Carrots & Sticks – Do Public Employment Service Policy Mixes Matter for Job Seekers' Post-Unemployment Earnings?

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Abstract: Public Employment Service (PES) units often fundamentally shape the treatment of individual job seekers by applying specific strategies (mixes) of labor market policies. Interestingly, not much evidence on this issue can be found. This paper empirically assesses the role of PES policies for the job seekers' earnings in the 3.5 years after unemployment entry. We exploit the substantial variation in (the intensity of) policy use between the PES agencies in Switzerland, relying on a vast register data base covering a fourth of the full unemployment inflow from 2000 to 2005. We estimate, in the first step, the PES-specific intended policies by types ("carrots" and "sticks"). I.e., we propose a method to estimate the (unknown) intended policies using actual treatment realizations. In the second step, we relate these estimated intended policies to the mid-run earnings outcomes of the individuals. We find that the intended PES policies, for "carrots" and for "sticks", both have significant impact on earnings. In particular, the interaction (mix) of the two policy types is of importance. This interaction is negative. This means that an intense sanction regime cannot be compensated by intensifying training. It seems more advisable to keep either training or sanctions low.

JEL Codes: J65, J68

Keywords: Policy interactions, unemployment insurance, ex-ante effects, competing risks, sanctions, training, post-unemployment outcomes

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1 Introduction

Active labor market policy is an important tool to fight unemployment and to improve the matching on labor markets. Several OECD countries spend more than one percent of their GDP on active labor market policy (ALMP). Existing literature has documented the effects of specific policy interventions on participants (see Card, Kluve, Weber 2009 for a survey of that literature). But, interestingly, not much evidence can be found in the literature about the role of Public Employment Service (PES) units as policy makers: PES often follow strategies of preferably applying certain *mixes* of labor market policies.

Most studies in the program evaluation literature ignore the parallel presence and *interactions* of programs and policies. On one hand, different types of programs may be applied within the treatment strategy for an individual – supportive ones (building up human capital) and restrictive ones (sanctioning non-compliant behavior). It is highly unlikely that the effects of these programs do not interact. On the other hand, such programs may, intendedly, exert effects on every employed individual "at risk" of getting into it even before the imposition of the program. These *ex-ante effects* may be important in the case of different policies, as evidence shows for different countries (Lalive, Van Ours and Zweimüller 2005; Black, Smith, Berger and Noel 2003; Arni, Lalive, Van Ours 2009; Van den Berg, Bergemann, Caliendo 2009). Therefore, it is of high relevance for comprehensive policy evaluation to *jointly model* different program types as well as intended policies (ex-ante effects) and ex-post treatment effects. This aim is in the focus of this paper.

This paper discusses the role of such PES policies and their effects on the job seeker's earnings. Specifically, we distinguish between policies that are likely to be perceived positively by participants ("carrots") and policies that are likely to be perceived as negative ("stick"). We define the first group of policies to cover training and job search assistance, and the second group to cover benefit sanctions and workfare programs. We observe how frequently about 150 PES in Switzerland use these policies and discuss how to reconstruct intended policy from actual (observed) policy. To estimate such intended policies, we use the ideas of the competing risks approach, known from duration analysis.

In a second step, we assess the relation between such PES-specific intended policies (as well as the imposed treatments) and realized earnings in the long run, i.e. up to 3.5 years after unemployment. As a source of exogenous variation in the application of intended policies to job seekers we use the fact that the regional organisation of the PES was subject to some change over time. Some PES have been merged or split up, some municipalities have been reassigned to a neighboring PES. We argue that these changes, implemented by the superior administrative level (the Cantons in Switzerland), were driven by motives of organisational efficiency and not by motives related to the economic performance and outcomes of the respective regions. Exploiting this kind of variation, we apply panel data methods to estimate the second step regressions.

Our methodological approach is most related to Feracci, Jolivet and van den Berg (2010).

However, there are several key differences. First, our research question is different: We analyse the *mix* of different types of policies, whereas they focus on one policy (training, in different intensities). So, we are interested in the question how the different relative intensity of applying "carrot"- and "stick"-types of policies may influence the earnings outcome of the job seekers. Which combinations of "carrots" and "sticks" are related to the best earnings outcomes?

So, the second difference to the above-mentioned paper is that we focus on labor earnings rather than the probability of long-term unemployment (as they do). Third, we focus on measuring intended treatment rather than actual treatment (they discuss this issue in some sensitivity analysis). Fourth, our identification strategy is (potentially) stronger as we can rely on (arguably) exogenous variation in the organisation of PES and therefore PES-related policy. Our paper is also related to Rosholm and Svarer (2008) in sharing some similar ideas on the analysis of intended vs. actual treatment. The main differences are (among others) that we focus on earnings rather than unemployment and consider a different research question.

Our analysis complements existing research in several ways. First, we discuss how to measure ALMP policies in a setting where we do not know them (at entry into unemployment) and individuals can leave before being affected. Second, we document the effects of these policies both on participants and non-participants. Third, we document the role of such policies for earnings rather than employment. Fourth, we look at effects on the medium run outcome. Fifth, we consider combinations of policies (rather than only one), so we drop the usual (but often unrealistic) assumption of no direct interaction between different treatments within an unemployment spell.

We find that the intended PES policies (ex-ante effects), for "carrots" and for "sticks", both have significant impact on earnings. In particular, the interaction (mix) of the two considered policy types is of quantitative importance. Interestingly, this interaction is negative. The use of stick policy in high intensity reduces the positive effect of carrot policy on earnings. This result is found for the ex-ante effects as well as for the ex-post treatment effects. This suggests that the *isolated* presence (or application) of only one type of policy is more beneficial in terms of long-run earnings for the concerned individuals.

The remainder of the paper is structured as follows. In section 2 we outline the relevant characteristics of the institutional background of Swiss unemployment insurance and labor market policies. Section 3 discusses the empirical strategy on how we jointly model policies. In Section 4 the data and some descriptive analyses are presented. Section 5 discusses the estimation results. Section 6 concludes and considers some policy implications.

2 Institutional Background

The potential duration of unemployment benefits in Switzerland is 400 days for individuals who meet the contribution and employability requirements. From age 55 on, benefits are extended by additional 120 days. The replacement ratio is 80%; and 70 % for job seekers who earned

more than CHF 4030 prior to unemployment and are not caring for children.[1] Job seekers have to pay all earnings and social insurance taxes except the unemployment insurance tax rate (which stands at about 2 %). This means that the gross replacement rate is similar to the net replacement rate. After this entitlement period the unemployed have to rely on social assistance. Social assistance is means tested and replaces roughly 76% of unemployment benefits for a single job seeker with no other sources of earnings (OECD, 1999).

Job seekers are entitled to unemployment benefits if they meet two requirements. First, they must have paid unemployment insurance 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.[2] 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 use to fill in the details of the jobs to which they have applied. Job seekers are typically required to apply to about 10 jobs per month. Participation in a labor market program is monitored by the caseworker 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, too (see Lalive, Van Ours and Zweimüller 2005 and Arni, Lalive and Van Ours 2009 for more details about the procedures of the sanction system).

There is *remarkable discretion* in terms of how labor market programs and sanctions are used across PES. Even though the legal rules are the same all over the country, the authorities on the level of the cantons and in particular the caseworkers have considerable leeway in the strength of applying the rules. With respect to sanctions, caseworkers may adjust, to some degree, the

[1] 1 CHF = 0.81 Euro

[2] 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.

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. See Behncke, Frölich and Lechner (2010) for a detailed discussion of caseworker discretion; they document it by a broad caseworker and PES manager survey conducted in December 2004.

The Swiss labor market policy distinguishes mainly four types of labor market policy: (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 focus on 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 as being, in first order, sticks is that they are broadly disliked by the job seekers. Thus, they try to avoid them – for reasons of stigmatisation and fear to be "locked in" into these programs over the longer run – by not proposing them to caseworkers. The above-mentioned survey by Behncke et al (2010) 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 done, first, since subsidized temporary employment is largely searched and proposed by the job seekers themselves, caseworkers do not have much discretional choice in these respects. It is thus, secondly, hard to use this type of program in a strategic way, in the sense of a "carrot" or "stick" policy.

In section 4 we will report the size of discretion, in terms of policy intensities, for "carrots" and "sticks".

3 Empirical Strategy

This section discusses the conceptual framework on how we model and identify the joint assessment of ex-ante and ex-post effects of "carrots" and "sticks".

3.1 Estimation

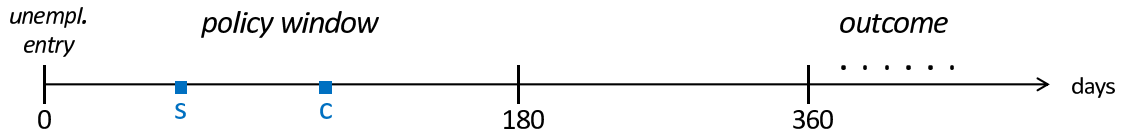
Each PES agency is characterized by its intended policy (ex-ante effect) with respect to two treatments. Treatments that are perceived as helpful and that job seekers want to attend are indexed by c ("carrots"), and treatments that are perceived as negative are indexed by s ("sanctions"). Intended policies are not observed; they describe the initial intention of the PES to apply a certain type of program in a certain intensity. Technically, intended policies can be described as hazard rates of entering the treatments which may differ by PES unit p as follows

$$\begin{aligned}\theta_c(\tau|p,x) &= \lambda_{c,p}(\tau)\psi_c(x)\\ \theta_s(\tau|p,x) &= \lambda_{s,p}(\tau)\psi_s(x)\end{aligned}$$

where τ is elapsed duration of unemployment, $\lambda_{j,p}$ is a PES agency specific baseline hazard, and $\psi_j()$ is a function of observed characteristics x that is common to all agencies, and $j \in \{s, c\}$.

Note that we do not allow for unobserved characteristics since we have access to full pre-unemployment earnings information. Moreover, this simple specification assumes that intended treatments do not vary with calendar time within PES.

We consider the following setup of the timing of events:



We analyze policies within the first 6 months of the unemployment spell (this can be varied in later stages of the project). Let t_j be the realized start of treatment j (set to 180 days for those who do not receive treatment within 6 months). Then $D_j = I(t_j \leq 180d)$ identifies those treated with treatment j. The hazard of leaving unemployment depends on actual treatment and intended treatment intensity

$$\theta_u(\tau|p,x,D_c,D_s,g(\theta_c,\theta_s)) = \lambda_{u,p}(\tau)\psi_c(x,D_c,D_s,g(\theta_c,\theta_s))$$

Note that $g()$ is a function that specifies how intended realizations of treatment intensity affect the current rate of leaving unemployment.

A policy definition (by PES) based on actual treatments, i.e. of the form $\tilde{P}_{j,p} = \bar{D}_{j,p}$, can be misleading. It is endogenously selective: the observation of actual treatment depends on the (endogenous) fact whether the individual is still unemployed at the time of treatment imposition. I.e., individuals may leave unemployment before the intended policy can be applied, or the individuals (potentially) anticipate possible treatment rates. Therefore we need to rely on intended policies.

How can we back out intended treatment intensity? The *competing risks approach* is a useful tool to do so. It takes into account the dynamic adaptation of risk sets (respective durations get censored by competing events). First, focus on the first of three events in a job seeker's unemployment spell. Either she exits from unemployment, or enters a treatment of type j. Let $\tau_j = min(t_u, t_j, 180)$ be the time in unemployment without treatment. The distribution of τ_j is then characterized by

$$S(\tau|p,x) = exp(-\int_0^\tau (\theta_c(z|p,x) + \theta_s(z|p,x) + \theta_u(z|p,x,g(\theta_c,\theta_s))dz))$$

Note this distribution no longer depends on actual treatment (if no anticipation holds). But the distribution of realized time to exit or treatment still depends on intended treatment.

The intended treatment functions $\theta_c(\tau_c)$ and $\theta_s(\tau_s)$ – they describe instantaneous ex ante likelihoods of being treated after τ_j time periods without leaving unemployment – can be estimated using the vast collection of control variables available:

$$\theta_j(\tau_j(t_u, t_j)|x, y^-, c_t) \tag{1}$$

whereby x represents a vector of control variables that features detailed information on socio-demographics and benefit-related characteristics. Moreover, we control for the past employment and earnings history, represented by vector y^-, and we account for inflow time effects (business cycle) by adding a vector of half-yearly dummy variables c_t.

As a first strike, we estimate θ_c and θ_s by use of a simple exponential duration model (which implies constant hazard rate over time). This will be replaced by a more flexible form in the later stage of this work. We assume independent risks conditional on the large set of x, y^-, c_t.

Our *1st stage estimation of intended policies* proceeds as follows: First, we estimate and predict the hazards θ_j using individual spells, by policy type j and PES p. Then, we average the predicted hazards by PES and policy: $\hat{P}_{j,p} = \bar{\hat{\theta}}_{j,p}$. These estimated policies could also be seen as a hazard-based analog to the propensity score.

In the *2nd stage* of our estimation procedure, we estimate (as a baseline specification) the following earnings equation:

$$\begin{aligned} y_i &= x_i'\beta + y_i^{-\prime}\phi + \pi^c \hat{P}_{c,p}(\tau_c) + \pi^s \hat{P}_{s,p}(\tau_s) \\ &\quad + \delta^c D_i^c + \delta^s D_i^s + \sum_t \gamma c_t + \varepsilon_i \end{aligned} \tag{2}$$

whereby the dependent variable y_i features average monthly earnings over a post-unemployment period up to 42 months after unemployment entry (end of observation window). In the results section 5 we will further detail and discuss the specific design of the earnings variable and the corresponding sampling.

This specification is extended in several ways: First, we allow for *interactions* of intended policies $\hat{P}_{j,p}$ and of individual treatments D_i^j. They are crucial to capture the effect of policy mixes. Second, we assess and allow for non-linearities in policies: We will estimate a specification that features polynomials and – in particular – a specification that allows for different discrete categories of policy levels, i.e. dummies for 'low', 'mid' and 'high' policy intensities of policies j. Note that these two extensions represent possible specifications of the above mentioned $g(\cdot)$ function of intended policies. Third, we will estimate models with fixed effects in order to take into account unobservable characteristics that vary locally and by year: One specification features municipality fixed effects, a second specification features municipality $\cdot$ year fixed effects.

The next subsection will discuss these more in detail. Finally, note that we have not yet adjusted the standard errors of the second stage estimations to the use of estimated 1st-stage-$\hat{P}$s; this will be done in a later version of this paper.

3.2 Identification

Our empirical strategy relies on three core identifying assumptions. In analogy to the conceptual discussion in Ferracci, Jolivet and Van den Berg (2010), we outline and discuss in the following the three assumption.

The first is the SUTVA (Stable Unit Treatment Value Assumption). The classical version of the SUTVA assumes that the individuals and their treatment effects are independent of their peers and surrounding areas. In contrast to that, we explicitly allow for treatment externalities across individuals *within* a PES region p by means of adding $P_{j,p}$, i.e. $Y_i = Y_i(D_i^c, D_i^s, P_{c,p}, P_{s,p})$. But we assume no policy spillovers: Potential outcomes are independent of D_i^j and $P_{j,p}$ in other PES regions.

The second core assumption is the conditional independence assumption (CIA) on the *individual* level. This means that $Y(d^c, d^s, P_{c,p}, P_{s,p}) \perp\!\!\!\perp D^c, D^s | x, y^-, c_t, p_{c,p}, p_{s,p}$[3]. Thus, this CIA must hold *within* each PES region p. We need this CIA to hold in both, the 1st stage and the 2nd stage estimations. If this is the case, we can rule out that treatment assignment is endogenous.

In order to complete the discussion on individual-level identification we would like to add a caveat at this point with respect to the interpretation of the 1st-stage predicted hazards, i.e. the backed-out intended policies: It is important to note that the applied competing risks approach does not capture changes of compliance behavior which are *not* correlated to the durations that constitute τ_j. So, e.g. changes in job search behavior as a reaction on anticipation of expected future policy interventions that do not alter t_j or t_u are not taken into account by the competing risks approach.

The third important identifying assumption is the CIA on the level of the *PES regions*. It states, in our case, that $E_p[Y(d^c, d^s, p_{c,p}, p_{s,p})] \perp\!\!\!\perp P_{c,p}, P_{s,p} | x, y^-, c_t, d^c, d^s$. This means that the expected value of the potential outcomes is independent of the set of possible policies, *given* a large set of x variables, past employment and earnings history, calendar time dummies and the control for individual treatments. This CIA assumption needs to be fulfilled in order to properly identify the effects of (intended) policies on earnings outcomes. So, this assumption is supposed to rule out issues of endogenous assignment of policies.

However, one may argue that, in spite of the vast amount of control variables used, the CIA on the level of PES policies may be violated by some processes of *endogeneity of policy choice*. To be specific, assume that there is an unobserved v that is correlated with policy P_j ($j \in \{s, c\}$) and individual's earnings y. Which could be the potential sources of v? In particular, there are

[3]Note that in the CIA representations we distinguish between upper case letters for random variables and lower case letters for realized variables. This distinction is not applied in the rest of the text.

two types of sources of potential policy endogeneity:

1. Political preferences, leisure preferences, culture, attitudes in PES region p which affect as well y
2. Local labor market conditions in PES region p
   (a) static structure like industry composition, UE level etc
   (b) local dynamics in UE rate, or shocks like plant closures
   (c) past labor market conditions

It is reasonable to assume that the sources of type (1) are static in the shorter run of 3 to 4 years. The same holds for the first sub-category of type (2). These sources of v can, thus, be taken into account by a *fixed effects* strategy. The most precise fixed effects that we can apply, based on the data available, are FE on the *municipality* level. Their advantage, compared to municipality FE, is that may take into account differences of preferences or of the structure of the local economy *within* a certain PES region. The other two sub-categories of (2), however, are dynamic over time. We therefore apply *fixed effects that interact municipality · year*.

What remains in terms of variation when controlling for this type of FE? The remaining variation is generated by organisational changes in the assignments of municipalities to PES regions, on one hand. The sources are reorganisations of the PES system over time (mergers, splits of PES regions; re-assignments of certain municipalities to other PES). On the other hand, we keep the variation that is generated within big cities that are split into several PES regions. Note that people cannot freely choose the PES in this case, they are assigned by location of their residence within the city. We argue that these two kinds of remaining variation are exogenous with respect to the outcome variables, *given* all the x variables, the past employment history and the micro-regional fixed effects, *and* the fact of having modeled (taken into account) the important types of treatment- and policy effects jointly. Taken all this together, we are confident that we isolated identifying variation which is exogenous to a very high degree. We will further analyse this issue by a series of robustness checks.

4 Data and Descriptive Analyses

We use a very rich base of register data from Switzerland. Switzerland is an especially interesting and fruitful case for analysing the role of PES policies: The PES enjoy a large leeway to forge their specific strategy in implementing the different types of policy ("carrots" and "sticks"). Moreover, the Cantons (the next higher administrative level of the unemployment insurance organisation) do have a big freedom in questions of organisation and implementation as well. See section 2 for details.

As a basic sample, we consider a fourth of the complete inflow into registered (full-time) unemployment in Switzerland between 1/1/2000 and 30/6/2005, up to age 61.5. The unemployment insurance database (AVAM/ASAL) provides a vast amount of socio-demographic and benefit-rights-related information. To this base we merged a two further UI database that cover the (daily) history of all active labor market policy events, on one hand, and sanctions (including warnings), on the other hand. Finally, to construct the outcome and the past employment history (as an important set of controls), we added social security data (AHV; monthly precision) which covers (non-)employment and earnings in the six years before and up to 42 months after unemployment entry.

In total, we consider 168 PES across Switzerland. This covers more than 90% of the available PES regions and more than 95% of the unemployed population. We excluded PES entities which counted less than 100 observation within the mentioned inflow period – in order to avoid degrees-of-freedom problems in the PES-specific estimation steps (see last section).

As one of the dependent variables we use the sum of the employment earnings that were generated over the months 13 to 42 (=30 months) after unemployment entry (in CHF, deflated). As a second concept for the dependent variable, we sum up the monthly employment earnings within the period between exit from the first unemployment spell and the end of the observation window in month 42. We divide this amount by the number of months of this duration, in order to obtain average monthly employment earnings for the post-unemployment period. Following the first concept for the dependent variable, the data report a mean/median/standard deviation of 103'278/98'063/84'993 CHF for the described sample and a duration of 30 months.

At the core of our empirical strategy are the the frequencies of use of the "carrot" and "stick" (ex-ante) policies. In order to get an impression of the variation we exploit in our estimation strategy, we report in the following the *observed* policy intensities as descriptive statistics. Table 1 shows the remarkable variation in observed policies. They are measured as the frequency of imposition of the respective policy type within the first 6 months of unemployment: The policy intensities for "carrot" types of programs vary between 0.07 and 0.37. On average we observe that every fifth individual (0.21) is subject to a training or job search assistance program. Every sixth person (0.16) is sanctioned or has to join a workfare program within the first 6 months of unemployment. The "stick" intensity varies between 0.02 and 0.37 across the PES.

Figure 1 reports the broad variation of *policy mixes*, i.e. combinations of "carrots" and "sticks" policy intensities, within Switzerland. The implemented policy mixes broadly cover the two-dimensional policy space in the ranges between 0 and 0.4, respectively. This suggests that there is enough two-dimensional support for exploitation of the policy mix variety as identifying variation in our estimation strategy. This avoids extrapolation into policy space areas that are not covered by real data points. A non-parametric analysis of the correlation of "carrot" and "stick" policy intensities – by use of the locally weighted regression smoother – demonstrates that there is no systematic covariation between the two policy types.

Finally, we check whether the described policy variation is driven by compositional differences in the unemployed population across PES. In order to do so, we regress the observed policy intensities on all the available control variables (using a simple OLS by PES). Next, we set the X vector to zero and predict the policy. This yields the policy intensity *conditional* on the X vector. Note that we chose the baseline categories of the control variables such that the baseline represents the most often observed category (in the case of discrete variables) or the mean (in the case of continuous variables). This results in Figure 2. It shows that the remaining policy variation is still a quite evenly distributed cloud of points, scattered over the ranges of 0 and 0.5. There is still no pattern or correlation visible. So, as well conditional on X, we find a broadly covered two-dimensional support of policy mixes to be exploited as identifying variation.

5 Estimation Results

5.1 1st Stage Estimation: Intended Policies

Following the empirical strategy, as described in section 3, we first estimate the intended (ex-ante) policies. This amounts to estimating and predicting intended "carrot" and "stick" policy intensities for each of the 168 PES and the two policy types separately.

Table 2 reports the means, medians and standard deviations of the estimated intensities of the intended policies. The average intended frequency of use of "carrots" across PES amounts to 0.48, with a standard deviation of 0.30. So, at the beginning of the unemployment spell, the average PES policy stipulates for roughly every second job seeker a training or job search assistance program within the first 6 months of unemployment. At the end, every fifth is finally assigned to such a program within 6 months, as the lower panel of Table 2 shows (which is reproduced here for reasons of convenience). The comparison of these two figures reveals the importance of competing risks: an large part of the initial population of the unemployed does not reach the stage where it would have been assigned to a program – as it leaves unemployment before. This shows the importance of taking these dynamic procedures into account.

A similar pattern is visible for the case of the "stick" policies in Table 2. In more than every third case, 0.37 (s.d. 0.24), the PES-level average intended policy consists in assigning a sanction or a workfare program to the unemployed. Note that the intended policy to assign a sanction could as well be interpreted as a measure for the intention to implement a certain monitoring intensity. In the case of "sticks" too, the intended policy intensity is, as expected, higher than the observed one (which was roughly of the size of one sixth). The first two scatter plots of intended vs. observed policies in Figure 3 prove graphically that the intended policy is more intense in every PES: all the scatter points are above the 45-degree-line. The last scatter plot overlaps the observed and intended two-dimension policy mixes and visualizes the quantitative difference between the two.

Figure 4, finally, shows the two-dimensional distribution of intended policy mixes. In the

case of intended policies too, the cloud of scatter points is well distributed over the "carrot"- and "stick" intensity support. Again, there is no common pattern or correlation visible. The intended policy mixes cover a broad part of the two-dimensional policy space. This is good news as it avoids extrapolation into policy space areas that are not covered by real data points. The same conclusion holds when conditioning on the X vector.

5.2 2nd Stage Estimation: Policy Effects on Earnings

In the second stage of our empirical strategy we estimate the effects of ex-post treatments and ex-ante intended policies on monthly earnings beyond unemployment. We start the analysis with a basic OLS model. It features the average monthly earnings in the months 13 to 42 after unemployment entry as a first version of the dependent variable. The sample is restricted to unemployed individuals who exit unemployment within the first 12 months after inflow. This avoids an overlap of the unemployment histories and the outcome variable. We include a very broad set of control variables which cover socio-demographic characteristics, benefit-related information, as well as controls for the earnings- and employment history in the 5 years before unemployment and calendar time dummies for the control of inflow time (business cycles), in half-year-steps. More details can be found in sections 3 and 4. The variables of interest are, on one hand, the ex-post treatment effects of being confronted with a "carrot" (training, job search assistance program) or a "stick" (sanction, workfare program) within the first 6 months of unemployment, represented by δ^c and δ^s. On the other hand, we include as well the intended policy intensities as estimated in the first stage[4], π^c and π^s.

Table 3 reports the estimation results of this basic model for the treatment and policy effects. Column (1) features a specification that assumes no interactions, i.e. *fully isolated* treatment and policy effects. All the effects are highly significant (with exception of π^c which is modestly significant). The ex-post treatment effects of the "carrot" and "stick" programs, δ^c and δ^s, show the expected signs: Getting a training or job search assistance program amounts to a positive effect on average monthly earnings in the longer run of about 134 CHF; this is almost 5% of the average monthly employment earnings. Getting a penalty in form of a sanction or a workfare program results in an important negative monthly effect of -432 CHF. Not surprisingly, the ex-ante effects for "carrots" and "sticks" are weaker – as they only reflect expected utility or earnings losses: 5.2 CHF and -30.9 CHF for a change in the policy intensity by 0.1. However, the negative ex-ante "stick" effect is of size of about 1% of the monthly earnings – an amount which is clearly of importance.

In the next step, we allow for *interactions* between "carrots" and "sticks" treatments and policies; column (2) of Table 3 reports the results. Whereas the treatment interaction is insignificant, the policy interaction clearly is not. The estimated parameters reveal that in fact

[4]Note that, for the estimation of the standard errors, we did not yet take into account that the intended policy variables are estimated. We will adjust that in a later stage by applying a joint estimation procedure.

the *interaction of the intended policies is of high importance (quantitatively) and clearly negative*. This means that the positive effect of "carrot" programs on earnings becomes *weaker* the higher the "stick" intensity is that comes along as parallel policy. At high levels of sanctioning, the originally positive effects of "carrots" turns into negative.

Do these results hold as well when we drop the sample restriction that excludes the longterm unemployed (beyond 12 months)? This is analysed in colums (3) and (4) of Table 3. The dependent variable consists now of averag monthly earnings over the months within the *post-unemployment* period. This period is defined as the duration from the exit from the first unemployment spell until the end of the observation window, i.e. 42 months after unemployment entry. We see that, from a qualitative point of view, there are only two changes: First, the treatment effect of the "carrots" turns into negative. Second, looking at column (4), we see that the interaction of the treatments is positive now. These changes may reflect the fact that for longterm unemployed supportive programs may show a negative effect if people remain in these programs for a long duration ("lock-in"). However, if they are pushed to pass the programs efficiently, by the application of "sticks", the negative effect is alleviated. It is important to note that with respect to the *ex-ante policies*, the substantial *negative interaction remains*, on a very comparable level to before.

What happens when introducing the *fixed effects*, as discussed in section 3? Table 4 reports the results. The individual treatment effects (δ) remain of the same size as before. The sign of the "carrot" effect, however, switches back to being positive – independent of the specification of the outcome variable. The most preferred specification, column (4), reveals a positive "carrot" effect of 91 CHF per month (3% of monthly earnings) and a negative "stick" effect of -308 CHF (10%) per month – if we assume no interactions. Once we drop that assumption we find a significant and substantial negative interaction of the treatment effects. The fact of being sanctioned as well reduces the positive effect of the training.

Looking at the effects of the ex-ante policies – which affect the *whole* (!) unemployed population, not only the treated –, we observe positive effects of increasing the "carrot" or "stick" intensities in a world where these two policy channels are supposed to be independent. The effect sizes are important: 99 (3%) and 74 CHF (2.5%) per month, respectively. Even more substantial is the *negative interaction effect* between the two types of policies. It amounts to -132 CHF per month. The graphical illustration in Figure 5 helps interpreting the *total (ex-ante) effect*, combining the isolated effects plus the interaction. The figure reveals that at a level of about 0.55 of intensity of the intended "carrot" policy, the total effect of the "stick" turns into negative. This is slightly above the average level of intended "carrot" policy across PES. Thus, PES that apply "carrots" in an above-average manner eliminate the initially positive policy effect of "sticks". Taking up the "stick" perspective (second panel within Figure 5), we see that only at very high "stick" intensities the positive effect of increasing the "carrot" policy intensity gets eliminated.

Another way of aggregating these insights is the analysis of *marginal effects* in the two dimensions of (intended) policies. This is done in Table 5. This analysis applies the fixed effects specification of column (4) in Table 4, but splits up the two types of (ex-ante) policies in three discrete categories: 'low', 'mid' and 'high'. The category thresholds are chosen such that each of the three policy levels encompasses about one third of the PES, for "carrots" and "sticks" respectively. The category dummies are specified such that the low level of both policy types are the baseline categories. So, the other eight cells contain the marginal effects of moving from low policy levels to mid or high levels in one or both dimensions. From this point of view, we can conclude that *moving to a policy mix which features low "stick" levels and medium "carrot" levels is most beneficial in terms of earnings effects.* It is interesting to observe that high activity in both policy dimensions is worse as compared to doing (almost) nothing.

Finally, in order to get a further idea on the quantitative relevance of the ex-ante policy effects, we present two possible policy experiments (based on the same reference specification (4) as above):

- Policy experiment 1 could be entitled "cut budget": Assume a PES policy that aims at reducing efforts in using programs in both dimensions: Reducing the "stick" rate from 0.32 to 0.20 (median + 1/2 s.d.) and, in parallel, reducing as well the "carrot" rate from 0.40 to 0.25 would result in -135 CHF of monthly earnings (-4.5%).

- Policy experiment 2 could be named as "being supportive": Assume a PES policy that aims at reducing the "stick" rate from 0.32 to 0.20 (median - 1/2 s.d.) and, in parallel, increasing the "carrot" rate from 0.40 to 0.55. This would result in +83 CHF (+2.8%) of monthly earnings.

. It may be instructive to compare these effect sizes to the impacts of socio-demographic variables:

- Speaking one foreign language, instead of zero: +125 CHF

- Education: disposing of a higher professional education, instead of an apprenticeship: +510 CHF

- Being aged in the 50ies, instead of the 30ies: -260 CHF

This demonstrates that the change of a PES policy strategy is of importance for the unemployed individuals. Applying the strategy "being supportive" is almost as important for the earnings in the longer run as speaking a foreign language. This effect of an intended policy is remarkable – even more, given the fact that these ex-ante policies apply to the *full* unemployed population, independent of being treated or not.

6 Conclusion

This paper considers the question of how policy *mixes*, implemented by Public Employment Service (PES) agencies in unemployment insurance, affect the earnings outcomes of previously unemployed people in the longer run (up to 3.5 years after unemployment entry). We thus drop here the assumption commonly used in program evaluation that interactions of different types policies, which are present at the same time, are not of importance. We *jointly* model the presence of supportive and restrictive policy strategies. The former, "carrots", consist of training and job search assistance programs – the latter, "sticks", feature sanctions or workfare programs (which are highly disliked by a majority of the unemployed). Moreover, we jointly consider the existence of intended policies (ex-ante effects) and ex-post treatment effects.

We use a comprehensive collection of register data from Switzerland which covers one fourth of the inflow population into unemployment between 2000 and mid 2005. The availability of a vast collection of control variables on socio-demographics, benefit-related information, 5 years of past employment and earnings history, daily labor market policy events and time controls provides a credible base for the use of conditional independence assumptions (on individual and PES level) for identification. Moreover, we apply a refined fixed effects strategy that uses municipality · year fixed effects to account for the influence of time-dynamic, locally varying unobservables. We implement a two-stage estimation procedure: In the first stage, we back out intended policy intensities of use of "carrots" and "sticks" by PES region, relying on a competing risks framework. In the second stage, we introduce these intended policies into a model that allows the estimation of policy and treatment effects on monthly employment earnings of individuals in the longer run.

With respect to ex-post treatment effects we find that "sticks" (sanctions) have a substantial negative effect, "carrots" (training) a positive effect on earnings up to 3.5 years after unemployment entry. This confirms existing evidence from several countries. However, in addition we allow for the *interaction* of these treatment effects and find that being confronted with both, "sticks" and "carrots", does *negatively* affect the treatment effects. This interaction effect almost undoes the positive impact of the "carrot" program.

Considering intended (ex-ante) policies we observe that presence of each of the two policy types *in isolation* would be beneficial: The increase of the "stick" or the "carrot" intensities in a world without interactions would both result in positive effects on earnings. The fact that *policy mixes matter*, however, affects the earnings outcome in a negative way. The *negative policy interactions* imply the following: The presence of a "stick" policy reduces the positive ex-ante effect of the "carrot" policy, and vice versa. We see, thus, a trade-off when jointly considering the policy effects: The positive ex-ante effect of the "carrot" ("stick") policy turns into negative if the "stick" ("carrot") regime intensity is too high. Applying a model that that calculates the marginal effects of moving between discrete policy levels (high/mid/low), we find that it is most favorable for the individual employment earnings if the PES agency implements a policy mix

which features middle intensity of "carrot" use and low intensity of "stick" use.

What can we learn for policy design? First, the *effect of policy mixes is of quantitative importance* for the individual's employment earnings after unemployment exit. This is true for the ex-ante (intended policy) effect as well as for the ex-post treatment effect. Thus, ignoring the interactions of supportive and restrictive policy elements provides a biased picture of the total impact of a policy strategy. Second, the fact that policy (and treatment) interactions are negative shows that the *compensation hypothesis cannot be confirmed*: The negative long-run effect of sanctions, as found e.g. by Arni, Lalive and Van Ours (2009), cannot be compensated by the positive long-run effects of training, as found e.g. by Crépon, Ferracci and Fougère (2012). Being sanctioned in addition to a training program reduces its positive effect substantially.

Third, the empirical findings so far rather suggest another option: The reduction of monitoring and sanction intensity, given an average level of training/"carrot" use, would be more beneficial for the individual's earnings. Another positive alternative, albeit a bit less favorable, would be the combination of a low training and a high sanction level. Note, however, that these findings and the model specification are still at a preliminary state. The analysis needs to be enriched by alternative specifications and a series of robustness checks.

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Figures

Figure 1: Observed policy mixes by PES: scatter plot; correlation (locally weighted regression smoother)

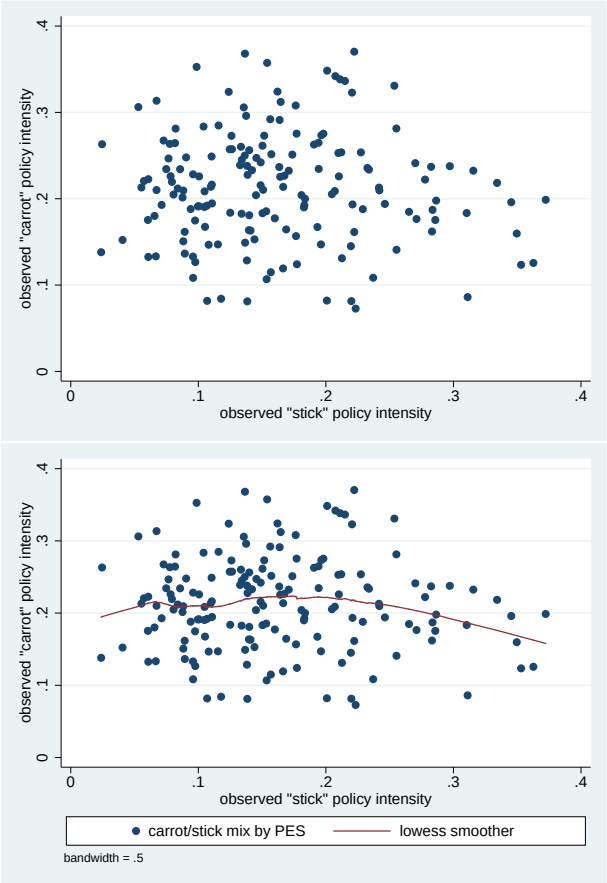


Figure 2: Observed policy mixes by PES, conditional on X: scatter plot of predicted policies, given baseline values of X

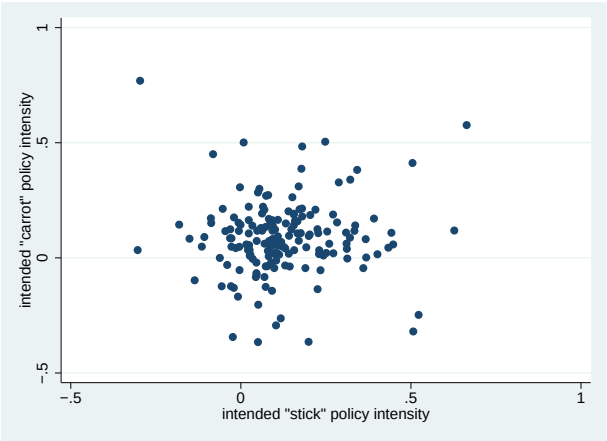


Figure 3: Observed vs. Intended policies by PES: "carrots"; "sticks"; overlap of observed and intended policy mixes

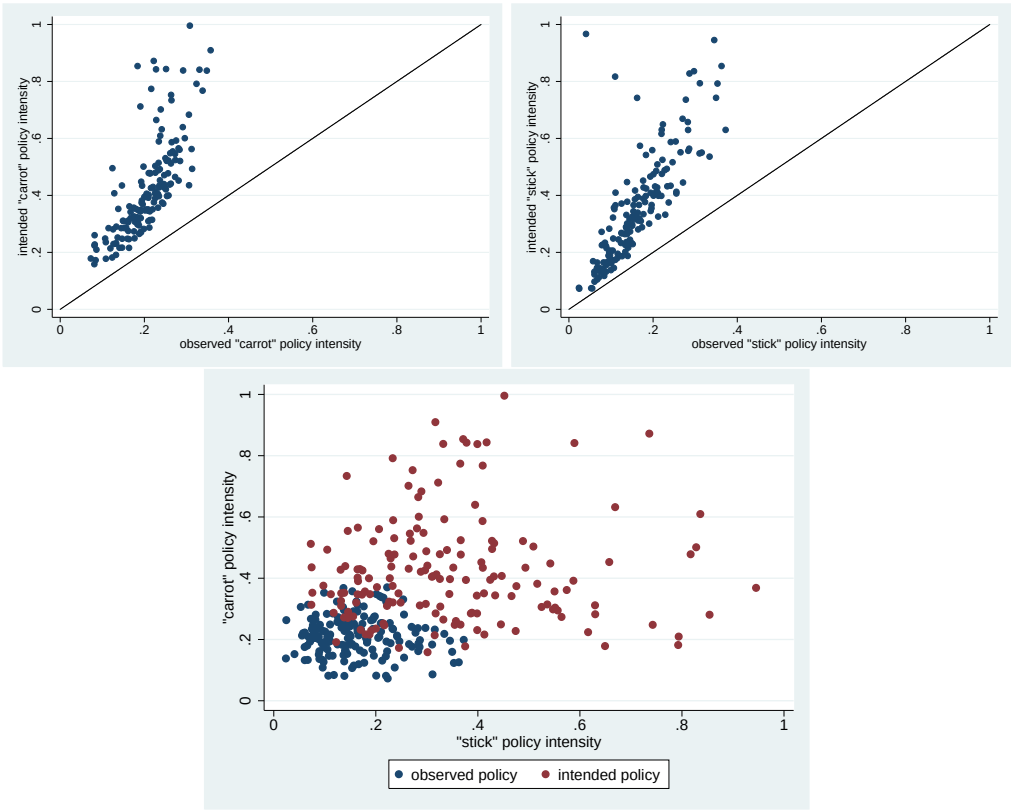


Figure 4: Intended policy mixes by PES: scatter plot; correlation (locally weighted regression smoother); conditional on X

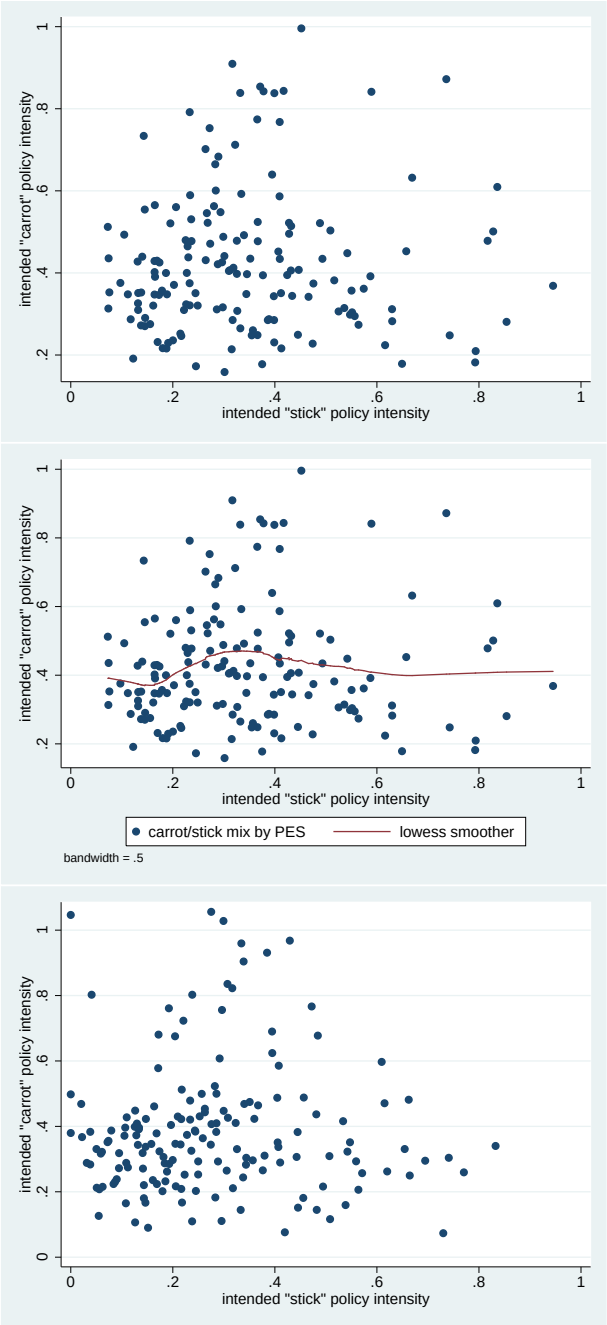
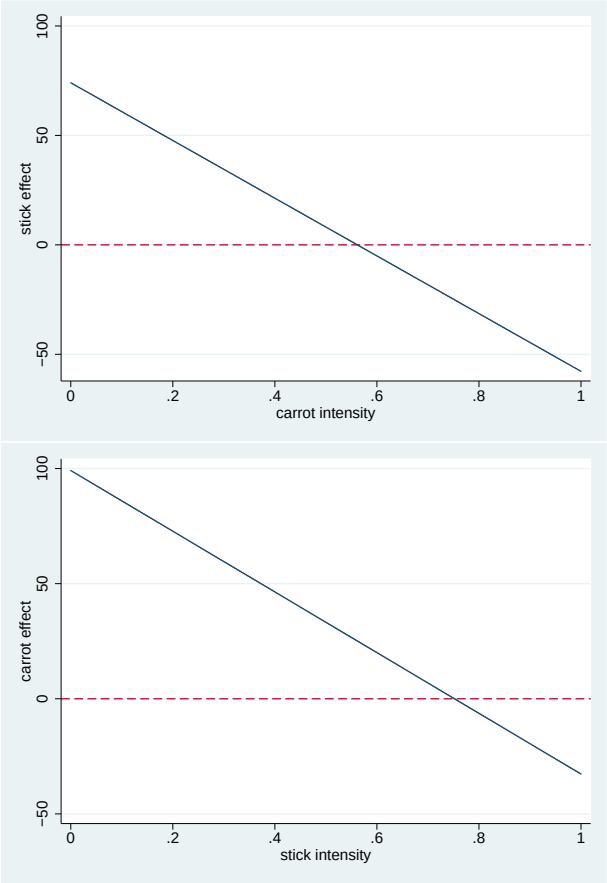


Figure 5: Total effects of "carrots" & "sticks": isolated effects plus interactions. Stick/carrot policy effects as a function of intended intensities $\hat{P}^c/\hat{P}^s$



Tables

Table 1: Observed (ex-ante) policy intensities: frequency of policy imposition within the first 6 months of unemployment)

	policy intensities	
	"carrot"	"stick"
mean	0.213	0.162
median	0.212	0.150
s.d.	0.064	0.075
min	0.073	0.024
max	0.370	0.372
PES	168	168

Source: UIR-SSR Database

Table 2: Intended (ex-ante) policy intensities: predicted frequency of policy imposition within the first 6 months of unemployment)

		policy intensities	
		"carrot"	"stick"
int.	mean	0.476	0.368
	median	0.401	0.324
	s.d.	0.297	0.240
obs	mean	0.213	0.162
	median	0.212	0.150
	s.d.	0.064	0.075
	PES	168	168

Note: The observed policy intensities are reproduced for reasons of convenience. *Source:* UIR-SSR Database

Table 3: 2nd stage results on average monthly earnings – basic OLS estimation: ex-post treatment effects (δ) and ex-ante intended policies (π) and their interactions

	(1)	(2)	(3)	(4)
outcome:	*13-42 mt*		*post-unempl.*	
	baseline	*interactions*	*baseline*	*interactions*
δ^c	134***	134***	-91***	-103***
	(16)	(18)	(13)	(15)
δ^s	-432***	-433***	-436***	-451***
	(15)	(16)	(12)	(14)
π^c	5.2*	24.8***	4.9**	25.6***
	(2.9)	(5.6)	(2.5)	(4.8)
π^s	-30.9***	-12.6**	-21.7***	-2.5
	(3.6)	(5.7)	(3.1)	(4.9)
$\delta^c \cdot \delta^s$		-11.9		51*
		(36.9)		(28)
$\pi^c \cdot \pi^s$		-40.5***		-42.6***
		(9.5)		(8.0)
obs	164'615	164'615	210'635	210'635
R^2	37.16	37.17	35.07	35.08

Note: In all the regressions (1) to (4) we control for a wide range of socio-demographic and benefits-related variables as well as time dummies. See sections 3 and 4 for details. The dependent variable "13-42 mt" represents the monthly average of the employment earnings generated over these months (after unemployment entry); the sample is restricted to individuals who are unemployed for max. 12 months. The dependent variable "post-unempl." consists of the monthly average of the employment earnings gained in the period after exit from the first unemployment spell; the sample is not restricted w.r.t. unemployment duration. The policy effects (π) are calculated for a change of the policy intensity by 0.1. *Source:* UIR-SSR Database

Table 4: 2nd stage results on average monthly earnings – fixed-effects estimation: ex-post treatment effects (δ) and ex-ante intended policies (π) and their interactions

	(1)	(2)	(3)	(4)
outcome:	*13-42 mt*		*post-unempl.*	
	FE:m	*FE:m · t*	*FE:m*	*FE:m · t*
δ^c	146***	147***	91***	91***
	(21)	(19)	(21)	(17)
δ^s	-420***	-417***	-311***	-308***
	(17)	(17)	(15)	(15)
π^c	69.6***	105.2***	71.1***	**99.1*** **
	(26.3)	(22.7)	(25.1)	(20.3)
π^s	53.1*	89.5***	48.8*	**74.1*** **
	(29.3)	(29.6)	(25.6)	(26.7)
$\delta^c \cdot \delta^s$	-40.0	-39.7	-71.2**	-67.9**
	(34.9)	(37.7)	(28.9)	(29.0)
$\pi^c \cdot \pi^s$	-105.3***	-146.9***	-97.5***	**-131.8*** **
	(38.9)	(33.6)	(35.3)	(30.6)
obs	164'615	164'615	210'635	210'635
R^2 within	36.09	36.03	34.65	34.61
R^2 overall	36.98	36.68	35.46	35.24

Note: In all the regressions (1) to (4) we control for a wide range of socio-demographic and benefits-related variables as well as time dummies. See sections 3 and 4 for details. The dependent variable "13-42 mt" represents the monthly average of the employment earnings generated over these months (after unemployment entry); the sample is restricted to individuals who are unemployed for max. 12 months. The dependent variable "post-unempl." consists of the monthly average of the employment earnings gained in the period after exit from the first unemployment spell; the sample is not restricted w.r.t. unemployment duration. The policy effects (π) are calculated for a change of the policy intensity by 0.1. *Source:* UIR-SSR Database

Table 5: Marginal effects by level of policy intensity: moving from P^c_{low}/P^s_{low} to ...

	P^c_{low}	P^c_{mid}	P^c_{high}
P^s_{low}	0	0	0
	0	627***	597***
P^s_{mid}	218**	-478***	-281***
	0	-69***	98***
P^s_{high}	355***	-323***	-322***
	0	-52***	-81***

Note: The first line in each cell reports the "stick" effect, the second line the "carrot" effect. Intensity thresholds that distinguish the low/middle/high policy levels: .239/.403 for "sticks", .344/.478 for "carrots".

Source: UIR-SSR Database