutical Benefits Agency, which among other things decides which medicines that should be subsidized. The subsidy system incrementally reduces patient costs for all prescription drugs. For purchases up to 1,150 SEK (≈130 USD) within a 12-month period, the patient pays the full cost for the

[5] We study this period because of data restrictions for the prescription drug register that we use to examine relevant health conditions.

[6] The regions are free to set their own patient fees for outpatient and inpatient visits, but a national cap on co-payments limits the total amount that a patient has to pay out-of-pocket each calendar year. In Stockholm, a visit to a doctor in primary care costs 200 SEK (≈ 25 USD) as of 2019.

medicine. After that, the subsidy amounts to 50% of the cost and then gradually increases to 75% and finally 90%. Both the health care system and prescription drug rules are universal: exactly the same rules apply no matter if you are unemployed, employed, subject to a sanction or participate in training.

Unemployment insurance: Unemployed individuals older than 20 years are eligible for unemployment insurance (UI) benefits if they register at the Swedish Public Employment Service (PES), actively search for a job, are able and willing to work at least 17 hours per week, and worked (at least 80 hours per month) at least 6 months during the past year. If these requirements are fulfilled a job seeker is entitled to UI benefits at the basic level, which during our period of analysis (2006-2008) was 320 SEK per day (≈35 USD). If the job seeker is in addition also a member of an UI fund for at least 12 months, they have access to income-related UI benefits with a replacement rate at 80%[7], starting from the basic UI level and up to a ceiling at 680 SEK (≈75 USD) per day. The relatively low ceiling means that many individuals will have a replacement rate below 80%. For instance, for the average wage rate in Sweden in 2007, the replacement rate is roughly 55%. The potential benefit duration is 300 days, and since benefits are paid for five days each week this corresponds to 420 calender days. The sanctions described below apply to both the basic level and the income-related UI benefits.

2.2 Labor Market Policies

The existing literature on labor market policies for unemployed workers typically distinguishes 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 programs (Arni et al., 2017). For our analysis, we focus on two types of labor market policies representing these two different reintegration strategies.

Training: 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

[7] In 2007, lower replacement rates was introduced: the 80% remained for the first 200 days of unemployment and after that it is 70%.

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 computer skills (15.1% of participants), office/warehouse work (15.1%), manufacturing (11.6%), machine operators (9.8%) and health care (6.1%). 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 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 described by Richardson and Van den Berg (2013), individuals who are interested in participating in a training program are often invited to an information meeting that takes place 30 days before the enrollment. It includes information about the content of the course and the eligibility rules. If the individual applies to the course, the final decision regarding enrollment is taken by a caseworker at the PES based on the individual's needs and skills.

Sanctions: Second, sanctions are benefit reductions for a limited period of time that are imposed if the unemployed's search behavior is not in accordance with the UI guidelines. The monitoring is carried out by the caseworker of the PES office. If they detect a violation they should send a notification to the UI fund, which decides whether to impose a sanction. In practice, before sending the notification the PES contacts the individual to rule out the possibility that the apparent infringement was the result of a misunderstanding, which means that all individuals are informed about the notifications. The decision about the sanction is taken quickly by the UI fund, in most cases within two or three weeks since the notification.[8] In more than 85 percent of cases, the UI fund approves a sanction (Van den Berg and Vikström, 2014).[9] For the period studied in this paper, the refusal of suitable job offers without a valid reason is the main reason for a sanction.[10] For such violations, the sanction is a 25% benefits reduction for a period of 40 days for first-time offenders, 50% for 40 days for second-time offenders, and a third violation 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 includes failure to actively

[8]These numbers are based on calculations made up on our request by The Swedish Unemployment Insurance Board.

[9]After the sanction decisions the sanctioned might appeal to revert the decision, but only a negligible fraction (2 percent) are partially or fully reversed.

[10]This applies for about 63% of the benefit sanctions in 2006 and 2007, while the remaining sanctions are imposed due to other violations of the UI entitlement conditions described below, e.g. related to job search requirements or ALMP participation.

search for a job, not showing up at meetings, failing to apply to assigned jobs. In these cases the sanction implies that the UI benefits are terminated for an indefinite period of time.[11] 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.

Sickness absence: The sanction rules apply to individuals with unemployment insurance benefits. If UI benefit recipients, however, call in sick, they receive sickness benefits instead of UI benefits. In general, the level of sickness benefits is the same as the level of UI benefits, but otherwise the rules are different: the individual no longer faces job search requirements, and since the sanctions apply to the UI benefits and not to the sickness benefits, the individual can postpone the sanction by calling in sick. However, when returning to UI benefits, the individual has to serve the full 40 suspension days (or whatever remains). Moreover, since the job search requirements do not apply when you claim sickness benefits, calling in sick limits the risk of a second sanction (or even a first sanction). Another difference is that all individuals on sickness benefits have to submit a doctor's certificate after seven benefit days. For the period studied here, there was no upper time limit on the sickness benefits, even though the doctor's certificate occasionally has to be re-renewed.

2.3 Possible Effects of Labor Market Policies on Health Outcomes

In general, we expect two types of mechanisms to be relevant when analyzing health effects of labor market policies. First, both policies, training programs and sanctions, aim to promote the job seekers' reintegration into the labor market. Previous evaluations of training programs in Sweden show that they are associated with negative lock-in effects during the program, but have a positive impact on re-employment prospects after the program and earnings in the long-run (de Luna et al., 2008; Richardson and Van den Berg, 2013; Van den Berg and Vikström, 2019). For sanctions, results by Van den Berg and Vikström (2014) show that they increase job finding rates, and encourage job seekers to accept lower wages and work on a lower occupational level. Given the adverse effects of unemployment on the individual health status, one could expect that both policies may have an *indirect effect* on treated job seekers through their positive impact on the individual labor market prospects.

[11] In addition to this, UI benefits can be reduced upon inflow into unemployment, if the individual has left employment without a valid reason, in which case UI is suspended for a maximum of 45 days. We do not analyze this type of sanction because it concerns actions and violations that took place before the individuals became unemployed.

Exposure to the policy measures could also have different *direct effects*, however. Participating in a training program affects the job seekers' daily routines as they typically have to attend the training course every working day of the week over a period of several months. This could be beneficial for health since unemployed individuals often suffer from a lack of structured daily routines (Goodman et al., 2017). The participation is also assumed to increase social interactions which can improve health (see, e.g., Cohen, 2004) and may fulfill some of the needs met by employment, notably psychosocial needs and a sense of control. The acquisition of new skills may lead to higher levels of self-esteem and may thereby improve mental health as well (Axelsson and Ejlertsson, 2002; Waters and Moore, 2002). Of course, attending a training course for several months imposes time constraints that can prevent them from visiting a health care worker or picking up medication at the pharmacy. The sickness absence outcome variable is less sensitive to this.

Sanctions do not directly influence the daily routines of treated individuals but the reduced financial means as well as unpleasant interactions with authorities may increase the stress level, which is assumed to have negative health consequences (Cohen, 1996). Moreover, given the negative implications of a benefit sanction, job seekers who have been notified might have incentives to avoid or at least to postpone the imposition of the sanction. Therefore, they might try to get a medical certificate, respectively to report sick, which would suspend the sanction for the period of sickness absence. Previous evidence by Van den Berg et al. (2019) for Germany, which has a similar sanctions regime as Sweden, albeit with historically higher sanction rates, indicates that only a small share of sickness absence in unemployment reflects attempts to avoid benefit sanctions, while drug prescriptions should be less susceptible to this than registered sickness absence.

3 Data and Empirical Strategy

3.1 Data Registers

Our study is based on data from several Swedish administrative records with labor market and health information. The first register, called *Datalagret*, from the Swedish Public Employment Service (PES) covers all registered unemployed persons. It contains day-by-day information on the unemployment status. This includes UI eligibility, participation in training programs, and the reason for the unemployment spell to end. Second, we exploit information provided by the unemployment insurance funds on all benefit sanctions ($ASTAT$), including the timing, the main reason, and the size of the benefit reduction. Third, the population register (*Louise*) provides yearly information on the entire Swedish population, with a set of socio-economic and background variables (e.g., age, sex, income, immigration status, marital status, employment status and social insurance benefits).

For the empirical analysis, we consider all new unemployment spells between January 2006 and December 2007 of individuals between 20 and 60 years. We randomly draw one entry if an individual enters unemployment several times during the observation period. Moreover, we exclude all individuals who had been registered at the PES within the six months preceding unemployment. The latter ensures that we only consider fresh entries into unemployment (so no returnees from ALMP or periods of sickness, etc.). Our final estimation sample includes 368,487 unemployment spells, with 7,725 individuals who participated in a training program and 2,898 individuals with a sanction.

Besides this labor market information, we construct health outcomes based on the *Swedish Prescribed Drug Register*, which was established in July 2005 and is maintained by the Swedish National Board of Health and Welfare. It contains information on the universe of individual drug prescriptions, including the type of medication and the date of the prescription. Validation studies show that the quality of the register is high (Wettermark et al., 2007), and it is used in various epidemiological research studies (see, e.g., Kramers, 2003; Hollander et al., 2013; Mezuk et al., 2014). All drugs are classified by 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.[12] Each bottom-level ATC code stands for a pharmaceutically used substance, or a combination of substances, in a single indication (or use).[13]

3.2 Health Variables

We use the prescription records to capture a small set of health problems that may respond (and be diagnosed) relatively fast after a trigger such as exposure to a labor market policy measure. This allows to measure short-run health responses and should help to avoid issues with

[12]This classification system is controlled by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOCC). The first version was introduced in 1976.

[13]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.

reverse causality. The health problems need to be relatively common among typical unemployed individuals in order to have some statistical power.

First, we consider *cardiovascular diseases* (ATC Code C), which deals with disorders of the heart and blood vessels. This constitutes the number one cause of death globally. Common examples include, e.g, high levels of blood pressure, strokes and heart attacks. It covers 22% of all Swedish prescriptions in 2006–2007 and includes beta-blockers, ACE inhibitors (high blood pressure) and statins (to lower cholesterol levels).[14]

Second, we consider *mental health problems* (ATC Codes N05 and N06) including stress and depressions. They account for about 15% of all prescriptions in 2006 and 2007 and the majority involves antidepressants, sedatives and anti-anxiety agents.[15] Notice that somatic conditions are often connected to mental health problems (Üstün and Sartorius, 1995; Wood et al., 1998; Pickering, 2001) so that the health outcome variables may capture a common underlying health problem and/or a broader set of health effects. In the analyses we use binary indicators of whether an individual has been prescribed drugs for each of the two categories, in periods of up to six months. Such periods should not be too small as there may be some time in-between the onset of disease and obtaining a drug prescription. Summary statistics are discussed in Subsection 3.3 below.

We examine sickness absence within the UI system as a supplementary outcome. This may also capture health effects that do not directly lead to drug prescriptions. We consider two different measures: (i) the number of days receiving sickness benefits and (ii) the occurrence of sickness spells that last for more than seven days. This is motivated by the facts that individuals have to submit a doctor's certificate when receiving sickness benefits for more than seven days and doctor visits are often accompanied by a prescription.

3.3 Empirical Strategy

The key challenge is to deal with selectivity in exposure to the labor market policy measures. As discussed by Eriksson (1997), Carling and Richardson (2004) and Richardson and Van den Berg (2013), caseworkers have a large influence and a large degree of discretionary power over

[14]Weber and Lehnert (1997) summarizes the strong evidence for a positive association between unemployment per se and cardiovascular health. The risk of cardiovascular diseases is strongly connected to an individual's lifestyle factors that may also be related to unemployment, such as stress, diets, physical activity, as well as smoking and drinking habits (see e.g. Mattiasson et al., 1990; Anderson et al., 1991; Janlert et al., 1992; Eriksson et al., 2006).

[15]Unemployment per se is widely found to be negatively associated with psychological well-being and mental health; see, for instance, Iversen and Sabroe (1988), Clark and Oswald (1994) and Maier et al. (2006).

training enrollment and sanctions and hence may base their decision to some extent on the job seeker's health status. To deal with this, we apply conditional difference-in-differences (DiD). For sake of brevity we refer to training and/or sanctions as the "treatments".

Accounting for observed heterogeneity: Treated and non-treated individuals may be different before becoming unemployed. As shown in Table 1 about 4% of the sample had a prescription related to the cardiovascular diseases and 10% related to mental health problems within the last six months before the entry into unemployment. However, participants in training programs seem to have a better health status, as they less often had prescriptions in the past than non-participants. Although those who received a sanction are similar to the non-treated with respect to previous drug prescriptions, there are various statistically significant differences in socio-demographic characteristics and labor market histories with respect to the control group for both types of treatments. For instance, participants in training programs are more often male, less likely to hold an university degree, more often have young children, and had higher earnings in the past. Recipients of benefit sanctions are slightly more often male, are substantially older, less likely to be Swedish citizens and are more often married. Regarding their labor market histories, they have more unemployment experience and higher income in the previous three years.

We therefore account for various background characteristics including socio-demographic information and labor market histories which have consistently been shown to affect selection into labor market programs (Dolton and Smith, 2011; Lechner and Wunsch, 2013). We also adjust for various other types of previous prescriptions before the beginning of the unemployment spell. Specifically, we include separate dummy variables indicating whether the individual redeemed a prescription related to one for the 14 top-level ATC codes to account for existing health differences between treated and control at entry into unemployment. We use inverse probability weighting (IPW) with weights obtained from logit estimations to adjust for these characteristics (see, e.g., Hirano et al., 2003; Busso et al., 2014).[16]

Since the selection of individuals into the treatment is likely to change over the course of the unemployment spell, we explicitly take the elapsed unemployment duration before the treatment

[16] We examine the mean standardized bias (MSB) (Rosenbaum and Rubin, 1983), which assesses the distance of the covariates before and after weighting, as a useful way to summarize the degree to which the weighting procedure reduces any potential bias induced by differences with respect to observed characteristics. For all cases, we see a substantial reduction of the overall MSB relative to the raw MSB before applying the weighting procedure (see Table A.1 in Appendix A).

into account. Specifically, for each month $t = 1, ..., 12$ after entry into unemployment we compare individuals who start the treatment in month t with those who are also unemployed at least until month t, but have not been treated yet (although they may enter the treatment later on, see e.g. Abbring and Van den Berg, 2003; Sianesi, 2004; Fredriksson and Johansson, 2008).

Accounting for selection due to unobserved confounders: DiD allows us to explicitly account for any time-constant unobserved differences between treated and comparison individuals if both groups follow the same trend. Moreover, since prescription outcomes are also available in the time period between the entry into unemployment and the start of the treatment, we can explicitly account for health differences in unemployment prior to the treatment.

Table 2 shows the prescription fractions in the six-month window before, respectively after the start of the treatment. Note that in the aggregate there is an increase in the prescription fraction when going from pre- to post-treatment, suggesting a negative effect of longer unemployment on health. The increase in prescription fractions is smaller for participants in training programs compared to the non-treated, while there is no clear pattern for those who receive a sanction.

For the empirical analysis, we consider ΔY_i as the individual outcome of interest, which refers to the difference between an indicator of having a prescription before and after a potential treatment. The pre-treatment reference period is limited to a six-month window before the start of the treatment. Recall that the prescription drug records are only available from July 2005 onwards. Since individuals may be notified about an upcoming treatment in the month before the actual start of the treatment and this may affect their health in that month, we only consider prescriptions in the five preceding months ($t-6$ to $t-2$) to determine the reference level. Obviously, such a reference period may include part of the previous employment spell. As mentioned in Section 2.1, usually patients are allowed to buy the prescription drugs for three months at a time, so within the six-month reference period all patients with an existing prescription need to visit the pharmacy for new drugs, and this will show up in our data.[17]

[17] For instance, for a job seeker who is treated in the third month of the unemployment spell, the reference period covers the first month of the unemployment spell ($t-2$ relative to the treatment start) and the last four months of the previous employment spell ($t-3$ to $t-6$ relative to the treatment start).

This gives two treatment groups (training and sanctions) and two control groups. For each group, we estimate the effect of the treatment for a given month of the elapsed unemployment duration:[18]

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

where $E[\Delta Y_{it}^1 | D_{it} = 1]$ denotes the differences in the outcome variable over time for a treated individual in month t and can be observed in the data, while the expected counterfactual outcome $E[\Delta Y_{it}^0 | D_{it} = 1]$ is inferred from comparable individuals in the control group. Using the IPW framework, the estimated treatment effect is then given by:

$$\tau = \sum_{t=1}^{12} \frac{n_t}{n} \left\{ \frac{1}{n_t} \sum_{i}^{n_t} \frac{D_{it} \Delta Y_{it}}{\hat{p}_t(X_i)} - \frac{1}{n_t} \sum_{i}^{n_t} \frac{(1 - D_{it}) \Delta Y_{it}}{1 - \hat{p}_t(X_i)} \right\}, \tag{2}$$

where n_t denotes the number of individuals who are still at risk of being treated in a given month t while $\hat{p}_t(X_i)$ characterizes the estimated propensity that individual i is treated in month t. For our supplementary outcomes, sickness absence and employment, we estimate treatment effects using IPW, but do not account for pre-treatment levels.[19]

3.4 Comparing treated and controls before the moment of treatment

The plausibility of the common-trend assumption in DiD analysis is usually checked by examining outcomes among treated and controls before the moment t at which the treated are exposed to the treatment. As is well known, anticipation of a treatment may cause such pre-treatment trends to deviate shortly before t, so that it is often advisable to examine outcomes further back in time for this purpose. In our setting, we face an additional methodological caveat as we do not observe (warnings of) sanctions that are never realized because the individual leaves unemployment before the sanction is imposed. Individuals may use information about future treatments in such a way that they leave unemployment before the treatment takes place. In such cases, their treatment is not realized and we do not observe the planned date of the treatment. Such individuals leave unemployment before t and enter neither the treatment group at t nor the control group for t. They may be systematically different from those who do end up

[18]The overall treatment effect is then obtained by the weighted average of the effects for each month t, where weights are given by the share of individuals who is still at risk of being treated in a given month.

[19]Employment outcomes are not defined in the pre-period by design as everyone is unemployed when starting the treatments. Moreover, the data at our disposal do not contain detailed information on sickness absence before entry into unemployment because employers pay sickness benefits during the first 14 days. The period in-between entry into unemployment and exposure to a policy measure is not suitable for capturing pre-exposure sickness absence, as the length of this period among those who are exposed varies in a way that is difficult to incorporate in the analysis.

being treated at t, as (i) they dislike the treatment and (ii) their labor market conditions are relatively favorable since they are able to obtain a job before t. The fact that they dislike the treatment may mean that their health would suffer from exposure to the treatment. In that sense any presence of anticipation of a sanction may lead to an under-estimate of its negative effect on health.

Pre-trends: With this in mind, we proceed with the discussion of the common-trend assumption by estimating the difference of the drug prescription prevalence between treated and controls for the months $t-5,..,t-2$, relative to $t-6$ (the first month of the observation period). As shown in Figure 6, there are no statistically significant differences until and including $t-2$. This provides suggestive evidence that the two groups display a common trend in health in this period.

As mentioned above, for individuals who are treated within the first six months of the unemployment spell, the pre-treatment period includes part of the previous employment spell, while for those who are treated from month seven onwards the pre-treatment period falls completely within the unemployment spell. To test whether this affects the validity of the common trend assumption, Figure A.2 in Appendix A divides the sample with respect to the elapsed unemployment duration and shows separate pre-trends for potential treatments with $t+1$ to $t+6$, respectively $t+7$ to $t+12$. The findings are not at odds with the common trend assumption for the period until and including month $t-2$, for every specification.

The month immediately prior to the treatment: The results in Figure 6 for $t-1$ also suggest that there is a health response to notifications about the upcoming treatment. This response is rather small and statistically insignificant in the case of training[20], but stronger for mental health problems in the case of sanctions. Specifically, in the month before the sanction is imposed, but after the job seeker may have received a warning, the prescription fraction increases by 0.9 percentage points, which represents a treatment effect of about 37% relative to the control group. This is statistically significant at the 1%-level. Presumably, the threat of a benefit reduction increases the stress level which in turn affects mental health.

[20]Even though the effect is insignificant, we see an increased rate of prescriptions related to cardiovascular diseases for training participants in the month before the treatment. One could speculate that this is a by-product of the job seeker's attempt to avoid the treatment by getting a doctor's certificate. This, however, seems unlikely for two reasons. First, as discussed in Subsection 2.2, only individuals who indicate their interest in participating are invited to information meetings with the caseworker. Second, participants have a 0.43 percentage points lower probability to report sick for more than seven days in the month before the program start ($p < 0.01$), which contradicts the notion that they try to avoid the treatment.

Individuals who anticipate a future sanction may try to avoid it by visiting a health care center to obtain a medical certificate enabling them to claim sickness. A larger number of prescriptions could then be a by-product of the visit to the health care center. There is, however, no increase with respect to the likelihood of sickness absence in the corresponding month (τ = 0.002; $p-$value = 0.505). We conclude that a causal pathway running through a medical certificate is unlikely to be relevant.

4 Results

4.1 Direct and Indirect Health Effects of Labor Market Policies

For all outcome variables, we distinguish between three different time periods ($t+1$ to $t+3$, $t+1$ to $t+6$ and $t+1$ to $t+12$). For drug prescriptions, we only consider the first prescription that the individual received after the start of the treatment.[21] For sickness absence, we distinguish between the average number of days receiving sickness benefits per month and an indicator variable for the occurrence of a sickness spell that lasts at least seven days and therefore requires a doctor's certificate. The results are summarized in Table 3.

Training: As shown in columns (1) and (2) of Table 3, there is a substantial reduction with respect to the likelihood of having a prescription related to cardiovascular and mental health problems for training. Over the course of 12 months, training participants have a lower probability of receiving a prescription related to the cardiovascular system of 0.44 percentage points, while the effect on mental health problems is 0.63 percentage points. Relative to the baseline level of the control group, this refers to treatment effects of 7.5%, respectively 6.5%. We also estimate the total effect (over the period $t-1$ to $t+12$), taking both the post-treatment effect and the effect at $t-1$ into account.[22] Although both effects, for cardiovascular diseases and mental health problems, become slightly smaller, there is still a significant reduction of the probability to receive a prescription, which is very similar to the post-treatment effect. The estimation results for sickness absence confirm our findings for drug prescriptions. Participating in a training program leads to a substantial reduction with respect to number of days receiving sickness benefits by about 20% (column 3) and the likelihood to experience a sickness spell that is longer than seven days by about 12% (column 4) over the course of one year after the

[21] Panel A of Table 5 shows also estimates considering an indicator for having more than one prescription within 12 months after the treatment as the outcome variable. The results are similar to the baseline estimates.

[22] This allows us to draw inference about the relevance of temporal shifts of doctor visits, respectively drug prescriptions to the pre-treatment period due to the announcement of the training program.

start of training. We conclude that training has a favorable impact on the participants' health status with fewer prescriptions related to cardiovascular and mental health problems, as well as a reduced likelihood of sickness absence.

Moreover, in column (5), we analyze the employment effects of training programs to examine the importance of direct, respectively indirect effects. There is a substantial lock-in effect for participants in training programs of about 4.8 percentage points within the first three months after the enrollment, which turns into a strong positive effect one year after the program start, in line with previous evaluations of training programs in Sweden (see, e.g., Richardson and Van den Berg, 2013). This means that there is no evidence for a connection between the health and employment effects: the reduction of drug prescriptions for participants in training starts when individuals are still enrolled in the program and before the positive effect on re-employment prospects occurs. This suggests that the more direct effects of training, such as the change of daily routines and more favorable social contacts, seem to be more relevant for the participants' health status than the indirect effect through improved employment outcomes.

Sanctions: Panel B of Table 3 shows the effects of benefit sanctions. There are no significant effects on the likelihood of receiving drug prescriptions. This is true shortly as well as several months after the sanction was imposed and holds for both types of prescriptions. However, when considering sickness absence, the overall pattern looks very different. After a sanction is imposed, we observe a substantial increase with respect to the number of days receiving sickness benefits (column 3) and the likelihood to report sick for at least seven days in a row (column 4). Both effects are particularly pronounced in the first months after the sanction. There are two potential explanations for the differential effects of sanctions on drug prescriptions and sickness benefits. First, benefit sanctions could have negative health effects that are not directly associated with drug prescriptions, but cause job seekers to call in sick.[23] For instance, the increase in mental health problems observed in the month before the sanction is imposed might translate into more frequent sickness absence only with a time lag. Alternatively, job seekers might use sickness absence strategically to mitigate the economic consequences of a sanction and to avoid a future sanction. One reason is that when reporting sick individuals transfer from UI benefits to sickness benefits and the sanctions only apply to UI benefits. A more indirect reason is that calling in sick

[23]Table 5 (Panel B) shows treatment effects on any other type of drug prescriptions indicating that there is no deterioration of the health status in other dimensions that could explain the increase in the receipt of sickness benefits.

may help to avoid future sanctions since job seekers only face a minimum of search requirements when reporting sick (see also Van den Berg et al., 2019). Since 7.6% of the sanctioned individuals receive a second sanction, there is a real risk of receiving a subsequent sanction.

Finally, as shown in column (5), we find significant employment effects of sanctions (reemployment probability increases by 3.5 percentage points in the first six months). These positive employment effects may also explain why we do not find any negative post-treatment effects for sanctions on drug prescription: any negative health effects due to increased financial stress could be canceled by positive health effects due to the higher re-employment rate. Since we only observe the receipt of sickness benefits for individuals who remain unemployed, while we observe drug prescriptions for all individuals, this could explain the differential findings.

Overall, our 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 temporary reduction of financial means through the imposition of a sanction. The favorable effects of training on the participants' health condition seem to be unrelated to the employment effects, as they begin to evolve when participants are still locked in the program. Notifications regarding benefit sanctions can lead to a short-run increase in prescriptions related to mental health problems. While there is no clear evidence for longer lasting health effects, sanctioned job seekers call in sick more often which might reflect attempts to avoid future sanctions or the consequence of the increased mental health problems in response to the warning.

4.2 Effect Heterogeneity

To investigate the potential mechanisms discussed in Subsection 2.3, we now examine heterogeneous treatment effects with respect to different background characteristics. Specifically, we consider the job seekers' initial health status and their educational background. The results are summarized in Table 4. All estimates refer to the post-treatment effect measures over the period $t+1$ to $t+12$. Since there is only limited evidence that sanctions affect drug prescriptions, we focus our discussion on the heterogeneous effects of training programs, but for completeness we also report all results for sanctions.

Initial health status: Job seekers who are informed about the upcoming training program may shift doctor visits from the post- to the pre-treatment period because they anticipate time constraints after the start of the program. Since drug prescriptions are typically connected to doctor visits, this might also lead to a temporal shift of prescriptions. To test the relevance of this mechanism, we divide the estimation sample with respect to the individuals' initial health status. The idea is that temporal shifts of visits to doctors (a similar argument applies for pharmacy visits), are more relevant for individuals who already have a concrete health issue before the training program. To this end, Panel A of Table 4 shows separate treatment effects for individuals with and without an existing prescription within six months before the entry into unemployment. We can see that the reduction of drug prescriptions for participants in training programs is driven by individuals without an existing prescription. This is unlikely to be the consequence of temporal shifts of doctor visits since such a preventive behavior should only be relevant for individuals who are already aware of their health issue. This provides additional evidence that positive post-treatment and overall effects for training reflects an improvement of the participants' health status relative to the non-participants.

Education: Individuals with lower levels of education face the highest risk of lacking daily routines, meaningful social contacts and suffer from low levels of self-esteem when being unemployed (Waters and Moore, 2002). Therefore, we consider heterogeneous effect with respect to the level of education and distinguish between individuals who only have compulsory education, which ends after nine years of schooling and those who have a secondary degree or higher. As shown in Panel B of Table 4, the findings reveal that the favorable effects of training on the participants' 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. This supports the notion that the training program fulfills some of the needs typically met by employment, such as structured daily routines, social contacts, or improvements of self-esteem.

4.3 Robustness Analyses

In the following, we test the sensitivity of our results with respect to different sources of potential biases. Specifically, we consider different definitions of health outcomes based on the prescription records, take into account the presence other potential health effects, as well as other labor market programs. The results are summarized in Table 5 and discussed in detail below. Overall, the robustness analysis strongly supports the validity of our empirical approach.

Follow-up prescriptions: In our main specification, we only consider the first prescription after a potential treatment since all subsequent prescriptions might be a consequence of the first prescription. However, this approach neglects all potential treatment effects on follow-up prescription. To test the robustness of the estimated treatment effects with respect to subsequent changes in drug prescriptions, we consider an alternative indicator, which takes the value one if the individual has two (or more) prescriptions related to either cardiovascular diseases or mental health problems within 12 months after the potential treatment. As shown in Panel A of Table 5, the estimated treatment effects are almost the same as in our baseline specification (Table 3), which indicates that potential effects on follow-op prescriptions are negligible.

Other drug prescriptions: As discussed in Subsection 3.2, previous evidence from the medical and psychological literature suggests that potential health effects of labor market policies mainly manifest in cardiovascular and mental health problems. To test whether there are effects on other types of drug prescriptions, we now also consider ten alternative top-level ATC codes and estimate treatment effects on two outcome variables: (i) an indicator variable that refers to any other prescription within 12 months after the treatment and (ii) an index variable that takes values from zero to ten depending on the number of different top-level ATC codes with a prescription in a given period.[24] As shown in Panel B of Table 5, training programs have no effect on other drug prescriptions, while benefit sanctions seem to reduce the likelihood to have other prescriptions, but the effect is not clear-cut. The treatment on the indicator variable is statistically significant at the 10%-level, while the effect on the index is insignificant at conventional levels. Overall, there is only little evidence that other drug prescriptions play an important role when examine the health effects of labor market policies.

Other labor market programs: In Panel C, we exclude about 10% of the individuals in the control group who participate in other labor market programs during the first 12 months of the unemployment spell.[25] We therefore take into account that other policies, such as workfare programs or wage subsidies, might have health effects as well. The results show that the magnitude of estimated treatment effects of training programs becomes slightly larger compared to the baseline estimates. This indicates that, if at all, other programs might also have some

[24] Out of the 14 overall top-level ATC Codes, we exclude (C) Cardiovascular systems and (N) Nervous system (since both mainly refer to our main outcome variables of interest), as well as (G) Genito-urinary system and sex hormones (includes oral contraceptives and might be affected by fertility decisions rather than health effects) and (V) various.

[25] The three most common other labor market programs are work practice, intensified job counselling and start-up subsidies.

favorable health effects, but the impact seems to be limited. The estimated effects of sanctions are almost unaffected and still statistically insignificant.

5 Conclusions

The adverse connection between unemployment and the individual health status has been widely confirmed by previous studies. However, the unintended consequences of policies that are primarily designed to improve employment prospects on the participants' health status have been largely neglected so far. We combine Swedish administrative data on the universe of individual drug prescriptions with detailed labor market and sickness absence records and provide first and comprehensive evidence on the health effects of two commonly used labor market policies representing different reintegration strategies. Our results show that participating in a training program that aims to help participants acquiring new skills can be also beneficial for the health status of the unemployed worker as it reduces the likelihood to hold prescriptions for cardiovascular diseases and mental health problems. This effect is accompanied by reduced sickness absence and appears before the treatment could create a positive impact on the participants' employment status. The findings are particularly pronounced for individuals with a low level of education, who might face a higher risk of lacking daily routines when they are unemployed. We conclude that training programs improve the individual health status due to the direct effect on participants' life. Other behavioral responses, such as less frequent visits to the doctor or the pharmacy due to time constraints during the treatment, might play a role in the timing of prescriptions, but they are unlikely to explain the overall effects.

The imposition of benefit sanctions, which are restrictive interventions that financially punish non-compliance with UI guidelines, has no long-run effect on subsequent prescriptions for cardiovascular and mental health problems, but job seekers who receive a sanction call in sick more often. Although we cannot completely rule out that the latter partly reflects an attempt to avoid future sanctions, the findings suggests that the threat of an upcoming benefit sanction has negative health effects, at least in the short-run.

Our results are an important step towards a more holistic evaluation for the most relevant reintegration tools for unemployed workers in many industrialized countries. In particular, the reduction of health issues due to the participation in training programs would lead to a more favorable cost-benefit-ratio as they reduce health care expenditures. Our results imply a connection between labor market policies and health care expenditures, as more (less) drug prescriptions directly increase (decrease) the associated costs. Although the direct effects on drug prescriptions seem to be prevalent in our case – compared to the indirect effects through improved employment prospects – the interrelation of labor market policies policies, health and employment outcomes is surely a worthwhile focus for future research. In particular, improvements of the participants' health status might also have positive implications for their long-run employment prospects, which reduces future benefit payments and increases tax revenues.

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Tables and Figures

Table 1: Selected differences in baseline characteristics and prescription histories

	Non-treated	**Treated** A. Training	$P-$value	B. Sanctions	$P-$value
No. of observations	357,864	7,725		2,898	
Pre-unemployment health outcomes					
Prescription within last six months related to					
Cardiovascular diseases	0.040	0.035	0.030	0.040	0.957
Mental health problems	0.104	0.096	0.020	0.106	0.652
Total no. of prescriptions[(a)]	2.442	2.060	0.000	2.261	0.111
Background characteristics					
1) Socio-demographic information					
Female	0.526	0.337	0.000	0.475	0.000
Age categories					
20-24 years	0.247	0.270	0.000	0.210	0.000
25-34 years	0.342	0.351	0.135	0.316	0.003
35-44 years	0.231	0.236	0.291	0.232	0.919
45-54 years	0.131	0.118	0.000	0.172	0.000
55-60 years	0.048	0.026	0.000	0.071	0.000
Married	0.314	0.303	0.033	0.300	0.090
Educational level					
Compulsory school	0.219	0.223	0.350	0.207	0.122
Upper secondary school	0.465	0.544	0.000	0.529	0.000
Higher education	0.317	0.233	0.000	0.264	0.000
Children age 0-6					
One child	0.156	0.157	0.913	0.143	0.049
Two or more children	0.087	0.098	0.001	0.086	0.875
Local unemployment rate	0.057	0.060	0.000	0.056	0.001
2) Labor market histories					
Days in unemployment in year					
t-1	31.04	32.39	0.082	35.43	0.001
t-2	45.55	52.15	0.000	54.56	0.000
t-3	40.49	44.74	0.000	47.57	0.000
Eligible for UI	0.720	0.734	0.007	0.992	0.000
Wider job search	0.241	0.342	0.000	0.378	0.000
Registered as disabled	0.084	0.101	0.000	0.068	0.003
Yearly labor income in SEK in year					
t-1	83,824	85,733	0.140	126,115	0.000
t-2	79,644	79,512	0.916	116,053	0.000
t-3	77,702	78,010	0.801	106,795	0.000
Yearly UI benefits in SEK in year					
t-1	7,217	6,534	0.002	10,582	0.000
t-2	10,084	10,209	0.666	13,944	0.000
t-3	8,753	8,717	0.896	11,841	0.000
Yearly welfare benefits in SEK in year					
t-1	6,615	6,242	0.155	2,438	0.000
t-2	6,567	5,437	0.000	3,544	0.000
t-3	6,282	5,213	0.000	4,280	0.000

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.
[(a)]Include all prescriptions related 14 main ATC groups: (A) alimentary tract and metabolism, (B) blood and blood forming organs, (C) cardiovascular system, (D) dermatologicals, musculo-skeletal system, (G) genito-urinary system and sex hormons, (H) systematic hormonal preparations, (J) Antiinfectives for systematic use, (L) antineoplastic and immunomodulating agents, (M) musculo-skeletal system, (N) nervous system, (P) antiparasitic products, insecticides and repellents, (R) respiratory system, (S) sensory organs and (V) various.

Table 2: Differences in health and labor market outcomes

	Non-treated	**Treated**			
		A. Training	$P-$value	B. Sanctions	$P-$value
No. of observations	357,864	7,725		2,898	
Post-treatment outcomes within upcoming six months					
Prescription related to					
Cardiovascular diseases	0.048	0.030	0.000	0.048	0.725
Mental health problems	0.100	0.069	0.000	0.085	0.005
Sickness absence from UI	0.042	0.023	0.000	0.066	0.000
Exit from unemployment to work	0.233	0.289	0.000	0.353	0.000
Pre-treatment outcomes within previous six months					
Prescription related to					
Cardiovascular diseases	0.039	0.027	0.000	0.035	0.274
Mental health problems	0.088	0.062	0.000	0.083	0.335

Note: As not indicated otherwise, all variables relate to indicators whether the corresponding event (prescription, sickness absence or exit from unemployment) took place in the corresponding time interval. Pre-unemployment outcomes are measured within the last six months before the entry into unemployment. Post-treatment outcomes are measured within six months after a potential treatment. $P-$values refer to two-tailed t-tests based on equal means.

Table 3: Effects of labor market policy exposure on health outcomes and on employment

	Any drug prescription		Receipt of sickness benefits[(a)]		Exit from
	Cardiovascular diseases	Mental health problems	Avg. days per month	Any spell $\geq$ 7 days	unemployment to employment
	(1)	(2)	(3)	(4)	(5)
A. Training					
1) Post-treatment effects					
in $t+1$ to $t+3$	-0.0015	-0.0051**	-0.1812***	-0.0101***	-0.0478***
	(0.0017)	(0.0024)	(0.0280)	(0.0012)	(0.0037)
	[-3.7%]	[-5.7%]	[-28.9%]	[-37.4%]	[-31.8%]
in $t+1$ to $t+6$	-0.0028	-0.0057**	-0.2106***	-0.0130***	0.0348***
	(0.0018)	(0.0026)	(0.0396)	(0.0020)	(0.0052)
	[-5.8%]	[-5.7%]	[-23.5%]	[-29.3%]	[+14.9%]
in $t+1$ to $t+12$	-0.0044**	-0.0075***	-0.2607***	-0.0086**	0.1336***
	(0.0021)	(0.0028)	(0.0572)	(0.0035)	(0.0057)
	[-7.5%]	[-6.5%]	[-19.5%]	[-12.0%]	[+42.3%]
2) Total (incl. anticipation effect)	-0.0036*	-0.0063**	-0.2993***	-0.0126***	—
	(0.0021)	(0.0029)	(0.0592)	(0.0037)	
	[-5.1%]	[-4.4%]	[-20.9%]	[-15.1%]	
No. of observations	365,589	365,589	230,440	365,589	365,589
B. Sanctions					
1) Post-treatment effects					
in $t+1$ to $t+3$	-0.0014	0.0003	0.2714***	0.0162***	0.0093
	(0.0030)	(0.0046)	(0.0082)	(0.0043)	(0.0077)
	[-3.4%]	[+0.3%]	[+43.2%]	[+60.0%]	[+6.1%]
in $t+1$ to $t+6$	-0.0014	0.0003	0.2834***	0.0261***	0.0346***
	(0.0034)	(0.0049)	(0.0101)	(0.0062)	(0.0089)
	[-2.9%]	[+0.3%]	[+31.3%]	[+58.8%]	[+14.8%]
in $t+1$ to $t+12$	-0.0040	-0.0024	0.1042	0.0273***	0.0440***
	(0.0037)	(0.0053)	(0.1226)	(0.0081)	(0.0093)
	[-6.8%]	[-2.1%]	[+7.8%]	[+38.0%]	[+13.9%]
2) Total (incl. anticipation effect)	-0.0043	0.0035	0.1373	0.0306***	—
	(0.0037)	(0.0055)	(0.1299)	(0.0087)	
	[-6.7%]	[+2.8%]	[9.5%]	[+42.6%]	
No. of observations	360,762	360,762	227,245	227,245	360,762

Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation (column 1 and 2), respectively dynamic inverse probability weighting (column 3-5). Standard errors in parentheses and relative effects compared to the mean of the control group in square brackets. ***/**/* indicate statistical significance at the 1%/5%/10%-level.

[(a)] Considers only individuals who do not leave unemployment before the end of the corresponding interval.
Training: $N_{t+3} = 301,139$; $N_{t+6} = 261,917$; $N_{t+12} = 230,440$; $N_{total} = 230,440$.
Sanctions: $N_{t+3} = 296,318$; $N_{t+6} = 257,458$; $N_{t+12} = 227,245$; $N_{total} = 227,245$.

Table 4: Effects of labor market policy exposure on drug prescriptions for different subgroups

	A. Existing prescription				**B. Education**			
	No		Yes		Compulsory		Secondary or higher	
	Cardiov. diseases	Mental health	Cardiov. diseases	Mental health	Cardiov. diseases	Mental health	Cardiov. diseases	Mental health
1) Training	-0.0054***	-0.0080***	-0.0017	-0.0023	-0.0148***	-0.0154**	-0.0020	-0.0052*
	(0.0019)	(0.0025)	(0.0071)	(0.0106)	(0.0047)	(0.0060)	(0.0022)	(0.0031)
	[-10.2%]	[-7.7%]	[-1.9%]	[-1.4%]	[-20.1%]	[-12.4%]	[-3.6%]	[-4.6%]
No. of observations	301,345	301,345	64,244	64,244	79,953	79,953	285,636	285,636
2) Sanctions	-0.0013	0.0021	-0.0126	-0.0171	-0.0127	-0.0089	-0.0017	-0.0006
	(0.0037)	(0.0051)	(0.0112)	(0.0174)	(0.0083)	(0.0119)	(0.0041)	(0.0059)
	[-2.4%]	[+2.1%]	[-14.3%]	[-10.4%]	[-17.4%]	[-7.1%]	[-2.8%]	[-0.5%]
No. of observations	297,244	297,244	63,518	63,518	78,829	78,829	281,933	281,933

Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation described in Section 3.1. The outcome variable is given by an indicator for drug prescriptions within the first 12 months after the potential treatment ($t+1$ to $t+12$). Standard errors in parenthesis and relative effects compared to the mean of the control group in square brackets.. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 5: Robustness analysis for drug prescriptions within 12 months after treatment

	A. Follow-up prescriptions$^{(a)}$		**B. Other drug prescriptions**$^{(b)}$		**C. Other labor market programs**$^{(c)}$	
	Cardiovascular diseases (1)	Mental health problems (2)	Any other prescription (3)	Index other prescriptions (4)	Cardiovascular diseases (5)	Mental health problems (6)
Training	-0.0039**	-0.0063**	-0.0010	-0.0093	-0.0048**	-0.0084***
	(0.0020)	(0.0028)	(0.0045)	(0.0088)	(0.0021)	(0.0028)
No. of observations	365,589	365,589	365,589	365,589	331,650	331,650
Sanctions	-0.0016	0.0006	-0.0134*	-0.0223	-0.0042	-0.0028
	(0.0036)	(0.0053)	(0.0076)	(0.0159)	(0.0037)	(0.0053)
No. of observations	360,762	360,762	360,762	360,762	326,823	326,823

Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation. Standard errors in parentheses. ***/**/* indicate statistical significance at the 1%/5%/10%-level.

$^{(a)}$The outcome variable is an indicator for more than one prescription related to the corresponding health issues within 12 months after the potential treatment ($t+1$ to $t+12$).

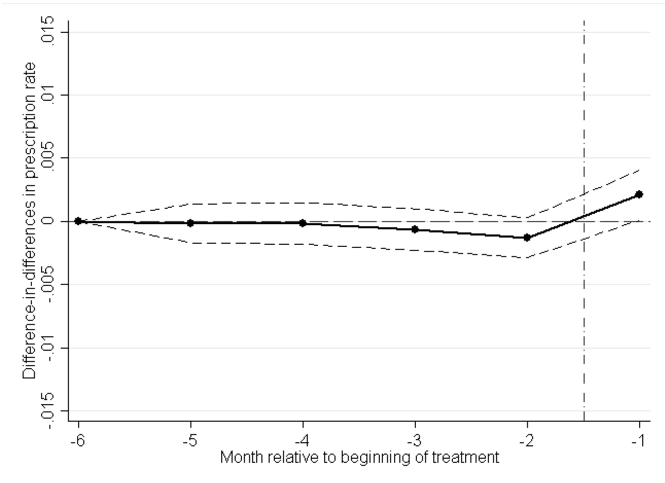
$^{(b)}$The outcome variable includes prescriptions related to other top-level ATC codes: (A) Alimentary tract and metabolism, (B) Blood and blood forming organs, (D) Dermatologicals, (H) Systemic hormonal preparations, excluding sex hormones and insulins, (J) Antiinfectives for systemic use, (L) Antineoplastic and immunomodulating agents, (M) Musculo-skeletal system, (P) Antiparasitic products, insecticides and repellents, (R) Respiratory system, (S) Sensory organs. Column 3 refers to an indicator for any prescription within these categories within the first 12 months after the potential treatment ($t+1$ to $t+12$). Column 4 refers to an index indicating the number of other top-level ATC codes with a redeemed prescription ($0 \equiv$ very low; $10 \equiv$ very high) in within the first 12 months after the potential treatment ($t+1$ to $t+12$).

$^{(c)}$Participants in other labor market programs within the first 12 months after entry into unemployment are excluded from the control group. The outcome variable is given by an indicator for drug prescriptions within the first 12 months after the potential treatment ($t+1$ to $t+12$).

Table 6: Differences in health outcomes in pre-treatment period

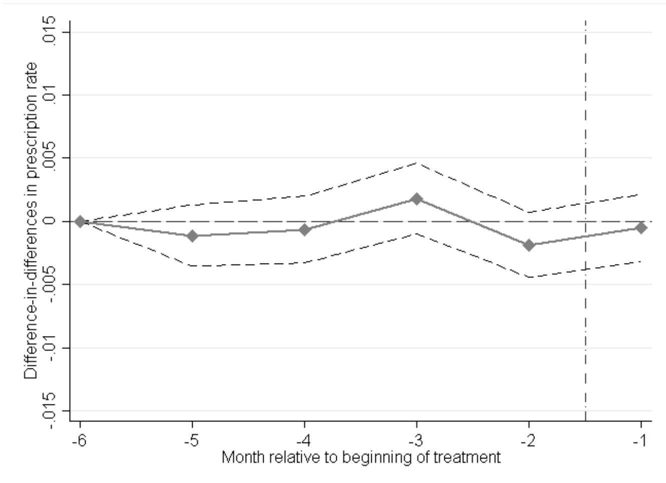
A.1 Training

Cardiovascular diseases



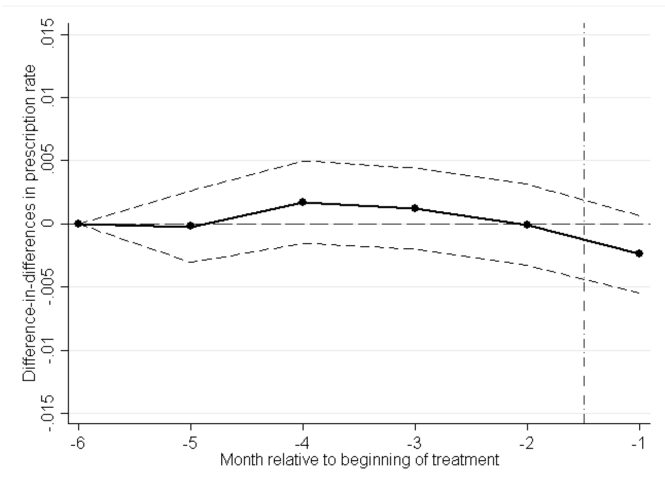
A.2 Training

Mental health problems



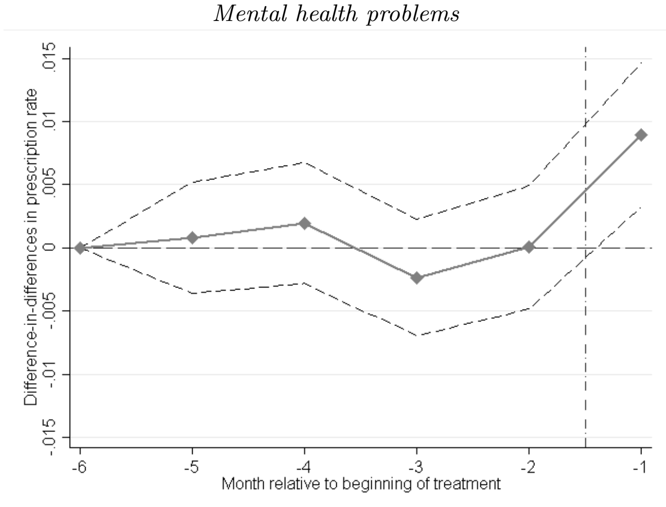
B.1 Sanctions

Cardiovascular diseases



B.2 Sanctions

Mental health problems



Note: Depicted are average effects on the treated within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes in the pre-treatment period $t-5$ to $t-1$ are measured relative to month $t-6$.
No. of observations: A. Training N=365,589; B. Sanction N=360,762.

A Supplementary Tables and Figures

Table A.1: Descriptive Statistics of the Propensity Score

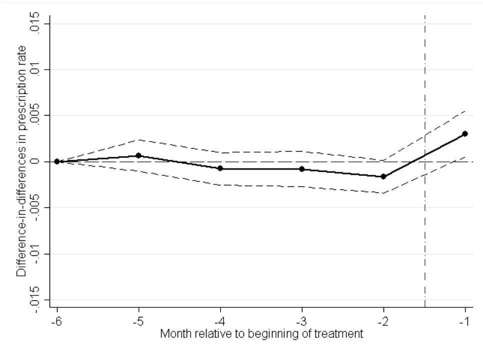
A. Training	No. of observations		Share treated	Pseudo-R^2	Mean standardized bias$^{(a)}$	
	Non-treated	Treated			before	after
	(1)	(2)	(3)	(4)	(5)	(6)
Elapsed unemployment duration						
1 month	365,281	308	0.0008	0.0445	9.4893	4.1600
2 months	341,116	895	0.0026	0.0485	9.2060	2.2567
3 months	294,585	1,109	0.0038	0.0453	8.8788	2.3090
4 months	244,489	1,096	0.0045	0.0383	8.9874	1.8501
5 months	209,956	808	0.0038	0.0478	9.2679	2.1639
6 months	186,494	721	0.0039	0.0441	8.9469	3.0644
7 months	167,691	625	0.0037	0.0451	8.5191	2.4353
8 months	152,413	568	0.0037	0.0371	7.4457	3.0957
9 months	138,187	467	0.0034	0.0501	9.5297	2.9427
10 months	127,087	416	0.0033	0.0450	8.2850	3.9400
11 months	117,302	410	0.0035	0.0554	10.0166	3.4857
12 months	108,699	302	0.0028	0.0535	9.4637	4.6057
B. Sanctions	No. of observations		Share treated	Pseudo-R^2	Mean standardized bias$^{(a)}$	
	Non-treated	Treated			before	after
	(1)	(2)	(3)	(4)	(5)	(6)
Elapsed unemployment duration						
1 month	360,541	221	0.0006	0.0653	12.8827	4.3513
2 months	336,819	452	0.0013	0.0710	13.1583	3.2860
3 months	291,004	393	0.0013	0.0737	14.0590	3.8785
4 months	241,669	335	0.0014	0.0701	12.4269	3.9740
5 months	207,700	244	0.0012	0.0754	12.5852	5.0130
6 months	184,713	246	0.0013	0.0659	11.7574	4.5508
7 months	166,331	204	0.0012	0.0773	13.1718	4.9735
8 months	151,436	185	0.0012	0.0926	13.6713	5.5980
9 months	137,504	173	0.0013	0.0787	12.2771	4.6195
10 months	126,657	163	0.0013	0.0688	12.1056	5.2821
11 months	117,123	159	0.0014	0.0917	15.4822	6.0734
12 months	108,699	123	0.0011	0.0981	15.7171	5.3112

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

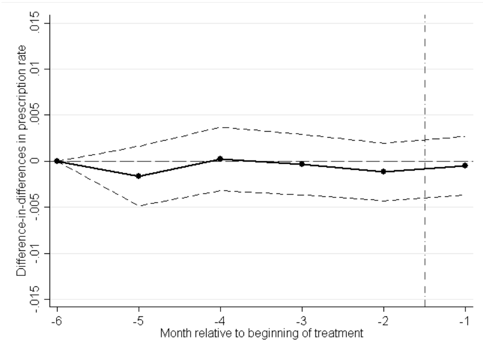
$^{(a)}$Standardized bias for variable x is defined as: $SB(x) = 100(\bar{x}_c - \bar{x}_t)/\sqrt{\frac{1}{2}(s^2_{xc} + s^2_{xt})}$, with $\bar{x}_c$ being the mean of the control group, $\bar{x}_t$ the mean of the treatment group, s^2_{xc} the variance of the control group and s^2_{xt} the variance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

Table A.2: Differences in health outcomes in pre-treatment period by timing of treatment

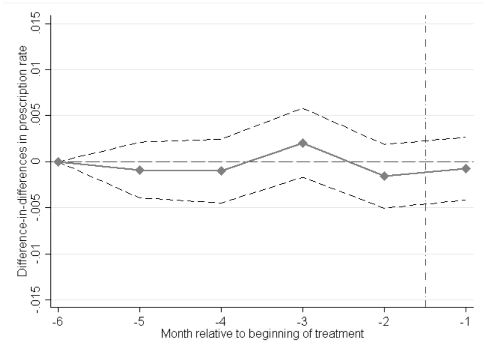
Training
Cardiovascular diseases
(a) Treatment month 1-6



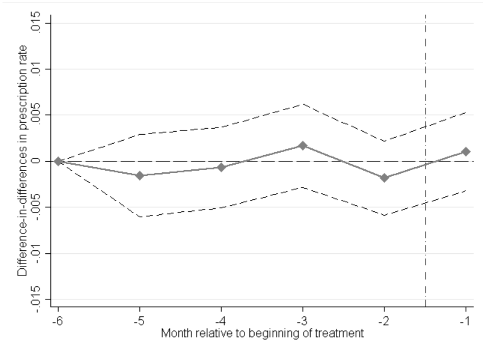
(b) Treatment month 7-12



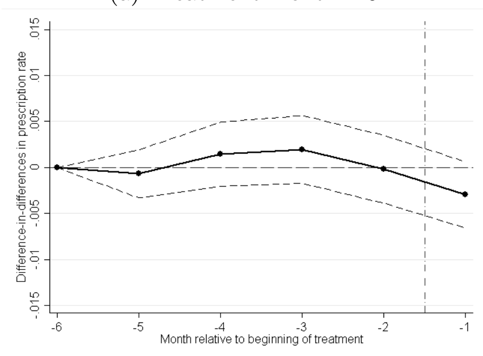
Training
Mental health problems
(a) Treatment month 1-6



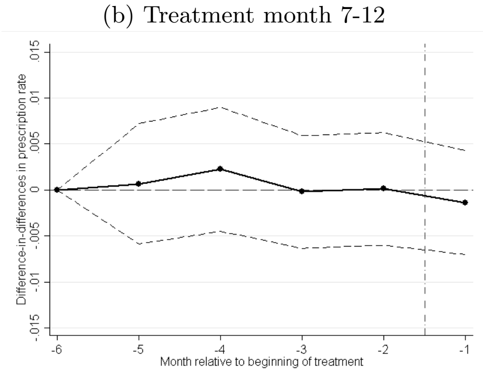
(b) Treatment month 7-12



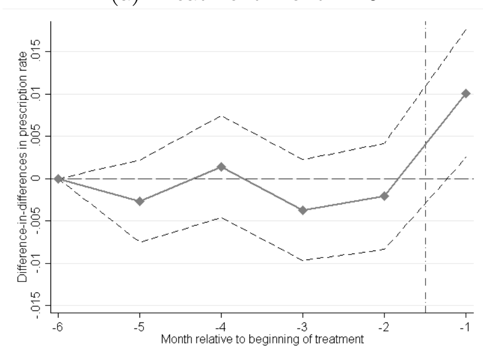
Sanctions
Cardiovascular diseases
(a) Treatment month 1-6



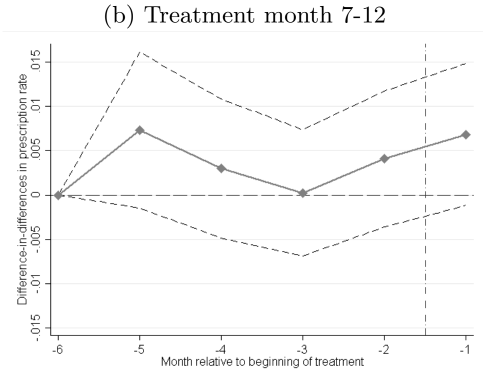
(b) Treatment month 7-12



Sanctions
Mental health problems
(a) Treatment month 1-6



(b) Treatment month 7-12



Note: Depicted are average treatment effects on the treated within the first 12 months using inverse probability weighting (IPW) and 90% confidence intervals. Outcomes in the pre-treatment period $t-5$ to $t-1$ are measured relative to month $t-6$.