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TREATMENT VERSUS REGIME EFFECTS OF CARROTS AND STICKS

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Abstract

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Keywords

active labor market programs | policy regime | treatment effect | employment | earnings | unemployment | caseworkers

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Treatment Versus Regime Effects of Carrots and Sticks

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Abstract:
Public Employment Service (PES) agencies and caseworkers (CW) often have substantial leeway in the design and implementation of active labor market policies (ALMP) for the unemployed, resulting in variation of usage. This paper presents a novel framework in which this variation is used for the joint assessment of different ways in which ALMP effects can operate. We examine an additional layer of impacts - beyond the treatment effects on the treated job seekers - called regime effects, which potentially affect all job seekers and which are defined by the extent to which programs are intended to be used in a market. We propose a novel method to jointly estimate regime effects for two types of programs, supportive (carrots) and restrictive (sticks) programs. We apply this to contrast regime and treatment effects on unemployment durations, employment, and post-unemployment earnings using register data that contain PES and case-worker identifiers for about 130,000 job seekers. The results show that "carrots" and "sticks" treatments prolong unemployment, but carrots increase earnings whereas sticks decrease them. We find regime effects of a similar order of magnitude. Higher intended usage of carrots and sticks reduces unemployment durations, but carrots raise earnings whereas sticks decrease them. We also find interaction effects between carrots and sticks policies. Regime effects are economically substantial. Our comprehensive cost-benefits analyses show that modest increases in the intended usage of carrots and sticks reduce the total cost of an unemployed individual by up to 10%.

JEL Codes: J65, J68, J64
Keywords: active labor market programs, policy regime, treatment effect, employment, earnings, unemployment, caseworkers.

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1 Introduction

Active labor market policies (ALMP) are important tools to fight unemployment and to improve the matching of workers and jobs in labor markets. Several OECD countries spend more than one percent of their GDP on ALMP. The existing literature has documented the effects of specific policy interventions on participants, see e.g. Card *et al.* (2010). But, interestingly, not much evidence can be found in the literature about the role of Public Employment Service (PES) units and caseworkers (CW) as policy makers: PES often apply mixtures of policies. Within PES, CW often have substantial leeway in dealing with their clients. Indeed, the frequency with which individuals are exposed to policies may vary substantially across PES and local labor markets.

This paper discusses the effects of PES and CW *policy regimes* on job seekers' outcomes, notably on their job search durations and earnings. We propose a novel method to estimate such regime effects, based on observed policy usage and on register data with PES and CW identifiers. We distinguish between programs with a supportive nature ("carrots") and policies that constrain individual behavior ("sticks"). The first group of policies is taken to cover training and job search assistance, and the second group to cover benefit sanctions and workfare programs.

We observe how frequently the different PES and CW use these policies. To reconstruct intended policies from actual (observed) program participation or treatment exposure, we apply methods based on the competing risks approach in duration analysis, in contexts where PES and CW determine ALMP assignment. The competing risks analysis enables us to measure ALMP exposures in a setting where the subsequent individual treatment status is not yet observed at entry into unemployment and where individuals can leave unemployment before being exposed at all. In a second step, we assess the relation between CW- and PES-specific intended policies and actual treatments on the one hand, and realized individual earnings and employment in the years after unemployment on the other hand. Thus, we examine medium to long run outcomes. The effects of these policies are thus assessed both on actual participants and on non-participants.

A key feature of our study is that we provide a novel framework that allows us to analyze several kinds of policy effects at once: treatment effects vs regime effects, as well as marginal effects and interaction effects if there is a variety of programs, like carrots and sticks programs. A core advantage of such a comprehensive framework is that it provides the opportunity to jointly assess the relative importance of different kinds and types of ALMP effects. In all of this, we do not make the usual but often unrealistic assumption of no interaction between different treatments within an unemployment spell. The effects of a "sticks" policy regime might depend

on whether a "carrots" policy is present or not. The presence of a strongly supportive carrots regime might lead job seekers to feel that the sticks policy is less of a threat, so that the threat effect of sticks policies may be lower if used in combination with carrots policies. Carrots policies, on the other hand, might need to go one-for-one with sticks policies. For instance, a PES that places many job seekers into training might want to enforce a rigorous adherence with job search requirements.

In our empirical analysis we use a rich base of register data from Switzerland. Switzerland is an especially interesting and fruitful case for analyzing the role of policy regimes: The PES enjoy a large leeway to forge their specific strategy in implementing the different types of policy (i.e., including what we refer to as carrots and sticks). As a rule, job seekers are assigned to CW based on exogenous and pre-determined characteristics (last name, industry, etc.). Conditional on these characteristics, assignment to CW is plausibly random. For PES policies, we analyze outcomes within labor market regions. The latter have been created originally to cover travel-to-work areas and represent local labor markets. As our baseline sample, we take a fourth of the complete inflow into registered (full-time) unemployment in Switzerland in the years 2000 to 2005, up to age 61. This covers over 150 different PES and 700 CW. The unemployment insurance database provides a large amount of socio-demographic and benefit-entitlement-related information. To this base we merged a further database that covers the (daily) history of all ALMP events, including sanctions. Finally, to observe the outcome and the past employment history, we added social security data (monthly precision) which covers (non-)employment and earnings in the six years before and up to 42 months after unemployment entry.

The analysis of regime effects is related to the literature on ex ante effects of possible future treatments on not-yet-treated individuals. For job search assistance programs, these are analyzed in, for example, Blundell *et al.* (2004) and van den Berg *et al.* (2014). For training programs they are analyzed in van den Berg *et al.* (2009). The former studies exploit the national introduction of a new policy whereas the latter study uses self-reported assessments by newly unemployed workers about the rate at which future treatments take place. Restrictive sticks policy regimes may induce individuals to search for work more actively and reduce their reservation wage, thus affecting labor market outcomes of treated and not-yet-treated. Ex ante effects of sticks policies have been analyzed in studies of policies in which the compliance to job search directives for unemployment benefits recipients is monitored. A particularly relevant study is Rosholm and Svarer (2008) who examine ex ante threat effects of sanctions. They use a methodology that at first sight looks similar to ours, as they allow the transition rate to work to depend on the

sanction rate. We discuss their study in more detail in Section 4 below when we present our methodological approach. Yet other studies have examined effects of warnings or notifications of the likelihood of future individual treatments (see Lalive *et al.* (2005) for a "sticks" policy, and Crépon et al. (2014) for a "carrots" policy). These empirical studies on ex ante effects generally find evidence of what may be called ex ante *attraction*: individuals who expect participation in a supportive program may reduce search intensity before the treatment and become more selective in terms of the jobs they accept, whereas for constraining programs this is reversed.

Regime effects are potentially more comprehensive than ex ante effects. Regime effects may also apply after a treatment has occurred, and they may capture a general comprehensive guidance approach of the PES and CW towards their clients, over and beyond the assignment of treatments. Regime effects may also include information spillover effects regarding policy intentions.[1] A key methodological difference with the studies on ex ante effects is that we exploit arguably exogenous PES and CW variation in policy regimes. Moreover, we jointly analyze a number of different policies. As we shall see, this makes it particularly important for our purposes to distinguish between intended exposures and actual treatment exposures in the empirical inference. In principle, since we estimate how the relative intensities of applying carrots and sticks policies influence earnings outcomes, we are able to answer the question which combinations of carrots and sticks maximize earnings, although, obviously, it is an open question to what extent such a policy would be supported by caseworkers and PES with substantial discretionary power. In any case, our empirical strategy allows us to directly identify the contributions of specific policy makers (PES and CW).

Few studies in the literature on the treatment effects of ALMPs estimate effects of multiple programs and their interactions. In this subset, even fewer consider a contrast between supportive and restrictive programs. van der Klaauw and van Ours (2013) is an exception, studying the effect of both re-employment bonuses and benefit sanctions on the re-employment chances of welfare recipients. Also, van den Berg, Bergemann and Caliendo (2010) show that newly unemployed workers report widely different subjective probabilities of future participation in training programs and in workfare, and that this is reflected in their job search behavior.[2]

[1] Policy regimes may lead to equilibrium effects through changes in labor demand and labor supply and their composition. However, at the CW level, no such equilibrium effects should be expected. Moreover, aggregate equilibrium effects are driven by actual policies (e.g., the actual number of individuals whose skills are upgraded in a training program) instead of intended policies. See e.g. Crépon *et al.* (2013) who analyze equilibrium effects of job search assistance programs, randomizing the fraction of treated within markets with randomization probabilities that vary across markets. Ferracci *et al.* (2014) consider effects of a training program and allow outcomes to depend on the fraction of unemployed that is trained. In terms of sources of exogenous variation there is an analogy with our paper, as they exploit exogenous variation in the fraction trained across regional employment offices. Both studies find crowding out of non-participants.

[2] Pavoni *et al.* (2013) discuss the optimal combination of work-first and job-search-first programs in a theoretical

Yet another related branch of literature studies CW-driven effects on unemployed individuals' outcomes. These effects, captured by activities like counseling or monitoring, appear to be substantial. For a recent overview of the evidence, see Rosholm (2014). This literature, however, does not directly assess the contribution of the CW to the estimated treatment effects (due to missing CW identifiers). In addition to this, there is evidence that CW do use their discretionary power, in that the variation in CW-induced ALMP assignments is substantial across caseworkers after correction for worker characteristics (see Eriksson, 1997, for an early randomized study). There is an analogy to the effect of physician-specific effects on sickness absence; see Markussen et al. (2013).

We end the introduction to this paper with a brief preview of our core findings and with an outline of the remainder of the paper. First, we find strong treatment effects as well as regime effects. Both supportive and restrictive policy regimes shorten unemployment durations. Supportive regimes increase earnings whereas restrictive regimes decrease them. Interactions between the different types of regime matter as well. Thus, ignoring the effects and interplays of policy regimes means ignoring a non-negligible part of the over-all ALMP effects – and thus means ignoring the role of the policy makers and policy implementers in UI systems.

The next section provides information on the institutional background of the empirical analysis, in Switzerland during our observation window. Section 3 presents the data and provides a descriptive analysis.[3] Section 4 presents the empirical approach to estimating policy regimes and discusses identification of the main parameters. We pay particular attention to the issue that individuals may influence the (latent) rate at which certain "sticks" treatments arrive. We also examine whether a relation exists between caseworker policy regimes on the one hand and the personality of the caseworker in his behavior towards clients on the other hand, since in the current study we are interested in the former but not in the latter. Here, as in other parts of the paper, we exploit insights from in-depth survey interviews held among caseworkers and PES offices. Section 5 provides a descriptive analysis of the measured policy regimes. Section 6 presents the main results. Here we also study various interaction effects between policies, and we provide a comprehensive cost-benefits analysis. Section 7 concludes.

setting where skills depreciate over the course of the unemployment spell.

[3]Below we also discuss additional existing literature with Swiss data in some more detail.

2 Institutional Background

The entitlement duration of unemployment insurance (UI) benefits in Switzerland is 400 days for individuals who meet the contribution and employability requirements. From age 55 onwards, benefits are extended by an additional 120 days. The replacement ratio is 80%; however, it is 70 % for those who earned more than CHF 4030 per month prior to unemployment and who are not caring for children.[4] Job seekers have to pay all earnings and social insurance taxes except the UI tax rate (which stands at about 2%). This means that the gross replacement rate is close to the net replacement rate. After the entitlement period, the unemployed have to rely on social assistance. The latter is means-tested and equals about 76% of unemployment benefits for an individual who is single and has no other sources of earnings.

Enrollment in UI has two requirements. First, the individual must have paid UI taxes for at least twelve months in the two years prior to registering at the public employment service (PES). Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job - they must be "employable". If a job seeker is found not to be employable there is the possibility to collect social assistance.

The entitlement criteria *during* the unemployment spell concern job search requirements and participation in active labor market programs. Job seekers are obliged to make a minimum number of applications to "suitable" jobs each month[5] and they are obliged to participate in active labor market programs during the unemployment spell. Compliance with the job search and program participation requirements is monitored by roughly 2500 caseworkers at 150 PES offices. When individuals register at the PES office, they are assigned to a caseworker on the basis of either previous industry, previous occupation, place of residence, alphabetically or the caseworker's availability. Job seekers have to meet at least once a month with the caseworker. Caseworkers monitor job search by checking that job seekers fill in the details of the jobs to which they have applied (monthly protocol of applications) and by asking them to present the sent applications at the meetings. Job seekers are typically required to apply to about 8 to 10 jobs per month. Participation in a labor market program is monitored by the caseworker

[4] In our observation window, 1 CHF = 0.96 Euro on average.

[5] A suitable job has to meet four criteria: (i) the travel time from home to job must not exceed two hours, (ii) the new job contract can not specify longer hours of availability than are actually paid, (iii) the new job must not be in a firm which lays off and re-hires for lower wages, and (iv) the new job must pay at least 68% of previous monthly earnings. Potential job offers are supplied by the public vacancy information system of the PES, from private temporary help firms or from the job seeker's own pool of potential jobs. Setting the minimum number of job applications is largely at the discretion of the caseworker at the PES.

because program suppliers only get paid for the actual number of days a job seeker attends the program. Moreover, non-participation is subject to sanctions as well (Lalive *et al.*, 2005; Arni *et al.*, 2013).

There is *remarkable discretion* in how often labor market programs and sanctions are used across PES. The authorities at the level of the canton and, in particular, the caseworkers have considerable leeway in the strictness with which rules are followed and guidelines are applied. With respect to sanctions, caseworkers may adjust, to some degree, the target number of required applications and the monitoring intensity. Caseworkers count the number of new applications in all cases and they may also check up on the applications claimed by job seekers. In the case of labor market programs, caseworkers dispose of some discretion in the assignment decision, with respect to participation, choice of program type and timing (Behncke *et al.*, 2010a).

The Swiss labor market policy distinguishes between four types of policy treatments: (i) Human capital training programs (this includes, as the mostly used sub-category, job search assistance programs); (ii) workfare programs (within public or non-profit institutions); (iii) subsidized temporary employment (during the unemployment spell); (iv) sanctions.

In this paper, we regroup these into two distinctive program types: *carrots* and *sticks*. The first group, *supportive* programs, comprise all kinds of training and job search assistance, thus type (i). The second group, *restrictive* programs, aggregates sanctions and workfare programs, thus types (ii) and (iv). The reason why we consider workfare programs first and foremost as sticks is that they are broadly disliked by the job seekers. Thus, they try to avoid them – for reasons of stigmatization and fear to be "locked in" into these programs over the longer period – by not proposing them to caseworkers. The above-mentioned survey by Behncke *et al.* (2010a) provides evidence that supports this interpretation. The remaining category of labor market policy, (iii), will be used as part of the control variables. This is because subsidized temporary employment is largely searched and proposed by the job seekers themselves, so caseworkers do not have much discretional choice in this respect. It is thus hard to use this type of program in a strategic way, in the sense of a carrots or sticks policy.

3 Data and Descriptive Statistics

Our analysis uses data from two sources. The unemployment insurance register contains administrative information on all spells of registered joblessness. For our sample we extract all the spells that started between July 2000 and June 2005 for job seekers who were 61.5 years old or less when they registered at the PES. This data records unemployment duration: this is the

number of days a job seeker is registered with the local PES. Note that unemployment duration can deviate from days on unemployment benefits. Individuals may register with the PES before they lost their job. Job seekers may, in principle, also de-register before they start on the new job. Unemployment duration is still a useful concept for our analysis since job seekers need to be registered to follow ALMPs. The data also contain detailed information on the timing of ALMP participation and benefit sanctions events in daily precision. The data informs on where job seekers live, which PES is in charge of the job seeker, and also information on the caseworker in charge. Usually, caseworker assignments are fix over the course of the unemployment spell but there are exceptions[6]. We focus on the caseworker initially assigned to the individual. We have detailed information on socio-demographics, employability, occupation, benefit variables, household size, and whether a person has filed an application for disability insurance benefits.

Our second data source is social security register data. This data covers a 25 % random sample of all workers between 1982 to 2008. The data provide information on employment and earnings for every month between 1982 and 2008. We use this data source to construct 5 years of pre-unemployment history for every spell of joblessness. We also use it to construct our main outcome variables. We look at real monthly employment earnings in the period of 3.5 years after leaving unemployment. We also separately record the number of months a job seeker has been employed, and the average earnings during the employment months during the 3.5 year post-unemployment period. This allows us to decompose earnings into an employment and into earnings while employed component.

Table 1 provides descriptive statistics of the key variables for our main estimation sample of 131,037 job search spells of eligible men aged 20 to 61.5 years. About one job seeker in five, 22 %, enter a supportive program, or about one job seeker in five, 19 %, a restrictive program during their period of job search. The median time until the supportive program starts is 97 days, restrictive programs start somewhat earlier, after 71 days. Most job seekers are either married (46 %) or single (45 %), and fewer ones are divorced or widowed (proportion not shown). A substantial proportion of job seekers in our data have completed a 4 year vocational training after compulsory schooling (50%). The second most important educational attainment is compulsory schooling (28%). Relevant proportions of job seekers have either completed a short vocational training of 2 years (9%) or a tertiary degree (10 %). Male job seekers typically work as blue collar factory workers (13%), construction workers (13 %), or in the restaurants or cleaning sector (13

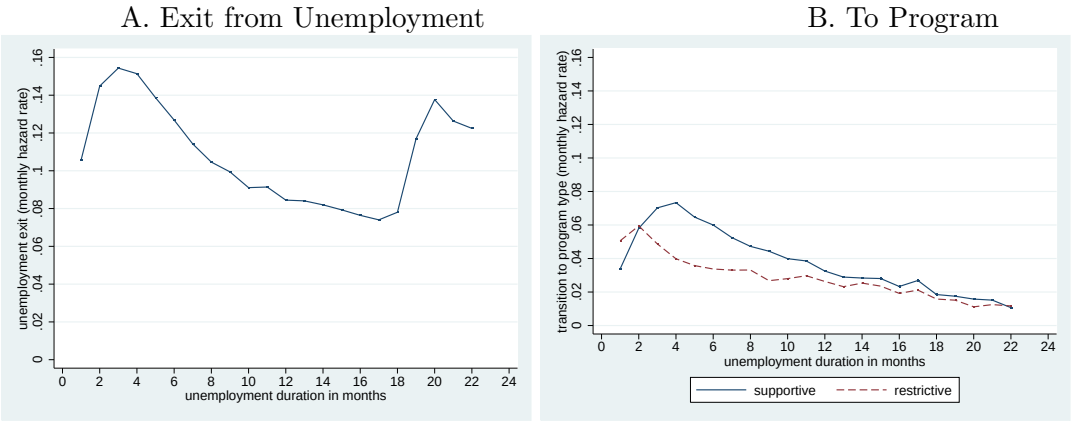
[6]E.g. some areas tested practices where caseworker assignment switches after 6 or 9 months; but this is a minor quantity. Other reasons for occasional assignment changes are that caseworkers leave the PES to look for another job or when they are sick.

%). Descriptive statistics also show information on employability, a caseworker assessment of the chances the job seeker will find work. Most job seekers have medium employability indicating no large problems with job placement (72 %), but a sizeable proportion also have low employability (15 %) as well as excellent employability (13 %, not shown in table). Job seekers are 36 years old on average, on average living with 2.16 persons in a household. About 42 % of all job seekers do not have Swiss nationality, and 39 % do not speak the local language as their mother tongue.

The median unemployment duration in our sample is 144 days. We measure unemployment as the number of days between registering at the PES agency until de-registering from the PES. This is the period during which job seekers in Switzerland have access to active labor market programs (regardless of their current employment status).

Figure 1A shows the empirical exit rate from unemployment, i.e. de-registrations from the PES. Job seekers leave unemployment initially at a rate of 10 percent per month. The transition rate then increases, peaks at 15 percent per month after 3 months of unemployment, and gradually decreases to 7 percent per month after 18 months of unemployment. Benefits end for most job seekers after 18 months of unemployment. As usual, we observe an increase in the transition rate out of unemployment shortly before the expiration of benefits entitlements.

Figure 1: Transition rates



Notes: Graph A shows the empirical transition rate out of unemployment. Graph B shows the empirical transition rate to restrictive (sticks) programs, and to supportive (carrots) programs. Restrictive programs are benefit sanctions and workfare programs. Supportive programs include job counselling and training programs.

Source: Swiss UIR-SSR Data.

Figure 1B shows the empirical transition rate from unemployment to a supportive program. In the beginning of the unemployment spell, just short of 4 percent of all job seekers start a supportive program. The probability of entering a supportive program then increases to a maximum of 7 percent per month, and it decreases gradually to a level just above 1 percent

Table 1: Descriptive Statistics

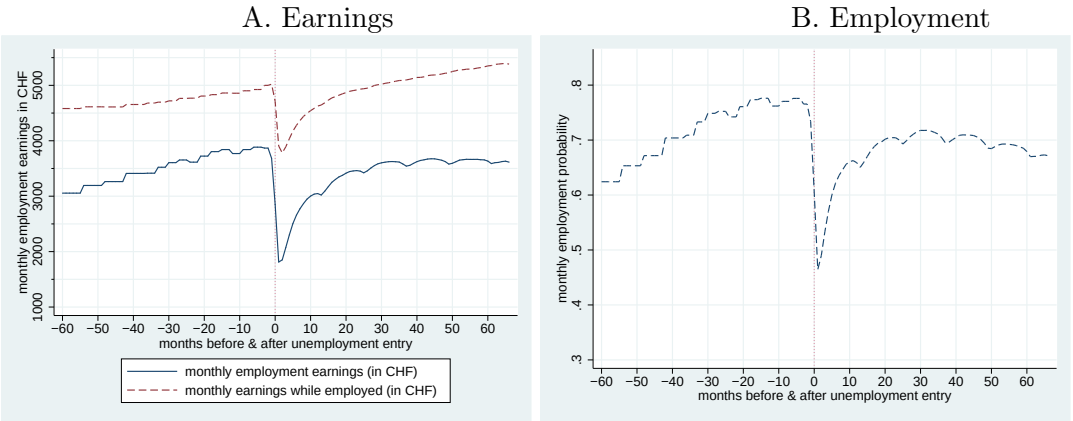
		mean	sd
Unemployment duration	(median, days)	144	
Realized treatments			
supportive ("carrots")	(incidence)	0.219	
supportive: duration	(median, days)	97	
restrictive ("sticks")	(incidence)	0.187	
restrictive: duration	(median, days)	71	
Socio-demographic characteristics (selection)			
marital status	single	0.453	
	married	0.463	
education	compulsory (-9y.)	0.276	
	vocational short (-11y.)	0.094	
	vocational degree (-13y.)	0.504	
	high school (-13y.)	0.028	
	tertiary	0.098	
occupation	blue collar	0.136	
(3 biggest)	construction	0.138	
	gastronomy, cleaning	0.134	
employability	low	0.145	
	middle	0.718	
age (years)		36.1	11.0
household size		2.16	1.35
not swiss		0.422	
does not speak local language		0.394	
# unemployment spells		131 037	

Notes: Sample used in main estimations (men, aged 20-61.5). Mean proportions if no other unit is stated. Realized treatments: incidence=at least on realized treatment of corresponding type (supportive, restrictive); duration=duration from unemployment entry to realization of the treatment.
Source: Swiss linked Unemployment Insurance and Social Security Register (UIR-SSR) Data

after 22 months. The transition rate to restrictive programs follows a fairly similar pattern, but it is substantially below that of supportive programs throughout the unemployment spell. Note that the duration dependence of all transition rates that we show in Figure 1 might be spurious as we do not control for heterogeneity in these plots.

We now turn to discussing employment and earnings measures. Figure 2 shows earnings and employment paths for the job seekers in our estimation sample, relative to the calendar date of PES registration, which is normalized to zero on the horizontal axis. "Earnings for employed workers" represent average earnings among individuals who are employed during a month. Employment is the proportion of individuals in our sample who hold a job in a month. These two measures can be combined into our total average-population "earnings" measure. In employment, these "earnings" are taken to equal to actual earnings whereas in non-employment they are set to zero. The total "earnings" measure can decrease for a number of reasons: either employed workers are paid less, or fewer individuals hold a job, or both.

Figure 2: Earnings and employment paths



Notes: Graph A shows two earnings measures. "Earnings while employed" (dashed line) represent average earnings among those who are employed during the month. "Earnings" (solid line) measure average earnings, i.e. with zero earnings in case of non-employment. Graph B depicts employment. Earnings and employment paths are relative to the month of entry into unemployment according to the unemployment duration measure in the unemployment register.

Source: Swiss UIR-SSR Data.

Total average"earnings" increase somewhat before the unemployment and decrease sharply upon entering unemployment. Note that they do not reduce to zero. There are two reasons for this. First, a substantial proportion of job seekers register at the PES even before losing their job. Secondly, very short unemployment durations may lead to non-zero earnings in the months on or right after the unemployment registration. By construction, the average "earnings while employed" exceed the average total "earnings". Unemployment does not reduce earnings compared to the pre-unemployment level.

Figure 2B shows employment. Most job seekers are employed before registering at the PES, even though the employment rate is far from 100 % in the month prior registering. The employment rate then decreases substantially but does not reach zero in the month when job seekers register at the PES. Again, this shows that entering our state of "PES-registration" unemployment and leaving a job are not necessarily concurrent. The employment rate quickly increases in the first 6 months of the spell, and continues to increase more gradually thereafter, stabilizing at the pre-unemployment level.

4 Conceptual Framework

In this section we explain the methodology and we show how its key assumptions are justified by the institutional setting of the Swiss labor market.

4.1 Variation in policy regimes

We define a policy regime as the intended intensity of use of a program. In Switzerland, policy regimes may vary across PES offices and across caseworkers. They may vary at the PES level because this is the de facto unit that implements the procedures leading to policy regimes. A 2003 survey among 98 heads of PES shows to what extent PES directors are managed by the canton, and how strictly they manage their caseworkers (Table 2). Heads of PES are not completely free in their work, and most of them do not let their caseworkers do as they please. However, more than half of the heads of PES obtain only rough guidelines and are free to define their strategies within those guidelines. A similar proportion sets only rough guidelines for caseworkers and lets them choose freely within those guidelines. Our empirical approach will exploit exactly this within-canton and within-PES leeway to estimate policy regime effects.

Table 2: Leeway in the Swiss ALMP System

	Canton to PES percent	PES to Caseworker percent
(1) no guidelines (freedom)	4.08	13.27
(2) rough guidelines	57.14	56.12
(3) detailed guidelines	32.65	29.59
(4) very detailed guidelines	5.10	0.00

Notes: Responses to the question "How detailed are the directions that you receive from your supervising agency (canton)?" for the Canton to PES column, and "How detailed are the directions that you give to your caseworkers?" for the PES to Caseworker column. One person provided no answer to both questions. 98 heads of PES.
Source: Frölich *et al.* (2007) Survey.

The literature cited in Section 1 suggests that, in our context, supportive ALMP (carrots) may have regime effects that affect treated as well as non-treated, for example through ex ante effects. One reason for ex ante effects that has not been examined before but that may be relevant in our context is that PES and CW with strongly supportive regimes may invest more effort in getting to know the job seeker in order to set up a more targeted treatment plan. Such a more effective service may enhance the job seeker's search efficiency (how and where to search) and increase job offer arrival rates before the actual job search assistance program participation takes place.[7] We also expect that restrictive (sticks) policies have regime effects that affect treated and non-treated. Interestingly, Lalive *et al.* (2005) and Arni *et al.* (2013), who study the effects of unemployment benefit sanctions in Switzerland, document that non-sanctioned job seekers leave unemployment more quickly in PES that use sanctions more often.

4.2 Ranking different policy regimes

If a policy affects outcomes then actual usage of the program does not necessarily provide an accurate description of intended usage, for two reasons. First of all, an individual may leave unemployment before participating in the program. Secondly, regimes may generate ex ante effects which, depending on the policy, may allow individuals to influence the actual usage of the treatment themselves. We now describe how we deal with these issues.

The first issue can be dealt with by invoking duration analysis with competing risks. Let t_u, t_c and t_s denote the time (in unemployment) until job exit, participation in a carrot ALMP, and exposure to a sticks policy treatment, respectively. Next, let t_p denote the duration until the first event in the unemployment spell. The latter follows a competing-risks process: $t_p = min(t_s, t_c, t_u)$. We define the corresponding treatment dummies as follows: $D_c = 1$ if $t_p = t_c$, and $D_s = 1$ if $t_p = t_s$.

Our operationalization of the intended policies by PES and by CW is based on the hazard rates of the latent durations t_c and t_s, respectively, for each PES and for each CW, adjusted for individual characteristics x and for the elapsed unemployment durations at the moment of treatment. We adopt the following proportional-hazard functional forms,

$$\theta_c(t, x, \{D_j^L\}, Y^-, \tau) = \lambda_c^L(t) \exp(x'\beta_c^L + \sum_{j=1}^{N_L} \gamma_{c,j}^L D_j^L + (\alpha_c^L)'Y^- + \sum \eta_{c,\tau}^L) \tag{1}$$

[7] Lechner *et al.* (2013) show that Swiss ALMPs do not increase firm growth and stability of its employment, aspects that we do not focus on.

$$\theta_s(t, x, \{D_j^L\}, Y^-, \tau) = \lambda_s^L(t) \exp(x'\beta_s^L + \sum_{j=1}^{N_L} \gamma_{s,j}^L D_j^L + (\alpha_s^L)'Y^- + \sum \eta_{s,\tau}^L) \tag{2}$$

where t is the elapsed duration of unemployment and L indexes the two layers of implementation, so $L \in \{PES, CW\}$. Further, D_j^{PES} with $j = 1, .., N_{PES}$ is a set of PES dummy variables and D_j^{CW} with $j = 1, .., N_{CW}$ is a set of CW dummy variables. Finally, η is a set of half-yearly inflow cohort dummies (where τ denotes calendar time at the moment of inflow), and Y^- is a series of variables that controls for the earnings history of the individual in the last 60 months before unemployment (split in 17 time intervals/parameters).

In the absence of systematic unobserved heterogeneity, the above hazard rates θ_c and θ_s can be estimated in isolation from each other. Specifically, the competing-risks approach treats all t_p spells that end in an event that differs from participation in the program of interest as independent right-censoring of the time to participation in that program.[8,9] Notice that this is not related to the existence of a policy regime effect of θ_c and θ_s on t_u.

Next, for each individual with a given x and a given PES and CW, we can estimate individual probabilities that a treatment event occurs within two years in the absence of other events, using

$$F_c^L = 1 - \exp\left(-\int_0^{730} \theta_c(t, x, \{D_j^L\}, Y^-, \tau)dt\right)$$

and analogously for F_s, with θ_c and θ_s specified as in equations (1) and (2). As explained by e.g. Van den Berg (2001), the signs and relative magnitudes of the estimated covariate effects on (one minus) the survival probability are robust with respect to the omission of unobserved heterogeneity, whereas this does not always apply to the covariate effects on the hazard rates. This is one reason to prefer F_c and F_s over θ_c and θ_s as regime indicators. A second reason is that F_c and F_s naturally cover a time interval whereas θ_c and θ_s assume different values at different elapsed durations. In the next subsection we use the estimated F_c and F_s as regressors representing the intensity or strictness of the policy regimes.

We now turn to the second issue mentioned at the beginning of this subsection, namely that job seekers may ex ante influence the rate at which they are treated in response to the perceived

[8] We discuss the role of unobserved covariates below. Note that even in their absence, some regularity assumptions need to be satisfied; for example, it is not allowed that one type of treatment can only occur after the other type.

[9] We could extend the competing-risks setting by including observations of the occurrence of a second treatment if that is of a different type than the first. However, this would simultaneously necessitate the estimation of the causal effect of the first treatment at durations in-between the first and second treatment. Clearly, this means a loss of all the computational advantages of our approach. Moreover, it means that we would need to address the occurrence of consecutive treatments of the same type as well, and the estimated intended-policy indicators would be sensitive to the assumptions about chain reactions between treatments and treatment effects as well as the contents of the second treatment.

policy regime. In that case the "latent" competing-risks hazard rates θ_c and θ_s depend both on the regime and on the reaction to the regime, so that they may not fully characterize the intended policy intensity anymore. With supportive (carrot) policies this is not likely. If the corresponding treatments are deemed attractive then their supply will always be rationed by the administrative unit.[10] Even if individuals can influence the rate at which they participate in a carrots program, the ranking of the estimated latent hazard θ_c across PES (or CW) will probably not revert the ranking of intended usage across PES (or CW). Therefore, the indicators based on this hazard should still reflect the ranking of intended usage. As a result, the effects of the indicators we constructed are still informative on the effects of the intended usage.

With restricted (sticks) policies such as workfare, a similar line of reasoning can be applied. However, with sanctions, the individuals have a much stronger influence in the occurrence of the treatment. As a result the effect of the strictness of the policy regime on the sanction rate (i.e., on the rate as estimated in the competing risks analysis) may be non-monotonic. To see this, consider a policy where individuals' search effort s is stipulated to meet or exceed a lower threshold value s^*. Individuals suspected to violate this rule are monitored at the rate p_0, and if it is detected that $s < s^*$ then a sanction is imposed.[11] The strictness of the policy regime is then p_0 whereas the sanction rate is $p_0 \cdot \mathrm{I}(s < s^*)$. The former is the quantity of interest whereas the latter is obtained from the competing risks analysis (it equals θ_s in the absence of other sticks policies). Clearly, if $p_0 = 0$ then both of these are equal to zero. If p_0 increases then the fraction of individuals with $s < s^*$ will decrease because of the strategic ex ante reaction, but the actual sanction rate will then typically be positive. However, if $p_0 \to \infty$ then each violation leads to a punishment. If the punishment is sufficiently large then each individual will choose s such that $s \geq s^*$. Thus, the policy-regime strictness goes to infinity but the sanction rate goes to zero. As a result, an estimated sanction rate of zero is compatible both with a very lax regime and with a very strict regime. This means that the estimate of θ_s is not informative of the intended policy, unless we restrict attention to low to moderate levels of the intended usage and/or sanctions only constitute a minor fraction of the total package of sticks policies. These conditions are met in our setting.

[10] Clearly, individuals have an incentive to stay unemployed in order to benefit from the treatment, e.g. by rejecting job offers and reducing search effort. However, such strategic ex ante reactions are part of the policy regime effects that we are after in the analysis of the outcomes of interest.

[11] This setting is inspired by the job search model with monitoring and sanctions in Abbring *et al.* (2005).

4.3 Effects on outcomes

We are interested in measuring how policy regimes affect the unemployment exit hazard or earnings after leaving unemployment. We estimate the following unemployment exit hazard

$$\begin{aligned}\theta_u(t) &= \lambda_u(t)\exp\Big(x'\beta_u + \delta_{s,u}D_s(t) + \delta_{c,u}D_c(t) + \pi^{cw}_{s,u}F^{cw}_s + \pi^{cw}_{c,u}F^{cw}_c \\ &+ \pi^{pes}_{s,u}F^{pes}_s + \pi^{pes}_{c,u}F^{pes}_c + \alpha'Y^- + \sum\eta_\tau + \sum\mu_m\Big)\end{aligned}$$

whereby μ_m is a set of spatial fixed effects and the other notation is as in the previous subsection. Note that we exploit past earnings information to control for employment-related selective differences between the individuals. Also note that the treatment dummies D are now time-varying, to distinguish between the time before and after treatment. In subsequent analysis we also allow for interaction effects between the various policy regime indicators.

To demarcate units for the spatial fixed effects, we use the 106 commuting zones constructed by the Swiss authorities to capture local labor markets. These zones are usually called *Mobilité Spatiale* regions (in short, MS regions). These do not coincide with PES. In our data, there are on average 4.6 PES per MS, with a standard deviation of 2.9. In cities constituting one MS there may be a considerable number of PES. For example, the metropolitan areas of Geneva and Zürich contain 11 and 8 PES, respectively.[12,13] Notice that in analyses where PES regime effects are assumed absent, we may use the PES as the relevant spatial unit for the spatial fixed effect.

Similar to the above equation, the effects of policy regimes and treatments on earnings and employment after the unemployment spell ended are modelled as follows:

$$\begin{aligned}Y &= x'\beta_Y + \delta_{s,Y}D_s + \delta_{c,Y}D_c + \pi^{cw}_{s,Y}F^{cw}_s + \pi^{cw}_{c,Y}F^{cw}_c + \pi^{pes}_{s,Y}F^{pes}_s \\ &+ \pi^{pes}_{c,Y}F^{pes}_c + \tau' f(t_u) + \alpha'Y^- + \sum\eta_t + \sum\mu_m + \varepsilon_Y\end{aligned}$$

where, obviously, α, μ and η attain different values than in the equation for θ_u. Y can represent different post-unemployment outcomes (over a time window of 3.5 years) like average monthly earnings or employment probabilities (i.e. employment stability measures). We control as well

[12] We do not include MS dummies in θ_c and θ_s in equations (1) and (2) since for our purposes that does not make much sense if we also include the PES dummies. Furthermore, their effects are difficult to distinguish from the CW effects since CW do not often move across MS in our observation window.

[13] Notice that in the outcome equation for say individual i the F_c and F_s terms are estimated in a first stage from which individual i is not excluded. Formally, those estimates reflect in part the actual treatment statuses of individual i, in a mechanical way. However, with the sample sizes we have this is of negligible order, and a more sophisticated procedure would be computationally very challenging.

for the completed[14] unemployment duration t_u (in polynomial form) and PES or MS region fixed effects μ_m. Note that adding past earnings in the earnings equation implies that we estimate treatment effects in a flexible approach in the Diff-in-Diff spirit. Also note that, jointly, the analyses of employment and of earnings while employed entail a decomposition of the total effect on post-unemployment earnings.

Clearly, since the equations to be estimated are regression-like specifications without complex error structures, all model parameters are identified provided that regressors are not perfectly correlated to each other. We proceed to discuss the latter assumption as well as the key assumptions that underlie the identification of the causal effects of interest from the regression-like specifications, starting with the latter. First, notice that to identify the causal effects of attending a particular program and the causal regime effects, we make conditional independence assumptions (CIA).[15] Our framework estimates the treatment and regime effects within one equation, so regime effects are identified conditional on treatments and vice versa. Obviously, this means that inference on regime effects and treatment effects requires less assumptions than in the case where only treatment effects or only regime effects are analyzed. For example, our treatment effect estimates control for the fact that the caseworker may influence both the individual treatment status and the outcome of interest, where the latter channel runs through the policy regime imposed by the caseworker. Identifying treatment effects on the unemployment exit rate further requires the assumption of "no anticipation" (NA) (Abbring and van den Berg, 2003). NA explicitly allows for regime effects that are included in the model. It rules out that individuals have private advance information on the timing of future treatments, that is not captured by the model specification. NA is justified for Swiss ALMP, as the time between the knowledge that a decision is made to assign a program or a sanction, and its realization, is usually shorter than two weeks (Lalive *et al.*, 2005, 2008).

To justify the identification of causal caseworker regime effects we must examine how job seekers are allocated to caseworkers. The Behncke *et al.* (2010a) survey provides information on this (multiple answers are possible): 24 % of all caseworkers indicate that their clients are assigned randomly, 50% by industry, 55% by occupation, 44% by work-load.[16] Hence, random

[14] In total, 5.8% of the unemployment spells are right-censored. One reason for the low censoring rate is that we continue to follow everyone after UI entitlement exhaustion by using social security data. We right-censor unemployment durations exceeding 730 days. Some of these may be due to coding errors in the transition date out of unemployment. For the censored observations we use the censored duration in the above equation and we use actual earnings on the left-hand side.

[15] Arni *et al.* (2013) study sanction effects using Swiss data and find that modelling selection due to unobservables becomes unnecessary for the unemployment duration analysis once one conditions on pre-unemployment earnings and employment histories.

[16] Other reasons for assignment were employability and age but these were mentioned by fewer than 10 % of all

or quasi-random assignment appears plausible.

There is a thin line between the CW policy regime and what we might call the "caseworker style". Caseworkers may differ in terms of how they interact with job seekers: how friendly they are, how much empathy they feel for their clients etc. Such a caseworker style may be correlated with the caseworker's ALMP assignment policy. The interpretation of our regime effect estimates depends on this. If caseworker style is important and correlated to our regime indicators then the estimates at least partly reflect how the CW interact with job seekers on a daily basis. Behncke *et al.* (2010b) show that caseworkers differ in their attitudes to their work: some would love to help their client, some consider controlling their client as their main task, and some feel that this matters for job search success. We further assess this issue using the Behncke *et al.* (2010a) survey. We first construct a measure of how important each caseworker believes restrictive or supportive policies are.[17] We then correlate the importance of supportive and restrictive programs with caseworker style. We find absolutely no correlation with supportive policies (correlation coefficient –0.0234) and a small, positive, correlation with restrictive policies (correlation 0.1345). Thus, we feel confident that caseworker style does not drive our results.[18]

Identification of PES regime effects requires conditional independence (within MS region) of PES regime with respect to outcomes. This assumption appears plausible as job seekers can not choose which PES takes care of them, so endogenous mobility between PES is not an issue. A challenge to this assumption might be called (by analogy to the above-mentioned "caseworker style") the "PES style". We can not rule out that PES style plays a role. But our data cover a very wide range of activities that feed into the job search process. We are likely to capture most of these activities. Moreover, we discuss below that the two key activities of a PES, assignment to restrictive programs and assignment to supportive programs, are virtually unrelated. Orthogonality between these two key policy dimensions bolsters our confidence in our assumption of orthogonality with respect to other unmeasured dimensions.

caseworkers.

[17] We proceed as follows. The seven types of job seekers are: job seeker who enters unemployment after an apprenticeship, job seeker with good prospects, job seeker with bad prospects, qualified Swiss, un-qualified Swiss, qualified Immigrant, un-qualified immigrant. Caseworkers indicate for each job seeker profile whether they think restrictive and supportive programs are important. We aggregate the number of times a caseworker finds a program is important and end up with a number that ranges from zero to seven. Seven indicates that a caseworker would use the program regardless of the type of job seeker he or she is serving. Zero indicates the caseworker would never use the program.

[18] One may consider using the caseworker specific use of a treatment as an instrument for treatment itself, as in e.g. Markussen *et al.* (2012). This approach fails in our setting, since caseworkers and job seekers entertain a long-term relationship that reveals information on the caseworker regime to the job seeker before any treatment. Indeed, it is one of our aims to investigate whether job seekers react to this information. Adopting the caseworker candidate instrument, we would have to assume that caseworker regime effects do not exist. For this reason we do not present analyses excluding actual treatment effects since such an analysis merely averages policy regime effects and actual treatment effects.

Distinguishing between caseworker regime effects and PES regime effects in the outcome equations is only meaningfully possible if there is no one-on-one connection between caseworkers and PES. With a one-on-one connection, any caseworker is attached to one PES only, and the caseworker is employed at that PES from the beginning until the end of the observation window. In that case there is a deterministic relation between caseworker dummies and PES dummies, and the PES effect is not identified from the effects of its caseworkers without invoking arbitrary assumptions. Since we estimate the caseworker policy intensities separately from the PES policy intensities, and since both intensities are non-linear functions of the underlying dummy variables, the estimation of an outcome equation would not necessarily result in a singular information matrix. However, such estimation results would be purely driven by ad hoc non-linear functional form restrictions. Fortunately, we do observe job transition movements by caseworkers into and out of PES and between PES. As many as 23% of the caseworkers move at least once between different PES. Some of these coincide with observed reorganizations of PES units, including some mergers of different PES units into larger PES units. All this leads to variation that is useful to distinguish between CW and PES regime effects. We carried out sensitivity analyses in which we only use subsets of the full data, namely those where movements of CW are relatively common, and those where PES areas changed over time. The estimates are very similar to those obtained with the full data set. Of course, it is inevitable that with heterogeneous CW effects, the estimated PES effects will capture to some extent the composition of the caseworkers. We should point out that all results below are replicated for specifications in which PES regime effects are switched off.

We finish this subsection by comparing our methodology to Rosholm and Svarer (2008) who developed an innovative approach to estimate ex ante threat effects of ALMP. They restrict attention to sanction policies. Specifically, they estimate a bivariate duration model, for the duration until a sanction and the unemployment duration, controlling for selection on unobservables by way of a random effects specification. In addition, they include the sanction hazard rate as an explanatory variable for exit out of unemployment. The underlying idea for this is that it should capture the ex ante threat effect of sanctions on the exit out of unemployment. This resembles our inclusion of F_s as a regressor in θ_u. However, note that our set of sticks policies is larger than just the sanction policy. More importantly, we use PES and CW as exogenous indicators to characterize the sticks policy regime, so that our results are not driven by the functional-form requirement that the covariates in (a function of) θ_s and the other covariates in θ_u should not act additively in θ_u.

5 Descriptive Analysis of Policy Regimes

To gauge the variation in the actual usage of the "carrots" and "sticks" policies and the variation in estimated policy regime intensities, as well as their interrelations, this section provides some descriptive statistics. The observed frequency of usage of a policy (or the "observed intensity") is measured by the frequency of imposition of at least one treatment of the respective program type within a spell of unemployment. On average we observe that one in every five individuals (0.22) is subject to a training or job search assistance program and also that one in every five individuals (0.19) is sanctioned or has to join a workfare program during unemployment. This is true both for PES and for caseworker regimes (Table 3).

In contrast, the policy regime intensities (or "intended policy intensities") as estimated in Subsection 4.2 are substantially higher. About three job seekers out of five (0.58) would enter a supportive program within two years if there is no possibility to leave unemployment or to be confronted with a restrictive program. Likewise, about one in two job seekers (0.53) would face a restrictive program according to the intended policy regime. The standard deviation of the intended policy intensity is also substantially larger than the standard deviation of the observed intensity.

Table 3: Observed frequencies of policy usage and intended policy intensities, by PES and by caseworkers. Descriptive statistics

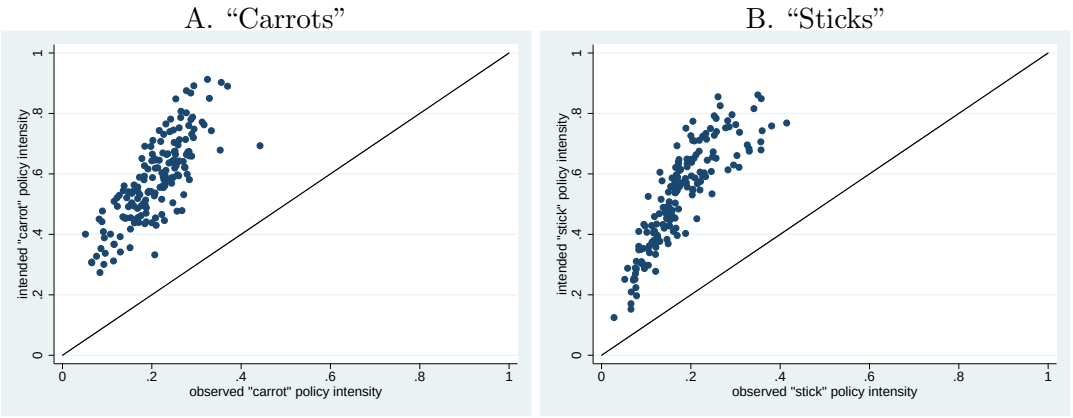
			mean	median	s.d.
observed	*PES*	*"carrot"*	0.2155	0.2247	0.0615
	PES	*"stick"*	0.1853	0.1723	0.0727
	cw.	*"carrot"*	0.2150	0.2263	0.0647
	cw.	*"stick"*	0.1848	0.1852	0.0690
intended	*PES*	*"carrot"*	0.5834	0.5865	0.1817
	PES	*"stick"*	0.5328	0.5318	0.2161
	cw.	*"carrot"*	0.5847	0.5982	0.1842
	cw.	*"stick"*	0.5360	0.5389	0.2155
		Observations	131,037		

Notes: Calculations based on main sample (males aged 20-61.5). cw. = caseworker. Observed frequencies are averages per PES or per caseworker. The estimation of the intended policy intensities is described in Subsection 4.2. We distinguish between 168 PES and 717 caseworkers (small caseloads below 100, males and females, are aggregated in a residual caseworker category/dummy variable).
Source: Swiss UIR-SSR Data.

Figure 3 plots intended vs observed intensities across PES.[19] Intended policy intensities are

[19]Results are similar at the caseworker level; those are available upon request.

Figure 3: Observed and intended policy intensities by PES; "carrots"; "sticks"



Notes: This figure shows intended use of programs on the vertical axis vs observed use of programs on the horizontal axis. The solid line represent the 45 degree line. Each dot is a PES agency.

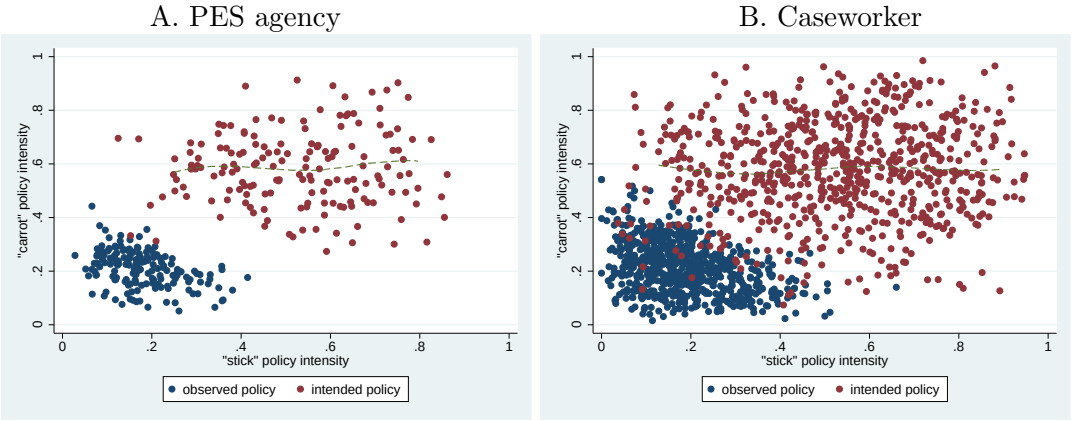
always larger than observed policy intensities. If this were not the case for some PES then this would signify a model specification problem in the sense that the specifications of θ_s or θ_c are too restrictive. The discrepancy between observed and intended intensities tends to be especially important for extreme regimes, i.e. those that intend to place everyone to a supportive or to a restrictive treatment.

The fact that actual and intended policy usage are not perfectly related is important for at least two reasons. First, it means that discarding the competing risks analysis and instead using actual observed intensities would lead to biased effects. Secondly, since PES equilibrium effects are captured by actual usage by PES rather than intended usage by PES, it follows that our intended PES policy regime intensities are not synonymous to PES equilibrium effects.

We are also interested in the degree of concurrence of restrictive and supportive policies. Figure 4A reports the variation of policy mixes, i.e. combinations of carrots and sticks policy intensities. The actual observed policy mixes broadly cover the two-dimensional policy space in the ranges between 0 and 0.4. This suggests that there is substantial two-dimensional variation to support its exploitation in our estimation strategy. Figure 4B shows the corresponding results for policies at the caseworker level.

Figure 4 also displays the correlation between intended carrot and stick policies. There is no correlation between the intended intensity of the two policy instruments. This could mean that PES regime for one policy is determined in isolation of the regime for the other policy. Moreover, it is consistent with our maintained hypothesis that "PES style" is not driving our results. If PES

Figure 4: Observed vs. intended policies by PES and caseworker



Notes: This figure shows observed (dark) and intended (red) intensities of carrots and sticks. Each dot represents a PES agency (left) or a case-worker (right). The dashed line presents the association between the two policies (based on lowess smoother).

were planning their comprehensive policy regime mix, one might expect a correlation between the intended intensities.

6 Results

6.1 Baseline estimation results

Effects on earnings: Table 4 reports results on earnings. The dependent variable captures average earnings after leaving unemployment over a period of 42 months (3.5 years). All estimates control for the full set of individual control variables and a full set of PES dummies (columns 1 to 5), or labor market region (MS) dummies (columns 6 and 7). Standard errors are robust to heteroskedasticity but assume independence across spells.

Column (1) to (4) in Table 4 show the effects program participation. Supportive programs increase earnings, restrictive programs decrease them. Sanctions are especially detrimental to earnings after leaving unemployment, reducing them by 348 CHF or about 10 percent of average monthly earnings. Workfare programs also reduce earnings but the reduction is 70 CHF per month, around 2 percent of monthly earnings. Estimating the program participation effects jointly reveals that the treatment effect on carrots is somewhat smaller, and the stick effect is not as negative, since the baseline earnings now is non-participants for both estimates. Attending a supportive program increases earnings by 158 CHF per month, around 5 percent of earnings. A restrictive program reduces earnings by 309 CHF or almost 10 percent.

Table 4: The effect of carrots and sticks policies and treatments on monthly earnings (over 3.5 years; men)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
carrot TE	280.5***		162.3***	152.7***	157.9***	159.9***	164.3***
	(19.36)		(21.33)	(21.27)	(21.34)	(21.22)	(21.32)
stick TE		-368.8***		-308.6***	-291.8***	-283.0***	-274.3***
		(16.30)		(17.96)	(18.09)	(18.04)	(18.25)
sanction TE			-348.3***				
			(18.84)				
workfare TE			-70.16**				
			(34.56)				
carrot policy CW					10.39		-50.74
					(66.87)		(64.69)
stick policy CW					-415.9***		-215.3***
					(68.37)		(57.45)
carrot policy PES						842.4***	873.7***
						(141.2)	(144.6)
stick policy PES						-1,132***	-1,053***
						(169.6)	(162.3)
intercept	3,223***	3,216***	3,215***	3,210***	3,416***	3,455***	3,535***
	(70.79)	(70.79)	(70.79)	(70.79)	(82.38)	(110.7)	(116.2)
obs. (spells)	131,037	131,037	131,037	131,037	131,037	131,037	131,037
R^2	0.380	0.381	0.382	0.382	0.382	0.377	0.377
FE at level	PES	PES	PES	PES	PES	MS	MS

Note: TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Columns (5) to (7) in Table 4 discuss policy regime effects, on top of program participation effects. Case-workers who intend to use restrictive programs more frequently tend to reduce their job seekers earnings after leaving unemployment, and the effect is sizeable. Increasing the intended use of restrictive programs by 10 percentage points reduces a job seekers earnings by 42 CHF, or around 1.5 percent. Case-workers who use supportive programs more frequently do not affect their job seekers' earnings.

Column (6) shows that the variation in policy regimes at the PES level also matters. Increased use of restrictive policies reduces job seeker's earnings. The PES effect is stronger than the case-worker effect: a 10 percentage point increase in the use of restrictive programs decreases a job seeker's earnings by 113 CHF per month, about 3.5 percent of earnings. Interestingly, increased use of supportive programs has the opposite effect: 84 CHF per month, 2.6 percent, more if supportive programs increase by 10 percentage points.

Column (7) shows the full model. Results indicate that all aspects matter: program participation, case-worker policy regimes, and PES policies. The program participation effects and the

PES policy effects are similar to the models that included only part of all explanatory variables. Case-workers who use restrictive policies more intensely reduce their client's earnings by 22 CHF per month, rather than 42 CHF per month, as reported earlier.

Effects on unemployment exit rate: Table 5 shows how policy regimes and program participation affect the transition rate from unemployment to regular jobs. Columns (1) to (3) show the effects of program participation on the rate of leaving unemployment. Results indicate that both supportive and restrictive programs reduce the hazard of leaving unemployment. That supportive programs prolong unemployment duration is well in line with existing research (Card *et al.*, 2010). Our result on restrictive policies goes counter the existing literature on benefit sanctions. But bear in mind that our set of restrictive policies encompass both benefit sanctions and participation in workfare programs. When we decompose the effects of these two programs on the unemployment exit rate, we find a positive effect of benefit sanctions on the unemployment exit hazard, and a negative effect of work-fare programs on the exit hazard (see Table 10 in the Appendix). Our results are not contrary to the existing literature.

Table 5: The effect of carrots and sticks policies and treatments on unemployment exit (hazard rate; men)

	(1)	(2)	(3)	(4)	(5)	(6)
carrot TE	-0.342***		-0.407***	-0.415***	-0.405***	-0.412***
	(0.00819)		(0.00882)	(0.00887)	(0.00883)	(0.00887)
stick TE		-0.00731	-0.171***	-0.182***	-0.171***	-0.180***
		(0.00795)	(0.00855)	(0.00861)	(0.00856)	(0.00861)
carrot policy CW				0.137***		0.136***
				(0.0282)		(0.0284)
stick policy CW				0.234***		0.216***
				(0.0243)		(0.0243)
carrot policy PES					0.235***	0.158***
					(0.0553)	(0.0574)
stick policy PES					0.321***	0.249***
					(0.0421)	(0.0432)
intercept	-4.718***	-4.750***	-4.724***	-4.917***	-5.257***	-5.359***
	(0.0311)	(0.0311)	(0.0311)	(0.0362)	(0.0423)	(0.0434)
obs. (spells)	131037	131037	131037	131037	131037	131037
log-likelihood	-198722	-199705	-198519	-198455	-198693	-198636
FE at level	PES	PES	PES	PES	MS	MS

Note: TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 .

Column (4) in Table 5 shows policy regime effects at the caseworker level. Results indicate that both, increasing the rate at which job seekers enter supportive and restrictive programs

increases their rate of leaving unemployment. But the quantitative magnitude differs. Supportive policies increase the rate of leaving unemployment about half as strongly as restrictive policies. Column (5) discusses policy regime effects at the PES level. Also for the PES, our results indicate that both regimes that use supportive as well as restrictive policies often tend to produce a higher exit from unemployment. But either the case-worker or the PES results could be spurious. This is why column (6) introduces both case-worker regimes as well as PES regimes. Interestingly, all levels of policy implementation matter. Supportive policies improve the rate of leaving unemployment roughly to the same extent, regardless of whether the PES or the case-worker enacts it. Restrictive policies increase the rate of leaving unemployment by even more than supportive policies, again at about the same magnitude on both levels of policy implementation.

6.2 Explaining the post-unemployment effects: employment vs. earnings

Earlier we have seen that supportive policies improve earnings of participants, and supportive PES policy regimes also increase earnings after leaving unemployment. These effects might arise because job seekers accept worse paid jobs or keep those jobs for a shorter time period.

Table 6 shows treatment and regime effects on employment, the proportion of the time we observe after unemployment spent in employment, and earnings while employed,[20] average earnings in the months a job seeker is considered as employed. Consider first results for employment. Columns (1) to (3) show treatment effects, case-worker, and PES effects, separately. Column (4) shows results on all dimensions. Treatment effects are positive for supportive policies and negative for restrictive policies. Supportive programs therefore enhance job stability whereas restrictive policies can induce job seekers to accept less stable employment. Policy regimes also affect job stability. PES offices that use supportive policies more intensely help their clients find jobs that they keep over a longer period whereas PES that build on restrictive policies tend to reduce job stability of their clients. Case-workers also matter but less than the PES. Case-worker use of supportive policies does not affect job stability, whereas use of restrictive policies reduces job stability but by substantially less than PES use of restrictive policies.

Table 6 also shows results for average earnings of workers. Bear in mind that workers are a sub-set of the population, so these results do not have a causal interpretation. Columns (5)-(7) add treatment and policy regime effects step by step. Column (8) provides results on all dimensions that we consider. Supportive policies increase earnings while employed by about 50

[20] These only concern the individuals observed to be employed after exit out of unemployment and hence exclude the small percentage of those with right-censored unemployment durations.

Table 6: Explaining the post-ue effect: employment propensity (proportion of months employed within observation window) vs. monthly earnings while employed (over 3.5 years; men)

	effect on employment				*effect on earnings while employed*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
carrot TE	0.0347***	0.0350***	0.0353***	0.0355***	37.17*	43.03**	44.13**	48.96**
	(0.00273)	(0.00274)	(0.00273)	(0.00274)	(20.05)	(20.11)	(20.10)	(20.18)
stick TE	-0.0397***	-0.0376***	-0.0361***	-0.0351***	-234.0***	-224.0***	-221.5***	-216.2***
	(0.00271)	(0.00273)	(0.00271)	(0.00273)	(17.53)	(17.59)	(17.59)	(17.75)
carrot policy CW		0.0122		0.00530		-94.67		-117.5*
		(0.00863)		(0.00860)		(60.59)		(60.32)
stick policy CW		-0.0526***		-0.0289***		-215.3***		-101.5*
		(0.00791)		(0.00757)		(60.49)		(52.12)
carrot policy PES			0.0539***	0.0516***			651.3***	716.6***
			(0.0177)	(0.0182)			(127.1)	(131.4)
stick policy PES			-0.131***	-0.120***			-642.0***	-610.6***
			(0.0153)	(0.0152)			(155.3)	(149.9)
intercept	0.718***	0.738***	0.747***	0.754***	3,882***	4,044***	4,072***	4,134***
	(0.0101)	(0.0116)	(0.0140)	(0.0144)	(64.98)	(76.25)	(105.1)	(110.5)
obs. (spells)	131,037	131,037	131,037	131,037	119,033	119,033	119,033	119,033
R^2	0.169	0.170	0.167	0.167	0.431	0.432	0.426	0.426
FE at level	PES	PES	MS	MS	PES	PES	MS	MS

Note: The outcome variable in models (1) to (4) is the proportion of months employed within the post-unemployment observation window (max./usually 42 months); in models (5) to (8) the outcome variable is average monthly earnings when employed within the same period. TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 .

CHF, or about 1-2 percent of average monthly earnings while employed. Job seekers who have been exposed to restrictive policies earn considerably less, 216 CHF per month, or 5-6 percent. Turning to case-worker regimes, both intensive use of restrictive and supportive policies tend to reduce job seeker's earnings while employed. The effects are fairly small and on the margin of statistical significance. What's important for job seekers are the policies adopted by the PES that is taking care of them. PES offices that place a strong emphasis on supportive policies manage to place job seekers in jobs that pay them substantially more when they are employed. PES who use restrictive policies intensely reduce the earnings their clients take home substantially.

Lechner *et al.* (2011) study short- and long-run effects of publicly sponsored training for job seekers in Germany. Initial results all indicate that training pro-longs unemployment. But medium-run and long-run effects of these programs are positive, suggesting that the most intensive forms of training can raise employment rates by up to 10 percentage points which are sustained for up to eight years. These effects are consistent with our findings for supportive programs.

An interesting pattern of results emerges. Supportive policies help participants by placing them into more stable employment (at the cost of pro-longed unemployment). Restrictive policies may (or may not) improve the speed at which job seekers leave unemployment, but damage their post-unemployment prospects via reduced stability of employment and lower earnings while employed. Case-workers matter strongly for exit from unemployment, but they affect post-unemployment job prospects relatively little. PES are both key to how job seekers leave unemployment and they shape post-unemployment job prospects strongly.

6.3 Policy interaction effects between carrots and sticks

We have seen that policy regime effects matter, both for case-workers and especially for PES offices. But so far, we have considered supportive and restrictive policies in isolation. Here we study whether combining policy regimes changes their effectiveness.

Table 7: Policy interactions between carrots and sticks: marginal substitution/compensation effects when deviating from median policy intensity (ue exit and post-ue earnings; men)

	unemployment exit		*post-ue earnings*	
	(1)	(2)	(3)	(4)
carrot TE	-0.412***	-0.410***	164.3***	164.1***
	(0.00887)	(0.00888)	(21.32)	(21.33)
stick TE	-0.180***	-0.180***	-274.3***	-274.3***
	(0.00861)	(0.00861)	(18.25)	(18.24)
carrot policy CW	0.136***	0.141***	-50.74	-54.16
	(0.0284)	(0.0284)	(64.69)	(65.00)
stick policy CW	0.216***	0.225***	-215.3***	-204.4***
	(0.0243)	(0.0245)	(57.45)	(59.55)
carrot policy PES	0.158***	0.200***	873.7***	868.7***
	(0.0574)	(0.0578)	(144.6)	(142.4)
stick policy PES	0.249***	0.243***	-1,053***	-1,050***
	(0.0432)	(0.0433)	(162.3)	(161.8)
policy interaction CW		0.165*		375.7
		(0.0892)		(230.0)
policy interaction PES		0.403***		-454.6*
		(0.101)		(264.5)
intercept	-4.937***	-4.933***	3,341***	3,342***
	(0.0246)	(0.0246)	(60.20)	(60.25)
obs. (spells)	131,037	131,037	131,037	131,037
log-likelihood	-198636	-198615		
R^2			0.377	0.377
FE at level	MS	MS	MS	MS

Note: The policy and interaction variables are defined here as deviations from the respective median policy intensity. TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 .

Table 7 repeats our baseline estimates for policy regime effects on unemployment exit in column (1) and displays results that have interaction terms for policy regimes in column (2). Results in column (2) indicate that both policy interactions are positive and significantly different from zero. The effects are particulary important for PES. These results indicate that having a bit of both, supportive and restrictive, policies improves their effect on unemployment exit.

Columns (3) and (4) in Table 7 show results for earnings after leaving unemployment. Results in column (4) indicate that the interaction term is negative. A PES office using supportive policies and restrictive policies could therefore increase earnings of its clients by reducing the amount of restrictive policies that it uses. Thus, the assessment if mixing supportive and restrictive policies is a good idea in terms of labor market outcomes depends on whether a short-run or rather a longer-run perspective is adopted by the policy makers. More intense policy mixes support faster job take-up but tend to reduce post-unemployment earnings perspectives. The interaction term on case-worker policy regimes is positive but marginally fails to be statistically significant.

Based on these findings, we conclude that regime effects not only matter in isolation but also via their interplay. Furthermore, the results suggest an economic trade-off between the short and the long run: if policy makers focus on the first, they could choose a high intensity in both regimes; but this would not be suitable if they focus on the long-run earnings perspective. To achieve a more comprehensive, quantitative assessment of this trade-off, it is important to consider marginal effects as well as costs and benefits of regime changes. This is done in the next subsection.[21]

6.4 Marginal effects and costs and benefits of regime changes

We use the results of Table 7 to run "policy experiments": we calculate marginal effects to quantify how changes in regime intensities aiming at an increase of θ_u or of earnings actually affect the individual outcomes.[22]

Experiment 1 analyzes the policy maker's (potential) objective to increase the speed of unemployment exit. This is implemented by shifting the carrot and stick policy regime intensities from median level half a standard deviation upwards, respectively. The resulting marginal

[21] Our specification assumes that regime effects apply to treated and non-treated job seekers alike. If the effect is just an ex ante effect for the first treatments then this is maybe too restrictive. However, recall that policy regimes are potentially more general than just ex ante effects. The policy regime does not become irrelevant after the first treatment, and it is an open question whether the intensity of the involvement of CW and PES increases over time or not. We have estimated models that allow for interaction effects. It turns out that we do not find that regime effects become less relevant after the first treatments.

[22] Dehejia (2005) discusses a related but different issue, namely what are the gains from asking the caseworker to decide on ALMP. He finds that a flexible assignment rule may provide large gains compared to more rigid rules.

Table 8: Quantification experiments: adapting policy regime intensity with objective (1) to increase unemployment exit; (2) to improve post-unemployment conditions. Marginal effects

marginal effects	*unemployment exit* (% change hazard)		*post-ue earnings* (% non-treated earn.)	
	case-worker	PES	case-worker	PES
Experiment 1: carrots ↑ and sticks ↑ by 0.5 s.d.				
direct effect	0.0379	0.0454	(-0.0081)	-0.0103
+ interaction effect	0.0212	0.0524	(0.0137)	-0.0165
total	*0.0591*	*0.0978*	*(0.0057)*	*-0.0268*
Experiment 2a: carrots ↑ and sticks ↓ by 0.5 s.d.				
direct effect	-0.0112	-0.0080	(0.0051)	0.0576
+ interaction effect	-0.0038	-0.0097	(-0.0026)	0.0033
total	*-0.0150*	*-0.0178*	*(0.0024)*	*0.0609*
Experiment 2b: carrots ↓ and sticks ↑ by 0.5 s.d.				
direct effect	0.0113	0.0081	(-0.0051)	-0.0576
+ interaction effect	0.0006	0.0020	(0.0004)	-0.0007
total	*0.0119*	*0.0101*	*(-0.0047)*	*-0.0582*

Notes: Marginal effects are computed based on the results in Table 7. They evaluate shifts of carrot and stick policy regimes from median level by 0.5 standard deviations (see figures in Table 3). See Appendix A.2 for a description how marginal effects are calculated. Figures in parentheses indicate marginal effects which are partially based on statistically insignificant policy regime effect results.

effects are reported in Table 8. The experiment yields a remarkable increase of the job seeker's unemployment exit hazard rate, by 10% due to case-worker regime and 5.9% due to PES regime. Both together are in size almost as large as the stick treatment effect on unemployment exit (19.7%, see Table 5). The marginal effect of a shift in regimes would, however, affect all the unemployed job seekers. The downside of shifts of the type of experiment 1 is an additional earnings loss of 2.7% (due to PES policy), as compared to median intensity regimes.

Alternatively, policy makers could rather set their focus on improving the post-unemployment earnings conditions. Experiment 2 tests two options how this could potentially be achieved (given the nature of the interaction effect): shifting the intensities of the carrot regime up and of the stick regime down (a) – or the opposite policy change (b). As the results in Table 8 demonstrate, only the policy adaptation (a) empirically achieves the given objective. The PES policy change to improve support by more intensely using the carrot regime and less intensely using the stick regime results in improved earnings effects by about 6%, as compared to median intensity regimes. The cost is slight increases in unemployment duration (driven by PES and case-worker regimes). Experiment 2b fails to achieve the objective; increasing the already dominant stick regime effect by making this policy even more stringent turns out to be negative for individual

earnings.

Table 9: Costs and benefits (for UI) of changing regimes : changes in payments due to marginal regime effects

	Experiment 1 *(C +0.5sd, S +0.5sd)*	*Experiment 2a* *(C +0.5sd, S -0.5sd)*	*Experiment 2b* *(C -0.5sd, S +0.5sd)*
(A) changes in benefit payments			
...in days of UE per person	-20.2	6.1	-3.3
...in CHF per person	-2116.1	637.9	-344.4
benefit cuts due to sanctions (CHF p.p.)	-95.6	100.70	-95.6
(B) change in cost of treatments			
carrots			
...for training (CHF p.p.)	287.0	287.0	-293.2
...for job search assistance (CHF p.p.)	106.9	106.9	-106.2
sticks			
...for enforced sanctions (CHF p.p.)	16.9	-17.6	16.9
...for workfare programs (CHF p.p.)	212.9	-206.5	212.9
Total change in cost (A+B; CHF p.p.)	-1588.1	908.4	-609.7
...in % of total benefit cost p.p.	-6.2%	3.6%	-2.4%

Notes: See Appendix A.2 for a description of the simulations and computations of the benefit and cost figures above. 1 CHF = 0.92 EUR = 1.02 USD.

In a second step we analyze how the described policy shift options perform in a cost-benefit analysis. This analysis is done from the perspective of the UI: it compares changes in benefit payments due to marginal regime effects on unemployment durations (and benefit cuts because of sanctions) with the incremental cost of adapted treatment intensities. The details on the performed simulations are described in section A.2 of the Appendix.

The results are reported in Table 9. They show that experiment 1 – a joint increase of carrot and stick regime intensities by half a standard deviation (about 10 percentage points of intended intensity) – yields a reduction of the individual unemployment duration by 20 calendar days per person. This corresponds to a saving of a bit more than 2000 CHF per person. Note that the regime effects affect the full sample of unemployed. Relative to this amount, the savings due to additionally imposed sanctions are marginal[23]. The direct cost of additional supportive treatments due to the upward shift of the carrot regime amount to about 400 CHF p.p. The direct cost of strengthening the stick regime results in 240 CHF p.p. Thus, in total the policy makers save almost 1600 CHF or 6.2% of total benefit cost p.p. by marginally tightening both

[23]The average total duration of sanctions imposed on the additionally affected people (due to the strengthened stick regime) is about 14 work days. This amounts to 0.36 work days of additional benefit cut per person of the full sample.

regimes. This comes, however, at the cost of reduced individual earnings perspectives (see above).

The monetary trade-off is visible as well in experiments 2a and 2b. Additional support is costly in terms of additional benefit payments and training/job search assistance costs – but it supports individual post-unemployment earnings. The opposite is true for a tightening of the restrictive regime. Thus, the cost-benefit analysis of marginal regime effects clearly shows that regime effects financially matter.

Knowledge about the quantitative importance of regime effects allows the policy makers to effectively adapt their policy strategies according to their preferred objective. The experiments show that feasible changes in regime intensities result in non-negligible changes of the effects of policy mixes on labor market outcomes (which reflect accordingly in the UI cost).

7 Conclusions

Policy regime effects are quantitatively important. Both caseworkers and PES agencies can strongly increase the exit rate from unemployment by using supportive or restrictive policies more intensively. Quantitatively, restrictive programs are more important than supportive programs. PES agencies are also very important for earnings after leaving unemployment. Supportive policies increase earnings after job seekers leave unemployment, restrictive policies reduce them. Interestingly, caseworkers are much less important for post-unemployment outcomes than for the duration of the unemployment spell.

We also find treatment effects to be important. Both supportive and restrictive programs tend to prolong unemployment, restrictive programs do so because of the work-fare component. Supportive programs then increase earnings after unemployment substantially, whereas restrictive programs decrease what job seekers take home.

What do our results imply for labor market policy? First, we document that policy regimes matter. Compared to the actual treatment effects, policy regime effects are small, but they affect a much larger group of job seekers, so they produce sizeable aggregate effects. Evaluations of ALMP that only take treatment effects into account miss a substantial part of the effects triggered by ALMPs. From a methodological point of view, our findings imply that omission of regime effects in studies of treatment effects may lead to violation of unconfoundedness assumptions. Second, supportive policies dominate restrictive policies in our setting. Both supportive and restrictive policy regimes shorten unemployment duration but supportive policy regimes increase earnings whereas restrictive policy regimes decrease them. A similar comparison holds

for actually attending the programs. Both supportive and restrictive programs prolong unemployment, but supportive programs increase pay whereas restrictive programs reduce it.

Third, interactions between policy regimes are present and need to be taken into account. Agencies that focus on maximizing exit from unemployment would find using both restrictive and supportive programs helpful. This strategy saves UI costs, since the reduction in unemployment benefits dominates the additional treatment expenses. However, agencies that place a positive weight on the post-unemployment trajectories of their clients will prefer to specialize in one set of policy rather than combine them.

At least three topics for further research emerge. First, it is interesting to obtain data that reveal (or at least shed light on) the reasons for why caseworkers use different policies. This should facilitate the transmission of the best-case practice to all caseworkers. Secondly, it is useful to consider extensions that allow for systematic unobserved heterogeneity of unemployed workers. This requires even larger data sets and leads to an even higher computational burden. Thirdly, it would be useful to have data that are directly informative on the intended sticks policy regime so that we do not have to use observed punishment rates to infer the intended regime. It is an open question whether survey interviews across caseworkers and PES allow for reliable quantitative measurements of the intended sticks policy.

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A Appendix

A.1 Supplementary table

Table 10: The transition of a single-treatment-effect setup to the "carrots and sticks" setup: Decomposition of treatment effects on unemployment exit (hazard rate; men)

model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
carrot	*yes*				*yes*	*yes*	*yes*	*yes*
stick		*yes*	*decomp.*	*decomp.*	*decomp.*	*decomp.*	*yes*	*yes*
censored				*yes*	*yes*		*yes*	
carrot TE	-0.342***				-0.430***	-0.415***	-0.419***	-0.407***
	(0.00819)				(0.00947)	(0.00883)	(0.00946)	(0.00882)
stick TE		-0.0073					-0.0819***	-0.171***
		(0.00795)					(0.00912)	(0.00855)
sanction TE			0.0590***	0.0430***	0.0224**	-0.102***		
			(0.00841)	(0.00973)	(0.00960)	(0.00892)		
workfare TE			-0.340***	-0.481***	-0.546***	-0.535***		
			(0.0188)	(0.0210)	(0.0207)	(0.0192)		
intercept	-4.718***	-4.750***	-4.759***	-4.578***	-4.683***	-4.734***	-4.673***	-4.724***
	(0.0311)	(0.0311)	(0.0311)	(0.0355)	(0.0324)	(0.0311)	(0.0324)	(0.0311)
obs. (spells)	131037	131037	131037	131037	131037	131037	131037	131037
parameters	296	296	297	297	298	298	297	297
log-likelihood	-198722	-199705	-199479	-168675	-190673	-198251	-191070	-198519
FE at level	PES	PES	PES	PES	PES	PES	PES	PES

Notes: decomp.=decomposition of stick effect into effect of sanction and effect of workfare program; TE=treatment effect; FE=fixed effect; censored= spell is censored at occurrence of first event of the other type (than the reported TE; in model 4, the control group spells are censored at occurrence of a carrot event). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 .

A.2 Marginal regime effects and simulations for cost-benefit analysis

A.2.1 Marginal regime effects on exit hazards and post-unemployment earnings

In the case of the unemployment exit outcome we calculate the marginal effect – expressed as a percentage change in the exit hazard – as follows: Define $\tilde{F}_n = med(F_n)$ as the median intensity of policy regime $n \in \{c, s, cs\}$ (c=carrot, s=stick, cs=interaction of both); whereby $\tilde{F}_{cs} = \tilde{F}_c\tilde{F}_s$. Also define $\tilde{F}'_n = med(F_n) + 0.5sd(F_n)$, i.e. the policy intensity shifted by 0.5 standard deviation (+ or −, depending on the experiment); whereby $\tilde{F}'_{cs} = \tilde{F}'_c\tilde{F}'_s$. The direct effect is $\frac{\theta(x,\tilde{F}'_c,\tilde{F}'_s,\tilde{F}_{cs})-\theta(x,\tilde{F}_c,\tilde{F}_s,\tilde{F}_{cs})}{\theta(x,\tilde{F}_c,\tilde{F}_s,\tilde{F}_{cs})}$; this reduces to $exp[\pi_c 0.5sd(F_c) + \pi_s 0.5sd(F_s)] - 1$. The additional interaction effect amounts to $\frac{\theta(x,\tilde{F}'_c,\tilde{F}'_s,\tilde{F}'_{cs})-\theta(x,\tilde{F}'_c,\tilde{F}'_s,\tilde{F}_{cs})}{\theta(x,\tilde{F}_c,\tilde{F}_s,\tilde{F}_{cs})}$. Note that – for simplicity of exposition – we ignore here the fact that every type of policy regime appears twice, once for case-workers

and once for PES. Thus, we can compute the presented effects separately for case-workers and for PES.

In the case of post-unemployment earnings the total marginal effect around median policies is $\pi_c sd(F_c) + \pi_s sd(F_s) + \pi_{cs}[\tilde{F}_c sd(F_s) + \tilde{F}_s sd(F_c) + sd(F_c)sd(F_s)]$, with obvious decomposition in direct and interaction effect. This absolute marginal earnings effect is finally standardized by expressing it as a percentage change in non-treated average earnings (3342 CHF). The results of these computations are reported in Table 8 in the main text.

A.2.2 Marginal regime effects on unemployment durations and treatment cost

Additional cost and benefits from marginal regime effects arise (primarily) from the resulting changes in unemployment durations, on one hand, and changes in the amount of assigned treatments, on the other hand. Thus, to perform a cost-benefit analysis we need to quantify these changes in a tractable unit and multiply them with corresponding cost/benefit rates per unit. To achieve this, we proceed in several steps.

(1) Predict the effects of a marginal regime change on unemployment durations. A shift of the policy regimes affects everyone in the sample. Therefore, we first simulate the marginal effect of changing the regimes by 0.5 s.d. on expected unemployment duration for the full sample. Using the estimation results of the interacted model for $\theta_u(t)$ (see Table 7, column (2)), we compute the following individual-level predictions:

$$E[T_u|x, \tilde{F}'_c, \tilde{F}'_s, \tilde{F}'_{cs}] - E[T_u|x, \tilde{F}_c, \tilde{F}_s, \tilde{F}_{cs}]$$

following the (simplified) notation defined in the section above. I.e., we calculate the predicted individual unemployment durations with the policy regime variables set once to median $+/-$ $0.5 s.d.$ and once to median level (leaving the treatment effects and other x variables unchanged). The difference between the averages of the individual predicted unemployment duration under changed and under median regime represent the marginal regime effects (of the corresponding policy experiment). Since this effect applies to everyone, it is evaluated for the full sample.

(2) More (or less) individuals get carrots/sticks due to the marginal regime change: use F_c/F_s rank to determine these additionally (non-)affected people. Due to the nature of the policy regime definition, as derived in section 4.2, the ranking by F_c and F_s directly allows us to determine in the data who is additionally (not) affected (any more) by

the marginal regime shift. Since F_c and F_s represent probabilities to be affected by a certain type of treatment event, it suffices to sort the population according to these intensity measures to determine the group within 0.5 s.d. above (below) the median intensity as the additionally (non-)affected individuals. These groups are determined for each of the four regimes ("carrot" or "stick", by CW or PES).

(3) Predict the treatment durations/incidences which are implied by applying a carrot/stick regime: for additionally (non-)affected individuals. Our UIR-SSR database contains detailed individual-level information on realized treatment durations or incidences for the observed treated individuals. In particular, we observe and distinguish four treatment types per individual: (1) total duration of training, (2) total duration of job search assistance program participation, (3) total duration of workfare program participation, (4) total number of enforced benefit sanctions and the related number of days of cut benefits. We use these data to predict the duration/incidence of treatment of additionally (non-)affected individuals as follows. First note that – due to the fact that by far not everyone is observed treated (see Table 3) – we have to deal with large proportions of zeros in the treatment durations/incidences. Therefore, we first regress an indicator of zero treatment outcome on all the control variables (as used in the main estimations) for the population affected by the respective median intensity regime. Then, we predict the probability of a zero treatment outcome for the additionally (non-)affected individuals and rank/sort them according to this probability. Assuming that the proportion of zeros is the same as in the median intensity population, we determine according to this ranking who is supposed to get a non-zero treatment exposure. For those, we predict the outcome duration/incidence of the corresponding treatment type, based on a regression[24] of the treatment outcome on all controls for the non-zero outcomes in the respective median intensity population. Note that these steps are performed for each of the four types of treatments and for each policy regime ("carrot" or "stick", by CW or PES).

(4) Cost data per treatment type and year: sources and calculation of per-unit figures. The Swiss State Secretariat of Economic Affairs (SECO) provided us specific cost data per type of treatment and year (from the process data monitoring of the UI). We combine this information with individual-level counts of treatment durations/incidences from the UIR-SSR database to calculate the average treatment cost per unit (for ALMP: per day; for sanctions:

[24] We use OLS regression here. We tested, as an alternative, to apply poisson regression. The "goodness of fit" (as measured by the absolute distance between the prediction and the realization in the median intensity samples) was, however, not better for the latter.

per incidence) and type and year. Moreover, we collected the figure of the average daily benefits paid to males by year from official statistics[25].

(5) Calculation of total cost: (treatment durations/incidences · per-unit costs), summed over all relevant treatment types. Finally, we multiply the predicted treatment durations/incidences with the corresponding cost figures. For "carrot" regimes we consider training and job search assistance treatment durations, for "stick" regimes incidences of imposed sanctions and workfare durations. To complement the picture, we also compute the saved days of benefits due to sanctions in the same manner. By summing up these components of additional (less) cost across additionally (non-)affected individuals, we obtain the total marginal cost of the corresponding regime change. Since the regime change affects everyone, this cost is divided by the full sample population. We report the final figures by treatment type and as a total per regime. This total change in cost is, moreover, expressed as a proportion of the average total benefit cost (i.e., the predicted unemployment duration under median regimes multiplied by daily benefits) of a job seeker.

All these steps are performed for each case of the three experiments 1, 2a and 2b, which imply positive or negative changes of the "carrot" and/or "stick" intensities by 0.5 standard deviations, respectively. The results are reported in Table 9 in the main text.

[25]Figures for the years 2000 to 2005. Source: Staatssekretariat für Wirtschaft SECO (2007): Arbeitslosigkeit in der Schweiz 2006, Bern, p.31.