Health Effects of Labor Market Policies: Evidence from Drug Prescriptions

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We exploit individual-level labor market and prescription drug records to study unintended effects of labor market policies on participants' health status. We examine two popular and commonly used interventions that represent different reintegration strategies for unemployed workers: training programs and benefit sanctions. To establish a causal relationship we exploit the longitudinal aspect of the prescription records and estimate dynamic difference-in-differences models comparing treated and non-treated individuals before and after the treatment. Our results show that supportive interventions, such as training programs, reduce drug prescriptions related to cardiovascular and mental health diseases by about 6-7% within a year after the program start. The direct effect of participating, e.g. due to a change of daily routines, seem to be more important than the indirect effect through improved employment prospects. Restrictive interventions, such as benefit sanctions, have no long-lasting effects on drug prescriptions, but the notification of an upcoming sanction leads to a short-term increase in prescriptions for mental health issues, which is possible induced by higher stress levels.

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1 Introduction

The connection between individuals' employment situation and their health status has been widely investigated by the medical, psychological and economic literature. It has been shown that job displacement can have various negative implications. For instance, it can be associated with harmful health-related attitudes such as more smoking and alcohol consumption (Eliason and Storrie, 2009; Black et al., 2015), drug use (Carpenter et al., 2017), 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). It also well-documented that unemployment (Dooley et al., 1996; Catalano et al., 2000; McKee-Ryan et al., 2005) is directly connected to worse mental and physical health conditions, which can be provoked by reduced financial means (Ettner, 1996; Benzeval and Judge, 2001) and a lower socio-economic status (Adler et al., 1994).[1]

However, despite the extensive literature on the connection between unemployment and health, only a few studies have examined how public policies can influence this connection. While there is some evidence for the connection between health-related policies and labor market outcomes (see e.g. Currie and Madrian, 1999), only a small – but growing – literature analyzes the relationship between labor market policies that are primarily designed to improve re-employment prospects of unemployed individuals and health-related outcomes (Huber et al., 2011). The existing evidence originates to a large extent from the psychological literature and typically relies on *subjective measures* of well-being (see Coutts et al., 2014; Puig-Barrachina et al., 2019, for overviews). Participants in supportive programs, like job search assistance (Vinokur et al., 2000; Vuori and Silvonen, 2005), training (see Creed et al., 1998; Machin and Creed, 2003) or work practice (see Oddy et al., 1984; Strandh, 2001) in general report less psychological symptoms related to mental health issues and higher levels of subjective well-being (Andersen, 2008; Crost, 2016; Wulfgramm, 2011). However, beside concerns about the causal interpretation and the external validity of these results, it is unclear whether this pattern translates into improvements of objective measures of the participants' health. For instance, training programs, as a key element of labor market policy in many countries, may require a high level of participants'

[1]Beside the negative health effects of unemployment, which seem to dominate on the individual level, another strand of the literature emphasize that less economic activities on the macroeconomic level, e.g. an economic downturn, may also have positive health effects through reduced job-related stress and less time-constraints (Ruhm, 2000, 2003, 2005; Adda, 2016; Cutler et al., 2016).

commitment and the personal assessment of the program[2] could influence subjective measures of well-being without affecting objective health-related outcomes. Moreover, very little is known how activation policies that aim to incentivize a certain individual behavior affect well-being. Since a more generous unemployment insurance system is associated with higher levels of life satisfaction (Ochsen and Welsch, 2012), it could be expected that restrictive policy interventions may lead to increased social and economic stress, possibly associated with negative health effects.

This paper therefore uses drug prescriptions, in combination with episodes of sickness benefit receipts, as *objective measures* of the individual health status to examine the *causal effects* of two popular and commonly used labor market policies – training programs and benefit sanctions – on participants' health. Both policies may have very different health consequences. Whereas training aims to upgrade the skills of the unemployed workers, which may have positive implications (see Andersen, 2008), sanctions focus on activating individuals who do not search for a job hard enough and therefore might increase social and economic stress. To this end, we focus on a sample of new entries into unemployment in Sweden in 2006/07 and exploit rich administrative data with information on the unemployment status, participation in labor market policies and health outcomes (which are available for the entire population). Importantly, to measure short-, as well as long-run health responses, we use daily information from prescription drug records. These records classify all redeemed prescriptions based on the Anatomical Therapeutic Chemical (ATC) Classification System and allow us to identify two common types of health problems that can be assumed to show up and to be diagnosed quickly after a potential intervention: cardiovascular problems and mental health issues. Both of them allow us to capture the different health effects of training and sanctions, such as effects via improved employment and increased stress due to program participation.

Drug prescriptions represent an objective measure of the individual health status, which provides important insights beyond the existing literature focusing on subjective measures. First, they are unaffected by the individual's personal assessment of the treatment, which might influence the subjective well-being even in the absence of actual health effects. Second, our findings will not depend on the design of specific survey items, which would make it difficult to generalize

[2] On the one hand, it has been shown that unemployed workers who are at risk of participating in a program are ecnouraged to leave unemployment in various countries, like the US (see Black et al., 2003), Denmark(Geerdsen, 2006; Geerdsen and Holm, 2007; Rosholm and Svarer, 2008; Graversen and Van Ours, 2008, 2011) or Sweden (see Carling and Larsson, 2005; Hägglund, 2011), which indicates that ALMP programs might be disliked by the unemployed in many situations. On the other hand, van den Berg et al. (2014) for the UK and Crépon et al. (2018) for France document a waiting effect upon notifications about job search assistance and training programs, which indicates that the unemployed generally like to participate.

findings based on ordinal scales on subjective well-being (see Bond and Lang, 2019). Finally, in contrast to survey data, prescriptions records do not suffer from underreporting, which might problematic since especially mental illnesses might be associated with stigma (Bharadwaj et al., 2017).

The main empirical challenge for establishing a causal relationship is that the propensity to participate in training and to experience a benefit sanction may be correlated with the individual health condition. To handle this, we estimate a dynamic difference-in-differences model, which combines three econometric approaches. First, we adjust for a rich set of observed backgrounds characteristics that are typically used when evaluating labor market policies, e.g. sociodemographics and employment histories (Lechner and Wunsch, 2013), as well as detailed drug prescriptions before the entry into unemployment. Second, we explicitly acknowledge that the treatment may start after any elapsed unemployment duration to address that the selection into the treatment changes over the course of time. Finally, we directly exploit the longitudinal aspect of our data with exceptionally detailed health information available already before the treatment. The latter is utmost important as it allows us to account for existing health differences between treated and non-treated individuals and specifically changes of the individual health status during the unemployment spell, which most likely affect the likelihood to participate in a training program or to receive a benefit sanction. This identification strategy allows us to avoid any bias that would arise from unobserved differences between treated and non-treated and might be correlated with their health status.

Our main findings show that both policies have very different implications for the job seekers health-related outcomes. Participating in a training program reduces the probability to have a prescription for drugs that are related to cardiovascular and mental health problems within one year by 6-7%. It is accompanied by a reduced usage of sickness benefits and appears before the treatment could create a positive impact on the participants' employment prospects. A further subgroup analysis shows that the findings are particularly pronounced for vulnerable groups like low-educated, who might lack daily routines more often, and older job seekers, who have more health issues in general. The findings seem to reflect real improvements of the participants health status due to the direct effect of the training program on participants' life, while other explanations, such as a reduction of doctor or pharmacy visits due to time constraints during the treatment are unlikely to explain the overall pattern.

For benefit sanctions, which are more restrictive interventions that financially punish non-compliance with UI guidelines, there is no long-run effect on subsequent prescriptions, but job seekers who receive a sanction make greater usage of sickness benefits. The latter might be an attempt to avoid future sanctions since job seekers who are sanctioned once are more aware of the fact that they are monitored. Moreover, we observe a strong increase in prescriptions related to mental health issues in the month before the sanction was imposed when the individual already received a warning, which could reflect a short-term health effect possibly due to higher levels of stress.

Our results add to the existing evidence regarding the effectiveness of training and sanctions. 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; Card et al., 2017). Sanctions also 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). Moreover, there are some studies considering unintended consequences of public policies, such as anticipation effects for job seekers who expect to be treated in the future (see e.g. Black et al., 2003; van den Berg et al., 2009) or spillover effects on non-treated individuals (Rothstein, 2010; Crépon et al., 2013; Gautier et al., 2018), but very little is known with respect to individual outcome variables that are not the primary objective of a policy.[3] We provide first evidence that common tools of labor market policy can have sizable effects on the health status of participants that should be taken into account when assessing the costs and benefits of an intervention. This is particularly important as better health conditions might help individuals to avoid future unemployment (e.g. García-Gómez et al., 2010). The study provides a first step towards a more holistic consideration 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

[3]Some related examples include the effects of minimum wages on public health (Leigh et al., 2019), the connection between tax credits and infant (Hoynes et al., 2015), respectively maternal health (Evans and Garthwaite, 2014) or midlife mortality Dow et al. (2019).

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. By European standards, Sweden has had a relatively low unemployment rate. In the beginning of the period studied in this paper (January 2006 to December 2008) 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 next months, it was still not higher than 6.4% by December 2008. Most Swedish workers (91%) are covered by collective bargaining wage agreements at the sectoral (or occupational) level, typically with bargaining at three levels: central and establishment level agreements, and individual bargaining. Both employment-protection laws and collective agreements regulate terminations. The general rule is that firms that need to displace workers, must follow the last-in-first-out principle, which means that the more recent hires are displaced before workers with longer tenure.

2.1 Unemployment and Health Insurance in Sweden

Unemployment insurance: Unemployed individuals older than 20 years are eligible for 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 requirement 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). For the median wage earner this amounts to a replacement rate of roughly 20%. 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%[4] starting from the basic UI level and up to a ceiling at 680 SEK (≈75 USD) per day. The potential benefit duration is 300 days, and since benefits paid for 5 days each week this corresponds to 420 calender days if you collect full-time benefits. The sanctions described below apply to both the basic level and the income-related UI benefits.

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

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. 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.[5]

Medicines are regulated in a separate nationwide system. To ensure universal access to high-quality and effective treatments, the Swedish government subsidizes a wide range of medicines. The subsidies are regulated by the Dental and Pharmaceutical Benefits Agency, which among other things decide which medicines should be tax-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 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.

2.2 Labor Market Policies

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 programs (Arni et al., 2017). For our analysis, we focus on two types of labor market policies representing these 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 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

[5] In Stockholm, a visit to a doctor in primary care costs 200 SEK (≈ 25 USD) as of 2017.

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 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. Information from the PES reveals that training participants usually are notified about an upcoming participation within the month before the beginning of the training program.

Sanctions: Second, sanctions are benefits 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. This decision is taken quickly, in most cases within two or three weeks since the notification. For the period studied in this paper, the refusal of suitable job offers without a valid reason is the main reason for a sanction.[6] 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 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 includes failure to actively 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.[7] 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 benefits. 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

[6]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 guidelines, e.g. related to job search requirements or ALMP participation.

[7]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.

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 doctors certificate after seven benefit days.

2.3 Expected 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 show that they are associated with negative lock-in effects during the program, but have a positive impact on re-employment prospects shortly after the program and earnings in the long-run (see 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 lower occupational level. Given the adverse effects of unemployment on the individual health status, one could expect that both policies may have an *indirect health effect* on treated job seekers through their positive impact on the individual labor market prospects.

However, beside these indirect consequence triggered by improvements of the employment outcomes, both policies could also have *direct health effects* that might be very different. Participating in a training program directly affects the job seekers daily routines as they typically have to attend the training course everyday for a period of several months. This could be particularly beneficial for the health status since unemployed individuals typically 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 outcomes (see e.g. Cohen, 2004), and the acquisition of new skills might lead to higher levels self-esteem and thereby counteract some of the negative consequences of unemployment (Axelsson and Ejlertsson, 2002; Waters and Moore, 2002). Moreover, attending a training course for several months might impose time constraints on the participants that could prevent them from visiting the doctor or picking up medication at the pharmacy. This could lead to fewer redeemed prescriptions in the short-run, but it might also has additional implications for the health status and in the long-run.[8] In summary, we expect that participating in a supportive training program improves the participants' health outcomes.

Sanctions, on the other hand, represent a very different type of intervention and therefore the direct effects of the policy might be very different. While benefit sanctions do not directly influence the daily routines of treated individuals (like training programs), the reduced financial means, as well as unpleasant interactions with the authorities might increase the job seekers' stress level, which is assumed to have negative consequences for the job seekers health status (Cohen, 1996). Moreover, given the negative implications of a benefit sanction, job seekers who have been notified 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.[9] It should be noted that the rate of sanctioned job seekers in Sweden was rather low before 2013 (see Lombardi, 2019), since caseworkers generally considered the system as too harsh and therefore were reluctant to use this policy instrument (see van den Berg and Vikström, 2014), which emphasizes the negative perception of sanctions. However, this might also create an even stronger threat effect for those who actually receive a warning or a benefit sanction as it increases the job seekers' awareness that they are monitored and could therefore reinforce real health effects of a warning or sanction as it further increases stress levels, but also behavioral responses in order to avoid the future benefit reductions. In summary, we expect that benefit sanctions have a detrimental direct effect on the health status of the unemployed.

3 Data and Empirical Strategy

3.1 Data

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 active labor market programs, and the reason for the unemployment spell to end. Second, we exploit information

[8] It might be the case that forward looking participants anticipate these time constraints and move doctor visits to the pre-treatment period.

[9] One could expect a similar effect for training programs if the job seeker does not want to participate in the program. The presence of such a threat effect of labor market policies with respect to the job search behavior has been shown by a variety of studies (see e.g. Black et al., 2003; Lalive et al., 2005; Rosholm and Svarer, 2008; van den Berg et al., 2009; Crépon et al., 2018).

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). Taken together, we have daily information on labor market status, sanctions and participation in training, and rich background information for each individual.

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 have been registered at the PES within the last six months, which 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 employment plausible. 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 (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.[10] Each bottom-level ATC code stands for a pharmaceutically used substance, or a combination of substances, in a single indication (or use).[11]

[10] It is controlled by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOCC), and was first published in 1976.

[11] 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.

3.2 Health and Labor Market Outcomes

Based on the prescription records, we define two types of health problems that are assumed to respond and to be diagnosed quickly after a potential treatment. This allows to measure short-run health responses before, during and after the training/sanction. It also mitigates issues with reversed causality. Moreover, the health problem should be relatively common among typical unemployed individuals in order to have some statistical power. First, we consider *cardiovascular diseases* (ATC Code C), which describe disorders of the heart and blood vessels and is the number one cause of death globally. Common examples include, e.g, high levels of blood pressure, strokes or heart attacks and it covers about 22% of all Swedish prescriptions in 2006 and 2007. Previous research often assumes that the effects of unemployment often manifest in the cardiovascular system (see Weber and Lehnert, 1997, for an overview) since the risk of cardiovascular diseases is strongly connected to an individual's lifestyle factors that may 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). Since labor market programs, especially training, aim to affect the daily routines of the unemployed, potential treatment effects might be materialized through prescriptions with respect to the cardiovascular system.

Second, we consider *mental health problems* (ATC Code N05 and N06) that are often related to stress or depressions, which accounts for about 15% of all prescriptions in 2006 and 2007. 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. Moreover, the previous literature has documented an effect of labor market policies on subjective measures of well-being that are closely connected to the mental health status (Creed et al., 1998; Machin and Creed, 2003; Andersen, 2008) and potential treatment effects should find an expression in the corresponding prescriptions. Eventually, somatic conditions are often connected to mental health issues (Üstün and Sartorius, 1995; Wood et al., 1998; Pickering, 2001) and therefore the outcome variable might capture a broader set of health effects. Besides the health outcomes based on the prescription data, we also examine effects on exits from unemployment to employment and the usage of sickness benefits within the UI system (see Section 2.2 for the institutional details) for periods that are longer than seven days (which require a doctor visit).

3.3 Empirical Strategy

The aim of our empirical analysis is to identify the causal effect of training programs and sanctions on the probability to have a prescription related to cardiovascular diseases and mental health problems. There are three main concerns, which could imply a correlation between the individual health status and the likelihood to participate in a training or to receive a sanction.[12] To handle this, we apply a dynamic difference-in-differences (DID) strategy as described below.

Accounting for observed heterogeneity. First, treated and non-treated individuals might differ already when becoming unemployed. As shown in Table 3 about 4% of the sample had a prescription related to the cardiovascular diseases and 10% related to mental health issues within the last six months before the entry into unemployment. However, participants in training programs seem to have better a 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, are less often Swedish citizens and are less likely to hold an university degree compared to the non-treated. Moreover, they tend to have more often young children, more unemployment experience 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 relative to those not participating in any program. Regarding their labor market histories, they have more unemployment experience and earned substantially higher income during the last three years.

[Insert Table 1 about here]

Therefore, we account for a rich set of background characteristics including socio-demographic information and labor market histories, which have consistently shown to be key drivers of the selection into labor market programs (see Dolton and Smith, 2011; Lechner and Wunsch, 2013),

[12] As discussed by Eriksson (1997) and Carling and Richardson (2004), caseworkers have a large influence and a large degree of discretionary power over training enrollment, respectively sanctions and might base their decision to some extent on the job seeker's health status.

as well as various other types of previous prescriptions before the beginning of the unemployment spell. Specifically, we control for 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).[13]

Dynamic selection. Moreover, as generally discussed by Sianesi (2004) and Fredriksson and Johansson (2008), the selection of individuals into the treatment is likely to change over the course of the unemployment spell, which could create a bias when comparing outcomes of treated and non-treated in a static framework. In our setting, this could be a severe problem since individuals with health issues are also less likely to leave unemployment (see e.g. Lindholm et al., 2001; Stewart, 2001; García-Gómez et al., 2010; Rosholm and Andersen, 2010), and this affects the likelihood to be treated over the course of time (see Biewen et al., 2014). Therefore, we explicitly take into account the elapsed unemployment duration before the treatment. Specifically, for each month after the entry into unemployment $t = 1, ...12$, we compare individuals who start the treatment in month t with those who are also unemployed at least until month t, but did not have been treated yet (although they may enter the treatment later on). Table 2 illustrates the number of treated and non-treated over the course of the unemployment spell. It can be seen that for training the share of treated increases for the first four months of the unemployment spell from 0.08% to 0.45% and than decreases to 0.28% at the end of the last year, while it varies only moderately for sanctions.

[Insert Table 2 about here]

Difference-in-differences. Finally, the experience of unemployment (before starting the treatment) might already has an influence on the individual health status, which in turn could affect the selection into the treatment. Therefore, we exploit the longitudinal aspect of our data with detailed health information already before the treatment, and focus on the difference between the outcome variables and their pre-treatment levels before the start of the treatment. This DID

[13] We have examined the mean standardized bias (MSB) (Rosenbaum and Rubin, 1983), which assesses the distance of the covariates before and after weighting, which is 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, while (see Table A.1 in Appendix A).

approach allows us to explicitly account for any time-constant unobserved differences between treated and comparison individuals if both groups follow the same trend (below we show that this is the case). 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 immediately prior to the decision about the participation, respectively the sanction. Table 3 shows the prescription probabilities in the six month window before, respectively after the start of the treatment. First, it should be noted that there is an increase in the prescription rate over the course time for cardiovascular and mental health issues, which generally illustrates the negative effect of unemployment on the two specific health problems. Moreover, the increase in prescription rates (for both health issues) is smaller for participants in training programs compared to the non-treated, while there is no clear pattern for those who receive sanction.

[Insert Table 3 about here]

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 valid prescription before and after a potential treatment. Since treated individuals are notified about an upcoming program participation or sanction already in $t-1$, the month before the actual start of the treatment, we only consider valid prescriptions in the five preceding months ($t-6$ to $t-2$) to determine the reference level. Obviously, the reference period could partly cover episodes at the beginning of unemployment (up to the month of the treatment), but also the end of the last employment spell depending on the exact month of the treatment start.[14] Therefore, it is even more important to explicitly take into account the elapsed unemployment duration as described before.

This gives two treatment groups (training and sanctions) and two control groups. For each group, we estimate the causal effect of the treatment for a given month of the elapsed unemployment duration:[15]

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

[14] 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). Note that the second month of the unemployment spell ($t-1$ relative to the treatment start) is then excluded from the reference period as they already received a notification.

[15] 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.

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. As already mentioned, by estimating treatment effects using differences between before and after the treatment, we account for any time-constant unobserved differences between the treated and the non-treated. 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 is still at risk of being treated in a given month t and $\hat{p}_t(X_i)$ characterizes the estimated propensity that individual i is treated in month t.

4 Results

This section presents the estimation results for the two types of drug prescriptions. Before examining the effects of training and sanctions on the individual health status after the beginning of the treatment, we first consider pre-treatment outcomes. To take a closer look into the potential effect mechanisms, we examine effects on employment and the probability of receiving sickness benefits, as well as various subgroup analyses.

4.1 Pre-trends and Anticipation Effects

Pre-trends: To examine the validity of the DID approach, we first consider drug prescriptions in the pre-treatment period. Specifically, for the five months before the beginning of the training program, respectively the imposition of the sanction we estimate the treatment effects relative to $t-6$ (the first month of the observation period). As shown in Figure 1, there are no statistically significant differences between treated and non-treated neither for training nor for sanctions in the reference period until $t-2$. Since treated individuals have not received any information about the upcoming treatment in this time period, the results indicate that both groups follow the same trend with respect to drug prescriptions for cardiovascular diseases and mental health problems in absence of the treatment. Therefore, we can conclude that estimating the treatment effect on the differences in outcomes relative to the reference period from $t-6$ to $t-2$ generally allows to avoid any bias that might arise from reverse causality or unobserved heterogeneity.

[Insert Figure 1 about here]

It should be noted that for individuals who are treated within the first six months of the unemployment spell, the pre-treatment period covers (partly) episodes of the last 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.1 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 $t+12$. Importantly, there is no indication for a violation of the common trend assumption in any specification.[16]

Anticipation Effects: Moreover, Figure 1 also reveals that there are some reactions in response to the notification about the upcoming treatment in month $t-1$. These effects are rather small and statistically insignificant for training programs, but we observe a strong increase in prescriptions related to mental health issues for sanctions in $t-1$. Specifically, in the month before the sanction is imposed, but after the job seeker has received a warning, the prescription rate increases by 0.9 percentage points, which represents a treatment effect of about 37% relative to the control group of non-treated individuals. The effect is statistically significant at the 1%-level. There are several possible explanations to this anticipation effect. First, the result might reflect the fact that the threat of a benefit reduction increases the individual stress level which impairs the mental health status. However, apart from that, behavioral reactions that are not necessarily connected to a deterioration of the health condition may also be important. Individuals who are informed about a future sanction could try to avoid it by visiting their physicians to get a medical certificate, which would allow them to avoid or postpone the sanction, while the larger number of prescriptions would be a by-product of the more frequent visits to the doctor. In Section 4.3, we exploit additional data to analyze these mechanisms.

It should be also noted that some individuals may react to a notification and leave unemployment before the treatment actually starts. Since we do not observe the planned treatment, those individuals are placed in the control group. This may impose an bias on our results if those who experience a health shock due to the notification would leave unemployment before the treatment can be realized (as they would be placed in the control group). However, since rather few individuals are trained or sanctioned (relative to the large control group), this should be a minor concern. Moreover, it is important to note that the presence of such a bias would

[16]Table A.3 in the Appendix also shows separated treatment effects for those who are treated within the first six month of the unemployment, respectively those who are treated after month six.

reduce the magnitude of the estimated effects and therefore our estimates can be interpreted as a lower bound.

4.2 Effects after the Start of the Treatment

We now turn to the post-treatment effects. For the empirical analysis, we only consider the first prescription that the individual received after the start of the potential treatment and distinguish between three different time periods ($t+1$ to $t+3$, $t+1$ to $t+6$ and $t+1$ to $t+12$).[17] The results are summarized in Table 4. As shown in Panel A.1, there is a substantial reduction with respect to the likelihood of having a prescription related to cardiovascular and mental health issues for training program. Over the course of 12 months, participants in training programs have a lower probability of receiving a prescription related to the cardiovascular system of 0.44 pp, while the effect on mental health issues is 0.63 pp. Relative to the baseline level of the control group, this refers to treatment effects of 7.1%, respectively 5.7%. Both effects are statistically significant at the 5%, respectively 1%-level.

Moreover, we also estimate the total effect, which takes both post-treatment effects (over the period $t-1$ to $t+12$) and the anticipation effects in $t-1$ into account. This is important since the announcement of the training program might encourage some potential participants to shift doctor visits to the pre-treatment period as they anticipate time constraints once they enter the program. Although both effects, for cardiovascular diseases and mental health issues, becomes slightly smaller, there is still a significant reduction of the probability to hold a prescription, which is very close to the post-treatment effect. In total, the training program has a favorable impact on the participants' health status with fewer prescriptions related to cardiovascular and mental health issues.

[Insert Table 4 about here]

The results for sanctions in Panel B of Table 4 show no significant effects on the likelihood of receiving drug prescriptions. It holds both shortly as well several months after the sanction for both types of prescriptions. Together with the results for training, it indicates 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. However, it should be noted that all treatment

[17] Panel A of Table A.2 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 very similar to the baseline estimates.

effects take into account both, the direct health effect of a treatment and the indirect effect through possible changes of the employment status.

4.2.1 Sickness Benefits and Employment Effects

Next, we exploit additional data regarding the receipt of sickness benefits and employment outcomes to shed light on the underlying mechanisms. We have seen that there are sizable treatment effects, especially for sanctions, already in the month before the treatment. This could reflect that individuals try to avoid the upcoming treatment by visiting their physicians to obtain a medical certificate receive more prescriptions as a by-product of these doctor visits. To test this mechanism, we use episodes of sickness benefits as an additional outcome variable. This is informative since individuals can try to avoid the economic consequences of a sanction or the participation in a training program by reporting that they are sick. In this case they receive sickness benefits rather than regular UI benefits, while sanctions only apply to UI benefits. We focus on periods of sickness benefits that last for more than seven days since those require a doctor's certificate and doctor visits are often accompanied by a prescription.

Moreover, we also examine the employment effects of both treatments and compare them to the health effects in the post-treatment period. This allows us to shed light on the question whether, for instance, the improved health status of participants in training programs is caused by corresponding improvements of the participants' employment prospects. For both sickness benefits and employment, we estimate treatment effects based on a dynamic IPW (as described in Section 3.3), but do not account for pre-treatment levels since they are not available by definition.

[Insert Table 5 about here]

As shown in column 1 of Panel A of Table 5, participating in a training program reduces the likelihood to report sick, which is in line with the notion that training improves the participants' health status as indicated by the estimates on drug prescriptions. This is also true even in the month before the treatment, which indicates that potential participants do not try to avoid the treatment by reporting sick. Moreover, column 2 of Table 5 shows the effects on the probability to leave unemployment for regular employment for the three time periods consider before ($t+1$ to $t+3$, $t+1$ to $t+6$ and $t+1$ to $t+12$). There is a substantial lock-in effect for participants in training programs of about 4.9 pp within the first three months after the enrollment, which turns into a strong positive effect one year after the program start. 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 reemployment prospects occurs. It implies 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.

For sanctions, column 1 of Panel B shows that participants have a higher likelihood to report sick after the sanction is imposed, but not in the month before the sanction, where we observe a strong increase in prescriptions related to mental health issues. Hence, there is no clear evidence that higher prescription probability is just a by-product of doctor visits as an attempt to avoid the sanction. It rather seems to be the case that the warning causes a higher stress level that translates into mental health problems. Moreover, there is also some evidence that individuals use sickness benefits to mitigate the economic consequences of a sanction and to avoid a future sanction. Especially after the sanction was imposed treated job seekers report sick much more often than the non-treated, while there is no effect on drug prescriptions in the post-treatment period.[18] 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 reporting in sick may help you 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, we find significant employment effects of sanctions (reemployment probability up 3.5 pp in the first six months). These positive employment effects may explain why we do not find any negative post-treatment effects for sanctions: any negative health effects due to increased financial stress could be canceled by positive health effects due to the higher re-employment rate.

[18] Table A.2 (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.

4.3 Effect Heterogeneity

To further investigate the potential mechanisms discussed in Section 2.3, we now examine heterogeneous treatment effects with respect to different background characteristics. Specifically, we consider the job seekers initial health status, their educational background, gender and age. The results are summarized in Table 6. All estimates refer to the total effect taking into account the post-treatment effect ($t+1$ to $t+12$) and the anticipation effect in $t-1$. Since there is only limited evidence that sanctions affect drug prescriptions, we focus our discussion on the heterogeneous effects of training programs.

[Insert Table 6 about here]

Initial health status. Our baseline results raise the question whether there is a shift in drug prescriptions from the post-treatment period to the pre-treatment period, for instance due to temporal shifts of doctor visits for people who anticipate that the training program will lead to time constraints. 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 or pharmacies are more relevant for individuals who already have a concrete health issue before the training program. To this end, Panel A of Table 6 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. We argue that this is unlikely to be the consequence of a temporal shift in 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 a real improvement of the participants' health status relative to the non-participants.

Education. Next, 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. Interestingly, as shown in Panel B of Table 6, 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. It seems plausible that 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). Hence, the training program might improve the participants' health status by counteracting those negative implications of unemployment.

Gender. It is well known that there are substantial gender differences with respect to the behavior related to health care. 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, we conduct a subgroup analysis with respect to gender. As shown in Panel C of Table 6, the favorable effects of training programs 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 finding might be related to the fact that women generally seek social support when becoming unemployed (Leana and Feldman, 1991) and the training program increases social interactions, which appear to have a positive effect on subjective well-being (see, e.g.m Kawachi and Berkman, 2001).

Age. Finally, it seems obvious that any type of health effect generally plays a bigger role for older people. Therefore, we distinguish between those below and above age 40. As shown in Panel D of Table 6, we find a strong and statistically significant reduction in drug prescriptions related to cardiovascular and mental health problems for participants in training programs within the sample of individuals above age 40, but not for those below. Given that older individuals might have a higher potential for an improvement of the actual health status, the findings support the notion that our estimates reflect real improvements of the participants health status.

4.4 Robustness Analysis

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 of other labor market programs, and the definition of the reference period. The results are summarized in Table A.2.

Follow-up prescriptions. In our main specification, we only consider the first prescription after a potential treatment since all subsequent prescriptions are a consequence of the first prescription. However, this approach neglects all effects on the intensive margin. To test whether the estimated treatment effects might change when taking into account the intensive margin, we consider an alternative indicator, which takes the value one if the individual has at least two prescriptions related to either cardiovascular diseases or mental health issues within 12 months after the potential treatment. As shown in Panel A of Table A.2, the estimated treatment effects are almost the same as in our baseline specification, which indicates that potential effects on the intensive margin are negligible.

Other drug prescriptions. For benefit sanctions, we find an increase in the likelihood the receive sickness benefits, but no effect on drug prescriptions. This indicates that job seekers who received a sanction once might try to avoid future sanctions by reporting sick, which would release them from job search requirements. However, it could be also the case that the treatment has other health effects that are not captured by prescriptions related to cardiovascular and mental health issues, but affect the likelihood to report sick. Therefore, we additionally estimate the treatment effects on other types of drug prescriptions including ten alternative top-level ATC codes. Specifically, we consider two outcome variables: (i) an indicator variable that refers to any other prescription within a given time period and (ii) an index variable that takes values from to zero and ten depending on the number of different top-level ATC codes with a prescription in a given period. As shown in Panel B of Table A.2, 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. Importantly, there is no evidence that sanctions might lead to more prescriptions, which indicates that the greater usage of sickness benefits is not the consequence of other health effects.

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. 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 become slightly larger compared to the baseline estimates. This indicates that, if at all, other programs might also have some favorable health effects, but the impact seems to be limited. The estimated effects of sanctions are almost unaffected and still statistically insignificant.

Reference period. In the baseline specification, we estimate the DID model using a reference period of six months prior to the treatment. Due to the dynamic assignment into the treatment the reference period therefore covers some individuals (who are treated within the first six month of the unemployment spell) also episodes of the last employment spell. For others (who are treated after month six), the reference period only covers episodes within the unemployment spell. To test the sensitivity of our results with respect to the definition of the reference period, we now exploit an alternative reference period that is given by the last six months before entry into unemployment. As shown in Panel D of Table A.2, the estimated treatment effects for training programs are 2-2.5 times larger when using this alternative reference period. This suggest that the selection into training programs is not only affected by the individual health status at the entry into unemployment, but also by changes of the health conditions between the entry and the start of the program. Specifically, individuals who experience improvements (deteriorations) of the health conditions are more (less) likely to enter a program. Our baseline model provides unbiased estimates as we explicitly acknowledge these health changes over time, while the alternative model is likely to suffer from endogeneity bias as it neglects the evolution of the health status over time.

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 completely neglected so far. 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 commonly used labor market policies representing different reintegration strategies. Based on a dynamic difference-in-differences approach we identify the causal health effects of training programs and benefit sanctions on prescriptions related to cardiovascular diseases and mental health problems.

We can 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 both health issues. This effect is accompanied by a reduced usage of sickness benefits 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 and older individuals, who generally have a higher risk to suffer from health problems. In summary, the findings seem to reflect real improvements of the participants health status due to the direct effect of the training program 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-term effect on subsequent prescriptions for cardiovascular and mental health problems. However, sanctioned job seekers make greater usage of sickness benefits. As unemployed who report sick are exempted from their job search requirements, the increase in sickness benefits might reflect an attempt to avoid future sanctions since job seekers who are sanctioned once are more aware of the fact that they are monitored. Moreover, there is strong increase in prescriptions related to mental health issues in the month before the sanction was imposed, but after the individual already received a warning. This suggests that the threat of a benefit sanction a short-term health effect possibly due to higher levels of stress.

By providing first evidence with respect to the unintended consequences of labor market policies, our study represents an important step towards a more holistic consideration 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. It should be noted that, independently of the underlying mechanisms, our results imply a connection between labor market policies and health care expenditures, as more (less) drug prescriptions increase (decrease) the associated costs. These costs will be affected even if there is no real health effect and the estimated effects would be just a by-product of a behavioral response such as changes in the frequency of doctor or pharmacy visits. Moreover, real 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. Therefore, when designing new interventions, policy makers should bear in mind that any treatment that has a strong impact on an individual's daily life could have unintended effects on outcome variables, which might have been neglected in the first place.

Our empirical analysis faces two main challenges. First, we do not have access to individual-level data containing information regarding notifications about the program participation or warnings about benefit sanctions. This would be necessary for a more valid analysis of the anticipation effects with respect to health outcomes. Second, beside reflecting real health effects, differences in redeemed drug prescriptions could be also created by purely behavioral responses. Having access to additional information about doctor visits and issued prescriptions, would allow a profound understanding of the potential effect mechanisms. Future research should make additional efforts to combine all these different data sources.

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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
Swedish citizen	0.374	0.321	0.000	0.339	0.000
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 15 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: Number of observations over time

	A. Training		B. Sanctions	
	Non-treated	Treated	Non-treated	Treated
Elapsed unemployment duration				
1 month	365,281	308	360,541	221
2 months	341,116	895	336,819	452
3 months	294,585	1,109	291,004	393
4 months	244,489	1,096	241,669	335
5 months	209,956	808	207,700	244
6 months	186,494	721	184,713	246
7 months	167,691	625	166,331	204
8 months	152,413	568	151,436	185
9 months	138,187	467	137,504	173
10 months	127,087	416	126,657	163
11 months	117,302	410	117,123	159
12 months	108,699	302	108,699	123

Note: Depicted are the number of observations who are unemployed over the course of time separated by the treatment status.

Table 3: 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 4: Baseline results: Health effects of labor market policies

	Cardiovascular diseases (1)	Mental health problems (2)
A. Training		
1) Post-treatment effects		
in $t+1$ *to* $t+3$	-0.0015	-0.0051**
	(0.0017)	(0.0024)
	[-4.7%]	[-8.3%]
in $t+1$ *to* $t+6$	-0.0028	-0.0057**
	(0.0018)	(0.0026)
	[-5.2%]	[-4.8%]
in $t+1$ *to* $t+12$	-0.0044**	-0.0075***
	(0.0021)	(0.0028)
	[-7.1%]	[-5.7%]
2) Total (incl. anticipation effect)	-0.0036*	-0.0063**
	(0.0021)	(0.0029)
	[-5.1%]	[-4.4%]
No. of observations	365,589	365,589
B. Sanctions		
1) Post-treatment effects		
in $t+1$ *to* $t+3$	-0.0014	0.0003
	(0.0030)	(0.0046)
	[-3.6%]	[+1.5%]
in $t+1$ *to* $t+6$	-0.0014	0.0003
	(0.0034)	(0.0049)
	[-2.2%]	[-0.6%]
in $t+1$ *to* $t+12$	-0.0040	-0.0024
	(0.0037)	(0.0053)
	[-6.3%]	[-2.1%]
2) Total (incl. anticipation effect)	-0.0043	0.0035
	(0.0037)	(0.0055)
	[-6.3%]	[+2.7%]
No. of observations	360,762	360,762

Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation described in Section 3.1. 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: Treatment effects on sickness absence and labor market outcomes

	Sickness benefits[(a)]	Exit to regular employment
	(1)	(2)
A. Training		
1) Post-treatment effects		
in $t+1$ *to* $t+3$	-0.0101***	-0.0478***
	(0.0012)	(0.0037)
	[-39.6%]	[-31.8%]
in $t+1$ *to* $t+6$	-0.0130***	0.0348***
	(0.0020)	(0.0052)
	[-31.0%]	[+14.9%]
in $t+1$ *to* $t+12$	-0.0086**	0.1336***
	(0.0035)	(0.0057)
	[-12.1%]	[+42.3%]
2) Total (incl. anticipation effect)	-0.0126***	
	(0.0037)	
	[-15.1%]	
3) Anticipation effect in $t-1$	-0.0043***	
	(0.0009)	
	[-29.5%]	
No. of observations	365,589	365,589
B. Sanctions		
1) Post-treatment effects		
in $t+1$ *to* $t+3$	0.0162***	0.0093
	(0.0043)	(0.0077)
	[+62.8%]	[+6.1%]
in $t+1$ *to* $t+6$	0.0261***	0.0346***
	(0.0062)	(0.0089)
	[+61.4%]	[+14.8%]
in $t+1$ *to* $t+12$	0.0273***	0.0440***
	(0.0081)	(0.0093)
	[+38.2%]	[+13.9%]
2) Total (incl. anticipation effect)	0.0306***	0.0440***
	(0.0087)	(0.0093)
	[+36.5%]	[+13.9%]
3) Anticipation effect in $t-1$	0.0016	
	(0.0024)	
	[+10.8%]	
No. of observations	360,762	360,762

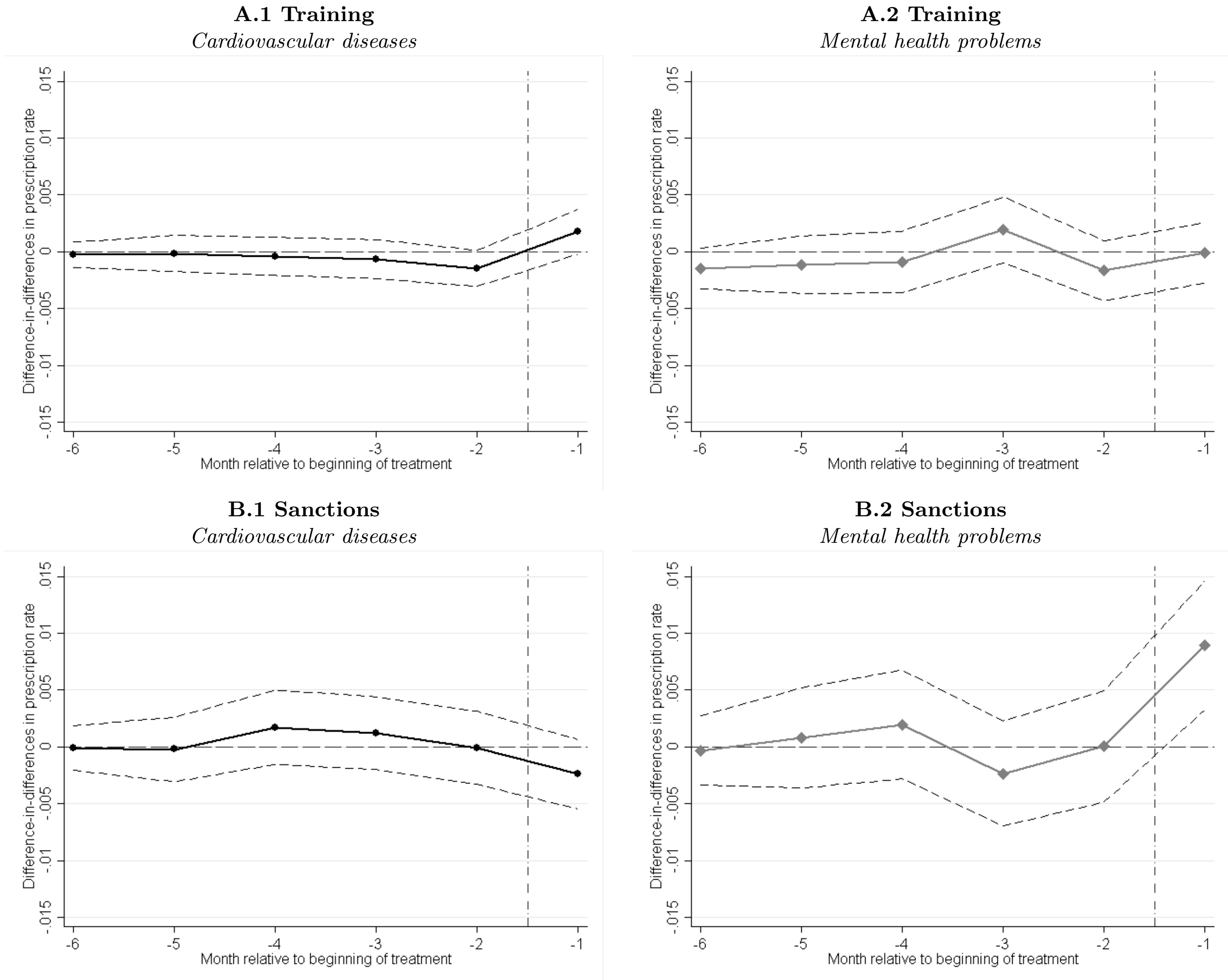
Note: Depicted are dynamic average treatment effects on the treated pooled for all treatments within the first 12 months using inverse probability weighting (IPW). Outcomes refer to an indicator whether the individual called-in sick/left unemployed to work within the corresponding time period relative to the beginning of the potential treatment. Standard errors in parenthesis. ***/**/* indicate statistically significance at the 1%/5%/10%-level.
[(a)] Refers to episodes of sickness benefits longer than seven days and considers only individuals who not left 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 6: Health effects for different subgroups

	A. Existing prescription			
	No		Yes	
	Cardiov. diseases	Mental health	Cardiov. diseases	Mental health
1) Training	-0.0044**	-0.0064**	0.0023	0.0013
	(0.0021)	(0.0026)	(0.0074)	(0.0111)
No. of observations	301,345	301,345	64,244	64,244
2) Sanctions	-0.0010	0.0040	-0.0145	0.0031
	(0.0037)	(0.0052)	(0.0106)	(0.0183)
No. of observations	297,244	297,244	63,518	63,518
	B. Education			
	Compulsory		Secondary or higher	
	Cardiov. system	Mental health	Cardiov. system	Mental health
1) Training	-0.0137***	-0.0124*	-0.0003	-0.0035
	(0.0050)	(0.0064)	(0.0023)	(0.0032)
No. of observations	79,953	79,953	285,636	285,636
2) Sanctions	-0.0140*	-0.0017	-0.0014	0.0049
	(0.0080)	(0.0132)	(0.0040)	(0.0060)
No. of observations	78,829	78,829	281,933	281,933
	C. Gender			
	Men		Women	
	Cardiov. system	Mental health	Cardiov. system	Mental health
1) Training	-0.0062**	-0.0022	0.0025	-0.0123**
	(0.0025)	(0.0031)	(0.0039)	(0.0058)
No. of observations	174,835	174,835	190,754	190,754
2) Sanctions	0.0005	0.0037	-0.0084	0.0038
	(0.0047)	(0.0068)	(0.0056)	(0.0088)
No. of observations	171,231	171,231	189,531	189,531
	D. Age			
	Below 40 years		Above 40 years	
	Cardiov. system	Mental health	Cardiov. system	Mental health
1) Training	-0.0002	-0.0038	-0.0117*	-0.0113*
	(0.0019)	(0.0031)	(0.0064)	(0.0066)
No. of observations	261,624	261,624	103,965	103,965
2) Sanctions	-0.0016	0.0013	-0.0088	0.0066
	(0.0031)	(0.0062)	(0.0085)	(0.0106)
No. of observations	257,682	257,682	103,080	103,080

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 period $t-1$ to $t+12$ (including the anticipation effect). Standard errors in parenthesis. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Figure 1: Differences in health outcomes in pre-treatment period



Note: Depicted are dynamic 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 $t-6$.
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: Robustness analysis

	A. Follow-up prescriptions[a]		B. Other drug prescriptions[b]		C. Other labor market programs[c]		D. Alternative reference period[d]	
	Cardiovascular diseases (1)	Mental health problems (2)	Any other prescription (3)	Index other prescriptions (4)	Cardiovascular diseases (5)	Mental health problems (6)	Cardiovascular diseases (7)	Mental health problems (8)
Training	-0.0039**	-0.0063**	-0.0010	-0.0093	-0.0048**	-0.0084***	-0.0081***	-0.0171***
	(0.0020)	(0.0028)	(0.0045)	(0.0088)	(0.0021)	(0.0028)	(0.0024)	(0.0035)
No. of observations	365,589	365,589	365,589	365,589	331,650	331,650	365,589	365,589
Sanctions	-0.0016	0.0006	-0.0134*	-0.0223	-0.0042	-0.0028	-0.0037	-0.0000
	(0.0036)	(0.0053)	(0.0076)	(0.0159)	(0.0037)	(0.0053)	(0.0046)	(0.0060)
No. of observations	360,762	360,762	360,762	360,762	326,823	326,823	360,762	360,762

Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation described in Section 3.1. Standard errors in parenthesis. ***/**/* indicate statistically 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 $t+12$).

[d] The reference period is defined relative to the entry into unemployment (last six months) rather than the start of the treatment. 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 A.3: Heterogeneous Health Effects by Timing of Treatment

	Treatment in month 1-6		Treatment in month 7-12	
	Cardiovascular diseases (1)	Mental health problems (2)	Cardiovascular diseases (3)	Mental health problems (4)
Training	-0.0022	-0.0058*	-0.0078*	-0.0071
	(0.0022)	(0.0030)	(0.0040)	(0.0056)
No. of observations	365,589	365,589	168,316	168,316
Sanctions	-0.0058	-0.0031	-0.0041	0.0003
	(0.0037)	(0.0056)	(0.0080)	(0.0107)
No. of observations	360,762	360,762	166,535	166,535

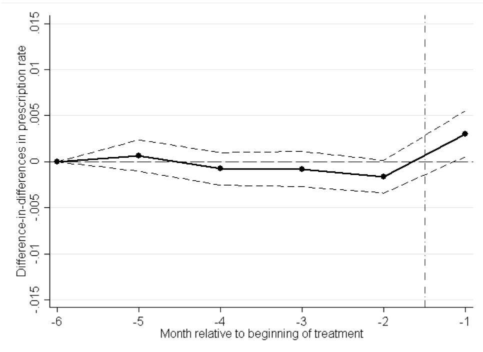
Note: Depicted are average treatment effects based on the dynamic difference-in-differences estimation described in Section 3.1. The outcome variable is given by an indicator for drug prescriptions within the first 12 months after the potential treatment ($t + 1$ to $t + 12$). Standard errors in parenthesis.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

Figure A.1: Differences in health outcomes in pre-treatment period by timing of treatment

Training

Cardiovascular diseases

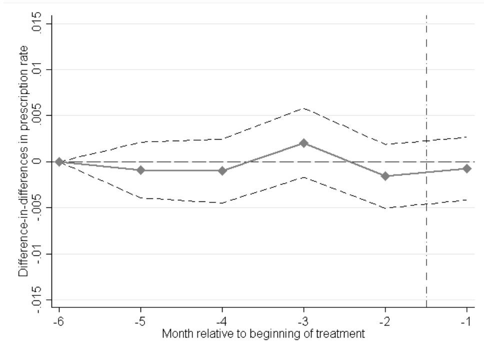
(a) Treatment month 1-6



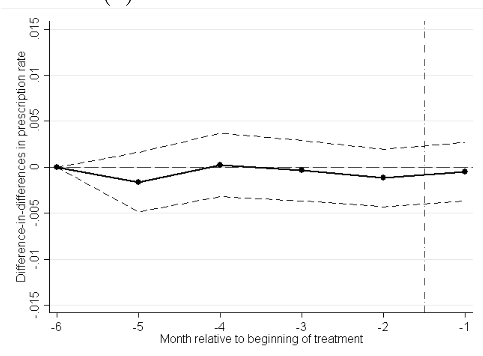
Training

Mental health problems

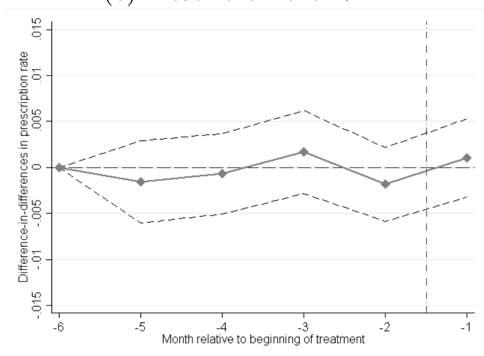
(a) Treatment month 1-6



(b) Treatment month 7-12



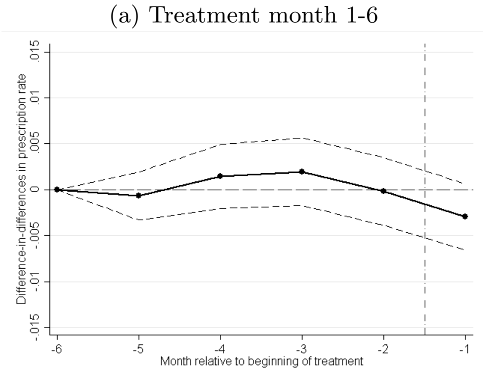
(b) Treatment month 7-12



Sanctions

Cardiovascular diseases

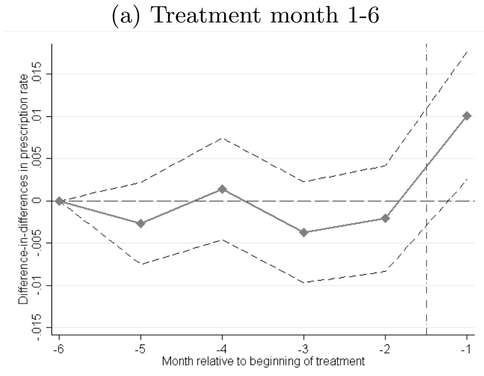
(a) Treatment month 1-6



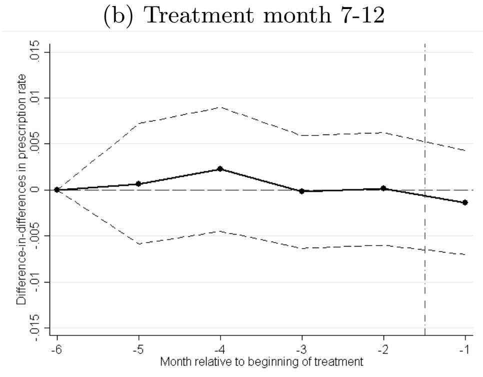
Sanctions

Mental health problems

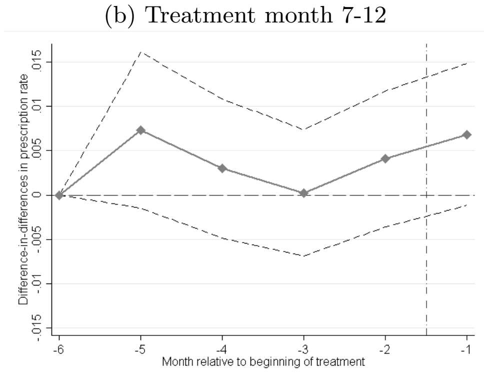
(a) Treatment month 1-6



(b) Treatment month 7-12



(b) Treatment month 7-12



Note: Depicted are dynamic 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 $t-6$.